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Changes in relative wages in the 1980s: Returns to observed and unobserved skills and black–white wage differentials

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Abstract

We assess the potential contribution of a rise in the return to unmeasured productivity correlated with education and race to the dramatic increase in the college–high-school wage differential and the stagnation of black–white wage convergence during the 1980s. A relatively unrestricted error-components panel data model is used to estimate the rise in the unobserved skill premium. Identification of the model is based on across-group variation in changes in within-group log-wage variances over time. In the absence of credible instruments for education and race, we calibrate the impact of time-varying 'ability' biases under various assumptions on the extent of non-random sorting of ability. Both between-cohort and within-cohort changes are examined using earnings data on men from multiple Current Population Surveys. There is systematic variation in changes in within-group wage variances over time, suggesting about a 10–25% rise in the unobserved skill premium during the 1980s. In addition, there are noticeable differences across cohorts in changes in the college–high-school wage gap. However, the model estimates imply that the rise in the return to ability can account for *at most* 30–40% of the observed rise in the college premium for young workers. Similarly, young, well-educated black men experienced *at least* a 0.13 log point decline in wages relative to their white counterparts between 1979 and 1991. © 2000 Elsevier Science S.A. All rights reserved.

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1. Introduction

During the 1980s, wage inequality among men in the United States grew along several dimensions.[1] Most notably, after declining in the previous decade, the measured college–high-school wage differential rose dramatically in the 1980s. Wage inequality within narrowly defined demographic groups based on education and experience also rose, continuing a trend that began in the early 1970s. Finally, wage convergence between black and white men stagnated in the 1980s after fifteen years of black economic progress dating back to the mid-1960s.

While many studies have focused on proposing and evaluating various explanations for these observed changes, a debate has arisen concerning their possible connection. Several economists have attributed rising within-group residual wage dispersion to an increase in the return to unobservable 'skill' or 'ability'. Consequently, if individual ability (e.g., cognitive intelligence and family background) varies by educational attainment, then changes in conventional measures of the college premium may be driven in part by changes in the payoff to ability. Similarly, if unmeasured determinants of productivity vary by race (e.g., school quality), then the recent slowdown in black–white wage convergence may not be fully attributable to an increase in labor market discrimination.

This study assesses the potential contribution of a rise in the return to unobserved skill correlated with education and race to changes in relative wages during the 1980s. The ideal analysis of true changes in the college premium and market discrimination over time would involve a controlled experiment in which the researcher would randomly assign schooling and race across individuals and then observe subsequent changes in relative wages along these observable dimensions over time. Random assignment ensures that the distributions of 'ability' are identical in the different groups. Clearly, this ideal and variations on it, such as quasi-experimental analyses, are not attainable.

To identify the contribution of time-varying ability biases to observed changes in college–high-school and black–white wage differentials it is necessary to identify both (1) the extent of ability sorting at a given point in time and (2) the growth in the return to unobserved ability. This study uses a relatively unrestricted error-components model of changing wage inequality to estimate the rise in

[1] See, for example, Blackburn et al. (1990), Katz and Murphy (1992), Levy and Murnane (1992), and Juhn et al. (1993).

the unobserved skill premium. Identification of the model is based on across-group variation in within-group log-wage variances from multiple periods. In our model, changes in the payoff to unmeasured productivity have clear implications for the systematic behavior of within-group wage dispersion across groups and over time. We show that the relationship between changes in overall residual dispersion and changes in the return to ability is not uniquely determined and depends on changes in the fraction of residual wage variation attributable to transitory components (e.g., luck and measurement error). In addition, since identification of the error components does not rely on the autocovariance structure of wages, estimation of the model does not require a panel data set that follows the same individuals over time.

In contrast to the error-components panel data literature, our approach also does not rely on the existence of exogenous variables that can purge the correlation of education and race with the unobservable individual effects. In the absence of credible instruments for schooling and race, this study calibrates the impact of a rising value of skill under various assumptions on the magnitude of unobserved skill differences across education and race groups. Specifically, we model the analytical form of the non-stationary heterogeneity biases in conventional estimates of changes in the college premium and wage discrimination.

The model is implemented using a series of large, independent cross-sectional samples of men from the Current Population Survey (CPS). In the context of our application, estimation based on this type of data can be both robust and more precise than estimation based on smaller-sized panel data sets. We examine both between-cohort and within-cohort changes in relative wages to gauge the sensitivity of the results to potential age and cohort effects, respectively. Our procedures also account for censoring in the wage data attributable to top-coding in the CPS and to the minimum wage. Both instrumental variables and minimum distance estimation methods are used to obtain estimates of the change in the return to ability that are purged of potential attenuation biases arising from measurement error in wages.

Our parsimonious model of changing inequality appears to provide an accurate description of changes in within-group wage dispersion over time. The male CPS earnings data show that there is systematic variation in within-group log-wage variances across groups and over time, suggesting about a 10–25% rise in the return to ability during the 1980s. In addition, there are noticeable differences across cohorts in changes in the college–high-school wage gap. However, consistent with the findings of recent studies which use observable measures of ability (i.e., test scores), it appears that time-varying ability biases cannot account for all of the growth in the college premium in the 1980s, even given our largest estimate of the rise in the unobserved skill premium. In particular, the increase in the return to ability can account for *at most* 30–40% of the observed rise in the college premium for relatively young workers. Similarly, an increase in the return to skill alone cannot account for the slowdown of black economic progress. For example, young, well-educated black men experienced *at least* a 0.13 log point decline in wages relative to their white counterparts during the 1980s.

2. An error-components model of changing inequality

Recently, there has been a growing research focus on the potential relationship between changes in between-group and within-group inequality during the 1980s. It has been documented that the increase in residual wage dispersion within groups defined by education and experience cells accounts for at least half of the total growth in inequality during the 1980s (e.g., Juhn et al., 1993). Many have interpreted this rising within-group inequality as reflecting an increase in the return to unmeasured productivity or 'ability'. If individual ability varies by education and race, then wage differentials between observable groups may rise as a spurious artifact of an increase in the return to unobserved ability and not due to true changes over time in the 'causal' impact of education and race on earnings.

One body of research has proposed and used 'direct' measures of skill or ability, such as test scores and observable measures of school quality, to control for unobserved heterogeneity biases in conventional estimates of changes in the college premium and wage discrimination. Most studies find little evidence that controlling for test scores has a significant impact on the estimated increase in the college–high-school wage gap.[2] The evidence on the effects of controlling for school quality and test scores on estimated changes in the black–white wage gap is more mixed.[3] However, it is not clear that test scores will account for all potential sources of unobservable productivity differences across individuals. In addition, the changes in the wage structure were originally documented using large, nationally representative samples from the Current Population Survey (CPS). On the other hand, almost all of the studies that use test scores are based

[2] Bishop (1991), Blackburn and Neumark (1993), Ferguson (1993), Cawley et al. (1998), and Taber (1998) use the Armed Forces Qualifying Test (AFQT) scores from the National Longitudinal Survey of Youth (NLSY) to account for unobserved ability differences across individuals. With the exception of Ferguson, these studies find that controlling for AFQT scores has little effect on the estimated rise in the college premium. Using student achievement test scores from the National Longitudinal Study of the High School Class of 1972 and the 1980 High School and Beyond surveys, Murnane et al. (1995) conclude that much of the rise in the return to education for 24-yr olds (about 38–100%) is due to an increase in the ability premium.

[3] Card and Krueger (1992) and Grogger (1996) find that black–white school quality differences cannot account for most of the changes in the racial wage gap among men. O'Neill (1990), Ferguson (1993), Maxwell (1994), and Neal and Johnson (1996) use AFQT scores to account for black–white productivity differences. These studies suggest that ability differences and changes in the return to ability account for much of the stagnation of black economic progress during the 1980s.

on the National Longitudinal Survey of Youth (NLSY), which contains relatively small samples of very young workers. The confounding of age and time effects in changes in wage differentials is potentially relevant for this age group.

In this study, we use an error-components approach to the evaluation problem as an alternative to using test scores as a proxy for ability. A few studies have estimated the rise in the unobserved skill premium by allowing unobserved productivity to be the permanent component in an error-components model of wage determination.[4] However, few of these studies explicitly allow for the unobservable permanent component to be correlated with education and race, and, therefore, for the existence of time-varying ability biases in estimates of changes in the college premium and wage discrimination.

This study uses a relatively unrestricted error-components model of changing wage inequality in which estimation of changes in the return to skill is based on variation in within-group log-wage variances across groups and over time. In our model, across-group heteroskedasticity in residual variances, and changes in it, can be used to identify a relatively rich earnings model that allows for non-stationarity in both the permanent and transitory error components. Since the autocovariance structure of wages is not needed to identify the parameters of interest, the model can be implemented using a time-series of cross-sections instead of panel data. This is convenient since the model can be estimated using the large samples contained in the CPS, which is the data source most often used to document rising inequality.

In addition, our approach does not rely on the existence of instrumental variables that can purge the correlation of education and race with the unobservable individual effects. Our model has direct implications for the analytical form of changes in ability biases over time. Consequently, we can calibrate the effects of the estimated increase in the ability premium under various assumptions on the magnitude of ability sorting across education and race groups. In contrast, Taber (1998) uses a dynamic programming model of endogenous self-selection of education in which exclusion restrictions (parental education and number of siblings) are used to identify an earnings model that allows for time-changing ability biases. Implementing the model with NLSY data, Taber finds that all of the observed increase in the return to college during the 1980s can be attributed to an increase in the ability premium. It is important to see whether this surprising result can be replicated in a study based on CPS data that does not rely on valid exclusion restrictions and distributional assumptions for identification.

Fig. 1 summarizes trends in inequality during the 1980s along the three dimensions of interest: the measured college–high-school wage differential, the

[4] Consequently, our approach is similar in spirit to the frameworks used by Juhn et al. (1991), Card and Lemieux (1994, 1996), Moffitt and Gottschalk (1995), and Taber (1998). This is also the implicit framework underlying Smith (1993).

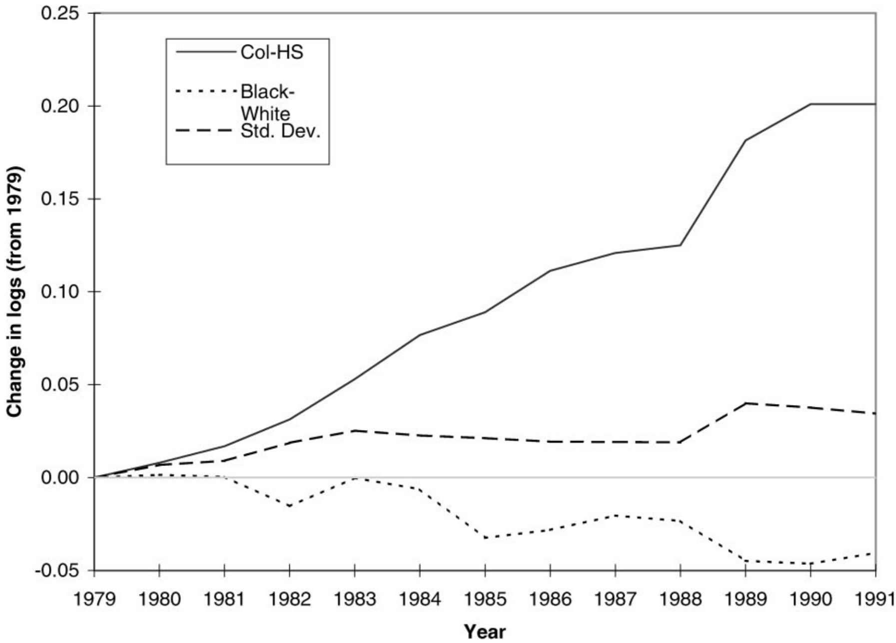

Fig. 1. Observed college–high-school, black–white wage differentials, and within-group wage dispersion during the 1980s. Note: Quantities on the vertical axis denote the change from the base year (1979) in the regression-adjusted college–high-school and black–white log-wage differential as well as the change in the overall residual standard deviation of log-wages. The 1979 levels of each series are 0.297, -0.148, and 0.398 for the college–high-school, black–white wage differentials and the residual standatd deviation, respectively. Details on their computation are described in the text.

black–white wage differential, and the standard deviation of 'within-group' wages. These are computed using data from the CPS Merged Outgoing Rotation Group files from 1979 to 1991.[5] As previously documented, the 1980s witnessed a dramatic increase in the college–high-school wage gap of about 0.20 log points. In addition, the regression-adjusted black–white differential widened

[5] For each year, the sample consists of white and black men between the ages of 18 and 64 with real wages (based on either the edited hourly wage or the edited usual weekly earnings divided by the edited usual weekly hours, in \$1991) between \$2 and \$60 an hour. The college–high-school and black–white series are the estimated coefficients on the indicators for 16 + years of schooling and black from a regression of log-wages on a full set of single-year potential experience (age-education-6) dummies, 3 education category dummies ($<$ 12, 13–15, 16 +), and a black indicator. The residual dispersion series is the root mean squared error of the regression of log-wages on a fully interacted set of dummy variables for experience, education, and race.

by 0.04 log points, from −0.15 to about −0.19.[6] Finally, the residual standard deviation of wages rose about 0.04 log points during the decade. If log-wages are normally distributed, this would imply that the 90–10 percentile wage differential among workers with the same observable characteristics rose by about 0.10 log points. This rising residual dispersion is often interpreted as evidence of an increase in the return to ability or the 'price' of unobserved skill. Next, we show that true changes in the college premium and wage discrimination are unidentified without information on the magnitude of the omitted ability biases and the growth in the return to ability.

2.1. Ability biases in a simple error-components model

First, it is necessary to specify an error-components model of wage determination that allows for an unobserved skill bias and a non-stationary return to this component. The model used in this study, while parsimonious, also allows for the transitory component of earnings (e.g., due to luck or measurement error) to have a non-stationary variance. In addition, identification of the model requires relatively few assumptions about the time-series properties of the error components. Our model implies that within-group wage dispersion will increase when the return to unobservable skill increases, *all else held constant*. However, the model also has the implication that the ability premium may have risen even if we observe that within-group residual dispersion has not.

Suppose that log-wages are determined by the following error-components process:

$$w_{ijt} = r_t k_j + u_{ijt}, \quad i = 1, \ldots, N; \quad j = 1, \ldots, J; \quad t = 1, \ldots, T$$
$$u_{ijt} = \psi_t a_{ijt} + \varepsilon_{ijt}, \tag{1}$$

w_{ijt} is the natural logarithm of the hourly wage for individual i in group j at time t, k_j is the level of observable productive 'skills' that is common to all members of group j, and r_t is the return to this skill at time t. There are two unobservable components, a_{ijt} and ε_{ijt}, which represent the level of unobservable productive skills for each individual in the group and the transitory random component of wages, respectively.[7] ψ_t is the return to unobservable 'ability' or productivity at time t.

[6] Bound and Freeman (1992) document the substantial slowdown in black–white wage convergence and the expansion in the black–white wage gap among young men that occurred in the 1980s. Smith (1993) evaluates the hypothesis that the slowdown can be attributed to a legislative environment increasingly opposed to affirmative action and equal employment opportunity policies.

[7] Note that ε_{ijt} can also be thought of as another unobserved 'skill' component that is independent of the observable and unobservable *permanent* skill components. For our analysis, we are only concerned about changes in the return to unobserved skill that affect estimates of changes in the college premium and wage discrimination.

The following assumptions are sufficient for identifying the model:
A1: a_{ijt} and ε_{ijt} are independent.
A2: $\mathrm{E}(a_{ijt}|i \in j) = a_j$ and $\mathrm{Var}(a_{ijt}|i \in j) = \sigma^2_{aj}$ for all t.
A3: $\mathrm{Var}(\varepsilon_{ijt}) = \sigma^2_{\varepsilon t}$ for all j.
A4: $\mathrm{E}(k_j \varepsilon_{ijt}|\psi_t a_{ijt}) = 0$.
We show below how Assumptions A1–A3 allow for simple identification of changes in the factor, ψ_t, loading onto the unobservable permanent component. Notice that A2 allows the unobservable permanent component to have different means and variances across the observable groups, while A3 allows the transitory error component to have a time-varying variance possibly due to changes in the importance of luck or measurement error in wage variation. Few assumptions are made on the time-series properties of the two error-components other than the mean stationarity of the permanent component. The effect and variance of the permanent component is allowed to be non-stationary through the loading factor ψ_t. The assumptions also allow for heteroskedasticity in the unobservables, but identification of ψ_t is based on the assumption that the transitory component is homoskedastic across groups.[8]

Assumptions A2 and A4 allow for identification of changes in the return to observable characteristics over time, the parameters of interest r_t, conditional on identification of changes in the return to unobservable skills, ψ_t. A2 allows the unobservable 'ability' of an individual to vary over time as long as the mean of ability among individuals in an observable group is stationary. While this is a sufficient condition, identification actually only requires stationarity of the *difference* in the means of the unobservable permanent component between the comparison groups of interest (e.g., college vs. high school). A4 assumes that the observable characteristics are uncorrelated with the transitory error component given information on the ability component and changes in its return. The model also assumes that the sources of ability bias can be summarized by a single index/component, a_{ijt}, although the mean of this component is allowed to vary across groups.[9]

The difficulty in identifying true changes in the college premium (and wage discrimination) is clear in the above model. Suppose that there are two groups of workers, HS and COL; those with a high school degree and those who have completed college or higher, respectively. Then

$$\mathrm{E}(w_{i,\mathrm{COL},t}) - \mathrm{E}(w_{i,\mathrm{HS},t}) = r_t + \psi_t(a_{\mathrm{COL}} - a_{\mathrm{HS}}), \tag{2}$$

[8] There are no studies that we are aware of that allow for heteroskedasticity in transitory error components that are allowed to be non-stationary over time. The assumption suggests that the *variance* of luck is similar across groups.

[9] Using NLSY data, Cawley et al. (1998) only use the first principle component of the ten ASVAB test scores that comprise the AFQT to summarize the contribution of these observable ability measures to the wage–education relationship.

where $k_{\mathrm{COL}} - k_{\mathrm{HS}}$ has been normalized to 1, and r_t is the causal impact of a college degree on log-wages at time t. The second term in the equation is the bias in the conventional estimate of the return to college at time t based on cross-sectional comparisons. It arises if individuals non-randomly sort into different education groups based on productivity differences ($a_{\mathrm{COL}} \neq a_{\mathrm{HS}}$).

The magnitude of this ability bias can change over time and is directly proportional to changes in the return to ability. In particular,

$$[\mathrm{E}(w_{i,\mathrm{COL},t}) - \mathrm{E}(w_{i,\mathrm{HS},t})] - [\mathrm{E}(w_{i,\mathrm{COL},1}) - \mathrm{E}(w_{i,\mathrm{HS},1})] = (r_t - r_1) + (\psi_t - 1)(a_{\mathrm{COL}} - a_{\mathrm{HS}}), \quad (3)$$

where ψ_1 has been normalized to 1, so that ψ_t is the change in the return to unobserved productivity relative to the base year. The second term in Eq. (3) is the source of time-varying ability bias in conventional estimates of the change in the college premium based on a series of cross-sections. It depends on both the amount of non-random ability sorting across education groups and changes in the return to unobserved skill. Consequently, observed changes in the college–high-school wage gap may partially be due to changes in ψ_t rather than r_t.

Identification of true changes in the college premium requires information on the mean productivity differences between groups in the initial period, ($a_{\mathrm{COL}} - a_{\mathrm{HS}}$). Although there are many empirical studies which attempt to account for ability bias in conventional cross-sectional estimates, there is no strong consensus on its size.[10] In the absence of credible instruments, our approach is to remain agnostic about the magnitude of the unobserved skill gaps by education and race. Given ψ_t, we provide a range of estimates of the true changes in the college premium (and wage discrimination) that correspond to a range of beliefs about the magnitude of ($a_{\mathrm{COL}} - a_{\mathrm{HS}}$). Our study provides bounds for the true changes in relative wages based on different assumptions on the fraction, λ, of the observed wage gap in the initial period that is attributable to mean differences in unobservable skill:[11]

$$\lambda_c = \frac{a_{\mathrm{COL}} - a_{\mathrm{HS}}}{\mathrm{E}[w_{i,\mathrm{COL},1}] - \mathrm{E}[w_{i,\mathrm{HS},1}]} \quad \text{or} \quad \lambda_b = \frac{a_{\mathrm{BLACK}} - a_{\mathrm{WHITE}}}{\mathrm{E}[w_{i,\mathrm{BLACK},1}] - \mathrm{E}[w_{i,\mathrm{WHITE},1}]}.$$

The focus of this study is on credible identification and estimation of the change in the unobserved skill premium, ψ_t. Based on our model, one cannot identify ψ_t using a single time series of residual dispersion statistics such as the

[10] See Willis (1986) for an overview of the theoretical underpinnings of earnings functions and associated estimation problems. Card (1998) provides a summary of recent attempts to purge the self-selection/omitted ability biases that may contaminate estimates of the return to education. He concludes that simple OLS estimates may have a positive ability bias of about 10%.

[11] Chay (1995) uses this approach to model unobserved productivity differences between black and white men.

residual standard deviation series in Fig. 1 if there is a transitory unobservable component in the wage process, ε_{ijt}. Identification is particularly difficult if this component's variance is non-stationary and one does not want to use restrictions on the autocovariance of wages. Specifically, Eq. (1) and the assumptions imply

$$\mathrm{Var}(w_{ij1}|i\in j) = \sigma^2_{wj,1} = \sigma^2_{aj} + \sigma^2_{\varepsilon,1} \quad \text{and}$$
$$\mathrm{Var}(w_{ijt}|i\in j) = \sigma^2_{wj,t} = \psi_t^2\sigma^2_{aj} + \sigma^2_{\varepsilon,t}, \tag{4}$$

which can be used to solve for ψ_t,

$$\psi_t = \sqrt{\frac{\sigma^2_{wj,t} - \sigma^2_{\varepsilon,t}}{\sigma^2_{wj,1} - \sigma^2_{\varepsilon,1}}}. \tag{5}$$

Consequently, within-group wage variances for group j at two points in time are not sufficient for identifying ψ_t, since there are only two 'observations' and four unknown parameters (ψ_t, σ^2_{aj}, $\sigma^2_{\varepsilon,1}$, $\sigma^2_{\varepsilon,t}$).

Eq. (5) has several interesting implications. Even when wage variances rise from periods 1 to t, there could be a decline in the ability premium ($\psi_t < 1$) if the increase in the noise variance ($\sigma^2_{\varepsilon,t}$ vs. $\sigma^2_{\varepsilon,1}$) is sufficiently large. Similarly, a fall in the wage variance from periods 1 to t does not necessarily imply that the return to ability did not increase. Note that if the transitory component is stationary ($\sigma^2_{\varepsilon,1} = \sigma^2_{\varepsilon,t}$), a rise (fall) in within-group wage variances unambiguously implies $\psi_t > 1$ ($\psi_t < 1$). However, ψ_t can be made arbitrarily large (small) by setting $\sigma^2_{\varepsilon,1} = \sigma^2_{\varepsilon,t}$ arbitrarily close to $\sigma^2_{wj,1}(\sigma^2_{wj,t})$. For example, suppose that $\sigma^2_{wj,1} = 0.16$ and $\sigma^2_{wj,t} = 0.2$ (as in Fig. 1) and $\sigma^2_{\varepsilon,1} = \sigma^2_{\varepsilon,t}$. If all of the base period wage variance within group j is attributable to individual ability differences ($\sigma^2_{aj} = 0.16$), then $\psi_t = 1.12$, the simple ratio of the standard deviations. On the other hand, if the variance of unobserved ability is 0.01 in period 1, then $\psi_t = 2.24$.

Consequently, identification of true changes in relative wages depends crucially on the relative magnitudes of $\sigma^2_{\varepsilon,1}$, $\sigma^2_{\varepsilon,t}$, and σ^2_{aj}. Using the data from Fig. 1, Table 1 presents a range of changes in the college premium and wage discrimination between 1979 and 1991 that arise under various assumptions about (1) the ability bias in the base period and (2) the relative magnitudes of the variances of the unobservable components.[12] The proportion of the 1979 wage variance that is attributable to the transitory component, $\theta \equiv \sigma^2_{\varepsilon,1}/\sigma^2_{wj,1}$, increases along the columns of each panel. The fraction of the 1979 wage gap between the two groups that is due to unobservable productivity differences, λ, increases along the rows of each panel. The upper panel of the table is for the case of constant 'noise' variances over time, while the lower panel allows the noise variance to fall

[12] For ease of exposition, the table assumes that the variances of wages are the same in each group. The identification strategy of the paper is based on the fact that wage variances vary across groups.

Table 1
Influence of ψ_{91} on estimates of changes in the college premium and discrimination, 1979–1991[a]

	Constant 'noise' variance over time							
	Change in college premium				Change in discrimination			
		$\theta = 0$	$\theta = 0.5$	$\theta = 0.9$		$\theta = 0$	$\theta = 0.5$	$\theta = 0.9$
	$\psi_{91} = 1$	$\psi_{91} = 1.09$	$\psi_{91} = 1.17$	$\psi_{91} = 1.67$	$\psi_{91} = 1$	$\psi_{91} = 1.09$	$\psi_{91} = 1.17$	$\psi_{91} = 1.67$
$\lambda = 0$	0.201	0.201	0.201	0.201	− 0.0405	− 0.0405	− 0.0405	− 0.0405
$\lambda = 0.25$	0.201	0.194	0.188	0.151	− 0.0405	− 0.0371	− 0.0342	− 0.0157
$\lambda = 0.5$	0.201	0.188	0.176	0.102	− 0.0405	− 0.0338	− 0.0279	0.0090
$\lambda = 1$	0.201	0.174	0.151	0.002	− 0.0405	− 0.0272	− 0.0154	0.0585
	Declining 'noise' variance. Noise variance (1991) = 0.9* (Noise variance (1979))							
	Change in college premium				Change in discrimination			
		$\theta = 0$	$\theta = 0.5$	$\theta = 0.9$		$\theta = 0$	$\theta = 0.5$	$\theta = 0.9$
	$\psi_{91} = 1$	$\psi_{91} = 1.09$	$\psi_{91} = 1.47$	$\psi_{91} = 1.92$	$\psi_{91} = 1$	$\psi_{91} = 1.09$	$\psi_{91} = 1.47$	$\psi_{91} = 1.92$
$\lambda = 0$	0.201	0.201	0.201	0.201	− 0.0405	− 0.0405	− 0.0405	− 0.0405
$\lambda = 0.25$	0.201	—	0.166	0.133	− 0.0405	—	− 0.0231	− 0.0065
$\lambda = 0.5$	0.201	—	0.131	0.064	− 0.0405	—	− 0.0058	0.0275
$\lambda = 1$	0.201	—	0.062	− 0.072	− 0.0405	—	0.0290	0.0955

[a]Entries in the tables are 1979–1991 changes in the college premium or discrimination under differing values of ψ_{91} and λ. λ refers to the fraction of the base year wage gap attributed to unobserved skill, θ refers to the fraction of the base year wage variance attributed to 'noise', and ψ_{91} is the return to unobserved skill in 1991, relative to 1979. Numbers are computed using data from Fig. 1. Details provided in text.

$(\sigma^2_{\varepsilon,t} = 0.9\sigma^2_{\varepsilon,1})$. The entries are the 1979–91 true changes in relative wages corresponding to different 'beliefs' about λ and θ (and the implied ψ_{91}) and are calculated using Eqs. (3) and (5).[13]

When $\lambda = 0$ or $\psi_{91} = 1$ (no initial ability bias or no change in the return to ability), the conventional estimate of the change in relative wages, based on regression-adjusted wage gaps from two cross-sections, is unbiased.[14] From the table, the size of the time-varying ability biases are increasing in both λ and θ. In addition, the biases in the conventional estimates are more sensitive to the assumptions about λ the greater is θ (and hence ψ_{91}). For example, when $\theta = 0.5$ in the upper panel, $\psi_{91} = 1.17$ and the true change in the college premium varies

[13] Specifically, $(r_t - r_1) = \{[E(W_{i,\mathrm{COL},t}) - E(W_{i,\mathrm{HS},t})] - [E(W_{i,\mathrm{COL},1}) - E(W_{i,\mathrm{HS},1})]\} \cdot [1 - (\psi_t - 1)\lambda]$.
[14] We discuss below how this study attempts to hold $(a_{\mathrm{COL}} - a_{\mathrm{HS}})$ constant over time.

between 0.15 and 0.20 log points. When $\theta = 0.9$, $\psi_{91} = 1.67$ and the true change varies between 0 and 0.20 log points depending on λ.

If the noise variance is stationary, the upper panel suggests that the conventional estimates of relative wage changes are not severely biased except under relatively extreme conditions on the amount of ability sorting and the contribution of noise to residual wage variation ($\lambda = 1$ and $\theta = 0.9$). However, the lower panel shows that the ability bias in these estimates can be quite large for plausible values of λ and θ when the transitory component variance is declining over time. This results from the fact that the true rise in the value of skill is greater when there is a decline in the noise variance for a fixed $\theta > 0$. For example, the final column of the lower panel shows that if there are no ability biases, the data suggests that wage discrimination increased by 0.04 log points during the 1980s. However, if all of the initial black–white wage gap is attributable to unobserved productivity differences, then it is possible that wage discrimination actually *fell* by 0.10 log points during the decade; an improvement that was masked by a near doubling of the unobserved skill premium.

2.2. Identification of ψ_t

Based on Eq. (1) and Assumptions A1–A3, it is possible to identify changes in the ability premium, ψ_t, from systematic across-group variation in within-group wage variances over time. An advantage of this identification strategy is that it does not require the full specification of the time-series properties of the error components, which is commonly used in residual autocovariance structure models of error-components. As a result, a series of cross-sections is sufficient for identifying ψ_t, and panel data is not required. In addition, the approach accommodates a non-stationary transitory component.

First, consider the case where the transitory component variance is restricted to be stationary, $\sigma^2_{\varepsilon,t} = \sigma^2_{\varepsilon,s}\ \forall t \neq s$. Eq. (4) implies

$$\sigma^2_{wj,t} = \sigma^2_{\varepsilon,1}(1 - \psi_t^2) + \psi_t^2 \sigma^2_{wj,1}. \tag{6}$$

If the population within-group log-wage variances were known, ψ_t would be identified as long as there are at least two groups (e.g., college vs. high school) with differing variances in the initial period. Specifically, for each group, period t's wage variance is a linear function of the period 1 variance with the slope of the relationship measuring the squared change in the ability premium. $\sigma^2_{\varepsilon,1}$ is also identified in this case from the constant term.

Now suppose that the transitory error variance is allowed to be non-stationary. Then

$$\sigma^2_{wj,t} = (\sigma^2_{\varepsilon,t} - \psi_t^2 \sigma^2_{\varepsilon,1}) + \psi_t^2 \sigma^2_{wj,1}. \tag{7}$$

Again, two different within-group wage variances in the initial period is sufficient for identifying ψ_t. Although $\sigma^2_{\varepsilon,1}$ and $\sigma^2_{\varepsilon,t}$ are not separately identified, the sign of the intercept in Eq. (7) provides information on the relative proportional growth in the scale of the transitory component, ε_{ijt}, relative to the scale of the permanent component, a_{ijt}.[15]

Before proceeding, it is useful to discuss the identification of ψ_t in more detail and the relationship of our approach to other approaches in the error-components literature. Eq. (7) suggests that changes in the return to unobserved skill can be identified from systematic changes over time in heteroskedasticity in the residual wage variances across groups. Based on Assumptions A1–A3, a proportional increase in differences in log-wage variances across groups is evidence of an increase in the ability premium. This interpretation hinges on the assumption that the permanent component is the source of heteroskedasticity and that the transitory component variance, while non-stationary, is constant across groups. If true, comparing changes in the difference in log-wage variances between two groups over time 'differences out' the transitory error variance, while the secular changes in wage variances that occur for all groups is a proxy for the non-stationary noise variance.

If the noise component is heteroskedastic across the observable groups, then the resulting estimates of ψ_t will be biased.[16] However, this assumption may not be unreasonable. First, even given the large samples used in our analysis, we would not be able to statistically reject the hypothesis that the noise variances are constant across groups due to the large sampling errors on the estimated variances. In addition, there are no studies that we know of that have allowed for heteroskedastic noise variances, even studies which have explicitly examined measurement error in earnings data (e.g., Bound and Krueger, 1991; Bound et al., 1994). Finally, Bound and Krueger (1991) find little evidence of any *mean* correlation between measurement error in CPS earnings data for men and covariates such as education, race, age, marital status and region of residence. Admittedly, the above does not provide direct evidence on our key assumption. However, we show below that the model embodied in Eq. (7) does fit differential changes across groups in within-group log-wage variances relatively well.

The restrictions of our model can be compared to those of other models traditionally used in the literature. Many autocovariance structure error-component models are interested in the parameters associated with the unobservables, and do not allow the observables to be correlated with the

[15] Gottschalk and Moffitt (1994) and Moffitt and Gottschalk (1995) use panel data from the Panel Study of Income Dynamics (PSID) to document non-stationarity of the transitory component. They find that the variance of the noise component rose during the 1980s. Also, this rise can explain about half of the overall increase in residual earnings variances, with the rest attributable to an increase in the variance of the permanent component. This suggests that studies that only allow for a stationary noise component may overstate the true rise in the return to unobserved skill.

[16] However, note that the model estimates of ψ_t will be *consistent* in the presence of heteroskedasticity in ε_{ijt} that is not systematically related to education and race.

unobservables. Since these models are generally estimated using panel data sets with relatively few individuals (e.g., less than 2000), identification of error-component parameters such as ψ_t relies on assumptions on the time-series properties of the error terms. These approaches require several time periods of observation and use restrictions on the autocovariance of wages (e.g., an AR(1) transitory component) for identification.[17]

By contrast, the error-components parameters of our model are identified based on restrictions on the *cross-sectional* variation in log-wage variances. As a result, our approach requires only two time periods and can be implemented using a series of large cross-sectional samples, which are readily available relative to long panel data sets. In addition, no assumptions are made about $\mathrm{Cov}(\varepsilon_{ij1}, \varepsilon_{ijt})$ and $\mathrm{Cov}(a_{ij1}, a_{ijt})$. This is attractive if restrictions on the evolution of cross-sectional wage variances are more justified than restrictions on the serial correlation pattern of wages.[18] Finally, while both approaches can accommodate nonstationary error variances, the autocovariance structure approach assumes homoskedasticity in the permanent component and is misspecified if this is not the case. We relax this pooling constraint and use heteroskedasticity to identify ψ_t.

There are error-components models in the literature that allow for endogenous covariates that are correlated with the individual effects.[19] All of these approaches require the existence of exogenous variables or transformations of time-varying endogenous variables that are uncorrelated with the individual effects. Under assumptions on the relations of these variables to the error components, they can be used as instruments for the time-invariant regressors (education and race) under nonstandard or unrestricted formulations of the residual autocovariance matrix. However, with the exception of Holtz-Eakin et al. (1988), none of these studies allow for non-stationary individual effects, which is the crucial source of bias in this study. In addition, identifying credible instruments for education and race that will be orthogonal to the time-varying individual effects does not seem to be a viable option. Our approach to this problem is to estimate the rise in the ability premium and then calibrate the time-varying omitted ability biases under various assumptions on the amount of ability sorting across groups.

[17] Based on Eq. (1), a typical autocovariance structure model might assume B0: $\varepsilon_{ijt} = \rho\varepsilon_{ijt-1} + v_{ijt}$; B1: $(a_{ijt}, \varepsilon_{ij0}, v_{ijt})$ are jointly independent; B2: Var $(a_{ijt}) = \sigma_a^2$ and $\mathrm{Var}(v_{ijt}) = \sigma_v^2\ \forall j, t$; and B3: $\mathrm{Var}(\varepsilon_{ij0}) = \sigma_\varepsilon^2\ \forall j, t$. It is easy to show that in this simple model, with a stationary transitory component, ψ_t is just-identified with 3 time periods.

[18] Bound and Krueger (1991) find evidence that CPS earnings' measurement errors are serially correlated over two years. Moffitt and Gottschalk (1995) find that the transitory component of PSID earnings is composed of serially correlated shocks that die out within three years.

[19] See Hausman and Taylor (1981), Amemiya and MaCurdy (1986), and Breusch et al. (1989). Arellano and Bover (1995) provide an excellent overview of estimation of these types of models.

3. Data and measurement issues

The ideal data for estimating the model described in Eq. (1) and Assumptions A1–A4 would be an extremely large and long panel data set with no attrition (e.g., $N = 50{,}000$ to $100{,}000$ and $T = 10$ to 15).[20] In the absence of this ideal, a series of large, independent cross-sectional samples may be preferable to small panel data sets. First, large cross-sections are more appropriate for models which use restrictions on the across group variation in within-group wage variances since small panel data sets may have too few individuals in each group to relax the pooling constraint. In addition, the focus of this study is on 'robustly' identifying ψ_t and not on the autocorrelation structure of the unobservables nor on efficient estimation. Consequently, the identifying restrictions on the homoskedasticity of the permanent component and the time-series properties of the error components that are typically used for small panel data sets are relatively unattractive when compared to Assumption A3 used in this study.

Our analysis uses data from the 1979–1991 CPS Merged Outgoing Rotation Group files which contain earnings information for one-quarter of the individuals in each monthly CPS survey. Combining the observations from the monthly surveys results in annual samples which are equivalent to three full monthly CPSs. The extremely large sample sizes allow for a reasonably precise analysis of narrowly defined cells. In addition, analyzing the same data source most commonly used to document changes in the wage structure during the 1980s avoids issues concerning differences in sampling procedures and survey design that arise in analyses of longitudinal data. In a related point, our approach can be extended for long time periods while potentially circumventing the nonrandom missing data problems that are common in panel data studies. Finally, as shown below, the independence of the CPS samples over time is very useful for consistent estimation of ψ_t in the presence of classically distributed errors-in-variables.

3.1. Between- vs. within-cohort analysis

Assumption A2 and Eq. (3) suggest that calibrating the true changes in relative wages depends on having fixed differences in mean unobserved productivity between the two comparison groups over time. If mean ability differences are changing over time due to composition effects, for example, then our derivation of the analytical form of the changing ability bias will be incorrect. This restriction is often thought to be justified when examining longitudinal

[20] With a large sample of continuous wage earners, one could also allow the transitory component variances to vary across groups j by using identifying restrictions on the autocovariances of log-wages.

data with repeated wage observations on a fixed set of individuals over time. As Deaton (1985) points out, however, one can construct synthetic panel data by following fixed birth-year cohorts with repeated cross-sections. In this study, we examine both longitudinal labor market entry cohorts (within-cohort analysis) and groups of workers with the same experience in 1979 and 1991 (between-cohort analysis).

The assumption that $(a_{\mathrm{COL}} - a_{\mathrm{HS}})$ is constant over time is not testable in either the within-cohort or between-cohort context. However, we examined the demographic characteristics of the groups from 1979–1991 to gauge the plausibility of this assumption (results available from the authors). Significant changes over time in the distribution of educational attainment in a cohort may imply that the constancy of $(a_{\mathrm{COL}} - a_{\mathrm{HS}})$ is less likely to hold. Not surprisingly, the distribution of education was very stable from 1979–91 for both white and black workers in each of the five-year labor market entry cohorts that had 12 more years of experience in 1991 than in 1979. On the other hand, there were substantial differences in the educational distributions between cohorts of workers that had the same amount of experience in 1979 and 1991. In particular, for the two oldest experience cohorts of white men and for all black male experience cohorts, the 1991 between-cohort group had higher levels of education than the 1979 group. This suggests that stationary $(a_{\mathrm{COL}} - a_{\mathrm{HS}})$ and $(a_{\mathrm{BLACK}} - a_{\mathrm{WHITE}})$ may be more credible for within-cohort comparisons than for between-cohort comparisons. Assumption A2 may also be more plausible in the within-cohort context.[21]

However, a shortcoming of the within-cohort analysis is that it does not allow for experience/age effects in changes in the college–high-school and black–white wage gaps (i.e., life-cycle changes in relative wages). If the return to experience is unequal for the two comparison groups, then our approach will yield biased estimates of the true change in relative wages, $(r_t - r_1)$. As a result, this study reports the results from both between- and within-cohort analyses. The former will be biased if there are substantial cohort effects in relative wages, while the latter will be misspecified in the presence of age effects.

Table 2 presents a summary of both between- and within-cohort relative wage changes from 1979–91 for four separate experience cohorts: men with 6–10, 11–15, 16–20, and 21–25 yr of experience in 1979.[22] The upper panel presents

[21] In fact, we find that our model appears to fit within-cohort wage variances better than between-cohort variances. In addition, relative employment rates by education group and race and mean education and black–white relative education among employed men within the same labor market entry cohorts are very stationary from 1979–91. However, there are substantial between-cohort differences in 1979 and 1991. These results are available from the authors upon request.

[22] An examination of within-cohort changes in educational distributions suggests that among men with 5 or less years of potential experience in 1979, many were still in the process of obtaining more education. Among men with more than 25 yr of experience in 1979, college-educated men were much more likely to exit the work force by the end of the 1980s than the less educated.

Table 2
Between- and within-cohort reduced-form changes in relative wages, 1979–1991, by experience cohort[a]

			Between cohort		Within cohort	
		1979	1991	Change	1991	Change
College–high-school differential						
Experience						
	6–10	0.225	0.466	0.241	0.399	0.174
	11–15	0.277	0.451	0.174	0.429	0.152
	16–20	0.281	0.406	0.125	0.402	0.121
	21–25	0.299	0.421	0.122	0.400	0.100
Black–white differential						
Experience						
	6–10	– 0.125	– 0.169	– 0.044	– 0.194	– 0.070
	11–15	– 0.173	– 0.212	– 0.039	– 0.145	0.028
	16–20	– 0.164	– 0.207	– 0.043	– 0.183	– 0.019
	21–25	– 0.162	– 0.152	0.010	– 0.168	– 0.006
Experience: 6–10						
Education						
	$\leqslant 12$	– 0.157	– 0.173	– 0.016	– 0.148	0.009
	$\geqslant 13$	– 0.059	– 0.161	– 0.102	– 0.214	– 0.155
Residual						
Standard deviation						
Experience						
	6–10	0.393	0.424	0.031	0.443	0.050
	11–15	0.400	0.427	0.027	0.445	0.045
	16–20	0.403	0.438	0.035	0.463	0.060
	21–25	0.403	0.445	0.042	0.468	0.065

[a] College–high-school and black–white wage differentials computed from OLS regressions including single-year experience dummies, and dummies for education < 12, 12, 13–15, and > 15. Residual standard deviation is from regressions that include complete interactions of experience, education, and race.

the regression-adjusted college–high-school wage gap changes. First, note that both the between-cohort and within-cohort changes in the college–high-school differential vary by experience cohort, with the size of the increase monotonically declining in the age of the experience cohort for both types of changes. The wage gap expanded 0.17–0.24 log points in the youngest cohort, but only 0.10–0.12 log points in the oldest cohort. In addition, for the youngest experience cohort, the within-cohort increase in the gap is almost 30% less than the between-cohort increase, suggesting that the cohort effects in the returns to education may be substantial for this group. The fact that the within-cohort changes are uniformly less than the between-cohort changes suggests that age effects may be less of an issue than composition changes.

The middle panel shows that black–white wage convergence did appear to stagnate among men during the 1980s. This is especially true of the youngest experience cohort, where the black–white wage gap expanded by about 0.04–0.07 log points. The second half of the panel presents black–white relative wage changes in the youngest experience cohort by two education categories, those with a high-school degree or less and those with at least some college. It appears that young well-educated black men, in particular, lost substantial ground relative to their white counterparts from 1979–1991. The fact that the loss was larger within cohort than between cohort (-0.16 vs. -0.10 log points) suggests that changes in the relative school quality of black workers cannot explain the rising racial earnings disparity. In addition, if the age effects in relative wages are similar in the two education groups, then the results imply that none of the -0.16 log point within-cohort expansion of the gap is attributable to 'life-cycle' changes.

The bottom panel of the table presents changes in the within-group residual standard deviation of log-wages. Two points worth noting are that the rise in residual wage dispersion is greater for the oldest experience cohort relative to the youngest cohort and that the within-cohort increase is substantially greater than the between-cohort increase for all four cohorts. This foreshadows the below finding that the within-cohort analysis results in a greater estimated rise in the return to unobserved skill than the between-cohort analysis. From Table 2 and the evidence below, we conclude that the 'composition' biases arising in a between-cohort analysis may be substantially larger than the 'life-cycle' biases that exist in the within-cohort approach.

3.2. Censoring issues in the CPS data

The CPS data files 'top-code' usual weekly earnings at \$999 from 1979 until 1988 and at \$1999 from 1989 on. When examining annual changes in relative wages, it is apparent that the within-cohort college–high-school series and residual dispersion series exhibit discrete jumps from 1987 to 1989, presumably due to the change in the top-code. Censoring at the top-code is especially problematic since identification of the model relies on across-group variation in within-group variances. Estimated wage variances that do not account for this censoring will be seriously biased, particularly for college graduates.

Fig. 2 presents information on the fraction of college graduates censored at the top-code from 1979–1991. Panel A shows that as nominal weekly earnings increased during the 1980s, the top-code rate of white college graduates increased steadily before falling sharply after 1988. Panel B shows that this type of censoring can be quite serious in the few years preceding 1989 for each of the four experience cohorts. Top-code rates for college graduates in the three older

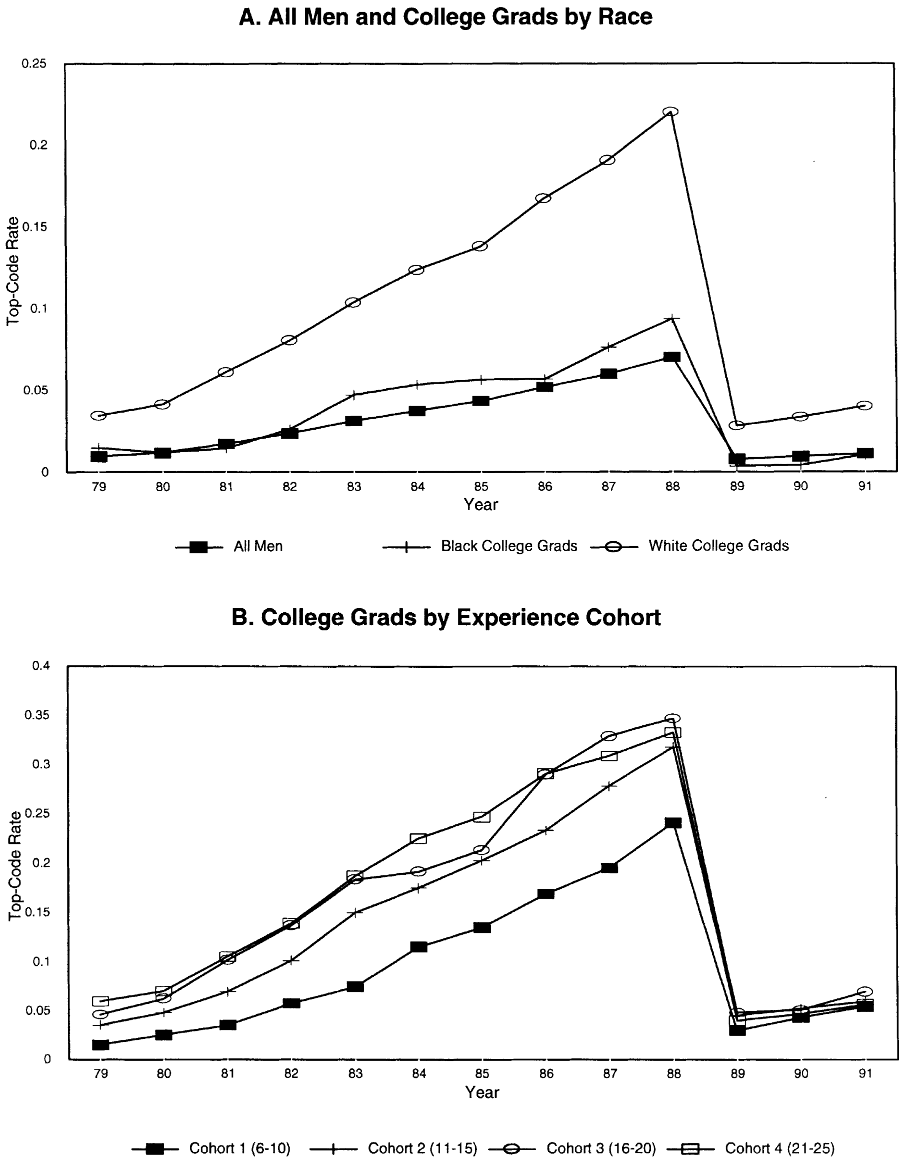

Fig. 2. Fraction of college graduates top-coded, 1979–91. (a) All men and college grads by race; (b) College grads by experience cohort.

cohorts increased to more than 30% by 1988. The amount of censoring is relatively low from 1979 to 1981 and 1989 to 1991. This is fortunate since the main focus of this study is on long-term changes in relative wages during the

1980s. However, using data from the mid-80s as a source of 'over-identification' of the model parameters may result in biased estimates. Consequently, we only present results using data from 1979–1981 and 1989–1991.[23]

There is another potential source of time-varying censoring bias in the wage data. Until there were legislated increases in the nominal floor in 1990 and 1991, the real value of the federal minimum wage fell steadily from 1979 to 1989. DiNardo et al. (1996) find strong evidence that the change in the real minimum wage is an important factor in explaining changes in the lower tail of the observed wage distribution. The erosion of the real value of the minimum wage may cause within-group wage dispersion to rise among low-wage workers independently of any change in the return to unobserved skill. To account for this potential problem, demographic cells in which more than 5% of the men are earning at or below the federal minimum wage (plus 5 cent/h) were excluded from the analysis.[24] When we used the more sophisticated approach outlined in Lee (1998) to 'directly' adjust the data for the minimum wage censoring, the resulting estimates of the rise in the ability premium were very similar to the estimates reported here. However, it should be noted that in the ideal situation there would be no minimum wage censoring.[25]

4. Estimates of the increase in the ability premium

Based on Eqs. (6) and (7), ψ_t is over-identified whenever there are more than two systematically different within-group log-wage variances in a given period. With a sufficiently large number of wage observations per group, asymptotic normality approximations for the finite sample properties of the sample

[23] We experimented with two different approaches to handling the top-coding issue for the mid-80s. First, we used an i.i.d. normal distribution assumption on the within-cell unobserved components to estimate the cell variances (and means) by cell-level Tobit maximum likelihood estimation. However, we found that the Tobit approach systematically overestimated the wage variances for heavily censored college-educated groups due to 'abnormally' long tails. This systematic bias increased during the mid-80s due to increases in the censoring rates. In our second approach, a within-group *symmetry* assumption was used to calculate variances based on the average of the squared deviations from the median for all observations below the median. Although this approach worked better than the Tobit approach, it still appeared sensitive to violations of the symmetry assumption. As a result, we are only using the years 1979–1981 and 1989–1991 to identify the model. See Chay and Lee (1997) for more details.

[24] Note that few cells were dropped due to this exclusion restriction. This is because the analysis focuses on men who are well into their labor market careers (i.e., have at least six years of potential experience).

[25] The minimum wage censoring implies that the residual dispersion series in Fig. 1 may overstate the true rise in aggregate residual inequality (Lee, 1998). Our model shows that the return to unobserved skill, ψ_t, may have increased even if residual dispersion did not due to non-stationarity in the transitory error variance.

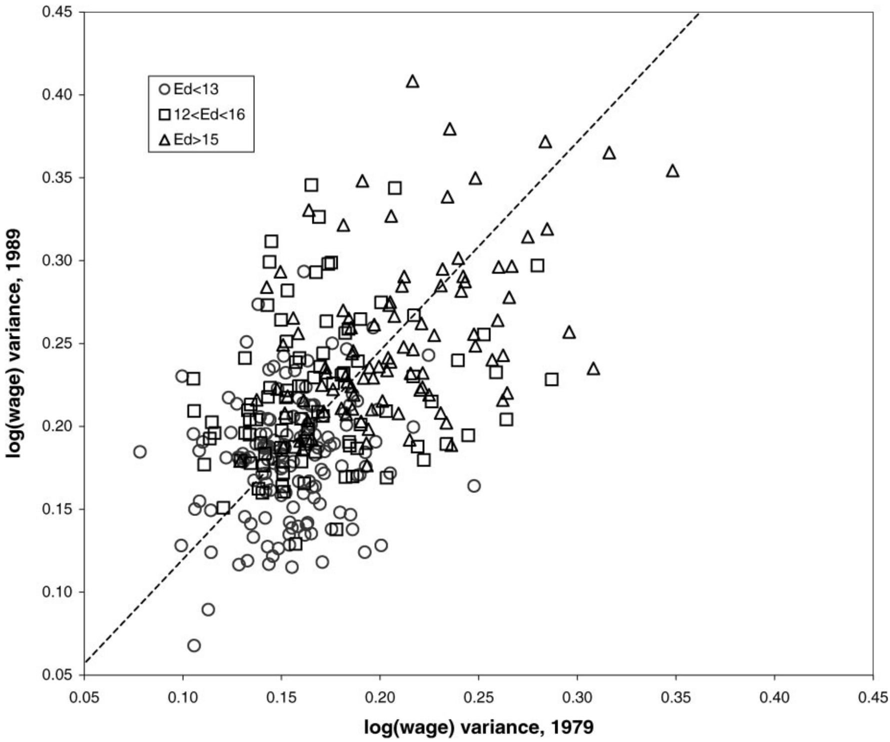

Fig. 3. Between-cohort within-group log(wage) variances: 1989 vs. 1979. Note: Plot of within-group log(wage) variance in 1989 against the variance in 1979. Groups are defined by race, age, and education. Circles, squares, and triangles represent cells with 12 or less, 13–15, and 16 and more years of education. Dashed line represents fitted line from two-sample IV estimate from Table 4a.

estimates of the within-group variances can be used to derive test statistics of the linear restrictions implied by the equations. To visually gauge the empirical fit of the model, Figs. 3 and 4 plot the 1989 log-wage variance of each experience–race–education cell against the 1979 cell variance for the between-cohort and within-cohort data, respectively.[26] The circles, squares, and triangles denote those cells containing individuals with 12, 13–15, and 16 or more years of education, respectively.

[26] Excluded from the analysis were cells with fewer than 30 observations, cells in which the median individual is earning at the top-code, and cells in which more than 5% of the individuals are earning at or below the federal minimum wage (plus 5 cent/h). For the within-cohort data, cells with less than 6 yr of potential experience in 1979 were excluded. We retained as many cells as possible, while excluding cells for which estimation of the true wage variance might be biased since it relied on too many assumptions on the latent shape of the wage distribution.

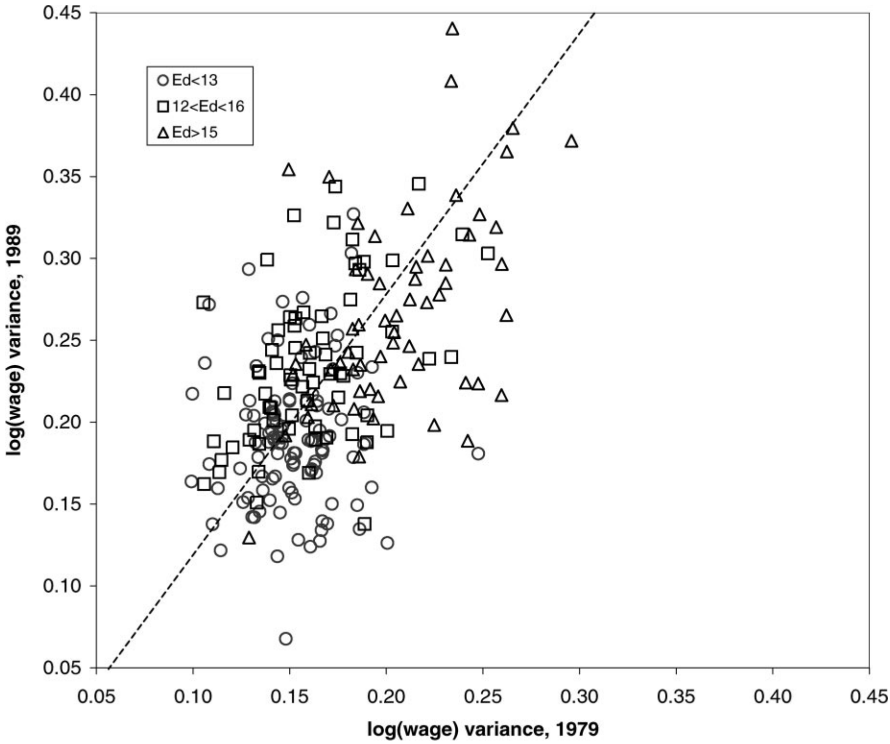

Fig. 4. Within-cohort within-group log(wage) variances: 1989 vs. 1979. Note: Plot of within-group log(wage) variance in 1989 against the variance in 1979. Groups are defined by race, age, and education. Circles, squares, and triangles represent cells with 12 or less, 13–15, and 16 and more years of education. Dashed line represents fitted line from two-sample IV estimate from Table 4a.

In both figures, there is a systematic, heteroskedastic relationship between wage variances and education. Within-group variances in 1979 are generally greater in the high education cells than in the low education cells. In the context of our model, this implies that the variance of unobserved skill rises with years of schooling. Also, in both figures there is a strong positive correlation between within-group variances in 1989 and variances in 1979 of workers with identical observable characteristics. The log-wage variances of the cells containing college graduates grew more during the 1980s than the variances of the cells with individuals who have a high school degree or less. In our model, the fact that the cells with larger variances in 1979 experienced a greater growth in wage variances between 1979 and 1989 implies that there was an increase in the ability premium ($\psi_{89} > 1$).

The differential growth in variances seems to be well explained by the parsimonious linear model described by Eq. (7). The model fits within-cohort changes in heteroskedasticity better than between-cohort changes, suggesting that the restrictions of the model, A1–A4, may be more appropriate for the within-cohort data. In particular, the single-index linear model of unobserved ability and changes in its premium does not appear unreasonable. Also, there seems to be enough variation in within-group wage variances to precisely identify the slope parameter in Eq. (7). The dashed lines in the figures represent our estimates of the intercept and slope of the regression relating the variances in 1979 and 1989 (their derivations are described in detail below). For the between-cohort analysis, the estimated slope is 1.25, implying that the return to ability increased 12% ($\psi_{89} = 1.12$) from 1979 to 1989. For the within-cohort data, the slope estimate is 1.58, which implies that the ability premium increased 26% ($\psi_{89} = 1.26$) during the 1980s.

If there are a large number of groups, then each group can be treated as an observation, and one can consider the limiting distribution of the estimator of ψ_t as the number of groups gets large. In practice, however, there may be a relatively small number of observations per group. Since Eqs. (6) and (7) are implemented using the estimated sample variances within each group instead of the population variances, approximation error in these estimates will lead to attenuation bias in the estimated slope coefficient, ψ_t^2, due to errors-in-variables. In particular, the sample analog of Eq. (7) is

$$\hat{\sigma}^2_{wj,t} = (\sigma^2_{\varepsilon,t} - \psi_t^2\sigma^2_{\varepsilon,1}) + \psi_t^2\hat{\sigma}^2_{wj,1} - \psi_t^2 u_{j1} + v_{jt}, \tag{8}$$

where u_{j1} and v_{jt} are defined to be the mutually independent sampling errors of the sample estimates ($\hat{\sigma}^2_{wj,1}$ and $\hat{\sigma}^2_{wj,t}$) of the population variances, $\sigma^2_{wj,1}$ and $\sigma^2_{wj,t}$, respectively.[27] Eq. (8) is a typical bivariate errors-in-variables linear regression model. The ordinary least squares estimate of ψ_t^2 will be inconsistent and biased downward in finite samples, where the size of the attenuation bias is:

$$\text{Bias}(\widetilde{\psi}_t^2) = -\psi_t^2\left(\frac{\text{Var}[u_{j1}]}{\text{Var}[\sigma^2_{wj,1}] + \text{Var}[u_{j1}]}\right). \tag{9}$$

The term in brackets is the noise-to-total variance ratio.

Consequently, in order to obtain consistent estimates of the rise in the ability premium, the estimation methods used must account for the 'measurement' error in Eq. (8). This study uses three different approaches to this problem: (1) grouped 'Wald' estimation, (2) instrumental variables approaches, and (3) minimum distance estimation. We find that all three approaches result in similar estimates of changes in the return to ability and are consistent with the visual impression left by the raw data in Figs. 3 and 4.

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[27] Eq. (8) comes from substituting $\hat{\sigma}^2_{wj,1} = \sigma^2_{wj,1} + u_{j1}$ and $\hat{\sigma}^2_{wj,t} = \sigma^2_{wj,t} + v_{jt}$ into Eq. (7).

4.1. Estimates of ψ_t based on aggregated groups

Our first approach makes use of the fact that ψ_t is just-identified when the wage variances of only two groups are known in two separate periods. A 'Wald' estimator based on two groups and two time periods reduces the attenuation bias problem since the number of observations in the aggregated groups are large, thereby reducing the sampling error variances ($\mathrm{Var}[u_{j1}]$) of the estimated log-wage variances.[28]

We divide the sample into two arbitrary groups, and then calculate a 'weighted average' within-group wage variance for the two groups in 1979 and 1989. More formally, the J different experience–race–education cells are divided into two sets containing J_1 and J_2 cells, respectively. Then Eq. (7) implies that

$$\psi_t^2 = \frac{\sigma^2_{wJ_2,t} - \sigma^2_{wJ_1,t}}{\sigma^2_{wJ_2,1} - \sigma^2_{wJ_1,1}}, \tag{10}$$

where

$$\sigma^2_{wJ_1,t} = \frac{1}{\sum_{j=1}^{J_1} N_j} \sum_{j=1}^{J_1} N_j \sigma^2_{wj,t}$$

and similarly for $\sigma^2_{wJ_2,t}$, and N_j is the number of observations in group j. Eq. (10) illustrates that as the difference between the (average) within-group wage variance between two groups expands over time, the implied return to unobserved skill rises.[29]

Although the aggregation choice is arbitrary, Figs. 3 and 4 suggest that a natural starting point is to define two groups based on educational attainment. The upper panel of Table 3 presents the weighted average within-group variances in 1979 and 1989 for workers with 12 and 16 + yr of schooling and between 6–25 yr of experience in 1979. The between- and within-cohort numbers are for those with 6–25 and 16–35 yr of experience in 1989, respectively. The first row presents the results when all four 5-yr experience cohorts are combined, while the remaining rows contain the disaggregated estimates broken out by cohort. For the combined cohorts, the 1979 average residual variance for those with at least a college degree is about 30% greater than the variance for those with only a high school degree (0.194 vs. 0.148). This empirical regularity holds for each of the four experience cohorts.

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[28] This approach also circumvents the top-coding problem. However, since grouped estimation does not account for the censoring due to the minimum wage, Wald estimates of ψ_t will tend to be biased downward.

[29] A finding that one group's wage variance is smaller than the other group's in one period, but larger in the next period is inconsistent with our model (it implies a negative value for ψ_t^2).

Table 3
Within-group wage variances and estimates of ψ_{89}, between and within experience cohorts, 1979–1989[a]

			Between			Within		
	1979		1989			1989		
Exper.	HS	COL	HS	COL	ψ_{89}	HS	COL	ψ_{89}
6–25	0.148	0.194	0.179	0.227	1.024 (0.056)	0.188	0.245	1.109 (0.064)
6–10	0.148	0.175	0.169	0.221	1.386 (0.160)	0.181	0.220	1.205 (0.157)
11–15	0.151	0.201	0.175	0.238	1.112 (0.101)	0.200	0.231	0.780 (0.124)
16–20	0.145	0.214	0.181	0.220	0.754 (0.085)	0.183	0.285	1.216 (0.113)
21–25	0.149	0.207	0.200	0.233	0.756 (0.118)	0.191	0.292	1.315 (0.141)
Total # of obs.	16,653	10,400	19,003	13,230		13,658	8819	

			Between			Within		
	1979		1989			1989		
Exper.	Black	White	Black	White	ψ_{89}	Black	White	ψ_{89}
6–25	0.154	0.161	0.170	0.195	2.019 (0.692)	0.173	0.209	2.365 (0.793)
6–10	0.153	0.155	0.156	0.188	3.844 (6.282)	0.176	0.197	4.307 (13.273)
11–15	0.157	0.162	0.177	0.193	1.784 (1.497)	0.172	0.207	2.001 (0.965)
16–20	0.169	0.164	0.171	0.197	— —	0.176	0.218	— —
21–25	0.136	0.166	0.179	0.207	0.959 (0.224)	0.166	0.225	1.444 (0.257)
Total # of obs.	3155	38,634	3823	43,405		2689	31,107	

[a]Entries are the estimated residual variance of a log(wage) regression on fully interacted sets of single-year experience, single-year education, and race dummies. The upper panel includes only those with 12 or 16 + yr of education. The lower panel includes all educational groups. Standard errors of the between-cohort and within-cohort estimates of ψ_{89} are in parentheses.

An increase in the ability premium implies that the college–high-school variance differential should be larger in 1989 than in 1979. The next set of columns in the table suggests that educational variance differentials did not grow significantly between cohorts. For workers with 6–25 yr of experience, the between-cohort data implies only a 2.4% rise in the return to ability

($\psi_{89} = 1.024$).[30] The disaggregated results show that while the ability premium may have risen in the two youngest cohorts, it appears to have contracted in the two oldest cohorts.

We suggested above that the assumptions of our model, in particular A2, are more likely to hold in the within-cohort data. The next set of columns show that the college–high-school variance gap expanded more within cohorts than between cohorts, suggesting an 11% increase in the unobserved skill premium across the four cohorts, on average ($\psi_{89} = 1.109$). For three of the four experience cohorts, there is a 21–32% estimated increase in the return to ability. However, the estimated sampling errors are relatively large.

The bottom panel of the table replicates the analysis for the case where the two groups are defined by race. First, note that in 1979 there is much less heteroskedasticity in residual variances with respect to race than with respect to education, with only a slightly higher wage variance for white workers than for blacks (0.161 vs. 0.154 across cohorts). However, the growth in the amount of racial heteroskedasticity from 1979 to 1989 is striking, both between- and within-cohorts. The estimates of ψ_{89} based on this growth imply that the ability premium more than doubled ($\psi_{89} = 2.02$–2.37) during the 1980s. However, since both the initial year variance differential and the number of blacks in the sample are relatively small, the corresponding sampling errors are extremely large precluding meaningful inference.

4.2. Instrumental variables estimates of ψ_t

There are several potential drawbacks to the two-group Wald estimation approach. First, the aggregation of the J cells into two groups is arbitrary and may result in the loss of useful identifying variation. In addition, since the estimated group variances are not adjusted for the minimum wage censoring, the resulting estimates of ψ_{89} may be biased downward. As a result, a disaggregated analysis of the J experience–race–education cells may be preferable, especially since it accounts for the minimum wage censoring by dropping the cells that are affected by the minimum wage (as well as those affected by the top-code).[31] As described above, a problem with a disaggregate analysis is that since the sample sizes of the cells are smaller, there might be small-sample attenuation biases in estimates of ψ_t arising from nontrivial sampling errors in the estimated cell-level wage variances.

Eq. (8) can be estimated consistently if one can identify instruments that are correlated with $\hat{\sigma}^2_{wj,1}$ but uncorrelated with the measurement error, u_{j1}. The first

[30] The asymptotic standard errors are calculated using a first-order Taylor series approximation of Eq. (10).

[31] Our approach to the censoring problems is similar in spirit to the approach prescribed in Chamberlain (1994).

instrumental variables approach that we use is based on the fact that each annual CPS contains two *strictly random* subsamples of the same population. Specifically, there are two rotation groups of individuals in each CPS that correspond to whether the respondent is in his fourth or eighth month in the sample (rotation groups 4 and 8). These two subsamples were independently drawn from the population, and are therefore independent of each other. As a result, one can purge the attenuation bias in estimates of ψ_t by using one rotation group's estimates of the cell variances as an instrument for the estimates from the other rotation group. We refer to this approach as the 'two-sample' IV estimator.

More formally, from Eq. (8):

$$\hat{\sigma}^2_{wj,89} = (\sigma^2_{\varepsilon,89} - \psi^2_{89}\sigma^2_{\varepsilon,79}) + \psi^2_{89}\hat{\sigma}^2_{wjs,79} - \psi^2_{89}u_{js79} + v_{j89}, \quad (11)$$

$$\hat{\sigma}^2_{wj,79} = \left(\sigma^2_{\varepsilon,79} - \frac{1}{\psi^2_{89}}\,\sigma^2_{\varepsilon,89}\right) + \frac{1}{\psi^2_{89}}\,\hat{\sigma}^2_{wjs,89} - \frac{1}{\psi^2_{89}}\,v_{js89} + u_{j79},$$

where the s subscript denotes which subsample (rotation group) is used for each cell. Terms without the s subscript denote estimates (and errors) that are based on the entire CPS sample (both rotation groups). The second equation in (11) is the 'reverse regression' equation in which the dependent variable is the 1979 within-group wage variance. In order to use all available information from the two years of data, we estimate both the slope (ψ^2_{89}) from the first equation and its reciprocal ($1/\psi^2_{89}$) from the reverse regression, while instrumenting the independent variables in both equations.

Let s' denote the complementary subsample (i.e., the other rotation group) of s. Then $\hat{\sigma}^2_{wjs',79}$ and $\hat{\sigma}^2_{wjs',89}$ are valid instruments for $\hat{\sigma}^2_{wjs,79}$ and $\hat{\sigma}^2_{wjs,89}$, respectively. To derive estimates of the slope and the inverse of the slope, the two regression equations were 'stacked' and then estimated via instrumental variables using the appropriate instruments and weighting by the combined 1979 and 1989 sample sizes for each cell. For example, the between-cohort two-sample IV estimate is based on 1324 observations (331 cells × 2 subsamples per group × 2 linear relations). In addition, the Huber–White robust estimator of the variance–covariance matrix of the two sets of estimates was calculated.[32]

Table 4A presents the two-sample IV estimates of the slope (Column 1) and inverse slope (Column 2) from the two linear relations in Eq. (11) for the between-cohort (Row 1) and within-cohort (Row 2) data. For the between-cohort analysis, the two implied estimates of ψ_{89} are 1.108 and 1.127, suggesting that the ability premium rose about 11–13%. The within-cohort estimates (1.302 and 1.228) imply that the return to ability rose 23–30%. The similarity of the

[32] The estimator of the variance–covariance matrix allows for unrestricted heteroskedasticity and intra-cluster correlated errors, where each of the J cells is a separate cluster.

Table 4. (A) Two-sample IV estimate of ψ_{89}, between- and within-cohort[a]

	Slope (1)	1/Slope (2)	ψ_{89} (3)	χ^2 test (4)	Obs.
Between cohort	1.228	0.787	1.117	0.078	1324
	(0.124)	(0.088)	(0.047)		
Within cohort	1.694	0.663	1.257	0.330	972
	(0.252)	(0.082)	(0.063)		

(B) IV estimates of ψ_{89} using different years, between- and within-cohort[b]

	Between cohort			Within cohort		
Year used as IV	Slope (1)	ψ_{89} (2)	Obs.	Slope (3)	ψ_{89} (4)	Obs.
1981	1.189	1.090	274	1.262	1.124	207
	(0.132)	(0.060)		(0.229)	(0.102)	
1991	1.226	1.107	269	1.577	1.256	180
	(0.131)	(0.059)		(0.186)	(0.074)	
(2SLS) 1981, 1991	1.187	1.089	237	1.360	1.166	163
	(0.122)	(0.056)		(0.186)	(0.080)	

[a]Slope coefficient is estimated by regressing the cell variances (where cells are defined by age, education, race, and sample group) in 1989 on the respective 1979 variance, using the alternate sample as an instrument. The sample size of the age, education, and race cells are used as weights. (1/Slope) coefficient is estimated by the same method but with the 1979 variances as the dependent variable. An estimate of the covariance of the estimated Slope and (1/Slope) is obtained by stacking the two regressions and calculating a heteroskedasticity-consistent and intercorrelation-consistent estimate of the variance–covariance matrix of the estimated Slope and (1/Slope), where the cluster is the group defined by age, education and race. ψ_{89} is estimated by minimum distance using the reduced-form slope coefficients. Chi-square goodness-of-fit (1 dof) is reported for each estimate of ψ_{89}. Estimated standard errors in parentheses.

[b]Robust standard errors in parentheses. Reduced-form slope coefficients are from IV regressions (sample-weighted) of the cell variances in 1989 on those of 1979, using other years' variances as instruments. Estimate of ψ_{89} and its standard error are computed from the estimated slope by taking a first-order Taylor expansion of the square root of the slope, around the true value.

estimates based on the slope and inverse slope suggests that the model may be properly specified.

Given the two 'reduced-form' estimates of (ψ_{89}^2) and ($1/\psi_{89}^2$), we can test the over-identifying restriction implied by our model, while simultaneously deriving a more efficient estimator of ψ_{89} provided that the restriction holds. For the between-cohort data, for example, this is done by minimizing the quadratic form:

$$\left(1.228-\psi_{89}^{2},\ 0.787-\frac{1}{\psi_{89}^{2}}\right)\hat{V}^{-1}\left(1.228-\psi_{89}^{2},\ 0.787-\frac{1}{\psi_{89}^{2}}\right)' \tag{12}$$

with respect to ψ_{89}^2, where $\hat{V}$ is the estimated robust covariance matrix of the reduced-form estimates. The results of this procedure are shown in the next set of table columns and suggest that the rise in the return to unobserved skill was about 12% between cohorts and 26% within cohorts from 1979 to 1989. The estimates are reasonably precise. In addition, the resulting goodness-of-fit test statistics, which are asymptotically $\chi^2(1)$ distributed, imply that the over-identifying restriction of our model cannot be rejected at conventional levels of significance in our data. This result was foreshadowed by the reasonable fit of the regression lines in Figs. 3 and 4.[33]

The estimated two-sample IV regression lines are plotted in Figs. 3 and 4. The estimated intercepts in Figs. 3 and 4 are -0.0058 and -0.0443, respectively. If the transitory component of wages is assumed to have a constant variance over time, then Eq. (6) implies that $\sigma_\varepsilon^2 = 0.024$ and 0.076 for the between- and within-cohort data, respectively. Since the mean within-group variances in 1979 are 0.168 and 0.164, these estimates suggest that about 14% and 47% of the within-group wage variance in the initial period, between- and within-cohorts respectively, is attributable to the transitory error component. More generally, the transitory component variance may be non-stationary, as in Eq. (7). Then the estimated constants suggest that the amount of the overall rise in residual wage variances attributable to an increase in the noise variance is slightly less than the increase attributable to a growth in the variance of the permanent component. It should be noted, however, that the estimated standard errors of the intercepts are relatively large.

A simple alternative to the 'two-sample' IV estimator is an instrumental variables estimator which uses within-group variances from another year as an instrument for the 1979 within-group variance. This approach takes advantage of the independence of the CPS samples over time. Here, we only use the estimated cell variances from the 1981 and 1991 CPSs as instruments in order to avoid the top-code censoring problem that is pervasive in the mid-decade

[33] The fact that our parsimonious model provides a reasonable description of differential growth in wage variances across very diverse cells/sectors is somewhat surprising given that it allows for only one factor, ψ_t, to account for this differential growth. Allowing for only one uniform change in the price of unobserved skill appears to be sufficient for explaining the majority of across cell differences in wage variance growth during the 1980s. This result can be viewed as contrasting slightly with the findings of Heckman and Scheinkman (1987).

CPSs.[34] This approach will result in consistent estimates of ψ_{89}^2 provided that: (1) the sampling errors of the estimated cell variances in 1979 and 1989 are uncorrelated with the sampling errors of the estimated variances in 1981 and 1991; and (2) the true population cell variances in 1979 and 1989 are uncorrelated with the sampling errors from any of the years. Table 4B presents the between-cohort and within-cohort estimates of the reduced-form slope coefficient and the implied ψ_{89} when the 1981 and 1991 cell sample variances are used as instruments.

In the between-cohort data, the estimated ψ_{89} is 1.09 and 1.11 when the 1979 cell variances are instrumented by the 1981 and 1991 variances, respectively. These figures are 1.13 and 1.26 for the within-cohort data. The final row of the table reports the two-stage least squares estimates of ψ_{89} when both the 1981 and 1991 variances are used as instruments simultaneously. They imply that the ability premium increased 9% between cohorts and 17% within cohorts between 1979 and 1989. The commonality of the estimates across the two different instruments, relative to their associated sampling errors, suggests that the identifying assumptions of our model and the implied orthogonality conditions are empirically reasonable. In addition, these estimates are similar in magnitude to the two-sample IV estimates. Note, however, that the sampling errors of the estimates are larger due to the smaller number of cells used in estimation.

4.3. Minimum distance estimates of ψ_t

With multiple years of data, ψ_t is over-identified, and a test statistic of the hypothesis of the equality of the alternative estimates of ψ_t can be derived to gauge the empirical validity of our parsimonious error-components model. In particular,

$$\operatorname{plim}(\hat{\psi}_{s,1}^2 \cdot \hat{\psi}_{t,s}^2) = \psi_t^2, \tag{13}$$

where $\hat{\psi}_{s,1}^2(\hat{\psi}_{t,s}^2)$ is a consistent estimator for $\psi_{s,1}^2(\psi_{t,s}^2)$, which is the slope coefficient when $\sigma_{wj,s}^2(\sigma_{wj,t}^2)$ is expressed as a linear function of $\sigma_{wj,1}^2(\sigma_{wj,s}^2)$. We exploit all of the over-identifying restrictions suggested by Eq. (13) using a method-of-moments framework in which the empirical autocovariance matrix of the estimated within-group wage variances is fit to the elements of the theoretical autocovariance matrix implied by our model.

Given T periods, let M be the $T \times T$ theoretical autocovariance matrix of the estimated within-group variances. With the addition of sampling errors to

[34] We do not use the 1980 and 1990 CPSs to form additional instruments since the overlapping rotation scheme of the CPS creates nonindependent samples in consecutive years.

Eq. (4), our model implies that the lower triangular portion of M is

$$\begin{bmatrix} \mathrm{Var}[\sigma_{aj}^2] + \sigma_{u1}^2 & & & \\ \vdots & \ddots & & \\ \psi_{T-1}^2 \,\mathrm{Var}[\sigma_{aj}^2] & \cdots & \psi_{T-1}^4 \,\mathrm{Var}[\sigma_{aj}^2] + \sigma_{uT-1}^2 & \\ \psi_T^2 \,\mathrm{Var}[\sigma_{aj}^2] & \cdots & \psi_{T-1}^2 \psi_T^2 \,\mathrm{Var}[\sigma_{aj}^2] & \psi_T^4 \,\mathrm{Var}[\sigma_{aj}^2] + \sigma_{uT}^2 \end{bmatrix}, \tag{14}$$

where σ_{ut}^2 is the average variance of the sampling errors across all cells for year t. We are using $T(T+1)/2$ moments to identify $2T$ unknown parameters. Consequently, the model is over-identified whenever 4 or more years of data are used. For the reasons discussed above, we use data from 1979, 1981, 1989, and 1991 to estimate the parameters in Eq. (14).

We minimize the quadratic criterion function:

$$(\mathrm{Vec}[\hat{M}] - \mathrm{Vec}[M])' \,(\mathrm{Vec}[\hat{M}] - \mathrm{Vec}[M]), \tag{15}$$

with respect to the parameters in Eq. (14), where $\hat{M}$ is the empirical autocovariance matrix of the cell variances.[35] Altonji and Segal (1994) present Monte Carlo evidence that suggests that optimal minimum distance estimation of covariance structures can potentially be seriously biased in small samples. As a result, we choose the identity matrix as the weighting matrix in the criterion function since identically weighted minimum distance performed the best in their simulations.

The minimum distance results for the between-cohort (Column 1) and within-cohort (Column 2) data are presented in Table 5. The top portion of the table reports the estimates of ψ_{81}, ψ_{89} and ψ_{91}. It shows that the return to unobserved skill rose by about 10–13% between cohorts and 21–23% within cohorts by the end of the 1980s relative to 1979. This finding is almost identical to the instrumental variables estimation results. The estimates of the 'nuisance' parameters $\mathrm{Var}[\sigma_{aj}^2]$ and $\sigma_{u,t}^2$ are presented in the bottom half of the table. They give a sense of how much of the variation in within-group variances in each year is attributable to 'signal' and how much is due to sampling error 'noise'. For the between-cohort data, slightly less than half of the observed variation in within-group variances is attributable to 'true' variation in the group-specific skill variances. Although the 1979 'signal-to-noise' ratio is higher in the

[35] $\hat{M}$ contains weighted variances and covariances, where the weights are the sum of the number of observations in 1979 and 1989. We also calculated the variance–covariance matrix of $\hat{M}$, which was used to derive sampling errors for the estimated parameters and a goodness-of-fit test statistic.

Table 5
Minimum distance estimates of ψ_t, between- and within-cohort, 1979–1991[a]

		Between cohort (1)	Within cohort (2)
Return to skill (Base = 1979)	1979	1	1
	1981	0.908	0.832
		(0.044)	(0.062)
	1989	1.100	1.231
		(0.057)	(0.071)
	1991	1.134	1.209
		(0.061)	(0.091)
Skill variance (1979)		0.00060	0.00050
		(0.00010)	(0.00010)
Sampling error var.	1979	0.00053	0.00038
		(0.00007)	(0.00008)
	1981	0.00035	0.00037
		(0.00004)	(0.00005)
	1989	0.00056	0.00057
		(0.00007)	(0.00011)
	1991	0.00048	0.00089
		(0.00008)	(0.00010)
Goodness of fit		3.03	14.72
Degrees of freedom		2	2
Number of cells		237	163

[a]Estimated by minimum distance (weighted by the identity matrix). Estimated autocovariance matrix of the cell variances is fit to the error-components model described in text. Estimated standard errors in parentheses.

within-cohort data, it appears that the variance of the sampling error noise component increased during the 1980s.

5. Changes in the college premium and wage discrimination

What do our estimates of the rise in the payoff to unobservable skill imply about the magnitudes of the ability biases in conventional estimates of true changes in relative wages? Our estimates of the increase in the ability premium vary between 9–13% in the between-cohort analysis and about 17–26% within cohorts. Given these numbers, Table 1 suggests that the conventional estimates of changes in the college premium and wage discrimination are relatively free of bias even if there is perfect sorting on ability in the initial period, $\lambda = 1$

Table 6
Estimates of the changes in the college premium and discrimination, assuming $\psi_{91} = 1.30$, 1979–1991[a]

	$\psi_{91} = 1.30$ Change in college premium (1979–1991)							
	Between cohort Years of exp. in 1979				Within cohort Years of exp. in 1979			
	6–10	11–15	16–20	21–25	6–10	11–15	16–20	21–25
$\lambda = 0$	0.241	0.174	0.125	0.122	0.174	0.152	0.121	0.100
$\lambda = 0.25$	0.224	0.153	0.104	0.099	0.157	0.131	0.100	0.078
$\lambda = 0.5$	0.207	0.132	0.083	0.077	0.140	0.111	0.078	0.055
$\lambda = 1$	0.174	0.090	0.040	0.032	0.106	0.069	0.036	0.010
	Change in discrimination (1979–1991)							
	Within cohort Years of exp. in 1979				Within cohort (6–10 yr of exp. in 1979)			
	6–10	11–15	16–20	21–25	Ed $\leqslant$ 12	Ed $\geqslant$ 13		
$\lambda = 0$	− 0.069	0.028	− 0.019	− 0.006	− 0.059	− 0.155		
$\lambda = 0.25$	− 0.060	0.041	− 0.007	0.006	− 0.047	− 0.150		
$\lambda = 0.5$	− 0.051	0.054	0.005	0.018	− 0.036	− 0.146		
$\lambda = 1$	− 0.032	0.079	0.030	0.042	− 0.012	− 0.137		

[a]Entries are the implied 1979–1991 changes in the college premium or 'discrimination' coefficient, given an upper bound of our estimates, $\psi_{91} = 1.30$, and under alternative assumptions about the fraction of the initial wage gap that is due to unobserved productivity differences, λ. The reduced-form changes in the differentials are taken from Table 2.

(see columns with $\psi_{91} = 1.09$ and 1.17). In particular, the college premium still rises by about 0.15–0.17 log points and the black–white wage gap expands 0.02–0.03 points. The estimated ψ_{91} is simply not large enough to support the claim that the observed increases in college–high-school and black–white wage differentials during the 1980s can be completely explained by an increase in the return to an ability component which varies across groups.

The data does not support the hypothesis that time-varying ability biases account for the observed relative wage changes even when we consider a relatively large estimate of ψ_{91}. Given the conventional reduced-form estimates of wage gap changes provided in Table 2, Table 6 summarizes the true changes in the college premium and discrimination from 1979–1991 as a function of λ given a 30% increase in the return to ability ($\psi_{91} = 1.30$). Although almost all of our estimates of ψ_{91} are 0.1–0.2 log points smaller, it is useful to construct an upper bound on how much a rise in the ability premium could bias reduced-form estimates of changes in educational and racial wage differentials.[36]

The upper panel of the table presents both the between-cohort (first set of columns) and within-cohort (second set of columns) movements in the college premium during the 1980s for the four different 5-yr experience cohorts. It appears that the 'causal' effect of education increases over time for a wide range of beliefs about the initial period ability bias, λ. Even if half of the 1979 wage gap is attributable to unobserved productivity differences ($\lambda = 0.5$), a 30% increase in the unobserved skill premium explains much less than half of the observed rise in the college–high-school gap in each cohort and for both the between- and within-cohort data. Even if there is pure ability sorting in the initial period ($\lambda = 1$), the return to college rises between 0.07 and 0.17 log points for the two youngest cohorts in the between- and within-cohort data. For the two oldest cohorts, it appears that the increase in the education differential is smaller and that ability differences and changes in its return could, in principle, play a significant role. However, for the youngest cohort of workers, the rise in the return to ability can account for *at most* 30–40% of the observed expansion in the college–high-school wage gap between 1979 and 1991. This result is consistent with the findings of Cawley et al. (1998) based on NLSY data and AFQT scores.

The bottom panel of Table 6 presents *within-cohort* changes in the black–white wage gap for each of the cohorts. Given a 30% increase in the return to unmeasured productivity, it appears that true changes in labor market discrimination are slightly more sensitive to the assumptions on λ. For example, in the 16–20 experience cohort, if the 1979 black–white gap is fully attributable to discrimination, then wage discrimination against black workers increased by about 0.02 log points by the end of the decade. On the other hand, if one assumes that the initial wage differential is entirely due to black–white productivity differences, then the results imply that racial discrimination actually declined by 0.03 log points during the 1980s.

However, it appears that black men in the youngest cohort experienced a substantial decline in relative wages from 1979–1991, and that even our 'generous' estimate of the growth in the ability premium cannot explain this widening gap. Even under the assumption that discrimination against young black workers did not exist in 1979, wage discrimination seems to have risen by 0.03 log points. This result is even more pronounced among relatively well-educated black men in the youngest cohort. The final two columns of the panel

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[36] Choosing $\psi_{91} = 1.30$ also allows for the possibility that our estimates may be slightly biased down due to the minimum wage censoring. In particular, when we adjusted the cells in our selected sample for the declining relative value of the minimum wage and potential 'spillover' effects, as suggested in Lee (1998), the estimates of ψ_{89} in Table 4B and 5 increased by about 0.05 (but no more than 0.10).

show relative wage changes in the youngest cohort separately for individuals with at most a high school degree and those with at least some college education.[37] It appears that better-educated black workers sustained enormous losses in the 1980s relative to their white counterparts even after accounting for the rising skill premium. In particular, the economic status of black men with at least some college education fell *at least* 0.14 log points, even under the extreme assumption that all of the 0.06 log point wage advantage for white workers in 1979 is attributable to racial differences in productivity.

Suppose there was 'reverse' discrimination against better-educated white workers in 1979, with the 1979 gap understating the true productivity gap by 0.06 log points. We would still find that wage discrimination against well-educated black men increased by about 0.11 log points, which implies the existence of wage discrimination in 1991 of about 0.05 log points. Suppose one claimed that racial discrimination does not exist in 1991 (i.e., all of the −0.21 log point gap in 1991 is due to ability differences). This would imply that 'reverse' discrimination provided a 0.10 log point wage advantage to college-educated black workers in 1979 and that discrimination still increased by 0.10 log points during the decade. We conclude that the hypothesis that the growing wage gap between young black and white workers during the 1980s was largely due to a rise in the skill premium is inconsistent with our model estimates and data.

6. Conclusion

This study assessed the potential contribution of a rise in the return to unobserved ability correlated with education and race to the dramatic increase in the college–high-school wage differential and the stagnation of black–white wage convergence during the 1980s. A relatively unrestricted error-components model was used to estimate the rise in the unobserved skill premium. Identification of the model is based on across-group variation in changes in within-group log-wage variances over time. Therefore, panel data is not required to estimate the model parameters. In the absence of credible instruments for education and race, we calibrated the impact of time-varying 'ability' biases under various assumptions on the extent of non-random sorting of ability. Both between-cohort and within-cohort changes were examined using earnings data on men from multiple Current Population Surveys.

There is systematic across-group variation in changes over time in within-group wage variances, suggesting about a 10–25% rise in the ability premium

[37] Only the youngest experience cohort exhibits a significant interaction between race and education. For the other three cohorts, estimated changes in black–white differentials are similar in the two education categories.

during the 1980s. In addition, there are noticeable differences across cohorts in changes in the college–high-school wage gap. However, the model estimates imply that the rise in the return to ability can account for *at most* 30–40% of the observed rise in the college premium for young workers during the 1980s. Similarly, young, well-educated black men experienced *at least* a 0.13 log point decline in wages relative to their white counterparts between 1979 and 1991.

We hesitate to conclude that the growth in the college–high-school wage gap that remains after absorbing the ability biases is fully attributable to a rise in the market value of a college degree. Such an inference is dependent on our assumption that 'quality' differences between college and high school graduates are held fixed over time in a within-cohort (or between-cohort) analysis. Future research might attempt to obtain credible estimates of changes in relative worker quality over time. In addition, improving our understanding of the driving forces behind the increasing differentials between education groups (e.g., relative demand vs. relative supply shifts) would be very useful. Finally, a second area of future research should focus on directly linking the diverse trends in black–white earnings differentials documented in this study to measures of various channels through which these changes occurred (e.g., affirmative action enforcement).

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