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Field of Study and Earnings Inequality among the Highly Educated: 1993-2010

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Abstract

Field of study specializes individuals' human capital in ways that might be either substitutable or complementary to technological change. We study changes in the earnings distribution of the college-educated population between 1993 and 2010 using the National Survey of College Graduates. After documenting changes that increase earnings inequality, we decompose them into composition and wage-structure effects. We find that composition effects account for virtually none of the growth of inequality and, in fact, are surprisingly small, even after we incorporate field of study into the decomposition. We conclude with speculation about why large inter-field changes in earnings did not lead to comparable changes in the flow of entrants.

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1 Introduction

The earnings distribution in the United States has been spreading since the 1980s (Bound and Johnson 1992, Katz and Murphy 1992, Levy and Murnane 1992 and Juhn et al. 1993). During the 1980s the spreading took place across the entire distribution—the middle quantiles of the earnings distribution grew faster than bottom quantiles while quantiles at the top grew faster than those in the middle. In the 1990s and 2000s, however, the changes became increasingly concentrated in the top end of the distribution (Piketty and Saez 2003, Autor, Katz and Kearney 2008, Lemiuex 2008).

These distributional changes have been closely associated with widening gaps between skill levels. Mincer (1997) and Deschnes (2001) show that during the 1990s (log) wages became an increasingly convex function of years of education—workers with the most education have pulled away from the moderately educated at a faster rate than the moderately educated have pulled away from those with little education. Since the early 1990s this has been especially true of the gap between college graduates and those with post-graduate degrees. Beyond this, little is known about the drivers of inequality in this high skill group. Yet college-educated workers are a large and growing segment of the labor force. In 1993, 28 percent of employed adults (over age 25) had a bachelor's degree or higher.[1] By 2010, the figure was 36 percent.

In this paper we use the National Survey of College Graduates (NSCG) to document changes in the earnings distribution for college-educated workers between 1993 and 2010 and to decompose these changes into composition and wage-structure effects. The NSCG is uniquely suited to this investigation, providing large, nationally representative, samples for 1993, 2003, and 2010 of individuals with at least a bachelor's degree. The questionnaires elicit far more information about this segment of the labor force than any other data source, including salary, type of degree (bachelor's, master's, Ph.D., professional), field of study for all degrees, and dates when degrees were received.

We first summarize changes in the earnings distributions for college educated men and women between 1993 and 2010. We find that between 1993 and 2010 the gap the between 90th percentile of the earnings distribution and the 10th percentile of the earnings distribution widened by about 16 log points for both genders. For men, it appears that most of the growth of inequality was concentrated below the median of the earnings distribution (i.e. the 50-10 gap widened more than the 90-50 gap). For women, on the other hand, it appears that most of the growth of inequality was concentrated above the median of the earnings distribution (i.e. the the 90-50 gap widened more than the 50-10

[1] Authors' calculations from BLS tabulations.

gap).

Second, we apply methodology developed by Chernozhukov, Fernandez-Val, and Melly (2014; CFM hereafter) to investigate how much of the changes in earnings inequality can be accounted for by composition effects stemming from changes in the distributions of experience and level of educational attainment—the characteristics traditionally used to study earnings inequality. We find that little, if any, of the growth in inequality among college graduates is due to changes in these characteristics; the growth of inequality is almost entirely due to wage-structure effects according to these decompositions. Since the distribution of educational attainment in a labor force has a great deal of inertia, we consider whether effects are visible when we consider only individuals with less than ten years of potential experience (subsequently the "entering cohorts") and those who exit between surveys ("exiting cohorts"). The entering female cohort, who have pursued advanced degrees at a higher rate than their predecessors, shows a small composition effect. However, the effect is quite uniform across the quantiles and thus does not contribute significantly to changing inequality. There is almost no discernable composition effect for the exiting cohorts.

Our most important contribution is that we then augment the usual set of characteristics by incorporating field of study into the decomposition exercises. Field of study can be interpreted as a specialization of the human capital of graduates. Technological change is likely to complementary to some fields' human capital, but substitutable for other fields. Thus there is a strong presumption that skill-related technological change rewards some fields at the expense of other fields, whether it is simply a bias toward certain skills (e.g., STEM fields) or polarization. Altonji, Kahn and Speer (2014) argue the latter and find that the substantial widening of earnings differentials among undergraduate majors is related to the task composition of the occupations of individuals in the major.[2] We ask a complementary question: How much did the changing distribution of fields of study contribute to the shifts in the earnings distribution?

When fields are incorporated, we find some evidence of additional composition effects, but these effects appear to stem more from changes in the distribution of characteristics induced by exit, rather than changes induced by the choices of entering cohorts. Moreover, none of the change in inequality is associated with entering cohorts. We conclude with a discussion of potential explanations for the apparently tepid response of entering cohorts to wage signals.

[2] Also Altonji, Kahn and Speer (2013) find that the relative return to major is magnified during a recession.

2 Data

The 1993, 2003, and 2010 National Surveys of College Graduates conducted for the National Science Foundation capture representative snapshots of the college-educated segment of the U.S. population.[3] The samples were drawn from respondents to the 1990 or 2000 Census long-form or 2009 American Community Survey who reported having received four-year degrees. For the present purpose, the NSCGs are superior to the Current Population Survey or American Community Survey for four reasons. First, they provide more detail about respondents' tertiary education, including the dates degrees were received and the field of study for each degree.[4] Second, Black, Sanders and Taylor (2003a) argue that the education data are more accurate in the NSCG than in the CPS or Census. Third, the surveys are much larger than the college-educated subset of the CPS, allowing us to exploit the available detail effectively. The 1993, 2003, and 2010 surveys include 148,905, 100,402, and 77,188 individuals, respectively. Fourth, Bollinger and Hirsch (2006, 2013) have argued that nonresponse to earnings questions is a serious problem with the CPS earnings data. Although the NSCG uses hot-deck methods to impute missing earnings data, item nonresponse rates for earnings are much lower—about 10 percent for NSCG salary compared to 20-30 percent for CPS earnings—and imputation cells are more detailed.[5]

Our analysis uses "basic annual salary" on the respondent's principle job. The survey question explicitly excludes other forms of compensation.[6] The question refers to current annual salary on the job held during a specific week in April 1993, October 2003, or October 2010, i.e., it is a point-in-time measurement and there is no reference period per se.[7] The salary data are top-coded, but we conduct our analysis only up to the 90th percentile, which is well below the top-codes. The salary data are deflated to 1993, second quarter, dollars using the personal consumption expenditures price index.

[3] Between these dates the NSCG is restricted to science and engineering graduates.

[4] Beginning in 2009 the ACS asks for field of first undergraduate degree, but does not obtain this information for graduate degrees.

[5] The NSCG item nonresponse rate is from `http://www.nsf.gov/statistics/srvygrads/#sd` accessed August 21, 2014.

[6] The 2003 and 2010 surveys also include a question about total earned income for calendar 2002 and 2009, but it is impossible to identify full-year, full-time earners and item non-response rates were higher.

[7] There is a small inconsistency in the salary variable between 1993 and the other two years. In all three surveys respondents are asked about the *annual* basic salary on their principle job as of a particular week. In the 2003 and 2010 surveys, but not the 1993 survey, respondents were also asked how many weeks that salary covers, and the salary data were annualized on this basis. In the 2003 and 2010 surveys the overwhelming majority of those working full-time during the reference week who reported that their salary covered significantly less than 52 weeks were teachers.

Our sample includes individuals who worked full-time at their principal job and were under age 65.[8] We exclude self-employed individuals (10-12 percent of full-time workers in our age range) because the salary data are not comparable to the data for employees: The question about salary instructs self-employed individuals to report all earned income for an unspecified reference period, while it explicitly excludes bonuses, overtime, etc. for employees. We also exclude people who reported receiving their highest degree before age 15.[9]

We define potential experience as months since completion of highest degree. Throughout, we use sampling weights provided by NSF.

3 Overview of college graduates' earnings

Table 1 shows the breakdown of degree types by year. In all years the majority of the men and women in our samples had earned a bachelor's degree and no higher, but what is important for the current study is the change over time in the share of men and women in each degree type. The most significant such change is that more women have pursued advanced degrees in recent years. In 1993 only 33.8 percent of the women had an advanced degree. By 2010 that number had risen to 38.7 percent. Most of the change occurred between 1993 and 2003.

Figure 1 displays median earnings by degree type for each of the three survey years (charts of mean earnings are similar). There is no evidence in this figure of progressive convexification of earnings among college graduates. For women, the earnings gap between professional and other degrees rose sharply between 1993 and 2003, causing the education-earnings profile to be more similar in shape to men's (though at a substantially lower level), but there was otherwise little change in the slope.

[8] Full-time is defined by direct report of the respondent for 1993 and by 35 hours or more per week for 2003 and 2010.

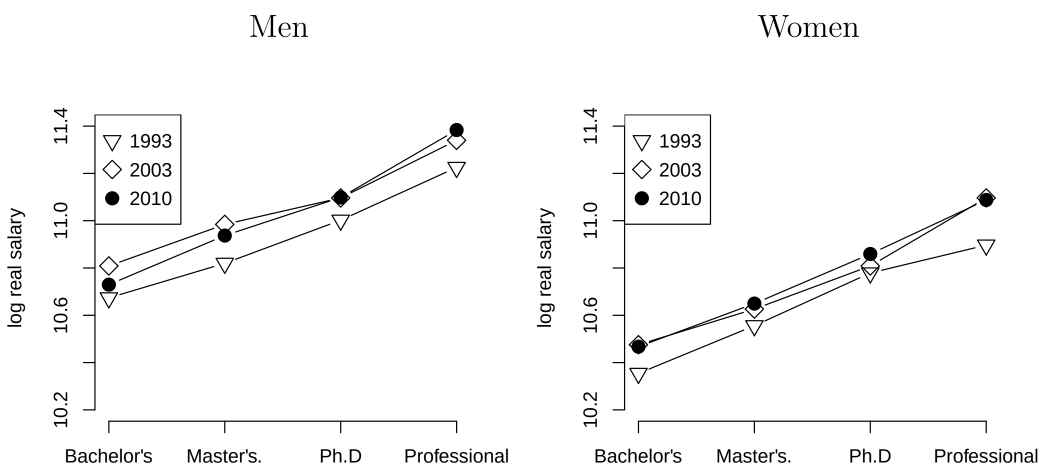
[9] This impacted 136 observations, all in the 1993 data. Several categories of individuals whom we would otherwise exclude are eliminated by the criteria already mentioned, namely those working post-retirement jobs; those with field of study coded as unknown, "other," or suppressed; and individuals with missing values for highest degree type or salary.

Table 1: Distribution of degrees

	Bachelor's	Master's	Doctoral	Professional
		Men		
1993	65.9	23.8	4.8	5.4
2003	63.9	25.5	4.9	5.7
2010	63.5	26.8	4.8	5.0
		Women		
1993	66.2	28.4	2.2	3.2
2003	61.8	31.6	2.9	3.7
2010	61.3	31.2	2.9	4.6

Notes: Weighted estimates are for the sample of full-time workers described in section 2. All standard errors are less than 0.5 percentage point.

Figure 1: Median earnings by degree type



Notes: Weighted estimates for the sample of full-time workers described in section 2.

Table 2: Earnings inequality changes

	Men			Women		
	All	Undergrad.	Graduate	All	Undergrad.	Graduate
			1993-2010			
Δ90-10	0.161	0.198	0.147	0.158	0.215	0.115
	[0.125, 0.210]	[0.172, 0.265]	[0.105, 0.216]	[0.134, 0.202]	[0.152, 0.285]	[0.046, 0.180]
Δ90-50	0.067	0.077	0.047	0.115	0.149	0.080
	[0.025, 0.091]	[0.051, 0.121]	[-0.001, 0.087]	[0.093, 0.158]	[0.117, 0.191]	[0.018, 0.117]
Δ50-10	0.094	0.121	0.100	0.044	0.066	0.036
	[0.070, 0.141]	[0.088, 0.178]	[0.077, 0.161]	[0.016, 0.065]	[0.004, 0.121]	[-0.007, 0.093]
			1993-2003			
Δ90-10	0.098	0.140	0.088	0.052	0.108	0.027
	[0.074, 0.129]	[0.097, 0.165]	[0.053, 0.112]	[0.030, 0.076]	[0.089, 0.143]	[-0.030, 0.048]
Δ90-50	0.031	0.034	0.029	0.042	0.097	0.072
	[0.020, 0.063]	[-0.008, 0.053]	[0.002, 0.052]	[0.023, 0.065]	[0.072, 0.125]	[0.039, 0.102]
Δ50-10	0.066	0.105	0.059	0.010	0.011	-0.045
	[0.039, 0.083]	[0.089, 0.130]	[0.033, 0.079]	[-0.013, 0.031]	[-0.007, 0.042]	[-0.087, -0.045]
			2003-2010			
Δ90-10	0.063	0.058	0.059	0.106	0.108	0.088
	[0.018, 0.113]	[0.030, 0.135]	[0.024, 0.132]	[0.079, 0.155]	[0.040, 0.169]	[0.033, 0.174]
Δ90-50	0.035	0.043	0.017	0.072	0.052	0.008
	[-0.013, 0.056]	[0.016, 0.101]	[-0.023, 0.058]	[0.051, 0.113]	[0.018, 0.092]	[-0.052, 0.048]
Δ50-10	0.028	0.016	0.042	0.033	0.056	0.081
	[0.012, 0.086]	[-0.016, 0.063]	[0.019, 0.102]	[0.004, 0.055]	[-0.009, 0.102]	[0.051, 0.162]

Notes: Weighted estimates are for the sample of full-time workers described in section 2.Bootstrap 90% confidence intervals in brackets (basic bootstrap using 500 replicates).

Table 2 reports statistics that summarize changes in the earnings distribution. These statistics are noisy, so we mention only broad features of inequality change.[10] Over the entire 1993-2010 period, there was a substantial broadening of the earnings distribution for both men and women. For both men and women, the gap between the earner at the 90th percentile and the earner at the 10th percentile of the earnings distribution widened by about 16 log points, but the figure was about 20 log points for those with only an undergraduate degree. However, the widening was concentrated below the median for men, but above the median for women, and this was true at both the graduate and undergraduate levels. Details for the sub-periods are shown in the table.

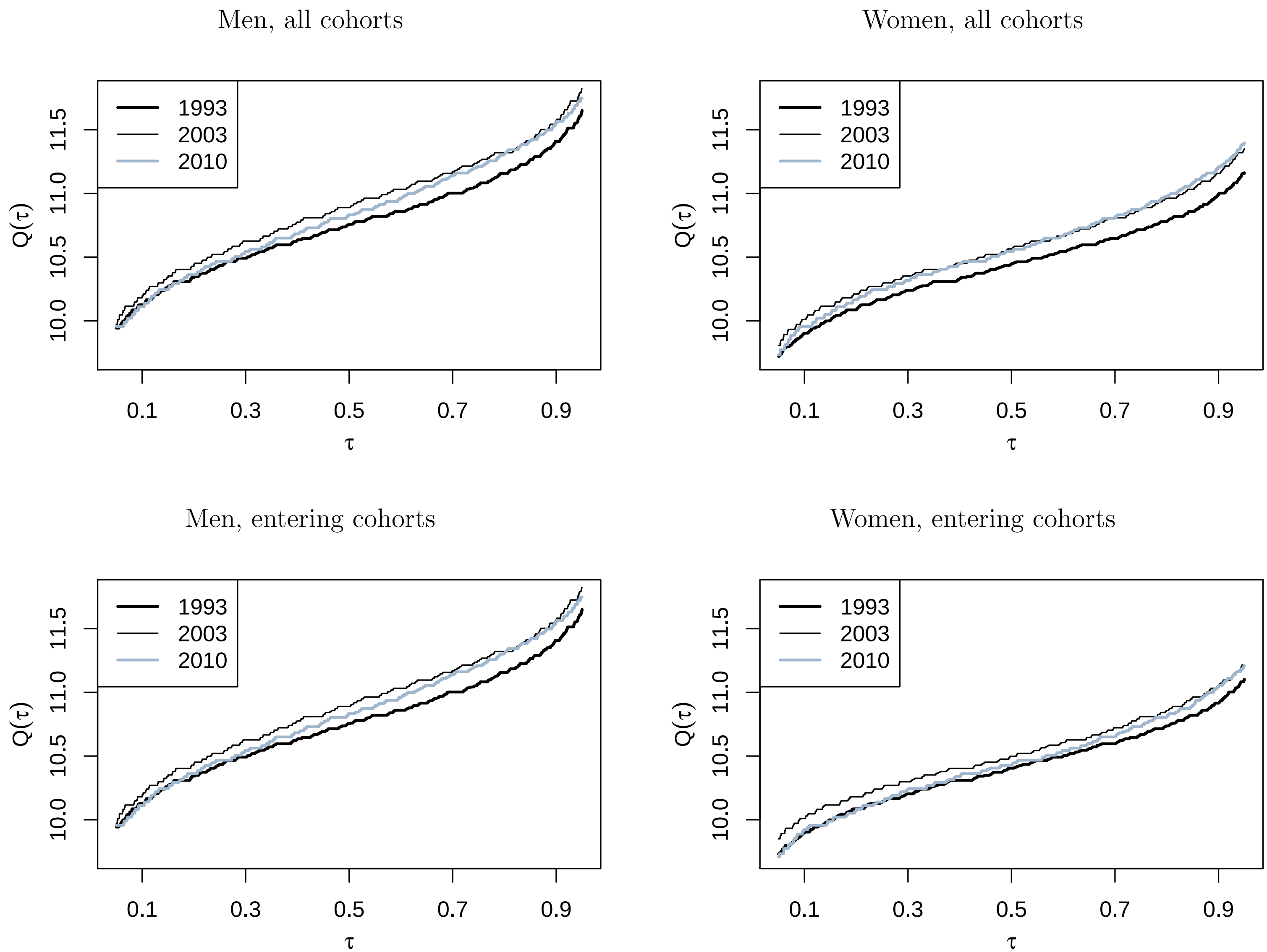
[10] The imprecision of these simple statistics is not really very surprising since each estimate in Table 2 combines the variability from estimates of four separate quantiles. The lumpiness of the salary data accounts for the strong asymmetry of some of the confidence intervals because one end of the interval may vary little from one bootstrap realization to the next.

Figures 2 and 3 provide a better overview of the changes over time, less sensitive to the noise embedded in specific quantiles. The top panels of Figure 2 display the quantile functions for men and women in each of the three years. The bottom panels show the quantile functions for individuals who received their highest degrees within the previous ten years, whom we refer to subsequently as the "entering cohort." In this figure, changes in earnings inequality change the slope of the quantile function. It can be difficult to discern the changes over time from the levels, so we also show the change at each quantile in Figure 3. Here inequality changes correspond to nonzero slopes.

The left panels of each figure reveal that men's earnings increased across the board from 1993 to 2003, although the increase was somewhat smaller for earners at the bottom end. Between 2003 and 2010, real earnings fell at nearly every point of the distribution, at least partly reflecting the effects of the Great Recession. These losses were more pronounced for the entering cohort. The net result is that earners near the bottom of the distribution lost most or all of the ground gained between 1993 and 2003, while those near the top lost less.

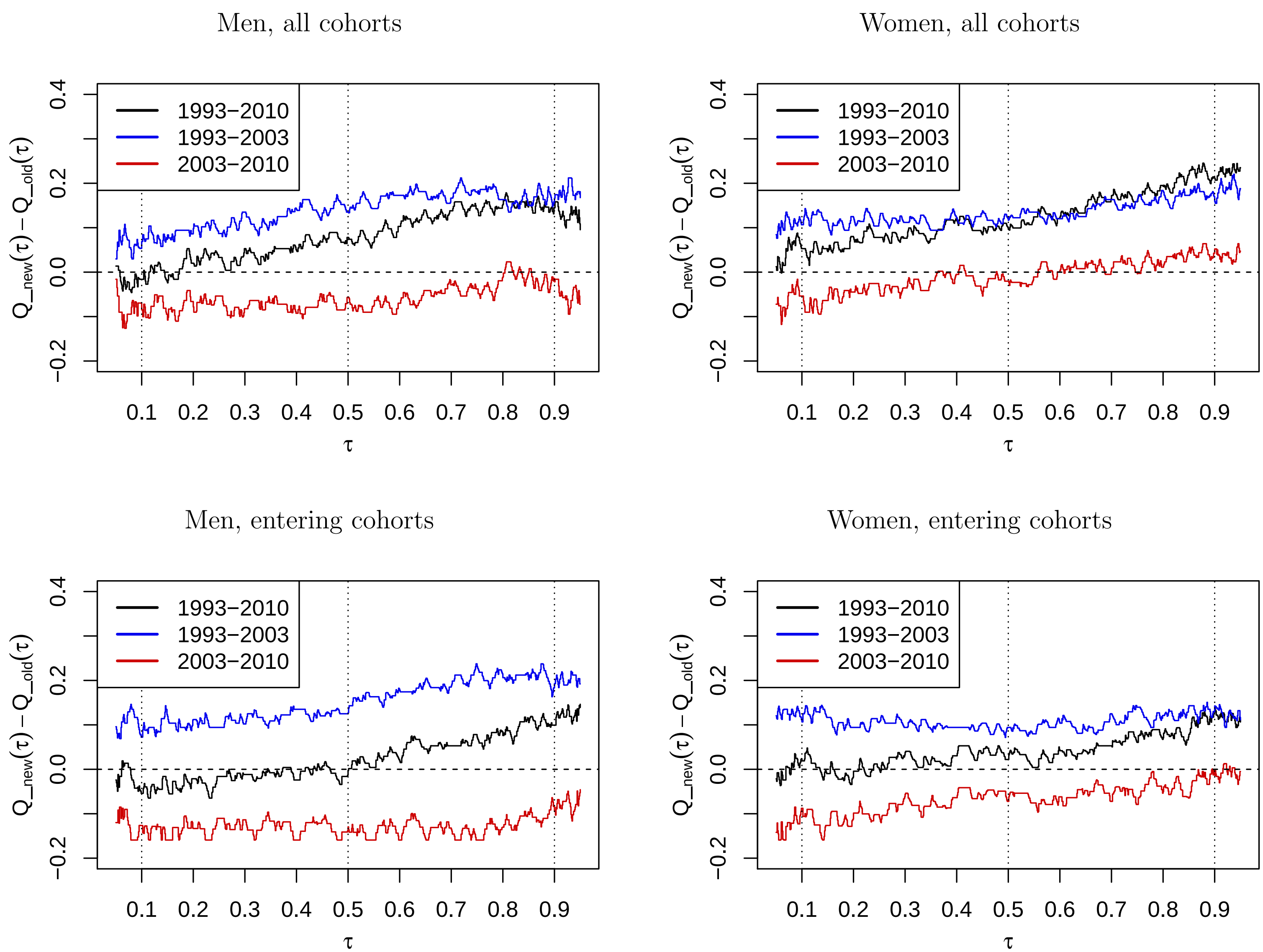
The right panels show that between 1993 and 2003 women's earnings display a similar pattern: an upward shift at all points in the distribution with a slightly smaller increase in the lower tail. Between 2003 and 2010, women at the bottom end of the earnings distribution saw earnings declines while those at the top end experienced rising real earnings. The net result is a significant spreading of the distribution. Again, the entering cohort lost more between 2003 and 2010 and these losses were inversely related to position in the distribution. It is also apparent from figure 2 that the distributions are noticeably more compressed for women than for men.

Figure 2: Quantiles of log real salary



Notes: Weighted estimates are for the sample of full-time workers described in section 2. Shaded areas are 95 percent confidence intervals.

Figure 3: Change in quantiles of log real salary



Notes: Weighted estimates are for the sample of full-time workers described in section 2.

4 Decomposition methodology

4.1 Decomposition method

The core of our analysis is the construction of counterfactual distributions: What would the 2010 earnings distribution look like if returns to characteristics were held at their 1993 levels? Stripped to bare essentials, constructing these counterfactual distributions involves estimating a distribution conditioned on characteristics in one year, then integrating that distribution over a different (counterfactual) distribution of characteristics from a different year. The counterfactual distribution in turn implies a decomposition of the distributional shift into a composition effect and a wage-structure effect. As usual, literal interpretations of these components are based on the assumption that changes in characteristics, such as level of education, are not the underlying causes of wage changes and vice-versa. Standard supply-and-demand logic, though, implies that supply or demand shocks generally result in both price and quantity changes.

The decomposition method we employ uses distribution regressions as described by Chernozhukov, Fernández-Val and Melly (2014). Let y represent (log) earnings, X the set of available conditioning variables (experience, degree, etc.), and $F(y_t|X_s)$ the distribution of year t earnings conditional on year s characteristics. When $t = s$ this is just the distribution of earnings; when $t \neq s$, it represents a counterfactual. When $s > t$ we will refer to a forwards counterfactual, and when $s < t$, a backwards counterfactual.

Using the forwards counterfactual between 1993 and 2010 ($F(y_{93}|X_{10})$), for example, the difference between the 2010 and 1993 earnings distributions can be decomposed as follows:

$$F(y_{10}|X_{10}) - F(y_{93}|X_{93}) = [F(y_{10}|X_{10}) - F(y_{93}|X_{10})] + [F(y_{93}|X_{10}) - F(y_{93}|X_{93})]. \tag{1}$$

We follow CFM in labeling the second term as a "composition effect" and describe below how we estimate it. The remaining difference, between the later earnings distribution and the counterfactual, we call the "wage structure" effect.[11] Some decomposition methods produce estimates of "residual" or "within group" inequality change. When the entire conditional distribution is estimated as in CFM's approach or that of Machado and Mata (2005), most of that information is encoded in changes in the shape of the conditional

[11] The composition and wage structure effects can also be defined using the backwards counterfactual, $F(y_{10}|X_{93})$.

distribution via differences in the parameters of the distribution (or quantile) regressions.

An outline of the algorithm for finding the forward counterfactual, $F(y_{93}|X_{10})$, that is, the counterfactual distribution of 2010 earnings given the 1993 returns to characteristics, follows. Other pairings of years, such as backwards counterfactuals, are exactly analogous.

1. Set up a fine grid of values of y: y_j, $j = 1 \dots J$.
2. For each y_j, using 1993 data, regress $I(y \leq y_j)$ on an appropriate function of X.[12] The predicted probability of $y \leq y_j$ given $X = x$ from these distribution regressions provides an estimate of the conditional distribution at y_j, $F(y_{j,93}|x)$.
3. Calculate probabilities predicted by the 1993 distribution regressions for each member of the 2010 sample.
4. Estimate the counterfactual distribution by averaging the predicted probabilities from step 3 (i.e., integrate over the empirical distrbution of 2010 characteristics), using 2010 survey weights (the estimation in step 2 uses 1993 weights). If the estimated distribution is not monotonic, use rearrangement to refine the estimate (Chernozhukov, Fernńdez-Val, and Galichon, 2010).

We present forwards counterfactuals because the NSCG sample was substantially larger in 1993 than in 2010, thus producing more precise estimates in step 2 for a forwards counterfactual than for a backwards counterfactual. This consideration is especially important when we introduce detailed field of study into the regressions in step 2. Comparing conclusions with a backward counterfactual (reversing the roles of 1993 and 2010) is sometimes a useful robustness check, which we employ as appropriate.

Although, in principle, linear probability models could be used in step 2 (Fortin, Lemieux, and Firpo, 2011), some simple Monte Carlo simulations indicated that they did not perform well in this application. Therefore, our first set of counterfactuals are based on the following logit model:

$$\Pr\big(I(y_i \leq y_j)\big) = \Lambda \left(\alpha + \sum_{k=1}^{4} \beta_k E_i^k + \sum_{m=1}^{3} \gamma_m D_{mi} + \varepsilon_i. \right), \qquad (2)$$

[12]An alternative method, developed by Machado and Mata (2005), estimates quantile regressions on a grid. However, as noted earlier, salary data are “lumpy,” and CFM argue in favor of distribution regressions in that circumstance.

In this equation $\Lambda(\cdot)$ is the logistic CDF, y_i is log-salary for individual i, y_j is a grid point, E_i is potential experience measured as time since receipt of highest degree, and D_{mi} are dummies for levels of graduate degrees (master's, Ph.D., or professional). The specification inside the Λ function is intended to be Mincerian in flavor. (In section 6 we expand the specification to include field of study.)

Our interest centers on the segments of the distributions between the 10th and 90th percentiles, but our grid comprises every half log point between the 3rd and 97th percentiles of the 1993 distribution in order to ensure that the grid encompasses the 10th and 90th percentiles of the distribution in the counterfactual year.

4.2 Monte Carlo illustration of decomposition method

To help set intuition about CFM's relatively new method, we created 20,000 cases of artificial data for three hypothetical years: In year 1, master's degrees were randomly assigned to 20 percent of the sample and doctorates to 10 percent. Experience and survey weights were assigned by randomly drawing with replacement from the NSCG 1993 experience distribution. Finally, the data-generating process for log-salary was the following:

$$\ln(\text{salary}_i) = 10 + 0.35\,\text{MA}_i + 0.65\,\text{PhD}_i + 0.025E_i - 0.00025E_i^2 + \varepsilon_i,$$

where $\varepsilon_i \sim N(0, 0.2)$ (the variance of 1993 log-salary was about 0.2). Finally, since a large fraction of NSCG salary responses are rounded to the nearest \$1,000, we convert half of the log-salaries to dollars, round to the nearest \$1,000, and convert the result back to logarithms in order to illustrate the effect of the rounding.[13]

In year 2, 40 percent of cases were randomly assigned master's degrees and 15 percent doctorates, and experience and weights were drawn from the NSCG 2010 distribution. The data-generating process for log-salary was the same as in year 1.

The year 3 sample was constructed in the same way as year 2, except that the data-generating process was changed to

$$\ln(\text{salary}_i) = 10.07 + 0.45\,\text{MA}_i + 0.75\,\text{PhD}_i + 0.025E_i + 0.00025E_i^2 + \varepsilon_i.$$

[13]In fact, 56 percent of 1993 salaries appear to be rounded to the nearest \$1,000, 24 percent to the nearest \$5,000, and 15 percent to the nearest \$10,000. This fact has important consequences that we describe below.

To summarize: The difference between years 1 and 2 is entirely composition effect (plus noise from sampling variability), while the difference between years 2 and 3 is due entirely different returns to education (and noise).

The results of forward counterfactuals are shown in Figure 4. The interpretation of the upper-left panel is that changing characteristics to their year-2 values, but rewarding them at their year-1 shadow prices reproduces the year-2 distribution. Put differently, there is essentially no wage-structure effect in the lower-left panel, exactly what we built into the data.[14] The sharp zig-zags in the lower panel come from the prominent stair-stepping visible in the upper panel, which is in turn a direct consequence of rounding a significant fraction of salaries to the nearest \$1,000. (The steps and zig-zags become progressively smaller as τ increases because \$1,000 is a smaller and smaller percentage of salary.)

The right panels show the forward counterfactual for years 2 and 3 where, by construction, there is only a wage effect: Rewarding year-3 characteristics at year-2 shadow prices reproduces the year-2 distribution because any difference in the distributions of characteristics arises only from sampling variability.[15]

4.3 Bootstrapping counterfactuals

To estimate uniform confidence bands for counterfactuals, we follow the bootstrapping procedure developed by CFM, which involves resampling both the regression sample (1993 in this case) and the counterfactual data (2010). We use an empirical bootstrap at both stages.[16] The (resampled) 1993 weights are used in producing each set of replicate regression coefficients (step 2 in the outline above), and 2010 weights are used in averaging predicted probabilities (step 4).

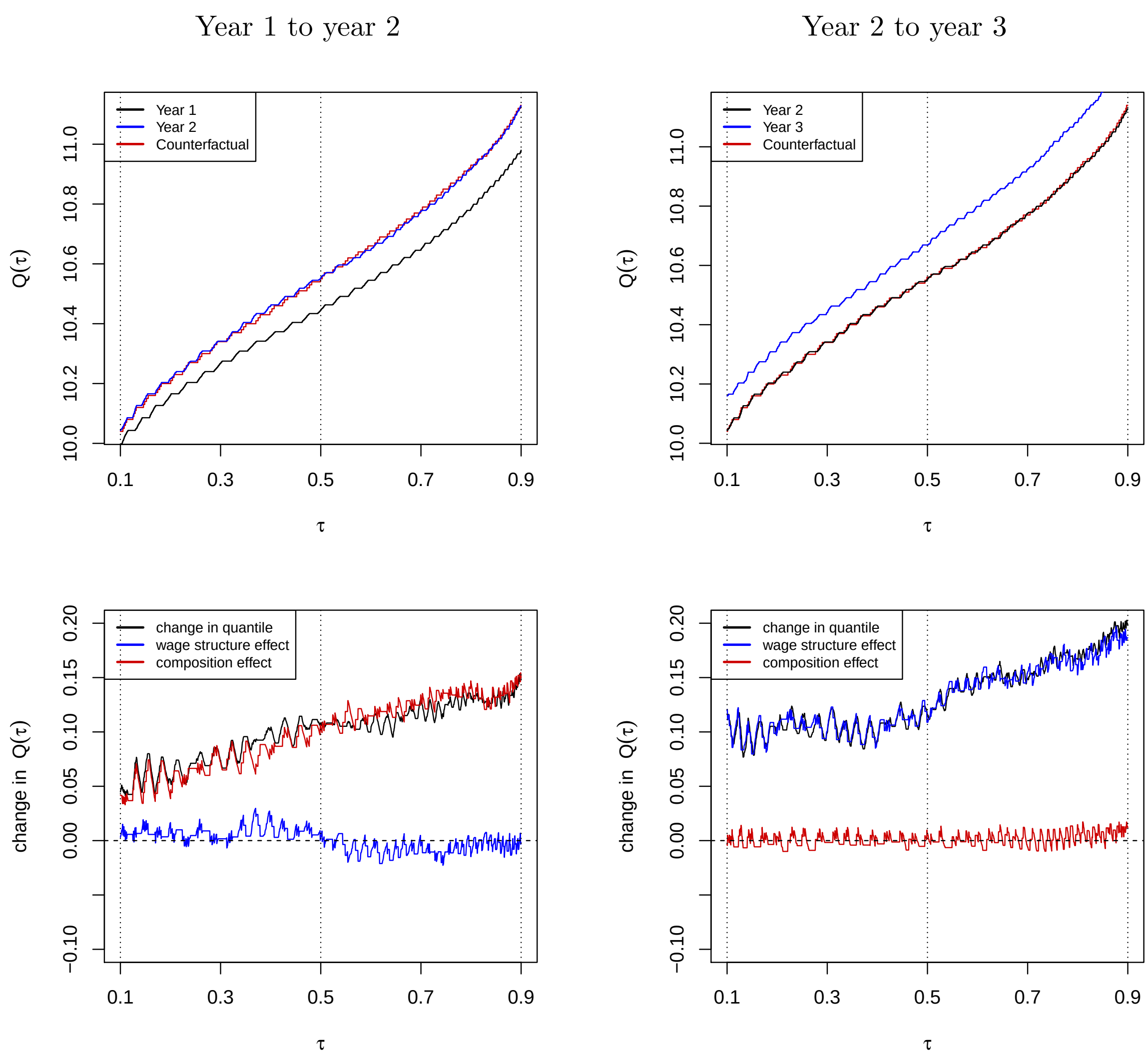
From the bootstrap realizations of the counterfactuals we calculate uniform confidence bands, which allow inference about the entire segments of the counterfactual distributions we estimate ($[0.1, 0.9]$). The appendix offers a detailed outline of the procedure for estimating the uniform confidence

[14] The backward counterfactual (not shown) also says there is no wage structure effect, but in reverse: rewarding year-1 characteristics at their year-2 shadow prices reproduces the year-1 distribution. Using a linear probability model in place of a logit produced a significant wage structure effect, which is the failure alluded to in section 4.1.

[15] Again, the backward counterfactual produces the same interpretation.

[16] This simple resampling scheme is not ideal, given that the NSCG uses stratified sampling, but the public-use data provides only generalized variance parameters, which are not applicable, nor can they be used to identify strata. Ignoring stratification likely results in somewhat wider confidence bands.

Figure 4: Counterfactuals on artificial data



bands as adapted from CFM for this application. Given economists' interest in upper- vs. lower-tail inequality, there is some question whether uniform inference about the entire $[0.1, 0.9]$ interval is appropriate, but given that we find little evidence that composition effects contribute to inequality (change in the slope of composition effects), this turns out to be of little consequence.

5 Decompositions based on experience and highest degree

Returning to real data, Figure 5 shows the results of decompositions using experience and highest degree type for men and women.[17] The top panels show all men and all women while the bottom panels are limited to entering cohorts. The decompositions displayed in Figure 5 use the same sample as those below, which excludes field-degree cells below a certain size, but this alters our conclusions only slightly, as noted below.

When we consider all cohorts, the striking conclusion from Figure 5 is that for both men and women virtually none of the change in the earnings distribution between 1993 and 2010 is attributed to the composition effect. (It follows, of course, that there is no effect of composition on inequality.) Moreover, the confidence bands are fairly narrow. The same is true for entering male cohorts though the confidence band is wider since the samples are smaller.[18]

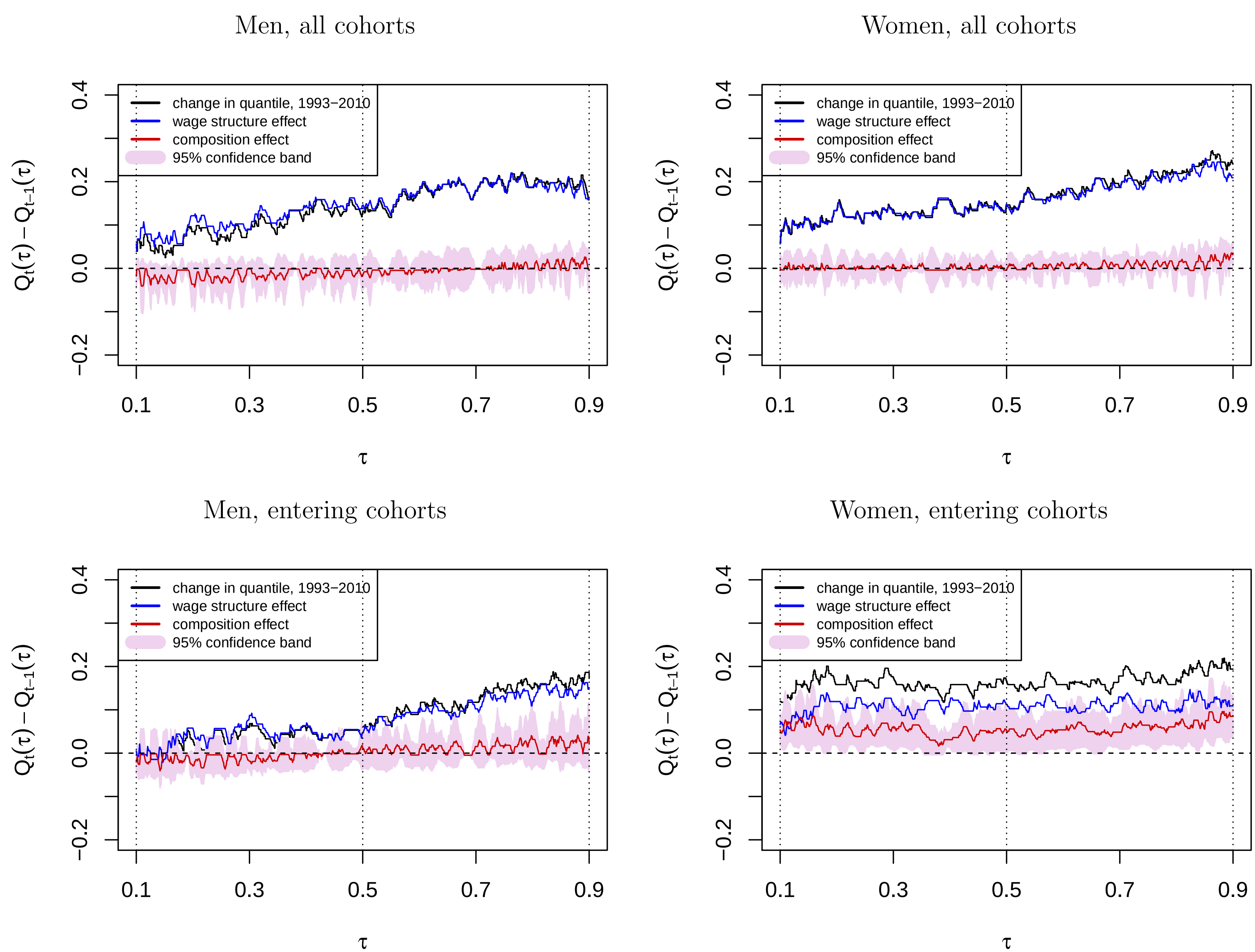
For entering female cohorts the estimated composition effect has an economically significant magnitude. The composition effect is fairly flat and thus there is no evidence that changes in these characteristics affected inequality. Thus part of the increase in wages, but not the increase in inequality, for entering female graduates is probably attributable to changes in experience and degree level. The 95 percent confidence band crosses zero, but clearly with a slightly smaller confidence level it would not.[19] A small positive composition

[17]Because our discussion of results centers on the surprisingly small magnitude of the composition effects, we minimize chart clutter by omitting confidence bands for the wage-structure effects. The width of the confidence bands is generally approximately the same as the bands for composition effects. The computations for this paper were performed using R version 3.1.1 (R Core Team, 2014) and the R survey package (Lumley, 2014).

[18]The reader might have noticed that some of our confidence bands appear to have "holes." In some cases the band is simply very narrow. In some cases the band is simply very narrow. In other cases—near the 70th percentile in the upper-left panel of figure 5, for example—the band is not defined given a reasonable number of bootstrap replicates. Both phenomena result from heaping of the earnings data. The appendix offers a detailed explanation.

[19]Composition effects from the corresponding backwards counterfactuals (not shown) are essentially zero.

Figure 5: Forward decompositions using experience and highest degree



effect for the entering cohorts of women cannot be ruled out and would be consistent with the fact that these entering cohorts have pursued advanced degrees at a higher rate than their predecessors (Table 1). However, this conclusion is sensitive to our exclusion of small fields; when we use the full sample, the composition effect is very close to zero.[20]

Although in this paper we emphasize the entering cohorts, it is important also to consider exiting cohorts when interpreting what happens to the overall distributions. For both men and women, the composition effects in exiting cohorts are as close to zero, as those in the top panel of figure 5, so we do not show them.

6 Decompositions incorporating field of study

Field of study represents an important dimension of specialization of human capital and therefore an important component of an individual's occupational choice. A bachelor's degree in history is different from a bachelor's degree in mechanical engineering. They lead to vastly different average wages (Black, Sanders and Taylor, 2003b) and other occupational characteristics. More to the point, if the wage differential widens between mechanical engineering and other majors, students at the margin in various fields should switch into mechanical engineering (though admittedly the cross-elasticity between history and mechanical engineering is probably rather small). It follows that technological change that (dis)favors certain occupations should alter the flows into different fields. Altonji, Kahn, and Speer (2014) document the magnitude of changes in returns and connect them with technology (the polarization hypothesis, in particular).

A technical issue also points to the use of fields in our decompositions. Consider the specification in equation (2). If heterogeneity *within* a cell changes—for example, if the composition of the master's degree cell shifts between 1993 and 2010 toward fields with higher pay—the estimated coefficient for that cell will change, even if there is no change in the field-specific returns. Since that composition change is invisible to the equation (2) specification, neglecting heterogeneity can allow composition changes to appear as wage-structure effects.

Thus we turn to a more detailed specification in which the distribution

[20]When we use the full sample, backwards and forwards counterfactuals show composition effects very close to zero for all four groups shown in figure 5.

regressions include highest degree interacted with field of highest degree:

$$\Pr\big(I(y_i \leq y_j)\big) = \Lambda\left(\sum_{k=1}^{4} \beta_k E_i^k + \sum_{m=1}^{4}\sum_{\ell=1}^{N_f} \eta_{m\ell} F_{\ell i} D_{mi} + \varepsilon_i\right), \tag{3}$$

where $F_{\ell i}$ is field of study in highest degree. In other words, $F_{\ell i} D_{mi}$ encodes i's graduate economics degree and ignores her undergraduate theatre major.[21] If coefficients in the distribution regressions are imprecisely estimated, sampling variability will appear as wage-structure effects. We therefore exclude field-degree cells smaller than 80 for all cohorts and 50 for entering cohorts.[22] These exclusions leave us with 85 percent of the otherwise valid male responses with $N_f = 124$ and 80 percent of the female responses with $N_f = 81$. For entering cohorts, we retain 78 percent of valid male responses with $N_f = 82$ and 75 percent of valid female responses with $N_f = 68$.

Figure 6 shows the results of decompositions based on equation (3). Small composition effects are evident in three of the panels. The effects are nearly flat, so again they do not contribute much to changes in inequality.[23]

As before, the bottom panels focus on the entering cohorts. For this part of our analysis, the entering cohorts should be particularly informative because they are closer to their field-degree choice and thus likely more responsive to observed changes in earnings signals. The results for recent male graduates make plain that changes in field of study are not driving changes in either the level or shape of the earnings distribution. Surprisingly, they seem to make virtually no discernable difference at all.

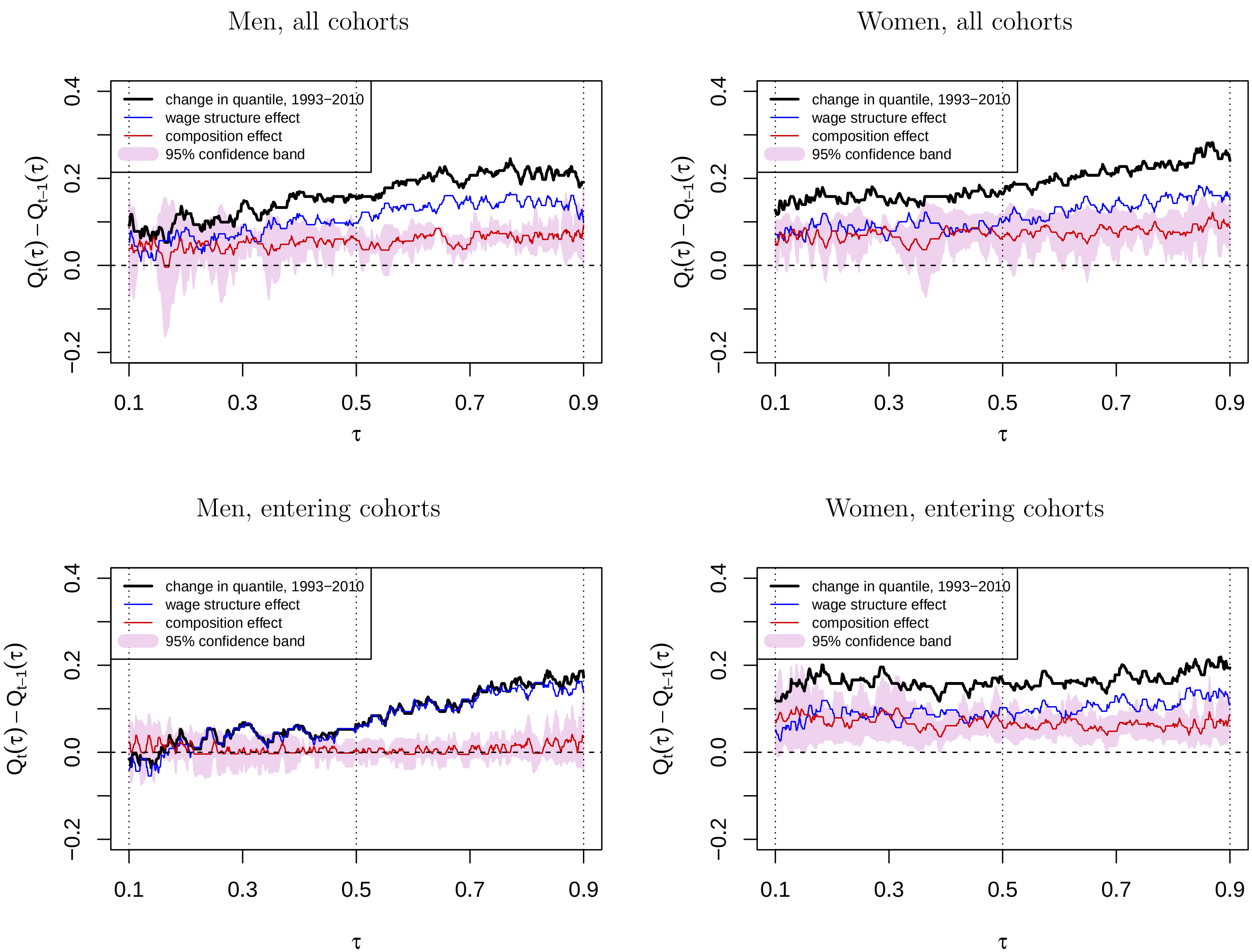
It is also somewhat surprising that there is a larger composition effect for all male cohorts than for entering cohorts. There is no analogous difference in figure 5. We offer two possible explanations. First, the cohort who exited between 1993 and 2010 differed in their field choices from those who replaced them. Counterfactuals using only the exiting cohort, ages 48–65 (in 1993 those who could not be included in the 2010 sample) indicate just that.

[21]Some interactions between the field of a bachelor's degree and field of highest degree are probably economically significant, but the cell sizes for most such interactions would be tiny, so we do not attempt such a specification. Note that for brevity equation (3) is written with *all* two-way field-degree interactions terms and therefore omits the constant and main effects.

[22]We set a higher minimum cell size when using all cohorts for tractability in bootstrapping confidence bands, but this does not significantly affect the size of the composition effects. Also, for the reason discussed in the previous paragraph, we do not create catch-all cells combining the small field-degree cells.

[23]Backwards counterfactuals (not shown) show small *negative* composition effects. Recall, however, that the backwards counterfactuals are not completely comparable because they involve fewer field-degree cells because the 2010 sample is much smaller.

Figure 6: Forward decompositions with field-degree interactions



A second possibility is that the difference could be a consequence of the smaller set of field-degree cells used in the entering-cohort counterfactual. However, when we restricted the all-cohort counterfactual to the entering cohorts' smaller set of field-degree cells, the composition effect was small and was positive only above the median.

The composition effect for female cohorts is interesting in a different way: Comparing the lower-right panels of figures 5 and 6 reveals that incorporating field of study into the analysis adds very little to the composition effect and virtually nothing to inequality.

The uniform confidence bands cross the zero line at some percentiles in all four panels, so the strict conclusion would be that the composition effects cannot be statistically distinguished from zero at the 95 percent confidence level. But, except for entering male cohorts, a more nuanced interpretation seems appropriate for two reasons. First, a slightly lower confidence level would obviously change the conclusion for entering female cohorts. The second issue is a technical one explained in detail in the appendix: The extreme narrowness of the confidence bands in some places is due to heaping of the salary data at multiples of \$1,000 (and especially \$10,000), which is largely measurement error. A corollary is that spikes in the confidence bands occur in regions where the salary data are sparse, but the sparseness is partly a direct consequence of artificial heaping elsewhere. More accurate salary data, then, would remove some, perhaps all, of the confidence-band spikes that cross zero.

Also, however one assesses this ambiguity about whether the composition effects differ from zero when fields are included, it is worth emphasizing that point estimates of the wage structure effects are larger than the our estimates of the composition effect at nearly every τ in every panel. It is clear that, with the possible exception of entering female cohorts, most of the change in the earnings distributions comes from wage structure effects.

More importantly, the wage structure effects account for essentially all of the changes in earnings *inequality.* That is, where the graph of the 1993-2010 change in quantiles is upward sloping, the upward slope is attributed to the wage structure effect not the composition effect.

We have estimated forwards and backwards counterfactuals for the 1993-2003 and 2003-2010 subperiods. Although shifts in the earnings distribution look different for the subperiods, as documented in section 3, all of the counterfactuals imply very small (or even no) composition effects and no evidence that the composition effects contribute much to the growth in earnings inequality among college graduates. These supplementary results are available from the authors on request. This consistency suggests that the finding is not specific to labor market conditions in any particular year (2010 in particular). We now turn to some potential explanations.

7 Why are the composition effects so small?

Our interpretation of the results in the previous sections is that composition effects are surprisingly small, particularly among entering cohorts.[24] This is true even when we incorporate detail on field of study. The latter is particularly puzzling given the obvious connections between technological change and returns to fields. In this section we discuss some possible interpretations of these findings.

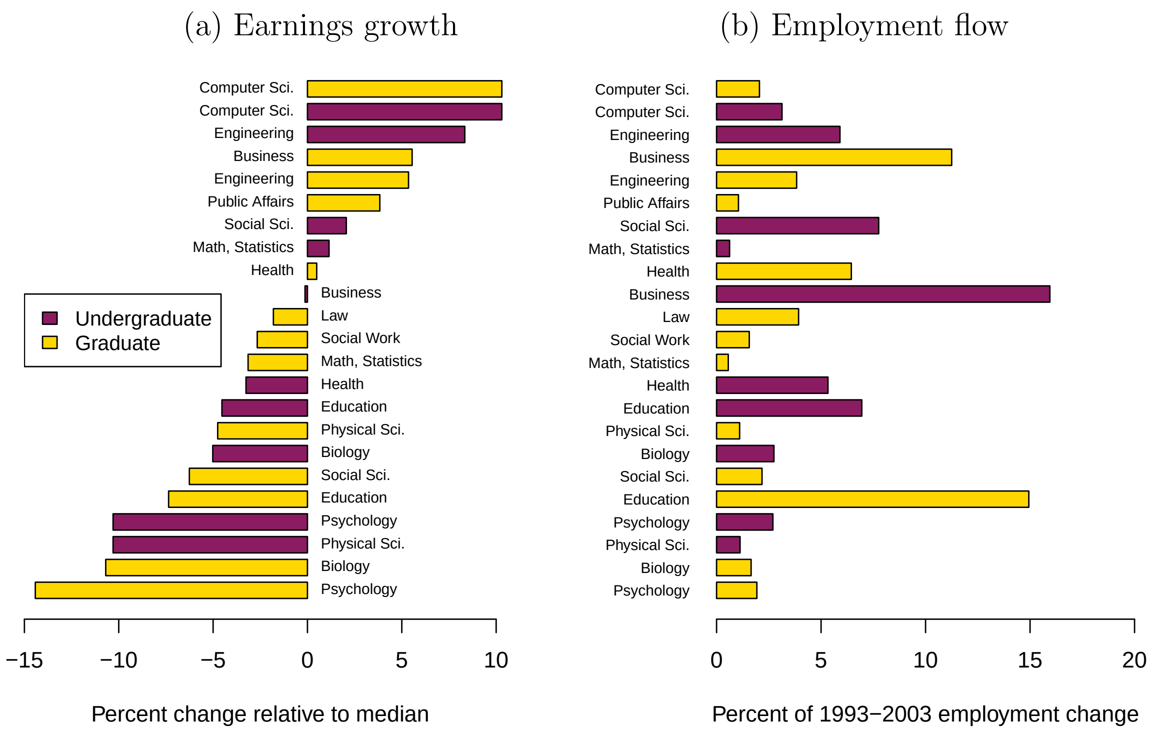
First we note that while students are the marginal decision makers regarding choice of fields, the flow of students into specific fields has apparently not been very sensitive to changing returns to fields. Figure 7 illustrates this point. The NSCG questionnaire classifies fields in 27 areas. We interacted these areas with an indicator for attaining a graduate degree. Panel (a) shows 1993-2003 earnings growth of the entering cohort relative to the overall median in the entering cohort for the largest area-degree cells (by sample size). Panel (b) shows the flows of new entrants, keeping area-degree cells in the same order as panel (a).[25]

Panel (a) provides compelling evidence of large changes in supply and/or demand for different areas. If student flows shifted strongly toward areas with increasing pay, the shape of panel (b) would resemble panel (a), and we should observe a mix of composition and wage-structure effects. Clearly, however, the correlation is low. Students moved toward some of the areas with rapidly growing relative wages, but not others; they deserted some fields with shrinking wages, but not others. The largest employment flows by far are simply those associated with the largest areas, business and education. Those with graduate business degrees experienced decent earnings growth and the number of new entrants was fairly high. On the other hand, holders of undergraduate business degrees saw essentially zero earnings growth, yet the employment flow was even higher. Real earnings fell for those with education degrees, but despite this, the flows towards these popular fields of study were large and positive. This is hardly surprising, but it does not lead to composition effects.

[24] This is not to say there are no composition effects when comparing groups rather than years. In another paper we find that composition explains a significant share of the gap between male and female wage distributions.

[25] Pairing the 2003-2010 employment flows with either 1993-2003 or 2003-2010 earnings growth leads to conclusions similar to those we discuss, but we wish to illustrate the point without potential confusion caused by labor-market effects of the Great Recession.

Figure 7: Entering cohort earnings and employment growth, 1993-2003



Notes: Earnings growth in panel (a) is relative to the median for all individuals in the entering cohort. Employment flow in panel (b) is entering-cohort employment in the area-degree cell as a fraction of change in total employment for the full sample. The bars in panel (b) do not add to 100 percent due to exits and the omission of other area-degree cells.

Cross-field demand shifts should induce positive correlation between quantity (employment) changes and price (wage) changes. We see no reason that student preference shocks would be significantly correlated with earnings growth. Thus, panel (b) can be interpreted as a measure of the absolute supply response to earnings changes in panel (a).[26] In order to see a positive composition effect, employment flows must be positively correlated with relative earnings growth (assuming the labor market was more or less in equilibrium at the start of the period). Otherwise, the composition effects from unrelated movements among areas simply offset one another: Students who choose fields that are weakly rewarded by the labor market create a negative composition effect, while other students who choose fields that are strongly rewarded by the labor market create offsetting positive composition effects. This is the scenario suggested by figure 7.

[26] Absolute in the sense of not being scaled to area employment—this is not intended to be an elasticity calculation

Our discussion of Figure 7 is supported by a more detailed analysis: Using field-degree cells with at least 50 respondents (as in the decompositions) the correlations between salary growth and employment flow are not significantly different from zero for any combination of inter-survey intervals; the largest of these correlations is 0.21 ($p = 0.11$) for wage growth and employment flow over the entire 1993-2010 period.

We conclude that the absence of large composition effects in the presence of big shifts in the earnings distribution is indicative of tepid and/or unsystematic supply response to large demand shifts, and suggest several complementary explanations. First, information about returns could be slow to disperse. Second, students' response to earnings information may be constrained by preparation, ability, and/or financial resources. Stinebrickner and Stinebrickner (2014) surveyed and tracked students and found that 19.8 percent of students planned to major in science or math but only 7.5 percent graduated with a major in science or math. Third, there may be capacity constraints in some fields. Physicians' choice of specialties, for example, are constrained by limitations on residency slots (National Resident Matching Program, 2014). Similarly, slots in some undergraduate programs at large universities are rationed by minimum GPA requirements or other barriers to entry. Colleges and universities do not respond quickly to changes in the demand for particular fields. Fourth, it is possible that many students place a sufficiently high value on non-pecuniary features of their chosen major that very few students are close to indifferent between their first and second choice of available field. This would imply very small wage cross elasticities, especially between dissimilar fields.

Some of these hypotheses are amenable to further study. The information diffusion explanation could be tested experimentally by presenting a treatment group with detailed information about the returns to different fields of study. In principle, contingent-valuation methods could be used to estimate the cross-wage elasticity of field choices.

8 Conclusion

Using the methods proposed by Chernozhukov, Fernández-Val, and Melly (2014) we decompose changes in the distribution of college graduates' earnings between 1993 and 2010 into composition and wage-structure effects, incorporating field of study into the analysis. We find, first, that composition effects are unimportant in most cases and small in the remainder. Second, we find that composition effects, including field of study, do not explain any of the increasing earnings inequality among college educated workers. We hypothesize that slow information diffusion to students, constraints on students' capabilities, and constraints on the growth of enrollment in key fields account for some of the puzzle of small composition effects. We believe these findings are an important consideration for policies intended to boost student interest in particular areas, such as STEM fields.

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Appendix: Calculating uniform confidence bands for counterfactuals

This appendix restates CFM's Algorithm 3 for estimating a uniform $(1-\alpha)$ confidence band for the quantile composition effect from a forward counterfactual (such as the composition effects in figures 5 and 6). The composition effect is $\widehat{\Delta}_{93|10}(\tau) = \widehat{Q}_{93|10}(\tau) - \widehat{Q}_{93|93}(\tau)$, where τ indexes quantiles and $\widehat{Q}_{93|10}(\tau)$ is the quantile function calculated from the 1993-2010 forward counterfactual. Let $\tau \in \mathcal{T}$, where the index set, $\mathcal{T}$, is, say, $[0.1, 0.9]$. For numeric implementation, we consider τ on a grid of T points on $\mathcal{T}$.

One of the subtle issues in implementing the algorithm is that n, which is a "sample-size index" important for the asymptotic arguments of CFM's paper, also appears in their description of the algorithm, apparently presenting an ambiguity about implementation. However, we illustrate below that n actually cancels out of the calculations; sample sizes affects the calculations only via the resampling process. The steps shown below correspond to those in CFM's description, but are specialized to our application.

1. Bootstrap $\widehat{\Delta}_{93|10}(\tau)$ by estimating distribution regressions on B resamples of 1993 data and calculating counterfactuals, $\widehat{\Delta}_{93|10}$ using B resamples of 2010 data and the B resamples of 1993 data. This step produces a $T \times B$ matrix of bootstrap realizations, $\widehat{\Delta}^*_{93|10}$. As mentioned in the text, we use an empirical bootstrap.

2. For each τ, Algorithm 3 calculates $\widehat{Z}^*(\tau) = \sqrt{n}\big[\widehat{\Delta}^*_{93|10}(\tau) - \widehat{\Delta}_{93|10}(\tau)\big]$. Instead we calculate $\widetilde{Z}^*(\tau) = \widehat{\Delta}^*_{93|10}(\tau) - \widehat{\Delta}_{93|10}(\tau)$. Computationally, this step produces a $T \times B$ matrix $\widetilde{Z}^*$.

3. For each τ, Algorithm 3 calculates an estimator of the asymptotic variance of $\sqrt{n}\big[\widehat{\Delta}_{93|10}(\tau) - \Delta_{93|10}(\tau)\big]$ as:

   $$\widehat{\Sigma}(\tau)^{1/2} = \big(Q^{\widehat{Z}^*}_{0.75}(\tau) - Q^{\widehat{Z}^*}_{0.25}(\tau)\big)/\big(Q^{\Phi}_{0.75} - Q^{\Phi}_{0.25}\big),$$

   but instead we calculate the following expression:

   $$\widetilde{\Sigma}(\tau)^{1/2} = \big(Q^{\widetilde{Z}^*}_{0.75}(\tau) - Q^{\widetilde{Z}^*}_{0.25}(\tau)\big)/\big(Q^{\Phi}_{0.75} - Q^{\Phi}_{0.25}\big) = \widehat{\Sigma}(\tau)^{1/2}/\sqrt{n}.$$

   Here $Q^{\widehat{Z}^*}(\tau)$, $Q^{\widetilde{Z}^*}(\tau)$ and Q^{Φ} are the quantiles of $\widehat{Z}^*$, $\widetilde{Z}^*$ and the standard normal, respectively. $Q^{\widetilde{Z}^*}_j(\tau)$ is the jth quantile of row τ of the matrix $\widetilde{Z}^*$, thus $\widetilde{\Sigma}^{1/2}$ also has length T.

4. For each replicate b calculate

$$\hat{t}_b = \sup_\tau \widehat{\Sigma}(\tau)^{-1/2}|\widehat{Z}^*(\tau)| = \sup_\tau \frac{\sqrt{n}|\widetilde{Z}^*(\tau)|}{\sqrt{n}\,\widetilde{\Sigma}(\tau)^{1/2}} = \sup_\tau \frac{|\widetilde{Z}^*(\tau)|}{\widetilde{\Sigma}(\tau)^{1/2}}.$$

   The leftmost maximization is a direct adaptation of Algorithm 3, however, note that the rightmost expression does not depend on $\sqrt{n}$. This step produces a length B vector $\hat{t}$. Computationally, $\hat{t}$ comprises the column maxima of $\text{diag}(\widetilde{\Sigma}^{1/2}) \times \widetilde{Z}^*$ (i.e., the matrix formed by dividing each of the T rows of the matrix $\widetilde{Z}^*$ by the corresponding element of $\widetilde{\Sigma}^{1/2}$).[27]

5. Set $\hat{t}_{1-\alpha} = Q_{1-\alpha}(\hat{t})$. This is a scalar.

6. The endpoint functions of the asymptotic confidence band are described by CFM's equation (3.12), rewritten for $\widehat{\Delta}_{93|10}(\tau)$ rather than the counterfactual CDF:

$$\begin{aligned}\widehat{\Delta}^{\pm}_{93|10}(\tau) &= \widehat{\Delta}_{93|10}(\tau) \pm \frac{\hat{t}_{1-\alpha}\widehat{\Sigma}(\tau)^{1/2}}{\sqrt{n}} \\ &= \widehat{\Delta}_{93|10}(\tau) \pm \frac{\hat{t}_{1-\alpha}\left(\sqrt{n}\,\widetilde{\Sigma}(\tau)^{1/2}\right)}{\sqrt{n}} \\ &= \widehat{\Delta}_{93|10}(\tau) \pm \hat{t}_{1-\alpha}\widetilde{\Sigma}(\tau)^{1/2}\end{aligned}$$

As noted in the main text, our calculated confidence bands appear to have some "holes." In some cases the band is simply very narrow. In other cases—near the 70th percentile in the upper-left panel of figure 5, for example—the band is not defined. Both case result from heaping of the earnings data, particularly at multiples of \$10,000. For instance, the 70th-72nd percentiles of the 1993 male earnings distribution are all exactly \$60,000. This creates a large jump in the empirical CDF of log-earnings at $\ln(60,000)$ and, consequently, in the CDF of the counterfactual distribution because the coefficients in the distribution regressions jump at that log-salary grid value. If the jump in the counterfactual distribution is large enough, as it is in this case, the probability is very low that, for example, the 71st percentile of any bootstrap realization will not be $\ln(60,000)$. This means that $Q^{\widetilde{Z}^*}_{0.75}(0.71) - Q^{\widetilde{Z}^*}_{0.25}(0.71)$ is small, or even zero, in step 3 for any practical B, making $\widetilde{\Sigma}(0.71)^{1/2}$ small or zero. In the former case the band is narrow. In the latter case, $\hat{t}_b$ is undefined for all b. In practice our approach is to ignore this problem, technically by redefining

[27] This is the step that produces uniform inference on an interval $\mathcal{T}$; calculating a pointwise confidence interval at τ amounts to setting $\mathcal{T} = \{\tau\}$, making the maximization in this step trivial.

$\mathcal{T}$ to exclude these problematic values. The explanation above suggests that visually this matters little. In any case, it should be borne in mind that the heaping that causes this phenomenon is largely measurement error. If we were able to impute the true salary values for the 2 percent of men who reported salaries of \$60,000, or any other multiple of \$1,000, the confidence bands would be smoother. Our preference was to avoid imposing an arbitrary smoothing rule, though clearly a case can be made for some smoothing.