trial relations literature clearly shows. Although the defeat of PATCO was extremely important, the undermining of construction unionism, spearheaded by the Business Roundtable, was happening in the 1970s, and the beginning of concessions by major unions occurred in that same decade, the most prominent example being the 1979 contract concluded between the UAW and Chrysler as part of Chrysler's bailout package. Nor, on the other side, was BASF the only dramatic victory for labor in the complex industrial relations landscape of the 1980s: Pittston coal miners, New York State electrical workers, and Las Vegas hotel workers, among others, registered important gains, after major strikes.

Facts about any individual labor victory or defeat, while often highly illuminating, are rarely if ever a projection in miniature of the trajectory of U.S. labor relations. Minchin's detailed account of the struggle around the 1980s lockout at BASF is an important piece of recent labor history (which I heartily recommend to everyone interested in the subject), but it must be put into a broader context to be fully understood.

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Labor Economics

Monopsony in Motion: Imperfect Competition in Labor Markets. By Alan Manning. Princeton: Princeton University Press, 2003. 424 pp. ISBN 0-691-11312-2, $45.00 (hardback).

> *…another bad case of monopsony in motion … monopsony can break your heart … the capitalists will rue the day when workers win their way.*
>
> musical tribute to *Monopsony in Motion,* at http://econ.lse.ac.uk/staff/amanning/work/book.html

The haunting electronic beltings of John DiNardo, in what has to be the only theme song ever written for an economics book, might send a shiver down the spine of the most conservative neo-classical labor economist. Beyond its entertainment value—it will surely convince a few people that advanced material in labor economics is not always somniferous—I believe the song is a shot across the bow of perfect competition's ship, as Alan Manning sets sail to challenge a number of accepted explanations of stylized facts in labor economics.

What would you do if your employer cut your hourly wage by one cent? "Lace up my running shoes and sprint for the nearest office exit," you might say, parrying a silly question with a silly answer. And yet such questions naturally arise when one assumes that all firms operate in perfectly competitive labor markets. The one-cent-per-hour wage cut is, in fact, the seemingly daft example with which Alan Manning introduces *Monopsony in Motion* and sets the stage for his careful examination of the various puzzling implications of taking perfect competition as the baseline when thinking of how the labor market operates.

Manning emphasizes that blind adherence to the perfectly competitive paradigm means implicitly accepting a divergence between theoretical predictions and observed empirical regularities. Among the more important theoretical predictions that appear to be on shaky ground are that minimum wages unequivocally reduce employment (there is a dearth of corroborating empirical evidence); that firm size and wages are uncorrelated, all else constant (empirical evidence shows rather large positive employer size–wage correlations); and that workers bear the full costs of general training (empirical evidence shows that many firms provide general training). Often, these and many other observed empirical regularities are "explained away" either by advancing new theories, viewing them as anomalies, or even attributing them to monopsonistic behavior—without explicitly naming it as such. However, until now, there has been no unifying thread in the explanation of these observations.

Manning presents a fascinating table indicating that only 1.5% (my calculations) of the pages in standard labor economics textbooks are devoted to monopsony. Perhaps even more telling would have been the observation that less than 0.75% of the labor economics publications catalogued in *Econlit* since 1969 include "monopsony" or "segmented labor markets" as either main subjects or keywords. Moreover, where there has been work in the area, rarely is it bold enough to suggest that an entire paradigm be changed. A review of the monopsony literature by Boal and Ransom in 1997 (*Journal of Economic Literature,* Vol. 35 [March], 1997, pp. 86–112) found that monopsony based on location and small numbers of employers is rare, but has a large effect when it exists, and that monopsony power based on frictions is widespread, but with smallish effects on average. However, the authors never suggested that these results pose a significant threat to the perfectly competitive paradigm.

No doubt, to some extent the issue of monopsony has been ignored by many labor economists. However, it is also likely in some cases that they use market power to explain empirical observations, but simply are unwilling to "give it a name." One example of this tendency Manning astutely notes is the wide acceptance of "frictional unemployment" as an explanation for why the natural rate of unemployment exceeds 0%. Though the very term "frictional unemployment" screams monopsony, the prevalence of this explanation has not given rise to any more general suspicion that frictions play an important role in other areas of the labor market.

Neoclassical hubris should not be blamed for these glaring omissions as much as fear of having to rewrite a half-century's worth of textbooks and research papers. Undoubtedly the lofty ambition of *Monopsony in Motion,* to change the perfectly competitive paradigm to one in which all employers have at least some market power, sounds threatening. However, Manning skillfully disarms this threat by observing that in the field of Industrial Organization, neo-classical economists freed from the straitjackets originally fitted to them by their perfectly competitive dogmatism have enjoyed increased flexibility. These economists' acceptance that all firms have at least some market power in the product market neither forced them to rewrite textbooks nor undid the hundreds of years of economic research in the field.

Not until the final chapter does Manning explicitly make the point that an acknowledgment of the role played by monopsony need not explode neo-classical economics. One reason he delays may be to allow readers to arrive at the same conclusion on their own, but it may also be that he wishes to end on a harmonious note. His view is that in most areas of labor economics, taking a monopsonistic perspective does not overwrite existing labor economics, but instead adds to it. He is not telling us to ignore obvious demand and supply factors, for example, but simply to take monopsony into account as another important variable when we think about cases that are at the "frontier" of labor economics. For instance, we all know why economics professors are paid more than English professors; what is harder to understand is the causes of changes of wage inequality within the profession and the impact of wage-setting institutions on the profession.

Manning demonstrates that textbook static models of monopsony and their explanations of the origins of employer market power (think one-company towns) need refinement before we can move forward with the acceptance of this new paradigm. He advances dynamic models of monopsony and general equilibrium models of oligopsony, and allows for a more general consideration of the choice set facing individual employers. These models are all built on the assumption that the labor supply curve facing any individual firm is upward-sloping for three reasons: lack of employee information about labor market opportunities, individual heterogeneity in job preferences, and mobility costs. Using these tools, he generates empirically testable predictions to systematically analyze each of the major topics typically covered by standard labor economics textbooks.

Given the breadth and depth of the issues Manning covers—clearly, a staggering amount of work went into this book—even skeptical readers will not be able to dismiss his theory lightly. They may find flaws in some of the theoretical assumptions, or disagree with the implications of certain empirical results, but the sheer volume of the cases in which a monopsonistic worldview explains (or improves upon existing explanations of) the empirical regularities compels attention.

This book would be an excellent supplement to a graduate course in labor economics. The theoretical models are extremely well presented, and Manning highlights how each model diverges from a perfectly competitive one using both the model's mathematical output and, more important, intuitively compelling verbal comparisons. Though the theory and empirical formulations are sometimes complicated, Manning never takes his focus off what monopsonistic labor markets mean for real world behavior. Further, each chapter presents a thoughtful and nearly comprehensive review of the relevant empirical work related to the topics discussed, except where the volume of previous literature forbids doing so. Many of these works are already a part of the graduate labor discipline, and it is refreshing to see them cast in monopsony's new light.

Monopsony in Motion is also a goldmine of ideas and methodologies for the aspiring empirical labor economist. The author spells out, often in great detail, how to mechanize many of the theories that he puts forth (which is rare in a textbook), but even more valuable is his explicit highlighting, on about a dozen occasions, of areas in which more research is needed. In many cases, he presents the theory, the empirical strategy, and even the data that would be needed to proceed. The wide reach of the book also suggests that taking a monopsonistic view of the labor market may enhance research well beyond the topics covered explicitly in this text.

Probably Manning's most significant contribution in this book is his insistence, backed by strong evidence, that policy-makers should rely on conclusions that are drawn from thoughtful empirical work rather than blindly subscribe to the predictions of a fragile theory. At least since the days of the negative income tax experiments in the United States, most empirical labor economists, if asked, would have agreed with that sober advice, but the multitude of examples Manning provides makes it less ignorable than ever.

The book is so well written that even the most complicated material in it is readable. The presentation is also commendably well balanced, given Manning's stated intention to change our minds. Nary a theoretical model or empirical result is presented without caveats calling attention to possible theoretical or methodological pitfalls. Not only is this an admirably honest approach, but it also aids in understanding the material.

Graduate students and experienced scholars alike will benefit greatly by considering this book the next time they set out to teach a labor economics course or to write an empirical paper. *Monopsony in Motion* deserves a place on our bookshelves alongside the other seminal works in labor economics.

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International and Comparative Industrial Relations

Inequality around the World. By Richard B. Freeman. New York: Palgrave (Global at St. Martin's), 2002. xiii, 280 pp. ISBN 0-333-77354-3, $75.00 (cloth).

One way of reading this volume is as a practical illustration of how many different ways one can think about inequality. Because people differ in the reasons why they are concerned with "inequality," there are a variety of possible measures. Because the specifics of definition can matter a great deal, in practice, to measured levels and trends, comparisons of inequality are often complex and occasionally ambiguous. Hence, any study of inequality should begin by specifying *why* it focuses on inequality *of what* and *among whom.*

If an analyst's interest in "inequality" derives from a concern for equity in compensation for work effort, then the distribution *of earnings, among individuals* is the appropriate focus. In this tradition, the book begins with three chapters that discuss inequality in returns to labor—but even among these papers there are significant differences. The first chapter, by Richard Freeman and R. H. Oostendorp, is a cross-national comparison of the inequality between occupations in average wages (pre-tax). It stresses the role of country-specific institutional factors in determining the occupational wage structure and the compressive impact of economic development on average occupational skill differentials (but emphasizes the difficulty in comparing national data sources with different earnings concepts, occupational specifications, and so on). R. G. Gregory's chapter focuses on the trends in individual earnings inequality over time within a single country (Australia) without reference to occupation. M. Manacorda's chapter similarly uses microdata on individual (full-time) earnings and also stresses the role played by institutions—specifically, the method of wage indexation (Scala Mobile)—on wage differentials and the return to education in Italy.

Methodologically, these three papers have little in common, but they do share at least one general moral: that institutions matter for earnings inequality. In particular, Gregory demonstrates that until the mid-1970s, Australia's centralized wage award system generated levels of employment and wage growth similar to those in the relatively unregulated U.S. labor market, and that since 1975 there has been no employment dividend to moving away from the tradition of using labor market regulation to reduce inequality, although there has been a substantial increase in wage subsidies, with associated problems for incentives and public finances.

However, if our interest in "inequality" derives from a concern with economic well-being—in particular, if we are concerned with the contrast between great affluence for some and poverty for many others—then we need to con-