Job Loss and Job Change: Discussion by the Authors

Comment by John T. Addison and Pedro Portugal

All three papers deal with important policy issues. Ruhm, whose analysis ranges the most widely, goes furthest in recommending policy activism. Podgursky and Swaim (P&S) are somewhat more restrained, but it is clear that they too favor special assistance for displaced workers. In our own paper we see advantage in workers being given notification of their impending redundancy in the specific context of plant closings and relocations, although our treatment admittedly does not examine the role of firm heterogeneity in producing the observed pattern of results. Nevertheless, it is the case that our understanding of the displaced worker problem remains rudimentary.

Ignoring, for now, potentially important problems that have to do with the definition of a displaced worker, our focus here will be on the computation of the earnings loss. P&S deal with before-and-after earnings comparisons, or relative earnings losses. Ruhm makes a broad comparison of the earnings of stayers and movers and has the advantage of a somewhat longer follow-up period. The use of a control group is advantageous in computing wage losses for fairly obvious reasons, but simple comparisons of the earnings of movers and stayers are misleading (see Mincer 1986). In particular, any estimation using a control group should seek to model explicitly the decision rule that motivates some workers to move and others to stay put. Another selectivity problem addressed by P&S, but not by Ruhm, concerns the selection of workers into employment status. We return to this issue below.

Focusing for the moment on the before-and-after approach, and recognizing as a possibility that the displaced could have higher earnings than their nondisplaced counterparts if not all stayers, what conclusions can be drawn from the observed wage changes? A particularly difficult question concerns the sacrifice of firm-specific training investments. It is conventional to argue that this loss can be discovered by looking at the pre-displacement wage tenure coefficient (e.g., Hamermesh 1987). But, as Ruhm correctly notes, tenure on the lost job is positively correlated with post-displacement wages. (P&S detect a generally insignificant coefficient on tenure and focus their attention instead on lost specific investments resulting from changes in industry and occupation).

The basic problem with the tenure variable is that it is incapable of distinguishing between general and specific training investments in the case of younger workers and of detecting switching from specific to general training investments in the case of workers who either are informed of or otherwise know about their impending redundancy. Our own work would suggest that the conventional measure of lost specific training investments—that is, assuming a zero effect of pre-displacement tenure on the new job—could inflate estimates of the mean earnings loss by up to 45 percent (Addison and Portugal 1987). That said, there is still the issue of apparently large earnings losses

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associated with industry and occupational shifts.

There is currently much dispute over the meaning of the tenure coefficient in standard Mincerian earnings functions. The basic problem is that, once one accounts for individual and job match heterogeneity,1 the partial effect of tenure on wages is small; instead, general labor market experience (coupled with purposive mobility) accounts for the bulk of wage growth over a career. Unfortunately, this argument raises a new set of problems for computing earnings losses from displacement—although it does suggest other rationales for the contribution of tenure on the first job to post-displacement earnings—since the returns to a good job match, no less than to firm-specific investments, may be dissipated by displacement.

Much more work is urgently required on the tenure-earnings relation. Indeed, the outcome of such work might offer stronger support for the labor market segmentation argument advanced byRuhm than does his own evidence. That evidence is rather poor. Notably, the argument that women and blacks gain less from mobility than do white men can hardly be said to provide evidence of labor market segmentation. Furthermore, the race variable is never significant in the mover sample and the composition of the female sample is simply too restrictive to permit any general statement.

Of the three studies, only ours specifically examines unemployment duration. One aspect of our inquiry calls into question the uncritical use of an unemployment insurance variable as a regressor in the wage or duration equations. P&S, on the other hand, do consider the selection of workers into employment status in formulating their selectivity argument. (They do not, however, seek to correct the wage equation for selection into labor force status.) P&S obtain a negative selectivity coefficient, as have other researchers in the field. We are sympathetic to their conclusion that the negative coefficient is spurious. Unemployment duration has opposing influences on wage offers, intensifying search effort on the one hand, but stigmatizing the searcher and depreciating human capital, on the other. Of the two influences, the latter is likely to dominate.

Note, too, that there is a reverse line of causation running from wage offers to unemployment duration. The effect of wage offers on duration of unemployment could well dominate. More generally, we would argue that the sign of the selectivity coefficient is sensitive to specification, namely, the use of reduced-form versus structural-form equations and the definition of the dependent variable (e.g., wage level, change in wages, or—the intermediate case—displacement wage as a function of the pre-displacement wage).

Other complications in the computation of earnings loss concern severance pay and other separation settlements, the existence of rents on the pre-displacement job, and lost training investments by the firm (a flat tenure-earnings profile is, of course, consistent with substantial firm-financed investments). The force of the severance pay argument is in one sense balanced by lost pension and fringe entitlements. In neither case do we have data. But, to the extent that *certain types* of job losses are accurately predicted, severance pay may be an important form of ex post compensation. In other cases of more predictable job loss, the pre-displacement wage may compensate workers ex ante. Clearly, as is implicit in all three papers, there is no reason for us to be concerned with lost monopoly rents. At issue is the scale or even the existence of such rents. One flimsy clue as to the existence of rents might be the regression toward the mean detected by P&S. A better indication, however, would be given by the coefficient on an appropriately specified union membership variable. The issue of

For example, if more able workers are more stable and receive higher wages *on account of their greater ability,* then failing to take this association into account will upwardly bias the return to tenure. Similar bias arises from good jobs paying higher wages than others, which will reduce the likelihood of quits; and from some job matches being better than others, the reward for which also reduces turnover.

lost firm-financed training is relevant because it highlights the difficulties posed by any compensation principle.

If earnings net of monopoly rents are not compensated by the market, if the worker-financed firm-specific component of earnings is large, and if the size of the displaced worker population is substantial, then an efficiency argument may be advanced for some form of ex post compensation in the form of, say, lump-sum payments or retraining allowances. But the full facts of the case have yet to be documented. This uncertainty inhibits the formulation of precise policy recommendations.

The three studies do prOVide us with more information on displaced workers, even if they raise rather more questions than they answer. Support for new sweeping policy initiatives is not yet indicated, although one paper makes a case for advance notification and all contribute materially to the analysis of the consequences of displacement. As is conventional, we have tended to focus here on points of disagreement or lack of clarity and not on matters about which there is general agreement.

Comment by Michael Podgursky and Paul Swaim

Policy discussions of plant shutdowns and job displacement have proceeded largely in the absence of reliable data. As recently as 1983, congressional hearings on structural unemployment produced estimates of the number of dislocated workers that ranged from 100,000 to three million and widely divergent assessments of the adjustment difficulties that result from permanent job loss (U.S. Congress 1984). These three articles attempt to fill some of the gaps in our knowledge of the incidence and economic effects of job displacement.

Our paper and Ruhm's, based though they are on two very different micro data files, report surprisingly similar results. We both find that average earnings losses following displacement are modest—on the order of 10 percent—but that the dispersion is very high, with a sizable minority of displaced workers experiencing very large reductions in earnings. Furthermore, both studies show that most losses persist for at least five years. Although Addison and Portugal do not emphasize the variation in weeks of joblessness following displacement, we have shown elsewhere that such variation is pronounced, and that workers who are jobless for a long period tend to suffer the largest earnings losses once reemployed (Podgursky and Swaim 1987). These several findings dearly show that many displaced workers suffer considerable adjustment difficulties.

The findings reported in the two papers on earnings (ours and Ruhm's) do differ in a number of respects, but these differences seem largely attributable to differences in the data sets used. For example, Ruhm's female sample—mostly poor heads of households—generally were not adversely affected by displacement, whereas our more representative sample of displaced women experienced larger median losses than men.

One strength of the PSID data used by Ruhm is that the earnings trajectory of displaced workers can be directly compared to that of nondisplaced workers. The 1984 Displaced Worker Survey (DWS), which we used, lacks retrospective data for nondisplaced workers, but has the advantage of being more current, more representative of the national work force, and, perhaps, more precise in distinguishing displaced workers from workers discharged for cause. Both articles also present findings that are not easily reconciled with market-clearing, which suggests that future research on displaced workers might utilize efficiency wage models or other models of job rationing.

In their analysis of duration of joblessness, Addison and Portugal conclude that advance notice significantly reduces expected spell lengths, but that this effect is greatly reduced in the presence of unemployment insurance benefits.[1] These findings must be viewed with caution. As the authors correctly note, some workers answering "yes" to the "advance notice" question may not have received formal notice of impending layoff, but merely expected to be laid off. A negative answer to this question, however, is also ambiguous. As can be seen in their Table 1, 10.8 percent of the presumably surprised "no" group reported zero weeks of joblessness, as compared to 15.8 per cent of the forewarned "yes" group. We suspect that some of the former group were not completely surprised and initiated job search before the shutdown. Nonetheless, the finding that a rough measure of advance notice is associated with shorter jobless spells is certainly of policy interest.

Although Addison and Portugal state their findings carefully, a casual reader might conclude from their article that unemployment insurance largely eliminates the positive effect of advance notice on jobless duration. No such inference can be drawn, since the dummy variable for receipt of UI benefits (BEN) is so clearly endogenous. As their Table 1 shows, advance notice increases the probability of zero weeks of joblessness, but has no discernible effect for workers with positive jobless spells. Workers helped by advance notice are thus not unemployed long enough to collect UI, even though most of them surely would have become eligible had they remained unemployed longer. The fact that a worker received UI benefits (i.e., BEN = 1) thus indicates that advance notice failed to save that worker from unemployment, but it does not follow that UI was responsible for that failure.

This simultaneity problem is compounded when interactions between BEN and other independent variables are added to the model. For example, years of tenure on the former job (TENURE) acquires a significant negative coefficient when BEN*TENURE is added to the model. The coefficient for tenure, however, now registers the combined effect of at least one year of job tenure (hence likely eligibility for UI) *and* nonreceipt of UI by the worker. Not surprisingly, this combination is very strongly associated with short spells of joblessness, but what is the economic significance of this association? Again, BEN = 1 is serving primarily as a proxy for at least two weeks of joblessness, and its coefficient is not a measure of the impact of the UI system on job search.

The January 1984 DWS was the first large national data file designed to identify displaced workers. These papers, and others published and circulating, show what a valuable statistical resource it represents. We are pleased that a similar survey was repeated in January 1986 and another is scheduled in January 1988.[2] We hope the 1988 survey will be further revised and extended to provide more useful information on important policy variables, such as the receipt and length of advance notice, UI eligibility, and duration of unemployment. The impending availability of so much new information on displacement guarantees many more studies on this important topic, and further tests of the findings of the three papers presented here.

1. We label the dependent variable the duration of joblessness rather than unemployment, since the relevant question on the DWS did not refer to active job search. (The survey question read "Since [the worker] left that job, how many weeks was [the worker] without work?") The reported spells may thus include intervals that would normally be classified as labor force withdrawal. Although weeks of joblessness may *overstate* weeks of unemployment, it may *understate* total search time, since it excludes search prior to job loss.

2 In addition to these household surveys, the Bureau of Labor Statistics has also begun reporting establishment data on mass layoffs and plant closings based on state UI claims data. See U.S. Department of Labor (1987).

Comment by Christopher J. Ruhm

The papers in this volume, along with recent related work, provide much new information and contain important lessons for researchers and policy makers. Five "stylized facts" on the causes and consequences of labor mobility have emerged. (1) Although blue-collar and manufacturing workers are most likely to experience permanent layoffs, labor displacement occurs in all sectors of the economy and all types of workers are at risk. (2) There is a wide variance in post-layoff adjustment patterns, with most workers experiencing small income losses or modest improvements in wages, but a significant minority suffering large reductions in compensation and extended unemployment. (3) Among that minority, some workers' wage losses prOVe to be transitory, but for most the wage loss persists for several years. (4) There are important differences among demographic groups in post-layoff adjustment patterns. In particular, nonwhites and women experience greater difficulties than whites and men. (5) Prior notification of impending permanent layoffs is associated with modest reductions in future joblessness, but the reasons for that association are unclear.

Podgursky and Swaim's (P&S) paper complements and improves upon earlier work using the 1984 Displaced Worker Survey (DWS). Their findings of wide variances in displacement outcomes and severe adjustment difficulties for nonwhites and women closely accord with the results of my paper. Since the two studies cover different time periods and data sources, this consistency justifies considerable confidence in the results.

P&S should be applauded for including short-tenure workers in their analysis, rather than following the example of some other researchers who have eliminated workers with less than three years' seniority in the preseparation job. Defining displacement in terms of previous seniority is dangerously restrictive, given that displaced workers may have recently left a job of longer duration, and recent evidence calls into question the extent to which adjustment problems increase with tenure.[1]

Some of P&S's results must be qualified by limitations in the DWS. Because that source provides no information on previous wages or employment conditions for workers not experiencing displacement, P&S are forced to use occupation and sector wage trends when estimating the earnings changes of displaced workers. Similarly, their measure of the persistence of wage changes requires the assumption that similar types of workers were released in each year between 1979 and 1984. Fluctuating economic conditions during the period cast doubt upon this assumption.

I am more concerned, however, with the interpretation of the regression results. P&S argue that the observed negative correlation between previous wages and post-displacement wage growth implies that workers with substantial human capital lose the most from displacement. Given that education, experience, and seniority have been controlled for, it is more likely that high previous wages imply *positive error terms* (the luck of finding a relatively high-paying job). To the extent that preseparation wages contain a random disturbance, some regression to the mean is virtually inevitable.

P&S's interpretation of the negative sign on the inverse Mill's ratio is particularly questionable. Although I suspect that high-paying jobs are rationed, with nonwhites and women at the end of the queue, the evidence presented in this paper is unconvincing. In three of four cases, the inverse Mill's ratio is statistically insignificant, and, in any case, a negative ratio can be explained in a variety of ways.[2]

Further, the sign of the Mill's ratio is

1. For example, see Ruhm (1987).

2. The negative sign indicates that reemployed individuals have *lower* expected wages than those unemployed at the survey date but says nothing about the process generating this result.

quite sensitive to the type of earnings function estimated, with small changes in specification causing the variable to switch from significantly negative to positive.[3] A better strategy would be to look for labor market rationing directly—by focusing, for example, on discouraged workers, involuntary part-time employment, and instances of repeated job turnover.

Addison and Portugal (A&P) present the first analysis of prior notification on post-separation joblessness, using micro data from a nationally representative sample. Table 1, which shows hazard and failure rates, is particularly interesting. It indicates that prior notification reduces joblessness by around a month—primarily by increasing the probability that workers become reemployed *without intervening unemployment.*

A&P do not point out that for workers experiencing positive levels of unemployment, *there is essentially no difference between the notified and non-notified groups.* For example, 83.5 percent of notified workers with some joblessness are unemployed at least four weeks, 72.1 percent for eight or more weeks, and 62.0 percent for thirteen or more weeks. For non-notified workers, the corresponding percentages are 83.7, 70.5, and 62.7 percent. Given this surprising finding, I wish that greater effort had been made to determine the mechanisms by which prior notification assists workers in mOVing directly to new jobs without experiencing unemployment.

I find it difficult to interpret A&P's hazard regressions. For example, given that the primary impact of prior notification occurs prior to the layoff date, it is correct, by definition, but provides little new information to observe that the receipt of unemployment benefits attenuates the impact of prior notification. Workers able to avoid unemployment will not receive benefits; those who are unsuccessful experience joblessness similar to that of their non-notified counterparts.

I also wish that A&P had acknowledged several weaknesses in the DWS for their study. Where their hazard model assumes that post-displacement *unemployment* occurs in a single spell, the survey data do not permit them to distinguish between time out of the labor force and unemployment, nor can they tell whether joblessness occurs in a single spell or in multiple occurrences, punctuated by short periods of employment. The effect of unobserved worker heterogeneity also deserves more attention when interpreting the regression results (particularly the interactions between unemployment benefits and prior notification or job tenure). Finally, it would have been useful to broaden the analysis to include partial (as well as total) plant shutdowns.

The three papers in this volume provide important information on worker adjustment to permanent job terminations, but much remains to be done. Until our understanding becomes more complete, policy makers should be cautious about accepting the "conventional wisdom" about who suffers the most from labor displacement and how they can best be helped.

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3. Regressions showing these results are available upon request.

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