

**Increasing Residual Wage Inequality: Composition Effects,  
Noisy Data, or Rising Demand for Skill?**

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**Abstract**

Using data from the May and Outgoing Rotation Group (ORG) supplements of the CPS, this paper shows that a large fraction of the growth in residual wage inequality between 1973 and 2003 is due to spurious composition effects. These composition effects are linked to the secular increase in the level of experience and education of the workforce, two factors associated with higher within-group wage dispersion. I also show that both the level and growth in residual wage inequality are overstated in March CPS data that have been used in most previous studies. Measured wages are noisier in the March than in the May/ORG CPS because the March CPS does not measure directly the hourly wages of workers paid by the hour.

Once these factors are corrected for, I find that residual wage inequality only accounts for a small share of the overall growth in wage inequality. Furthermore, *all* of the growth in residual wage inequality occurs during the 1980s. This closely mirrors the pattern of change in other “between-group” wage differentials like the college-high school wage premium. Overall, the magnitude and timing of the growth in residual wage inequality provides little evidence of a pervasive increase in the demand for skill due, for instance, to skill-biased technological change.

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## 1. Introduction

The growth in wage inequality over the last three decades is one of the most extensively researched topics in labor economics. It is well documented that the growth in the college-high school wage premium since the late 1970s is an important factor in the overall growth in wage inequality (Bound and Johnson, 1992, Katz and Murphy, 1992). However, residual or within-group wage inequality –i.e. wage dispersion among workers with the same education and experience– is generally believed to account for most of the growth in overall wage inequality (Juhn, Murphy and Pierce, 1993, JMP hereinafter).<sup>1</sup> According to JMP, residual wage inequality increased steadily from the early 1970s on, while the college-high school wage premium remained stable (or declined) during the 1970s before increasing sharply in the 1980s. For the 1990s, recent survey pieces by Acemoglu (2002) and Katz and Autor (1999) conclude that residual wage inequality kept growing, while Card and DiNardo (2002) and Beaudry and Green (2004) show that the college-high school wage premium was relatively stable during this period.

The growth in residual wage inequality represents a challenge for the standard human capital based approach to wage determination. First, it is unsettling that most of the growth in wage inequality appears to be due to the residual wage component that cannot be systematically linked to standard human capital variables like experience and education. Second, the difference in the timing of changes in residual wage inequality and other wage differentials like the college-high school wage premium is disturbing (Levy and Murnane, 1992, Mincer, 1997). If these various dimensions of wage dispersion were all linked to similar factors like technological change, they should more or less vary in a similar way over time.

JMP attempt to reconcile these facts using a two-factor model for observed (education and experience) and unobserved skills. They argue that the growth in residual wage inequality primarily reflects an increase in the price of unobserved skills driven by

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<sup>1</sup> Put differently, the dispersion in the residuals from a flexible wage regression grew more than the systematic component of wages predicted by the regression. This is perhaps not surprising since standard regressors such as experience and education account for a relatively small fraction the variance of the wages (R-square is typically in the .2-.3 range).

a large and steady growth in the relative demand for skills that started in the early 1970s.<sup>2</sup> Like Bound and Johnson (1992) and Katz and Murphy (1992), JMP also view this increase in the relative demand for skills as the driving force behind the expansion of the college-high school wage premium during the 1980s. They argue that during the 1970s, however, the growth in the relative demand for skill was offset by an even stronger growth in the relative supply of college-educated labor. By contrast, residual wage inequality grew in the 1970s because the relative supply of unobserved skills did not increase during this period (or other periods). Unlike the case of the college-high school premium, the role of growing relative demand on residual wage inequality was not “masked” by a large increase supply in the 1970s.

While JMP’s synthesis of the causes of growing wage inequality had a major impact in labor economics and other fields, there are a number of problems with their story.<sup>3</sup> First, it only works for a very specific production function, namely a CES function in education (or experience) and unobserved skills (Acemoglu, 2002).<sup>4</sup> By contrast, when unobserved skills (e.g. school quality or cognitive skills) are close substitutes for education, increases in the supply of education should depress both the college-high school premium and the returns to unobserved skills.<sup>5</sup>

Second, both Card and DiNardo (2002) and Beaudry and Green (2004) point out that the pattern of change in wage inequality in the 1990s is hard to reconcile with a simple supply and demand explanation. In particular, the college-high school premium increased much less in the 1990s than in the 1980s despite the fact that relative supply kept increasing at the same rate. The “supply” explanation for why residual wage

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<sup>2</sup> Most subsequent studies have argued that skill-biased technological change (SBTC) was the main factor responsible for the steady growth in skill prices. In particular, the computer and information technology revolution has emerged as the leading hypothesis for explaining the growth in the relative demand for skills since the early 1970s (Berman, Bound and Griliches, 1994, Autor, Katz and Krueger, 1998).

<sup>3</sup> Beyond labor economics, JMP’s interpretation of growing residual inequality as an increase in the skill premium has laid the foundations for a large and influential literature on economic growth, technical change, and inequality (see Acemoglu, 2002 and Aghion, 2002 for recent surveys of this literature).

<sup>4</sup> Acemoglu (2002) illustrates this point using a “two-by-two” factor model. The first factor is education (college and high school) and the second factor is unobserved skills (low and high). The CES assumption implies that the substitutability between college workers with low and high (unobserved) skills is the same as the substitutability between high skill college workers and low skill high school workers, which is not a very appealing property of the production function.

<sup>5</sup> Years of schooling and school quality are perfect substitutes in an “efficiency units” model of schooling where schooling (in efficiency units) is the product of years of schooling and school quality. In this model, residual inequality and the college-high school premium should move exactly together over time.

inequality grew in the 1970s while the college-high school wage premium did not grow over that period simply does not work for the 1990s.

Third, if the increase in the price of unobserved skills was the most important source of growing wage inequality, we should have seen a large increase in the return to various measures of “ability” and in the male-female, or black-white, wage gap (to the extent that part of these gaps are due to differences in unobserved skills). The fact that none of those wage differentials expanded over the last three decades is a major challenge to the view that the return to unobserved skills grew a lot during this period.<sup>6</sup>

In this paper, I show that these concerns are misplaced because the “common wisdom” about the magnitude and timing of changes in residual wage inequality is inaccurate. In particular, I show that residual wage inequality only accounts for a small share of the growth in overall wage inequality. I also show that the timing of changes in residual wage inequality closely matches the timing of changes in the college-high school wage premium. Both measures grew in the 1980s but declined or remained stable during the 1970s and 1990s.

These findings have important explanations for our understanding of the sources of change in wage inequality. First, they mean that most of the growth in wage inequality can be linked to standard human capital variables like experience and education. Second, the fact that residual wage inequality moves in parallel with other wage differentials suggests that both dimensions of wage inequality are shaped by the same underlying forces, thus resolving the “puzzle” raised by Levy and Murnane (1992) and Mincer (1997).

These findings are at odds with most of the previous literature that gives a much more prominent role to residual wage inequality. Accordingly, most of the paper deals with issues linked to the measurement of residual wage inequality. I show that the difference between my findings and those of earlier studies are due to a combination of several factors. First, I use data on hourly wages from the May and Outgoing Rotation Group (ORG) supplements of the Current Population Survey (CPS), while earlier studies

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<sup>6</sup> See Heckman and Vytlačil (2001) who show that the return to cognitive ability has not changed much during the 1980s and 1990s. See Card and DiNardo (2002) for evidence that the black-white wage gap was relatively stable during the 1980s and 1990s, while the male-female wage gap declined substantially during this period.

have typically used the March Supplement of the CPS. In Section 5, I explain why the May/ORG CPS is better suited than the March CPS for studying the evolution of residual, or within-group, wage dispersion. The main problem with the March CPS is that it poorly measures the wages of workers paid by the hour (the majority of the workforce). The fraction of workers paid by the hour grows substantially over time within narrowly defined group of workers, which results in spurious growth in within-group wage dispersion in the March CPS.

A second importance difference is that, unlike most other studies, I control for composition effects. As explained in detail in Section 2, controlling for composition effect is critical for interpreting the growth in residual wage inequality as a consequence of growing skill prices. Otherwise, growing residual wage inequality could simply reflect increasing inequality in the distribution of unobserved skills. For instance, it is well recognized since Mincer (1974) that wage dispersion increases as a function of experience (past the overtaking point) because of differential investments in on-the-job training (OJT). I show in Section 3 that residual wage dispersion is also much higher among more- than less-educated workers. This suggests that unobserved skills are becoming more unequally distributed as the levels of education and experience increase over time. Perhaps surprisingly, I conclude that most of the growth in residual wage inequality (in the May/ORG CPS) is in fact a spurious consequence of secular growth in both education and experience.<sup>7</sup>

A final difference with earlier studies is that much more data are now available for studying secular changes in residual wage inequality. For example, I use wage data for up to 2003 while the last year of wage data available to JMP was 1989. Composition effects play a much bigger role in changes in residual inequality in the 1990s and early

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<sup>7</sup> Two earlier studies that implicitly control for composition effects in residual inequality are DiNardo, Fortin, and Lemieux (1996) and JMP. Using a re-weighting procedure that I also use here, DiNardo, Fortin, and Lemieux (1996) show that a third of the growth in residual inequality between 1979 and 1988 is due to composition effects. This result is very similar to what I find here for the same time period. The decomposition procedure of JMP should also, in principle, adjust for composition effects. JMP only report, however, the joint effect of changes in the composition of the workforce on the between- and within-group components of wage inequality. The separate impact of composition effects on residual wage inequality is not separately reported in their study.

2000s than in the 1970s and 1980s. This may explain why composition effects remained relatively unnoticed in the earlier literature.

The paper is organized as follows. In Section 2, I discuss in more detail the link between residual wage inequality, unobserved skill prices, and composition effects. I present the May/ORG CPS data and show basic trends in within-group wage inequality for twenty experience-education groups in Section 3. Section 4 presents the main results on the evolution of residual wage inequality once composition effects are adjusted for. Section 5 shows why both the level and the growth in residual wage inequality are overstated in the March CPS. Section 6 concludes by suggesting possible explanations for the trends in residual wage inequality since the early 1970s. I argue that a standard Mincer-type human capital pricing approach that also account for the impact of changes in the real value of the minimum wage explain quite well the evolution of residual wage inequality between 1973 and 2003.

## **2. Residual wage inequality, skill prices, and composition effects**

From an economic perspective, residual wage inequality is a useful concept because of its potential connection to skill prices. In this Section, I explain why this connection is only valid when composition effects are controlled for. I also discuss the re-weighting approach that I later use to control for composition effects.

To fix ideas, consider a standard Mincer-type wage equation:

$$(1) \quad \ln w_{it} = x_{it}b_t + \varepsilon_{it},$$

where  $w_{it}$  is the hourly wage rate of individual  $i$  at time  $t$ ;  $x_{it}$  is a vector of observed skills (education and labor market experience);  $b_t$  is the return (or price) to observed skills;  $\varepsilon_{it}$  is the standard regression residual. Following JMP, the residual is interpreted as the product of some unobserved skills,  $e_{it}$ , with the return to unobserved skills,  $p_t$ :

$$(2) \quad \varepsilon_{it} = p_t e_{it}.$$

Equation (2) has no empirical content unless some assumptions are imposed on the distribution of unobserved skills. Following JMP and Chay and Lee (2000), I assume that the distribution of unobserved skills among workers with the same level of experience and education is stable over time. In other words, the conditional distribution function  $F_t(e_{it}|x_{it})$  is time-invariant:

$$(3) \quad F_t(e_{it}|x_{it}) = F(e_{it}|x_{it}) \text{ for all time periods } t.$$

JMP discuss some possible objections to this assumption. In particular, more recent cohorts may have more unequally distributed skills and abilities than older cohorts for the same level of experience and education. This could be the result, for instance, of growing inequality in school quality. JMP test this hypothesis by decomposing the growth in within-group wage dispersion into cohort and time effects. They conclude that time effects, as opposed to cohort effects, appear to be driving the growth in within-group wage inequality. This finding is consistent with the assumption that the conditional distribution of unobserved skills is constant across time (and cohorts).

By contrast, the stronger assumption that the unconditional distribution of unobserved skills,  $F_t(e_{it})$ , is also stable over time is clearly inconsistent with a large body of research in labor economics. In particular, it is well recognized since Mincer (1974) that wage dispersion increases as a function of experience (past the overtaking point) because of differential investments in on-the-job training (OJT). In other words, inequality in the distribution of unobserved skills (OJT) increases with experience. Similarly, Farber and Gibbons (1996) show that inequality in wages and unobserved skills (as valued by the market) also increase as a function of experience in a simple learning model.<sup>8</sup> In both of these models, the aging of the workforce results in a more dispersed unconditional distribution of unobserved skills as increasingly more weight is put on older workers with more unequally distributed skills. This can result in significant

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<sup>8</sup> In Farber and Gibbons (1996), wages are equal to the expected value of productivity given the available information about the past productivity of workers. There is little wage inequality among inexperienced workers since the market does not yet know who is productive and who is not. Inequality increases as a function of experience as the market learns who is productive (skilled) and who is not. From the point of view of the econometrician, inequality in unobserved skills (what is valued by the market) thus grows as a function of labor market experience.

composition effects in the 1980s, 1990s, and 2000s because of the aging of the baby-boom generation. Similar arguments can be made in the case of education. For example, Mincer (1997) shows that the within-group variance of wages increases as a function of education in a standard Becker (1967) human capital model with heterogeneity in the marginal costs and benefits of investments in education. Alternatively, there may be more heterogeneity in school quality at the college than high school level.

The role of composition effects is easily illustrated by looking at the variance of wages when observed skills,  $x_{it}$ , are divided into a finite number of cells  $j$ . The unconditional variance of unobserved skills,  $\text{Var}(e_{it})$ , is linked to the conditional variance,  $\text{Var}(e_{it} | j)$ , by the simple variance decomposition

$$(4) \quad \text{Var}(e_{it}) = \sum_j \theta_{jt} \text{Var}(e_{it} | j),$$

where  $\theta_{jt}$  is the share of workers in experience-education group  $j$  at time  $t$ . Under the assumption that the conditional variances are stable over time, equation (4) can be rewritten as

$$(5) \quad \text{Var}(e_{it}) = \sum_j \theta_{jt} \sigma_j^2,$$

where  $\text{Var}(e_{it} | j) = \sigma_j^2$  for all  $t$ . Since the conditional variance  $\sigma_j^2$  is different for different skill groups  $j$ , changes in the skill composition of the workforce (the  $\theta_{jt}$ 's) will also change the unconditional variance of unobserved skills.

The residual variance of wages,  $\text{Var}(\epsilon_{it})$ , is obtained by substituting equation (5) into the “skill pricing” equation (2)

$$(6) \quad \text{Var}(\epsilon_{it}) = \text{Var}(p_t e_{it}) = p_t^2 \sum_j \theta_{jt} \sigma_j^2.$$

In this model, an increase in the residual variance can only be interpreted as an increase in skill prices  $p_t$  when the skill composition of the workforce (the  $\theta_{jt}$ 's) is held constant. Fortunately, equation (6) suggests a straightforward way of holding the skill composition

of the workforce constant. The residual variance just has to be recomputed at some counterfactual values of the shares,  $\theta_j^*$ , that remain constant over time.

To see this, rewrite the residual variance as a function of  $V_{jt}$ , the variance of wages within each skill group  $j$

$$(7) \quad \text{Var}(\varepsilon_{it}) = \sum_j \theta_{jt} V_{jt},$$

where  $V_{jt} = p_t^2 \sigma_j^2$ . The counterfactual residual variance,  $V_t^*$ , is

$$(8) \quad V_t^* = \sum_j \theta_j^* V_{jt}.$$

When the number of skill groups is small relative to sample sizes, the within-group variance  $V_{jt}$  can be computed for each skill group  $j$ . It is then straightforward to estimate the counterfactual variance by replacing the year-specific shares,  $\theta_{jt}$ , by some average or base year shares,  $\theta_j^*$ .

As it turns out, Mincer (1974) computed such counterfactual variances. After dividing the data in about one hundred experience-education cells, he shows that the variance of wages would have been substantially larger in 1959 if older workers had been as highly educated as younger workers. Since this is basically what happened in the U.S. labor market over the last 40 years, the results in Mincer (1974) suggest that composition effects may indeed be playing an important role in the evolution of residual wage inequality over the last few decades.<sup>9</sup>

In Section 3, I present some basic trends in residual and within-group inequality by dividing data in a limited number of experience-education cells (twenty). Working with these coarse cells helps illustrate which factors are driving the overall changes in residual inequality. To see this, consider the following decomposition of the change in the residual variance between a base period  $s$  and an end period  $t$ :

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<sup>9</sup> Card and Lemieux (2001a, 2001b) show that the level of educational attainment of young workers remains relatively constant over the 1975-95 period. As a result, young workers in the early 2000s are not much more educated than older workers. This stands in sharp contrast with the situation that prevailed in the 1959 census data analyzed by Mincer (1974). Mincer shows that the variance of log annual earnings in 1959 would have increased from 0.668 to 0.721 if workers at all experience levels had had the same level of education as younger workers (7-9 years of experience).

$$\begin{aligned}
(9) \quad V_t - V_s &= \sum_j (\theta_{jt} V_{jt} - \theta_{js} V_{js}) \\
&= \sum_j \theta_{js} (V_{jt} - V_{js}) + \sum_j (\theta_{jt} - \theta_{js}) V_{jt}.
\end{aligned}$$

Equation (9) shows that the overall change in the residual variance can be decomposed into two terms. The first term on the right hand side of equation (9),  $\sum_j \theta_{js} (V_{jt} - V_{js})$ , is a weighted average of changes in the within-group variance. In terms of equation (8), this represents the change in the counterfactual variance,  $V_t^*$ , when the counterfactual weights,  $\theta_j^*$ , are set at the base period level ( $\theta_j^* = \theta_{js}$ ).

The second term on the right hand side of equation (9),  $\sum_j (\theta_{jt} - \theta_{js}) V_{jt}$ , captures composition effects. Composition effects result in a spurious growth in the residual variance when changes in the weights,  $\theta_{jt} - \theta_{js}$ , are positively correlated with the within-group variances,  $V_{jt}$ .

When the number of cells is small enough, equation (9) suggests a simple approach for separating the role of rising skill prices from composition effects. Since  $V_{jt} = p_t^2 \sigma_j^2$ , the most direct evidence on rising skill prices is that the within-group variances,  $V_{jt}$ , are also growing over time. This can be readily checked by comparing these variances in a base and end period. Equation (9) then shows how these changes can be aggregated into a single factor,  $\sum_j \theta_{js} (V_{jt} - V_{js})$ .

From an estimation point of view, however, dividing the data in a limited number of coarse experience-education cells may be too restrictive. One alternative is to construct finer cells based on single years of education and experience. Unfortunately, cell sizes based on single years of age and education are too small (and sometimes empty) in most CPS samples (see footnote 13). Following Lemieux (2002) and DiNardo, Fortin and Lemieux (1996), I address this problem by estimating a flexible logit model to re-weight the data in a way that keeps the distribution of skills constant over time.

Another major advantage of the re-weighting procedure over the variance decomposition in equation (9) is that, in addition to the variance, any other distributional statistics like the Gini coefficient or the 90-10 gap can also be computed in the re-weighted samples (DiNardo, Fortin and Lemieux, 1996). In Section 4, I report estimates

of residual wage inequality based on the 90-10 gap in addition to the variance that I use throughout the paper.<sup>10</sup>

### **3. Data and Trends in Within-Group Inequality by Skill Groups.**

In this Section, I briefly present the May/ORG CPS data and show the basic trends in within-group wage dispersion for twenty experience-education groups. I use equation (9) to illustrate which factors —rising skill prices or composition effects— are driving the growth in the residual variance.

#### *a. Data*

Data issues are discussed in more detail in Section 5 that compares the hourly wage measure constructed from the May/ORG and March CPS Supplements. I only briefly discuss here how the May and ORG supplements of the CPS are processed. Following most of the literature, the key wage measure on which I focus in this paper is the hourly wage rate. The main advantage of this measure is that theories of wage determination typically pertain to the hourly wage rate. For example, the interplay of demand and supply considerations has direct implications for the hourly price of labor. By contrast, the impact of these factors on weekly or annual earnings also depends on the responsiveness of labor supply to changes in the hourly wage rate.

I use the Dual Jobs Supplement of the May CPS for 1973 to 1978. The May CPS asks questions about wages on the main job held during the survey week to all wage and salary workers. For workers paid by the hour, the May CPS asks workers directly about their hourly rate of pay. This is the hourly wage measure that I use for this group of workers (about sixty percent of the workforce). For the other workers, I compute an hourly wage rate by dividing weekly earnings by weekly hours of work. I use the same procedure for the 1979 to 1993 ORG supplements that ask the same wage questions as

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<sup>10</sup> In principle, JMP’s “residual imputation procedure” could also be used to account for the role of composition effects in changes in residual wage dispersion. JMP essentially suggest replacing the empirical distribution of residuals for given values of experience and education by a counterfactual distribution for the same skill group (e.g. the empirical distribution of residuals for all years pooled together). As discussed in Lemieux (2002), however, it is unclear how this procedure can be implemented in practice unless the data are divided in large enough cells. By contrast, the re-weighting procedure is very easy to implement even in cases where cells are “too small”.

the May CPS. The wage questions in the 1994 to 2003 ORG supplements are similar except that workers not paid by the hour can choose the periodicity at which they report earnings. I compute their hourly wage rate by dividing earnings by hours over the corresponding time period.<sup>11</sup> The merged outgoing rotation group (MORG) files combine this information for all 12 months of the year. One important advantage of the MORG supplement is that it is roughly three times as large as the May or March supplements of the CPS.<sup>12</sup>

Unlike in the ORG and March supplements of the CPS, in the May CPS wages were not allocated for workers who refused to answer the wage questions. To be consistent, I only keep workers with non-allocated wages in the 1979-2003 ORG supplement. As a consequence, I have to drop observations for 1994 and the first eight months of 1995 in which the CPS did not flag workers with missing wages. Following most of the literature, I trim extreme values of wages (less than \$1 and more than \$100 in 1979 \$), adjust top-coded earnings by a factor of 1.4, and weight wage observations by hours of work (in addition to the usual CPS weights). I also keep workers age 16 to 64 with positive potential experience.

All the measures of residual and within-group inequality used in this paper are computed from the residuals of a regression of log wages on an unrestricted set of dummies for age, years of schooling, as well as interactions between nine schooling

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<sup>11</sup> Starting with the 1994 CPS, workers are first asked what is the earnings periodicity (hourly, weekly, bi-weekly, annual, etc.) that they prefer to use in reporting their earnings on their current job. But once again, all workers paid by the hour are asked for their hourly wage rate. Hourly rated workers are asked this question even if “hourly” is not their preferred periodicity in the first question. Workers not paid by the hour are then asked to report their earnings for the periodicity of their choice. An hourly wage rate can again be computed by dividing earnings by usual hours of work over the relevant period. In 1994, The CPS also introduced “variable hours” as a possible answer for usual hours of work. I impute hours of work for these workers using a procedure suggested by Anne Polivka of the BLS.

<sup>12</sup> The May 1973-78 and March supplements are administered to all (eight) rotation groups of the CPS during these months. By contrast, only one quarter of respondents (in rotation groups 4 and 8) are asked the questions from the ORG supplement each month. Combining the 12 months of data into a single MORG file yields wage data for 24 rotation groups compared to 8 in the March or May supplements. Note that the size of the March Annual Demographic Supplement was substantially increased in the survey year 2001 to get more precise estimates of children health insurance coverage by states. As a consequence, the March 2001 to 2003 files are almost half as large (instead of a third as large) as the MORG files for these years. The March samples are also slightly larger than the May samples as they include an over sample for Hispanics.

dummies and a quartic in age.<sup>13</sup> Separate regressions are estimated for both men and women in each year.

*b. Basic trends in within-group variances*

I first divide workers into twenty skill groups based on five education categories (high school dropouts, high school graduates, some college, college graduates, and college post-graduates) and four experience categories (1-10, 11-20, 21-30, and 31 years or more of potential experience). Table 1 and Figure 1 show the within-group variances for each experience-education group at the beginning and end of the sample period. Since the residuals are computed from a very flexible regression, the within-group variance (variance of residuals) for a given group is smaller than the variance of unadjusted wages for the same group. To improve the precision of the estimates, I pool years 1973 to 1975 for the base period, and years 2000 to 2002 for the end period.

Figure 1a (men) and 1b (women) plot the within-group variances in 2000-02 against the 1973-75 variances. In these figures, different symbols are used for different education groups, while the size of the symbol identifies the experience level (smallest symbol for the least experienced group, largest symbol for the most experienced group). If the within-group variances were constant over time, they would line up exactly on the 45 degrees line. Perhaps surprisingly, Figure 1a shows that, for men, most skill groups are scattered around the 45 degrees line. Only four (college graduates with 1-10, 11-20, and 21-30 year of experience, and college post-graduates with 1-10 years of experience) of the twenty groups are clearly above the 45-degree line. For the sixteen other education-experience groups, there is no discernable pattern of increase in the within-group variance.

Several other clear patterns also emerge from Figure 1a. In particular, the within-group variance grows as a function of both experience and education. For example, in

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<sup>13</sup> While it would be ideal to use an unrestricted set of age-education dummies in the wage regressions, in practice many age-education cells are quite small in the March and May supplements of the CPS. The flexible specification I use fits the data quite well. In the larger ORG samples, using a full set of age-education dummies only raises the R-square by about half a percentage point relative to the specification used in the paper. Note also that variables like race, marital status and other socio-economic variables are often used in standard wage regressions. I only use years of schooling and years of age (or potential experience) as regressors to focus on arguably “pure” measures of skills.

both 1973-75 and 2000-02, high school dropouts with 1 to 10 years of experience have the lowest variance (around 0.10) while college post-graduates with 31 years and more of experience have the largest variance (around 0.40). This suggests that composition effects may be quite important since both experience and education increase over time.

Another interesting pattern in Figure 1a is the systematic difference in the growth in the within-group variance by education level. For all four experience levels, the variance declines over time for high school dropouts but increases for college graduates. There is little systematic change in the variance for the three other education groups.

The results for women are qualitatively similar to those for men with the exception of women with some college education. For this education group, the within-group variance systematically increases between 1973-75 and 2000-02. As in the case of men, the within-group variance increases for college graduates, decreases for high school dropouts, and remains relatively unchanged for high school graduates and college post-graduates.

Tables 1a (men) and 1b (women) reproduce these within-group variances in columns 1 and 2. The change in the within-group variance is reported in column 3. The groups for which the within-group variance increases by more than 0.04 are highlighted (in bold) in column 3. Columns 4 and 5 show the share of each skill group in the workforce in 1973-75 and 2000-02, respectively, while column 6 shows the change in the shares over time.

Like Figure 1, Table 1 clearly shows that the within-group variance is strongly linked to experience and education. Since the twenty groups are ranked by education and experience, groups at the top of the table (low education and experience) show much less wage dispersion than groups at the bottom of the table (high education and experience).

Table 1 also shows that there is a large and systematic decline in the share of workers in groups with low within-group wage dispersion. This is most obvious when groups are divided by education. For women, column 6 of Table 1b shows that, for all experience groups, the share of women with a high school degree or less has declined over time. By contrast, the share of women with some college education or more has increased for each and every experience group. With two minor exceptions, the same pattern holds for men in Table 1a. The other clear pattern is that the share of more

experienced workers relative to young workers systematically increases for all education groups (except high school dropouts). This reflects the aging of the baby boom generation.

Overall, Table 1a and 1b clearly show a systematic growth in the share of experience-education groups that exhibit large within-group variances. The correlation coefficient between the within-group variance in 2000-02 ( $V_{jt}$ ) and the change in share ( $\theta_{jt} - \theta_{js}$ ) is 0.55 for men and 0.68 for women. This suggests a large and positive composition effect term in equation (9).

Panel B of Table 1a and 1b shows more explicitly the magnitude of composition effects. The first row of the panel repeats the weighted average of the within-group variances when the weights used are the actual shares in the corresponding year. The 1973-75 shares are used to weight the 1973-75 variances, and the 2000-02 shares are used to weight the 2000-02. The weighted averages correspond to the unadjusted residual variances for 1973-75 and 2000-02, respectively. Table 1a shows that the residual variance for men increases by 0.041 between these two time periods.

The second row of Panel B shows that the change in the residual variance is much smaller (0.012) when the shares are held at their 1973-75 level. As a result, only about a quarter of the 0.041 change in the residual variance is due to the increase in the within-group variances. Consistent with Figure 1a, this means that, on average, the within-group variance in 2000-02 is only 0.012 above the 45 degrees line. The remaining change in the residual variance, 0.029 (0.041 minus 0.012) is due to composition effects.

The results for women reported in Table 1b are quite similar. Composition effects account for 0.035 of the 0.047 increase in the residual variance. Only a quarter of the total increase (0.012) is due to the changes in within-group group variances, holding the shares constant at their 1973-75 level.

Interestingly, the last row of Panel B shows that the residual variance increases more when shares are held at their 2000-02 levels instead. The intuition for this result is that using the 2000-02 shares instead of the 1973-75 shares puts more weight on college graduates who experience a sharp increase in their within-group variances, and less weight on high school dropouts who experience a decline in their within-group variances.

Figure 2 provides some information on the detailed year-by-year evolution of the within-group variance for each of the five education groups. To control for changes in the experience distribution of the workforce, the variance for each education group is defined as the simple average of the within-group variances over the four experience groups. For example, the within-group variance for college graduates in Figure 2 is the average of the within-group variances for college graduates with 1-10, 11-20, 21-30 and 31 or more years of experience.

I only show the detailed evolution in the within-group variance by education groups for two reasons. First, it would not be practical to show the detailed evolution in the within-group variance for each of the twenty experience-education groups. Second, Table 1 and Figure 1 suggest that, conditional on education, the change in the within-group variance is relatively similar across experience groups. In other words, education (as opposed to experience) accounts for most of the variation in the growth in the within-group variance across the twenty experience-education groups.

The results for both men (Figure 2a) and women (Figure 2b) are different for different time periods. For both men and women, the within-group variances are either stable or declining during the 1970s. The within-group variances then grow substantially for each and every group during the 1980s. In the 1990s, however, there is a divergence in the trends by education groups. For college graduates and post-graduates, the within-group variance increases slightly or remains constant between 1990 and 2000. For all other education groups, however, the within-group variance declines during the 1990s. The decline is particularly pronounced for high-school dropouts. Finally, the within-group variances grow mildly for most groups during the early 2000s.

Taken together, the results in Figures 1 and 2 indicate that, for most groups, there is relatively little change in the within-group variance between 1973 and 2003. The only exception is college graduates and women with some college education. For these particular groups, however, almost all of the growth in the within-group variance is concentrated in the 1980s.

#### **4. Changes in Residual Inequality: Re-weighting Results**

Having established the basic patterns of changes in the within-group variance for twenty coarse experience-education cells, I now turn to a re-weighting approach to analyze in more detail the role of composition effects in changes in residual wage inequality. As discussed in Section 2, one advantage of the re-weighting approach is that it is easily implemented even when the data cannot be divided into fine experience-education cells. Another advantage of the approach is that it can be used to compute counterfactual measures of residual wage dispersion other than the variance.

The counterfactual variances are computed by replacing the sample weight for worker  $i$ ,  $\omega_{it}$ , by a counterfactual weight  $\omega_{it}^*$ . The actual residual variance is computed from the individual level data as

$$V_t = \sum_i \omega_{it} r_{it}^2,$$

where  $r_{it}$  is the estimated residual for worker  $i$  at time  $t$ . The counterfactual variance is

$$V_t^* = \sum_i \omega_{it}^* r_{it}^2.$$

The counterfactual weights  $\omega_{it}^*$  are chosen in a way that makes the (counterfactual) distribution of skills at time  $t$  the same as in an appropriate base year (for example 1973). These weights are obtained by multiplying the sample weights  $\theta_{it}$  by a re-weighting factor. The re-weighting factor is computed using the estimates from a logit model for the probability of being in year  $t$  relative to the base year (see Lemieux, 2002, for more detail). For example, the counterfactual weights for 2003 when 1973 is used as base year are computed by estimating a logit model on data for years 1973 and 2003 pooled together. The dependent variable is a dummy variable for year 2003, while the explanatory variables are the age and education variables. I use the same set of explanatory variables in the logit as in the wage regressions (full set of indicators for age and education plus interactions between education and a quartic in age). The predicted probability that worker  $i$  is in year 2003,  $P_i$ , is then used to compute the counterfactual weight as

$$\omega_{it}^* = [(1 - P_i) / P_i] \omega_{it}.$$

Once the counterfactual weights have been computed, it is straightforward to compute alternative measures of residual wage dispersion. For example, the actual 90-10 residual gap is defined as the difference between the 90<sup>th</sup> and the 10<sup>th</sup> centile of the wage residuals when the usual sample weights  $\omega_{it}$  are used. The counterfactual 90-10 residual gap is

readily obtained by recomputing the 90<sup>th</sup> and the 10<sup>th</sup> percentiles using the counterfactual weights  $\omega_{it}^*$  instead.

Figures 3a (men) and 3b (women) compare the actual residual variance from 1973 and 2003 to the counterfactual variances that would have prevailed if the distribution of skills (experience and education) had remained at its 1973 (or 2003) level. The composition effects are the difference between the actual and counterfactual variances. Figure 3a shows that the residual variance grows by about 0.04 over the whole sample period. Consistent with Figure 2a, most of the growth is concentrated in the first part of the 1980s. The residual variance remains essentially unchanged in the 1970s and 1990s but grows between 1999 and 2003.

By contrast, the counterfactual variance in the late 1990s / early 2000s is only about 0.01 higher than in the mid-1970s when the distribution of skills is held constant at its 1973 level. Consistent with Table 1a, Figure 3a suggests that about three quarters of the growth in the residual variance is a spurious consequence of composition effects (when the distribution of skills is held at its 1973 level).

In terms of timing, Figure 3a shows that composition effects play a negligible role during the 1970s but become very important during the 1980s and 1990s. It is clear from Appendix Table 1 why composition effects are not important during the 1970s. The table shows that while the workforce became more educated between 1973 and 1980, it also became less experienced with the entry in the labor market of the largest baby boom cohorts (born in the late 1950s). The positive impact of growing educational achievement on the residual variance is thus offset by the fact that the workforce became younger (lower within-group wage inequality) during this period. By contrast, Appendix Table 1 shows that both experience and educational achievement increased in the 1980s and 1990s, leading to an unambiguous positive composition bias in the growth of the residual variance.

A closer examination of Figure 3a also shows evidence of a cyclical effect in the residual variance. During the recessions of 1981-83, 1990-92, and 2000-2002, the actual variance grows faster than the counterfactual variance. This is consistent with less-skilled workers—who tend to have a lower within-group variance—being more adversely affected in terms of their employment during recessions. It is well known that

composition effects tend to hide the pro-cyclicality of the level of real wages (Barsky et al, 1994). By analogy, Figure 3a suggests that composition effects tend to over-state the counter-cyclical pattern in wage inequality over the business cycle (inequality grows during recessions).

Figure 3a also shows the counterfactual variance when the distribution of characteristics is held constant at its 2003 level. The results are qualitatively similar, though not as dramatic, as those obtained by holding characteristics at their 1973 level. In particular, the counterfactual variance declines less dramatically in the 1990s when characteristics are held at their 2003 instead of 1973 level.<sup>14</sup>

The results for women in Figure 3b are qualitatively similar to those for men. Composition effects explain most of the growth in the within-group variance between 1973 and 2003 when characteristics are held at their 1973 level. Composition effects also play a qualitatively similar, though less dramatic, role when characteristics are held at their 2003 level instead.

The results for both men and women are summarized in Table 2. The table confirms that composition effects account for most of the growth in the residual variance between 1973 and 2003 when the distribution of experience and education is held at its 1973 level. Once again, the results are less dramatic when the distribution of experience and education is held at its 2003 level instead. Even in this case, however, composition effects still account for about half of the growth in the residual variance for men, and for a third of the growth in the residual variance for women.

Table 2 also compares the growth in the residual variance to the growth in the total variance of wages (both within- and between-group components) over the same periods. Interestingly, over the whole 1973-2003 period, the residual component of the variance accounts for less than half of the growth in the total variance (43 percent for men, 46 percent for women). This finding is at odds with several previous studies that tend to find that most of the growth in wage inequality is due to the residual component.

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<sup>14</sup> The difference stems from the fact that holding characteristics at their 1973 level puts relatively more weight on high-school dropouts who experience a clear decline in their within-group variance (Figure 2). By contrast, holding characteristics at their 2003 level puts relatively more weight on college graduates who experience a clear increase in their within-group variance.

I explain in Section 5 that this earlier finding appears to be an artifact of measurement problems in the March CPS.

When the distribution of experience and education is held at its 1973 level, the remaining growth in the residual variance only accounts for 14 percent of the 1973-2003 growth in the total variance of wages for men, and 5 percent for women. These percentages increase to 23 and 31 percent, respectively, when the distribution of experience and education is held at its 2003 level instead. Table 2 also shows that when the skill distribution is held constant, there is more growth in the residual variance between 1979 and 1989 than for the whole 1973-2003 period. This result holds for both men and women when skills are either held at their 1973 or 2003 levels. For example, the residual variance for men increases by 0.034 between 1979 and 1989 when the distribution of skills is held at its 2003 level. This is larger than the 0.025 change over the whole period. This means that for the other sample periods (1973 to 1979 and 1989 to 2003) pooled together, the residual variance *declined* by 0.09.

In light of the discussion in Section 2, these findings suggest that changes in the prices of unobserved skills only play a modest role in the overall growth in wage inequality between 1973 and 2003. For men, changes in the prices of unobserved skills account for no more than a quarter of the growth in overall inequality. For women, changes in the price of unobserved skills account for between 5 and 31 percent of the overall growth inequality.

The results also imply that all of the growth in the price of unobserved skills is concentrated in the 1980s. This finding is difficult to reconcile with the skill-biased technological change (SBTC) hypothesis that typically states that the relative demand for skills also increased during the 1970s and the 1990s. I return to the question of what else may explain the pattern of growth in residual wage inequality in Section 6.

Finally, the main findings are robust to the choice of alternative measures of wage dispersion. Figure 4 reproduces the results of Figure 3 using the 90-10 residual gap instead of the residual variance. The results are very similar to those for the residual variance. As in the case of the residual variance, almost all the growth in the 90-10 residual gap is concentrated in the 1980s (first half of the 1980s for men in Figure 4a). Furthermore, most of the growth in the 90-10 residual gap appears to be a spurious

consequence of composition effects. When the distribution of experience and education is held at its 1973 level (dotted line in the figures), the 90-10 residual gap in the early 2000s is barely higher than in 1973.

Interestingly, all of the remaining growth in the 90-10 is driven by inequality growth in the upper end (90-50) of the distribution. In fact, Appendix Figure 3 shows that, for both men and women, the 50-10 gap declined while the 90-50 gap increased over time once composition effects are controlled for. This mirrors the earlier finding that residual inequality increased in the upper part of the wage distribution (college educated workers) but decreased or remained stable in the lower part of the distribution (high school graduates and dropouts).

## **5. What is wrong with the March CPS?**

As mentioned in the Introduction, the findings of Section 3 and 4 are at odds with most of the previous literature on residual wage inequality. In addition to composition effects, one potential explanation for this difference is that I use data on hourly wages from the May and ORG supplements of the CPS, while earlier studies typically use the March Supplement of the CPS.

In this Section, I show that the key problem with the March CPS is that it poorly measures the wages of workers paid by the hour (the majority of the workforce). To establish this point, however, I first need to process the two different data sources to make them as comparable as possible. I then turn to several pieces of evidence to show that both the level and trends in residual inequality are systematically biased in the March CPS because of the mismeasurement of the wages of workers paid by the hour.

### *a. Data processing*

Both the May/ORG and the March CPS can be used to compute hourly wage rates. The March Supplement of the CPS asks about total earnings during the previous year. An hourly wage rate can then be computed by dividing last year's earnings by total hours worked last year. The latter variable is computed by multiplying two other variables available in the March CPS, usual weekly hours of work last year and weeks worked last year.

For historical reasons, however, many studies based on March CPS data proxy for hourly wage rates by focusing only on the earnings of full-time (and sometimes full-year) workers. The reason is that prior to 1976, the March CPS only asked about full-time/part-time status last year (instead of usual hours of work last year). Furthermore, the information about weeks worked last year was limited to few intervals (0, 1-13, 14-26, 27-39, 40-47, 48-49, 50-52) in the pre-1976 March CPS. One important drawback of this alternative wage measure, however, is that it is limited to the subset of the workforce that works full-time (and sometimes full-year). This is particularly problematic for women. It also fails to control for the dispersion in hours of work among workers who work full-time (35 hours and more a week).

Since we now have almost 30 years of data for which hourly wages rates can be directly computed for all workers, I limit the analysis of wages in the March CPS to the period starting with the earnings year 1975 (March 1976 survey). Another reason for starting with the wage data for 1975 is that the other wage measure available in the May/ORG CPS is only available from May 1973 on. Since one contribution of the paper is to compare the two data sources, the gain of using a more precise and comparable measure of hourly wages from the March CPS clearly outweighs the cost of losing two years of data for 1973 and 1974.<sup>15</sup>

There are important differences between the way wages are measured in the March and May/ORG CPS. First, while the March CPS asks about retrospective measures of wages and earnings (last year), the May/ORG supplement asks about wages at the time of the survey. Second, the May/ORG wage questions are only asked to wage and salary workers. By contrast, the March CPS asks separate questions about wage and salary earnings and self-employment earnings. To get comparable wage samples, I limit my analysis of the March data to wage and salary earnings. One problem is that when workers have both wage and salary and self-employment earnings, we do not know how many hours of work pertain to wage and salary jobs vs. self-employment. To minimize the impact of these considerations, I limit my analysis to wage and salary workers with

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<sup>15</sup> Another problem is that since missing wages were not allocated in the May 1973-78 CPS, allocated wages and earnings should be excluded from the March CPS for the sake of comparability. Unfortunately, individual earnings allocation flags are not available in the March CPS prior to the 1976 survey (Lillard, Smith, and Welch, 1986). Though family earnings allocation flags can be used instead (see JMP), this is one more reason for focusing on the March CPS data starting with the earnings year 1975.

very limited self-employment earnings (less than ten percent of wage and salary earnings).

Another difference is that the ORG supplement only asks questions about the worker's main job (at a point in time) while the March CPS includes earnings from all jobs, including second jobs for dual job holders. Fortunately, only a small fraction of workers (around 5 percent typically) hold more than one job at the same time. Furthermore, these secondary jobs represent an even smaller fraction of hours worked.

Finally, since the May/ORG CPS is a "point-in-time" survey, the probability that an individual's wage is collected depends on the number of weeks worked during a year. By contrast, a wage rate can be constructed from the March wage information irrespective of how many weeks (provided that it is not zero) are worked during the year. This means that the May/ORG wage observations are implicitly weighted by the number of weeks worked, while the March wage observations are not.

One related issue is that several papers like DiNardo, Fortin and Lemieux (1996) also weight the observations by weekly hours of work to get a wage distribution representative over the total number of hours worked in the economy. Weighting by weekly hours can also be viewed as a reasonable compromise between looking at full-time workers only (weight of 1 for full-time workers, zero for part-time workers) and looking at all workers as "equal" observations irrespective of the number of hours worked. Throughout the paper, I thus weight the March CPS observations by annual hours of work, and weight the May/ORG observations by weekly hours of work.

In both the March and ORG supplements of the CPS, a growing fraction of workers do not answer questions about wages and earnings. The Census Bureau allocates a wage or earnings item for these workers using the famous "hot deck" procedure. The CPS also provides flags and related sources of information that can be used to identify workers with allocated wages in all years except in the January 1994 to August 1995 ORG supplement.<sup>16</sup> By contrast, in the May 1973-78 CPS, wages were *not* allocated for

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<sup>16</sup> Allocation flags are incorrect in the 1989-93 ORG CPS and fail to identify most workers with missing wages. Fortunately, the BLS files report both edited (allocated) and unedited (unallocated) measures of wages and earnings. I use this alternative source of information to identify workers with allocated wages in these samples.

workers who failed to answer wage and earnings questions.<sup>17</sup> For the sake of consistency across data sources, all results presented in the paper only rely on observations with non-allocated wages, unless otherwise indicated.

Wages and earnings measures are topcoded in both the March and May/ORG CPS. Topcoding is not much of an issue for workers paid by the hour in the May/ORG CPS. Throughout the sample period, the topcode remains constant at \$99.99 and only a handful of workers have their wage censored at this value. By contrast, a substantial number of workers in the March CPS, and non-hourly workers in the May/ORG CPS, have topcoded wages. When translated on a weekly basis for full-year workers, the value of the topcode for annual wages in the March CPS tends to be comparable to the value of the topcode for weekly wages in the May/ORG CPS. For instance, in the first sample years (1975 to 1980) the weekly topcode in the May/ORG CPS is \$999 compared to \$962 for full-year workers in the March CPS (annual topcode of \$50,000). In the last sample years (1998 to 2001), the weekly topcode in the ORG CPS is \$2884, which is identical to the implied weekly topcode for full-year workers in the March CPS (annual topcode of \$150,000 divided by 52). Following most of the literature, I adjust for topcoding by multiplying topcoded wages by a factor 1.4.

In Appendix A, I discuss in detail how the data are processed to handle topcoding in a consistent fashion over time. One particular problem is that until March 1989, wages and salaries were collected in a single variable pertaining to all jobs, with a topcode at \$50,000 until 1981 (survey year), \$75,000 from 1982 to 1984, and \$99,999 from 1985 to 1988. Beginning in 1989, the March CPS started collecting wage and salary information separately for main jobs and other jobs, with topcodes at \$99,999 for each of these two variables. The topcodes were later revised to \$150,000 for the main job and \$25,000 for other jobs in March 1996. I explain in Appendix A how I “re-topcode” total wage and

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<sup>17</sup> There has been some confusion in the literature because of the lack of good documentation on the allocation of missing wages in the 1973-78 CPS. Several papers assume that, like in the March CPS prior to 1976, wages were allocated but not flagged in the May 1973-78 CPS. For example, Katz and Autor (1999) compare a (May CPS) sample without allocated wages in 1973 to a sample with allocated wages in 1979. This likely overstates the growth in residual wage inequality during the 1970s since residual wage dispersion is generally higher when allocated wages are included than when they are not (see Figure 7). See Hirsch and Schumacher (2004) for a detailed discussion of how wages are allocated (or not allocated) in the May/ORG CPS.

salary earnings at \$99,999 in the March 1989 to March 1995 surveys, and at \$150,000 from March 1996 on.

Finally, I also follow the existing literature by trimming very small and very large value of wages to remove potential outliers. Following Card and DiNardo (2002), I remove observations with an hourly wage of less than \$1 or more than \$100 in 1979 dollars. I also limit the analysis to workers age 16 to 64 with positive potential experience (age-education-6).

*b. Mismeasurement of the wages of workers paid by the hour in the March CPS*

From the above discussion, it is clear that wages computed using the March and May/ORG CPS could differ for a variety of reasons including the treatment of self-employment earnings, topcoding, etc. Instead of looking systematically at all possible sources of differences between the two data sources, I focus on the fact that earnings are collected on a yearly basis in the March CPS, while workers can report their earnings at different periodicities in the May/ORG CPS.

In particular, around 60 percent of workers in the May/ORG CPS are paid by the hour (see Figure 8 for more detail). These workers report a direct measure of their hourly wage rate in the May/ORG CPS. In the March CPS, however, they have to report their total annual earnings and hours of work that are then used to compute an average hourly wage rate.

In the absence of measurement error, it should not matter whether hourly wages are computed directly from questions about hourly wage rates, or indirectly by dividing earnings by hours of work. Several validation studies show, however, that there is substantial measurement error in the earnings reported in the CPS or similar surveys.<sup>18</sup>

It is plausible to think that asking directly hourly-rated workers about their hourly wage rates provides a more accurate wage measure than dividing earnings by hours. If it is easier for workers paid by the hour to report directly their hourly wage rate, this direct measure will likely be less affected by measurement error than the indirect wage measure based on average hourly earnings. For example, a minimum wage worker will likely

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<sup>18</sup> Mellow and Sider (1983) compare employee and employer responses in the January 1977 Validation Study of the CPS. Bound and Krueger (1991) compare employee responses from the March 1977 and 1978 CPS to employer reported Social Security Earnings.

know and correctly report the exact value of the hourly wage at which he or she is paid. The same worker would probably have more difficulty reporting total hours and earnings during the year. In fact, the U.S. Census Bureau and other national statistical offices often mention the case of the minimum wage as one reason for asking directly workers paid by the hour about their hourly wage rate.

My basic hypothesis is that for hourly-rated workers, the hourly wage rate indirectly computed from the March CPS is a more noisy measure of the true hourly rate of pay than the hourly wage rate collected in the May/ORG CPS. For workers not paid by the hour, the hourly wage rate has to be indirectly computed by dividing earnings by hours in both the May/ORG and the March CPS. Therefore, I do not expect the hourly wage from the March CPS to be a more noisy measure for these workers.

Under the additional assumption of classical measurement error, this hypothesis yields several clear empirical predictions.<sup>19</sup> The most direct prediction is that the variance of March CPS wages should be larger than the variance of May/ORG CPS wages among workers paid by the hour. I test this prediction by comparing the variance of the two wage measures for workers paid by the hour and workers not paid by the hour.

One problem with implementing this test is that the March CPS does not ask individuals whether they are paid by the hour or not. Fortunately, this problem can be resolved by exploiting the rotation group feature of the CPS. Among individuals sampled in the March CPS, roughly one quarter of individuals rotate out of the CPS in each of the four following months, including March. This means that from 1979 on, all individuals in the March CPS should eventually be part of the outgoing rotation group in March, April, May or June. In principle, their responses to the ORG supplement questions can thus be matched to their March CPS records. As discussed below, however, not all March respondents can be matched because of attrition and other data problems.

Prior to 1979, it is still possible to match the May CPS responses to the March responses for the March respondents who are still in the CPS in May (about half of the March respondents if there is no attrition). My empirical strategy is thus to match the March CPS respondents to either their ORG or May CPS records. From this matched

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<sup>19</sup> Under classical measurement error, the measurement error in March wages (for workers paid by the hour) is assumed to have mean zero and be independent of all observable variables.

sample, I can then use the information from the ORG or May CPS questions to divide workers into those paid and not paid by the hour.

Working with these matched samples involves a number of data issues that are beyond the scope of this paper. In particular, between five and ten percent of respondents cannot be matched because of attrition and other data problems.<sup>20</sup> Also, while the March and May/ORG wage records are for the same respondent, they are not for the same period (March wage is for last year, May/ORG wage is for the current survey month). This means that some workers coded as “paid by the hour” may not have been paid by the hour in the previous year. Focusing on workers who both report a wage in the month of the survey (the May/ORG wage) and in the previous year also results in a more “stable” sample of workers. Fortunately, Appendix Figures 1a and 1b show that while the level of wage inequality is lower for this matched sample than for the full sample, the trends in inequality are very similar for the two samples.

Despite these data limitations, a striking pattern of results emerges from Figures 5a (men) and 5b (women). These figures contrast the variance of the March and May/ORG wages for the two groups of workers (paid by the hour or not). For both men and women, the variance of March wages is systematically larger than the variance of May/ORG wages for workers paid by the hour. By contrast, there is no systematic difference in the variance of March and May/ORG wages for workers not paid by the hour. Figures 5a and 5b provide clear evidence that the key difference between the March and May/ORG wages is that the wages of workers paid by the hour are more noisily measured in the March CPS.

The extent of measurement error in March CPS for workers paid by the hour is both quantitatively and statistically significant. For men (Figure 5a), the average difference in the variance is 0.064, which represents about a third of the average variance in the May/ORG CPS (0.198). If the May/ORG wages were measured without error, this would imply a noise to signal ratio of about a third in the March CPS. The results are similar for women. The average difference in variances (0.055) also represents a third of the average variance of wages in the May/ORG CPS (0.167).

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<sup>20</sup> Since I am only matching months close by, the matching rates are much higher than in most applications where records in one year are matched to the record for the same respondent one year later.

Despite these large differences in levels, the pattern of change in the variances over time is very similar in the March and May/ORG CPS. For both wage measures, the variance of wages for hourly workers is flat in the 1970s, grows sharply in the 1980s, and remains relatively constant thereafter. For workers not paid by the hour, however, the variance of wages keeps increasing steadily during the 1990s. This is consistent with the fact that workers not paid by the hour are much more educated than workers paid by the hour (see below), and that within-group inequality increases for college educated workers (Section 3).

There is a second empirical prediction about measurement error in the March CPS that can be tested without resorting to the matched sample. Under the assumption of classical measurement error, the additional noise in the March CPS measure of wages (for hourly workers) should not affect estimates of the conditional means of wage (by education, age, etc).<sup>21</sup> This means that measurement error should have no effect on the between-group variance of wages (i.e. the dispersion in conditional means). If hourly wages from the March CPS are simply a noisier measure of hourly wages than wages in the May/ORG CPS (for hourly workers), then the two wage measures should yield similar between-group variances of wages. The measurement error should just increase the within-group, or residual, variance of wages in the March relative to the May/ORG CPS.

Figures 6 and 7 confirm this empirical prediction. Figure 6a shows the evolution of the between-group variance for men over the 1975-2002 period for the two measures (March and May/ORG) of hourly wages.<sup>22</sup> In the case of hourly wages computed from the March CPS, I report the between-group variance with and without observations with allocated earnings. The figure shows that including observations with allocated earnings has essentially no impact on the between-group variance. This suggests that the mean of allocated wages by age and education categories are similar to the mean for observation with valid (non-missing) wages.

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<sup>21</sup> The assumption is reasonable since both Mellow and Sider (1983) and Bound and Krueger (1991) find that measurement error in the CPS earnings in the late 1970s is uncorrelated with typical regressors like experience and education.

<sup>22</sup> Figures 6 and 7 report the variance of wages by earnings year (year of the survey in the May/ORG CPS, previous year in the March CPS). I report data for 1975 to 2002 that correspond to the 1976 to 2003 survey years in the March CPS.

More importantly, the two wage measures yield very similar between-group variances of log wages. Both the levels and the trends in the two series are very similar. In particular, *all* the growth in the between-group variance is concentrated during the first half of the 1980s. The between-group variance is essentially constant between 1975 and 1980, and after 1985. This finding is very robust to the choice of hourly wage measure.

The results for women in Figure 6b are also robust to the choice of wage measure. The between-group variance obtained from the May/ORG and the March CPS (with and without allocators included) all show the same basic pattern. The between-group variance declines in the 1970s, grows sharply in the first half of the 1980s, and grows more slowly thereafter. One natural explanation for the continuing growth in the between-group variance throughout the 1980s and 1990s is that age-earnings profiles are getting steeper during this period because of the increased attachment of women to the labor force.<sup>23</sup>

Turning to residual wage dispersion, Figure 7a shows that, for men, the residual variance of March CPS wages (without allocated earnings) is systematically larger than the residual variance of May/ORG wages. The results in Figure 7b for women are very similar. A set of strong conclusions thus emerges from Figures 5, 6 and 7. First, Figure 5 clearly shows that wages are more noisily measured in the March CPS. Consistent with classical measurement error, Figures 6 and 7 show that these measurement problems do not affect between-group wage dispersion but spuriously inflate residual wage dispersion in the March CPS.

The findings in Figures 5, 6 and 7 clearly indicate that, relative to the May/ORG CPS, residual wage inequality is biased up in the March CPS because this wage measure poorly captures the hourly wage rate for workers paid by the hour. The ORG CPS supplement thus provides both a much larger sample of wage workers and more accurately measured wages. There is thus a strong case for using the May/ORG instead of the March CPS for studying the evolution of wage inequality over time.

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<sup>23</sup> See Blau and Kahn (1997) and Fortin and Lemieux (1998). The continuing growth in the between-group variance during the 1980s and 1990s may be a spurious consequence of the fact that age (or potential experience) is a poor and changing proxy for underlying actual experience. Wage differences across age groups may thus be growing even if wage differences across groups based on actual experience remain constant.

*c. Spurious Trends in Residual Wage Inequality in the March CPS?*

If the bias in residual wage inequality in the March CPS were constant over time, using the May/ORG or the March CPS would have little consequence for the interpretation of the sources of change in residual wage inequality. Unfortunately, Figure 7 shows that both the level and the secular growth in residual inequality are larger in the March than in the May/ORG CPS. In the case of men (Figure 7a), the residual variance of wages in the May/ORG CPS is stable during the 1970s, grows rapidly in the early 1980s, and remains fairly constant from the mid-1980s to the late 1990s. By contrast, the residual variance grows steadily from 1975 to 2002 when hourly wages are computed using the March CPS. Among women, there is also more growth in the residual variance of March relative to May/ORG wages, though the difference is not as marked as in the case of men.

One potential explanation for this difference is that the fraction of workers paid by the hour has been growing over time (Hamermesh, 2002). To see this, consider the case where, for hourly-rated workers, March wages are equal to May/ORG wages plus a measurement error with variance  $\sigma^2$ . It follows that the difference between the March and May/ORG variance for group  $j$  at time  $t$  is  $H_{jt}\sigma^2$ , where  $H_{jt}$  is the fraction of workers paid by the hour. The variance of residuals for group  $j$  in the March ( $V_{jt}^M$ ) and May/ORG ( $V_{jt}^O$ ) CPS are linked by the formula:

$$V_{jt}^M = V_{jt}^O + H_{jt}\sigma^2.$$

Under the assumption that the measurement error variance is constant over time, it follows that

$$\Delta V_{jt}^M = \Delta V_{jt}^O + \Delta H_{jt}\sigma^2,$$

where  $\Delta H_{jt}\sigma^2$  is the spurious growth in the March CPS variance due to changes in the fraction of workers paid by the hour.

Figure 8 shows that the fraction of workers paid by the hour has indeed increased over time. Since education is by far the most important factor explaining the propensity to be paid by the hour, I only report the fraction of workers paid by the hour by education

group in Figure 8.<sup>24</sup> Figure 8a (men) and 8b (women) show that the fraction of workers paid by the hour has increased by up to 15 percentage point (depending on the education group) between the mid-1970s and the late 1980s. Since Figure 5 suggests that  $\sigma^2$  is of the order of 0.05 to 0.07, the growth in the fraction of workers paid by the hours may have resulted in a spurious increase of up to 0.01 in the variance of March wages. This is very substantial relative to the 0.03 growth in the residual variance in the May/ORG CPS during the same period.

Another related problem with the March CPS is that the large difference in the fraction of workers paid by the hour by education category likely masks the importance of composition effects. Remember that composition effects represent the difference between the actual residual variance and the counterfactual variance obtained by replacing the skill composition in the end period (say 2003) by the skill composition in the base period (say 1973). As discussed earlier, the counterfactual puts much more weight on less educated workers and less weight on more educated workers. This results in large composition effects in the May/ORG CPS because the within-group variance among highly-educated workers is much larger than among less-educated workers.

The difference in the within-group variance across education should be lower in the March than in the May/ORG CPS because the variance among less-educated workers is inflated by the larger fraction of those workers being paid by the hour. Consider, for example, the case of college post-graduates relative to high school dropout. In the early 2000s, between 80 and 90 percent of high-school dropouts are paid by the hour compared to just more than 10 percent among college postgraduates, a difference of about 70 percentage points (Figure 8). In light of the evidence in Figure 3, this suggests that the within-group variance of high-school dropouts in the March CPS is inflated by about 0.05 relative to college postgraduates.<sup>25</sup> This represents about a third of the 0.15-0.20 difference in the within-group variance between college post-graduates and high-school dropouts during the same period. Consistent with this prediction, a closer examination

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<sup>24</sup> The fraction of workers paid by the hour declines as a function of experience. Relative to education, however, experience has a smaller impact (in absolute value) on the probability of being paid by the hour.

<sup>25</sup> In Figure 3, the difference between the March and the May/ORG variance among workers paid by the hour is over 0.07 in the early 2000s. Multiplying this difference by the difference in the fraction of workers paid by the hour (about 0.70) yields about 0.05.

of the March data indeed indicates that the difference between the variance of these two groups is about 0.05 lower in the March than in the May/ORG CPS data.<sup>26</sup>

Because of this problem, the reweighting procedure should yield smaller composition effects when applied to the March CPS instead of the May/ORG CPS. Appendix Figure 2 indeed show that composition effects in the March CPS are about a third smaller as in the May/ORG CPS (see Appendix B for more discussion of these results).

In summary, the mismeasurement of the wages of workers paid by the hour in the March CPS results in two major problems for analysis of residual wage inequality based on this data set. First, the growth in the fraction of workers paid by the hour in the 1970s and 1980s generates a spurious growth in within-group wage dispersion during this period. Second, the role of composition effects in the growth of residual wage inequality is understated because of the large difference between the fractions of highly- and less-educated workers who are paid by the hour. These findings reinforce the earlier conclusion that the May/ORG CPS should be used instead of the March CPS for analyzing the sources of change in residual wage inequality.

## **6. Residual wage inequality and rising returns to skill: how much of a connection?**

The “common wisdom” about residual wage inequality is that it grows steadily over time and accounts for most of the overall growth in wage inequality (JMP, Katz and Autor, 1999, Acemoglu, 2002). A very different picture emerges when the composition of the workforce is held constant over time and wages are measured using the “better” May/ORG CPS instead of the March CPS. In particular, I find that residual wage inequality only accounts for a modest share of the growth in overall inequality between 1973 and 2003. I also find that both the time-series and cross-sectional pattern of residual wage inequality move in tandem with other “between-group” wage differentials. From a time-series point of view, the growth in both residual and between-group wage inequality in all concentrated in the 1980s (Figure 6 and 7). From a cross-sectional point of view, the group of workers for which residual inequality grows the most (college-

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<sup>26</sup> For example, among men in 2000-01, the ORG CPS variance is 0.319 and 0.132 for college postgraduates and high school dropouts, respectively (difference of 0.187). The March CPS variance for the same groups in 2000-01 is 0.374 and 0.226 (difference of 0.146).

educated workers) also happens to be a group for which relative wages expanded dramatically over the last 30 years.<sup>27</sup>

These findings have important implications for the interpretation of the role of residual wage inequality in the overall growth in wage inequality. Generally speaking, I find that changes in residual wage inequality are not that important and appear to be driven by the same factors that also influence other “between-group” wage differentials. Interestingly, the findings also help “resolve” the problems with the traditional explanation proposed by JMP to explain the pattern of change in residual wage inequality over time. One problem is that the presumed large growth in the return to unobserved skills is inconsistent with the evolution of other wage differentials like the gender gap (Card and DiNardo, 2002). The other problems have to do with explanations for the differential timing in the growth of residual and between-group inequality. These are no longer problems since I show that the returns to unobserved skills did not grow very much, and that when they did they moved in tandem with other “between-group” wage differentials.

More specifically, the time-series pattern of change in residual wage inequality generally reinforces the conclusion of Card and DiNardo (2002) that the timing of the growth in wage inequality is difficult to reconcile with the SBTC hypothesis. This paper poses a further challenge to the SBTC hypothesis since I find that residual inequality actually *declined* in periods other than the 1980s. Technological change can only explain these changes under the implausible assumption that while technological change was biased in favor of skilled workers during the 1980s, it was biased in favor of unskilled workers during the other periods.<sup>28</sup>

This suggests looking at other possible explanations for changes in residual wage inequality like the minimum wage. In particular, DiNardo, Fortin, and Lemieux (1996)

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<sup>27</sup> Because of space limitations, I do not present detailed information on other wage differentials. Both Mincer (1997) and Deschênes (2001) show, however, that (log) wages became an increasingly convex function of years of schooling since the 1970s. In other words, the gap between high school and college educated workers expanded dramatically, while the gap between high school graduates and dropouts remained more or less constant (or even declined in some cases).

<sup>28</sup> It is possible to argue that during the 1970s, the demand for skills due to SBTC was offset by a large increase in the supply of skills (college-educated labor). This “supply” story does not work for the 1990s, however (Beaudry and Green, 2004, Card and DiNardo 2002)

find that the decline in the real value of the minimum wage during the 1980s accounts for about a third of the increase in residual wage inequality. Lee (1999) finds an even larger effect by allowing for spillover effects of the minimum wage. Interestingly, the basic trends in the real value of the minimum wage are closely related to the trends in residual wage inequality documented above. For example, Row C of Table 2 shows that the real value of the minimum wage declined in the 1980s and early 2000s, while residual inequality increased during those two periods. By contrast, residual inequality declined when the real value of the minimum wage increased during the 1970s and 1990s. I explore the connection between the minimum wage and residual wage inequality in more detail in Appendix C and conclude that, as illustrated in Figure 9, changes in the real value of the minimum wage between 1973 and 2003 track remarkably well the changes in residual wage inequality over this period.

While the minimum wage explains well the time series pattern of the residual variance, it is not a credible explanation for the substantial growth in within-group inequality for the most highly educated workers, or for the related expansion in wage inequality in the upper part of the residual distribution (residual 90-50 gap). Clearly, something else needs to be brought in to explain the growth in inequality in the top end of the wage distribution.

Piketty and Saez (2003) also document a dramatic increase in inequality in the top end of the earnings distribution (using tax data) since the 1970s. They argue that both the timing (over the long run) and the extent of the growth in inequality at the top end are hard to reconcile with SBTC. They rather favor an alternative explanation based on social norms. It is nonetheless possible for more nuanced forms of technological change to disproportionately benefit some workers at top end of the wage distribution. For example, Goos and Manning (2003) show that the model of technological change of Autor, Levy, and Murnane (2003) where computerization only replaces “routine”, as opposed to “unskilled”, tasks can lead to an expansion of wage inequality in the upper part of the wage distribution, but in a reduction in inequality in the lower part of the distribution.<sup>29</sup>

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<sup>29</sup> The idea is that workers in the middle of the distribution are those who perform “skilled but routine” task. They are thus the most adversely affected by technological change.

A more traditional but related “human capital” explanation is that when the return to college education increases, we also expect the return to a “good” college education to increase even more. This idea can be captured in a “single index” model where total educational input is simply the product of years of schooling with school quality. This implies that school quality and years of schooling are perfect substitutes and that the within-group variance for college graduates (variance of school quality times the return to college) is proportional to the return to college. The important point here is that a standard human capital approach can explain the cross-sectional pattern of change in both residual wage inequality and between-group wage differentials that is observed in the data.

In summary, the fact that residual wage inequality growth is not that important quantitatively and moves in tandem with between-group inequality generally “simplifies the story” on changes in wage inequality. It leaves only two key questions to be answered. The first question is why the overall growth in wage inequality is so concentrated in the 1980s? The second question is why wage inequality has expanded so much in the upper end of the wage distribution? I have suggested possible answers to these questions but much nonetheless remain to be done in future research.

Finally, an important by-product of the paper is to show that the March CPS does not provide very accurate measures of wages for the majority of workers who are paid by the hour. The ORG supplement of the CPS provides more accurate measures of hourly wages for much larger samples of workers than the March CPS. Over thirty years of data are now available when the ORG CPS is combined with the 1973-78 May CPS. There is thus a strong case for using the May/ORG CPS, instead of the March CPS, for studying the determinants and the evolution of the structure of wages in the United States since the early 1970s.

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## **APPENDIX A: Topcoding**

### *Topcoding adjustments in the May/ORG and March CPS*

As mentioned in Section 5, adjusting for topcoding is relatively straightforward in the May/ORG CPS. Since the topcode for the hourly wage of workers paid by the hour is quite high (\$99.99), topcoding is not an issue for this group of workers. For workers not paid by the hour, the topcode on the edited weekly earnings variable goes from \$999 in 1973-1988 to \$1923 in 1989-1997, and \$2884 in 1998-2002. Between 1986 and 1988, however, it is possible to use the unedited weekly earnings variable which is topcoded at \$1999 instead of \$999. Though the unedited variable is not computed for workers who fail to respond to the earnings question, this does not matter here since I only use data for workers with unallocated wages and earnings. I thus use the unedited earnings variable for the 1986-88 period.

The situation is more complicated in the March CPS. Until March 1989 wages and salaries were collected in a single variable pertaining to all jobs, with a topcode at \$50,000 until 1981 (survey year), \$75,000 from 1982 to 1984, and \$99,999 from 1985 to 1988. Beginning in 1989, the March CPS started collecting wage and salary information separately for main jobs and other jobs, with topcodes at \$99,999 for each of these two variables. The topcodes were later revised to \$150,000 for the main job and \$25,000 for other jobs in March 1996.

Prior to March 1996, the earnings variable of workers who are topcoded simply takes the value of the actual topcode. Starting in March 1996, however, the value of earnings for topcoded workers is replaced by the mean earnings among all topcoded workers. Mean earnings are separately computed for different demographic groups. For example, in the March 2001 CPS, the mean for topcoded main job earnings ranges from \$195,699 for white females not working full-time full-year, to \$335,115 for full-time full-year white males. The corresponding means for these two groups are \$39,320 and \$56,879 for wage and salary earnings on other jobs.

To maintain consistency over time, I first construct a topcoded variable for total wage and salary earnings from March 1989 on. For 1989-1995, I simply keep the pre-1989 \$99,999 topcode. Since both main job and other job earnings are separately topcoded at \$99,999, I simply add these two earnings variables and topcode the sum at \$99,999. After various experiments, I decided to use a topcode of \$150,000 for total wage and salary earnings from 1996 on. Unfortunately, it is not possible to topcode total wage and salary earnings in a way that is completely consistent with the pre-1996 situation. The problem is with workers who earn less than \$125,000 on their main job but have earnings from other jobs topcoded at \$25,000. It is not possible to know whether total earnings of these workers are above or below \$150,000. After some experiments, I decided to compute total earnings as the sum of main job earnings (censored at \$150,000) and earnings on other jobs where I use the actual earnings provided in the CPS.

For example, consider a full-time full-year white male who earns \$90,000 on his main job but has his earnings topcoded at \$25,000 for other jobs in the March 2001 CPS. I compute total earnings as the sum of \$90,000 and \$56,879 (see above), which yields \$146,876. Since this is below the \$150,000 topcode, I do not compute further adjustments for this worker. By contrast, I would censor at \$150,000 the total earnings of the same worker if he earned \$100,000 instead of \$90,000 on his main job (total of \$156,876).

These adjustments likely have little impact since, in the March 1996-2002 CPS, less than one percent of workers have main job earnings below \$125,000 and are topcoded on their other jobs earnings. Finally, once total wage and salary earnings have been censored in a consistent fashion, I multiply the earnings of workers at this consistent topcode by the standard 1.4 factor.

## **APPENDIX B: Accounting for Composition Effects in the March CPS Wage Data**

Appendix Figures 2a (men) and 2b (women) compare the actual residual variance using the March CPS hourly wage rate to the residual variance that would have prevailed if the distribution of age and education had remained at its 1975 level. The re-weighting methodology used to compute the counterfactual variance is the same as for the May/ORG CPS (Figure 3). The figures show that the impact holding the distribution of characteristics constant is less dramatic in the March CPS than in the May/ORG CPS data. As discussed in Section 5c, an important part of this difference is due to differences (over skill groups) and changes (over time) in the fraction of workers paid by the hour.

Despite these differences, adjusting for composition effects still has a significant impact on the economic interpretation of the trends in the residual variance in the March CPS. In particular, the figures show essentially no growth in the within-group variance after 1987-88 when the distribution of experience and education is held at its 1975 level. For women, the pattern of growth in residual inequality in the March CPS is similar to the one in the May/ORG CPS (with or without adjustments for composition effects). All the growth in within-group inequality is concentrated in the first half of the 1980s. For men, the post-1980 growth in residual inequality also becomes qualitatively similar to the one in the May/ORG CPS. The only remaining discrepancy is that residual inequality grows rapidly in the March CPS during the 1970s, while it remains stable in the May/ORG data.

## **APPENDIX C: The Effect of the Minimum Wage on Residual Wage Inequality**

Figure 9 illustrates the close connection between the evolution of the minimum wage and the residual variance between 1973 and 2003. The figure compares the residual variance when characteristics are held constant at their 1973 level to the predicted variance from a regression that includes a linear trend and the log real minimum wage as regressors. This simple regression model explains the data quite well. The R-square is 0.81 and 0.88 for men and women, respectively. This is a remarkably good fit since there is almost no time trend in the dependent variable.

For both men (Figure 9a) and women (Figure 9b), the minimum wage has a strong impact on the residual variance. The regression models are reported in the figures and show large t-statistics for the effect of the minimum wage (t-statistic of 9 for men, and 12 for women). Consistent with DiNardo, Fortin and Lemieux (1996), the effect of the minimum wage is also larger for women than men. The fit of the model is most impressive for women. The large increases in the minimum wage in 1973-74, 1989-91, and 1995-97 all closely match corresponding declines in the residual variance. By contrast, the three periods where the minimum wage declined in real terms for failing to be indexed (1981-1989, 1992-1995, and 1998-2003) all correspond to clear increases in residual wage inequality.<sup>30</sup>

The estimated effect of the minimum wage is very similar when the regressions are fit to the residual variance that holds the distribution of characteristics at its 2003 (instead of 1973) level. In this case, however, the underlying time trend is small and positive, while it is negative and significant when characteristics are held at their 1973 level. These results suggest that there is essentially no growth in residual inequality left once composition effects and the impact of the minimum wage have been accounted for.

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<sup>30</sup> The three most important increases in the minimum wage are: from \$1.60 to \$2.00 in May 1974, from \$3.35 in March 1990 to \$4.25 in April 1990, and from \$4.25 in September 1996 to \$5.15 in September 1997. The real value of the minimum wage was substantially eroded by inflation as the minimum wage remained fixed at \$3.35 from January 1981 to March 1990, at \$4.25 from April 1991 to September 1996, and at \$5.15 from September 1997 to now.

Table 1a: Within-group variance of wages by experience-education cell for men, 1973-75 and 2000-02

	Within-group variance			Workforce share		
	1973-75	2000-02	Change	1973-75	2000-02	Change
	(1)	(2)	(3)	(4)	(5)	(6)
<i>A. By education and experience</i>						
Dropout:						
1-10	0.118	0.083	-0.035	0.065	0.035	-0.030
11-20	0.169	0.130	-0.038	0.052	0.026	-0.026
21-30	0.170	0.154	-0.017	0.055	0.025	-0.029
31+	0.180	0.162	-0.019	0.123	0.028	-0.095
High school graduates:						
1-10	0.130	0.130	0.000	0.137	0.082	-0.055
11-20	0.145	0.181	0.035	0.094	0.085	-0.009
21-30	0.162	0.196	0.034	0.069	0.086	0.017
31+	0.188	0.217	0.029	0.074	0.058	-0.016
Some college:						
1-10	0.143	0.152	0.008	0.076	0.077	0.001
11-20	0.173	0.204	0.031	0.036	0.075	0.039
21-30	0.216	0.227	0.012	0.025	0.072	0.048
31+	0.245	0.256	0.011	0.020	0.046	0.026
College graduates:						
1-10	0.161	0.224	<b>0.064</b>	0.048	0.061	0.014
11-20	0.204	0.276	<b>0.072</b>	0.022	0.063	0.041
21-30	0.220	0.310	<b>0.091</b>	0.017	0.051	0.034
31+	0.299	0.332	0.033	0.009	0.024	0.015
Post-graduates:						
1-10	0.217	0.316	<b>0.099</b>	0.034	0.023	-0.010
11-20	0.324	0.324	0.000	0.023	0.033	0.009
21-30	0.327	0.302	-0.025	0.015	0.033	0.018
31+	0.420	0.369	-0.051	0.006	0.016	0.010
<i>B. Weighted Average (using alternative shares)</i>						
Actual shares	0.173	0.214	0.041			
1973-75 shares	0.173	0.185	0.012			
2000-02 shares	0.191	0.214	0.023			

Table 1b: Within-group variance of wages by experience-education cell for women, 1973-75 and 2000-02

	Within-group variance			Workforce share		
	1973-75	2000-02	Change	1973-75	2000-02	Change
	(1)	(2)	(3)	(4)	(5)	(6)
<i>A. By education and experience</i>						
Dropout:						
1-10	0.099	0.056	-0.043	0.057	0.026	-0.031
11-20	0.130	0.090	-0.040	0.039	0.015	-0.024
21-30	0.125	0.106	-0.019	0.050	0.018	-0.032
31+	0.139	0.123	-0.017	0.103	0.023	-0.080
High school graduates:						
1-10	0.106	0.108	0.002	0.179	0.070	-0.109
11-20	0.145	0.157	0.011	0.095	0.072	-0.023
21-30	0.144	0.172	0.028	0.092	0.086	-0.006
31+	0.162	0.178	0.016	0.097	0.074	-0.023
Some college:						
1-10	0.118	0.137	0.019	0.077	0.091	0.014
11-20	0.134	0.198	<b>0.065</b>	0.025	0.081	0.057
21-30	0.152	0.209	<b>0.057</b>	0.020	0.084	0.064
31+	0.160	0.220	<b>0.060</b>	0.020	0.054	0.034
College graduates:						
1-10	0.134	0.179	<b>0.045</b>	0.055	0.076	0.020
11-20	0.170	0.260	<b>0.090</b>	0.015	0.058	0.043
21-30	0.173	0.262	<b>0.088</b>	0.014	0.052	0.038
31+	0.195	0.254	<b>0.059</b>	0.010	0.021	0.010
College post-graduates						
1-10	0.154	0.239	<b>0.085</b>	0.022	0.026	0.004
11-20	0.238	0.259	0.021	0.012	0.027	0.015
21-30	0.204	0.217	0.013	0.011	0.034	0.023
31+	0.280	0.234	-0.046	0.006	0.013	0.007
<i>B. Weighted Average (using alternative shares)</i>						
Actual shares	0.136	0.183	0.047			
1973-75 shares	0.136	0.148	0.012			
2000-02 shares	0.149	0.183	0.034			

Table 2: Composition Effects and Changes in the Residual Variance of Log Hourly Wages, May/ORG CPS

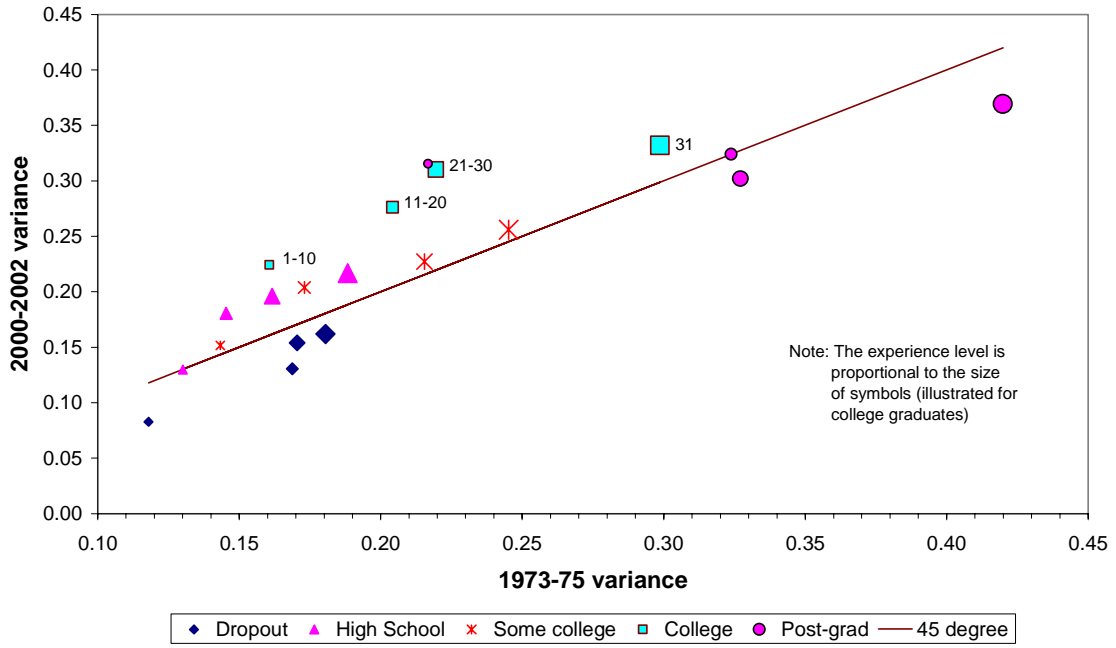
	1973- 1979	1979- 1989	1989- 1999	1999- 2003	1973- 2003
<i>A. Men</i>					
Residual variance:					
Actual change	-0.003	0.036	-0.003	0.017	0.047 [43]
1973 skills distribution	-0.003	0.027	-0.019	0.011	0.015 [14]
2003 skills distribution	-0.008	0.034	-0.013	0.012	0.025 [23]
Total variance:	-0.002	0.080	0.007	0.024	0.109 [100]
<i>B. Women</i>					
Residual variance:					
Actual change	-0.014	0.047	-0.001	0.013	0.045 [46]
1973 skills distribution	-0.017	0.036	-0.019	0.005	0.005 [ 5]
2003 skills distribution	-0.012	0.040	-0.006	0.008	0.030 [31]
Total variance:	-0.026	0.092	0.017	0.015	0.098 [100]
<i>C. Real value of the minimum wage (logs)</i>					
	0.103	-0.391	0.135	-0.099	-0.252

Note: Numbers in square brackets represents the percentage of the 1973-2003 change in the total variance of wages (both within- and between-group components) that is attributable to this variance component.

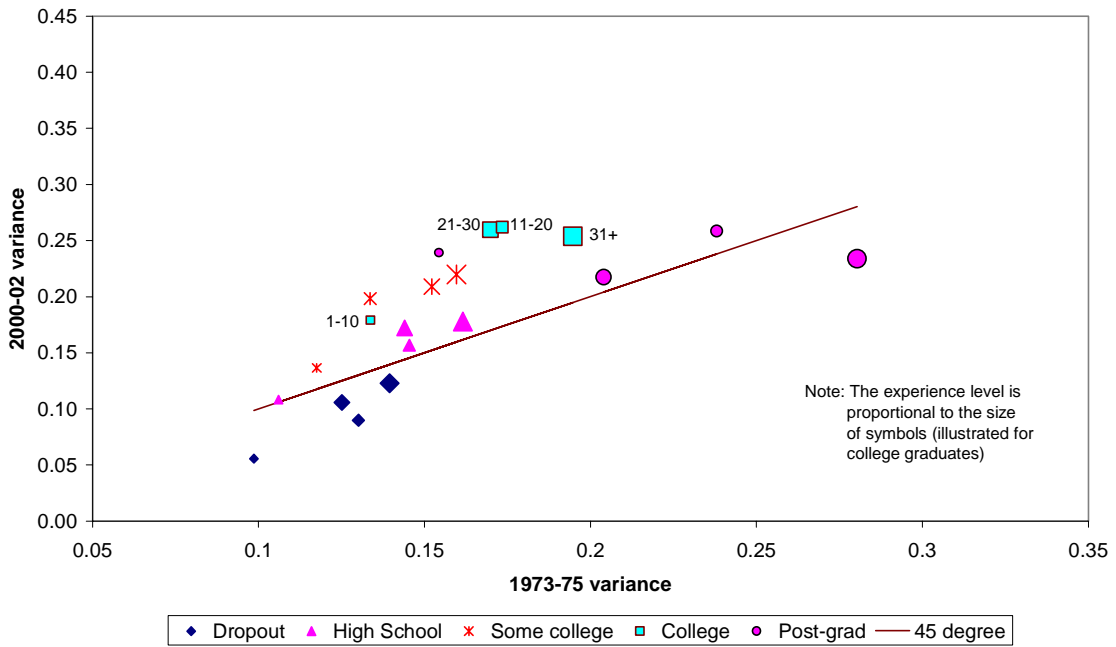
Appendix Table 1: Percentage distribution of workers by education and experience groups, May/ORG CPS

	Men				Women			
	1973-74	1980	1990	2002	1973-74	1980	1990	2002
<i>A. Education categories</i>								
High School Dropout	30.4	23.0	15.9	11.3	25.7	17.5	11.4	7.8
High School Graduate	37.4	37.9	38.1	31.1	46.3	46.0	41.5	29.7
Some college	15.3	18.1	20.4	27.1	13.7	18.7	23.2	31.1
Bachelors' Degree	9.1	11.6	14.8	20.1	9.3	11.0	14.8	21.1
Post-graduate Degree	7.7	9.4	10.9	10.6	5.0	6.9	9.2	10.3
<i>B. Years of Experience</i>								
0-10	35.8	39.4	31.9	27.0	38.5	41.4	33.8	28.3
11-20	22.7	24.5	32.8	27.8	18.5	22.8	29.5	24.8
21-30	18.2	16.4	19.5	27.1	19.1	16.6	21.0	27.4
31+	23.3	19.7	15.8	18.1	23.9	19.3	15.7	19.4

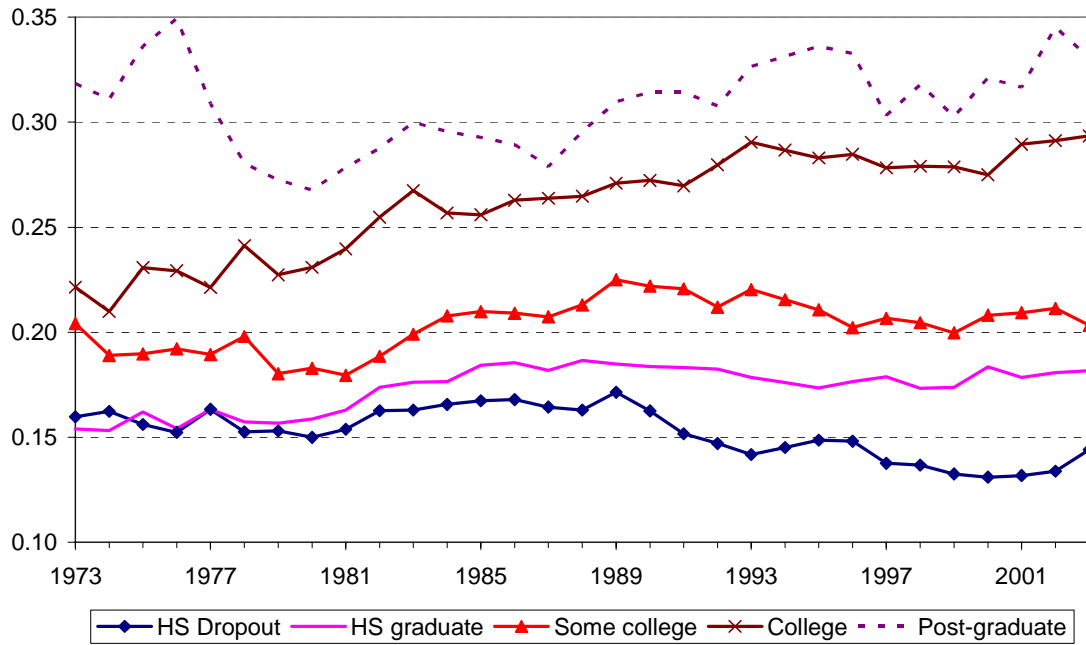
**Figure 1a: Change in the Male Within-Group Variance by Experience and Education Levels: 1973-75 to 2000-02**



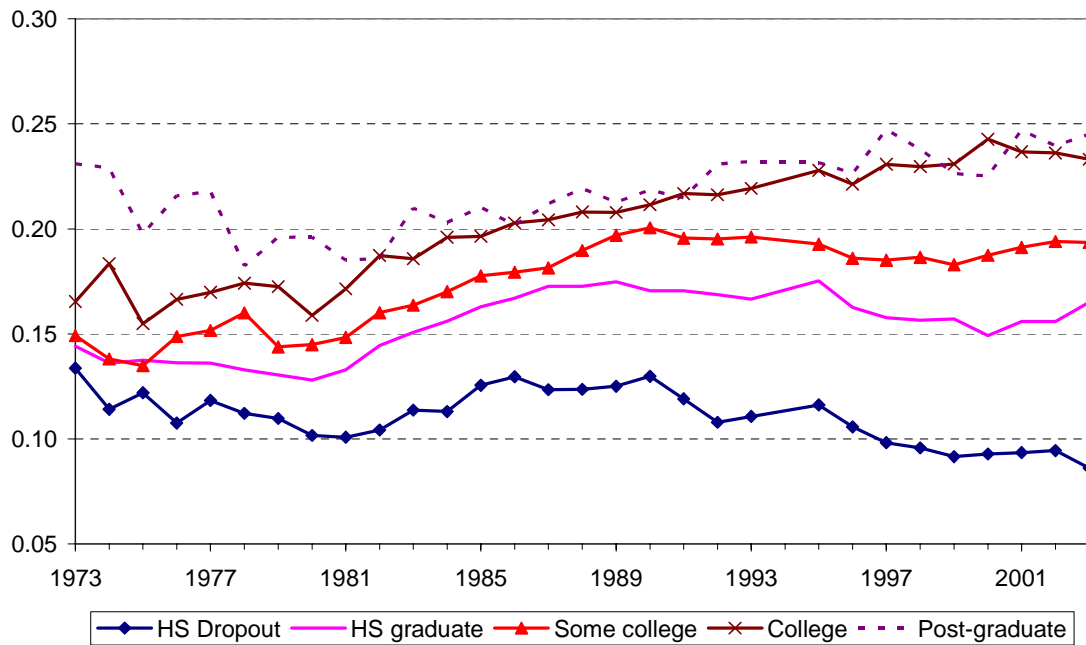
**Figure 1b: Change in the Female Within-Group Variance by Experience and Education Levels: 1973-75 to 2000-02**



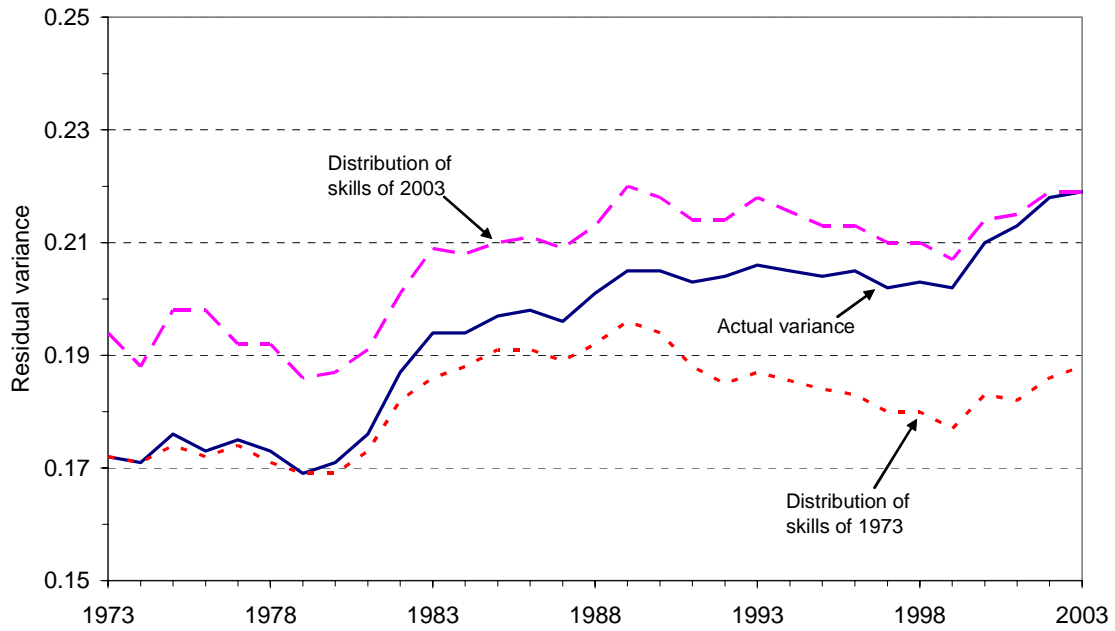
**Figure 2a: Within-group variance by education group for men,  
(average of the four experience groups)**



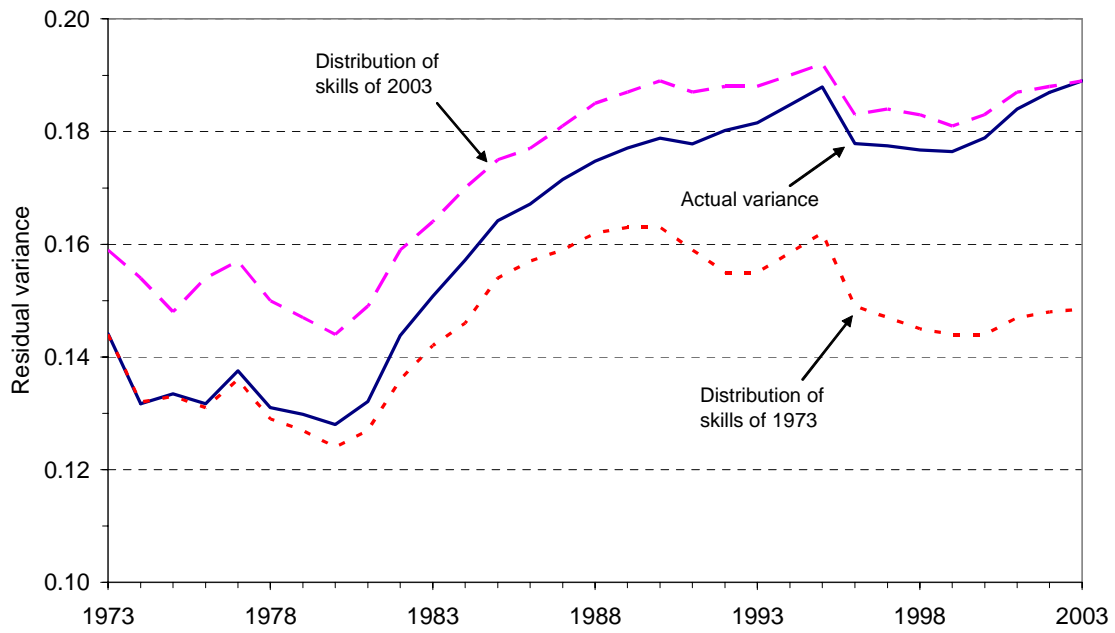
**Figure 2b: Within-group variance by education group for women  
(average of the four experience groups)**



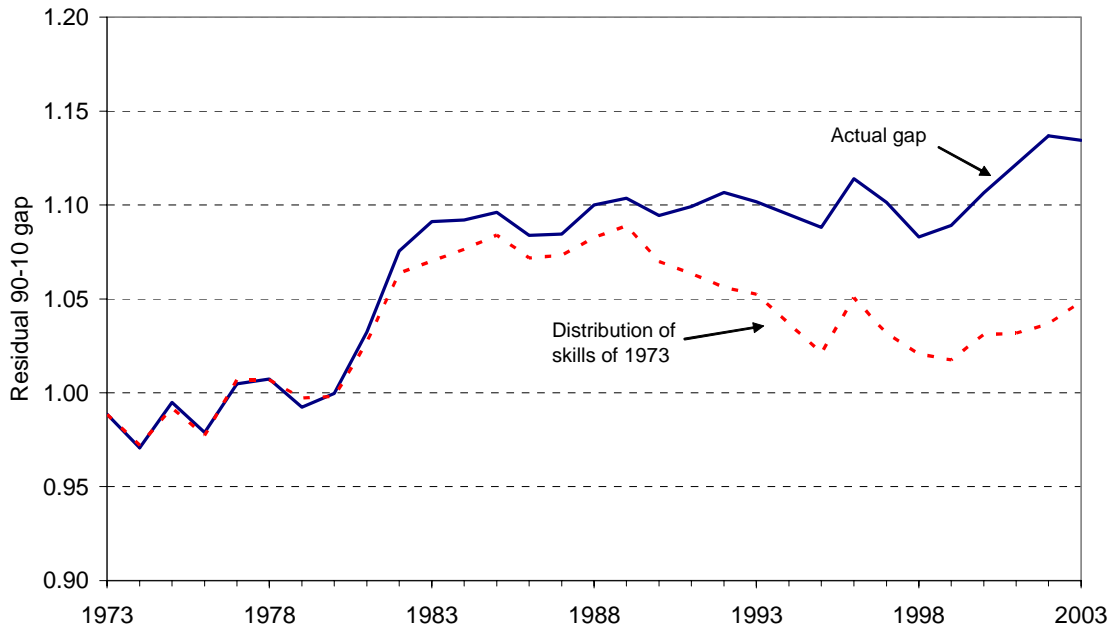
**Figure 3a: Actual and counterfactual residual variance of wages for men, 1973 to 2003**



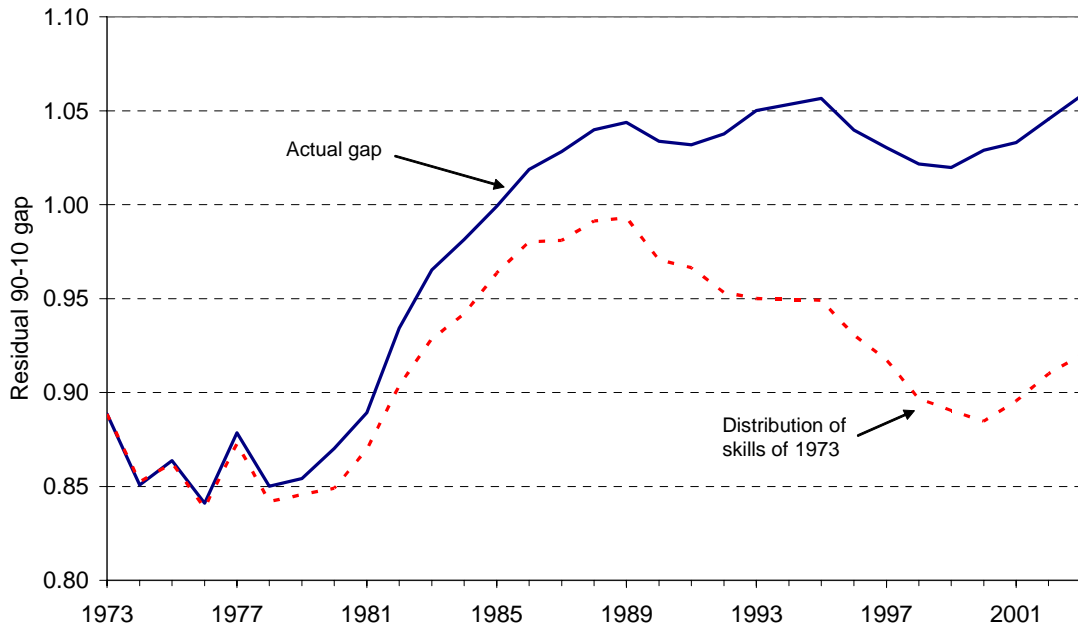
**Figure 3b: Actual and counterfactual residual variance of wages for women, 1973 to 2003**



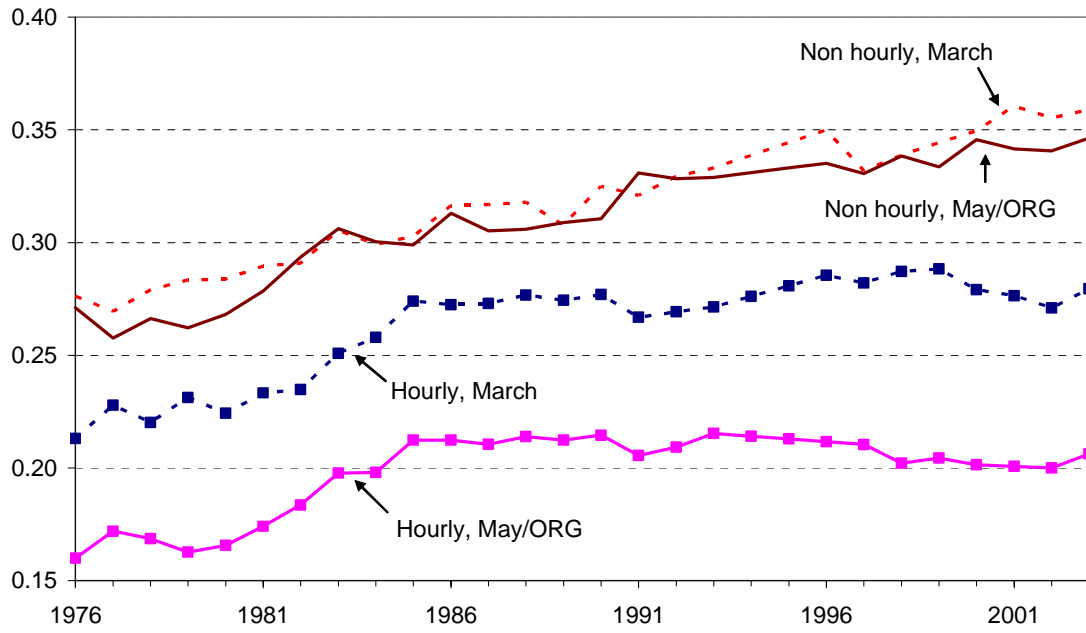
**Figure 4a: Residual 90-10 wage gap for men, holding distribution of skills at their 1973 level**



**Figure 4b: Residual 90-10 gap for women, holding distribution of skills at their 1973 level**



**Figure 5a: Variance of log hourly wages of men with both May/ORG and March wages (matched sample)**



**Figure 5b: Variance of log wages of women with both May/ORG and March wages (matched sample)**

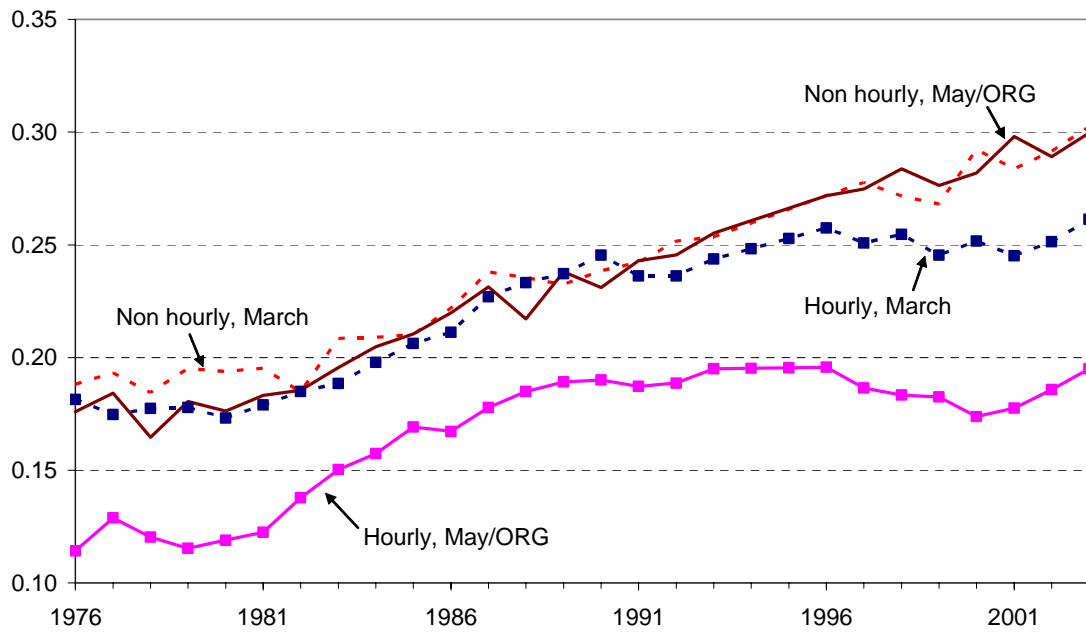


Figure 6a: Between-group variance, men

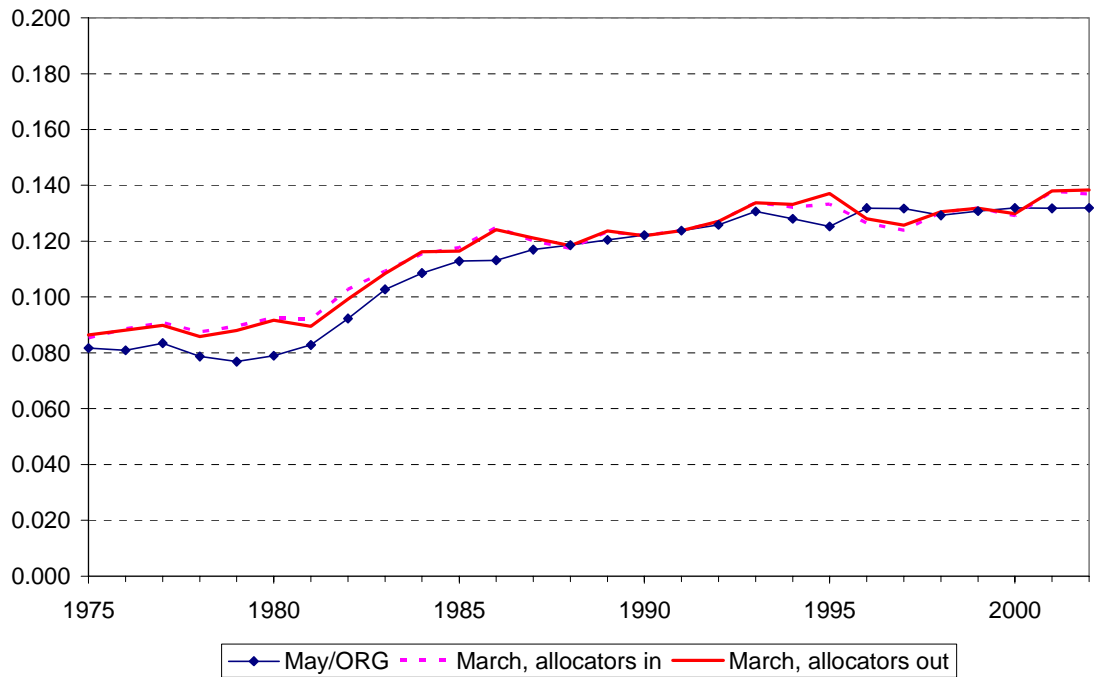


Figure 6b: Between-group variance, women

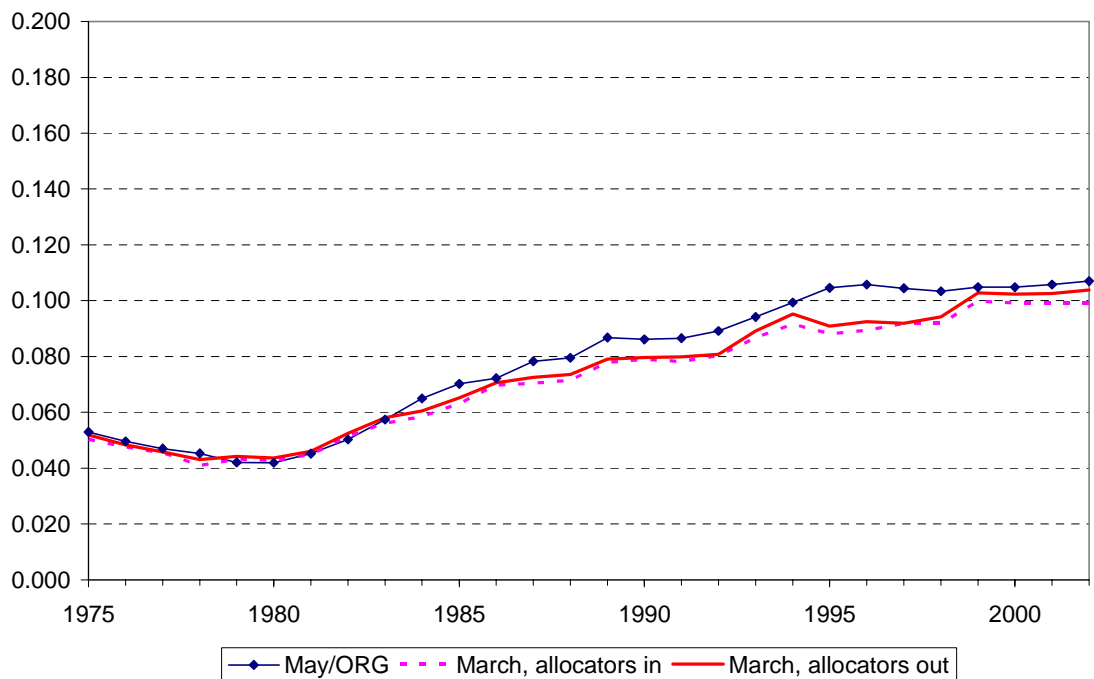


Figure 7a: Residual variance of wages, men

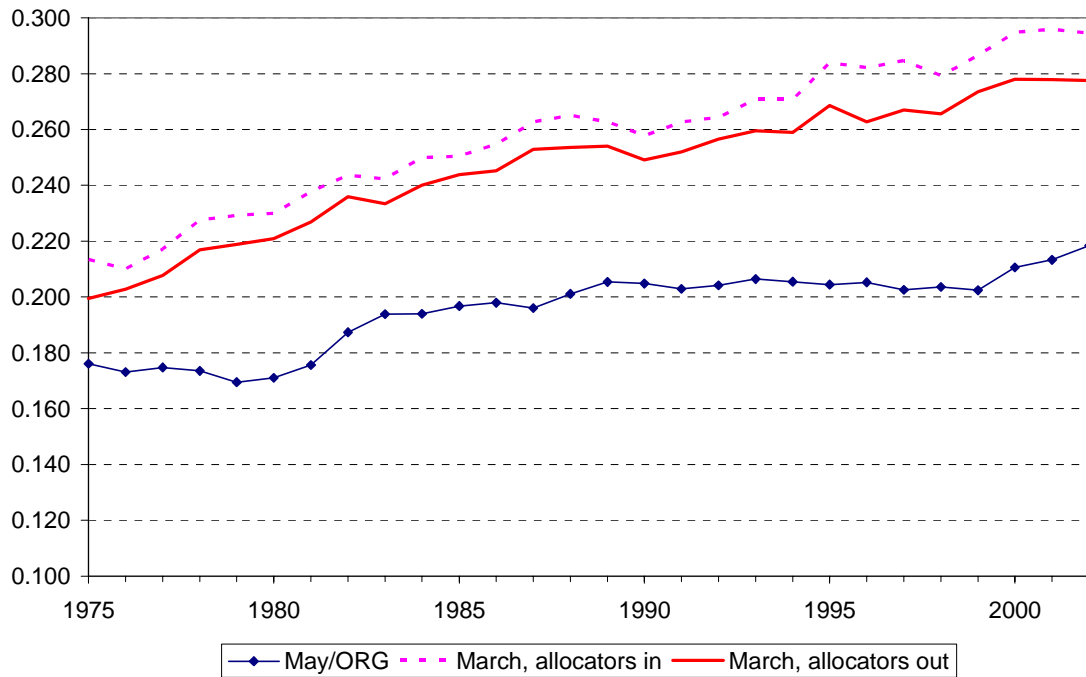
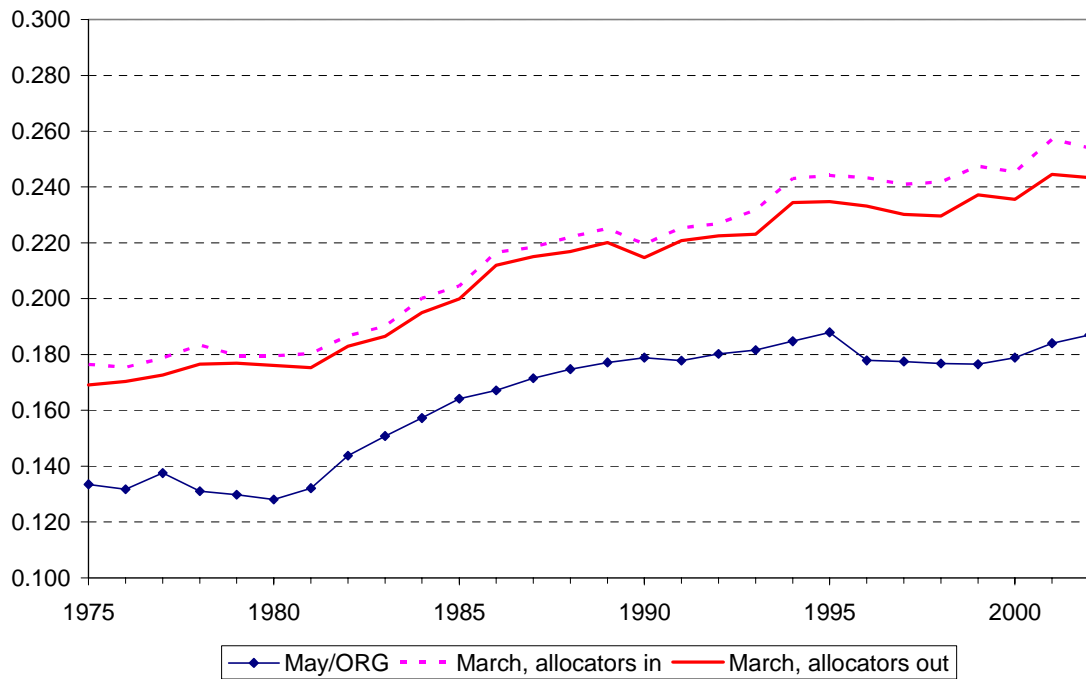
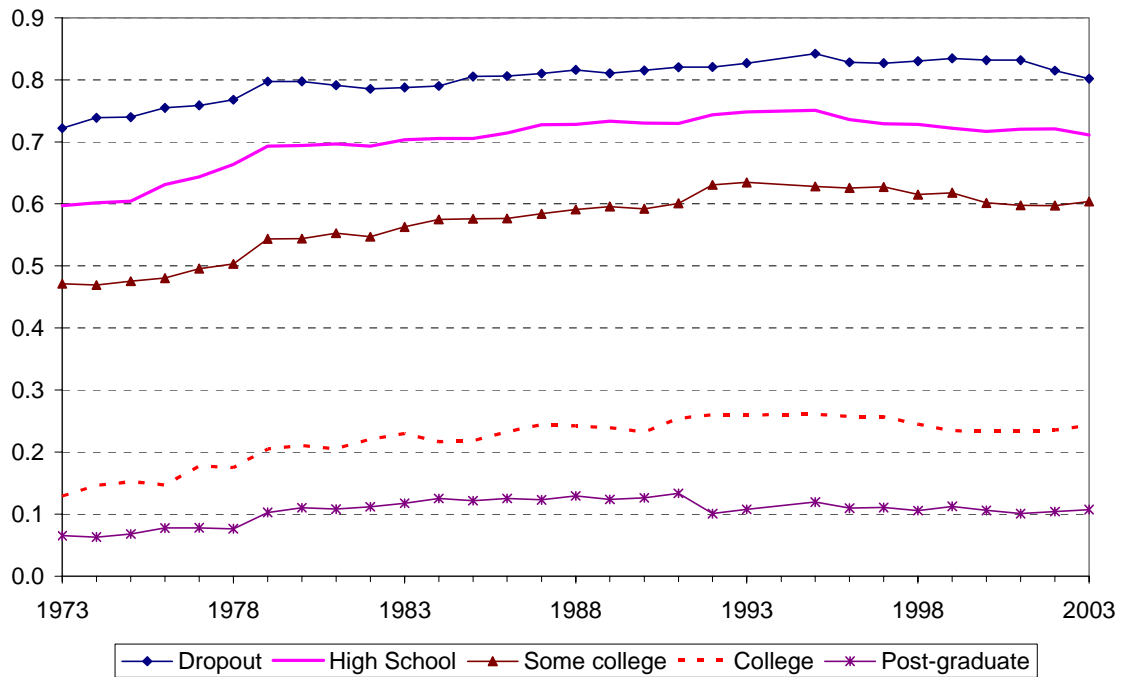


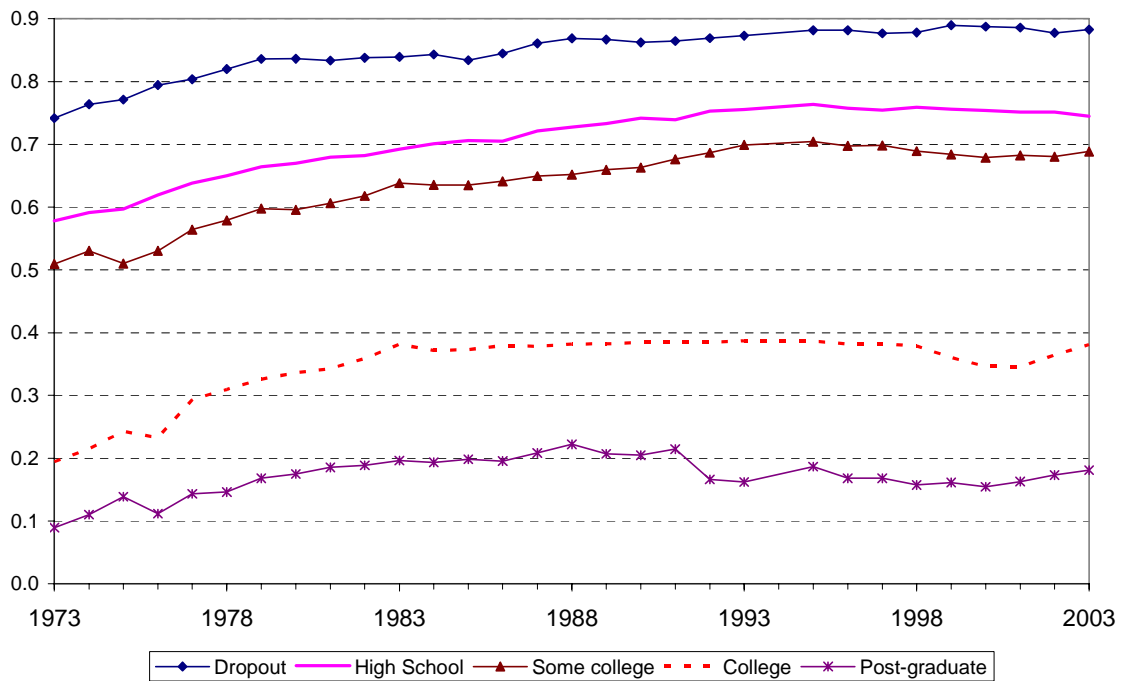
Figure 7b: Residual variance of wages, women



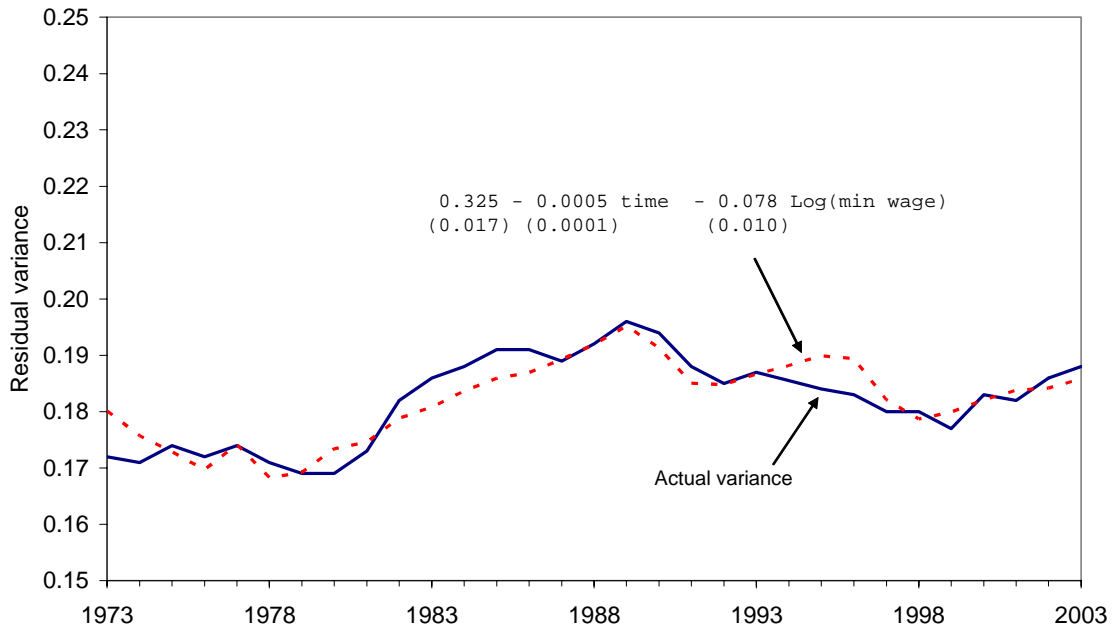
**Figure 8a: Fraction of men paid by the hour, by education category**



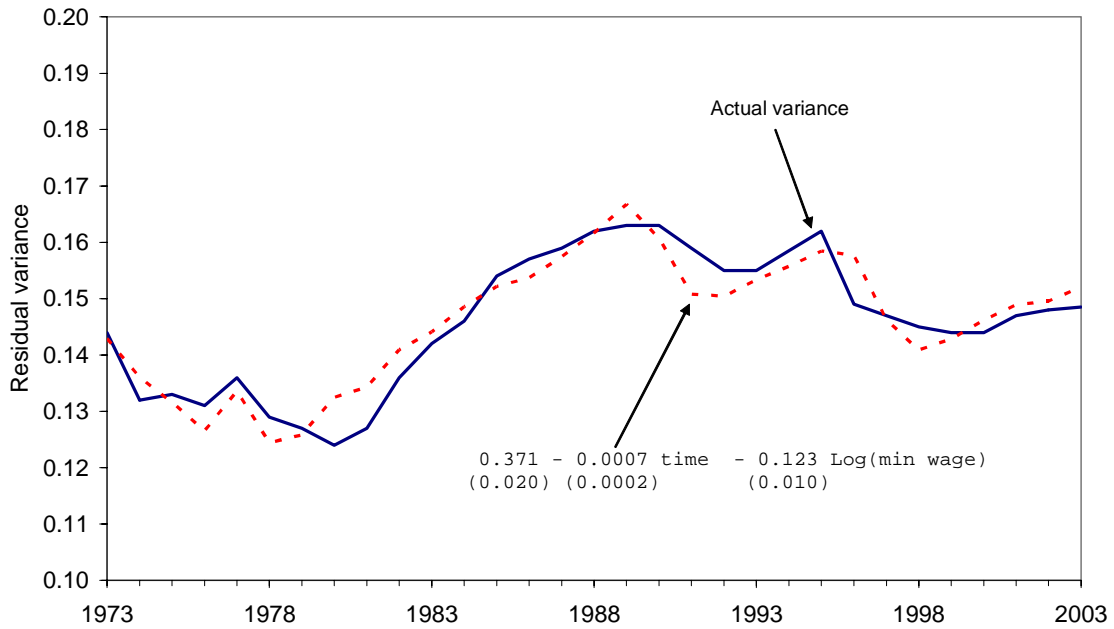
**Figure 8b: Fraction of women paid by the hour, by education category**



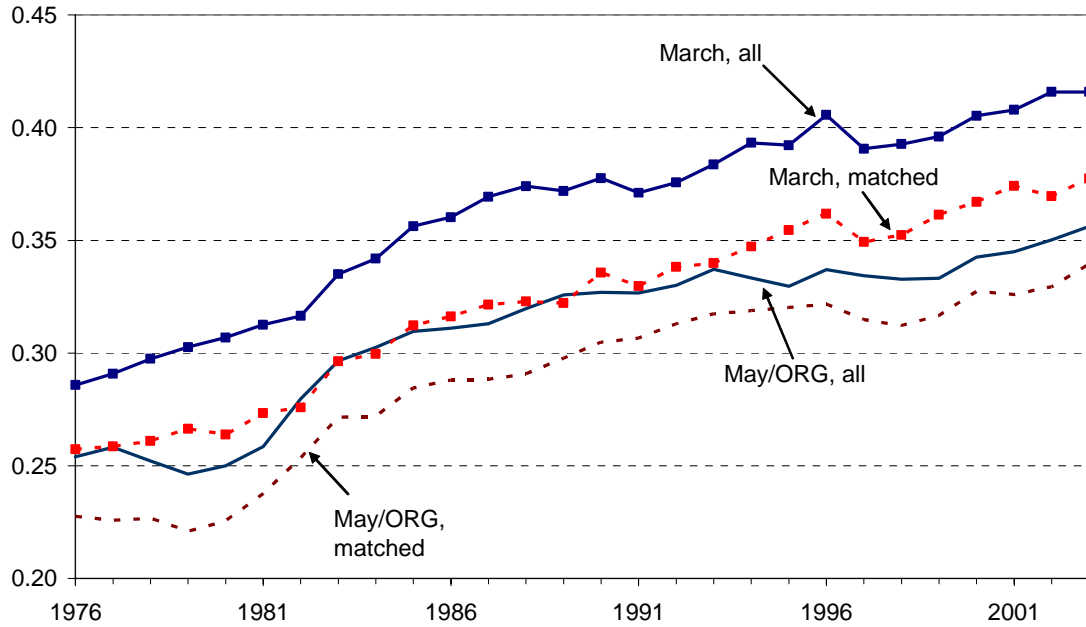
**Figure 9a: Male residual variance predicted using the minimum wage (holding characteristics at their 1973 level)**



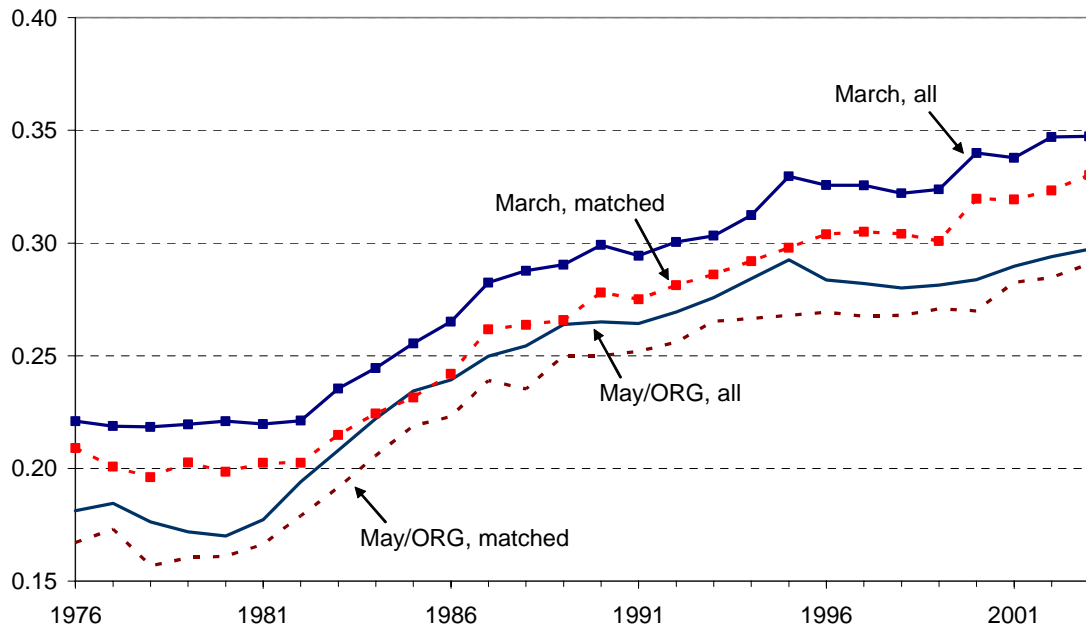
**Figure 9b: Female residual variance predicted using the minimum wage (holding characteristics at their 1973 level)**



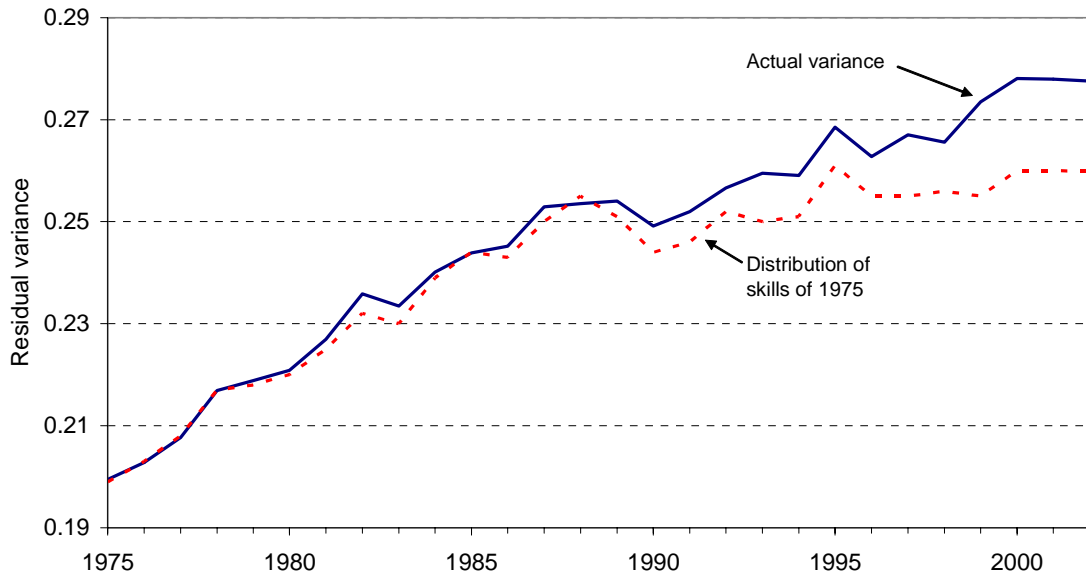
**Appendix Figure 1a: Variance of log hourly wages for all workers and matched workers only, men**



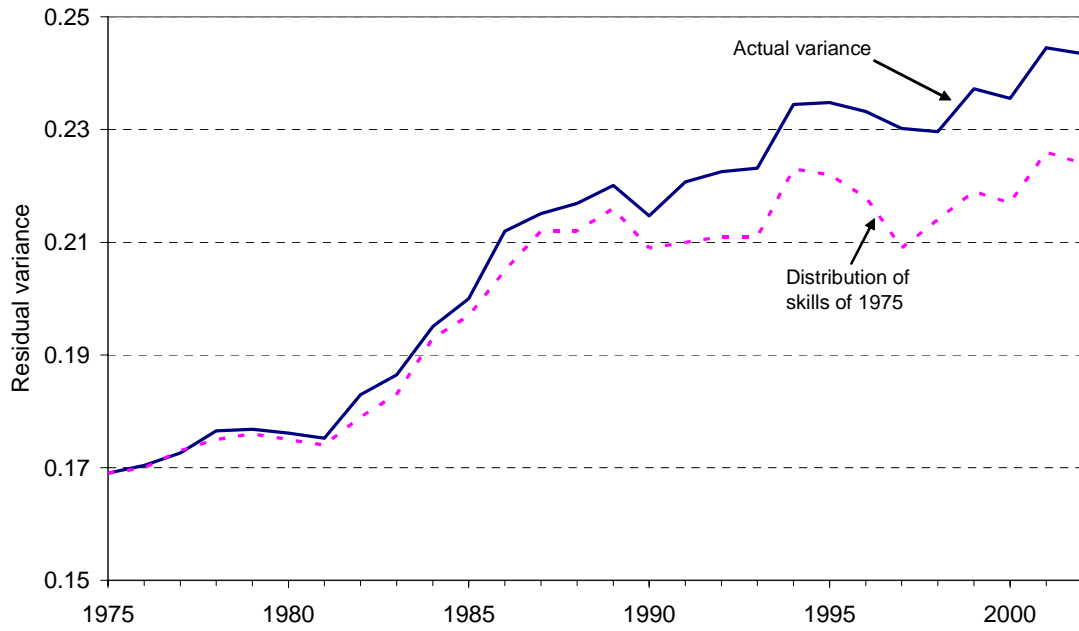
**Appendix Figure 1b: Variance of log hourly wages for all workers and matched workers only, women**



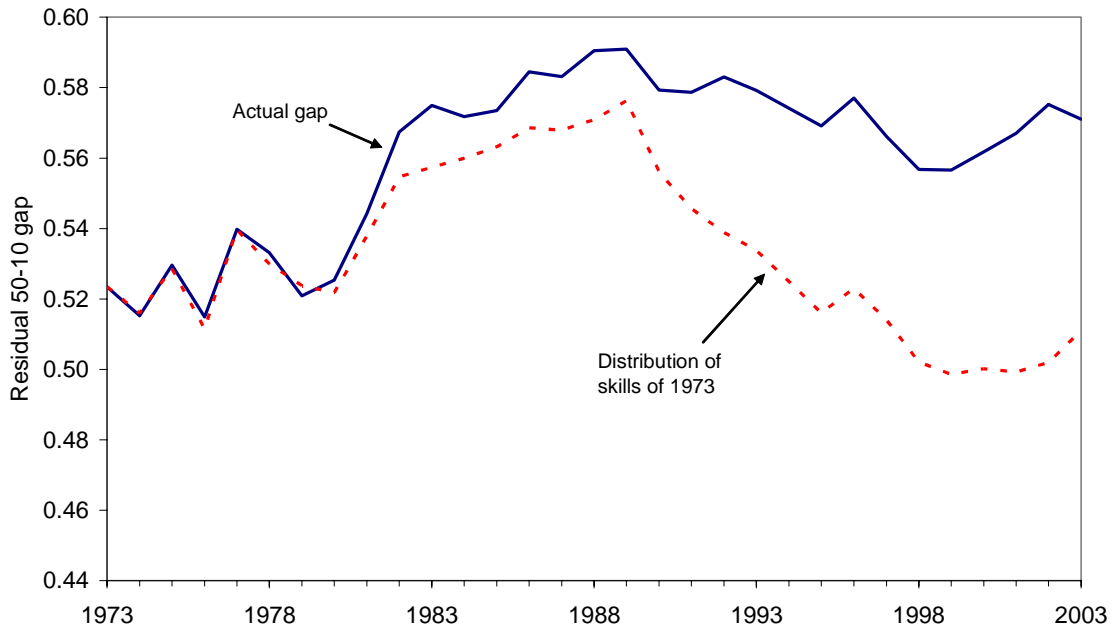
**Appendix Figure 2a: Residual variance for men in the March CPS, holding distribution of skills at their 1975 level**



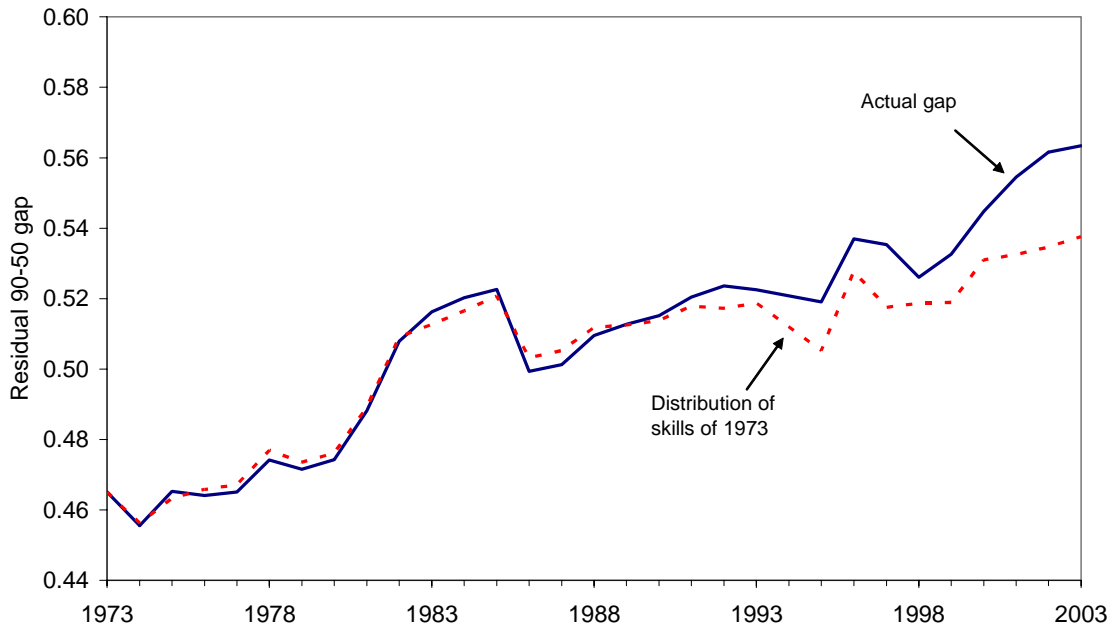
**Appendix Figure 2b: Residual variance for women in the March CPS, holding distribution of skills at their 1975 level**



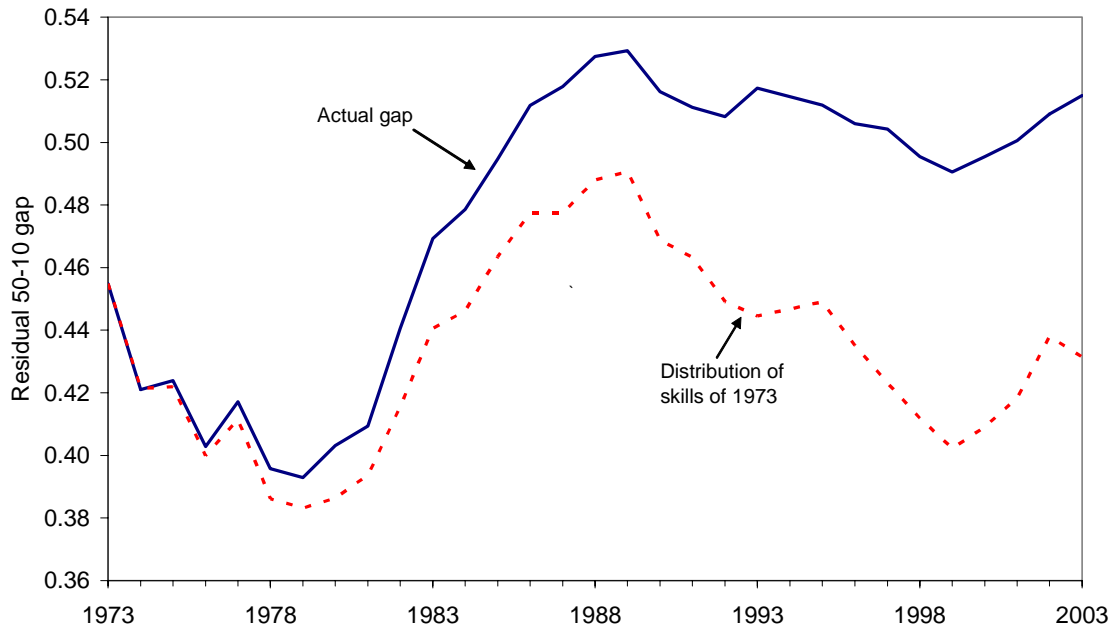
**Appendix Figure 3a: Residual 50-10 wage gap for men, holding distribution of skills at their 1973 level**



**Appendix Figure 3b: Residual 90-50 wage gap for men, holding distribution of skills at their 1973 level**



**Appendix Figure 3c: Residual 50-10 wage gap for women, holding distribution of skills at their 1973 level**



**Appendix Figure 3d: Residual 90-50 wage gap for women, holding distribution of skills at their 1973 level**

