EARNINGS RISK AND THE U.S. HOUSING MARKET

by

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ABSTRACT

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The goal of this dissertation is to develop, refine, and employ empirical measures of earnings risk—especially permanent risk—and determine their effect on behavior, with applications specific to the recent mortgage bubble.

Chapter 1 aims to identify covariates of risks associated with permanent and temporary earnings shocks. The distinction is significant both because permanent shocks are more consequential and because measurement error would contaminate only measures of temporary risks. Generalizing methods used in previous work, we allow risk to vary both across individuals and over individuals' careers, so they could be used to study behavioral responses to risk even with individual fixed effects. We find the majority of overall earnings risk is temporary, yet permanent risks vary notably across race and education.

Chapter 2 uses the measures developed in chapter 1 to analyze the information structure in the mortgage market during the first decade of the 2000s. A feature of last decade's mortgage crisis was that the credit risk of borrowers was not accurately priced

into their mortgages (e.g., the increased risk of delinquency was not associated with a higher interest rate). I confirm this belief by using a measure of labor earnings risk, a major source of risk at the household level. I find that borrowers who are granted a mortgage, labor earnings risk measured at origination predicts a greater probability of delinquency, yet its relationship to loan terms is weak. Further results also lend evidence to theories involving information problems: increases in permanent labor earnings risk measured at origination (*i*) increase borrowers' perceived risk of falling behind on payments and (*ii*) influences lenders' restructuring decisions. This disconnect is not due to supply side explanations I can indirectly test such as market distortions induced by lending policies. Since I cannot provide clear evidence of lenders' ability to appreciate this risk at the point origination, room remains to test the importance of the incentive for lenders to resell mortgages in the secondary market.

Chapter 3 investigates the divergence of the gap in homeownership rates that occurred over the entire course of the recent housing-related business cycle. After controlling for a rich set of factors determining the willingness and ability to own a home, I find that most of the change in the white/non-white homeownership gap, particularly during the housing bust, can be explained by observable factors. The main contributors are marriage and fertility, especially during the boom; geography, income instability, and education also contribute during the bust.

This dissertation is dedicated to my parents.

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All errors are my own.

1 Heterogeneity in Exposure to Permanent and Temporary Earnings Shocks

1.1 Introduction

The distinction between permanent and temporary earnings shocks has been widely appreciated at least since Friedman's (1957) seminal work on the Permanent Income Hypothesis. Just as we expect individuals' consumption to bear a stronger relationship with their permanent income than to deviations from it, we also expect it to respond much more substantially to permanent shocks than to temporary ones. Permanent shocks are more consequential not only due to their greater duration, but also because they cannot be easily smoothed by borrowing and saving. The distinction matters even ex ante: the risk of permanent shocks may discourage investments with irreversible costs, but in the absence of credit constraints temporary shocks should not have profound effects on behavior, except perhaps to encourage precautionary savings or to maintain access to credit markets. There are also implications for data analysis, as errors in reporting individuals' earnings are much more likely to contaminate measures of temporary earnings risk than those of permanent risk (Duncan et al. 1985).

For all of these reasons, it would be desirable to measure the risks of permanent and temporary earnings shocks separately. Furthermore, to the extent that our interest is motivated by behavioral responses to permanent shocks or the risk thereof, it would be essential to allow some disaggregation. At the very least, this would involve computing separate measures of risk for different subgroups of the population, but ideally one would prefer measures that were computed at the individual level and at relatively high frequencies. However, presumably because it has been motivated by other issues like trends in aggregate inequality and churning of the income distribution, most previous work has focused mainly on measuring risks (or trends in risk) that affect the entire population. Only a few papers have even attempted to construct separate measures of permanent and temporary earnings risk at the individual level, and none have allowed risks to vary within individuals' careers.

This paper aims to fill that gap. First, it adapts an empirical approach developed by Carroll and Samwick (1997) in order to allow risks faced by individuals to change over time. The implicit assumption that individuals' earnings risks were constant over time was not very restrictive for their study because they only analyzed a few years of data, but relaxing it makes an important difference when we examine observations over several decades. We then create a second set of estimated risks that vary at the individual level by adapting a related strategy that Meghir and Pistaferri (2004) had used to measure such risks at the population level (stratified only by broad levels of education). While motivated by exactly the same model, the statistics that emerge from these two strategies are computed quite differently, so we then verify that they lead to similar conclusions. Our results confirm earlier conclusions that earnings risk is mainly transitory in nature (potentially representing mostly measurement error), and our estimates of permanent risk are somewhat smaller than those in previous work, yet they still indicate that a permanent earnings shock that is one standard deviation in magnitude causes annual earnings to fluctuate by about 10 percent. We also find that permanent risks vary notably across educational and racial groups.

The following section elaborates on both our goals and the strategies used in previous research, leading into the adaptations we propose in 1.3. The next two sections describe our data and the practical details of our estimation procedure; 1.6 then presents the empirical results; and 1.7 concludes with a few remarks and avenues for future research.

1.2 Estimating Earnings Risk

Analyses of earnings risk begin by decomposing individuals' earnings (or a similar measure) into three parts meant to describe the information available to the econometrician and to the individuals themselves:

$$y_{igt} = F(A_{it};\theta_g) + Q(A_{it};\theta_i, g) + u_{it}, \tag{1}$$

where y_{igt} represents the log earnings of person i from demographic group g in year t. Function F represents heterogeneity that can be predicted by the econometrician; it is typically specified as a polynomial (often a quartic) in the person's age A_{it} and perhaps other factors, with coefficients θ that are common to everyone in group g. The second component Q is intended to represent additional heterogeneity that is known to the individual but not directly observed by the econometrician. In practice this is often specified as an individual fixed, or perhaps a random effect whose variance may differ across groups or over time (e.g., Gottschalk and Moffitt 1994 and Carroll and Samwick 1997), but some models allow for heterogeneity in expected earnings growth rates by using a higher-order polynomial with person-specific coefficients (Gordon 1984; Haider 2001; Guvenen 2007). The sum $F+Q$ is then interpreted as the individual's expectation of his or her own earnings, so it represents predictable heterogeneity rather than risk.

If so, the final component—the residual u_{it}—represents deviations from the person's own expected age-earnings profile, reflecting realizations of risk. Most studies of earnings risk simply attempt to measure the variance of u across large groups of people and/or over long periods of time. For example, Gottschalk and Moffitt's (1994) well-known study uses the variance of these residuals over two long (8-year) periods and across several large groups of workers (e.g., stratified by broad levels of education or one-digit industries) to measure the share of cross-sectional earnings inequality that is attributable to transitory shocks rather than differences in individuals' permanent incomes. Many other studies attempt to determine only how the variance of u has evolved over time.

Presumably one reason for analyzing aggregates is that individuals actually experience only a small subset of their potential outcomes, especially over the relatively short periods in which they are observed, so aggregating observations across many individuals provides a more complete view of the potential outcomes. The same idea motivates a slightly different approach that has been used in the few studies that have constructed measures of earnings risk that vary across individuals (Carroll and Samwick 1997; Saks and Shore 2005; Drewianka 2010). In essence, the idea is to assume that individuals with similar characteristics X_{it} face similar risk, which can then be measured by combining data on outcomes from many individuals with similar characteristics. This is implemented by first constructing noisy measures of earnings risk for a sample of individuals based on their own outcomes, then regressing those measures on their characteristics X_{it}, and finally interpreting the fitted values as more accurate measures of the risks the individuals actually faced, even though they did not experience all of those possible outcomes personally. This approach has also been characterized as an instrumental variables strategy for overcoming the measurement error in the original statistics that were computed for each individual (Carroll and Samwick 1997).

Despite the theoretical importance of permanent shocks, relatively few individual-level studies attempt to examine shocks of different duration. Those that do typically augment the usual earnings specification (1) with an additional assumption that u_{it} has permanent and transitory components:

$$u_{it} = \pi_{it} + v_{it} \qquad (2)$$

$$\pi_{it} = \pi_{i(t-1)} + \eta_{it}. \qquad (3)$$

As the innovation to a the random walk component π, η is a shock that changes $i's$ earnings permanently, while the white-noise component v_{it}represents a shock that affects $i's$ earnings only temporarily.[1] Both are assumed to be mean-zero and uncorrelated with one

[1] A few papers have relaxed the unit root assumption (3) by estimating a similar specification with an autoregressive (AR) parameter. However, estimates of that parameter typically exceed 0.95 and are not

another ($E[\eta_{it}v_{it'}] = 0$ for all t and t'), with respective variances $\sigma^2_{\eta i}$ and σ^2_{vi}, and permanent shocks are assumed to be uncorrelated over time ($E[\eta_{it}\eta_{it'}] = 0$ for all t and t'), so any autoregressive components are subsumed into v. Several empirical studies of earnings dynamics conclude that the transitory component v can be modeled as an MA(1) or MA(2) process, which implies that its autocorrelation becomes negligible at lags longer than two years (MaCurdy 1982; Abowd and Card 1989; Meghir and Pistaferri 2004). Most work thus proceeds under the assumption

$$E(v_{it}v_{i't'}) = 0 \text{ for } |t-t'| \geq 3. \tag{4}$$

1.2.1 Carroll and Samwick's strategy

Carroll and Samwick (1997) propose and implement a strategy for measuring $\sigma^2_{\eta i}$ and σ^2_{vi} at the individual (household) level. They work with the unpredictable part of the growth in household $i's$ earnings between two periods t and $(t+k)$. Under the model above, this growth can be written as

$$u_{i(t+k)} - u_{it} = \left(\sum_{j=1}^{k} \eta_{i(t+j)}\right) + \left(v_{i(t+k)} - v_{it}\right), \tag{5}$$

so under the assumptions above, for $k \geq 3$,

$$E\left(\left[u_{i(t+k)} - u_{it}\right]^2\right) = k\sigma^2_{\eta i} + 2\sigma^2_{vi}. \tag{6}$$

Carroll and Samwick's approach is thus to compute $\left(u_{i(t+k)} - u_{it}\right)^2$ for each (i,t,k), then for each i, regress

$$\left(u_{i(t+k)} - u_{it}\right)^2 = k\beta_i + 2\alpha_i + \varepsilon_{itk}. \tag{7}$$

significantly different from 1 (Storesletten, Telmer, and Yaron 2004; Karahan and Ozkan 2012), implying the random walk assumption is appropriate.

Estimates of β_i and α_i are then interpreted as measures of $\sigma^2_{\eta i}$ and σ^2_{vi}, respectively. As explained above, these measures are likely to be extremely noisy, especially when individuals' reported earnings histories are short.[2] Thus, when they use these measures to investigate households' precautionary saving behavior, they do not use the estimates of (β_i, α_i) themselves, but rather use fitted values from regressions of (β_i, α_i) on a vector of characteristics X_i:

$$\beta_{itkj} = X_{it}\psi_\beta + \varepsilon_{\beta it} \quad (8)$$

$$\alpha_{itkj} = X_{it}\psi_\alpha + \varepsilon_{\alpha it}. \quad (9)$$

1.2.2 Meghir and Pistaferri's strategy

A second strategy for measuring the variance of permanent earnings shocks has been proposed by Meghir and Pistaferri (2004). Like Carroll and Samwick, their approach examines residual earnings growth that occurs between periods t and $(t+k)$. they multiply it by the residual earnings growth that occurs over the longer window between periods $(t-j)$ and $(t+k+q)$, where $j,q>0$. See Figure 1.1 for a visual depiction.

Intuitively, under the model of earnings above, the *permament* earnings growth between t and $(t+k)$ is uncorrelated with that between $(t-j)$ and t, and likewise with that between $(t+k)$ and $(t+k+q)$. Moreover, under condition (4) the transitory shocks experienced at times t and $(t+k)$ will be uncorrelated with those experienced at times $(t-j)$ and $(t+k+q)$. Thus, the expected value of the product described above will be the variance of

[2] For example, Carroll and Samwick's study must infer two parameters (α_i, β_i) for each household from only nine observations on its earnings growth, one for each possible pair of years in 1981-87 with $k \in \{3,4,5\}$.

permanent shocks between times t and $(t+k)$. More formally, for $min(j,q) \geq 3$

$$E\left(\frac{1}{k}\left[u_{i(t+k)} - u_{it}\right]\left[u_{i(t+k+q)} - u_{i(t-j)}\right]\right) = E\left(\frac{1}{k}\left[\sum_{h=1}^{k}\eta_{i(t+j)} + v_{i(t+k)} - v_{it}\right]\right.$$
$$\left.\times\left[\sum_{h=1-j}^{k+q}\eta_{i(t+j)} + v_{i(t+k+q)} - v_{i(t-j)}\right]\right)$$
$$= \frac{1}{k}\sum_{h=1}^{k}\sigma^2_{\eta i(t+h)}. \tag{10}$$

Meghir and Pistaferri then interpret (10) as a moment condition and implement it using $k = 1$ and assuming that $\sigma^2_{\eta it} = \sigma^2_{\eta t}$ for all i within groups defined by broad levels of education. Combining this condition with other predictions about the autocovariance matrix based on the model of earnings dynamics, they estimate the parameters using a minimum distance estimator. In other words, they essentially compute the mean value of the statistic on the left-hand side of (10) for three groups stratified by their educational attainment, while Sabelhaus and Song (2009) subsequently applied the same strategy to sub-populations defined by age.

1.3 Allowing for Variation Across Individuals and Over Time

Despite their merits, neither of the two method described above generates estimates of permanent and transitory risks that differ across people and over their lifetimes. This was not a concern for either of the studies that developed those methods, but they would be desirable for investigating the effect of risk on behavior, for instance, particularly if there were some concern that those choices were affected by unobserved individual characteristics that might be correlated with their exposure to risk. For example, Carroll and Samwick note that their analysis could understate the relationship between households' estimated earnings risk and their wealth if, for instance, differences in risk aversion caused some individuals both to pursue less-risky careers and to save more. Such problems might be reduced or avoided by using fixed effects estimates, but this is impossible if estimates of earnings risk do not vary over workers' careers.

This section thus develops extensions of Carroll and Samwick's and Meghir and Pistaferri's measures that allow additional variation in permanent and transitory earnings risk. In both cases the new estimator is quite closely related to the original version, except that they now allow the relevant statistics to be computed at much finer levels of disaggregation. We also introduce an additional modification to Carroll and Samwick's approach that is more robust to trends in the variance of temporary earnings shocks.

1.3.1 Disaggregating Carroll and Samwick's estimator

We begin by suggesting a minor modification of Carroll and Samwick's method that allows risk to vary by time, so the objects we seek to estimate should be written as $\sigma^2_{\eta it}$ and σ^2_{vit}, rather than $\sigma^2_{\eta i}$ and σ^2_{vi}. Suppose we have collected residuals from the model in (1) and constructed the various $u'_{itk}s$ required below. Note that (2) and (3) imply (for $\min(j,k) \geq 3$),

$$\sigma^2_{vit} = -E\left(\left[u_{i(t+k)} - u_{it}\right]\left[u_{it} - u_{i(t-j)}\right]\right), \tag{11}$$

while equations (6) and (11) similarly imply

$$\sigma^2_{\eta it} = \frac{1}{k}E\left(\left[u_{i(t+k)} - u_{it}\right]^2 + 2\left[u_{i(t+k)} - u_{it}\right]\left[u_{it} - u_{i(t-j)}\right]\right). \tag{12}$$

Therefore, define the following statistics for each i, t, k, and j:

$$\beta_{itkj} \equiv \frac{1}{k}\left(\left[u_{i(t+k)} - u_{it}\right]^2 + 2\left[u_{i(t+k)} - u_{it}\right]\left[u_{it} - u_{i(t-j)}\right]\right) \tag{13}$$

$$\alpha_{itkj} \equiv -\left(u_{i(t+k)} - u_{it}\right)\left(u_{it} - u_{i(t-j)}\right). \tag{14}$$

For each j, β_{itkj} is an estimator of $(1/k)\sum_{h=1}^{k}\sigma^2_{\eta i(t+h)}$ α_{itkj} is an estimator of σ^2_{vit} . Of course, a given individual will not experience his full range of possible outcomes, so these statistics will likely be extremely noisy. However, if risks are a function of observed characteristics X, we can obtain a much better estimate by combining the statistics across people with similar characteristics and across windows of different lengths j and k. Following Carroll and Samwick (1997) and Drewianka (2010), if we assume

$$E(\sigma^2_{\eta it}|X_{it}) = X_{it}\psi_\eta \tag{15}$$

$$E(\sigma^2_{vit}|X_{it}) = X_{it}\psi_v, \tag{16}$$

then we can estimate those variances using fitted values from regressions (8) and (9) above.

This method is also related to the method-of-moments strategy used used by Meghir and Pistaferri and in much related work from the literature on earnings dynamics. For example, one could estimate $\sigma^2_\eta(X)$ as the empirically observed moment $E(\beta|X)$; in other words, the average value of the $\beta's$ for observations with the same $X's$. Our approach is analogous, but it simplifies exploration of the cross-sectional variation because the observations remain disaggregated until the final step.

1.3.2 A more robust variation

Although we have tried to maintain the closest link possible to Carroll and Samwick's approach while developing this estimation strategy, it is fairly easy to see that β may be biased if the variance of transitory shocks is not constant over time. To see this, note that equation (13) can be rewritten as

$$\beta_{itkj} = \frac{1}{k}\left(\left[u^2_{i(t+k)} - u^2_{it}\right] - 2u_{i(t-j)}\left[u_{i(t+k)} - u_{it}\right]\right) \tag{17}$$

$$= \frac{1}{k}\left(u_{i(t+k)} - u_{it}\right)\left(u_{i(t+k)} + u_{it} - 2u_{i(t-j)}\right). \tag{18}$$

Then the for $min(k, j) \geq 3$ the model implies

$$
\begin{aligned}
E(\beta) &= \frac{1}{k}E\left(\left[\sum_{h=1}^{k}\eta_{i(t+h)} + v_{i(t+k)} - v_{it}\right]\right.\\
&\times \left.\left[\sum_{h=1}^{k}\eta_{i(t+h)} + 2\sum_{h=1-j}^{0}\eta_{i(t+h)} + v_{i(t+k)} + v_{it} - 2v_{i(t-j)}\right]\right) \quad (19)\\
&= \frac{1}{k}\left(\sum_{h=1}^{k}\sigma^2_{\eta i(t+h)} + \sigma^2_{vi(t+k)} - \sigma^2_{vit}\right). \quad (20)
\end{aligned}
$$

Thus, β is biased if $\sigma^2_{vi(t+k)} \neq \sigma^2_{vit}$, which is troubling in light of evidence that the transitory earnings risk has increased over time (Haider 2001; Shin and Solon 2011)—suggesting $E(\beta)$ is apt to overstate $\sigma^2_{\eta it}$. This was not necessarily a problem for Carroll and Samwick's study, as they tried (in effect) to estimate the average σ^2_{vi} over time, and σ^2_{vi} may plausibly be assumed constant over the relatively short period they examine. However, we shall show that this bias is more worrisome in our study.

Fortunately this shortcoming can be overcome with a simple modification. Our α_{itkj} statistics are unbiased estimators of σ^2_{vit} for each period, so we can use them to remove the transitory variances in equation (20) directly. Specifically, we can compute an unbiased estimate of $\sigma^2_{\eta it}$ as

$$\beta^*_{itkj} \equiv \beta_{itkj} - (\alpha_{i(t+k)kj} - \alpha_{itkj}). \quad (21)$$

1.3.3 Disaggregating Meghir and Pistaferri's estimator

Similarly, we can disaggregate Meghir and Pistaferri's approach by defining a new statistic

$$\gamma_{itkjq} \equiv \frac{1}{k}\left(u_{i(t+k)} - u_{it}\right)\left(u_{i(t+k+q)} - u_{i(t-j)}\right). \quad (22)$$

The model then implies for all (j,q) with $min(j,q) \geq 3$,

$$E(\gamma_{itkjq}) = \frac{1}{k}\sum_{r=1}^{k}\sigma^2_{\eta i(t+r)}.$$

For the sake of comparison, we can also define a new measure of transitory risk analogous to α_{itkj} by

$$\phi_{itkjq} \equiv \frac{\left(u_{i(t+k)} - u_{it}\right)^2 - k\cdot\gamma_{itkjq}}{2}. \tag{23}$$

1.3.4 Comparing the two approaches

One shortcoming of Carroll and Samwick's approach is that it can only estimate the average level of earnings risk over a relatively long period in which it may not actually be constant, particularly if the agent changes (e.g. jobs or locations). Algebraically speaking, the problem arises because the α and β (see equations 13 and 14) involve several terms like $u_{it}u_{i(t+k)}$, for example, so it becomes critical that $E(u_{it}u_{i(t+k)}) = 0$. As we have noted, prior empirical evidence shows that this condition holds only for $k \geq 3$, so we are limited to considering differences in earnings over a period of at least 3 years in order to avoid confounding permanent earnings shocks with others that are moderately persistent.

In contrast, our generalization of the Meghir-Pistaferri approach places no minimum requirement on k. While it does require $min(j,q) \geq 3$, γ is an unbiased estimator even for k as small as 1. This can be seen intuitively using the "overlapping windows" analogy depicted in Figure 1.1: unlike β or β^*, the γ statistic does not involve any products between the residuals from the two ends of the interior window (though it does involve several products between those values and the those from the bounds of the outer window), so there is no cause to worry whether the residuals from those two "interior" periods were correlated. In other words, γ allows us to identify the relationship between changes in workers' time-varying characteristics (e.g., industry, occupation) and their risk of permanent earnings shocks over the shortest period possible. In practice, we shall implement the method using $k = 2$. this choice may be preferable in principle considering that some shocks may occur in the middle of the year, and it is a more practical choice for our data because it eventually becomes biennial.

Another advantage of the disaggregated Meghir-Pistaferri approach is is the ability to compute more estimates of $\sigma^2_{\eta it}$ and σ^2_{vit} using the same data series. For each (i,t,k) we can compute one β_{itkj} for each j, but one γ_{itkjq} for each *combination* (j,q), providing additional evidence that helps to cull the signal from the noise. For example, in the empirical exercise below, we will ultimately construct about 360,000 γ statistics versus "only" 145,000 β statistics. However, this is not just an advantage relative to our disaggregation of Carroll and Samwick's approach, as Meghir and Pistaferri's original approach used only one of the many possible combinations of (j,q).[3]

It should also be stressed that the choice of k is now irrelevant—-γ is an unbiased estimator even for k as small as 1. A consequent advantage is we can now identify the relationship between changes in workers' time-varying characteristics (e.g., industry, occupation) and their risk of permanent earnings shocks over the narrowest possible window. In practice, we shall implement the method using $k = 2$; this choice may be preferable in principle considering that some shocks may occur in the middle of the year, and it is a more practical choice for our data because it eventually becomes biennial.

On the other hand, one advantage of the disaggregated Carroll-Samwick approach is the ability to compute estimates using shorter panels. If we define ζ as the minimal lag or lead used, the α and β statistics require the respondent to appear in the data for $(2\zeta+1)$ years, whereas the ϕ and γ statistics require $(2\zeta+1+k)$ years. In practice, we will use $\zeta = 4$ and $k = 2$ for the $\gamma' s$, so we need earnings observations over a period of at least 9 years to compute the β statistic, but over a period of at least 11 years to compute the γ statistic.

[3]Specifically, Meghir and Pistaferri implement their moment condition for $(k,j,q) = (1,2,2)$; in other words, the sample statistic they use to for the parameters is $E\left[\left(u_{i(t+1)} - u_{it}\right)\left(u_{i(t+3)} - u_{i(t-2)}\right)\right]$.

1.4 Data

The data examined below are from the 1970-2007 waves of the Panel Study of Income Dynamics (PSID), where possible using the version recoded for use by the Cross-National Equivalent File (CNEF). Although the CNEF was originally intended to harmonize the PSID for comparison with longitudinal studies from other countries, the main advantages of using it here are that the variables in the CNEF are coded consistently over time and that this version of the data have been used at least once to analyze earnings risk at the individual (Drewianka 2010). See Burkhauser et al. (2000) for a more extended discussion of the CNEF. The range of years used allows us to compute annual risk estimates from 1972-2003 using the "β" approach and from 1974-2001 using the "γ" approach.

From this survey, we extract a sample of men aged 16-65 who are not disabled or students, and who report positive earnings. We exclude women mainly to avoid confounding our estimates of earnings risk with the wage affects associated with women's lower (but rapidly increasing) rates of labor force participation, which could easily be mistaken for permanent shocks. One might also be concerned that the positive earnings requirement may unnecessarily omit men who experience a full year of unemployment, a large negative earnings shock that has some strong covariates (especially employment in the construction sector, Drewianka and Mercan 2009). We nevertheless exclude those observations because in practice the estimates are quite sensitive to the way we treat them.

Unlike some other studies, we include observations from both the main sample and from the Survey of Economic Opportunity (SEO), a special over-sample consisting of descendents of low-income families from 1966 (see Hill 1992 for more details). The main impetus for including SEO observations is that it more than quadruples the sample size among non-whites, thereby allowing stronger inferences on racial differences. This is a key question because evidence from the PSID Validation Study indicates that race is one of the few strong covariates of measurement error in earnings (Duncan et al. 1985), suggesting a plausible alternate explanation for previous findings that blacks experience greater earnings instability than whites. Meghir and Pistaferri (2004) argue that there is little harm in including the SEO sample in studies of earnings shocks because any differences in the distributions of earnings between the SEO and main samples would be washed away when the data are differenced as in equation (5), and Drewianka (2010) has shown that the distribution of reported earnings (as well as hours, hourly wages, age, and education) for the SEO sample is very similar to the distribution among men of the same race in the main sample; suggesting the SEO may be more representative than one might have expected.[4] Nevertheless, all specifications include dummy variables for SEO status and the estimated coefficients are rarely large or statistically significant.

Since our goal is to identify covariates of earnings risk, it is necessary to link variables across survey waves. A given wave includes information about the respondents' current characteristics (e.g., their occupation or industry) and their earnings in the previous year, but our analysis uses those characteristics to predict earnings shocks that will happen over the following year, which are reported in the survey wave gathered two years hence.

While most of the literature examines log real annual earnings, we instead analyze log real *weekly* wages (measured in 2007 dollars). We do this because we are most interested in measuring permanent risk accurately, as it should have much greater behavioral relevance. To see the point, note that

$$\begin{aligned} Var[\log(\text{earnings})] &= Var\left[\log\left(\frac{\text{earnings}}{\text{weeks worked}}\right)\right] \\ &+ Var[\log(\text{weeks})] + 2Cov[\log(\text{earnings}), \log(\text{weeks})]. \end{aligned} \tag{24}$$

We conjecture that variation in individuals' log(weeks) is typically either (*i*) transitory or (*ii*) planned by the individual, and if so most changes in log(weeks) would not represent

[4] The similarity between those race-specific distributions could possible reflect churning in the income distribution or intergenerational mean reversion, but it also may arise simply because some men entered the SEO sample through marriage, despite not having been eligible for it on their own.

permanent risk. Moreover, previous work has found that its covariance with log(weekly earnings) is quite modest (Haider 2001). We thus suspect that analyzing log(earnings/week) will reduce the noise in our measures of permanent risk.

Other variables used included in the second stage regressions (8) and (9) are individuals' degree of self-employment, educational status, and level of risk tolerance. We include the first since one would expect workers who are (fully) self-employed to have more volatile incomes. Although we ultimately removed students, results were robust to their inclusion. Finally, since this is a study about risk, we included a measure of a person's risk tolerance that was asked during the 1996 wave of the PSID (plus a dummy variable for respondents who did not appear in that wave). The variable was originally constructed by posing hypothetical gambles to respondents regarding whether or not they would take a job that could double their income, but had a variable probability of reducing their by some percentage. See Kimball, Sahm, and Shapiro (2008, 2009) for further discussions.

Table 1.1 reports summary statistics stratified by covariates. We have a total of 115,843 observations of annual earnings from 11,919 unique individuals, but since second- and fourth-differences cannot be computed for some observations; we are ultimately able to use about 4,300 individuals to analyzeγ and 5,000 for β. Most are white, though blacks are overrepresented due to the inclusion of the SEO data. They work in a variety of industries and occupations, and over ten percent of the observations represent workers who are at least partially self-employed. Around ten percent were not working as of the interview.

1.5 Practical Implementation

1.5.1 First stage specification

Following much of the literature on earnings dynamics, we implement the population age-earnings profile F as a fourth-order polynomial. We estimate it separately for each of 24 demographic groups–each combination of the man's race (white or non-white), birth cohort (pre-baby boom: pre-1946, the first half of the baby boom: 1946-1955, the second half of the baby boom: 1956-1964, and post-baby boom: post-1964), and highest level of education completed (less than high school, high school graduate, more than high school). In each case we add individual fixed effects (discussed in the next paragraph), a dummy variable for men from the SEO sample (though this parameter is almost always statistically insignificant), a linear time trend, and random effects for each year to account for macroeconomic shocks that may vary across groups. (Since those random effects are also a form of risk, we shall ultimately add them to the fitted values from our earnings risk regressions to form our final estimate of an individuals' earnings risk.) One might be concerned about not including other covariates in the first stage specification (i.e., $E\left[u_{i(t+k)} - u_{it}|X_{it}\right] \neq 0$), but most are constant over time and would thus be redundant given the inclusion of individual fixed effects.

Those individual fixed effects are our way of implementing the person-specific age-earnings profile, Q. A specification with higher-order person-specific terms would fit the individuals' expected earnings more precisely, but that consideration must be weighed against the substantial risk of over-fitting the model, especially for respondents with relatively short reported earnings histories. Fortunately, conclusions about the covariates of earnings risk are not very sensitive to the choice between specifications of Q. We experimented with several more elaborate specifications, including person-specific growth rates (using either fixed or normally-distributed random effects) and some higher-order terms, but our estimates of the difference $(u_{i(t+k)} - u_{it})$ were quite highly correlated $(\rho \geq 0.98)$ across the five separate specifications of Q we tried. Among other things, this is consistent with Hryshko's (2012) evidence against heterogeneity in individual's deterministic earnings growth rates. At any rate, since our estimators are constructed from such differences, little is lost by using the simplest specification, and doing so leaves more degrees of freedom for the estimation of the group age-earnings profile F.

One might be concerned that the person-specific age-earnings profile $(F+Q)$ will provide a closer fit to the actual earnings history of individuals who appear in the sample for a smaller number of years. However, note that apart from the fixed effect, the estimated age-earnings profile will be identical for everyone within each of the 24 groups, and the individual fixed effect just ensures that the mean residual u is zero for each person. Moreover, since we can only compute β (respectively γ) when we observe a person's earnings over a period of at least 9 (11) years, our analysis will not include data from any individuals with especially short reported earnings histories.

1.5.2 Computation of permanent and temporary earnings risk

Once those residual differences $(u_{i(t+k)} - u_{it})$ are estimated, we can compute the statistics we use to measure the risks: α, β, β^*, γ, and ϕ. Because the PSID became biennial after 1997, we opted to work with even-numbered lags; specifically, we compute our statistics using all even-numbered lags j and (where applicable) leads q between 4 and 16. Thus, for a single person-year pair (u_{it} and $u_{i(t+k)}$) we can potentially compute 7 values of β_{itkj} and α_{itkj} by varying j, while the ability to vary both j and q allows construction of up to 49 values of γ_{itkjq} and ϕ_{itkjq}. Since our goal is to measure risk over short widows of time, we use $k = 2$ for γ and ϕ, and $k = 4$ for β and α; recall that we need $k \geq 3$ for β and α, but not for γ and ϕ.

1.5.3 Second stage procedure

Our final step involves regressing the α, β, β^*, γ, and ϕ statistics computed in the previous step on covariates X, as in equations (8) and (9) above. This step is of primary interest both because it reveals the covariates most associated with the components of earnings risk and because the fitted values (e.g., $E[\gamma|X]$) are our best measure of the risks facing individuals at a point in time.

One important complication is that for each method we have created multiple statistics that represent each person-year's earnings risk, but those measures are inherently heteroskedastic. This is especially problematic for the γ statistics: those that use longer leads and lags (j and q) are noisier because they include more interactions between permanent shocks inside the focal time period (between times t and $t+k$) and those outside of it. We thus weight the observations by their estimated precision when we run regressions like (8) and (9). The details of that procedure are described more fully in the Appendix.

1.6 Empirical Results

1.6.1 Predictions of earnings risk

Table 1.2 lists selected centiles of the distributions of the computed statistics. In the first panel, the first three columns reflect the raw β, β^*, and γ while the next three columns report centiles of the fitted values from regressions like (8), and the remaining columns report fitted values from otherwise identical regressions that also include individual fixed effects; the second panel reports analogous distributions for α and ϕ. For the measures of permanent risk, roughly 40 percent of the raw statistics are less than zero, as are around 20 percent of the OLS fitted values for β. this is disconcerting because they are supposed to estimated variances. There are many fewer negative fitted values for our preferred measures γ (8 percent) and β^* (4 percent), however. There are slightly more negative FE fitted values in most instances.

Figures 1.2 and 1.3 present the same information graphically. The red lines are plots of the indicated fitted value, with a 95 percent confidence band plotted as dotted blue lines on either side for centiles 2 through 98. Not only are there fewer negative fitted values for γ and β^*than for β, but their confidence intervals are also tighter. For example, only one percent of the $\widehat{\gamma_{OLS}}$ fitted values are negative and statistically significant at the 5 percent level, suggesting the reassuring hypothesis that the negative estimates may result simply from sampling error. Similar conclusions follow for the other statistics as well. The distributions of the fitted values for our two measures of temporary risk (α and ϕ) are fairly similar to one another, though the confidence intervals are somewhat wider for ϕ.

Table 1.3 presents correlations between the various calculated and fitted values to help us assess whether the measures capture the same thing. In Panel A for permanent risk, we find a very low correlation both between γ and β and also γ and β and their associated fitted values. The correlation between $\beta^*$and β and γ is better. Specifically, the correlation between $\beta^*$and γ, almost 0.4, compared to β and β^*, about 0.14 only. However, this should not be surprising given that β is likely contaminated by the presence of transitory risk; and one would hope that the other two measures which attempt to correct for this would capture some of the same thing. Similar qualitative patterns emerge for correlations between the statistics and their predicted values. Fitted values for $\beta^*$and γ are negatively correlated with β. The situation improves in Panel B when looking at transitory risk. The correlations, both between calculated ϕ and α; and ϕ and α and their corresponding fitted values are much stronger. The fitted values of ϕ correlate well with $\alpha's$ fitted values. This is puzzling: γ and β are not strongly correlated, but ϕ and α are, despite having been computed from γ and β, respectively.

Because γ and ϕ are our preferred measures of risk, and because γ and β^* are similar, the following section focuses mainly on γ and ϕ, unless otherwise specified, though we do report estimates for β, β^*, and α for comparability with previous work. Additionally, we consider only the fitted values from the long OLS regressions since the means are closer, much less dispersed, and fewer are negative than their FE counterparts.

1.6.2 Unconditional earnings risk

Before turning to specific results, some preliminary observations are worth noting. Table 1.4 presents average values of annual total earnings risk u_{it}^2 and the five estimates for permanent and transitory earnings risk stratified by various demographic, industrial, and occupational characteristics. Where comparable, Carroll and Samwick's (1997, Table 1) estimates of permanent and transitory risk are presented in the columns labeled "CS." Again, the analysis computes each γ and ϕ statistic using two-year differences (i.e., $k = 2$), for all even-numbered values of j and q between 4 and 16, so we have many individual estimates for each person-year.

It is encouraging that there are very few negative values reported for any of the statistics, and many of those are close to zero (e.g., γ for electrical engineers). In most instances, mean values for γ and (especially) β^* are smaller than the corresponding values for β. Carroll and Samwick's measure of permanent risk is usually higher than our γ, while their measure of temporary risk is always lower; the same is true for Meghir and Pistaferri's (2004) estimates (not shown).

Now we turn to identifying factors that are correlated with earnings risk. Whites, those in the main sample (not the SEO), and self-employed people experience higher permanent risk that most of their competing categories. Permanent risk appears to follow a U-shaped pattern over the life-cycle. Like Meghir and Pistaferri (2004), we find that high school graduates are less exposed to permanent earnings shocks than either dropouts or (especially) college graduates. In addition, those related to construction and farming do not seem to have above average exposure to permanent risk. Finally, it is reassuring that persons with higher reported risk tolerance appear to experience greater risk of permanent earnings shocks.

In contrast, transitory risk appears to be experienced more uniformly across the SEO designation and level of education. However, fully self-employed individuals and blacks face a substantially larger exposure to risk. Workers in the agriculture- and construction-related sectors have some of the highest reported values, as do those in legal services; this suggests that the majority of workers in these sectors are exposed to risks which are temporary in nature, and this appears to be the case for most categories Individuals not working at time of survey have some of the highest values of transitory earnings risk.

No clear pattern emerges over the life-cycle, though perhaps the second half of workers' careers is defined by greater risk, as are more recent cohorts. The same could be said regarding risk tolerance, except that the very highest levels are associated with large increases in risk. Men who are single or widowed have the highest raw exposure to permanent risk and the divorced, separated, and widowed the highest exposure to raw temporary risk.

One potential explanation for this lack of significance is collinearity between covariates. We have thus run “shorter” regressions (results not reported) that only included one variable or category of variables (i.e., just occupation or just partially and fully self-employed) in addition to the year effects. While there is an increase in significance for some variables, these stripped down regressions largely confirm the results of their “longer” counterparts.

For permanent risk, African and Asian Americans face lower risk compared to whites, and now both high school dropouts and those in the SEO sample face decreased exposure. The degree of risk tolerance was not included in the long regressions since it was only part of the 1996 PSID wave and therefore not available for all individuals. Given the significance of the other variables, it is likely that its inclusion would have changed little. However, in the shorter regressions, we found that those with the highest levels of risk tolerance face higher permanent risk than those with the lowest level, with coefficients increasing in risk tolerance. There emerged decreased risk for those aged 36-45 relative to 26-30 year olds and increased exposure for singles relative to married persons.

When examining the short regressions for transitory risk, almost all racial groups face less risk than whites, except for African Americans. With regard to the life-cycle, the short regressions indicate that compared to the youngest workers, older workers are subject to lower temporary risk; while none of these coefficients are significant, the significance increases as workers age, coming close to marginal (i.e., 10 percent level) significance for the oldest workers. Lastly, the degree of risk tolerance is not significant and there is no clear qualitative pattern, though the divorced face less temporary risk than married persons.

1.6.3 Covariates of earnings risk

The mean values of permanent and temporary earnings risk given in Table 1.4 provide preliminary evidence that the estimates will vary meaningfully across groups. However, they do not necessarily provide a good sense of which factors have the strongest influence in constructing our estimates of earnings risk because many of the proposed covariates are correlated with one another. In other words, we wish to determine which differences in the raw means are statistically significant. To investigate this point further, Table 1.5 presents estimates from regressions of the statistics on a number of variables one may use to predict an individual's permanent and transitory earnings risks. We focus on the regressions which generated the OLS fitted values for γ and ϕ.[5]

When one examines Table 1.5, several interesting and perhaps surprising points emerge. First, very few covariates are significant predictors of either type of risk, though temporary risk has a few more strong predictors. Examining permanent risk we find that high school graduates face less risk than those with more than a high school degree. A few occupations have relatively less permanent risk: those in transportation, engineering, architecture, mathematics, and medicine. A surprising result may be that workers who are partially self-employed face statistically significantly less permanent risk compared to those working only for someone else, yet in 1.6.2, they had the highest level of permanent risk by self-employment status.

Considering transitory risk more closely, we again do not see much significance within the long regressions either. Both groups which have some degree of self-employment have higher temporary risk relative to not being self-employed to any degree. It is worth noting that those thought to face larger or more temporary earnings shocks—those in the construction and agricultural industries or not working at the time of the survey—do not face a significantly larger increase, though in some cases, the sign is positive. Hispanics face less transitory risk compared to whites.

It may therefore be tempting to conclude that none of the variables selected predict any of the statistics very well. However, the magnitude of the point estimates (and the signs in some cases) suggests a more substantial relationship, in addition to potential collinearity among the demographic and employment variables. Arguably more relevant are the fitted

[5]The results from the FE regressions indicate that the estimates from the two different specifications are not particularly sensitive to within-group heterogeneity.

values, which we discussed earlier.

Given this paper's motivation, another noteworthy finding is that the predicted values vary not only across workers, but within workers' careers. A simple variance decomposition of the fitted values reveals that within-individual fluctuations account for about 50-65 percent of variation in the statistics.

1.7 Concluding Remarks

We have modified, implemented, and compared two methods for measuring individuals' earnings risk. Results from both of our methods attribute less variation than some previous work to permanent shocks, although this still corresponds to a standard deviation of permanent risk on the order of 10 percent per year. Although most earnings risk is evidently transitory in nature (or possibly an indication of measurement error), permanent risks do vary notably across educational and racial groups.

Unlike their predecessors in the literature, both of the methods we propose not only distinguish between risks of permanent and temporary earnings shocks, but also produce estimates that vary across workers and within their individual earnings histories, a feature that should make them particularly useful for studies that aim to measure the effect of risk on behavior. For example, one might use such measures to assess the extent to which earnings risk affects individuals' ex ante choices about investments in human capital, marriage and fertility, or homeownership, or perhaps their risk of costly outcomes like divorce or bankruptcy.

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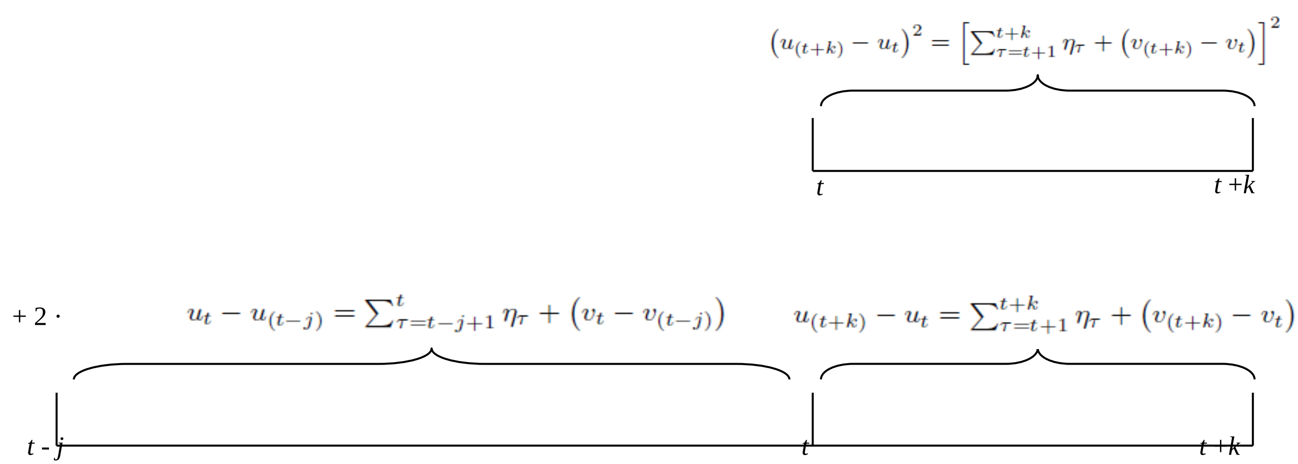
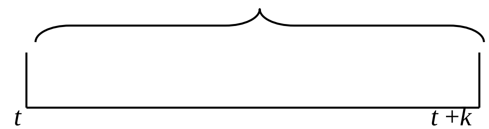


Figure 1.1A. Diagrammatic Representation of Carroll & Samwick (1997)

$$u_{(t+k)} - u_t = \sum_{\tau=t+1}^{t+k} \eta_\tau + \left(v_{(t+k)} - v_t\right)$$



$$u_{(t+k+q)} - u_{(t-j)} = \sum_{\tau=t-j+1}^{t} \eta_\tau + \sum_{\tau=t+1}^{t+k} \eta_\tau + \sum_{\tau=t+k+1}^{t+k+q} \eta_\tau + \left(v_{(t+k+q)} - v_{(t-j)}\right)$$

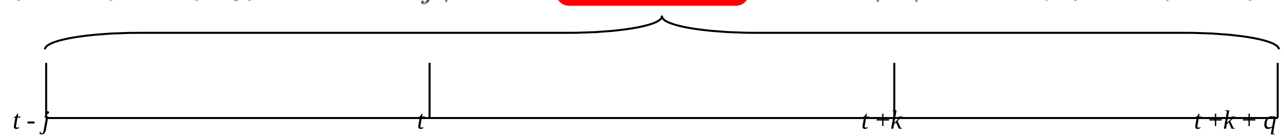


Figure 1.1B. Diagrammatic Representation of Meghir & Pistaferri (2004)

Table 1.1: Summary Statistics

Dependent Variable	Mean	SD	Variable	Number	Share	Variable	Number	Share
Log of Real Weekly Wage	6.76	0.73	Birth Cohort			Occupation		
			pre-1930	13,658	11.8	Uncategorized/Missing	29,437	25.2
Variable	**Number**	**Share**	1931-1940	13,061	11.3	Not working at interview	11,417	9.8
Race			1941-1945	10,691	9.2	Priv. Bus. Leadr.	9,425	8.1
White	78,057	67.4	1946-1950	18,302	15.8	Transport. Oper.	5,701	4.9
Black	32,666	28.2	1951-1955	21,164	18.3	Machine Fitter	5,279	4.5
Other	1,852	1.6	1956-1960	16,590	14.3	Labor./Craftsmn.	4,114	3.5
Native American	1,817	1.6	1961-1964	8,660	7.5	Bricklay./Carpt.	3,491	3.0
Hispanic	756	0.7	1965-1969	6,406	5.5	Convey. Oper.	2,979	2.6
Asian	640	0.6	post-1970	7,311	6.3	Vendor	2,778	2.4
Unknown	55	0.1	Industry (2-digit)			Architect/Engineer	2,249	1.9
Age			Uncategorized/Missing	29,239	25.2	Inspector	2,204	1.9
16-20	1,366	1.2	Not working at interview	10,357	8.9	Electr. Enginr.	2,195	1.9
21-25	12,796	11.1	Retail	8,051	7.0	Security Servic.	2,153	1.8
26-30	20,607	17.8	Other Services	6,422	5.5	Educator	1,903	1.6
31-35	19,833	17.1	Constr. Relate.	6,068	5.2	Pipe Fitter	1,852	1.6
36-40	17,003	14.7	Public Administration	5,730	5.0	Ofc. Worker etc.	1,774	1.5
41-45	14,066	12.1	Mechanical Eng.	4,879	4.2	Mathematician	1,697	1.5
46-50	11,295	9.8	Other Trans.	4,054	3.5	Janitor	1,692	1.5
51-55	8,636	7.5	Wholesale	3,772	3.3	Soldier	1,647	1.4
56-60	6,286	5.4	Wood/Paper/Print	3,681	3.2	Farm Manager	1,520	1.3
61-65	3,955	3.4	Educ./Sport	3,671	3.2	Mailman	1,367	1.2
Education			Health Service	2,792	2.4	Cook/Waiter	1,183	1.0
Less than Hthan HS	20,750	17.9	Agriculture, forestry	2,453	2.1	Related Medical Job	1,179	1.0
HS grad	42,357	36.6	Electrical Eng.	2,267	2.0	Business Managr.	1,150	1.0
More than HS	52,736	45.5	Postal System	2,213	1.9	Insurance Rep.	1,091	0.9
PSID Sample			Energy/Water	2,198	1.9	Eng. Tech. Expert	1,039	0.9
Main	73,317	63.3	Construction	2,045	1.8	Scientist	956	0.8
SEO	42,526	36.7	Iron/Steel	2,005	1.7	Accountant	886	0.8
Self-Employment			Food Industry	1,546	1.3	Painter	771	0.7
Not at all	101,139	87.3	Clothing/Text.	1,436	1.2	Remaining (50)	10,714	10.2
Partially	1,739	1.5	Legal Services	1,419	1.2	Total Observations	115,843	
Fully	12,965	11.2	Remaining (13)	9,545	8.2	Unique Individuals	11,919	

Note: Observations are for men, aged 16 to 65, from the 1970-2007 waves of the Panel Study of Income Dynamics (PSID) who report positive earnings and at least 52 hours of work in the previous year and are not disabled or students. Data are annual up to and including 1997 and then biennial afterwards.

(continued on next page)

Table 1.1 (cont): Summary Statistics

Variable	Number	Share	Variable	Number	Share	Variable	Number	Share
Risk Tolerance			Marital Status			Industry (1-digit)		
Very Low	20,246	17.5	Married	93,292	80.5	Services	30,560	26.4
Low	12,331	10.6	Single	12,614	10.9	Manufacturing	27,509	23.8
Low-moderate	10,748	9.3	Divorced	6,238	5.4	Trade	15,830	13.7
Moderate	9,527	8.2	Separated	2,867	2.5	Construction	11,234	9.7
High	8,186	7.1	Widowed	813	0.7	Transport	7,908	6.8
Very High	4,384	3.8	Not available	18	0.0	Not working at interview	7,403	6.4
Not available	50,420	43.5				Bank/Insurance	4,632	4.0
						Agriculture	3,951	3.4
						Uncategorized/Missing	3,001	2.6
						Energy	2,722	2.4
						Mining	1,092	0.9

Note: Observations are for men, aged 16 to 65, from the 1970-2007 waves of the Panel Study of Income Dynamics (PSID) who report positive earnings and at least 52 hours of work in the previous year and are not disabled or students. Data are annual up to and including 1997 and then biennial afterwards.

Table 1.2A: Distributions of Computed and Fitted Values for Permanent Earnings Risk

Centile	β	β^*	γ	β_{OLS}	β^*_{OLS}	γ_{OLS}	β_{FE}	β^*_{FE}	γ_{FE}
0	-31.61	-4.84	-10.62	-0.13	-0.05	-0.57	-0.48	-0.08	-0.55
4	-0.17	-0.10	-0.16	-0.04	0.00	0.00	-0.06	-0.01	-0.02
8	-0.08	-0.05	-0.08	-0.02	0.00	0.00	-0.04	0.00	-0.01
10	-0.06	-0.04	-0.06	-0.02	0.00	0.00	-0.03	0.00	-0.01
12	-0.05	-0.03	-0.05	-0.01	0.00	0.00	-0.02	0.00	0.00
16	-0.03	-0.02	-0.03	-0.01	0.00	0.00	0.00	0.00	0.00
20	-0.02	-0.01	-0.02	0.00	0.01	0.00	0.00	0.01	0.00
24	-0.01	-0.01	-0.01	0.00	0.01	0.01	0.01	0.01	0.00
28	-0.01	-0.01	-0.01	0.01	0.01	0.01	0.01	0.01	0.01
30	-0.01	0.00	-0.01	0.01	0.01	0.01	0.02	0.01	0.01
32	0.00	0.00	-0.01	0.01	0.01	0.01	0.02	0.01	0.01
36	0.00	0.00	0.00	0.02	0.01	0.01	0.02	0.01	0.01
40	0.00	0.00	0.00	0.02	0.01	0.01	0.03	0.01	0.01
44	0.00	0.00	0.00	0.02	0.01	0.01	0.03	0.01	0.01
48	0.00	0.00	0.00	0.03	0.01	0.01	0.04	0.01	0.01
50	0.00	0.00	0.00	0.03	0.01	0.01	0.04	0.02	0.01
52	0.00	0.00	0.00	0.03	0.01	0.01	0.04	0.02	0.01
56	0.01	0.00	0.00	0.03	0.01	0.01	0.05	0.02	0.02
60	0.01	0.01	0.01	0.04	0.02	0.01	0.06	0.02	0.02
64	0.01	0.01	0.01	0.04	0.02	0.01	0.06	0.02	0.02
68	0.02	0.01	0.01	0.05	0.02	0.02	0.07	0.02	0.02
70	0.02	0.02	0.02	0.05	0.02	0.02	0.07	0.02	0.02
72	0.02	0.02	0.02	0.05	0.02	0.02	0.08	0.02	0.02
76	0.03	0.02	0.03	0.06	0.02	0.02	0.08	0.02	0.02
80	0.04	0.03	0.04	0.07	0.02	0.02	0.09	0.02	0.02
84	0.06	0.04	0.05	0.07	0.02	0.02	0.10	0.03	0.02
88	0.09	0.06	0.08	0.08	0.03	0.02	0.11	0.03	0.03
90	0.12	0.07	0.09	0.09	0.03	0.03	0.12	0.03	0.03
92	0.15	0.09	0.12	0.09	0.03	0.03	0.13	0.03	0.03
96	0.31	0.16	0.23	0.11	0.03	0.03	0.15	0.04	0.04
100	31.48	4.45	10.72	0.39	0.12	0.30	0.71	0.11	0.71

Table 1.2B: Distributions of Computed and Fitted Values for Temporary Earnings Risk

Centile	α_{OLS}	φ_{OLS}	α_{FE}	φ_{FE}
0	-0.24	-0.35	-0.76	-0.93
4	-0.02	-0.05	-0.04	-0.05
8	0.00	-0.02	0.00	0.00
10	0.00	-0.02	0.01	0.00
12	0.01	-0.01	0.01	0.01
16	0.01	0.00	0.02	0.02
20	0.02	0.00	0.03	0.03
24	0.03	0.01	0.04	0.03
28	0.04	0.02	0.04	0.04
30	0.04	0.02	0.05	0.05
32	0.04	0.02	0.05	0.05
36	0.05	0.03	0.06	0.06
40	0.06	0.04	0.07	0.07
44	0.06	0.05	0.08	0.08
48	0.07	0.06	0.09	0.09
50	0.08	0.06	0.09	0.09
52	0.08	0.06	0.10	0.10
56	0.09	0.07	0.11	0.11
60	0.11	0.08	0.13	0.12
64	0.12	0.10	0.14	0.14
68	0.14	0.11	0.16	0.15
70	0.15	0.12	0.18	0.15
72	0.16	0.12	0.19	0.16
76	0.18	0.14	0.21	0.18
80	0.21	0.16	0.23	0.19
84	0.23	0.18	0.25	0.21
88	0.26	0.21	0.28	0.23
90	0.27	0.23	0.29	0.24
92	0.29	0.24	0.31	0.25
96	0.34	0.29	0.37	0.30
100	0.80	1.51	0.83	1.62

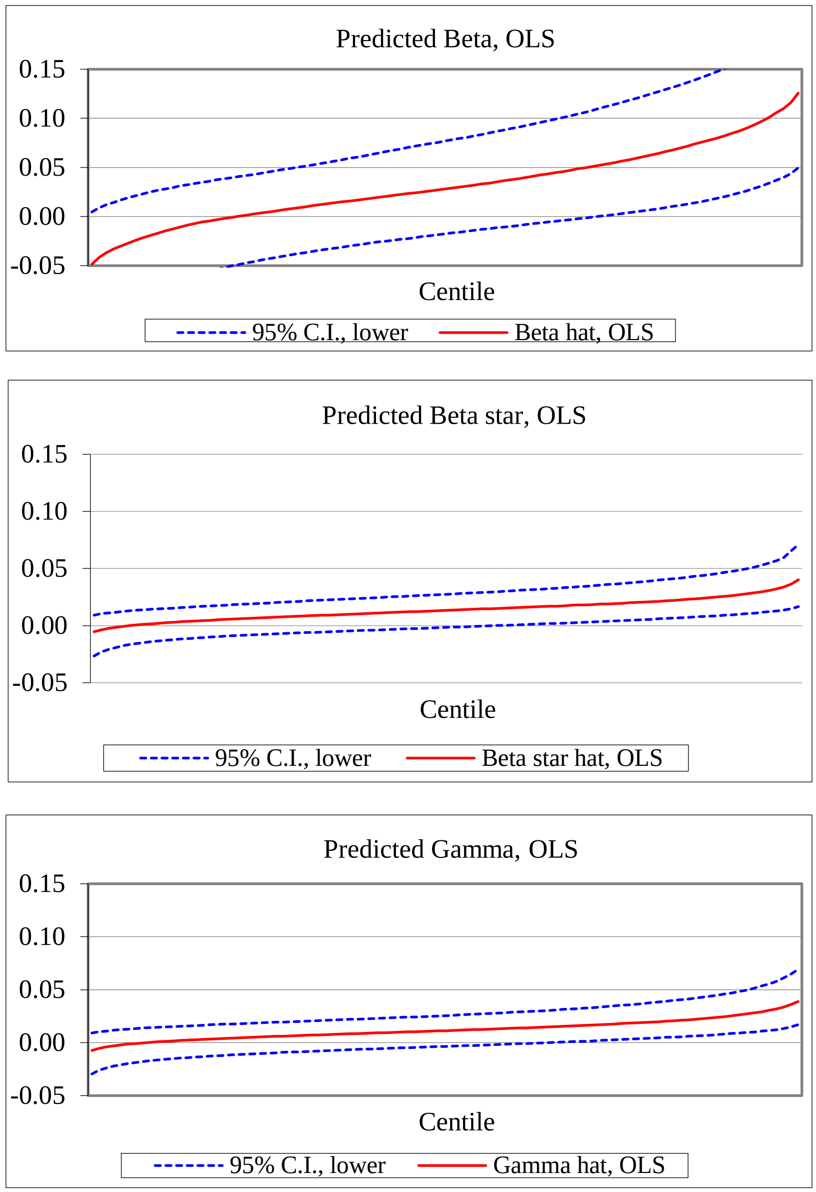


Figure 1.2A. Fitted Values of Component Estimators of Permanent Earnings Risk, OLS
(centiles 2 trhough 98 shown)

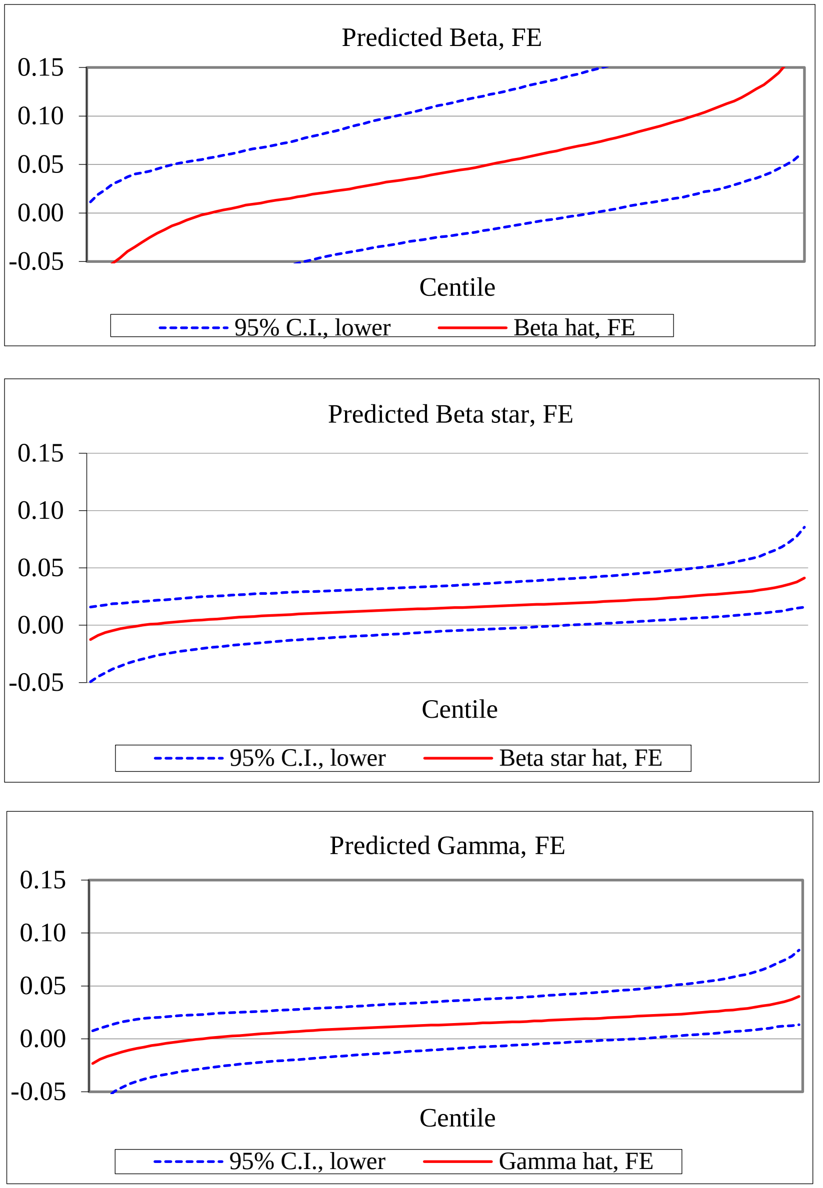
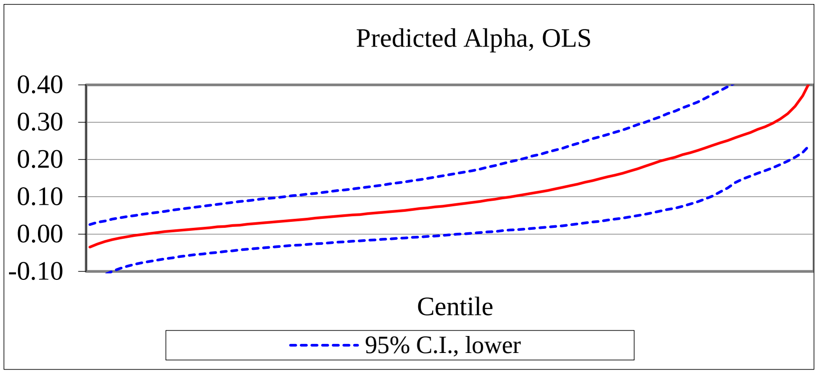


Figure 1.2B. Fitted Values of Component Estimators of Permanent Earnings Risk, FE
(centiles 2 trhough 98 shown)



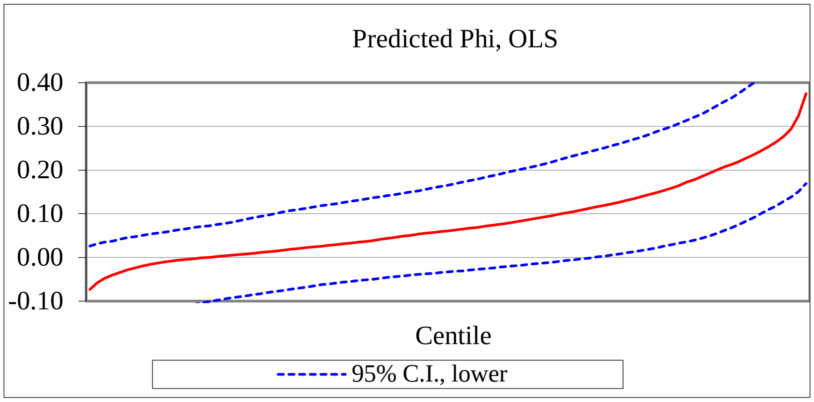


Figure 1.3A. Fitted Values of Component Estimators of Temporary Earnings Risk, OLS (centiles 2 through 98 shown)

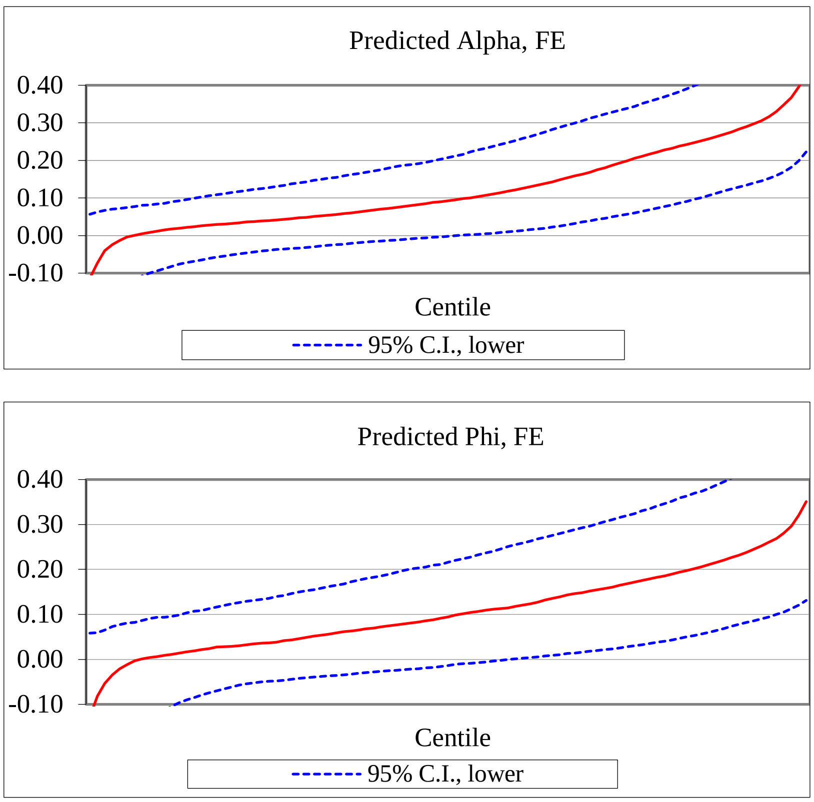


Figure 1.3B. Fitted Values of Component Estimators of Temporary Earnings Risk, FE
(centiles 2 through 98 shown)

Table 1.3: Correlations between different measures of permanent and temporary earnings risk

A. Correlations between Permanent Measures: Statistics and Predictions -- Beta, Beta star, and Gamma

	β	β^*	γ	β_{OLS}	β^*_{OLS}	γ_{OLS}	β_{FE}	β^*_{FE}	γ_{FE}
β	1.00								
β^*	0.14	1.00							
γ	0.06	0.39	1.00						
β_{OLS}	0.07	0.01	0.01	1.00					
β^*_{OLS}	0.02	0.07	0.05	0.10	1.00				
γ_{OLS}	0.02	0.06	0.08	0.08	0.61	1.00			
β_{FE}	0.07	0.00	0.00	0.73	0.08	0.03	1.00		
β^*_{FE}	-0.02	0.03	0.02	-0.16	0.49	0.27	0.03	1.00	
γ_{FE}	-0.01	0.02	0.05	-0.08	0.18	0.65	0.05	0.49	1.00

B. Correlations between Temporary Measures: Statistics and Predictions -- Alpha and Phi

	α	φ	α_{OLS}	φ_{OLS}	α_{FE}	φ_{FE}
α	1.00					
φ	0.62	1.00				
α_{OLS}	0.11	0.11	1.00			
φ_{OLS}	0.09	0.10	0.76	1.00		
α_{FE}	0.09	0.08	0.79	0.63	1.00	
φ_{FE}	0.06	0.07	0.54	0.71	0.74	1.00

Table 1.4: Means of Component Estimators of Permanent and Temporary Earnings Risk by Covariates, x100

Category	u^2	CS_β	β	β^*	γ	CS_α	α	φ
	Total Risk		*Permanent*				*Temporary*	
Whole Sample	18.97	2.17	3.16	1.13	1.21	4.40	11.05	8.59
Race								
White	19.22		3.50	1.21	1.32		11.09	8.52
Black	18.80		2.03	0.90	0.87		11.04	9.70
Native American	16.87		2.21	0.66	0.63		14.02	4.94
Asian	15.99		4.40	0.69	0.83		5.90	2.01
Hispanic	12.13		1.47	1.63	1.82		7.10	5.44
Other	16.80		3.73	0.89	1.14		8.39	4.55
Age								
21-25	18.28		0.98	1.54	1.82		14.57	9.26
26-30	18.38	2.05	2.75	1.54	1.59	4.18	9.51	8.06
31-35	15.61	1.70	3.19	1.22	1.30	4.11	8.08	7.91
36-40	18.82	2.19	2.91	0.99	1.07	4.22	9.72	7.42
41-45	17.98	0.30	2.63	1.10	0.76	7.80	13.22	10.44
46-50	20.88	2.48	2.56	1.11	1.65	4.04	11.90	8.55
51-55	21.50		3.25	0.89	1.21		14.06	10.89
56-60	23.36		7.76	1.37	1.25		12.27	9.23
Education								
Less than HS	17.70		3.27	0.94	1.06		10.27	8.28
HS grad	17.29	2.77	2.04	0.84	0.81	4.31	10.49	8.75
More than HS	20.74		3.84	1.35	1.48		11.59	8.55
PSID Sample								
Main	19.61		3.32	1.20	1.30		11.42	8.59
SEO	17.82		2.75	0.91	0.93		10.10	8.58
Self-Employment								
Not at all	18.97		2.91	1.09	1.18		7.33	5.75
Partially	32.71		7.58	1.63	2.66		13.90	12.40
Fully	51.32		4.34	1.35	1.22		36.08	28.19

CS_β are the β results taken directly from Carroll & Samwick's tables. Continued on next page.

Table 1.4: Means of Component Estimators of Permanent and Temporary Earnings Risk by Covariates, x100

Category	u^2	**CS**$_\beta$	β	β^*	γ	**CS**$_\alpha$	α	φ
	Total Risk		*Permanent*				*Temporary*	
Birth Cohort								
pre-1930	13.77		3.79	1.35	1.33		6.42	5.51
1931-1940	17.43		4.50	0.87	1.07		9.79	6.00
1941-1945	16.84		1.95	0.88	0.99		8.94	7.60
1946-1950	20.48		3.70	0.98	1.19		12.24	8.39
1951-1955	20.04		2.29	1.26	1.28		13.48	11.45
1956-1960	19.14		2.89	1.72	1.57		11.55	8.35
1961-1964	22.28		4.49	1.73	1.48		8.59	16.43
1965-1969	25.71		1.21	2.32	2.54		15.05	12.39
post-1970	17.75		-1.28	0.55	1.08		23.53	29.85
Risk Tolerance								
Very Low	20.77		3.33	0.88	0.98		9.95	9.10
Low	18.21		3.34	0.72	0.96		8.08	6.11
Low-moderate	18.32		1.99	1.41	1.30		10.28	8.79
Moderate	19.46		2.64	1.61	1.29		9.72	6.52
High	23.57		4.04	1.34	1.74		16.43	8.85
Very High	31.28		5.14	2.19	2.21		25.69	19.54
Not available	16.46		3.00	1.01	1.14		10.10	7.88
Industry (1-digit)								
Services	17.66		2.97	1.20	1.36		10.59	7.78
Manufacturing	11.82	2.49	3.41	1.07	1.01	2.75	5.22	4.56
Trade	18.98	2.31	4.55	1.20	1.25	4.89	10.72	7.72
Construction	19.03	3.13	2.93	0.90	0.83	4.94	14.91	11.02
Transport	15.34		3.03	0.65	0.66		7.93	5.61
Not working at interview	50.85		-3.83	1.49	0.04		49.46	36.20
Bank/Insurance	19.66	1.18	-0.73	0.87	1.52	5.10	17.18	10.48
Agriculture	52.24	4.01	6.26	2.08	1.79	7.94	24.37	31.65
Uncategorized/Missing	13.57		-0.98	7.70	12.67		82.97	25.11
Energy	12.14		2.00	1.27	1.21		4.44	4.14
Mining	16.57	3.89	9.45	0.97	1.99	-0.18	10.26	12.24

CS$_\beta$ are the $\boldsymbol{\beta}$ results taken directly from Carroll & Samwick's tables. Continued on next page.

Table 1.4: Means of Component Estimators of Permanent and Temporary Earnings Risk by Covariates, x100

Category	u^2	**CS**$_\beta$	β	β^*	γ	**CS**$_\alpha$	α	φ
	Total Risk		*Permanent*				*Temporary*	
Industry (2-digit)								
Uncategorized/Missing	12.47		1.68	1.23	1.30		7.39	5.39
Not working at interview	38.86		-0.99	1.62	1.09		32.67	19.79
Retail	21.67		4.16	0.96	1.12		13.16	7.34
Other Services	23.43		2.69	1.82	2.05		16.38	16.43
Constr. Relate.	19.12		3.85	0.92	0.38		12.87	10.43
Public Administration	14.10		2.94	0.80	0.86		4.75	5.21
Mechanical Eng.	13.58		2.75	1.23	1.07		5.87	5.61
Other Trans.	18.41		1.79	0.83	1.24		9.43	5.90
Wholesale	22.21		6.84	1.51	1.60		7.80	10.59
Wood/Paper/Print	15.47		6.41	0.87	0.42		6.95	5.90
Educ./Sport	13.79		3.30	0.42	0.81		6.95	5.71
Health Services	23.15		3.74	1.73	2.25		8.60	6.87
Agriculture, forestry	53.14		7.18	1.79	1.92		24.51	32.34
Electrical Eng.	13.01		5.90	0.58	0.63		5.76	4.83
Postal System	17.21		6.43	0.24	0.24		5.70	5.35
Energy/Water	13.60		2.15	1.30	1.03		4.54	4.74
Construction	28.14		0.97	0.46	1.39		24.18	16.57
Iron/Steel	12.36		3.29	0.83	1.06		3.47	5.36
Food Industry	12.47		3.29	1.82	2.65		7.12	3.67
Clothing/Text.	14.80		2.42	1.34	1.58		4.87	4.26
Legal Services	43.69		-5.36	-0.39	1.83		59.32	24.50
Marital Status								
Married	17.65		3.34	1.09	1.14		9.98	7.56
Single	23.70		2.08	2.11	1.88		15.46	8.47
Divorced	24.19		0.69	0.85	1.18		10.70	14.41
Separated	26.21		1.78	1.29	0.94		21.37	20.62
Widowed	27.85		-0.72	0.73	1.40		22.26	21.40
Not available	10.84		-0.74	-1.14	-0.28		1.58	0.31

CS$_\beta$ are the $\boldsymbol{\beta}$ results taken directly from Carroll & Samwick's tables. Continued on next page.

Table 1.4: Means of Component Estimators of Permanent and Temporary Earnings Risk by Covariates, x100

Category	u^2	CS_β	β	β^*	γ	CS_α	α	φ
	Total Risk		*Permanent*				*Temporary*	
Occupation								
Uncategorized/Missing	12.65		1.67	1.23	1.31		7.42	5.40
Not working at interview	38.87		-1.01	1.63	1.08		32.87	19.93
Priv. Bus. Leadr.	27.59		6.01	1.82	1.77		14.74	10.45
Transport. Oper.	17.36		0.76	0.39	0.79		10.36	6.35
Machine Fitter	13.43		0.69	0.45	0.83		9.43	9.48
Labor./Craftsmn.	20.10		2.12	0.42	1.10		10.76	8.76
Bricklay./Carpt.	24.98		4.97	0.01	1.06		17.70	12.94
Convey. Oper.	18.45		5.29	0.71	0.71		8.34	6.16
Vendor	19.54		5.85	1.73	1.76		16.10	7.85
Architect/Engineer	11.96		2.68	1.07	1.00		6.89	4.52
Inspector	10.39		5.64	0.61	0.74		3.82	4.70
Electr. Enginr.	13.24		2.12	0.47	-0.17		9.16	7.11
Security Servic.	14.66		1.83	0.90	1.01		6.83	5.47
Educator	15.26		3.78	0.09	0.82		10.04	7.37
Pipe Fitter	15.14		0.52	0.77	0.59		9.74	7.63
Ofc. Worker etc.	15.78		1.26	0.82	1.44		10.96	6.37
Mathematician	13.49		6.57	0.71	1.53		4.49	4.81
Janitor	17.14		2.29	1.16	1.02		7.25	6.78
Soldier	9.45		3.62	0.96	0.58		3.03	3.66
Farm Manager	71.53		9.50	2.46	1.75		30.37	45.50
Mailman	13.18		1.31	0.91	1.06		6.06	3.62
Cook/Waiter	22.20		-1.18	-0.08	0.32		16.38	10.97
Related Medical Job	14.35		2.37	1.11	0.82		5.19	2.64
Business Managr.	12.78		3.84	1.81	1.34		7.13	4.49
Insurance Rep.	22.88		0.99	1.70	2.08		16.61	10.22

CS_β are the $\boldsymbol{\beta}$ results taken directly from Carroll & Samwick's tables. Continued on next page.

Table 1.4: Means of Component Estimators of Permanent and Temporary Earnings Risk by Covariates, x100

Category	u^2	CS_β	β	β^*	γ	CS_α	α	φ
	Total Risk	*Permanent*				*Temporary*		
Year								
1974	18.99		0.48	1.66	2.20		11.32	10.57
1975	12.41		1.16	0.89	0.49		7.03	6.06
1976	12.53		1.21	0.59	0.79		7.73	6.13
1977	11.88		2.42	1.03	0.74		6.37	6.46
1978	13.05		1.77	1.49	0.93		6.56	5.15
1979	12.71		1.47	0.77	1.31		6.21	6.17
1980	13.64		2.42	1.75	1.65		6.28	4.94
1981	16.08		1.74	1.17	0.86		7.90	6.30
1982	13.78		2.10	0.91	1.43		5.68	7.07
1983	14.50		1.28	0.79	0.94		7.34	6.45
1984	15.39		1.66	0.34	1.15		8.74	6.93
1985	15.26		1.69	0.98	1.02		9.30	6.29
1986	13.87		1.53	1.02	0.86		7.78	6.49
1987	13.41		2.96	1.41	1.25		7.40	4.51
1988	15.26		7.08	1.55	1.75		10.23	5.99
1989	15.65		9.19	0.94	1.01		6.78	5.91
1990	14.40		7.39	0.00	1.11		6.56	14.48
1991	18.87		6.49	0.73	0.70		8.26	12.45
1992	30.68		3.78				23.04	
1993	41.28		0.65	1.06	0.31		16.07	21.90
1995	32.83		-1.34	1.79	2.81		30.78	18.10
1997	22.74		3.80	1.60	1.23		15.80	14.70
1999	22.72		3.00		2.25		11.01	14.70
2001	26.83		0.48				18.39	

CS_β are the $\boldsymbol{\beta}$ results taken directly from Carroll & Samwick's tables.

Table 1.5A: Estimated Effects of Covariates on the Variance of Permanent Earnings Shocks (OLS), x100

	β			β^*			γ		
Variable	**Est.**	**S.E.**	**P-val.**	**Est.**	**S.E.**	**P-val.**	**Est.**	**S.E.**	**P-val.**
Race (vs. White)									
Black	-0.20	0.60	0.75	0.03	0.21	0.89	-0.11	0.19	0.57
Native American	-0.50	0.70	0.45	-0.30	0.39	0.45	-0.43	0.27	0.12
Asian	-0.80	0.80	0.32	-0.26	0.43	0.55	-0.69	0.35	0.05
Hispanic	0.80	1.70	0.62	0.54	1.66	0.74	0.76	1.50	0.61
Other	0.90	1.50	0.56	-0.34	0.29	0.24	0.37	0.47	0.43
Age (vs. 26-30)									
21-25	1.30	1.20	0.30	0.21	0.58	0.71	0.64	0.58	0.27
31-35	2.40	2.80	0.38	-0.39	0.30	0.19	-0.26	0.33	0.43
36-40	2.00	3.20	0.54	-0.64	0.45	0.15	-0.57	0.42	0.17
41-45	2.20	3.80	0.57	-0.67	0.63	0.29	-0.80	0.54	0.14
46-50	2.70	4.30	0.53	-0.67	0.72	0.35	-0.20	0.69	0.77
51-55	3.70	4.60	0.42	-0.84	0.89	0.35	-0.49	0.85	0.57
56-60	6.40	4.90	0.19	-1.31	1.66	0.43	-0.47	1.15	0.68
Education (vs. More than HS)									
Less than HS	0.00	0.60	0.99	-0.30	0.28	0.28	-0.10	0.23	0.66
HS grad	-0.10	0.70	0.89	-0.39	0.18	0.03	-0.41	0.16	0.01
PSID Sample (vs. Main)									
SEO	0.40	0.60	0.50	-0.21	0.20	0.29	-0.20	0.17	0.24
Self-Employment (vs. Not at all)									
Partially	3.40	1.90	0.07	0.36	0.66	0.59	1.60	0.79	0.04
Fully	-1.70	2.20	0.44	0.23	0.47	0.63	-0.54	0.39	0.17
Birth Cohort (vs. 1951-1955)									
pre-1930	-1.00	3.20	0.77	0.39	0.94	0.68	0.09	0.91	0.92
1931-1940	-0.20	2.30	0.93	-0.07	0.68	0.92	0.14	0.63	0.82
1941-1945	-2.40	2.60	0.34	-0.03	0.50	0.95	0.02	0.43	0.96
1946-1950	0.90	0.80	0.21	-0.30	0.32	0.36	-0.07	0.28	0.80
1956-1960	0.60	1.20	0.65	0.08	0.47	0.87	-0.02	0.34	0.95
1961-1964	3.30	2.70	0.21	0.21	0.59	0.73	-0.23	0.59	0.70
1965-1969	1.40	2.40	0.56	0.17	1.05	0.87	0.59	1.02	0.56
post-1970	-0.50	3.90	0.89	-3.04	1.92	0.11	-5.53	3.53	0.12

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Table 1.5A: Estimated Effects of Covariates on the Variance of Permanent Earnings Shocks (OLS), x100

	β			β^*			γ		
Variable	**Est.**	**S.E.**	**P-val.**	**Est.**	**S.E.**	**P-val.**	**Est.**	**S.E.**	**P-val.**
Industry (vs. Retail)									
Uncategorized/Missing	0.73	2.03	0.72	0.43	0.98	0.66	-0.92	1.33	0.49
Not working at interview	-0.27	2.08	0.90	0.00	0.71	1.00	-0.07	0.94	0.94
Other Services	-0.96	1.88	0.96	1.21	0.63	0.06	1.23	0.68	0.07
Constr. Relate.	-0.23	20.30	0.61	-0.23	0.50	0.64	-0.48	0.53	0.37
Public Administration	-2.03	1.90	0.29	-0.35	0.49	0.48	-0.41	0.64	0.53
Mechanical Eng.	-0.61	1.69	0.72	0.35	0.44	0.43	0.00	0.43	1.00
Other Trans.	0.08	1.66	0.96	0.05	0.45	0.91	-0.15	0.47	0.74
Wholesale	2.88	3.75	0.44	0.59	0.71	0.41	0.13	0.63	0.84
Wood/Paper/Print	3.28	2.93	0.26	0.21	0.72	0.77	-0.39	0.51	0.45
Educ./Sport	-2.82	1.94	0.15	-0.02	0.57	0.97	-0.31	0.53	0.56
Health Service	-0.29	2.23	0.90	1.87	1.68	0.27	0.88	0.90	0.33
Agriculture, forestry	-1.23	2.89	0.67	1.02	1.23	0.41	0.48	1.58	0.76
Electrical Eng.	1.87	3.01	0.54	-0.22	0.53	0.68	-0.20	0.42	0.64
Postal System	3.28	2.51	0.19	0.03	0.55	0.95	-0.52	0.52	0.32
Energy/Water	-1.13	1.77	0.52	0.90	0.51	0.08	0.04	0.47	0.94
Construction	-3.48	2.42	0.15	-0.74	0.74	0.32	-0.01	1.05	1.00
Iron/Steel	0.43	1.82	0.81	0.43	0.59	0.47	0.30	0.55	0.59
Food Industry	-0.09	1.82	0.96	0.76	0.73	0.30	1.36	0.85	0.11
Clothing/Text.	-0.02	2.03	0.99	0.64	0.77	0.40	0.89	0.75	0.24
Legal Services	-7.89	5.28	0.14	-0.76	1.03	0.46	-0.81	1.28	0.53
Occupation (vs. Priv. Bus. Leadr.)									
Uncategorized/Missing	-10.05	3.91	0.01	-0.73	1.41	0.60	0.31	1.69	0.86
Not working at interview	-7.75	4.01	0.05	-0.15	1.22	0.90	-0.76	1.30	0.56
Transport. Oper.	-6.03	1.71	0.00	-1.48	0.47	0.00	-0.84	0.46	0.07
Machine Fitter	-5.40	1.40	0.00	-1.04	0.42	0.01	-1.05	0.53	0.05
Labor./Craftsmn.	-3.82	1.74	0.03	-0.89	0.48	0.07	-0.56	0.53	0.30
Bricklay./Carpt.	0.38	2.71	0.89	-1.43	0.75	0.06	-0.45	0.75	0.55
Convey. Oper.	-1.84	2.27	0.42	-0.97	0.48	0.04	-0.95	0.48	0.05
Vendor	-2.46	2.47	0.32	-0.63	0.72	0.38	-0.35	0.71	0.62
Architect/Engineer	-3.42	1.45	0.02	-1.29	0.54	0.02	-1.07	0.47	0.02

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Table 1.5A: Estimated Effects of Covariates on the Variance of Permanent Earnings Shocks (OLS), x100

	β			β^*			γ		
Variable	**Est.**	**S.E.**	**P-val.**	**Est.**	**S.E.**	**P-val.**	**Est.**	**S.E.**	**P-val.**
Occupation (vs. Priv. Bus. Leadr.)									
Inspector	-1.03	2.96	0.73	-0.82	0.49	0.09	-0.99	0.50	0.05
Electr. Enginr.	-4.59	1.42	0.00	-1.36	0.48	0.01	-1.49	0.71	0.04
Security Servic.	-2.85	1.68	0.09	-0.76	0.63	0.23	-0.39	0.76	0.61
Educator	0.30	2.14	0.89	-1.32	0.61	0.03	-0.88	0.57	0.12
Pipe Fitter	-5.31	1.50	0.00	-1.28	0.47	0.01	-1.12	0.70	0.11
Ofc. Worker etc.	-4.20	1.76	0.02	-0.81	0.50	0.10	-0.12	0.68	0.86
Mathematician	0.32	1.86	0.87	-1.75	0.66	0.01	-1.09	0.61	0.07
Janitor	-1.70	1.75	0.33	-0.35	0.78	0.65	-0.78	0.93	0.40
Soldier	-1.15	2.03	0.57	-0.79	0.67	0.24	-0.87	0.77	0.26
Farm Manager	4.62	4.74	0.33	-0.62	1.75	0.72	-0.06	2.23	0.98
Mailman	-6.92	1.79	0.00	-0.90	0.53	0.09	-0.38	0.56	0.49
Cook/Waiter	-8.28	2.44	0.00	-2.05	0.68	0.00	-1.80	1.08	0.10
Related Medical Job	-3.00	1.80	0.09	-2.20	1.52	0.15	-1.51	0.83	0.07
Business Managr.	-3.21	2.21	0.15	-0.07	0.87	0.94	-0.55	0.54	0.31
Insurance Rep.	-1.85	2.44	0.45	-0.11	1.04	0.92	-0.10	1.00	0.92
Eng. Tech. Expert	-1.85	1.84	0.31	-1.10	0.57	0.05	-0.90	0.83	0.28
Marital Status (vs. Married)									
Single	-0.24	0.87	0.79	0.61	0.49	0.21	0.75	0.48	0.12
Divorced	-2.16	0.70	0.00	0.42	1.01	0.68	0.13	1.17	0.91
Separated	0.40	1.44	0.78	0.02	0.40	0.96	-0.32	0.43	0.46
Widowed	-1.99	1.16	0.09	-0.23	0.39	0.55	0.40	0.70	0.57
Not available	2.09	4.87	0.67	-2.88	1.26	0.02	-2.90	1.35	0.03
	Mean	**S.D.**		**Mean**	**S.D.**		**Mean**	**S.D.**	
Calculated	3.11	58.14		1.13	13.28		1.16	21.26	
Fitted Value (OLS)	3.20	4.32		1.41	1.09		1.25	1.15	
Fitted Value (FE)	4.63	6.31		1.48	1.31		1.22	1.62	

Notes: All regressions include year effects. The bottom panel presents the statistics used as the dependent variable in the regressions, along with their generated fitted values.

Table 1.5B: Estimated Effects of Covariates on the Variance of Temporary Earnings Shocks (OLS), x100

		α			*φ*	
Variable	**Est.**	**S.E.**	**P-val.**	**Est.**	**S.E.**	**P-val.**
Race (vs. White)						
Black	1.57	1.04	0.13	0.57	1.31	0.67
Native American	-0.17	1.38	0.90	-1.51	1.31	0.25
Asian	-2.67	2.65	0.31	-2.04	2.13	0.34
Hispanic	2.33	6.86	0.73	-3.52	1.60	0.03
Other	0.32	2.52	0.90	-3.17	2.22	0.15
Age (vs. 26-30)						
21-25	0.40	3.00	0.90	0.45	1.95	0.82
31-35	-3.76	4.57	0.41	-6.39	7.96	0.42
36-40	-5.01	6.41	0.43	-7.61	9.69	0.43
41-45	-5.70	7.08	0.42	-9.25	10.17	0.36
46-50	-7.20	7.45	0.33	-8.48	10.18	0.41
51-55	-6.61	7.82	0.40	-8.61	9.99	0.39
56-60	-5.15	7.98	0.52	-9.70	10.21	0.34
Education (vs. More than HS)						
Less than HS	1.03	1.24	0.41	0.67	1.32	0.61
HS grad	0.36	1.23	0.77	0.57	1.88	0.76
PSID Sample (vs. Main)						
SEO	0.94	0.92	0.30	0.29	1.47	0.84
Self-Employment (vs. Not at all)						
Partially	10.23	2.72	0.00	6.24	2.71	0.02
Fully	20.29	2.97	0.00	21.28	5.51	0.00
Birth Cohort (vs. 1951-1955)						
pre-1930	1.89	5.98	0.75	5.49	9.49	0.56
1931-1940	1.78	5.10	0.73	5.94	9.19	0.52
1941-1945	2.21	5.28	0.68	6.29	9.57	0.51
1946-1950	-0.02	2.00	0.99	2.73	3.41	0.42
1956-1960	-5.12	2.07	0.01	-2.79	1.96	0.15
1961-1964	-10.67	3.66	0.00	-3.94	5.14	0.44
1965-1969	-7.42	5.75	0.20	-9.43	6.85	0.17
post-1970	-0.46	10.29	0.96	-7.95	9.90	0.42

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Table 1.5B: Estimated Effects of Covariates on the Variance of Temporary Earnings Shocks (OLS), x100

	α			*φ*		
Variable	**Est.**	**S.E.**	**P-val.**	**Est.**	**S.E.**	**P-val.**
Industry (vs. Retail)						
Uncategorized/Missing	-2.18	4.38	0.62	6.01	5.25	0.25
Not working at interview	-8.17	2.77	0.00	-1.60	3.65	0.66
Other Services	-1.64	2.58	0.53	0.75	4.02	0.85
Constr. Relate.	-5.96	2.55	0.02	-3.59	3.81	0.35
Public Administration	0.95	5.69	0.87	9.90	8.07	0.22
Mechanical Eng. Other	-5.40	2.00	0.01	0.85	2.99	0.78
Trans. Wholesale	-4.45	2.37	0.06	2.64	4.23	0.53
Wood/Paper/Print	-4.74	2.60	0.07	2.46	4.96	0.62
Educ./Sport	-4.35	2.46	0.08	-1.00	4.07	0.81
Health Service	-2.59	2.99	0.39	5.58	4.72	0.24
Agriculture, forestry	-6.71	3.68	0.07	-3.17	3.64	0.38
Electrical Eng.	-5.25	5.90	0.37	1.96	12.74	0.88
Postal System	-4.09	2.56	0.11	-2.05	3.69	0.58
Energy/Water	-5.43	2.48	0.03	-2.33	3.67	0.53
Construction	-5.73	2.17	0.01	10.19	13.21	0.44
Iron/Steel	1.72	3.42	0.62	4.50	6.76	0.51
Food Industry	-5.31	2.10	0.01	1.59	3.65	0.66
Clothing/Text.	-6.08	2.42	0.01	-5.71	4.11	0.16
Legal Services	-8.40	2.61	0.00	-1.97	3.94	0.62
	4.57	6.61	0.49	3.74	7.37	0.61
Occupation (vs. Priv. Bus. Leadr.)						
Uncategorized/Missing	20.40	6.97	0.00	0.37	5.71	0.95
Not working at interview	31.04	6.26	0.00	14.15	3.52	0.00
Transport. Oper.	5.07	2.11	0.02	1.98	3.43	0.56
Machine Fitter	2.33	1.83	0.20	7.11	3.78	0.06
Labor./Craftsmn.	2.77	2.21	0.21	2.14	3.32	0.52
Bricklay./Carpt. Convey.	2.17	2.72	0.43	-4.63	3.70	0.21
Oper.	0.93	1.98	0.64	-0.42	2.46	0.86
Vendor	1.00	2.46	0.69	-0.43	4.06	0.92
Architect/Engineer	-0.12	1.56	0.94	-3.25	3.12	0.30

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Table 1.5B: Estimated Effects of Covariates on the Variance of Temporary Earnings Shocks (OLS), X100

	α			φ		
Variable	**Est.**	**S.E.**	**P-val.**	**Est.**	**S.E.**	**P-val.**
Occupation (vs. Priv. Bus. Leadr.)						
Inspector	-0.63	2.72	0.82	0.31	1.59	0.85
Electr. Enginr.	0.62	5.29	0.91	2.72	5.39	0.61
Security Servic.	-8.90	6.53	0.17	-0.97	5.76	0.87
Educator	1.25	6.85	0.86	1.04	2.91	0.72
Pipe Fitter	4.95	5.23	0.34	5.09	2.70	0.06
Ofc. Worker etc.	-3.55	3.59	0.32	2.39	2.25	0.29
Mathematician	0.55	2.80	0.84	-2.69	1.68	0.11
Janitor	1.78	5.43	0.74	-0.51	3.13	0.87
Soldier	-7.38	6.92	0.29	-4.42	5.38	0.41
Farm Manager	21.20	16.47	0.20	7.80	7.63	0.31
Mailman	1.18	2.27	0.60	2.62	2.60	0.31
Cook/Waiter	53.05	33.65	0.12	15.75	10.77	0.14
Related Medical Job	-0.01	2.88	1.00	0.99	3.20	0.76
Business Managr.	-3.20	2.20	0.15	-2.09	3.46	0.55
Insurance Rep. Eng.	-8.28	5.68	0.15	0.52	4.54	0.91
Tech. Expert	-2.59	3.09	0.40	-1.78	1.82	0.33
Marital Status (vs. Married)						
Single	1.10	2.43	0.65	4.14	2.43	0.09
Divorced	-4.44	2.83	0.12	-1.04	2.84	0.71
Separated	5.36	3.23	0.10	3.26	2.35	0.17
Widowed	4.89	4.58	0.29	11.13	6.95	0.11
Not available	-17.50	7.18	0.02	-30.74	14.59	0.04
	Mean	**S.D.**		**Mean**	**S.D.**	
Calculated	11.12	128.39		8.58	86.39	
Fitted Value (OLS)	11.17	11.24		8.47	10.86	
Fitted Value (FE)	12.24	12.89		10.49	11.60	

Notes: All regressions include year effects. The bottom panel presents the statistics used as the dependent variable in the regressions, along with their generated fitted values.

2 The Influence of Earnings Risk on Mortgage Terms and Performance

2.1 Introduction

While there seems to be general agreement that excessive risk-taking in the residential real estate market was an important factor underlying the most recent recession in the U.S., there are several competing hypotheses about the cause of that excessive risk-taking. The simplest possibility is that no one fully appreciated the risks that were being taken, but most observers instead suggest some sort of informational asymmetry. For example, it is often alleged that borrowers had some awareness of the risks they faced, but still applied for loans because they recognized the implicit insurance provided by bankruptcy laws. Other observers suggest that lenders may have originated mortgages that they should have considered excessively risky because they could collect origination fees and quickly sell the mortgage to an unsuspecting secondary market. Still another view is that regulations like the Community Reinvestment Act (CRA) created incentives for lenders to make some loans that would ordinarily have been considered too risky. Unfortunately, it is challenging to distinguish between these hypotheses because we do not directly observe what risks were known to each agent.

This paper seeks to shed some light on the issue by investigating whether a series of choices and responses made by agents were correlated with a measure of the risks facing individual borrowers. It implements this strategy using the Panel Study on Income Dynamics (PSID), one of the few large data sets that has longitudinal data on both a wide range of individuals' characteristics and information about the status of their mortgages. The longitudinal aspect is particularly important because it allows investigation of potential relationships between information that was available at the time the mortgage was originated and the subsequent performance of those mortgages.

The investigation begins by creating the measure of individuals' risk, focusing on the risk of shocks to one's earnings from labor. While this is not the only risk that individuals face, it is presumably one of the most important and least insurable, and the data indicate that this risk varies considerably across individuals. The paper measures risk using the method of Meghir and Pistaferri (2004), which has the advantage of allowing separate estimates for the risks of permanent and temporary shocks. Two of the paper's first results demonstrate (*i*) that this measure varies considerably across individuals, and (*ii*) that labor earnings risk, as computed at the time the mortgage was originated, is a strong predictor of subsequent delinquency.

The paper then investigates the information structure by assessing whether agents' choices actually vary with those risks. Specifically, I investigate the relationship between this measure of labor earnings risk at the time a mortgage was originated and subsequent outcomes and decisions made by borrowers and lenders. Intuitively, the idea is that the relationships between borrowers' or lenders' decisions and earnings risk indicate they are able to forecast (explicitly or implicitly) and respond to earnings risk. Furthermore, there is some indication of moral hazard if the choices become more closely related to earnings risk in contexts where recognizing the relationships lead to more favorable outcomes for the decision-makers.

The paper then investigates the information structure by asking whether this measure of risk predicts several outcomes. In the case of borrowers, the test is relatively straightforward. The PSID asks respondents a series of questions about their perception of their own financial risk. As one might expect, the estimate of borrowers' labor earnings risk is a significant predictor of their responses to those questions. For example, borrowers with high levels of estimated risk are much more likely to admit difficulty managing their household budgets and to express a fear of falling behind on mortgage payments. Clearly this suggests that borrowers have at least some indication of the risks they face.[6]

[6]This finding is complimented by Mian and Sufi (2015) and Jiang et al. (2014a) who document the relationship between worse mortgage performance and income misstatement by borrowers.

The results are somewhat more complex in the case of lenders. The paper first shows that there is no relationship between borrowers' estimated risk and the terms of their mortgages (e.g., the interest rate). This may not seem surprising, as lenders generally offer the same terms to all borrowers who qualify for a particular class of mortgage, but this reasoning is misleading because heterogeneity in lenders' standards should nonetheless create an equilibrium in which high-risk borrowers pay higher interest rates (for example, lenders who offer more favorable loan terms should attract more applicants and thus can be more selective, and conversely, those who are less selective can only remain profitable if they charge higher rates. Presumably borrowers prefer lenders who offer more favorable terms but would turn to other lenders if they anticipated their applications would be rejected). Thus, the fact that high-risk borrowers do not pay systematically higher interest rates than low-risk borrowers might seem to suggest that lenders are uninformed about borrowers' risk.

However, this pattern is also consistent with the claim that mortgage originators may overlook borrowers' risk if they can dump the loan onto the secondary market. This view is also supported by another finding: lenders respond differently to delinquent mortgages depending on the borrowers' risk, and they even seem capable of distinguishing between risks of temporary and permanent shocks. At a minimum this suggests that lenders have some capability of assessing borrowers' risk, at least in certain situations. Moreover, note that those situations in which lenders appear to respond to borrowers' risk occur at a stage in which lenders stand to make higher profits by judging risk levels well, whereas there are some reasons (i.e., resale) to think that lenders' profits would have suffered if they had reacted to borrowers' estimated risk. The fact that their choices become more closely related to earnings risk in contexts where recognizing the relationships lead to more favorable outcomes for the decision-makers arguably provides additional evidence suggesting that lenders are reasonably well-informed about borrowers' risks.

The evidence is less favorable for the hypothesis that reforms like the CRA played an important role. Intuitively, if such regulations provided incentives for lenders to approve loans that would otherwise be deemed too risky, we should expect to find a stronger relationship between risk and loan terms in markets where such regulations affected fewer borrowers. The data are not consistent with this prediction, however.

While it should be clear already that this exercise can provide only circumstantial evidence, some additional limitations should be acknowledged up front. To begin with, the statistic used in this paper is merely an estimate of individuals' risk, so it obviously measures their actual risk with error. Even so, the fact that it proves to be a strong predictor of delinquency suggests that it captures a key part of the desired variation. Furthermore, note that measurement error would tend to bias the conclusions away from what the paper ultimately finds: it would obscure a true relationship between risk and behavior, yet in spite of that bias the evidence still provides some indication both borrowers and lenders had some awareness of variation in borrowers' risk. It needs to be emphasized (and cannot be overemphasized) at the outset that the approach taken here is not about critiquing and attempting to improve lenders' credit risk models, but rather performing an empirical exercise that can provide insights into the nature of the recent crisis at a micro level. Because of the importance of this concern, it will be further addressed at length in the next section.

2.2 Background

2.2.1 Institutional considerations

Although earnings risk is just one type of risk facing borrowers that can affect their probability of delinquency, it is presumably one of the most important risks they face. Labor income accounts for the vast majority of most households' total incomes and it is much more difficult to insure against shocks to labor income than against shocks to most other types of income. All of this suggests that borrowers' demand for mortgages should be sensitive to their earnings risk. In section 4 I will show that my measure of earnings risk does in fact predict borrowers' likelihood of delinquency, as well as indicate that they appreciate this relationship.

Some readers may object to the proceeding analysis of lenders on the grounds that that it would be illegal to act on some of the information available to the econometrician, such as the borrower's race. First, there is of course no guarantee that lenders actually follow the law. Nonetheless, a more complete response to this point is that statistical relationships need not reflect causality: even if we were to find that borrowers' race predicted lenders' decisions, that would not necessarily imply any discriminatory intent because we could not immediately rule out the possibility that the statistical relationship actually reflected a legitimate response to a permissible factor—such as credit scores—that was unobserved by the econometrician and was correlated with race. Because this paper can only demonstrate broad statistical relationships, the findings provided below are not evidence that any single borrower or lender did anything inappropriate or illegal.

This observation extends into a response to the more general critique that the information possessed by lenders differs from that available to the econometrician. For example, lenders presumably know borrowers' credit scores (which is not reported in the PSID), but not their earnings history (which is), so the measure of earnings risk used in this paper would surely differ from that used by lenders—if they used one at all.

Further, the idea that earnings risk is related to lenders' decisions and if they price this into mortgages is likely more controversial. One possible objection is that lenders typically make loans to all successful applicants at some fixed interest rate, so earnings risk would only affect mortgage lending at the extensive margin. But this reasoning is incomplete because differences in lenders' standards should still lead to an equilibrium in which riskier borrowers obtain mortgages at higher interest rates: borrowers who are rejected as too risky by some lenders could still be able to get a mortgage from other lenders who charge higher interest rates.[7]

[7] Some readers may find it helpful to note that this point is similar to the common classroom example where heterogeneous reservation wages create an elastic aggregate labor supply curve even when individual workers supply labor inelastically at the intensive margin.

A more fundamental objection is that it is unreasonable to expect lenders to respond to earnings risk since they do not observe it well. For example, while the Uniform Residential Loan Application (URLA)[8] provides lenders with information on the loan-to-value (LTV) of the mortgage and borrowers' debt-to-income ratios (DTI), it contains limited information on borrowers' employment and earnings—in most cases, just the identity of the employer, their current monthly income, tenure at their first and most recent job, and their self-employment status.[9] Nevertheless, even though this list does not include an explicit measure of borrowers' earnings risk, lenders may still consider earnings risk *implicitly* if the models they use to make lending decisions assign weights to the available factors in rough proportion to their contribution to a prediction of earnings risk. Note that even a very simplistic statistical model designed to predict the expected profitability of a loan would tend to capture earnings risk insofar as it were an important determinant of profitability, so it may not be too far-fetched to believe that lenders may act *as if*[10] they were taking earnings risk into consideration even if they do not observe it directly.

Similar reasoning also suggests that it is not prohibitive that the available variables differ considerably from the information listed in a URLA.[11] While this deficiency precludes the assessment of lenders' compliance with regulation, it does not necessarily prevent the investigation of the more basic question of whether lenders act as if they were considering earnings risk. One very important factor that determines borrowers' ability to finance a mortgage (and that can be measured) is the uncertainty in their earnings. Another important issue is if my measure of borrowers' earnings risk is an effective proxy for their actual risk, since measurement error would tend to bias estimates toward the conclusion that lenders are uninformed. However, in spite of that potential bias, it will be shown that there is an

[8] Freddie Mac Form 65 or Fannie Mae Form 1003. https://www.fanniemae.com/content/guide_form/1003rev.pdf

[9] See sections 4 and 5 of the URLA. The URLA does not provide a detailed earnings history. The form does require additional employment and earnings information only if certain cases, such as if the borrower's current tenure is less than two years or if they are self-employed.

[10] Recall Milton Friedman's example of of using formulas from geometry and physics to predict a pool player's shots.

[11] The PSID contains much more detailed information than the URLA about borrowers' employment conditions and their earnings history, but it does not include important factors like their credit scores.

indication of a response to risk. Therefore, I can cast doubt on the idea that information about borrowers' earnings risk (and their risk of delinquency) is completely one-sided.

2.2.2 Theoretical expectations

This study is certainly not the first work to attempt to analyze the potential causes and their contribution to the recent dysfunction of the mortgage market. In addition to borrower income misstatement already mentioned, a very brief list other studies have shown roles for securitization (Levitin and Wachter 2012), other misreporting (Griffin and Maturana 2014), the secondary market (Jiang et al. 2014b), the political economy of housing and lending policies (Mian et al. 2013), the supply side of the mortgage market (Mian and Sufi 2009; Justiniano et al. 2015), and household leverage (Mian and Sufi 2010 and 2011). Given the empirical approach taken here and the subsequent findings, I now focus on work that is the most immediately relevant.

Without information problems, risk should be accurately priced into the terms of the mortgage, especially given the relatively recent switch to risk-based pricing in consumer lending (Einav et al. 2013). For instance, interest rates should be increasing in borrowers' risk and decreasing in their credit score (Edelberg 2006). One could still expect mortgage performance to be correlated with earnings instability arising from "trigger events" such as job loss (Avery et al. 1996). If a borrower's risk were clear to everyone involved, we would expect riskier borrowers to pay higher interest rates. This would be a clear prediction under the risk-based pricing that has become more common in recent years (Einav et al. 2013), but it would also be true even if lenders charged a fixed interest rate and the riskiest borrowers were more likely to be denied credit (Chatterjee et al. 2011). Even in this case, rejected applicants could subsequently apply to lenders charging higher interest rates who might still find it profitable to lend to someone deemed too risky by lenders who charge lower interest rates.

Since these predictions are inconsistent with the evidence presented below, it is likely that the market suffers from some sort of informational problem. Past work has shown that information problems can result in the deterioration of mortgage quality and that lenders are able to perceive this problem (Demyankyk and Hemert 2011). Also, lenders may be willing to grant and price mortgages this way because they do not bear the full risk of default. For example, lenders may originate mortgages and immediately sell them into the secondary market, finding willing buyers since the terms do not reflect the underlying risk. This way they get the profit from originating the mortgage and are free of any losses should the mortgage go sour. Another possibility is that lenders are insulated from losses because mortgages which they hold onto are insured by the government. This would be true of FHA-insured or other government-backed loans that would shield lenders from downside risk.

Alternatively, borrowers may have better information about their risk then lenders. If borrowers can hide this information from lenders, mortgage terms and delinquency rates would not correspond well (e.g., high risk borrowers would get "cheap" mortgages). Borrowers' option to default would protect them from a portion of the downside risk. Some work has shown borrowers self-select into mortgages with certain features, especially the interest rate type (i.e., fixed versus adjustable interest rates) based on their risk profiles (Liu and Sing 2013).

One final consideration has to do with homeownership policies and their potential unintended consequences. One such policy that came under fire in the early days of the Financial Crisis was the Community Reinvestment Act. This law which required lenders to meet the demand for credit in their locales, including lower income neighborhoods, and it developed means of evaluating lenders' success in meeting those credit needs. However, critics have argued that such rules incent lenders to grant subprime loans to borrowers who would not otherwise be deemed good credit risks (Agarwal et al. 2012).

2.3 Data Description & Earnings Risk Measures

2.3.1 Sample

Mortgage data Data on loan terms and characteristics come from the 2001-2011 waves of the PSID. This is because the focus is on the period during the housing bubble and its bursting, and also because before 2001, information is either less detailed or non-existent. Information on loan terms is provided by asking the borrower, not the lender. They include the interest rate level (measured in basis points) and its type (fixed or variable); the amount of principal left; the monthly payment;[12] the number of years left to pay; the house value,[13] which is noisy because it is determined by simply asking the respondent what they would get for their home if they sold it today; the year the mortgage was originated; and the number of years remaining on the mortgage. Also included are borrowers' DTI where debts include credit card debt, student loans, medical and legal bills, and loans from relatives;[14] and net wealth (which can be negative) where the debts are the same as listed and assets include IRAs, stocks, annuities; checking and savings accounts; CDs and other bonds; life insurance; and employer pension.

Mortgage performance variables come from the supplemental files on Housing, Mortgage Distress, and Wealth for 2009 and 2011. These include indicators for whether a person is behind on payments (i.e., delinquency); the number of months behind on payments; if the lender has begun the foreclosure process; as well as whether the mortgage has been refinanced and if mortgage debt has been restructured, though again, no details are provided as to the type of refinancing or how the mortgage debt was restructured. The data set also includes a somewhat novel variable for borrowers' self-reported perceptions about the likelihood that they will fall behind on their mortgage payments by asking them if it is

[12]For about 35-40 percent of mortgages, this includes property taxes and insurance, but there is no way to net these contributions out.

[13]This variable is then used to compute home equity and the LTV. However, auto loans are offset by a durable and fairly liquid asset of comparable value.

[14]This does not include debt from auto loans because the current principal remaining is unavailable. While some variables may allow this to be backed out, this would create a very noisy measure.

highly likely nor not likely at all that they will fall behind on payments.[15]

I should also note some unfortunate limitations. First, data are only available for those who obtained mortgage financing. I have no information on mortgage rejection. Further, I do not know who originated the loan nor who currently holds it; nor whether foreclosures have been completed; whether or when a loan was paid off; what type of refinancing or restructuring took place (e.g., was it a cash-out refinance or was it modified though the federal government's Home Affordable Modification Program); or how the debt was restructured. It is also unknown whether the mortgage is full- or no-doc, conventional or government-insured, nor is there lien information or the amount of downpayment made. Also, I do not have credit scores.[16] Finally, most outcomes of interest are relatively recent additions to the PSID (e.g., the 2009 and 2011 surveys), so the number of observations for these variables is limited. Therefore, delinquency, foreclosure, etc. cannot be analyzed during the time before the financial crisis.

I am only interested in individuals who have a home mortgage, particularly those for whom I have mortgage and wealth information at origination, so the sample used for most of the analysis will be somewhat smaller. In other words, some people have entered the data after they obtained a mortgage, there is no information on them in the year of mortgage origination. There remain 5,442 people who have a mortgage, for a total of 14,944 person-year observations. Table 2.1 provides a summary of these mortgage variables and the age-earnings and risk profiles of the people who have a mortgage. Thus in what follows, mortgages originated during the time period 2001-2011 that are still in the sample during 2009-2011 will be analyzed.

Table 2.1A shows that there are just over 250 borrowers who fall behind and just over

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[15] Available only for 2011, the PSID also asks whether if they feel ("yes" or "no") they have difficulty managing their personal finances, though not many people who are delinquent on mortgages, require restructuring, etc. admit to having problems handling their money.

[16] Although this important piece of information is missing, it can be argued that it strengthens the findings since I find an effect of earnings risk on delinquency despite the usual assumption that the econometrician likely has less information than either borrowers or lenders.

50 who have had foreclosure started: a greater number for both would be desirable.[17] The level of the interest rate and the high percentage of borrowers who have a fixed-rate mortgage suggests that most of the borrowers in the sample are at least near-prime or better. Table 2.1B indicates that most distressed mortgages correspond to borrowers which lower earnings and wealth, negative shocks, along with greater risk, DTI, and interest rates. The number of distressed mortgages increases as one moves closer to the peak of the bubble and then declines thereafter. These facts are reassuring since they fall in line with intuition. Also interesting is how actual delinquency (Behind on Payments) and anticipated delinquency (Prob. of Falling Behind) match up in many respects.

Earnings risk data The PSID is also used to obtain the earnings histories and income shocks of individuals, as well as the measures of risk. Where possible, I used the version recoded for use by the Cross-National Equivalent File (CNEF)—this being for all but the 2011 wave and supplementary variables. The data are yearly through 1997, after which surveys are biennial. To be included in the sample, an individual must be a male, aged 16 to 65, working at least 52 hours per year, report positive earnings, and not be disabled. I also include people from the Survey of Economic Opportunity which increases the proportion of low income and minority respondents (Hill 1992). Given that some of the blame of the crisis was put on policies targeting these groups, their addition is advantageous. The earnings variable is the natural logarithm of real (in constant 2011 dollars) yearly earnings.[18] A wave of data records worker characteristics at the time of survey, but earnings are retrospective, so current worker traits are matched with the income about to be earned in the coming year. While the analysis of mortgages will only use data from the last decade of this time series, the calculation of earnings risk is based on individuals' earnings his-

[17]The same is true for the number of borrowers who admit to having trouble managing their personal finances.

[18]Drewianka & Oberg (2014) examine the natural logarithm of the real weekly wage, the present paper analyzes the broader risk of all annual income because it should be more relevant to lenders and borrowers. They also experimented with other more complex specifications, including higher-order random effects, but found little improvement.

tories over 1970-2011 in order to get better estimates of individuals' predicted earnings, realized shocks, and risk. This sample begins with a total of 131,049 person-years made up of 16,971 unique individuals.

2.3.2 Summary of empirical procedure for estimating individuals' earnings risk

With earnings risk (or instability), we are interested in looking at how individuals' earnings deviate from their expected path, as defined by an age-earnings profile (equation 25).

$$y_{it} = F(A_{it};\theta) + \mu_i + \lambda_t + \lambda_s + u_{it}, \tag{25}$$

where, y_{it}, is $i's$ earnings in year t, $F(A_{it};\theta)$, a quartic in age, A_{it}, that varies across groups defined by birth cohort, age group, and race; μ_i, a person-specific intercept; and u_{it} is a deviation (i.e., shock) to earnings; year, λ_t, and state, λ_s, fixed effects are also included.[19] Fitted values of earnings, $E(y_{it})$, are interpreted as the expected earnings path of the borrowers, and the residuals are interpreted as the realized earnings shocks—deviations from the expected earnings path.

For an approximation of earnings risk, I regress the squared residuals, u_{it}^2, on a set of employment and demographic controls to obtain the fitted value, $\hat{\sigma}_{u_{it}}^2 = E[u_{it}^2|X]$. This use of the fitted values can be interpreted as instrumenting in order to reduce measurement error (Carroll and Samwick, 1997). The variables in X used to obtain the fitted values are the highest level of education completed, interacted with the year and a quartic in age; occupation and 2-digit-level industry; student status; degree of self-employment; birth group; race and SEO status; and state.

[19]The setup here and the specification in subsection 3.2 give rise to what is known as a Restricted Income Profile. Another choice would be a Heterogeneous Income Profile which allows for an individual-specific growth rate in income. However, Hryshko (2012) finds that the Restricted Income Profile is the correct specification when using the PSID.

2.3.3 Decomposing individuals' earnings risk

For the purposes here it is useful to decompose u_{it} into permanent and temporary components. An advantage of this is to see if borrowers and lenders act as if they make such a distinction between permanent and temporary risks. Shocks that take a long time to dissipate are likely of greater consequence to both borrowers and lenders. For instance, borrowers who face higher permanent risk can be blocked from credit markets because they may have greater difficulty in servicing their debt; or in the event they obtain it, they may pay premiums through higher interest rates. Borrowers who miss payments due to increased exposure to temporary risk may be less likely to be foreclosed upon and receive relief through debt restructuring since these income shortfalls are just that: short.[20]

The most standard model of the earnings process decomposes u_{it} (equation 26), the observed earnings shock, into a permanent component, π_{it}, where η_{it} is the permanent shock (equation 27); and a temporary shock, v_{it}.

$$u_{it} = \pi_{it} + v_{it}, \tag{26}$$

$$\pi_{it} = \pi_{i(t-1)} + \eta_{it}. \tag{27}$$

Permanent and temporary earnings risk are represented by the variances σ_η^2 and σ_v^2 of the permanent and temporary shocks η and v, respectively. The shocks are mean-zero and are assumed to be uncorrelated with one another. Permanent shocks are also assumed to be uncorrelated with one another, $E(\eta_{it}\eta_{it'}) = 0$ for all t and t'. The temporary shocks are assumed to satisfy covariance restrictions consistent with empirical findings, specifically, $E(v_{it}v_{i't'}) = 0$ for $|t-t'| \geq 3$ (MaCurdy 1982 and Abowd and Card 1989).[21] The appendix

[20]The decomposition will also address the problem of measurement error since the misreporting of earnings by respondents will appear in earnings shocks. However, misreporting probably is not persistent, so the temporary measure of earnings risk will capture any measurement error and leave the permanent risk measure uncontaminated.

[21]Note that $\sigma_u^2 \neq \sigma_\eta^2 + \sigma_v^2$. If we account for the auto-correlation in the temporary shock, we could

contains details for calculating the estimates of σ_{η}^2 and σ_{ν}^2, which shall be represented by γ and ϕ.[22]

2.3.4 Using the earnings risk measures

My use of the earnings risk measures draws inspiration from Carroll & Samwick (1997) who use them to test various models of precautionary savings and Saks & Shore (2005) who use them in a model of career choice. I am essentially adapting their strategy for this particular application.

Recall that I want to know if and how earnings risk can predict whether borrower will have difficulty servicing their mortgage debt. As an example, when overall risk $\hat{\sigma}_{u_{it}}^2 = E[u_{it}^2|X]$, as measured at origination, is included as an explanatory variable, do increases in overall risk correspond to higher rates of delinquency later in the life of the mortgage?

The timing of the variables used in the analysis of mortgages will vary depending on the issue. When we restrict attention to the time of origination (i.e., the vintage year where $t = 0$), variables will be denoted z_0; this will be true with the risk measures. In other cases, we will use contemporaneous observations (where $t = T$), which will be denoted z_T; this will be true of realized earnings shocks and the outcome variables (e.g., delinquency). The exception will be when examining risk in relation to loan terms (e.g., the interest rate). In that case, I will use the earnings shock at the time the mortgage is granted, u_0, so that these two variables will be measured at $t = 0$.

A second point worth discussing are the variables that provide variation in predicted risk independent of the explanatory variables. The variables used to obtain the fitted values which will be excluded from the coming regressions are occupation, 2-digit-level industry, SEO status, age, birth group, highest level of education attained, degree of self-

specify it as an MA(2) process, $\nu_{it} = \sum_{h=0}^{2} \psi_h \nu_{i(t-h)}$. Then $u_{it} = \sum_{l=0}^{a} \eta_{i(t-l)} + \sum_{h=0}^{2} \psi_h \nu_{i(t-h)}$ and $E(u_{it}^2) = \sum_{l=0}^{a} \sigma_{\eta i(t-l)}^2 + \sum_{h=0}^{2} \psi_h^2 \sigma_{\nu i(t-h)}^2$.

[22]The results for the fitted values of total, permanent, and temporary risk are broadly similar to those reported in Drewianka and Oberg (2014).

employment, and student status.[23] I exclude these variables for a couple reasons. First, since I am using labor earnings risk, occupation and industry are going to be the main determinants of the volatility in earnings. Second, earnings risk is what lenders (and borrowers) are using as a measure of credit risk, so the excluded variables have to be something a lender might use to assess credit risk and approve a mortgage. I do not use race as an excluded control for earnings risk, but lenders are legally prohibited from using this factor in their decision. Thus earnings risk as a measure of credit risk should not include it either.

2.4 The Disconnect Between Earnings Risk and Loan Terms

First, I demonstrate that the measures of earnings risk actually correspond to credit risk by showing that increases in earnings risk measured at the time of origination succeed in predicting higher rates of delinquency measured at a later time. I will then show, surprisingly, that there is no relationship between individuals' earnings risk, directly or through predicted delinquency, and the terms of their mortgage.

2.4.1 Earnings risk predicts delinquency

Table 2.2 presents the results from a Cox Proportional Hazard model of mortgage delinquency. The reported coefficients are hazard ratios. Estimates provide the proportional change in the probability of delinquency during a particular period that is associated with a 1-unit increase in the explanatory variable, given that the borrower has not been previously delinquent. The explanatory variables of interest are overall risk, permanent risk, and temporary risk, all measured when the mortgage was granted and have been normalized by their standard deviations. Other explanatory variables include individuals' expected earnings and their earnings shocks, where the latter are measured at the time the outcome variables were measured, which represent the realized outcomes of the risks that were mea-

[23] Given the small sample sizes in the regressions that follow, there is a legitimate concern about over-fitting the models. However, I experimented with a more extensive list of controls and excluding fewer than those listed, but the main results remain intact.

sured at origination. Other controls, also measured at the time of origination, include the natural logarithm of the monthly payment; race; and a dummy indicating whether there is more than 15 years left on the mortgage.[24] Regressions are stratified by an interaction between the year the loan was taken out and the U.S. Census region to allow for substantial differences in the magnitude of housing market shocks. I allowed for within-group heterogeneity with a shared frailty (essentially a random effect in this context), but the test statistic was never even marginally significant. Standard errors are clustered at the individual level.

Results from two specifications are provided. In the left-hand side of the table, the earnings shock at the time of potential delinquency is paired with either overall risk; the right-hand side does the same for the decomposition of risk. The results show that increases in earnings risk increase the chance of delinquency, particularly risk that is temporary in nature.[25] The fact that the risk measures predict delinquency is good news because it indicates that I am in fact measuring a proxy for credit risk.[26]

Also note that negative earnings shocks (i.e., decreases in u_T or lower than expected earnings) correspond to higher rates of delinquency. Predicted earnings and the size of the monthly payment at origination have the expected effects as well.

2.4.2 Earnings risk is unrelated to loan terms

Now that we see earnings risk measures credit risk and predicts default, I want to know if it has a positive relationship with other forms of credit risk, such as the interest rate.[27] Figure

[24] I have tried including the interest rate in this specification and also for those in section 5. The point estimate of the hazard ratio for the interest rate was not always statistically significant, and when it was, the point estimate was always 1. This is not surprising since I already control for the natural logarithm of the monthly payment (which is determined by the loan amount and interest rate, and sometimes also escrow payments for insurance or property taxes. In the next subsection, I find that increases in the natural logarithm of the monthly payment is associated with an increasing interest rate. Including LTV and DTI did not affect the results either.

[25] It may come as a bit of a surprise that temporary risk rather than permanent risk predicts delinquency. One possibility is that borrowers with higher permanent risk were either denied credit by lenders or that borrowers with higher permanent risk did not bother to apply for a mortgage.

[26] In some related regressions, increases in earnings risk are also associated with individuals being less likely to have a mortgage.

[27] Other loan terms examined were the monthly payment relative to monthly earnings and the interest rate as a deviation from the average, where the average was taken over state, year, loan length, and interest rate

2.1 shows a scatter plot of the interest rate (in basis points so that 100=1%) graphed against overall risk. One can see that there is no clear correlation between the two.[28]

Table 2.3 further confirms that lack of relationship. The results are from OLS regressions of the interest rate on the earnings and risk profiles. The other covariates are the natural logarithm of the interest rate, age, race, and dummies for year and region, all measured at the year of origination. Earnings shocks this time are measured at origination as well. Standard errors are clustered at the borrower level. Neither risk nor shocks significantly affect the interest rate.

One possibly is that lenders are more concerned about actual delinquency rather than earnings risk. Because earnings risk predicts delinquency, Table 2.3 includes results where earnings risk is replaced with the predicted delinquency[29] obtained from the regressions in the last subsection. Since many of the same covariates were used in those regressions, the only one repeated here is race. I also include fixed effects for the interaction between the year-region[30] of origin to account for the strata in the baseline hazard. Standard errors have been bootstrapped. Like earnings risk, predicted delinquency has no relationship with the interest rate.

Thus, while earnings risk can predict delinquency, it does not have a positive relationship with the interest rate, either directly or indirectly. I look for some indication that this disconnect occurs due to a lack of awareness, or else whether it reflects a knowing decision by borrowers, lenders, or both.

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type (i.e., fixed or variable). The findings for these other loan terms also showed no connections with earnings risk.

[28] Plots graphing DTI and LTV versus the interest rate also exhibit no relationship.

[29] Specifically, this is given by the linear prediction, $X\hat{\beta}$, from the Cox PH model. See the appendix for details.

[30] This is also interacted with whether the number of years left on the mortgage is more than 15.

2.5 Who, If Anyone, Recognizes Earnings Risk?

Since the measure of earnings risk evidently captures what it is intended to capture and that it affects delinquency, I want to see if lenders and borrowers can recognize and react to it. It is fair to expect that they should, as they clearly hold much better information than the econometrician. However, I shall still find that my measure of earnings risk predicts both borrowers' beliefs about their potential to fall behind and lenders' decisions about what to do with a distressed mortgage.

2.5.1 Borrowers' anticipation of delinquency

First I investigate whether the disconnect is due to borrowers. Borrowers were asked how likely they think they are to fall behind on payments, which I term "anticipated delinquency." Table 2.4 presents the findings of a logit[31] model on borrowers' self-reported chance of missing payments. While it may be asking quite a lot of borrowers to answer a question like this well, it is certainly the type of question a borrower ought to ask themselves when applying for a mortgage.

Again, the outcome and earnings shocks are asked and measured, respectively, at time $t = T$, while earnings risk is measured at origination ($t = 0$). The controls are the same, except that instead of stratifying on an interaction between year and region of origin, I include dummies for year of origin and state of origin. The estimates presented are semi-elasticities, evaluated at the mean value of the covariates, and standard errors are clustered at the individual level. The estimates represent the proportional (or percentage) change in anticipated delinquency given, for example, a one standard deviation increase in earnings risk.

While overall risk does not affect borrowers' self-reported chance of falling behind on payments, for the group who thinks they will fall behind, permanent risk does—a 1

[31] The responses are "very likely" and "not likely." I do not include "somewhat likely" for comparability to actual delinquency.

standard deviation increase in permanent risk raises the the chance borrowers think they will fall behind by 34 percent (the raw marginal effect for permanent risk is 1 percent).[32] This suggests borrowers are aware of and can quantify (in a more informal, back-of-the-envelope way, perhaps) their earnings risk and chance of delinquency. While this suggests they have and understand information, I cannot yet say that it is asymmetric (supporting the hypothesis that borrowers took out mortgages they knew they would have trouble repaying) until I know if lenders are equally well-informed.

2.5.2 Lenders' ability to observe earnings risk

When examining if the disconnect is due to lenders, we consider two outcomes that would directly affect the wealth of those who actually hold a mortgage (who may differ from those who issued it): foreclosure and restructuring. The survey questions ask borrowers if their lender has begun the foreclosure process; and separately if their lender has restructured their mortgage debt. Both foreclosure and restructuring are undesirable outcomes from the perspective of the lender, since they will have to take some kind of loss on the mortgage. However, if both borrowers and lenders to restructure and agree to it,[33] this is a Pareto improvement.[34] Because of this, we employ a Competing Risks model instead of the Cox PH model.[35] The other controls are the same.[36] The coefficients provided are referred to as sub-hazard ratios and are interpreted the same as the hazard ratios in the Cox model, except

[32] Further evidence of this is found in another variable in the PSID which asks borrowers if they have trouble managing their personal finances. While there are no differences in risk for this outcome, those who respond "yes" experience large negative shocks at $t = T$ compared to those who respond "no" and experience positive shocks.

[33] Restructuring may not be possible if, for example, borrowers' ability to repay is too damaged or if there were barriers to negotiation.

[34] It is possible that the lender could be better off under foreclosure if they can get something that is actually worth more than the house.

[35] Given the computational complexity of the Competing Risks model, it was not practical to stratify on the year-region of origin, so I replaced this with continuous explanatory variables that capture what seem to be the most salient sources of variation across states and years: the annual change in housing prices with the state and the yearly unemployment rate within the state.

[36] I also tried including the earnings risk variables as measured at the time of the foreclosure-restructuring decision (i.e., $\hat{\sigma}_T^2 = E_T[\sigma^2|X]$). While they display the same patterns in means as their $t = 0$ counterparts in table 2.1B, they were not significant in the regressions here.

that they correspond to the base category (e.g., foreclosure), not the competing category (e.g., restructuring).

The right-hand side of the table 2.5A presents the findings when restructuring is the base category and foreclosure the competing category. The risk measures known at origination determine the restructuring decision as increases in all of them increase the chance of restructuring. The actual earnings shock experienced is also quite influential. This implies that, in the case of restructuring, lenders are at least able to distinguish between the expected and unexpected components of borrowers' current earnings.[37] Since both permanent and temporary risk influence the decision to restructure, I rerun the restructuring specifications to determine if they have distinct effects (i.e., do lenders distinguish between the two types of risk). After conditioning on overall risk (which includes permanent risk), permanent risk still has its own effect.

The left-hand side of the table presents the findings when foreclosure is the base category and restructuring is the competing category. None of the earnings risk measures is predictive of foreclosure, but the lack of precision is likely due to the small number of borrowers in this category. However, it appears that the lender may able to make a distinction between the duration of the shock—specifically, while still insignificant, it is permanent risk which appears more important.

Based on the evidence I have found, it can be seen that the disconnect between earnings risk and the interest rate is due to lenders ignoring it despite being able to recognize risk and to distinguish between the nature of the risk. This and the finding that borrowers are aware of (the consequences) of earnings risk, suggests that while the typical asymmetric information hypothesis cannot be ruled out, it may be somewhat different since both sides seem to be potentially responsible for the disconnect. While lenders respond to earnings risk and can also distinguish between its type, this is only preliminary evidence that there

[37] Like delinquency, the fact that permanent risk is not predictive of any of the distress outcomes may occur because lenders denied these borrowers credit or borrowers did not apply for a mortgage.

is a supply side contribution to the disconnect.[38] I therefore explore this in more detail.

2.6 Roles for Subprime Lending

One common hypothesis for the disconnect may arise is due to federal homeownership policies attempting to increase homeownership among certain groups, like those with low incomes,[39] who were then channeled into subprime mortgages. The policy thus created incentives for lenders to ignore or discount risk. To test the possible roles specific to the the subprime mortgage market, I return to the interest rate-predicted delinquency regressions by supplementing the data using Home Mortgage Disclosure Act (HMDA) data for the years 2001-2010.[40] Three controls will allow me to see if factors associated to local subprime lending in general, and federal involvement in the housing market in particular, played a role. The first two variables provide the rate of subprime mortgage origination in every state and year during this time period for conventional and government-insured (e.g., insured by the Federal Housing Administration, Veterans Administration, etc.), where the latter can help test for any lender moral hazard. The last is the state-year rate of conventional subprime mortgage originations for home purchases, where the rates are allowed to differ by race, so that I can test the validity of certain targeted housing policies (e.g., the CRA).

The association between risk of delinquency and the interest rate was shown to be non-existent for the entire sample. However, it is possible that the expected positive relationship could reveal itself in state-years where the rates of subprime lending were lowest, lowest being defined as rates in the bottom third of the lending rate distribution for that state-

[38] Also, the lenders responding to the realized risk are not necessarily those who originated the mortgage.

[39] Or also certain racial groups. Rerunning the regressions from sections 4 and 5 by interacting risk with the race dummies did not indicate that minorities were more likely to be delinquent, face different interest rates, or undergo foreclosure

[40] These HMDA data files (http://www.metrotrends.org/natdata/hmda/hmda_ download.cfm) and the procedures for constructing them were initially developed by the Urban Institute to support DataPlace (www.dataplace.org). The data are licensed under the Open Database License (http:/www.metrotrends.org/natdata/ODbL.cfm).

year (i.e., the mortgage market may have functioned normally during state-years with low incidence of subprime lending). Table 2.6 presents OLS results like those in section 4 for the interest rate regressed on predicted delinquency and a white/non-white indicator, but just for the sample of low state-year subprime lending rates. Fixed effects for the local housing market conditions are again included (defined on an interaction between region-year of origin and whether there are more than 15 years left on the mortgage).[41]

While most of the coefficients on predicted delinquency display the correct sign, none of them are statistically significant or look different from those in Table 2.3. However, I point out that the sample sizes are quite low. Thus, of all the explanations we can examine, only those on the demand side of the mortgage lending market appear to be responsible for the disconnect.

2.7 Conclusion

Evidence presented here documented a peculiar feature of the mortgage market in the years leading up to the recent financial crisis. Using labor earnings risk as a measure of credit risk at the time of origination, I am able to predict eventual borrower delinquency. However, earnings risk is not priced into the mortgage (either directly or indirectly through predicted delinquency), as shown by a disconnect between it and the interest rate. The evidence also suggests that the disconnect is in part due to borrowers ignoring this earnings risk. I found no evidence for the supply side explanations I was able to test such as moral hazard arising through government-insured loans, homeownership policies targeting minority groups, or local conditions in the sub-prime housing market. However, given the small samples sizes and evidence found elsewhere (Agarwal et al. 2012), this rejection should be taken with a grain of salt. Also, since lenders can recognize earnings risk and attempt to mitigate its consequences, it seems highly unlikely that an explanation does not exist.

[41] I experimented with other specifications using these controls such as defining the strata differently and including the lending variables as controls in various forms. All showed no relationship.

Because the owner of the mortgage at this stage may not be the originator, future work could focus on one of the very important supply side hypotheses I was not able to test: the resale of mortgages by originators into the secondary market. In principle, one could use state resale rates to see if the disconnect between delinquency and loan terms is more severe in states which had a higher percentage of mortgages resold. If this is the case, policy could have lenders hold onto mortgages they originate for a longer period of time. Since they would bear the risk for a duration of the loan, they would have an interest to price risk into them.

Given some of the data limitations when testing the supply side hypotheses, it would be beneficial to obtain information at a smaller geographic level, say the county. Only being able to define local housing markets by region and even states results in the loss of variation within those areas. Also, while there was quite a bit of data on conventional sub-prime mortgages, analogous variables for government sub-prime mortgages would provide a more certain rejection of those hypotheses.

Lastly, regardless of how I would specifically proceed, it would be highly desirable to obtain more detailed loan-level data on the variables I mentioned were missing, especially those related the acceptance and denial of credit. This would go a long way in providing a broader and more complete picture.

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Table 2.1A: Summary Statistics, Mortgage Variables and Earnings & Risk

Variable	**Mean**	**SD**	Variable	**Number**	**Share**	Variable	**Mean**	**SD**
A-E Profile			**Interest Rate, type**			Debt-to-Income	0.31	1.03
$E_0(y)$: Predicted Earnings	10.983	0.570	Fixed	6,824	91.19	Net Wealth	$84,354	$256,811
u_T: Earnings Shock	0.017	0.499	Variable	659	8.81		**Number**	**Share**
u_0: Earnings Shock	0.013	0.553	**Behind on Mortgage**			**Refinanced**		
Risk			"Yes"	263	4.92	"Yes, refinanced"	6,941	49.96
$E_0(u^2\|X)$: Overall	0.216	0.131	"No"	5,078	95.08	"No, original"	6,951	50.04
$E_0(\gamma\|X)$: Permanent	0.014	0.018	**Foreclosure Started**			**Restructured**		
$E_0(\varphi\|X)$: Temporary	0.152	0.079	"Yes"	53	20.23	"Yes"	608	11.39
Mortgage Terms & Characteristics			"No"	209	79.77	"No"	4,730	88.61
Interest Rate, lvl. (100=1%)	512.27	240.53	**Probability of Falling Behind**			**Difficulty with Money**		
Loan Size at origination	$149,544	$120,962	"Very Likely"	138	2.59	"Yes"	30	0.97
Principal Left	$152,422	$128,590	"Somewhat Likely"	509	9.56	"No"	3,065	99.03
Monthly Pmt.-to-Earnings	0.26	0.15	"Not Likely"	4,678	87.85	**Overall, have a mortgage**		
Term Length	23	8		**Mean**	**SD**	Total Observations	13,922	
Years Left	21	9	**No. Months Behind**			Unique Individuals	5,442	
Loan-to-Value	0.63	0.29	All	0.22	1.83	**Data at loan origination**		
Equity	$116,903	$194,843	"Yes, behind"	4.42	7.09	Total Observations	1,388	

Note: Mortgage variables are drawn from the 2001-2011 waves of the Panel Study of Income Dynamics (PSID). Mortgage Distress variables are only available for the 2009 and 2011 waves. Dollar amounts are in 2011 dollars. Risk and earnings are measured at origination; earnings shocks are dissaggregated into shocks that occurred at $t=0$ (origination) and $t=T$. They use observations are for men, aged 16 to 65, from the 1970-2011 waves who report positive earnings, at least 52 hours of work in the previous year, are not disabled, and have a mortgage. Data are annual up to and including 1997 and then biennial afterwards. See Section 3 for definitions and descriptions of variables.

Table 2.1B: Characteristics of Distressed Mortgages

	Behind on Payments		Prob. of Falling Behind		Foreclosure Started/Restructured		
	"Yes"	"No"	"Very Likely"	"Not Likely"	Foreclosure	Restructured	Neither
A-E Profile							
$E_0(y)$: Predicted Earnings	10.84***	11.06***	10.85**	11.07**	10.81***	10.91***	11.05***
u_T: Earnings Shock	-0.16***	0.02***	-0.16**	0.03**	0.11	0.00	0.02
u_0: Earnings Shock	-0.09**	0.03**	0.01	0.03	-0.07	-0.06	0.01
Risk							
$E_0(u^2 \mid X)$: Overall	0.25**	0.21**	0.21	0.21	0.199	0.232	0.217
$E_0(\gamma \mid X)$: Permanent	0.02	0.01	0.02*	0.01*	0.018	0.016	0.014
$E_0(\varphi \mid X)$: Temporary	0.17*	0.15*	0.15	0.15	0.152**	0.167**	0.149**
Wealth and Home							
Debt-to-Income	0.36	0.28	0.27	0.28	0.22	0.32	0.25
Net Wealth	$15,896**	$95,138**	$56,148	$100,134	$13,500***	$23,107***	$107,337***
Loan-to-Value	0.84***	0.67***	0.84***	0.67***	0.88***	0.78***	0.65***
Monthly Pmt.-to-Earnings	0.51	0.47	0.43	0.47	0.43***	0.60***	0.42***
Interest Rate, lvl. (100=1%)	618.97**	529.95**	509.52	531.81	715.00	514.36	521.29
Year of Origination							
2001	6		1		0	7	
2003	9		6		2	15	
2005	27		12		5	62	
2007	63		25		15	86	
2009	20		15		0	101	
2011	7		0		1	36	

All variables measured at origination, unless otherwise noted. *, **, *** denote significance levels of 10%, 5%, and 1%, respectively.

Table 2.2: Effects of Realized Shocks & Risk on Delinquency, Actual						
	Delinquency, Actual: Behind on Mortgage Payments?					
	H.R.	*S.E.*	*P-val.*	*H.R.*	*S.E.*	*P-val.*
A-E Profile						
u_T: Earnings Shock	0.58***	0.11	0.00	0.59***	0.11	0.00
Risk						
$E_0(u^2/\sigma_{u\text{-}sq.}\|X)$: Overall	1.39**	0.23	0.05			
$E_0(\gamma/\sigma_\gamma\|X)$: Permanent				0.94	0.11	0.58
$E_0(\varphi/\sigma_\varphi\|X)$: Temporary				1.43***	0.26	0.05

Results are from a Cox PH model. Earnings risk measures are normalized by their standard deviations. Standard errors clustered at the individual level. Stratified by region-year of origin. Controls measured at origination are predicted earnings, the natural logarithm of monthly payment, and race. Also included is an indicator for whether the remaining term of the mortgage is greater than 15 years. Estimates are hazard ratios. n=889.

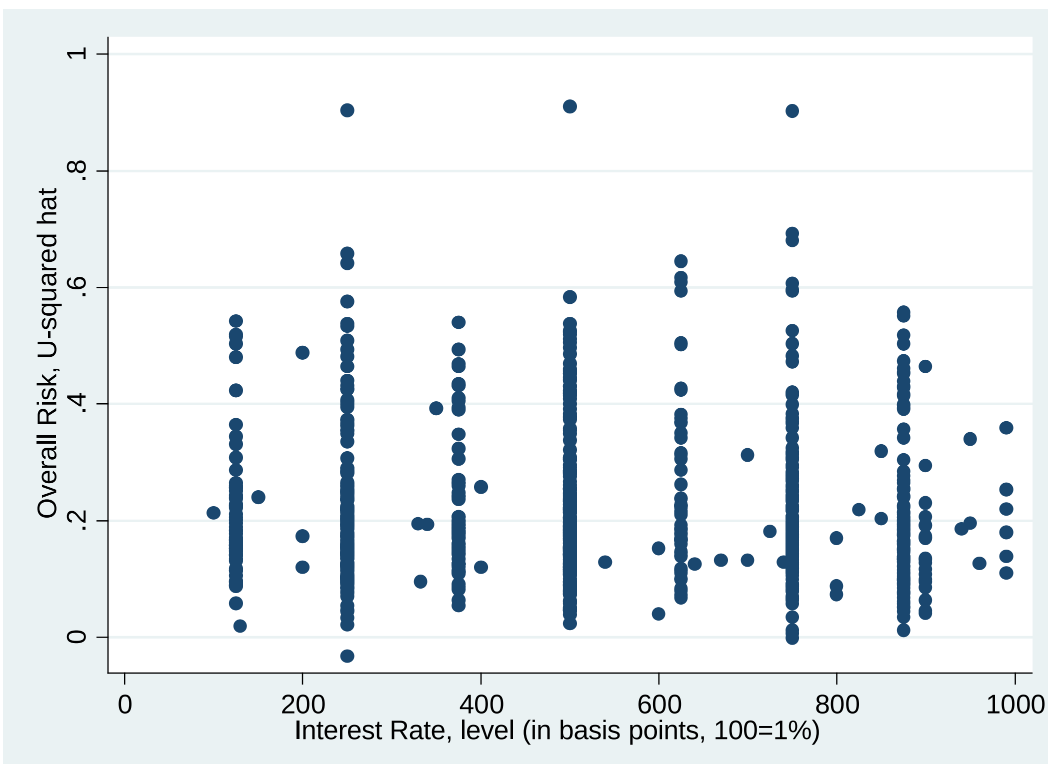

Figure 2.1: Overall Earnings Risk and the Interest Rate Level (variables measured at origination, t=0)

Table 2.3: Effects of Realized Shocks, Risk, & Predicted Delinquency on the Interest Rate, Level

	Interest Rate, Level (in basis points, 100=1%)								
	Coef.	*S.E.*	*P-val.*	*Coef.*	*S.E.*	*P-val.*	*Coef.*	*S.E.*	*P-val.*
A-E Profile									
u_0: Earnings Shock	8.77	15.31	0.57	9.65	15.33	0.53			
Risk									
$E_0(u^2/\boldsymbol{\sigma}_{u\text{-}sq.}\|X)$: Overall	-1.59	11.54	0.89						
$E_0(\boldsymbol{\gamma}/\boldsymbol{\sigma}_{\gamma}\|X)$: Permanent				7.69	11.92	0.52			
$E_0(\boldsymbol{\varphi}/\boldsymbol{\sigma}_{\varphi}\|X)$: Temporary				-0.66	11.23	0.95			
Predicted Delinquency									
$P(D\|X)$							6.36	15.19	0.68

Results are from an OLS regression model. Earnings risk measures are normalized by their standard deviations. Standard errors clustered at the individual level. Controls measured at origination are predicted earnings, the natural logarithm of monthly payment, age, race, and dummies for region-year of origin. n =696. Predicted delinquency is the linear prediction, $X\boldsymbol{\beta}$, from the actual delinquency regressions using overall risk, $E_0(u^2|X)$ from Table 2. The predicted delinquency regressions include controls for race and fixed effects based on the region-year of origin interacted with whether the number of years remaining on the term is greater than 15. Standard errors are bootstrapped. n =655 .

Table 2.4: Effects of Realized Shocks & Risk on Delinquency, Anticipated

	Delinquency, Anticipated: How Likely to Fall Behind?					
	Semi-El.	*S.E.*	*P-val.*	*Semi.El.*	*S.E.*	*P-val.*
A-E Profile						
u_T: Earnings Shock	-0.29	0.35	0.41	-0.28	0.36	0.44
Risk						
$E_0(u^2/\boldsymbol{\sigma}_{u\text{-}sq.}\|X)$: Overall	-0.22	0.32	0.50			
$E_0(\boldsymbol{\gamma}/\boldsymbol{\sigma}_{\gamma}\|X)$: Permanent				0.35***	0.12	0.00
$E_0(\boldsymbol{\varphi}/\boldsymbol{\sigma}_{\varphi}\|X)$: Temporary				-0.28	0.27	0.31

Results are from a logit regression model. Earnings risk measures are normalized by their standard deviations. Standard errors clustered at the individual level. Controls measured at origination are predicted earnings, the natural logarithm of monthly payment, age, race; and dummies for year and region of origination and whether the years left to pay the mortgage exceeds 15. Semi- elasticities evaluated at the mean. n =991

Table 2.5A: Effects of Realized Shocks & Risk on Foreclosure and Restructuring

	Lender Started Foreclosure Process?						Mortgage Debt Restructured?					
	S.H.R.	*S.E.*	*P-val.*	*S.H.R.*	*S.E.*	*P-val.*	*S.H.R.*	*S.E.*	*P-val.*	*S.H.R.*	*S.E.*	*P-val.*
A-E Profile												
u_T: Earnings Shock	1.44	0.53	0.32	1.51	0.63	0.32	0.57***	0.09	0.00	0.56***	0.09	0.00
Risk												
$E_0(u^2/\sigma_{u\text{-}sq.}\|X)$: Overall	0.99	0.25	0.98				1.17*	0.11	0.10			
$E_0(\gamma/\sigma_\gamma\|X)$: Permanent				1.21	0.24	0.33				1.17*	0.09	0.06
$E_0(\varphi/\sigma_\varphi\|X)$: Temporary				0.84	0.36	0.68				1.21**	0.12	0.05

Results are from a Competing Risks regressions model. Earnings risk measures are normalized by their standard deviations. Standard errors clustered at the individual level. Controls measured at origination are predicted earnings, the natural logarithm of monthly payment, age, and race. Other controls are change in housing prices in the state of the mortgage, the unemployment rate, and whether the mortgage has more than 15 years left on its term. Estimates are sub-hazard ratios. In the left panel, the base category is foreclosure and the competing category, restructuring. n =900. In the right panel, the base category is restructuring and the competing category, foreclosure. n =861.

Table 2.5B: Effects of Realized Shocks & Risk on Restructuring

	Mortgage Debt Restrucuted?					
	S.H.R.	*S.E.*	*P-val.*	*S.H.R.*	*S.E.*	*P-val.*
A-E Profile						
u_T: Earnings Shock				0.56***	0.09	0.00
Risk						
$E_0(u^2/\boldsymbol{\sigma}_{u\text{-}sq.}\|X)$: Overall	1.15	0.11	0.14	1.17*	0.11	0.09
$E_0(\boldsymbol{\gamma}/\boldsymbol{\sigma}_{\gamma}\|X)$: Permanent	1.17*	0.10	0.08	1.18**	0.01	0.05

Results are from a Cox PH model. Earnings risk measures are normalized by their standard deviations. Standard errors clustered at the individual level. Stratified by region-year of origin. Controls measured at origination are predicted earnings, the natural logarithm of monthly payment, and race. Also included is an indicator for whether the remaining term of the mortgage is greater than 15 years.. Estimates are hazard ratios. n=887.

Table 2.6: Effect of Predicted Delinquency on Interest Rate for State-Years with Low Rates of Subprime Lending

	Conventional Orig.			**Government Orig.**			**Conv. Orig. for Homes Purchases**		
Predicted Delinquency	*Coef.*	*S.E.*	*P-val.*	*Coef.*	*S.E.*	*P-val.*	*Coef.*	*S.E.*	*P-val.*
$P(D\|X)$	56.76	93.04	0.54	23.65	113.47	0.84	-4.15	20.18	0.84

Predicted delinquency is the linear prediction, $X\boldsymbol{\beta}$, from the actual delinquency regressions using overall risk, $E_0(u^2|X)$ from Table 2. The predicted delinquency regressions include controls for white/non-white and fixed effects based on the region-year of origin interacted with whether the number of years remaining on the term is greater than 15. "Low" rates of sub-prime are those state-years where the rate of origination was in the bottom third of the distribution. For the rate of conventional sub-prime originations, $n=57$; for the rate of government-insured (i.e., FHA, VA) sub-prime originations, $n=56$; for the rate of conventional sub-prime originations for home purchases by race, $n=242$. Standard errors are bootstrapped.

3 The Housing Bubble and the Evolution of the Homeownership Gap

3.1 Introduction

While the aggregate homeownership rate in the U.S. has tracked the business cycle fairly closely over the past two decades, there has been at the same time a trend toward a larger gap in homeownership between whites and non-whites. Figure 3.1 which plots the white/non-white homeownership differential biennially for 1999-2009. Indeed the experience over the most recent business cycle is not uniform, as Figure 3.2 indicates. Here homeownership rates are plotted separately for whites and non-whites, indexed by the base year 1999. While there was an increase in homeownership for whites during the boom, this rise is absent for non-whites and is in fact a small decline. While homeownership fall substantially for whites during the bust, the drop is much sharper and more severe for non-whites.

There are several potential explanations for this structural trend. To begin with, figure 3.2 shows that during the boom, divergence was not simply the fact that both whites and non-whites experienced growth, but with ownership among whites outpacing that of non-whites. This may be somewhat surprising given policies which aim to increase homeownership among low-income and minority individuals (e.g., the Community Reinvestment Act). Perhaps they did help low-income persons, but these happened to be predominantly white. Relative growth in income and other assets for whites could have made it more likely for them to obtain financing; or they may have been more informed about financing options given the changes and innovations mortgage financing and products. It is also possible that non-whites were perceived as too risky, but due to criticisms of excessive risk-taking, this explanation seems implausible.

Continued and accelerated divergence during the bust may have been due to a greater incidence of delinquency and foreclosure among non-whites. Income instability, lower income, and greater leverage could have made it more difficult to repay mortgages and keep homes. Non-whites may have been less informed about federal measures to help with repayment through refinancing or restructuring (e.g., Home Affordable Modification Program); or the possibility of working directly with their lender. A final possibility which could have contributed to divergence over both periods is some form of discrimination—lending and distress relief was more common among whites than non-whites.

This paper examines the divergence in the homeownership gap during the boom and bust to see which reasons are most plausible. I use a rich set of controls from the PSID to account for preferences in the willingness to own and also the ability to own; controls for various forms of assets, debt, income, and risk allow me to investigate the unique environment and developments within the mortgage market during the last decade. In addition to standard demographic and employment controls, these others include factors affecting mobility, household formation, creditworthiness, and wealth. I employ the methods of Juhn, Murphy, and Pierce (1991) who decompose the changes in differentials into explained/observed components and unexplained/residual components.

I find that the majority of the changes in the homeownership gap over both the boom and bust years are largely explained by family structure. Other factors like geography, earnings risk, income and wealth, and education actually contribute to its decrease. Thus some of the earlier explanations for the divergence during the boom do not seem to hold water. The change in the residual component contributes relatively less, but still is a sizable portion and is attributable to either unobservable characteristics or discrimination.

Family structure is still the largest contributor during the bust, but a greater array of factors matter: geography, earnings risk, and education. The contribution of earnings risk suggests that lenders were relatively more risk averse when it came to lending to or perhaps helping distressed non-white borrowers; the contribution of education may suggest that non-white borrowers were less aware of avenues which could help them keep their homes. Again, even though a sizable fraction of the gap in any given year remains unexplained, the residual component still changes relatively little compared to the predicted component. In fact, the change in the residual component is negligible, casting doubt on the role of unobserved characteristics or discrimination playing a role during the bust.

3.2 Related Work

This section only details recent research regarding the homeownership gap; a lengthy and comprehensive report by Herbert et al. (2005) reviews the literature up until that time. That report followed President George W. Bush's 2002 call to increase homeownership among minority and low-income persons and measures to help with financing. One of the important points that this report identifies is that almost no study exists that accounts for selection into homeownership through such channels as the household formation decision, household mobility, preferential tax treatment for homeowners, income risk, and other supply factors. However, they note, and Haurin and Rosenthal (2008) find, that accounting for the household formation decision is not particularly important: it only matters for a couple percentage points of the homeownership gap for a subset of homeowners, particularly younger households. Therefore in what I estimate, I do not jointly model and estimate one of these demand determinants, but still control for many of them.

I start my analysis in 1999, and studies which have looked at changes in the homeownership gap up until that time have found the following. Coulson and Dalton (2010) use Census data from 1960-2000 and find little change in the gap. They point out that changes in how observables affect homeownership would have caused the gap to narrow significantly, if not close, by now, but were offset by opposite changes in the residual component. Using the Current Population Survey for 1989 and 1999, Bostic and Surette (2001) find that the homeownership gap between middle- and low-income owners decreased, but that observables can only explain this decline for owners in the top 40 percent of the income distribution. The decrease is sharpest for the poorest and minorities, but it is left largely unexplained. This is attributed to housing policy and favorable mortgage lending and marcoeconomic conditions during this period.

The most relevant and similar work is that of Fesselmeyer et al., specifically a 2014 working paper, which utilizes the Census' American Community Survey (ACS). They also decompose the changes in the homeownership gap into explained and unexplained portions. However, because they use quantile regression, they are mainly concerned with the information available at the ends of the homeownership distribution; and they only investigate the bust, not the boom as well. This is important not just because they haven't studied this part of the most recent business cycle, but also because the change in the homeownership gap is not cyclical over this period, at least for the mean (i.e., the homeownership gap does not converge during the boom). My results regarding marital status are consistent with what they find for those at the middle of the homeownership distribution.[42]

This should perhaps not be surprising. In an investigation on default and foreclosure, Bayer et al. (2013) uncover that minorities are more likely, compared to whites, to become delinquent on their mortgages, default, and lose their homes. These propensities are even higher within racial groups for those in low employment areas and with high debt-to-income ratios (DTI). The findings are even more pronounced to those considered to be sub-prime and for mortgages originated at the peak of the bubble. The former point found here, and other studies using credit score information, motivates the inclusion of earnings risk measures. Oberg (2015) showed that these were a strong predictor of mortgage distress including delinquency and restructuring, and were associated with whether or not one had a mortgage. The bust years are also of strong interest since the official homeownership rate likely overstates that effective rate by more than 5 percent as argued in Haughwout et al. (2010).

While recession years are typically of more interest than expansion years, especially for policy, boom years affect on the homeownership gap should not be ignored since they can

[42] During the boom period, Fesselmeyer et al. (2013) decomposed changes in the house value gap, employing the same methods and using the American Housing Survey. Fesselmeyer et al. (2012) decomposed differences in the homeownership gap, again using quantile regression, but for 2007 only (i.e., they didn't look at changes or divergence pre-crash).

perhaps shed some light on what followed (as Bayer et al. 2013 make clear and what factors determine the long-run trend). Additionally, while Fesselmeyer et al. (2014) have a sizable amount of important controls, the PSID has its own unique controls I use for geography, income and wealth, and risk which allow me to evaluate some additional hypotheses.

3.3 Empirical Method

The main analysis below employs the methods developed by Juhn, Murphy, and Pierce (1991) in their examination of the change in the black-white wage gap and subsequently used by Blau and Kahn (1996, 1997) in their studies of the gender wage gap. The methods decompose the divergence in the homeownership gap into those which can be explained by changes to observed characteristics (e.g., individuals' levels of income, wealth, or earnings risk) and the prices or returns to these characteristics (e.g., how important or how strongly income, wealth, and earnings risk are to lenders); and to a part that is left unexplained, possibly due to between-group inequality (e.g., discrimination) and within-group inequality (e.g., because of immigration).

More formally, consider a linear probability model for homeownership

$$y_{igt} = X_{igt}\beta_t + e_{igt}, \tag{28}$$

where y_{igt} equals 1 for individual i in group g (white or non-white) for year t (1999, 2005, or 2009) if they own their home and 0 if they rent; X_{igt} are their observed characteristics; β_t are the observed prices or returns to characteristics in year t; and e_{igt} are the residuals, where it is assumed that $E(e_{igt}) = 0$.

Suppressing the individual and group subscripts, the homeownership gap can be expressed as

$$y_{Wt} - y_{NWt} = (X_{Wt} - X_{NWt})\beta_t + (e_{Wt} - e_{NWt}), \tag{29}$$

or

$$\Delta y_t = \Delta X_t \beta_t + \Delta e_t. \tag{30}$$

This gives rise to a form of the Blinder-Oaxaca (1973) decomposition. The first term on the right-hand side of equation (30), $\Delta X_t \beta_t$, is the explained part of the overall difference and the last term, Δe_t, is the residual or unexplained part.

To further help understand the change in the homeownership gap, the residuals can be decomposed into two parts

$$e_{igt} = \omega_t \cdot \varepsilon_{igt}. \tag{31}$$

The term ε_{igt} is individual i's standardized residual and ω_{igt} a loading factor that allows for changes within the white/non-white residual distributions. Equation (30) then becomes

$$\Delta y_t = \Delta X_t \beta_t + \omega_t \Delta \varepsilon_t. \tag{32}$$

With this standardization of the residuals, the divergence in the homeownership gap is

$$\begin{aligned} \Delta y_2 - \Delta y_1 &= [(\Delta X_2 - \Delta X_1) \cdot \beta_1 + \Delta X_1 \cdot (\beta_2 - \beta_1)] \\ &+ [(\Delta \varepsilon_2 - \Delta \varepsilon_1) \cdot \omega_1 + \Delta \varepsilon_1 \cdot (\omega_2 - \omega_1)]. \end{aligned} \tag{33}$$

The top line of equation (33) is a breakdown of changes in the predicted or explained gap. The first term reflects changes in characteristics that may contribute to the divergence in the homeownership gap. For instance lower marriage rates for non-whites could lead to divergence. Divergence could also be due to changes in observed prices of or returns to characteristics. As an example, during the bust, as lenders became more risk averse, they may have been less willing to lend to non-whites who would be considered subprime due to their high DTI's, low wealth, or high earnings risk.

The second line of equation (33) has analogous counterparts. The first term reflects the change in the homeownership gap due to changes residual rankings of individuals. This is usually attributed to changes in unobserved characteristics that are not controlled for or discrimination. The second term reflects changes in residual inequality or unobserved prices: the homeownership gap could change because of immigration.[43]

3.4 Data Description & Sample

The data are drawn from the Panel Study of Income Dynamics (PSID), where the main variable of interest asks respondents whether they own their own home or rent. This variable runs the full length of the PSID, the present study focuses on the period of interest 1999-2009.[44] The trend in homeownership does match these other sources pretty well with a couple minor exceptions. Figure 3.3 plots the homeownership rate for 1970-2009 using data from the Census and the Panel Study of Income Dynamics (PSID). While the PSID data track the Census series pretty closely, the former is a little more volatile during the first couple decades and more pro-cyclical during the last couple of decades. In both series, one can see that for most of the 70s, 80s, and then into the 90s, the rate was relatively stagnant, at just under two thirds of the population. Then in the mid-1990s the rate takes of, increasing until the recent recession, where it began to plummet, almost back to the point to where its ascent began.

Tables 3.1 & 3.2 provide an overview of selected factors affecting the willingness and ability to own a home and how they differ across whites/non-whites. Because interest lies change of the homeownership gap, for each variable I provide samples means or shares for each group in each year I will be using. I also calculate the differences between groups in a

[43] In the empirical implementation below, a third term in both the predicted and residual gap changes which captures simultaneous changes in quantities and prices. Though it is calculated, it is not presented. Also, an extended discussion of the residual decomposition is provided in the appendix for the interested reader.

[44] The last available year for the PSID, 2011, is excluded due to a large jump in the homeownership rate.

given year, along with the changes in these differences by comparing the beginning of the boom to its end (and likewise for the bust). I include standard demographic and employment controls such as age, education, marital status, self-employment status. Also included are measures of, earnings, wealth, and debt-to-income (DTI);[45] and some geographic indicators. There are about 3,000-3,500 observations for each year used in the decomposition, about two-thirds of which correspond to white individuals. Data are for non-disabled men, ages 24-62, reporting at least 52 hours of annual hours and positive earnings. During this period, there are 24,406 observations for 6,514 people.

Over the course of the boom, the gap between household earnings and wealth increased between blacks and whites, as did household leverage (i.e., DTI). The change in relative education shares increased (decreased) at the high school graduate (dropout) level. While unemployment in state of residence actually rose for both groups, the differential fell;[46] Looking at just highly urbanized areas (that which are in metro areas of 1 million or more people), while there are significant differences between the groups, there was only a small change in these differences. Further, there were no huge changes in earnings risk.[47] The starkest changes took place regarding marital status. Over the course of the boom, there was an increase in the gap between the shares of white/non-white persons who were married, single, and divorced/separated.[48] In fact, the percentage of non-whites who are married is falling much faster than among whites.

Some of the qualitative patterns are the same when looking at the bust, such as factors in the income and wealth and education panels (and even shrink in magnitude). One factor

[45] Permanent earnings are the predicted earnings from equation (25) in the Appendix. Debts include credit card debt, student loans, medical and legal bills, and loans from relatives; and net wealth (which can be negative) where the debts are the same as listed and assets include IRAs, stocks, annuities; checking and savings accounts; CDs and other bonds; life insurance; and employer pension. Mortgage debt is not included.

[46] In the analysis below, I included the unemployed, but found no changes in the results.

[47] Earnings risk is measured as in Drewianka and Oberg (2014) who combined and extended the methods developed by Meghir and Pistaferri (2004), along with Carroll and Samwick (1997). Since it is not a major contributor to the change in the homeownership gap, and thus not central to this study, an outline of the additional data and methods used for earnings risk are available in the appendix for the interested reader.

[48] This may partly explain the increase in the share of white households relative to non-whites who have a wife working at least half-time.

that changes sign is state unemployment rate which shows convergence between the two groups. Again, the glaring factor is marital status, which continues its strong divergence relative to other factors. Geography is quite large compared to the boom, indicating that relative urbanization fell.

3.5 The Change in the Homeownership Gap

I know decompose changes in the homeownership gap into predicted and residual components. The controls used are grouped into broad categories: income and wealth includes permanent and household earnings, realized earnings shocks, DTI, and wealth; risk includes permanent and temporary risk; age is defined by age groups; education has maximum education which can be high school graduates or at least some college; employment has degree of self-employment, state unemployment rate, and whether the wife is at least half-time; family structure includes marital status and the number of children in the household; finally, geography contain the total number of times moved while the individual was in the survey (relative to the number of years they were in the survey), whether residence is in their state of birth, region of birth, whether they live in an urban or rural area and if urban (and how urban it is), Census division, and house price index from Freddie Mac.

3.5.1 The bubble, 1999 to 2005

The top panel of Table 3.3 provides the decomposition for the beginning and end of the bubble years, along with the difference in each component. One can see that for both 1999 and 2005, most of the gap in homeownership is residual, but the majority of the change in that gap is due to predicted factors—the total homeownership gap went up by over 2.5 percent and a little more than 1.5 percent of that is due to observable controls. What is perhaps surprising is that the change in the gap is positive: during the expansion of the housing bubble, homeownership rates diverged.

The factors that stand out in increasing the predicted gap have changes that are primarily due to changes in observed quantities. Columns will not add up to the totals (overall, quantity, and price) at the top of this panel since not all factors from the decomposition are listed. Similarly, the overall change due to each factor will not necessarily equal the sum of its quantity and price effects because the contribution due the the interaction between simultaneous changes in quantities and prices is not listed.

In addition to explaining some of the within-group changes in homeownership, the largest contributor to the divergence in homeownership over the bust is family structure, marital status and fertility. This is a quantity effect, so the relative shares of those single and divorced/separated, and number of children in the household saw earlier increased for non-whites. The only other factor that acted to increase the gap was age, but this is not a large amount.

The remaining observables worked in the opposite direction, but not enough to offset the increase. Their changes are mostly due to changes in prices, or their return. Among the larger contributors are earnings risk and income and wealth. The positive quantity effects for these variables show that they increased homeownership for whites relative to non-whites. However, the negative price effects suggest that lenders became less sensitive to earnings risk, income, and wealth for non-whites, meaning that they were more willing to grant mortgages to borrowers who could be considered high-risk. This is consistent with what has been observed in the mortgage market during the boom, particularly the subprime portion. Some explanations could be a decrease in discrimination in lending[49] or homeownership policies like the Community Reinvestment Act. The last contributor to the decrease is geography, mainly driven by, oddly perhaps, states in the Census-defined West-South-Central division (Arkansas, Louisiana, Oklahoma, and Texas).

While the change in the residual gap is less than the predicted gap, it is still sizable. Since it is a quantity effect, it is mostly the result of changes in residual positions between whites and non-whites which can be due to discrimination (within-group inequality actually

[49]This would be a decrease in discrimination based on these factors. The increase in the residual gap, because it was a quantity effect, suggests that unexplained discrimination may have risen.

led to a decrease in the residual gap).

3.5.2 The bust, 2005 to 2009

Table 3.4 provides analogous results for what I have defined as the beginning and end of the bust of the housing bubble. Following the crash in the housing market, the homeownership gap increased further, as seen in the top panel. The divergence is now larger than during the boom, which is likely to be expected. Surprisingly though, almost the entire divergence is due to predictable factors (even though a sizable within-year portion is still residual).

There are some other noteworthy differences. While family structure is still the main driver, it is smaller than during the boom, and a much larger set of factors contributed to the continued divergence. In fact, the only set of factors contributing to convergence is income and wealth, even though it rose for whites relative to non-whites. Given that the price effect is still negative, lenders still do not care as much about these factors. This could be the result of policies aimed to help distressed borrowers keep their homes, an example being the Home Affordable Modification Program.

Earnings risk matters more during this period, actually reversing its pattern from the previous period. Now it seems that lenders are less likely to extend mortgages to high-risk non-whites; the quantity effect is even smaller this time, indicating the their was no relative change in earnings risk between the groups.

Two other sets of factors contributed to the divergence. The first is education, which is almost entirely its return (i.e., there was no relative increase or decrease in educational attainment during this period). Perhaps non-whites are less informed about policies they can use or measures they can take to keep their homes if they are having trouble making payments. Second, the return to living in certain geographic areas rose. This was driven by a couple of the more rural counties and states in the West-South-Central Census division and now also West division (which can be pretty rural themselves: Arizona, Colorado, Idaho, New Mexico, Montana, Utah, Nevada, and Wyoming).

Finally, the overall change in the residual gap is small, and its source is within-group inequality rather than changes in uncontrolled for characteristics or discrimination.

3.6 Conclusion

Minorities were hit much harder by the collapse of the housing market. Here it was shown as a dramatic relative reduction in their rates of homeownership. Yet, perhaps surprisingly, most of the changes in the differences of homeownership are largely attributable to observable characteristics, notably family structure and geography (and also earnings risk): very little is due to the change in the residual component. In fact, it is during the housing boom that changes in the residual component of homeownership are larger.

These findings complement recent work on changes in the homeownership gap, particularly what Fesselmeyer et al. (2014) found for those at the median of the homeownership distribution during the bust. Future work could employ these quantile methods during the boom years and use the rich set of controls available in the PSID. Previous research that has modeled the joint homeownership-household formation decision has only shown this to be important, in terms of coverage and size. Yet as other work points out, homeownership could be endogenous in other ways. These would also be worth modeling and estimating.

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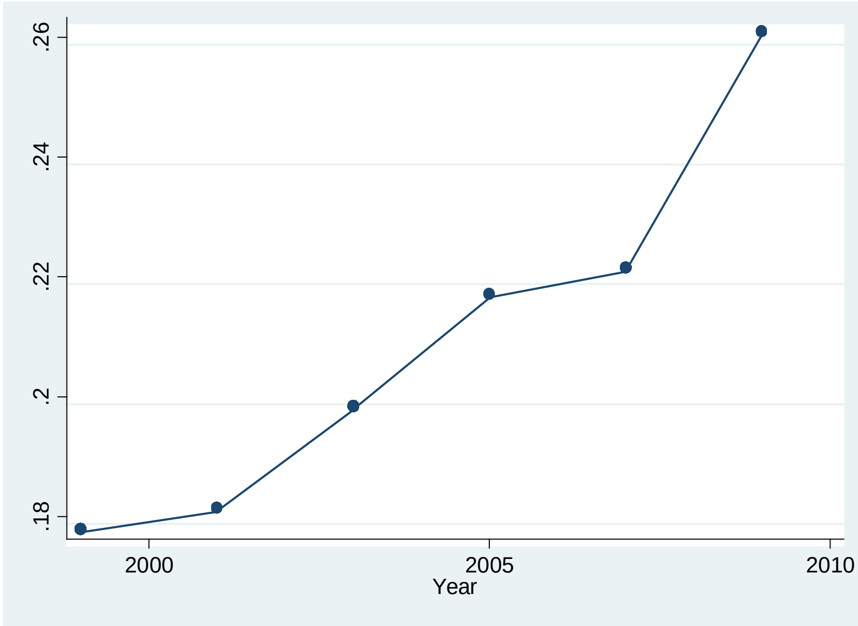

Figure 3.1: White/Non-White Homeownership Differential

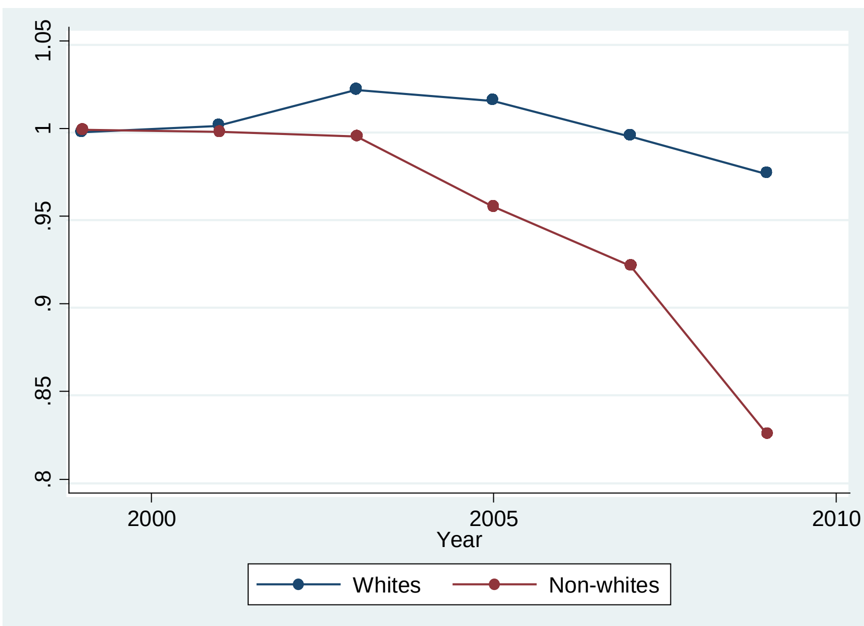

Figure 3.2: 1999-Indexed White and Non-White Homeownership Rates

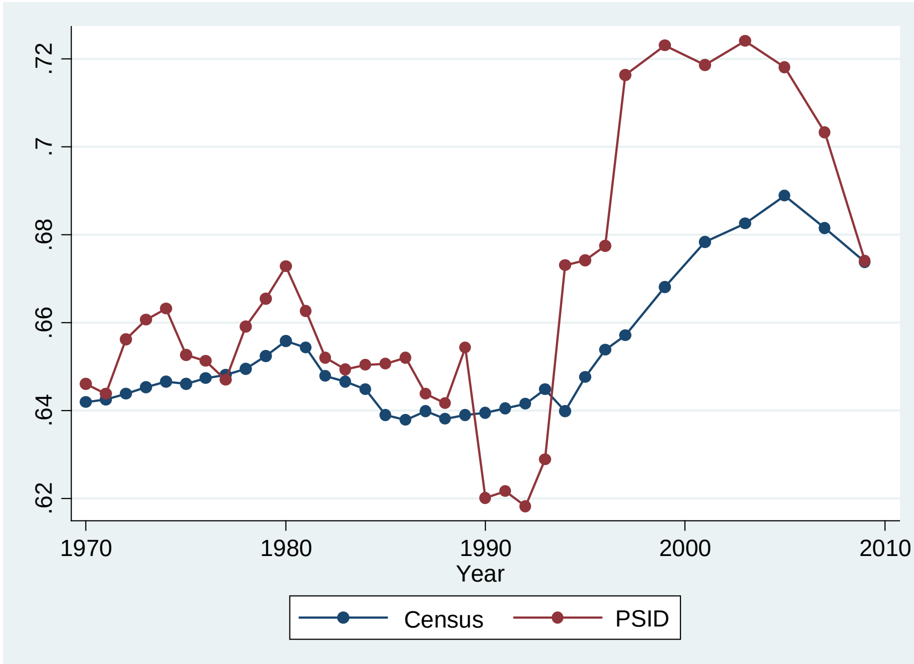

Figure 3.3: Aggregate Homeownership Rate, Census and PSID

Table 3.1: Summary Statistics - Boom, 1999 to 2005

	1999			*2005*			***Diff.-in-Diff.***
	W	*NW*	*W−NW*	*W*	*NW*	*W−NW*	*2005−1999*
Income & Wealth							
Individual Permanent earnings	10.69	10.76	-0.07	10.62	10.60	0.03	0.10
Household Earnings	$93,998	$63,895	$30,103	$105,252	$67,537	$37,715	$7,612
Debt-to-Income	0.59	0.71	-0.12	0.59	0.87	-0.28	-0.16
Net Wealth	$126,147	$50,806	$75,341	$117,335	$21,626	$95,709	$20,368
Earnings Risk							
$E(\gamma)$: Permanent Risk	0.010	0.007	0.003	0.008	0.006	0.002	-0.001
$E(\varphi)$: Temporary Risk	0.144	0.157	-0.013	0.174	0.189	-0.015	-0.003
Age	41.21	40.04	1.16	41.70	41.05	0.64	-0.52
Education							
Less than High School	6.85	16.49	-9.64	6.97	13.05	-6.08	3.56
High School	33.96	42.98	-9.02	32.45	44.49	-12.04	-3.02
More than High School	59.18	40.53	18.65	60.58	42.46	18.12	-0.53
Employment							
Fully Self-Employed	13.62	6.08	7.54	15.17	10.13	5.04	-2.50
Wife Employed at least Half-time	57.51	55.54	1.97	57.02	51.75	5.27	3.30
State Unemployment Rate	4.31	4.58	-0.27	5.56	5.64	-0.08	0.19
Family Structure							
Number of Children in Household	1.03	1.34	-0.31	0.95	1.10	-0.15	0.16
Married	79.26	70.17	9.09	78.29	64.26	14.03	4.94
Single	9.86	16.88	-7.02	11.73	21.80	-10.07	-3.05
Divorced or Separated	10.44	12.17	-1.73	9.59	13.29	-3.70	-1.97
Geography							
Total Times Moved (rel. to time in data)	0.42	0.48	-0.06	0.72	0.77	-0.05	0.01
Metro Area (pop. of 1 mil.+)	37.11	51.13	-14.02	37.03	50.57	-13.54	0.48

Sample is for men, ages 24-62, not disabled, and, except employment level, report positive earnings and a minimum of 52 annual hours of work. Data is from the Panel Study of Income Dynamics (PSID). Dollar amounts in 2011 dollars. Summary statistics for selected covariates and covariate categories.

Table 3.2: Summary Statistics - Bust, 2005 to 2009

	2005			**2009**			**Diff.-in-Diff.**
	W	*NW*	*W−NW*	*W*	*NW*	*W−NW*	*2009−2005*
Income & Wealth							
Individual Permanent earnings	10.62	10.60	0.03	10.59	10.54	0.05	0.02
Household Earnings	$105,252	$67,537	$37,715	$107,817	$65,168	$42,649	$4,934
Debt-to-Income	0.59	0.87	-0.28	0.77	1.12	-0.35	-0.07
Net Wealth	$117,335	$21,626	$95,709	$99,605	$31,674	$67,931	-$27,778
Earnings Risk							
$E(\gamma)$: Permanent Risk	0.008	0.006	0.002	0.006	0.004	0.003	0.001
$E(\varphi)$: Temporary Risk	0.174	0.189	-0.015	0.201	0.224	-0.022	-0.007
Age	41.70	41.05	0.64	41.83	40.84	0.99	0.35
Education							
Less than High School	6.97	13.05	-6.08	6.09	12.62	-6.53	-0.45
High School	32.45	44.49	-12.04	32.26	44.54	-12.28	-0.24
More than High School	60.58	42.46	18.12	61.65	42.84	18.81	0.69
Employment							
Fully Self-Employed	15.17	10.13	5.04	15.01	10.17	4.84	-0.20
Wife Employed at least Half-time	57.02	51.75	5.27	56.98	48.85	8.13	2.86
State Unemployment Rate	5.56	5.64	-0.08	5.64	5.87	-0.24	-0.16
Family Structure							
Number of Children in Household	0.95	1.10	0.15	0.98	0.99	0.02	0.13
Married	78.29	64.26	14.03	76.35	58.80	17.55	3.52
Single	11.73	21.80	-10.07	13.92	27.62	-13.70	-3.63
Divorced or Separated	9.59	13.29	-3.70	9.03	12.85	-3.82	-0.12
Geography							
Total Times Moved (rel. to time in data)	0.72	0.77	-0.05	1.04	1.13	-0.09	-0.04
Metro Area (pop. of 1 mil.+)	37.03	50.57	-13.54	37.08	48.78	-11.70	1.84

Sample is for men, ages 24-62, not disabled, and, except employment level, report positive earnings and a minimum of 52 annual hours of work. Data is from the Panel Study of Income Dynamics (PSID). Dollar amounts in 2011 dollars. Summary statistics for selected covariates and covariate categories.

Table 3.3: Changes in Differences in the Homeownership Gap - Boom, 1999 to 2005

	Decomposition of Differentials		
Year	*Total*	*Predicted*	*Residual*
1999	17.28	7.81	9.46
2005	19.85	9.33	10.52
Change in Total	2.58	1.52	1.06
	Change in Predicted		
	Overall	*Quantity*	*Price*
	1.52	1.92	-1.48
Income & Wealth	-0.66	0.63	-1.07
Earnings Risk	-0.34	0.12	-0.45
Age	0.39	0.41	0.04
Education	-0.53	-0.40	-0.41
Employment	-0.26	-0.11	-0.49
Family Structure	3.33	2.00	0.90
Geography	-0.85	-0.63	-0.92
	Change in Residual		
	Overall	*Quantity*	*Price*
	1.06	1.56	-0.30

Results are for non-disabled men, ages 24-62, who report positive earnings and at least 52 annual hours of work. Differentials and changes in differentials are all multiplied by 100. The interaction between simultaneous changes in prices and quantities is omitted.

Table 3.4: Changes in Differences in the Homeownership Gap - Bust, 2005 to 2009

		Decomposition of Differentials	
Year	*Total*	*Predicted*	*Residual*
2005	19.85	9.33	10.52
2009	22.86	11.85	11.00
Change in Total	3.00	2.52	0.48

		Change in Predicted	
	Overall	*Quantity*	*Price*
	2.52	1.35	1.72
Income & Wealth	-0.13	0.18	-0.20
Earnings Risk	0.50	0.02	0.38
Age	0.07	0.14	0.15
Education	0.36	0.01	0.35
Employment	0.12	0.42	0.04
Family Structure	0.97	0.59	0.22
Geography	0.58	-0.03	0.77

		Change in Residual	
	Overall	*Quantity*	*Price*
	0.48	0.09	0.32

Results are for non-disabled men, ages 24-62, who report positive earnings and at least 52 annual hours of work. Differentials and changes in differentials are all multiplied by 100. The interaction between simultaneous changes in prices and quantities is omitted.

Appendix A: Distribution of Statistics

Note that γ can be written as

$$\begin{aligned}
\gamma_{itkjq} &= \frac{1}{k}\left(\sum_{h=1}^{k}\eta_{i(t+h)} + v_{i(t+k)} - v_{it}\right)\left(\sum_{h=1-j}^{k+q}\eta_{i(t+h)} + v_{i(t+k+q)} - v_{i(t-j)}\right) \\
&= \frac{1}{k}\left(\sum_{h=1}^{k}\eta_{i(t+h)}^2 + 2\sum_{h=1}^{k}\sum_{r>h}\eta_{i(t+h)}\eta_{i(t+r)}\right) \\
&\quad + \frac{1}{k}\left(\sum_{h=1}^{k}\eta_{i(t+h)}\right)\left(v_{i(t+k+q)} - v_{i(t-j)} + \sum_{h=1-j}^{0}\eta_{i(t+h)} + \sum_{h=k+1}^{k+q}\eta_{i(t+h)}\right) \\
&\quad + \frac{1}{k}\left(v_{i(t+k)} - v_{it}\right)\left(v_{i(t+k+q)} - v_{i(t-j)} + \sum_{h=1-j}^{k+q}\eta_{i(t+h)}\right). \qquad (34)
\end{aligned}$$

Under the assumptions of the earnings model, the expected value of first term in (34) is $E(\frac{1}{k}\sum_{h=1}^{k}\eta_{i(t+h)}^2) = \frac{1}{k}\sum_{h=1}^{k}\sigma_{\eta i(t+h)}^2$, and every other term is the product of mean-zero, uncorrelated random variables and are thus themselves mean zero. Therefore, for all j and q, γ_{itkjq} is an unbiased estimator of the mean variance of permanent shocks facing person i between times t and $t+k$. However, since γ_{itkjq} is likely a very noisy estimator, we have proposed to estimate of $\sigma_{\eta it}^2$ by $E(\gamma_{it}|X)$ (see equations 15 and 8).

While one could in principle compute $E(\gamma_{ikjq}|X_{it})$ as fitted values from a regression of the various γ_{itkjq} statistics on X_{it}, this is not efficient because equation (34) also suggests the γ_{itkjq} statistics are heteroskedastic because (even for a given k) the right-hand side includes more terms when j and q larger (or perhaps because the distributions of the $\eta's$ and $v's$ may change over time). It would thus be more efficient to weight by the inverse variance of each when estimating regression (8).

We approximate those variances under the assumption that η and v are homoskedastic over time, i.e., $E(\eta_{it}^2) = \sigma_{\eta i}^2$ and $E(v_{it}^2) = \sigma_{vi}^2$; it would then follow that the variance of γ_{itkjq} is a quadratic function of $(j+q)$. For example, if (simply for the sake of specificity) we also assumed that shocks η and v were normally distributed, each term in (34) other than the first would be a product-normal random variable. If $G(\sigma_y^2, \sigma_z^2)$ denotes the cumulative distribution function for a random variable created by multiplying two mean-zero normally distributed random variables with variances σ_y^2 and σ_z^2, it follows that $k(k-1+j+q)$ of those remaining terms in (34) are distributed $\frac{1}{k}G(\sigma_{\eta i}^2, \sigma_{\eta i}^2)$, another $2(2k+j+q)$ terms are distributed $\frac{1}{k}G(\sigma_{vi}^2, \sigma_{\eta i}^2)$, and the last four are distributed $\frac{1}{k}G(\sigma_{vi}^2, \sigma_{vi}^2)$. Since each of those components is, for a given k, either a constant or linear in $(j+q)$, the variance of γ_{itkjq} is a quadratic function of $(j+q)$.

We thus use the following feasible implementation. First, each γ_{itkjq} is calculated and used to compute an unweighted estimate $\widehat{\gamma_{it}}$ as the fitted value from ordinary least squares estimation of the regression analogous to (8). The squared residuals $\varepsilon^2 \equiv \left(\gamma_{itkjq} - \widehat{\gamma_{it}}\right)^2$ are then regressed on a quadratic in $(j+q)$, a vector of year fixed effects, and those year fixed effects interacted with $(j+q)$. (The latter objects are included to relax the homoskedasticity assumption above.) The fitted values from that second regression $\left(E\left[\varepsilon^2 | j+q\right]\right)$ are then interpreted as estimates of the variance of the γ_{itkjq} statistics and used as the inverse weights to produce our WLS estimates.

Appendix B: Definition of Predicted Delinquency

The motivation for regressing the interest rate on predicted delinquency stems from the idea that there is an interest rate at which a mortgage loan is profitable. Therefore, represent a lender's loss function at origination as $G(h_0, h_1, ..., h_T)$ where h_t is the chance of delinquency at time t. From the Cox PH model, h_t is

$$h(t; X_0) = h_{og}(t)e^{X\beta}. \tag{35}$$

where $h(t;X)$ is the hazard function; $h_{og}(t)$ is the baseline hazard, common to all people in group g, which I have defined based on year-region of origin; and $e^{X\beta}$ is the individual hazard where X contains all the covariates listed earlier, including earnings risk.[50]

Log-linearizing G around $\bar{h}$ yields

$$ln(G|_{\bar{h}}) \approx ln\left[G_o(\bar{h})\right] + \sum_{t=0}^{T} \frac{\partial ln\left[G(\bar{h})\right]}{\partial h_t} \cdot \left[ln(h_t) - ln(\bar{h})\right] \tag{36}$$

$$= K + \sum_{t=0}^{T} k \cdot ln(h_t) \tag{37}$$

for $K \equiv ln\left[G_o(\bar{h})\right] - \sum_{t=0}^{T} \frac{\partial ln\left[G(\bar{h})\right]}{\partial h_t} \cdot ln(\bar{h})$, $k \equiv \sum_{t=0}^{T} \frac{\partial ln\left[G(\bar{h})\right]}{\partial h_t}$, and $ln(h_t) = ln(h_{og}) + X_0\beta$. Therefore, G is approximately equal to $K'_g + k' \cdot X_0\beta$ where $K'_g \equiv K + \sum_{t=0}^{T} k \cdot ln(h_{og})$ and $k' \equiv (T+1) \cdot k$.

The regression for the origination interest rate i_0, with predicted delinquency as a regressor, is then

$$i_0 = \pi_{og} + \delta_{race} + \kappa \cdot (X_0\hat{\beta}) + \varepsilon, \tag{38}$$

where π_{og} represents K'_g and is composed of the year-region of origin indicators interacted with whether the loan has more than 15 years left; δ_{race} are dummies for race or white/non-white; ε is the error; and $X_0\hat{\beta}$ is the linear prediction from the Cox PH model where the covariates were measured at origination. While the π_{og} depend on time, they are the same for everyone in state-region of origin g. The part of the loss function, and thus predicted delinquency, that lenders are most interested in then is $X_0\hat{\beta}$, which does not depend on time.[51] That is why interest lies in $\hat{\kappa}$.

[50]For the lender, the potential loss will depend on more than the sequence of $h's$. However, at least some of these factors are either directly controlled for in the interest rate regressions or indirectly in the delinquency regressions.

[51]Certainly some characteristics may change over time and the lender could attempt to forecast these changes at origination.

Appendix C: Discussion of Residual Decomposition

To determine positions within the residual distribution of person i who in group g during year t, assume

$$\tau_{igt} = F_t\left(\omega_t \cdot \varepsilon_{igt}\right). \tag{39}$$

The term τ_{igt} is their quantile in the residual distribution, F_t. Assuming F_t^{-1} is the inverse of the residual distribution allows one to recover the residual when the quantile is known:

$$\omega_t \cdot \varepsilon_{igt} = F_t^{-1}\left(\tau_{igt}\right). \tag{40}$$

These assumptions help in understanding the two residual terms in equation (31). The first, $(\Delta\varepsilon_2 - \Delta\varepsilon_1) \cdot \omega_1$ measures changes in residual quantiles by utilizing the white residual distribution from year 1, F_1^W and the non-white residuals—$\varepsilon_{i,NW,1}$—to find their quantile in the white residual distribution: $F_1^W\left(\varepsilon_{i,NW,1}\right) = \tau_{i,NW,1}^W$. This quantile for year t' is then used to find the hypothetical or imputed residual for year 1 and compare it with the actual: $F_1^{-1W}\left(\tau_{i,NW,2}^W\right) = \varepsilon_{i,NW,1}^W$ vs. $\varepsilon_{i,NW,1}$.

Lastly, the second residual term in equation (31), $\Delta\varepsilon_1 \cdot (\omega_2 - \omega_1)$, measures within-group inequality or changes in the residual distribution because it utilizes the white residual distribution from year 2, F_2^W, to get the quantiles: $F_2^W\left(\varepsilon_{i,NW,2}\right) = \tau_{i,NW,2}^W$. These quantiles are used to get the hypothetical or imputed residual for year 2 and compare it with the actual residual from year 2: $F_1^{-1W}\left(\tau_{i,NW,2}^W\right) = \varepsilon_{i,NW,1}^W$ vs. $\varepsilon_{i,NW,2}$.

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