sectors, union wage gains in the public sector would not necessarily represent a monopoly rent and a wealth transfer from taxpayers to public employees, as suggested by E & E on page 315. Instead, the union wage effect would represent, in whole or in part, the theoretically predictable offset to monopsony rents reaped by taxpayers under nonunion conditions. E & E's figures suggest that a public sector union does little more than raise wages to the level of similar workers in the nonmonopsonized private sector,[3] offering no support for the "extortion" hypothesis of Wellington-Winter.

In short, if productivity is equal (or similar) in the two sectors and if monopsony is absent, we have no consistent explanation of how public managers are able to pay their nonunion workers significantly less than apparently equivalent nonunion workers in the private sector. Even if we accept either of these reasonable alternative explanations, the arguments that public managers have objectives other than cost minimization or are less willing to resist "excessive wage demands by unions" are still not confirmed by E & E's regression results.

E & E also advocate the contracting of refuse collection to the private sector, with the expectation that private sector managers are more apt to minimize costs and to resist union wage increases, thus allowing the contracting cities to capture the monopoly rents that would otherwise be appropriated by public sector unions. E & E apparently overlooked their own "facts," namely: (1) private sector wages (union and nonunion) are roughly equal to those in the unionized public sector, and (2) the most cost-effective category in the E & E study (given equal productivity) is public refuse collecting with nonunion workers.

Finally, we would cite another disconcerting feature of E & E's regression results. In each of the six regressions, the authors use both an opportunity wage and the probability of obtaining employment *(ER)*. Aside from questions of the theoretical appropriateness of including both such variables is the fact that the coefficients on *ER*, expected to be positive, are exclusively negative and statistically different from zero in half of the regressions (see E & E's Table A3). Nowhere in the article is this unexpected result analyzed or explained.

[3]One anonymous reviewer suggested that the regional distribution of cities was not controlled and could affect the magnitude of the union wage effect, especially in the public sector. We agree that this further hampers interpreting E & E's results.

Conclusion

Although a narrow view of E & E's results might confirm the Wellington-Winter hypothesis, these results are equally or more amenable to alternative explanations. Since the authors failed to offer a consistent explanation of the difference between public and private sector nonunion wages, since they failed to find wage differences between public sector union workers and both private sector union and private sector nonunion workers, and since the unexpected results for the powerful *ER* variable were consistently negative, it is difficult to place much confidence in their analysis. E & E's results thus provide little support for policy prescriptions based on presumptions that public unions are strong monopolists or that public managers possess insufficient incentives to resist union wage demands. Concentration on a few of their coefficients might substantiate E & E's arguments, but a less myopic view argues for caution in accepting the Wellington-Winter hypothesis without further detailed analysis and more convincing evidence.

LEON B. PERKINSON

Economist
Economic Research Service
U.S. Department of Agriculture
Raleigh, North Carolina

ROBERT M. FEARN

Professor
Department of Economics and Business
North Carolina State University

Reply

In their comment, Perkinson and Fearn (P & F) claim that our empirical test of "the Wellington-Winter hypothesis that public unions are more effective in raising wages than similar unions in the private sector . . . raises more questions than it answers." They argue that we overlook a number of important issues because of our "myopic concentration on the difference between union and nonunion wages in the two sectors."

We believe, in contrast, that much of what Perkinson and Fearn claim stems more from their affliction with hyperopia than from our myopia: they seem, that is, to focus best on issues furthest removed from those we analyze in our paper. As a consequence, they fail to see clearly the power of our empirical results.

Perkinson and Fearn make four points. First, they argue that our finding that public unions are more successful than private unions in raising wages is not believable because we fail to explain why our findings also show that the average wage levels of nonunion workers are lower in the public sector than in the private sector. Perkinson and Fearn presumably believe that unless we can fully explain wage *levels* in the two sectors, our tests that concentrate on sectoral differences in the union wage relationship cannot be valid.

This inference is unwarranted, unless P & F know more than we do about the missing variables needed to explain the public/private wage differentials of nonunion workers in this industry. In our opinion, the factors most likely to explain these differences are fringe benefits, job security, and, possibly, labor productivity and quality. Consider the case of fringe benefits. Everything that we know about this industry suggests that fringe benefits of all kinds are considerably greater in the public sector than in the private sector. A lower wage for nonunion public workers, therefore, may well be the result of a negative "compensating differential." A similar argument can be made about job security. The effects of differences in labor productivity and labor quality on the public/private differential are more speculative. It is quite possible, however, that the first two factors alone can explain the entire wage differential to which P & F refer.

In their rush to attach methodological significance to this wage difference, our critics simply choose to overlook the discussion in our paper of how differences in factors such as fringe benefits and productivity might affect our conclusion. On page 313 of our article, for example, we discuss both the fringe benefit and the productivity issues that they raise, and we conclude that these are not likely to engender the methodological biases that they suggest. To invalidate our findings, P & F would have to argue that public sector unions raise worker productivity by more than private sector unions do, or that private sector unions raise fringe benefits by more than public unions do. They make neither argument, and for good reason: all evidence points in the opposite direction.

Thus, while acknowledging that we do not explain overall public/private wage differentials (nor do we try), we find nothing in P & F's comment to suggest that by not doing so, we biased our tests of the Wellington-Winter hypothesis in the direction of accepting this hypothesis.

The second point P & F make is their most interesting, although they are again confused about its relevance to our study. They argue that "the pattern of wage relatives among and across sectors... could be explained equally well by the presence of monopsony in the public sector *sans* unionism." The main difficulty we have with this argument is its assumption that the labor market for public sector refuse collectors is a segmented market. This assumption is highly questionable. Refuse collectors are unskilled workers, who can choose among many alternative job opportunities in the private sector. For example, in many cities where households are served by public refuse collection, commercial enterprises are served by private refuse collection. Workers can easily move from one sector to the other. Moreover, there are many similar unskilled jobs in manufacturing, retailing and construction. Thus, unlike employees in many other public sector occupations, such as police, firefighters, and teachers, refuse collection workers need not look exclusively or even primarily to the public sector for employment. It is difficult, therefore, to attach much significance to the monopsony argument in the case of our study.

Even if we agree with P & F that public

occupations may have monopsony power, the point they should emphasize, but do not, is that it may not be all bad for public sector unions to possess the "excessive" power envisioned by Wellington and Winter. This power would counteract public sector monopsony power, resulting in a kind of Galbraithian "countervailing power" equilibrium, a solution that they may feel is more equitable.

While this is clearly an interesting and provocative policy issue, our paper does not address it. Instead, we focus solely on the question whether public sector unions have more success in raising wages than private sector unions? Our finding that they do still stands. Whether this is desirable or undesirable can only be determined in the context of specific public sector occupations and labor markets. In the case of public refuse collection, we do not see the desirability of public unions exercising monopoly power, since we do not believe that public sector monopsony power in fact exists.

Third, P & F claim we fail to explain the unanticipated estimates for one of our independent variables, *ER,* which is meant to be a proxy measure of the probability of obtaining employment. This variable is significantly negative only for cities with public sanitation service. Unfortunately, our insight has not increased appreciably since the publication of our article — we still cannot explain this result. In case P & F are concerned, however, we wish to point out that the inclusion of *ER* is not responsible for our results with respect to unionism, which is, after all, the central point of our paper.

Finally, P & F question our brief endorsement at the end of our paper of contracting out refuse collection to private carters as a "possible strategy to temper the power of public sector unions." In questioning the wisdom of this strategy, they unveil the following syllogism: since wages are no higher in the public sector than in the private sector (a finding in our paper), and since there is no difference in public and private sector labor productivity (by their assumption), it follows that private collection cannot be more efficient or desirable than public collection. We are not impressed by this reasoning. What does impress us, however, are the findings of a study of this precise issue conducted with the very data we use, a study that takes into account *all* costs relevant to the argument.* That analysis finds that collection costs per unit of output are lower when sanitation services are contracted out to private collection firms. We agree, nevertheless, that the endorsement or condemnation of any such policy requires careful study of the product and market that would be affected.

In summary, we do not believe that P & F make a single point that in any way invalidates our finding that public sector unions have greater power than do unions in the private sector. Perhaps it is not our myopia but their hyperopia that needs treatment. A little clearer focus on the issues at hand would have saved readers a considerable amount of eyestrain.

LINDA N. EDWARDS

Professor
Queens College
The City University of New York

FRANKLIN R. EDWARDS

Professor
Graduate School of Business
Columbia University

*Barbara Stevens, "Scale, Market Structure, and the Cost of Refuse Collection," *Review of Economics and Statistics,* Vol. 60, No. 3 (August 1978), pp. 438–48.