Earnings Variability in the United States:
An Examination Using Matched-CPS Data

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The remarkable changes in the U.S. income distribution in recent decades have prompted a rapidly growing literature both documenting the phenomenon and investigating its causes. An important innovation in this literature is the decomposition of trends in cross-sectional earnings variance into components observed by the analyst--returns to education and experience--and components unobserved by the analyst--namely, the residuals of wage regressions. Juhn, Murphy, and Pierce (1993) demonstrate that, although total cross-sectional inequality of earnings rose steadily throughout the 1970s and 1980s, observed components and unobserved components followed significantly different trends. Returns to education and experience declined significantly in the 1970s, shrinking overall inequality, while unobserved components rose steadily throughout the period. On net, the unobserved component outweighed the observed components and earnings inequality rose. Both observed and unobserved components of earnings variance increased together throughout the 1980s.

This paper focuses on the trends in the unobserved components. We further decompose the unobserved components of income into "permanent" and "transitory" components. The first represents income variability due to unobserved individual effects that do not change over time, such as ability, while the second captures income variability arising from year-to-year fluctuations, sometimes called income instability. By decomposing these two factors, our study makes a contribution to the literature on income inequality and the recent literature that focuses on trends and recent developments in income instability, income mobility, and job instability.

Understanding the relative contribution of permanent and transitory components of earnings to the widening cross-sectional earnings distribution is important for a couple of reasons. First, the implications of growing inequality differ from implications of growing income instability for a host of economic decisions regarding consumption, family formation, human capital investments, savings behavior, and so forth. Second, the search for underlying causes will likely differ for contributions arising from permanent versus transitory components. For example, a rise in transitory variance may reflect changes in the returns (or losses) from job turnover or from changes in how firms smooth employee pay, while a rise in the permanent component may reflect changes in how the labor market rewards skills unobserved to data analysts.

Our motivation for this study is two-fold. First, to document basic trends in the variance

of the permanent and transitory components of income. Second, to isolate the major factors contributing to changing earnings instability. In particular, we look at changes over time in the variability of wages and labor supply; changes in the variance of wages for workers who remain in a given job or industry and workers who change jobs or industries; and changes across income, education, age, region, and industry groups. Our goal is to motivate future research on the economic consequences of earnings instability.

In this paper, we make use of March Current Population Survey (CPS) data. While designed primarily as a cross-sectional survey, the interview structure of the CPS permits matching of individuals across surveys in adjacent years. This yields a relatively large sample of two-year panels. A detailed examination of the matched CPS data is useful for several reasons. First, most of the analysis of the widening wage inequality has been carried out using the March CPS data; hence, our samples are directly comparable to the work of many previous researchers. Second, given the size of the matched CPS data, we can examine trends at a much more disaggregate level.

Our main findings are as follows.

(1) The transitory and permanent components of earnings variance follow significantly different trends over our 30-year sample period (1968-1997 CPS survey data). While earnings instability today is higher than the late 1960s and early 1970s, it has declined more or less steadily, aside from business-cycle effects, from a peak in the early 1980s. Permanent income variance, by contrast, rises steadily throughout our sample period.

(2) Transitory earnings variance changed little over our 30-year period for high school dropouts, young workers, and workers in the lower-fifth of the income distribution. Earnings instability has been and remains high for those individuals.

(3) Shifts in the age, education, and industrial composition of the workforce have dampened the rise in earnings instability; that is, if the demographic and industrial composition of the workforce today were the same as it was 25 to 30 years ago, earnings variability would be higher today than it is.

(4) Transitory earnings variance is very sensitive to the macro economic conditions as measured by the prime age male unemployment rate. The permanent component of income, by contrast, changes very little with unemployment conditions.

(5) From a distributional viewpoint, most of the change in earnings variance can be attributed to a rise in both large positive and large negative swings in income. The majority of earners see only small changes in year-to-year earnings fluctuations and the fraction of workers with relatively stable incomes has remained remarkably stable over time.

The paper unfolds as follows. Section II discusses the data. Section III discusses parameters of the earnings process that can be recovered from the matched Current Population Survey (CPS) and general trends in both the transitory and permanent components of earnings. In this section we also discuss the importance of job loss or job instability in explaining earnings instability. The paper concludes with a summary.

II. Matched CPS data

Some recent work decomposing cross-sectional earnings variance has been carried out on Panel Study of Income Dynamics (PSID) data (Gottschalk and Moffitt [1994]; Moffitt and Gottschalk [1995]; Haider [1997]). The PSID data is a useful choice for this type of analysis because of its broad coverage of the population and time periods. The PSID also provides long panels of earnings information which is useful for estimating stochastic models of the earnings process which involve persistence in shocks over time. A limitation of the PSID data is its small size which makes it difficult to analyze trends for disaggregate groups of workers. In addition, a number of social scientists have questioned its representativeness of the U.S. population because of its unusual sample design.

A. Basic CPS Data and the Matching Technique

The data for our study consists of 24 two-year panel data sets with approximately 175,000 total observations. The panels are constructed from the March annual demographic supplement to the CPS and span the 1968 through 1997 survey years. The earnings and weeks worked questions refer to the calendar year preceding the March interview. As a result, our sample measures labor market outcomes for the years 1967 through 1996. Throughout the paper we focus mainly on the natural logarithm of total wage and salary income, which are deflated by the Consumer Price Index. In order to focus on earnings from employment, this measure

excludes income from self-employment, though it is worth mentioning that its deletion makes little difference in the results presented in this paper.

For the purposes of our analysis, we selected samples that are representative of all workers with a reasonably strong labor force attachment. Our sample consists of civilian men between age 18 and 63 in the first year of each panel and must satisfy several sample-inclusion criteria (see Appendix A for details). First, the sample is limited to workers who were not in school, who were not primarily self-employed and who had positive weeks of work and positive earnings in both interview years. Second, observations with imputed earnings in either sample year were eliminated. Third, workers were eliminated from the sample if their earnings fell into either the top or bottom 1.5 percent of the earnings distribution in either survey year. This procedure essentially amounts to deleting from the top of the earnings distribution all observations with top-coded annual earnings in either survey year and amounts to deleting from the bottom of the distribution all observations with annual earnings below around $2,000 (real 1997 dollars).

The basic CPS survey is fielded monthly to households which are interviewed eight times over a sixteen-month period. The survey structure allows short panels to be created across interview months. More specifically, households are interviewed for four consecutive months, followed by an eight-month hiatus, after which they are again interviewed for four months. As a result, households on their first four-month stretch of interviews--about half the data in any given March--can be matched to their March interview in the following year.[1] The remainder of this section describes some basics of the matching procedure we employ and some caveats about the data. Appendix A covers the same material in greater detail and discusses the year-to-year comparability of the CPS surveys.

Matching individuals across surveys requires several steps. First, *households* must be matched. The CPS is a survey of residences, not households, and starting in 1968 the Census Bureau has maintained unique residence identifiers, which are common across interviews. Nevertheless, additional information is required to match households across interview months.

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[1] See Peracchi and Welch (1995) for an excellent discussion of the general matching procedure for the CPS data and the selection issues that arise in the process.

There are three primary reasons why a household may not be matched. First, the household identification code may not be consistently coded because of random transcription error at some stage between the interview and data entry. Second, households may refuse to continue to participate in the survey, or interviewers may not be able to locate the household. In particular, households that move residences are not followed in the CPS surveys. Third, in a number of survey years, 1993 for instance, the Census Bureau reduced the size of the sample by dropping households after their initial surveys.

Once a household is matched, individuals within the household must be matched. Individuals who move out of the household are not followed to their subsequent residences and cannot be matched. Similarly, individuals who move into the household will not have a prior interview. Up until 1994, individual characteristics such as age and sex must be used to uniquely identify individuals within the household. The percentage of matched individuals will depend on the number of variables used to construct the match. We adopt a stringent set of match criteria to guard against the possibility of spurious matches across years (see Appendix A for details). Starting in 1994, the CPS contains unique person identifiers that are common across surveys which can be used to guarantee accurate matching.

It is important to keep in mind that the matched-CPS samples are not random subsamples from the March surveys. In particular, individuals who move or change family characteristics are less likely to be matched. Our matched sample is somewhat older and more likely to be married--and hence, more stable--than those individuals we are unable to match. However, we find that the match rate shows little variability over time and there is no evidence of changes in match probabilities by age or marital status.[2] Since individuals who are matched experience less change than those who are not matched, our estimates of earnings variances are likely to be smaller than those for the population as a whole.

B. Sample Selection and Variable Creation

Three variables – weeks worked, job switching, and industry switching – which we use in

[2] We are able to match approximately 87 percent of households across a pair of years. Within these households, 93 percent of individuals can potentially be matched (based simply on the number of individuals in year one and year two households.) Of these we match approximately 87 percent.

our analysis require some discussion. Full details can be found in Appendix A. First, weeks worked in the previous year is reported as a categorical variable prior to 1976 and as a continuous variable thereafter. To be consistent we map the post-1976 data into the pre-1976 categories using a procedure devised by Finis Welsh.[3]

We construct measures of both industry switching and job switching. Unfortunately, job changing cannot be reliably ascertained in the March CPS before the 1976 interviews (1975 data). Starting in 1976, however, the CPS reports the number of primary employers an individual had over the previous calendar year. This information in conjunction with information on employment, unemployment, and layoffs, allows us to reliably determine whether an individual was employed by the same primary employer over the two year period.

Industry switching is based on three-digit industry measures. We are particularly conservative about its construction. Our procedure allows us to divide the data into stayer and switcher categories that are virtually free of measurement error. More specifically, we classify individuals as industry stayers, industry switchers, and "indeterminates." The indeterminate category contains observations on industry switchers, but where measurement error is a likely possibility. In our analysis below, earnings of indeterminates behave much like earnings of switchers, though standard test statistics reject the notion that the two are the same (see below).

III. Earnings Instability - Basic Trends

This section begins with a discussion of some basic models of earnings, how they relate to the commonly used measures of income instability, and what parameters can be identified from two-year panels. Section B describes out basic results. Sections C decomposes earnings trends into transitory and permanent components, and Section D looks at trends across various subgroups.

[3]Categorizing the post-1976 data in this way makes surprising little difference in the variance of weeks worked or in any of our analysis using average wages.

A. Basic model for earnings

As a starting point, this section begins with a basic statistical model of earnings. Let $ln\ E_{it}$ denote the natural logarithm of real earning for worker i in period t and let X_{it} denote characteristics of the individual that are observed by the analyst.

$$\ln E_{it} = X_{it}\beta_t + \varepsilon_{it} \tag{1}$$

$$\varepsilon_{it} = \mu_i + \nu_{it} \tag{2}$$

We allow the returns to the observed characteristics, β_t, to differ by year. The overall error term, ε_{it}, is assumed to consist of a permanent individual effect, μ_i – which may be arbitrarily correlated with X_{it}, and an idiosyncratic transitory effect, $\nu_{it,}$ which is assumed to be independent of μ_i. We assume that the individual effect, μ_i, is constant across a two-year period. The population variance of individual effects, σ^2_{ut}, is allowed to vary over time in order to pick up long-run changes in the underlying return to unobserved ability and/or changes in the distribution of μ in the labor force. We label σ^2_{ut} the "permanent" variance of earnings. The variance of the idiosyncratic component, σ^2_{vt}, is also allowed to vary by year. We label σ^2_{vt} the "transitory" variance of earnings.[4] The overall residual variance is the sum of the permanent and the transitory residual variances.

This simple error component model has been extensively tested in the literature against more general stochastic models [see Lillard and Willis (1978), Hause (1977, 1980), MaCurdy (1982), and Abowd and Card (1989), Moffitt and Gottschalk (1995)]. Our formulation assumes that there is no serial correlation in the transitory error component. This implies that the covariance of overall earnings residuals for the same worker is independent of the time period between these residuals. This property has been rejected using panel data in favor of more general models – typically estimated on PSID or NLS data – that have error structures that follow an AR(1) or an ARMA(1,1) process. Recent examples of such exercises based on PSID

[4]There are differences in the literature on what is included in the variance of permanent earnings. Moffitt and Gottschalk (1995) and Dynarski and Gruber (1997) include just the individual fixed effects. Haider (1997) includes the individual fixed effects as well as variations across years in the returns to education.

data are Moffitt and Gottschalk (1995) and Haider (1997).

The lack of long panels does not affect our results on the trends in the overall cross-sectional variance of earnings over time. The key issue is to clearly state what parameters we can consistently identify from the data. Our earnings process implies the following simple formulation for the variances of the transitory component of earnings.

$$\sigma_{\nu t}^2 + \sigma_{\nu t-1}^2 = E(\varepsilon_{it} - \varepsilon_{it-1})^2. \tag{3}$$

We estimate this by pooling the squared difference in the estimated overall earnings residuals for matched workers across years in our sample, and regressing these on a set of year indicators. We estimate the permanent variance as the difference between the estimated overall residual variance and the estimated transitory residual variance.

If the transitory earnings shocks follow an AR(1) process, then we must adjust our methods for identifying the permanent and transitory components of earnings residuals. In this case, the covariance of earnings residuals for a worker estimates the following.

$$\nu_{it} = \rho \nu_{it-1} + \eta_{it}. \tag{4}$$

$$E(\varepsilon_{it} \cdot \varepsilon_{it-1}) = \sigma_{\mu t}^2 + \frac{\rho \sigma_\eta^2}{1-\rho^2}, \tag{5}$$

where ρ is the autoregressive parameter and σ^2_η is the variance of the innovation to the transitory earnings shock, ν_{it}. The residual covariance overstates the permanent variance if ρ is positive. If ρ is constant over time – and the analysis of Moffitt and Gottschalk (1995) suggest that it was constant through the late 1980s – then the ratio of the true transitory variance to the estimated transitory variance is given by $1 / (1-\rho)$. We can use this relationship to adjust both the transitory and permanent variance estimates for autocorrelation.

More general statistical models of the earnings process are not likely to impact in a meaningful way the decomposition of the overall residual earnings variance. Haider (1997) estimates the transitory and permanent earnings variances implied from AR(1) and ARMA(1,1) models for the PSID over the period from 1967-1991 and finds only slight differences in the levels of the transitory and permanent components.

B. Trends in the overall residual earnings variance

In this section, we examine the patterns in earnings variability over the past thirty years. We begin with the variability of the estimated overall earnings residuals and then present a decomposition into their permanent and transitory components. We focus on the variability of residual earnings after accounting for year-specific returns to age, education and industry. Specifically, in each year we estimate a regression of log real earnings controlling for a quartic in age, four education groups (high school dropout, high school graduate, some college, and college graduate), six broad industry groups (agriculture, construction, manufacturing, retail trade, services, and government).

The pattern of overall residual earnings variability from 1967 to 1996 is given in Figure 1. The variance trended up from the late 1960's throughout the 1970's. The average for the period 1977-1981 was roughly 40 percent higher than for the period 1967-1971. The variance increased sharply from 1981 to 1982 in response to the economic recession. For the remainder of the 1980's, the variance slowly trended down with the expanding economy. The recession of the early 1990's again pushed the variance higher. From 1992 to 1996 the variance declined slightly following its usual pattern during an expansion. Over the past thirty years the residual earnings variance has risen roughly seventy percent.

An important issue is the degree to which the trends in the overall residual earnings variance reflect shifts in the underlying composition of workers over time versus changes in earnings variability within each sector. To address this we pooled the squared earnings residuals across years and regressed them on the same demographic and industry controls as above, and a set of year indicators. The regression results are presented in the first column of table 1. We use the coefficients on the year indicators to construct a residual variance series that holds the composition of the workforce constant. We adjusted the 1967 variances to be the same between the unadjusted series and the composition constant series. Figure 2 plots both the unadjusted residual variances and the composition constant residual variances.

Holding fixed the composition of workers makes little difference to the residual variance estimates up until the recession of the early 1980's. Over the 1980's and 1990's adjusting for composition shifts increases the implied residual earning variance by two to three percentage points. Looking over the thirty-year period, the unadjusted residual variance increased by

roughly 70 percent, while the composition constant residual variance increased by roughly 76 percent. The rise in the overall residual earnings variance over this period, then, is not due to workers shifting into historically high variance sectors.

The life-cycle profile for the overall residual earnings variance is explored in figure 3. This figure presents the implied residual variance from specification (1) of table 1 for a high school graduate working in construction in 1980. The residual variance declines sharply until a worker reaches his early thirties. The variance remains essentially unchanged over the next twenty years. By a worker's mid-fifties, the residual variance turns sharply higher until retirement. This U-shaped pattern is consistent with human capital models of life-cycle earnings and empirical findings by Hause (1980).

C. Trends in the Variance of Transitory and Permanent Earnings

In this section, we decompose the overall residual earnings variance into transitory and permanent components. Gottschalk and Moffitt (1994) report that in the PSID data the variances of these two components increased by roughly forty percent from 1970 to 1987. The average level of the variance of the transitory earnings component over this period in the PSID data was roughly half that of the permanent earnings component. An implication is that the growth in the variance of the transitory component of earnings explains roughly one-third of the overall increase in earnings variability. This points out the potentially important role played by earnings instability in the growing inequality exhibited by the U.S. earnings distribution.

An important question is whether the share of the increase in earnings variability due to the transitory component is robust across different data sets. We find qualitatively similar results to the PSID using the matched-CPS data. Figure 4 compares Dynarski and Gruber's (1997) transitory earnings variance estimates derived from the PSID to our matched-CPS estimates. There is a very close agreement in the two series until 1989 when the PSID estimates increase ahead of the CPS estimates.

Figure 5 presents the trends in the permanent and transitory components of the residual earnings variance from the matched-CPS data derived using the assumption that the transitory earnings shocks are serially uncorrelated. Table 2 gives averages by five year intervals, as well as decompositions by age, education, and industry. For the periods 1967 to 1976, the share of the

residual variance due to transitory component ranged from 27 to 30 percent in our matched-CPS data. Comparing the average for 1972-1976 to 1982-1986, we find that the transitory component increased 33 percent while the permanent component increased 40 percent. The matched-CPS data, then, gives broadly the same picture as the PSID data over this time period.

Looking over the full thirty-year time period, we find that the transitory variance increased by roughly 63 percent. The transitory variance reached its peak in the 1982-1986 period, and has trended down over the last ten years. In contrast, with the exception of the late 1980s the permanent variance has continued to rise over the entire thirty year period. Overall, the permanent variance has risen by 71 percent during the past thirty years. During the 1970s and early 1980s the increase in the overall residual earnings variance reflected increases in both the transitory and the permanent earnings variances. However, this pattern changed during the late 1980's. The transitory variance for the period 1987-1991 declined nearly 19 percent from its average over the earlier five year period. In contrast, the permanent variance declined by less than 3 percent between these same two periods. The transitory variance declined modestly over the recent expansion, while the permanent variance resumed its rise but at a much moderated pace.

The above results were derived using the assumption that the transitory earnings shocks facing workers are serially uncorrelated. This assumption has been empirically rejected in the PSID data, so it is important to ask how robust the findings are to relaxing this assumption. As discussed in the first subsection, we can not identify the degree of persistence in the transitory shocks in the matched-CPS data. However, we can take a parameter estimate derived from the PSID data and decompose the overall residual variance into permanent and transitory variances conditional on this parameter estimate. The simplest extension is to assume that the transitory shocks follow a AR(1) process. Moffitt and Gottschalk (1995, table 4 (I)) report an autoregressive parameter (ρ) estimate of 0.35 (with a standard error of 0.017) for this earnings model.

Figure 6 shows the impact of allowing the transitory earnings shocks to follow an AR(1) process with ρ set equal to 0.35. While this does not change the trend in the transitory variance over time, it raises the estimate of the share of the overall residual variance due to the transitory variance. For example, over the period from 1967-1971 the transitory variance made up roughly

28 percent of the overall residual variance under the assumption of no serial correlation. Assuming an AR(1) process with a ρ of 0.35, raises this share to around 43 percent. Persistence in the transitory earnings shocks raises the relative contribution of the transitory variance in explaining the growth in the overall residual variance from less than a third (no serial correlation) to over forty percent. The no serial correlation case, then, provides a likely lower bound on the true relative contribution of earnings instability to the trend growth in the overall residual wage variance.

Variations in levels and trends in transitory earnings variances by age, education, and industry are given in figures 7 - 9 and in Table 2. Transitory earnings variances decline quickly with age as illustrated in figure 7. The transitory earnings variance displays a U-shape over the life-cycle with a less pronounced upturn towards retirement as compared to the overall residual earnings variance. This is illustrated in figure 3 where we plot the age effects from a regression of the transitory earnings variance (calculated both assuming ρ=0 and ρ=0.35) on a quartic in age, education, industry and year effects.[5] The upward trend in transitory earnings variances was experienced by workers in all age groups.

When we group workers by their education level in figure 8, we find that transitory earnings variances decline with a worker's level of education, with most of the decline taking place if a worker is a high school graduate. However, this relationship did not exist in the late sixties and became prominent in the data by the recession of the early eighties. Workers across the education spectrum have experienced higher transitory earnings, with the largest relative increases taking place for high school dropouts.

Differences in transitory earnings variances by major industry are presented in figure 9. Construction displays the highest level of transitory earnings variances and is the most volatile over the cycle. The Construction and Trade industries experienced the largest relative increases in transitory variances over the thirty year period, while Manufacturing experienced the smallest relative increase.

Gottschalk and Moffitt (1994) examine the importance of composition effects in

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[5]The implied transitory earnings variances are for a high school graduate working in construction in 1980.

determining the trends in the transitory earnings variance in the PSID by controlling for age, education, industry, self-employment status, part-time status, and the unemployment rate. They find that the post-1978 increase in the transitory earnings variance falls from 0.044 (unadjusted) to 0.029 (adjusted). Compositional shifts explain only 25 percent of the change, leaving a significant increase in the 1980's in the "within" transitory earnings variance.

The overall importance of composition shifts in explaining the upward trend in the transitory variance in the matched-CPS data is illustrated in figure 10 which graphs the year effects from our composition regression (specification 2 of Table 2). As in the case for the overall residual variance, we find that if we held the composition of the labor force fixed at its 1967 employment shares, the transitory variance would have increased by a greater degree over our period of study (76% versus 63%). Most of the impact of compositional shifts occurred during the 1980s. The growing instability of earnings like the growing variability of earnings is not being driven by workers shifting from traditionally stable sectors to more variable sectors.

How is the distribution of the transitory shocks changing through the sample period? If the difference in the transitory shocks were normally distributed in all years, then the variance is a sufficient statistic to describe the changes. In fact, we reject with a p values less than .001 the normality assumption in all years using a Komologrov-Smirnoff test. Therefore, table 4 captures changes in the distribution by looking at the fraction of the changes that are above or below specified levels of the distribution of the difference in earnings shocks in 1967. Columns (1) and (2) of the table show that the a large fraction of the rising variance of transitory shocks is explained by an increase in both large positive and large negative changes. Column (3) demonstrates that the middle of the distribution has been relatively constant.

D. The variance of average weekly wages and average weeks worked

The underlying determinants of the rise in the residual earnings variance should depend in part on the extent to which the trends reflect growing variability of average wages as compared to growing variability of annual weeks of work. Declining unionization, movements toward more flexible compensation systems and incentive pay, increased usage of overtime, and declines in internal labor markets would lead to more variable average weekly wages. Increases in job instability would lead to more variable annual weeks of work.

To explore this we decompose the variance in the overall earnings residuals into the variance of residual average weekly wages, the variance of residual annual weeks worked, and their covariance. Since we examine the residual of annual earnings in our earlier analysis, we regress both the average weekly wage and the annual weeks worked on the same set of control variables – a quartic in age, four education groups, and six major industry groups. Separate regressions are estimated by year. We use the residuals from these regressions to estimate the variance of residual average weekly wages, the variance of residual annual weeks worked, and their covariance.

The changes over time in the variance of residual average weekly wages and residual annual weeks are presented in figure 11. There is some evidence that the variance of average weekly wages is procyclic. The dominant feature of the figure, though, is the pronounced upward trend in the wage variance over the period. Comparing the average over 1967-1971 to 1992-1996, the variance of average weekly wages grew by roughly 53 percent.

Turning to the variance in residual annual weeks worked, prior to 1983 the variance in residual weeks followed a similar trend to the variance in the residual weekly wage albeit with more cyclical variation. However, since 1983 the two series have moved in opposite directions. Comparing the average over 1967-1971 to 1992-1996, the variance of annual weeks worked grew by a modest 10 percent.

Most of the cyclical movements in the variance of residual earnings results from labor supply changes over the business cycle. In contrast to the average weekly wage findings, there is less evidence of a significant trend in the variance of residual weeks worked once the cyclical movements are removed. To check this, we regress the variance of the residual annual weeks worked on the unemployment rate, and a quadratic time trend. The linear and quadratic terms in the time trend are jointly insignificant.

We decompose the trends in the variance of the residual weekly wage into permanent and transitory components in figure 12. What is clear from this figure is that the rise in the residual weekly wage is being driven primarily by an increase in the permanent component. Over the entire sample period, the permanent component increased by 64 percent while the transitory component increased by 34 percent. From 1987 to present, the transitory component has actually fallen by 7 percent while the permanent component increased by 9 percent.

The important role played by the permanent variance component in explaining the rise in the overall variance of the weekly wage is consistent with the Juhn, Murphy, and Pierce (1993) hypothesis that there has been an increase in returns to quality in the labor force. To the extent that employers observe more quality attributes of workers than is revealed in our data, higher returns to quality would show up both as higher returns to observed quality characteristics such as years of schooling and work experience, and higher returns to unobserved quality characteristics that are captured in the individual fixed effects. This will result in an increase in the observed variance in the distribution of the permanent component of the variance of the weekly wage.

The fact that upward trend in the residual earnings variance reflects increased variability of weekly wages and not weeks worked is also consistent with the evidence suggesting that job stability has not changed significantly over this time period. Farber (1995) examines job tenure distributions from the CPS mobility supplements and finds that there has been little significant change over time. Allen *et al* (1998) find in a sample of 59 firms that average tenure rose during the 1990s. Diebold et al (1997) and Neumark et al (1998) look at retention probabilities implied by the CPS data and find little change in general, and some evidence of lower retention probabilities in the early 1990s for young and less educated workers. Farber (1996) uses the displaced worker surveys from the CPS to examine the rate of involuntary job displacement over time. For males as a whole, the displacement rate declined from in the 1980's and rose in the early 1990's. The increase in the 1990's was quite high for older workers (aged 45-64), and in general was higher than what would have been predicted by the relative tightness in the labor market at the time. There is some evidence, then, that job instability rose during the early 1990's for some groups of workers, but little evidence that job instability changed much over the 1980's.

One possible explanation for increased variability of average weekly wages is that firms have been moving to more flexible pay systems. An increased reliance on merit and incentive pay instead of across-the-board wage increases would show up as increased residual variance of average weekly wages. Groshen and Levine (1997) examine this question using salary survey data from a sample of large employers located across three Midwestern cities. While the survey data goes back to 1956, data on individual wages is available starting in 1980. Around 80 percent of firms responded that they implemented or strengthened their form of incentive pay over the

decade. Groshen and Levine find, though, that there is no trend in the within "job" variability of wages from 1980 to 1996.[6]

E. The importance of job loss in explaining the trends in transitory variances

Considerable attention has been given to the question of the extent to which the increase in transitory earnings variances can be attributed to job loss. While the incidence of job loss has been roughly constant (apart from business cycle swings) over the 1980's, the consequences of job loss may have changed. Gottschalk and Moffitt (1994) find that the transitory earnings variance is roughly three times higher for job changers in the PSID as compared to job stayers. In addition, the data indicate little evidence of a trend in the transitory earnings variance for job stayers up until the late 1970's. Starting in the early 1980s, the transitory variances for both job stayers and job changers increased. While the absolute increase was significantly higher for job changes, the relative increase was only slightly higher. Gottschalk and Moffitt emphasize the point that the transitory earnings variances increased even among workers who did not experience a job change in the 1980's, and suggest that focusing on the consequences of job displacement is not likely to yield a complete explanation for the changes occurring during the 1980's.

Stevens (1996) addresses the same issue also using the PSID data. Her analysis departs from Gottschalk and Moffitt in that she allows for the possibility that job displacement can result in higher transitory earnings variances not only in the year of the displacement, but for several years thereafter.[7] She considers a workers as "displaced" for ten years following the actual job displacement. She reports that the transitory earnings variances for displaced workers are roughly comparable to nondisplaced workers in the period prior to their displacement. Following displacement, their transitory earnings variances are more that double that of nondisplaced workers over the period 1967-1986, and roughly fifty percent higher over the period 1987-1991. However, the relative increase in transitory earnings variances between 1969-1977 and 1978-

[6]A job is defined as a detailed occupation by a specific employer.

[7]Stevens (1997) finds that workers experiencing an involuntary job loss are more likely in the next 4 to 5 years to experience additional job losses.

1986 is 34 percent for nondisplaced workers and 40 percent for displaced workers. There is clear evidence, then, that job displacement can have important consequences for variability of a worker's earnings, but that this variability was increasing even for workers in stable jobs.

Starting in 1976, we can determine in the CPS data if a worker remained with the same employer between interview years. Given our short panels, we are limited in our ability to look for long-term implications of involuntary job loss as in Stevens (1996).[8] Figure 13 shows the transitory earnings variances for workers who stayed with the same employer between March interviews, and workers who switched employers. As in the PSID, the level of the transitory earnings variance is significantly higher for job switchers. Job stayers experienced a larger relative increase in the transitory variance than job switchers over the period 1977-1981 to 1982-1986 (41 percent for stayers as compared to 26 percent for switchers). The transitory variance for both groups declined significantly between 1982-1986 and 1987-1991. Over the post 1976 period, the cumulative increase in the transitory variance for job stayers was only around 10 percent, while the cumulative increase for job changers was less than 5 percent.

In order to get a longer perspective, we have to switch to looking at workers who changed 3-digit industries (a subset of all job changers) as compared to workers who did not change industries (a mixture of job stayers and job changers who stayed in their same industry). We use the method described earlier to eliminate spurious industry changers.

The transitory earnings variances for industry stayers and industry changers over our thirty-year sample period is presented in figure 14. We also include the transitory variances for a group which we call "indeterminants." These are likely industry changers but likely include more measurement error in their industry coding. Over the entire 30-year period, the transitory variance increased by 48 percent for industry stayers and 81 percent for industry changers. Much of this increase was concentrated in the early 1970's, and both transitory variances have declined during the late 1980's and early 1990's. For the post 1976 period, we see that the industry decomposition tracks the employer decomposition fairly closely. Both the employer and industry figures cast some doubt on the hypothesis that involuntary job losses are the primary determinant

[8]In future work we will attempt to match our samples to the CPS Displaced Worker Surveys. This will allow us to look at job displacement effects over a five-year window in the 1980's, and over a three-year window for the early 1990's.

of the overall rise in transitory earnings variances.

IV. Summary and Conclusions

In summary the data support a large rise in the variance of transitory earnings shocks since the late 1960s. In the past 10 years, however, the variance has fallen from its peak in the early 1980s and the variance today is roughly equal to the variance 20 years ago. Interestingly, the permanent component of earnings variance has risen almost monotonically over the past 30 years. The rise in earnings instability is not the consequence of workers shifting into more variable sectors of the economy. The cyclical variability of earnings instability arises from the cyclical variability of weeks worked. The trend in earnings instability, then, primarily reflects the trend increase in the residual variance of average weekly wages. Earnings instability is significantly higher for workers who change employers especially if this entails a change of industry. However, the workers who remained with the same employer experienced a larger relative increase in earnings instability over the post-1976 period.

While the implications of rising earnings inequality have been explored, the consequences of increased income variability are less well understood. Dynarksi & Gruber (1997) look at the important question of a household's ability to smooth consumption given increased income variability. They find that an average head of household's income variation is partially offset by government taxes and transfers. A dollar change in income results in around a 10¢ change in nondurable consumption, and a 17¢ change in durable consumption. It is likely, though, that impact of income variability is more severe for specific types of durables. In particular, growing income variability could have important consequences for a household's decision to make the transition from renting to owning. Given the high leverage and the relative size of the mortgage payments to income, households facing a more erratic income stream may elect to defer the purchase decision (or tradeup decision) until they accumulate additional savings to act as a buffer. Other consequences may be an increase in the household's demand for and use of credit, as well as an increase in likelihood that the household becomes delinquent on outstanding lines of credit. Dulitzky (1997) examines the implications for the design of pension plans, in particular the impact of the length of the window used to average income for the purpose of setting the retirement benefit.

Should we care that the transitory variance has risen? Does the rise have any economic significance? This question cannot be answered in this paper nor has it been fully addressed in the literature. We intend to take up this question in future work by studying the consequences of earnings instability.

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Appendix A: CPS Data

Description of the CPS Samples and Sample Inclusion Criteria

The data for our study consist of 24 two-year panel data sets with approximately 175,000 total matched-pairs of observations. The panels are constructed from the March annual demographic supplement to the Current Population Survey (CPS) and span the 1968 through 1997 survey years. The earnings and weeks worked questions refer to the calendar year preceding the March interview. As a result, our sample measures labor market outcomes for the years 1967 through 1996.

Our sample consists of civilian men between age 18 and 63 in the first year of each panel and must satisfy several sample-inclusion criteria. First, the sample is limited to workers who were not in school (about 6% of the data in all years), and who were not primarily self-employed and had positive weeks of work and positive earnings in both interview years. These workers compose 17% of the data in 1968 and 23% in 1997. Workers who are out of the labor force and have no earnings account for most of the rise. Second, observations with imputed earnings in either sample year were eliminated. This amounts to another 20-30% in each year (imputation and potential biases from deleting imputed observations are discussed in more detail below). Third, workers were eliminated from the sample if their earnings fell into either the top 1.5% or bottom 1.5% of the earnings distribution in either survey year. As this restriction must hold in both years, this restriction eliminates another 3.5 to 4% of the data from each panel. This procedure essentially amounts to deleting from the top of the earnings distribution all observations with top-coded annual earnings in either survey year and deleting from the bottom of the distribution all observations with annual earnings below around $2,000 (real 1995 dollars).

Matching Cross-Sections into Panels

Although CPS data is typically analyzed as a cross-section, the basic CPS survey is fielded monthly to households which are interviewed eight times over a sixteen-month period. The survey structure allows short panels to be created across interview months. More specifically, households are interviewed for four consecutive months, followed by an eight-month hiatus, after which they are again interviewed for four months. Thus, a household interviewed in March is reinterviewed in the

following March. As a result, households on their first four-month stretch of interviews--about half the data in any given March--can be matched to their interviews in the following year. See Peracchi and Welch (1995) for an excellent discussion of the general matching procedure and the selection issues arising from matching March CPS data for a subset of the years we study in this paper. The remainder of this section describes some basics of the match procedure we employ and some caveats about the data.

Until 1994, the Census did not provide a unique person identifier within the household. They did provide individuals with a line number which can be used as a first attempt to match individuals across years. However, changes in household composition would change individual line numbers, resulting in mismatches. Therefore we matched individuals on their line number, age, sex, and race. In cases where individuals could not be matched on their line number, we required them to match on age, sex, race, family relationship, and education. Individuals were allowed to age by zero, one, or two years and their education was allowed to increase by zero or one year. Beginning in 1994, the line number provides virtually a unique identifier for individuals who are in the household in both years, and most individuals are matched using the line number, age, sex, and race.

In some cases, we would get more multiple individuals from one year matched with a single person from another year. We used variables such as marital status, education, family relationship, and veterans status to break these "ties". These cases consisted of only a very small portion of our data.

Lack of Common Censes

Several years posed special difficulties. Some pairs of years could not be matched at all due to changes in the CPS survey design (mostly due to updates from the Census). These years include 1971/1972, 1972/1973, 1985/1986 and 1995/1996. Furthermore, there were no line numbers for the pairs of years 1975/1976, 1977/1978, and 1978/1979. As a result, individuals were matched on age, sex, and race only (with tie breakers as described above). This would be expected to create a slightly higher rate of mismatch than requiring matches on line number as well. So changes in wages and labor force participation across years for individuals in these years should be taken as an upper bound.

From the above discussion, it is clear that the probability an individual will be matched is

related to individual characteristics. In particular, individuals who move and change family characteristics are less likely to be matched. Our matched sample is somewhat older and more likely to be married than those individuals we are unable to match. However this bias does not change over time. Since the individuals who are matched experience less change than those who do not, our estimates of changes in earnings for a given individual across the two years are likely to be smaller than those for the population as a whole. It is unclear how this will affect the trend.

Overall, our match rates are remarkably constant over time. We are able to match approximately 87 percent of households across a pair of years.[9] Within these households, 93 percent of individuals can potentially be matched (based simply on the number of individuals in year one and year two households.) Of these we match approximately 87 percent. This is true even in the years that are somewhat different--for instance during the mid-1970s when there are no line numbers and post-1994 when the line numbers become a sufficient variable for matching. The resulting data sets (which include males and females of all ages) are of fairly comparable size. Ten of the 24 data sets include between 53,000 and 57,000 individuals, while 17 include between 50,000 and 60,000 observations. Notable exceptions are the 1973/1974 and 1975/1976 data sets, each containing approximately 33,000 individuals, as a result of low household match rates, and the 1994/1995 and 1996/1997 matches, each containing approximately 46,000 observations, as a result of a cut in the sample size of the CPS. Even in these years, the low sample sizes are not the result of changes in the proportion of potential individuals matched and the samples do not appear to be biased in any unusual way.

Since we are interested in analyzing changes in earnings variability over time, it is also important to understand how changes in the CPS questionnaire might affect our estimates. The CPS survey instrument was relatively stable over the entire sample period until 1994, when it went through a major revision [see Polivka and Rothgeb (1993), Polikva and Miller (1995), and Polivka (1996)]. The 1994 revisions involved both rewording of questions and changes in the method for collecting and recording the data. An important element of the redesign was a move to data collection using laptop computers. This is expected to improve CPS data accuracy in several ways

[9] There are two exceptions. In 1973/1974 and 1975/1976 we are able to match only 58 percent of the households. Discussions with numerous individuals at the Census have not produced an explanation.

including directly filling in all information that is unchanging between surveys, and carrying out a variety of edit checks. In addition, the ability to match workers across surveys was simplified. These factors should reduce the relative earnings variance in the post 1994 period.

Dealing with Census Imputations

The CPS uses a hot-deck method to impute missing responses to variables. Imputations induce additional measurement error when individuals are matched across years. In particular, the use of imputed data will create a downward bias in the estimated covariance of wages across two years resulting in an overestimate of the transitory component of earnings. To avoid these problems we delete all individuals with imputed wage and salary income.

There was a change in the CPS processing of imputed values in 1989. Prior to 1989, there are flags identifying whether an individual has imputed wage and salary income.[10] Individuals receive this individual item flag whether wage and salary income was the only value that had to be imputed or whether wage and salary income was imputed as part of an entire group of variables requiring imputation.[11] Beginning in 1989 the Census used a new system to process the data. Individuals requiring only a few imputed values still receive item specific allocation flags. Again, we deleted all individuals with flags indicating that their wage and salary income was imputed. However, starting in 1989 individuals for whom many items were imputed as a group did not receive item specific imputation flags, rather they received a single flag marking them as receiving numerous imputations.[12] Therefore we also deleted these people from our sample. It is theoretically possible that in doing so, we are actually deleting people who have valid income data and who would not have been deleted under the old system of identifying imputations. However, in practice,

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[10] Prior to 1975, the wage and salary income allocation flag was defined at the family level. Therefore it is possible that in these years, we delete some individuals who did not actually have imputed values for their own income.

[11] For more information on the imputation process see Lillard, Smith, and Welch, 1986 and the CPS documentation.

[12] At this time, there was also a change in the way the imputations were made, but since we delete individuals with imputed income values it is not necessary to discuss that here. See the CPS documentation for more information.

since the Census replaces valid data with imputed data for those individuals missing many variables, it is likely that under the old processing system the vast majority of these individuals also would have had their income data imputed and received an individual item flag indicating an imputed income value.

An important issue is how the deletion of imputed data may bias the sample composition. At least some of the data in the survey must be imputed as a result of more or less random error. Before 1994, the basic CPS questionnaire and the March supplement questionnaire were completed and processed as separate documents. Failure of the interviewer to properly transcribe an individual's case identifier on either survey prevented proper merging of the data. In this case, these observations were considered missing and imputed using the techniques discussed above.

Although there is probably not any change in our sample resulting from the change in the way the imputations are flagged, increases in the non-response rate may have an effect on our outcomes. As noted by Lillard, Smith, and Welch (1986), the incidence of non-reporting of income has increased over time and it is positively correlated with income. One might also suspect that individuals with more variable income are less likely to report their income, because it is harder to determine the amount. In this case, as the proportion of non-respondents increases over-time, we may increasingly under-estimate the changes in wages across years for an individual.

To investigate this issue we pool together all observations with at least one non-missing earnings value. Workers with missing earnings for both years are still deleted. We code an indicator variable which takes a value of zero for workers with non-missing earnings in both years (our base sample), and which takes a value of one for a workers with missing earnings in one year. We regress the squared residuals from our earnings model on the workers age, education, industry, year effects and the missing earnings indicator. The data indicate that all else constant workers with missing earnings in one year have an overall earnings variance in the other year that is 0.047 higher than similar workers with no missing earnings. Given the rise in non-reporting observed in our sample (from 18% to 34%), correcting for this difference would slightly increase the trend increase in the overall residual earnings variance.

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Measurement Error Issues

An important issue in interpreting earnings variability estimates is the problem of measurement error. Bound & Krueger (1991) estimate that measurement error accounts for a sizeable fraction of reported earnings changes. Murphy and Topel (1986) argue for a much smaller role of measurement error in earnings changes. The nature of the measurement error process is important for determining where it will show up in our decomposition. For example, if the measurement error is largely uncorrelated over time, then it will be picked up in our measure of the variance of transitory income. The focus of this paper, though, is on the trends over time and not the level of earnings variance at any point in time. Earlier studies using the PSID data have argued that there is no reason to believe that the importance of measurement error has increased over time. In fact, improved survey methods and training may lead to less measurement error. This is an issue we currently have under study.

Variable Creation

In addition to wage and salary income, several other variables used in our analysis require some discussion. In the raw data, weeks worked in the previous year is reported as a categorical variable prior to 1976 and a continuous variable thereafter. To be consistent we map the post-1976 data into the pre-1976 categories. This makes surprising little difference on the variance of weeks worked. Because there is no imputation flag for weeks worked prior to 1989, we are unable to delete individuals with imputed values. However, this should not have a significant effect on the trend. We create our own average weekly wage last year variable, by using the recoded annual weeks worked variable and annual wage and salary income.

Our measure of industry change relies on the structure of the matched CPS survey.[13] For this analysis, we use the three digit industry variable, recoded to be constant over time. First, we attempt to eliminate as much measurement error as possible from the industry coding. Call the first year of the matched survey time t — t refers to the year in which the question is asked, not the time period to which the question refers. The variable ind_t refers to the individual's industry during the survey reference week, which is in year t. However, the variable $indlyr_t$ which is also asked in year t, actually refers to the time period $t-1$. Since we use a matched sample, we have four variables on

[13] This method is described in Murphy and Topel (1987).

industry ind_t, $indlyr_t$, ind_{t+1} and $indlyr_{t+1}$.

In March of the first year, individuals are asked their industry in the reference week (ind_t). Then at the $t+1$ survey, individuals are asked their industry last year ($indlyr_{t+1}$). This variable refers to the calendar year t, which includes the time period covered by the variable ind_t. Therefore, we have two independent observations about a person's industry in March of year t. If ind_t does not equal $indlyr_{t+1}$ then the individual either switched jobs after March of year t or there is miscoding in one or both of the industry codes. However, we cannot distinguish the two. We categorize these individuals as indeterminate in our data. They make-up between 22 and 26 percent of our survey. We assume that those individuals for whom $indlyr_{t+1}$ is equal to ind_t have correctly identified their industry in March of year t. The remainder of the analysis on industry change is conducted on this subsample.

We now exploit the coding of the industry variables to identify industry changers. In the basic CPS, which is administered to survey participants first, individuals are asked to provide information on their industry during the reference week (ind_t). When the question of major industry in the previous year arises in the March supplement ($indlyr_t$), individuals are asked what was their longest job held in the prior year. If the interviewer believes this is the same job as the worker presently holds, the interviewer probes to verify whether this is true. If the jobs are the same, then the current industry coding (ind_t) is copied to the industry last year coding ($indlyr_t$). Therefore *if* ind_t *does not equal* $indlyr_t$, *it is because the person changed jobs* (and symmetrically for ind_{t+1} and $indlyr_{t+1}$). While it is still possible for a workers to change jobs, not change industries, and misreport his/her industry classification in a given year, this method will eliminate all industry changes for individuals who do not change jobs.

For our subsample we know the person's industry at time t. We can thus compare his industry at time t to his industry at time $t-1$ (using the $indlyr_t$ variable). Since this variables is hard coded to the variable ind_t used to determine the industry at time t, we can be sure that any changes in the industry code represent an actual industry change. There is some additional possibility of identifying false industry changes due to transcription errors, but it seems unlikely that this is large. Furthermore, it seems reasonable that the transcription error remained relatively constant until the 1994 survey when it was largely eliminated through the use of computers.

The last variable we create is one measuring employment stability. Our goal is to divide

individuals into two categories: stayers, who experience stable employment over the entire two year period, and movers, who experience a transition between employers or between employment and unemployment or out of the labor force. For this variable, we rely on the CPS variables concerning employment status during the reference week, the number of employers a person had during the previous year, the number of weeks worked during the previous year, the weeks of unemployment reported if the person is unemployed during the reference period ($wksunem_t$), and the number of spells of unemployment during the previous year. Since the number of employers variable is only available beginning in 1976, our job change variable begins in 1976. We define the period of employment transition over a 24 month period.

Specifically, individuals categorized as stayers are those who report having only a single employer in year *t-1* and year *t*, who work at least 39 weeks in both years[14], and who report either

1) zero weeks of unemployment at the year t survey and less than 12 weeks at the year t+1 survey, and no spells of unemployment in either year;

2) or who report that they are out of work for a reason that implies a temporary absence, such as short term lay-off or illness.

Individuals who report having more than one employer in either year, who reported working less than 39 weeks, who reported that they were expecting to start a new job, or who reported positive spells of unemployment (not including short-term lay-offs) were categorized as movers.

The question then arises as to who we miss by this measure. Individuals who remain with the same employer but witch jobs with no intervening spell of unemployment will be categorized as stayers. Similarly, workers who switch employers at the end of one year and start a job with a new employer during the second year, with no spell of unemployment would also be categorized as stayers, assuming that they report having only one employer in each year. In fact, anyone working more than 39 weeks in each year, with only one employer in each year, and who did not spend any time unemployed would be missed in this measure. However, it seems unlikely that many prime age

[14]At first, we divided the non-movers into two categories, those with at least 48 weeks of employment and those with between 39 and 47 weeks of employment (inclusive). The remaining criteria was to identify individuals such as teachers who work fewer weeks, but with no job change. This group looked very similar to the group with at least 48 weeks of employment, so they were combined.

men move directly between the out of the labor force state and employment. Furthermore, since this method of coding employment transitions is constant over time, it should not affect out trends.

Figure 1. Residual Earnings Variance

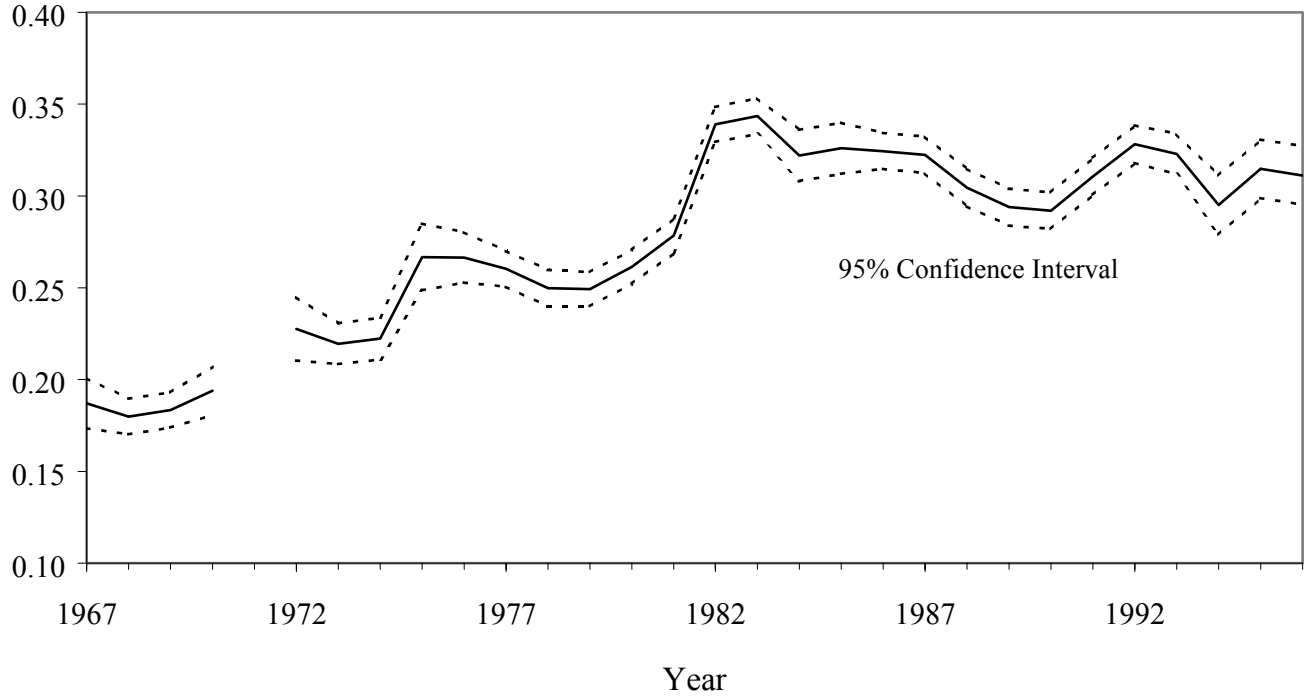

Figure 2. Residual Earnings Variances

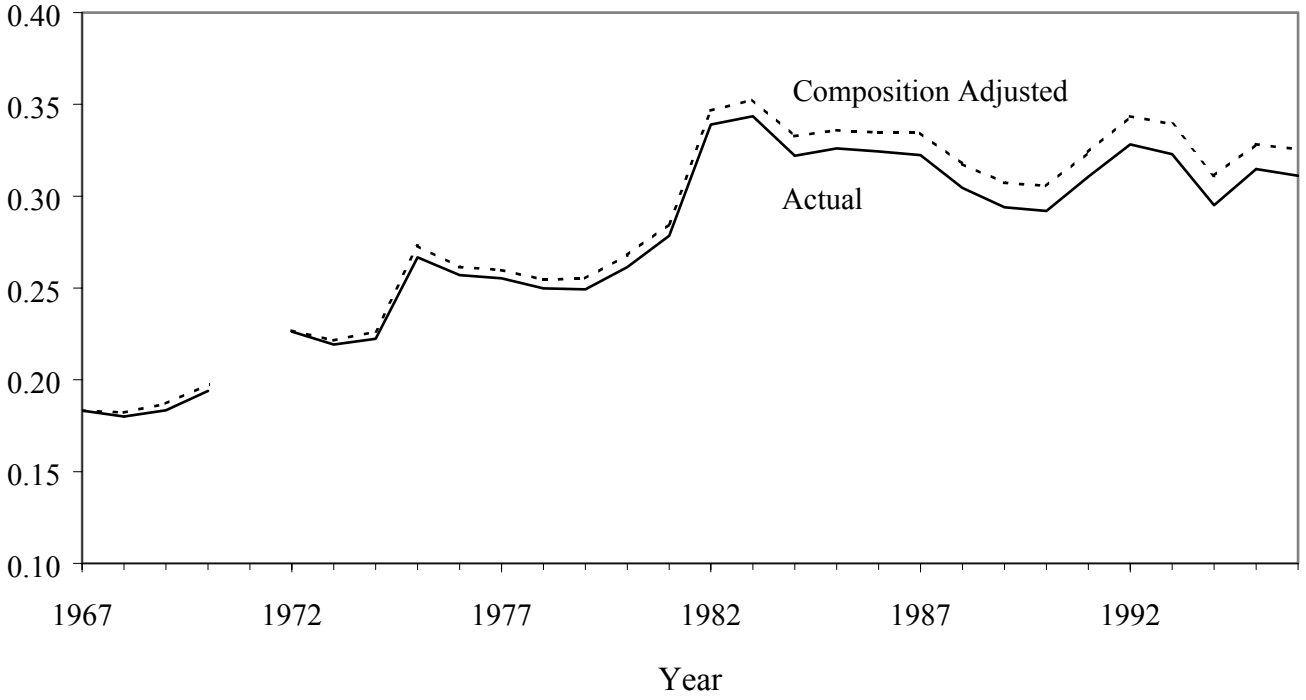

Figure 3. Age - Residual Variance Profiles

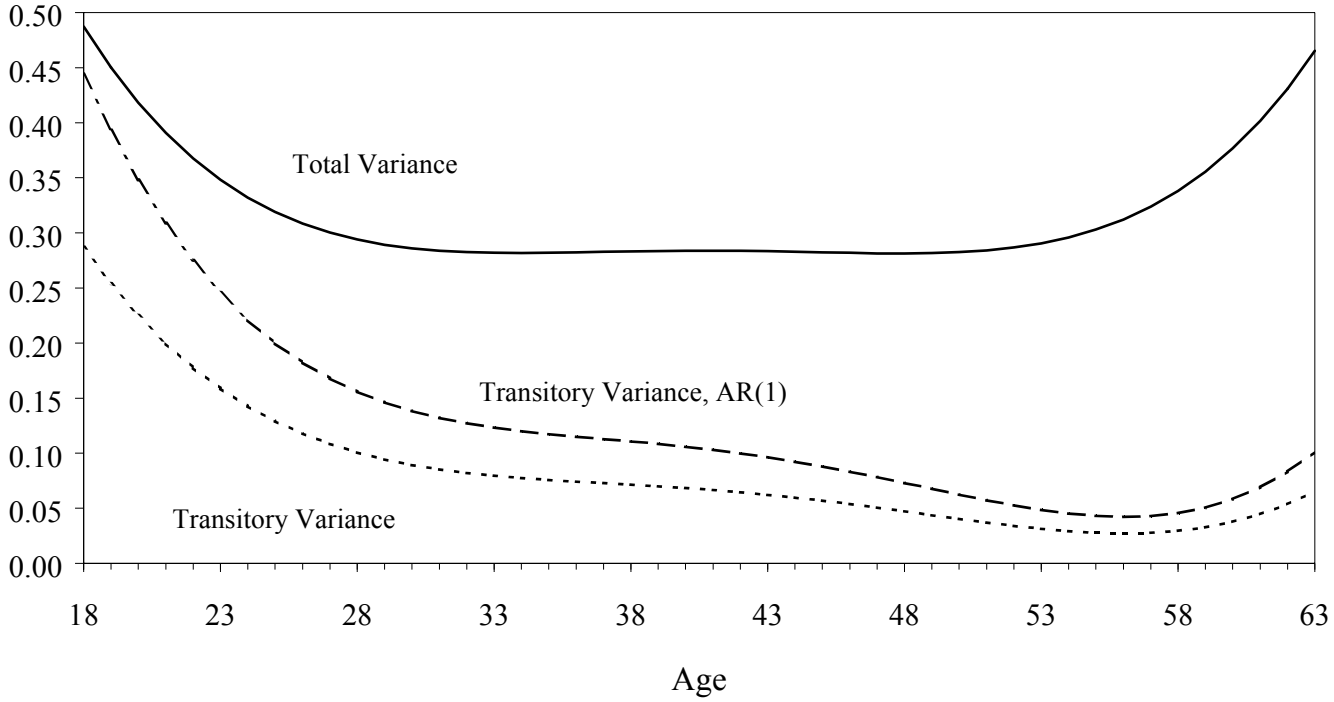

Figure 4. Transitory Variance

Matched CPS versus the PSID (from Dynarski and Gruber, 1997)

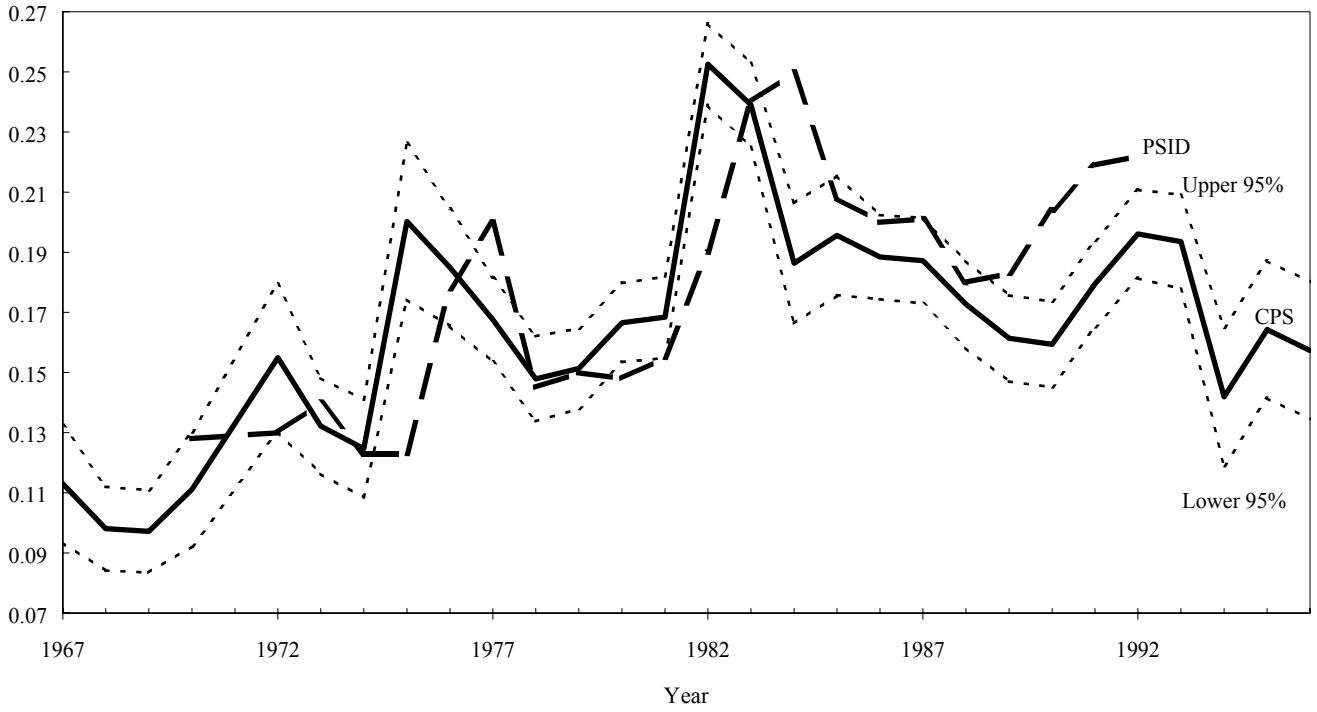

Figure 5. Variance Decomposition

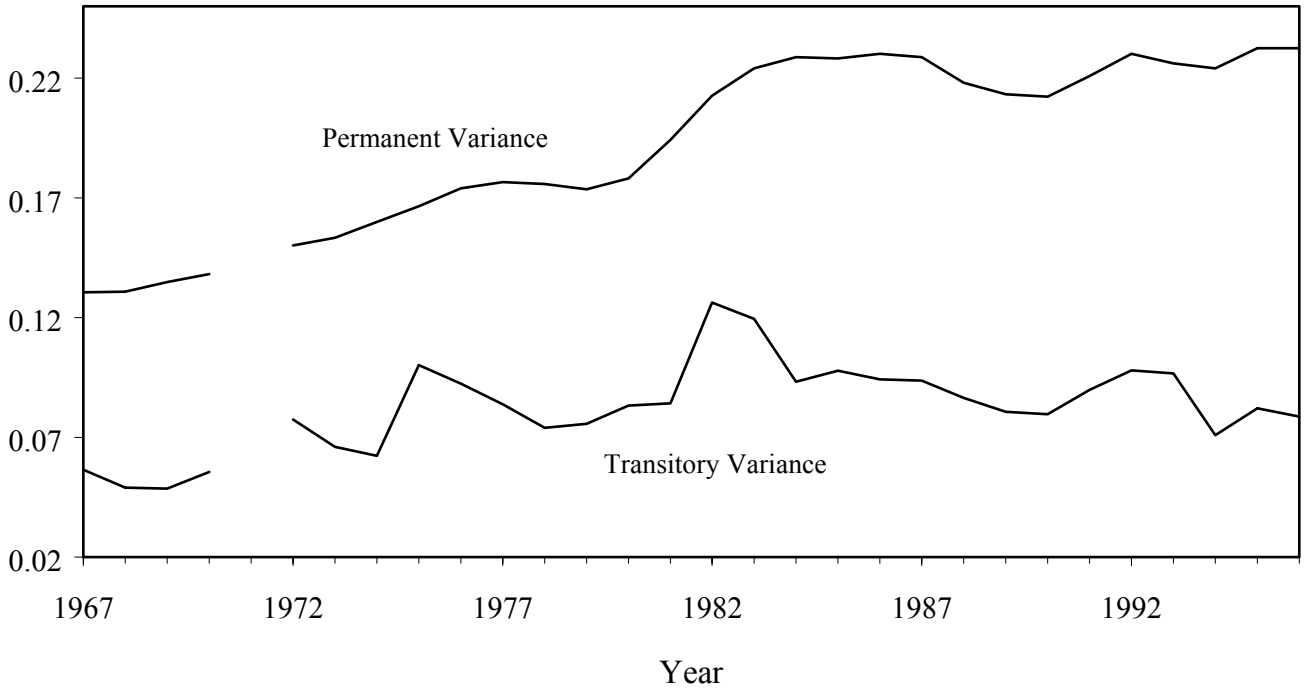

Figure 6. Impact of Serial Correlation

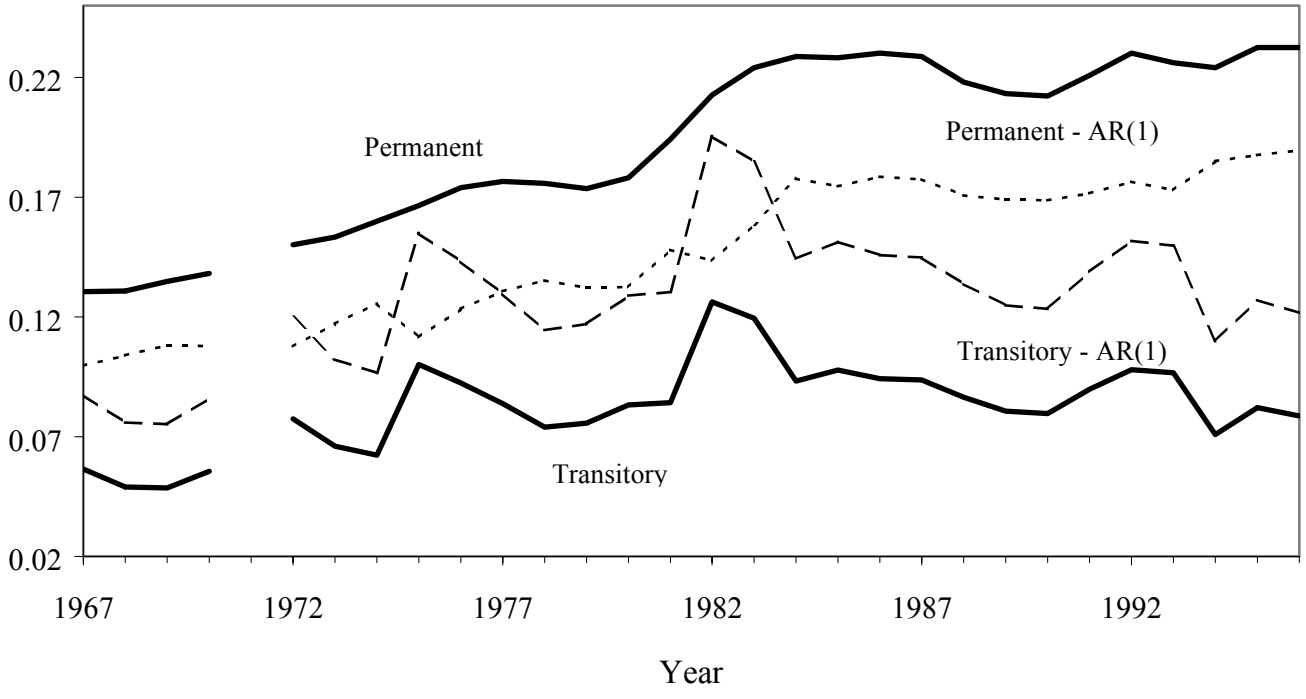

Figure 7. Transitory Earnings Variance

By Age

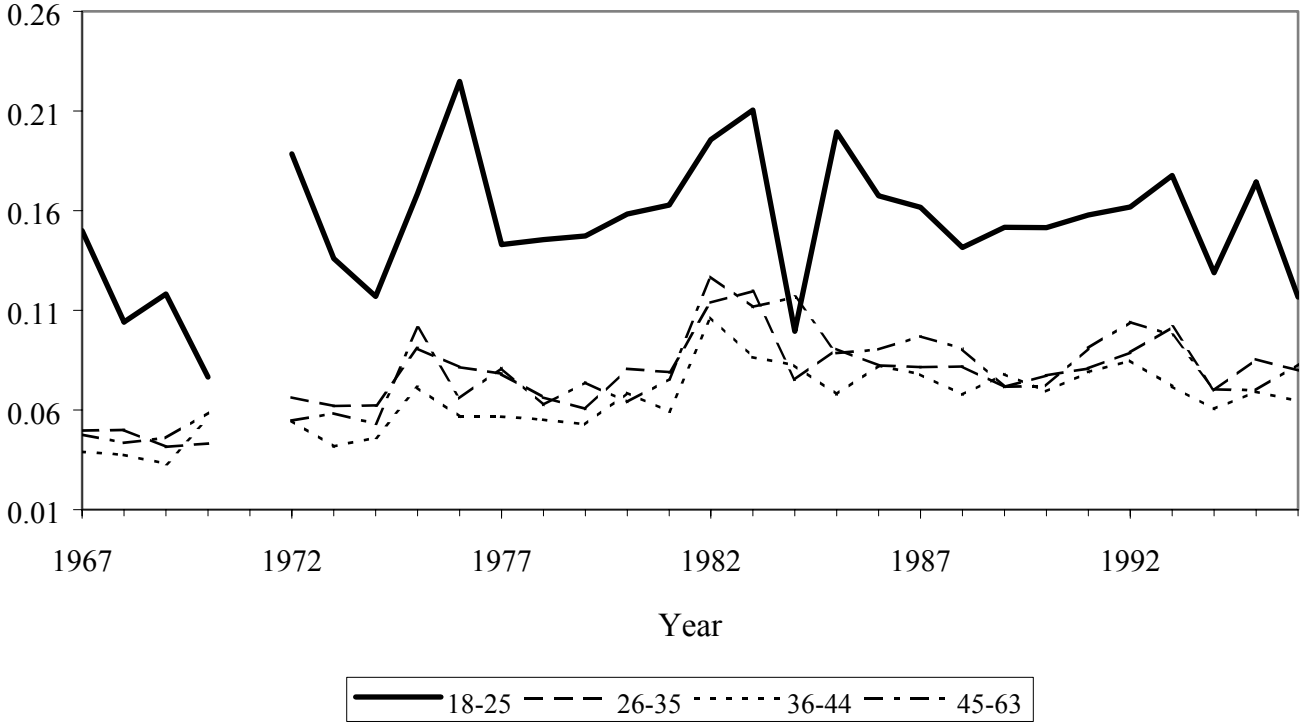

Figure 8. Transitory Earnings Variance

By Education

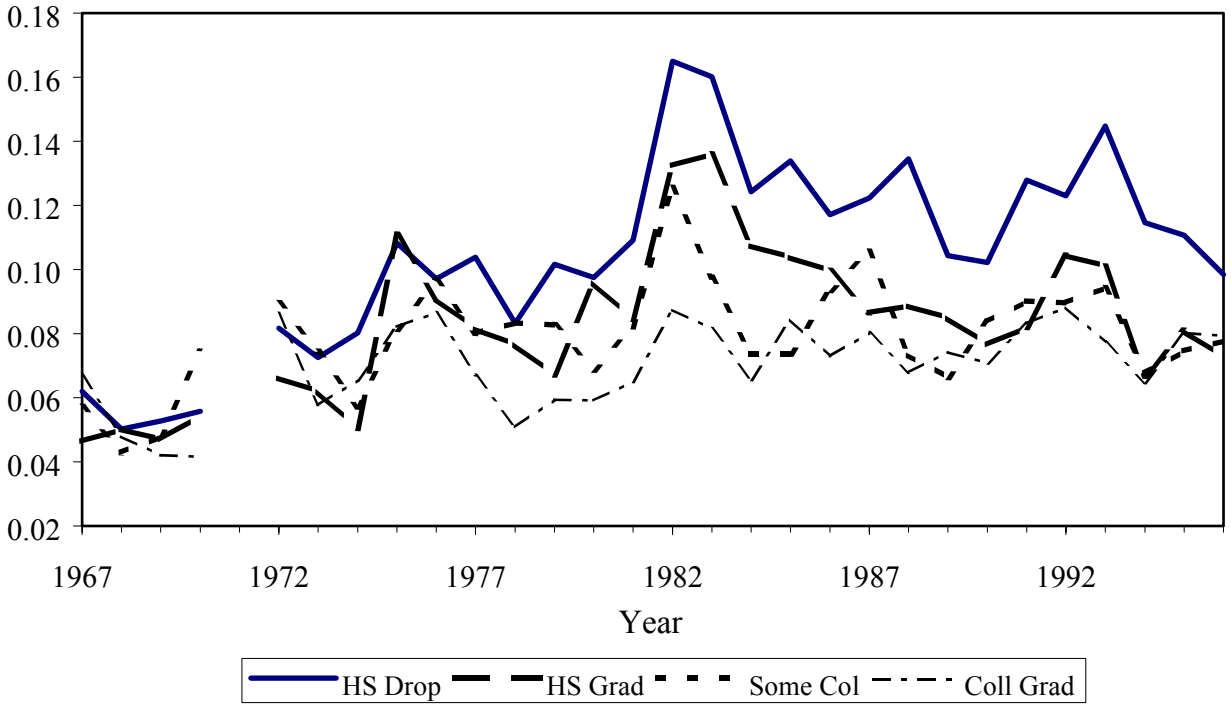

Figure 9. Transitory Earnings Variance

By Industry

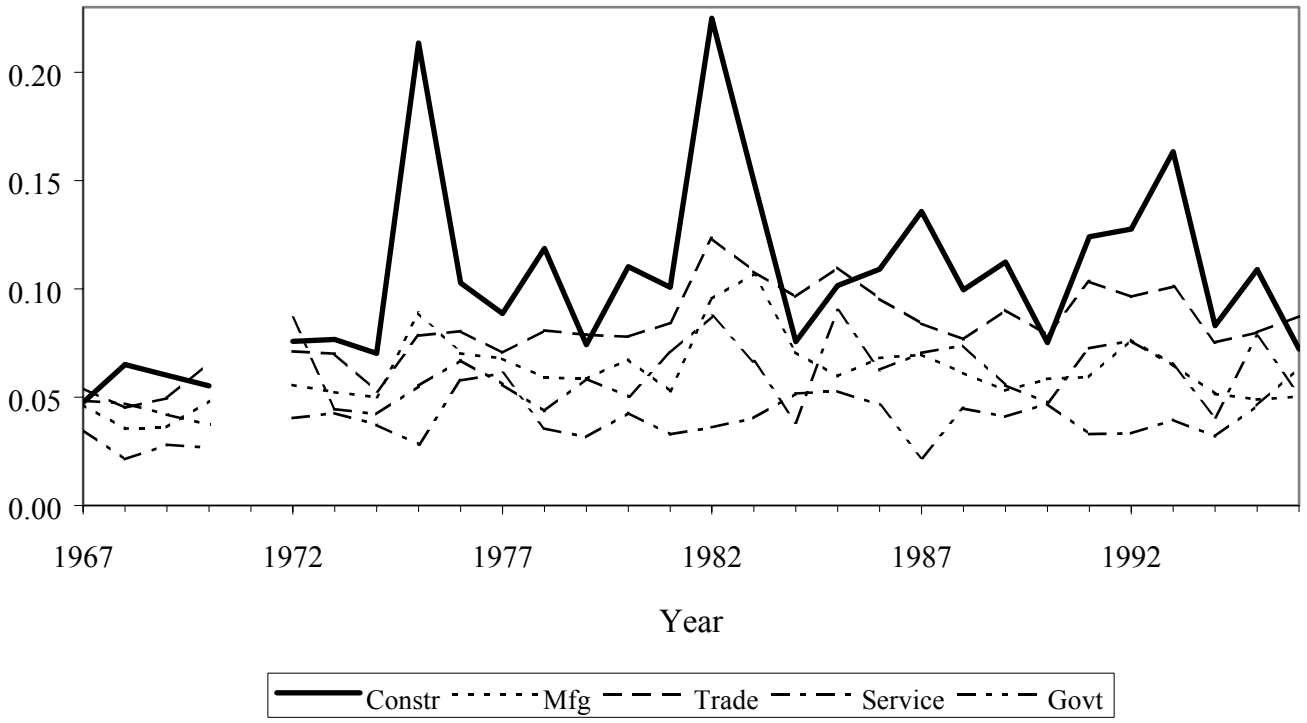

Figure 10. Transitory Earning Variance

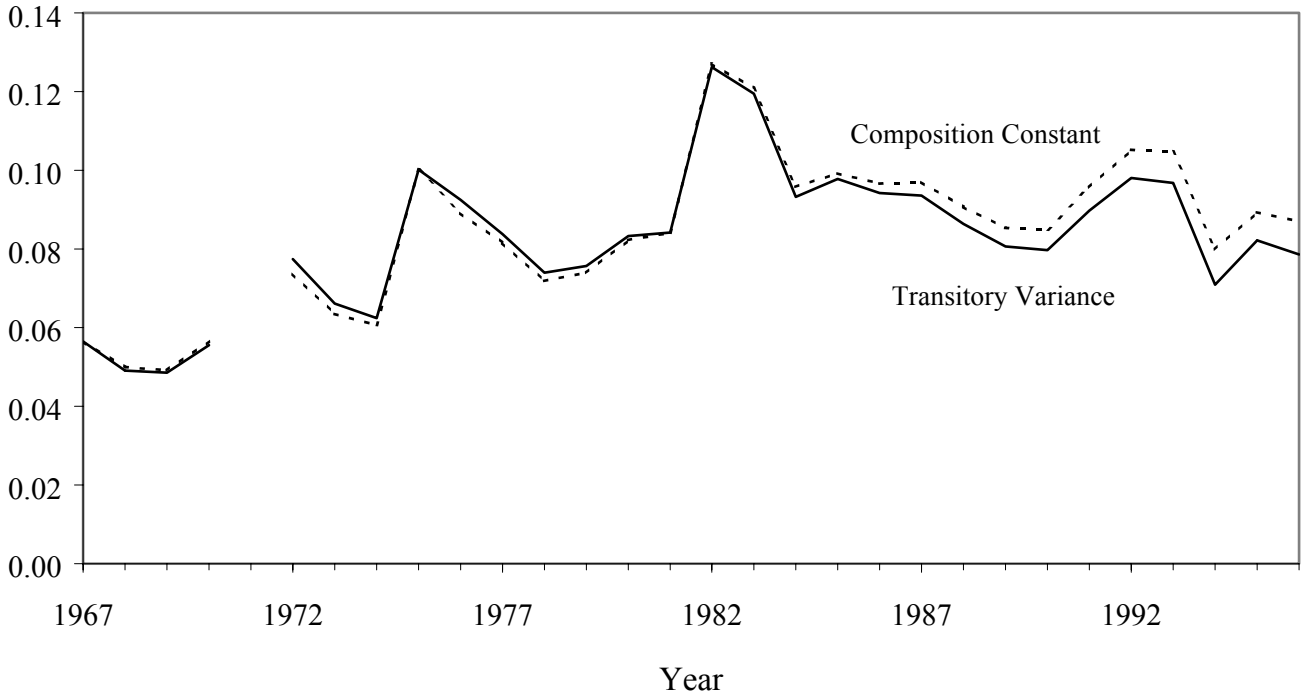

Figure 11. Variance of Weekly Wages and Annual Weeks

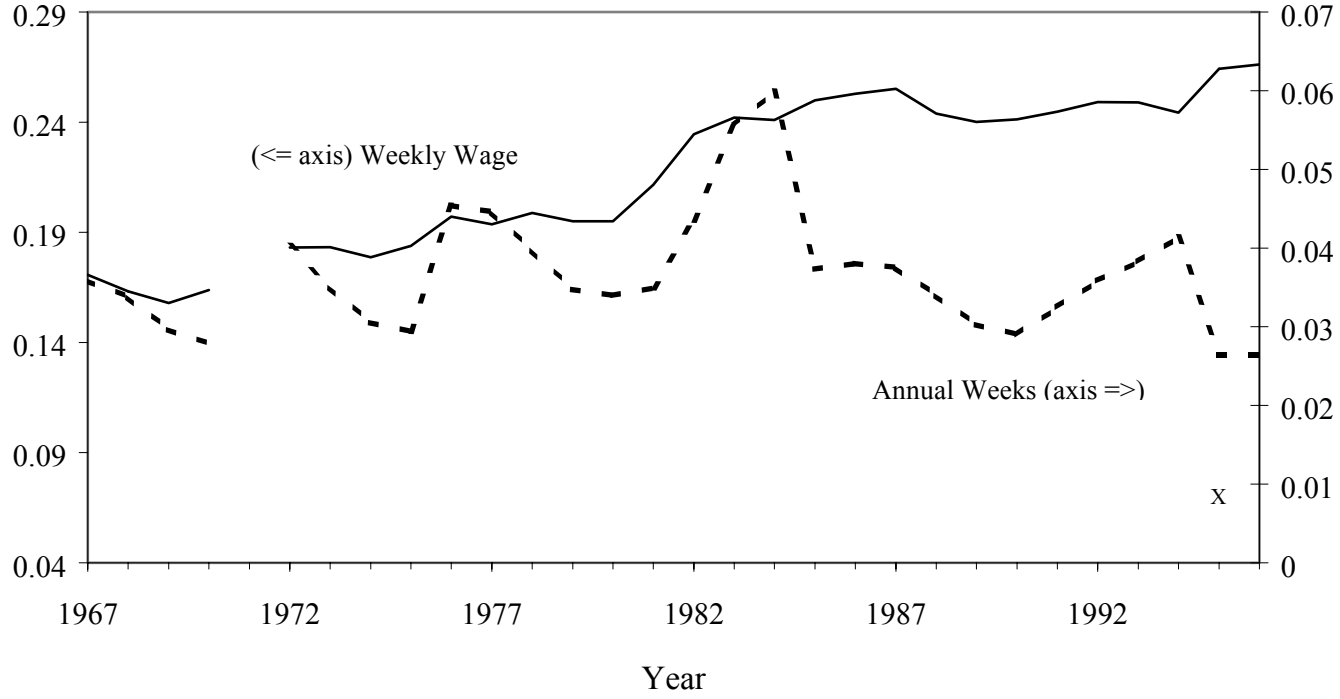

Figure 12. Permanent and Transitory Variance of Weekly Wages

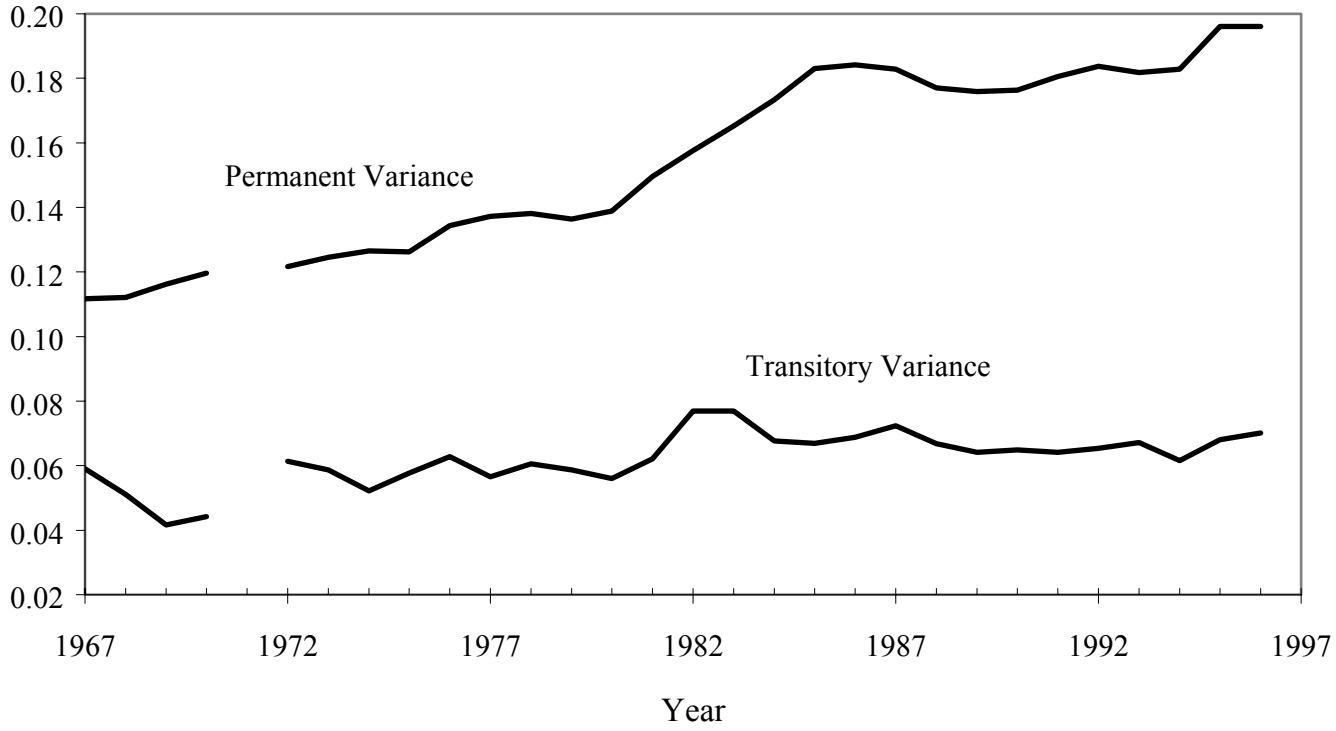

Figure 13. Transitory Earnings Variances

Job Changers and Stayers

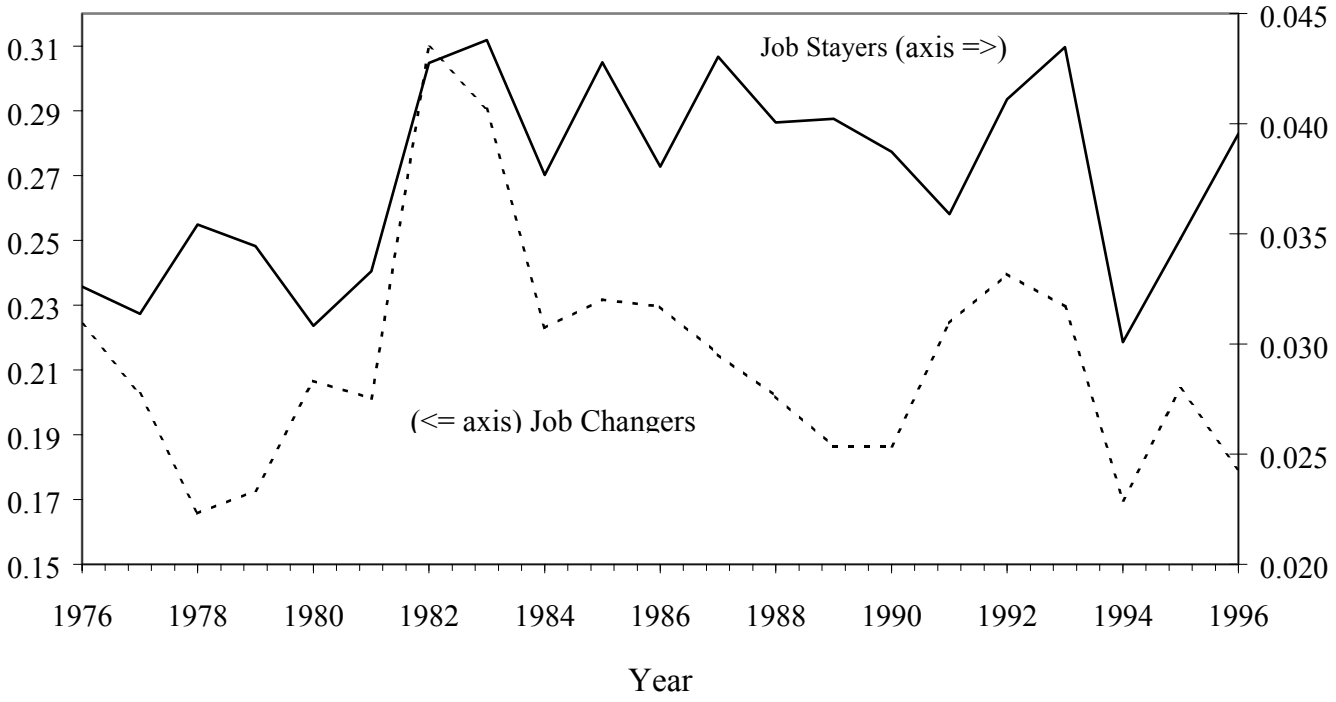

Figure 14. Transitory Earnings Variances

Industry Movers and Stayers

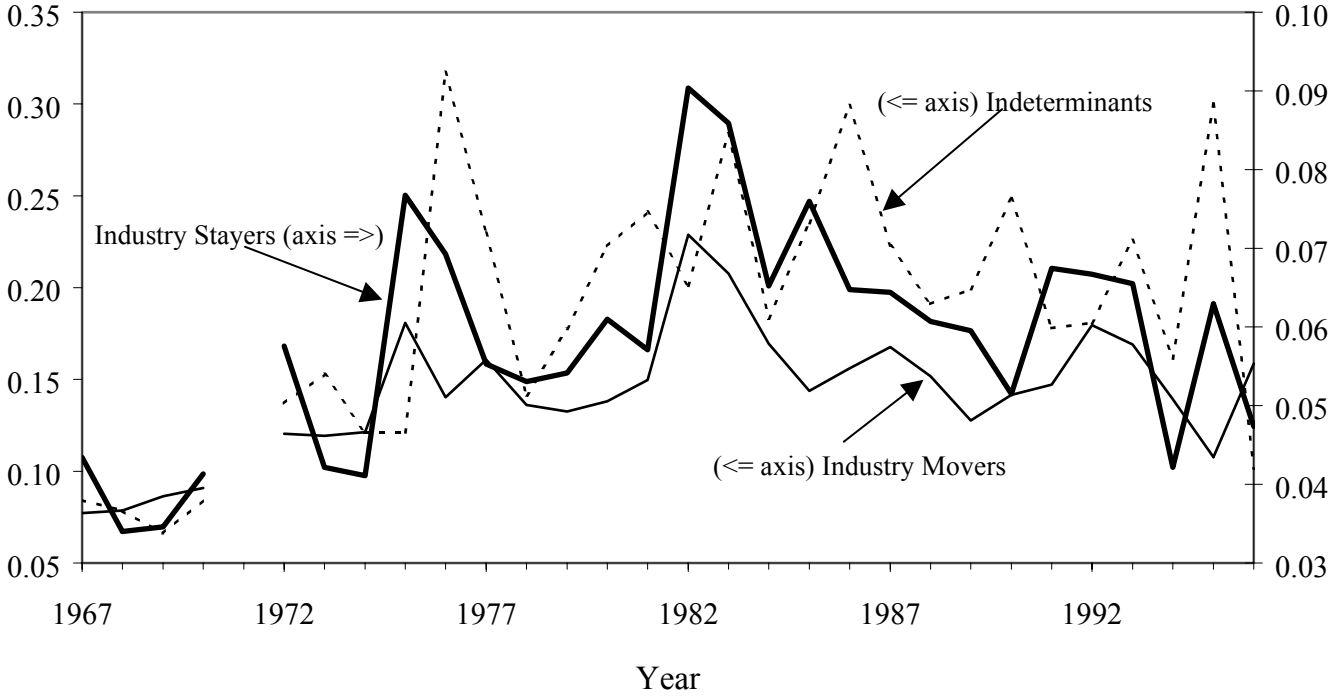

Table 1. Determinants of the Residual Variance in Earnings

Variable	Overall	Transitory
Age	−0.245 (0.014)	−0.193 (0.010)
Age Squared ($x10^2$)	0.920 (0.056)	0.695 (0.040)
Age Cubed ($x,10^3$)	−0.152 (0.009)	−0.110 (0.007)
Age Fourth ($x10^6$)	0.932 (0.060)	0.641 (0.040)
High School Graduate	−0.061 (0.003)	−0.019 (0.002)
Some College	−0.062 (0.003)	−0.018 (0.002)
College Graduate	−0.076 (0.003)	−0.021 (0.002)
Construction	−0.091 (0.006)	0.015 (0.004)
Manufacturing	−0.201 (0.006)	−0.027 (0.004)
Trade	−0.099 (0.006)	−0.005 (0.004)
Services	−0.117 (0.006)	−0.017 (0.004)
Government	−0.227 (0.007)	−0.042 (0.005)
R-Square	0.183	0.042

Notes: Left-out category is a high school dropout in agriculture. Standard errors are given in parentheses. Year effects are included. N = 351,084

Table 2. Variance of Transitory and Permanent Earnings, 1967-1996

Sample	1967-1971	1972-1976	1977-1981	1982-1986	1987-1991	1992-1996
Overall Transitory	0.0524	0.0797	0.0802	0.1062	0.0860	0.0853
		[44.3]	[52.9]	[102.5]	[64.1]	[62.7]
Age Categories:						
18 - 25	0.1122	0.1670	0.1513	0.1746	0.1529	0.1519
		[48.9]	[34.9]	[55.6]	[36.3]	[35.4]
26-35	0.0461	0.0726	0.0729	0.0963	0.0786	0.0852
		[57.6]	[58.3]	[109.0]	[70.5]	[84.8]
36-44	0.0412	0.0541	0.0585	0.0851	0.0744	0.0702
		[31.5]	[42.1]	[106.8]	[80.6]	[70.5]
45-63	0.0489	0.0666	0.0714	0.1069	0.0845	0.0850
		[36.2]	[46.0]	[118.4]	[72.8]	[73.7]
Education Categories:						
High School Dropout	0.0552	0.0880	0.0991	0.1401	0.1182	0.1183
		[59.4]	[79.6]	[153.8]	[114.3]	[114.4]
High School Graduate	0.0496	0.0760	0.0811	0.1158	0.0837	0.0851
		[53.5]	[63.6]	[133.8]	[69.0]	[71.8]
Some College	0.0556	0.0798	0.0794	0.0928	0.0838	0.0808
		[43.5]	[42.7]	[66.9]	[50.7]	[45.3]
College Graduate	0.0497	0.0757	0.0603	0.0784	0.0753	0.0779
		[52.4]	[21.5]	[57.8]	[51.6]	[56.9]
Industry Categories:						
Construction	0.0569	0.1078	0.0985	0.1322	0.1093	0.1110
		[89.3]	[72.9]	[132.1]	[92.0]	[94.9]
Manufacturing	0.0416	0.0634	0.0611	0.0802	0.0603	0.0585
		[52.3]	[46.7]	[92.6]	[44.9]	[40.5]
Trade	0.0537	0.0705	0.0785	0.1066	0.0867	0.0880
		[31.3]	[46.1]	[98.5]	[61.3]	[63.8]
Service	0.0434	0.0591	0.0558	0.0690	0.0640	0.0662
		[35.4]	[27.7]	[58.0]	[46.6]	[42.4]
Government	0.0278	0.0412	0.0407	0.0454	0.0375	0.0429
		[48.3]	[46.6]	[63.3]	[35.1]	[56.9]
Overall Permanent	0.1333	0.1608	0.1796	0.2248	0.2187	0.2291
		[20.3]	[34.5]	[68.2]	[63.7]	[71.5]

Notes: Average transitory earnings variance. Cumulative percent change from 1967-1971 period given in square brackets.

Table 3.Distribution of Individual Changes in Wage and Salary Income
(standard errors in parentheses)

YEAR	(1) Proportion below the 1967 10th percentile	(2) Proportion above the 1967 90th percentile	(3) Proportion between the 1967 40th and 60th percentiles	(4) Proportion between the 1967 25th and 75th percentiles	(5) Proportion between the 1967 10th and 90th percentiles
1967	0.10	0.10	0.20	0.50	0.80
	[.003]	[.003]	[.004]	[.006]	[.004]
1968	0.10	0.10	0.21	0.51	0.80
	[.003]	[.003]	[.005]	[.006]	[.005]
1969	0.11	0.10	0.22	0.51	0.80
	[.003]	[.003]	[.005]	[.005]	[.004]
1972	0.11	0.11	0.20	0.48	0.78
	[.004]	[.004]	[.006]	[.007]	[.006]
1973	0.11	0.11	0.19	0.48	0.78
	[.004]	[.004]	[.005]	[.006]	[.005]
1974	0.12	0.12	0.19	0.46	0.76
	[.005]	[.005]	[.006]	[.007]	[.006]
1976	0.11	0.11	0.21	0.50	0.78
	[.004]	[.004]	[.005]	[.006]	[.005]
1977	0.11	0.12	0.20	0.49	0.77
	[.004]	[.004]	[.005]	[.006]	[.005]
1978	0.12	0.12	0.20	0.49	0.76
	[.004]	[.004]	[.005]	[.006]	[.005]
1979	0.12	0.12	0.18	0.47	0.76
	[.003]	[.003]	[.004]	[.005]	[.004]
1980	0.13	0.12	0.19	0.47	0.75
	[.004]	[.004]	[.004]	[.005]	[.005]
1981	0.15	0.14	0.18	0.43	0.72
	[.004]	[.004]	[.004]	[.005]	[.005]
1982	0.14	0.13	0.18	0.46	0.72
	[.004]	[.004]	[.004]	[.006]	[.005]

YEAR	(1) Proportion below the 1967 10th percentile	(2) Proportion above the 1967 90th percentile	(3) Proportion between the 1967 40th and 60th percentiles	(4) Proportion between the 1967 25th and 75th percentiles	(5) Proportion between the 1967 10th and 90th percentiles
1983	0.13	0.14	0.17	0.45	0.73
	[.004]	[.004]	[.004]	[.006]	[.005]
1985	0.14	0.13	0.19	0.45	0.73
	[.004]	[.004]	[.004]	[.006]	[.005]
1986	0.13	0.13	0.19	0.46	0.74
	[.004]	[.004]	[.004]	[.006]	[.005]
1987	0.12	0.13	0.20	0.47	0.76
	[.004]	[.004]	[.005]	[.006]	[.005]
1988	0.13	0.13	0.21	0.47	0.75
	[.004]	[.004]	[.005]	[.006]	[.005]
1989	0.13	0.12	0.20	0.47	0.76
	[.004]	[.004]	[.005]	[.006]	[.005]
1990	0.13	0.13	0.19	0.46	0.74
	[.004]	[.004]	[.005]	[.006]	[.005]
1991	0.13	0.13	0.21	0.47	0.75
	[.004]	[.004]	[.005]	[.006]	[.005]
1992	0.14	0.12	0.20	0.46	0.74
	[.004]	[.004]	[.005]	[.006]	[.005]
1993	0.14	0.13	0.20	0.46	0.74
	[.005]	[.004]	[.005]	[.007]	[.006]
1995	0.13	0.12	0.19	0.46	0.75
	[.004]	[.004]	[.005]	[.007]	[.006]