# WORKPLACE HETEROGENEITY AND THE RISE OF WEST GERMAN WAGE INEQUALITY*

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**Abstract**

We study the role of establishment-specific wage premiums in generating recent increases in West German wage inequality. Models with additive fixed effects for workers and establishments are fit in four sub-intervals spanning the period from 1985 to 2009. We show that these models provide a good approximation to the wage structure and can explain nearly all of the dramatic rise in West German wage inequality. Our estimates suggest that the increasing dispersion of West German wages has arisen from a combination of rising heterogeneity between workers, rising dispersion in the wage premiums at different establishments, and increasing assortativeness in the assignment of workers to plants. In contrast, the idiosyncratic job-match component of wage variation is small and stable over time. Decomposing changes in mean wages between different education groups, occupations, and industries, we find that increasing plant-level heterogeneity and rising assortativeness in the assignment of workers to establishments explain a large share of the rise in inequality along all three dimensions.

JEL Codes: J00, J31, J40

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# I Introduction

Wage inequality has risen in many countries, attracting the sustained attention of policy makers and the general public (see Katz and Autor [1999]; and Acemoglu and Autor [2011] for detailed reviews). Most existing studies explain the rise in inequality as a consequence of supply and demand factors that have expanded the productivity gap between high- and low-skilled workers (e.g., Katz and Murphy [1992], Bound and Johnson [1992], Juhn, Murphy and Pierce [1993], Goldin and Katz [2008]). Economists have long recognized, however, that some firms pay higher wages than others for *equally skilled* workers (e.g., Slichter [1950], Rees and Schultz [1970], Krueger and Summers [1988], Van Reenen [1996]). The magnitude of this workplace component of wage inequality is explored in several recent papers, including Abowd, Kramarz, and Margolis [1999], Goux and Maurin [1999], Abowd, Creecy, and Kramarz [2002], Gruetter and Lalive [2009] and Holzer et al. [2011].[1] Virtually all these studies find significant employer-specific wage differentials. To date, however, there is little evidence on whether these differentials have widened over time, and if so whether they can help explain the rise in cross-sectional wage inequality.[2]

In this paper we use detailed administrative data from West Germany to study trends in the dispersion of the workplace-specific wage premiums earned by individuals on different jobs, and measure the contribution of workplace heterogeneity to the rise in inequality. Wage inequality has widened substantially in Germany over the past two decades (see Dustmann, Ludsteck, and Schönberg [2009]). Figure I, for example, shows the evolution of various real wage percentiles for full time male workers in West Germany, indexed to a base of 1996.[3] Over the 13-year period from 1996 to 2009, the gap between the 20th and 80th percentiles of

[1] An earlier generation of studies (e.g., Davis and Haltiwanger [1991], Groshen [1991], Bernard and Jensen [1995]) documented substantial between-plant variation in wages but was unable to deal fully with nonrandom assignment of workers to firms.

[2] Barth et al. [2011] find that between plant inequality has grown over time in the U.S. but are unable to fully account for changes in the pattern of sorting of workers to firms due to limitations in their data. Cardoso [1997, 1999] provides evidence that between plant wage variation grew in Portugal but is again unable to fully account for selection of workers into firms based upon unobservables. Skans et al. [2009] provide similar between-plant evidence for Sweden.

[3] The data underlying this figure are described in detail in Section III, below.

wages expanded by approximately 20 log points, roughly comparable to the rise in inequality in the U.S. labor market over the 1980s.[4]

The German labor market presents an important test case for assessing changes in wage-setting behavior and the role of firm-specific heterogeneity. After a decade or more of disappointing economic performance (Siebert [1997]), the country implemented a series of labor market reforms in the late 1990s and early 2000s, and has recently emerged as one of the most successful economies in the OECD.[5] There is widespread interest in the sources of this recent success and the lessons it may hold for other countries.

To separately identify the impact of rising heterogeneity in pay across different workers and rising heterogeneity in the pay received by the same individual on different jobs, we divide the period between 1985 and 2009 into four overlapping intervals and fit separate linear models in each interval with additive person and establishment fixed effects, as in Abowd, Kramarz, and Margolis [1999] – henceforth, AKM. We then compare the estimates of the person and establishment effects across intervals to decompose changes in the structure of wages.

In an initial methodological contribution we present new evidence on the quality of the approximation to the wage structure provided by AKM's additive worker and firm effects specification. We show that the strong separability assumptions of the AKM model are nearly (but not perfectly) met in the data. In particular, generalized non-separable models with fixed effects for each job yield only a small improvement in explanatory power relative to the AKM specification, both within narrow time intervals and between intervals. We also check for patterns of endogenous mobility that could lead to systematic biases in the AKM specification and find little evidence of such patterns.

Our main substantive contribution is a simple decomposition of the rise in wage inequality

[4]For example, Katz and Murphy [1992] show that the 90-10 gap in log weekly wages for full time male workers rose by 0.18 between 1979 and 1987, while Autor, Katz and Kearney [2008] show that the 90-10 gap in log weekly wages for full time full year male workers rose by 0.25 between 1979 and 1992.

[5]For overviews of recent changes in the German labor market see Eichhorst and Marx [2009], Burda and Hunt [2011], and Eichhorst [2012].

among full time male workers in West Germany. We find that the increase is attributable to increases in the dispersion of both the person-specific and workplace-specific components of pay, coupled with a rise in the assortativeness of job matching that magnifies their joint effect.[6] Overall, the rise in the variance of the person component of pay contributes about 40% of the overall rise in the variance of wages, the rise in the establishment component contributes around 25%, and their rising covariance contributes about a third. We find qualitatively similar results for full time female workers.

We go on to use our estimated models to decompose the rise in between-group inequality across different education, occupation, and industry groups. We find that two-thirds of the increase in the pay gap between higher and lower-educated workers is attributable to a widening in the average workplace pay premiums received by different education groups. Increasing workplace heterogeneity and rising assortativeness between high-wage workers and high-wage firms likewise explain over 60% of the growth in inequality across occupations and industries.

Finally, we investigate two potential channels for the rise in workplace-specific wage premiums: establishment age and collective bargaining status. Classifying establishments by entry year, we find a trend toward increasing heterogeneity among establishments that entered the labor market after the mid-1990s, coupled with relatively small changes in the dispersion of the premiums paid by continuing establishments. The relative inequality among newer establishments is linked to their collective bargaining status: an increasing share of these establishments have opted out of the traditional sectoral contracting system and pay relatively low wages. These patterns suggest that rising wage inequality in West Germany is related to institutional changes in the wage setting process, though the underlying source of these coincident trends is less clear.

[6] Recent contributions by Andersson et al. [2012] and Bagger, Sorensen, and Vejlin [2012] also document increases in assortative matching between workers and employers. Those studies use related statistical methods but assume that person and establishment effects are stationary over the entire sample period.

# II Background - Macro Trends and Institutional Changes

As background for our empirical analysis this section briefly summarizes some of the major changes that have affected the West German labor market since the early 1980s. Two critical events were the collapse of the Soviet empire and the reunification of East and West Germany.[7] An immediate consequence of these political developments was massive in-migration to West Germany. Approximately 1.7 million former residents of East Germany moved to the west in the early 1990s (see Burda [1993] and Wolff [2009]). An even larger number of ethnic Germans (approximately 2.8 million people) arrived from Russia and the former East Bloc countries during the 1990s (Bauer and Zimmerman [1999]). These new migrants – many of whom lacked modern training and language skills – contributed to the rise in unemployment in West Germany (Glitz [2012]) and the build-up of pressure for labor market reform.

More subtly, the decision to impose West German wage scales on the less-productive East led to fissures in the traditional collective bargaining system (Burda [2000]). Until the 1990s most West German firms accepted the provisions of the major sectoral agreements negotiated between employer associations and large unions. These pay scales proved to be far too high for East German firms, however, leading to massive defections from the system – a process that is allowable under German law (see Ochel [2003]). Eventually the defections spilled over to the West, prompting a sharp decline in the fraction of employees covered by collective agreements. For example, data presented by Kohaut and Ellguth [2008] and Ellguth, Gerner, and Stegmaier [2012] show a fall in collective bargaining coverage in West Germany from 83% in 1995 to 63% in 2007.[8]

A second and related phenomenon is the adoption of "opt-out" or "opening" clauses by

[7] These processes began in the late 1980s and ended in the early 1990s. The Berlin wall fell in November 1989. Economic reunification was achieved in spring 1990 with the elimination of the East German mark in June of that year. The country was officially reunified in October 1990.

[8] As discussed by Fitzenberger et al. [2012], not all workers in a given establishment necessarily have the same contractual coverage status. For example, managers and temporary workers (who are included in our data set) are exempt. The coverage rates in the text assign a single establishment status to all employees and should be interpreted carefully.

firms that have remained part of the sector-level agreement (e.g., Hassel and Rehder [2001], Heinbach [2006]). Originally intended for firms in the East, these clauses allow individual plants to depart from the agreement, typically in response to the threat of closure or job loss.[9] A study by Hassel and Rehder [2001] suggests that about one-half of the top 120 firms in West Germany had signed opening clauses by 2000 and that about 43% of their employees were covered by these pacts.[10]

By the mid-1990s the unemployment rate in Germany had risen to nearly 10 percent. Intensifying pressure for labor market reform led to the passage of the *Labor Law Act for Promotion of Employment* in October 1996. This law extended the maximum duration of fixed-term contracts, raised the establishment size threshold for dismissal protection, and reduced the replacement rate for sick leave pay. The trend toward liberalization was partially reversed in 1999 with a law bringing "marginal jobs" (part-time jobs with low earnings) that were previously excluded from Social Security taxes into the tax system.[11] With the recession in 2001, however, pressures for reform re-emerged, ultimately leading to adoption of the Hartz Reforms in 2003-2005. These lowered the generosity of benefits for exhaustees of regular unemployment benefits, while introducing subsidies for low-wage jobs (Jacobi and Kluve [2006]). In addition, the employee portion of Social Security taxes for "mini-jobs" (jobs paying less than 400 Euros per month) was eliminated, providing a further impetus for the expansion of part-time/low wage work.

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[9] For example, the 1993 sectoral agreement in metal working provided for an opening clause (or hardship clause) for plants in East Germany. As of the mid-1990s, the East German metalworking employer association estimated that 60% of plants were making use of the clause (EIRO [1997a]). The 1997 sectoral agreement in the chemical industry in West Germany allowed for plant-specific wage cuts of up to 10% to save jobs (EIRO [1997b]).

[10] Many firms that recognize the sectoral contract also pay a wage premium *above* the sectoral minimum (see Jung and Schnabel [2009]). We are not aware of any research that shows whether this so-called "wage cushion" component has become more or less variable over time.

[11] The firm size threshold for dismissal protection was also raised back to 10 employees, and fixed term contracts were limited to initial employment only (instead of being renewable).

# III Data

We use earnings records from the German Social Security system that have been assembled by the Institute for Employment Research into the Integrated Employment Biographies (IEB) datafile (see Oberschachtsiek et al. [2009]). These data include total earnings and days worked at each job in a year, as well as information on education, occupation, industry and part-time or full time status. With the exception of civil servants and self-employed workers, nearly all private sector employees in Germany are currently included in the IEB.

For our main analysis we focus on daily wages at *full-time* jobs held by men age 20-60. Since the IEB does not include hours of work, limiting attention to full time jobs reduces the impact of hours dispersion that could confound trends in inequality.[12] Fewer than 7 percent of male workers in the IEB have no full time job in a year, so the inclusion of wages for part-time men has only a small impact on the trends we study. As a check on our main conclusions we present a parallel analysis for full-time female workers. A smaller majority of German women work full time (e.g., only about 64% in 2000), raising potential concerns about selectivity biases.[13] Unfortunately, many part-time women work in mini-jobs that were not covered by Social Security until 1999, so it is difficult to study trends for part-time female workers using IEB data. The available data, however, suggest that general trends for all female workers are not be too different from those for full time women (see below).

To construct our sample we begin with the universe of full time jobs held by workers age 20-60 in each year from 1985 to 2009. We exclude mini-jobs (which are only included after 1999) and jobs in which the employee is undergoing training. As explained in more detail in the Online Appendix, we sum the earnings received by a given individual from each

[12] A detailed analysis by Dustmann, Ludsteck, and Schönberg [2009, Appendix Table 7] of data from the German Socio-economic Panel shows no change in the variance of hours among full time male workers in West Germany after 1990, suggesting that hours variation is not a major source of the rise in wage dispersion we document.

[13] The relative size of the full time female workforce has been relatively stable over the past 25 years, suggesting that the selectivity biases among full time workers may be relatively constant. The rise in female employment rates since the mid-1990s in Germany has mainly occurred through an expansion of part-time employment.

establishment in each year and designate the one that paid the highest total amount as the main job for that year. Most full time workers are employed at only one establishment in any year (the average is around 1.1 per year) and there is no trend in the number of jobs held per year, so we believe the restriction to one job per year is innocuous (see Appendix Table A.1). We calculate the average daily wage by dividing total earnings by the duration of the job spell (including weekends and holidays). An individual who has no full time job in a given calendar year is assigned a missing wage for that year.

The establishment identifiers in the IEB are assigned for administrative purposes and may combine multiple work sites owned by the same firm if they are in the same industry and municipality. A new establishment identifier (EID) is issued whenever a plant changes ownership, so the "death" of an establishment identifier does not necessarily mean that the plant has closed, nor does the "birth" of a new EID necessarily mean that a new plant has opened.[14] While this makes it difficult to identify plant closings, for purposes of modeling wage determination we believe it is appropriate to treat an ownership change as a potential change in the workplace component of pay, even when a plant remains open. A new owner, for example, may introduce a bonus system that alters the workplace component of pay. In cases where a new EID is assigned to a continuing plant, there is no bias in treating the "new" EID as a new establishment, only a potential loss in efficiency, since the old and new establishments can have the same impacts on wages.

Table I illustrates some basic characteristics of our wage data, showing information for every 5th year of the sample for men in the upper panel and women in the lower panel. The data set includes 12 to 14 million full time male wage observations in any year, and 6 to 7 million full time female wage observations. As shown in column 2 of the table, average real daily wages of full time men rose by about 8% between 1985 and 1990, then were relatively stable over the next 20 years. Average real daily wages of full time women rose by 11% between 1985 and 1990 and another 6% between 1990 and 1995, but then stabilized at a

[14] Using clusters of worker flows between establishments, Schmeider and Hethey [2010] estimate that only about one half of EID births and deaths in the IEB are true plant openings or closings.

level about 30 log points below the mean for men.[15] The standard deviation of log wages for both gender groups rose slightly between 1985 and 1995, then surged over the next 15 years, rising by 12 log points for men and 10 log points for women from 1995 to 2009.

An important limitation of the IEB data is the censoring of earnings at the Social Security maximum. As shown in column 4 of Table II, 10 to 12 percent of male wage observations and 1 to 3 percent of female wage observations are censored in each year. To address the problem of censoring we follow Dustmann, Ludsteck, and Schönberg [2009] and use a series of Tobit models – fit separately by gender, year, education level (5 categories), and age range (4 10-year ranges) – to stochastically impute the upper tail of the wage distribution. Since our primary interest is in models that include person and establishment effects, we develop an imputation procedure that captures the patterns of within person and within establishment dependence in the data. Specifically, our Tobit models for a given year include the worker's earnings and censoring rate in all other years, as well as the mean earnings and censoring rate of his or her co-workers in that year. Using the estimated parameters from these models, we replace each censored wage value with a random draw from the upper tail of the appropriate conditional wage distribution (see the Online Appendix for details).

The impact of this imputation procedure is illustrated in columns 5 and 6 of Table I, where we show the means and standard deviations of log daily wages after replacing censored observations with allocated values from the Tobit models. For women the means and standard deviations are only slightly higher than in columns 2 and 3, reflecting the relatively low censoring rates. For men, the allocation procedure matters more: on average the imputation raises the estimated mean log wage by 2.7 percentage points and the estimated standard deviation by 4.5 percentage points, with slightly larger effects in years with a higher censoring rate.

Although we believe that this imputation procedure works reasonably well, a natural concern is that our results – particularly for men – would be somewhat different if we used

[15] See Anotnczyk, Fitzenberger, and Sommerfeld [2010] for further discussion of recent trends in male-female wages differences in Germany.

a different technique.[16] To address this concern, we present robustness checks based on the subset of full time male workers with apprenticeship training. This group, which includes about 60 percent of the German male workforce, has relatively low censoring rates. As we show below, our main conclusions are very similar when we use only the apprentice subsample.

# IV Overview of Trends in Wage Inequality

Figure II plots four measures of wage dispersion for full time males: the standard deviation of log wages (including imputed wages for censored observations), the gap in log wages between the 80th and 20th percentiles, the gap between the 80th and 50th percentiles, and the gap between the 50th and 20th percentiles. To facilitate comparisons we normalize the gap measures by dividing by the corresponding percentile gaps of a standard normal variate.[17] If log wages were normally distributed, the normalized gaps and the standard deviation would all be equal. While this is evidently not the case, the *trends* in the standard deviation and the normalized gaps are quite similar. In particular, the standard deviation rose by 15 log points from 1985 to 2009, the (normalized) 80-20 gap rose by 16 log points, the 80-50 gap rose by 15 log points, and the 50-20 gap rose by 18 log points. Since the gap measures are unaffected by censoring, these similarities suggests that our imputation procedure does not lead to major biases in estimating the change in wage dispersion over time. Another notable feature of the data in Figure II is that the growth rates in all four measures of inequality increased in the mid-1990s. For example, the growth rate of the standard deviation of log

[16] Dustmann, Ludsteck, and Schönberg [2009] present an extensive robustness analysis in which they evaluate several alternatives to their basic Tobit imputation models, and conclude that they give similar results. We conducted our own validation exercise by taking data for younger men with apprenticeship training (who have censoring rates under 1%), artificially censoring the data at thresholds such that 10, 20, 30, or 40 percent of the observations were censored, applying the same Tobit models used in our main analysis to these samples, imputing the upper tail observations in each sample, and then re-estimating trends in the dispersion of wages. The results, summarized in the online Appendix, suggest that use of imputed wages leads to a slight upward bias in the measured variance of wages in each year, but no bias in the measured trend in inequality.

[17] For example, we divide the 80-20 gap by $\Phi^{-1}(0.8) - \Phi^{-1}(0.2) = 1.683$, where $\Phi(.)$ represents the standard normal distribution function.

wages increased from 0.23 log points per year in the 1985-1996 period to 0.96 log points per year from 1996 to 2009.

Figure III compares the trend in the normalized 80-20 wage gap for full time men with the trends for three other groups of workers: all men (i.e., full- and part-time), full time women, and all women.[18] As noted earlier, the fraction of male workers with no full-time job in a year is relatively low in West Germany (around 2% in 1985, and just under 7% in 2008), so the addition of part-time workers to the male sample has only a small effect on measured inequality. Wage inequality among full time female workers is higher than among full time men and rises a little less over our sample period, though the general trends for the two groups are quite similar. Inequality in daily wages for **all** regularly employed females (i.e., including all jobs except untaxed mini-jobs) is even higher, perhaps reflecting the variation in hours of work among part-timers, but rises more slowly than for full time women or men. Even for the broadest sample of female workers, however, there is evidence of an acceleration in inequality in the mid-1990s. Given the broad similarity in trends across the various groups, we concentrate for the remainder of this section on full time men. We return to consider full time women in more detail in Section VI.

### Trends in Residual Wage Inequality

Starting with the seminal U.S. studies by Katz and Murphy [1992] and Bound and Johnson [1992], analysts have noted that a large share of recent rises in wage inequality have occurred *within* conventional skill groups. This is also true in West Germany, as we show in Figure IV, which plots the residual standard deviations of log wages from a series of linear regression models, each fit separately by year. As a point of departure the top line in the figure shows the trend in the unadjusted standard deviation of wages, which rises from 0.37 to 0.52 between

[18] For this analysis we use the Sample of Integrated Labor Market Biographies (SIAB), a two-percent public use sample that combines information from the IEB with other administrative data bases (Dorner et al. [2011]). We use the same procedures as for our IEB sample but do not impute wages for censored observations. Riphahn and Schnitzlein [2011] use the SIAB data to document trends in inequality for men and women together in West and East Germany. Their results for West Germany are very similar to ours. They show that wage inequality in East Germany has risen somewhat faster than in the West.

1985 and 2009. The second line in the figure is the standard deviation of the residuals from a standard Mincerian earnings function (with dummies for four education levels and a cubic experience term) fit separately by year. Residual inequality rises a little less than overall wage inequality (from 0.30 in 1985 to 0.43 in 2009), but exhibits the same shift in trend in the mid-1990s.

Several recent studies have suggested that part of the rise in U.S. wage inequality is attributable to a rise in the variation in wages across industries (e.g., Bernard and Jensen [1995]) and/or occupations (e.g., Autor, Levy and Murnane [2003]; Autor, Katz and Kearney [2008]). The third, fourth and fifth lines in the figure show the trends in the residual standard deviation of wages after controlling for industry (∼300 dummies with separate coefficients in each year), occupation (∼340 dummies), and industry × occupation (∼28,000 dummies). While time-varying industry and occupation controls clearly add to the explanatory power of a standard wage equation, they have only a modest impact on the trend in residual inequality.[19] We return in Section VI, below, to examine trends in between-occupation and between-industry inequality in light of our econometric decomposition of wage inequality into person and establishment effects.

In contrast to the rather modest effect of industry and occupation controls, the bottom line in Figure IV shows that adding dummies for each establishment (with year-specific coefficients) has a sizeable impact on the trend in inequality. Within-plant inequality, as measured by the residual standard error of the regression model rises by only 0.05 between 1985 and 2009, compared to the 0.13 rise for the baseline model that controls for education and experience. This contrast suggests that rising heterogeneity in wages offered by different employers may explain some of the rise in German wage inequality. We caution, however, that non-random sorting of workers to establishments makes it very hard to interpret the estimates

[19] A basic human capital model (dummies for education and cubic in experience) has an $R^2$ coefficient of about 0.35. Adding industry or occupation controls raises the $R^2$ to about 0.50. Adding the interaction of occupation and industry raises it to about 0.60. In an earlier draft (Card, Heining and Kline [2012]) we also presented specifications that control for Federal State. However, there is little change in mean wages across states so these controls add very little to the basic Mincer specification.

from wages models with establishment effects but no controls for unobserved worker skills. Even if there are no workplace-specific wage premiums, one could still observe significant and increasingly important establishment effects if workers at the same establishment have similar unobserved abilities and the returns to these abilities are rising over time, or if the degree of sorting across establishments is rising.

To study trends in workplace sorting we developed two indexes that are described at greater length in an earlier version of this paper (Card, Heining and Kline [2012]). The first is an index of educational sorting based on the coefficient from a regression of the mean level of schooling at worker $i's$ establishment in year $t$ on his own schooling measured in that year. As noted by Kremer and Maskin [1996], this coefficient can range from 0 (no sorting) to 1 (perfect sorting). The value of the index for full time male workers increases steadily, from 0.34 in 1985 to 0.47 in 2009.

Our second measure of sorting examines the degree of occupational segregation across workplaces. Specifically, we divide three digit occupations into 10 equally sized groups, based on mean wages in each occupation during the period 1985-1991. We then compute Theil indices of segregation for the decile groups across establishments in each year. The Theil index ranges from 0 (perfect integration) to 1 (perfect segregation) and can be interpreted as a rescaled likelihood ratio test for the null hypothesis that every establishment employs the national occupation mix (Theil and Finezza [1971]). Over our sample period the index rises steadily from 0.46 to 0.53, implying that high- and low-wage occupations are increasingly segregated between establishments.

Overall we conclude that the degree of sorting of different education and occupation groups to different establishments has risen in West Germany over our sample period. This rise may account for some share of the the increasing importance of establishment effects in a wage model.

## An Event Study Analysis of the Effect of Job Changes on Wages

If the variation in wages across establishments is mainly due to sorting then people who change workplaces will not necessarily experience systematic wage changes. If, on the other hand, different establishments pay different average wage premiums, then individuals who join a workplace where *other* workers are highly paid will on average experience a wage gain, while those who join a workplace where others are poorly paid will experience a wage loss. Figures Va and Vb present simple event-study analyses that examine the wage effects of job transitions in the early (1985-1991) and later (2002-2009) years of our sample, classifying the origin and destination workplaces by the mean wages of other workers at those workplaces.

Specifically, we begin by calculating the distribution of mean co-worker wages across all person-year observations in a given time interval (1985-91 or 2002-09). For job changers with at least two consecutive years in both the old and new jobs, we classify the *old* job based on the quartile of co-worker mean wages in the last year at that job, and the *new* job based on the quartile of co-worker mean wages in the first year on that job. We then assign job changers to 16 cells based on the quartiles of co-worker wages at the origin and destination workplaces. Finally, we calculate mean wages in the years before and after the job change event in each cell.[20]

For clarity the figures only show the wage profiles for workers leaving quartile 1 and quartile 4 jobs (i.e., those with the lowest-paid and highest-paid co-workers). Online Appendix Table A.3 provides a complete listing of mean wages before and after the job change event for each of the 16 cells in the two intervals. In that table we also show the numbers of movers in each cell (which range from 30,000 to 500,000) and a trend-adjusted wage change for each job change group.

The figures suggest that different mobility groups have different wage levels *before* and

[20] We drop observations at establishments with only one full time male employee. We also exclude job changers who spend a year (or more) out of full time employment in between consecutive full time jobs. Note that job changers could move directly from job to job, or have an intervening spell of non-employment (or part time employment). Finally, since the sample periods include 7 or 8 years, some individuals can appear in the event study more than once.

*after* a move. For example, average wages prior to a move for workers who go from quartile 4 to quartile 1 jobs are lower than for those go from quartile 4 to quartile 2 jobs, with similar patterns for the other mobility groups. Within mobility groups there is also strong evidence that moving to a job with higher-paid co-workers raises pay. People who start in quartile 1 jobs and move to other quartile 1 jobs have relatively constant wages, while those who move to higher quartile jobs experience wage increases. Likewise, people who start in quartile 4 jobs experience little change (other than a modest upward trend affecting all groups in 1985-1991) if they move to another quartile 4 job, but otherwise suffer wage losses, with larger losses for those who move to lower-quartile jobs.

Comparing Figures Va and Vb, it appears that the size of the wage gains and losses associated with job transitions grew dramatically from the late 1980s to the 2000s. In the 1985-1991 period, for example, a transition from quartile 1 to quartile 4 was associated with a trend-adjusted wage increase of roughly 23 log points, while in the 2002-2009 period, the same transition was associated with a 47 log point increase. Likewise, in the 1985-1991 period, a transition from quartile 4 to quartile 1 was associated with a trend-adjusted wage loss of 22 log points, while in the 2002-2009 period, the same transition yielded a 43 log point drop. This striking growth in the magnitude of the wage gains and losses associated with job mobility is one of our key findings, and underlies our results in Section VI on the growing role of establishment heterogeneity in wage inequality.

Another interesting feature of Figures Va and Vb is the approximate symmetry of the wage losses and gains for those who move between quartile 1 and quartile 4 establishments. The gains and losses for other mover categories exhibit a similar degree of symmetry, particularly after adjusting for trend growth in wages (see Appendix Table A.3). This symmetry suggests that a simple model with additive worker and establishment effects may provide a reasonable characterization of the mean wages resulting from different pairings of workers to establishments.[21]

[21] Notice that if the mean log wage paid to worker $i$ at establishment $j$ can be written as $m_{ij} = \alpha_i + \psi_j + z_{ij}$, where $z_{ij}$ is a random error, then the average wage gain for moving from establishment $j$ to establishment

A final important characteristic of the wage profiles in Figures Va and Vb is the absence of any Ashenfelter [1978] style transitory dip (or rise) in the wages of movers in the year before moving.[22] The profiles of average daily wages are remarkably flat in the years before and after a move. Taken together with the approximate symmetry of the wage transitions noted above, these flat profiles suggest that the wages of movers may be well-approximated by the combination of a permanent worker component, an establishment component, and a time varying residual component that is uncorrelated with mobility.

# V Econometric Model and Methods

With this background we now turn to our econometric framework for disentangling the components of wage variation attributable to worker-specific and employer-specific heterogeneity. In a given time interval our data set contains $N^*$ person-year observations on $N$ workers and $J$ establishments. The function $\boldsymbol{J}(i,t)$ gives the identity of the unique establishment that employs worker $i$ in year $t$. We assume that the log daily real wage $y_{it}$ of individual $i$ in year $t$ is the sum of a worker component $\alpha_i$, an establishment component $\psi_{\boldsymbol{J}(i,t)}$, an index of time-varying observable characteristics $x_{it}'\beta$, and an error component $r_{it}$:

$$y_{it} = \alpha_i + \psi_{\boldsymbol{J}(i,t)} + x_{it}'\beta + r_{it}. \tag{1}$$

Following AKM, we interpret the person effect $\alpha_i$ as a combination of skills and other factors that are rewarded equally across employers. Likewise, we interpret the index $x_{it}'\beta$ as a combination of lifecycle and aggregate factors that affect worker $i$'s productivity at all jobs.

$k$ is $\psi_k - \psi_j$, while the average gain for moving from $k$ to $j$ is $\psi_j - \psi_k$, i.e., the wage changes for movers in the two directions are equal and opposite. If wages contain a common trend component, the trend-adjusted wage changes will be symmetric.

[22] Ashenfelter [1978] noted that participants in job training programs were likely to experience a transitory dip in earnings in the year prior to entering the program. Our setting is different because we are studying job transitions, and because we measure average daily wages rather than annual earnings. Changes in the number of days worked at a constant wage (due to a spell of unemployment after a job loss, for example) will not affect our estimates.

We include in $x_{it}$ an unrestricted set of year dummies as well as quadratic and cubic terms in age fully interacted with educational attainment. Finally, we interpret the establishment effect $\psi_j$ as a proportional pay premium (or discount) that is paid by establishment $j$ to all employees (i.e., all those with $\boldsymbol{J}(i,t) = j$). Such a premium could represent rent-sharing, an efficiency wage premium, or strategic wage posting behavior (e.g., Burdett and Mortensen [1998], Moscarini and Postel-Vinay [2012]).

We assume that the error term $r_{it}$ in equation (1) consists of three separate random effects: a match component $\eta_{i\boldsymbol{J}(i,t)}$, a unit root component $\zeta_{it}$, and a transitory error $\varepsilon_{it}$:

$$r_{it} = \eta_{i\boldsymbol{J}(i,t)} + \zeta_{it} + \varepsilon_{it}.$$

The match effect $\eta_{ij}$ represents an idiosyncratic wage premium (or discount) earned by individual $i$ at establishment $j$, relative to the baseline level $\alpha_i + \psi_j$. We assume that $\eta_{ij}$ has mean zero for all $i$ and for all $j$ in the sample interval. Match specific wage components arise in models in which there is an idiosyncratic productivity component associated with each potential job match, and workers receive some share of the rents from a successful match (e.g., Mortensen and Pissarides [1994]). The unit root component $\zeta_{it}$ captures drift in the portable component of the individual's earnings power. Innovations to this component could reflect (market-wide) employer learning, unobserved human capital accumulation, health shocks, or the arrival of outside offers which, in some models (e.g., Postel-Vinay and Robin [2002]), bid up the offered wage at the current job and other potential jobs. We assume that $\zeta_{it}$ has mean zero for each person in the sample interval, but contains a unit root.[23] Finally, the transitory component $\varepsilon_{it}$ represents any left-out mean reverting factors. We assume that $\varepsilon_{it}$ has mean zero for each person in the sample interval.

Let $y$ denote the stacked $N^* \times 1$ vector of wages sorted by person and year, $D \equiv \left[d^1, ..., d^N\right]$ an $N^* \times N$ design matrix of worker indicators, $F \equiv \left[f^1, ..., f^J\right]$ an $N^* \times J$ design matrix of firm indicators, $X \equiv \left[x^1, ..., x^K\right]$ an $N^* \times K$ matrix of time varying covariates, and $r$ an

[23] Thus, the mean zero restriction on this component *defines* the person specific intercept $\alpha_i$.

$N^* \times 1$ vector of composite errors. Then our model can be written in matrix notion as:

$$\begin{aligned} y &= D\alpha + F\psi + X\beta + r \\ &= Z'\xi + r \end{aligned} \tag{2}$$

where $Z \equiv [D, F, X]$ and $\xi \equiv [\alpha', \psi', \beta']'$.

We estimate (2) by ordinary least squares (OLS). These estimates solve the standard normal equations:

$$Z'Z\xi = Z'y \tag{3}$$

A unique solution requires that the matrix $Z'Z$ has full rank. As shown by AKM and Abowd, Creecy, and Kramarz [2002], the establishment and person effects in (1) are only separately identified within a "connected set" of establishments that are linked by worker mobility. To simplify estimation, we restrict our analysis to the largest connected set of establishments in each time interval. Within the largest connected set – which includes over 95% of the workers and 90% of the establishments in each interval – we normalize the establishment effects by omitting the last establishment dummy. The Online Appendix provides details of our procedure for obtaining a solution to the normal equations. In brief, we use an iterative conjugate gradient algorithm (as in Abowd, Creecy, and Kramarz [2002]) which solves for the vector of coefficients $\xi$ without actually inverting the matrix $Z'Z$.

## Assumptions on the Assignment Process

For OLS to identify the underlying parameters of interest, we need the following orthogonality conditions to hold:

$$E\left[d^{i\prime}r\right] = 0 \;\forall i, E\left[f^{j\prime}r\right] = 0 \;\forall j, E\left[x^{k\prime}r\right] = 0 \;\forall k. \tag{4}$$

The assumption that all three components of $r$ are orthogonal to the time-varying covariates $x^k$ is standard. Moreover, our assumptions on the means of $\eta_{ij}$, $\zeta_{it}$, and $\varepsilon_{it}$ imply that $E\left[d^{i\prime}r\right] = 0$. Thus, the key issue for identification is whether the composite errors $r$ are orthogonal to the vectors of establishment identifiers $f^j$.

A sufficient condition for $E\left[f^{j\prime}r\right] = 0$ to hold for every establishment $j$ is that the assignment of workers to establishments obeys a strict exogeneity condition with respect to $r$:

$$P\left(\boldsymbol{J}\left(i,t\right)=j|r\right)=P\left(\boldsymbol{J}\left(i,t\right)=j\right)=G_{jt}\left(\alpha_i,\psi_1,...,\psi_J\right)\ \forall i,t \tag{5}$$

where the employment probability functions $G_{jt}\left(.\right)$ sum to 1 for every worker in every period.[24] Importantly, (5) does **not** preclude systematic patterns of job mobility related to $\alpha_i$ and/or $\{\psi_1,...,\psi_J\}$.[25] For example, a comparison of the number of job movers underlying the profiles in Figures Va and Vb suggests that workers are more likely to move from low to high wage establishments than to move in the opposite direction (see Online Appendix Table A.3). This does not represent a violation of (4) because our fixed effects estimator conditions on the actual sequence of establishments at which each employee is observed. Similarly, higher (or lower) turnover rates among lower-productivity workers is fully consistent with (4), as is the possibility that high skilled workers are more (or less) likely to transition to workplaces with higher wage premiums. Finally, as the subscripts on the function $G_{jt}\left(.\right)$ make clear, mobility may be related to fixed or time-varying nonwage characteristics of establishments, such as location or recruiting effort. Such mobility aids in identification by expanding the connected set of establishments.

We now consider three forms of "endogenous mobility" that violate (5) and could cause biases in our approach. The first is sorting based on the idiosyncratic match component of

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[24]**Proof:** $E\left[f^{j\prime}r\right] = E\sum_{i,t} f^j_{it}r_{it} = E\sum_{i,t} E\left[f^j_{it}|r\right]r_{it} = E\sum_{i,t} G_{jt}\left(\alpha_i,\psi_1,...,\psi_J\right)r_{it} = \sum_{i,t} G_{jt}\left(\alpha_i,\psi_1,...,\psi_J\right)E\left[r_{it}\right]=0.$

[25]For instance, mobility might follow a stationary Markov process: $P\left(\boldsymbol{J}\left(i,t+1\right)=j'|\boldsymbol{J}\left(i,t\right)=j\right) = H_{j,j'}\left(\alpha_i,\psi_1,...,\psi_J\right)$ which (along with an appropriate initial condition) would lead the set of worker-firm assignments to obey (5) in each period.

wages, $\eta_{ij}$. This form of sorting – which is familiar from the standard Roy [1951] model – changes the interpretation of the estimated establishment effects, since different workers have different wage premiums at any given establishment, depending on the value of their match component.[26]

It is possible to test for such sorting in two ways. First, if workers tend to select jobs based on the match component, then we would expect the (trend adjusted) wage *gains* for workers who move from one establishment to another to be quite different from the wage *losses* for those who make the opposite transition. Ignoring any wage growth arising from experience or year effects, and any correlation of mobility with $\zeta_{it}$ or $\varepsilon_{it}$, the expected wage change for a worker who moves from establishment 1 to establishment 2 between period $t-1$ and $t$ is:

$$E\left[y_{it}-y_{it-1}|\boldsymbol{J}\left(i,t\right)=2,\boldsymbol{J}\left(i,t-1\right)=1\right]=\psi_2-\psi_1+E[\eta_{i2}-\eta_{i1}|\boldsymbol{J}\left(i,t\right)=2,\boldsymbol{J}\left(i,t-1\right)=1],$$

while the expected wage change for a worker who moves in the opposite direction is

$$E\left[y_{it}-y_{it-1}|\boldsymbol{J}\left(i,t\right)=1,\boldsymbol{J}\left(i,t-1\right)=2\right]=\psi_1-\psi_2+E[\eta_{i1}-\eta_{i2}|\boldsymbol{J}\left(i,t\right)=1,\boldsymbol{J}\left(i,t-1\right)=2].$$

By contrast, under our maintained assumptions, the expected wage changes are $\psi_2-\psi_1$ and $\psi_1-\psi_2$, respectively. As the importance of the match components increases, the sorting bias terms $E[\eta_{i2}-\eta_{i1}|\boldsymbol{J}\left(i,t\right)=2,\boldsymbol{J}\left(i,t-1\right)=1]$ and $E[\eta_{i1}-\eta_{i2}|\boldsymbol{J}\left(i,t\right)=1,\boldsymbol{J}\left(i,t-1\right)=2]$, both of which are positive, will dominate, leading to wage gains for movers in both directions. We have already seen from the simple event studies in Figures Va and Vb that the gains associated with transitioning from a low- to a high- co-worker-wage establishment are roughly equal to the losses associated with moving in the opposite direction. Moreover, the mean wage changes for workers who move between establishments in the same co-worker

[26] See French and Taber [2011] for a detailed discussion of Roy-type models and references to the related literature.

wage quartile are close to zero in interval 4 (a period with negligible aggregate wage growth), suggesting that there is no general mobility premium for movers. We examine these issues in more detail below by looking directly at wage changes for workers who move between establishments with different estimated fixed effects, and reach the same conclusions: wage gains and losses are (roughly) symmetric for movers between higher- and lower-wage establishments, and there are no wage gains for moving between establishments with similar estimated fixed effects.

Second, if match effects are important, a fully saturated model that includes a separate dummy for each job ought to fit the data much better than our additively separable baseline model. As we show below, the job match model has a better fit statistically, but the improvement is small. The standard deviation of $\eta_{ij}$ implied by the improvement in fit is in the relatively modest range of 0.06-0.08, which limits the scope for potential endogeneity.[27]

A second form of endogenous mobility may arise if drift in the expected wage a person can earn at all jobs (i.e., the shocks to the unit root error component $\zeta_{it}$) predicts firm-to-firm transitions. For example, in learning models with comparative advantage (e.g., Gibbons et al., [2005]) some components of worker ability are revealed slowly over time. If these abilities are valued differently at different establishments, workers who turn out to be more productive than expected will experience rising wages at their initial employer, and may also be more likely to move higher-wage establishments (i.e., firms specializing in skilled workers).[28] Likewise workers who turn out to be less productive than expected will experience wage declines, and will be more likely to move to lower-wage establishments. Such patterns will lead to an overstatement of the importance of establishments, as the drift component $\zeta_{it}$ in wages will be positively correlated with the change in the establishment effects. The absence of any systematic trends in wages prior to a move for workers who transition to

[27] Small match effects in wages do not necessarily imply small match effects in productivity as workers may simply have low bargaining power in negotiating with their employers. Several recent studies have found a low bargaining share for workers (Card, Devicienti, and Maida [2010], Cahuc, Postel-Vinay and Robin [2006], Carlsson, Messina, and Skans [2011]).

[28] Gibbons et al. [2005] consider the case where different *sectors* value skills differently. Their model could be extended to deal with differences across employers within a sector.

better or worse jobs casts doubt on the importance of learning as a major source of bias in our estimates.[29]

The drift component $\zeta_{it}$ could also be correlated with mobility patterns if workers obtain outside offers which bid up their wages and also predict transitions to higher-wage establishments (as in Postel-Vinay and Robin [2002]). This "offer-shopping" mechanism implies that OLS estimates of an AKM-style model may overstate the importance of establishment effects. However, offer shopping cannot explain the patterns of wage losses experienced by workers who move to lower-wage establishments.[30] Nor can it explain the symmetry in the wage gains and losses associated with transitions between high and low paying establishments exhibited in Figures Va and Vb.

A third form of endogenous mobility could arise if fluctuations in the transitory error $\varepsilon_{it}$ are associated with systematic movements between higher- and lower-wage workplaces. Suppose for example that $\varepsilon_{it}$ contains an industry by year component and that workers tend to cycle between jobs at higher-wage employers that are relatively sensitive to industry conditions, and jobs at low-wage employers that are more stable. In this scenario, workers who have recently experienced a positive (negative) transitory wage shock will be more likely to move to higher (lower) wage establishments, leading to an attenuation in the estimated employment effects. As noted in the discussion of Figures Va and Vb, however, there is little evidence that mobility patterns are related to transitory wage fluctuations, suggesting that any correlation between mobility patterns and the $\varepsilon_{it}$'s is small.

[29] Note that instantaneous learning – in which workers are suddenly revealed to be more or less productive and make a job transition – could generate spurious establishment effects without detectable blips or dips in wages prior to a job transition (Gibbons and Katz [1992]). While bias from such a process would be difficult to detect, empirical estimates suggest that employer learning occurs over a horizon of several years (Lange [2007]).

[30] Wage losses are possible in such models but should not easily be predicted by the average coworker wage at the origin and destination establishment.

### Variance Decompositions

Using equation (1), the variance of observed wages for workers in a given sample interval can be decomposed as:

$$\begin{aligned} Var(y_{it}) &= Var(\alpha_i) + Var\left(\psi_{\boldsymbol{J}(i,t)}\right) + Var(x'_{it}\beta) \\ &\quad +2Cov\left(\alpha_i, \psi_{\boldsymbol{J}(i,t)}\right) + 2Cov\left(\psi_{\boldsymbol{J}(i,t)}, x'_{it}\beta\right) + 2Cov(\alpha_i, x'_{it}\beta) + Var(r_{it}). \quad (6) \end{aligned}$$

In our analysis below we use a feasible version of this decomposition which replaces each term with its corresponding sample analogue.[31]

As discussed in the Online Appendix, sampling errors in the estimated person and establishment fixed effects will lead to positive biases in our estimates of $Var(\alpha_i)$ and $Var\left(\psi_{\boldsymbol{J}(i,t)}\right)$. In addition, correlation between the sampling errors of the person and establishment effects is likely to induce a negative bias in the estimated covariance between the person and establishment effects (Andrews et al. [2008], Mare and Hyslop [2006]). We do not attempt to construct bias-corrected estimates of $Var(\alpha_i)$, $Var(\psi_{\boldsymbol{J}(i,t)})$, or $Cov\left(\alpha_i, \psi_{\boldsymbol{J}(i,t)}\right)$.[32] Instead, we analyze trends in the estimated moments under the assumption that the biases are similar in earlier and later sample intervals.

# VI Results

We estimate model (1) using data from the four overlapping sample intervals: 1985-1991, 1990-1996, 1996-2002, and 2002-2009. Columns 1-4 of Table II show, for each interval, the number of person-year observations for full time male workers, the number of individuals,

[31] For instance, $Var(y_{it})$ is estimated by $S_y \equiv \frac{1}{N^*-1}\sum_{i,t}(y_{it} - \overline{y})^2$ where $\overline{y} \equiv \frac{1}{N^*}\sum_{i,t} y_{it}$. Likewise, $Cov(\alpha_i, \psi_{\boldsymbol{J}(i,t)})$ is estimated by $S_{\widehat{\alpha},\widehat{\psi}_{\boldsymbol{J}(i,t)}} \equiv \frac{1}{N^*-1}\sum_{i,t}(\widehat{\alpha}_i - \overline{\widehat{\alpha}})\widehat{\psi}_{\boldsymbol{J}(i,t)}$ where $\widehat{\alpha}_i$ and $\widehat{\psi}_{\boldsymbol{J}(i,t)}$ refer to estimated person and establishment effects respectively and $\overline{\widehat{\alpha}} \equiv \frac{1}{N^*}\sum_{i,t}\widehat{\alpha}_{it}$.

[32] We construct unbiased estimates of the standard deviation of $r_{it}$ using the root mean squared error of the regression, which adjusts for the number of parameters estimated in the regression model.

and the mean and standard deviation of log wages. In each interval we have 85-90 million person-year observations on wages for about 17 million individual workers. As expected from the patterns in Table I, the standard deviation of wages rises substantially from 0.38 in interval 1 (1985-1991) to 0.51 in interval 4 (2002-2009). Mean log wages rise about 5 percent from interval 1 to interval 2 and then are quite stable.

Columns 5-8 present a parallel set of statistics for the largest connected set of workers in each interval. Mobility rates between establishments are sufficiently high in West Germany that, in each interval, 97% of person-year observations and approximately 95% of all workers are included in the connected set. Mean wages for observations in the connected set are slightly higher than in the overall population of full-time male workers, while the dispersion of wages is slightly lower. Neither the relative size of the connected group, nor the relative mean/standard deviation of wages in that group change across the four intervals, suggesting that there is little or no loss in focusing attention on the largest connected group for the remainder of the paper.

Table III summarizes the estimation results for full time male workers in each of the four intervals in our analysis. As shown in the top two rows of the Table, our models include 16-17 million person and 1.2-1.5 million establishment effects in each interval. To summarize our findings, we report the standard deviations of the estimated person and establishment effects, the standard deviation of the time-varying covariate index, and the correlations between these components. We also report the root mean squared error (RMSE) from the model and the adjusted $R^2$ statistic, both of which take account of the large number of parameters being estimated in our models.

The results in Table III point to several interesting conclusions. First, the person effects and the establishment effects both become more variable over time. The correlation between the person and establishment effects also rises substantially, from 0.03 in period 1 to 0.25 in period 4. Relative to these two main components, the covariate index $x'_{it}\widehat{\beta}$ exhibits less dispersion, especially in the three later intervals, when aggregate wage growth was negligible.

A second observation is that the residual standard deviation of wages (measured by the RMSE) is relatively small and rises only slightly over time. The high explanatory power of the AKM model is reflected in the adjusted $R^2$ statistics, which increase from 90% to 93% across the intervals.

Table III also shows the RMSE's and adjusted $R^2$ statistics from models with unrestricted match effects (i.e., separate dummies for each person-establishment combination). These models fit somewhat better than the baseline AKM models, confirming the presence of a match component in wages. However, the estimated standard deviation of the match effects rises only slightly over time, from 0.060 to 0.075. This small change is consistent with our interpretation of the match effects as uncorrelated random effects. If instead they were specification errors caused by incorrectly imposing additivity of the person and establishment effects, we would expect the relative fit of the AKM model to deteriorate over time as the variances of the person and establishment effects increase in magnitude.

Additional insight into the nature of the match-specific error components comes from examining the errors for different groups of workers at different establishments. Violations of the separability assumptions in the AKM model might be expected to cause relatively large mean residuals for particular types of matches – say, cases where highly skilled workers are employed at low-wage establishments. To search for such neglected interactions, we divided the estimated person and establishment effects in each interval into deciles, and compute the mean residual in each of the 100 person $\times$ establishment decile cells. Figure VI shows the mean residuals in each cell using data from interval 4. Reassuringly, the mean residuals in each cell are small, and uniformly less than 1% in magnitude.[33] The largest deviations appear among the lowest-decile workers and the lowest-decile establishments: for these groups there appear to be small but systematic departures from the additive separability assumptions of the AKM model. A complete investigation of these nonseparabilities is clearly a topic for future research, but given the small magnitude of the deviations we suspect that they have

[33] We emphasize that there is no mechanical reason for the mean residuals in each cell to be close to zero. Although there are 20 linear restrictions on the 100 cell means, there are 80 remaining degrees of freedom.

relatively little effect on our basic conclusions.

A related diagnostic focuses on the ability of the model to capture wage dynamics associated with job changes. Figure VII presents an event-study analysis for job transitions in the 2002-2009 period, similar to the event study in Figure Vb but classifying origin and destination workplaces by the quartile of their estimated establishment effects. As in Figures Va and Vb, there is little evidence of transitory wage shocks in the year just before (or just after) a job change. The average wage gain for those who move from a quartile 1 to a quartile 4 establishment is also very similar to the average wage loss for those who move in the opposite direction, confirming the symmetry prediction from our model.

The wage changes in Figure VII for people who move between quartile groups are relatively large, reflecting the relatively large dispersion in the estimated establishment effects. In contrast, people who switch jobs but stay within the same quartile group have small average wage changes. The absence of a general mobility premium for these workers suggests that job mobility is not driven by idiosyncratic job-match effects. We have also examined the mean wage *residuals* for transitions between the various origin and destination cells. We find relatively small mean residuals (under 3% in absolute value) in every transition cell. We take this as evidence that, at a minimum, our approach provides a good first approximation to the wage determination process, consistent with the relatively high adjusted $R^2$ statistics for the model.

## Decomposing changes in the structure of wages

We now turn to the implications of the estimated models in Table III for understanding the rise in wage inequality over time. As noted, the estimated person and establishment effects both exhibit increasing dispersion over time. Perhaps even more striking is the rise in the correlation between these effects. This increase suggests a fundamental change in the way workers are sorted to workplaces.[34] The increase in assortative matching is illustrated in

[34]It is important to remember that these components only provide a description of the covariance structure of *wages*. As pointed out by Lopes de Melo [2008], Lentz and Mortensen [2010], and Eeckhout and Kircher

Figures VIIIa and VIIIb, which show plots of the joint distributions of the estimated person and establishment effects in intervals 1 and 4, using the same decile categories as in Figure VI. The joint distribution for our earliest sample interval (Figure VIIIa) shows little evidence of assortative matching. In contrast, the joint distribution for our last interval (Figure VIIIb) shows a clear tendency for higher wage workers to sort to establishments offering larger wage premiums.

To quantify the separate contributions of rising dispersion in person and establishment effects, and increases in assortative matching, we conduct a simple variance decomposition based on equation (6) in each interval. Table IV summarizes the results of this decomposition. Between intervals 1 and 4, the variance of the person effects rose from 0.084 to 0.127, representing about 40% of the overall increase in the variance of wages, while the variance of the establishment effects rose from 0.025 to 0.053, contributing another 25%. The covariance term also rose from 0.003 to 0.041, adding about 34% of the total rise in wage variance.

Table IV also reports three simple counterfactual scenarios that help to illustrate the relative importance of the various terms. Under the first counterfactual we hold constant the correlation of worker and firm effects (i.e., no change in sorting) but allow the variances of the person and establishment effects to rise. Under this scenario, the variance of wages would have risen by 0.077, or about 70% of the actual rise, suggesting that the increase in sorting can account for about 30% of the rise in variance. In the second counterfactual we hold constant the variance of establishment effects but allow the variance of the person effects and the correlation between the person and establishment effects to rise. Under this scenario the variance of wages rises by 0.072, suggesting that the rise in dispersion of establishment effects accounts for about a third of the rise in the variance of wages. Finally, in the third scenario we hold constant sorting and the rise in the variance of the establishment effects, leading to a counterfactual rise in the variance of wages of 0.047 (about 40% of the total actual increase) attributable to the rise in the dispersion of the person effects.

[2011], the correlation between worker and establishment wage effects need not correspond to the correlation between worker and establishment productivity.

## Robustness Check: Men with Apprenticeship Training Only

As discussed earlier, a concern with the German Social Security data is censoring, which affects 10-14 percent of men in any year of our sample. Censoring is particularly prevalent among older, university-educated men, up to 60% of whom have earnings above the maximum rate. To address this concern, we re-estimate our main models using only data for men whose highest educational qualification is an apprenticeship. This relatively homogeneous group represents about 60% of our overall sample and has a censoring rate of about 9% per year. Over the 1985-2009 sample period wage inequality for apprentice-trained men rose substantially, though not as much as over the labor force as a whole, reflecting a widening of education-related wage gaps (see below). Specifically, between interval 1 and interval 4 the standard deviation of log wages for apprentice-trained men rose from 0.328 to 0.388 – an 18% rise – versus the 35% increase for all full time men.

Online Appendix Tables A.4 and A.5 summarize the estimation results for this subsample, using the same format as Tables III and IV. In brief, the results are qualitatively very similar to the results for the entire sample. Specifically, the rise in wage inequality is attributed to a rise in the dispersion of the person-specific component of pay, a rise in the dispersion of the establishment-level component, and a rise in their covariance. We infer that our main conclusions are robust to our procedure for handling censoring in the Social Security earnings data.

## Results for Full Time Females

In this section we briefly summarize our findings for full-time female workers. As noted in Figure III, the standard deviation of wages for full time female workers follows a very similar trend to the standard deviation for men, with a modest rise from 1985 to 1995, followed by a more rapid upward trend after 1996. Trends in residual wage inequality are also very similar for full time female workers and for full time men. In the Online Appendix, we show trends in the residual standard deviations of wage models for women that introduce controls for education and experience, industry effects, occupation effects, and a complete set of establishment dummies (see Online Appendix Figure A.2). As is the case for men, industry and occupation controls explain an important share of wage variation among full time women, but do not explain much of the rise in inequality since the 1990s. Models with establishment effects explain a much larger share of the rise, suggesting that wage differentials between establishments have risen substantially for women.

To explore the role of workplace specific wage premiums more formally, we fit a series of AKM-style models for full time women similar to the models in Table III. We show in the Online Appendix (Appendix Tables A.6 and A.7) that the rise in inequality of female wages in West Germany is attributable to a combination of widening dispersion in the person-component of wages (about 50% of the overall rise), widening dispersion in the wage premiums at different workplaces (about 25% of the rise), and a rise in the assortative matching of workers to plants (about 20% of the rise). While these results are qualitatively very similar to our findings for men, they suggest that the rise in matching assortativeness explains a smaller share of the rise in the variance of wages for women than men (20% versus 34%).

We suspect that some of the difference between men and women in the measured role of assortative matching may be due to decreased precision in our estimates of the worker and plant effects in the female sample. In particular, there is likely to be a larger negative bias in the estimated covariance between the worker and plant components in samples with fewer observed job matches per establishment or per worker (see Mare and Hyslop [2006]). In our 2002-2009 sample interval, the largest connected set of male workers and plants has 17.4 job matches per plant and 1.65 matches per worker. The largest connected set of female workers and plants, by comparison, has only 11.9 job matches per plant and 1.48 matches per worker, suggesting that the covariance between the worker and establishment components may be more negatively biased for women.

# VII Decomposing Between-Group Wage Differentials

Germany, like many other countries, has experienced substantial increases in the wage gaps between groups of workers with different skill characteristics. The model in (1) allows a simple decomposition of between-group wage gaps into a component attributable to the average permanent skill characteristics of workers, a component attributable to the average workplace premium of the establishments at which they work, and a component reflecting the mean values of the time-varying observables. Consider a discretely valued time invariant worker characteristic $G_i$. From (1) and (4), the mean wage for workers in group $g$ can be written:

$$E_g\left[y_{it}\right] = E_g\left[\alpha_i\right] + E_g\left[\psi_{\boldsymbol{J}(i,t)}\right] + E_g\left[x'_{it}\beta\right], \tag{7}$$

where $E_g\left[.\right] \equiv E\left[.|G_i = g\right]$ denotes the expectation in group $g$. Using this result, the change in the mean wage differential between any two groups $g_1$ and $g_2$ can be decomposed into the sum of a relative change in the mean of the person effects in the two groups, a relative change in the mean of the establishment effects, and a relative change in the mean of the time-varying characteristics. The establishment component is particularly interesting in light of the evidence presented so far of increased assortativeness in the matching of workers to workplaces, which may differentially effect different education, industry and occupation groups.

## Education

Table V presents a decomposition of changes in the mean wages of different education groups in West Germany between the first and fourth intervals relative to men with apprenticeship training.[35] Column 1 of the table shows the change in the relative wages of each group: note that wages of the less educated groups have fallen relative to the base group, while the wages of the more educated groups have risen. Columns 2 and 3 show the relative changes in

[35] For this analysis, we assign each worker a constant level of education in each time interval, based on the modal value of observed education.

mean person and establishment effects (again, relative to apprentice-trained workers), while column 4 shows the remaining component. A striking conclusion from this decomposition is that 70% of the relative rise in wages of university-educated men, and 80% of the relative fall in the wages of workers with no (or missing) qualifications, is attributable to changes in their relative sorting to establishments that pay higher or lower wage premiums to all workers. Put differently, the increasing returns to different levels of education in Germany are driven primarily by changes in the quality of the jobs different education groups can obtain, rather than by changes in the value of skills that are fully portable across jobs.

## Occupation

Autor, Levy, and Murnane's [2003] seminal study of technological change and task prices has led to renewed interest in the study of the occupational wage structure.[36] Our model provides a new perspective on this issue. Specifically, equation (7) implies that the between-occupation variance in mean wages can be decomposed as:

$$\begin{aligned} Var\left(E_g\left[y_{it}\right]\right) &= Var\left(E_g\left[\alpha_i\right]\right)+Var\left(E_g\left[\psi_{\boldsymbol{J}(i,t)}\right]\right)+Var\left(E_g\left[x_{it}'\beta\right]\right) \\ &\quad +2Cov\left(E_g\left[\alpha_i\right],E_g\left[\psi_{\boldsymbol{J}(i,t)}\right]\right)+2Cov\left(E_g\left[\alpha_i\right],E_g\left[x_{it}'\beta\right]\right) \\ &\quad +2Cov\left(E_g\left[\psi_{\boldsymbol{J}(i,t)}\right],E_g\left[x_{it}'\beta\right]\right), \end{aligned} \tag{8}$$

where $E_g\left[.\right]$ denotes the expected value in occupation group $g$. Evaluating this equation in different time intervals using sample analogues, we can decompose changes in the variation in wages across occupations into components due to rising dispersion in the mean person effect between occupations, rising dispersion in the mean establishment wage premium earned by workers in different occupations, and changes in the covariance of the mean person effect and the mean establishment wage premium earned by workers in different occupations.[37]

[36] See Acemoglu and Autor [2011] for a review of the related literature.

[37] Because the variance components are calculated using occupational averages of the worker and establishment effects, they do not suffer from the sampling error-induced biases that affect the variances and covariances at the individual level discussed in Section IV and in the Online Appendix.

Panel A of Table VI reports the three main components of equation (8) evaluated in each of our four sample intervals, as well as the changes in the variance components between interval 1 and interval 4 and the shares of the overall change in between-occupation variance accounted for by each component.[38] The estimates suggest that the largest share of the rise in between-occupation inequality (42%) is attributable to a rise in the covariance between the mean person effect in an occupation and the mean establishment wage premium for that occupation. In other words, people in higher-paid occupations are increasingly concentrated at establishments that pay all workers a higher wage premium, while those in lower-paid occupations are increasingly concentrated at low-wage establishments. Another 28% is attributable to the variance in the wage premiums at different workplaces. Only about 30% of the rise in wage differentials between occupations is due to increasing variation in the permanent person-specific component of wages. We conclude that rising workplace heterogeneity and sorting are very important for understanding the rise in occupation-related wage differentials among West Germany men.

## Industry

The bottom panel of Table VI provides a parallel decomposition of the dispersion in mean wages across industries.[39] A large literature (e.g., Krueger and Summers [1988], Katz and Summers [1989]) has examined the between-industry structure of pay differences. A still-unresolved issue is the extent to which industry-specific wage premia are attributable to unobserved differences in worker quality (Murphy and Topel [1990], Gibbons and Katz [1992], Goux and Maurin [1999], Gibbons et al. [2005]) versus firm-specific pay policies like efficiency wages. Our additive effects framework suggests that both components are important. In the 1985-91 interval, for example, variation in mean worker effects explains about 35% of

[38] Since workers can change occupation within one of our sample intervals, occupation is not a fixed worker characteristic and the decomposition in equation (7) is not exact. In our samples, however, the residual component is very small.

[39] The IEB has a changing set of industry codes. We develop a crosswalk by comparing the industry codes assigned to the same establishment in adjacent years under the different coding systems. To simplify the crosswalk we use a 2-digit level of classification, with 96 categories.

between-industry wage variation, variation in establishment effects explains a similar share, and their covariance adds another 20%.

Over the 1985-2009 period, the inequality in average wages across industries has risen substantially: the standard deviation in mean wages, for example, rose by 30% from interval 1 to interval 4. The entries in column 5 of the table suggest that rising dispersion in worker quality explains a sizeable share (about 44%) of this rise,while rising dispersion in establishment-specific pay premiums contributes another 19%. As with the between-occupation wage structure, however, a relatively large share (42%) is due to the increasing sorting of high-wage workers to industries that pay a higher average wage premium to all workers.

# VIII Rising Establishment Heterogeneity: Cohort Effects and Collective Bargaining Status

The increasing dispersion in establishment-level pay premiums between the first and last intervals of our sample raises the question of how this increase occurred. Was it generated by a divergence in the pay premiums offered by continuing establishments, by a change in the policies of newly created establishments, or by a combination of the two? To address this question, we calculated the standard deviations of the estimated establishment effects in each of our four sample intervals by birth year of the establishment. The results are plotted in Figure IX. Note that we have up to four estimates of the dispersion of establishment effects for a given "birth" cohort, depending on when the birth occurred. For establishments that first appear in 1987, for example, we have one estimate based on 1985-91 data, and three others based on data from the three later intervals. For establishments born in the final years of our sample we have only one estimate, based on data from our fourth interval (2002-2009).

The pattern of the estimated standard deviations in Figure IX suggests that there is a lifecycle pattern in the measured heterogeneity of firms. The distribution of establishment effects is relatively wide for new establishments but tends to narrow and then stabilize over time.[40] Among continuing establishments there is not much rise in the dispersion of firm effects between interval 1 and interval 4. For example, among the large set of establishments that are present in 1985, the standard deviation of estimated effects only rises from 0.15 in interval 1 to 0.17 in interval 4. In contrast, the standard deviation in estimated effects for all workplaces rises from 0.16 to 0.23 (see Table IV). We conclude that much of the rising heterogeneity in establishments is attributable to new establishments, particularly those that emerge after 1996. Adjusting for lifecycle effects, establishments born in the late 2000s have about 25% higher standard deviations in their establishment effects than those born before 1996.

One potential explanation for the increasing dispersion of the wage premiums at new establishments is a rise in the fraction of plants that have opted out of the traditional collective bargaining system and pay relatively low wages. To investigate this explanation we merged collective bargaining status information from the IAB's Linked Employer Employee database (LIAB) to establishments in our analysis sample.[41] Overall, the mean establishment effect for plants with no collective bargaining is about 8-10 percentage points lower than the mean for plants with either form of collective bargaining.[42] Moreover, as shown in Figure X, the dispersion in estimated effects is higher for uncovered establishments, with a substantial fraction of these plants paying wage premiums in the lowest two deciles of overall effects.[43]

As shown by the top line in Figure IX, there is a strong cohort effect in the fraction of new

[40] The earliest estimate of the dispersion in establishment effects is particularly wide for plants born in the last years of an interval. We suspect that this is mainly due to the higher sampling errors for the estimated effects of establishments that are only observed in a few years.

[41] See Alda, Bender, and Gartner [2005] for a description of the LIAB data base. The LIAB has about 13,000-14,000 establishments in the years after 2000, and a smaller sample in earlier years.

[42] In a regression of the interval-3 establishment effect on dummies for a sectoral or plant-specific agreement measured in the year 2000 LIAB, the coefficients are 0.10 (standard error=0.01) for a sectoral agreement and 0.10 (standard error=0.01) for a plant-specific agreement. The coefficients are 0.08 and 0.09 respectively when dummies are added for the birth cohort of the establishment.

[43] We assigned decile cutoffs for the establishment effects so that 10 percent of person-year observations in our third sample interval (1996-2002) fall into each decile. Establishments in the LIAB are relatively large and on average have more positive establishment effects than the population as a whole.

plants that are covered by collective bargaining.[44] Among establishments that enter between 1986 and 1996, 50-55% are covered by collective bargains. Among those entering after 2007, by comparison, fewer than 30% are covered. Interestingly, the trend in coverage seems to exhibit a turning point at about the same time (circa 1996) as the trend in the standard deviation of estimated establishment effects, suggesting that the trend toward increasing heterogeneity across newer plants may be linked to the relative fall in collective bargaining coverage. Of course, it is difficult to assign a causal role to collective bargaining, since firms in Germany can choose whether to adopt some form of collective bargaining or not. At a minimum, however, the evidence in Figures IX and X suggests a proximate role for declining collective bargaining coverage among establishments that have begun operation since the mid-1990s in the rise in establishment-level heterogeneity.

# IX Conclusion

West Germany has experienced substantial increases in wage inequality over the past 25 years. Observers have been divided over the rise is primarily attributable to supply and demand factors (including trade and technology) or to changes in labor market institutions (Dustmann, Ludsteck, and Schönberg [2009], Eichhorst [2012]). Our analysis approaches this question from a different perspective, asking how much of the rise in inequality is due to rising variation in the the component of individual pay that is fully portable across jobs, and how much is due to a rise in the variation in pay premiums offered by different employers. We find that inequality has widened in both dimensions. Moreover, people who would tend to earn more at any job are increasingly concentrated at establishments that offer above-average wages for all employees, magnifying the effects on overall inequality. In contrast, we find that the dispersion in the worker-specific job match component of wages is relatively small and stable over time.

[44] To estimate coverage rates by birth cohort, we first estimate the coverage rate by birth year for each year of the LIAB from 1999-2008. We then take the average coverage rate across years for each birth cohort.

A key question is whether similar trends have occurred in other developed economies – particularly those with significant increases in wage inequality – or will occur in the future. Existing work by Barth et al. [2011] suggests that workplace heterogeneity may in fact be an important part of recent rises in wage inequality in the US. It will be of interest to see whether recent reforms throughout Western Europe lead to the same kinds of trends witnessed in West Germany since the mid-1990s.

Another unresolved issue is the source of rising workplace heterogeneity. Descriptively, we find that that the distribution of establishment wage premiums varies substantially by firm cohort, with newer firms exhibiting greater dispersion. This could reflect differences in technology choices or management practices of younger versus older firms (Bloom and Van Reenen [2007]), institutional constraints on the pay practices of older firms, or a variety of other factors. In Card, Heining, and Kline [2012] we show that establishments offering higher wage premiums have higher survival rates, suggesting that the wage premiums are related to firm profitability. Productivity various enormously across firms and plants (e.g., Bernard et al. [2003], Foster, Haltiwanger, and Syverson [2008], Hsieh and Klenow [2009]), and some share of these differences may be captured by workers through rent-sharing. To explain a rise in workplace heterogeneity, however, requires either a widening of productivity differences over time or a rise in dispersion of the share of the rent that workers capture at different firms. Developing and testing new models of wage determination that acknowledge the importance of workplace-specific wage premiums is a high priority for future research.

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Table I:  Summary Statistics for IAB Samples of Full Time Men and Women

| | Number Observations (1) | Log Real Wage, Unallocated: Mean (2) | Log Real Wage, Unallocated: Std. Dev (3) | Percent Censored (4) | Log Real Wage, Allocated: Mean (5) | Log Real Wage, Allocated: Std. Dev (6) |
|---|---|---|---|---|---|---|
| *a.  Full Time Men* | | | | | | |
| 1985 | 11,980,159 | 4.221 | 0.387 | 10.63 | 4.247 | 0.429 |
| 1990 | 13,289,988 | 4.312 | 0.398 | 11.92 | 4.342 | 0.445 |
| 1995 | 13,101,809 | 4.340 | 0.415 | 9.78 | 4.361 | 0.447 |
| 2000 | 12,930,046 | 4.327 | 0.464 | 10.31 | 4.352 | 0.502 |
| 2005 | 11,857,526 | 4.310 | 0.519 | 9.36 | 4.336 | 0.562 |
| 2009 | 12,104,223 | 4.277 | 0.535 | 10.00 | 4.308 | 0.586 |
| *b.  Full Time Women* | | | | | | |
| 1985 | 6,068,863 | 3.836 | 0.462 | 1.52 | 3.840 | 0.470 |
| 1990 | 7,051,617 | 3.942 | 0.476 | 2.01 | 3.947 | 0.486 |
| 1995 | 7,030,596 | 4.026 | 0.483 | 1.95 | 4.030 | 0.491 |
| 2000 | 7,009,075 | 4.019 | 0.532 | 2.47 | 4.026 | 0.545 |
| 2005 | 6,343,006 | 3.999 | 0.573 | 2.36 | 4.006 | 0.588 |
| 2009 | 6,566,429 | 3.979 | 0.587 | 2.80 | 3.988 | 0.606 |

Notes: samples includes employees in West Germany age 20-60, working full time in non-marginal jobs. Real wage is based on average daily earnings at the full time job with highest total earnings during the calendar year, adjusted for inflation using the Consumer Price Index.  Unallocated wage data in columns 2 and 3 are based on raw daily wage data, which are censored at Social Security maximum for the corresponding year.  Allocated wage data in columns 5 and 6 include stochastic allocation of censored observations based on Tobit model.  See text.

Table II: Summary Statistics for Overall Sample and Individuals in Largest Connected Set

| | All full time men, age 20-60 | | | | Individuals in Largest Connected Set | | | |
|---|---|---|---|---|---|---|---|---|
| | Number | Number | Log Real Daily Wage | | Number | Number | Log Real Daily Wage | |
| Interval | person/yr. obs. | Individuals | Mean | Std. Dev. | person/yr. obs. | Individuals | Mean | Std. Dev. |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| 1985-1991 | 86,230,097 | 17,021,779 | 4.344 | 0.379 | 84,185,730 | 16,295,106 | 4.351 | 0.370 |
| *ratio: largest connected/all* | | | | | *97.6* | *95.7* | *100.2* | *97.7* |
| 1990-1996 | 90,742,309 | 17,885,361 | 4.391 | 0.392 | 88,662,398 | 17,223,290 | 4.398 | 0.384 |
| *ratio: largest connected/all* | | | | | *97.7* | *96.3* | *100.2* | *97.9* |
| 1996-2002 | 85,853,626 | 17,094,254 | 4.397 | 0.439 | 83,699,582 | 16,384,815 | 4.405 | 0.432 |
| *ratio: largest connected/all* | | | | | *97.5* | *95.8* | *100.2* | *98.3* |
| 2002-2009 | 93,037,963 | 16,553,835 | 4.387 | 0.505 | 90,615,841 | 15,834,602 | 4.397 | 0.499 |
| *ratio: largest connected/all* | | | | | *97.4* | *95.7* | *100.2* | *98.8* |
| Change from first to last interval | | | 0.043 | 0.126 | | | 0.045 | 0.128 |

Notes: Sample consists of full-time male workers ages 20-60 employed in non-marginal jobs and not currently in training. Daily wage is imputed for censored observations using Tobit model. "Connected set" refers to group of firms connected by worker mobility over the sample interval (for details, see Abowd, Creecy, and Kramarz, 2002).

Table III: Estimation Results for AKM Model, Fit by Interval

| | Interval 1<br>1985-1991<br>(1) | Interval 2<br>1990-1996<br>(2) | Interval 3<br>1996-2002<br>(3) | Interval 4<br>2002-2009<br>(4) |
|---|---|---|---|---|
| *Person and Establishment Parameters:* | | | | |
| Number person effects | 16,295,106 | 17,223,290 | 16,384,815 | 15,834,602 |
| Number establishment effects | 1,221,098 | 1,357,824 | 1,476,705 | 1,504,095 |
| *Summary of Parameter Estimates:* | | | | |
| Std. dev. of person effects (across person-year obs.) | 0.289 | 0.304 | 0.327 | 0.357 |
| Std. dev. of establ. effects (across person-year obs.) | 0.159 | 0.172 | 0.194 | 0.230 |
| Std. dev. of Xb (across person-year obs.) | 0.121 | 0.088 | 0.093 | 0.084 |
| Correlation of person/establ. effects (across person-year obs.) | 0.034 | 0.097 | 0.169 | 0.249 |
| Correlation of person effects/Xb (across person-year obs.) | -0.051 | -0.102 | -0.063 | 0.029 |
| Correlation of establ. effects/Xb (across person-year obs.) | 0.057 | 0.039 | 0.050 | 0.112 |
| RMSE of AKM residual | 0.119 | 0.121 | 0.130 | 0.135 |
| Adjusted R-squared | 0.896 | 0.901 | 0.909 | 0.927 |
| *Comparison Match Model* | | | | |
| RMSE of Match model | 0.103 | 0.105 | 0.108 | 0.112 |
| Adjusted R-squared | 0.922 | 0.925 | 0.937 | 0.949 |
| Std. Dev. of Match Effect* | 0.060 | 0.060 | 0.072 | 0.075 |
| *Addendum* | | | | |
| Std. Dev. Log Wages | 0.370 | 0.384 | 0.432 | 0.499 |
| Sample size | 84,185,730 | 88,662,398 | 83,699,582 | 90,615,841 |

Notes: Results from OLS estimation of equation (1) in text. See notes to Table II for sample composition. Xb includes year dummies interacted with education dummies, and quadratic and cubic terms in age interacted with education dummies (total of 39 parameters in intervals 1-3, 44 in interval 4). Match model includes Xb and separate dummy for each job (person-establishment pair).

*Standard deviation of match effect estimated as square root of difference in mean squared errors between AKM model and match effect model.

## Table IV: Decomposition of the Rise in Wage Inequality

| | Interval 1 (1985-1991) | | Interval 4 (2002-2009) | | Change from Interval 1 to 4 | |
|---|---|---|---|---|---|---|
| | Var. Component | Share of Total | Var. Component | Share of Total | Var. Component | Share of Total |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Total variance of log wages | 0.137 | 100.0 | 0.249 | 100.0 | 0.112 | 100 |
| Components of Variance: | | | | | | |
| Variance of person effect | 0.084 | 61.3 | 0.127 | 51.2 | 0.043 | 39 |
| Variance of establ. effect | 0.025 | 18.5 | 0.053 | 21.2 | 0.027 | 25 |
| Variance of Xb | 0.015 | 10.7 | 0.007 | 2.8 | -0.008 | -7 |
| Variance of residual | 0.011 | 8.2 | 0.015 | 5.9 | 0.003 | 3 |
| 2cov(person, establ.) | 0.003 | 2.3 | 0.041 | 16.4 | 0.038 | 34 |
| 2cov(Xb, person+establ.) | -0.001 | -1.0 | 0.006 | 2.4 | 0.007 | 7 |
| ***Counterfactuals for Variance of log wages:*** * | | | | | | |
| 1. No rise in correl. of person/estab. effects | 0.137 | | 0.213 | | 0.077 | 69 |
| 2. No rise in var. of estab. effect | 0.137 | | 0.209 | | 0.072 | 64 |
| 3. Both 1 and 2 | 0.137 | | 0.184 | | 0.047 | 42 |

Notes: See notes to Table II for sample composition. Calculations based on estimated AKM models summarized in Table III. Entry in column 5 is change in variance component from interval 1 to interval 4. Entry in column 6 is ratio of the change in the variance component to the total change in variance of wages reported in first row of table (as a percentage).

*Counterfactual 1 computes the counterfactual rise in variance assuming the correlation between the person and establishment effects remains at its interval 1 value -- i.e. imposing the restriction that $Cov_4$(person, establ.) = $\rho_1$ $Var_4(\text{person})^{1/2}$ x $Var_4(\text{establ.})^{1/2}$ where the 4 subscript refers to the interval 4 value of the statistic and $\rho_1$ is the correlation between the person and establishment effects in interval 1. Counterfactual 2 assumes that the variance of establishment effects remains at its interval 1 level. Counterfactual 3 imposes both of these restrictions.

Table V: Decomposition of Changes in Relative Wages by Education Level, 1985-1991 vs 2002-2009

| | Change in Mean Log Wage Relative to Relative to Apprentices (1) | Change in Mean Person Effect (2) | Change in Mean Establishment Effect (3) | Remainder (4) |
|---|---|---|---|---|
| *Highest Education Qualification:* | | | | |
| 1. Missing/none | -14.6 | 1.8 | -12.2 | -4.2 |
| 2. Lower secondary school or less (no vocational training) | -10.5 | -0.1 | -6.3 | -4.1 |
| 4. Abitur with or without vocational training* | 10.1 | 0.0 | 2.6 | 7.5 |
| 5. University or more | 5.7 | 1.5 | 3.9 | 0.3 |

Notes: Wage changes are measured between intervals 1 (1985-1991) and 4 (2002-2009). Remainder (column 4) represents changing relative contribution of Xb component.

*Abitur refers to "Allgemeine Hochshulreife", a certificate of completion of advanced level high school.

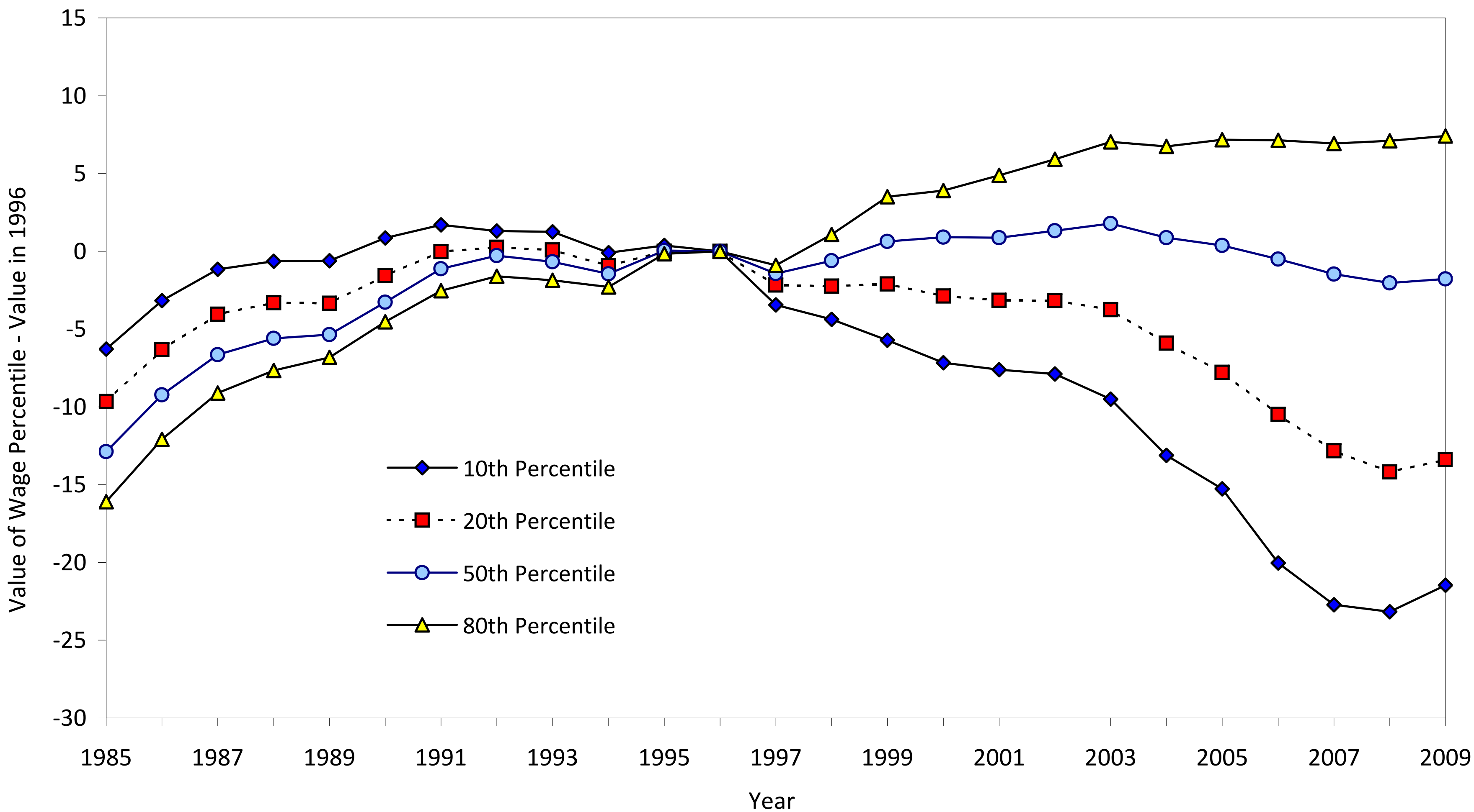
Table VI: Contribution of Person and Establishment Effects to Wage Variation Across Occupations and Industries

| | Interval 1 1985-1991 (1) | Interval 2 1990-1996 (2) | Interval 3 1996-2002 (3) | Interval 4 2002-2009 (4) | Change in Variance (Int. 1 to Int 4)* Change (5) | Share (6) |
|---|---|---|---|---|---|---|
| ***Panel A: Between Occupations (342 3-digit occupations)*** | | | | | | |
| Std. dev. of mean log wages | 0.233 | 0.243 | 0.263 | 0.289 | 0.029 | 100 |
| Std. dev. of mean person effects | 0.186 | 0.203 | 0.198 | 0.207 | 0.008 | 28 |
| Std. dev. of mean estbl. effects | 0.101 | 0.104 | 0.124 | 0.135 | 0.008 | 28 |
| Correlation of mean person effects and establ. effects | 0.110 | 0.171 | 0.238 | 0.291 | 0.012 | 42 |
| ***Panel B: Between Industries (96 2-digit industries)*** | | | | | | |
| Std. dev. of mean log wages | 0.173 | 0.184 | 0.203 | 0.224 | 0.020 | 100 |
| Std. dev. of mean person effects | 0.103 | 0.114 | 0.128 | 0.140 | 0.009 | 44 |
| Std. dev. of mean estbl. effects | 0.104 | 0.110 | 0.108 | 0.121 | 0.004 | 19 |
| Correlation of mean person effects and establ. effects | 0.242 | 0.301 | 0.422 | 0.403 | 0.008 | 42 |

Notes: decompositions based on estimated AKM models summarized in Table III. Occupation is based on main job in each year; establishments are assigned one industry per interval, using consistently-coded 2-digit industry.

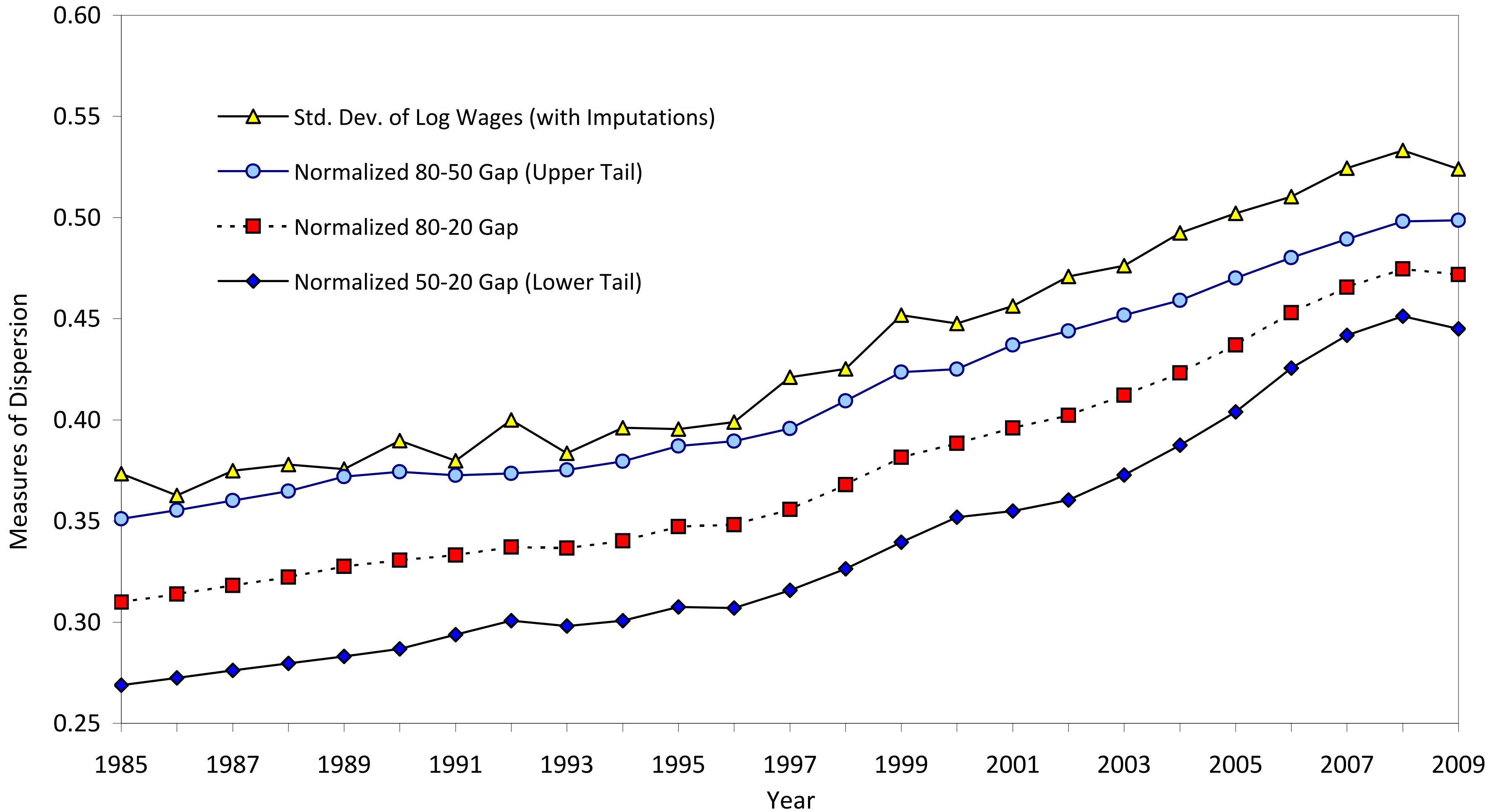
*Entry in column 5 represents change in variance or covariance component. Entry in column 6 is the share of the total change in variance explained. Shares do not add to 100% because Xb component and its covariances are omitted.

Figure I: Trends in Percentiles of Real Log Daily Wages for West German Men



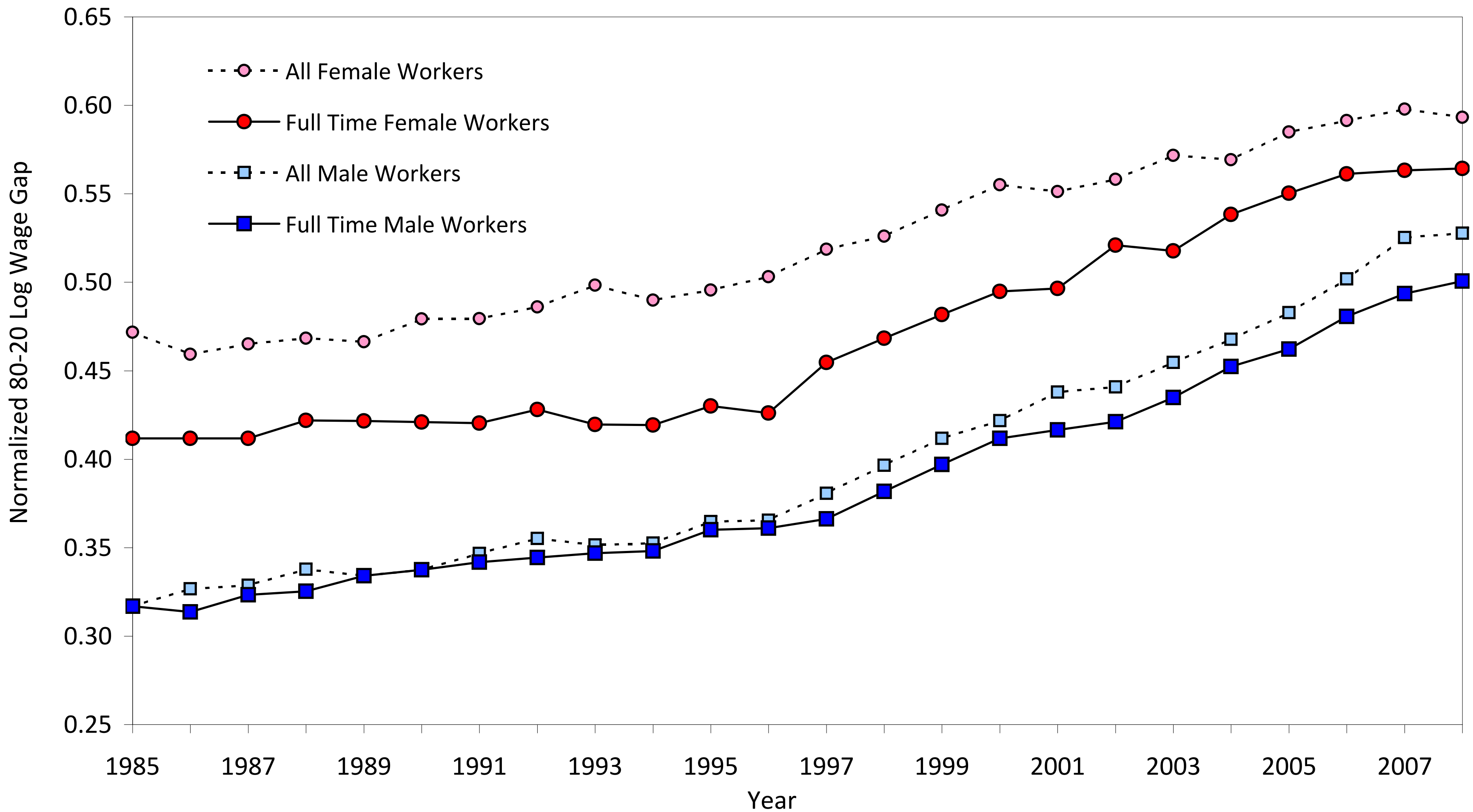

Note: figure shows percentiles of log real daily wage for full time male workers on their main job, deviated from value of same percentile in 1996 and multiplied by 100.

Figure II: Trends in Wage Inequality for Full Time Male Workers



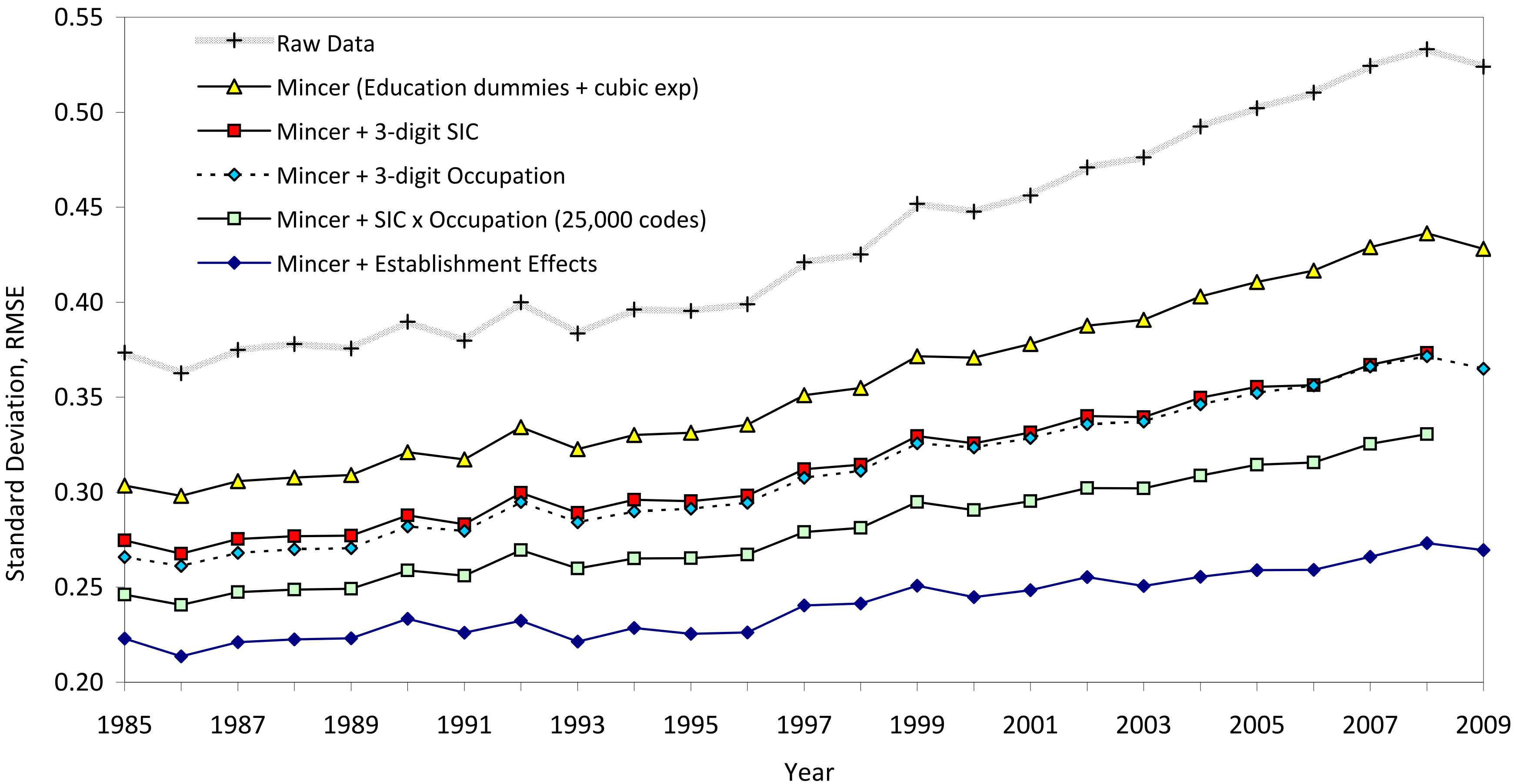

Notes: figure shows measures of dispersion in real daily wage for full time male workers. Normalized percentile gaps are differences in percentiles divided by corresponding differences in percentiles of standard normal variate.

Figure III: Wage Inequality Trends for Alternative Samples of Workers



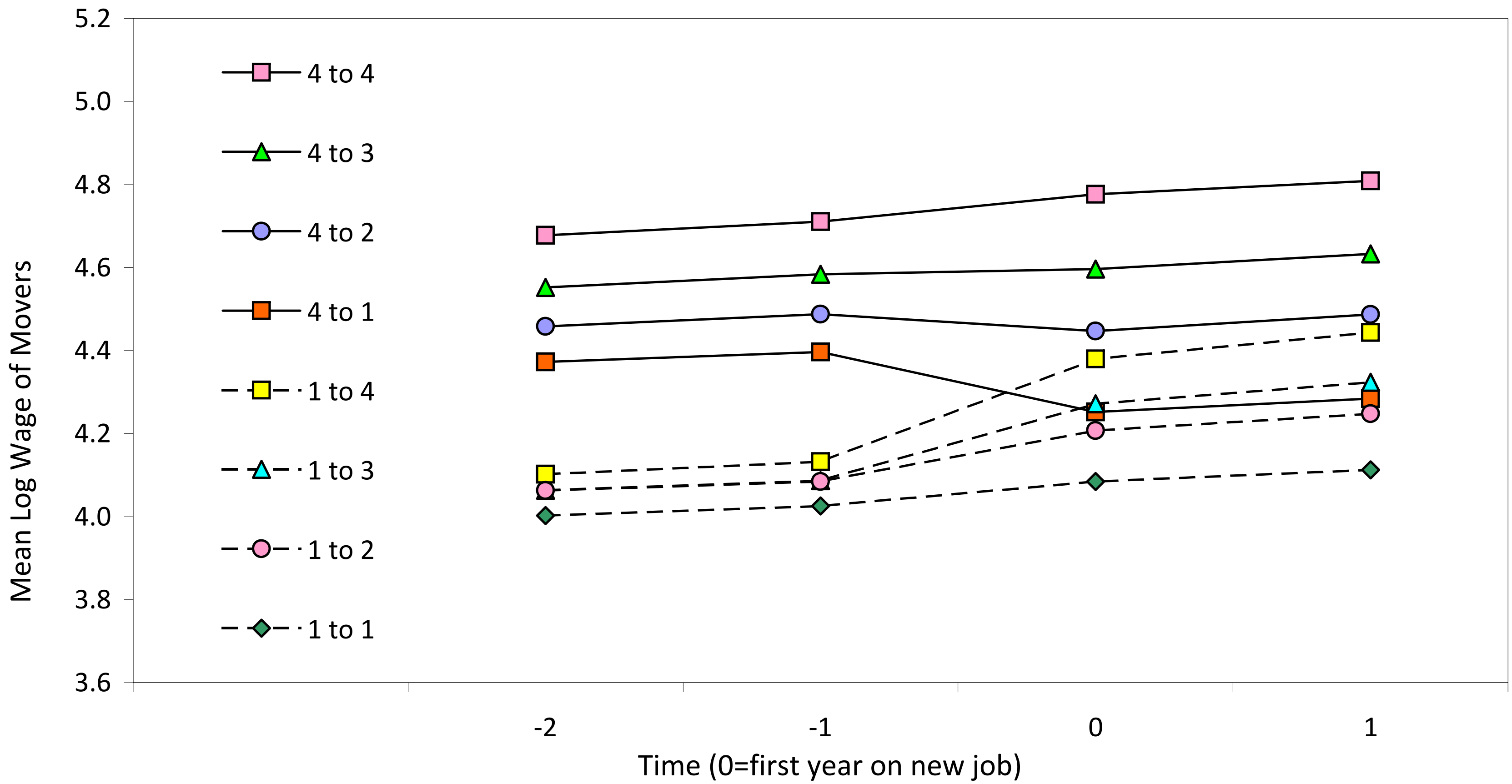

Notes: based on tabulations of SIAB . Measured wage is average daily wage in job with highest total earnings in the year. Wage gap is the difference between the 80th percentile of log real wages and the 20th percentile, divided by 80-20 gap for a standard normal variate.

Figure IV: Raw and Residual Standard Deviations from Alternative Wage Models



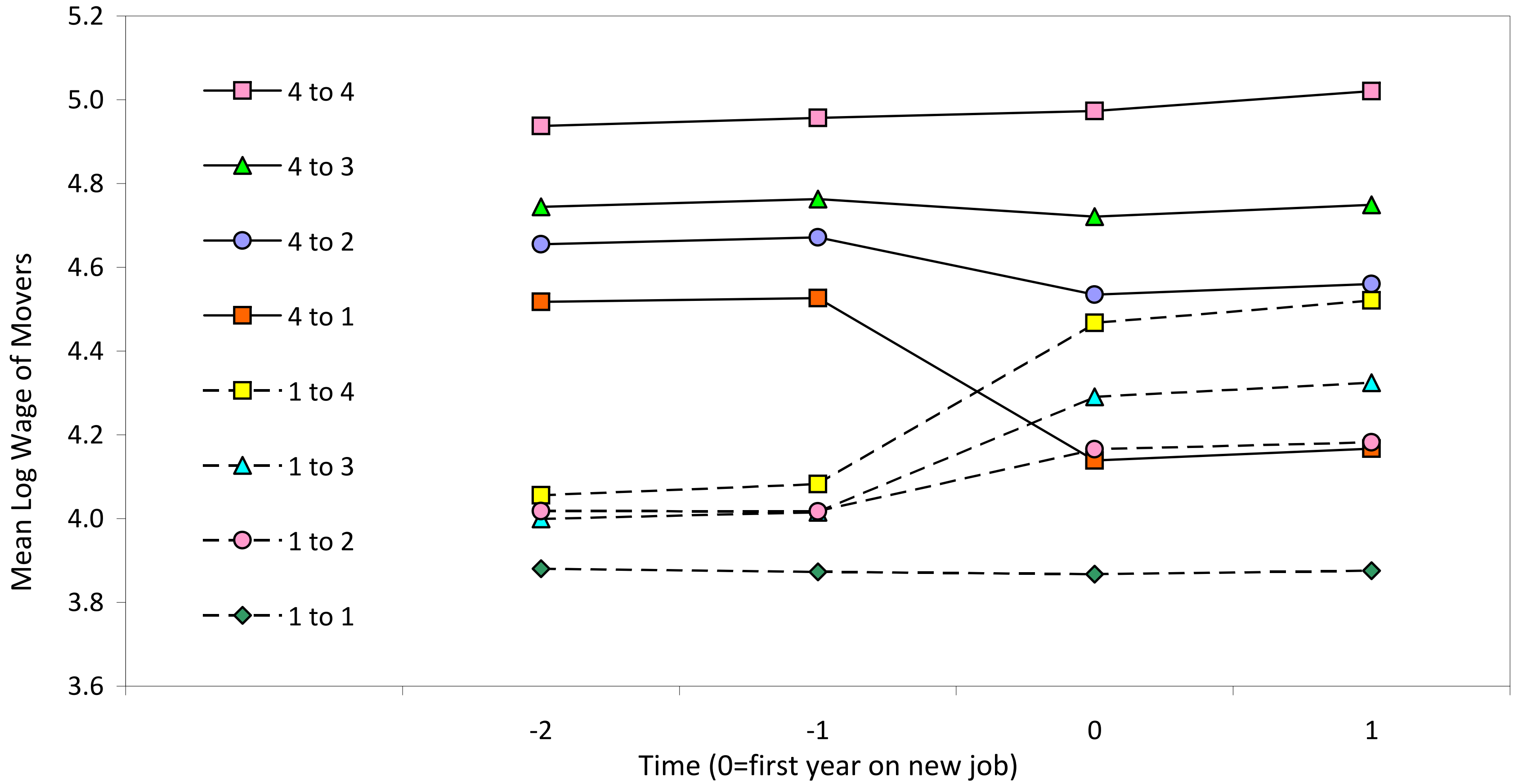

Notes: See note to Figure II. Figure shows measures of dispersion in actual and residual real daily wage for full time male workers. Residual wage is residual from linear regression model. "Mincer" refers to model with dummies for education categories and cubic in experience, fit separately in each year. Other models add controls as indicated.

Figure Va: Mean Wages of Job Changers, Classified by Quartile of Mean Wage of Co-Workers at Origin and Destination Establishment, 1985-91



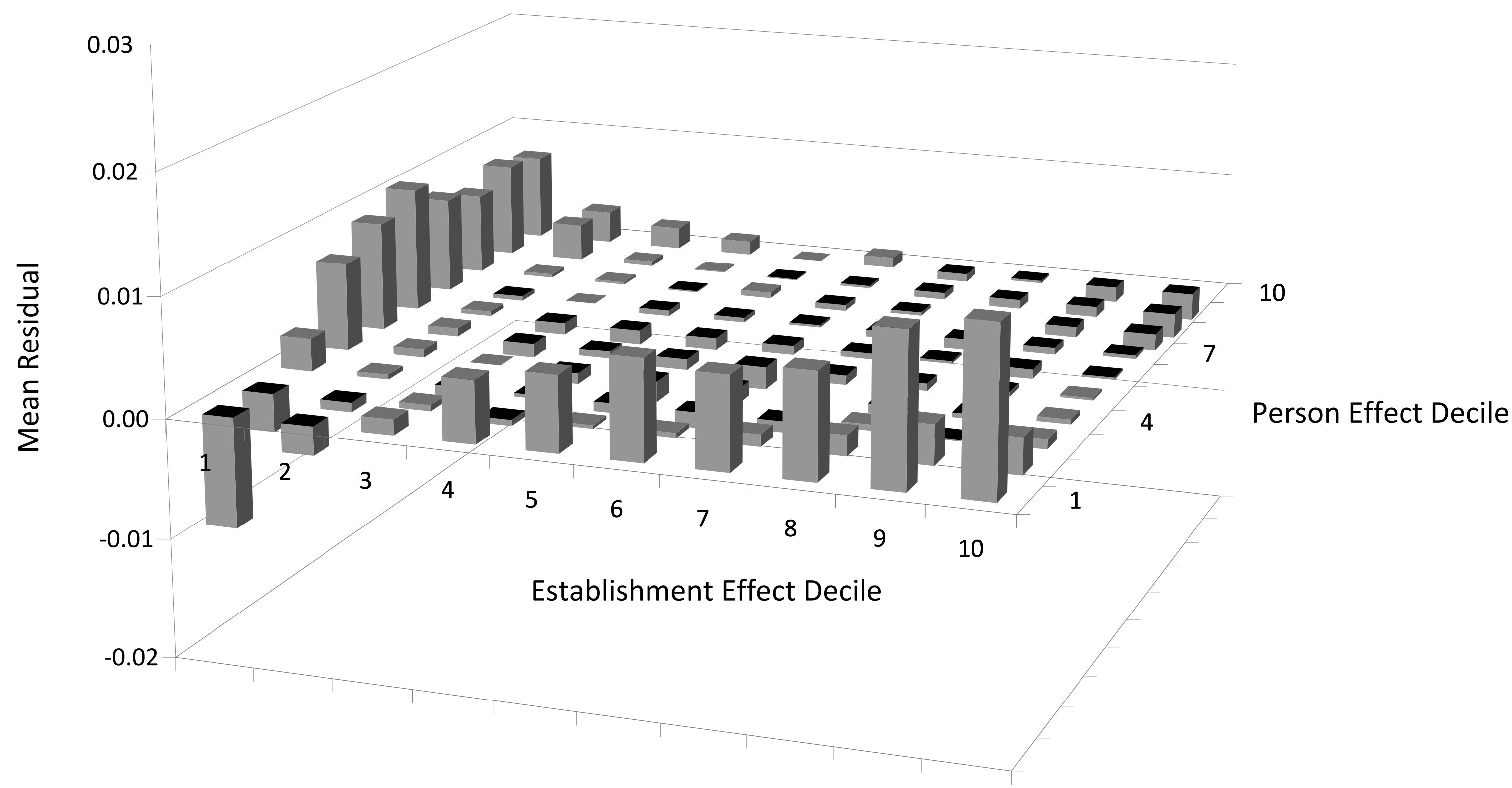

Notes: figure shows mean wages of male workers observed in 1985-1991 who change jobs in 1987, 1988 or 1989, and held the preceding job for 2 or more years, and the new job for 2 or more years. "Job" refers to establishment with most earnings in year, excluding part time work. Each job is classified into quartiles based on mean wage of co-workers (quartiles are based on all full time workers in the same year).

Figure Vb: Mean Wages of Job Changers, Classified by Quartile of Mean Wage of Co-Workers at Origin and Destination Establishment, 2002-09



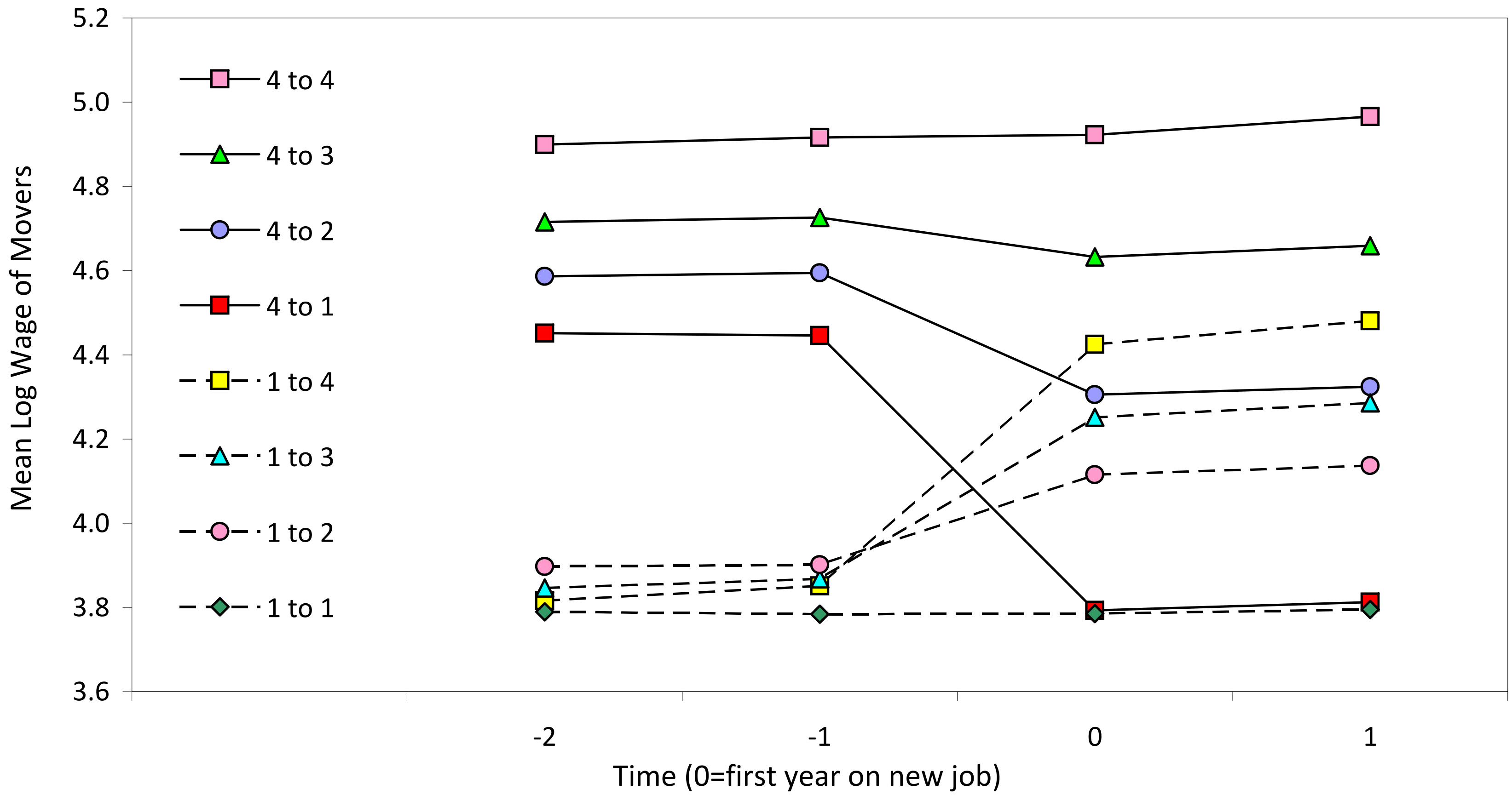

Notes: figure shows mean wages of male workers observed in 2002-2009 who change jobs in 2004-2007 and held the preceding job for 2 or more years, and the new job for 2 or more years. "Job" refers to establishment with most earnings in year, excluding part time work. Each job is classified into quartiles based on mean wage of co-workers (quartiles are based on all full time workers in the same year).

Figure VI: Mean Residual by Person/Establishment Deciles, 2002-09



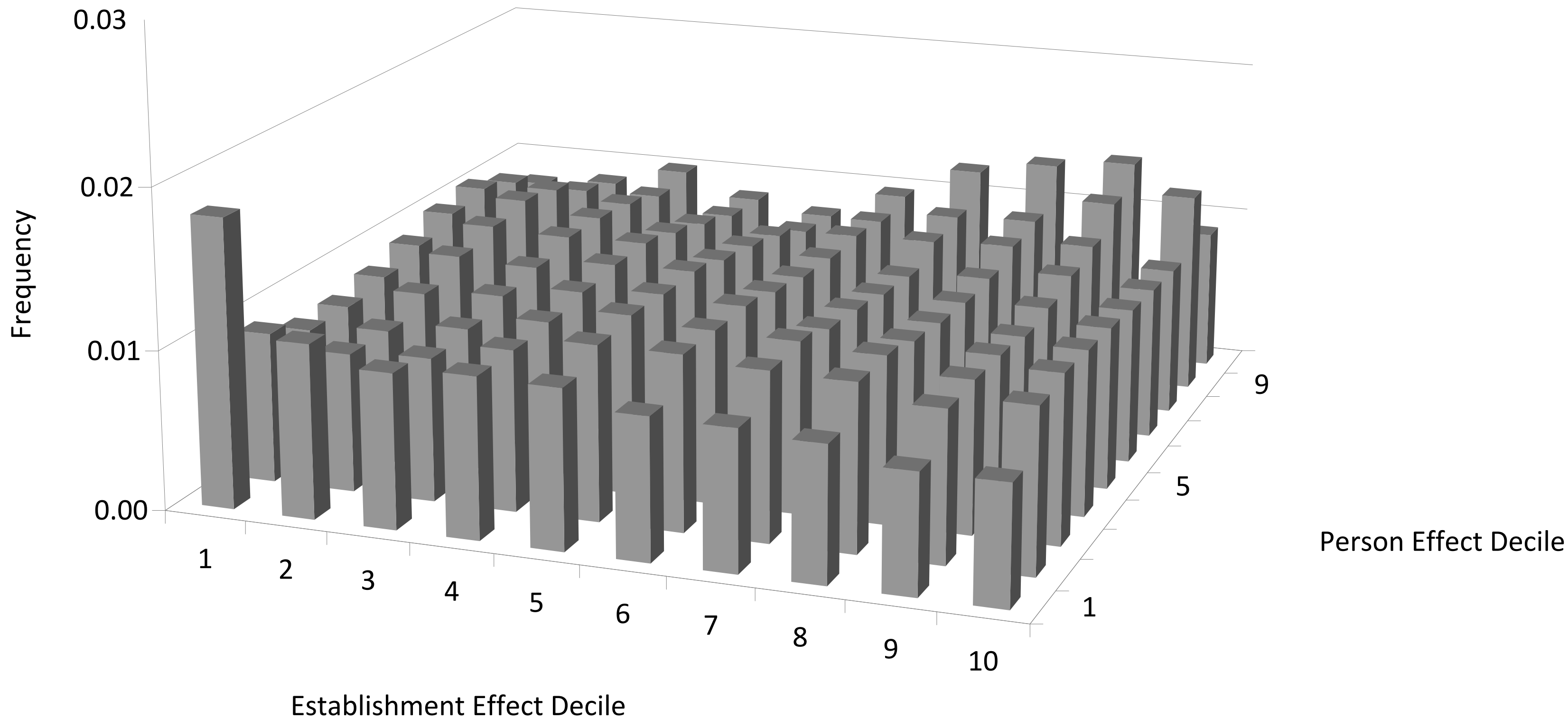

Notes: figure shows mean residuals from estimated AKM with cells defined by decile of estimated establishment effect, interacted with decile of estimated person effect. See column 4 of Table III for summary of model parameters.

Figure VII: Mean Wages of Movers, Classified by Quartile of Establishment Effects for Origin and Destination Firms, 2002-2009



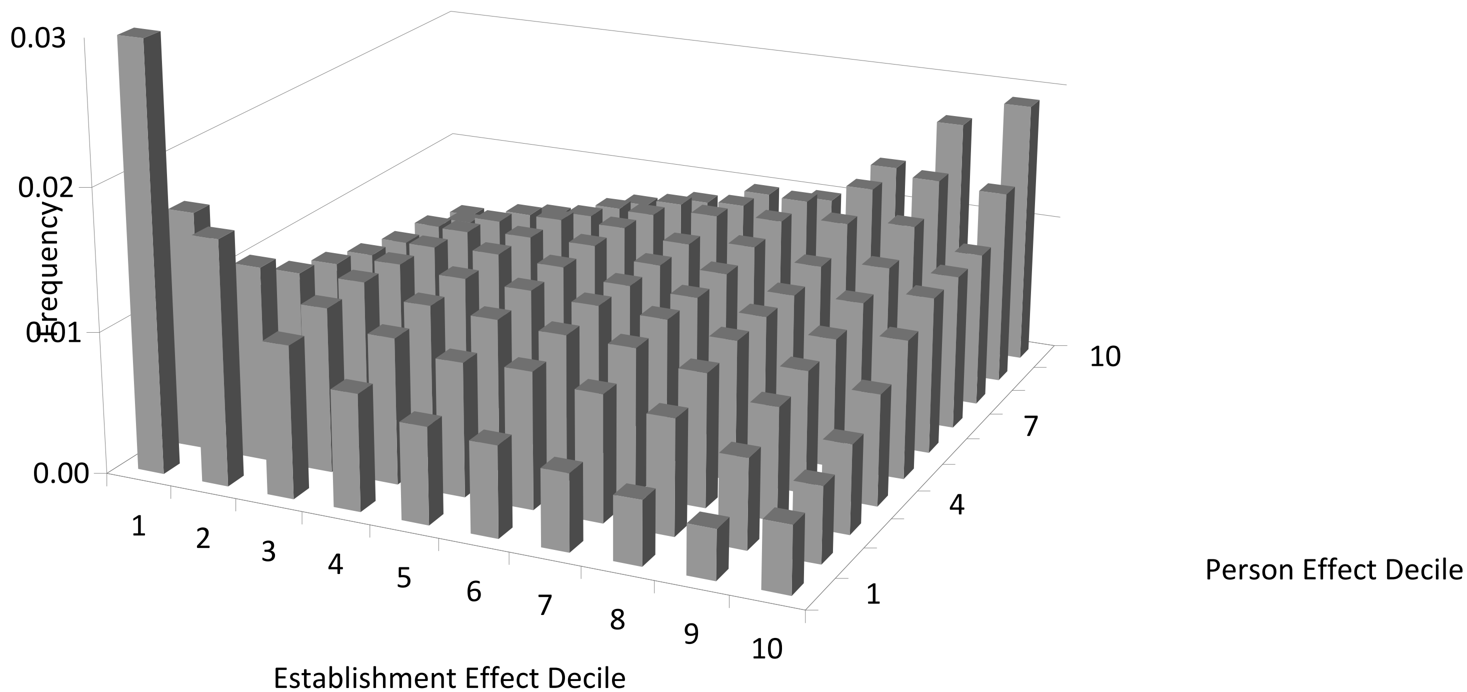

Notes: figure shows mean wages of male workers observed in 2002-2009 who change jobs in 2004-2007, and held the preceding job for 2 or more years, and the new job for 2 or more years. "Job" refers to main job in year, excluding part time jobs. Each job is classified into quartiles based on estimated establishment effect from AKM model presented in Table IIII column 4.

Figure XIIIa: Joint Distribution of Person and Establishment Effects, 1985-91



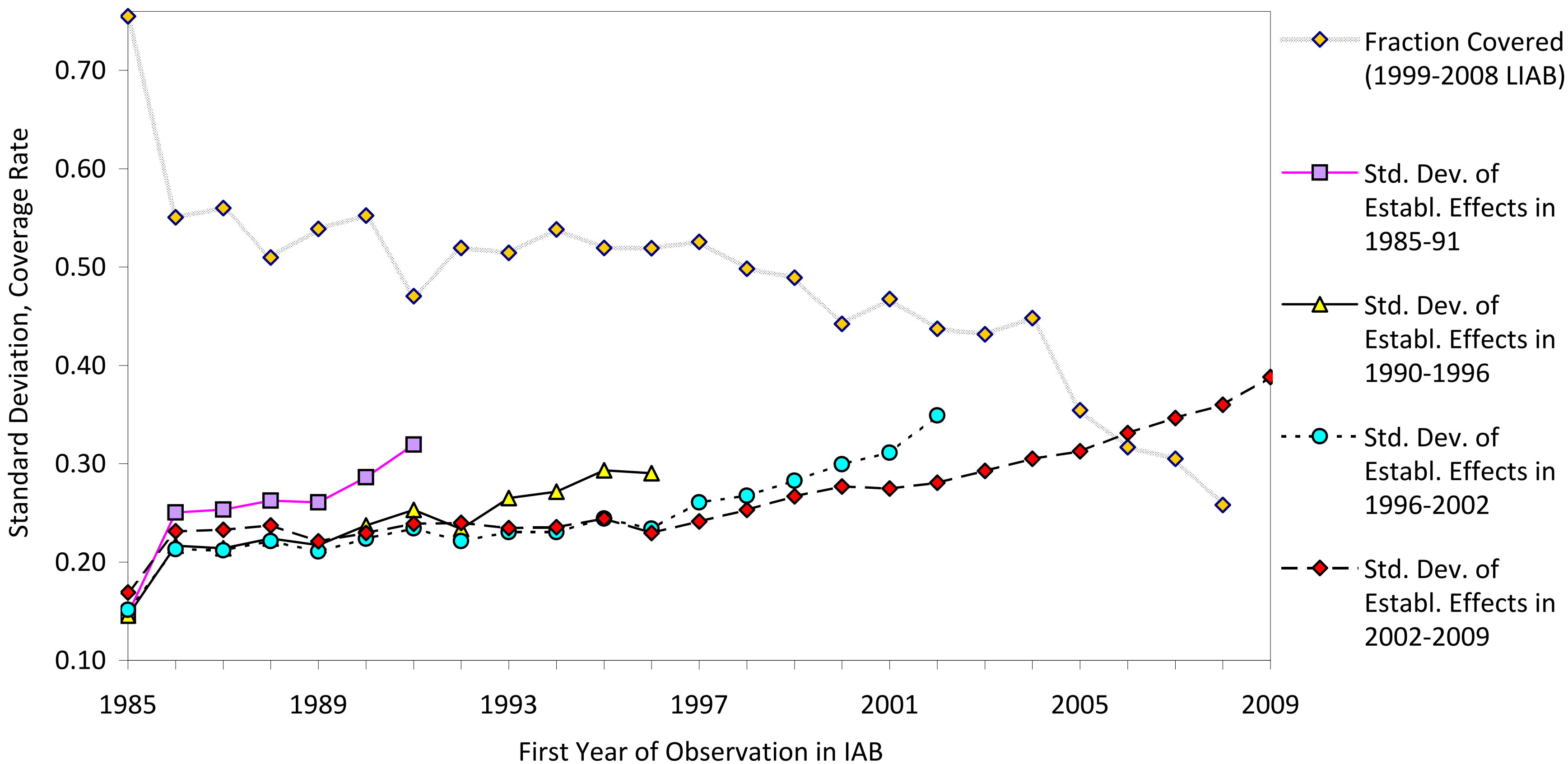

Note: figure shows joint distribution of estimated person and establishment effects from AKM model. See Table III column 1 for summary of model parameters.

## Figure XIIIb: Joint Distribution of Person and Establishment Effects, 2002-09



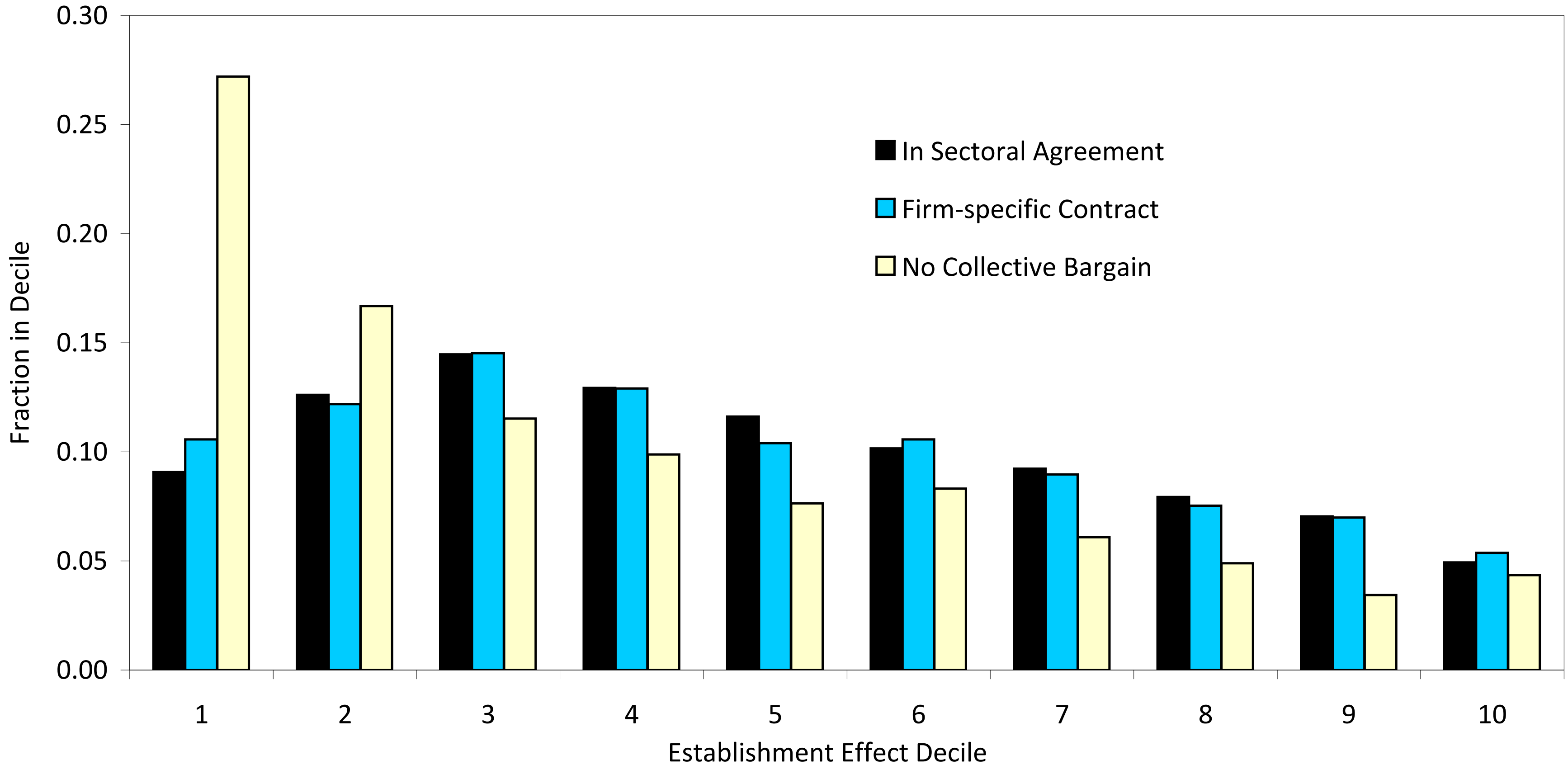

Note: figure shows joint distribution of estimated person and establishment effects from AKM model. See Table III column 4 for summary of model parameters.

## Figure IX: Standard Deviation of Establishment Effects and Fraction Covered by Collective Agreements, by Birth Year of Establishment




Notes: figure shows standard deviation of estimated establishment effects in a given observation interval (1985- 1991, 1990-1996, 1996-2002, or 2002-2009) for establishments that are present in that interval and first appeared with positive full time male employment in the IEB data in the "birth year" indicated on the x-axis. Figure also shows fraction of establishments in a given birth year surveyed in the 1999-2008 LIAB that are covered by collective agreements.

Figure X: Distribution of Establishment Effects by Collective Bargaining Status
(Based on Establishment Effects for 1996-2002, Bargaining Status in 2000 LIAB)




Note: figure shows distribution of collective bargaining coverage status (no collective bargain, covered by firm-specific agreement, or covered by sectoral agreement) for 7,080 establishments in 2000 Wave of LIAB that can be linked to IEB data. Establishments are classified into deciles of their estimated establishment effects from AKM model fit to 1996-2002 data.

# Online Appendix to Card, Heining, and Kline (2013)

January 29, 2013

## Contents



In this appendix, we provide additional details on the construction of our sample, the nature of our imputation procedure, and provide some supplementary results.

# 1 Sample Construction and Processing

## 1.1 Creation of Wage Data

Our basic data source is the Integrated Employment Biography (IEB) database. The IEB consists of information on employment spells at a given establishment (or firm) within a calendar year, taken from notices of employment filed by the employer. Each notice of employment has a beginning date (e.g., January 1), an end date (e.g., December 31), the average daily wage earned by the employee (censored at the Social Security maximum earnings level), indicators for the legal status of the job (including whether the job is full time or part time and whether the job is a "marginal job" subject to reduced Social Security taxes), as well as information on the gender, birth date, educational qualification and occupation of the worker, and the industry and geographic location of the establishment.

We process the data in two steps. First, we collapse all spells that are recorded as full-time jobs at the same employer in a given year into a single person-firm-year record, summing total earnings at each employer. Row 1 of panels A and B in Appendix Table A.1 shows the numbers of spell records in the IAB data file for full-time male and female employees age 20-60 working in non-marginal jobs in 1985 (the first year of our sample), 1997 (the middle year of our sample) and 2009 (the last year). Row 2 in each panel shows the number of person-firm-year records: on average there are about 1.06 spells per person-firm-year in the early years of our sample, rising slightly to 1.11 spells per person-firm-year in 2009 (see row 4).

In the second step we select one observation per person per year by selecting the person-firm record with the highest total earnings in a given year (and excluding any observation with a daily wage $<$ 10 Euros). On average each person has about 1.1 different employers per year, with only a small upward trend over our sample period (see row 5 in each panel). Line 3 in panels A and B shows the numbers of person-year observations for full time men and women in our final data set.

## 1.2 Education

Education levels in the IEB are coded into 6 categories, plus a missing or undetermined category. We group these into 5 classes: (1) missing; (2) primary/lower secondary or intermediate school leaving certificate, or equivalent, with no vocational qualification; (3) primary/lower secondary or intermediate school leaving certificate, or equivalent, with a vocational qualification; (4) upper secondary school certificate ("Arbitur") with or without a vocational certificate; (5) degree from Fachhochschule or university. For simplicity we refer to the third category as "apprentices" and the fifth category as "university graduates". For an individual who is observed in multiple notifications from the same employer in the same year we assign the highest education category for that person-firm-year observation. Within each job spell we assign the modal education category observed for an individual in the years he is at the same job.

## 1.3 Occupation and Industry

In the IEB data, each job notification includes information on occupation and industry. For an individual who is observed in multiple notifications from the same employer in the same year we assign the highest occupation category and the highest industry category for that person-firm-year observation. Within job spells the industry code is constant in 97% of spells. In the remaining cases we assign the highest industry category observed over the years of the job. We do not assign a fixed occupation code to job spells.

## 1.4 Tobit imputations

As illustrated in Table I of the paper, roughly 10 percent of person-year observations for male workers and 1-2 percent of the observations for female workers are censored at the Social Security maximum. We follow Dustmann, Ludsteck, and Schönberg [2009] and fit a series of Tobit models to log daily wages. We then impute an uncensored value for each censored observation using the estimated parameters of these models and a random draw from the associated (left- censored) distribution.[1]

Since we are fitting models that include both a person and year effect, we want the imputation model to reflect individual and job-specific components of the wage. We therefore construct, for each individual in each year, the mean of his log wage in all other periods, and the fraction of other years that the individual's wage is censored. For individuals who are only observed in one year, we set the mean log wage in other years to the sample mean, and the fraction of censored wages in other years equal to the sample mean, and include a dummy in the model for those who are observed only once. We also construct the mean log wage for the individual's co-workers in the current year (i.e., the "leave out mean" of log wages at his employer) and the fraction of co-workers who are censored in the current year (the "leave out mean" of the censoring rate at his employer). For individuals who work at an establishment with only 1 full time male employee we set the mean log wage for co-workers equal to the sample mean, and the fraction of co-workers with censored wages equal to the sample mean, and include a dummy in the model for employees of 1-worker firms.

We then fit a series of 500 Tobit models separately by year, education (5 values: missing; no qualification; apprenticeship; some post secondary; and university graduate), and 10 year age range (20-29; 30-39; 40-49; 50-60), including the following variables: age, mean log wage in other years, fraction of censored wages in other years, number of full time male employees at the current firm and its square, dummy for 11 or more employees, mean years of schooling and fraction of university graduates at the current firm, mean log wage of co-workers and fraction of co-workers with censored wages, dummy for individuals observed only 1 year between 1985 and 2009, dummy for employees of 1-worker firm. Appendix Table A.2 shows

[1] Specifically, we impute an upper tail as follows. Suppose that the estimated Tobit model for $y$ (the log of wages) has $y \sim N(X'\beta, \sigma)$, and consider a censored observation, such that $y \geq c$, where $c$ is the censoring point. Let $k = \Phi[(c - X'\beta)/\sigma]$, where $\Phi$ represents the standard normal density, and let $u \sim U[0,1]$ represent a uniform random variable. Then we impute an uncensored value for $y$ as: $y^u = X'\beta + \sigma\Phi^{-1}[k + u \times (1-k)]$.

the coefficient estimates for models for 40-49 year old apprentices in 1985, 1997, and 2009.

## 1.5 Validation Exercise

To evaluate the quality of the approximation to the upper tail provided by our Tobit specification, we performed a validation exercise in which we artificially censor the upper tail of wages for a group of workers with very low censoring rates, then fit Tobit models (with the same explanatory variables as in our main procedure) and stochastically impute the upper tail of wages. We then compare the standard deviation of wages for the original sample with the standard deviation from the censored/imputed sample. We use male workers age 20-29 with an apprenticeship education in years from 1990 to 2009 as the population of interest. These workers have an average censoring rate over the 20 year period of 0.7%. We select artificial censoring points so that 10, 20, 30, or 40 percent of workers are censored in each year, and fit separate Tobit models by year for each censored subsample.

Appendix Figure A.1 shows actual standard deviation of wages for the test sample, which rises from 0.296 to 0.348 (an increase of 0.052) between 1990 and 2009, as well as the standard deviations from the censored/imputed samples with differing censoring rates. The standard deviation of the imputed series is uniformly higher than the standard deviation of the raw data, with a larger upward bias at higher censoring rates. For example, when the censoring rate is 40%, the estimated standard deviation is upward biased by about 0.04 (or 13%) in every year. Fortunately, the upward bias is relatively constant, so the trend in the dispersion of wages is very similar whether we use the raw data or any of the censored/imputed series. This leads us to conclude that our Tobit imputation procedure performs relatively well, even for subgroups with very high censoring rates.

A concern about our imputation procedure is that it may alter the relative share within versus between establishment variation. To check this, we fit linear regression models with year dummies and establishment effects to observations from 2002 to 2009 (the same time span as our fourth sample interval), using the raw wage data for 20-29 year old men with an apprenticeship education, and the censored/imputed data. The sample has 8,426,930 person-year wage observations on employees at 668,285 establishments. The R-squared coefficients from the different samples were as follows:

| | |
|---|---|
| raw data: | 0.721 |
| 10% censored | 0.719 |
| 20% censored | 0.718 |
| 30% censored | 0.714 |
| 40% censored | 0.707 |

We conclude that the imputation procedure successfully maintains the relative share of the variance of wages attributable to within-establishment variation, even at very high censoring rates.

# 2 Computational Methods

Because our dataset is very large and identification of the establishment effects derives entirely from movers, we conducted estimation in two steps. First, in each interval, we extracted the sample of workers who switched establishments over the relevant time period. We fit the model to this sample of movers and recovered the estimated vector of establishment effects $\widehat{\psi}$ along with the coefficients $\widehat{\beta}$ corresponding to the time varying covariates $x_{it}$.[2] Then, for each worker who stayed at the same establishment over the sample interval, we computed an estimate of his person effect as follows:

$$\widehat{\alpha}_i = \frac{1}{T_i}\sum_t \left(y_{it} - \widehat{\psi}_{\mathbf{J}(i,t)} - x'_{it}\widehat{\beta}\right)$$

where $T_i$ is the number of periods that individual $i$ is observed in the sample interval. Our root mean squared error calculations were conducted by reducing the degrees of freedom by one for each connected stayer mean estimated.[3]

Our estimation tasks were performed in Matlab. Code for our analysis is available online. We used a variant of the depth first search algorithm implemented in the open source matlabBGL package to find the largest connected set of establishments in each data interval. The design matrices $Z \equiv [D, F, X]$ were stored as sparse matrices. To compute the least squares solutions we solved the normal equations in equation (3) of the paper using Matlab's preconditioned conjugate gradient routine (see Shewchuck [1994] for a lucid introduction). To speed the process we used an incomplete Cholesky factorization of $Z'Z$ as the preconditioner with threshold dropping tolerance of 0.01.

[2] This yields an inefficient estimator of $\beta$. However, in a sample of roughly 90 million observations, precision is not a major concern. A separate issue is that our two step procedure only ensures orthogonality between the AKM residuals $\widehat{r}_{it}$ and $x_{it}$ in the sample of movers. In practice, the correlation in the sample of establishment stayers between $x'_{it}\widehat{\beta}$ and the AKM residuals is very small in each interval, with the largest correlation occuring in interval 3 and amounting to approximately $-.01$.

[3] That is we used the formula $RMSE = \sqrt{\frac{SSR}{dof}}$ where $SSR$ is the sum of squared residuals across all person-year observations in the interval and $dof = N^* - N - (J-1) - rank\,(X)$, where $N^*$ is the number of person year observations including the stayers, $N$ is the number of connected individuals including the stayers, and $J$ is the number of connected establishments.

# 3 Bias in the estimated covariance matrix of person and establishment effects

It is well known that sampling errors in the estimated person and establishment effects may lead to inflated estimates of the standard deviation of each component and negatively biased estimates of the covariance between the person and establishment effects. This has led some authors (e.g., Andrews et al., 2008) to propose parametric bias corrections to the estimated components. To illustrate the logic of such an approach, and the difficulties involved, we denote the population quantities of interest as:

$$\begin{aligned}
\sigma^2_{D\alpha} &\equiv \frac{1}{N^*-1}\alpha' D' Q_1 D\alpha \text{ (Variance of person effects)} \\
\sigma^2_{F\psi} &\equiv \frac{1}{N^*-1}\psi' F' Q_1 F\psi \text{ (Variance of establishment effects)} \\
\sigma_{D\alpha,F\psi} &\equiv \frac{1}{N^*-1}\psi' F' Q_1 D\alpha \text{ (Covariance of person and establishment effects)}
\end{aligned}$$

where $Q_1 \equiv I - 1\left(1'1\right)^{-1} 1'$ is a symmetric demeaning matrix.

OLS estimation of (2) yields a coefficient vector:

$$\widehat{\xi} = \xi + \left(Z'Z\right)^{-1} Z'r$$

with $E\left[\left(Z'Z\right)^{-1} Z'r\right] = 0$. The sampling variance of this vector can be written:

$$\begin{aligned}
V_{\widehat{\xi}} &\equiv E\left[\left(\widehat{\xi}-\xi\right)\left(\widehat{\xi}-\xi\right)'\right] \\
&= \left(Z'Z\right)^{-1} Z'\Omega Z \left(Z'Z\right)^{-1}
\end{aligned}$$

where $\Omega \equiv E\left[rr'\right]$ is the $N^* \times N^*$ variance covariance matrix of the errors.

The sample analogues to the population quantities can be expressed in terms of the following quadratic forms:

$$\begin{aligned}
\widehat{\sigma}^2_{D\alpha} &\equiv \frac{1}{N^*-1}\widehat{\xi}' A_{D\alpha}\widehat{\xi} \\
\widehat{\sigma}^2_{F\psi} &\equiv \frac{1}{N^*-1}\widehat{\xi}' A_{F\psi}\widehat{\xi} \\
\widehat{\sigma}_{D\alpha,F\psi} &\equiv \frac{1}{N^*-1}\widehat{\xi}' A_{D\alpha,F\psi}\widehat{\xi}
\end{aligned}$$

where $A_{D\alpha} \equiv \begin{bmatrix} D'Q_1D & 0 & 0 \\ 0 & 0 & 0 \\ 0 & 0 & 0 \end{bmatrix}$, $A_{F\psi} \equiv \begin{bmatrix} 0 & 0 & 0 \\ 0 & F'Q_1F & 0 \\ 0 & 0 & 0 \end{bmatrix}$, $A_{D\alpha,F\psi} \equiv \begin{bmatrix} 0 & F'Q_1D & 0 \\ 0 & 0 & 0 \\ 0 & 0 & 0 \end{bmatrix}$.

Unbiasedness of OLS and standard results on quadratic forms imply that for any matrix $A$,

$$E\left[\widehat{\xi}' A \widehat{\xi}\right] = \xi' A \xi + tr\left(AV_{\widehat{\xi}}\right).$$

Therefore the bias in our estimates of the variance components corresponds to the trace term in the above expression, which in turn depends critically upon $V_{\widehat{\xi}}$. Previous work has focused on evaluating this bias expression under the assumption that the $r$ are independent and identically distributed in which case $\Omega = I\sigma^2$ and $V_{\widehat{\xi}} = (Z'Z)^{-1}\sigma^2$.

Unfortunately, the bulk of the literature on earnings dynamics (MaCurdy, 1982; Abowd and Card, 1989; Meghir and Pistaferri, 2004) suggests a substantially more complicated error structure of earnings with complex forms of temporal dependence and heteroscedasticity. Errors in modeling the structure of $\Omega$ will induce errors in estimation of $V_{\widehat{\xi}}$ which is why, at least since the work of White [1980], economists have sought robust variance estimates that don't rely upon estimation of all elements of $\Omega$. Unfortunately, robust variance estimation is not possible in our setting because the estimates $\widehat{\xi}$, while unbiased, are not consistent.

In unreported results we have attempted parametric corrections allowing for a match component and a moving average component to the errors $r$. These corrections yielded small changes in the estimated variance-covariance matrix $\widehat{V}_{\widehat{\xi}}$ and had trivial effects on the trends of the various components. In sampling experiments we found the corrections to provide a poor guide to the degree of bias created by working with subsamples of the data. We suspect this is because our model for the errors is insufficiently rich – a problem we are unlikely to be able to solve in a convincing way. For this reason, our decompositions in section 7 of between group means are of particular interest because these results are based upon group averages involving tens of thousands (or in some cases millions) of observations, in which case biases due to sampling error become largely irrelevant.

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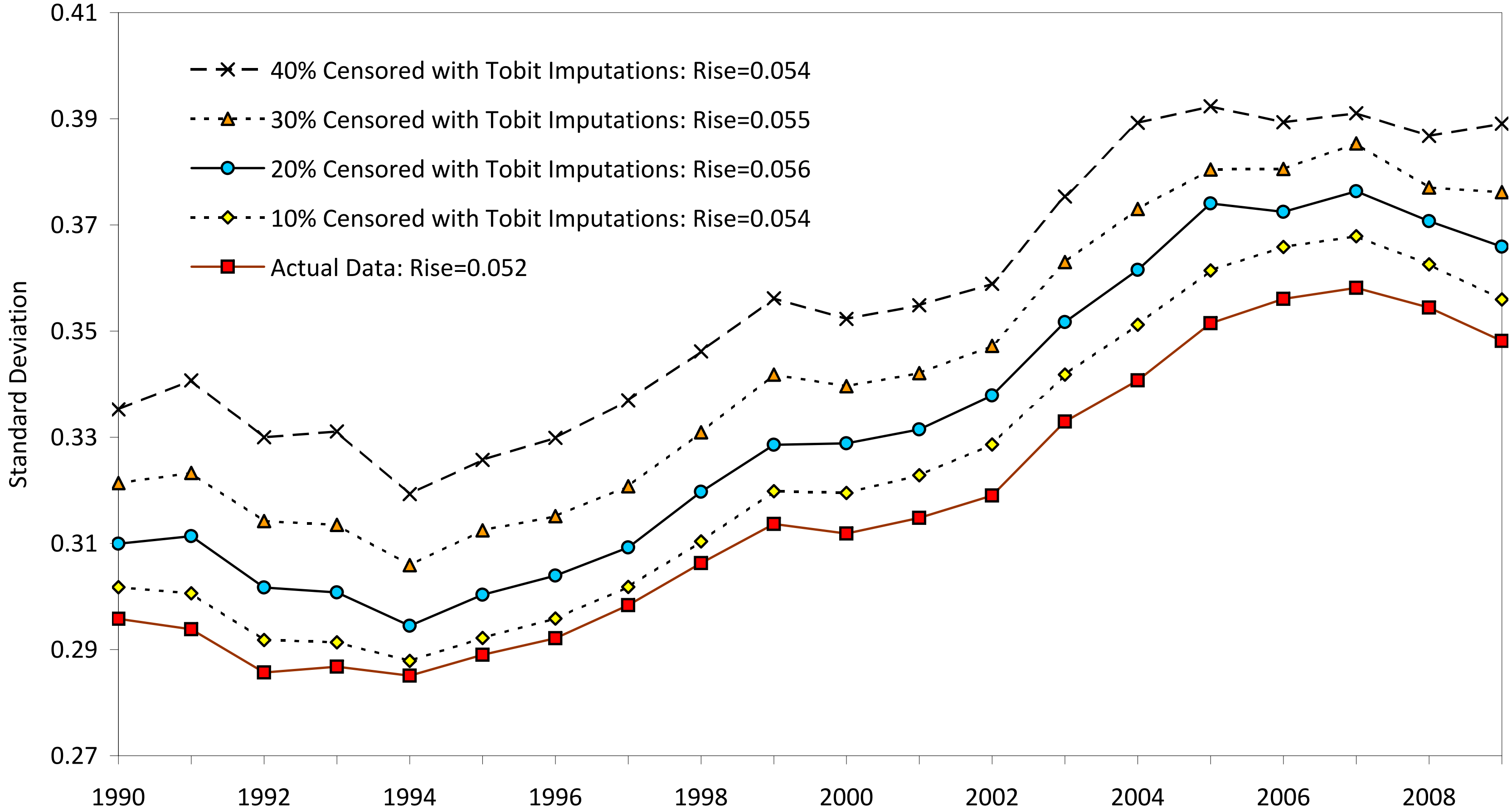
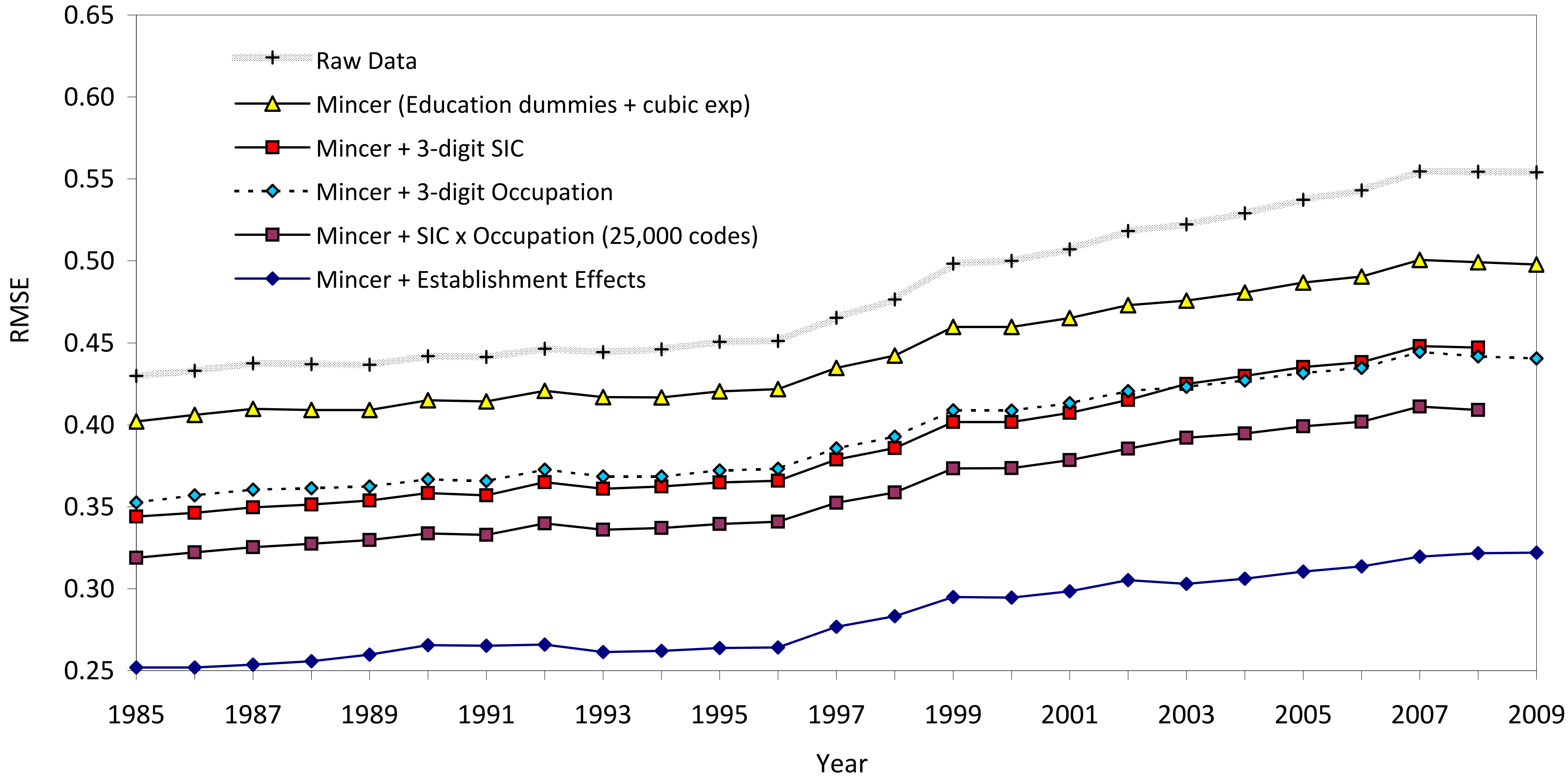
# 4 Appendix Figures and Tables

Appendix Figure A.1: Trends in Standard Deviations of Log Wages - Male Apprentices Age 20-29, Actual and Artificially Censored/Imputed Data




Note: Actual data has censoring rate of 0.5% or less in every year. Data are artificially censored at real wages levels yielding average censoring rates of 10,20,30, or 40% over the entire sample period. Then Tobit models are fit separately by year, with same covariates as used in main imputation model, and upper tail observations are randomly imputed using same procedure as in main imputation model.

## Appendix Figure A.2: Raw and Residual Standard Deviations from Alternative Wage Models for Full Time Females




Notes: See notes to Figure IV. Figure shows measures of dispersion in actual and residual real daily wage for full time female workers. Residual wage is residual from linear regression model. "Mincer" refers to model with dummies for education categories and cubic in experience, fit separately in each year. Other models add additional controls as indicated.

## Appendix Table A.1: Job Spells and Main Jobs in IEB Data Base

| | 1985 | 1997 | 2009 |
|---|---|---|---|
| A. Male Workers | | | |
| 1. Number of Full Time Job Spells (age 20-60, non-marginal jobs) | 13,987,548 | 14,919,079 | 14,911,559 |
| 2. Number of unique person-firm-year observations | 13,181,917 | 13,968,286 | 13,456,305 |
| 3. Number of person-year observations (highest-paying job only) | 11,980,159 | 12,661,995 | 12,104,223 |
| 4. Average number of spells per job in year | 1.06 | 1.07 | 1.11 |
| 5. Average number of jobs per year | 1.10 | 1.10 | 1.11 |
| B. Female Workers | | | |
| 1. Number of Full Time Job Spells (age 20-60, non-marginal jobs) | 6,965,926 | 7,937,037 | 8,142,682 |
| 2. Number of unique person-firm-year observations | 6,642,114 | 7,408,237 | 7,328,008 |
| 3. Number of person-year observations (highest-paying job only) | 6,068,863 | 6,758,622 | 6,566,429 |
| 4. Average number of spells per job in year | 1.05 | 1.07 | 1.11 |
| 5. Average number of jobs per year | 1.09 | 1.10 | 1.12 |

Notes: each "job spell" represents a notification of employment in the IEB data base. For each gender, the entry in row 1 is the number of such notifications for full time, non-marginal jobs held by men age 20-60. Row 2 shows the number of unique person-firm-year observations after collapsing multiple spells at the same employer in the same year. Row 3 shows the number of person observations after selecting the person-firm-year observation with the highest total earnings in the year as the "main job" in a given year. Row 4 gives the ratio of row 1 to row 2. Row 5 gives the ratio of row 2 to row 3.

## Appendix Table A.2: Selected Tobit Models for Male Apprentices, Age 40-49

| | 1985 | 1997 | 2009 |
|---|---|---|---|
| Moments of Unadjusted Log Wage Data: | | | |
| Mean | 4.382 | 4.4612 | 4.434 |
| Std. Deviation | 0.2677 | 0.3104 | 0.3563 |
| Fraction Censored | 0.155 | 0.107 | 0.0726 |
| Parameter Estimates from Tobit model: | | | |
| Intercept | 0.539 | -0.108 | -0.194 |
| | (0.030) | (0.020) | (0.003) |
| Age/10 | 0.026 | -0.009 | -0.103 |
| | (0.002) | (0.002) | (0.003) |
| Fraction of person's other wage | 0.268 | 0.270 | 0.699 |
| observations censored | (0.003) | (0.005) | (0.011) |
| Mean of log wage for person | 0.693 | 0.909 | 0.819 |
| in other years | (0.003) | (0.003) | (0.004) |
| Dummy for firm size > 10 | -0.006 | -0.006 | 0.005 |
| | (0.002) | (0.002) | (0.002) |
| Fraction of workers at firm with | -0.045 | -0.013 | 0.007 |
| university degree* | (0.012) | (0.011) | (0.014) |
| Mean years of schooling of workers | -0.014 | -0.010 | -0.007 |
| at firm* | (0.002) | (0.002) | (0.002) |
| Fraction of co-workers with | 0.093 | 0.000 | 0.135 |
| censored wage** | (0.007) | (0.007) | (0.010) |
| Mean log wage of co-workers** | 0.187 | 0.163 | 0.374 |
| | (0.004) | (0.004) | (0.004) |
| Dummy if person observed in | -0.398 | -0.935 | -0.808 |
| only 1 year | (0.005) | (0.013) | (0.016) |
| Dummy if firm has only 1 | -0.223 | -0.178 | -0.386 |
| worker | (0.005) | (0.005) | (0.006) |
| Estimated scale parameter | 0.147 | 0.135 | 0.195 |
| | (0.001) | (0.001) | (0.001) |
| Sample size | 62,889 | 58,392 | 65,904 |

Notes: standard errors in parentheses. Table entries are coefficient estimates from Tobit models fit to log real daily wages, with censoring at the Social Security maximum contribution rate. Models also include firm size (number of current-year full time male employees) and its square.

*Characteristics of full-time male (non-marginal) employees at the same firm.

**Statistic is calculated for full-time male employees at the same firm, excluding the individual of interest. For employers with one firm, statistic is set to mean.

Appendix Table A.3: Mean Log Wages Before and After Job Change, by Quartile of Mean Co-workers' Wages at Origin and Destination Establishments

| Origin/destination Quartile* | Number of Observations: (1) | Mean Log Wages of Movers: 2 Years Before (2) | 1 Year Before (3) | 1 Year After (4) | 2 Years After (5) | Change from 2 Years Before to 2 Years After: Raw (6) | Adjusted** (7) |
|---|---|---|---|---|---|---|---|
| Interval 1: 1985-1991 | | | | | | | |
| 1 to 1 | 333,648 | 4.00 | 4.03 | 4.08 | 4.11 | 0.11 | 0.00 |
| 1 to 2 | 206,251 | 4.06 | 4.08 | 4.21 | 4.25 | 0.18 | 0.07 |
| 1 to 3 | 136,119 | 4.06 | 4.09 | 4.27 | 4.32 | 0.26 | 0.15 |
| 1 to 4 | 82,193 | 4.10 | 4.13 | 4.38 | 4.44 | 0.34 | 0.23 |
| 2 to 1 | 125,376 | 4.16 | 4.18 | 4.14 | 4.18 | 0.01 | -0.07 |
| 2 to 2 | 204,787 | 4.23 | 4.25 | 4.29 | 4.32 | 0.09 | 0.00 |
| 2 to 3 | 158,360 | 4.26 | 4.28 | 4.36 | 4.40 | 0.14 | 0.05 |
| 2 to 4 | 86,038 | 4.30 | 4.32 | 4.47 | 4.53 | 0.23 | 0.14 |
| 3 to 1 | 59,334 | 4.25 | 4.26 | 4.16 | 4.19 | -0.05 | -0.15 |
| 3 to 2 | 91,474 | 4.32 | 4.34 | 4.33 | 4.37 | 0.06 | -0.05 |
| 3 to 3 | 173,160 | 4.38 | 4.41 | 4.45 | 4.49 | 0.10 | 0.00 |
| 3 to 4 | 136,569 | 4.46 | 4.49 | 4.59 | 4.64 | 0.18 | 0.07 |
| 4 to 1 | 30,110 | 4.37 | 4.40 | 4.25 | 4.28 | -0.09 | -0.22 |
| 4 to 2 | 41,079 | 4.46 | 4.49 | 4.45 | 4.49 | 0.03 | -0.10 |
| 4 to 3 | 91,177 | 4.55 | 4.58 | 4.60 | 4.63 | 0.08 | -0.05 |
| 4 to 4 | 290,921 | 4.68 | 4.71 | 4.78 | 4.81 | 0.13 | 0.00 |
| Interval 4: 2002-2009 | | | | | | | |
| 1 to 1 | 541,307 | 3.88 | 3.87 | 3.87 | 3.88 | 0.00 | 0.00 |
| 1 to 2 | 197,982 | 4.02 | 4.02 | 4.17 | 4.18 | 0.16 | 0.17 |
| 1 to 3 | 88,768 | 4.00 | 4.02 | 4.29 | 4.32 | 0.33 | 0.33 |
| 1 to 4 | 49,167 | 4.06 | 4.08 | 4.47 | 4.52 | 0.47 | 0.47 |
| 2 to 1 | 208,184 | 4.20 | 4.19 | 4.02 | 4.03 | -0.17 | -0.16 |
| 2 to 2 | 333,219 | 4.32 | 4.31 | 4.30 | 4.31 | -0.01 | 0.00 |
| 2 to 3 | 137,528 | 4.38 | 4.38 | 4.44 | 4.46 | 0.08 | 0.08 |
| 2 to 4 | 59,080 | 4.47 | 4.48 | 4.63 | 4.67 | 0.20 | 0.21 |
| 3 to 1 | 73,218 | 4.34 | 4.33 | 4.02 | 4.04 | -0.30 | -0.31 |
| 3 to 2 | 133,606 | 4.46 | 4.46 | 4.39 | 4.40 | -0.06 | -0.07 |
| 3 to 3 | 275,521 | 4.55 | 4.55 | 4.56 | 4.57 | 0.02 | 0.00 |
| 3 to 4 | 150,989 | 4.68 | 4.69 | 4.77 | 4.81 | 0.13 | 0.11 |
| 4 to 1 | 32,664 | 4.52 | 4.53 | 4.14 | 4.17 | -0.35 | -0.43 |
| 4 to 2 | 47,193 | 4.65 | 4.67 | 4.53 | 4.56 | -0.09 | -0.18 |
| 4 to 3 | 127,276 | 4.74 | 4.76 | 4.72 | 4.75 | 0.00 | -0.08 |
| 4 to 4 | 546,855 | 4.94 | 4.96 | 4.97 | 5.02 | 0.08 | 0.00 |

Notes: entries are mean log real daily wages for job changers who are observed with at least 2 years of data prior to a job change, and two years after. Sample excludes movers to/from establishments with 1 worker.

*Quartiles are based on mean wages of co-workers at old job in year prior to move, and in new job in year after move.

**Trend-adjusted mean wage change, calculated as mean wage change for origin-destination group, minus mean change for job movers from the same origin quartile who remain in same quartile.

Appendix Table A.4: Estimation Results for AKM Model, Fit by Interval for Male Apprentices

| | Interval 1<br>1985-1991<br>(1) | Interval 2<br>1990-1996<br>(2) | Interval 3<br>1996-2002<br>(3) | Interval 4<br>2002-2009<br>(4) |
|---|---|---|---|---|
| *Person and Establishment Parameters:* | | | | |
| Number person effects | 10,128,342 | 10,346,742 | 9,288,956 | 8,145,059 |
| Number establishment effects | 1,008,959 | 1,078,911 | 1,093,438 | 990,608 |
| *Summary of Parameter Estimates:* | | | | |
| Std. dev. of person effects (across person-year obs.) | 0.241 | 0.249 | 0.271 | 0.285 |
| Std. dev. of establ. effects (across person-year obs.) | 0.152 | 0.159 | 0.170 | 0.202 |
| Std. dev. of Xb (across person-year obs.) | 0.121 | 0.080 | 0.073 | 0.064 |
| Correlation of person/est effects (across person-year obs.) | -0.035 | 0.008 | 0.048 | 0.098 |
| Correlation of person effects/Xb (across person-year obs.) | 0.050 | 0.075 | -0.029 | 0.020 |
| Correlation of establ. effects/Xb (across person-year obs.) | 0.064 | 0.064 | 0.048 | 0.082 |
| RMSE of AKM residual | 0.110 | 0.109 | 0.110 | 0.110 |
| Adjusted R-squared | 0.887 | 0.891 | 0.901 | 0.919 |
| *Comparison Match Model* | | | | |
| RMSE of Match model | 0.096 | 0.095 | 0.094 | 0.094 |
| Std. Dev of Match Effect* | 0.054 | 0.052 | 0.056 | 0.057 |
| Std. Dev. Log Wages | 0.328 | 0.329 | 0.349 | 0.388 |
| Sample size | 54,993,845 | 56,701,812 | 51,031,280 | 50,700,611 |

Notes: see notes to Table III. Models reported here are estimated for subsample of full time male workers with apprenticeship training only. Xb includes year dummies and quadratic and cubic terms in age (total of 8 parameters in intervals 1-3, 9 in interval 4). Match model includes Xb and separate dummy for each job (person-establishment pair).

*Standard deviation of match effect estimated as square root of difference in mean squared errors between AKM model and match effect model.

Appendix Table A.5: Decompositions of Rise in Wage Inequality for Apprentices

| | Interval 1 (1985-1991) | | Interval 4 (2002-2009) | | Change from Interval 1 to 4 | |
|---|---|---|---|---|---|---|
| | Var. Component | Share of Total | Var. Component | Share of Total | Var. Component | Share of Total |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Total variance of log wages | 0.108 | 100.0 | 0.150 | 100.0 | 0.043 | 100 |
| Components of Variance: | | | | | | |
| Variance of person effect | 0.058 | 53.7 | 0.081 | 54.2 | 0.024 | 55 |
| Variance of establ. effect | 0.023 | 21.3 | 0.041 | 27.1 | 0.018 | 42 |
| Variance of Xb | 0.015 | 13.6 | 0.004 | 2.7 | -0.011 | -25 |
| Variance of residual | 0.010 | 8.9 | 0.010 | 6.6 | 0.000 | 1 |
| 2cov(person, establ.) | -0.003 | -2.4 | 0.011 | 7.5 | 0.014 | 32 |
| 2cov(Xb, person+establ.) | 0.005 | 4.9 | 0.003 | 1.9 | -0.002 | -6 |
| ***Counterfactuals for Variance of log wages:*** * | | | | | | |
| 1. No rise in correl. of person/estab. effect | 0.108 | | 0.143 | | 0.035 | 82 |
| 2. No rise in var. of estab. effect | 0.108 | | 0.131 | | 0.023 | 55 |
| 3. Both 1 and 2 | 0.108 | | 0.125 | | 0.018 | 42 |

Notes: see notes to Table IV. Calculations based on estimated AKM models summarized in Appendix Table A.4.

*Counterfactual 1 computes the counterfactual rise in variance assuming the correlation between the person and establishment effects remains at its interval 1 value -- i.e. imposing the restriction that $Cov_4$(person, establ.) = $\rho_1$ $Var_4(\text{person})^{1/2}$ x $Var_4(\text{establ.})^{1/2}$ where subscript 4 refers to the interval 4 value of the statistic and and $\rho_1$ is the correlation between the person and establishment effects in interval 1. Counterfactual 2 assumes that the variance of establishment effects remains at its interval 1 level. Counterfactual 3 imposes both of these restrictions.

## Appendix Table A.6: Estimation Results for AKM Model for Full Time Female Workers

| | Interval 1<br>1985-1991<br>(1) | Interval 2<br>1990-1996<br>(2) | Interval 3<br>1996-2002<br>(3) | Interval 4<br>2002-2009<br>(4) |
|---|---|---|---|---|
| *Person and Establishment Parameters:* | | | | |
| Number person effects | 9,660,968 | 10,155,014 | 9,756,379 | 9,559,738 |
| Number establishment effects | 1,079,129 | 1,176,133 | 1,191,607 | 1,196,201 |
| *Summary of Parameter Estimates:* | | | | |
| Std. dev. of person effects (across person-year obs.) | 0.332 | 0.332 | 0.365 | 0.397 |
| Std. dev. of establ. effects (across person-year obs.) | 0.232 | 0.227 | 0.247 | 0.277 |
| Std. dev. of Xb (across person-year obs.) | 0.145 | 0.098 | 0.086 | 0.087 |
| Correlation of person/establ. effects (across person-year obs.) | -0.009 | 0.039 | 0.044 | 0.069 |
| Correlation of person effects/Xb (across person-year obs.) | -0.249 | -0.140 | -0.117 | -0.089 |
| Correlation of establ. effects/Xb (across person-year obs.) | 0.040 | 0.024 | 0.009 | 0.041 |
| RMSE of AKM residual | 0.137 | 0.135 | 0.147 | 0.157 |
| Adjusted R-squared | 0.894 | 0.900 | 0.901 | 0.909 |
| *Comparison Match Model* | | | | |
| RMSE of Match model | 0.117 | 0.118 | 0.125 | 0.133 |
| Adjusted R-squared | 0.922 | 0.924 | 0.928 | 0.934 |
| Std. Dev. of Match Effect* | 0.070 | 0.067 | 0.077 | 0.083 |
| *Addendum* | | | | |
| Std. Dev. Log Wages | 0.420 | 0.427 | 0.467 | 0.521 |
| Sample size | 40,846,416 | 44,351,293 | 41,576,298 | 44,751,361 |

Notes: see notes to Table III. Models reported here are estimated for subsample of full time female workers. Xb includes year dummies and quadratic and cubic terms in age (total of 8 parameters in intervals 1-3, 9 in interval 4). Match model includes Xb and separate dummy for each job (person-establishment pair).

*Standard deviation of match effect estimated as square root of difference in mean squared errors between AKM model and match effect model.

## Appendix Table A.7: Decomposition of the Rise in Wage Inequality -- Full Time Females

| | Interval 1 (1985-1991) | | Interval 4 (2002-2009) | | Change from Interval 1 to 4 | |
|---|---|---|---|---|---|---|
| | Var. Component | Share of Total | Var. Component | Share of Total | Var. Component | Share of Total |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Total variance of log wages | 0.176 | 100.0 | 0.272 | 100.0 | 0.095 | 100 |
| Components of Variance: | | | | | | |
| Variance of person effect | 0.110 | 62.6 | 0.158 | 58.1 | 0.048 | 50 |
| Variance of establ. effect | 0.054 | 30.6 | 0.077 | 28.2 | 0.023 | 24 |
| Variance of Xb | 0.021 | 11.9 | 0.008 | 2.8 | -0.013 | -14 |
| Variance of residual | 0.014 | 7.8 | 0.019 | 6.9 | 0.005 | 5 |
| 2cov(person, establ.) | -0.001 | -0.8 | 0.015 | 5.6 | 0.017 | 17 |
| 2cov(Xb, person+establ.) | -0.021 | -12.1 | -0.004 | -1.5 | 0.017 | 18 |
| ***Counterfactuals for Variance of log wages:*** * | | | | | | |
| 1. No rise in correl. of person/estab. effects | 0.176 | | 0.255 | | 0.078 | 82 |
| 2. No rise in var. of estab. effect | 0.176 | | 0.247 | | 0.070 | 74 |
| 3. Both 1 and 2 | 0.176 | | 0.232 | | 0.056 | 59 |

Notes: See notes to Table IV. Calculations based on estimated AKM models summarized in Appendix Table A.6.

*Counterfactual 1 computes the counterfactual rise in variance assuming the correlation between the person and establishment effects remains at its interval 1 value -- i.e. imposing the restriction that $Cov_4(\text{person, establ.}) = \rho_1 \, Var_4(\text{person})^{1/2} \times Var_4(\text{establ.})^{1/2}$ where the 4 subscript refers to the interval 4 value of the statistic and $\rho_1$ is the correlation between the person and establishment effects in interval 1. Counterfactual 2 assumes that the variance of establishment effects remains at its interval 1 level. Counterfactual 3 imposes both of these restrictions.