Editors' Introduction: The Effects of New Work Practices on Workers

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The effects of employee involvement have been widely studied, including a special issue of this journal (Ichniowski et al. 1996). This issue examines the effects of involvement and related programs on employees. This introduction examines several relevant theories and describes the methodological challenges. Of the many outcomes of interest ranging from employee satisfaction to health and safety, we focus our literature review on one: wages. The results of the studies we review vary widely, as do the programs those studies examine. Our reading is that many programs have no effect on wages, while on average, the effect is a small increase in wages after companies introduce new work systems with higher employee involvement.

Since at least the 1930s, many management writers have advocated employee involvement (EI) in decision making as a way to improve both organizational performance and employees' lives. Although discussions shifted from suggestion systems and T-groups to quality of work life, quality circles, self-directed teams, total quality management (TQM), and beyond, the basic premise remains: Workers have insights into how to improve their jobs. Thus management and workers can strike a win-win bargain so that workers contribute more ideas and receive benefits of higher pay, greater job security, and improved working conditions. Sometimes the literature emphasizes intangible rewards: Involvement inherently makes work more interesting and enjoyable, thus increasing intrinsic satisfaction and motivation.

The recent period may be distinctive in the level of interest these ideas have generated. Since the deep recession of the early 1980s and the rise of Japanese competition around that time, recommendations for employee involvement have moved from a limited circle of reformers to mainstream recommendations for raising productivity (Dertouzos, Lester, and Solow 1989). Management texts promoting the "mutual gains enterprise" and similar

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Industrial Relations, Vol. 43, No. 1 (January 2004). Published by Blackwell Publishing, Inc., 350 Main Street, Malden, MA 02148, USA, and 9600 Garsington Road, Oxford, OX4 2DQ, UK.

concepts are now commonplace (e.g., Kochan and Osterman 1994). The argument is further supported by the view that low-value-added jobs are moving abroad and that high-value-added operations, particularly those serving fast-changing markets and using information technology, require skills upgrading and downward delegation of decision making to front-line workers (Piore and Sabel 1984; Zuboff 1988; Appelbaum et al. 2000:10ff).

Many researchers concerned with workers' economic well-being believe that employee involvement can help reverse declining real wages and the growing inequality of the last 20 years. At a time when it has become harder for people with working-class jobs to earn middle-income wages, many have come to view earlier conceptions of work humanization in a new light. They present EI as a new model of more skilled, higher-wage jobs for less skilled workers that is an alternative to the low-wage, low-skill, dead-end jobs strategy that many employers find attractive, which can replace or buttress the declining union employment relations model (Bluestone and Bluestone 1992; Appelbaum and Batt 1994; Appelbaum et al. 2000).

By the early 1990s, a broad consensus emerged that both workers and firms would benefit economically and in terms of job satisfaction and organizational climate if workers were better trained; given greater opportunity to exercise their skills through job redesign, decreased supervision, and involvement in decision making; and motivated to contribute through various forms of productivity bonuses and gain sharing. Employer surveys indicating widespread adoption of employee involvement practices suggest that managers agree that the new programs are worth trying (Osterman 2000). (It is possible, though, that management enthusiasm has declined in the weak labor market of 2000–2004.)

Other studies support a more cautious view of the magnitude and significance of these changes, including middle and lower management resistance to change (Zuboff 1988), token or faddish adoption (Abrahamson and Fairchild 1999), and poor execution (Vallas 2003). The harshest critics describe workplaces in which management uses employee involvement to control workers and intensify work (Graham 1993; Barker 1993) as part of a more general labor cost control strategy, which also may include real wage reductions, union avoidance, outsourcing, offshore production, and less stable employment arrangements.

Despite differences, most researchers agree that the growing acceptance of employee involvement by managers in industrialized nations represents a potentially significant transformation of management philosophy and practice. Yet systematic research on the effects of employee involvement on both employees and employers remained thin until the late 1990s, and research conclusions continue to be in a state of flux.

Many of the early studies focused on the implications of new work practices for organizational outcomes such as productivity and profitability, where the evidence remains mixed. Most new plans have little effect, whereas serious efforts to increase front-line workers' incentives, skills, and decision-making authority typically increase organizational performance (Ichniowski et al. 1996). Until quite recently, systematic research on the implications for employees has been less common, and the evidence remains at least as mixed.

A focused effort to address implications for workers seemed appropriate given the current state of research. The studies in this special issue use some of the strongest national and firm-level data to investigate the key questions in this area, such as whether new work practices improve workers' wages, employment security, working conditions, workplace voice, and job satisfaction, as advocates hope and practitioners believe. This introduction discusses the theories that can provide a link between new work practices and workers' outcomes, particularly wages, and then reviews the challenges involved in this research. We summarize the research on wages, discuss the other articles in this issue that deal with a broader range of outcomes, and conclude with suggestions for how future research can build on the lessons learned.

Theories Relating Workplace Practices and Employee Outcomes[1]

There are at least five theories that may explain why workplaces with employee involvement, profit sharing, and other new workplace practices may have different outcomes for employees than more traditional workplaces: human capital, compensating differences, efficiency wages, incentives and complementarity, and theories centering on conflict over distributive issues within the firm, sometimes known as "management by stress." Given the large literature on how EI affects job satisfaction and the importance of economic outcomes for current debates, we focus on how work practices affect wage levels but also discuss implications for other outcomes, such as employee satisfaction and turnover. We briefly note several additional theories that relate employee involvement to these additional outcomes.

Human Capital Theory. Human capital theory argues that workers with higher skill levels receive higher compensation. Employee involvement may require workers with more general skills to perform more complex tasks, which may result in more rigorous selection and hiring criteria and increase

[1] This section draws on Helper, Levine, and Bendoly (2002).

the demand for and wages of more educated workers. New practices also may require more firm-specific skills, which would increase employer-provided training and wages as well. At the same time, holding all else constant, starting wages should be bid down as workers compete for the high post-training wages (Becker 1975). This complication reveals how tests correlating workplace practices with average wages can miss important effects. At the same time, if labor markets are imperfect, then starting wages need not be bid down, and training leads to a turnover efficiency wage, as described below.

Compensating Differences Theory. This theory argues that workers who face undesirable working conditions will receive higher wages. If employee involvement requires extra effort, then plants with employee involvement also should offer better compensation and working conditions in the form of higher wages, more bonuses, or increased safety. For example, when employee involvement means that workers have to do more tasks in the same amount of time, such as checking quality in addition to making parts, they often believe that they should receive additional compensation.

Conversely, if employees regard employee involvement as a benefit because problem-solving tasks and job redesign relieve the tedium of traditionally organized work, then firms that have it could offer lower wages. Theories of job design (Hackman and Oldham 1980) stress that most employees desire autonomy on the job.

In the case of bonuses or profit sharing, base wages may be lower, but total average compensation slightly higher to compensate employees for higher risk. At the same time, it may take a few years for the average compensation level to adjust. For example, a new incentive plan initially may be added to market-level compensation. If the incentive plan has positive payouts for several years, base pay may fall behind the market as the firm finds that it can hire sufficient labor without pay raises (due to the attractiveness of the incentive plan).

Compensating differences also may have little effect on the relationship between EI and wages (see Brown 1980). When a national survey asked workers if they would trade off wages, a 10 percent pay raise, for improvements in nonwage job characteristics, they showed much less interest in job enrichment compared with near-money benefits (Table 1), a finding supported by a contemporaneous two-plant survey (Giles and Holley 1978). Compensating differences theory assumes that important nonpecuniary elements of jobs translate into monetary equivalents, but people may keep pecuniary and nonpecuniary rewards in separate mental accounts (Thaler 1999, Zelizer 1994).

TABLE 1

EMPLOYEES WILLINGNESS TO TRADE A 10 PERCENT PAY RAISE FOR OTHER JOB CHARACTERISTICS, QUALITY OF EMPLOYMENT SURVEY (1977)

	Percentage Favoring Alternative				
Alternative	All	Mgr./Prof.	Cler./Sales	Craft	B.C./Serv.
Intrinsic					
1. More freedom to decide how to do work	18.1	18.7	14.1	14.5	21.9
2. More interesting work	26.4	21.9	28.3	21.9	30.6
Extrinsic					
3. Better medical insurance	47.0	38.7	37.4	54.3	56.3
4. Better retirement benefits	54.2	53.4	42.8	60.9	59.2
N	1311	367	281	228	435

SOURCE: Handel 2000.

Efficiency Wage Theories. Efficiency wage theories predict that paying higher wages may increase workers' productivity through three main channels. [Katz (1987) and Levine (1993) review this literature.] A higher wage may increase worker effort due to the greater cost of job loss, so workers would want to reduce the chances of being dismissed for low effort. A higher wage also may increase effort by increasing workers' loyalty to the firm, which may be especially important in systems requiring greater discretionary effort from employees and in which effort and output in group activities such as problem solving are costly to monitor (Akerlof 1984; Milgrom and Roberts 1995). Indeed, the core concept of the mutual-gains enterprise or high-commitment systems (Walton 1985) is consistent with Akerlof's (1982) theory of labor contracts as partial gift exchange and the role of fairness conceptions in determination of expectations, effort, and wages. Finally, a higher wage may reduce firms' turnover and recruitment costs, which also may be important if EI requires increased firm-specific training.

Incentives and Complementarity. The prescriptive literature on organizational design emphasizes the importance of aligning decision-making rights with incentives to make good decisions. This premise also appears in the prescriptive compensation and employee involvement literatures (e.g., Lawler, Mohrman, and Ledford 1995), expectancy theory in psychology, exchange and work design models in sociology and organizational behavior (e.g., Pfeffer 1994), and the rational models of economics, agency theory, and transaction cost economics (e.g., Wruck and Jensen 1994).

If undertaken seriously, the use of greater employee involvement involves substantial changes in decision-making rights when front-line employees collect and analyze more data and suggest and implement improvements. In

these circumstances, it makes sense to structure incentives in ways that reward quality and improvement and align front-line workers' goals with their new authority (Milgrom and Roberts 1995; Levine 1995). Because workplaces with greater employee involvement depend more on employee initiative, the theory of complementarities between involvement and incentives implies pay practices such as gain-sharing, profit-sharing, and stock-ownership plans will be more common in workplaces with higher levels of employee involvement. If these forms of variable compensation substitute for base pay, shift earnings risk to workers, or are introduced in the context of concession bargaining (Bell and Neumark 1993), then one would observe lower regular wages in their presence, although perhaps less employment variability in some cases as well. However, if the firm's strategy is to introduce a supplement or at least avoid putting current pay levels at risk, then total earnings may be no different or slightly higher. If the practices work as intended and increase motivation and productivity, earnings may be significantly greater, assuming that firms share gains with workers.

However, arguments from within both social psychology and agency theory suggest that the incentive effects of certain variable-pay and stock-ownership schemes may be compromised by the fact that any individual's ability to affect the firm's overall performance is usually small, which means that the link between effort and reward is highly indirect, disturbed by exogenous forces, and vulnerable to free riding. Nevertheless, some plans may be effective if they can institute an effective culture of peer monitoring or a sense of ownership among workers and identification with business goals (Blasi, Conte, and Kruse 1996).

The incentives and complementarity literature also suggests that EI increases employment stability to reassure employees that their suggestions for increasing productivity will not result in layoffs, which would result in withdrawal of participation (Levine and Parkin 1996). This complementarity yields the hypothesis that employee involvement will be found in conjunction with policies to increase employment security.

Employee involvement also may be to some extent its own incentive (Cotton 1993). That is, jobs with more autonomy may reduce the disutility of effort, in part because employees find it less onerous to perform a job they have helped design. According to these theories, EI generally will predict higher employee satisfaction but not necessarily high wages.

Management by Stress. This school of thought believes that EI is simply a method of sweating the workforce and curbing worker power and influence. Firms reduce employee and union power by using peer pressure in small work groups to enforce discipline and by appropriating workers' tacit

knowledge (Graham 1993; Parker and Slaughter 1988; Sheahan et al. 1996). This view predicts increased workloads, faster work pace, closer monitoring, and more job stress without offsetting compensating differences such as higher wages.

EI programs often codify workers' tacit knowledge in "standardized work sheets," often in conjunction with ISO 9000 certification, which can increase productivity by diffusing best practices across employees and encouraging systematic improvement (Adler 1993). However, these "scab sheets," as some union activists refer to them, also make it easier to increase workloads, replace trained workers with relatively unskilled ones, move work to lower-wage locations, and replace workers who strike. In these cases, EI in one plant could reduce wages at this and other plants even as effort and work pace intensify.

Case studies provide examples of firms that devolve responsibilities to workers but refuse to increase wages (Bailey and Bernhardt 1997:30ff; Zuboff 1988:298ff). Press reports indicate that some firms ask for wage cuts, more skills, and increased participation simultaneously, although others suggest that participatory workplaces are willing to pay higher wages.[2] Many researchers argue that workers require union representation to give them the leverage to compel firms to share gains resulting from EI programs given the unequal bargaining power of firms and workers in the current environment.

Total quality management (TQM) and standardized work pose additional threats to worker safety and health. TQM emphasizes a reduction in variation and often is combined with just-in-time (JIT) inventory practices that eliminate buffer stocks and worker control over work pace in order to maximize total work time. The result can be more standardized and repetitive work and increased workloads that raise the risk of repetitive-motion injuries such as carpal tunnel syndrome, confirming the suspicions of many that TQM represents a more developed form of Taylorism (Adler, Goldoftas, and Levine 1997).

Managers can use employee involvement to reduce union power by acting on problems more quickly through EI channels, bypassing the union and

[2] For business press accounts of wage concessions in firms that adopted high-performance practices, see "The New World of Work," *BusinessWeek*, Special Report, October 17, 1994, p. 86; "The Factory Worker," *BusinessWeek*, September 30, 1996, p. 66; "The New Deal: What Companies and Employees Owe One Another," *Fortune*, June 13, 1994, p. 50. For a contrary view, see "Breaking the Chains of Command," *BusinessWeek*, Special Issue, "The Information Revolution," 1994, p. 113. Drago's (1996) study of employee involvement in Australia suggests that employers adopting EI are bifurcated into two groups, one that embraces a commitment-oriented philosophy similar to Walton (1985) and another that treats workers as disposable and motivates EI through fear of job loss.

thus decreasing workers' perceptions of union effectiveness [see Parker and Slaughter (1988) for examples and Freeman and Rogers (1999:114ff) for another view]. Management can create new positions for line workers, such as team leader or quality coordinator, that offer highly motivated individuals opportunities for mobility and service to fellow workers without the inconvenience of union elections. Using workers as *de facto* subforemen also can serve a divide-and-rule function similar to that previously ascribed to internal labor markets (Barker 1993; Edwards 1979).

In nonunion workplaces, management-sponsored problem-solving channels can reduce employees' dissatisfaction. To the extent that lower dissatisfaction reduces the need for compensating differences and/or reduces the threat of unionization, wages also can be restrained. These effects are hard for researchers to observe because of the difficulty involved in gaining access to plants with undesirable working conditions, but Freeman and Rogers (1999:83, 113ff) present suggestive evidence that EI reduces desires for unionization in nonunion workplaces.

EI also may strengthen worker bargaining power by increasing workers' feelings of solidarity through increased interaction and firm-specific knowledge, which makes them less easily replaced. JIT inventory also makes it easier for employees to disrupt the production process so that worker noncooperation or other reactions to perceived unfairness are more costly to the firm.

Wage Inequality. EI has attracted significant interest from industrial relations researchers as a possible source of higher-wage jobs for less skilled workers at a time when wage growth has remained weak for more than two decades and unions and government protections for workers have declined. However, changes in the level of workers' wages can have different consequences for wage inequality. Most advocates of EI believe that it is equalizing because it raises wages of less skilled workers (Appelbaum and Batt 1994; Appelbaum et al. 2000). By contrast, a more recent view, which accepts the connection between information technology and organizational innovation, argues that because EI increases skill demands in a manner similar to computers, it is also skill-biased and disequalizing (Caroli and van Reenen 2001; Bresnahan, Brynjolfsson, and Hitt 2002). [For reviews of the research on technology, skills, and inequality, see Handel (2003a, 2003b).]

In fact, EI's effects on inequality are potentially much more complex than either view allows and depend on how EI changes the shape of the earnings distribution and the measure of inequality used. Assuming that EI raises the wages of some workers earning below the mean but not so much that their wages rise above the mean, there are at least four possibilities:

1. *EI has no effect on anyone else's wages.* This might occur if EI simply involves some firms making better use of worker abilities that are fairly plentiful, perhaps by using workers' untapped knowledge and giving them more basic training, which is one theme in the literature. This probably will reduce overall inequality, as measured by the variance of log wages, for example, by reducing inequality between the upper and lower halves of the distribution even though it will likely increase inequality (variance) within the lower half.

2. *EI reduces wages of other workers below the mean but does not affect those above the mean.* If EI requires more scarce abilities among less skilled workers, as suggested by descriptions of more intensive training and stricter preemployment screening among blue-collar workers, for example, then employer demand for some workers who were previously eligible for those jobs will fall, as will their wages, even as wages rise for others previously paid similar wages. Because some workers are now actually worse off, inequality within the lower half of the wage distribution will increase more than in the first case, and the change in overall inequality will depend on whether or not the disequalizing impact is more than fully offset by the reduced inequality between the upper and lower halves of the distribution [see Freeman and Medoff (1984) for a similar discussion of the union wage effect].

3. *EI affects the lower half of the wage distribution in the ways described by either case 1 or 2 above but also decreases the demand for managers and supervisors.* This would occur in the case of significant delayering and managerial downsizing and compresses the upper portion of the wage distribution, contributing an equalizing effect to the outcomes in the previous cases.

4. *EI affects the lower half of the wage distribution in the ways described by either case 1or 2 above but also increases the demand for skilled managers and supervisors.* This would occur if the latter concentrate now on more strategic issues and have more complex interpersonal roles. The increased dispersion in the upper portion of the wage distribution will contribute a disequalizing effect to the outcomes in the previous cases.

In almost all cases the question of whether equalizing or disequalizing effects dominate is open. The impacts also will vary by inequality measure. For example, ratios of wages between 10th, 50th, and 90th percentiles may

not change to the same extent as variances if most of the workers affected move between intermediate percentiles. In addition, the full effects of EI on inequality may not be visible within establishments if EI results in the reallocation of workers across establishments such that within-establishment homogeneity rises.

Finally, if EI does not affect wages, then it holds no implications for inequality in either direction.

Summary. Human capital theory, efficiency wage theory, and the form of compensating differentials theory emphasizing the burdens of EI practices for workers suggest that EI raises wages. The form of compensating differentials theory emphasizing the benefits of EI for workers with respect to job satisfaction and the very different theory of management by stress, which emphasizes lower worker bargaining power, suggest that EI lowers wages. The incentives and complementarity literature mostly emphasizes changes in the composition of compensation, implying increased use of variable pay but no strong conclusions as to the level of total compensation.

Theories of firm-specific training and efficiency wages, but not compensating differences or management by stress, also imply that high-involvement workplaces should have lower levels of voluntary turnover, i.e., quits. Firm-specific training and the incentives and complementary literature also imply lower levels of involuntary separations, whereas management-by-stress theories imply higher layoff rates.

Efficiency wage theories, particularly the gift-exchange variety, imply greater job satisfaction, whereas management-by-stress theories clearly imply harsher working conditions and lower satisfaction. The theory of compensating differences can be used to predict either greater or less job satisfaction. By contrast, human capital theory and agency theory variants of the incentives and complementarity literature treat the employment relationship as so narrowly economic and rationalistic that affective considerations, such as job satisfaction, are largely excluded.

Methodological Challenges[3]

While theory and intuition suggest numerous hypotheses regarding EI's effects on workers, there are several difficulties to testing them conclusively that can be grouped under the categories selection bias, response bias, measurement problems, statistical power, and theoretical uncertainties

[3] This section draws on Ichniowski et al. (1996).

surrounding the identification of clusters or "bundles" of EI practices believed to have synergistic effects.

Selection Bias. Because work practices are not assigned at random, there is the possibility of spurious relations between EI and worker outcomes resulting from selection effects operating on either the firm or worker side.

On the firm side, rent-sharing theories raise two possibilities for a noncausal correlation between wages and EI. The correlation may be positive, but not causal, if rich firms share rents with workers through both EI and high wages. The correlation may be negative, but not causal, if desperate firms try new practices and cut wages. In fact, these possibilities reflect continuing uncertainty among researchers as to whether any omitted variable bias is likely to be positive or negative when modeling firm or worker outcomes as a function of EI practices. Plausible logical arguments and industry- and firm-specific cases can be cited for either position. For example, an analysis of a British sample of establishments finds that both rising and falling demand conditions predict organizational change, whereas analysis of a sample of French establishments finds that neither is significant (Caroli and van Reenen 2001:1475).

On the worker side, pre-existing or unrelated worker quality differences may drive an association between EI and wages. More effective management may lead firms to use more rigorous selection procedures and hire workers with greater human capital, as well as adopt EI practices, inducing a correlation between wages and involvement. However, if the EI program is a relatively token effort, or if the additional skills requirements of a serious EI program are relatively modest and easily acquired rather than requiring much greater human capital themselves, these workers would receive the high wages even if they worked for a non-EI employer. In this case one could not conclude that EI increases the demand for more skilled labor. The significant number of case studies describing nominal or stalled EI programs and the relative scarcity of systematic empirical studies of EI's skills requirements make this kind of potential coefficient bias a concern.

A random-assignment experiment can estimate the effect of innovative work practices on organizational performance with a single cross section of data. In nonexperimental studies, cross-sectional data make it very difficult to rule out the possibility that omitted variables affect the result. Controlling for selection effects requires longitudinal data to control for the prior wage level of both the establishment and the worker. For example, Renaud, St-Onge, and Magnan (this issue) take advantage of data from before workers join a stock-ownership plan to test whether future plan members were already good performers before joining the plan. We are pleased to

note that three of the studies in this issue (Renaud, St-Onge, and Magnan; Black, Lynch, and Krivelyova; Cappelli and Neumark) analyze multiple periods of data.

However, longitudinal data are not without problems. As discussed below, measurement error is likely to be a significant issue in research on EI. Measurement error biases regression coefficients toward zero, and errors can be magnified when using longitudinal data to examine changes in work practices because it is usually more precise to measure whether something exists than to measure it twice, each time with error, and then identify how much it has changed. The use of administrative data in the article by Renaud, St-Onge, and Magnan should minimize the problem, but such data are relatively rare.

In addition, longitudinal analyses raise questions regarding the lag that is expected between treatment and outcome. Changes in employees' skills, motivation, and organizational commitment and their effects on the production system and wages are unlikely to occur immediately, especially if firms introduce workplace changes gradually. However, the effects of EI may wash out as a result of myriad intervening circumstances, or the effects themselves may fade if the lag is too long.

These complications do not mean that longitudinal strategies should be avoided, only that better measures of innovative work practices need to be collected, and more consideration needs to be given to the time patterning of their introduction and their likely effects.

Response Bias. If the sample of organizations is nonrandom and survey respondents and nonrespondents differ in important ways, studies also may suffer from response bias. Researchers usually rely on data from establishments or firms that voluntarily agree to be observed or complete a phone or mail survey. The need for cooperation means that firms that enjoy above-average success with their workplace innovations may be more likely to participate than those which are less successful, who may prefer to remain silent. Consequently, the study may overstate program gains.

Measurement Problems. In an experiment, the researcher has a well-defined treatment and knows who receives it. Nonexperimental researchers evaluating innovative work practices lack the luxury of experimental application of the intervention, leading to a number of different sources of measurement error.

Many of the constructs central to innovative work practices are based on subjective judgments. For example, a "semiautonomous work team" may be a totally autonomous group without outside direction, or it may be a traditional

work group with a supervisor who held a single team meeting 6 months ago. Some data show significantly higher incidence for self-directed teams than for job rotation (Osterman 2000:186), even though most assume that teams are a more advanced practice and usually subsume job rotation, suggesting that respondents may have interpreted the meaning of teams too broadly. Similar problems likely affect measurement of other imperfectly defined constructs, such as TQM (Hackman and Wageman 1995). The existence of practices may be overreported when they exist only on paper or involve only small numbers of workers or are mistakenly identified with more prosaic or less participatory practices (Bailey and Bernhardt 1997), biasing estimates of their effects on worker outcomes downward.

While careful construction of the survey and multiple measures of the same construct can go a long way in alleviating this problem, it is likely to remain a serious source of error. For example, Eaton (1994) found that managers and union leaders often disagree about whether or not a specific program actually existed in the same establishment—other studies such as the U.K. and Australian Workplace Industrial Relations Surveys also find low interrater reliability.

Even in the absence of reporting error, some establishment surveys measure the presence of practices in an establishment but not the number or characteristics of workers involved.

Many studies measure innovative work practices using only one respondent per firm or establishment, in which case idiosyncratic opinion or interpretation of the questions can distort the results (Wright et al. 2001). The problem can be exacerbated when the respondent is a top-level manager, who may have limited knowledge of what is actually happening at the workplace level. Responses from single corporate-level executives of large companies may be particularly noisy and potentially biased indicators of actual workplace practices. In contrast, Renaud, St-Onge, and Magnan (this issue) analyze administrative data, which reduces mismeasurement due to recall error and biases.

Measuring outcomes can be as difficult as measuring work practices. As the breadth of outcomes analyzed in the articles in this special issue indicate, there are many potential outcomes for workers. Safety and wages, for example, may move in different directions. A study that examines just one outcome could be quite misleading.

If predictor measurement error is random relative to the true value, then it biases the estimated effect of innovative work practices on outcomes toward zero. Some strategies for dealing with this simple type of measurement error have not been used in studies employing EI practices as predictors. For example, correcting the bias from purely random measurement

error is possible if the data set has as few as two respondents from each establishment.

Unfortunately, measurement errors may be systematically related to the true level of performance or of innovative work practices. Some respondents may exaggerate their own success with innovative work practices, whereas others may fail to report unsuccessful efforts at implementing innovative work practices. Using more than one respondent per organization may reduce such errors, as would having the researcher conduct site visits and interview multiple respondents at different levels and in different roles within the organizations (as in MacDuffie 1995).

Statistical Power. Measurement error can be reduced with intensive site visits and industry-specific survey items. Direct observation generally works better than surveys to determine whether a "team" really makes its own decisions or takes on the responsibilities that researchers usually associate with the term. However, case studies do not provide a basis for generalizations, and in most survey studies the use of direct observation results in smaller sample size [see Appelbaum et al. (2000) for an exception]. This makes it more difficult to estimate precisely the effects of work practices on outcomes and increases the chances of incorrectly concluding that a given practice has no effect. Moreover, if some practices are more effective when used in combination, it is difficult to detect these interaction effects in small samples. Finally, it is unclear how well results from studies of single industries or organizations generalize to other settings.

Identifying Bundles. Most of the different theories reviewed earlier imply that the different dimensions of employee involvement—increased task complexity, responsibility, autonomy, training, and gain sharing—are interdependent and mutually reinforcing. This suggests that combinations or bundles of practices should be more effective than the simple sum of effects for the individual practices. For example, it may be far more effective both to train front-line employees in problem solving and to permit them to solve more problems than to make either change alone. This means that correctly specifying the independent variable requires a reasoned transformation of the measures of EI practices.

However, there are many EI practices, and theory does not provide clear guidance as to which bundles may be most effective, whether some practices are substitutes (e.g., employee stock ownership or profit sharing) or complements, or even how to measure bundles (e.g., interaction effects, additive indices, factor analysis, cluster analysis, or rational combinations). Some measurement strategies implicitly assume that different EI practices are

substitutes even though theory suggests that complementarities can be important. In short, it is difficult to identify which workplaces have introduced a theoretically sound bundle of practices or experience the true treatment and which do not.

Some studies suggest that the confusion extends beyond researchers to employers themselves. Studies find that practices do not cluster in meaningful bundles within establishments (Osterman 1994; Gittleman, Horrigan, and Joyce 1998) and that many more establishments adopt a few EI practices rather than a coherent bundle (Forth and Millward this issue; Blasi forthcoming), as theory and some, but not all, research suggests is most effective (Ichniowski et al. 1996; Cappelli and Neumark 2001).

No matter how researchers measure bundles, the method of identifying workplaces as more or less innovative is always subject to some error. The most convincing results use multiple methods and test whether different procedures yield similar groupings of work practices and predict similar performance results (Ichniowski, Shaw, and Prennushi 1997). In our symposium, for example, Forth and Millward use multiple measures of bundles: (1) the total number of practices, (2) whether an establishment has at least two practices along a given dimension (such as two forms of EI or two forms of information sharing and training), and (3) whether an organization has at least one work practice from each of multiple facets (e.g., both an EI practice and a training and information-sharing practice). While no single measure is conclusive, the robustness of results to multiple sensible (but imperfect) measures is reassuring.

In short, selection and response bias, measurement error, low statistical power, and difficulties surrounding conception of the independent variable make it difficult to measure the true effect of work practices such as EI on worker outcomes. These problems can lead researchers to find no relationship when there truly is one or can lead them to believe that there is a relationship when none exists. Unfortunately, we cannot even be sure of the sign of the bias—that is, whether estimated results will be unrealistically positive or negative.

Research on the Relationship Between EI and Wages

Despite methodological challenges, the spread of EI practices and the expectations that they affect worker outcomes have prompted a growing empirical literature. Of particular interest is whether EI can be a source of higher-paid jobs for the majority of workers whose real wages have stagnated or declined in the last 25 years while inequality has grown, particularly in

light of the waning influence of unions, increased competitive pressure on firms, and increased relative power of employers. Because of the recent research focus on wage effects and its importance for these questions, a review of the research on the relationship between EI and wages seems useful. A number of articles in this issue also focus on this relationship (Black, Lynch, and Krivelyova; Handel and Gittleman; Forth and Millward; and Renaud, St-Onge, and Magnan), reflecting the continuing high level of interest in this subject.

The other articles in this issue investigate the relationship between EI and other important worker outcomes, such as employment stability (Cappelli and Neumark), working conditions (Brenner, Fairris, and Ruser), worker voice (Bryson), and job satisfaction (Batt). These issues are either the focus of a much smaller body of research or, in the case of job satisfaction, a much larger body of literature that has been summarized extensively.

This review is divided into two sections based on whether the studies use nationally representative samples or focus on particular industries, firms, and establishments. This corresponds to some of the methodological divisions over broad as opposed to context-sensitive measures of EI practices and the issue of the generalizablility of industry- or firm-specific samples. All the studies discussed below are also summarized in Table 2.

Nationally Representative Studies. Cappelli (1996) presented one of the first studies to systematically investigate whether high-performance work practices are associated with higher wages for production workers and less inequality between workers and managers within establishments. The study used the 1994 National Employers Survey (NES), a nationally representative sample of private, for-profit business establishments with at least 20 employees. The dependent variable was the log annual average earnings, and models control for a wide range of establishment characteristics. The EI variables were percentage of nonmanagerial workers in self-managed teams, binary indicator of the presence of a TQM program, span of control, and number of managerial levels.

Cappelli found that each additional percentage of nonmanagerial workers in self-managed teams was associated with 0.1 percent higher annual earnings, implying that workers in an establishment with all workers in teams receive average earnings that are 10 percent more than those in otherwise similar establishments with no workers in teams. TQM was associated with 5 percent higher earnings, and each additional managerial level was associated with a percentage point decline in production workers' earnings, consistent with the expectation that decreased hierarchy is associated with higher wages for workers, whereas span of control was not associated with

TABLE 2

SUMMARY OF RESEARCH ON THE EFFECTS OF HIGH-PERFORMANCE WORK PRACTICES ON WAGES

Study	Year	Sample (n)	Earnings	Results
		Nationally Representative Studies		
Cappelli (1996)	1994	Private, for-profit establishments with at least 20 employees (n = ~3000)	Log annual average pay	Pct. in teams: $\beta = 0.001$* TQM: $\beta = 0.050$* Span of control: $\beta = -0.005$ No. of mgt. levels: $\beta = -0.010$*
Cappelli & Neumark (2001)	1977–1997	Manufacturing subsample from 1994 survey (see above) and 1997 wave (n = 433)	Log labor costs per worker	Job rotation: $\beta = 0.017$ Cross-training: $\beta = -0.003$ Pay for skill: $\beta = -0.045$ Meetings: $\beta = 0.004$ Pct. in teams: $\beta = -0.033$ Team training: $\beta = 0.046$* TQM: $\beta = 0.059$* Profit sharing: $\beta = 0.021$
Black, Lynch, Krivelyova (this issue)	1993–1996	Manufacturing establishments, panel (n = 233)	Log average wage	*Production workers:* Job rotation: $\beta = -0.020$ Teams: $\beta = -0.020$ Union × teams: $\beta = 0.080$ Meetings: $\beta = -0.002$ Union × meetings: $\beta = -0.000$ Profit sharing: $\beta = -0.004$ Union × profit sharing: $\beta = -0.004$
Osterman (2000)	1992–1997	Establishments with at least 50 employees (n = ~300–400)	Log annual earnings	*Number of high-performance work practices:* Core workers: $\beta = 0.821$ All workers: $\beta = -1.435$

TABLE 2 (cont.)

Study	Year	Sample (n)	Earnings	Results		
Handel & Gittleman (this issue)	1995	Private establishments with at least 50 employees and random sample of employees within them (n = ~1070 for both samples)	Log average establishment wage, log individual hourly wage		*Establishment*	*Employee*
				Job rotation:	β = −0.038	−0.058*
				Job redesign:	β = −0.022	−0.032
				Quality circles:	β = −0.043	0.083*
				Self-directed teams:	β = −0.030	0.045
				Peer performance review:	β = 0.169*	0.050
				EI in equipment purchase:	β = 0.037	0.023
				Pay for skill:	β = 0.019	0.005
				Profit sharing:	β = 0.088*	0.025
				TQM:	β = 0.002	0.050
				Just-in-time:	β = −0.055	−0.001
Forth & Millward (this issue)	1998	Private establishments with more than 10 employees and a sample of their employees (n = 1140 establishments, 16,020 employees)	Log hourly wage	Quality circles: β = 0.010 Cross-training: β = 0.006 Teams: β = 0.025 Briefing groups: β = 0.005 Information disclosure: β = 0.028* Human relations training: β = −0.008 Internal recruitment: β = 0.009 Job security guarantee: β = 0.071* Stock/profit sharing/bonus: β = 0.013		
Freeman & Rogers (1999)	1994	National sample of workers in nongovernmental organizations with at least 25 employees, excluding upper management (n = 2408)	Item asks workers in EI programs if they received a pay increase as a result	31 percent participate in an EI program; within this group, 36 percent reported receiving a pay increase as a result		
Caroli & van Reenen (2001)	1984–1990 (Br.), 1992–1996 (Fr.)	National sample of establishments (n = 378, Britain; 515, France)	Occupational wage bill shares	*Effect of EI initiated between 1980–1984 (Britain)* Unskilled: β = −0.0316* *Effect of managerial delayering (France)* Unskilled: β = −0.016* Skilled manual: β = 0.016*		

Bauer & Bender (2001)	1993–1997	Matched West German establishment-employee sample (n = 1175 establishments)	Mean log establishment daily wage	Delayering: β = 0.011* Decentralization: β = −0.001 Teams: β = 0.005			
Industry, Firm, and Establishment Studies							
Appelbaum, Bailey, Berg, Kalleberg (2000)	1995–1998	Production workers, steel (n = ~1700 workers), apparel (n = ~1000 workers), medical electronics (n = ~900 workers) in 40 establishments	Log weekly earnings, including contingent pay		*Steel*	*Apparel*	*Medical*
				Self-directed teams:	β = 0.004	0.100*	0.031
				Off-line teams:	β = 0.025*	0.000	−0.003
				Autonomy:	β = 0.031*	0.051*	0.008
				Communication:	β = 0.068*	0.034*	0.020
				Overall participation:	β = 0.001*	0.002*	0.001
Hamilton, Nickerson, Owan (2003)	1995–1997	Garment workers, weekly data from single establishment (n = 20,193 person weeks)	Log hourly and weekly pay	*Team-based production:* Hourly wages: β = 0.158* Weekly pay: β = 0.260*			
Helper, Levine, Bendoly (2002)	1993	Automotive supplier establishments (n = 318)	Log average wage for nonskilled workers	*EI index (standardized):* Union workers: β = 0.036* Nonunion workers: β = 0.052*			
Arthur (1992)	1988–1989	Steel minimills (n = 30 establishments)	Average total labor cost per worker (standardized)	Cost reducers: μ = −0.85* Collective bargainers: μ = 0.90* Commitment: μ = 0.02			
Kelley (1996)	1991	Machining establishments (n = 973)	Log average worker's wage	*Correlation of employment system and wages:* Participative plants: r = −0.02 Union: r = 0.20* Apprenticeship: r = 0.12*			
Colvin, Batt & Katz (2001) (also Batt 2001a)	1998	Call centers (n = 242)	Log median earnings, managers and workers	*Teams:* Off-line, workers: β = 0.05 Self-directed, workers: β = 0.09 Off-line, mgrs: β = −0.10* Self-directed, mgrs: β = −0.11*			

TABLE 2 (cont.)

Study	Year	Sample (n)	Earnings	Results
Batt (2001b)	1994	Regional telephone company	Log annual earnings, technicians	Self-directed team: μ = \$46,683 Traditional group: μ = \$46,507 Not sig. different, but overtime may give team members 4 percent premium
Hunter & Lafkas (2003)	1994–1995	"Typical" bank customer service representatives (n = 303)	Log annual earnings	Discretion: $\beta = -0.062$ Discretion × routine IT: $\beta = 0.289$* Quality circles: $\beta = 0.056$* QC × routine IT: $\beta = 0.100$
Tullar (1998)	n.a.	Job descriptions, food and beverage distributor (n = 99 job titles)	Hay points	Reengineering increased study-rated Hay points 26 percent, but employer gave no pay increase
Bailey & Bernhardt (1997)	1994–1995	Case studies, six retail firms (fast food, shoe store, Home Depot, department stores), observation and interviews with managers, HR, and workers	Interview responses	Variable changes in skills, task complexity, participation, and autonomy but little reported change in wages, benefits, promotion opportunities, and skills
Azfar & Danninger (2001)	1988–1994	Young, white, male nonunion workers (n = 1978)	One-year difference in log hourly wages	Employer profit-sharing program: $\beta = 0.030$*
Rénaud, St-Onge, Magnan (this issue)	1996–1998	Employees, Canadian bank (n = ~2000 persons, 4940 person-year observations)	Log annual earnings	*Stock plan participation:* Year_{t-1}: $\beta = 0.044$* Year_{t}: $\beta = 0.059$* Year_{t+1}: $\beta = 0.079$* $\text{Year}_{t\geq 2}$: $\beta = 0.084$*

NOTE: Sample sizes are approximate because they often vary across the models summarized in the table. Coefficients were selected among alternatives within each study from the best-specified model according to generally accepted criteria. Results in the table were usually not sensitive to the choice of models.

*$p < 0.10$ Many effects are significant at higher levels of precision.

wages, contrary to expectation. Cappelli (1996:147) also found that TQM, but not the other measures, was associated with manager reports that skill requirements of production jobs increased in the preceding 3 years. None of the EI variables significantly affected inequality within establishments, as measured by the ratio of production and supervisory earnings (Cappelli 1996:150).

A key problem with cross-sectional models is the possibility of omitted variable bias, which Cappelli and Neumark (2001) addressed using panel data. They restricted their analyses to manufacturing establishments in the 1994 NES and a second wave administered in 1997, along with linked establishment data from the Longitudinal Research Database (LRD) for 1977, which was derived from the Census of Manufacturers. Models assumed that the workplace practices were absent in 1977, permitting the estimation of first-difference models using two panels for 1977–1993 and 1977–1997 constructed by matching establishments in each of the NES datasets to the LRD 1977 data file. The dependent variable was total labor cost per worker, which includes the cost of benefits and payroll taxes, as well as wages and salaries. The EI variables were: percentage of nonmanagerial workers involved in job rotation, regular meetings to discuss work-related problems, pay-for-skill plans, and self-managed teams and binary indicators for the presence of a TQM program, cross-training, teamwork training, and a profit-sharing, bonus, or gain-sharing plan for nonmanagerial workers. Since some of these variables were only available for the 1993 wave and the results for the others are generally little different in models using the 1977–1996 panel, Table 2 reports only the 1977–1993 results.

When EI variables were added singly to a first-difference model including control variables for the 1977–1993 panel, only the presence of teamwork training and TQM programs had positive effects on log labor costs, both in the 4 to 6 percent range. The absence of a significant effect for teams themselves may indicate that the team-training item does a better job identifying establishments with "true" teams and that team measures by themselves suffer from significant measurement error. In cross-sectional models for 1993, profit sharing also was significantly associated with total labor costs ($\beta = 0.058$). Job rotation, meetings, and teams were also available for both years of a short panel (1993–1997) constructed from both NES waves, but none was significantly associated with labor costs in either panel or pooled cross-section models (Cappelli and Neumark 2001:772).

Capelli and Neumark also estimated six models in which different combinations of EI variables were entered jointly and with interactions among them to test the common belief that the practices are mutually supporting and must be used in bundles to be fully effective. They defined the bundles

on the basis of prior theory and intuition to avoid data mining. Very few results showed a consistent positive effect on wages. After one of the most thorough and thoughtful tests of the bundling thesis, they concluded that "there is not consistent evidence of statistically significant effects of bundles of work practices" (Cappelli and Neumark 2001:760).

Black, Lynch, and Krivelyova (this issue) also use the NES panel of manufacturing establishments to investigate whether EI practices affect wage levels and inequality, as well as employment stability. The dependent variables are log average wages for all workers and for different occupations. The EI variables are whether at least 20 percent of nonmanagerial workers participate in job rotation and belong to self-managed teams, whether at least 50 percent of nonmanagerial workers meet regularly in groups to discuss workplace issues, and the availability of profit sharing for any group.

Using the 1993–1997 NES panel, Black, Lynch, and Krivelyova find that nonmanagerial participation in EI practices has no effect on production workers' wages within manufacturing. These panel results also hold for the NES 1997 cross section, which has a larger sample size ($n = 590$) and does not suffer from potential exacerbation of measurement error in the EI variables that can result from differencing (Black, Lynch, and Krivelyova, Table 4, this issue). However, the panel models also indicate that nonmanagerial participation in different EI practices is associated with higher wages for managers, supervisors, and technical workers, particularly in unionized establishments, which the authors interpret as indicating that the practices exert greater demands on these personnel. Likewise, profit sharing in unionized establishments is associated with greater wage inequality between nonproduction and production workers. Black, Lynch, and Krivelyova also find that the presence of some EI practices in 1993 is associated with increased risk of employment reductions between 1993 and 1996 but that other practices are associated with decreased risk and even effects of the same EI practice can vary by union status.

Osterman (2000) analyzed a nationally representative panel survey of establishments with 50 or more employees, which was administered in 1992 and 1997. The dependent variable was the log median annual earnings. The EI variable was constructed from questions on the use of job rotation, quality circles, self-managed teams, and TQM among core employees, defined as the largest group of nonsupervisory workers involved in production of the good or service. The main EI variable was a count of the number of practices involving at least 50 percent of the core workers within an establishment in 1992.

Osterman found no association between the use of high-performance work practices in 1992 and earnings growth for either core or all employees

within an establishment in 1997. Cross-sectional results for 1997 also showed no association between new work practices and higher earnings. The longitudinal results remained unchanged when the model included a variable for previous changes in employment levels (1990–1992) to control for possible confounding effects of persistent weakness in establishment performance that might cause both adoption of EI practices and slower wage growth. When the practices were entered jointly without the index, the use of teams actually had a negative effect, and the others had no effect on wages (Osterman 2000:190ff).

Nor was the absence of wage gains offset or explained by increased job security. The use of high-performance work practices in 1992 was associated with a greater probability of an establishment laying off at least 5 percent of its regular (noncontingent) workforce between 1995 and 1997 in all models. Interestingly, use of a high-performance work system was not associated with overall employment reductions within establishments, suggesting churning or reorganization rather than simple downsizing. This also was opposite to expectations for a high-commitment, training-intensive human resources system. Osterman also found lower use of contingent labor in these workplaces, which was more expected.

Osterman (2000:190ff) concluded, "The bottom line is that there is very little evidence that HPWOs [high-performance work organizations] have delivered on their promise of 'mutual gains.'" While teams may require mutual sharing of gains and stable membership, these considerations may be overridden by the firm's competitiveness strategy and restructuring demands, even though the absence of rewards for discretionary effort and greater job insecurity are likely to result in a withdrawal of commitment by employees, not least due to the fear that productivity-enhancing suggestions may endanger their jobs. Osterman (2000:180) pointed out that his results, unlike more optimistic accounts, were more consistent with the general wage stagnation and increased employment insecurity experienced during the period covered by his surveys despite the great expansion in the use of high-performance work practices, which reinforces the face validity of his results.

Handel and Gittleman (this issue) use the Survey of Employer-Provided Training for 1995, conducted by the U.S. Bureau of Labor Statistics (BLS). The data are a nationally representative sample of private establishments with 50 or more employees and include surveys of employees as well as management respondents within establishments, permitting the estimation of both establishment- and employee-level models. The dependent variables are log average establishment wage and log individual hourly wage. The EI variables are 10 binary indicators for the presence within an establishment

of job rotation, job redesign/reengineering, quality circles, self-directed work teams, peer performance review, employee involvement in technology/equipment purchase decisions, pay for skill, profit sharing, TQM, and a JIT inventory system.

Handel and Gittleman find few consistent positive impacts of EI on wages. This impression remains unchanged when they restrict the employee sample to production and service workers and restrict the establishment sample to those in which production and service workers account for two-thirds of total employment. Various attempts to test for the effects of bundles of practices, including simple counts, measures derived from principal components analysis, and various interaction specifications, also do not alter results substantively. Interacting EI practices with a union indicator also does not show a consistent wage premium relative to unionized establishments and employees without EI practices.

In explaining these findings, Handel and Gittleman note that very few studies consider whether the changes in skill and responsibility associated with EI are large enough to affect wages significantly and whether any productivity gains may accrue to profits in an age of reduced worker power. In this case predictions from efficiency wage theory may not hold. Employers may be able to induce more effort and cooperative behaviors from workers without having to offer above-market wages in exchange.

Forth and Millward (this issue) use the British Workplace Employee Relations Survey 1998, a nationally representative survey of British establishments with 10 or more employees that also included linked employee-level survey data. The analyses include only private establishments, and the dependent variable is the employee's log hourly wage. Models control for an extensive array of individual and establishment-level variables, presumably ameliorating any selection problems. Forth and Millward classify their EI variables into three categories, *task practices* (teams, cross-training or functional flexibility, and quality circles), *individual supports* (briefing groups, business information disclosure, and human relations training for workers), and *organizational supports* (internal promotion, job security guarantees, and worker eligibility for stock ownership, profit sharing, or performance-based pay).

Forth and Millward find that only regular business information disclosure to workers and employment security guarantees are associated with higher wages when the EI variables are entered jointly. The lack of association between teams and wages does not change when they use narrower definitions of teams that require teams to decide their own work methods and select their own leader. Forth and Millward also test for the impact of bundles by defining traditional workplaces as those which are low on all

three of their EI dimensions, mixed systems as those which score high on one or two dimensions, and high-involvement workplaces as those which score high on all three dimensions. Only workers in high-involvement establishments receive a significant wage premium over workers in traditional establishments ($\beta = 0.080$), and this is largely driven by the similar premium for job security, which is included in the scale. However, employees in traditional or mixed-approach workplaces that also offer job security do not earn more than their counterparts in similar establishments that do not have a job security guarantee, suggesting the kind of synergy predicted by theory. Forth and Millward also find that the presence of a powerful union increases the returns to working in a high-involvement workplace.

Freeman and Rogers (1999) administered a nationally representative survey to workers who were not upper management and worked for nongovernmental organizations employing at least 25 workers, covering about 75 percent of the total workforce. They found that 52 percent of respondents worked for employers that used EI and that 31 percent participated in EI practices themselves (Freeman and Rogers 1999:92). Among participants, 36 percent said that they received a pay increase as a result of their involvement, but the survey did not elicit further detail on the nature of the EI program, magnitude of the pay increase, or whether responses included bonus or gain-sharing pay (Freeman and Roger 1999:107,168).

Caroli and van Reenen (2001) combined several datasets for Britain and France to investigate the effects of organizational change on changes in different occupations' shares of establishments' total wage bill. For Britain, they found that unskilled workers' share of the total wage bill declined more than 3 percent between 1984 and 1990 among establishments reporting in 1984 that they made "changes in the last 4 years with the aim of increasing employees' involvement in the operation of the establishment" relative to otherwise similar establishments. There were no significant effects for semiskilled and skilled manual workers, clerical workers, supervisors, and managers. Most of the change in wage bill shares reflected changing employment shares rather than changing wages (Caroli and van Reenen 2001:1470ff).

In France, managerial delayering between 1989 and 1992 was associated with a 1.6 percent decline in the share of the wage bill going to unskilled manual workers between 1992 and 1996 and equivalent growth in the share going to skilled manual workers relative to otherwise similar establishments. Curiously, managerial delayering did not affect the wage bill shares of middle managers, senior managers, or clerical workers, even though the primary (negative) impact would be expected among these groups (Caroli and van Reenen 2001:1470ff). The French data also contained information on

quality circles, TQM, and JIT production, but the study did not report any estimated effects on wage shares.

Bauer and Bender (2001) used matched employer-employee panel data from West Germany for 1993–1997. The analyses included establishments with at least 20 employees. The dependent variable was mean log establishment daily wage, and the models controlled for establishment performance (recent sales trends), as well as an array of other establishment- and individual-level variables. The EI variables were binary indicators for whether the establishment reduced the number of hierarchical layers, delegated responsibilities downward, and introduced self-directed teams between 1993 and 1995. The data did not contain information on the number of employees covered by these policies and practices, only whether the establishment made any of these changes.

Bauer and Bender modeled not only mean establishment wages but also wages at the 20th, 50th, and 80th percentiles and the size of the differences between wages at those ranks. The first-difference model for 1993–1997 indicated that delayering raised wages by about 1 percent for those at the mean, median, and 80th percentile but was not associated with higher wages for those at the 20th percentile. Downward delegation of responsibilities had no effect on wages for any group, and self-managed teams increased wages by less than 1 percent for employees at the 80th percentile only (Bauer and Bender 2001:Table 4). These results generally were robust to alternative specifications, including cross-sectional models. However, the use of teams was also associated with a significant increase in the 80–20 and 50–20 wage differentials between 1993 and 1997 (Bender and Bauer 2001:Table 6). The authors did not propose an explanation as to why the team variable had such a seemingly large effect on wage differentials when it had such a minor impact on the components of those differentials.

Industry, Firm, and Establishment Studies. Nationally representative studies offer the prospect of greater generalizability, but studies of specific industries or employers may be able to reduce measurement error by using more context-sensitive measures of EI and have less variation in unobservable variables that may bias coefficient estimates.

Appelbaum et al. (2000) conducted one of the most thorough industry-level studies of workplace innovation in three industries: steel, apparel, and medical electronics. They visited over 40 manufacturing establishments across industries, interviewed managers, and administered surveys to workers. The sample included production workers and models estimated at the employee level. The dependent variable was weekly earnings, including compensation from bonuses and similar contingent-pay schemes. Controls

included company fixed effects as well as more standard background variables, meaning that results were based on differences in wages between workers who did and did not participate in EI practices within the same employer. The EI variables were binary indicators for membership in self-directed and off-line teams (such as quality circles that meet away from production areas), standardized scales for decision-making autonomy and communication outside the primary work group, and an overall participation scale that was a standardized additive sum of the others (Appelbaum et al. 2000:116ff).

Appelbaum et al. found that self-directed teams were associated with weekly earnings that were about 10 percent higher than otherwise in apparel but were not associated with higher pay in the other industries. Off-line teams were associated with 2.5 percent more pay for steel workers. Those with above-average autonomy in steel enjoyed a 3 percent earnings advantage over those with below-average autonomy, whereas those in apparel earned 5 percent more. The corresponding figures for communication were 6.8 percent for steel and 3.4 percent for apparel. The standardized scale of participation combining these variables also was significant for steel and apparel, and a standard deviation difference in the scale implied a wage advantage of 2.5 percent for steel workers and 4 percent for apparel workers (Appelbaum et al. 2000:172).

In contrast, high-autonomy production workers in medical electronics did not receive higher pay. Appelbaum et al. (2000:223ff) hypothesized that the absence of any effects for this group was due to the educational upgrading in high-involvement workplaces in this industry alone, which absorbs the effects of the other variables.

Appelbaum et al. (2000:222) tested for selection effects using a Heckman procedure and found no evidence of selectivity bias net of model regressors for the autonomy, communication, and overall participation models. They found some evidence that selection into self-directed teams was positively associated with unobserved characteristics that also raise earnings, whereas selection into off-line teams seemed to be associated with characteristics that lower wages.

Because the coefficient of variation did not vary systematically across groups defined by whether they participated in different EI practices, Appelbaum et al. (2000:223) concluded that high-performance work practices did not increase earnings inequality, although they cautioned that the wage premium in steel and apparel may be inconsistent with this conclusion.

Hamilton, Nickerson, and Owan (2003) examined the effect of team or module production on wages and productivity in a single garment factory between 1995 and 1997. This was the same kind of contrast that Appelbaum

et al. (2000) studied in their apparel industry sample. Hamilton, Nickerson, and Owan analyzed weekly data on worker productivity, pay, hours worked, and team membership. Because the factory introduced teams during the period covered by their data, they could follow workers' performance and wages as they moved from traditional to team production and estimate fixed-effects models. In contrast to almost all other studies, all data came from company records rather than from self-reports or subjective evaluations of productivity differences.

Using fixed-effects models, Hamilton, Nickerson, and Owan found that an individual's productivity increased 14 percent after joining a team compared with 18 percent when not controlling for worker fixed effects, suggesting relatively modest selection effects. These gains appeared to have been shared with workers, whose hourly and weekly pay increased 16 and 26 percent, respectively (Hamilton, Nickerson, and Owan 2003:482, 487).

It is unclear how much of the change reflected increased autonomy as opposed to a more effective incentive system or peer pressure. However, in light of the more objective nature of the measures and the ability to control for selection effects, this is one of the strongest pieces of evidence that new production systems sometimes can benefit workers economically.

Higher-paid workers were some of the first to join the team system when participation was voluntary, even though they may have been expected to fare more poorly when grouped with lower-performing workers. Consequently, the authors inferred that teams also provided nonpecuniary benefits in the form of greater autonomy and task variety. They also noted that the presence of a union may have ensured the sharing of productivity gains (Hamilton, Nickerson, and Owan 2003:494ff).

Helper, Levine, and Bendoly (2002) combined survey data and field research to investigate the effects of EI practices on outcomes for unskilled and semiskilled workers in auto supply factories. The dependent variable was log average establishment worker wage. The main EI variable was a standardized index combining plant manager responses to numerous items on job task content, workers' policy influence, teams, joint labor-management committees, new-style pay practices, training, and employment security policies, among others (Helper, Levine, and Bendoly 2002:345ff).

The authors found that a plant scoring an additional standard deviation in this index paid a wage premium of 3 to 5 percent depending on union status. They investigated possible causal mechanisms linking involvement and wages by employing different specifications and adding further controls. For example, contrary to human capital theory, training did not explain the relationship between wages and a workplace participation subindex. This relationship appeared to be most consistent with efficiency wage

theories because the presence of EI predicted greater employee loyalty, as reported by managers, and the high wages also predicted lower turnover. The results were least consistent with compensating differences because plants with bonuses or no-layoff policies did not pay lower base wages.

The relationship between EI and wages probably was not the result of selection effects, such as a greater likelihood that plants facing difficulties chose to adopt EI policies, because managers in high-involvement plants were actually less likely to report that their plants had layoffs or downsizing in the preceding 4 years than managers at low-involvement plants. Helper, Levine, and Bendoly (2002:367ff) also found no evidence that EI affected plants' survival or employment growth between 1993 and 1999.

Arthur (1992) presented information on mean production worker wages in a sample of steel minimills using different industrial relations strategies. He classified minimills into six categories on the basis of a cluster analysis of survey items measuring decentralization of decision making, participation in teams, general training, percentage of skilled workers, span of control, use of formal grievance procedures, number of company-sponsored social events, wages, benefits, and bonuses. Wages were average total labor costs per production and maintenance worker (i.e., wages, benefits, bonus/incentive pay, and taxes) (Arthur 1992:495). The values in Table 2 are cluster means for the standardized wage variable (mean = 0, SD = 1).

While establishments classified as "cost reducers" ($n = 8$) paid wages 0.85 standard deviation below the sample mean, collective bargainers ($n = 9$) paid 0.90 standard deviation above the mean, and the few commitment maximizers ($n = 3$) paid almost exactly the mean wage (Arthur 1992:498). Although the sample was quite small [i.e., there were only 54 minimills in the entire United States at the time (Arthur 1992:492)], the results are rather striking.

Kelley (1996) surveyed manufacturing establishments in 21 industries that produced machined metal goods of various sorts. She classified establishments as participative, union/seniority-based system, and traditional craft apprenticeship system depending on their responses to a number of survey items. Participative plants tended to have joint problem-solving committees, autonomous teams, technical training, and employee stock ownership (Kelley 1996:383). However, when each establishment was coded with binary indicators for management system, the simple correlation of machining workers' wages with the presence of a participative system was near zero, whereas there were significant, positive associations between wages and the other two systems (Kelley 1996:384). Since Kelley's research question related to firm performance, she did not conduct more elaborate tests of these relationships.

Colvin, Batt, and Katz (2001) examined the effect of worker teams on the level of both managers' and workers' pay in a cross-sectional survey using a representative sample of call centers in telecommunication industries, including telephone, cable television, and internet service providers. Managers were call center employees holding positions above first-line supervisors, and the workers were sales and service representatives. The dependent variables were the log median annual earnings of managers and workers and the ratio of manager-worker earnings within establishments. Controls included recent establishment performance in the form of revenue trends, as well as other establishment-level variables. The EI variables were the percentage of core workers participating in regular off-line problem-solving groups and the percentage participating in self-directed teams. Colvin, Batt, and Katz hypothesized that the transfer of responsibilities from managers to workers would reduce managerial pay and interoccupational wage differentials.

The percentage of workers participating in both kinds of teams had no significant effect on workers' wages, but both teams were associated with 1 percent lower managerial pay for every additional 10 percent of workers in teams. Both kinds of teams were associated with lower within-establishment inequality between managers and workers as well (Colvin, Batt, and Katz 2001:924ff). Batt (2001a:440ff) found that the results for workers are robust to alternative specifications.

In a separate survey of technicians in a regional Bell telephone operating company, Batt (2001b) compared participants in self-managed teams with those in traditional work groups. Since the technicians' work was already both skilled and free from close supervision, they differed mainly in the degree to which they performed administrative and coordination tasks that belonged to supervisors in traditional groups, such as task assignments, work scheduling, quality and inspection, and dealing with customers, suppliers, and other internal departments. Batt sampled all self-managed teams in a nine-state region, randomly selected a traditional group under the same district manager, and then matched the survey data with company performance records for individuals. The EI variables were membership in a self-managed team, multiskilling, and participation in an off-line problem-solving or quality-improvement group, to which both members of self-managed teams and traditional work groups belonged in roughly equal numbers (~20 percent) (Batt 2001b:12).

Batt found that members of self-managed teams performed more supervisory kinds of coordination functions, spent less time in direct labor, and worked more overtime hours, but she found no similar effects for either multiskilling or off-line teams. Because none of the EI variables was associated with increased labor productivity, Batt concluded that the teams'

main value for the company was the substantial savings in labor costs that result from the elimination of supervisors and the greater efficiency of the teams in performing their indirect labor (Batt 2001b:14ff). The implications for workers' wages were a little murkier. The annual earnings of team members were a statistically insignificant 0.4 percent greater than those working in traditional groups (Batt 2001b:12). However, team members worked more overtime, leading to an estimated annual earnings premium of 4 percent (Batt 2001b:19). Since technicians valued overtime pay and the work was voluntary, Batt concluded, "Technicians in teams gained a modest pay premium as a share in the gains of a productivity-enhancing work innovation" (2001b:21).

Hunter and Lafkas (2003) examined the effects of employee discretion and quality circles on the wages of customer service representatives in banking. The dependent variable was the annual earnings of a typical customer service representative in local bank branches. The EI variables were a binary indicator for use of quality circles and an index of employee discretion to make eight decisions on the job, since prior field work indicated little use of other forms of employee involvement.

Quality circles were associated with a 5.6 percent earnings premium, whereas customer service representative discretion was associated with higher wages when interacted with a measure of computer use for routine procedures. In models excluding education, the analogous interaction effect for quality circles also was significant ($\beta = 0.139$), suggesting that part of the synergy between information technology and employee involvement works through increased human capital requirements.

Tullar (1998) investigated the consequences of business process reengineering, which involves job enrichment, decentralization of decision making, use of teams, greater customer orientation, and a reorganization of departments to encompass all phases of production or service rather than traditional functional departmentalization. Four students were trained to use the Hay system, and each rated 99 job descriptions from two branches of a food and beverage distribution company in the southeast United States. The branches were very similar except that one had experienced reengineering. Raters assigned Hay points using written job descriptions with the job titles omitted for approximately 50 jobs from each branch, 25 before and 25 after the reengineering was completed at the experimental branch. The study used average ratings as the dependent variable.

Average Hay points for the two branches did not differ prior to reengineering, and the average for the control did not increase over time, but the average Hay points for the reengineered plant rose 26 percent. However, the company did not offer higher pay for those in the reengineered plant whose

job demands increased. Tullar argued that this kind of behavior may be responsible for the high failure rates of reengineering initiatives because it likely generates perceptions of inequity even if the jobs are more intrinsically motivating. "This study shows that at least by the Hay criteria, reengineered jobs ought to pay more" (Tullar 1998:979).

Hunter, MacDuffie, and Doucet (2002) find that production workers' pay at six Chrysler plants increased after they adopted a team organization in the early 1990s. Although statistical data was unavailable, the changes that the union and management negotiated with the shift to skill-based pay implied average wage gains around 2 percent to 4 percent (Lovell et al. 1992). The correlation between workers' reports of the number of specified tasks their team performed and their simple report regarding whether they received a pay increase as a result of the change to teams was small but significant ($r = 0.07$, $p < 0.05$). However, there is no correlation between the number of team tasks and an individual's education, most of the new team tasks did not demand complex cognitive skills, and the new system used the existing workforce (Larry W. Hunter, personal communication).

Bailey and Bernhardt's (1997:191ff) case studies of six diverse retail firms found that EI initiatives were associated with few changes in wages, benefits, promotion opportunities, and skills, although in many cases the EI initiatives were rather token. They concluded that a management style with low employee involvement in decision making could be quite efficient in some contexts. The competitive character of product and labor markets in retail also meant that the pressure on employers was to convert productivity gains into lower prices rather than higher wages. They also raised the possibility that many of the jobs (e.g., cashier) might be intrinsically low skill, repetitive, and not amenable to great change.

The recent interest in innovative work practices includes heightened attention to variable-pay systems. The literature on human resources bundles generally has viewed variable pay as a supporting work practice. However, others, influenced by agency theory, consider variable pay useful as a stand-alone incentive device even without significant changes in job tasks or decision rights. Firms may be motivated to implement stock plans in particular by tax considerations or the firm's need for capital (Kruse 1996; Carberry 2000). Use of different forms of variable compensation seems to have grown in the last 20 years, but previous findings with respect to their effects on base wages or total labor costs have been mixed (Wadhwani and Wall 1990; Bell and Neumark 1993; Blasi, Conte, and Kruse 1996).

Azfar and Danninger (2001) investigated the effect of profit-sharing programs on the wage growth of a sample of young white males in nonunion establishments using the National Longitudinal Survey of Youth.

The dependent variable was the change in log hourly wages over 1 year for workers employed between 1988 and 1994. Models also controlled for lagged hourly wage, among other variables. The authors found that the presence of a profit-sharing program was associated with a 3 percent faster growth rate in hourly wages over a 1-year period.

Renaud, St-Onge, and Magnan (this issue) analyze the effect on regular earnings of participation in a company stock-purchase plan in a Canadian bank. The sample includes both lower- and higher-level white-collar employees between 1996 and 1998, and the data come from the company's personnel files, permitting estimation of employee fixed-effects models.

While participants in the stock-purchase plan earned more in the year prior to their joining relative to 2 years prior, their salaries continued to rise in the years following entry into the company stock-purchase plan. The authors rule out pure selection explanations by noting that the level of and change in compensation in prior years do not predict decisions to join the stock plan. They conclude that company stock ownership exerts incentive and motivational effects that improve individual performance and that plan membership favorably impresses superiors through various mechanisms in ways that also positively influence compensation decisions.

Summary. Since most studies estimated far more models than are included in Table 2 and the practices themselves are heterogeneous, there is no single best way to summarize the results of the 21 studies just described. However, Table 2 does include what can be reasonably considered the best estimates from those studies for as wide a range of practices as possible. Table 3 presents a simple tabulation of results classifying each coefficient in Table 2 according to effect size. Where the EI variable was a standardized scale, the effect size equals the coefficient (i.e., effect of a single standard deviation), and where the EI variable was in percentage form, the effect size was based on multiplying the coefficient by 100.

Caroli and van Reenen's (2001) study is excluded from Table 3 because they did not estimate a wage premium per se, finding that EI reduced the share of total wages going to the less skilled, representing mostly employment losses. Freeman and Rogers (1999) and Hunter, MacDuffie, and Doucet (2002) are also excluded from Table 3 even though they found positive wage effects because the magnitude and statistical significance of the effects are unknown.

The majority of both nationally representative and more focused studies indicate no significant effect of EI on wages, although the results from industry- or firm-specific studies tend to be more favorable (Table 3, panel A). This result may be because the more restricted focus controlled

TABLE 3

MAGNITUDE OF EI'S EFFECTS ON WAGES

		Positive and Insignificant Effects		Positive and Significant Effects			
	Negative Effects	>0–5 Percent	≥6 Percent	>0–5 Percent	6–9 Percent	≥10 Percent	Total
A. Study type							
National studies							
Percent	39.6	35.8	3.8	9.4	7.5	3.8	100
N	21	19	2	5	4	2	53
Industry, firm, or establishment studies							
Percent	14.7	32.3	5.9	26.5	8.8	11.8	100
N	5	11	2	9	3	4	34
All							
Percent	29.9	34.5	4.6	16.1	8.0	6.9	100
N	26	30	4	14	7	6	87
B. EI Measures				***Percent of Studies***			
Job rotation ($n = 4$)	75.0	25.0					100
Quality circles ($n = 4$)	25.0	25.0			50.0		100
TQM ($n = 4$)		50.0		25.0	25.0		100
Teams ($n = 22$)	27.2	36.4	9.1	9.1		18.2	100
Alternative pay ($n = 11$)	27.3	45.4		9.1	18.2		100
EI Scales ($n = 9$)	22.2	22.2	11.1	44.4			100

NOTE: Tabulations summarize results from Table 2. Caroli and van Reenen (2001) and Freeman and Rogers (1999) are excluded from table. Teams and alternative pay include all varieties of these practices; this pooling may account for some variation across results. Almost all negative effects were statistically insignificant. In Panel A, the Pearson $\chi^2(5) = 10.27$, $P = 0.068$, providing suggestive evidence that results from national and establishment studies differ. If the order of the columns is meaningful (which may not be true because the point estimates in insignificant results over 6 percent are all larger than the point estimates for significant results from 0 to 5 percent), it is appropriate to use an ordered test. Fisher's exact test shows the rows differ at the 5 percent level. In Panel B, the Pearson $\chi^2(25) = 33.74$, $P = 0.011$, which cannot reject that the patterns of results of the six work practice are drawn from identical distributions. Because we cannot put the rows in a natural order, no more precise statistic exists. The sample size of studies in Panel B is larger than in Panel A because many studies provide estimates for multiple programs.

more effectively for unobserved heterogeneity or because more context-appropriate survey items reduced measurement error. However, if these studies focused disproportionately on industries in which EI is intrinsically more effective, their results would not generalize to the overall economy.

However, even when the effects of new workplace practices on wages are statistically significant, they tend to be small and their causal status clouded by the possibility of selection effects. Most studies do not eliminate the possibility that high-wage establishments may adopt more high-involvement practices or high-involvement establishments may hire workers who would

receive equally high wages at low-involvement plants. Almost all the negative effects are insignificant, and the point estimates are small. A reasonable reading of the evidence suggests that EI's average effect is somewhere between 0 and 5 percent, although larger effects have been found in a small number of cases. These estimates are well below those for the union wage premium (Freeman and Medoff 1984), but problems with measurement error biasing coefficients toward zero are also greater in research on employee involvement.

In panel B of Table 3 we break down results separately for each new work practice. Our sample sizes of studies become quite small when we examine individual work practices. Results are most consistently positive when examining total quality management, with all four studies finding positive effects of TQM on wages (with two of the effects statistically significant). Conversely, three of the four studies examining job rotation found negative (but not statistically significant) results. None of the other programs (quality circles, teams, alternative pay systems, or scales of employee involvement and other new work practices) showed patterns that were distinctive compared with the overall average. Given the small number of studies of job rotation and of TQM, we cannot conclusively state that one set of programs is or is not consistently better or worse for employees (the test of similar distributions of outcomes across programs is distributed $\chi^2(25)$ and is not significant at the 10 percent level).

The Relationship Between EI and Other Worker Outcomes

While the effect of EI on wages has become a focus of research interest recently, other worker outcomes, such as employment stability, working conditions, and worker voice, have received less attention, and still others, such as job satisfaction, were the original focus of EI proponents and a long-standing research focus. The articles in this special issue address these questions using unique datasets.

Cappelli and Neumark (this issue) use the NES 1997 dataset to investigate whether EI practices are associated with layoffs, use of contingent labor, and quit rates. On the one hand, practices that lead to more training and flexible internal deployment of workers across jobs may alleviate the need for numerical flexibility in the form of layoffs and temporary workers. On the other hand, firms may use internal or functional flexibility and numerical flexibility as complementary strategies when they need to respond quickly to changing business circumstances in any manner possible. Numerical flexibility also may complement EI practices in two-tiered workplaces,

where they buffer valued and highly trained core workers from demand fluctuations. Finally, EI may inhibit voluntary turnover by building stronger worker commitment.

The dependent variables are the percentage of voluntary and involuntary separations in the previous year and whether the establishment used any contract or temporary workers. The EI variables are percentage of nonmanagerial workers involved in job rotation, meetings to discuss work-related problems, and self-managed teams and binary indicators for reengineering and the presence of a profit-sharing plan.

Cappelli and Neumark find more layoffs in establishments that use reengineering and fewer layoffs in manufacturing establishments with self-managed teams and profit sharing.

Use of contingent workers is more common in nonmanufacturing establishments that adopt reengineering and in manufacturing establishments that use self-managed teams. Contingent workers are used less often in manufacturing establishments that adopt reengineering.

Because the use of contingent workers is also positively associated with layoffs in both manufacturing and nonmanufacturing, Cappelli and Neumark conclude that contingent employment is not used to buffer core employees; both layoffs and contingent employment appear together as part of a more general strategy of numerical flexibility.

In manufacturing, job rotation, self-managed teams, and profit sharing are negatively related to quits, but meetings are positively related to quits. In nonmanufacturing, both meetings and job rotation are positively related to quits. These mixed findings, along with similar analyses of employment dynamics by Black, Lynch, and Krivelyova (this issue), Osterman (2000), and Drago (1996), suggest the difficulties in drawing uniform conclusions about whether EI practices restrain or contribute to employment instability, a topic of increased concern in recent years (Neumark 2000).

The earliest proponents of EI aimed to improve job satisfaction, and a large research literature since the late 1960s generally shows positive effects (see Cotton 1993). The positive effect of employee involvement on satisfaction is generally supported by more recent studies (Freeman and Rogers 1999; Appelbaum et al. 2000; Hodson 2001:190; Hunter, MacDuffie, Doucet 2002). Some surveys show no significant effects, but few show negative effects. At the same time, however, case studies show that EI can generate significant dissatisfaction when employers use it as a control device to increase work pace, as emphasized by the management-by-stress literature (Graham 1993).

Batt (this issue) examines the effects of teams on job satisfaction of workers in different occupations within a large unionized regional Bell telephone

company. She finds that self-managed teams, which significantly alter the internal division of labor between workers and managers, affected job satisfaction, but off-line teams, such as quality circles, which are more consultative, had no effect on job satisfaction. The twist in Batt's findings is that self-managed teams increased workers' job satisfaction but decreased their supervisors' job satisfaction. The greater job satisfaction among workers in teams is explained by their increased sense of discretion and job security, whereas the decreased sense of job security among the team supervisors accounted for their lower job satisfaction relative to supervisors of traditional work groups. Self-managed teams became identified with upper-management efforts to downsize supervisors and managers, and top management ultimately abandoned their plans to diffuse self-managed teams because of the resistance from lower management. While supervisor and middle-management resistance was a known pitfall in the early 1980s (Cotton 1993:55ff), Batt shows that current practice has not entirely succeeded in moving beyond the problem; organizational politics still matter.

Bryson (this issue) examines another dimension of the quality of work life: general management responsiveness to employees. Like Forth and Millward, Bryson uses linked employer-employee data from the British Workplace Employee Relations Survey of 1998. The dependent variable is a scale constructed from employee responses to items regarding the degree to which managers provide them with information on proposed changes, are open to employee comments and suggestions, deal with employee problems, and treat employees fairly. Models employ a rich set of controls, including independent measures of management quality. Bryson classifies the EI variables as direct voice (e.g., regular meetings between management and workers, briefing groups, and problem-solving groups), non-union representative voice (e.g., joint consultative committee), and union voice. All allow two-way communication between management and employees, giving employees the opportunity to voice their wishes and concerns.

Bryson finds that direct voice is associated with greater employee perceptions of managerial responsiveness than an absence of all voice mechanisms and either non-union representative voice or union representation; the combination of direct and non-union representative voice has the strongest effects. Union voice is not associated generally with perceptions of managerial responsiveness, whereas direct voice mechanisms are associated with perceptions of greater managerial responsiveness even among unionized workers. Because the negative union effects are strongest where the union representative is part time, Bryson concludes that in these circumstances union representation raises expectations but is unable to pursue goals effectively due to time constraints. Results are unchanged when the managerial

responsiveness scale is split into two variables, one measuring information sharing and consultation and the other measuring more grievance-related concerns. One might expect the new human resources practices such as direct voice to affect the first most strongly, whereas unions would have an advantage addressing the latter. However, in both cases direct voice tends to be positively associated with perceptions of managerial responsiveness, and part-time union representation tends to be negatively associated. From these results it appears that new human resources practices genuinely improve communication, organizational climate, and employees' quality of work life, at least in this respect.

Brenner, Fairris, and Ruser (this issue) investigate an important but underresearched question: the effect of EI on occupational health. EI practices, such as quality circles, job rotation, and self-directed work teams, are believed to enhance task variety, autonomy, and worker voice, all of which suggest that problems with repetitive-trauma disorders will be less common and addressed quickly when discovered.

However, aspects of the new production philosophy, such as TQM and JIT inventory, can result in decreased worker control over the pace and timing of work. These practices have been characterized in the management-by-stress literature as a form of neo-Taylorism that aims to shorten cycle times, eliminate buffer stocks, and increase regimentation, work loads, and total time worked per hour. Increased repetition, shorter recovery times, and speedups put workers at greater risk for repetitive-trauma disorders.

Brenner, Fairris, and Ruser match nationally representative establishment-level data in the BLS Survey of Occupational Injuries and Illnesses (1993) and the Survey of Employer Provided Training (1993). They model the rate of newly identified repeated-trauma cases as a function of various EI practices. Both quality circles and JIT inventory systems are associated with higher rates of repetitive-trauma disorders in manufacturing, and the interaction between the two is also significant. In contrast, work practices do not predict trauma rates in nonmanufacturing, which are also much lower than those in manufacturing.

Conclusion

The recent growth of research interest in employee involvement reflects both EI's growing use within the workplace and hopes that it may be a source of good jobs for workers. From the current evidence it appears that EI can improve organizational outcomes if the reforms are serious. The evidence on workers' welfare is quite mixed: Most involvement plans probably

have no effect, but the average effect appears to be a few percentage points increase. There is no evidence that EI programs decrease compensation and no consistent evidence that compensation rises by more than perhaps 5 percent. The evidence is too weak to know where in that range the average effect lies.

EI does not appear to generate a wage premium comparable to unionization (Freeman and Medoff 1984) and therefore seems limited as a substitute for that long-declining model of employment in the area of compensation. EI may be necessary to achieve high-wage job growth but is not sufficient. While EI advocates argue that a wage premium due to improved productivity is more sustainable than a premium originating in union bargaining power, unfortunately, and somewhat surprisingly, there seems to be no evidence that involvement consistently increases employment security.

In terms of the theories discussed earlier, each may contribute to the small overall effect—implying an even smaller effect per theory. In terms of human capital, some high-involvement firms appear to hire a somewhat more skilled workforce, and more increase training (recall that human capital theory predicts that training should reduce starting wages even as it increases post-training wages). Efficiency wage effects also may play a role, due to a need to elicit greater commitment and discretionary effort and minimize loss of training investments that would result from turnover. However, the small size of the EI premium also suggests that employers feel limited pressure to pay efficiency wages, perhaps reflecting workers' weak bargaining position. The evidence on satisfaction implies that wages may be held down by many workers' preference to work in high-involvement jobs, whereas the evidence on injuries suggests that any higher pay may be just a compensating differential. There is little consistent evidence of greater employment security associated with EI that might also function as a compensating differential.

Existing research does suggest that when EI is not used as a form of speedup, it gives workers more autonomy, recognizes the value of their contributions, improves job satisfaction and feelings of voice, and often lowers quit rates. By mitigating the more negative aspects of hierarchy, EI represents another positive step in the evolution of management practice comparable to the earlier restrictions on the "foreman's empire" (Nelson 1975) that transformed the supervisory role from at times that of a petty dictator to the more constrained "man in the middle" (Whyte and Gardner 1945).

While these findings do not support the most positive views of EI as a "high road" solution to the problems of poor wage growth and increased

inequality, they do not indicate that management by stress is typical, nor do they suggest that skill-biased organizational change is a significant cause of inequality growth.

All conclusions regarding the effects of EI must be qualified by the recognition that existing research has not eliminated potential problems such as measurement error and selection bias. Indeed, to some extent it is not surprising to find such disparate results given the lack of agreement on the best measures of EI. Future research requires standard and more behaviorally concrete measures of various EI practices to improve comparability across studies, minimize measurement error, and distinguish strong from weak adopters. More attention also must be given to understanding organizational performance and worker quality prior to adopting EI to exclude the possibility that wage effects are biased by any preexisting differences between the wages of adopters and nonadopters.

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