

Back to Background Risk? *

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Abstract

Estimating the effect of background risk on individual financial choices faces two challenges. First, the identification of the effect requires a measure of *at least one* component of human capital risk that qualifies as "background" (a risk an individual cannot diversify away or avoid). Absent this, estimates suffer from measurement error and omitted variable bias. Moreover, measures of background risk must vary over time to eliminate unobserved heterogeneity. Second, solved identification, the size of the effect requires knowledge of the size of *all* the background risk actually faced. Existing estimates are problematic because most of the measures of background risk used fail to satisfy the "non-avoidability" requirement. This creates a downward bias which is at the root of the small estimated effect of background risk. To bypass the identification problem we match workers and firms and use the variability in the profitability of the firm that is passed over to workers to obtain a measure of risk that we show to be hardly avoidable. We then rely on this measure to instrument total variability in individual earnings and find that the marginal effects of background risk is much larger than estimates that ignore endogeneity. Using these estimates we bound the economic effect of human capital background risk and find that its overall effect is contained, not because its marginal effect is small but because its size is small. And size is small because firms provide substantial wage insurance.

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1 Introduction

This paper revisits the empirical importance of background labor income risk for explaining portfolio choice. It uses rich administrative household data from Norway to overcome the identification challenges that have plagued most of the empirical work on the subject.

The topic of background risk has a long history in macroeconomics and household finance. Starting with Ayiagari (1994), a large literature has studied how the presence of uninsurable idiosyncratic income risks in an incomplete market setting affects the patterns of individual and aggregate savings, consumption and portfolio allocations over the life cycle, as well as the behavior of asset prices. The theory argues that under plausible preference restrictions consumers who face uninsurable income risk respond by accumulating precautionary savings, raising labor supply, or more generally changing the pattern of human capital accumulation (see, eg., xxx). Furthermore, people reduce exposure to risks that they can avoid. In particular, they change the asset allocation of their financial portfolio by lowering the share invested in risky assets, thus tempering their overall risk exposure (e.g. Merton, 1971; Kimball, 1993; Costantinides and Duffie, 1996; Heaton and Lucas, 1996; Heaton and Lucas, 2000b).

Inspired by these theoretical predictions and the practical importance for many households of income risk, one set of papers have incorporated background risk in calibrated models of portfolio allocation over the life cycle and explored its ability to reproduce the patterns observed in the data (e.g. Viceira, 2001; Cocco, Gomes, and Maenhout, 2005; Heaton and Lucas, 2000a; Polkovnichenko, 2007). Another strand of papers has tried to assess the empirical importance of uninsurable income risk in explaining portfolio heterogeneity. A fair characterization of both strands of literature is that the effect of background labor income risk on portfolio allocation, though carrying the sign that theory predicts, is relatively small in size. As a consequence, the background risk channel has been dismissed as an important determinant of household portfolio choices or as a candidate explanation for asset pricing puzzles (such as the equity premium puzzle, see e.g. Cochrane, 2006).

In this paper we revisit the relevance of background labor income uncertainty for people's willingness to bear financial risk and question the conventional wisdom of the empirical literature. We argue that the empirical literature suffers from an identification problem that also affects calibration models of life cycle savings and portfolio allocation. Identification of the effect of uninsurable income risk is arduous and its quantification problematic. Identification is arduous for at least three reasons.

First, in order to identify the marginal effect of uninsurable uncertainty in returns to human capital one needs exogenous variation in background risk. A popular solution (e.g. Heaton and Lucas, 2000; Angerer, Xiaohong and Pok-Sang Lam, 2009; Betermier et al., 2011; Palia et al., 2014) is to measure background risk with the variance of (residual) log earnings or log income typically obtained from survey data on households (e.g., the PSID in the US). Another is to use subjective expectations of future incomes (e.g. Guiso et al., 1996; Hochguertel, 2003) or health status (which may be particularly relevant for the elderly, Edwards, 2008). Yet, as a recent literature suggests most of the variation in earnings is predictable and a reflection of

choice (e.g. Heckman et al., 2005; Primiceri and van Rens, 2009; Low, Meghir and Pistaferri, 2010; Guvenen and Smith, 2014); on the other hand, there are long-standing reservations regarding the validity and content of subjective expectations data, as well as practical data problems: subjective expectations data are rarely available alongside longitudinal data on assets. In sum, isolating background risk is not trivial. The empirical strategies described above introduce a sort of errors-in-variable problem that bias towards zero the estimated effect of earnings risk on portfolio choice.¹

Second, notwithstanding the problem of obtaining a conceptually correct measure of background risk, other econometric issues may make estimates of the effect of background risk on portfolio choice unreliable. For example, most of the evidence on the effect of background risk comes from cross sectional data, suffering from unobserved heterogeneity bias (i.e., unobserved risk aversion determines both income risk through occupational choice as well as the composition of one’s asset portfolio). Dealing with unobserved heterogeneity is difficult, as one requires panel data with variation over time in background risk, which is rare.²

A final issue is that most of the empirical literature uses survey data on assets. Survey data on assets are notoriously subject to measurement error and rarely sample the upper tail of the distribution (which is key, given the enormous skewness of the distribution). Moreover, both in survey and administrative data there is non-negligible censoring of stockholding because several investors choose to stay out of the stock market.

In this paper we develop an identification strategy that overcomes these problems and obtain appropriate data to implement it. First, we rely on idiosyncratic and unpredictable variation in the performance of the firm a person works for and on a clear identification of the pass-through of firm shocks to the worker’s wages in order to isolate one component of labor income that qualifies as background risk - e.g., one that cannot be avoided or insured. This is the component of the wage that fluctuates with idiosyncratic variation in firm performance, reflecting partial wage insurance within the firm. We show that this component can be used as an instrument for total residual labor income variation which allows to deal with the measurement error in background risk. Because this component varies over time, the availability of long panel data on firms and their workers makes it possible to deal with unobserved heterogeneity, thus circumventing the second obstacle to achieve identification.

We implement these ideas using administrative data for the whole population of Norway. Because Norway imposes a tax on wealth, each year Norwegian taxpayers must report their assets, item by item, to the tax authority. The data are available for a long time span and cover the entire wealth distribution, including

¹This is the case if the error is classical. If the measurement error is non-classical (i.e., when there is systematic overestimation of background risk), signing the extent of bias is more difficult.

²Betermier et al. (2011) in one exception. They deal with unobserved heterogeneity by looking at people who change industry and exploiting differences in income volatility across industries. They find that indeed people who move from low to high volatility industries reduce exposure to stocks significantly and interpret the finding as consistent with hedging. While this marks progress, movers solve one issue but raise another: moving is endogenous and it is hard to dispel the doubt that the same factor that triggers moving may be affecting rebalancing (to be fair, the authors are aware of this and show evidence that based on observables, movers and stayers share similar characteristics; but the problem is the change in time varying unobservables – such as risk preferences). In addition, the measure of earnings volatility they use – the industry mean of the volatility of net earnings – reflects both components that qualify as background risk and others that do not, as well as heterogeneity across industries in the cross-sectional distribution of time-varying determinants of individual earnings growth, besides cross-sectional heterogeneity in shocks to individual earnings growth. This makes it hard to estimate the economic effect of earnings risk.

the very top tail. These data allow us to compute portfolio shares at the household level. In addition we can merge the wealth data with matched employer/employees data from the social security archives. The latter contain information on workers' employment spells and earnings at each job, as well as measures of firm performance, mass layoffs, and closures due to firm bankruptcy. These data allow us to measure how workers' earnings respond to permanent and transitory shocks to the performance of the firm. Since the pass-through is non-zero (i.e., there is only partial insurance), we use measures of firm volatility to instrument total earnings variability when estimating the households portfolio shares in risky assets. In addition we complement the earnings variability measure of background risk with a measure of exposure to the risk of mass layoffs and firm closure, providing exogenous variation in the risk of job loss.

We document a number of important findings. First, ignoring the endogeneity of wage variability but accounting for unobserved heterogeneity, we reproduce the small marginal effect of background labor income risk on the portfolio allocation to risky assets that characterizes the empirical literature. However, when we instrument earnings variability with the firm-variation component of background risk, we find that the marginal effect is about 40 times larger. This suggests a large downward bias in prevailing estimates of the effect of background risk and, in principle, a potentially more important role for human capital risk in explaining portfolio decisions and assets pricing. In contrast, we find very small effects of employment loss risk, possibly because this type of risk is insured through generous social insurance programs in Norway.

As noticed above, empirical estimate of the effect of background risk on portfolio allocations face also a problem of censoring (a large fraction of investors hold no risky assets in their portfolio). Simultaneously accounting for censoring, fixed unobserved heterogeneity, and endogeneity is computationally unfeasible. The very few estimators that have been proposed in the literature are based on very strong assumptions that are unlikely to hold in our specific application. Nevertheless, assuming the various biases due to unobserved heterogeneity, endogeneity of wage variance and censoring are linear, we can gauge their sizes and obtain a back-of-the-envelope estimate of the marginal effect of background wage risk on the financial portfolio. When we do this we still find an estimate that is 30 times larger than the OLS (fixed effect) estimate. Using the estimated parameter we provide some bounds on the effects of background risk. Clearly, this depends both on the estimated marginal response to a change in background risk as well as on what is assumed to be background risk. Since the latter is not fully identified we provide an interval of its potential effect: an upper bound obtained assuming that all residual variation in earnings is background risk and a lower bound assuming that only the uninsured firm component is background risk. When we do so, we estimate an effect on the risky share between 1 and 4 percentage points, or between 5.6 and 22 percent of the sample mean of the portfolio share (with the actual effect being closer to the lower than the upper bound).

Thus, when quantifying the effect of background risk on portfolio choice, our conclusions are not different from what found in the existing literature. The key to understanding this apparently puzzling result is that the effect of risk on portfolio choice depends on two things: the elasticity of portfolio choice with respect to the risk and the size of the risk itself. Our estimates suggest that the elasticity is higher and the true

background risk smaller than typically found, while in the existing literature the opposite is true: estimated risk is overstated and the sensitivity is downward biased, thus reaching the right conclusion but for the wrong reasons. In turn, human capital risk is contained because firms provide workers with substantial insurance.

The rest of the paper proceeds as follows. Section 2 reviews the empirical literature and highlights our contribution. In Section 3 we set up an econometric framework where we illustrate the problems faced by empirical researchers when the goal is to identify the effect of background risk on financial decisions, and show how we tackle them. Section 4 describes the data sources and discusses some descriptive evidence. Section 5 presents our measures of background risk. Section 6 shows the marginal estimates of the effect of the latter on people’s financial decisions and discusses the economic effect of background risk on financial choices. Section 7 presents some robustness tests and extensions. Section 8 concludes.

2 Literature Review

TBA

3 Econometric Framework

Consider the following empirical model for the portfolio share in risky assets:

$$S_{it} = \mathbf{X}'_{it}\beta + \lambda B_{it} + r_i + \varepsilon_{it} \tag{1}$$

where S_{it} is the share of risky assets in individual i ’s financial portfolio at time t , X_{it} are socio-demographic characteristics related to portfolio choice (such as gender, education, total wealth, etc.), B_{it} a measure of background uninsurable risk, r_i an unobserved individual fixed effect (which may capture heterogeneity in risk tolerance), and ε_{it} an error term. The empirical literature has used some variant of the above model, coupled with some strategy to measure background risk. Success in identifying the parameter λ rides on the ability to account for the unobserved heterogeneity r_i and, as we show below, on the properties of measured background risk.

The typical empirically strategy for measuring background risk in returns to human capital consists of writing a wage process like:

$$\ln y_{ijt} = \mathbf{Z}'_{it}\gamma + \theta_f f_{jt} + v_{it}$$

where y_{ijt} are earnings earned by worker i in firm j at time t , \mathbf{Z}_{it} is a vector of deterministic wage determinants, f_{jt} a firm-specific shock, and v_{it} a component of worker’s earnings volatility that is partly under the control of the agent and unrelated to the fortunes of the firm (e.g., unobserved changes in general human capital). We assume that the error components f_{jt} and v_{it} are mutually uncorrelated. Firm shocks are passed through wages with pass-through coefficient θ_f . We can decompose the evolution of wages into two

components - one that is avoidable or evolves in an anticipated manner, and one that is unavoidable or evolves in an unanticipated way (shocks). Hence:

$$\ln y_{it} = \underbrace{\mathbf{Z}'_{it}\gamma + (1 - \theta_v)v_{it}}_{\text{Anticipated/Avoidable}} + \underbrace{\theta_v v_{it} + \theta_f f_{jt}}_{\text{Unanticipated/Unavoidable}} = A_{it} + U_{it}$$

The separation of v_{it} in a component that is anticipated/avoidable and one that is not comes from recognizing that part of what the econometrician identifies as “background risk” can be variability in earnings that reflects, at least in part, individual choices rather than risk that the consumer has to bear and that she would avoid if feasible. For instance, time out of the labor market does not necessarily reflect unemployment risk, but could be time invested in human capital accumulation. Some volatility can be generated by people working longer hours in response to adverse financial market shocks affecting the value of their portfolio. A recent literature in labor economics suggests that a non-negligible fraction of year-to-year fluctuations in labor earnings reflect heterogeneity or choice, rather than risk (see Heckman et al., 2005; Primiceri and van Rens, 2009; Low, Meghir and Pistaferri, 2010; and Guvenen and Smith, 2014).

It follows that the "true" measure of background risk should be:

$$\begin{aligned} B_{it} &= \text{var}(U_{it}) \\ &= \theta_v^2 \text{var}(v_{it}) + \theta_f^2 \text{var}(f_{jt}) \\ &= \rho_v V_{it} + \rho_f F_{it} \end{aligned} \tag{2}$$

where V and F are the worker-related and firm-related background risk variance components.

Unfortunately, this is not what is typically used in the empirical literature. First, since in survey data wages are measured with error ξ_{it} , observed wages is:

$$\ln y_{it}^* = \ln y_{it} + \xi_{it}$$

Second, the measure of background risk that is used is $\sigma_{it}^2 = \text{var}(\ln y_{it}^* - \mathbf{Z}'_{it}\gamma) = V_{it} + \rho_f F_{it} + \sigma_{\xi}^2 = B_{it} + v_{it}$, where $v_{it} = (1 - \rho)V_{it} + \sigma_{\xi}^2$. This differs from the true one because it includes the variance of measurement error and because it assumes that the volatility of the worker component v_{it} is unavoidable risk, while in fact a fraction of it reflects choice.

An OLS regression of S_{it} on the measure σ_{it}^2 (omitting individual fixed effects, r_i) gives inconsistent estimates of the sensitivity of portfolio choice to background risk. Indeed:

$$p \lim \hat{\lambda}_{OLS} = \lambda \frac{\left(\rho_v \text{var}(V_{it}) + \rho_f^2 \text{var}(F_{it}) \right)}{\left(\text{var}(V_{it}) + \rho_f^2 \text{var}(F_{it}) + \text{var}(\sigma_{\xi}^2) \right)} + \frac{\text{cov}(r_i, V_{it} + \rho_f F_{it})}{\left(\text{var}(V_{it}) + \rho_f^2 \text{var}(F_{it}) + \text{var}(\sigma_{\xi}^2) \right)}$$

The first term resembles a measurement error bias: background risk is mis-measured both because all variability in v_{it} is interpreted as risk, and because there is unaccounted noise that agents don't act upon. On the other hand, if higher risk tolerance is the only element of unobserved heterogeneity and it is associated to both less conservative portfolios and a more volatile wage process, then the second term is positive and may well counterbalance the "measurement error/conceptual risk" bias. Consider for example using occupation dummies to measure background risk. Empirically, self-employed have greater year-to-year wage volatility, while public employees face lower wage and employment risk. If allocation to occupations were random, theory would predict that the high risk types should hold more conservative portfolios than the low risk types. But typically, this is not what is found in the data. The self-employed invest more in stocks and have greater income volatility (**CITATION**). The "puzzle" can be explained by the fact that there is sorting into occupations based on attitudes towards risk which confounds the impact of background risk on portfolio choice because more risk averse individuals choose both low risk occupations and more conservative portfolios. In panel data in which one can control for fixed effects, this second bias term disappears and the sensitivity of portfolio choice to risk is downward biased, i.e.:

$$p \lim \widehat{\lambda}_{FE} = \lambda \frac{(\rho_v \text{var}(V_{it}) + \rho_f^2 \text{var}(F_{it}))}{(\text{var}(V_{it}) + \rho_f^2 \text{var}(F_{it}) + \text{var}(\sigma_\xi^2))} \quad (3)$$

Our strategy will be to recognize that the very notion of "background" risk requires that it is exogenous and that agents have little control over it. We use firm-derived measures of wage (and employment) risk to isolate one exogenous component of the variance of individual returns to human capital and use this as an instrument for the total variance of (residual) earnings σ_{it}^2 . In the above framework, this boils down to using F_{it} as an instrument for σ_{it}^2 (while controlling for fixed effects in the risky asset share equation).

To illustrate this strategy, suppose we have data on firm-specific shocks such that we can obtain an estimate of F_{it} . The latter qualifies as an instruments for the error-ridden measure of background risk σ_{it}^2 . First, under the assumption that the firm only offers partial wage insurance to the workers (an assumption supported by the evidence in Section 5), F_{it} has predictive power for σ_{it}^2 ; second, once occupational sorting is neutralized by controlling for individual fixed effects, F_{it} is orthogonal to the residual in the portfolio allocation decision as it only reflects variability in the productivity of the firm. It is easy to show that this strategy identifies the effect of background risk on portfolio choice as:

$$\begin{aligned} p \lim \widehat{\lambda}_{IVFE} &= p \lim \frac{\text{cov}(S_{it}, F_{it})}{\text{cov}(\sigma_{it}^2, F_{it})} \\ &= p \lim \frac{\text{cov}(\lambda(\rho_v V_{it} + \rho_f F_{it}) + r_i + \varepsilon_{it}, F_{it})}{\text{cov}(V_{it} + \rho_f F_{it} + \sigma_\xi^2, F_{it})} \\ &= \lambda \end{aligned} \quad (4)$$

It is important to notice that the reduced form estimate of the share of risky assets onto firm volatility does not identify the sensitivity of the portfolio allocation to background to risk, but

$$\begin{aligned}
p \lim \widehat{\lambda}_{RFFE} &= p \lim \frac{\text{cov}(S_{it}, F_{it})}{\text{var}(F_{it})} \\
&= p \lim \frac{\text{cov}(\lambda(\rho_v V_{it} + \rho_f F_{it}) + r_i + \varepsilon_{it}, F_{it})}{\text{var}(F_{it})} \\
&= \lambda \rho_f < \lambda
\end{aligned}$$

as firm shocks pass through only partially to wages. We stress this case because Hung, Liu, Tsai and Zhu (2014) propose precisely this type of exercise, assigning to individual investors the stock market volatility of the firm they work for as a measure of background income risk and estimating the portfolio response to this measure. This strategy ignores that the firm component enters with a pass-through coefficient $\rho_f < 1$. To be able to identify λ from the reduced form estimate one needs also to separately identify ρ_f . This point is missed by Hung, Liu, Tsai and Zhu (2014), and would only deliver consistent estimates of λ if the worker "owned the firm". On the other hand, papers that try to estimate the effect of background risk on portfolio choices using survey datasets such as the SCF or PSID, cannot identify its effect as they lack access to matched employer-employee data to estimate F_{it} and ρ_f .

The last issue we need to address is the fact that the dependent variable is censored: A non-negligible fraction of households have no risky assets in their financial portfolio. One way to handle this issue is to assume that equation (1) represents the *latent* demand for risky assets, but what is observed is a censored version of it:

$$S_{it}^c = S_{it} \times \mathbf{1}\{S_{it} \geq 0\}$$

Using a fixed effect-IV estimator in cases in which the dependent variable is censored implies that (4) no longer provides a consistent estimator. In principle, one could apply an estimator that deals with all three problems at once (fixed effects, endogenous regressors, and censoring of the dependent variable), such as the extension of the standard Tobit estimator considered by Honoré and Hu (20xx). In practice, this estimator (or bias-corrections of the type proposed by Fernandez-Val and Vella, 2008) does not work well in our administrative large-scale data set. We will instead consider some back-of-the-envelope exercises that compare the various estimators to get some heuristic knowledge about the true value of the parameter of interest λ .

In general, the data requirement for identifying the effect of background income risk are quite formidable. Matched employee-employer data are needed to obtain a proper measure of (at least one component of) background risk; to account for individual fixed effects the data need to have a panel dimension, and the panel needs to be long enough to generate variation over time in background risk. Finally, inference on

portfolio decisions is greatly facilitated if assets are measured without error, a requirement that is rarely met in household survey where measured financial assets are plagued with reporting error, under-reporting and non-reporting.

In the empirical analysis we use administrative data on wages and financial assets, where measurement error is virtually absent. These data are available for over 20 years and we can identify the employer: hence we are able to construct a measure of F_{it} that is individual-and time-varying. Because the data is a panel we can control for fixed effects and thus purge the estimates from unobserved heterogeneity correlated with measures of background risk while simultaneously driving portfolio choice (e.g. risk tolerance).

4 Data

To study whether households hedge (unavoidable) labor income risk by changing their risky financial portfolio, we employ high-quality data from Norway consisting of eight separate databases. All of our data are collected for administrative purposes, which essentially eliminates concerns about measurement error. The data sets can be linked through unique identifiers assigned to each individual and firm in Norway (similar to SSN's and EIN's for the US, respectively). Here we provide a broad description of these data sets, which unless otherwise specified cover the time period 1995-2010; the Data Appendix illustrates the features of the data in greater details and describes the sample selection.

The **Central Population Register** contains basic end-of-year demographic information (i.e., gender, birthdate, county of residence, and marital status) on all registered Norwegian residents. Importantly, it contains family identifiers allowing us to match spouses and cohabiting couples who have a common child. We merge this data set with information on educational attainment (from the **National Educational Database**) and information on end-of-year financial assets from tax records (**Administrative Tax and Income Register**).

Because Norway levies a wealth tax, each year Norwegians must report to the tax authority the value of all real and financial assets holdings as of the end of the previous calendar year. Data on traded financial assets, for a broad spectrum of assets categories, are reported (at their market value) directly by the financial institution that has the assets in custody (e.g., a mutual fund or a deposit bank). This has two main advantages: first, given the administrative nature of the data, financial assets are measured with virtually no error; second, because they are reported by a third party, the scope for tax evasion is absent. For stocks of non-listed and non-traded companies, asset valuation is based on annual reports submitted to the tax authorities by the companies themselves. If the tax authorities find the proposed evaluation unrealistically low, they can start a formal audit process, which limits the scope for undervaluation.

Besides the asset values data set, we have also access to the **Register of Shareholders** for the period 2004 to 2010. This register reports, on an individual basis, the number and value of individual stockholdings, together with the ID of the firm that issues the stock. This allows us to account for direct stockholding in the company where the worker works, a feature that turns out to be useful when we discuss various robustness

checks (Section 7).

Because we focus on the household as our decision unit, we aggregate assets holdings at the level of the family by summing up asset values across family members using the unique household ID described above.³ We then classify financial assets holdings into "risky assets" (R) - the sum of directly held stocks in listed and non-listed companies and mutual funds with a stock component - and "risk-free assets" (RF) - the difference between total financial assets and risky assets, which includes bank deposits, government bonds and money market funds - and define the portfolio risky assets share for each households $S_{it} = \frac{R_{it}}{R_{it} + RF_{it}}$. Because of limited stock market participation, $S_{it} = 0$ for non-participants, giving rise to censoring in our left-hand side variable. On average, in 2010, about 47% of Norwegian households hold stocks. Participation in the risky assets market has increased substantially after 1995 (see Figure 1). Including non-participants, the average portfolio share in risky assets is around 20% in 2010; conditional on participation, the average Norwegian household invests about 20% of its portfolio in risky assets in 2010, up from 14% in 1995.

Consistent with what found in the literature (see Guiso and Sodini, 2013), there is substantial cross sectional variation in the conditional risky share; as Figure 2 shows, its distribution spans the entire gamut – from people holding very small amounts to people investing their entire portfolio in stocks. The question we tackle in this paper is how much of this heterogeneity can be explained by background risk, if at all. Table 1 shows summary statistics for the portfolio data and the financial wealth of our Norwegian sample (data refer just to 2010 for simplicity).

The link between the workers and the firms, the **Employer-Employee Register**, contains for each worker data on all employment spells with each employer, and the compensation received. This allows us to trace the working history of each worker as they move across firms and occupational status.

We combine the Employer-Employee Register with the **Central Register of Establishments and Enterprises** and the **Balance Sheet Register** with the unique firm ID present in all of these data sets. The former contains data on industry classification and institutional sector, whereas the other contains accounting data on the firm assets, liabilities and income statement. Among other items, it includes data on the firm's value added and sales that we use to construct (statistically) shocks to the firm profitability.

Lastly, on the firm side the **Register of Bankruptcies** contains information on the date a firm enters a bankruptcy proceeding and is declared insolvent (if any). We use this data set to identify episodes of firm closure and enrich the measure of background risk based on workers earnings variance with a measure of employment risk. In fact, the total variance of income comes partly from wage variability conditional on working, and partly from income variability conditional on losing the job.

Combining these three firm level data set with the Employer-Employee Register allows us to assign each worker in the sample the variability of the firm he/she works for (which depends on the pass-through coefficient estimated in Section 5), and to obtain a measure of background risk that is theoretically more appropriate. Similarly, we can assign each worker the probability of job loss at that firm. Because our

³In Norway married couples are taxed jointly when it comes to wealth tax, but individually for income tax purposes.

measure of background risk depends on shocks to the firm that are in some degree passed over to workers, we focus on workers in the private sector (30% of workers are employed in the public sector in Norway). If there are multiple earners in the household (and both work in the private sector) we measure background risk with the one faced by the primary earner.

The last step in this data preparation involves merging this elaborate Employer-Employee Register back onto the household data. This gives a rich picture of the financial portfolio allocation for a panel of households present in the sample for about 15 years. It also gives us the opportunity to obtain measures of earnings and employment risks that individuals cannot easily avoid (i.e., induced by the changing fortunes of the firms they work for).

5 Measuring background risk

To identify a measure of labor income risk that can be arguably considered as unavoidable, we focus on shocks that change firm profitability, and hence may potentially result in variation in workers' pay (conditional on retaining the job) or even in job loss in more extreme cases. The validity of this strategy requires that: a) we measure firm-related shocks; b) we identify how much of these shocks are passed over to the worker's wages; c) show evidence that workers have to bear these shocks and cannot undo them easily, for instance by moving to another firm. This turns out to be feasible only in settings in which job market frictions are limited.

5.1 Earnings uncertainty: firm shocks and pass-through

Following Guiso, Pistaferri and Schivardi (2005), we measure firm j performance with its value added, VA_{jt} , and assume its log evolves according to the process

$$\begin{aligned}\ln VA_{jt} &= \mathbf{X}'_{jt}\varphi + Q_{jt} + u_{jt} \\ Q_{jt} &= Q_{jt-1} + v_{jt}\end{aligned}$$

where \mathbf{X}_{jt} is a vector of observables that captures the predictable component of firm's performance. The shock component is the residual term $Q_{jt} + u_{jt}$, the sum of a random walk component Q_{jt} with permanent shock v_{jt} and a transitory component u_{jt} . This process fits the data quite well [TBA].

Next, we model the earnings y_{ijt} (in logs) of worker i in firm j in a similar vein as a linear function of a predictable component that depends on a vector of workers observed characteristics, \mathbf{Z}_{ijt} , an individual random walk and transitory component, and a component that depends on the firm shocks with transmission coefficients θ^T and θ^P , respectively for transitory and permanent firm value added shocks. Hence:

$$\begin{aligned}\ln y_{ijt} &= \mathbf{Z}'_{ijt}\boldsymbol{\gamma} + P_{ijt} + \eta_{ijt} + \theta^T u_{jt} \\ P_{jt} &= P_{jt-1} + \varepsilon_{ijt} + \theta^P v_{jt}\end{aligned}$$

For firm-related background risk to matter, θ^T and θ^P must be positive and significant. That is, firms must pass over to the workers some of the shocks to their performance and not offer them full wage insurance. Using Italian data, Guiso et al. (2005) show that firms offer partial wage insurance to permanent and transitory shocks - that is the estimated values of θ^T and θ^P are positive but smaller than one - and that the pass-through is larger for permanent shocks. Their result has been shown to hold also in other countries, such as Portugal (Cardoso and Portela, 2009), Germany (Guertzen, 2010), Hungary (Katay, 2008), Sweden (Friedrich et al., 2015), Belgium (Fuss and Winttr, 2008), France (Biscourp et al., 2005) and across US industries (Lagakos and Ordonez, 2011) with remarkably similar patterns.

To establish the degree of pass-through of firm shocks to wages in Norway we use Guiso et al. (2005) methodology.⁴ Define the unexplained growth of firm value added, g_{jt} , and of workers' earnings, ω_{ijt} as :

$$\begin{aligned}g_{jt} &= \Delta(\ln V A_{jt} - \mathbf{X}'_{jt}\boldsymbol{\varphi}) \\ \omega_{ijt} &= \Delta(\ln y_{ijt} - \mathbf{Z}'_{ijt}\boldsymbol{\gamma})\end{aligned}$$

Guiso et al. (2005) show that the pass-through coefficients θ^T and θ^P can be identified by simple IV regressions:

$$\begin{aligned}\theta^T &= \frac{\text{cov}(\omega_{ijt}, g_{jt+1})}{\text{cov}(g_{jt}, g_{jt+1})} \\ \theta^P &= \frac{\text{cov}(\omega_{ijt}, g_{jt-1} + g_{jt} + g_{jt+1})}{\text{cov}(g_{jt}, g_{jt-1} + g_{jt} + g_{jt+1})}\end{aligned}$$

Accordingly, we run regressions for firm value added and workers' wages; in the first we control for year dummies, area dummies, sectoral dummies, log firm size, and in the second for year dummies, a quadratic in age, dummies for the quantity and type of schooling, firm size, dummies for whether the

⁴Needless to say, the possibility that firm-specific shocks are passed over to worker earnings requires that wages at least partly are determined at the firm level. This in turn depends on the structure of wage bargaining. In Norway, like in other Nordic countries, union density and coverage are high. However, in the private sector the coverage of collective bargaining agreements is actually "only" 55%, leaving ample room for many workers to have wages set outside the conventional framework. Even for workers whose wages are negotiated centrally, there is still ample room for local negotiation (or wage drift). Moreover, for white collars, collective bargaining only determines the procedures for setting wages, while the actual level of wages is negotiated on an individual basis. Finally, as reported by Loken and Stokke (xxxx), the share of private sector employees with a component of pay that is variable (and most likely related to the firm performance) has increased considerably from 10% in 1990 to 40% in 2005.

individual experienced periods out of work due to sickness, maternity leave, or unemployment, family size, area dummies, dummies for immigration status, and for family type. We then retrieve the residuals from these regressions and estimate θ^T and θ^P . Results for the pass-through estimates only are shown in Table 2.

Both parameters θ^T and θ^P are positive and estimated with great precision, implying that both permanent and transitory shocks to the firm value added are passed over to wages. As in Guiso et al. (2005), the wage response to permanent shocks to the firm performance (0.071) is significantly larger than the response to transitory shocks (0.018), which accords with intuition. The value of the F -test suggests that the instruments used to identify the two parameters are quite powerful while the Hansen J -test of the overidentifying restrictions reveals some misspecification for θ^T , possibly arising from the fact that the i.i.d. assumption is a bit too restrictive.

To have a reasonably long series of wage volatility measures, our strategy is to compute the overall variance of unexplained workers earnings growth over T periods using 5-year rolling averages:

$$\sigma_{it}^2 = \frac{\sum_{s=0}^{T-1} \omega_{ijt-s}^2}{T}$$

We use this measure as explanatory variable when estimating the risky portfolio share but instrument it with the variances of the unexplained firm value added growth - both permanent and transitor computed over the same T periods:

$$\begin{aligned} V_{jt}^P &= \frac{\sum_{s=0}^{T-1} g_{jt-s-1}(g_{jt-s-2} + g_{jt-s-1} + g_{jt-s})}{T} \\ V_{jt}^T &= \frac{\sum_{s=0}^{T-1} g_{jt-s-1}g_{jt-s}}{T} \end{aligned}$$

Notice that since the computation of these variances requires using lagged values of growth rates, it can only be implemented if the panel has a long time dimensions, which is the case in our data.

5.2 Firm closure risk

Our second measure of background labor income risk is employment risk. This risk too should in principle reflect idiosyncratic shocks to the (worker's) firm so that it can vary across workers and over time (unemployment risk arising from macroeconomic fluctuations in economic activity constitutes background but, being common to all workers is of little help for identifying the effect of background risk on financial decisions). We assume that the risk of a firm going bankrupt captures general firm distress climate. In particular, we use the Registry of Firm Bankruptcies, which records the date in which the firm is declared insolvent. We construct an indicator of firm closure risk if the worker is currently working in a firm that will be declared bankrupt in t years. We experiment by changing the lead value t .

The bottom part of Table 1 reports summary statistics for the two measures of background risk along

with the estimated variances of the firms shocks. We find that the variance of earnings growth in our sample is 0.05, with a small standard deviation of 0.1. In contrast, the variance of firm value added growth is much larger (0.18), with an extremely large standard deviation of 1.04. Finally, the risk of firm bankruptcy (the other measure of background risk we are going to use) in 2010 is small (0.3%). However, the consequences of job loss associated with firm destruction may be quite disastrous, at least for some workers, due to scarring effects.⁵

6 The effect of background risk on the risky portfolio share

Armed with these measures we now move to test whether investors mitigate the effect of background risk in their human capital by reducing exposure to financial risk - a risk that they can avoid by rebalancing their financial portfolio away from stocks. We start with regressions of the portfolio share of risky assets against a set of socio-demographic characteristics of the household, our measures of background risk, and households fixed effects to capture general heterogeneity in preferences for risk that can be correlated with background risk. Of course, these fixed effects capture also other sources of unobservable heterogeneity that may impact households portfolio allocation - such as differences in the precision of their information about stock returns (Peress, 2004) or in financial literacy (Calvet, Campbell and Sodini, 2009).

We start the analysis by simple regressions of the share of risky assets against the variance of unexplained earnings growth - the measure that is typically used in the empirical literature. For the time being, we also neglect the censoring issue, which we deal for in the next Section. Our empirical specification includes as controls a quadratic in age to model life cycle portfolio effects, year dummies which may capture passive variation in the asset share in response to changes in stock prices, dummies for family type and area of residence, as well as the value of lagged wealth and lagged earnings to account for well documented differences in assets allocation by wealth and income, partly due to fixed participation costs in the stock market and financial sophistication (Campbell, 2006). In addition, we control for firm size and, importantly, for household fixed effects. Results of these estimates are shown in Table 3.

The first column shows the regression on the variance of unexplained earnings growth as well as the measure of firm closure risk 1, 3 and 5 years ahead. All the estimated coefficients on these measures are consistent with the idea that workers who face unavoidable human capital risk tend to take less financial risk. The effect of earnings risk is negative and precisely estimated. We also find that the other measure of background risk discourage investment in risky assets. The risk of plant closure, with effects decaying as the closure event is more distant into the future, conforms with intuition. However, their economic effect is small: one standard deviation increase in the (residual) variance of log earnings would reduce the risky assets share by 0.12 percentage points. One standard deviation increase in the risk of firm closure one year

⁵Nilsen and Reiso (2010) study the long term unemployment consequences of displacement in Norway. They find that five years after job destruction, the likelihood of being unemployed is 17.2% among the "treated" group and only 7.8% among the "control" group. The negative effect decreases over time, but there is some unemployment "scarring" effect remaining even 10 years after the initial shock.

ahead would reduce the share invested in risky assets by 0.05 percentage points. Because the average risky assets share is 21%, these effects amount to a 0.6% and 0.002% respectively of the average sample share, too small an effect to matter. Hence, these estimates replicate the small economic effect of background risk that has been found in the literature.

The second column shows results of the reduced form regression of the share where the reduced form instruments are the firm permanent and transitory variance of firms value added, and find again negative coefficients and smaller responses. As argued in Section 2, this is consistent with the estimated effect of the variance of firm value added being the product of the true response of the share to background earnings risk and the effect of firms variability on the latter (typically considerably smaller than 1). Because of this a regression of the share on the variance of firm performance cannot identify the marginal effect of background risk. Things change considerably when we instrument total wage variance growth with the permanent and transitory variance of firm performance (Column 3). The coefficient on the worker earnings variance is negative and highly statistically significant and its size (in absolute terms) increases by a factor of 40 - from -0.0135 to -0.54, resulting in a very high sensitivity of portfolio decisions to background earnings risk. Depending on the set of instruments used (i.e., whether we use only the permanent/transitory firm variances, whether we add total firm variance, etc.) the estimate ranges between -0.54 and -0.49, reassuring that the much larger marginal effect of background risk of the IV estimates compared to the fixed effects estimates is not the reflection of a particular instrument combination but rather a reflection of the substantial bias of OLS estimates that ignore measurement error issues entailed by empirical measures of background risk used in the literature.

As for the marginal effect of firm closure risk, it is a bit larger (in absolute value) but its economic effect changes little.

6.1 Dealing with censoring

The estimates in Table 3 address two of the issues that identification of the effect of background risk poses - unobserved heterogeneity and the endogeneity problems that characterize the measures of background risk used in the literature. The third problem, neglected so far, is that half of our sample is censored from below at 0, i.e., there are about 50% stock market non-participants. One simple way to try deal with censoring is to estimate regressions for stock market participation. Table 4 shows estimates of a linear probability model of stock market participation with households fixed effects. The FEIV estimates of the wage background risk effect are much larger than the FE estimates but the coefficient is not statistically significant; the employment risk measure passes statistical significance but the size effect is again rather small. This suggests that most of the background risk action is on the intensive margin, while the extensive margin is dominated by “learning” or “fixed costs” considerations.

A more formal treatment of censoring that uses also the variation in the share invested in risky assets, is complicated because we have to deal simultaneously with three issues: endogeneity of the background risk

measure, unobserved heterogeneity in risk preferences which we capture with fixed effects, and censoring. Honoré and Hu (2008) propose an estimator that deals with these three issues at once, but their estimator is based on strong assumptions. For example, it requires that the endogenous variable is bounded from above and below (which in our case, where the endogenous variable is a variance, clearly is not).

Nevertheless, we can get a sense of the relative importance of the three issues for the estimates of the effect of background risk on the portfolio allocation by comparing four models:

1. Linear regression with households fixed effects (FE);
2. IV linear regression with households fixed effects (IVFE);
3. IV linear regression in which we replace fixed effects with a rich control function strategy that includes observable fixed heterogeneity (IVC);
4. IV tobit regression with the same control function (IVTC).

If the three issues (endogeneity, fixed effects, censoring) are all important, none of these models deliver consistent estimates. However, the bias of each of these four models is different and can potentially be compared (as we do below) to gauge their relative importance and thus enabling us to say something about the true value of λ .

We have already shown estimates for (1) and (2) in Table 3 and reproduce them in the first two columns of Table 5. In the third column we drop the fixed effects and replace them with a rich control function that now includes the length and type of education plus the gender of the household head. The estimate of λ drops (in absolute value) from -0.54 to -0.41 . Though large, this is not a dramatic drop from a qualitative point of view, an indication that the bias from omitting fixed effects is likely not large. Finally, the last column shows estimates of a formal Tobit IV model with the same control function as in column 3, which should eliminate the bias from neglecting censoring. The estimate of λ is now down to -0.29 - a somewhat more dramatic correction - indicating the importance of accounting for censoring. Yet, compared to the estimates in column 1 which disregard endogeneity, the Tobit IV too implies that the latter is the critical bias that pervades the empirical literature.

One heuristic way to gauge where the true value of λ stands is to suppose that the bias expressions are all linear and thus write the four estimators of λ as

$$\begin{aligned}
 p \lim \widehat{\lambda}_{FE} &= \lambda + C + E \\
 p \lim \widehat{\lambda}_{IVFE} &= \lambda + C \\
 p \lim \widehat{\lambda}_{IV} &= \lambda + F_1 + C \\
 p \lim \widehat{\lambda}_{IVTOBIT} &= \lambda + F_2
 \end{aligned}$$

where C and E denote the bias induced by ignoring censoring and endogeneity, and F_1 and F_2 denote the bias induced by ignoring unobserved heterogeneity (fixed effects) in the linear and non-linear case, respectively.⁶ Assuming $F_1 \cong F_2$, we have four equations in four unknowns; solving them we obtain an estimate of λ that is close to -0.4 , still quite sizable and an order of magnitude larger (in absolute value) than the estimates obtained ignoring measurement error in background risk as done in the literature.

6.2 Quantifying the effects of background risk

The quantitative assessment of the importance of background risk hinges on two ingredients: first, the size of λ , the marginal effect of a unit increase in background risk; second, the overall size of background risk. Our estimates suggest $\lambda \approx -0.4$ and provide estimates of employment risk resulting from mass layoffs/plant closures. Second, one needs to gauge the size of overall background risk. This is more problematic. But we can provide bounds of the overall effect. To estimate the size of effects of background risk we compute the effect on the portfolio share of one standard deviation increase in various measures of background risk, starting with on-the-job earnings variation and compare this to the average share of risky assets in portfolio, equal to 21% on average.

To aid interpretation, recall from (2) that true background risk ($B_{it} = \rho_v V_{it} + \rho_f F_{it}$) is related to firm-related shock volatility F_{it} and total worker earnings volatility ($\sigma_{it}^2 = V_{it} + \rho_f F_{it}$) by the relationships:

$$F_{it} \leq B_{it} \leq \sigma_{it}^2$$

Consider first a one-standard-deviation increase in the variance of the residual component of log earnings. This would reduce the share of risky assets in portfolio by about $-0.4 * 0.1 \approx -4$ percentage points - a reduction of about 19% of the average share in the sample, a non-negligible effect. Notice that, given the estimates reported in Table 4, most of the effect is on the intensive margin (the share of risky assets drops among stockholders) rather than on the extensive margin (participation rate appears unaffected by changes in background risk). Second, this estimate is an upper bound of the true effect of background risk because it assumes that all on-the-job wage variation (σ_{it}^2) is risk, which we already know is not (as long as $\rho_v < 1$). A lower bound can be obtained by assuming that only the firm contribution (F_{it}) is background risk. If we do so, we obtain a smaller effect in the range of 1 percentage point, equivalent to a 5% reduction in the average share in the sample.⁷

An *exact* quantification requires making some stronger assumptions in order to compute ρ_v , the extent of worker-related variation in earnings that is risk rather than reflecting choice. Note that from (3)

$$p \lim \hat{\lambda}_{FE} = \lambda \frac{\rho_v \text{var}(V_{it}) + \rho_f^2 \text{var}(F_{it})}{\text{var}(\sigma_{it}^2)}$$

⁶See the Appendix for the actual expression of these biases.

⁷This is obtained as follows. We know that the overall variance of residual earning is $\sigma_{it}^2 = \sigma_v^2 + (\theta^P)^2 \sigma_P^2 + (\theta^T)^2 \sigma_T^2$ and that $\theta^P = 0.07$ and $\theta^T = 0.02$. Hence the lower bound is the sum of the last two terms.

If a consistent estimate of λ can be obtained (for example from an IV-FE procedure), then one can also obtain an estimate of ρ_V since $\text{var}(\sigma_{it}^2)$, $\text{var}(F_{it})$, ρ_F and $\text{var}(V_{it}) = \text{var}(\sigma_{it}^2) - \rho_F^2 \text{var}(F_{it})$ are all measurable. If we use these estimates, we find that $\rho_V \approx 0.023$, implying that the true effect of background risk is much closer to the lower bound of the calculation above than to the upper bound. Hence, our conclusion is that the overall effect of background risk on portfolio choice is small and, indeed, our quantification exercise shows that it is similar to calculations present in the literature. The crucial difference is that we arrive at this conclusion for dramatically different reasons. While the empirical literature finds small marginal effects of background risk on portfolio choice (due to the downward biases discussed above) and large estimates of overall background risk (due to assuming that all variation in earnings is uninsured risk), we find the opposite: larger marginal effects and smaller background risk. The implication is that a large increase in uninsurable earnings volatility would generate large shifts in the composition of people's portfolio. Another implication is that larger transmission of firm-related shocks onto wages would also generate large portfolio reallocations. For example, if we were to increase the pass-through coefficient of permanent firm shocks on wages from 0.07 to 0.5, the standard deviation of the firm contribution would rise from 0.0214 to 0.1074. The lower bound of the effect of background risk on the share of risky assets in portfolio would increase from 1 percentage point to almost 5 percentage points. This exercise highlights the importance, once again, of "insurance within the firm".

Consider now employment risk. For this component of background risk, the estimated effect is quite small and somehow confirms the previous evidence.

These estimates beg the question of why, even in the lower bound scenario, wage risk is so more important than unemployment risk. One reason is that wage shocks may be more persistent than unemployment shocks. Indeed, the estimates of labor market frictions reported by Elsbj r et al. (2013) show that in Norway job offers when unemployed are not very frequent, but still it takes on average about 3 months to receive an offer (which may or may not be acceptable). Second, government insurance offers better protection against unemployment risk than against the risk of wage fluctuations – especially those induced by firm-related shocks. There is no insurance against not receiving bonuses or performance premia, but there is against being laid off.⁸ Third, the workers that are most exposed to the risk of job loss are also those that are less likely to participate in the stock market to begin with, implying that they cannot respond to an increased risk of losing the job by lowering investments in stocks.

⁸Social insurance programs in Norway are articulated and relatively generous. First, workers enjoy unemployment insurance; for permanent layoffs UI lasts for 52-104 weeks and replace, on average, 62% of the gross income in the last occupation. For temporary layoffs, UI is limited to 26 weeks within a 1.5 year period since layoff. Norway offers also disability insurance, which is obtained when the assessed loss in earnings capacity is of at least 50%. Eligibility is based also on income and assets and inability to work, and is assessed on a case-by-case basis. Moreover, individuals may have access to sickness and maternity benefits and active labor market programs to revamp their skills in case of displacement.

7 Robustness

7.1 Can Norwegian workers undo the transfer of risk on their wages?

For the measures discussed above to reflect exogenous background risk it is required that workers cannot anticipate firm-related shocks or avoid them once they realize. In principle, if labor markets were frictionless and workers could move rapidly and costlessly between firms, they would implicitly self-insure against fluctuations in their earnings induced by firm idiosyncratic shocks. Similarly, if they were able to foresee shocks to their firm and adapt in anticipation, the transfer of risk from the firm to workers' earnings would be eliminated. A discussion of whether workers are able to undo the transfer of risk is hence in order.

Like in other countries, the Norwegian labor market is far from being frictionless. According to Elsby et al. (2013), comparison of survey data on workers flows into and from unemployment for 14 OECD countries, shows that while Norway has not the most frictional labor market, the data show evidence of duration dependence, considerable uncertainty of receiving job offers when unemployed, and high rates of job destruction - actually higher than in most continental Europe countries. Moreover, Bagger and Henningsen (xxxx), using the same administrative data that we use, find even larger frictions than found by Elsby et al (2013) in the OECD surveys.

To check whether workers can smooth out labor market frictions by moving in anticipation of shocks to the firm, we look at “job mobility” as a function of current and *future* firm-related shocks. Clearly our measures of background risk gains credibility if current and future shocks to the firm have no explanatory power on current worker mobility. To test whether this is actually the case we estimate a probit-model for the event of job mobility as a function of current and future firm shocks, g_{jt} , g_{jt+1} and g_{jt+2} . In particular, we define an indicator $M_{ijt} = 1$ if individual i working at $t - 1$ with firm j switches employer in year t , and 0 otherwise. We then estimate a probit model:

$$\Pr(M_{ijt} = 1) = \Phi(\mathbf{W}_{ijt}\psi + \sum_{s=0}^2 \mu_s g_{jt+s})$$

where \mathbf{W}_{ijt} is a vector of the worker socio-demographic characteristics (including a quadratic in age, years and type of schooling, a male dummy, whether on unemployment insurance or sick leave, firm size, and year dummies). The second term captures current and future firm-specific shocks the worker has moved away from (if at all). If workers “anticipate” bad events happening at the firm level (and so manage to “undo” the risk coming from the firm side), we would expect the coefficients μ_1 and μ_2 to be negative and significant. Similarly, if workers react to current shocks, we would expect μ_0 to be negative and significant. Notice that with annual data the exact timing of the move can be problematic (we assign a worker to the employer he made most of his earnings with over the year), so it is possible that μ_0 turns significant even if there is no anticipation effect.

Table 6 shows the results of the estimates. The first column only controls for shocks to the growth of

firm performance and shows that future shocks to the firm growth have a positive effect on current mobility (contrary to the anticipation hypothesis) but it is very small and statistically insignificant. The second column adds also the indicators for whether the firm goes bankrupt within 1 and 2 years. Both coefficients have a positive sign (which would agree with the anticipation hypothesis), but again the effect is small and statistically insignificant despite 3.2 million observations, implying that there is no support for the idea that "rats leave the ship before it sinks". According to the estimates in Table 6, a positive shock today predicts job switching, which is puzzling and in any case in contrast with the anticipation/reallocation hypothesis.

One possible explanation for this puzzling result is serial correlation/mean reversion coupled with the difficulty of timing exactly "job switches" in annual data. To see this point, assume that value added growth follows a much simplified process, consisting of i.i.d. transitory shocks, i.e., $g_{jt} = u_{jt} - u_{jt-1}$. Then

$$\begin{aligned} \Pr(M_{ijt} = 1) &= \Phi(\mathbf{W}_{ijt}\psi + \sum_{s=0}^2 \mu_s g_{jt+s}) \\ &= \Phi(\mathbf{W}_{ijt}\psi - \mu_0 u_{jt-1} + (\mu_1 - \mu_0)u_{jt} + \mu_1 u_{jt+1}) \end{aligned}$$

If a firm is in (temporary) distress in period $t - 1$, we would observe the worker leaving and switching to another firm in period t when the previous firm shock has reverted to the mean.

All in all, the evidence above makes us confident that firm specific shocks inducing fluctuation in workers earnings and in their employment status constitute important sources of background risk.

7.2 Other concerns

In this section we discuss some robustness analyses and extensions.

First, background risk is likely to matter differently for people with different levels of wealth. Indeed, wealthier people can buffer background risk and because of this they may be less in need to hedge against it by reducing exposure to stocks, giving up the equity premium. That is, the effect of background risk should be less strong for those who have greater ability to self-insure. To account for the buffer-role of accumulated wealth, we interact the earnings variance with the lagged value of household wealth and run the IV estimates adding as instrument the variances of the firm interacted with lagged wealth.

Second, our instruments for the workers' unexplained wage volatility - the variance of the permanent and transitory component of shocks to firm growth - may be invalid if the worker has a dominant role in the firms. This may be the case if her decisions have a direct effect on the firm outcomes, such as when considering the top management of the firm. To account for the possible bias induced by workers with dominant position inside the firm, we follow two strategies. First, we focus on large firms (those with at least xx employees). Second, we drop workers who are in a top position in the firm.

Third, our instruments may be invalid if workers have all their stock investments concentrated in their firm's shares (a subtle form of "home bias"). In fact, this would give rise to an omitted variable problem

because the portfolio share of risky asset is inversely related to the variance of risky asset returns (as in classical Merton-type models), which for investors holding significant shares of their firm may be directly related to the variance of firm value added. Døskeland and Hvide (2011) find that among Norwegian direct stockholders, 20% of the stock portfolio is held in shares of current or previous (last 10 years) employers. Our final robustness check tries to account for "own-firm bias" in household portfolio. In particular, we redefine the portfolio to include only stocks in firms other than their own (i.e., the share of risky assets is redefined as $S_{it} = S_{it} = \frac{R'_{it}}{R'_{it} + RF_{it}}$, with R' being risky assets net of the value of own-firm stocks). Second, we drop individuals with any holdings in their own firm.

Results for these robustness checks are shown in Tables 7, 8 and 9. In table 7 the interaction of the wage variance measure with lagged wealth is positive and highly statistically significant, consistent with the idea that the hedging motive of human capital risk is particularly strong among people with fewer precautionary assets. A one standard deviation increase in variance of residual earnings growth lowers the risky portfolio share by 7.3 percentage points for people in the 25th percentile of the wealth distribution, by 5.4 points for those with median wealth and by 3.7 points for those in the 75th wealth percentile. In Table 8 we report regressions when we retain only large firms (with more than 10 and more than 20 employees, respectively). As can be seen these exclusions leave our estimates and conclusions unchanged.

Finally, in Table 9 we drop workers in top management positions and those who have some assets invested in their own firm. The results are, again, unaffected.

8 Conclusions

In this paper we have reassessed the importance of human capital uninsurable risk as an explanation for agents' reluctance to invest in stocks. Though in principle human capital risk can be an extremely important source of background risk and thus a fundamental factor for understanding portfolio choices and asset pricing (as long noticed in the literature), its role has been greatly diminished because empirically its effects on portfolio allocation has been found to be too small to matter. Our results suggest that it is too early to dismiss background risk as unimportant. We argue that the available evidence suffers from an identification problem that greatly biases the effect of background risk towards zero. We argue that achieving identification poses important conceptual challenges and formidable data requirements.

Using extremely rich Norwegian administrative data which minimize measurement error on portfolio composition and wages, we estimate firm-related measures of workers earnings variation to isolate exogenous changes in background risk. We show that once the endogeneity of usual measures of earnings risk is properly addressed and unobserved heterogeneity and censoring of stock investments are accounted for, the estimated sensitivity of the risky portfolio share to earnings risk can be up to 30 times larger than the estimates obtained ignoring these issues. While sensitivity to background wage risk is very large we find small sensitivity to employment (firm closure) risk.

Can background risk explain the large amount of heterogeneity in portfolio choice observed in data?

Answering this question requires a consistent estimate of the marginal effect of background risk, which we have, and a comprehensive measure of the size of background risk. For the latter we can identify upper and lower bounds. Our estimates set the effect of one standard deviation increase in background risk originating from human capital between a minimum of 1 percentage point decline in the risky portfolio share to a maximum 4 percentage points – out of a 21% average share of risky assets in portfolio. The actual effect is probably closer to the lower bound than to the upper bound. In this sense, background risk is still unable to explain much of the portfolio heterogeneity we see in the data. However, this is not because the marginal effect of risk is small. Rather, it is because the extent of uninsurable risk is small.

In this paper we have focused on one source of background risk - human capital. Given the large weight that human wealth has in the lifetime resources of most individuals, this is probably the most important source of background risk. But it is not the only one. For homeowners, unanticipated shocks to housing wealth is another, and given the illiquidity of housing it cannot easily be avoided; for entrepreneurs, private business wealth, is still another - and has been studied by Heaton and Lucas (2000a, 2000b). These three sources of background risk share one common feature: each one accounts for a substantial share of a consumer lifetime resources: thus, even if the effect of each one may be relatively contained, their joint effect on households assets allocation may be substantial. We have contributed to quantify one of them. More work is needed to quantify the others.⁹

⁹Palia et al. (2014) study the effect of volatility in returns to human capital, housing and private equity on the risky portfolio share. Unfortunately their study suffers from the endogeneity issues that we have stressed in this study by considering all measured variation in labor income, housing and private equity returns as background risk.

Calibration exercises show the potential importance of housing return risk for the composition of the financial portfolio (Cocco, 2005) and of returns to private wealth (Heaton and Lucas, 2002a). But a proper empirical assessment of these sources is still missing and faces the same identification problems as those faced by human capital risk.

A Data Appendix

A.1 Data sets

The analysis uses several data sources maintained by Statistics Norway that can be combined through unique personal and household identifiers over time.

The Central Population Register

The Central Population register contains end of year information on all Norwegian residents for the time period 1993-2011 and contains individual demographic information (ie. gender, day of birth, county of residence and marital status). It also contains family identifiers allowing us to match spouses and cohabiting couples with common children. Identifying un-married couples without common children is not possible in our sample period.

Administrative Tax and Income Records

Because households in Norway are subject to a wealth tax, they are every year required to report their complete income and wealth holdings to the tax authority, and the data are available every year from 1993 to 2011. Each year, before taxes are filed in April (for the previous year), employers, banks, brokers, insurance companies and any other financial intermediaries are obliged to send both to the individual and to the tax authority, information on the value of the asset owned by the individual and administered by the employer or the intermediary, as well as information on the income earned on these assets. In case an individual holds no stocks, the tax authority pre-fills a tax form and sends it to the individual for approval; if the individual does not respond, the tax authority considers the information it has gathered as approved. In 2011, as many as 2,4 million individuals in Norway (66% of the tax payers) belonged to this category.¹⁰ If the individual or household owns stocks then he has to fill in the tax statement - including calculations of capital gains/losses and deduction claims. The statement is sent back to the tax authority, which, as in the previous case receives all the basic information from employers and intermediaries and can thus check its truthfulness and correctness. Stockholders are treated differently because the government wants to save on the time necessary to fill in more complex tax statements and to reduce the risk of litigation due to miscalculated deductions on capital losses and taxes on capital gains. Traded financial assets are reported at market value. For stocks in non-listed companies that are not traded the company itself has to provide a tax report to the tax registry every year. In this report the company proposes a value of the company by the end of the year. This value should be the total net worth of the company, after deducting any debts. All assets have to be included in the valuation, except goodwill which is not included. The tax authority may adjust the value of the company upwards after going over the report, if it does not find the proposed value

¹⁰See the 2011 Annual Report from the Norwegian Tax Administration, <http://www.skatteetaten.no/en/>.

reasonable. Obviously this leads to undervaluation of the companies, but this is bound as unrealistically low figures would cause the tax authority to start a more thorough investigation.

This procedure, particularly the fact that financial institutions supply information on their customer's financial assets directly to the tax authority, makes tax evasion very difficult, and thus non-reporting or under-reporting of assets holdings are likely to be negligible.

The Norwegian National Educational Database

Educational attainment is reported by the educational establishment directly to Statistics Norway at the individual level, hence minimising the measurement error. The information includes on every student the highest level of education) at the individual level as of October every year.

The Register of Shareholders

The register consists of all Norwegian limited companies. Importantly the register contains information about shareholders and received dividends. Dividends are reported at the yearly level, and ownership is reported as of December 31st each year.

Employer-Employee Register

All firms hiring workers in Norway are required to report all work relationships to the Central Employer-Employee register. This includes registering the date and individual ID for the each time an employment relationship is established or terminated and when permanent changes are made to the registered information about working hours, job title (occupation code) and workplace (department). The register also contains the organization number of the firm and the sum of total payments (wages and remuneration) from the firm to the worker at a yearly level. When a worker has work relationships with several firms during the year, we select the firm with the highest payments to the worker that year as the main work-relationship.

The Central Register of Establishments and Enterprises

The register contains all enterprises and establishments in the private and public sector in Norway. For our purposes we select information on organization ID, geographical information, institutional sector, industrial classification (NACE), number of employees.

Firm Balance Sheet register

Contains accounts and balance sheet information from the financial statements of all non-financial firm. We extract all variables needed to calculate value added per worker. [THIS MAY BE SHORTENED]: Some of the main variables and definitions:

Operating income and operating expenses are ordinary income and expenses outside financial ones. Operating income is divided into sales revenues (taxable and tax-free), rental income, commission revenues, profits from the sale of fixed assets and other operating-related revenues. Operating expenses include changes in stocks, costs of raw materials and consumables used, wages and salaries, depreciation and write-downs of tangible fixed assets and intangible fixed assets as well as a number of different types of other operating expenses. Examples of operating expenses that are specified are subcontracting, repair and maintenance and expenses relating to means of transport.

Cost of raw materials and consumables used includes stock changes of work in progress and finished goods.

Wages and salaries include wages, holiday pay, employers' national insurance premium, pension costs and other personnel expenses.

Financial income and financial expenses are ordinary revenues and expenses relating to investments, securities, receivables and liabilities. The financial items also include share of earnings relating to foreign exchange gains and losses (agio) and value changes of market-based current asset investments.

Extraordinary revenues and expenses apply to material items that are unusual for the business and do not occur regularly.

Taxes represent taxes relating to the accounting result, and consist of taxes payable, expected reimbursement claims from owners and changes in deferred taxes. Taxes payable are the taxes expected to be assessed on the year's taxable income corrected for any discrepancy between calculated and assessed taxes the year before.

Allocation of the profit/loss for the year shows how a profit is allocated and losses are covered. It provides information on transfers to/from equity and dividends to owners.

Fixed assets cover assets that are mainly included in the enterprise's long-term creation of value and are intended for permanent ownership or use, as well as receivables and securities scheduled for repayment later than one year after the time of settlement. This includes tangible fixed assets broken down into buildings and facilities, facilities under construction, transport equipment, machinery etc. Long-term receivables and investments are included as fixed assets, such as investments in other activities and loans to enterprises in the same group.

Current assets are assets relating to the enterprise's sales of goods and services, or which are expected to have a functional period of less than one year in operation. This includes cash and short-term capital investments (cash, bank deposits, shares, bonds etc.), receivables and inventories. Receivables are current assets if it has been agreed or scheduled that they shall be repaid within one year after the end of the financial year.

Equity is the portion of the total capital belonging to the owners, and is shown as the value of assets less liabilities. Equity is classified in two main divisions, invested equity and retained earnings. Invested equity consists of share capital and share premium accounts. Retained earnings consist of fund for assessment

differences and other reserves/uncovered losses.

Liabilities cover all obligations that can come to place restrictions on the future use of the enterprise's resources, and are divided into provisions for liabilities and charges (pension commitments, deferred tax liabilities, etc.), other long-term liabilities and short-term liabilities. Long-term liabilities are legal or financial obligations not meant to be redeemed during the coming accounting period, and are not related to the enterprise's short-term sales of goods and services. Short-term liabilities are liabilities that fall due for payment within one year from the time of settlement, or are directly related to the enterprise's short-term sales of goods and services.

Register of Bankruptcies

The register contains the firm number and the exact date of bankruptcy at the firm level. All juridical objects, which includes all types of firms/enterprises and individuals who have unpaid accounts and are by definition insolvent, can be declared bankrupt.

A.2 Sample Selection

We start with a data set on income recipients that merges record from the Central Population Register and the Administrative Tax and Income Register. This merged data set includes 29,814,364 person-year observations for the period 1995 to 2010. Given that we need to use as an instrument a measure of firm-level risk, we focus on a sample of individuals who are continuously employed in the private sector (sector 710 or 717). This excludes those who are not working (unemployed, retired, disabled, etc.) and those who have a spell in the government sector. This sample selection leaves us with 9,888,562 observations. Next, we exclude individuals who are younger than 25 (and hence possibly still in school) and those older than 60 (who may have intermittent participation, and also have widespread access to early retirement, typically from the age of 62, see e.g., Vestad 2014). We are left with 7,566,412 observations. Merging this data set with firm-level information reduces the usable sample to 6,501,730 observations (this sample reduction is due to some missing information in the firm data set used to construct the measure of firm value added, exclusion of short lived firms -those that are active for less than 3 years- and some inconsistencies in the reported firm number in the Employer/Employee registry vs. the Balance sheet registry). Next, we exclude individuals who have earnings below the basic amount threshold of the Norwegian Social Insurance Scheme (*grunnbeløpet*) in one or more years and are left with 5,168,462 observations. Even though we restrict the sample of workers between 25 and 60 years of age, some students are still left in the sample, and will typically have low incomes.¹¹ Further, workers who have some period of disability or sick leave, will often have less than full-time positions, potentially in several firms. To reduce the impact of such outliers, we drop all the observations where earnings growth is less than -80% or more than 500% (and are left with 5,115,196

¹¹The incentive to stay below this threshold is significant as the government stipend to all students is reduced almost one-to-one for each dollar earned above a threshold only marginally higher than *grunnbeløpet*

observations). Since we run regressions at the household level, we keep only the primary earner of the household (4,846,766 observations left). The number of observations in the various regressions we run are less than this because we use lags for constructing some of the variables and instruments.

A.3 Back-of-the-envelope calculation of bias

Consider the simple model:

$$S_{it} = \lambda B_{it} + r_i + \varepsilon_{it}$$

which omits observable characteristics and where $B_{it} = \rho_v V_{it} + \rho_f F_{it}$. Moreover, we observe an error-ridden background risk measure:

$$\sigma_{it}^2 = V_{it} + \rho_f F_{it} = B_{it} + v_{it}$$

where $v_{it} = (1 - \rho) V_{it} + \sigma_\xi^2$. Finally, there is censoring:

$$S_{it}^c = S_{it} \times \mathbf{1}\{S_{it} > 0\}$$

Suppose we could observe the latent continuous variable S_{it} . Define $\tilde{x}_{it} = x_{it} - T^{-1} \sum_{t=1}^T x_{it}$ as a demeaned variable. Then an IVFE estimator that uses the latent variable would be consistent for λ :

$$p \lim \hat{\lambda}_{IVFEL} = p \lim \frac{E(\tilde{S}_{it} \tilde{F}_{it})}{E(\tilde{\sigma}_{it}^2 \tilde{F}_{it})} = \lambda$$

However, we observe S_{it}^c , not S_{it} . Our estimator is

$$\begin{aligned} p \lim \hat{\lambda}_{IVFE} &= p \lim \frac{E(\tilde{S}_{it}^c \tilde{F}_{it})}{E(\tilde{\sigma}_{it}^2 \tilde{F}_{it})} \\ &= p \lim \frac{E(\tilde{S}_{it} \tilde{F}_{it})}{E(\tilde{\sigma}_{it}^2 \tilde{F}_{it})} + p \lim \frac{E((\tilde{S}_{it}^c - \tilde{S}_{it}) \tilde{F}_{it})}{E(\tilde{\sigma}_{it}^2 \tilde{F}_{it})} \\ &= \lambda + CENS \end{aligned}$$

Note that this can also be rewritten as:

$$\begin{aligned}
p \lim \widehat{\lambda}_{IVFE} &= p \lim \frac{E(\widetilde{S}_{it}^c \widetilde{F}_{it})}{E(\widetilde{\sigma}_{it}^2 \widetilde{F}_{it})} \\
&= \lambda p \lim \frac{E(\widetilde{S}_{it}^c \widetilde{F}_{it})}{E(\widetilde{S}_{it} \widetilde{F}_{it})}
\end{aligned}$$

While it is well known that OLS is downward bias relative to Tobit in a static setting, in dynamic settings this is not necessarily the case. To see this point, consider the case in which we have only two periods. In this case, the within-group estimator is equivalent to a first-difference estimator, and the expression above becomes:

$$\begin{aligned}
p \lim \widehat{\lambda}_{IVFE} &= \lambda p \lim \frac{E(\Delta S_{it}^c \Delta F_{it})}{E(\Delta S_{it} \Delta F_{it})} \\
&= \lambda p \lim \frac{E(S_{it}^c \Delta F_{it}) - E(S_{it-1}^c \Delta F_{it})}{E(S_{it} \Delta F_{it}) - E(S_{it-1} \Delta F_{it})} \\
&= \lambda \eta
\end{aligned}$$

While it is true that $E(S_{it}^c \Delta F_{it}) \leq E(S_{it} \Delta F_{it})$ for all τ , the difference $E(S_{it}^c \Delta F_{it}) - E(S_{it-1}^c \Delta F_{it}) \geq E(S_{it} \Delta F_{it}) - E(S_{it-1} \Delta F_{it})$, implying that it is impossible to say whether the IVFE estimator that uses the censored dependent variable is downward or upward biased relative to the (unfeasible) one that would use the latent dependent variable. Intuitively, if changes in ΔF_t shift the extensive margin more than the intensive margin, it is even possible that $\eta > 1$.

A simple IV estimator that neglects fixed effects (and censoring) uses a control function to make the fixed effect a random effect, and hence would give:

$$\begin{aligned}
p \lim \widehat{\lambda}_{IV} &= p \lim \frac{cov(S_{it}^c, F_{it})}{cov(\sigma_{it}^2, F_{it})} \\
&= \lambda + CENS + FE_1
\end{aligned}$$

where $FE_1 = p \lim \frac{cov(S_{it}^c, F_{it})}{E(\widetilde{\sigma}_{it}^2, \widetilde{F}_{it})} - p \lim \frac{cov(\widetilde{S}_{it}^c, \widetilde{F}_{it})}{E(\widetilde{\sigma}_{it}^2, \widetilde{F}_{it})}$.

The Tobit IV estimator neglects fixed effects but is asymptotically equivalent to using S_{it} as dependent variable. One can show that

$$p \lim \widehat{\lambda}_{TOBITIV} = p \lim \frac{cov(S_{it}, F_{it})}{cov(\sigma_{it}^2, F_{it})} \\ \lambda + FE_2$$

where $FE_2 = p \lim \frac{cov(S_{it}, F_{it})}{E(\widehat{\sigma}_{it}^2, \widehat{F}_{it})} - p \lim \frac{cov(\widetilde{S}_{it}, \widetilde{F}_{it})}{E(\widetilde{\sigma}_{it}^2, \widetilde{F}_{it})}$.

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Table 1: Summary statistics (2010)

Variable	Mean	Std. Dev.	N
Age	46.278	8.307	160 204
Male	0.834	0.372	160 204
Less than High School	0.179	0.383	160 204
High School	0.561	0.496	160 204
Some College or more	0.260	0.439	160204
Family size	2.862	1.406	160 204
Value of risky assets	765 593	11 585 627	160 204
Value of safe assets	400 875	1 849 631	160 204
Share risky assets	0.252	0.326	160 204
Earnings	527 496	297 301	160 204
Variance earnings growth	0.052	0.103	160 204
Variance value added growth	0.186	0.578	26 288
Permanent shocks	0.057	0.187	23 370
Transitory shocks	0.056	0.285	24 808
Firm size	30	145	30 573
Fraction Firm Bankrupt	0.0036	0.0599	30 573

Table 2: Pass-through regressions

	(1)	(2)
	Permanent value added shocks	Transitory value added shocks
Pass-through coefficient	0.0705*** (0.0056)	0.0175*** (0.0053)
Constant	-0.0021*** (0.0002)	-0.0023*** (0.0002)
F-test instruments	134.21	688.46
J-test (p-value)	29.61%	0.16%
Observations	2,358,890	2,370,421

Clustered standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Table 3: Share of Risky Assets regressions

	(1)	(2)	(3)
	Fixed effect	Reduced form fixed effect	Fixed effect IV
σ_{it}^2	-0.0135*** (0.00300)		-0.544*** (0.210)
Firm bankrupt in 1 year	-0.0152*** (0.00404)	-0.0113** (0.00505)	-0.0205*** (0.00667)
Firm bankrupt in 3 years	-0.00430* (0.00245)	0.000832 (0.00272)	-0.00428 (0.00338)
Firm bankrupt in 5 years	-0.00318 (0.00271)	0.000350 (0.00285)	-0.00219 (0.00352)
V_{it}^P		-0.00308** (0.00125)	
V_{it}^T		-0.00275*** (0.000741)	
Observations	1972639	1655104	1184800

Clustered standard errors in parentheses. Regressions also control for Age, Age², log(W_{t-1}), log(Y_{t-1}), Family type dummies, Year dummies, Firm size, Area dummies.

* $p < .1$, ** $p < .05$, *** $p < .01$

Table 4: Regressions for Stock Market Participation

	(1)	(2)
	Fixed Effect	Fixed Effect IV
σ_{it}^2	0.00310 (0.00461)	-0.274 (0.324)
Firm bankrupt in 1 year	-0.0169*** (0.00592)	-0.0164* (0.00943)
Firm bankrupt in 3 years	-0.00689** (0.00348)	-0.00648 (0.00470)
Firm bankrupt in 5 years	-0.000972 (0.00398)	0.00180 (0.00514)
Observations	1972639	1184800

Clustered standard errors in parentheses. Regressions also control for Age, Age², log(W_{t-1}), log(Y_{t-1}), Family type dummies, Year dummies, Firm size, Area dummies.

* $p < .1$, ** $p < .05$, *** $p < .01$

Table 5: Additional Specifications

	(1)	(2)	(3)	(4)
	Fixed Effect	Fixed Effect IV	IV with control function	Tobit IV with control function
σ_{it}^2	-0.0135*** (0.00300)	-0.544*** (0.210)	-0.413*** (0.102)	-0.291** (0.156)
Firm bankrupt in 1 year	-0.0152*** (0.00404)	-0.0205*** (0.00667)	-0.0028 (0.0067)	-0.0145 (0.0121)
Firm bankrupt in 3 years	-0.00430* (0.00245)	-0.00428 (0.00338)	0.0039 (0.0045)	0.00247 (0.00814)
Firm bankrupt in 5 years	-0.00318 (0.00271)	-0.00219 (0.00352)	0.0052 (0.0047)	0.00620 (0.00868)
Observations	1972639	1184800	1230063	1230063

Clustered standard errors in parentheses. All regressions also control for Age, Age², log(W_{t-1}), log(Y_{t-1}),

Family type dummies, Year dummies, Firm size, Area dummies. In columns 3 and 4 we additionally control for length and type of education and head gender.

* $p < .1$, ** $p < .05$, *** $p < .01$

Table 6: Mobility Regressions

	(1)	(2)
	mover	mover
g_{jt}	0.242*** (0.0292)	0.242*** (0.0292)
g_{jt+1}	0.0278 (0.0267)	0.0278 (0.0267)
g_{jt+2}	0.0256 (0.0193)	0.0256 (0.0193)
Firm bankrupt in 1 year		0.453 (0.319)
Firm bankrupt in 2 years		0.0769 (0.344)
Observations	3219340	3219340

Note: Clustered Standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Interactions with Wealth

	(1)	(2)
	ratio	ratio
σ_{it}^2	-0.544*** (0.210)	-2.311*** (0.827)
σ_{it}^2 *Lagged log wealth		0.147** (0.0670)
Firm bankrupt in 1 year	-0.0205*** (0.00667)	-0.0218*** (0.00686)
Firm bankrupt in 3 years	-0.00428 (0.00338)	-0.00467 (0.00344)
Firm bankrupt in 5 years	-0.00219 (0.00352)	-0.00302 (0.00360)
Observations	1184800	1184800

Clustered standard errors in parentheses.

* $p < .1$, ** $p < .05$, *** $p < .01$

Table 8: Robustness by Firm Size

	(1)	(2)	(3)
	Baseline	Firm size>10	Firm size>20
σ_{it}^2	-0.544*** (0.210)	-0.821*** (0.284)	-1.045*** (0.333)
Firm bankrupt in 1 year	-0.0205*** (0.00667)	-0.0205*** (0.00737)	-0.0182** (0.00889)
Firm bankrupt in 3 years	-0.00428 (0.00338)	-0.00380 (0.00371)	-0.00413 (0.00389)
Firm bankrupt in 5 years	-0.00219 (0.00352)	-0.00565 (0.00369)	-0.000148 (0.00407)
Observations	1184800	984298	824304

Clustered standard errors in parentheses.

* $p < .1$, ** $p < .05$, *** $p < .01$

Table 9: Robustness by Position Inside the Firm and Ownership

	(1)	(2)	(3)
	Baseline	Excluding executives	Excluding owners
σ_{it}^2	-0.544*** (0.210)	-0.649*** (0.236)	-0.565*** (0.212)
Firm bankrupt in 1 year	-0.0205*** (0.00667)	-0.0158** (0.00704)	-0.0205*** (0.00669)
Firm bankrupt in 3 years	-0.00428 (0.00338)	-0.00222 (0.00353)	-0.00424 (0.00339)
Firm bankrupt in 5 years	-0.00219 (0.00352)	-0.000259 (0.00361)	-0.00211 (0.00353)
Observations	1184800	938981	1173031

Clustered standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

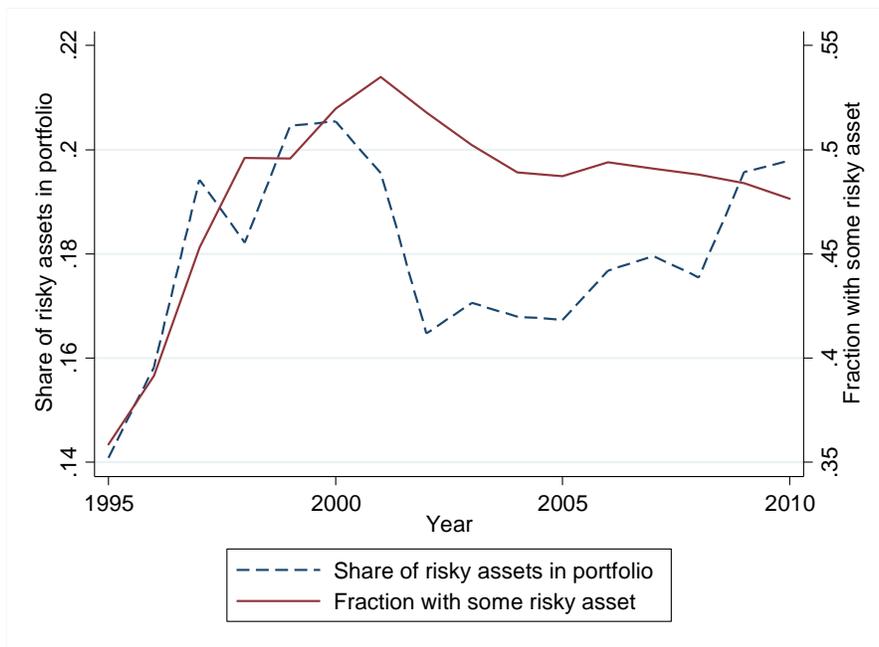


Figure 1: Stock Market Participation and Share of Risky Assets in Portfolio

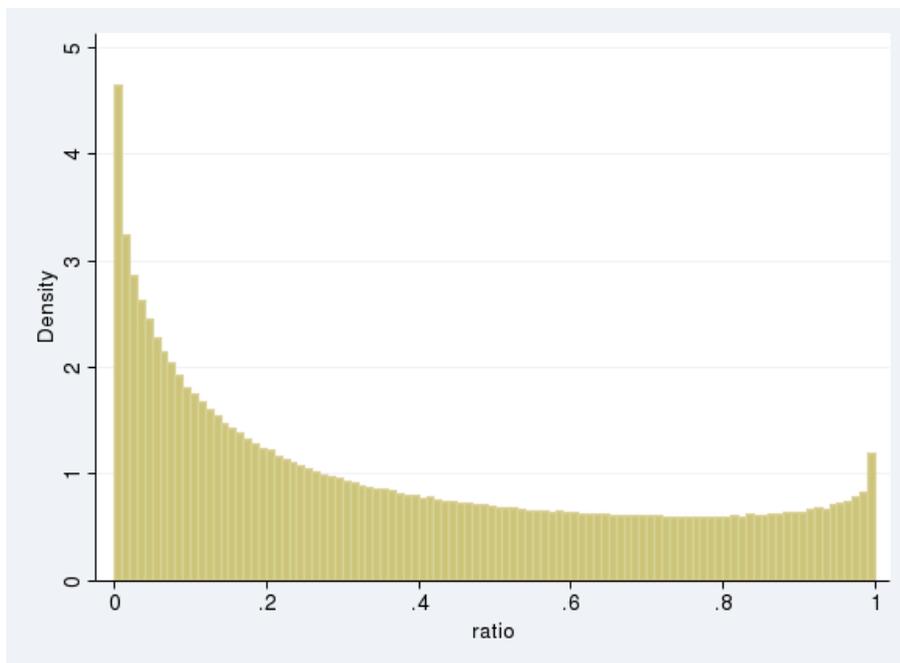


Figure 2: Share of Risky Assets in Portfolio (Conditional on Participation)