

Workplace Heterogeneity and the Rise of German Wage Inequality

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August 2012

Abstract

We study the role of establishment-specific wage premiums in generating recent increases in German wage inequality. Models with additive fixed effects for workers and establishments are fit in four distinct time intervals spanning the period 1985-2009. Unlike standard wage models, specifications with both worker and plant-level heterogeneity components can explain the vast majority of the rise in wage inequality. Our estimates suggest that the increasing variability of German wages results from a combination of rising heterogeneity between workers, rising variability in the wage premiums at different establishments, and increasing assortativeness in the matching of workers to plants. We use the models to decompose changes in wage gaps between different education levels, occupations, and industries, and in all three cases find a growing contribution of plant heterogeneity and rising assortativeness between workers and establishments.

*We are grateful to the IAB and to Stefan Bender for assistance in making this project possible, to Karl Schmidt for invaluable programming help, and to the Berkeley Center for Equitable Growth for funding support. We also thank Christian Dustmann, Bernd Fitzenberger, and seminar participants at EUI Florence and the NBER Labor Studies meetings for many helpful comments and suggestions. Robert Heimbach, Hedvig Horvath, and Michele Weynandt provided excellent research assistance.

1 Introduction

Many of the world’s developed economies have experienced dramatic increases in wage inequality in recent decades (see Katz and Autor, 1999; and Acemoglu and Autor, 2011 for detailed reviews). While the timing varies from country to country, in each case there was a widening of wage differentials between observable skill groups (e.g., age and education groups), coupled with a rise in within-group inequality. The standard framework for analyzing these changes focuses on shifts in supply and demand for different skill groups (e.g., Katz and Murphy, 1992; Bound and Johnson, 1992; Juhn, Murphy and Pierce, 1993; Goldin and Katz, 2008), mediated by institutional factors like minimum wages and unions (e.g., DiNardo, Fortin, and Lemieux, 1996). Using a variety of data sources, researchers have concluded that supply, demand, and institutions have all contributed to trends in wage inequality, albeit with different magnitudes in different countries and time periods (Freeman and Katz, 1994; Katz and Autor, 1999).

Although most studies focus on the causes of rising wage inequality between workers with different skills, economists have long recognized that employer heterogeneity may generate wage variation among workers with *identical* skills, even within narrowly defined economic sectors (Slichter, 1950; Reynolds, 1951; Lester, 1967; Rees and Schultz, 1970). There are large differences in productivity across firms and plants (Bernard, Eaton, Jensen, Kortum, 2003; Bloom and Van Reenen, 2007; Foster, Haltiwanger, and Syverson, 2008; Hsieh and Klenow, 2009). Modern search-theoretic models with realistic frictions suggest that such differences will lead to an employer-specific component of wage inequality among otherwise identical workers (see e.g., Lentz and Mortensen, 2010). Likewise, an earlier generation of studies argued that employers can offer efficiency wages to reduce shirking and boost morale (Akerlof, 1982; Shapiro and Stiglitz, 1984; Krueger and Summers, 1988; Katz and Summers, 1989). And, in a tradition dating back to Adam Smith, economists have argued that workplace-specific disamenities like longer hours or more strenuous effort will lead to compensating wage differences across jobs (e.g., Brown, 1980).

The magnitude of this workplace component of wage inequality is explored in a number of recent papers that utilize matched employer-employee datasets (Abowd, Kramarz, and Margolis, 1999; Goux and Maurin, 1999; Abowd, Creecy, and Kramarz, 2002; Gruetter and Lalive, 2009).¹ Virtually all these studies find a significant employer-specific wage compo-

¹An 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. Brown and Medoff (1989) provided important evidence of firm and establishment size effects on wages which are not easily explained by worker sorting.

ment. To date, however, little evidence exists on whether this component has become larger over time, and if so whether such changes can explain some of the trend in cross-sectional wage inequality.²

A priori, workplace heterogeneity could have become more or less important in recent decades. On one hand, increases in product market competition, coupled with institutional reforms, rising labor market intermediation (Autor, 2009) and reductions in the cost of information might be expected to *reduce* workplace-specific pay components and push labor markets closer to obeying the law of one price. On the other hand, the rise of firm-specific policies like pay-for-performance (Lemieux, MacLeod, and Parent, 2009), outsourcing (Abraham and Taylor, 1996; Dube and Kaplan, 2010), and the use of temporary workers (Segal and Sullivan, 1997), as well as rising complementarities between highly skilled workers (Kremer and Maskin, 1996) might lead to an increase in plant-level variation in wage outcomes. In addition, uneven rates of adoption of the technological innovations that have driven shifts in relative demand for different skill groups (e.g. Autor, Levy, and Murnane, 2002, 2003; Acemoglu et al., 2007; Acemoglu and Autor, 2011), could also lead to a rise in the employer component of wage inequality.

We contribute to the literature by studying the changing role of workplace heterogeneity in wage determination using detailed administrative data from West Germany over the period from 1985 to 2009. Several factors make the West German case particularly informative. First, as documented by Dustmann et al. (2009), Germany experienced large increases in wage inequality from the mid-1990s onward.³ Figure 1a shows the evolution of various real wage percentiles for full time male workers, indexed to a base of 1996.⁴ In the 13-year period from 1996 to 2009, the gap between the 20th and 80th percentiles of wages expanded by approximately 20 log points. By comparison, as shown in Figure 1b, the burst of wage inequality in the U.S. during the 1980s led to a more modest 15 log point rise in the 80-20 gap for male wages.⁵ Unlike the U.S., where changes in the federal minimum wage may have impacted the wages of low-skilled workers (Lee, 1999), there is no general minimum wage in Germany. Arguably, then, Germany provides a relatively “pure” case of inequality growth.

²Barth 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.

³Dustmann et al. (2009) also present data for earlier periods and show that, as suggested in Figure 1a, there was a general trend to toward rising inequality even prior to the mid-1990s.

⁴The data underlying this figure are described in detail in Section 3, below.

⁵The data in Figure 1b are based on real wage percentiles for men compiled from the outgoing rotation group files of the Current Population Survey by the Economic Policy Institute (EPI, 2009).

Consistent with this view, the rise in inequality since 1996 appears roughly symmetric, with comparable widening of the gaps in the upper and lower tails of the wage distribution.

[Figures 1a, 1b about here]

A second key feature is the availability of high quality administrative data derived from the German Social Security system. These data include spell-level information on average daily wages, education, and occupation, and are available for a relatively long period, including several years prior to the most recent surge in inequality.⁶

Third, Germany provides an interesting setting for analyzing the evolution of firm-specific heterogeneity and wage-setting more generally. In the past three decades Germany experienced reunification and the aftermath of the fall of communism, undertook multiple labor market reforms, witnessed a sharp decline in collective bargaining coverage, and emerged from a long period of under-performance (e.g., Siebert, 1997) to become one of the most successful economies in the OECD.⁷ There is widespread interest in the source of this recent success, and the lessons it may hold for other countries (e.g., Burda and Hunt, 2011).

To study the changing role of workplace heterogeneity we divide our 25-year sample period into four (overlapping) intervals: a period of relative stability from 1985 to 1991; a transitional period following reunification (1990-1996); a period spanning the wave of reforms initiated in the mid-1990s (1996-2002); and a period covering the most recent decade (2002-2009). In each interval, we fit linear wage models that include additive person and establishment fixed effects as in Abowd, Kramarz and Margolis (AKM, 1999). We then compare the estimates across intervals to assess changes in the structure of wages. By fitting separate models in each relatively short interval we can allow for longer-run changes in the earnings characteristics of individual workers and firms caused by changes in technology, innovations in workplace practices, or other factors.

In an initial methodological contribution we present new diagnostic evidence on the quality of the approximation to the wage structure provided by AKM's additive worker and firm effects model. We find that, in contrast to conventional wage models, the AKM model explains roughly 90% of the rise in wage inequality over our sample period. We also find

⁶By contrast, in the U.S., high quality employer-employee data is only available beginning in the 1990s, the geographic coverage is incomplete, and administrative education records are not available.

⁷For overviews of recent changes in the German labor market see Eichhorst and Marx (2009), Burda and Hunt (2011), and Eichhorst (2012).

that the separability assumptions of the AKM model are nearly (but not perfectly) met in the data. For comparison we fit generalized models that include separate fixed effects for each job (i.e., worker-establishment match) and show that these models provide only a small improvement in explanatory power relative to the AKM specification. Moreover, the difference in residual variance between the match model and the AKM model increases only slightly over time. Thus, a fully unrestricted match model provides almost no additional explanatory power for understanding the rise in wage inequality. We also conduct a series of checks for patterns of endogenous mobility that could lead to systematic bias in the AKM specification and find little evidence of such biases.

Our primary substantive contribution is a simple decomposition of the sources of rising wage inequality in Germany. We find that the increase is attributable to increases in the variability of both the person-component of pay (capturing differences in ability, training, motivation, etc.) and the establishment-level component of pay, coupled with a rise in the assortativeness of the job-matching process that magnifies their joint effect.⁸ In the earliest years of our sample the correlation between the worker- and establishment-level components is close to zero. By the final years, however, the correlation is roughly 0.25, and is clearly discernible in simple plots of their joint distribution. Overall, the rise in the variance of the person component of pay constitutes about 40% of the rise in the variance of wages, the rise in the establishment component constitutes around 25%, and their rising covariance constitutes about a third.

We go on to use our estimated models to decompose the changes in between-group inequality across different education, occupation, and industry groups. We find that nearly all of the increase in the conventional (Mincerian) return to schooling over our sample period is attributable to changes in the association between education and establishment quality. Better-educated workers in West Germany are increasingly concentrated at high-wage establishments, while less-educated workers are increasingly concentrated at low-wage establishments. We find that rising assortativeness between workers and establishments also explains an important share (around 40%) of the growth in inequality across occupations and industries.

Given the increasing variability in establishment-based pay premiums, we then briefly examine some of the proximate channels for this rising dispersion. Stratifying establishments

⁸Recent 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. They also study settings where the rise in cross sectional inequality is relatively modest.

by their “birth” year, we find a trend toward increasing heterogeneity among establishments entering after the mid-1990s, coupled with relatively small changes in the variability of the premiums paid by continuing establishments. The trend in inequality among newer establishments is linked to collective bargaining status: an increasing share of new establishments are outside the traditional sectoral contracting system, and pay relatively low wages. We also examine the correlation between pay policies and establishment and job survival. We find that establishments with higher wage premiums are *more* likely to survive than those with lower wage premiums, consistent with a rent-based explanation for these premiums. Job survival rates are also higher at higher-paying establishments, ruling out a simple compensating differentials explanation for the wage premiums.

Our results suggest that a rising share of wage inequality is attributable to variation in the job opportunities available to a given worker, and that both observed and unobserved dimensions of worker skill are increasingly associated with establishment-specific pay premiums. These findings raise a number of questions regarding the determinants of the aggregate composition of offered jobs and the process by which workers are matched with employers. We conclude with a brief discussion of the implications of our findings for the wage inequality literature and directions for future research.

2 Background - Macro Trends and Institutional Changes

Some of the key events that have affected the German labor market since the early 1980s are summarized in Figure 2, which also shows the evolution of the unemployment rate. A decisive turning point was the reunification of East and West Germany, which culminated with economic integration in June 1990 and full political integration several months later. Reunification and the end of communism in eastern Europe affected the West German labor market in many ways. One channel was migration: approximately 1.7 million former residents of East Germany (about 10 percent of the population) moved to the west (see Burda, 1993 and Wolff, 2009). An even larger inflow (approximately 2.8 million people) of ethnic Germans arrived from Russia and other former East Bloc countries between 1988 and 2000.⁹ These new migrants – many of whom lacked modern training and language skills – arguably contributed to the rise in unemployment in the 1990s (Glitz, 2012).

More subtly, the decision to impose West German wage scales on the far less productive east led to rising pressures on the sectoral bargaining system (see e.g., Hassel, 1999). Tradi-

⁹See Bauer and Zimmerman (1999).

tionally, most West German firms accepted the provisions of the major sectoral agreements negotiated between employer associations and large unions. Unlike Italy or Spain, however, German law does not require firms to follow these centralized agreements (see Ochel, 2003 for an informative overview). Instead, employers can leave the employer association (or refrain from joining in the first place) and either negotiate a workplace-specific agreement or opt out of collective bargaining altogether.

As many observers have noted, the fraction of employees covered by sectoral agreements has fallen since the early 1990s (e.g., Dustmann et al., 2009). The dashed line in Figure 2 shows the fraction of West German workers whose wages are set *outside* the sectoral agreement system, estimated from information collected by the Institute for Employment Research (IAB) from a large panel of establishments (Kohaut and Ellguth, 2008; Ellguth, Gerner, and Stegmaier, 2012). This fraction has risen from 27% in 1995 (with 10% of workers in enterprise-level agreements and 17% with no agreement) to 44% in 2007 (with 7% of workers in enterprise-level agreements and 37% with no agreement).¹⁰

[Figure 2 about here]

A second and related phenomenon is the adoption of “opt-out” or “opening” clauses by firms that have remained part of the sector-level agreement (e.g., Hassel and Rehder, 2001; Heinbach, 2006). These clauses allow employers – or individual plants of a large firm – to depart from the agreement, typically in response to the threat of plant closure or job loss.¹¹ A study by Hassel and Rehder (2001) suggests that about one-half of the top 120 firms in Germany had signed opening clauses by 2000, and that about 43% of their employees were affected by these pacts. Re-opening clauses more often pertain to hours than hourly wages (Heinbach, 2006). Rising dispersion in either hours or hourly pay will lead to rising inequality in earnings per day, which is the measure of pay available in our data. Overall we suspect that falling coverage by sectoral agreements and use of opt-out clauses by employers still

¹⁰As 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 rate shown in Figure 2 assigns a single establishment status to all employees and should be interpreted carefully.

¹¹For 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 opening clauses cutting wages by up to 10% to save jobs (EIRO, 1997b).

in the agreements has reduced the equalizing effect of the traditional collective bargaining system.¹²

By the mid-1990s the unemployment rate in Germany had risen to nearly 10 percent, and 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 from 1 to 2 years, raised the establishment size threshold for dismissal protection from 5 to 10 employees, and reduced the replacement rate for sick leave pay from 100% to 80%. The trend toward liberalization was partially reversed after the election of a left-leaning coalition in 1998.¹³ So-called “marginal jobs” (part-time jobs with low earnings) that had previously been excluded from coverage by Social Security taxes were brought into the tax system in 1999. With the recession in 2001, however, pressures for reform returned, ultimately leading to adoption of the Hartz reforms in 2003-2005. These reforms included major revisions to the benefit program for people who have exhausted regular unemployment benefits (see e.g., Jacobi and Kluge, 2006). The new program (*Arbeitslosengeld II*) provides in-work benefits for employees in low-wage jobs. In addition, the employee share of Social Security taxes for part-time jobs (so-called “mini-jobs”) was eliminated, providing further impetus for the expansion of part-time/low wage jobs.

A final institutional change documented in Figure 2 is the adoption of a minimum wage for construction workers in November 1996. The impetus for this law – the first legally-imposed minimum wage in Germany – was the rising presence of immigrant workers who were eligible to work in Germany but subject only to wage regulations in their home country (see Bachman et al., 2012). The minimum was set at a relatively low level, however, and research by Rattenhuber (2011) and Bachman et al. (2012) suggests it had little effect in West Germany (though a larger impact in the lower-wage East). More recently, minimum wages have been introduced for employees in the cleaning industry and the temporary help industry – see EIRO (2007, 2012).

¹²Many 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.

¹³The 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).

3 Data

We use earnings information from the German Social Security system provided through the Institute for Employment Research (IAB). Employers are required to submit a notice of employment for everyone employed in a calendar year, giving the starting and ending date of their job, total earnings received (censored at the maximum taxable earnings level), the establishment identification number, and other data, including the employee’s education, occupation, and trainee status, and whether the job was part-time or full time. Multiple records are submitted for workers who change job status – e.g., from part-time to full time – or who have interrupted employment spells during a year. Nearly all private sector employees in Germany are subject to Social Security taxation and are included in the data base. The major excluded groups are civil servants and the self-employed.

We limit our attention to *full-time* jobs held by men age 20-60 during the period from 1985 to 2009.¹⁴ We exclude marginal jobs (or “mini-jobs”) that are subject to reduced Social Security contributions¹⁵ and jobs in which the employee is undergoing training. As explained in more detail in the Data Appendix, we sum the earnings received by a given individual from each establishment in each year and designate the one that paid the highest total amount as the main job for that year. Most full time men work at only one establishment in any year (the average is 1.1 per year) and there is no trend in the number of jobs held per year, so the restriction to one job per year is unlikely to be important in explaining trends in wage inequality. 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 use of administrative data allows us to sidestep the nonresponse problems that increasingly plague survey data (Hirsch and Schumacher, 2004; Kline and Santos, 2012). We suspect, however, that our sample restrictions lead to an *understatement* of the overall level of wage inequality among German male workers, since we exclude the lowest wage workers, including trainees, part-timers, and those in mini-jobs. Moreover, since part-time work and mini-jobs are rising in importance, our sample definitions arguably understate the rise in

¹⁴Our focus on full time jobs is driven by the absence of data on hours of work. We exclude females because a relative high fraction work part-time. Anotnczyk, Fitzenberger, and Sommerfeld (2010) show that the male-female wage gap in Germany was quite stable between 2001 and 2006, though both groups were affected by declining collective bargaining coverage and other factors.

¹⁵These jobs were not subject to Social Security taxation prior to 1999, so it is impossible to conduct an analysis over our sample period that includes marginal jobs.

wage inequality.¹⁶ On the other hand, we cannot control for variation in hours of work. To the extent that hours of full time men have become more variable over time, the change in the dispersion of daily pay will overstate the rise in the variability of hourly wages.

The establishment identification numbers (EID's) we use to identify workplaces are assigned by the Social Security system on the basis of ownership, type of economic activity (i.e., sector), and municipality. Consequently, two fast food outlets owned by the same company in the same city are typically classified as a single establishment, whereas a foundry and a fabrication plant owned by the same company in the same municipality are classified as separate establishments. A new EID is issued when a plant changes ownership, and can also be issued for other reasons. Thus, the "death" of an establishment identifier does not necessarily mean that a plant has closed, nor does the "birth" of a new EID necessarily mean that a new plant has opened. (See Benedetto et al., 2007, for a discussion of the same issue in the U.S. context). Using clusters of worker flows between establishments, Schmeider and Hethley (2010) estimate that about one half of EID births and deaths in the IAB data are reorganizations or ownership changes rather than true plant openings or closings.

Since our primary interest is in wage determination, we believe it is appropriate to treat an ownership change as a potential change in the workplace component of pay. A new owner may add incentive pay or bonuses to the structure of compensation. Ownership changes may also be associated with major reforms in the organization of production, such as outsourcing or use of temporary workers. By allowing separate establishment effects for the old and new owners, we take an agnostic position on whether the change has a substantive effect on wages, since it is possible that the effects are the same.

Appendix Table 1 shows some basic characteristics of our data set. We have between 12 and 14 million workers with a wage in any year. Average real annual and daily earnings of the men in our sample rise between 1985 and 1990, but are quite stable over the next 20 years. In contrast, the dispersion in pay – measured by the standard deviation of daily wages – rises dramatically. At the establishment level, there are about 800,000 EID's in each year with at least one full time male employee. The size distribution of establishments (measured by the number of full time male employees) is highly skewed, but fairly stable over time, with only a small decline in the share of very large (over 350 workers) establishments. The median establishment in any year of the sample has 2 workers, but on average a typical worker has

¹⁶Eichhorst and Marx (2009) analyze data from the German Socio-Economic Panel (GSOEP), and show that the incidence of marginal jobs rises from about 2% of adults in the early 1990s to 5% by 2007. Part time employment also rises from 7% to 11% of adults.

about 1,000 (full time male) co-workers. Appendix Figure 1 shows the size distributions in 1985, 1995, and 2008, illustrating both their extreme skew and their stability over time.

An important limitation of our data set is the censoring of earnings at the annual Social Security earnings maximum. Appendix Figure 2 shows the fraction of censored observations in each year of the sample, which varies from 10 to 14 percent. To address the problem of censoring, we follow Dustmann et al. (2009) and use Tobit models (fit separately by year, education level, and age range) to stochastically impute the missing upper tail of the wage distribution. Since our primary wage determination models include person and establishment effects, we impute a censored value in a given year using information on the worker's earnings in other years, and on his co-workers' earnings in the same year. Specifically, the Tobit model for a given year includes the worker's earnings and censoring rate in all other years, as well as the mean earnings and censoring rate of his co-workers in that year. (See the Data Appendix for more detail and Appendix Table 2 for coefficient estimates from several representative models). 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.

The impact of this imputation procedure is illustrated in Appendix Figure 2, which shows the differences in the estimated mean and standard deviation of log daily wages in each year using the imputed values for the censored observations versus using the data with no adjustment for censoring. 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 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 analysis would yield different results if we used a different technique. To address this concern, we present robustness checks based on the subset of workers with apprenticeship training. This group, which includes about 60 percent of the German male workforce, has relatively low censoring rates. Reassuringly, our main conclusions are very similar when we use only the apprentice subsample.

4 Overview of Trends in Wage Inequality

Figure 3a plots four measures of wage dispersion over time: the standard deviation of log wages (including imputed wages for censored observations), the gap in log wages between

the 20th and 80th percentiles, the gap between the 50th and 80th percentiles, and the gap between the 20th and 50th percentiles. We normalize the gap measures by dividing by the corresponding percentile gaps for a standard normal variate.¹⁷ If log wages were normally distributed, the three normalized gaps and the standard deviation would all be equal. Inspection of the figure suggests that log daily wages have fatter tails than a normal distribution, but that the trends in the standard deviation and the normalized 80-20 gap are very similar. In particular, the standard deviation rises by 0.15 from 1985 to 2009 whereas the normalized 80-20 gap rises by 0.16. This similarity is reassuring because the 80-20 gap is unaffected by censoring. A second conclusion, which confirms the visual impression from Figure 1a, is that the rise in lower-tail inequality is slightly bigger than the rise in upper-tail inequality: the normalized 80-50 gap rises by 0.15, whereas the normalized 50-20 gap rises by 0.18. A third conclusion is that the rate of growth in all four measures of inequality accelerated sometime in the mid-1990s. For example, the standard deviation of log wages grew by 0.0023 per year from 1985 to 1996, and 0.0096 per year from 1996 to 2009.

[Figures 3a and 3b about here]

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 the recent rises in wage inequality have occurred *within* conventional skill groups. This is also true in our data, as shown in Figure 3b, which plots the same four measures used in Figure 3a, but applied to the residuals from a standard human capital earnings function fit separately by year.¹⁸ Residual inequality rises somewhat less than overall wage inequality (e.g., the standard deviation of residual wages increases by 0.13 from 1985 to 2009, about *4/5ths* of the rise in overall inequality), but exhibits the same acceleration in trend in the mid-1990s. Interestingly, residual inequality is nearly symmetric, suggesting that the increasing skew in overall wage inequality is largely explained by increasing returns to education and experience.

Explaining the Rise in Residual Wage Inequality

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 geographic areas (Moretti, 2012) occupations (e.g., Autor, Levy and Murnane, 2003; Autor, Katz and Kearney, 2008) or industries (Autor,

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

¹⁸The model include dummies for each level of education and a cubic in potential experience.

Katz, and Krueger, 1998; Bernard and Jensen, 1995). Figure 4 summarizes the contributions of these factors to the rise in German wage inequality. The top line in the figure shows the trend in the standard deviation of residual wage inequality, adjusting for education and experience as in Figure 3b. The other lines show the trends in residual standard deviation, adding controls (with separate coefficients in each year) for Federal State (10 dummies), industry (~ 300 dummies), occupation (~ 340 dummies), and industry \times occupation ($\sim 28,000$ dummies). While 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 6, below, to examine in further detail the trends in between-occupation and between-industry inequality in light of our econometric decomposition of wage inequality into person and establishment effects.

[Figure 4 about here]

In contrast to the limited effect of industry and occupation controls, the bottom line in Figure 4 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 root mean squared error (RMSE) of the regression model rises by only 0.05 between 1985 and 2009, compared to the 0.13 rise in the RMSE of the baseline model that controls for education and experience.

Although this contrast suggests that workplace heterogeneity may play some role in explaining the rise in German wage inequality, we emphasize that these wage models do not control for unobserved worker abilities. The rise in the explanatory power of the workplace effects may simply capture the non-random sorting of workers to establishments. Specifically, even if all workers receive the same wage regardless of where they work, one could still observe significant and increasingly important workplace effects if workers at the same establishment have similar unobserved abilities, and if the returns to these abilities are rising over time, or if the degree of sorting across establishments is rising over time.

Measures of Labor Market Sorting

Figure 5 shows that two measures of workplace sorting are indeed rising over time in West Germany. The first is an index of the degree of sorting of workers of different education

¹⁹A 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.

levels to individual workplaces. This measure consists of the coefficients b_t from a series of regressions of the average level of schooling at worker i 's establishment in year t (\bar{S}_{it}) on his own schooling (S_i):

$$\bar{S}_{it} = a_t + b_t S_i + e_{it}.$$

As noted by Kremer and Maskin (1996), b_t , can range from 0 (no sorting) to 1 (perfect sorting).²⁰ Over our sample period the value of b_t has increased by about 40 percent, from 0.34 to 0.47.

Our second measure of sorting examines the degree of occupational segregation across workplaces. Specifically, we divide occupations into 10 (approximately) equally sized groups, based on mean wages of full time male workers in the occupation in 1985-1991.²¹ We then compute Theil indices of segregation for the decile groups across establishments (Theil and Finezza, 1971).²² 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. Over our sample period the index rises by roughly 15 percent from 0.46 to 0.53. This rise is somewhat more nonlinear than was the case for the educational sorting index, with a rapid acceleration in the early 1990s and deceleration in the early 2000s. Together, the results in Figure 5 suggest that increased sorting could be at least part of the explanation for the increasing importance of workplace effects in a wage model.

[Figure 5 about here]

An Event Study Analysis

To investigate whether sorting is likely to explain the bulk of the workplace component of wages, Figures 6a and 6b provide simple event-study analyses of the wage effects of job

²⁰It is straightforward to show that b_t measures the fraction of the variation in schooling attainment that occurs between establishments.

²¹For example, occupation group 1 contains all three-digit occupations with mean wages in the bottom decile of occupational wages.

²²The Theil index may be written:

$$H = 1 - \frac{\sum_j \frac{N_j}{N} \sum_m \pi_{jm} \log(\pi_{jm})}{\sum_m \pi_m \log(\pi_m)}$$

where π_m is the employment share of occupation group m , π_{jm} is the employment share of occupation group m at firm j , and $\frac{N_j}{N}$ is the employment share of firm j . The formula is implemented using the definition $0 \cdot \log 0 = \lim_{\pi \rightarrow 0} \pi \ln \pi = 0$. Note that if $\pi_{jm} = \pi_m$ for all j, m that $H = 0$. Conversely, when each firm has only one occupation group, $H = 1$.

to job transitions in the first (1985-1991) and last (2002-2009) intervals in our sample. In both cases, we examine wages in the two years before and two years after a job change for workers moving between establishments with different mean pay levels. If between-plant wage variation is primarily driven by variation in unobserved worker quality, transitions between high and low wage plants should not be associated with major wage changes.

To avoid selecting on the job-changer's own wage we classify jobs based on the mean wages of his co-workers, omitting the wage of the individual being studied (i.e., using the "leave-out mean" wage).²³ Specifically, we begin by calculating the distribution of mean co-worker wages across all person-year observations in a given time interval. For each job changer, we then classify the old job based on the quartile of its mean co-worker wage in the last year at the job. We similarly classify the new job based on the quartile of its mean co-worker wage in the first year on the job. We divide job to job transitions into 16 cells based upon the co-worker wage quartile of the origin and destination job. For each transition category we then calculate mean worker wages in the years before and after the job change.²⁴ For simplicity, the figures present wage profiles for people leaving quartile 1 jobs (with the lowest co-worker wages) and quartile 4 jobs (with the highest co-worker wages). Appendix Table 3 provides a complete listing of mean wages before and after the job change for each of the 16 cells.

[Figures 6a,b about here]

Notice first that there is clear evidence of permanent heterogeneity between different mover groups: for example, the average wages of workers in quartile 4 jobs who move to quartile 1 jobs are lower than those who move to quartile 2 jobs, with similar patterns for the other mobility groups. However, the data also provide clear evidence that transitioning between low and high wage establishments has important effects on compensation. While workers moving between quartile 1 jobs have relatively low but constant wages, quartile 1 workers who move to jobs with higher paid coworkers experience wage increases, with the largest increases for those who move to quartile 4 jobs. Likewise, movers from 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 the largest losses among those moving to quartile 1 jobs.

²³In this exercise, we drop any observations at establishments with a single employee.

²⁴Note that we require each lost job to have been held for at least 2 years, and each new job to be held for at least 2 years. Since the sample period includes 8 years, some people can appear in the event study more than once.

Comparing Figures 6a and 6b, it is clear that the size of the wage gains and losses associated with job to job transitions grew dramatically over the range of our study. This impression is confirmed by the estimates in Appendix Table 3. In the 1985-1991 interval, a transition from the bottom to the top quartile was associated with a trend-adjusted wage increase of roughly 23 log points, while in the 2002-2009 interval, the same transition was associated with a 36 log point increase. Likewise, in the 1985-1991 interval, a transition from the top to the bottom quartile was associated with a trend-adjusted wage drop of 22 log points, while in the 2002-2009 interval, the same transition yielded a 48 log point drop. This striking growth in the magnitude of the wage changes associated with firm-to-firm transitions is a key finding, and underlies our results in Section 6 on the growing role of establishment heterogeneity in wage inequality.

Another remarkable feature of Figures 6a and 6b is the approximate symmetry of the wage losses and gains for those who move between quartile 1 and quartile 4 establishments. 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.²⁵ Though not shown in the Figures, the gains and losses for other mover categories exhibit a similar degree of symmetry (see Appendix Table 3), particularly after adjusting for trend growth in wages. Note as well that moving from a bottom quartile to a top quartile firm results in mean wages roughly equal to those of a worker at a top quartile firm prior to transitioning to a bottom quartile firm. Thus, among workers in a particular mobility class (e.g., those who worked at both a top and bottom quartile firm), the establishment effect for the current job explains most of the wage differences between top and bottom quartile workers.

A final important feature of the wage profiles in Figures 6a and 6b is the lack of an Ashenfelter (1978) style dip in the wages of workers who move from higher quartile jobs to lower quartile jobs.²⁶ Nor is there any indication of a positive transitory “blip” in the

²⁵Notice that if the mean log wage paid to worker i at establishment j can be written as $m_{ij} = \alpha_i + \psi_j + \mu_{ij}$, where μ_{ij} is a random error, then the average wage gain for moving from establishment j to establishment 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.

²⁶Ashenfelter (1978) noted that participants in job training programs were likely to have experienced a transitory dip in earnings in the year prior to entering the program. Our setting is somewhat different from Ashenfelter’s not only because we are studying job to job transitions, but because we have spell level data on average daily wages. Unlike in Ashenfelter’s study, changes in the number of days worked at a constant wage will not affect our estimates. And if job transitions are preceded by wage changes, we ought to have substantial power to detect them because our average daily wage measure in the year prior to the transition contains only the days over which the individual worked at the preceding job – which may be substantially less than the entire year.

year before a move from a lower quartile job to a higher quartile job. Taken together with the approximate symmetry of the wage transitions noted above, these findings provide suggestive evidence that the wages of movers may be well approximated by the combination of a worker component, an establishment component, and a time varying residual component that is uncorrelated with mobility.

5 Methods

With this background, we turn to a discussion of our econometric methods for disentangling the workplace component of wages from the component associated with worker abilities. We begin with some notation. In a given time interval, our data set consists of N^* person-year observations on N workers and J establishments. Following Abowd, Kramarz, and Margolis (1999) we index the assignment of workers to establishments via the function $\mathbf{J}(i, t)$ which yields the identity of the establishment associated with worker i in year t .

Within each time interval, we model the log wages y_{it} of individual i in year t as the sum of a time-invariant worker component α_i , a time-invariant establishment component $\psi_{\mathbf{J}(i,t)}$, a linear index of time-varying observable characteristics $x'_{it}\beta$, a mean-zero random match component $\eta_{i\mathbf{J}(i,t)}$, a unit root component of individual wages ζ_{it} , and a mean-zero transitory error ε_{it} :

$$\begin{aligned} y_{it} &= \underbrace{\alpha_i + \psi_{\mathbf{J}(i,t)} + x'_{it}\beta}_{\text{fixed effects}} + \underbrace{\eta_{i\mathbf{J}(i,t)} + \zeta_{it} + \varepsilon_{it}}_{\text{random effects}} \\ &= \alpha_i + \psi_{\mathbf{J}(i,t)} + x'_{it}\beta + r_{it} \end{aligned} \tag{1}$$

The person effect α_i captures the portion of an individual's earnings power that is fully portable across employers. Much of the variation in α_i presumably derives from worker productivity, but it may also reflect heterogeneity in bargaining ability, labor market discrimination, or other factors. The establishment effect ψ_j captures a proportional pay premium (or discount) that is common to all employees at workplace j . This premium may reflect rent sharing, compensating differentials, or other differences across establishments in their compensation policies. The vector of time-varying characteristics x_{it} captures changes in the portable component of an individual's earnings power. We include in x_{it} an unrestricted set of year dummies and quadratic and cubic terms in age, all fully interacted with educational attainment.²⁷ Thus, $x'_{it}\beta$ captures both lifecycle effects and changes over time in the relative

²⁷We omit the linear term in age as it is not separately identified from the year effects.

pay of different age/education groups. Because our estimation samples consist of relatively short intervals, the importance of these time varying factors is likely to be smaller than in studies focusing on career development (e.g., Keane and Wolpin, 1997; Topel and Ward, 1992).

The match effect η_{ij} allows for a time-invariant wage premium (or discount) for individual i at establishment j , relative to the baseline level $\alpha_i + \psi_j$. We assume that η_{ij} has mean zero for every person in every interval and every establishment in every interval. One source of match heterogeneity in wages is complementarity between the skills of the worker and the needs of the firm. To the extent that the worker has significant bargaining power, this complementarity will be rewarded in the form of higher wages. Other factors that affect an individual's average pay while working for a given employer, such as initial placement in the pay grid or other random factors, will also be included in η_{ij} .

The unit root component ζ_{it} captures drift in the portable component of the individual's earnings power. Innovations to this component could reflect employer learning, human capital accumulation, health shocks, or the arrival of outside offers which, in some models (e.g., Postel-Vinay and Robin, 2002), may bid up the offered wage at the current job and other potential jobs. By definition, ζ_{it} has mean zero for each person in each interval but contains a unit root. The potential presence of such a component motivates in part our decision to estimate worker-firm models over relatively short time intervals, during which the variance of ζ_{it} is limited. Finally, the transitory component ε_{it} represents any left-out mean reverting factors.

It will prove useful to consider the corresponding matrix formulation of this model as it clarifies our discussion of identification and estimation. Let y denote the stacked $N^* \times 1$ vector of wages sorted by person and year, D an $N^* \times N$ design matrix of worker indicators, F an $N^* \times J$ design matrix of firm indicators, X an $N^* \times K$ matrix of time varying covariates, and $r = \eta + \zeta + \varepsilon$, an $N^* \times 1$ vector of composite errors. Our model in (1) can be written 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 normal equations:

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

A unique solution to these equations requires that the matrix $Z'Z$ have full rank. This in turn requires a linear restriction on the establishment effects within each “connected set” of establishments – that is, the set of establishments connected directly or indirectly via worker mobility.²⁸ To simplify estimation, we restrict our analysis to the largest connected set of establishments in each time interval of interest, which turns out to include more than 97% of workers and 90% of establishments (see below). Within this connected set we omit the last establishment dummy, so that the establishment effects are measured relative to establishment J . The computational 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, Creedy, and Kramarz, 2002) which solves for the vector of coefficients ξ without actually inverting the matrix $Z'Z$.

Threats to validity

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

$$E[D'r] = 0, E[F'r] = 0, E[X'r] = 0. \quad (4)$$

The assumption of orthogonality between the composite error r and the time-varying x 's is standard. Moreover, under our assumptions on the means of η_{ij} , ζ_{it} , and ε_{it} , $E[D'r] = 0$. Thus the key concern for identification is whether the composite error is orthogonal to the matrix of establishment identifiers F . A sufficient condition is that establishment-to-establishment mobility patterns are independent of $(\eta, \zeta, \varepsilon)$ – a so-called “exogenous mobility” assumption. In this section we discuss the deviations from full independence suggested by different models of wage determination and mobility, and the likely impact of these deviations for OLS estimates of the AKM model.

By the Frisch-Waugh-Lovell theorem the estimates of ψ obtained from application of least squares to (2) are equivalent to those obtained by least squares estimation of the following equation:

$$y - \bar{y} = (F - \bar{F})\psi + (X - \bar{X})\beta + r - \bar{r}$$

where variables with overbars represent person-averages.²⁹ Bias in the establishment effects will arise if $r - \bar{r}$ is correlated with the columns of $F - \bar{F}$ – i.e. if the errors predict *particular*

²⁸See Abowd, Kramarz, and Margolis (1999) and Abowd, Creedy, and Kramarz (2002) for discussion.

²⁹That is, for any variable V , $\bar{V} \equiv D(D'D)^{-1}D'V$.

firm to firm transitions. If the variation in r_{it} merely predicts separations, but not particular types of transitions, there is no necessary bias in the estimated establishment effects.

To build intuition, consider the case where there are only two establishments, two time periods, and no covariates other than a dummy for the second time period with associated coefficient β_2 . Now our estimates of the establishment effects coincide with those from the first differenced specification:

$$\Delta y_{it} = \beta_2 + \psi_1 \Delta F_{it}^1 + \Delta r_{it}$$

where the Δ operator denotes first differences, F_{it}^1 is an indicator for working at establishment 1, and (per our earlier convention) we have normalized establishment 2's effect (ψ_2) to zero. The OLS estimator of ψ_1 is simply the mean wage change associated with workers joining establishment 1, minus the mean wage change associated with workers leaving firm 1. Suppose that r_{it} predicts mobility in the sense that workers passed over for promotions are more likely to separate from their current job. If workers at both establishments are equally likely to be passed over for promotions, then Δr_{it} will not predict the direction of flows and hence Δr_{it} will not be correlated with ΔF_{it}^1 . Thus, OLS is robust to a particular form of endogenous mobility.

To create bias, earnings innovations need to predict particular transitions. There are many ways that such forms of endogeneity could arise. We consider three mechanisms to be of particular interest, each corresponding to one of the components of r . First, establishments may be subject to transitory demand shocks that induce a group-level component in wages (i.e., ε_{it} in (1) might have an establishment \times year component). If workers tend to transition between tightly connected networks of establishments, then a bad year for a particular establishment may predict flows to a particular set of destinations. If this is so, establishment effects at those destinations will be overstated relative to the establishment that experienced the negative shock.³⁰ Note that under such a sorting mechanism, we should expect mobility to be preceded by a drop in wages. As noted in the discussion of Figures 6a/6b, however, such patterns do not seem to be present in the data. We present additional evidence to this effect later in the paper.

A second source of bias could arise if workers systematically sort to firms based upon the permanent match component of wages, η_{ij} . If workers tend to gravitate to jobs where η_{ij} is larger, the average wage of the workers actually observed at establishment j will include a

³⁰Over a long enough horizon these good and bad shocks to firms ought to average out to zero and the importance of such biases will diminish.

sorting bias component of the form that arises in a standard Roy (1951) model of sectoral choice.³¹ Substantial sorting based on the match effects changes the interpretation of the estimated establishment effects, since the impact of transitioning between establishments depends upon the comparative advantage of the individuals making the transition.

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 should expect to see that the (trend adjusted) wage *gains* for workers who move from one establishment to another are quite different from the wage *losses* for those who move in the opposite direction. In particular, in the two firm / two period case, the expected wage change for workers who move from establishment 1 to establishment 2 is:

$$E[y_{it} - y_{it-1} | i \text{ moves from 1 to 2}] = \beta_2 - \psi_1 + E[\eta_{i2} - \eta_{i1} | i \text{ moves from 1 to 2}],$$

while the expected wage change for workers who move from establishment 2 to establishment 1 is

$$E[y_{it} - y_{it-1} | i \text{ moves from 2 to 1}] = \beta_2 + \psi_1 + E[\eta_{i1} - \eta_{i2} | i \text{ moves from 2 to 1}].$$

By contrast, under our maintained assumptions, the expected wage changes are $\beta_2 - \psi_1$ and $\beta_2 + \psi_1$, respectively. As the importance of the match components increase, the sorting bias terms $E[\eta_{i2} - \eta_{i1} | i \text{ moves from 1 to 2}]$ and $E[\eta_{i1} - \eta_{i2} | i \text{ moves from 2 to 1}]$, both of which are positive, will dominate, and symmetry (about the aggregate trend β_2) will be violated. We have already seen from the simple event studies in Figures 6a and 6b (and the mean wage changes in Appendix Table 3) 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, which casts doubt on the importance of sorting based upon comparative advantage. We examine this question in more detail later by examining corresponding transitions from establishment with relatively high and relatively low estimated establishment effects, where we again find approximately symmetric effects.

Second, if match effects are important, a fully saturated match-effects model that includes a separate dummy for each job ought to fit the data much better than our additively separable baseline model. Specifically, the root mean squared error (RMSE) from the AKM specification should be much larger than the RMSE from a model with unrestricted job effects. As we show below, a job match model has a better fit statistically, but the reduction

³¹See French and Taber (2011) for a detailed discussion of Roy-type models and references to the related literature.

in RMSE is small (11-15%), implying that the magnitude of the match component is small (the implied standard deviation of η_{ij} is in the range 0.06-0.08).³² Likewise, any specification errors in the additively separable model in (1) – would be expected to become larger over time if workers and establishments are becoming increasingly heterogeneous. In fact, we find the gap in fit between the AKM model and an unrestricted match effects model is relatively stable over time, which is consistent with our characterization of the match component as an uncorrelated random effect.

A third form of endogenous mobility could arise if drift in the expected wage a person can earn at all jobs (i.e., the shocks to the unit root error component ζ_{it}) predicts firm-to-firm transitions. For example, in a learning model with comparative advantage (e.g., Gibbons et al., 2005) some components of worker ability are revealed slowly over time, and these components are valued differently at different establishments.³³ Under such a scenario, workers who turn out to be more productive than expected will experience rising wages at their initial employer, and will also be more likely to move higher-wage establishments (e.g. firms specializing in skilled workers). Likewise workers who 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 in an AKM-style model, as the drift component ζ_{it} in wages is positively correlated with the change in the establishment effects. As noted in the discussion of Figures 6a and 6b, however, we do not see systematic wage trends in the years prior to a move for workers who move to better or worse jobs, casting doubt on the importance of learning as a major source of bias in our estimates.³⁴

The drift component ζ_{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. Importantly, however, offer shopping cannot explain the patterns of wage losses

³²Note that 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, Deicienti, and Maida, 2010; Cahuc, Postel-Vinay and Robin, 2006; Carlsson, Messina, and Skans, 2011).

³³Gibbons 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.

³⁴Note that “lumpy” 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, existing estimates of the pace of employer learning suggest it usually occurs over a horizon of several years (Lange, 2007).

experienced by workers who move to lower-wage establishments.³⁵ Nor can it explain the symmetry in the wage gains and losses associated with transitions between high and low paying establishments exhibited in Figures 6a and 6b.

6 Results

We begin our presentation of results with an overview of the samples in each of our four time intervals. Columns 1-4 of Table 1 show the number of person-year observations, the number of individuals, and the mean and standard deviation of log wages within each interval. In each interval we have about 85-90 million person-year observations on wages for about 17 million individual workers. As expected from the patterns in Figure 3a, the standard deviation of wages rises substantially from about 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.

[Table 1 about here]

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 97% of person-year observations in each interval, and about 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 2 summarizes the estimation results for each of the four intervals in our analysis. As shown in the top two rows of the Table, we estimate roughly 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

³⁵Wage losses are possible in such models but should not easily be predicted by the average coworker wage at the origin and destination establishment.

of the time-varying covariate index ($x'_{it}\widehat{\beta}$), and the correlations between these components.³⁶ We also report the root mean squared error (RMSE) from the model and the adjusted R^2 statistic, both of which adjust for the relatively large number of parameters being estimated in our models.

[Table 2 about here]

The estimation results in Table 2 point to several interesting conclusions. First, the person effects and the establishment effects both become more variable over time. Second, the correlation between the person and establishment effects 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 third observation is that the residual component of wages is relatively small in magnitude and becomes only slightly more disperse over time. Of the 13 log point rise in the standard deviation of wages across the intervals, only 1.5 log points remain unexplained. The relatively high explanatory power of the AKM model is reflected in the adjusted R-squared statistics, which increase from 90% to 93% across the intervals.

The explanatory power of the model is depicted graphically in Figure 7, which shows a relatively stable RMSE for the AKM model over the four intervals. For comparison purposes we also graph the RMSE from a model with unrestricted match effects – that is, a model where each person-establishment combination receives a separate dummy variable. As shown in the last two rows of Table 2, this model fits somewhat better than the baseline AKM model, confirming the presence of statistically significant match effects. Importantly, however, the match effects model explains little more of the rise in inequality than our baseline model,

³⁶These moments are estimated across the person-year observations in the data set. For example, the standard deviation of the estimated person effects is calculated as:

$$S_{\widehat{\alpha}} \equiv \sqrt{\frac{1}{N^* - 1} \sum_i \sum_t d_{it} (\widehat{\alpha}_i - \bar{\widehat{\alpha}})^2}$$

where d_{it} is an indicator for individual i having a valid wage observation in year t and $\bar{\widehat{\alpha}} \equiv \frac{1}{N^* - 1} \sum_i \sum_t d_{it} \widehat{\alpha}_i$. Thus, our variability measure is weighted by the number of years each worker has a valid wage in the interval (which averages about 5 years, but can range from 1 to 8). Likewise the standard deviation of the estimated establishment effects is:

$$S_{\widehat{\psi}} \equiv \sqrt{\frac{1}{N^* - 1} \sum_i \sum_t d_{it} (\widehat{\psi}_{J(i,t)} - \bar{\widehat{\psi}})^2}$$

where $\bar{\widehat{\psi}} \equiv \frac{1}{N^* - 1} \sum_i \sum_t d_{it} \widehat{\psi}_{J(i,t)}$, which weights each establishment by the number of person-years of wage data observed at that establishment.

suggesting that rise in the residual variance in both models is due to a small increase in the transitory component of earnings.

[Figure 7 here]

The gap in RMSEs between the baseline model and the match effects model also rises only slightly over time. Based on this gap, the implied standard deviation of the match effects rises from 0.060 to 0.075. This relative small change is consistent with the hypothesis that the match effects are truly (uncorrelated) random effects as in (1). If instead they were specification errors generated by incorrectly assuming additivity of the person and establishment effects we would expect the fit of the match effects model to improve relative to the baseline model over time as the variance of the person and establishment effects increased.

Additional insight into the nature of the match-specific error components comes from examining the errors for different groups of workers at different establishments. If the match effects are truly random errors in the wage determination process, their mean should be zero for all different types of matches. The first order conditions for the OLS estimator ensure that the residuals for each worker and for each establishment have mean zero. However, there is no such mechanical restriction on *interactions* between different groups of workers and establishments, providing the basis for a test of the additivity assumptions in the AKM model.

To search for neglected interactions between worker and establishment characteristics, we divide the estimated person and establishment effects in each interval into deciles, and compute the mean residual in each of the 100 crossed person \times establishment decile cells. Figures 8a and 8b show the mean residuals in each cell using data from intervals 1 and 4. In all cases the mean residuals are very small, and uniformly less than 1% in magnitude.³⁷ 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 relatively little effect on our basic conclusions.

³⁷We 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.

[Figures 8a, 8b here]

A second and related diagnostic focuses on the ability of our model to capture the wage dynamics associated with job changes. If mobility is associated with important selection on transitory wage components we ought to see a dip in wages prior to firm transitions. Figure 9a presents an event-study analysis for job transitions in the 2002-2009 period that is similar to the one in Figure 6, but classifies jobs based on the quartile of the estimated establishment effects for the origin and destination workplaces. As in Figure 6, there is little evidence of transitorily low (or high) wages in the year just before a job change. Moreover, the magnitude of the average wage gain for those who move from a quartile 1 to a quartile 4 establishment is very similar to the magnitude of the average wage loss for those who move in the opposite direction.

[Figures 9a, 9b here]

The wage changes shown in Figure 9a for workers who move between quartile 4 and quartile 1 establishments are large (40-45%). This reflects the rather large (0.23) estimated standard deviation of establishment effects in interval 4 of our sample. Moving from an establishment that is one standard deviation above average in pay to one that is one standard deviation below average generates an approximately 46 percentage point change in wages.

Figure 9b plots the mean wage residuals from the AKM model for the same transitions shown in Figure 9a (note the much narrower range of the y-axis). Examination of this figure shows that our model predicts the mean wage changes associated with quartile to quartile firm transitions relatively well, with prediction errors of less than 3%. 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-squared statistics for the model.

Decomposing changes in the structure of wages

We now turn to the implications of the estimated models in Table 2 for understanding the rise in wage inequality over time. As noted, the estimated person and establishment effects both exhibit increasing variability over time. Perhaps even more striking is the rise in the

correlation between these effects.³⁸ This increase suggests a fundamental change in the way workers are sorted to workplaces.³⁹ Figures 10a and 10b provide a graphical illustration of this phenomenon by plotting the joint distributions of the person and establishment effects in intervals 1 and 4 (as in Figures 8a and 8b we divide the two sets of effects into deciles). In interval 1 there is only limited evidence of sorting, with the largest departure from randomness for workers in the bottom decile of person effects, who are disproportionately likely to work at decile 1 establishments. By interval 4, however, the degree of assortative matching has increased dramatically, as evidenced by the relatively high concentrations of high-wage workers at high-wage establishments, and of low-wage workers at low-wage establishments.

[Figures 10a, 10b here]

To quantify the separate contributions of rising dispersion in person and establishment effects, and increases in assortative matching, we conduct a simple variance decomposition of the form:

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

in each interval. Table 3 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. Thus all three components contributed to the rise in inequality. The trends in the various components are illustrated in Figure 11, which gives a visual impression of the relative shares of each term.

[Table 3 here]

³⁸As described in the Appendix, the estimated variance of the person and establishment components will tend to be inflated by sampling error while the covariance between them will tend to be understated. However, we suspect based upon the stable residual variation of our model that the trends in our estimates are unlikely to be driven by this bias. In the next section we consider between-group measures of covariance which avoid problems due to sampling error.

³⁹It 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 (2011), the correlation between worker and establishment wage effects need not correspond to the correlation between worker and establishment productivity.

[Figure 11 here]

Table 3 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 variability of the person effects.

Robustness Check: Men with Apprenticeship Training Only

As discussed earlier, one 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.

Appendix Tables 4 and 5 summarize the estimation results for this subsample, using the same format as Tables 2 and 3. 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 variability of the person-specific component of pay, a rise in the variability 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.

7 Decomposing Between-Group Wage Differentials

Germany, like the U.S., has experienced substantial increases in the wage gaps between groups of workers with different demographic and educational characteristics. The model in (1) allows a simple decomposition of between-group wage gaps into a component attributable to the time invariant characteristics of workers, a component attributable to the time invariant characteristics 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:

$$E[y_{it}|G_i = g] = E[\alpha_i|G_i = g] + E[\psi_{J(i,t)}|G_i = g] + E[x'_{it}\beta|G_i = g].$$

Using this framework, the change in the mean wage difference between any two groups g_1 and g_2 can be decomposed into a component due to differential changes in the permanent skill characteristics of workers in the two groups, a component due to differential changes in the average establishment wage premium earned by the two groups, and a component due to different evolutions of their time varying characteristics. Such a decomposition is particularly interesting in light of the evidence provided so far of increased assortativeness in the matching of workers to workplaces, which may strongly influence between group wage gaps.

Education

Table 4 conducts such a decomposition for changes in the relative mean wages of education groups in West Germany between the first and fourth intervals. Column 1 of the table shows the change in the relative wages of each group relative to men with apprenticeship training: note that the less educated groups' relative wages have fallen, while the more educated groups' have risen. Columns 2 and 3 show the relative changes in mean person and establishment effects (again, relative to apprentice-trained workers), while column 4 shows the remaining component. The striking conclusion from this decomposition is that nearly all the fall in relative wages of workers without post-secondary training, and most of the rise in relative mean wages of workers with university-level training is due to changes in their relative establishment effects. 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 to changes in the value of skills that are fully

portable across jobs.

[Table 4 here]

Figure 12 provides an alternative depiction of this decomposition. The horizontal axis shows the mean person effect in the education group in an interval relative to apprentices, while the vertical axis shows the mean establishment effect in the education group in an interval relative to apprentices. We plot the data for interval one (squares) and interval four (diamonds), along with OLS fits summarizing the degree of sorting of higher education groups to better-paying establishments in the two intervals. Notice first that the variation across groups in mean person effects is nearly constant across intervals, with only a slight increase in the mean person effect of university graduates. The variation in mean establishment effects, however, widens substantially between interval 1 and interval 4, implying a rise in education-related wages gaps over time.

[Figure 12 here]

An alternative decomposition, first proposed by Mundlak (1978), provides complementary evidence on these patterns while avoiding the complexity of the AKM estimation methodology. Specifically, consider a series of cross sectional regressions of earnings (y_{it}) on year of schooling attainment (S_i) of the form:

$$y_{it} = c_t + r_t S_i + u_{it}, \quad (5)$$

where the error u_{it} includes an establishment-level component:

$$u_{it} = \mu_{\mathbf{J}(i,t)t} + v_{it}.$$

The term $\mu_{\mathbf{J}(i,t)t}$ captures any general effect of working at establishment j in period t that is shared by all workers, regardless of education. Ignoring this group component in estimation of (5) will yield a population regression coefficient:

$$r_t^M \equiv \frac{Cov(y_{it}, S_i)}{Var(S_i)} = r_t + \lambda_t b_t,$$

where $b_t \equiv \frac{Cov(\bar{S}_{it}, S_i)}{Var(S_i)}$ is the educational sorting index plotted in Figure 5, and $\lambda_t \equiv \frac{Cov(\mu_{\mathbf{J}(i,t)t}, \bar{S}_{it})}{Var(\bar{S}_{it})}$ is the coefficient from a regression of the establishment effect on mean schooling

at the establishment. Thus, the standard (Mincerian) return to schooling in each year (r_t^M) consists of three distinct components: the return to schooling conditional on job quality (r_t), the return to mean establishment schooling (λ_t), and the association between worker and establishment schooling (b_t). Using the arguments in Mundlak (1978), we may directly estimate each of these components by fitting regressions of the form:⁴⁰

$$y_{it} = c_t + r_t S_i + \lambda_t \bar{S}_{it} + \omega_{it}.$$

As shown in Figure 13 (and previously in Figure 5), the educational sorting index b_t is rising over time. Moreover, the return to mean establishment schooling λ_t is also rising over time, from a value of around 0.04 in the mid-1980s to a value of 0.11 in the late 2000s.⁴¹ In contrast, the return to individual schooling r_t is actually decreasing slightly over time. Thus, the 20% rise in the Mincerian return to schooling for West German men between 1985 and 2009 is driven by a combination of increasing returns to working with well-educated co-workers, coupled with a rise in the segmentation of education groups across establishments. Confirmation of the findings in Table 4 with this simple alternative decomposition gives us further confidence that an important share of rising wage inequality in Germany is due to increasing workplace heterogeneity in pay, coupled with rising assortativeness in the matching of workers to establishments.

[Figure 13 here]

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.⁴² Figure 14a shows the evolution of this structure in West Germany between the late 1980s (interval 1) and the mid/late 2000’s (interval 4). Overall there is a widening of wage differences across occupations, as reflected by the slope of the line relating mean wages in interval 4 to mean wages in interval 1, which is 1.14 (standard error = 0.03) from unweighted OLS, and 1.27 (0.02) from a regression weighted by initial employment shares. Interestingly, the relationship

⁴⁰It can be shown that the OLS estimator of r_t in this specification coincides numerically with that resulting from estimation of (5) including establishment fixed effects. Thus, our estimates of r_t measure the “within-establishment” returns to schooling.

⁴¹We include a cubic in experience in all the models, so the returns to education correspond to the conventional Mincerian returns which hold constant labor market experience.

⁴²See Acemoglu and Autor (2011) for a review of the related literature.

between wages in the two intervals is, if anything, slightly concave, reflecting declines in real wages for the lowest-wage occupational groups.⁴³

[Figure 14a here]

Our models provide a new perspective on the changing occupational wage structure by allowing us to decompose the changes in between-occupation wage variation into components due to changes in the variability of workers' characteristics, changes in the variability of establishment characteristics, changes in the sorting of occupation groups to workplaces, and changes in the impacts of the time-varying characteristics. The results are summarized in Panel A of Table 5 which reports the standard deviations across occupations of the various wage components, the correlation of average person and establishment effects, and the variance shares in each interval. In the last column of the table we also show the changes in the variances between interval 1 and interval 4, and the shares of the overall change in variance accounted for by each component.

The estimates suggest that the largest share of the rise in between-occupation inequality (42%) is attributable to the rising correlation between person effects and establishment effects.⁴⁴ That is, higher-paid occupations are increasingly concentrated at high-wage establishments, while lower-paid occupations are increasingly concentrated at low-wage establishments. This result complements our earlier finding in Figure 5 that occupational skill groups are increasing segregated at particular establishments. As we discuss in more detail in the next section, one partial explanation for this increasing segregation between high-wage and low-wage occupations is outsourcing, which often involves firms replacing in-house jobs like janitorial services with outside contractors who are classified as working at a separate establishment. This increasing correlation is illustrated in Figure 14b, which plots the mean establishment effects in an occupation against the mean person effect in our first and last intervals.

[Table 5, Figure 14b here]

⁴³In an unweighted regression a quadratic term in initial-period wages is not significant. In a weighted model, however, the quadratic term is negative and significant ($t=4.7$).

⁴⁴Because the correlations are calculated across averages of groups of workers in each occupation they do not suffer from the sampling error-induced biases that affect the correlations at the individual level, discussed in the Appendix.

Industry

The bottom panel of Table 5 provides a parallel decomposition of the variability in mean wages across industries. 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 between-industry wage variation, variation in establishment effects explains a similar share, and their covariance adds another 20%. As shown in column 5 of the table, rising dispersion in worker quality explains a sizeable share (about 44%) of the rise in the variance of wages across industries, while rising dispersion in establishment 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 high-wage plants.

8 Rising Establishment Heterogeneity: Cohort Effects and Collective Bargaining Status

The increasing variability in establishment-level pay premiums between the first and last intervals of our sample raises the question of whether this rise was 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 investigate this question, we assigned each establishment a birth year based on the first year it appears in our sample.⁴⁵ We then calculated a variety of statistics for establishments in different birth cohorts. As shown in Appendix Table 6, about 950,000 establishments are present in the first year of our sample (1985), and another 100,000 or so are born each year. Mean wages of workers at new establishments move in parallel to average wages for all workers from 1986 to 1996, but exhibit a downward relative trend after 1996. The standard deviation of wages among workers at new plants is also relatively stable until the late 1990s, then begins to rise.

The fall in average wages at new establishments and the rise in wage dispersion among employees at these plants are both suggestive of an important cohort pattern in the evolution

⁴⁵As discussed in Section 3, the “birth” of an establishment may correspond to the sale of an existing plant from one owner to another.

of establishment pay inequality. These simple comparisons, however, do not control for potential changes in the characteristics of the workers at new establishments. To address this concern we plot the standard deviations of the estimated establishment effects by birth cohort in Figure 15. Note that establishments born relatively early in our study period are assigned up to four different establishment effects, depending on how long they survive. For example, establishments born in 1988 have a 0.26 standard deviation in their interval-1 effects (estimated using data from 1985 to 1991), a 0.22 standard deviation in their interval-2 effects (estimated using 1990-96 data), a 0.22 standard deviation in their interval-3 effects (estimated using 1996-2002 data), and a 0.24 standard deviation in their interval-4 effects (estimated using 2002-2009 data).

[Figure 15 here]

Consideration of the standard deviations plotted in Figure 15 suggests three main conclusions. First, there appears to be a systematic lifecycle pattern in the measured heterogeneity of firms. The distribution of establishment effects is relatively wide for recently-born establishments but tends to narrow and then stabilize over time. This is particularly true for plants born in the last year or two of an interval (e.g., plants born in 1990 and 1991 have relatively large dispersion in their interval-1 effects). In part this may reflect additional noise in the estimated establishment effects for plants that are only observed in one or two years. Second, among continuing establishments there is not much rise in the variability of firm effects between interval 1 and interval 4. This is perhaps clearest for the (large) set of establishments that are first observed in 1985. Among these establishments, the standard deviation of estimated effects only rises from 0.15 in interval 1 to 0.17 in interval 4. Finally, much of the rising heterogeneity in establishments appears to be due to “new” establishments, particularly those that first appear 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.

What features of new establishments – particularly those formed after 1996 – account for the extra variability in their wage premiums? One potential factor is collective bargaining status. Wages set under collective bargaining are less dispersed in Germany (e.g., Dustmann et al, 2009; Antonczyk et al., 2010), however existing research has not addressed the extent to which this association is driven by selectivity in the characteristics of workers covered by collective bargains.⁴⁶ To gather some evidence on the possible impact of collective bargain-

⁴⁶Gurtzgen (2007) suggests that a substantial fraction of the observed differences in mean wages between workers in different collective bargaining arrangements is due to selectivity.

ing we use information on the (relatively small) panel of establishments in the IAB Linked Employer Employee Panel (LIAB) that can be merged to our analysis sample.⁴⁷ Information is available in the LIAB since 1995 on whether an establishment has signed on to the appropriate sector-level contract, has negotiated a plant-specific collective bargaining agreement, or has chosen not to participate in any form of collective bargaining. We use this information in two ways. First, we compare the distributions of establishment effects for plants in each of the three possible collective bargaining categories. Second, we form an estimate of the fraction of establishments from each birth cohort in each category.

Figure 16 shows the distribution of interval-3 establishment effects for plants in the LIAB that can be linked to our sample, classified by their collective bargaining status in 2000. We show the fractions of establishments with wage premiums in each of 10 decile groups.⁴⁸ This simple figure illustrates a very striking difference between establishments with either a sectoral or plant-specific agreement, and those without any form of collective bargaining coverage. Whereas 22% of establishments with some form of coverage are in the first two decile groups, 44% of uncovered establishments fall into this range. This relative left skew is associated with a substantially lower (-10%) mean establishment effect for plants in the LIAB with no collective bargaining relative to plants with either form of bargaining.⁴⁹ The distributions of establishment effects are similar for other years in which we can link our estimated establishment effects to coverage information from the LIAB. In all cases the distributions of establishment effects for uncovered establishments exhibit strong left skew relative to covered establishments, and have a mean that is 8-10 percent lower.

[Figure 16 here]

The fraction of West German workers whose wages are set by collective bargaining has fallen sharply since the mid-1990s (see Dustmann et al, 2009, Table III). This decline is particularly pronounced for new establishments. The top line in Figure 15 shows the estimated

⁴⁷The LIAB has about 13,000-14,000 establishments in the years after 2000, and a smaller sample in earlier years.

⁴⁸The decile cutoffs for the establishment effects are formed 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.

⁴⁹In a regression of the interval-3 establishment effect on dummies for a sectoral or plant-specific agreement, the coefficients are 0.100 (standard error=0.05) for a sectoral agreement and 0.097 (standard error=0.01) for a plant-specific agreement. The coefficients are 0.080 and 0.085 respectively when dummies are added for the birth cohort of the establishment.

fraction of establishments from different birth cohorts without any form of collective bargaining.⁵⁰ This fraction has fallen from 50-55% for establishments born from 1986 to 1996, to 30% or less for establishments born after 2007. Interestingly, the trend in non-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. This coincident timing is consistent with the implication from Figure 16 that a rise in the fraction of uncovered establishments would be expected to lead to a rise in the overall dispersion of establishment effects. Of course, it is difficult to assign a causal role to collective bargaining, since firms endogenously choose whether to adopt some form of collective bargaining, or not. At a minimum, however, the evidence in Figures 15 and 16 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.

9 Interpretation

Our analysis has uncovered a number of new facts. The first is that both the person and establishment components of wages have become more variable over time. The rise in the variance of the establishment component suggests that Germany is increasingly a “high stakes” labor market where wages depend more than ever on finding a job at a particular set of employers – a process that is only partially under workers’ control. Indeed, the large wage losses associated with transitions from top to bottom quartile establishments in Figure 9a suggest that many of the job changes observed in our data are involuntary. Such transitions likely constitute an important component of earnings risk (Davis and Von Wachter, 2012).

The coincident timing of the trends in the variances of the worker and establishment components of wages is striking, and raises the question of whether both trends are due to a single underlying cause. One candidate explanation is new technologies that diffuse slowly and imperfectly across establishments (Griliches, 1957; Banerjee and Duflo, 2005; Hsieh and Klenow, 2009; Conley and Udry, 2010). The advantages conveyed by early technology adoption may (at least temporarily) yield profits that are partially shared with workers. If this is the case we expect high wage establishments to be successful and profitable enterprises.

⁵⁰We first estimate the non-coverage rate by birth year for establishments in our sample that can be merged to the LIAB, using coverage data for each year from 1999-2008. We then take the average non-coverage rate across years for each birth cohort.

An alternative explanation for the rising dispersion of establishment effects is the lifting of impediments to labor market clearing – changes which themselves may have been brought on by shifts in technology or international competition. For example, regulatory reforms and other changes may have made it easier for German employers to deviate from sectoral wage contracts that artificially raised wages – particularly for lower skilled workers – and compressed the wage structure. Under such a view, it is the low wage establishments that ought to be successful enterprises, having broken free of unproductive regulatory restrictions.

We lack data on establishment revenue or profitability, but an indirect test of these competing hypotheses is provided by comparing the survival probabilities of high wage and low wage establishments.⁵¹ Figure 17a shows empirical survivor functions by quartile of estimated establishment effects for the cohort of establishments that first appear in 1989. The data clearly indicate that high wage establishments are more likely to survive than low wage establishments, suggesting that some form of rent sharing may be going on. These patterns are confirmed for a broader set of cohorts in results from probit models for the likelihood of surviving for 5+ or 10+ years, and Tobit models for the duration of establishment life, presented in Appendix Table 7. We estimate the models separately for establishments born in different intervals, and include the estimated establishment effect, the mean person effect for all employees at the establishment, and controls for establishment size, average education of workers, and year of “birth”. These models confirm that establishments that offer higher wage premiums survive longer, as do those that employ higher-wage workers.

Additional insight is provided by looking at the survival of jobs at higher- versus lower-wage establishments. If the wage premium paid by high-wage establishments is a rent, we would expect employees at these plants to be less likely to quit, and for their jobs to last longer. If, on the other hand, the wage premium is a compensating differential for longer hours or more intense work effort, we would not expect a clear relationship between job duration and the size of the establishment effects. Figure 17b shows estimated survivor functions for jobs created in 1989 at establishments in each of the four quartiles of estimated effects. It is clear that jobs at higher wage establishments are substantially more likely to survive than those at lower-wage establishments.⁵² Again, this conclusion is confirmed by a series of probit and Tobit models, presented in Appendix Table 8. Controlling for an

⁵¹Several studies utilizing French data have found direct evidence that high wage firms are more profitable (e.g. Abowd, Kramarz, and Margolis, 1999; Goux and Maurin, 1999; Abowd and Kramarz, 2005).

⁵²A comparison of Figures 17a and 17b shows that establishments tend to survive much longer than jobs. In fact, in our samples only about 20% of job endings occur because of establishment death. Thus, the finding that jobs last longer at higher-paying establishments is not simply a reflection of the fact that these establishments live longer.

individual's person effect, his education, and the size of the establishment, jobs at higher-wage establishments last much longer.

Taken together, the patterns of establishment and job survival suggest that establishments that offer higher average wages are more profitable, and share some of these profits with workers. Whether these profits are driven by proprietary technological advantages, particular product niches, or other factors is clearly an important topic for future research.

[Figures 17a, 17b here]

A second fact uncovered in our analysis is the striking increase in the association between person effects and establishment effects. This increase could again be the result of technological changes which give rise to increasing heterogeneity in the skill demands of firms. For example, work by Autor, Levy, and Murnane (2002, 2003) suggests that firms that automate routine tasks may need workers capable of handling more difficult cognitive and supervisory tasks. Such workers are likely to be more adaptable and, in our context, to have higher person effects due to the portable nature of their skills.

An alternative explanation is that search frictions have decreased and that the best firms and workers are now able to find one another. This is possible, but sharply at odds with our finding of increasingly variable establishment effects. We would actually expect reductions in labor market frictions to *reduce* the relative importance of labor market rents and *lower* the dispersion in estimated establishment effects.

A variant of this argument is that regulatory and technological changes have allowed increasing sorting of high and low skill workers to specialized firms. One channel through which this could occur is outsourcing. In the face of falling wages for lower-skilled workers, firms with many highly-paid workers have an incentive to outsource lower-skill jobs to circumvent legal and morale problems caused by within-firm pay inequality. New technologies also make it easier to outsource certain specialized jobs. A rise in outsourcing is consistent with our finding of increased occupational segregation across establishments. On the other hand, the relative stability of the establishment size distribution across our four sample intervals (see Appendix Figure 1) suggests that the overall impact of such organizational changes may be modest.

Our third finding is that the rise in return to education is largely driven by changes in the payoff to working with highly-educated co-workers, coupled with increasing sorting of

highly-educated workers to high-education workplaces. Several explanations for this result are possible. One is that new technologies have increased the degree of complementarity between highly-educated coworkers (Kremer and Maskin, 1996). A second is that higher education provides access to networks that have become more important for finding a job at a high-wage employer. A third is that employers' hours requirements are becoming increasingly heterogeneous, and that higher educated workers are increasingly sorting to jobs with longer hours (Kuhn and Lozano, 2008). Since jobs last longer at higher-wage establishments, we believe that a pure compensating differences explanation is unlikely, though hours-related heterogeneity may explain some of the rise in establishment-level wage variation.

Fourth, we found that the distribution of establishment effects varies substantially by firm cohort, with newer firms exhibiting greater variance, and being less likely to participate in collective bargaining. These patterns could be the result of differences in technology choices or management practices (Bloom and Van Reenen, 2007) across cohorts of firms, or institutional factors that limit the ability of older firms to abandon existing wage bargains. In any case, trends among the the most recent cohorts suggest that the rise in the establishment component of wage variability will continue as new establishments continue to replace older ones.

10 Conclusion

West Germany has experienced substantial changes in the structure of wages over the past 25 years. Observers have been divided over whether those changes are driven primarily by technological changes or changes in labor market institutions (Dustmann et al., 2009; Eichhorst, 2012). Our analysis approaches this question from a different perspective, asking whether the proximate source of changes in the structure of wages involves the portable component of wages, or the component associated with which particular employer a worker is paired. The data reveal that both components are increasingly important – the gap between consistently high and low wage workers has grown, as in traditional one-factor models of skill pricing (Juhn, Murphy, and Pierce, 1993), but the gap between good and bad jobs has also grown, suggesting the job search and matching is an increasingly high stakes process that is more and more mediated by the education system.

These findings raise many questions regarding the nature of modern labor markets. Are the patterns uncovered here present in other developed economies? Which factors drive the matching of workers to firms? Why are there large firm cohort effects in wages? How much

of the displacement effects of job loss are due to establishment effects? How do shocks to the product market filter into changes in person and firm effects? With the proliferation of modern employer-employee datasets, we are hopeful that progress can be made on these important questions.

Appendix

Computation

Because 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 establishment over the relevant time period. We fit the model to this sample of movers and recovered the estimated vector of establishment effects $\hat{\psi}$ along with the coefficients $\hat{\beta}$ corresponding to the time varying covariates x_{it} . Then, for each worker who stayed at the same establishment over the sample interval, we computed an estimate of his person effect as follows:

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

where $T_i \equiv \sum_t d_{it}$ 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.⁵³

Our estimation tasks were performed in Matlab. Code for our analysis is available online. We use 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 $[D, F, X]$ were stored as sparse matrices. To compute the least squares solutions we solved the normal equations in (3) 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 1e-2.

⁵³That 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.

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:

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

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

OLS estimation of (2) yields a coefficient vector:

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

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

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

where $\Omega \equiv E[rr']$ 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:

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

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

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

$$E \left[\begin{matrix} \widehat{\xi}' \\ \widehat{\xi} \end{matrix} A \widehat{\xi} \right] = \xi' A \xi + \text{tr} \left(A V_{\widehat{\xi}} \right).$$

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 Ω 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 Ω . 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 estimates in section 7 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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Figure 1a: Trends in Percentiles of Real Log Daily Wage
West German Men Relative to 1996 Base

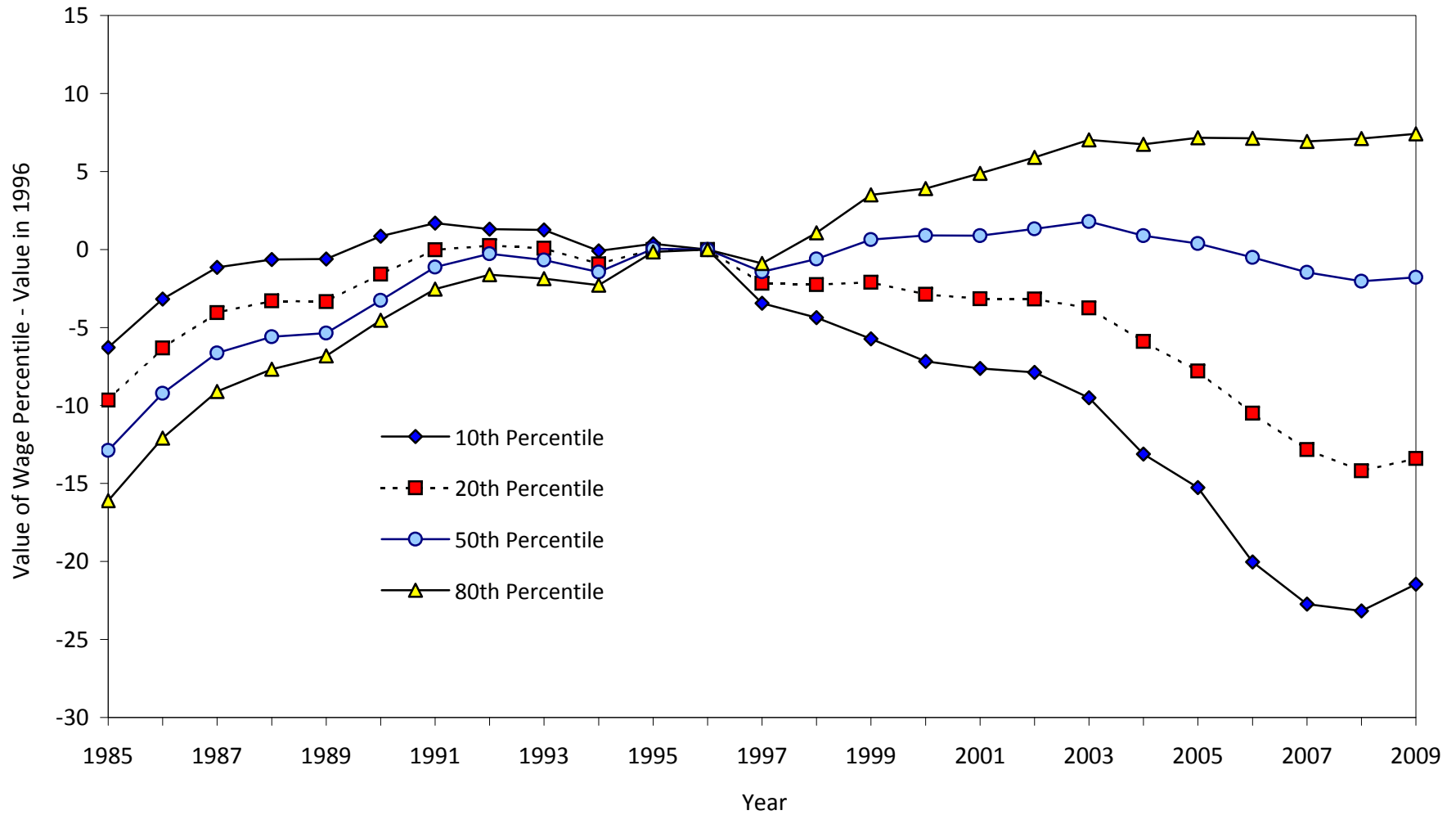


Figure 1b: Trends in Percentiles of Real Log Hourly Wages
U.S. Men Relative to 1979 base

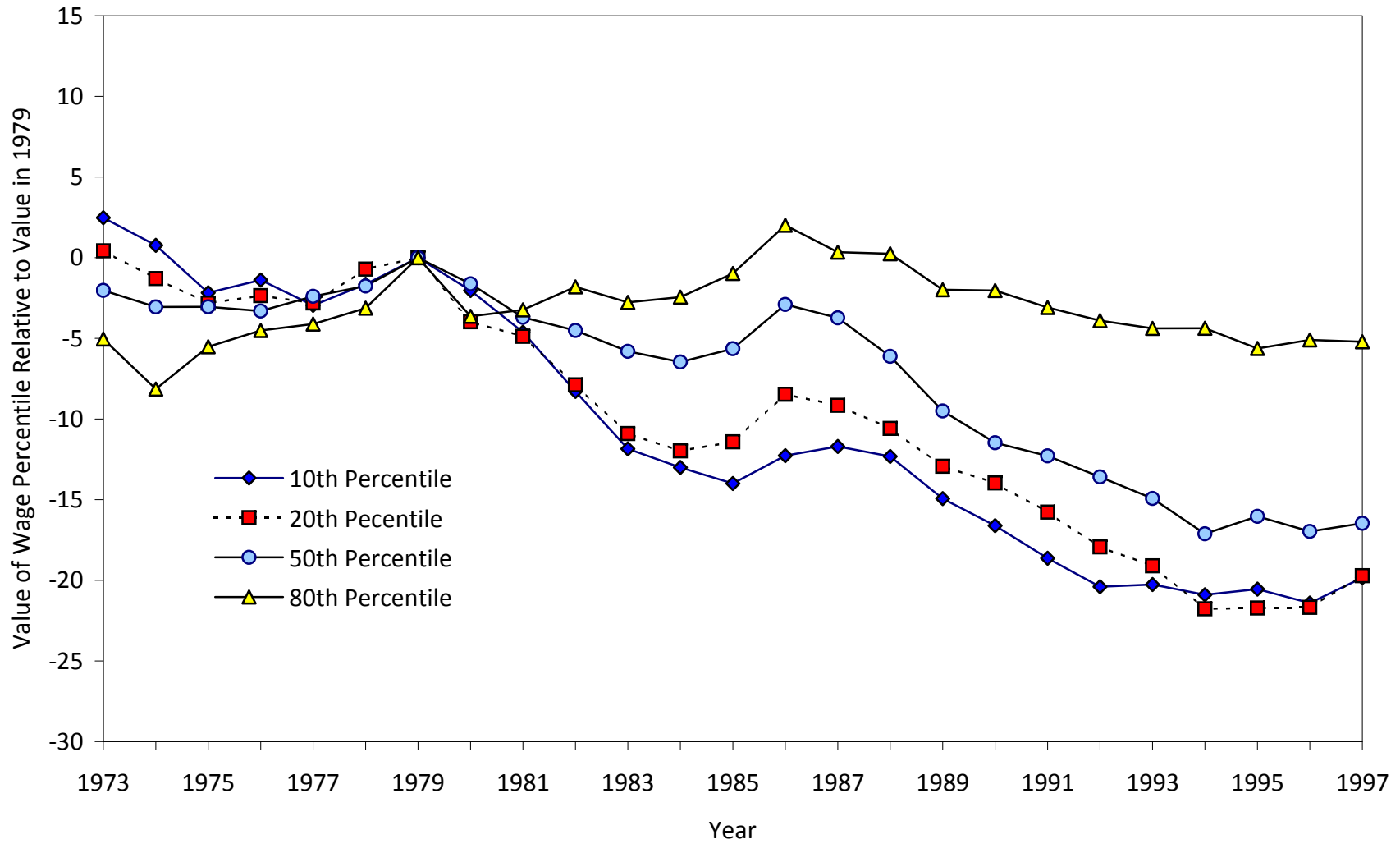


Figure 2: Macroeconomic Trends and Policy Changes in Germany, 1979-2011

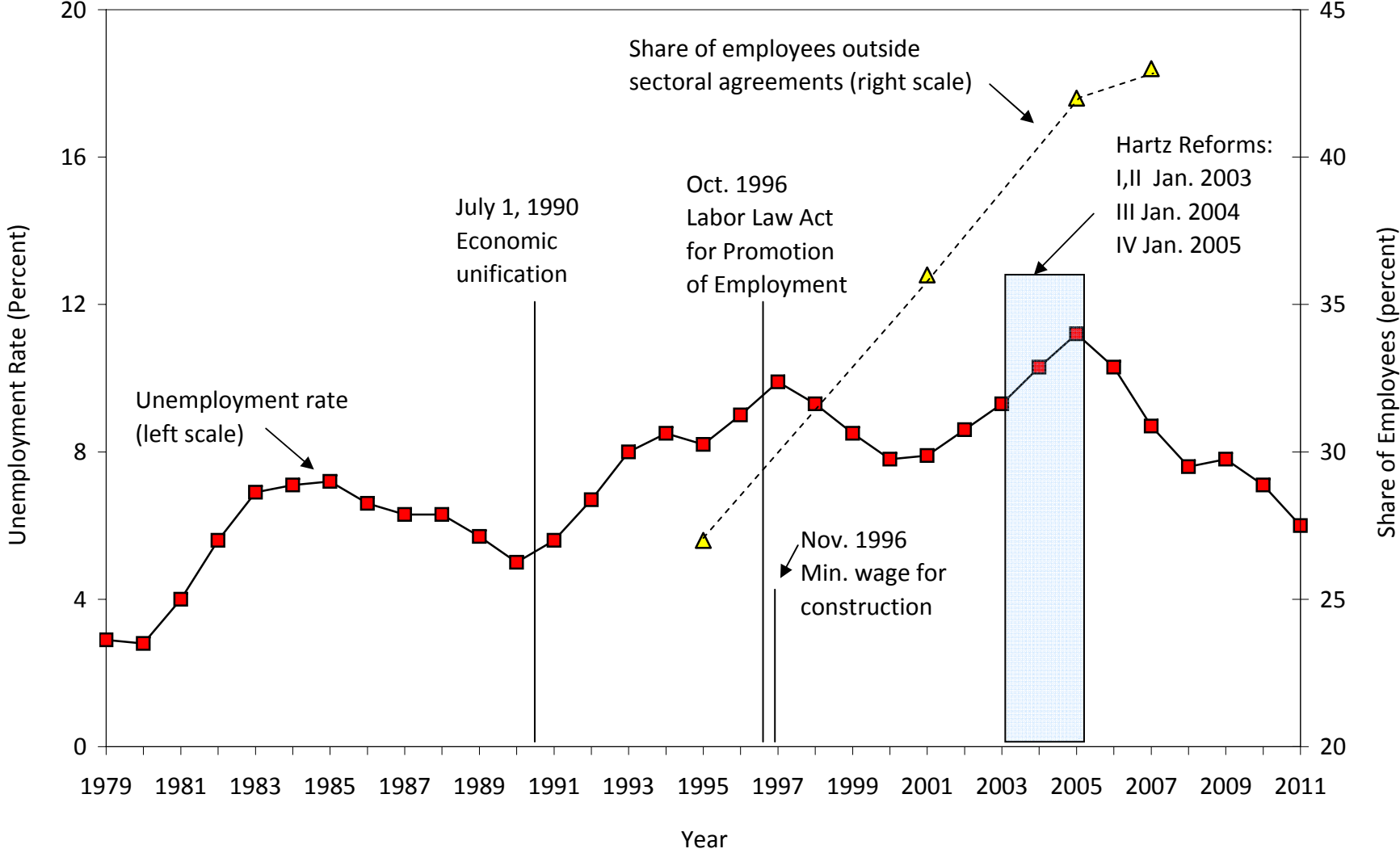


Figure 3a: Trends in Wage Inequality

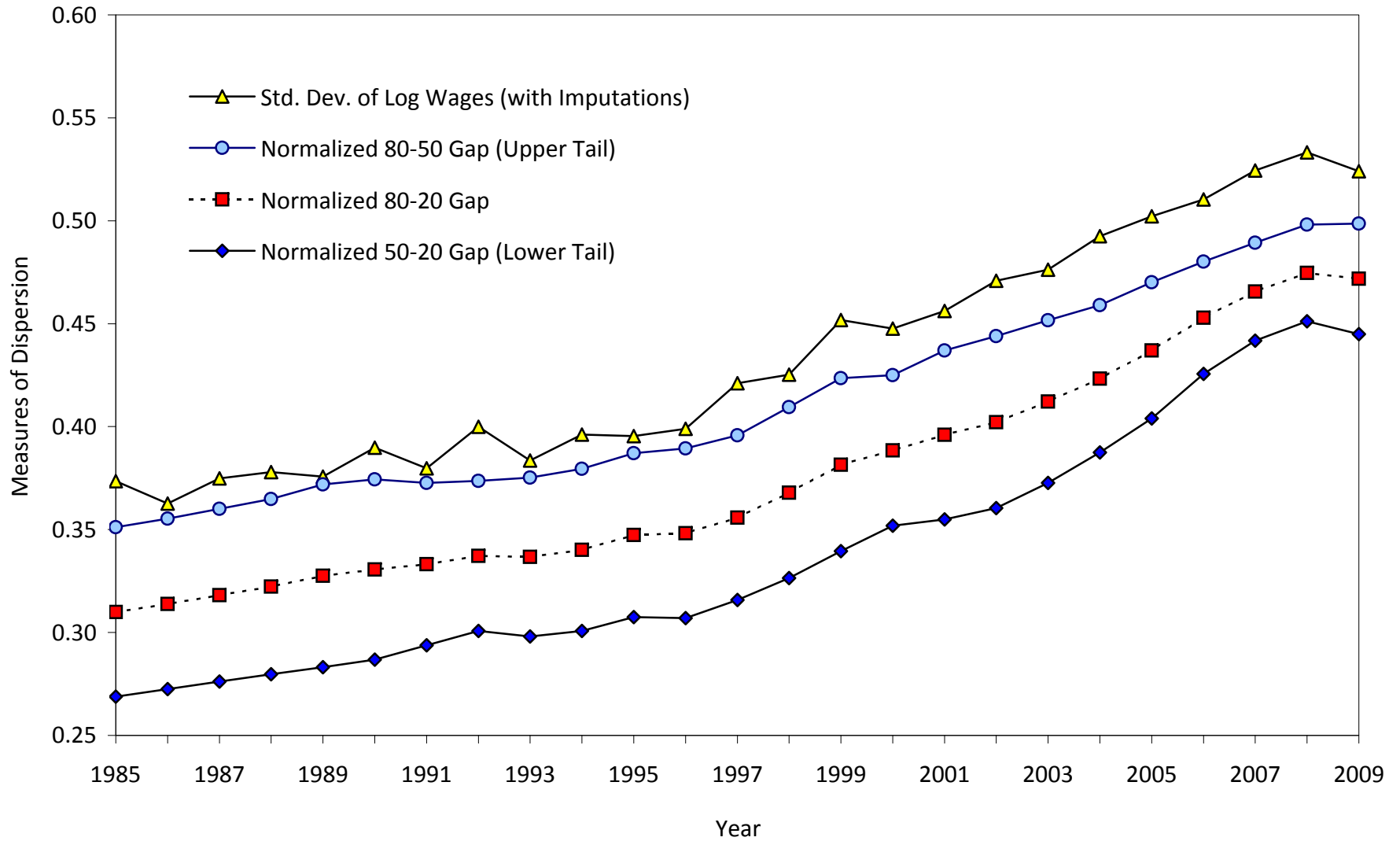


Figure 3b: Trends in Residual Wage Inequality

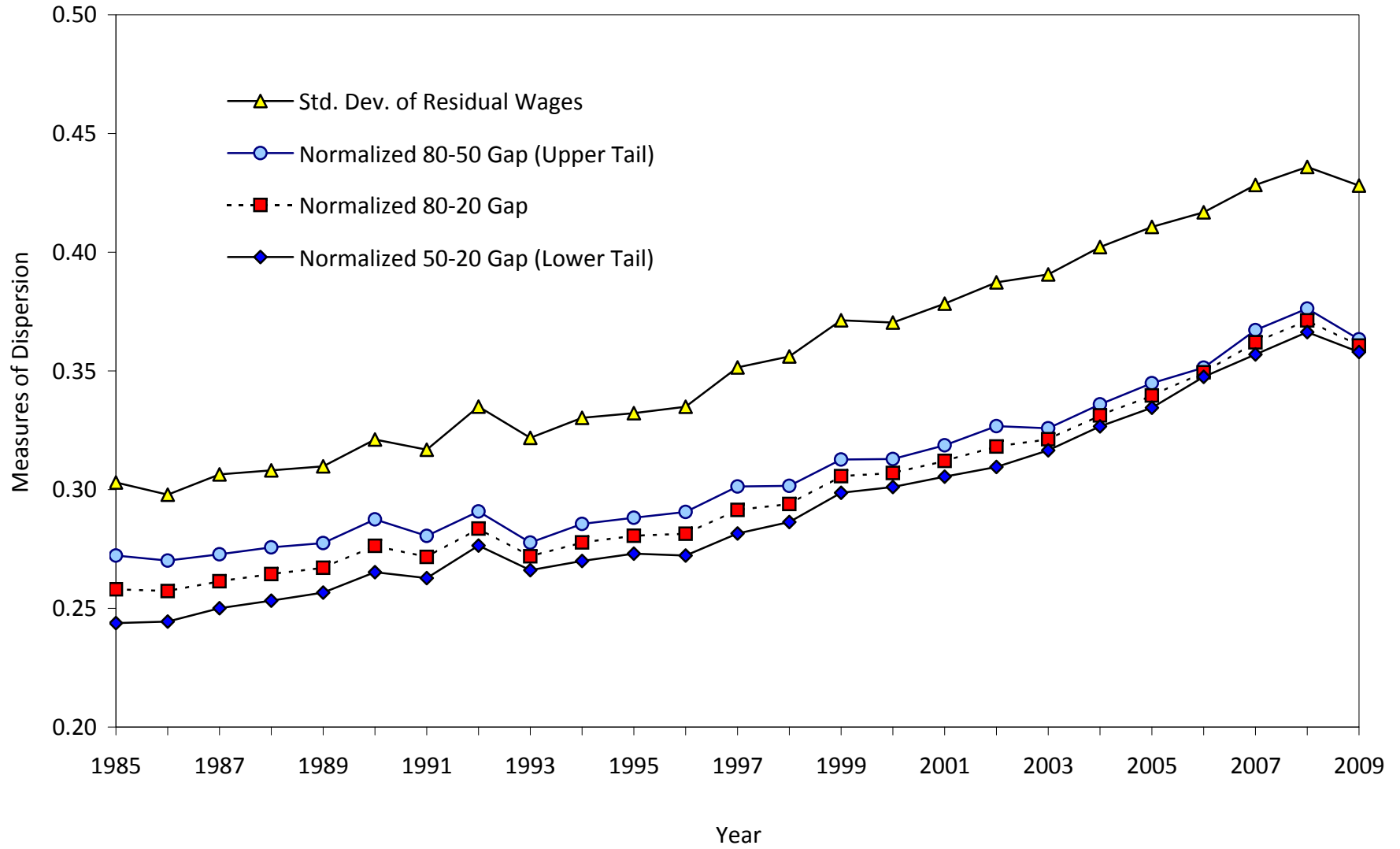


Figure 4: Residual Standard Deviations from Alternative Wage Models

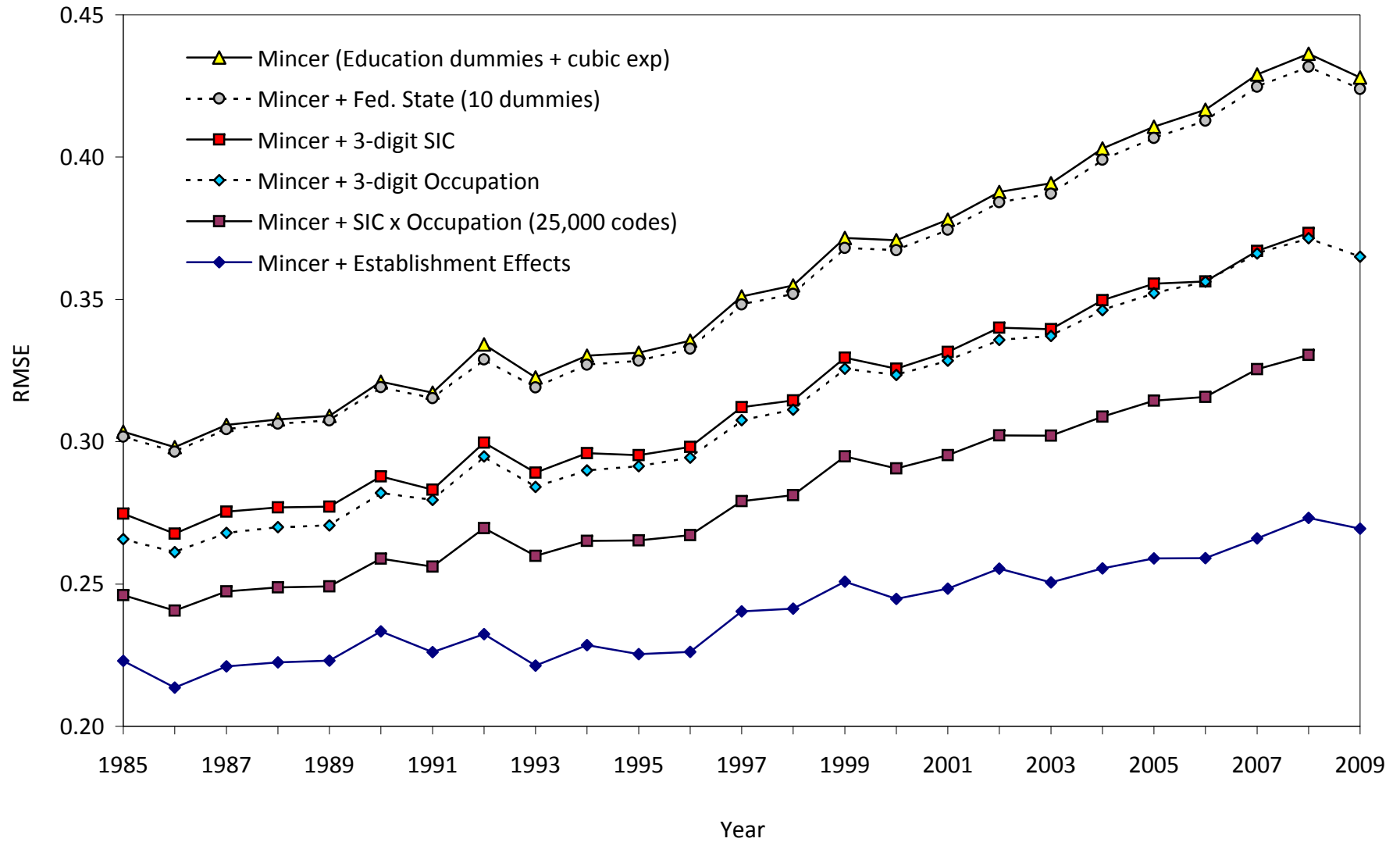


Figure 5: Sorting of Workers in Different Education and Occupation Groups Across Establishments

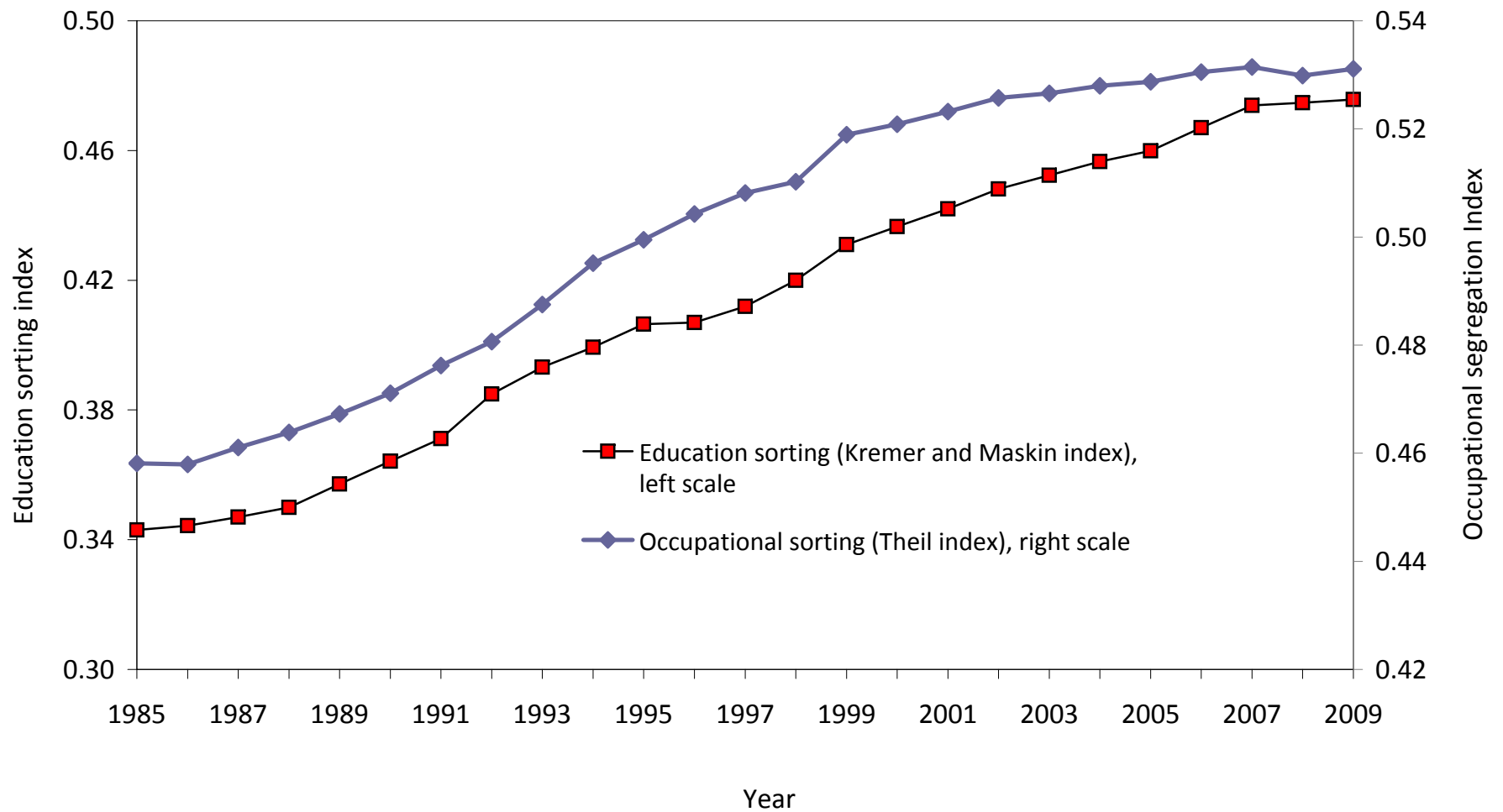


Figure 6a: Mean Wages of Movers, Classified by Quartile of Mean Wage of Co-Workers at Origin and Destination, Interval 1

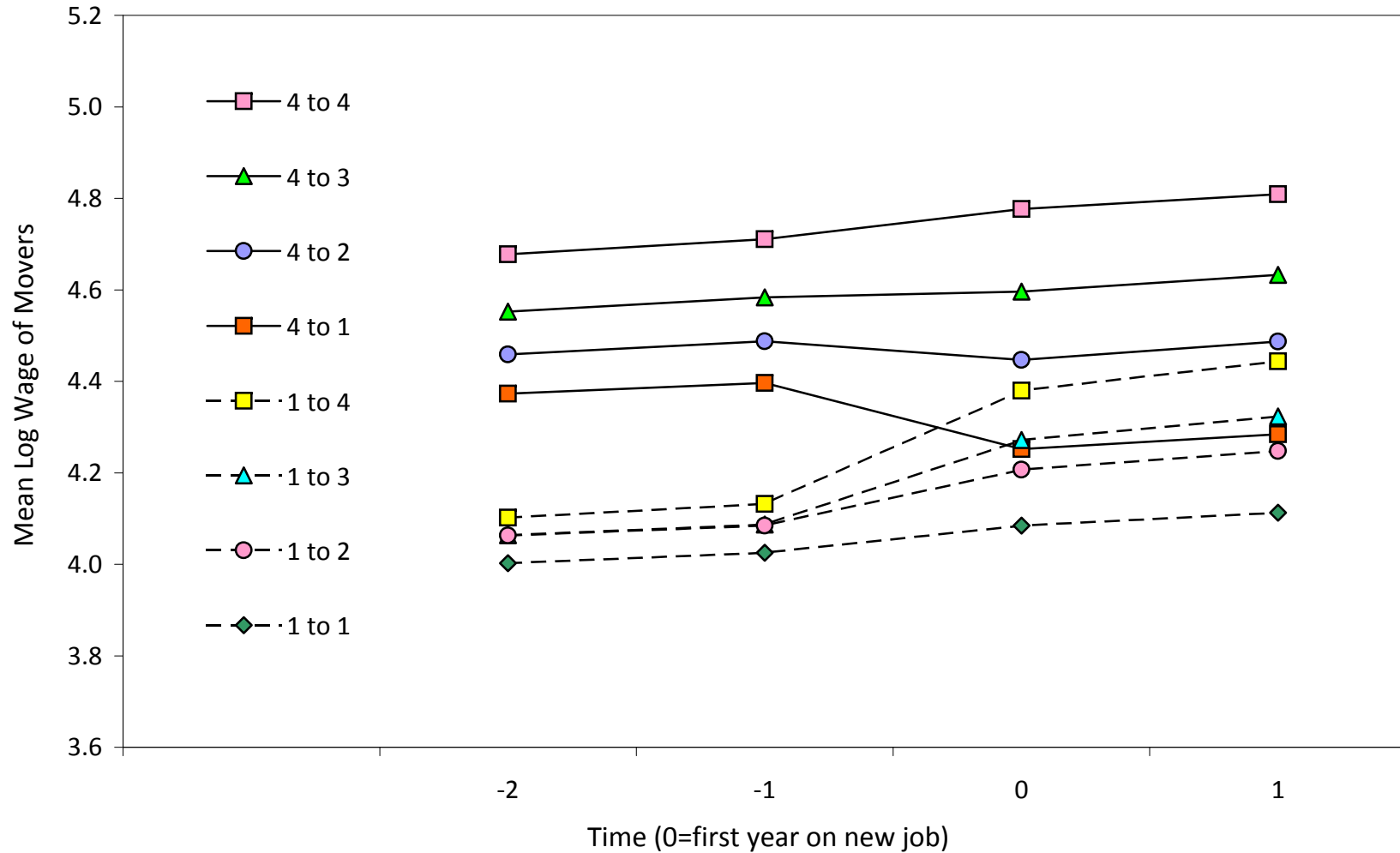


Figure 6b: Mean Wages of Movers, Classified by Quartile of Mean Wage of Co-Workers at Origin and Destination, Interval 4

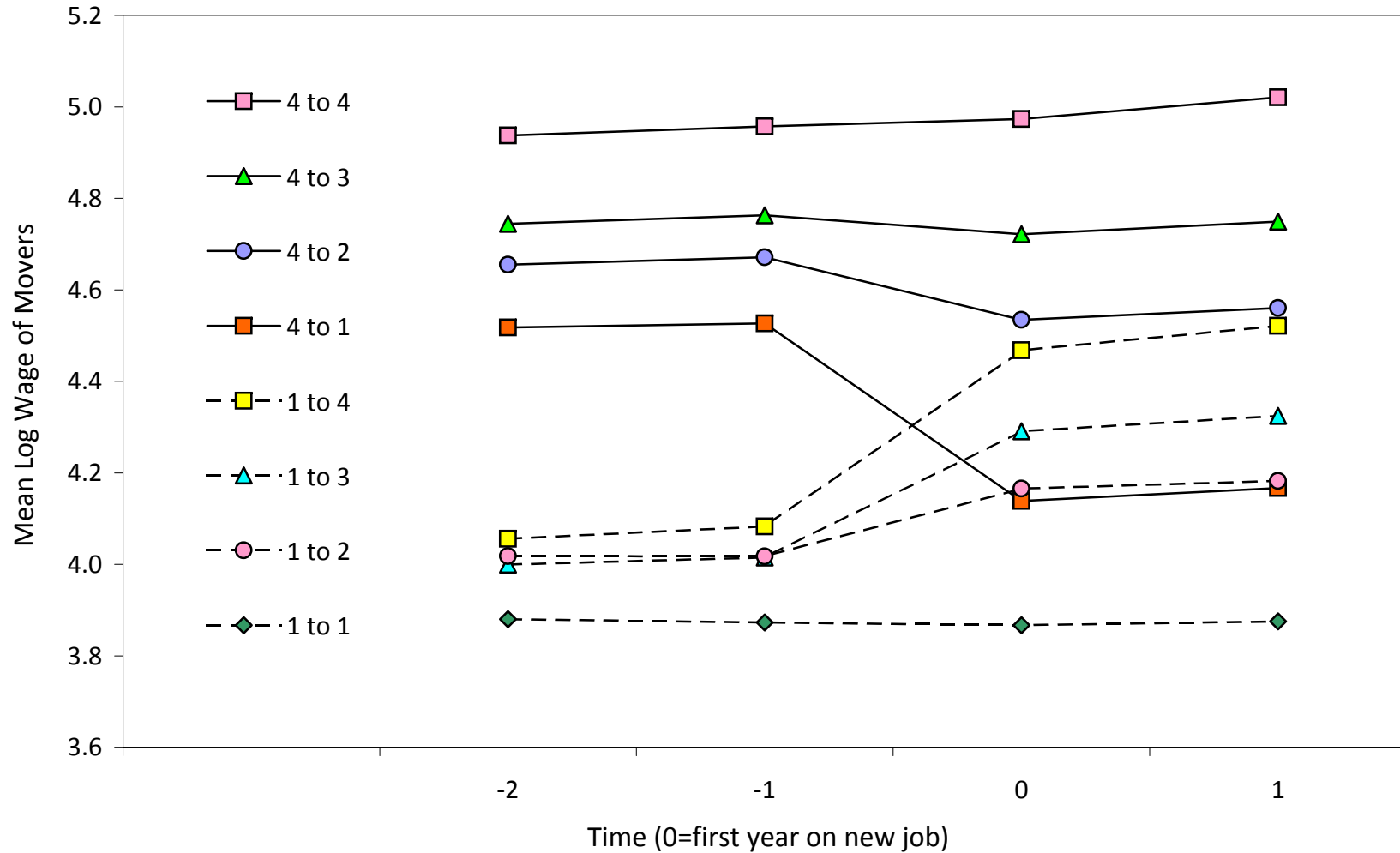


Figure 7: Relative Explanatory Power of AKM Model in Different Periods

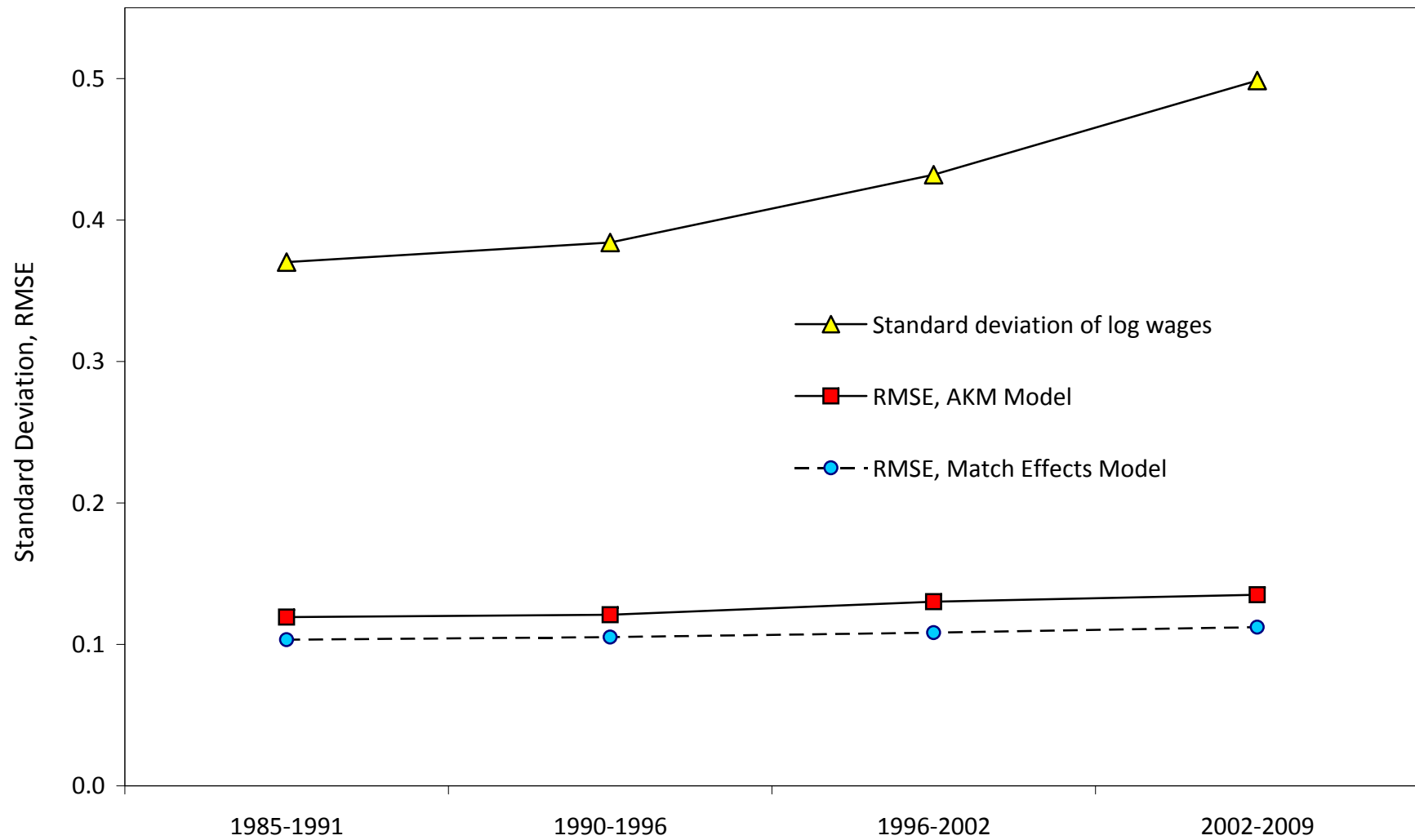


Figure 8b: Mean Residual by Person/Establishment Deciles, Interval 4

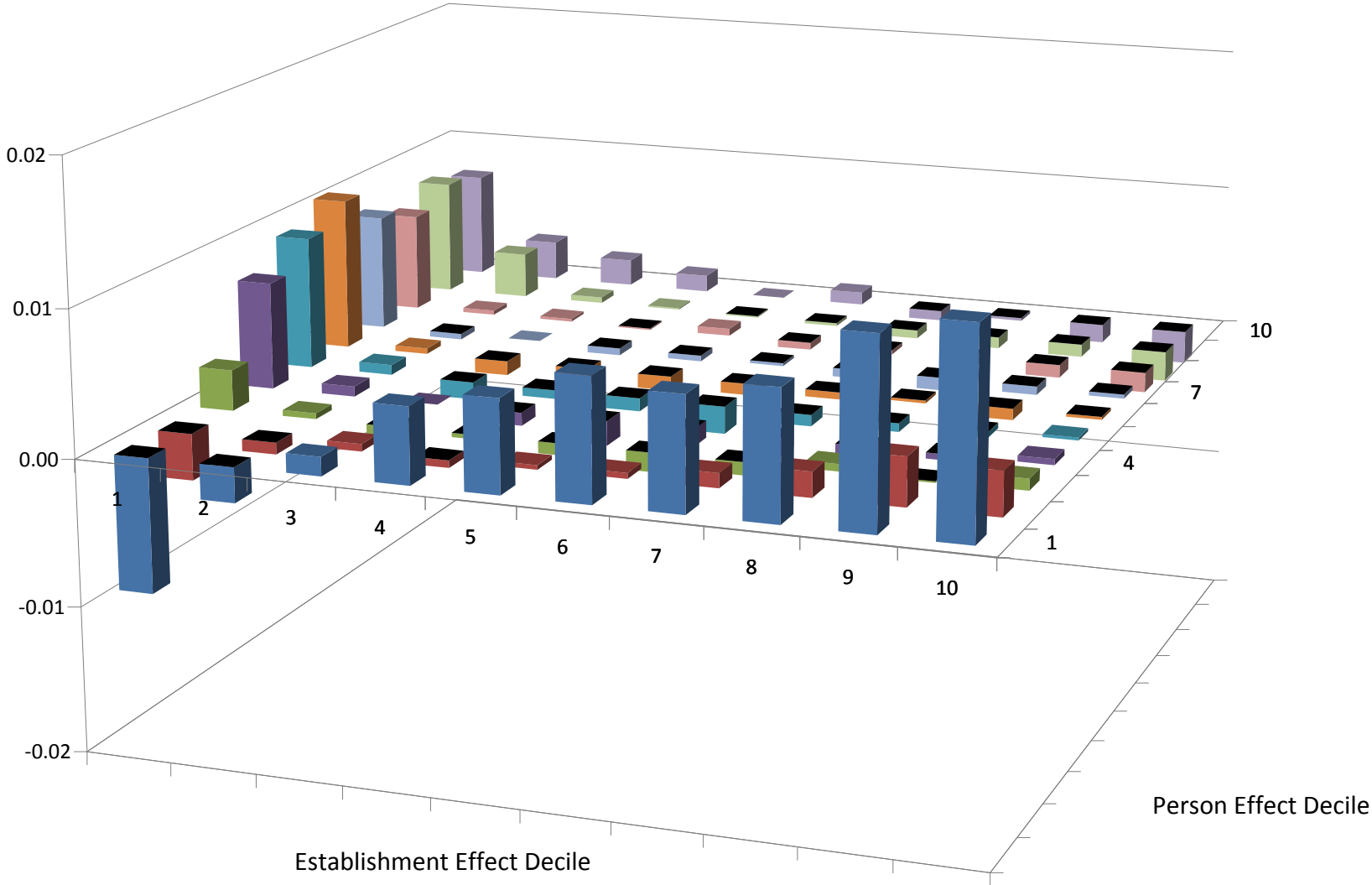


Figure 9a: Mean Wages of Movers, Classified by Quartile of Establishment Effects for Origin and Destination Firms

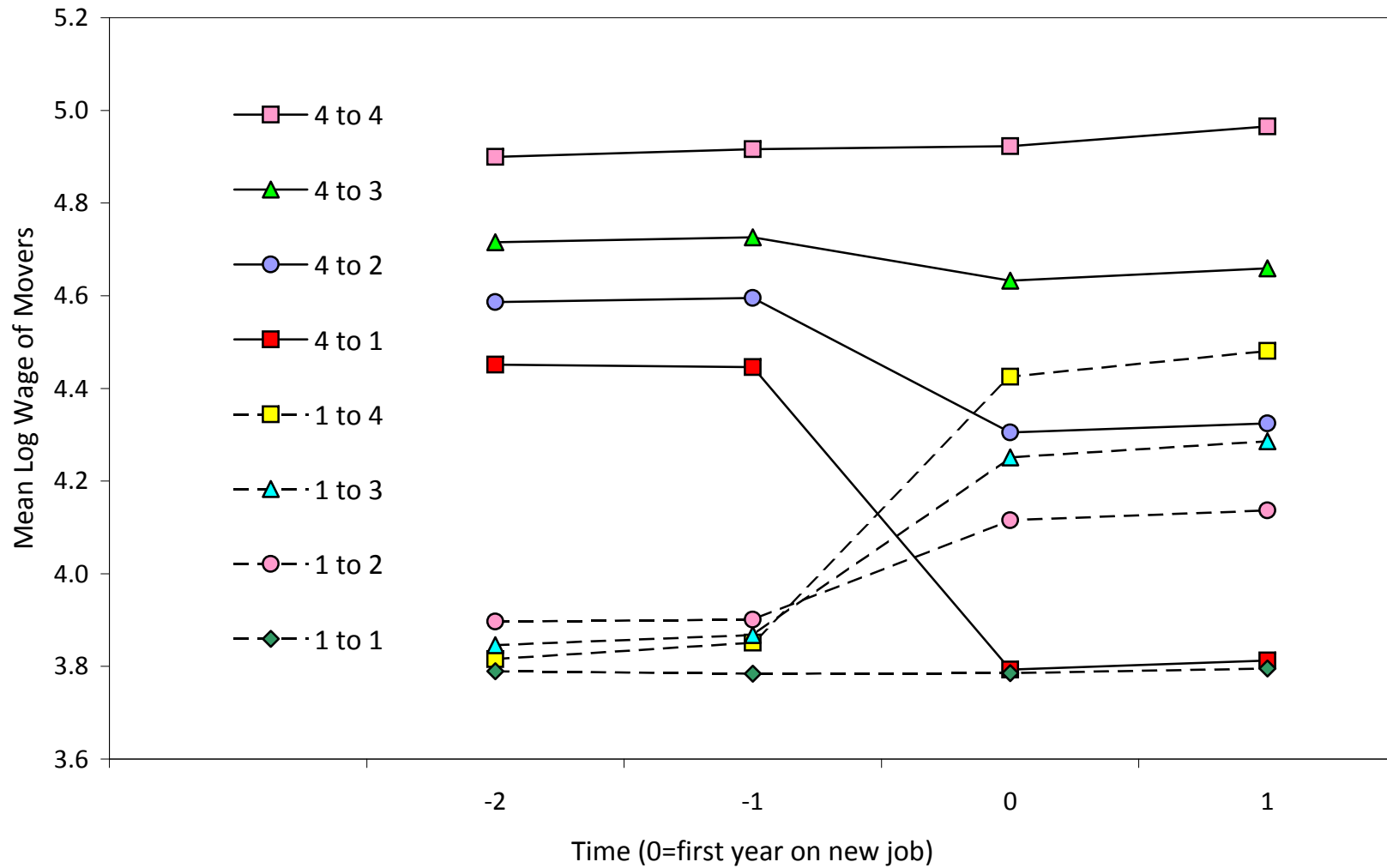


Figure 9b: Mean AKM Residuals of Movers, Classified by Quartile of Establishment Effects for Origin and Destination Firm

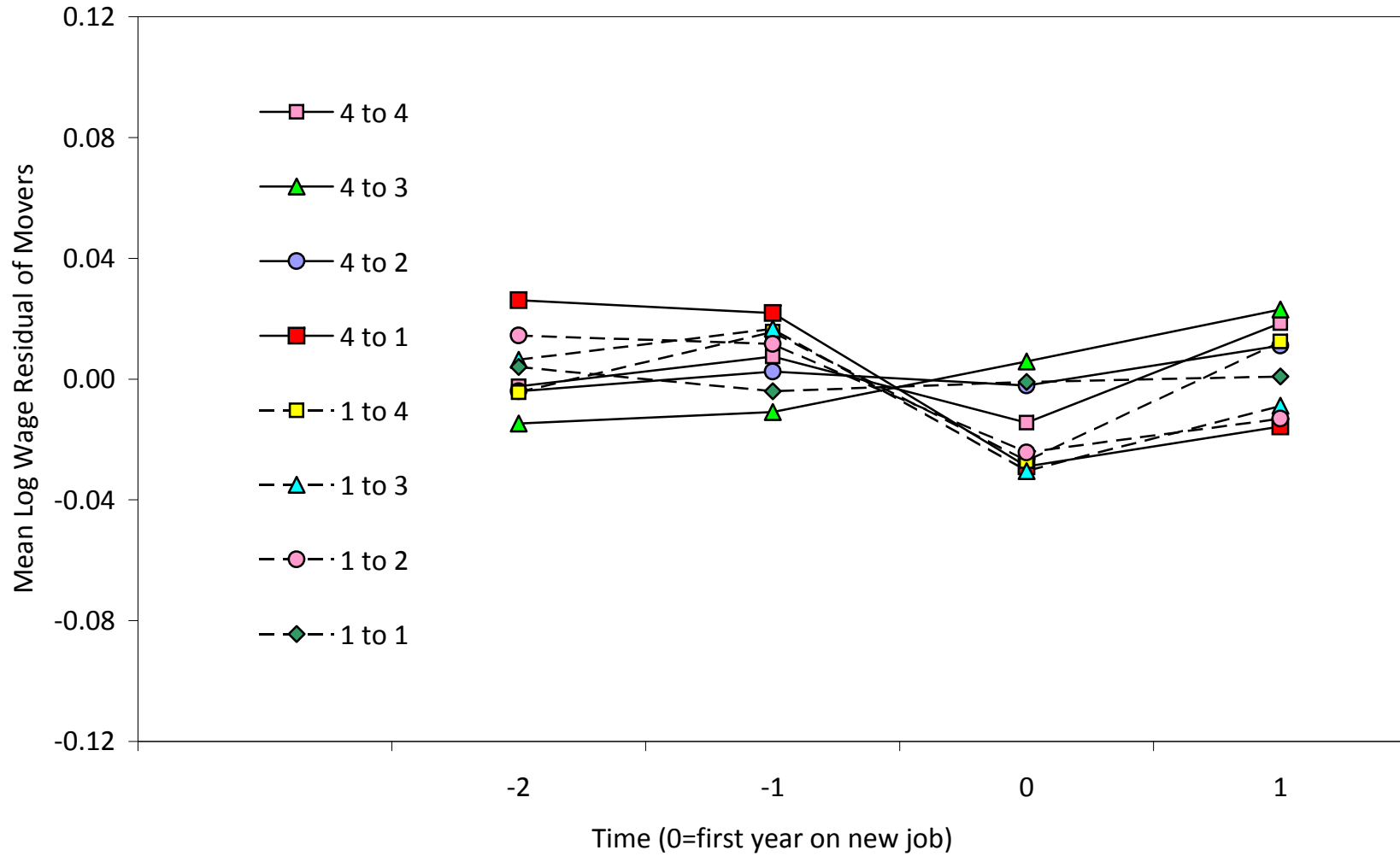


Figure 10a: Joint Distribution of Person and Establishment Effects, Interval 1

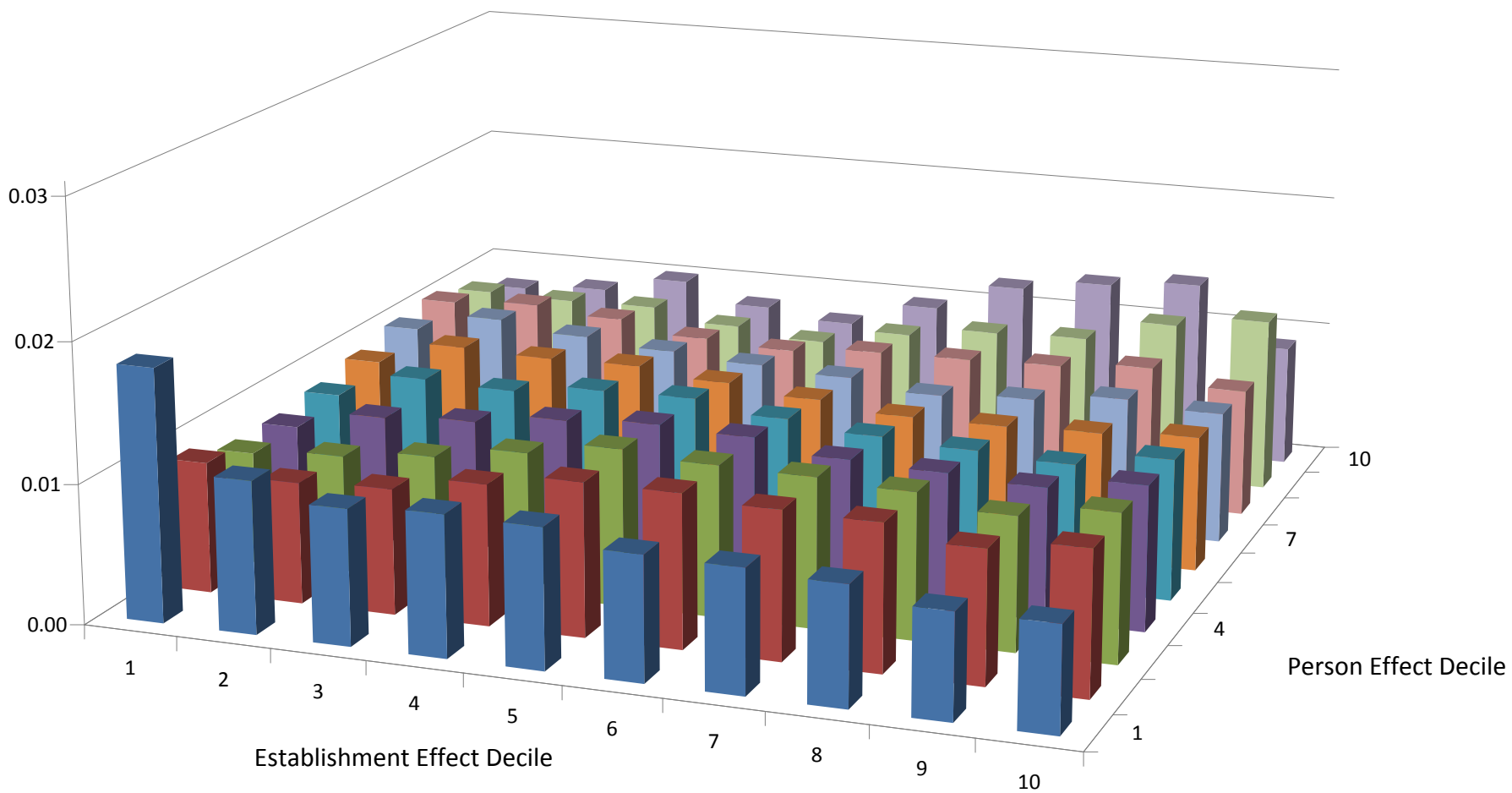


Figure 10b: Joint Distribution of Person and Establishment Effects, Interval 4

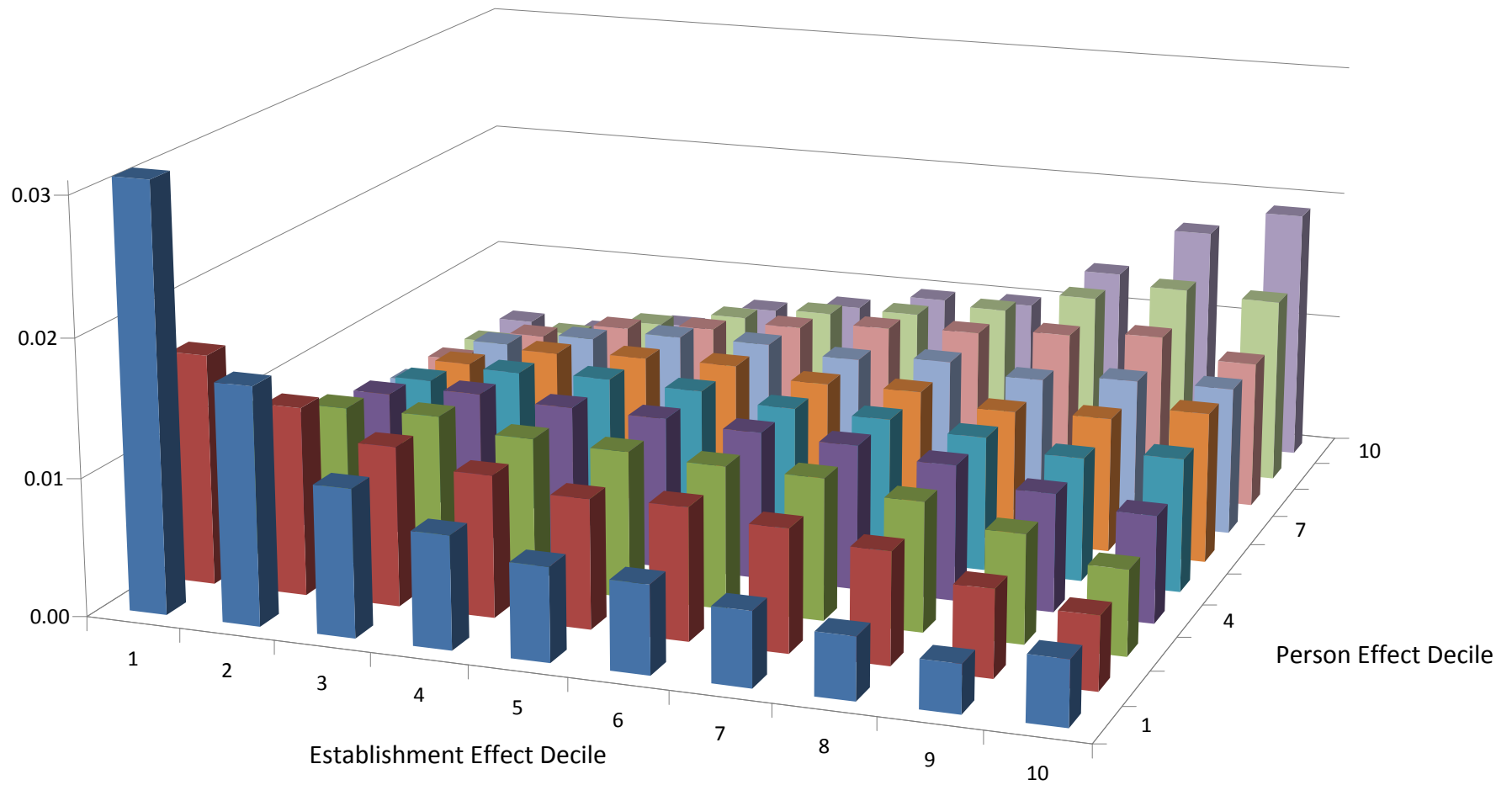


Figure 11: Decomposition of Variance of Log Wages

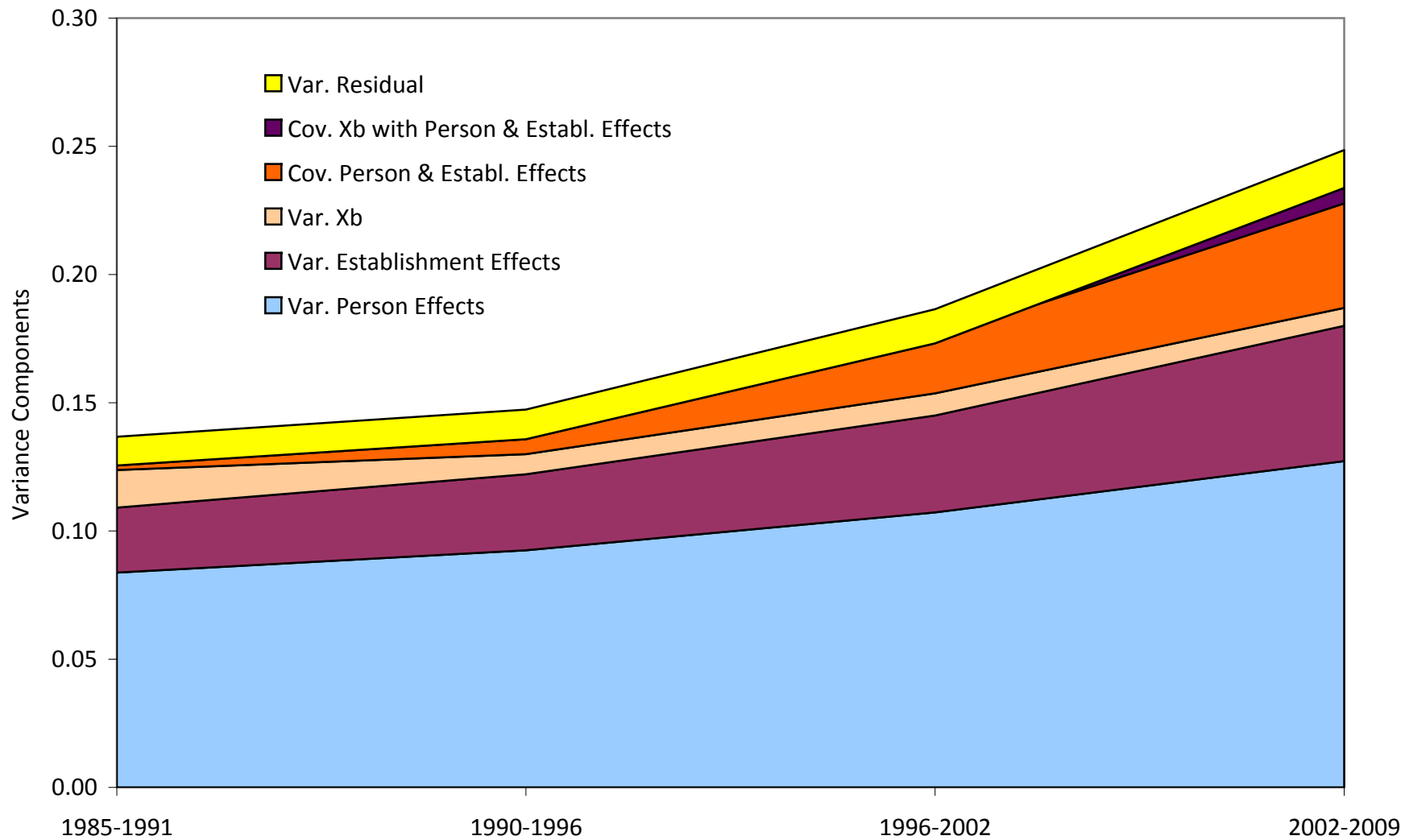


Figure 12: Assignment of Workers to Establishments by Education Group:
1985-91 vs. 2002-09

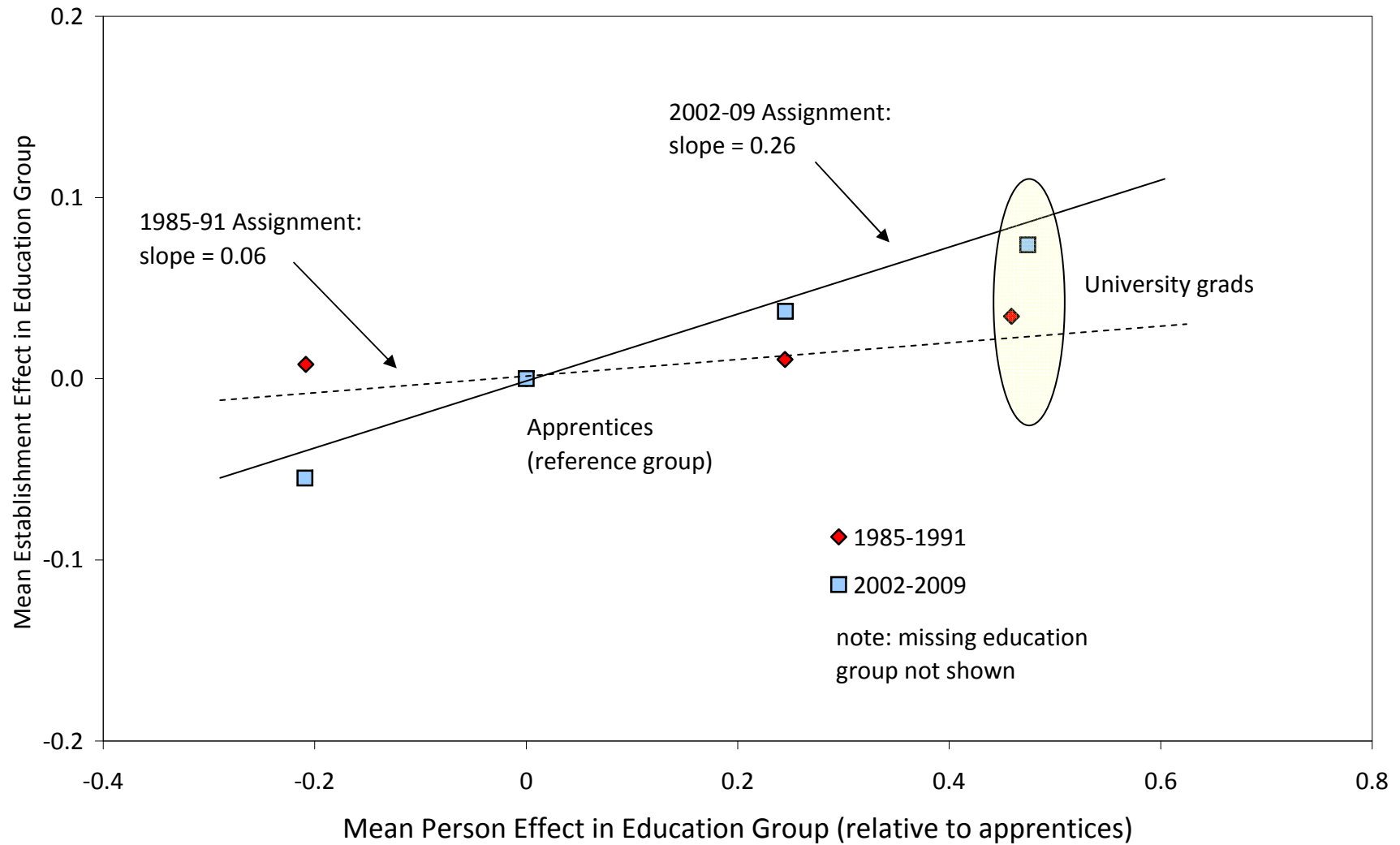


Figure 13: Mundlak Decomposition of Return to Education

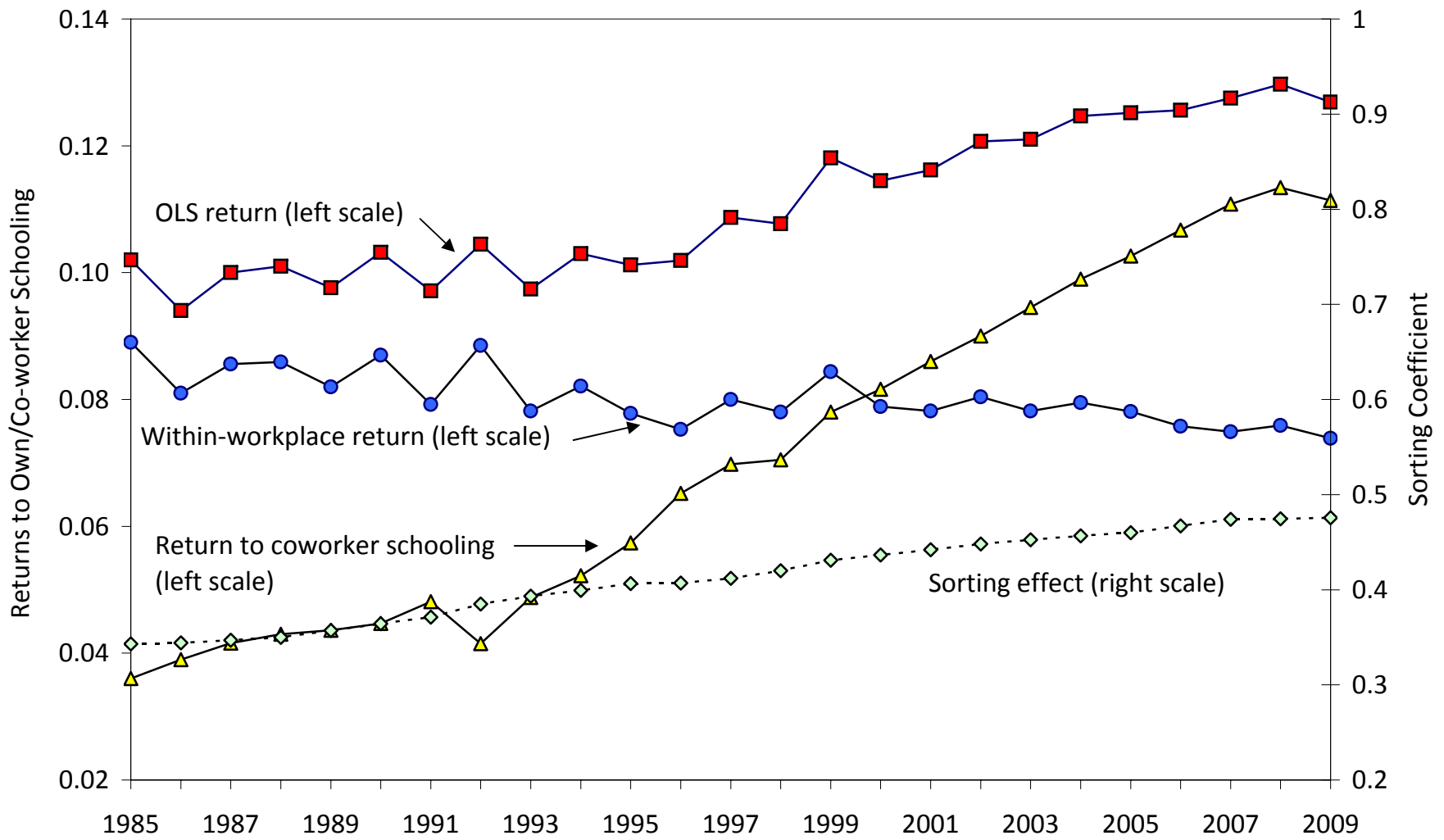


Figure 14a: Between-Occupation Wage Inequality: 2002-09 vs. 1985-91

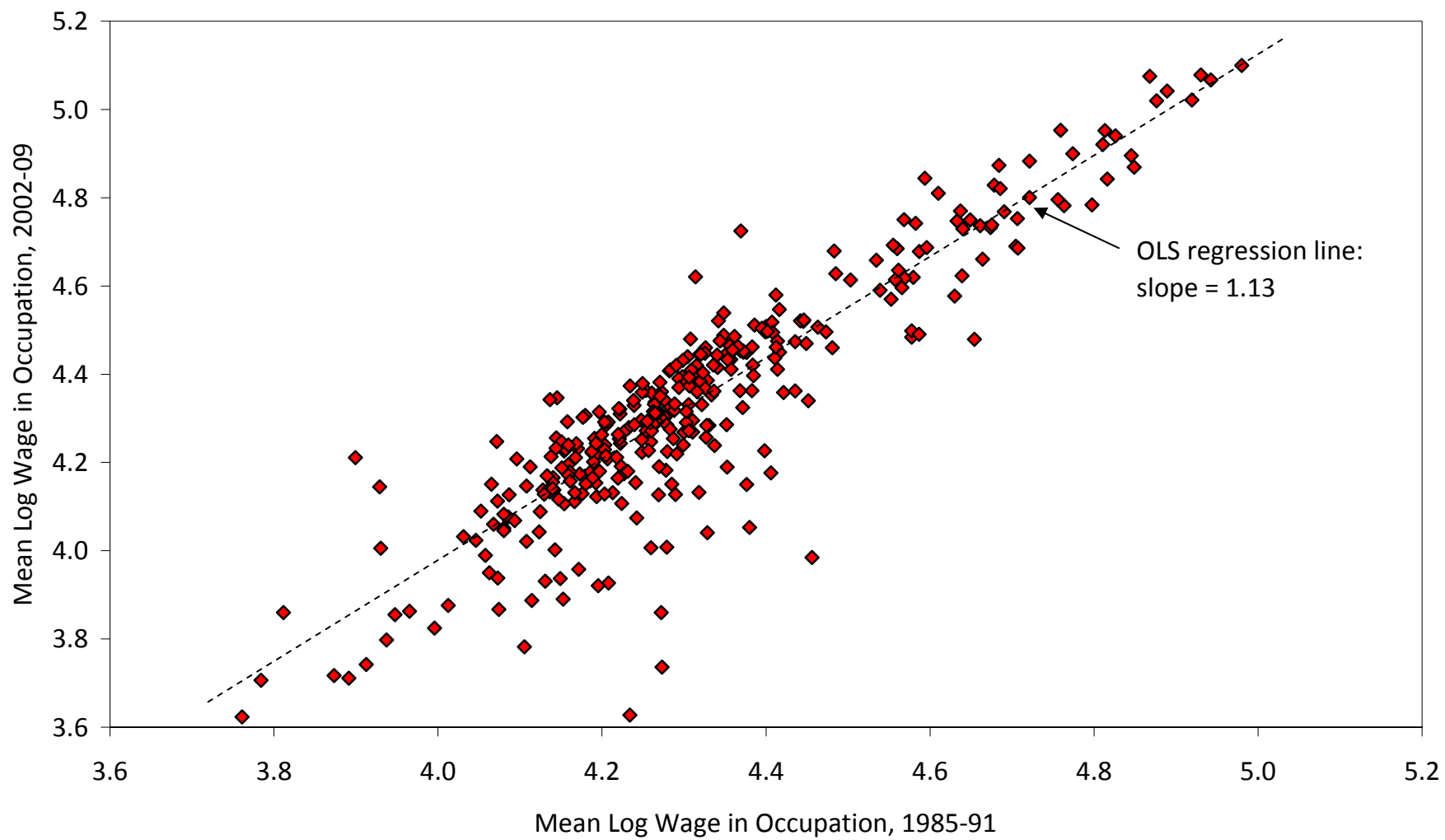


Figure 14b: Rising Correlation Between Mean Worker and Establishment Effects Across Occupations

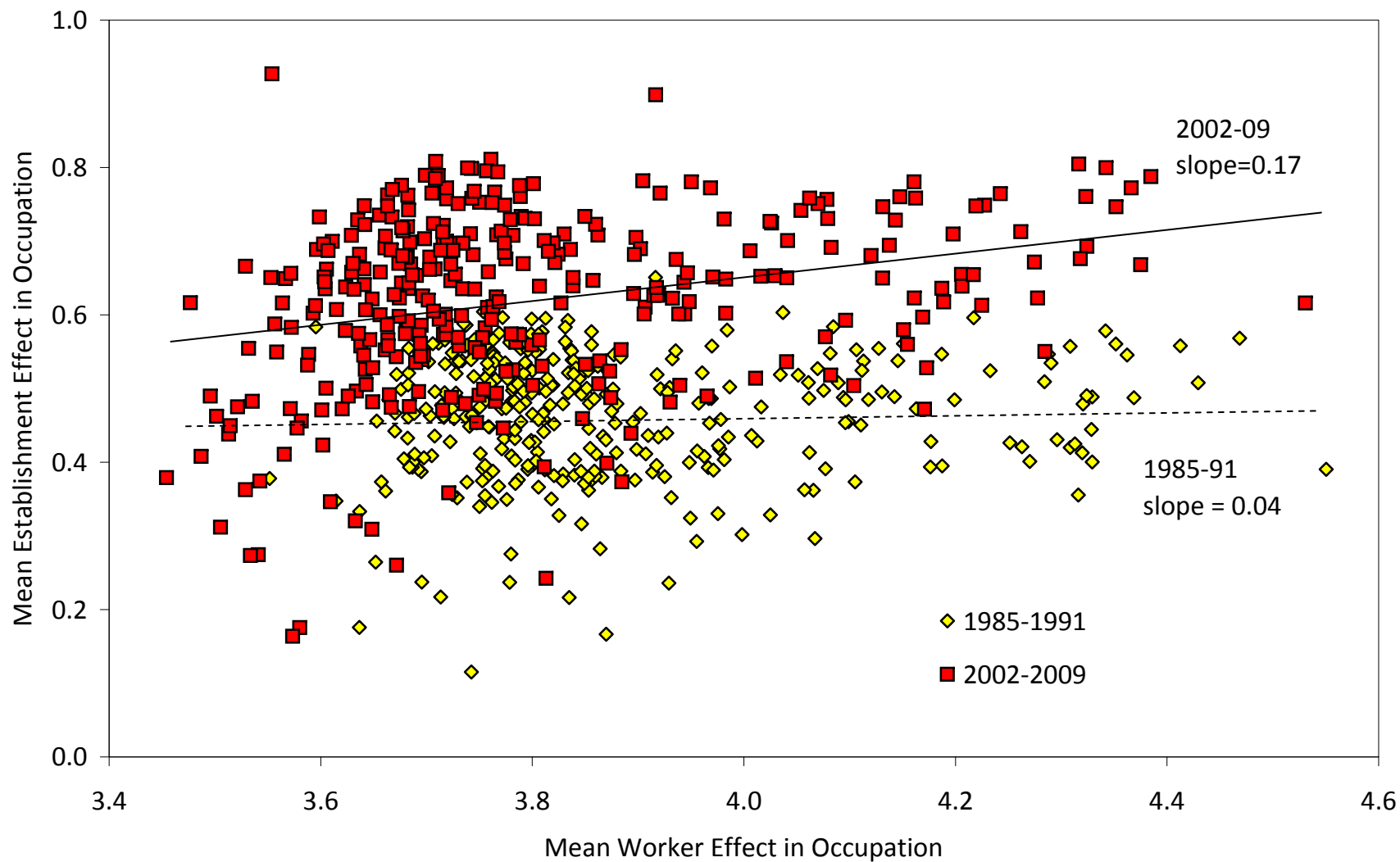


Figure 15: Standard Deviation of Establishment Effects and Fraction Covered by Collective Agreements, by Birth Year of Establishment

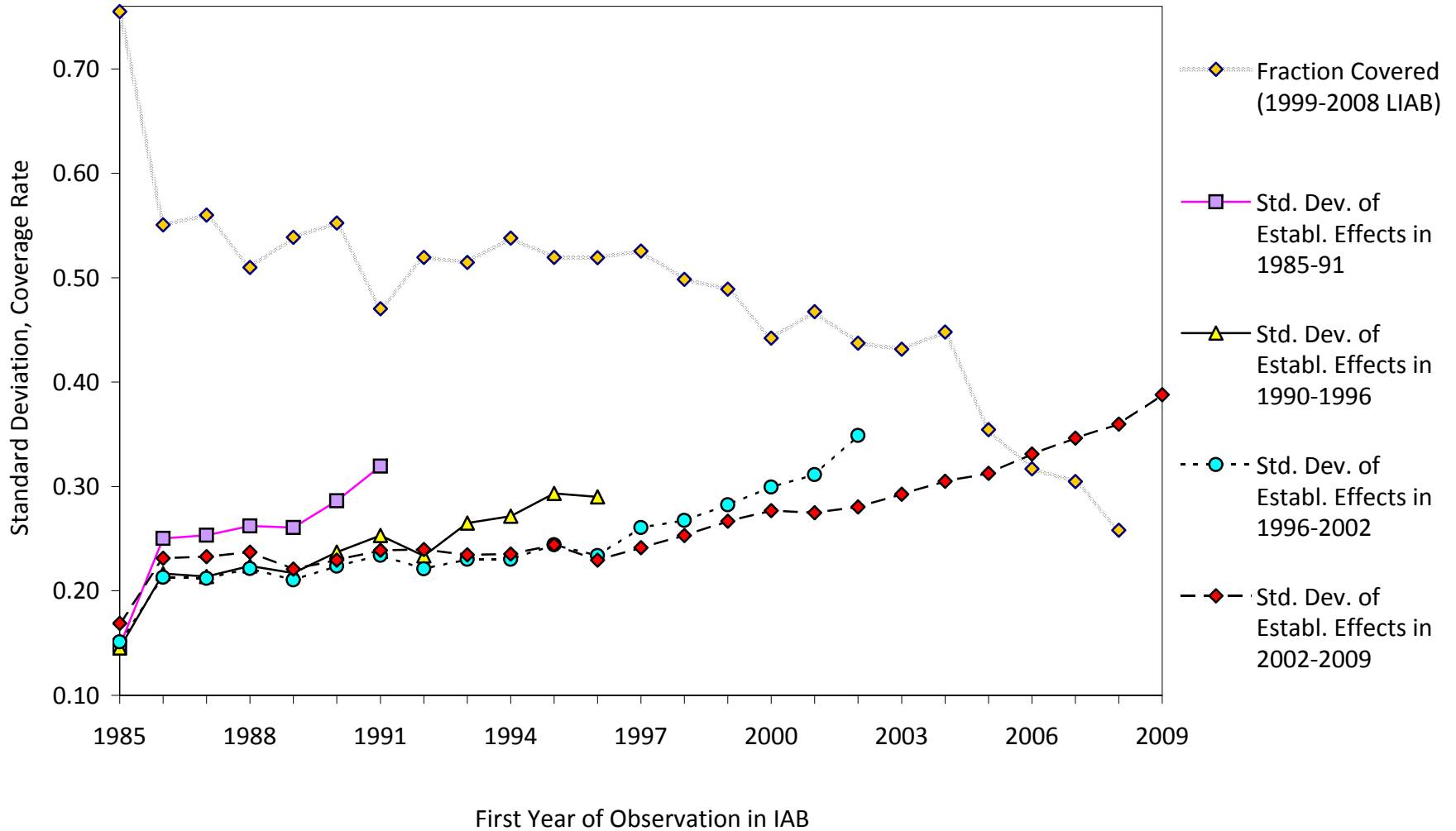
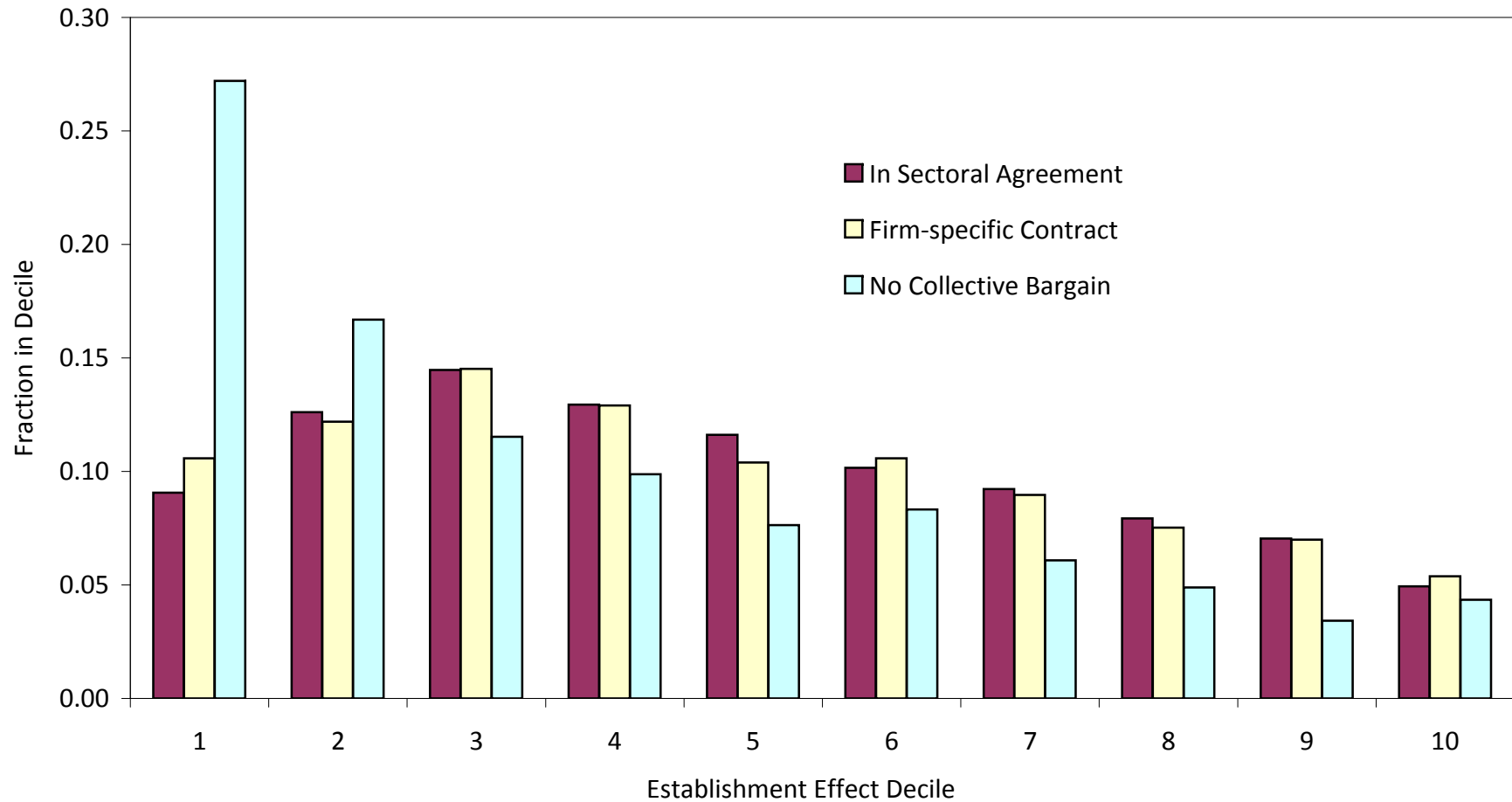


Figure 16: Distribution of Establishment Effects by Collective Bargaining Status
(Based on Establishment Effects for 1996-2002, Bargaining Status in 2000 LIAB)



note: sample includes 7,080 establishments in 2000 Wave of LIAB that can be linked to IAB

Figure 17a: Survivor Functions for Establishments First Observed in 1989
By Quartile of Estimated Establishment Effect

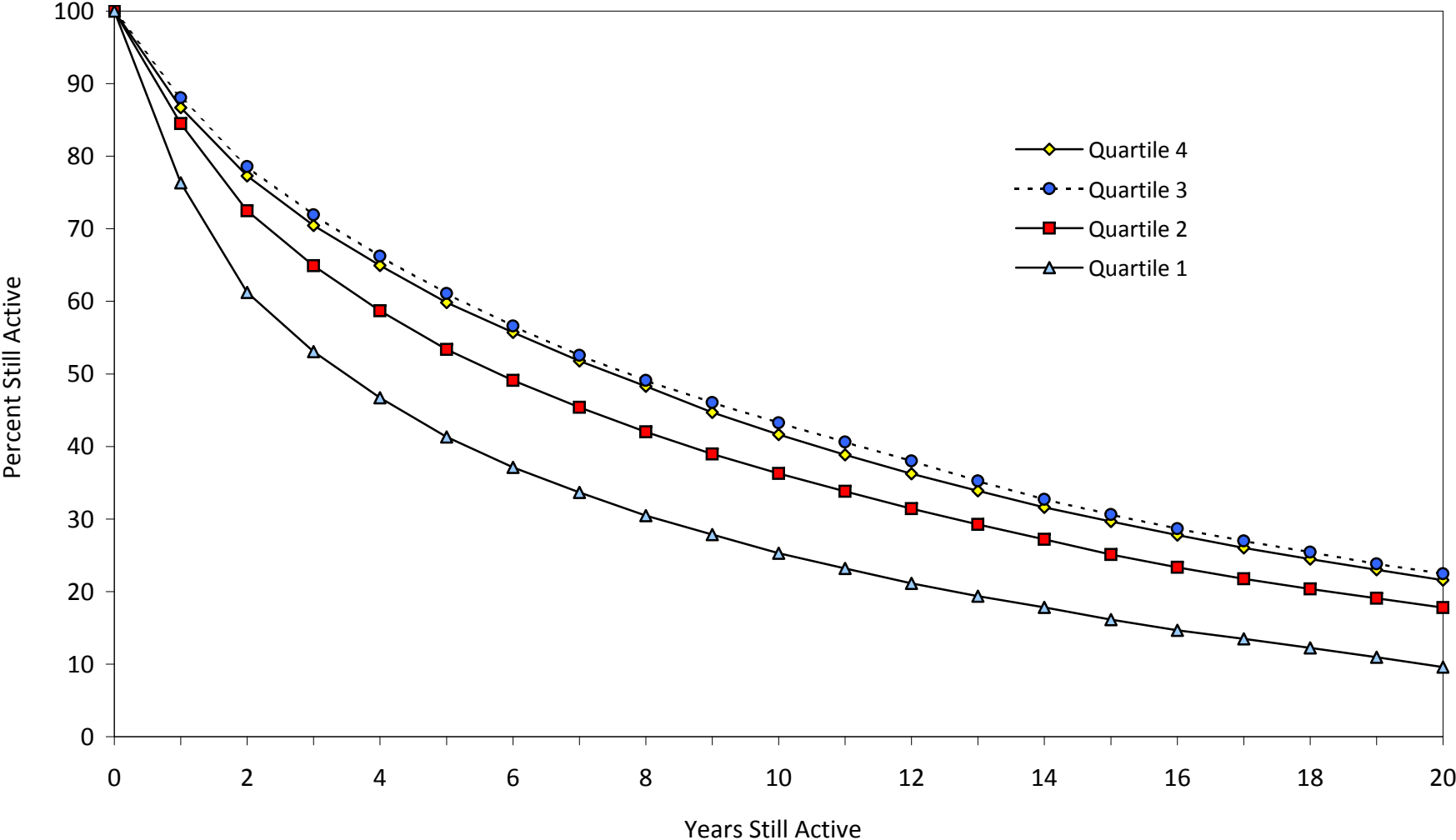


Figure 17b: Survivor Functions for Jobs Initiated in 1989
By Quartile of Estimated Establishment Effect

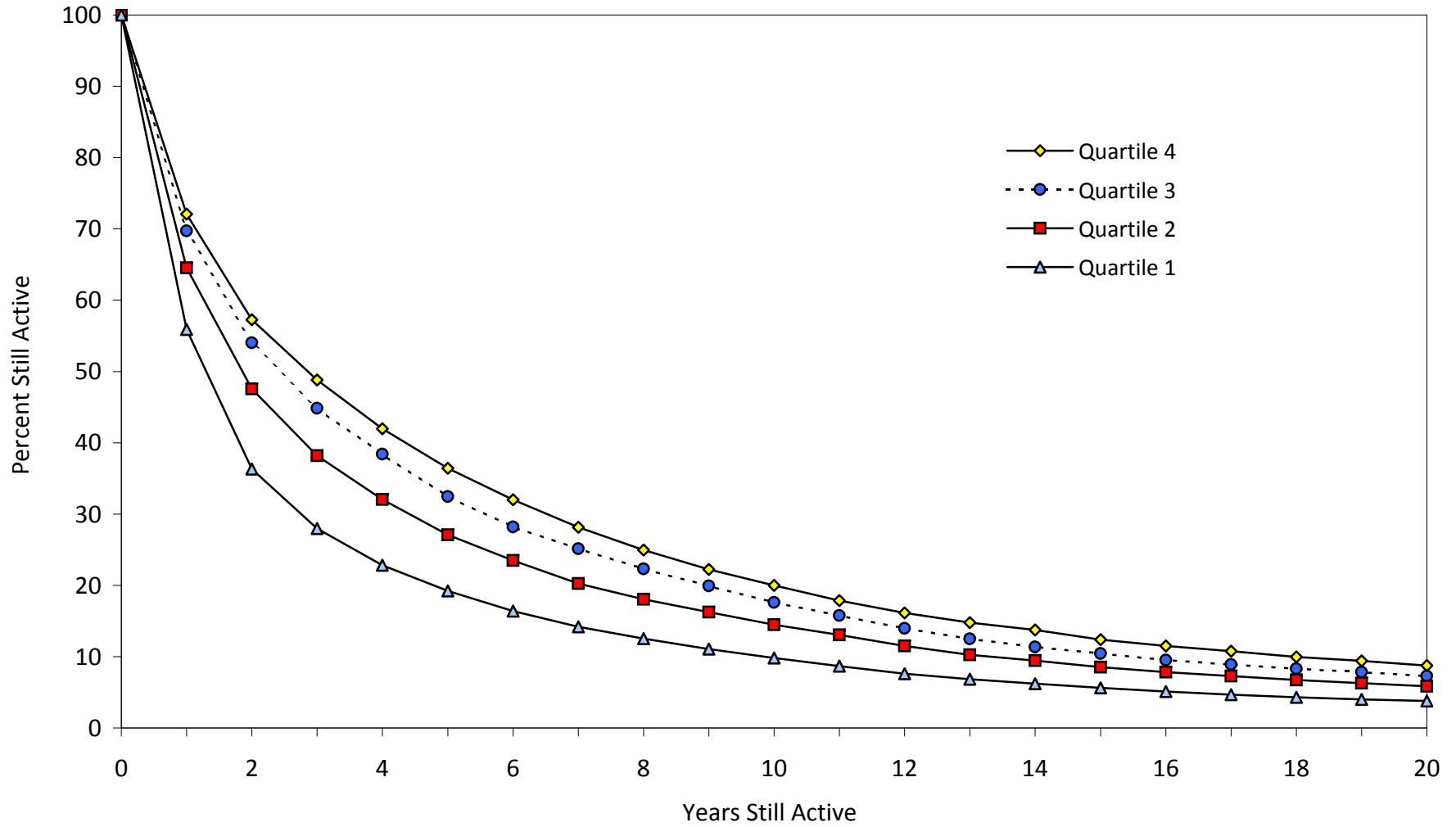


Table 1: Summary Statistics for Overall Sample and Individuals in Largest Connected Set

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

Notes: sample includes full-time male workers employed in non-marginal jobs and not currently in training. Daily wage is imputed for censored observations (see text).

Table 2: Estimation Results for AKM Model, Fit by Interval

	Interval 1 1985-1991 (1)	Interval 2 1990-1996 (2)	Interval 3 1996-2002 (3)	Interval 4 2002-2009 (4)
<i>Person and Establishment Parameters:</i>				
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
<i>Summary of Parameter Estimates:</i>				
Std. dev. of person effects	0.289	0.304	0.327	0.357
Std. dev. of establ. effects	0.159	0.172	0.194	0.230
Std. dev. of Xb	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
<i>Comparison Match Model</i>				
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
<i>Addendum</i>				
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: see notes to Table 1. 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.

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

Table 3: Decomposition of the Rise in Wage Inequality

	Interval 1		Interval 4		Change in Variance (percent of total change)*	
	Var. Component (1)	Share of Total (2)	Var. Component (3)	Share of Total (4)	(5)	
Total variance of log wages	0.137	100.0	0.249	100.0	0.112	100
<i>Components of Variance:</i>						
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
<i>Counterfactuals for Variance of log wages in interval 4:</i>						
1. No rise in correl. of person/estab. effects	0.137		0.213		0.077	69
2. No rise in var. of establ. 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 1. Calculations based on estimated AKM models summarized in Table 2.

*First entry is change in variance/variance component from interval 1 to interval 4. Second entry shows ratio of change in the variance component to the total change in variance of wages in row 1 (as a percent).

Table 4: Decomposition of Changes in Relative Wages by Education Level

	Change in Mean Log Wage Relative to Apprentices (1)	Change in Mean Person Effect (2)	Change in Mean Establishment Effect (3)	Remainder (4)
<i>Highest Education Qualification:</i>				
1. Missing/none	-14.6	1.8	-12.2	-4.2
2. Secondary Only	-10.5	-0.1	-6.3	-4.1
3. Apprentices	0.0	0.0	0.0	0.0
4. Some Post-Secondary	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 from 1985-1991 to 2002-2009. Remainder (column 4) represents changing relative contribution of Xb component.

Table 5: 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 Share (5) (6)	
Panel A: Between Occupations (342 3-digit occupations)						
Std. dev. of mean log wages	0.217	0.221	0.246	0.279	0.031	100
Std. dev. of mean person effects	0.183	0.195	0.195	0.207	0.009	31
Std. dev. of mean establ. effects	0.089	0.094	0.102	0.126	0.008	26
Corr. mean person/establ. effects	0.082	0.111	0.284	0.296	0.013	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 establ. effects	0.104	0.110	0.108	0.121	0.004	19
Corr. mean person/establ. effects	0.242	0.301	0.422	0.403	0.008	42

Notes: decompositions based on estimated AKM models summarized in Table 2. Individuals are assigned one occupation per interval; establishments are assigned one industry per interval.

* Entry in column 5 represents change in variance/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.

Data Appendix

1. Creation of person-year data set

The IAB data file consists of information on employment spells at a given establishment (or firm) within a calendar year. Each spell 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 at the same employer in a given year into a single person-firm-year record, summing total earnings at each employer. Line 1 of the table below shows the number of spell records in the IAB data file for full-time male 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). Line 2 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 line 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 no trend over our sample period (see line 5). Line 3 of the table below shows the number of person-year observations in our final data set.

	1985	1997	2009
1. Number spells (FT, non-marginal jobs, men age 20-60)	13,987,548	14,919,079	14,911,559
2. Number person-firm- year observations	13,181,917	13,968,286	13,456,305
3. Number of person- year observations (highest paying job)	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

Appendix Table 1 presents some summary statistics for our final sample, including the number of person observations in each year (which ranges from 11.9 to 14 million), average real annual earnings and real daily wage, and the mean and standard deviation of the log of daily wages. We use the consumer price index with a 1995=100 base to deflate all nominal values.

2. Tobit models to impute the upper tail of wages.

As shown in Appendix Figure 2, from 9 to 14 percent of person-year observations in our data set have average daily wages that are censored at the Social Security maximum. We follow Dustmann, Ludsteck and Schonberg (2009) and fit a series of Tobit models to log daily wages. We then impute an uncensored value for each censored value, using the estimated parameters of these models and a random draw from the associated (left-censored) distribution.¹

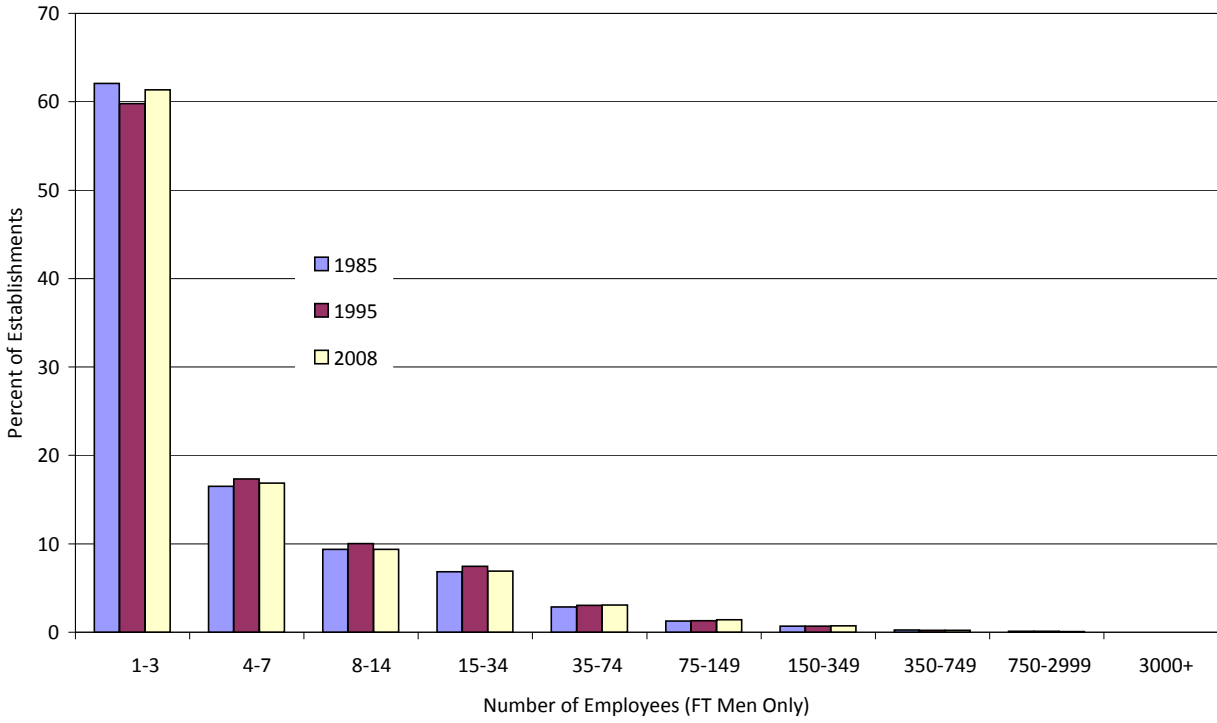
Since we are fitting models that include both a person and year effect, we want the imputation model to reflect individual and job-specific components of the wage. We therefore construct, for each individual in each year, the mean of his log wage in all other periods, and the fraction of other years 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 2 shows the coefficient estimates for models for 40-49 year old apprentices in 1985, 1997, and 2009.

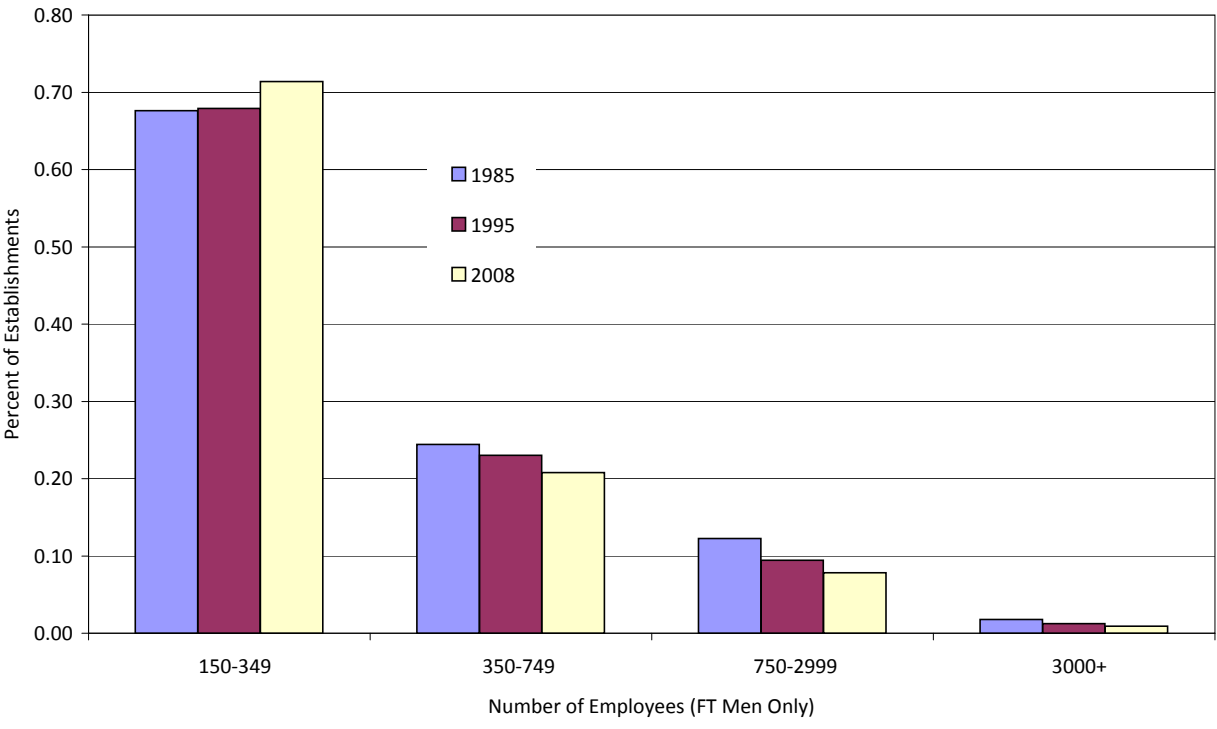
¹ Specifically, we impute an upper tail as follows. Suppose that the estimated Tobit model for y (the log of wages) has $y \sim N(X\beta, \sigma)$, and consider a censored observation, such that $y \geq c$, where c is the censoring point. Let $k = \Phi[(c - X\beta)/\sigma]$, where Φ represents the standard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

Appendix Figure 2 shows the gaps between the mean and standard deviation of log wages when we use the imputed values for censored observations and when we do not. The use of imputed values leads to a 0.02-0.04 higher value for the mean log wage, and a 0.03 to 0.06 higher value for the standard deviation of log wages. The gaps are particularly notable in 1992 and 2002, when the censoring rate was relatively high.

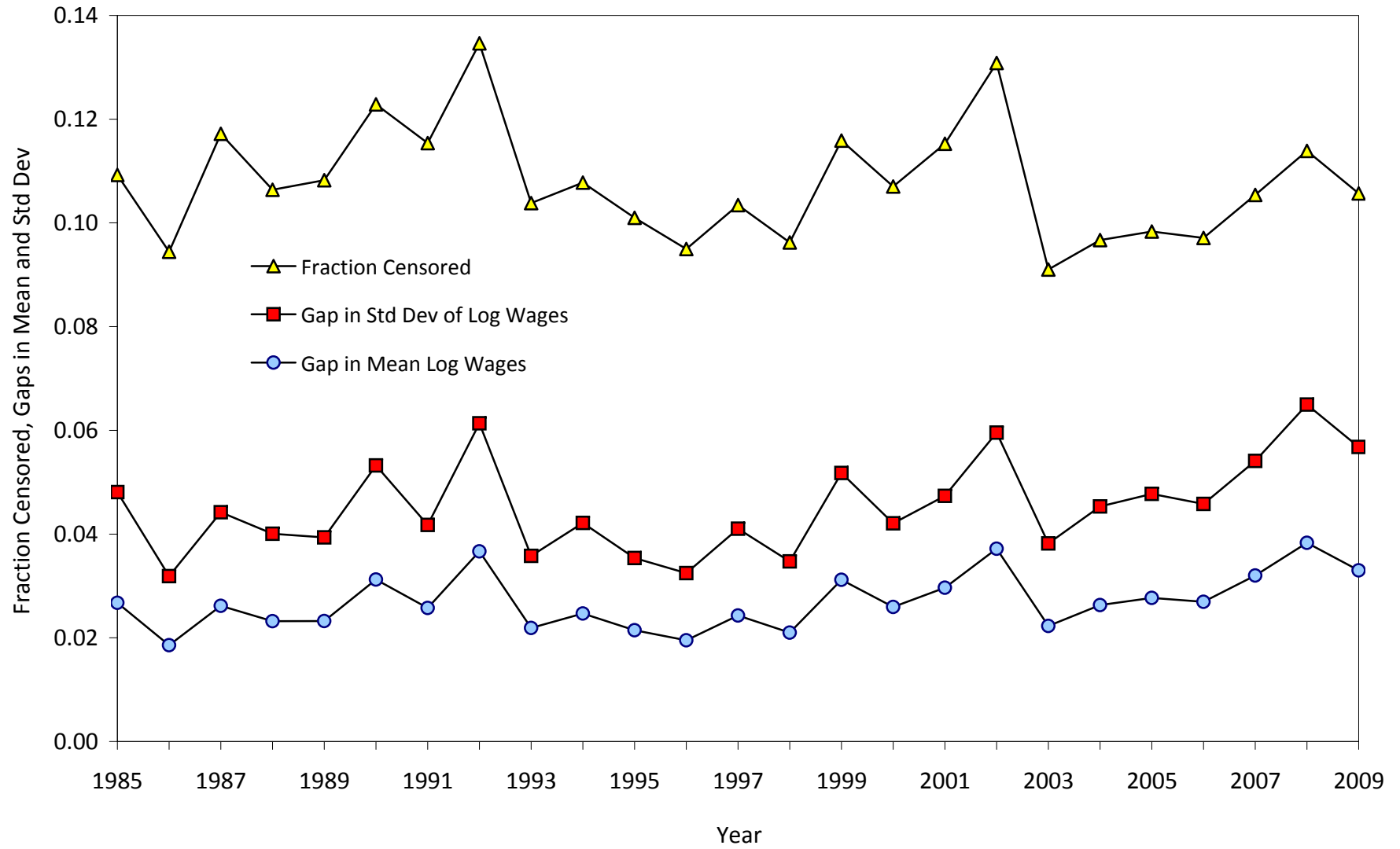
Appendix Figure 1: Establishment Size Distributions
 Panel A: Overall Distribution



Panel B: Upper Tail Only



Appendix Figure 2: Censoring of Wages and Gaps Between Imputed and Censored Series



Appendix Table 1: Basic Summary Statistics for IAB Sample

	Number Observations	Annual Earnings (real Euros)	Daily Wage (real Euro/day)		Log Real Daily Wage	
			Mean	Std. Dev	Mean	Std. Dev
1985	11,980,159	24,088	72.56	23.13	4.221	0.387
1986	12,275,222	25,092	75.24	24.45	4.255	0.396
1987	12,360,859	25,793	76.92	24.89	4.277	0.398
1988	12,467,192	26,403	78.19	25.93	4.291	0.404
1989	12,715,727	26,506	78.28	25.81	4.293	0.401
1990	13,289,988	26,967	79.72	26.02	4.312	0.398
1991	13,688,681	26,750	80.92	26.18	4.327	0.397
1992	13,991,600	27,052	81.12	26.33	4.329	0.398
1993	13,643,435	27,481	81.41	27.26	4.330	0.405
1994	13,248,077	27,002	81.22	27.95	4.325	0.412
1995	13,101,809	27,874	82.56	28.56	4.340	0.415
1996	12,813,804	27,251	82.42	29.02	4.335	0.423
1997	12,661,995	26,813	81.42	29.56	4.318	0.438
1998	12,677,201	27,271	82.29	30.59	4.325	0.448
1999	12,719,922	27,259	82.94	31.26	4.329	0.458
2000	12,930,046	27,388	82.95	31.60	4.327	0.464
2001	12,906,292	27,398	82.89	31.75	4.325	0.469
2002	12,610,572	27,425	82.86	31.89	4.323	0.475
2003	12,336,685	28,222	84.95	35.57	4.335	0.501
2004	12,083,070	28,100	84.01	35.73	4.320	0.510
2005	11,857,526	27,960	83.47	36.07	4.310	0.519
2006	11,977,347	27,642	82.73	36.44	4.297	0.528
2007	12,222,754	27,255	81.68	36.12	4.282	0.533
2008	12,323,655	27,169	80.99	35.72	4.274	0.533
2009	12,104,223	27,179	81.35	36.23	4.277	0.535

Notes: sample includes men in West Germany age 20-60, working full time in non-marginal jobs. Earnings data for each person is based on earnings for the year at the employer who paid the most in that year. Individuals who earned less than 10 euros per day are excluded. Daily wage is censored at Social Security maximum for the corresponding year (total earnings are likewise censored).

Appendix Table 2: Tobit Models for Apprentices, Age 40-49 in Selected Years

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

Notes: standard errors in parentheses. 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 3: Mean Log Wages Before and After Job Change, for Movers with Two or More Years of Wage Data Before and After Job Change

Origin/destination quartile*	Number of Changes: (1)	Mean Log Wages of Movers				4 Year Change	
		2 years before (2)	1 year before (3)	1 year after (4)	2 years after (5)	Raw (6)	Adjusted** (7)
<u>Interval 1: 1985-1991</u>							
1 to 1	333,648	4.003	4.025	4.085	4.113	0.110	0.000
1 to 2	206,251	4.063	4.085	4.207	4.248	0.185	0.075
1 to 3	136,119	4.064	4.087	4.271	4.323	0.260	0.150
1 to 4	82,193	4.102	4.132	4.380	4.444	0.342	0.232
2 to 1	125,376	4.160	4.178	4.144	4.175	0.015	-0.072
2 to 2	204,787	4.229	4.251	4.286	4.316	0.087	0.000
2 to 3	158,360	4.258	4.278	4.359	4.395	0.137	0.051
2 to 4	86,038	4.298	4.324	4.474	4.529	0.231	0.144
3 to 1	59,334	4.245	4.261	4.163	4.194	-0.051	-0.153
3 to 2	91,474	4.315	4.337	4.333	4.371	0.056	-0.046
3 to 3	173,160	4.384	4.409	4.452	4.486	0.102	0.000
3 to 4	136,569	4.460	4.487	4.594	4.635	0.175	0.073
4 to 1	30,110	4.373	4.396	4.252	4.284	-0.089	-0.220
4 to 2	41,079	4.459	4.488	4.447	4.487	0.028	-0.103
4 to 3	91,177	4.552	4.584	4.596	4.633	0.080	-0.051
4 to 4	290,921	4.678	4.710	4.777	4.809	0.131	0.000
<u>Interval 4: 2002-2009</u>							
1 to 1	541,307	3.880	3.873	3.867	3.875	-0.005	0.000
1 to 2	197,982	4.018	4.017	4.165	4.182	0.164	0.054
1 to 3	88,768	3.999	4.015	4.291	4.325	0.325	0.215
1 to 4	49,167	4.056	4.083	4.468	4.521	0.465	0.355
2 to 1	208,184	4.202	4.190	4.021	4.030	-0.171	-0.258
2 to 2	333,219	4.315	4.309	4.305	4.309	-0.007	0.000
2 to 3	137,528	4.381	4.377	4.437	4.456	0.075	-0.012
2 to 4	59,080	4.473	4.479	4.625	4.673	0.200	0.114
3 to 1	73,218	4.338	4.333	4.024	4.042	-0.296	-0.398
3 to 2	133,606	4.456	4.458	4.390	4.401	-0.055	-0.157
3 to 3	275,521	4.550	4.552	4.563	4.568	0.018	0.000
3 to 4	150,989	4.678	4.690	4.768	4.805	0.127	0.025
4 to 1	32,664	4.518	4.526	4.139	4.167	-0.351	-0.482
4 to 2	47,193	4.655	4.671	4.534	4.560	-0.095	-0.226
4 to 3	127,276	4.744	4.763	4.721	4.749	0.005	-0.126
4 to 4	546,855	4.937	4.957	4.973	5.021	0.083	0.000

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.

* 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.

** Four year wage change for origin-destination group minus change for job changers who remain in same quartile.

Appendix Table 4: Estimation Results for AKM Model, Fit by Interval for Apprentices Only

	Interval 1 1985-1991 (1)	Interval 2 1990-1996 (2)	Interval 3 1996-2002 (3)	Interval 4 2002-2009 (4)
<i><u>Person and Establishment Parameters:</u></i>				
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
<i><u>Summary of Parameter Estimates:</u></i>				
Std. dev. of person effects	0.241	0.249	0.271	0.285
Std. dev. of establ. effects	0.152	0.159	0.170	0.202
Std. dev. of Xb	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
<i><u>Comparison Match Model</u></i>				
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 2.

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

	Interval 1		Interval 4		Change in Variance (pct. of total change)	
	Var. Component (1)	Share of Total (2)	Var. Component (3)	Share of Total (4)	(5)	
Total variance of log wages	0.108	100.0	0.150	100.0	0.043	100
<u>Components of Variance:</u>						
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 in interval 4:						
1. No rise in correl. of person/estab. effects	0.108		0.143		0.035	82
2. No rise in var. of establ. 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 3. Calculations based on estimated AKM models summarized in Appendix Table 4.

Appendix Table 6: Number and Characteristics of New Establishments

First Year in IAB (1)	Number (2)	Employment (FT Men)		Pct. of Establishments:		Log Real Wage		Residual Wage		Human Capital Index	
		Mean (3)	Std Dev (4)	Always 1 worker (5)	Always >3 workers (6)	Mean (7)	Std Dev (8)	Mean (9)	Std Dev (10)	Mean (11)	Std Dev (12)
1985	951,170	12.4	123.7	22.4	26.3	4.044	0.443	-0.224	0.373	1.010	0.222
1986	122,770	3.3	11.7	43.7	7.4	3.848	0.539	-0.370	0.454	0.998	0.302
1987	98,037	3.7	17.1	42.9	8.4	3.848	0.534	-0.376	0.457	0.999	0.293
1988	95,999	3.9	20.5	42.1	9.0	3.862	0.531	-0.376	0.461	1.002	0.279
1989	94,434	4.2	23.7	41.0	9.8	3.900	0.526	-0.355	0.456	1.007	0.271
1990	103,813	4.2	18.9	40.5	10.1	3.910	0.519	-0.358	0.457	1.009	0.263
1991	105,670	4.0	19.4	41.0	10.3	3.902	0.520	-0.369	0.468	1.010	0.259
1992	117,003	5.0	26.2	38.3	12.8	3.917	0.500	-0.366	0.443	1.013	0.265
1993	107,323	4.3	29.1	40.1	11.7	3.909	0.513	-0.353	0.451	1.008	0.261
1994	107,704	4.4	20.4	40.3	12.1	3.905	0.514	-0.353	0.455	1.008	0.259
1995	109,800	4.5	26.3	40.4	11.9	3.898	0.520	-0.359	0.462	1.008	0.259
1996	107,663	5.1	22.2	39.1	13.1	3.891	0.519	-0.355	0.464	1.005	0.255
1997	110,277	4.7	43.2	39.6	12.9	3.857	0.536	-0.375	0.481	1.003	0.260
1998	116,886	4.7	26.6	40.1	12.7	3.868	0.538	-0.370	0.479	1.004	0.269
1999	127,818	4.6	25.5	40.1	13.1	3.854	0.547	-0.371	0.491	1.002	0.273
2000	122,357	4.6	20.7	40.7	13.7	3.861	0.562	-0.354	0.499	1.000	0.282
2001	117,078	4.7	26.1	42.0	14.1	3.848	0.578	-0.363	0.510	0.999	0.288
2002	110,888	4.6	32.5	43.5	13.9	3.818	0.580	-0.378	0.513	0.996	0.290
2003	107,526	4.5	17.1	43.5	15.1	3.800	0.585	-0.381	0.514	0.994	0.296
2004	107,681	4.4	22.8	44.7	14.9	3.762	0.588	-0.398	0.520	0.989	0.301
2005	103,718	4.2	19.8	45.7	14.9	3.745	0.591	-0.404	0.524	0.987	0.310
2006	103,802	4.1	19.4	47.4	15.1	3.734	0.591	-0.401	0.521	0.984	0.317
2007	106,549	4.0	17.3	50.5	15.9	3.711	0.626	-0.416	0.553	0.982	0.326
2008	98,890	3.6	20.5	55.0	17.1	3.730	0.620	-0.392	0.538	0.981	0.341

Notes: employment and wage data pertain to full time non-marginal male workers only. Residual wage (columns 9-10) is wage, deviated from prediction from a standard Mincer-type model fit separately by year. Human capital index (columns 11-12) is predicted wage from a standard Mincer-type model fit to all sample years (index is normalized to have value of 1 across all years).

Appendix Table 7: Probit, OLS and Tobit Models for Establishment Survival

	Probit: Survive 5 yrs. (1)	Probit: Survive 10 yrs. (2)	OLS Survival Time (3)	Tobit Survival Time (4)
<u>Panel A: Establishments First Observed 1985-1991 (n=1,221,098)</u>				
1. Establishment Effect (interval 1)	0.256 (0.004)	0.265 (0.004)	1.782 (0.023)	2.004 (0.030)
<i>[coefficient/σ]</i>				0.197
2. Mean Person Effect (interval 1)	0.259 (0.005)	0.234 (0.005)	1.619 (0.032)	1.927 (0.042)
<i>[coefficient/σ]</i>				0.189
3. Standard deviation of latent time (σ)	--	--	7.996	10.182 (0.008)
Mean of Dep. Var.	0.674	0.521	12.475	
Fraction Censored	--	--	.250	
<u>Panel B: Establishments First Observed 1990-1996 (n=603,417)</u>				
1. Establishment Effect (interval 2)	0.317 (0.005)	0.357 (0.005)	1.655 (0.020)	2.045 (0.025)
<i>[coefficient/σ]</i>				0.284
2. Mean Person Effect (interval 2)	0.367 (0.006)	0.348 (0.007)	1.821 (0.029)	2.234 (0.036)
<i>[coefficient/σ]</i>				0.310
3. Standard deviation of latent time (σ)	--	--	5.878	7.212 (0.008)
Mean of Dep. Var.	0.495	0.326	7.630	
Fraction Censored	--	--	0.211	
<u>Panel C: Establishments First Observed 1996-2002 (n=646,729)</u>				
1. Establishment Effect (interval 3)	0.279 (0.004)		0.895 (0.012)	1.245 (0.017)
<i>[coefficient/σ]</i>				0.243
2. Mean Person Effect (interval 3)	0.338 (0.006)		1.126 (0.017)	1.543 (0.023)
<i>[coefficient/σ]</i>				0.302
3. Standard deviation of latent time (σ)	--		3.849	5.115 (0.006)
Mean of Dep. Var.	0.449		5.710	
Fraction Censored	--		0.286	

Notes: standard errors in parentheses. All models include additional controls for log of number of employees, mean schooling of employees, and dummies for year first observed in IAB. Note that sample in panel A includes 748,192 establishments that were in operation in 1985, and whose duration is left censored.

Appendix Table 8: Probit, OLS and Tobit Models for Job Survival (Fit to 5% Sample of Jobs)

	Probit: Survive 3 yrs. (1)	Probit: Survive 5 yrs. (2)	OLS Survival Time (3)	Tobit Survival Time (4)
<u>Panel A: Jobs First Observed 1985-1991 (n=1,264,498)</u>				
1. Establishment Effect (interval 1)	0.488 (0.008)	0.444 (0.008)	1.309 (0.034)	1.339 (0.037)
<i>[coefficient/σ]</i>				0.206
2. Person Effect (interval 1)	0.764 (0.005)	0.664 (0.005)	2.508 (0.024)	2.530 (0.024)
<i>[coefficient/σ]</i>				0.389
3. Standard deviation of latent time (σ)	--	--	6.148	6.504 (0.004)
Mean of Dep. Var.	0.505	0.389	5.451	
Fraction Censored	--	--	0.059	
<u>Panel B: Jobs First Observed 1990-1996 (n=854,522)</u>				
1. Establishment Effect (interval 2)	0.700 (0.008)	0.712 (0.009)	1.938 (0.026)	2.039 (0.028)
<i>[coefficient/σ]</i>				0.411
2. Person Effect (interval 2)	0.816 (0.006)	0.713 (0.006)	2.286 (0.020)	2.360 (0.020)
<i>[coefficient/σ]</i>				0.476
3. Standard deviation of latent time (σ)	--	--	4.620	4.962 (0.004)
Mean of Dep. Var.	0.358	0.247	3.372	
Fraction Censored	--	--	0.074	
<u>Panel C: Jobs First Observed 1996-2002 (n=857,530)</u>				
1. Establishment Effect (interval 3)	0.792 (0.007)	0.815 (0.008)	1.729 (0.017)	1.952 (0.019)
<i>[coefficient/σ]</i>				0.512
2. Person Effect (interval 2)	0.612 (0.005)	0.508 (0.006)	1.413 (0.013)	1.544 (0.015)
<i>[coefficient/σ]</i>				0.405
3. Standard deviation of latent time (σ)	--	--	3.327	3.810 (0.003)
Mean of Dep. Var.	0.344	0.241	2.735	
Fraction Censored	--	--	0.141	

Notes: standard errors in parentheses. All models include additional controls for log of number of employees, dummies for schooling levels of worker, and dummies for job start year. Note that sample in panel A includes 583,456 jobs that were in progress in 1985, and whose duration is left censored.