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Abstract

Estimating the effect of background risk on individual financial choices faces two challenges. First, the identification of the marginal effect requires a measure of *at least one* component of human capital risk that qualifies as "background" (a risk that an individual cannot diversify 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, once the marginal effect is identified, an evaluation of the economic significance of background risk requires knowledge of the size of *all* the background risk actually faced. Existing estimates are problematic because measures of background risk 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 tackle the identification problem we match panel data of 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 is hardly avoidable. We rely on this measure to instrument total variability in individual earnings and find that the marginal effect of background risk is much larger than estimates that ignore endogeneity. We bound the economic impact 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 of background risk is small because firms provide substantial wage insurance.

Keywords: Background risk, Portfolio Choice, Labor Income Uncertainty.
JEL Codes: G11, D1, D8.

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1 Introduction

How important is background labor income risk for individuals' portfolio allocations? To properly answer this question we assemble a rich administrative household data set from Norway that allows us to overcome the identification challenges that plague most of the empirical work on the subject.

The topic of background risk - a risk that cannot be avoided or insured - has a long history in macroeconomics and finance. Starting with Aiyagari (1994), a large literature has studied how the presence of uninsurable idiosyncratic labor income risk 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 labor income risk respond by accumulating precautionary savings, raising labor supply, or more generally changing the pattern of human capital accumulation (e.g., Levhari and Weiss, 1974). 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 (Merton, 1971; Kimball, 1993; Constantinides and Duffie, 1996; Heaton and Lucas, 1996; Heaton and Lucas, 2000).

Motivated by these theoretical predictions and the undisputable importance for many households of labor income, one strand of research has incorporated background risk in calibrated models of (consumption and) portfolio allocation over the life cycle and explored its ability to help reproduce patterns observed in the data (e.g. Viceira, 2001; Cocco, Gomes, and Maenhout, 2005; Heaton and Lucas, 2000; Polkovnichenko, 2007). Another strand has tried to assess the empirical relevance 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 seems to have lost appeal as a quantitatively 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 reconsider the role of background labor income risk 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 identification problems that also affect calibrated 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 risk 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 households survey data (e.g., the PSID in the US). Another is to use second moments from 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 important practical data problems: subjective expectations data are rarely available alongside longitudinal data on assets, making it hard to deal with unobserved heterogeneity. In sum, isolating background risk is far from trivial. The empirical measures described above introduce a sort of errors-in-variable problem that biases towards zero the estimated effect of labor income risk on portfolio choice. Furthermore, as we shall discuss, the size of the downward bias can be substantial.

Second, notwithstanding the problem of obtaining a conceptually sound measure of background risk, other econometric issues may make estimates of its effect on portfolio (or other financial) choice unreliable. For example, most of the evidence on the effect of income risk comes from cross sectional data, inducing unobserved heterogeneity bias (e.g., 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.[1]

[1] Betermier et al. (2011) is 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 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 conceivable that the same factors that trigger moving also affect portfolio rebalancing. While the authors show evidence that movers and stayers share similar observable characteristics, selection on unobservables (such as risk preferences) may be driving mobility. 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. This makes it hard to estimate the economic effect of earnings risk on portfolio choice.

A final issue is that most of the empirical literature uses survey data on assets. These are notoriously subject to measurement error and rarely sample the upper tail of the distribution (which is key, given the enormous skewness in the distribution of wealth). 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 - i.e., 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 levies 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 population, including those in the very top tail of the wealth distribution. These data allow us to compute financial 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 in each job, as well as measures of firm performance, mass layoffs, and closures due to firm bankruptcy. Armed with these data we 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 workers' 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 firm closure, providing exogenous variation in the risk of job loss, which allows us to study the portfolio response to idiosyncratic tail background risk.

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 an order of magnitude 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 estimates 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 due to measurement error 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 (approximately) 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 an order on magnitude larger than the OLS (fixed effect) estimate, implying that the key force biasing the effect of background risk is measurement error (i.e., the assumption that all residual wage variability is risk).

Second, we find that marginal effects of background risk vary considerably across individuals depending on their level of wealth. The portfolio response of individuals at the bottom of the wealth distribution - those with little buffers to face labor income uncertainty - is twice as large as that of the workers with median wealth; the effect gets smaller as wealth increases and drops to zero at the top of the wealth distribution. Background risk is irrelevant for those with large amounts of assets despite the fact that their compensation is more sensitive (as we document) to firms shocks.

Third, using the estimated parameters we provide some bounds on the effects of background risk when the latter is caused by a reductions in the amount of wage insurance provided by firms and in the predictability of workers' wage shocks. Evaluated at the means of the portfolio sensitivity and of firms insurance and wage predictability, the effect of background risk is small: individuals with the average amount of background risk have a share of risky assets in portfolio that is 1/4 of a percentage point smaller than those with no background risk whatsoever. These numbers suggest that, when quantifying the effect of background risk on portfolio choice, our conclusions are not different from what found in the existing literature - despite the larger sensitivity to risk that we estimate. The key to understanding this apparently puzzling result is that the effect of risk on portfolio choice depends on two things: the response of portfolio choice to a change in the risk and the size of the risk itself. Our estimates suggest that the true marginal response is much larger and the true background risk much smaller than typically found. In the existing literature the opposite is true: estimated risk is overstated and (because of this) the sensitivity is downward biased, thus reaching the right conclusion but for the wrong reasons. In turn, we show that wage fluctuations risk is contained because firms provide workers with substantial insurance. If firms were to share shocks equally with their workers, the latter would reduce the demand for risky financial assets substantially, particularly for low wealth workers. In sum, the economic importance of human capital risk crucially hinges on the insurance role of the firm and the amount of assets available to the individual to buffer labor income shocks.

The rest of the paper proceeds as follows. Section 2 reviews the empirical literature and highlights our contribution. In Section 3 we illustrate the econometric problems that arise when trying to identify the effect of background risk on financial decisions, and show how we tackle them. Section 4 describes the data sources. Section 5 discusses the construction of our measures of background risk. Section 6 turns to the estimates of the marginal effect of background risk on people's portfolio allocation, presents several robustness tests and allows for wealth-driven heterogeneity in the portfolio response to background risk. We discuss the economic effect of background risk on the demand for risky financial assets in Section 7. Section 8 concludes.

2 Literature Review

Several papers provide evidence that labor income risk has a tempering effect on households portfolio allocation. In one of the first studies on the topic, Guiso et al. (1996) use a measure of earnings risk obtained from the subjective distribution of future labor income in a sample of Italian workers and find that households with more spread-out beliefs of future income invest a lower share in risky assets. However, the economic effect is small: households with above average subjective earnings variance invest a 2 percentage points lower share of their wealth in stocks than households with below average uncertainty. Because they use cross sectional data, unobserved heterogeneity cannot be controlled for.[2] Hochguertel (2003) also relies on a self-assessed subjective measure of

[2] Also using cross sectional data, Arrondel and Calvo-Pardo (2012) find a positive correlation between subjective income risk and the portfolio risky share of French households. They argue that the result can be explained by sample

earnings risk available for Dutch households. The data are longitudinal, allowing him to control for unobserved heterogeneity. However, the results are similar: a negative, small effect of subjective wage income risk on the share of risky assets.

One advantage of subjective expectations is that in principle they reflect all the information available to the household; one issue, however, is that elicitation can be problematic as household may have difficulties understanding the survey question. This may result in classical measurement error as well as in households mis-reporting the probability of very low income states. Both facts are consistent with the low estimated variances of income growth compared to those obtained from panel data estimates of labor income processes. Accordingly, several papers have measured labor income risk using panel data models of workers' earnings.

Heaton and Lucas (2000) use income data from tax records of a sample of US workers to measure wage income and proprietary income variability and correlate them with stock portfolio shares. They find a negative, but small and statistically insignificant, effect of wage income variability and a negative, statistically significant but still small effect of proprietary income variability on the demand for stocks. Unfortunately, inference is impaired both because portfolio data are imputed as well as because measured background risk - the unconditional standard deviation of wage income and proprietary income growth - may contain a large portion that reflects choice rather that risk. In addition, unobserved heterogeneity, particularly in the case of proprietary income, may be driving the results.

Angerer et al. (2009) overcome some of these problems. They use the US National Longitudinal Survey of Youth to estimate the residual variance of labor income growth, after conditioning on a number of observables. Thus, their measure of background risk reduces the weight of the predicable component and in addition they distinguish between transitory and permanent shocks to labor income. Perhaps because of this, compared to the previous papers they find somewhat larger effects, particularly in response to the variance of permanent shocks to labor income. Overall, a 10% increase in the standard deviation of labor income shocks lowers the portfolio stock share by 3.3 percentage points. More recently, Palia et al. (2014) have extended the analysis to consider several sources of background risk, including labor income, returns on housing, and entrepreneurial income. They estimate that one standard deviation increase in labor income risk lowers the share in stocks by 1.8 percentage points and find a larger effect on participation (a reduction of 5.5 percentage points). Needless to say, effects are larger when all sources of background risk increase at once.

selection of more risk tolerant workers into riskier occupations.

Yet, because they compute background risk as the standard deviation of the (unconditional) growth rate of earnings, their background risk measure is likely overstated.

Overall, this summary of the literature suggests relatively contained effects of background risk on the demand for risky assets. Idiosyncratic labor income risk has therefore, been dismissed as an important factor in explaining portfolio allocation heterogeneity and assets prices (Heaton and Lucas, 2008; Cochrane, 2006). Yet, the likely presence of (potentially severe) measurement error in background risk raises some doubts about this conclusion and thus on the assets prices implications. In the next section we set up an econometric framework and argue that empirical measures of background risk such as those used in the literature so far are very likely to generate substantial downward biases in the marginal effect of labor income risk (and other sources of background risk). We also suggests a methodology to obtain a well-defined measure of background risk and a consistent estimate of its marginal causal effect.

3 Econometric Framework

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

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

where S_{it} is the share of risky assets in individual i's financial portfolio at time t, W_{it} are socio-demographic characteristics related to portfolio choice (such as gender, education, total wealth, etc.), B_{it} a measure of background risk, r_i an unobserved individual fixed effect (which may capture heterogeneity in risk tolerance or financial literacy), and ε_{it} an error term. The empirical literature has used variants 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.

A general empirical strategy for measuring background risk in returns to human capital consists of writing a labor earnings process such as:

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

where y_{ijt} are earnings earned by worker i in firm j at time t, $\mathbf{Z}_{it}$ is a vector of observable wage determinants, 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), and f_{jt} a firm-specific shock. The econometrician does not observe the degree of the agent's control over v_{it}. We assume that the error components f_{jt} and v_{it} are mutually uncorrelated. Firm shocks are passed onto 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:

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

The separation of v_{it} in a component that is anticipated/avoidable and one that is not (with weight θ_v) 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. 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 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).

In keeping with this discussion, the "true" measure of background risk should be:

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

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}, the observed wage is:

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

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

An OLS regression of S_{it} on the measure σ^2_{it} (omitting individual fixed effects, r_i) gives inconsistent estimates of the sensitivity of portfolio choice to background risk.[3] Indeed:

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

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, the 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 this is not what is typically found in the data. The self-employed invest more in stocks and have greater income volatility (see, e.g., Georgarakos and Inderst, 2014). 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 one can control for individual fixed effects. Hence, the second bias term disappears and the sensitivity of portfolio choice to risk is downward biased, i.e.:

$$p\lim \widehat{\lambda}_{FE} = \lambda \frac{\rho_v var\left(V_{it}\right) + \rho_f^2 var\left(F_{it}\right)}{var\left(V_{it}\right) + \rho_f^2 var\left(F_{it}\right) + var\left(\sigma^2_{\xi}\right)} \tag{3}$$

The extent of the downward bias can be substantial. Even ignoring measurement error in earnings (i.e. setting $\sigma^2_{\xi} = 0$), if firms offer substantial wage insurance (i.e., the term ρ_f is "small") and if a relevant share of workers related variation in earnings is due to choice rather than to risk (i.e., ρ_v is small), then the OLS estimate of the effect of background risk can be much lower than the true effect.

Both conditions are likely to hold in practice. As documented by Guiso et al. (2005) using Italian data, firms offer partial but substantial wage insurance, implying a value of ρ_f much smaller

[3] Conditional on $\mathbf{W}_{it}$.

than 1 and close to 0.01 (since their estimate of θ_f is 0.1). In Section 5 we show that this result holds also in our Norwegian data. On the other hand, there is evidence that a lot of variation in individual earnings is predictable. For instance, Cunha and Heckman (2007) estimate that for US skilled workers only 8% of the increase in wage variability is due to increased uncertainty and 92% to heterogeneity. Using Italian subjective earnings expectations data (which incorporate more information than that typically available to the econometrician), Kaufman and Pistaferri (2009) calculate that only about 1/4 of the residual earnings growth variance is risk, while the remainder is predictable or noise.

We take these concerns seriously and 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:[4]

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

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

[4] Note that a simple cross-sectional IV estimator (which ignores fixed effects) will still be inconsistent, as $p\lim \widehat{\lambda}_{IV} = \lambda + p\lim \frac{cov(r_i, F_{it})}{cov\left(\sigma_{it}^2, F_{it}\right)}$.

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

as firm shocks pass through only partially to wages. Furthermore, the difference between the true sensitivity λ and the reduced form response $\lambda\rho_f$ can be very large if firms provide substantial wage insurance, i.e., ρ_f is "small". We stress this case because Hung et al. (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, while similar in spirit to ours, 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 et al. (2014), and their strategy would only deliver consistent estimates of λ if the worker "owned the firm" - i.e. in the absence of wage insurance. On the other hand, papers that use survey data sets such as the SCF or PSID to estimate the effect of background risk on portfolio choices, cannot identify its effect as they lack matched employer-employee data to estimate F_{it} and $\rho_{f.}$

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:

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

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 Honorè and Hu (2004). In practice, this estimator does not work well in our administrative large-scale data set. We will instead consider some back-of-the-envelope exercises that compare various estimators proposed in the literature to get some 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 households surveys because measured incomes and financial assets are plagued with reporting error, under-reporting and non-reporting (e.g. Hurst, Li and Pugsley, 2015).

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 15 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). In this sense, since we are able to simultaneously account for all the issues that plague existing empirical studies, we are giving the background risk model the best possible chance to succeed.

4 Data and Norwegian institutional insurance provisions

4.1 Data

To study whether households shelter against (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; Appendix A1 illustrates the features of the data in greater detail.

The **Central Population Register** contains basic end-of-year demographic information (i.e., gender, birth date, 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**).

To comply with the 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 authority by the companies themselves. If the tax authority finds the proposed evaluation unrealistically low, it 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 is employed, a feature that turns out to be useful when we discuss various robustness checks (Section 5.1).

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.[5] 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.[6] In the population (before any sample selection), participation in the risky assets market increases substantially in the 1995-2010 period (see Figure 1). During the same time period the the average portfolio share in risky assets also increases (the dashed line in Figure 1).

Consistent with what found in the literature (Guiso and Sodini, 2013), there is substantial

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

[6] In the original data, there are households holding extremely small amounts in stock accounts, due presumably to dormant accounts. We assume that genuine stock market participants have at least the equivalent of \$30 worth of risky assets in their portfolio. Imposing smaller or slightly larger thresholds has no effects on the results.

cross sectional variation in the conditional risky share. As Figure 2 shows, its distribution spans the entire [0-1] range – from people holding very small amounts to people investing their entire financial portfolio in stocks. In this paper we ask how much of this heterogeneity can be explained by background risk, if any.

Table 1 shows summary statistics for the portfolio data and the financial wealth of our Norwegian sample. Since we select younger households with the primary earner working in the private sector (see below), their average stock market participation is higher than in the whole population (55 percent); conditional on participation, the average Norwegian household in our sample invests about 38% of its portfolio in risky assets.

The **Employer-Employee Register** links workers to firms**;** for each worker it reports all employment spells with each employer, and the compensation received. This allows us to trace the working history of each worker as she moves 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 information on industry classification and institutional sector, whereas the other contains accounting data on the firm's 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 (if any) and is declared insolvent. We use this data set to identify episodes of firm closure and enrich the measure of background risk based on the variance of workers earnings with a measure of employment risk. In fact, the total variance of income comes partly from (high frequency) wage variability conditional on working, and partly from (low frequency) income variability conditional on losing the job.

Combining these three firm level data sets 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 risk of involuntary job loss at that firm. Because our measure of background risk depends on shocks to the firm that are in some degree passed over to workers, we focus on a sample of individuals who are continuously employed in the private sector (30% of the workers are employed in the public sector in Norway).[7]

[7] If there are multiple earners in the household (and both work in the private sector) we measure background risk

This excludes those who are not working (unemployed, retired, disabled, etc.) and those who have a spell in the government sector. We also exclude individuals who are younger than 25 (and hence possibly still in college) and those older than 60 (who may have intermittent participation and widespread access to early retirement, Vestad 2014). After these exclusions and a few others due to missing data at the firm level, we are left with a final sample of 4,846,766 observations. 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. Appendix A2 describes the sample selection in greater detail.

4.2 Employment and wage insurance in Norway

Portfolio (and savings) responses to wage fluctuations and risk of job loss clearly depend on how much insurance Norwegian workers can access through the welfare state. For example, no matter how large the volatility of wages, portfolio choice would be independent of it if background risk were fully insured.

Here we provide a broad description of social insurance programs in Norway, which are indeed relatively generous by international standards. First, workers enjoy generous unemployment insurance (UI). For permanent layoffs UI lasts for 52-104 weeks and replaces, 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%. Unlike the US, eligibility is means-tested (based on income and assets). Finally, individuals may have access to sickness and maternity benefits and active labor market programs to revamp their skills in case of displacement.

While Norwegian workers are better shielded than, say, US workers against extreme low realizations of their human capital (i.e., their consumption floor is higher), they do face substantial uninsured risk. First, government insurance offers large protection against unemployment risk but is fairly limited against the risk of wage fluctuations conditional on employment – especially those induced by firm-related shocks. There is indeed no insurance against wage cuts or *not* receiving bonuses, but there is against being laid off. While severe wage fluctuations induced by, say, work limitations are insured through the disability insurance system, the means-tested aspect of the program reduces the scope of insurance, in particular due to the relative low risk of a disability and the fungibility of savings (for example due to retirement or bequest motives). Second, unemployment

with the one faced by the primary earner.

insurance is time limited, and remaining unemployed is economically costly due to scarring effects (Nielsen and Reiso, 2011). Indeed, despite the institutional differences, in the 2001-2013 period average duration of unemployment in Norway was only 15% longer than in the US for people aged 25-54.[8]

5 Measuring Background Risk

In this and following sections we discuss our empirical findings. We start by motivating economically our instruments. Next, we estimate the marginal effect of background risk on portfolio allocation. Finally, we assess the robustness of our findings.

To construct a measure of labor income risk that can be arguably considered as unavoidable, we focus on shocks to firm profitability, which may induce variation in workers' pay (conditional on retaining the job) or even involuntary job loss in more extreme cases. This strategy requires that: a) we measure firm-related shocks; and b) we identify how much of these shocks are passed onto the worker's wages.

In principle, our instrument would be economically irrelevant if labor markets were frictionless and workers could move rapidly and without cost between firms. A frictionless labor market would, effectively, provide them with full insurance against firm idiosyncratic shocks. The fact that firm shocks are passed onto wages (as we document below) is of course *prima facie* evidence against this possibility.

Needless to say, the possibility that firm-specific shocks are passed onto workers' earnings requires that wages are at least partly 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 (2009), 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.

[8] See OECD statistics at http://stats.oecd.org/Index.aspx?DatasetCode=AVD_DUR.

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

$$\begin{aligned} \ln VA_{jt} &= \mathbf{X}'_{jt}\varphi + Q_{jt} + f^T_{jt} \\ Q_{jt} &= Q_{jt-1} + f^P_{jt} \end{aligned}$$

where $\mathbf{X}_{jt}$ is a vector of observables that captures the predictable component of firm's performance. The shock component is the residual $Q_{jt} + f^T_{jt}$, the sum of a random walk component Q_{jt} with permanent shock f^P_{jt} and a transitory shock component f^T_{jt}.

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.[9] Hence:

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

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. Replicating their methodology, their result has been shown to hold also in other countries, such as Portugal (Cardoso and Portela, 2009), Germany (Guertzgen, 2010), Hungary (Katay, 2008), Sweden (Friedrich et al.,

[9] These processes fit the data quite well. The first order autocovariances in the residual of the wage equation and in the firms value added equation are negative, economically large and highly statistically significant. The higher order autocovariances decay very rapidly (the second order autocovariance is 10 times smaller than the first order one in both processes). Not surprising given the very large number of observations, they retain statistical significance. Economically, however, autocovariances past the second lag are minuscule.

2015), Belgium (Fuss and Wintr, 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)'s methodology. Define the unexplained growth of firm value added, g_{jt}, and of workers' earnings, ω_{ijt} as :

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

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

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

Accordingly, we preliminarily run regressions for firm value added and workers' wages. In the first we control for year dummies, area dummies, sectorial 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 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 (the empirical analogs of g_{jt} and ω_{ijt} above), and estimate θ^T and θ^P. Results for the pass-through estimates 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 onto 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 restrictive. Given that transitory shocks play a small role, this is not worrying.

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 rolling averages:

$$\sigma^2_{it} = \frac{\sum_{s=0}^{T-1} \omega^2_{ijt-s}}{T}$$

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 transitory - computed over the same T periods:

$$\begin{aligned} F^P_{jt} &= \frac{\sum_{s=0}^{T-1} g_{jt-s}(g_{jt-s-1} + g_{jt-s} + g_{jt-s+1})}{T} \\ F^T_{jt} &= \frac{\sum_{s=0}^{T-1} g_{jt-s} g_{jt-s+1}}{T} \end{aligned}$$

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. We set $T = 5$ in what follows.[10]

5.2 Firm closure risk

Our second measure of background labor income risk is employment risk. This risk should also in principle reflect idiosyncratic shocks to the (worker's) firm so that it can vary across workers and over time.[11] We assume that the risk of firm bankruptcy captures the 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 average variance of earnings growth in our sample is 0.05, with a standard deviation of 0.11; both figures are small compared to those estimated from survey data (e.g. Gourinchas and Parker, 2002 and Cocco et al. 2005) partly reflecting absence of measurement error in our measure of earnings. In contrast, the variance of firm value added growth is much larger (0.16), with an extremely large standard deviation of 0.49. Finally, the risk of firm bankruptcy (the other measure of background risk we are going to use) in 2010 is small (0.2%). However, the consequences of involuntary job loss associated with firm

[10] The results are qualitatively similar if we use $T = 3$ or $T = 4$.

[11] Unemployment risk arising from macroeconomic fluctuations in economic activity constitutes background risk but, being common to all workers, is of little help in identifying the effect of labor income risk on financial decisions.

destruction may be quite disastrous, at least for some workers, due to scarring effects.[12] Allowing for job loss risk we can study the role of idiosyncratic tail background risk in households financial decisions whose importance for assets pricing has been recently stressed by Schmidt (2015).[13]

6 The Effect of Background Risk on the Risky Portfolio Share

Armed with these measures, we test whether and by how much investors react to 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 or even exiting the stock market altogether. We start with regressions of the portfolio share of risky financial 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 may also capture other sources of unobservable heterogeneity that may impact households portfolio allocation - such as differences in the precision of information about stock returns (Peress, 2004) or in financial sophistication (Calvet, Campbell and Sodini, 2009).

We start the analysis by simple fixed effects 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 with in the next section. Our empirical specification includes a rich set of 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 common changes in stock prices, and dummies for family type and area of residence. To capture well-documented differences in assets allocation due partly to fixed participation costs in the stock market and financial sophistication (Campbell, 2006), we control for lagged wealth. To account for interactions between levels of stockholding and housing (Cocco, 2004), we also control for homeownership status. Finally, and importantly, we control for household fixed effects. Results of these estimates are shown in column (1) of Table 3.

[12] Nilsen 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 still 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.

[13] Calibrated life cycle portfolio models find small effects of uninsurable wage risk on the portfolio share in stocks but larger effects, particularly at young age, for the idiosyncratic risk of a job loss associated with a large wage cut (Viceira, 2001; Cocco et al., 2005). However, this latter effect is obtained ignoring unemployment insurance.

The estimated coefficient on σ^2_{it} is 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 very precisely estimated. However, its size is small: one standard deviation increase in the (residual) variance of log earnings would reduce the risky assets share by 0.12 percentage points. Because the average risky assets share over the sample period is 21%, this amount to 0.6% 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 much 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, as shown in Table 2). Because of this, a regression of the share on the variance of firm performance cannot identify the marginal effect of background risk.

Estimates 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's earnings variance is negative and highly statistically significant and its size (in absolute terms) increases by a factor of 25 - from -0.02 to -0.5, resulting in a very high sensitivity of portfolio decisions to background earnings risk. Of course, the economic importance of background risk depend both on its marginal effect as well as on the size of background risk. In Section 7 we discuss the economic contribution of background risk in greater detail.

In all the specifications we have included also the risk of plant closure. We find that this tail measure of background risk discourages investment in risky assets, with effects decaying as the closure event is more distant into the future, which conforms with intuition. But the marginal effect is small.[14] Increasing the risk of plant closure by a factor of 10 relatively to its mean would

[14] The fact that workers reduce stock exposure in anticipation of plant closure suggests that they correctly perceive this risk. One may wonder whether the response we document is small because workers avoid the risk they face by abandoning in advance the "sinking ship" and smoothly relocating to another firm. To assess this possibility we estimate a probit model for the event of job mobility as a function of current and future firm shocks and worker's socio-demographic characteristics (results available on request). We find that future shocks to the firm growth and indicators for whether the firm goes bankrupt within 1-2 years have no statistically significant effect on mobility despite 3.2 million observations, implying that there is no support for the idea that "rats leave the ship before it sinks". The fact that workers adjust their investments in stocks in response to plant closure but do not relocate is consistent with the idea that mobility is costly to implement and that insurance through the labor market is hard to come by due to

reduce the share invested in risky assets by 0.07 percentage points, about 0.34% of the sample mean share. A larger marginal effect of wage risk than unemployment risk is consistent with the fact that the first source of variation is definitely uninsurable, while the second may be buffered (and actually is) by unemployment insurance.

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 on average about 45% stock market non-participants.

A formal treatment of censoring (e.g., through a Tobit approach) is unfeasible 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. Honorè and Hu (2004) 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 five different models: (1) Linear regression with households fixed effects (FE); (2) IV linear regression with households fixed effects (IVFE) (both of which we have already discussed in Table 3); (3) IV linear regression in which we replace the fixed effects with a rich control function strategy that includes observable fixed heterogeneity (IVC); (4) IV Tobit regression with the same control function (IVTC); and (5) a "double control function" estimator (2IVTC), in which one assumes a linear relationship between the fixed effect and the endogenous covariates, as in Chamberlain (1984).

If the three issues (endogeneity, fixed effects, censoring) are all important (and if the relationship between the fixed effect and the endogenous covariates takes a more general form), none of these models delivers consistent estimates. However, the bias of each of these models is different and can potentially be compared - as we do below - to gauge their relative importance and thus enable us to say something about the true value of λ. The online appendix provides a discussion of the different biases.

frictions.

We have already shown estimates for models (1) and (2) in Table 3 and reproduce the results of (2) in the first column of Table 4. In the second 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 (admittedly, very key determinants of risk tolerance or financial sophistication, see Guiso and Sodini, 2013). The estimate of λ drops (in absolute value) from -0.5 to -0.41 (which is consistent with the idea that omission of fixed effects generates an upward bias, for example because more risk tolerant investors select jobs with higher firm volatility). Though relatively large, this is not a dramatic drop from a qualitative point of view, an indication that the upward bias from omitting fixed effects is likely contained (at least conditioning on the rich control function). Column (3) shows estimates of a formal Tobit IV model with the same control function as in column (2), which should eliminate the bias from neglecting censoring. The estimate of λ is smaller but in the same ballpark, -0.32. The difference between IVTC and IVC can be interpreted as the bias induced by censoring.[15]

In the final column (4) we implement a "double control function" estimator.[16] In a first step we follow Blundell and Smith (1986), run a regression of our endogenous variable σ_{it}^2 on the (included and excluded) instruments and their means (to account for individual fixed effects in the wage variances, as suggested by Chamberlain, 1984), and save the residuals, $\widehat{e}_{it}$.[17] In a second step, we run a Tobit regression on σ_{it}^2, the residual $\widehat{e}_{it}$, the exogenous covariates W_{it}, and their means (to accout for individual fixed effects in the risky share equation). While the estimate is noisier due to the addition of many covariates, the size of the coefficient estimate is very similar, confirming the general pattern of results.

The fact that the IVFE, IVC, IVTC and 2IVTC estimates are of the same order of magnitude while the FE estimate is an order of magnitude less, suggests that the biases from ignoring censoring or unobserved heterogeneity are sizable but comparatively much smaller than the endogeneity bias. What is key is accounting for the latter.

[15] Since the Tobit model is non-linear while all the other models are linear, the bias induced by omitting fixed effects is different for the IVTC and IVC estimators. Hence, the difference between the two estimators reflects both censoring and the different incidence of fixed effects bias. We assume the latter difference is small.

[16] We thank Francis Vella for suggesting this approach.

[17] In other words, we assume that $\sigma_{it}^2 = z_{it}'\theta + m_i + \varepsilon_{it}$. Chamberlain (1984) suggests to model the fixed effect m_i as $m_i = z_{i0}'a_0 + ... + z_{iT}'a_T + l_i$. To reduce the computational burden, we assume instead $m_i = \overline{z_i}'a + l_i$.

6.2 Robustness

In this section we discuss various robustness analyses and extensions.

Instrument validity 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 can influence the outcome of the firm. This could be the case with the top managers of the firm because they exert a dominant role. To account for the possible bias induced by workers with dominant position inside the firm we focus on large firms, where arguably influence of any worker on firm productivity is diluted.

Our instruments may also be invalid if workers concentrate their stock investment in their firm's shares. 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 portfolio choice models), which for investors holding significant shares of their firm may be directly related to the variance of firm value added.[18] To account for potential instrument invalidity due to "own-firm bias" in household portfolio, we drop individuals with any holdings in their own firm.[19]

A final concern is that for a family what matters is the variation in total household earnings, rather than that of the primary earner. Indeed, within-family insurance (for example through added worker effects) may invalidate the use of the primary earner's wage volatility as a measure of background risk. To address this issue, we construct a measure of volatility based on household earnings (while continuing to use the same set of instruments as in the baseline regression - which refer to the primary earner).

Results for these various robustness checks are shown in Tables 5 and 6. In both tables the first column reproduces the baseline IV estimate of Table 3, third column. In Table 5 we report regressions when we retain only "large" firms (size above the 25th percentile of the distribution in the second column and above the median size in the third column, respectively). As can be seen, these exclusions - if anything- strengthen the estimated marginal effect of background risk and leave our qualitative conclusions unchanged.

In Table 6, we drop workers who have some assets invested in their own firm (second column)

[18] 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.

[19] The results are also robust to, instead of dropping individuals with holdings in their employers firm, redefining the risky portfolio to include only stocks in firms other than their own (i.e., the share of risky assets is redefined as $S_{it}^{'} = \frac{R_{it}^{'}}{R_{it}^{'}+RF_{it}}$, with R' being risky assets net of the value of own-firm stocks).

and redefine volatility to be the variance of household earnings (third column).[20] The results are again qualitatively unaffected. In the latter case, instruments are naturally less powerful but still pass conventional acceptability thresholds.

6.2.1 Heterogeneity

The effect of background risk on the demand for risky assets should be less important for households that have greater access to self-insurance (through accumulated assets). Similarly, pass-through coefficients of firm risk onto wages should be larger for wealthier individuals, as they are more willing to bear risk coming from the firm side due to their presumably higher risk tolerance.

These response heterogeneity predictions can be easily tested using interactions with household wealth. The results are reported in Table 7. In the top panel we report pass-through estimates. The first two columns replicate the estimates of the model of Table 2 using our sample (instead of the universe of private sector workers). Ignoring interactions with wealth, pass-through estimates are reassuringly very similar to those reported in Table 2. The last two columns show pass-through estimates when permanent and transitory firm shocks are interacted with wealth. As expected, firms offer less insurance to workers with higher wealth (and presumably higher risk tolerance or access to self-insurance), particularly against permanent shocks (the interaction with transitory shocks is not statistically significant).

In Panel B, we augment our baseline risky portfolio share regressions by interacting the variance of the worker's wages with lagged log financial wealth (and using as additional instruments the interaction of the latter with the firm's transitory and permanent shocks). We find again intuitive results: the marginal effect of background risk on the demand for risky assets declines with the level of financial wealth.[21]

[20] Household earnings volatility is obtained using the same methodology described in Section 5.1 (i.e., the variance of the residual of a regression of household earnings on observables).

[21] These results can also be used to address the criticism that our estimate of the marginal effect of background risk is high due to local (LATE) effects (Angrist and Imbens, 1994). It is well known that in the presence of response heterogeneity the IV estimator estimates (under some assumptions) not the "average treatment effect" (in our case, the average decline in the share of risky assets in portfolio that follows an increase in background risk), but a "local average treatment effect", which may be interpreted as the average treatment effect for the individuals who are mostly affected by a change in the instrument (i.e., the firm-related risk). For the LATE interpretation to be responsible for the high value of our baseline estimate, we need the coefficient of the interaction in the pass-through regressions to be of opposite sign to the coefficient of the interaction in the share regressions (those mostly affected by the change in the instruments, i.e., those with a larger pass-through coefficient, should be the ones with the larger

Figure 3 plots the pass-through effect (the dotted line on the left-hand scale, obtained considering permanent firm shocks only) and the marginal effect of background risk on the portfolio share for households (the continuous line on the right-hand scale) at different points of the distribution of wealth. Pass-through is always positive and it varies between 0.05 and 0.1 as wealth moves from the bottom to the top percentile.

The marginal effect of background risk on portfolio allocation is negative at all levels of wealth. However, while at the bottom of the distribution is large (around -1 or less), it drops around -0.5 around the median and is very close to zero at the top - consistent with the prediction of a self-insurance model. As we discuss in the next session, this wealth-induced heterogeneity in workers' insulation from firms shocks and in response to background risk translates in heterogeneity in the relevance of background risk. Furthermore, since total wealth and even more so the holdings of risky assets are heavily concentrated, the effect of background risk on the aggregate demand for risky assets is likely small - a calculation we perform formally in the next Section.

7 Quantifying the effects of background risk

The quantitative assessment of the importance of background risk hinges on two ingredients.

The first ingredient is the size of λ, the marginal effect of a unit increase in background risk arising from on-the-job wage variation. From the results reported in Table 4, $\lambda \geq -0.5$. We will perform calculations using the (absolute value) upper bound $\lambda = -0.5$. If the effect of background risk is small using this upper bound, it is *a fortiori* even smaller if we consider lower estimates of λ in absolute value.

The second ingredient is the size of overall background risk. Gauging the latter is more problematic. We cannot use the size of unobserved wage variance precisely because of the argument that not all variation is risk. However, we can bypass this problem because we can identify the sources of background risk and, by varying them, we can provide bounds of its overall effect on the portfolio share.

sensitivity of background risk to the demand for risky assets). However, we find exactly the opposite, suggesting that LATE is unlikely to be an issue. In unreported regressions we generalize this exercise by allowing the partial insurance coefficients to vary with a whole vector of observable individual and firm characteristics: length and type of education, wealth, firm size, age, gender. And the same we do for the portfolio share equation. Though we find that some of these variables (namely schooling, wealth and firm size) are significant shifters of the pass-though and/or of the effect of background risk on the share of risky assets in portfolio, we do not find anything systematic that would make us conclude that a LATE interpretation is justified.

Background risk is defined as:

$$B_{it} = \theta_v^2 V_{it} + \theta_f^2 F_{it}$$

For given values of estimated F_{it} and V_{it} - the variance of the firm's value added growth and the variance of the worker's earnings growth, respectively - its size depends on θ_v, the extent of worker-specific variation that is due to risk rather than choice, and the pass-through of firms shocks to wages θ_f. To assess the importance of background risk we do two exercises. First, we compute the contribution of current estimated background risk to the portfolio share as:

$$\widehat{\lambda}\widehat{B_{it}} = \widehat{\lambda}\left(\widehat{\theta_v^2}\widehat{V}_{it} + \widehat{\theta_f^2}\widehat{F}_{it}\right)$$

Second, we estimate the effect on the risky portfolio share of changing background risk from this estimated baseline by varying workers exposure to firm specific risk θ_f or increasing the share of worker-specific wage variation that is risk, θ_v :

$$\widehat{\lambda}\Delta B_{it} = \widehat{\lambda}\left((\theta_v^2 - \widehat{\theta_v^2})\widehat{V}_{it} + (\theta_f^2 - \widehat{\theta_f^2})\widehat{F}_{it}\right)$$

This computation assesses the economic importance of background risk by "shocking" the two parameters that capture workers' exposure to risk, one through institutions or extent of superior information workers may have about evolution of their wages, θ_v; the other through firm-provided insurance, θ_f. This exercise is of interest because, as shown by Lemieux et al. (2009) and Benabou and Tirole (2015), there is strong evidence of a rise of pay for performance wage schemes and high-powered incentives over the past decade, not only among workers in top positions but also among low rank employees.[22] And competitive pressure for talent could make incentives even more powered in the future.

To perform these calculations we take the pass-through coefficient with respect to permanent firm shocks, $\widehat{\theta}_f = 0.07$ (because the response to transitory shocks is tiny, and hence adding it would make little difference). We quantify the baseline share of worker-specific wage variation that is risk as follows: under the assumption that censoring bias is unimportant and insurance within the firm is substantial (both backed by the estimates in Table 2 and the evidence in Table 4)

[22] Lemieux et al. (2009) show that in the US between the 1970's and the 1990's, the fraction of workers paid based on the basis of performance rose from 38% to 45%, and for salaried workers from 45% to 60%. This pattern is not confined to the US. Bloom and Van Reenen (2010), for instance, document that the fraction of UK establishments using some form of performance pay rose from 41% in 1984 to 55% in 2004.

$p\lim \widehat{\lambda}_{FE} \approx \rho_v p\lim \widehat{\lambda}_{IVFE}$. Hence, $\widehat{\theta}_v \approx 0.2$. Finally, we estimate F_{it} and V_{it} using the variance of the firm's value added growth and the variance of the worker's earnings growth, respectively ($\widehat{F}_{it} = 0.16$ and $\widehat{V}_{it} = 0.053$, from Table 1).[23]

The surface we plot in Figure 4 is the economic effect of background risk on the share of risky assets in portfolio, computed as:

$$\widehat{\lambda}\left(\theta_v^2 \widehat{V}_{it} + \theta_f^2 \widehat{F}_{it}\right)$$

where we use the baseline estimate $\widehat{\lambda} = -0.5$. The crossing between the two darker lines on the surface marks the sample estimates combination $(\widehat{\theta}_v, \widehat{\theta}_f)$.

Evaluated at the average values of V_{it} and F_{it} and at the point estimates of the parameters $(\widehat{\lambda}, \widehat{\theta}_v, \widehat{\theta}_f)$ the economic effect of background risk is tiny: the predicted decline in the share of risky assets is -0.14 percentage points. However, if workers were to share equally the firm-specific risk ($\theta_f = 0.5$), for given θ_v, the effect would be as high as 2 percentage points (or 10 percent of the average share of risky assets in portfolio). In contrast, holding constant θ_f, increasing the amount of worker-specific variation that is due to risk, rather than choice, leaves the effect of background risk on the demand for stocks fairly small. Indeed, even if half of the worker-specific wage variation was risk, the effect of background risk would remain small: a predicted 0.7 percentage point decline. This is visible from the slope of the surface, which is steeper when we move along the θ_f-axis than when we move along the θ_v-axis.

We have documented substantial wealth-induced heterogeneity in pass-through of firm-related shocks onto wages as well as in the sensitivity of the demand for stocks to background risk. Consequently, we should expect substantial heterogeneity in the economic effect of background risk. To illustrate, we consider the effect for households at the 5th and 95th percentile of the wealth distribution. The estimates of $\widehat{\lambda}$ are, respectively, -0.97 and -0.097. The other important element that varies is the pass-through coefficient, which takes values 0.06 and 0.10, respectively for the 5th and 95th percentile of the wealth distribution. Evaluated at the average values of V_{it} and F_{it} and at the point estimates of the parameters $\widehat{\theta}_v, \widehat{\theta}_f$, the economic effect of background risk are still small in both groups (-0.23 percentage points at the 5th wealth percentile and -0.06 percentage points at the 95th percentile). Figure 5 reports the corresponding background risk effect surfaces for the two groups.

For the wealthy, neither variations in θ_f nor θ_v would affect their background risk response

[23] In fact, an estimate of V_{it} should subtract, from the variance of wage growth, the contribution of the firm component - which is however tiny given the extent of insurance within the firm.

much. The response surface is fundamentally flat. In contrast, the slope of the surface among the poor is much steeper; a reduction in firm insurance could potentially have large impact on their portfolio choice, reducing even further the amounts of wealth held in risky instruments. For these workers, sharing half of the shocks to their firms would lower the portfolio share in risky assets by about 15 percentage points, a very large drop. Also an increase in wage risk unrelated to the firm's fortunes could have a substantial impact. However, because these workers own a small fraction of total stocks, these larger effects are unlikely to generate large aggregate consequences (which we document next).

As our last exercise we look at the effect of background risk for the aggregate demand for stocks in the baseline and in the hypothetical scenarios in which we vary the extent of background risk faced by individuals. We allow for wealth-related heterogeneity in both the pass-through of firms shocks and the portfolio sensitivity to background risk. This exercise is relevant for understanding the role of background risk for assets prices.

To perform this exercise, we consider an increase in θ_f and θ_v from their point estimate to 0.5, so that workers share 50% of the permanent shocks to their firm and 50% of their personal wage variation is risk. For a given worker i with initial wealth A_{it-1} the effect on the risky share of rasing θ_f and θ_v from $(\widehat{\theta}_v, \widehat{\theta}_f)$ to $(0.5, 0.5)$ is:

$$\Delta S_i = (\widehat{\lambda}(A_{it-1}) \left(0.25\widehat{V}_{it} + 0.25\widehat{F}_{it}\right) - \widehat{\lambda}(A_{it-1}) \left(\widehat{\theta}_v^2 \widehat{V}_{it} + (\widehat{\theta}_f(A_{it-1}))^2 \widehat{F}_{it}\right)$$

and that on the individual demand for stocks:

$$\text{Change in demand for stocks} = A_{it-1}\Delta S_{it}$$

Accordingly, our estimate of the effect on the aggregate demand for stocks is

$$\text{\% change in aggregate demand for stocks} = (\sum_i A_{it-1}\Delta S_{it})/(\text{Total stocks}_{\text{t-1}})$$

We estimate this effect to be 0.2% on average over all sample years - a tiny response to a large change in background risk. Increasing the size of the shock by setting θ_f and θ_v to 0.8 leaves the result qualitatively unchanged. The reason why the the aggregate demand for stocks is insensitive to background risk is that the effect of background risk is small at high wealth levels, and the ownership of risky assets in concentrated precisely among the wealthy. In fact, we calculate that among the households with below median wealth increasing θ_f and θ_v to 0.5 lowers the demand for risky assets by 2.8% while it has a negligible effect among households with above median wealth.

Overall, the calculations in this section imply that background risk is economically important for individuals with low assets; for those who can count on a sufficiently high level of buffer savings the tempering effect of background risk is contained. The combination of very high sensitivity among the poor, low sensitivity among the wealthy and the concentration of risky assets in the hands of the latter implies a small effect of even large increases in background risk on the aggregate demand for risky assets, suggesting a small role of background risk as a driver of asset prices.

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. Even 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 in 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 25 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. At sample means and for the median wealth household the contribution of background risk is small. But, because marginal responses differ considerably depending on the buffers accumulated, the economic importance of background risk varies greatly: it is large for the poor and negligible for the wealthy. In this sense, background risk is a viable explanation of portfolio heterogeneity among low wealth people but not among the high wealth segment.

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.[24]

[24] Palia 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 (as it assumes that all measured variation in labor income, housing and private equity returns is 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, 2000b). 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 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.[25] 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

[25] See the 2011 Annual Report from the Norwegian Tax Administration, http://www.skatteetaten.no/en/.

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, expect 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 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 customers' 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 minimizing 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 liability 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. 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 (*grunnbelopet*) 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.[26] Further, workers who have some period of disability of 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 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.

[26] The 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 grunnbelopet.

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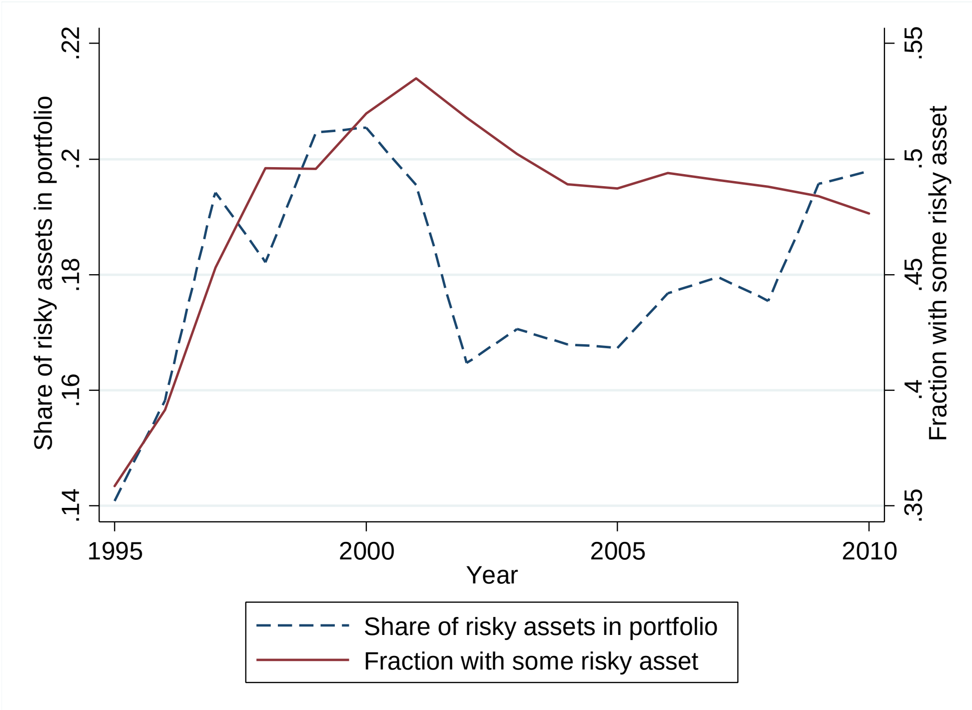
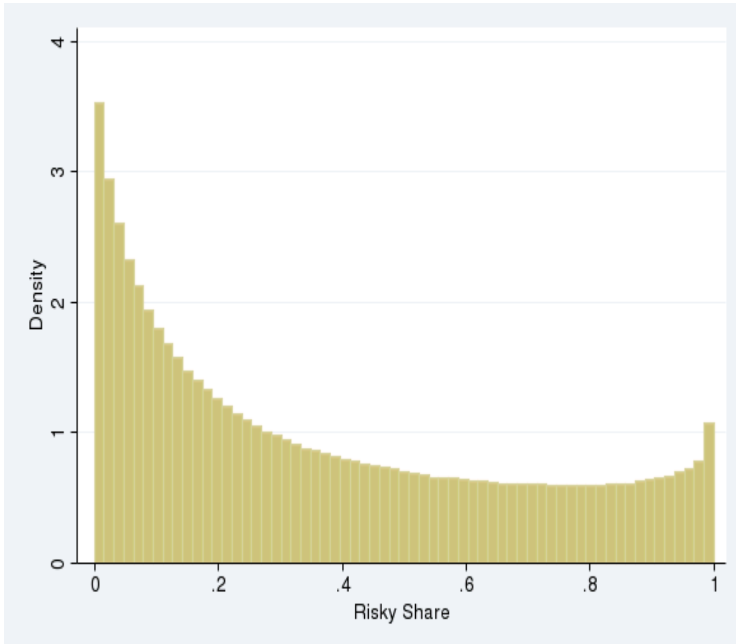
Figure 1: The evolution of stock market participation and the share of risky assets in portfolio.



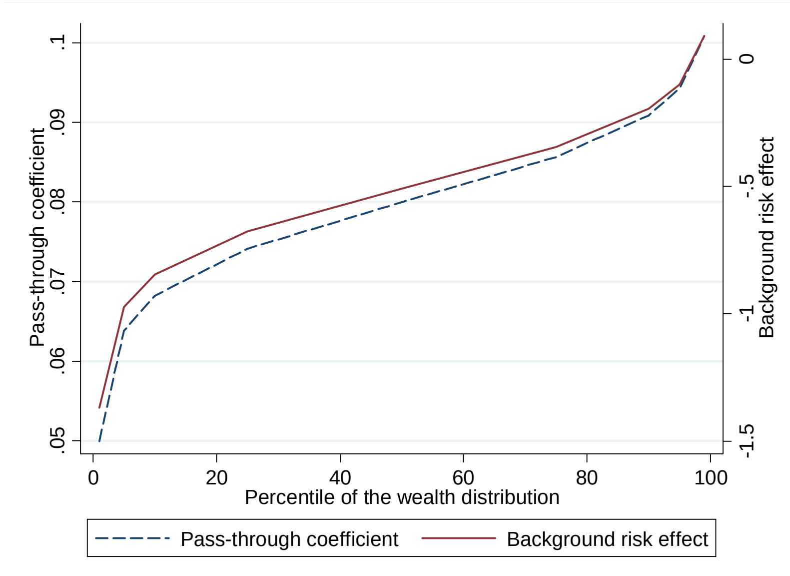
Note: The figure shows the average share in risky assets (including that of non stockholders left scale) and the fraction of stockholders (right scale) among Norwegian households by year.

Figure 2: The distribution of the share of risky assets in portfolio.



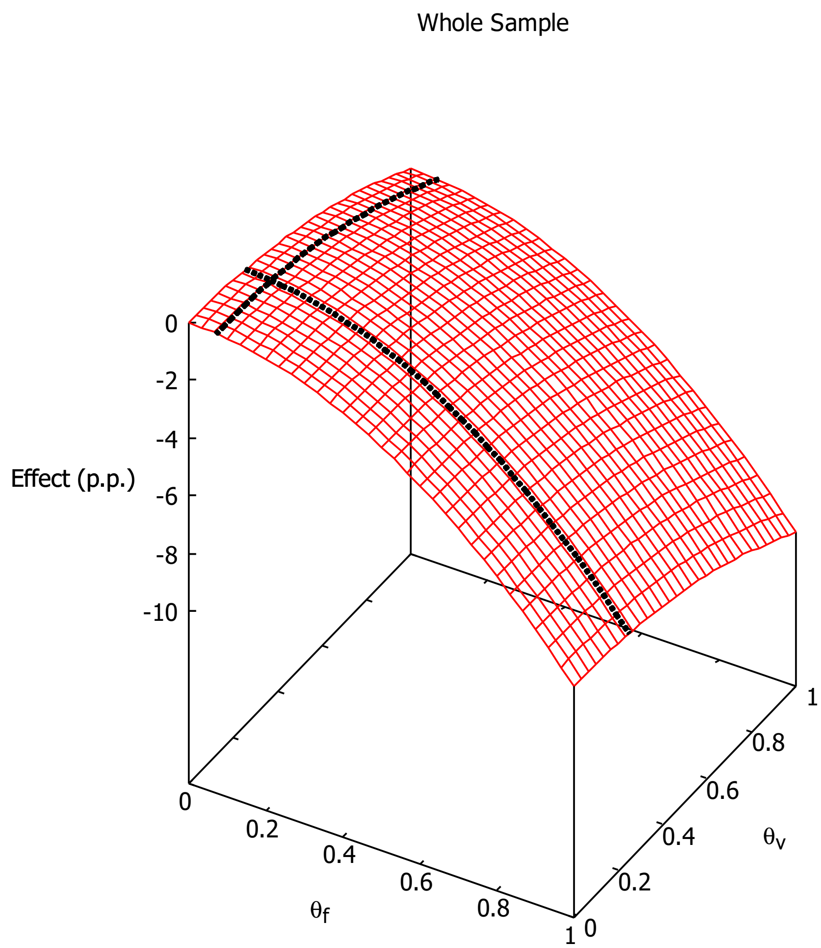
Note: The figure shows the sample distribution of the share of risky assets in the portfolio of Norwegian households.

Figure 3: Wealth-induced heterogeneity in pass through and marginal effect of background risk.



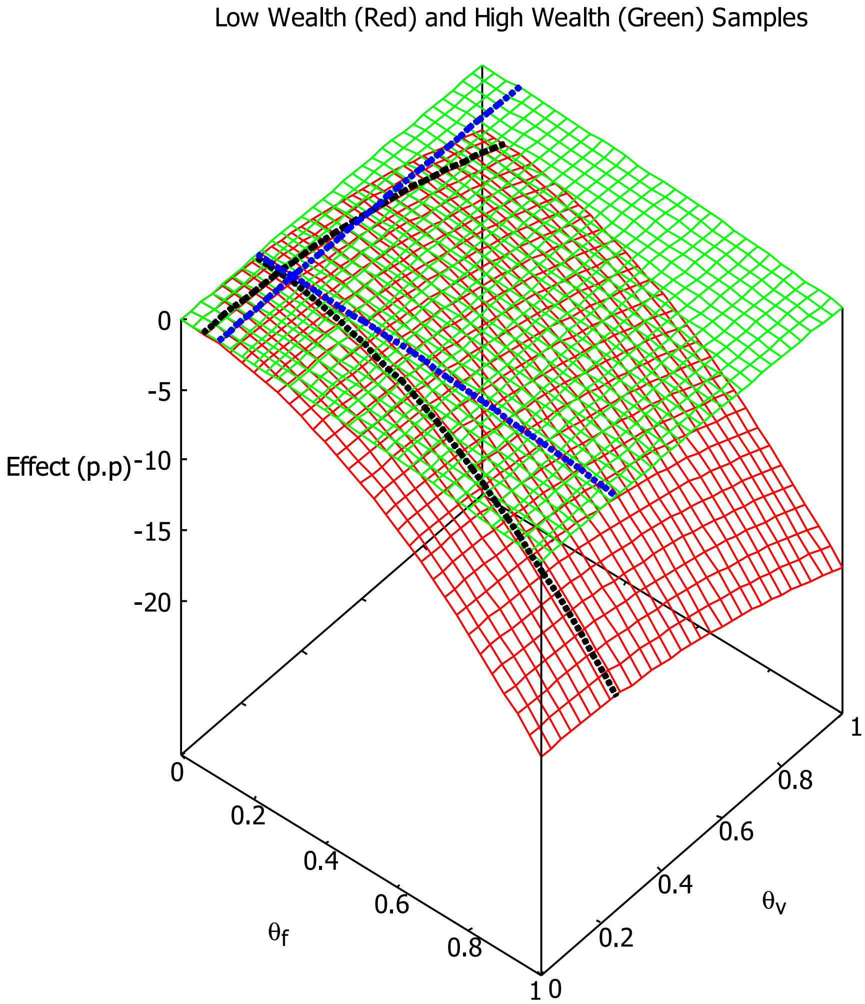
Note: The dotted line shows the pass through coefficient of permanent shocks to the firm onto workers wages by worker wealth percentile; its values are based on the estimates in Table 7 Panel A, and are measured on the left-hand scale. The continuous line shows the IV estimate of the marginal effect of background risk, obtained from Table 7 Panel B on the risky portfolio share by wealth percentile (values on the right hand scale).

Figure 4: The effect of background risk on the share of risky assets.



Note: The figure shows the percentage points reductions (vertical line) in the portfolio share in risky assets by Norwegian households as background varies when the level of insurance within the firms and the fraction of the wage variance that is not predictable vary. Estimates are obtained for the baseline estimate of the marginal effect of background risk on the portfolio (-0.5). The cross of the darker line on the surface of the figure corresponds to the point estimates of the pass through of permanent firm shocks and the share of wage variability that is not predictable.

Figure 5: The effect of background risk for low and high wealth samples.



Note: The figure shows the percentage points reductions (vertical line) in the portfolio share in risky assets by Norwegian households as background varies for two levels of the wealth corresponding to the 5th percentile(red surface) and to the 95th percentile (green surface) respectively. Variation in the level of background risk is due to different combinations of the level of insurance within the firms and the fraction of the wage variance that is not predictable. Estimates account for both differences in the pass through of permanent shocks and in the marginal portfolio response to background risk as wealth varies. The cross of the blue and black lines darker lines on the two surfaces corresponds to the point estimates of the pass through of permanent firm shocks and the share of wage variability that is not predictable.

Table 1: Descriptive Statistics, 1995-2010

Variable	**Mean**	**Std. Dev.**	**N**
Age	45.518	8.545	1,972,639
Male	0.816	0.387	1,972,639
Less than High School	0.196	0.397	1,972,639
High School	0.564	0.496	1,972,639
Some College or more	0.24	0.427	1,972,639
Family size	2.881	1.405	1,972,639
Value of risky assets	479,458.1	8,854,058.0	1,972,639
Value of safe assets	359,031.3	2,253,133.8	1,972,639
Share risky assets	0.207	0.293	1,972,639
Participation share	0.552	0.497	1,972,639
Cond. share risky assets	0.375	0.304	1,089,477
Earnings	415,686.0	233,517.7	1,972,639
Earnings, family	530,971.1	306,566.9	1,972,639
Variance earnings growth	0.053	0.105	1,972,639
Variance earnings growth, family	0.077	0.127	1,972,639
Variance value added growth	0.16	0.493	284,627
Permanent shocks	0.044	0.179	205,874
Transitory shocks	0.051	0.249	243,632
Firm size	26.88	141.374	347,813
Firm bankrupt in 1 year	0.002	0.044	1,972,639

Note: The table shows summary statistics of the demographic households and firm characteristics, portfolio and wealth variables and background risk measures used in the estimates for our reference sample (see Appendix A2). Asset and earnings values are NOK in 2010 prices (1 USD approx. 5.61 USD).

Table 2: Pass-through of firms' shocks to workers' wages

	(1) Permanent value added shocks	(2) Transitory value added shocks
Pass-through coef.	0.0705***	0.0175***
	(0.0056)	(0.0053)
Constant	-0.0021***	-0.0023***
	(0.0002)	(0.0002)
Hansen J-test p-value	0.54	0.00
F-stat 1st stage	134.21	688.46
Observations	2,358,889	2,370,420

Note: The table reports estimates of the pass through coefficient of permanent (Column (1)) and transitory shocks (Column (2)) to the firms performance onto its workers wages using the identification strategy of Guiso, Pistaferri and Schivardi (2005). Clustered standard errors are in brackets. Coefficient significance: *** at 1% or less; ** at 5%; * at 10%.

Table 3: The effect of background risk on the financial portfolio share

	(1) Fixed effect	(2) Reduced form fixed effect	(3) Fixed effect IV (*Baseline*)
σ^2_{it}	-0.0202***		-0.4986***
	(0.0029)		(0.1827)
F^P_{it}		-0.0033***	
		(0.0012)	
F^T_{it}		-0.0028***	
		(0.0007)	
Firm bankrupt in 1 year		-0.0112**	-0.0201***
		(0.0050)	(0.0066)
Firm bankrupt in 3 years		0.0008	-0.0040
		(0.0027)	(0.0034)
Firm bankrupt in 5 years		0.0006	-0.0020
		(0.0028)	(0.0035)
Lagged log wealth	0.0153***	0.0104***	0.0112***
	(0.0002)	(0.0002)	(0.0003)
Home ownership	0.0176***	0.0135***	0.0146***
	(0.0009)	(0.0009)	(0.0012)
Age	0.0224***	0.0195***	0.0222***
	(0.0006)	(0.0005)	(0.0017)
Age sq.	-0.0002***	-0.0001***	-0.0001***
	(0.0000)	(0.0000)	(0.0000)
Family size	-0.0005	0.0005	-0.0013*
	(0.0006)	(0.0006)	(0.0008)
Hansen J-test p-value			0.13
F-stat 1st stage			56.85
Observations	1,972,639	1,655,104	1,184,800

Note: The table reports estimates of the marginal effect of background risk on the risky financial portfolio share. Column (1) shows simple fixed effect (OLS) regressions; Column (2) reports reduced form regressions of the share on the two instruments - the variance of transitory and permanent shocks to firms value added. Column (3) shows IV estimates. Regressions also control for family type, area, and year dummies. Hansen J-test for instrument validity and F-stat for the power of the instruments are shown at the bottom of the table. Clustered standard errors are in brackets. Coefficient significance: *** at 1% or less; ** at 5%; * at 10%.

Table 4: Assessing the relevance of unobserved heterogeneity and censoring

	(1) Fixed effect IV (*Baseline*)	(2) IV with control function	(3) Tobit IV with control function	(4) Tobit w/ double control function
σ^2_{it}	-0.4986***	-0.4144***	-0.3199***	-0.373*
	(0.1827)	(0.1152)	(0.1806)	(0.1990)
Firm bankrupt in 1 year	-0.0201***	-0.0032	-0.0157	-0.0211
	(0.0066)	(0.0069)	(0.0125)	(0.0134)
Firm bankrupt in 3 years	-0.0040	0.0037	0.0025	0.0030
	(0.0034)	(0.0047)	(0.0085)	(0.0083)
Firm bankrupt in 5 years	-0.0020	0.0055	0.0057	0.0074
	(0.0035)	(0.0048)	(0.0089)	(0.0085)
Lagged log wealth	0.0112***	0.0535***	0.1187***	0.0614***
	(0.0003)	(0.0003)	(0.0005)	(0.0007)
Home ownership	0.0146***	0.0248***	0.0553***	0.0190***
	(0.0012)	(0.0011)	(0.0021)	(0.0026)
Age	0.0222***	0.0141***	0.0235***	0.0153***
	(0.0017)	(0.0011)	(0.0017)	(0.0017)
Age sq.	-0.0001***	-0.0002***	-0.0003***	-0.0004***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Family size	-0.0013*	0.0076***	0.0143***	0.0056***
	(0.0008)	(0.0007)	(0.0011)	(0.0016)
Male		0.0294***	0.0298***	0.0292***
		(0.0016)	(0.0027)	(0.0027)
Hansen J-test p-value	0.13	0.05	0.08	
F-stat 1st stage	56.85	347.91	347.91	
Observations	1,184,800	1,230,063	1,230,063	1,230,063

Note: The table reports estimates of the marginal effect of background risk on the risky financial portfolio share. Column (1) reproduces the IV benchmark regression of Table 3, Column (3); Column (2) shows IV estimated but replaces the fixed effect with a control function; Column (3) shows Tobit IV estimates with the control function; Column (4) shows Tobit estimates with double control functions. Regression in Column (1) also control for family type, year and area dummies. Regressions in column (2)-(3) add education length and type. Further, column (4) includes also means of the control variables at the individual level, and the residual from an OLS-regression of σ^2_{it} on the reported control variables, F^P_{it} and F^T_{it}, as well as the individual means of the latter. Hansen J-test for instrument validity and F-stat for the power of the instruments are shown at the bottom of the table. In the third column the reported p-value for the test for instrument validity comes from a two-step procedure for computational reasons. Clustered standard errors are in brackets. Coefficient significance: *** at 1% or less; ** at 5%; * at 10%.

Table 5: Robustness: firm size

	(1) Whole sample	(2) Firm size >25^{th} perc.	(3) Firm size >50^{th} perc.
σ^2_{it}	-0.4986***	-0.6262***	-0.7558***
	(0.1827)	(0.2107)	(0.2304)
Firm bankrupt in 1 year	-0.0201***	-0.0207***	-0.0208***
	(0.0066)	(0.0067)	(0.0071)
Firm bankrupt in 3 years	-0.0040	-0.0034	-0.0035
	(0.0034)	(0.0035)	(0.0036)
Firm bankrupt in 5 years	-0.0020	-0.0037	-0.0046
	(0.0035)	(0.0036)	(0.0037)
Lagged log wealth	0.0112***	0.0106***	0.0102***
	(0.0003)	(0.0003)	(0.0003)
Home ownership	0.0146***	0.0139***	0.0122***
	(0.0012)	(0.0013)	(0.0014)
Age	0.0222***	0.0215***	0.0200***
	(0.0017)	(0.0020)	(0.0022)
Age sq.	-0.0001***	-0.0001***	-0.0001***
	(0.0000)	(0.0000)	(0.0000)
Family size	-0.0013*	-0.0014*	-0.0015*
	(0.0008)	(0.0008)	(0.0008)
Hansen J-test p-value	0.13	0.19	0.36
F-stat 1st stage	56.85	44.53	38.04
Observations	1,184,800	1,124,682	1,038,205

Note: The table reports estimates of the marginal effect of background risk on the risky financial portfolio share. Column (1) reproduces the IV benchmark regression of Table 3, Column (3); Column (2) and (3) runs the IV estimates on the sample of large firms, respectively above the 25th percentile (Column (2)) and the median size (Column (3)). Regressions in Panel B also control for family type, area, and year dummies. Hansen J-test for instrument validity and F-stat for the power of the instruments are shown at the bottom of the table. Clustered standard errors are in brackets. Coefficient significance: *** at 1% or less; ** at 5%; * at 10%.

Table 6: Additional robustness checks

	(1) Baseline	(2) Excluding owners	(3) Family earnings variance
σ^2_{it}	-0.4986***	-0.5161***	
	(0.1827)	(0.1838)	
σ^2_{it}, family earnings			-0.5665***
			(0.2096)
Firm bankrupt in 1 year	-0.0201***	-0.0201***	-0.0224***
	(0.0066)	(0.0067)	(0.0069)
Firm bankrupt in 3 years	-0.0040	-0.0040	-0.0043
	(0.0034)	(0.0034)	(0.0034)
Firm bankrupt in 5 years	-0.0020	-0.0019	-0.0019
	(0.0035)	(0.0035)	(0.0035)
Lagged log wealth	0.0112***	0.0111***	0.0115***
	(0.0003)	(0.0003)	(0.0003)
Home ownership	0.0146***	0.0146***	0.0146***
	(0.0012)	(0.0012)	(0.0013)
Age	0.0222***	0.0220***	0.0234***
	(0.0017)	(0.0018)	(0.0014)
Age sq.	-0.0001***	-0.0001***	-0.0001***
	(0.0000)	(0.0000)	(0.0000)
Family size	-0.0013*	-0.0012	-0.0006
	(0.0008)	(0.0008)	(0.0008)
Hansen J-test p-value	0.13	0.12	0.14
F-stat 1st stage	56.85	56.09	35.63
Observations	1,184,800	1,173,031	1,184,800

Note: The table reports IV estimates of the marginal effect of background risk on the risky financial portfolio share. Column (1) reproduces the IV benchmark regression of Table 3, Column (3); Column (2) excludes from the sample workers in top management positions and those with own-firm stocks; Column (3) measures background risk with the variance of family earnings. Regressions also control for family type, area, and year dummies. Hansen J-test for instrument validity and F-stat for the power of the instruments are shown at the bottom of the table. Clustered standard errors are in brackets. Coefficient significance: *** at 1% or less; ** at 5%; * at 10%.

Table 7: Wealth-induced heterogeneity

	Panel A: Pass-through regressions			
	(1)	(2)	(3)	(4)
	Permanent value added shocks	Transitory value added shocks	Permanent value added shocks	Transitory value added shocks
Pass-through	0.0796***	0.0153***	0.0232	0.0010
	(0.0053)	(0.0062)	(0.0157)	(0.0116)
Pass-through*Lagged log wealth			0.0048***	0.0012
			(0.0012)	(0.0010)
Hansen J-test p-value	0.62	0.00	0.63	0.00
F-stat 1st stage	178.91	752.05	107.26	172.64
Observations	1,316,004	1,321,303	1,316,004	1,321,303

	Panel B: Risky share regressions
σ^2_{it}	-2.1392***
	(0.7799)
σ^2_{it}*Lagged log wealth	0.1379**
	(0.0652)
Hansen J-test p-value	0.27
F-stat 1st stage	28.82
Observations	1,184,800

Note: The table reports estimates of the pass-through (Panel A) and of the portfolio share (Panel B) allowing both the pass through and the marginal effect of background risk on the risky portfolio share to vary with the lagged value of individual wealth. Regressions in Panel B also control for family type, area, and year dummies. Hansen J-test for instrument validity and F-stat for the power of the instruments are shown at the bottom of the table. Clustered standard errors are in brackets. Coefficient significance: *** at 1% or less; ** at 5%; * at 10%.