Comment   Ronald G. Ehrenberg

Charles Brown has very ambitiously attempted to analyze whether the existence of "dead-end jobs" contributes to the youth unemployment problem. He assumes that the average rate of wage growth of individuals initially employed in an occupation and the proportion of these individuals who remain employed in the same industry for five years are both inversely related to the probability that individuals initially employed in the occupation find themselves in dead end-jobs. His basic methodological approach involves using data from the 1/100 sample of the 1970 Census of Population to calculate both of these variables for each three-digit occupation, merging these occupation-specific data into individual records from the 1973–75 Current Population Surveys, and then estimating equations in which the probability that an individual is unemployed at the CPS survey date is a function of the individual's personal characteristics and these occupation-specific variables. Conclusions are then drawn about the extent to which these occupation-specific variables influence young men's probabilities of being unemployed, of having voluntarily left their last job, and of having been laid off. The paper clearly represents a large commitment of time and effort and Brown should be commended for having undertaken it.

My major concern about Brown's approach is that it may not be possible to infer information about the characteristics of an occupation from either data on average wage growth of individuals initially in the

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occupation or data on the proportion of these individuals who remain employed in the same industry over a five-year period. Rather, what we may be observing is information about the characteristics of individuals who choose the occupation.

To illustrate this point, suppose there are two types of individuals: "peaches" who always choose or are selected into occupation 1, and "lemons" who always choose or are selected into occupation 2. Whether an individual is a peach or a lemon can be ascertained readily by employers, but the information used to make this judgment is *not* contained in the CPS survey. True to their names, lemons are "lemons," and as a result will exhibit lower rates of wage growth and higher probabilities of unemployment, which may also result in lower probabilities of their remaining in the same industry. In this situation, if one were to calculate measures of wage growth and industry retention rates for individuals initially in an occupation, and then find after controlling for *measured* personal characteristics, that these variables were correlated in the CPS data with the probability of an individual's being unemployed, one could not conclude that it was the occupational characteristic per se that caused this relationship. Rather, it may simply be that individuals in occupations classified as being "dead-end" ones, on average are lemons (even though we cannot observe this fact in the CPS data). Put another way, we cannot ascertain from Brown's analyses whether it is the characteristics of jobs or the characteristics of workers in those jobs that he has identified. This is a classic example of the problem of trying to distinguish between heterogeneity of individuals and state dependence (see Heckman 1978 for an example).

One might think that this problem could be solved if one could use occupational data that reflected specific technical job characteristics. For example, in some work that I am doing for the National Commission for Employment Policy, I am attempting to ascertain if the probability that an employed teenager becomes unemployed is related to the occupational characteristic data that are found in the *Dictionary of Occupational Titles*. These data have been used with some success by Quinn (1979) and Lucas (1977) in previous work on other subjects. The data include information for each three-digit occupation on a variety of job characteristics such as whether individuals in the occupation have a variety of responsibilities, find themselves in situations which involve repetitive operations carried out according to set procedures, have jobs that allow little or no room for independent action or judgment, are required to control directly or plan an entire activity or the activities of others, are required to perform adequately under stress, are required to have physical strength, and are required to work under poor working conditions (e.g., under extremes of cold, heat, or temperature change, wetness or humidity, noise and vibration, hazards, fumes, odors, toxic conditions,

dust, or poor ventilation). It seems plausible that may of these job characteristics are associated with dead-end jobs.

If in my own work I ultimately observe a correlation between these characteristics and the probability that an employed worker voluntarily leaves or loses his job, one might be tempted to conclude that occupational job characteristics do affect turnover. However, the problem of unobservable individual characteristics still is present. That is, if lemons are sorted (by themselves or employers) into jobs with poor characteristics, it is difficult to determine whether it is the characteristics of the job or the characteristics of the employees which are "causing" the high probabilities of unemployment. To resolve this problem, one must use a methodology which allows one to distinguish between heterogeneity and state dependence. This requires a longitudinal data base that contains a number of observations for each individual; the cross-section data used by Brown is inadequate for this purpose.

Setting this major conceptual issue aside, let me now turn to a discussion of some of the specifics of Brown's work. Brown focuses on young males; young people because their unemployment rates are so high, males to reduce complications which those not in the labor force introduce. In fact, because of the nature of the CPS data, his empirical work excludes individuals not currently in the labor force from the sample. This exclusion has the potential to bias his results substantially since individuals who have dropped out of the labor force may be those who are the most likely to have been in dead-end jobs. Moreover, the fraction of younger males who move from employment to out of labor force status each month is not insubstantial. For example, in Ehrenberg (1980) I show that the gross-flow data from the CPS indicate that during the 1967–77 period approximately 11% of the white males and 14% of black males aged 16–19 who were employed one month were *not* in the labor force the next month. These percentages drop to about 3.5% for males aged 20–24; however, these numbers should be contrasted with the less than .3% rate for white males aged 25–59. The magnitude of these labor force exit rates suggests that exclusion of individuals currently not in the labor force is unwarranted. This is another serious weakness of the CPS data and it again suggests the need to use a longitudinal data source such as the National Longitudinal Surveys or the Michigan Income Dynamics data when one attempts to analyze this question.

Brown's initial discussion suggests that the five-year average growth rate of earnings of individuals initially employed in an occupation is a reasonable measure of whether the occupation consists of dead-end jobs. Somewhat surprisingly, in his empirical research the average beginning wage rate in the occupation and the average wage rate that the individuals obtain five years later are entered as separate independent variables, rather than the growth rate of earnings per se being entered. If his initial

discussion was correct, some measure of the percentage or absolute change in wages in an occupation would be the relevant variable to include. This suggests that the coefficients of the current and future wage variables in his equations should bear certain relationships. In particular, if the percentage change is the correct variable in his equations, the coefficients of the logarithms of the current and future wages should be equal and opposite in sign. While this appears to occur in many cases, Brown does not formally test this implication himself.

Of course, one might question whether the relative wage growth of individuals initially employed in an occupation really does measure the extent to which the occupation is a dead-end job. Brown tabulates wage growth by occupation in table 12.1. Among the fifteen occupations with the lowest rates of wage growth we find clergymen, elementary school teachers, and secondary school teachers (but, fortunately, not college professors or economists). I doubt that one would really want to argue that being a clergyman is a dead-end job (especially if one considers the very long run). It seems clear that the wage growth measure must be capturing other factors, including nonpecuniary characteristics of jobs.

Brown's second proxy variable for the existence of dead-end jobs is the proportion of individuals in an occupation who remain in their initial industry of employment five years later. Estimates of this variable are found in table 12.2 for sixty large occupational groups. While this variable is capturing something in the empirical work, it is again not clear that it is capturing whether jobs are dead end. To draw such a conclusion first requires us to assume that skills learned in an occupation are industry-rather than occupation-specific. Furthermore, all of the eleven highest occupations in this ranking, save for police and telephone installers and repairmen (which is a highly industry-specific occupation since the vast majority of its members are employed by the Bell System), require individuals to have college degrees and are high-skill jobs. In contrast, the ten lowest-rated occupations are primarily low-skill jobs, with little formal educational or training requirements. Brown's industry retention rate variable, therefore, is very highly correlated with the skill level or educational requirements of occupations; it is not surprising then that he finds that unemployment probabilities are correlated with this variable. In my view, a much more interesting variable would be industry retention rates by occupation that standardize for the skill composition of occupations. The relevant question is not whether occupations in which college graduates wind up have lower turnover than those in which elementary and high school graduates are sorted, but rather if among the range of occupations open to elementary and high school graduates there are some dead-end and some non-dead-end jobs.

Brown's sample restrictions are also not always the ones I would have made. Restricting his sample to individuals who are not in school elimi-

nates most teenagers from the sample. Furthermore, it prevents us from learning how initial part-time employment of enrolled youths influences their subsequent labor market success. I have already commented on the effects of his exclusion of individuals currently not in the labor force. Finally, his classification of unemployed individuals into those who were laid off or lost their last job, those who quit, and those who could not be identified (e.g., those who dropped out of the labor force and then reentered) ignores the distinction between permanent and temporary layoffs. While one might expect that high skill level jobs would have a low probability of permanent layoff, to my knowledge nothing in the theory or empirical evidence on temporary layoffs suggests that the probability of temporary layoff is small for this group. Unfortunately, he cannot make this distinction with the CPS data. Again, a true longitudinal data base is required.

Rather than rehashing his results, let me summarize the main message of my comments. First, longitudinal data are required and an attempt must be made to distinguish between unobservable heterogeneity of workers and state dependence. Occupational characteristic variables used in the analysis which are truly characteristics of the job (such as the *Dictionary of Occupational Titles* data) rather than the characteristics of the individuals who inhabit the positions will help but not solve the problem. Third, it is important to include those people temporarily out of the labor force in the sample, to consider the part-time employment experience of individuals enrolled in school, and to distinguish between temporary and permanent layoffs. While Brown must be commended for undertaking his ambitious, creative, and time-consuming study, it is clear, as he notes in his conclusion, that the results in the paper are too weak to justify either a confident yes or no answer to the question, "Is there evidence of a relationship between lack of opportunity for advancement and youth unemployment?" It is my hope, and I am certain his, that future research on this subject will provide more precise answers to this question.

References

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