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## Comment Erica L. Groshen

What’s actually happened to the distribution of U.S. jobs recently? This chapter bolsters the evidence that the share of jobs in the middle-income ranges continues to decline but cannot yet answer the question of what is happening at the upper and lower ends: is the hollowing out caused by disproportionate expansion of low-wage jobs, high-wage jobs, or both? We still don’t know. Nevertheless, Abraham and Spletzer make several important methodological contributions that could only be accomplished by careful, even tedious work with normally inaccessible data and supporting information. And extensions of their work on these data holds promise for further progress.

In these comments, I offer my views on the importance of the chapter, consider the challenge of the title, suggest some extensions, and close with my take-aways from the chapter.

### Importance of the Paper

The goal of the chapter is to describe, on a granular level, how the distribution of jobs in the United States has changed since the mid-1990s. The answer and further work built on it can provide insight into the causes, consequences, and policy implications of the U.S. labor market’s profound transformation. From the causal perspective, this description could help estimate, project, and contrast the impacts of the recent evolution in trade, technology, human resource practices, or corporate structures. In terms of

consequences, the exercise could answer key questions about the path of wage inequality, particularly, what sort of society are we headed for if current trends continue? Finally, knowing likely trends in the distribution of jobs should be useful in guiding key policy decisions, including regional economic development plans, workforce training allocations, public finance budgeting that rely on tax revenues, and so on. Thus, the question is important.

The authors put forth a strong case that the Current Population Survey (CPS) is unlikely to provide adequate granularity and accuracy for these purposes because of its small sample size and potential for error arising from reliance on household members and self-reports. Indeed, efforts to date using the CPS yield markedly different results for exercises based on occupational versus industrial classifications.

Thus, they analyze a new source, the Occupational Employment Statistics (OES) from 1996 to 2004. The OES features large samples of employer-reported wages and employee counts for detailed occupation and industry codes. To verify their results and shed light on any differences, they also perform a very careful, comparable exercise on the CPS. Preparing a new data set for analysis is never easy, but this case stands out as remarkably complex, with a full set of bridging, timing, coding, and comparability issues. One can almost feel the pain each time another issue surfaced.

Yet the authors persisted and have produced some convincing results, some intriguing puzzles and a data set that can potentially reveal a lot more than could be covered in this chapter.

### **What Makes a “Good Job”?**

While this chapter falls well within the literature on good jobs versus bad jobs, the authors never actually try to answer the question in their title. Most likely, they skirt the issue because the concept of a “good job” is so poorly defined.

Fundamentally, this study and much of the literature begs the question: what makes a “good job”? Is it a high wage? Or is a good job one that pays well for the skill level of the worker, particularly, one that pays middle-class wages to lower-skill workers, such as many manufacturing jobs once did? If so, does that mean that middle-wage jobs for higher-skill workers are not “good jobs”? Reaching further, does job security matter, too? What about a good career path?

The chapter never takes a stand on these questions, which keeps the results too general for most policy purposes. Of course, the appropriate definition depends strongly on the policy context in which the term “good job” is being asked and could affect measure trends in the number of good jobs. Thus, the authors leave these distinctions to the reader and the policy community. They stick to carefully reporting the facts they uncover, not defining a good job—except implicitly as one that paid a wage in the upper third in 1996. By

contrast, readers with particular policy interests will need to take a stand to define, apply, and defend a particular set of criteria. They are likely to find the results reported here not very useful yet.

The authors clarify that their focus on jobs is a quantity-side, or compositional complement, to studies of changing wage structure (and relationship to demographics) found in the inequality literature. The advantage of this approach is that it offers a lens to look at the “within” variance, that is, wage variation unexplained by demographics. This approach is thus more complete and has a policy resonance because it is tied to the tangible attributes of occupation and industry.

The challenge posed by the approach is that these attributes (occupation and industry) are not neatly tied to human capital theory. Thus, it is conceptually difficult to move between the heterogeneity of occupations and industries and a conceptual story for patterns described in the paper. This challenge is neither new nor insurmountable, but it bears mentioning as a limitation of the approach taken here.

Thus, the chapter is essentially descriptive and has an implicit demand-side focus. That is, the trends observed are most easily described as the result of changes in the demand for labor. However, the authors are very careful not to overemphasize the demand-side perspective. Researchers who follow in their footsteps should bear in mind that labor supply could also have a role to play in the outcomes described here. For example, the bimodal distribution of skill level of recent immigrants to the United States would push in the direction seen here. And so could the impact of the simultaneous increase in college enrollment and the stagnation of the high school graduation rate.

### **Managers Puzzle**

Abraham and Spletzer spent a lot of time working on the large (CPS vs. OES) discrepancy for employment shares (and trends) for managers between the CPS and OES. This is an intriguing discrepancy between data sets that are comparable in so many other respects. It was worth their attention and bears further effort as it remains largely unresolved, despite their best efforts.

One possible explanation centers on the adjustment path for industries with rapid declines in production workers. Such declines could be due to technological change or outsourcing. When the production staff falls, firms may not cut managerial proportionally because need for these higher-skilled workers may depend more on production volume than number of workers supervised or because of some fixed or transition costs. With such reductions in span of control, firms might accurately reclassify the senior workers' jobs into nonmanagerial occupations, while the incumbents might reasonably continue to define themselves as part of management. The companies may not be eager to report themselves to investors and others as suddenly management-heavy. Thus, on the books, these downsizing firms have reasons to “downgrade” some positions and report it accordingly in the OES.

However, the employees may still consider themselves to have managerial jobs (and report it to the CPS) because they are doing much of the same work but supervising fewer employees to get the job done.

This story is eminently testable by investigating whether the discrepancy in levels and trends is wider in industries with fast declines in production jobs. Note that this may not be limited to manufacturing. Nonmanufacturing industries that come to mind include software and accounting firms.

### A Grid Approach

In follow-up work, it may be useful to consider a grid (two-dimensional) approach to exploit occupational and industrial heterogeneity more fully. In particular, it could shed light on the comparisons between the CPS and OES and to link results to explanations of the shrinking middle.

Right now, the authors use occupation and industry information only to identify comparable cells over time and across data sources. This is a minimal use of the rich information provided by occupation and industry codes and their characteristics.

As a start, identifying high- and low-wage occupations as well as high- and low-wage industries and tracing their wage and job count trends could provide a link to some explanations for the evolutions of the wage structure. That is, occupation wage differentials can be linked to skills and compensating differentials. By contrast, industry wage differentials are more likely driven by skill sorting, efficiency wages, and rent-sharing. This approach would lead naturally to questions about how the skill mix has changed within and between industries and how wage differentials have evolved.

Operationally, this approach would entail estimation of occupation and industry wage differentials above a set wage threshold, set to  $W$ , for bad jobs. Then if  $\beta$  = premium for high-wage occupations, and  $\alpha$  = premium for high-wage industries, we can classify jobs into three categories:

- Good jobs: Wage =  $W + \alpha + \beta$  (High-wage occupation in a high-wage industry)
- Middle jobs: Wage =  $W + \alpha$  or  $W + \beta$  (High-wage occupation or high-wage industry)
- Bad jobs: Wage =  $W$

Tracing the development of these categories could compare the importance of hypotheses such as the following:

- Loss of high-wage industry jobs ( $W + \alpha$ )—from less rent-sharing and firm-specific human capital? versus
- Gains in high-wage occupation jobs ( $W + \beta$ )—from skill-biased technical change and trade?

This approach could also lead to a comparison of the impact on inequality of changing wage differentials versus changes in the numbers of workers

per occupation/industry cell. And, has growth in  $\beta$  for some occupations transformed some occupations into good jobs? Similarly, have some formerly high-industry jobs transitioned from good jobs to middle or bad jobs?

### **A Few More Suggested Extensions**

To look further into the discrepancies between the OES, future work could look at wage dispersion within occupation/industry cells. For example, high mean wage or rank-order discrepancies (OES vs. CPS) for cells with high within-cell dispersion suggests troublesome industry or occupation definition conversions. Or high within-cell dispersion in cells with a high concentration of second jobs in the CPS suggests that the wages of full-time jobs may not be comparable. Sampling or sample size issues might also be more evident.

Finally, I offer two unanswered questions to consider:

- What role, if any, do regional variations play? If permitted by the sample sizes—this could also be very interesting.
- What about the excluded jobs in government, agriculture, self-employment and private households? What do we know about their size and trends, and how they would change the overall story?

### **Conclusion: What I Learned**

Methodologically, the paper makes a convincing case for the value of the OES as a resource for understanding the recent evolution of wages that is superior to the CPS. It also demonstrates the challenges in preparing such a rich data set for analysis. In particular, while we knew that occupations were hard for households to report consistently, now we see that coding them is difficult for statistical agencies also.

Substantively, here are my three personal take-aways:

- Middle-wage jobs lost shares during 1996 to 2004, although not quite as dramatically as seen in CPS results.
- CPS-based findings of almost exclusive growth in high-wage jobs is not confirmed in the OES data.
- There's an intriguing puzzle in the trend in management jobs that bears future work. Did management jobs grow by 1.7 million jobs (CPS) or decline by 2.6 million jobs (OES) from 1996 to 2004? And why do the CPS and OES differ by so much on this one point?

I look forward to seeing more results from further analysis of the OES data.