NBER WORKING PAPER SERIES

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Working Paper 7374
http://www.nber.org/papers/w7374

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
First Draft: February 1999
October 1999

The authors thank William H. Carter for careful research assistance, Arne Reznek and colleagues at the Center for Economic Statistics of the U.S. Bureau of the Census for their help with data issues, and participants in the MIT Labor Seminar for helpful comments. This research is part of a program of studies using the National Employer Surveys that is conducted by the National Center on Post-Secondary Improvement and funded by a grant from the U.S. Department of Education's Office of Educational Research and Improvement. The views expressed herein are those of the authors and not necessarily those of the National Bureau of Economic Research.

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NBER Working Paper No. 7374
October 1999
JEL No. M11, M12, J50

ABSTRACT

Interest in the potential effects of different systems for organizing work and managing employees on the performance of organizations has a long history in the social sciences. The interest in economics, arguably more recent, reflects a general concern about the sources of competitiveness in organizations. A number of methodological problems have confronted previous attempts to examine the relationship between work practices and the performance of firms. Among the most intractable has been a concern about establishing causation given heterogeneity biases in what have typically been cross-sectional data. The results from prior literature are suggestive of important productivity effects but remain inconclusive.

To address the major methodological problems we use a national probability sample of establishments, measures of work practices and performance that are comparable across organizations, and most importantly a unique longitudinal design incorporating data from a period prior to the advent of high performance work practices. Our results suggest that work practices that transfer power to employees, often described as "high performance" practices, may rise productivity, although the statistical case is weak. However, we also find that these work practices on average raise labor costs per employee. The net result is no apparent effect on efficiency, a measure that combines labor costs and labor productivity. While these results do not appear to be consistent with the view that such practices are good for employers, neither do they suggest that such practices harm employers. They are, however, consistent with the view that these practices raise average compensation and hence may be good for employees. Overall, then, the evidence suggests that firms can choose "high road" human resources practices that raise employee compensation without necessarily harming their competitiveness.

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I. Introduction

Arguments suggesting that alternative systems for managing employees and organizing their work might lead to superior employee performance and, in turn, superior organizational performance go back at least to the beginnings of industrialization. The original focus was on ownership structures, with the view that employee cooperatives and other forms of employee governance might not only be more just but might provide superior performance as well (see, e.g., Webb and Webb 1965 in the U.K., or Commons 1970 in the U.S.). Forceful critiques arose early in the 20th century in reaction to the mass production factory system that was heavily influenced by industrial engineers like Frederick Taylor. Entire academic fields like industrial psychology and industrial relations can trace their roots at least in part to these critiques. Although they became more sophisticated and differentiated over time, the alternative models all share the view that mass production systems sharply limit the control and decision-making autonomy of employees, ignoring important psychological and social needs of employees in the process. As a result, they not only fail to tap important sources of motivation and ideas from employees but in many cases cause employees to withhold effort and compliance with management directions.

Recent studies attempting to estimate the benefits associated with these "high performance" work practices are suggestive of important effects but, taken as a group, remain inconclusive. In this study, we use a national probability sample of establishments, measures of work practices and performance that are comparable across organizations, and most importantly a unique longitudinal design incorporating data from a period prior to the advent of high performance work practices. Together, these features of our study address the central methodological problems that have plagued past research. Our results suggest that work organization practices that expand employee involvement, often described as "high performance" practices, do not have an unambiguous effect on productivity (sales per employee), although the estimates tend to point to positive effects. There is stronger evidence that these work practices on average raise labor costs per employee, controlling for labor quality. Because the most

important aspect of labor costs is compensation, higher labor costs per employee would seem to benefit workers even though they are a liability, other things equal, for employers. The final result is that there appears to be no net effect on labor efficiency, a measure that combines the effects on labor cost and labor productivity.[1] While these results do not appear to be consistent with the view that such practices are good for employers, neither do they suggest that such practices harm employers. Overall, the evidence suggests that firms can choose "high road" human resources practices that raise employee compensation without necessarily affecting their competitiveness.

II. Related Research on Work Organization

The research on work organization and the attempts at reforming it is vast, and only a cursory review is attempted here, with an emphasis on more recent empirical research on the effects of work practices on organizational performance.[2]

Early Case Studies

Perhaps the earliest research attempting to evaluate the effects of alternative working systems were the Western Electric studies beginning in the 1920s, inductive research that both "discovered" many of the alternative principles for organizing work and evaluated the benefits from them (Roethlisberger and Dickerson 1943). Although there is some debate about what these studies actually show, most observers believe that the reforms put in place in those studies illustrated that individual and group performance could be enhanced by systems that acknowledge the need for social interaction and the role that group norms play in shaping work performance.

The next important set of studies, also case studies, were conducted some 20 years later in the U.K., originally in the mining industry (see, e.g., Trist 1981). They concluded that individual and group performance could be improved through work systems that recognized the interplay between new technology and work systems, hence the label "sociotechnical" for these studies. These studies were

[1] Note that this does not refer to technical efficiency in the economic sense of being on the production frontier, but rather efficiency in the sense of cost minimization by minimizing unit labor costs.

reacting not so much to mass production systems per se but to piece rates and the individualized incentive arrangements that were often part of such systems. The focus of the interventions was to expand the use of teams and the authority they had over workplace decisions. The interventions examined in these studies had the feel of experiments, although they did not necessarily meet all the characteristics of experimental design. And while there were no systematic evaluations of the outcomes of sociotechnical interventions, the case studies suggested substantial improvements in organizational performance. As with most studies of work organization, they examined the effects on individual work and workgroup productivity, not factoring in costs or examining relationships with overall firm performance. These studies had relatively little influence on U.S. industry but considerable influence in Scandinavia, especially Sweden.

Individual-Level Studies

The next generation of studies were more explicitly psychological in their orientation, focusing on the individual as the unit of analysis and psychological factors as explanations, in contrast to the prior studies where social relationships among workers or between workers and technology were emphasized. These new studies were motivated by the notion that the basic psychological needs that workers have were often unfulfilled by the nature of the tasks they performed. Analysis based on this framework was first made popular with the work of Herzberg (1966) and became a common research model by the late 1970s (e.g., Hackman and Oldham 1980). The interventions considered in this research were at the level of specific tasks that individual workers performed in their jobs. As with earlier research, the focus of attention was production work, and the main theories represented a counterpoint to the systems of work organization associated with scientific management. There were two broad types of studies in this literature. The first focused explicitly on employee involvement in decisions affecting their jobs. The second focused on the specific tasks that employees performed, with an important aspect typically being

[2] There are many good reviews of the work reform literature. Among recent ones are Cotton (1993) and Appelbaum and Batt (1994).

the extent to which they had control over how tasks were performed. But other aspects of jobs also played a role in these studies, such as the extent of variety in the tasks workers performed or the understanding of the end product that their tasks served.

The majority of these studies used employee attitudes as the dependent variable, especially job satisfaction. Cotton (1993) summarized the hundreds of studies examining work organization issues from the psychological perspective, most of which related innovations in work organization to job satisfaction or related concepts such as employee commitment. Few studies attempted to examine relationships with outcomes at the organizational level. Despite the incredible volume of research in this tradition, it is relatively easy to summarize its conclusions. Systems of work organization that empowered employees in decision making and jobs whose tasks enriched employees by meeting more of their needs were generally associated with more positive job-related attitudes such as satisfaction. Evidence that these innovations improved individual-level performance on measures such as absenteeism and turnover is somewhat more modest but still reasonably strong. As Cotton's review (1993) and others conclude, however, that there were few studies that examined relationships between these innovations and organizational performance as measured by productivity, efficiency, or financial performance at the organizational level, and little evidence of positive relationships in those few studies.[3]

Organization-Level Studies

The psychological research on work organization continues to pursue studies of work practices and individual outcomes. The more important innovation in the study of work systems during the 1990s has been research specifically designed to examine relationships with organizational performance. Some of the motivation for this research came from studies in the industrial organization economics tradition attempting to understand the components of profitability. Hansen and Wernerfelt (1989), for example,

[3] Some industrial psychologists have made considerable efforts to assess the costs and benefits of specific human resource practices (e.g., Cascio 1991). These assessments are primarily accounting calculations that do not attempt to estimate relationships with organization-level performance measures (e.g., they total the administrative costs of replacing employees to assess turnover costs but do not necessarily conduct empirical estimates of how turnover affects organizational performance).

found that internal characteristics of firms explained almost twice as much of the variance in profitability between firms as did characteristics of the industry and the firm's position in it, the traditional explanations for differences in profits. Rumelt (1991) found that only four percent of the variation in profitability across business units could be accounted for by factors associated with the unit's industry. Studies like these pointed to characteristics within the firm as the most likely source of differences in performance, and a great many of the potential differentiating within-firm characteristics relate at least indirectly to work practices or the way work is organized.

The primary motivation for research examining the effects of workplace practices on organizational performance, however, was the growing interest in Japanese management practices (see below), driven by concern about the inadequacies of U.S. manufacturing in general (e.g., Dertouzos, et al. 1988). The initial interest in Japanese management focused on employee management and concepts such as job rotation, teamwork, and practices that supported greater employee involvement (especially training and employment security). The first wave of the new research on work practices and organizational performance examined Japanese-influenced practices such as quality circles and found them associated with increased productivity (Katz , et al. 1983; Katz , et al. 1985). Perhaps the most influential research stemming from this interest was the International Motor Vehicle Project at MIT, which provided evidence of the superiority of Japanese assembly plant operations, in terms of quality and productivity, as compared with other producers (Womack, et al. 1990). Later, MacDuffie (1995) examined the same cross-sectional data more rigorously and found that work practices including teams and contingent compensation in combination with flexible production techniques led to higher quality and lower hours of labor per unit of output.

The approach of concentrating on a single industry and examining productivity has the decided advantage of ensuring that the performance measures are more comparable across observations. It also allows observations to be taken from units of analysis closer to where work practices occur, unlike

financial measures, such as shareholder value, which can only be obtained at the corporate level. Financial measures can more easily be swamped by unrelated factors; the stock price of a huge conglomerate like General Electric, for example, is likely to be driven by a wide range of factors in addition to work practices. In addition, it is very difficult to measure work practices accurately across an entire corporation. The downside of focusing on productivity is that it leaves open the question as to the costs of those productivity gains and whether, on balance, they increase firm efficiency or performance; this problem has been addressed in some, but not all, of the existing studies (see below). And the downside of focusing on a single industry, of course, is that it limits the ability to generalize the results to other settings.

A number of more recent studies followed the first wave of research on work practices and organizational performance, focusing in large part on productivity-related performance measures. Ichniowski, et al. (1997), for example, study steel finishing lines. They found that the introduction of "innovative work practices," among which are included teams and incentive pay, were associated with higher worker productivity, and that combinations of these practices yielded additional productivity improvements. A follow-up study by Ichniowski and Shaw (1998), using data for Japanese plants as well, reinforces the assertion in the prior study that there are synergies from introducing more of these practices. Delery and Doty (1996) attempt to examine the same relationships in the banking industry, albeit with cross-sectional data and a response rate of only 11 percent, and conclude that banks with more formalized employment systems (including formal training and appraisal systems and internal career opportunities) have higher returns on assets. Delery, et al. (1997) essentially repeat the study with a sample of trucking companies, again with cross-sectional data but this time with a 36 percent response rate. In this case, few of the much larger set of practices showed relationships either to cost ratios or returns on earnings. Cappelli and Rogovsky (1998) find that employee performance in a sample of public utilities is higher where employees are involved in decisions affecting work and individual tasks

but is no higher where they are involved in decisions governing the terms and conditions of employment. Batt (1999) compares the sales of call center employees within a telecommunications company operating under traditional, mass production work systems to those using total quality management (TQM) systems and self-directed teams. She finds higher performance associated with self-directed teams but no performance effects associated with TQM systems.

Another set of relatively recent studies has attempted to examine the relationships between work practices and overall *firm-level* performance using measures of financial outcomes. Ichniowski (1990) was the first of these, attempting the conceptually difficult task of relating sets of employment practices to firm-level financial performance. The low response rate (under seven percent) and small sample made it difficult to draw robust conclusions from his cross-sectional data, but the results seemed to suggest that a cluster of practices that included enriched job design was associated with higher financial performance as compared to other clusters of practices. The fact that somewhat similar sets of practices were associated with significantly worse performance could imply that there was a benefit to having just the right combination of practices. However, this sensitivity also highlights the usual caveat about the arbitrary nature of the clusters chosen to examine–given the large number of permutations possible among the practices, the a priori argument for choosing any particular cluster must be compelling.

Cooke (1994) examines the effects of employee participation and group incentive compensation plans (profit sharing and gain sharing) on value added per employee for a sample of manufacturing firms in Michigan. He reports that employee participation plans have significant positive effects that are bigger in union than in nonunion firms; positive effects of group-based incentives that are bigger in the nonunion sector when implemented in the absence of employee participation; and larger effects of jointly implementing both sets of practices in the union sector. However, statistical tests of the differences between union and nonunion firms were not reported, and only the difference with respect to implementing employee participation appears large enough to be statistically significant. Cooke also

finds that these work practices tend to increase wages, but by less than the effect on value added, so that performance–as measured by the effect on value added minus the effect on wages–appears to be enhanced by the same practices that boost value added. (Unfortunately, though, the statistical significance of these effects is not reported.)

Huselid (1995) follows Ichniowski's (1990) approach with cross-sectional data from a sample of publicly held companies with more than 100 employees. The data included 13 measures of employment practices (some of which were taken from the Ichniowski study), including questions about work practices and employee involvement. He uses factor analysis to construct two variables measuring the extent of various work practices, some of which could be considered part of high performance practices, and finds that greater use of such practices is associated with lower turnover and higher productivity per employee. The results were somewhat less successful at explaining overall firm financial performance. The factor capturing practices aimed at employee incentives was not significantly related to return on assets, and the factor measuring work systems was weakly related to Tobin's q. He also examines the question of synergies, mainly examining whether the practices were applied consistently across the organization, and finds little evidence of such synergies. Data limitations apparently prevented examining possible synergies between individual practices, the type of synergies that Milgrom and Roberts (1985) and others saw as potentially most important.

Huselid and Becker (1996) conducted a panel study of the relationship between work practices and organizational performance based on a 1994 follow-up survey (covering 1993) to Huselid's 1991 data. The study examines the same relationships as in the 1995 paper but uses the panel data to control for unmeasured firm-level heterogeneity via firm fixed effects. When this is done, the cross-sectional relationships become small and insignificant. As they note, the nature of their panel may have contributed to the absence of an apparent relationship. First, firms that adopt practices early on may do so because such practices are especially useful for them, while those that adopt them later on may have

waited in part because the practices do not make as much sense for them. The latter group may, for example, adopt them simply to follow trends or fads in their community, or after the costs of their implementation falls. Situations where the incidence of a practice is already high in the first wave of a panel imply that those who adopt the practice late in the sample period covered by the panel are late adopters. To the extent that this is the case, fixed-effects estimates based on longitudinal data are likely to focus more on the late adopters in the more recent period so that the estimates are based on firms for whom the effects may not be as strong (see also Freeman and Kleiner 1998). (Longitudinal data identifying when practices were introduced can help to address this problem as can data including the earliest adopters, such as we use in this paper.)

The second potential problem that may have contributed to the absence of a relationship concerns the exacerbation of measurement error that can occur in panel observations. The advantage of panel data is that they provide information on changes in practices that is necessary for estimating fixed-effects models. But such information is typically based on responses from more than one survey, which can compound measurement error. The Huselid and Becker surveys are only two years apart, and the work practices they study were already prevalent in 1991 with little difference in their use in 1991 and 1993 (Huselid and Becker 1996 p. 411). In such a case, the proportion of true changers relative to total reported changers is likely to be much lower than the proportion of the true incidence relative to reported incidence in a cross-section, because random misclassification of practices–if uncorrelated across years of the panel–will generally contribute a higher proportion of the "changers" than it does of those using a particular practice in a cross-section.[4] The problem should be less severe the farther apart the observations, as the rate of misclassification should remain the same, while the number of true changers increases. Thus, estimates that rely on the difference in values for observations over a short panel increase the role of measurement error and hence are likely to bias the estimated effects considerably

[4] Freeman (1984) demonstrates that this is true as long as the rate of misclassification is moderate, and provides a numerical example.

more towards zero. The extent to which this is the case, and whether it negates the advantages of addressing heterogeneity, depends on the extent of measurement error, its correlation over time, and the magnitude of the heterogeneity bias. Regardless, though, when panel data estimates of the effects of work practices are smaller (in absolute value) than cross-section estimates, we cannot tell whether this result is due to a reduction of bias from heterogeneity or an exacerbation of bias from measurement error, without additional information.[5]

Black and Lynch (1997) is perhaps most similar to the study we perform here, in part because they use some of the same data from the 1994 National Employers Survey (NES) that we use (described below). The authors examine some work organization practices, as well as other work-related or management practices such as computer use, benchmarking, and recruiting strategies. Theirs is the only study thus far to control for whether the quality of the workforce, as measured by education, changes with these practices. This may well be a crucial confounding factor in analyses like these; a work practice or management strategy associated with higher productivity may also be accompanied by a shift to a more-skilled workforce. In that case, some of the gains in performance otherwise attributed to the work practices might in fact be due to the more-skilled employees. Further, because the workforce changes are likely to raise labor costs, it would be difficult to assess the true net effect of work practices on profitability or efficiency unless changes in the workforce could be controlled. In other words, measures of the relationship between particular work practices and organizational performance are more likely to be accurate if they are estimated conditional on changes in workforce composition or quality that might accompany the change in work practices. In addition, Black and Lynch attempt to control for the problem of heterogeneity by using repeated observations over the 1988-1993 period taken from matched data in the Bureau of the Census's Longitudinal Research Database (LRD). They construct a within estimator in an attempt to generate more accurate estimates of the coefficients of the time-varying

[5] Huselid and Becker (1996) propose a method of addressing this measurement error bias in the panel estimates, which we discuss below. In using this method, results more consistent with the cross-sectional evidence were found

variables (capital, labor, and materials). They then estimate the effects of work practices in a second stage using the estimates from the first stage to form average residuals for each establishment, which they regress on the work practices (and other) variables from the NES. Because they have no information on work practices other than in 1993, the study effectively assumes that these practices and other variables are fixed over the 1988-1993 period. This approach has two limitations, however. First, as they acknowledge (p. 19), it does not correct for a correlation between unobserved establishment-specific effects and work practices which is the heart of the heterogeneity problem and, as explained below, is an issue that our approach addresses directly. Second, other evidence (see below) suggests that the work practices under consideration have expanded considerably in the 1988-1993 period, which makes it difficult to sustain the assumption necessary for their analysis that the incidence of work practices was constant over this period.[6]

Black and Lynch conclude that work practices per se have little relationship to labor productivity at the establishment level unless introduced in particular combinations with other work practices. For example, TQM only appears to affect performance when a large proportion of employees is involved in employee participation programs. Their results look only at productivity, and it would be important to know what the effects are on other dimensions of performance such as costs and the efficiency with which labor is used. As an example, Appelbaum, et al. (forthcoming) study the effects of work practices within several manufacturing industries on both performance and labor costs, although using cross-sectional data. They report that innovative work practices have little effect on establishment performance, but are associated with higher compensation for employees.

Easton and Jarrell (1998) examine the effects of introducing one particular practice–TQM programs–on the subsequent financial performance of publicly-held firms. Rather than relying on a random sample, their technique compared a sample of firms that introduced these practices to a matched

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only for one of the two performance measures they study.

set of otherwise similar firms that did not introduce them. (Of course, whether this matched sample eliminates bias from firm-level heterogeneity better than does a more conventional cross-section sample depends on how well the match controls for unobservables potentially associated with these work practices.) They find that firms that introduced TQM did better both in terms of traditional accounting measures of financial performance and stock prices.

Summary

A summary of the research on work practices and organizational performance might go as follows. Some innovative work practices seem to show positive relationships with organizational performance. The results tend to be stronger for studies within industries than for studies across industries, perhaps because measures of performance within industries are less contaminated by extraneous factors, and hence more accurately measure true performance differences across firms. But the measures used in industry studies, typically productivity, do not consider costs and therefore measure only productivity and not overall performance or competitiveness. In addition, the results from industry studies cannot be easily generalized. Some of the financial measures used in the other studies implicitly assess costs and benefits and therefore set a higher threshold for judging performance. Only one representative, national-level study found strong relationships between practices and financial performance (Huselid 1995), although those results could not be replicated using conventional methods with panel data (Huselid and Becker 1996). The other studies using financial outcomes found mixed results with some finding positive effects associated with some practices.

Our reading from all of the prior research is that the relationships that have been examined between work practices and performance outcomes show somewhat greater empirical support at the individual level than at the organizational level. The stronger evidence at the organizational level concerns productivity effects, whereas evidence on efficiency or overall performance effects, which

[6] Given that they are averaging productivity measures over years when, for some establishments, these work practices were not in effect, we would expect any estimated effects to reflect some average of the effects of having

factor in costs as well, is weaker and more sporadic. In addition, one might view the accumulated evidence as somewhat weakened by the fact that the variables showing significant relationships often differ across studies. Why apparently strong relationships between practices and performance at the individual level might not aggregate up to the organizational level is an issue for speculation. The relationships at the individual level may be so small that they are easily swamped by other factors, or they may ultimately have little power to explain organizational performance. In addition, most of the psychological studies that generated positive relationships at the individual level had few control variables, and it is possible that the results in these studies are not very robust with respect to omitted variables. Finally, few of these studies attempted to control for potential costs associated with these outcomes. It might be, for example, that the practices raise individual performance but also costs, perhaps in the manner that a wage premium might, which is why relationships with efficiency or profitability are harder to find.

In addition to these concerns, organizational-level studies have also been limited by some important methodological problems that may constrain finding stronger or at least more consistent empirical relationships between work practices and performance. The next section summarizes these methodological problems before outlining how we address them in our analyses.

III. Methodological Issues

Some of the problems facing existing studies relating work practices to organizational performance are common to all efforts to examine organizational performance. There is a paucity of representative samples because of the relative absence of surveys at the organizational level. Many of the studies to date use samples from specific industries, which limits their size and generalizability, while others use samples of convenience. Low response rates to surveys of organizations raise concerns about selection biases. Problems in generating measures of organizational performance are also common. As noted above, measures of productivity have the advantage of being meaningful and

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implemented these practices (at different points in the past) and no effect.

comparable across organizations, especially when they are used within industries, but because they ignore costs, it is difficult to tell much about efficiency or overall performance from them. Financial measures are preferred in this regard, but many of these, such as shareholder value, are only available for publicly-held companies. Also, shareholder value or profit and loss is only available for companies as a whole, whereas the independent variables of interest are typically only in effect or measured for smaller sub-units of the organization.

Other problems are more unique to the study of work practices. One important problem is the basic issue of what dimensions of work practices to study and what proxies to use for these practices. Becker and Gerhart (1996) note, for example, that many of the studies attempting to examine the effects of work practices on organizational performance describe themselves as examining "high performance work systems," but the set of practices that they use as proxies for those systems differ considerably. More generally, there is often little overlap between studies in the practices examined and the proxies used. In most cases, there is no common theory suggesting how the practices relate to performance. The most glaring theoretical gap is in the analyses of synergies among practices. Although most studies assert that there are positive interactions between practices, few present arguments as to how particular practices should interact with each other, instead simply assuming that more practices are better. Unfortunately, when researchers are presented with a wide array of possible work practices with little theoretical guidance as to how they interact, data mining possibilities abound, perhaps leading researchers to discover combinations of practices that appear to affect performance significantly in their data sets but are not relationships likely to be robust across data sets. This is not an issue of data but rather one of research design and strategy.

A more difficult problem to address in these studies is the issue of establishing causation, a particular challenge because in most cases the data are cross-sectional. The possibility that firms or establishments already performing well might have a greater ability and interest in adopting practices

that have come to be labeled "best practices" implies that the apparent positive effect of work practices on productivity or performance is not causal but rather may simply reflect firm- or establishment-level heterogeneity.[7] The issue of addressing heterogeneity to establish causation is, in our view, a central problem plaguing prior research.

To summarize, a persuasive test of the effects of work practices on organizational performance requires data that meet the following criteria. First, the sample should be representative of the population of employers, and the data should ideally be drawn at a level of analysis sufficiently close to where practices are established to expect relationships between practices and performance to hold. Second, the outcomes studied should include not only productivity but also some performance measure related to efficiency in order to assess whether productivity improvements are offset by cost increases.[8] Third, the data (and research design) should make it possible to establish whether a causal relationship exists between work practices and performance, accounting for the possibility that employers using particular work practices are different in other ways that are correlated with performance. Prior research has sometimes made advances along subsets of these criteria but has yet to address them all.

The analyses outlined below are similar to the organization-level studies reviewed in Section II in that they examine work practices and their relationship with organization-level performance. They differ from prior studies, and in our view represent an advance over them, because the characteristics of the data used allow us to overcome the methodological problems outlined above. The surveys from which the data are taken provide nationally representative samples of establishments and measures of performance that are comparable across establishments, addressing the first two methodological challenges noted above.

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[7] While this scenario is consistent with positive bias in regressions of performance measures on work practices, the opposite is also possible, if firms in dire straits tend to adopt new work practices (Huselid and Becker, 1996, p. 403). Related problems are the possibility of reverse causation, or that firms that adopt "best practices" in organizing work also adopt such practices in other areas, with the latter leading to improved organizational performance.

[8] There is something of a trade-off here in that units of analysis where such performance data–typically financial data–are available may be at higher levels than the units where work practices are implemented.

We are also able to match surveys to create panel data that allow us to address the issue of heterogeneity/causation. In particular, we are able to form panels in which one of the waves was conducted before the work organization practices under consideration were introduced. The ability to identify performance before these work practices were introduced offers important advantages over shorter panels of the type used by Huselid and Becker (1996) to address the heterogeneity issue. First, because we are not identifying the effects of work practices from "late adopters," we get closer to a true "average" effect of these practices, at least on those establishments that have adopted them thus far.[9] Second, and much more important, we avoid the issue of exacerbating measurement error that arises in panels. If none of the work practices was in place as of the first wave of the panel, then the sample of "changers" who statistically identify the effects of work practices when we difference the data consists of the entire set of establishments using these practices in the later panel. This is in contrast to the small subset of respondents who report changes in practices in short panels. That is, the work practices "treatment" variables are exactly the same in the cross-section specification and the specification incorporating fixed establishment effects, so there is *no* exacerbation of measurement error. Of course, this strategy hinges crucially on the absence of these work practices in the period covered by the earlier wave of the panel data set. A thorough discussion of this issue appears in Section VI, where we present research- and survey-based evidence, and in the Appendix, where we document the earliest anecdotal evidence and media reports of these work practices.

IV. Data and Measurement

Establishment-level surveys of employment practices conducted by the U.S. Bureau of the Census for the National Center on the Educational Quality of the Workforce provide data that allow us to address the methodological challenges outlined above that face analyses of the relationships between work practices and organizational performance. The original National Employers Survey (NES I) was

[9] In addition, if the effects of work practices take some time to emerge (or, in principle, to fade away), a long panel makes it more likely that enough time has elapsed to reveal these effects.

administered by the Bureau of the Census as a telephone survey in August and September of 1994 to a nationally representative sample of private establishments with 20 or more employees. It is structured to provide information about establishment performance and characteristics in 1993, although the questions regarding work practices refer to the time of the survey (i.e., in 1994), possibly introducing slight measurement error if there were some very recent introductions of these practices.[10]

The survey over-sampled establishments in the manufacturing sector and establishments with more than 100 employees. Public-sector employers, non-profit institutions, and corporate headquarters were excluded from the sample, as were establishments with fewer than 20 employees. Establishments with fewer than 20 employees represent approximately 85 percent of all establishments in the U.S., but those with more than 20 employees account for approximately 75 percent of all workers (Lynch and Black 1998). In addition, very small establishments are unlikely to be appropriate for or amenable to studying work practices because their working arrangements are often so informal and variable (changing day-to-day in response to even routine developments such as absenteeism) as to make it difficult to respond in a meaningful way to questions about work systems.

In administering the NES, the target respondent was the plant manager in the manufacturing sector and the local business site manager in the non-manufacturing sector. Other surveys have targeted the human resource manager. But in surveying establishments, what we want to measure is how work is actually done in the facility, not the policies that might exist in employee handbooks, and the best person to ask about actual operating practices is the person in charge of the establishment, not the manager in charge of personnel policies. The questionnaire was designed to allow for multiple respondents so that information could be obtained from establishments that kept financial information, for example, in a separate office–typically at corporate headquarters for multi-establishment enterprises. Computer Assisted Telephone Interviewing (CATI) was used to administer each survey, which took approximately 28 minutes to complete.

[10] The 1997 NES described below has the same structure.

The sampling frame for the survey was the Bureau of the Census's Standard Statistical Establishment List (SSEL), a comprehensive and up-to-date listing of establishments in the United States. Of the 4,633 eligible establishments that were contacted by Census, only 1,275 refused to participate in the survey. This represents a 72 percent response rate, which is substantially higher than any of the other surveys used in the studies summarized above. The usual reason given by employers about why they would not participate in the survey was that they did not participate in voluntary surveys or were too busy to participate. Analysis of the characteristics of respondents and non-respondents indicates that there was no significant pattern of differences at the two-digit industry level in the likelihood of participating in the survey (Lynch and Black 1998). The only differentiating characteristic of establishments less likely to participate was that manufacturing establishments with more than 1000 employees, constituting 0.1 percent of the sample, were less likely to do so. Of the 3,358 establishments participating in the survey, not all respondents completed all parts of the survey by the interview cutoff date of October 1, 1994, in which case they were excluded from the sample used below. The final numbers of surveys in which all parts of the survey were completed were 1,621 establishments in the manufacturing sector and 1,324 establishments in the non-manufacturing sector. This represents a 64 percent overall "completed" survey response rate.

The survey was repeated in August of 1997 (NES II), again administered by the U.S. Bureau of the Census via CATI. This survey covers 1996. The sampling frame was again drawn from the SSEL, targeting business establishments throughout the United States, excluding those with fewer than 20 employees and the other types of employers mentioned earlier, and oversampling the nation's largest establishments and those in the manufacturing sector. The sample for the NES II has three additional components, including: an oversampling of states involved in particular educational reform efforts (2,000 completed interviews in California, Kentucky, Michigan, Maryland, and Pennsylvania); approximately 2,500 completed interviews that comprise a representative sample of the rest of the United

States (45 states plus the District of Columbia); and a longitudinal component of about 900 completed interviews with business establishments that had participated in the NES I. A total of 5,465 establishments responded to NES II for a response rate of 78 percent.[11] The NES II data became available for use at the Bureau of the Census in January 1998.

Finally, labor costs per worker is a crucial performance measure in our analysis and is available in NES II but not NES I.[12] We therefore matched establishments in NES I to data from the 1993 LRD (or the 1992 LRD, if 1993 did not yield a match), to extract labor costs per worker.[13] To maintain consistency with the denominator of this variable, we also took total employment from the same observation.[14] The need to obtain labor cost data from the 1992 or 1993 LRD in using NES I, and the match to much earlier LRD observations in constructing our panels (discussed in the next section), limit the analysis to manufacturing establishments, which constitutes the sampling frame of the LRD.

Figure 1 outlines the construction of the data sets we analyze, along with their sample sizes. For completeness, we also present descriptive information on the short longitudinal panel based on the two NES files even though we report no analyses with that short panel. Appendix Table A1 provides further details on the construction of the samples, in particular how the sample sizes change from the original NES's as the various sample selection rules are imposed. The final products that we analyze are long panels with 433 complete observations (i.e., after accounting for item non-response, data errors, etc.) for the 1977-1993 panel, and 660 observations for the 1977-1996 panel. We also analyze a full 1993 cross-

[11] There were 915 respondents to the longitudinal component for a response rate of 88 percent. Excluding partial responses brought the longitudinal sample down to 766 and a completed response rate of 74 percent.

[12] We experimented with estimating labor costs in NES I based on data on the proportion of costs accounted for by labor (LC/TC) and other input costs, using the formula labor costs = ((LC/TC) × other input costs)/(1 – (LC/TC)). But these estimates were very noisy and did not replicate patterns expected and found in NES II, such as a negative association with the percent female.

[13] Although the sampling frame for NES I is the SSEL, the establishment size restrictions in the NES's and oversampling or certainty sampling of large establishments lead to considerable overlap between NES I and the 1993 LRD. Thus, we obtain approximately 40 percent of matches from the 1993 LRD, and 60 percent from the 1992 LRD (which includes the entire Census of Manufacturers).

[14] Further details appear in the notes to Table 2.

section with 663 observations, to incorporate information on workforce and plant-level controls not available in the LRD.

Table 1 outlines the work practice variables used in the analyses, their definitions in the survey, and descriptive statistics about them across surveys. The surveys asked a number of questions about work practices, with a particular focus on those that rely on employee involvement and related aspects of transformed work systems. As Table 1 indicates, some of these work practice variables are available only in NES I while the others are asked in both surveys. Rather than focusing only on NES I, though, we have chosen to construct and analyze two long panels (1977-1993 and 1977-1996, the latter using NES II) because of the importance of trying to establish robust results in this literature where, as noted earlier, such robust results are lacking. The work practice variables available in at least one of the two NES's are as follows:

> TQM–Formal TQM programs include employee involvement as an explicit component of their operations.
>
> SELFMNG–Self-managed or autonomous teams give their members authority over decisions that in other contexts are made by supervisors, such as how to perform their tasks or, in more advanced situations, which tasks to perform.
>
> MEET–Regularly scheduled meetings to discuss work-related problems include low-levels of employee involvement, such as quality circles run by supervisors, as well as higher levels where the employees manage the meetings and take decisions themselves.
>
> TRNTEAM–Teamwork training is a proxy for how seriously the organization takes teamwork. The argument is that, other things equal, establishments that invest in training teams are ones that allow teams to handle more important tasks.
>
> JOBROT–Job rotation is an aspect of job design that increases variety and employee understanding of other aspects of the operation. It is also a central aspect of cross-functional teams.
>
> TRNCROS–Cross-training is a proxy for how seriously the establishment takes job rotation, and teamwork generally to the extent that responsibilities are shared within teams. As above, establishments that invest in training employees for other jobs should be much more likely to make use of those skills by rotating employees across those jobs.
>
> PAYSKIL–Pay-for-skill programs specify certain skills that are important for the organization and then reward employees with increased compensation when they have acquired those skills.

Pay-for-skill is typically viewed as an important complement to teamwork and especially job rotation in that it supports learning the skills needed in such situations.

PSHR–Gainsharing, profit sharing, and related programs that provide employees a share in improved performance of the establishment are typically seen as a strong complement to employee involvement programs that give them the mechanism to contribute to greater performance. (In fact, observers suggest that gainsharing without involvement is largely ineffective.)

We also examine three other, different workplace practices that are sometimes associated with innovative work systems, in order to provide some comparisons with the relationships above:

BENCH–Benchmarking is a formal system of learning about practices at other organizations. It may also be a proxy for establishments that are in general more progressive about their practices.

COMPMNG and COMPWKR–The percentages of managerial and non-managerial employees who use computers on their job capture two potentially different dimensions of computer use, which has been the subject of considerable investigation in several other studies; the inclusion of these variables here provides a point of comparison with those other studies. Further, many observers believe that computer use is a signal or proxy for organizations that are more advanced in their operations.

V. Empirical Approach

The longitudinal component of the NES can provide panel data, much like the data used by Huselid and Becker (1996). The main drawback to this panel, as with their data, is that it covers a very short period and one in which the overall use of the work practices surveyed was already high in the first wave. This makes it likely that a high fraction of reported "changers" in use of work practices are spurious, in which case fixed-effects estimates will be severely contaminated by measurement error and likely to be more biased towards zero than the cross-section estimates.[15] In addition, the number of changers is small, likely rendering the data uninformative. This latter suspicion was confirmed in estimations using the short panel for many of the specifications discussed below. Estimated effects of

[15] Huselid and Becker (1996) provide a straightforward exposition of this well-known point for the case of continuous variables. The extension to indicator variables (such as those measuring work practices) is more complex (Freeman, 1984).

work practices in fixed-effects (first difference) specifications were extremely imprecise. Hence, these analyses are not reported in the paper.

Instead, we provide more informative fixed-effects analyses by constructing much longer panels based on matching establishments in the 1994 and 1997 NES cross-sections to data on these establishments prior to the introduction of the work practices we consider. In particular, we use the LRD for the year 1977. The LRD does not offer information about work practices but does contain information about establishment performance, productive inputs, and other plant-level characteristics and variables. The LRD is a panel of establishments including all establishments in years in which a Census of Manufacturers (CM) is carried out (years ending in a two or seven), and many fewer establishments in intervening years (from the Annual Survey of Manufactures). Among CM years–for which the most data are available–1977 is arguably the most recent period for which it is reasonable to assume that the work practices under consideration in this study had not yet been introduced.

Indeed, the central assumption on which our empirical approach is based is that the relevant work practices observed in the 1994 and 1997 NES's were introduced sometime after 1977. Under this assumption, changes in establishment performance between 1977 and 1993/1996 that are associated with the introduction of these practices (holding constant establishment-level characteristics, input use, and other relevant controls) can be ascribed to the introduction of these work practices. This design addresses the issue of causation that is otherwise confounded in cross-section estimates by heterogeneity across establishments that is potentially correlated with both use of innovative work practices and measures of organizational outcomes. Our "long panel" approach has the advantage of minimizing the concern about panel data noted by Huselid and Becker (1996)–that measured changes in work practices in a short panel beginning when these practices were already widespread are likely to be dominated by measurement error. Here, we avoid that problem. We have one survey-based observation on the establishment's work practices, and the other, implicit observation comes not from prior surveys but

from outside evidence indicating strongly that the practices did not exist in 1977. Measurement error may still exist in the 1993 or 1996 data. But because the "changers" that identify the effects of work practices in the fixed-effects estimations consist of all establishments implementing such practices, rather than those for whom reported use changed over a short period, the ratio of spurious to real changes is almost surely radically lowered, and the estimates are therefore much less likely to be biased.[16] Finally, this research design has the additional advantage of not relying in any way on respondents who discontinue use of work practices to identify the effects of these practices, which is advantageous given that adopting and discarding such practices may not have symmetric effects (although in principle this can be tested). A potential pitfall of the "long panel" analysis, however, is that establishment-level unobservables may be less likely to be fixed over a long than a short period, in which case the fixed-effects estimation is less likely to fully remove such unobservables.

Because the assumption that the relevant work practices were not in place before 1977 is crucial to our empirical approach, we describe in detail the evidence for it in the next section and in the Appendix.

VI. The Emergence of "High Performance" Work Systems

The common theme among the specific work practice variables considered in our study is an underlying management strategy that attempts to tap into employee knowledge and motivation through practices that permit greater employee involvement in workplace and job-related decisions. These practices are sometimes referred to with the potentially confusing term "high performance" work

[16] Huselid and Becker (1996) attempt to solve this problem in a different manner using their short panel. They use factor analysis of a number of work practices to construct a single proxy for "high performance" work practices. They then use estimates of Cronbach's "alpha" from their factor analysis as estimates of the reliability of readings on this proxy over time. This strikes us as a controversial approach, however, because the authors do not really have any information on the accuracy of the reports about these work practices. That is, they can evaluate the statistical reliability of the factor(s) identified from their analysis but not of the underlying data used to construct the factor(s). Thus, only if we accept the underlying data as accurate is their approach convincing; but, of course, the accuracy of the data is the fundamental problem. Another potential problem is that they are unable to implement their methods on regressions with any control variables.

systems.[17] While a wide range of practices are sometimes included under that heading, they tend to share this common theme.

Despite the incredible outpouring of research over time on the topic of transformed or "high performance" work systems, there is little evidence that the concept had much influence on industry through the 1970s. Several studies have attempted to understand why this academic interest did not translate into practice (see, especially Bailey 1992; but also Pfeffer 1994 and 1998).[18] The human relations movement underway following the Western Electric studies in practice never embraced employee involvement or fundamental reform of scientific management principles, focusing instead on paternalism and corporate communications in what many saw as cynical attempts to create a facade of corporate interest in employee welfare (see, e.g., Dubin (1958) for a description of the human relations approach). This movement died with the widespread institutionalization of industrial unionism in the 1950s. The sociotechnical studies had considerable influence in Swedish industry but essentially none in U.S. industry.

The psychology-based studies of job design that began to be popular in the 1970s were virtually all researcher-led experiments done on small subgroups of workers. One of the few examples where an entire facility adopted some of these job design concepts was the Gaines Dog Food plant in Topeka, Kansas, an experiment in the late 1970s that was studied so often that the researchers threatened to overwhelm the facility (see Walton (1982) for a review of the plant's experience). Interest in quality-of-worklife programs–essentially employee involvement in decisions concerning the conditions of the workplace that affect them (but not job tasks or work systems)–developed in the 1970s in response to concerns about poor morale that, in turn, may have contributed to declining productivity. The first widely published material examining whether poor work practices were contributing to low morale was

[17] The highly influential *America's Choice* (1989) report appears to have made the phrase "high performance work practices" popular. The confusion comes from assumptions about the etymology of the name–whether these practices have been already identified as associated empirically with superior performance on some dimension (either of employees or of the organization), as many assume and some assert, or whether it is just a name.

in 1974 (O'Toole 1974), and the first arguments for redesigning work through employee involvement to improve productivity came about three years later (see, e.g., Cumming and Malloy 1977). With the exception of a few well-publicized cases in the union sector (where involvement tended to be through union institutions rather than through individuals or teams), interest in reforming work practices along these lines made little headway as employers confronted the more immediate problems of recessions in 1973-75 and in 1980.

The interest among employers in high performance work systems and employee involvement more generally in the U.S. began in earnest following the rise of Japanese imports in manufacturing and the conclusions that U.S. manufacturers eventually drew about the apparent superiority of Japanese management practices. The most important concepts in Japanese manufacturing management as they relate to work organization are TQM, quality circles, and job rotation. Quality circles were meetings of production workers off-line to think of ways to improve quality. Quality circles were not teams, however, in that they had no collective tasks to perform. They were directed by management and had no autonomy in decision making (see Hill 1991). TQM involved having quality circles or other employee groups use statistical process control to measure and improve quality. Job rotation meant moving employees across different tasks in part to reduce fatigue, in part to create some variety to combat boredom, and in part to create some sense of how their tasks fit into the overall project.

Interest in Japanese management practices in the U.S. lagged the rise of Japanese import penetration by almost a decade. The popular books exposing the apparent advantages of Japanese practices did not appear until the 1980s (see, e.g., Ouchi 1982; Pascal and Amos 1982). In the automobile industry where Japanese success was most immediate, evidence indicates that U.S. companies did not begin any serious attempts to learn from Japanese practices until well into the 1980s. Indeed, it was not until Toyota began operating the New United Motors Manufacturing, Inc. (NUMMI)

[18] One simple hypothesis as to why employers were not embracing these practices that occurs to us is just that there was not much evidence that they benefited employers, especially at the time.

facility with General Motors in 1985 that U.S. auto makers began to believe that Japanese practices would work in the U.S. (see Womack, et al. 1990). Most observers see Japanese-inspired work practices then spreading from autos to other manufacturing and then to other industries (see, e.g., Kenny and Florida 1993).

Survey data on the introduction of these innovative practices might be more persuasive than a narrative account of their rise. But there are important caveats to the survey evidence. First, surveys typically do not even take place until there is already enough awareness of practices to make the results of interest. In this case, the first survey examining the incidence of transformed work practices in the economy took place in 1982. Second, the early surveys in particular rarely had representative samples (see below). Third, because the surveys sampled firms, rather than workplaces or establishments, and asked about whether practices existed, rather than their coverage, it is difficult to gauge the true extent of these practices. In such surveys, as noted below, a company like General Motors with 500,000 employees would be asked whether they had pay-for-skill, and the company would have to give a yes-or-no answer about the company as a whole. Having the work practice anywhere in the company could lead to a "yes" response even if coverage was extremely low. Bearing these caveats in mind, a review of survey data on the introduction of these practices follows.

The first survey that asks employers about transformed work practices appears to be in 1982, sponsored by the New York Stock Exchange (NYSE Office of Research 1992). It sampled firms with more than 100 employees with a response rate of 26 percent. The survey asked about approximately 17 categories of practices including such general ones as having formal training programs, with the response choices being "yes" or "no." Of the firms surveyed, 85 percent had none of the 17 practices being considered, including the very general items like formal training. With respect to specific practices relating to transformed work systems, approximately two percent had job rotation, two percent had production teams, and less than one percent had group productivity payments (although roughly four

percent had profit sharing).[19] Again, these figures simply mean that the firm had the practice somewhere in its organization.

A 1986 survey of 7,765 business units (albeit with a response rate of only 6.5 percent) sponsored by the U.S. Department of Labor found roughly two percent of these units had work groups that were allowed to manage themselves, about two percent had gainsharing (nearly three percent had profit sharing), and roughly three percent had some form of employee involvement in decision making someplace in their operations (Delaney, et al. 1989). Surveys conducted for the U.S. General Accounting Office (GAO) of the 1000 largest firms in the U.S. in terms of sales (The Fortune 1000) in 1987 and 1990 show something about the diffusion of these practices both over time and within firms. With respect to self-managed work teams, for example, the percentage of companies with these teams at least somewhere in their very large organizations rose from 28 percent to 47 percent over this period. The higher incidence in these surveys compared to the earlier ones may simply reflect the enormous scale of these large firms compared to the smaller average size in the previous surveys.[20] In neither period did more than one percent of these companies have 40 percent or more of their employees in such teams. These surveys also appear to be the first to ask about pay-for-skill. The percentage of these firms with pay-for-skill somewhere in their organization was 60 percent in 1987, but then appears to have declined to 51 percent in 1990 (Lawler, et al. 1992).

There are a number of other employer surveys examining various work practices since the late 1980s, and there is considerable variance in their reports of the incidence of these practices (see, e.g., Appelbaum and Batt 1994, Appendix A). Overall, however, the surveys seem to suggest considerable expansion of these practices during the early 1990s. Osterman's 1992 national probability sample of

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[19] The survey reports the percentage of firms with given practices as a percentage of those firms reporting any practices (that is, using the latter as the denominator). So in the text estimates of the percentage of firms in the entire sample with a given practice are calculated by multiplying the reported percentage (in the study) by the proportion with any practices.

[20] If, for example, one assumes that the average firm surveyed in the GAO study is many times larger than the average firm or business unit surveyed in the earlier studies, then even with the same incidence per employee, the incidence of practices per employer would be many times greater in the GAO surveys.

manufacturing establishments (with approximately a 50 percent response rate), for example, found that one-third of the establishments had TQM programs, 40 percent had self-directed teams, and 26 percent had job rotation (Osterman 1994). Our data for 1993, in Table 1, indicate slightly higher levels of TQM. Our job rotation and self-managed team questions ask about the percentage of employees covered, rather than simply whether the establishment has them. Comparison of our 1993 and 1996 data suggests growth in these practices across all of the work organization variables. This is true whether we compare the 1994 and 1997 data available in our two long panels (columns 3 and 4), or simply use the maximum number of observations on each work practice variable from the two cross-sections (columns 1 and 2); in both cases, though, the samples of establishments in 1994 and 1997 are different.

To summarize the narrative and the survey evidence, the incidence of transformational work practices based on principles of employee involvement appears to have been essentially nonexistent as of the late 1970s and very low as of the early 1980s. Through the mid-1980s, innovations such as job rotation, gainsharing, and teamwork of various kinds existed in only a small handful of firms with no evidence that they were at all widespread within those firms. The introduction of these practices expanded considerably in the early 1990s, however. One way to conceptualize the growth in these practices is that, by 1992, survey results indicated that these practices were more common within individual establishments than they were five years earlier anywhere within entire corporations.

Skeptics might argue that establishments might have had practices in place before 1977 that looked like some of these practices even before the terms for them became common. The evidence presented in the Appendix, which surveys the business press about examples of these practices and the first mention of the terms, suggests otherwise. Firms with such practices would have had a strong incentive to let the business community know about their practices and secure a reputation for themselves as innovators, and the business press at the same time was seeking out examples of innovative practices. And if such situations had been at all common, it is difficult to imagine that the

concepts would have received much subsequent attention as "new," when in fact they were seen as quite revolutionary after they appeared. Finally, recall that even when surveys first asked about the incidence of these practices, well after the concepts were introduced, they were quite rare.

Thus, based on this array of evidence, we are comfortable using establishment observations from the late 1970s to measure performance in the absence of "high performance" work practices. Correspondingly, we believe that the high incidence of these practices in 1994 and 1997 using the NES data generally indicates the true incidence of these practices. The following section describes our empirical analysis that attempts to uncover the causal effects of these work practices on organizational performance.

VII. Analyses and Results

The dependent variables we study as outcomes of the implementation of innovative work practices are establishment sales per worker, total labor costs per worker, and the ratio of these, which is a measure of the efficiency with which expenditures on labor are used. (This is the inverse of unit labor costs.)[21] Because the unit of analysis is the establishment, and most establishments are in multi-unit firms, profitability calculations are potentially problematic.[22] Most establishments in multi-establishment firms are simply cost centers, and those that calculate profit-and-loss estimates do so based on their own internal accounting principles, which implies that such estimates may not be comparable across observations. In contrast, sales, receipts, or the value of shipments are comparable measures of overall performance across establishments when controlling for industry.

One way to think about sales per worker and labor costs per worker is that they represent the potential benefits of these work practices to employers and employees, respectively; other things equal,

[21] All dependent variables are expressed in natural logs. With sales per worker and labor costs per worker expressed in logs, the regression of the log of the ratio of these variables (equivalent to the difference of logs) produces precisely the difference between the coefficients in the separate regressions, for each independent variable. However, running the regression for this difference in logs permits us to easily read off the statistical significance of the differential effects on productivity and labor costs, or equivalently on labor efficiency.

[22] Nonetheless, estimates of profitability have been used for establishment data such as these (Hellerstein, et al. 1998).

employers are better off with higher sales per employee, and employees are better off with higher labor costs per employee assuming that these costs are compensation-related as they are in this case.[23,24] Alternatively, we can think of these two variables as representing benefits and costs to employers. Higher sales per worker, ceteris paribus, benefit the employer. But to say anything about *net* benefits, we also need to know the effects of work practices on labor costs. Either way, combining information on productivity and labor costs, as we do by using our labor efficiency measure, lets us assess the extent to which higher costs associated with these work practices might offset the higher productivity/sales they may produce.

Table 2 reports descriptive statistics for these dependent variables in nominal terms.[25] As "background" information, Appendix Tables A2 and A3 report descriptive statistics for the establishment and workforce characteristics used as control variables, as well as explaining the definition and construction of these variables based on the NES's and additional calculations. Finally, Appendix Table A4 reports estimates of the baseline specifications including all of the controls used, without the work practice variables, for the various specifications we consider. These estimates replicate broad features of findings from the LRD, as well as data sets based on matches of workers to the LRD (Hellerstein, et al. 1999).[26]

In the results that follow, for each comparison of pairs of samples (i.e., cross-sectional and longitudinal), or of specifications with different dependent or independent variables, we restrict the

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[23] The labor cost measure has three components: wages and salaries; "Required Supplemental Labor Costs," which are government mandated payroll taxes; and "Voluntary Supplemental Labor Costs," which are employee benefits. Other costs associated with labor, such as training, recruiting expenses, etc., are not included in this measure.

[24] If work effort is also higher, then of course workers could be no better off. However, although this is theoretically possible, it seems to us a position worth considering seriously only in the face of evidence that workers are actually not better off. It is clear to us that policy makers, at least, look favorably on workplace changes that raise workers' earnings. As another example, we would probably view increased earnings as a result of more training to be a positive development, even if the higher earnings ultimately only compensate for training costs (including distortion of the time path of earnings).

[25] Natural logs are used in all of the regressions, eliminating any role for purely nominal influences.

[26] One perhaps surprising result is the absence of an effect of education on productivity, in Appendix Table A4. This appears to contradict findings in Black and Lynch (1997) using these same data. However, they also find no

sample to the same set of observations across all regressions in order to ensure that any differences in results across specifications or across analyses of different variables cannot be attributed to changes in the sample. We also estimated median regressions to assess the influence of potential outliers in the data; the results, available on request, were very similar to those presented here.

We report three sets of results: first, for the 1977-1993 panel; second, for the 1977-1996 panel; and third, for the full 1993 cross-section with additional controls added for workforce characteristics, which are not available in the 1977 LRD (or other years of the LRD). Although this latter specification cannot be used to address the issue of establishment-level heterogeneity, and hence does not embody our main methodological approach, it does give us independent information about the potential role of workforce characteristics in influencing relationships with establishment outcomes. As noted earlier, this information is potentially important because of the possibility that establishments implementing various work practices also change the composition of their workforce in the process. We do this additional analysis using the 1994 NES because it contains a larger set of questions on work practices, as shown in Table 1.

Productivity

Table 3 presents regression results relating work practices to sales per worker. Especially because most of the work practices have a common theme of employee involvement, they may tend to occur together in the same establishments and present collinearity problems in the empirical analysis. Therefore, Panel A reports results entering a single work practice separately, or in conjunction with closely-related practices (i.e., COMPMNG and COMPWKR), along with the control variables but without any other work practice variables. (In fact, the estimates tended not to be very different, albeit with somewhat higher standard errors, when the practices were entered jointly. These results are available on request.) The results for each work practice variable(s) are reported in separate rows

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effect of education when they break it up by occupation, which is similar to what we do in constructing an occupation-weighted average of education to come up with an overall measure.

(labeled 1, 2, 3, etc.). Columns 1 and 3 report cross-section estimates for the 1993 (column 1) and 1996 (column 3) data that could be successfully matched to the 1977 LRD. Columns 2 and 4 report the first-difference estimates using the 1977 data as well; recall that while the dependent variable and all of the control variables are differenced, the work practice variables effectively are not, given the assumption that these work practices were non-existent in 1977.

Many of the estimated effects appear positive across the alternative estimations. These include the effects of computer use by workers, meetings to discuss work-related problems, TQM, cross-training and teamwork training, and pay-for-skill. However, the statistical evidence is weak, as the only work practice variables showing a relationship with sales per worker at conventional levels of significance are computer use by workers and job rotation, and unlike all of the other work practice effects, the estimated effect of job rotation is negative. Looking across the set of work practice variables, in most cases (COMPWKR, MEET, SELFMNG, TQM, TRNTEAM, and PSHR, as well as JOBROT in the 1977-1996 panel), the estimated effect in the differenced equation is less positive than the estimated effect in the cross-section. This is consistent with unobservables that are positively related to productivity and are also positively correlated with the adoption of these work practices, a scenario we might expect.

Overall, then, although many of the estimates are consistent with positive productivity effects, the evidence is weak statistically. The evidence is weakened further by its inconsistency across specifications and samples. While the estimated effect of job rotation in the 1993 cross-section (column 1) is similar to that for the 1996 cross-section, the first-difference estimate in column 2 is near zero, as is the cross-section estimate in column 5.[27] Computer use among workers is also positively associated with sales per worker. The estimates are similar in the two panels, when we difference the data, and when we do not, although the results are significant only for the 1996 cross-section and the 1977-1996 panel. Here, as well, there is no evidence of an effect in the full 1993 cross-section, perhaps because

[27] The difference in the estimates between columns 1 and 5 is largely attributable to the additional control variables, rather than the additional observations.

computer use is also associated with changes in workforce composition. Thus, while we do not want to rule out positive effects of many of these individual work practices on productivity, we do want to emphasize that there is–in these data–at best a weak case for such an interpretation.

The second half of the table (Panel B) examines potential synergies among the work practice variables. The set of possible combinations of practices that might provide synergies is very large and, as noted above, prior research is not consistent in the choice of synergies to explore. To avoid data mining, all of the synergies in the "bundles" we explore were specified a priori, and results are reported for all of these whether significant or not. To keep the list of bundles manageable, we chose to examine first those practices that are intentionally designed to reinforce each other. Teamwork training, for example, is designed to improve performance in teams and should produce greater performance when used in combination with the employee meeting and self-managed team variables (specification 13). Cross-training by definition is designed to help teach other job skills used in the workplace and should therefore reinforce job rotation, which rotates employees into other tasks (specification 12).

The other synergies we examine are based on arguments from existing literature about the role that compensation mechanisms can play in supporting other practices. Profit and gainsharing plans that give employees a financial stake in improved performance help create incentives for them to use the opportunities that employee involvement creates to act in the interests of the organization.[28] Therefore we might expect to see positive synergies between these plans and employee-involvement practices–TQM, employee meetings, and self-managed work teams (specification 15). Similar arguments apply to pay-for-knowledge plans which support learning new workplace skills, which should be especially useful in situations where jobs are broader–job rotation, self-managed teams, and employee meetings in general (specification 14). The final synergy we examine is between job rotation and self-managed teams (specification 11), reflecting the hypotheses that self-managed teams can work more

[28] See, e.g., Levine and Tyson (1990) for one of many examples of this argument.

effectively when employees are able to rotate to the tasks that need doing and that job rotation is more useful when employees can control, through self-management, when and where to move.[29]

Including the interactions between the above variables leads to more significant relationships with the main effects. In specification 11, job rotation is negatively related to sales in both of the 1977-1996 specifications. Its interaction with self-managed teams, however, is positive, although only significant in the cross-sectional estimates, suggesting that these practices must be used in tandem to reap productivity benefits. Again, though, we do not get consistent evidence from the other panel or the full 1993 cross-section, so it is difficult to draw strong conclusions from these results. Specifications 14 and 15 yield more consistent results, most notably negative effects of self-managed teams in isolation but positive effects when combined with profit sharing or pay-for-skill. In specification 15, in particular, these results are robust across the alternative cross-section and fixed-effects estimates. However, to assess the total effects of these work practices, we have to take account of both the main and the interactive effects. Because these results generally go in opposite directions (e.g., self-managed teams, and its interaction with profit sharing, in specification 15), the interactive effects likely overstate the gains. We treat the issue of main and interactive effects more formally below when we discuss the more central issue of the effects of work practices on labor efficiency.

Summarizing the results for productivity suggests that, aside from these results on possible synergies between profit-sharing or pay-for-skill and self-managed teams, there is relatively little consistent, statistically significant evidence of beneficial productivity effects of these work practices based on employee involvement. Many of the estimated main effects in Panel A are positive, suggesting beneficial effects, but with the exception of computer use by workers,[30] none are statistically significant. This indicates a weak empirical case for productivity gains, although we do not rule out such gains based on our findings. Further emphasizing the fragility of the statistical evidence, a number of the stronger

[29] No doubt, of course, readers will think of other plausible interactions, including three, four, and other multiple interactions.

effects that do emerge in a particular specification are not robust across years of the NES nor when a more complete set of controls is included. Interestingly, though, first differencing the data to remove biases from heterogeneity has relative little influence on the estimates; although statistical significance is sometimes affected, point estimates change by less. If this result is generalizable–and, as noted below, it tends to hold for labor costs as well–it suggests that heterogeneity bias is not a major factor in the estimation of effects of work practices, which in turn would imply that cross-sectional data sets are informative.

Labor Costs

Table 4 presents exactly the same analyses, except this time using log labor costs per worker as the dependent variable. In Panel A, many more of the variables have significant relationships with the dependent variable. The significant main effects (for computer use by workers, benchmarking, meetings, TQM, team training, and profit sharing) are positive, suggesting that these practices raise labor costs. Moreover, these estimates are generally quite consistent across samples and estimators. In Panel B, in contrast to the results for specifications 14 and 15 for productivity, the interaction terms are relatively small and insignificant. However, the pattern of coefficients for specification 11 (in terms of both sign and significance) is very similar to that in the results for productivity. Again, though, we note that the estimates with this latter specification are quite different across samples (but not across estimators). These results might indicate either some gains or no costs from implementing these bundles of work practices, as productivity appears to be boosted with no effects on costs (specifications 14 and 15) or that productivity and labor costs move together. We report results that address this question more directly below.

An overall conclusion from Table 4 might be that many of these high performance work practices seem to raise labor costs per worker. Because compensation accounts for virtually all labor costs and the control variables in column 5 include some measures of worker quality (average education

[30] This is also the one consistent finding in Black and Lynch (1997).

and tenure, union status, percentage of women and minorities), these relationships appear to raise the average compensation of otherwise equivalent employees, rather than just resulting in a shift to higher quality workers. Finally, there are few apparent synergies between practices in their effects on labor costs per worker.

Labor Efficiency

Table 5 repeats the above analyses using the log of the ratio of sales per worker to labor costs per worker as the dependent variable. This table effectively combines the two previous analyses, or perhaps more accurately, examines the effects of work practices on labor efficiency. For example, a positive effect in this equation implies that productivity rises more than labor costs per worker, which is what we call an increase in efficiency.[31] To aid in interpreting the table, the variables identified in bold italics are those that had significant relationships with either sales per worker or labor costs per worker in Tables 3 or 4. When this was the case, the sign of the effects on sales per worker (SW) and labor costs per worker (LC) are also indicated, whenever possible; these sign "determinations" are based on whether t-statistics exceeded one with consistent signs of the estimates.[32] Note that in the majority of cases, the signs are in the same direction–both positive–suggesting that where these practices raise labor costs per worker, they also raise sales per worker, although we know that the latter effects were generally not statistically significant. Thus, the two should tend to offset each other in their effect on labor efficiency, although the extent to which there remains evidence of net benefits or net costs of course is an empirical question, and the evidence is not always in the same direction for sales and labor costs. What we see in Panel A of Table 5 are, by and large, empirical results consistent with offsetting relationships. There are essentially no significant main effect relationships, with the possible exception of benchmarking, which has a negative effect.

[31] Recall that the coefficients are identical to the difference between the results in Tables 3 and 4 but with the advantage of a simple statistical test of their significance.

[32] The notes to the table provide more details.

In Panel B, however, there are a number of interactive effects that are statistically significant and robust across samples and estimators. These include some of the interactive variables already highlighted in earlier tables, such as the interaction between profit-sharing and self-managed teams, which in specification 15 has a positive and significant effect on labor efficiency. In addition, some other interactions appear to have significant effects on labor efficiency in some specifications although they are generally negative (e.g., the interaction between pay-for-skill and job rotation in specification 14).

One might be tempted, based on these results, to conclude that there appear to be bundles of work practices that do increase labor efficiency, raising sales per worker by significantly more than labor costs per worker. In particular, this appears to be true for the implementation of self-managed teams along with either pay-for-skill or profit sharing. But the net effect of implementing a pair (or more) of work practices must include the main effects as well. Table 6 therefore reports the effect on labor efficiency of implementing pairs of practices, for the specifications where there was significant evidence of positive effects of bundles of work practices–including both the main effects and the interactive effects. These include the interactions of pay-for-skill with self-managed teams in specification 14 and the interaction between profit sharing and self-managed teams in specification 15. The more meaningful numbers are those evaluated at sample means, conditional on use of both practices considered, as these best represent how practices are typically implemented. These results are reported in the first row in each case, although for completeness we also report effects implied by 100-percent implementation. Because the signs of the interactive coefficients are generally opposite to the signs of the coefficients of the main effects, there turns out to be no statistically significant evidence that jointly implementing pay-for-skill or profit sharing along with self-managed teams increases labor efficiency; indeed, the point estimates are near zero. Even the implied effects of 100-percent implementation are not statistically significant.

VIII. Conclusions

How work might be organized so as to benefit employees and employers has been one of the central questions in social science throughout the past century. Despite the large body of research on this topic, empirical evidence of relationships between different work practices and outcomes affecting employers and employees has been extremely limited and has not yielded consistent results. The interest in measuring these effects has grown considerably during the 1990s but has been hampered by a series of difficult methodological challenges largely driven by data limitations.

The analyses presented in this paper offer several advantages over prior efforts to examine the relationships between work practices and outcomes. First, we are able to examine not only productivity but also labor costs in order to assess potential benefits to employees as well as the net gain to employers–captured in increases in labor efficiency–from implementing these practices. Our results, therefore, can also suggest something about the distribution of benefits between employers and employees. Second, the fact that the data are from a national probability sample of establishments makes our results easier to generalize to the economy as a whole. Third, and perhaps most important, the longitudinal nature of our data and its ability to measure performance before practices could reasonably be expected to have been introduced–in some cases before the practices were even known–offers a superior approach to controlling for heterogeneity as compared with prior studies.

Prior research on the effects of work practices presents a more mixed and less complete picture than is sometimes assumed. Earlier work focusing on psychological models found that practices associated with employee involvement, broadly defined, showed important benefits for employees but little evidence of benefits to employers. The more recent research on the productivity effects associated with work practices offers greater evidence of benefits to employers. The number of studies is relatively small, however, and many look only at productivity, as opposed to net effects that include some assessment of cost effects. Huselid (1995) offers perhaps the strongest evidence of net financial benefits

to employers that is associated with work practices, albeit not necessarily with employee involvement practices. But the cross-sectional nature of his data and the fact that the results did not hold up with the longitudinal extension of the data suggests the need to revisit the question. The special design of our longitudinal analysis makes it especially suited to address the Huselid and Becker (1996) critique that panel data may offer no improvement over cross-sectional data in testing for these relationships.

The results of our analyses, as noted above, suggest that the effects of these work practices on productivity appear to be positive, consistent with other recent research, although in our data little or none of this evidence is statistically significant. At the same time, there are benefits to employees from innovative work practices based on employee involvement in the form of higher labor costs/higher compensation. The fact that our analyses provide some controls for labor quality makes it clearer that the relationship is driven by the practices themselves and not by the related effect of using higher-quality, more expensive labor. Finally, there is no evidence of net benefits to employers associated with these practices, as labor cost increases tend to offset any productivity increases that may occur. On the other hand, neither is there evidence of net costs to employers associated with using these practices. Our results, then, parallel those of the earlier studies of work practices based at the individual level, suggesting gains to workers but providing little evidence of effects on organizational performance. And they replicate the findings of at least one contemporary industry study using organizational-level data (Appelbaum, et al., forthcoming). If one party to the employment relationship benefits from these practices without necessarily imposing costs on the other, there may be a Pareto improvement from innovative work practices, although of course our data cannot definitively address this welfare calculation.

Where our results may appear to be most at odds with the prior literature concerns the hypothesis about positive synergies between individual work practices. To be fair to the prior literature, however, the evidence for synergies comes only from productivity studies done in the context of particular

production processes and work practices, and the synergies are not identical across studies. The studies by Huselid (1995) and Black and Lynch (1997) that examine synergies among a representative population of employers find little evidence of these relationships. The latter, for example, find only one significant interaction and that only with the additional contingency of unionization.[33] Supporters of the notion that there are synergies between practices might well argue that different combinations of practices might reveal evidence of synergies. Short of testing every possible combination of work practices (at least nine such practices in our data) and every contingency (by industry, by union status, etc.) this theoretical possibility will always remain. But the fact that we find no evidence of synergies even among the most obvious and theoretically-grounded interactions is surely among the strongest evidence that is practical to bring to bear on this question.

Finally, it is constructive to think about our evidence from the perspective of employers contemplating the implementation of "high performance" work practices. While it is important to keep in mind the caveat that–statistically–a failure to find significant effects does not establish that there are no effects, our evidence suggests that such work practices do not affect labor efficiency (output per dollar of labor costs). Thus, despite raising labor costs/compensation, implementing such work practices should not hurt competitiveness. Indeed, it is possible that "high performance" work practices have other beneficial consequences (higher morale, greater adaptability, lower waste, etc.) that either do not affect firm performance measurably or do so in ways not captured by our performance measures. But the case for such assertions remains speculative.

[33] In addition, the estimated main effect of unionization is strongly negative, emphasizing the point made in the previous section that combined main and interactive effects may be much smaller than suggested by a large interactive effect.

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Appendix: Anecdotal Evidence and Initial Reports on the Introduction of Innovative Work Practices

Our approach to estimating the causal effects of work practices on establishment performance relies on the assertion that such practices were not present in U.S. establishments to any measurable extent as of 1977. Evidence for this assertion, in addition to the material presented in the main text, is presented below, separately for each aspect of work organization analyzed in the text.

TQM: Total quality management is a concept based on Japanese manufacturing practices and articulated by a series of authors, most notably Edwards Deming, whose first publication in English for a mass audience on the subject was in 1984 (Deming 1984). In terms of practice in the West, it appears to have evolved from the quality circle movement in the early 1980s and was first recognized as a movement in companies in the late 1980s. (See, e.g., Hill 1991, and Harris 1995, for evidence on the rise of TQM as a movement.) The first account of a TQM program in the U.S. that we found in the popular press was at the Allen-Bradley company in 1987 where it was referred to as TQMS, for "total quality management system" (PR NEWSWIRE 1987). The first survey of firms that asks about the incidence of TQM was in 1990 (Grant Thorton 1991).

Pay-for-Skill: This concept was first presented in 1985 as part of a more general critique by Edward Lawler of U.S. compensation practices. It was originally known as "skill-based pay" (Lawler 1985). The first account of a company using pay-for-skill is General Mills in 1991 (Ledford and Bergel 1991).

Job Rotation: This term has one of the longer legacies among work practice innovations. Academic interest in job rotation appears to begin in the late 1960s with the argument that job rotation may reduce boredom among employees performing simple tasks. The first academic reference we find to the

concept is in a very short note in 1968 in the context of implications for expanding shift work, which argues that job rotation may equally have negative affects on individual performance depending on the circumstances (Wilkerson and Edwards 1968). In the mid-1970s there were some experiments with job rotation as part of more general experiments in job enrichment, but they also seemed rare enough to merit articles about individual experiments (e.g., King 1974).[1] These experiments in job design were essentially abandoned by the late 1970s because employers found that they did not translate into gains for the organization.[2] The improvements observed were mainly in the attitudes of individual employees and, to a lesser extent, in performance measures at the individual level, such as turnover and absenteeism (see main text). The revival of interest in job rotation came with the interest in Japanese management practices in the late 1980s, in which job rotation was a part of a set of practices that included teamwork and continuous improvement, or Kaizan.

Cross-Training: The notion of cross-training would seem to be driven primarily by the interest in moving employees across different jobs, and to be closely related to job rotation. Job rotation when initially introduced referred to assembly line and other low-skill jobs where no additional skills were needed to perform in the other positions. Cross-training was initially introduced to refer specifically to situations where different jobs could not be performed without new skills. The first mention that we can find of an instance of cross-training is at Delta Airlines in 1980, where cross-training is presented as an intervention in helping employees understand how their individual jobs contribute to the overall goals of

[1] During the 1970s, the business press sometimes referred to executive development programs where managers were moved through different areas of the corporation in order to get experience as "job rotation," but this is a very different notion from the modern definition that refers to a regular movement across discrete job tasks, typically for production work, on a long-term or permanent basis.

[2] During the 1973-75 recession, some companies (particularly those with unions) went to a four-day week in an effort to avoid more severe layoffs. In the process of shortening the workweek, work assignments were often changed, and workers sometimes had to rotate across jobs in order to get the work done. Job rotation was an unintended consequence of this process and was frequently discussed in the business press (e.g., Feron 1974). But it was not the design of company policies nor was it conducted in ways that were designed to produce the effects associated with modern job rotation. Virtually all companies soon dropped their experiments with shortened workweeks and the job rotation that resulted from them.

the company (Dow Jones New Service 1980). The first academic mention of cross-training appears to be in 1986 (Gotz and Stanton 1986).

Benchmarking: No doubt the informal process of making comparisons with practices at other organizations is as old as time. But the systematic attempt to learn and copy individual practices from organizations that are seen as leaders in those areas (irrespective of whether they are competitors), and the term "benchmarking," is generally acknowledged to have begun in the corporate world in 1981 with the Xerox Corporation's efforts to improve its sliding competitive position against Japanese competitors (Jacobson and Hillkirk 1986). By the end of the 1980s, benchmarking was widespread and seen as an integral part of efforts to improve organizational performance (Fray 1988).

Self-Managed Teams: A subset of teamwork is self-managed teams, where the members of the team go beyond working together to manage themselves, taking on most of the responsibilities of supervision and directing their own work. The first mention of self-managed teams we can find is in 1989 (Luthans and Fox 1989), in an article about pay-for-skill. The article generally credited with bringing the concept to popular attention came in 1990 (Dumaine 1990), where self-managed teams are described as a "still controversial innovation" that only a few companies had experimented with during the 1980s.

Meetings: The concept of employees organized in regularly scheduled meetings to discuss workplace problems is obviously broad in its scope. But there is still little evidence that such arrangements existed before the 1980s. Quality circles seem to be the best-known common practice that fits this definition. Cotton (1993) indicates that two U.S. companies experimented with quality circle principles in the mid-1970s. Otherwise, the practice began to spread with the interest in Japanese management beginning in the mid-1980s. Stronger examples of scheduled meetings in the form of teams seem equally rare. One

estimate suggests that there were no more than 24 plants in the U.S. that had team work principles in 1976 (Hoerr 1986).

Gainsharing/Profit Sharing: The notion of sharing performance improvements with employees certainly has the longest academic history of any of the innovative workplace practices. Some gainsharing plans clearly were in operation before 1977, although the number of employers with such plans appears to have been very small. Perhaps the most popular incarnation of gainsharing ideas were those put forward by Joseph Scanlon in the 1950s and subsequently continued and adapted by his followers. Scanlon plans were almost entirely in unionized facilities. Cotton (1993) estimates that perhaps 500 companies have at one time adopted Scanlon plans, although it is difficult to know how many of those were in place before 1977. Reviews of specific gainsharing plans come up with reasonably small numbers–44 Scanlon plans in 1975 (National Commission on Productivity and Work Quality 1975), 72 examples of Improshare in 1982 (Fein 1982)–although these studies were not necessarily designed to establish the incidence in the population. The New York Stock Exchange survey figure noted above found roughly 4 percent of *firms* with some profit sharing in 1982. It is quite possible that some of the establishments in the 1977 LRD data may have had profit or gainsharing plans. But it also seems likely that the percentage with such plans was very small, especially as compared to the percentage that had them in 1994 (approximately 60 percent of *establishments*).

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FIGURE 1
Constructed Manufacturing Data Sets

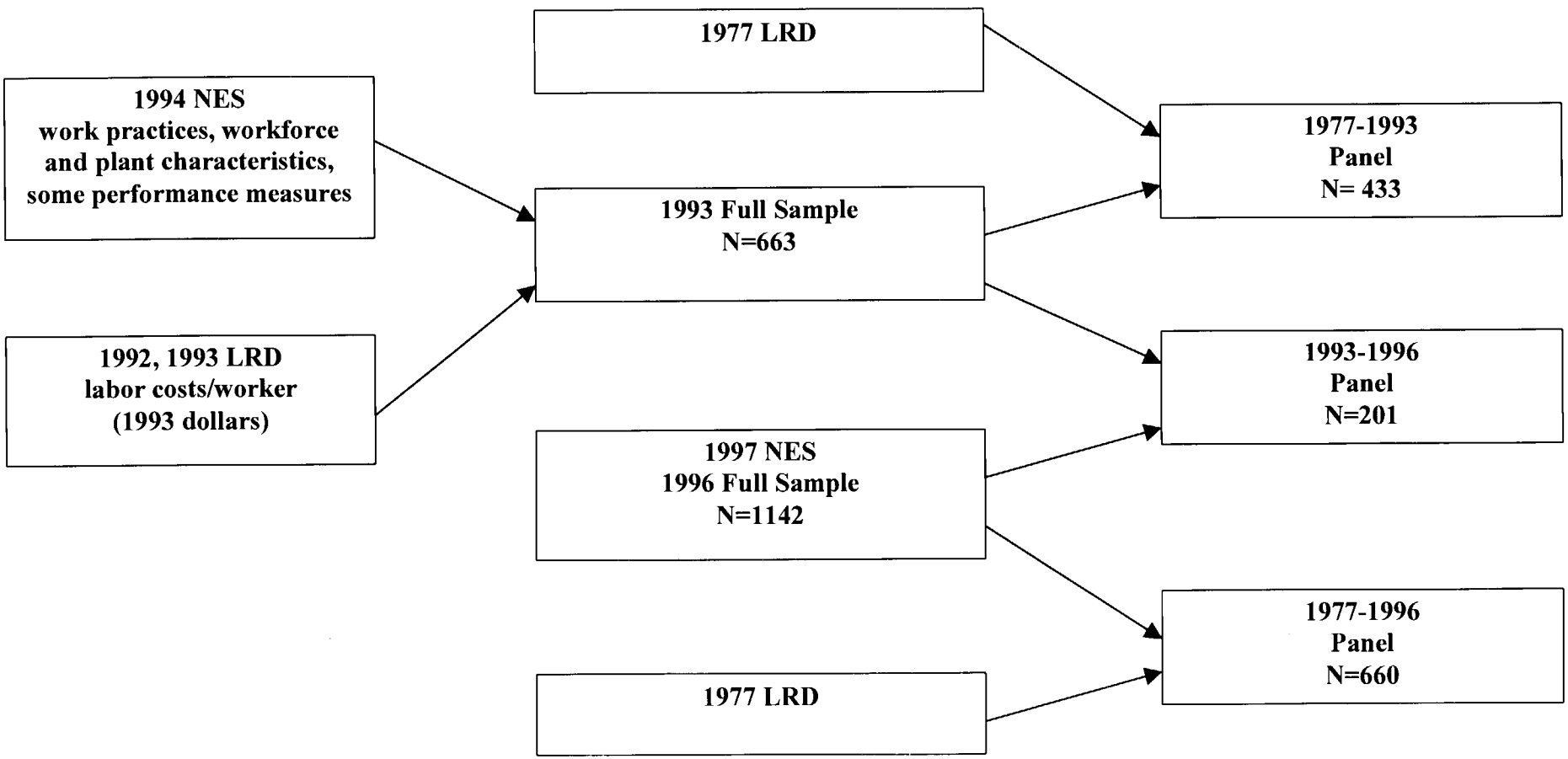

Table 1: Descriptive Statistics and Variable Descriptions for Work Practice Variables in 1994 and 1997 NES, Manufacturing Plants

	1993, maximum obs.	1996, maximum obs.	1977-1993 panel 1993	1977-1996 panel 1996	Variable description
	(1)	(2)	(3)	(4)	(5)
COMPMNG	66.35 (37.37)	74.25 (34.55)	67.26 (35.55)	72.68 (34.46)	What percentage of your managers and supervisors use computers in their jobs?
COMPWKR	25.13 (31.28)	28.16 (32.08)	26.45 (30.83)	27.28 (30.64)	What percentage of your production and non-supervisory employees use computers in their jobs? 1997: Substitute percent for percentage.
BENCH	.24	.22	.27	.21	Has your establishent participated in any benchmarking programs that compare practices and performances with other organizations?
MEET	40.43 (42.48)	49.43 (49.70)	35.87 (40.56)	43.71 (43.59)	What percentage of non-managerial and non-supervisory workers are involved in regularly scheduled meetings to discuss work-related problems? 1997: What percent of, or how many, ...
JOBROT	17.22 (30.69)	24.74 (34.58)	14.70 (27.36)	21.12 (31.55)	What percentage of non-managerial and non-supervisory employees are currently involved in job rotation? 1997: What percent of, or how many, ...
SELFMNG	12.10 (25.83)	16.52 (31.35)	8.40 (22.83)	12.32 (26.66)	What percentage of non-managerial and non-supervisory employees are currently involved in self-managed teams? 1997: What percent, or how many, ...
TQM	.42	...	.41	...	Has your establishment adopted a formal Total Quality Management program?
TRNCROS	.69	...	.64	...	Does your establishment pay for or provide: Cross-training?
TRNTEAM	.52		.44		Teamwork or problem-solving training?
PAYSKIL	21.88 (34.44)	...	22.29 (35.33)	...	What percentage of your employees receive pay for skill or pay for knowledge?
PSHR	.59	...	.61	...	Does your company have a profit sharing, bonus, or gain-sharing plan for any of the following categories or workers (yes/no)? Technical/technical support, office/clerical/sales/customer service, or production? Constructed variable: plan for at least one of these three non-management, non-supervisory occupational groups.
N	1529-1717	2152-2379	433	660	

For continuous variables, standard deviations are shown in parentheses in columns (1)-(4). In columns (3) and (4), samples are restricted to observations with all data used in regressions available in corresponding year. All estimates are weighted. The language in the 1994 and 1997 NES's is identical unless otherwise noted. "Yes" responses are coded as ones, and "No" responses as zeroes.

Table 2: Descriptive Statistics and Variable Descriptions for Measures of Performance, Manufacturing Plants

	1977-1993 panel		1977-1996 panel		
	1977	1993	1977	1996	Variable description
	(1)	(2)	(3)	(4)	(9)
SALESWKR	60.64	137.44	57.18	177.07	What was your establishment's total value of sales, receipts, or shipments (gross revenues, sales, or receipts, in 1000s) for calendar year ____? Constructed variable: SALESWKR = sales/TOTEMP.
	(77.34)	(139.98)	(77.20)	(627.41)	
Median	~43	~104	~41	~109	
Interquartile range	28	85	29	83	
LCWKR	14.37	33.54	13.54	28.41	What was the total labor cost (1000s) used in the production of your ____ sales? (1993 or 1992 data substituted from LRD) Constructed variable: LCWKR = total labor costs / TOTEMP
	(4.32)	(12.49)	(3.85)	(21.12)	
Median	~14	~33	~13	~24	
Interquartile range	6	18	5	16	
LABEFF	1.23	1.25	1.22	1.58	Log(SALESWKR) - Log(LCWKR)
	(.52)	(.54)	(.57)	(.91)	
Median	~1.18	~1.22	~1.12	~1.41	
Interquartile range	.61	.62	.66	.83	
N	433	433	660	660	

See notes to Table 1. The 1994 NES does not include a total labor cost variable; all that is available is the percentage of the establishment's total cost of production that is accounted for by labor costs. Consequently, data on labor costs were taken from the 1993 LRD for the same establishments (which covers the same period as the 1994 NES), or from the 1992 Census of Manufacturers if the establishment did not appear in the 1993 LRD. For data drawn from 1992, the labor cost figure was adjusted upward by the ECI. The demoninator (TOTEMP) was drawn from the corresponding data source. Because the 1994 NES survey has limited information on off-payroll workers (e.g., temporaries or contract employees) not covered in the other data sources, for cases where it was possible to establish that a fraction of workers in the 1994 NES were off-payroll, TOTEMP from the LRD or Census of Manufacturers was adjusted upward by the same fraction. In computing LABEFF, we always used SALESWKR from the NES data. Medians reported in this and other tables are "fuzzy" medians because of confidentiality requirements. The fuzzy medians are means of the true weighted median and a set of 9-14 closest observations on either side, where the true median is given extra weight while the weights of the surrounding observations are reduced linearly in proportion to their ranked differences from the median.

Table 3: Work Practice OLS Regressions for Log of Sales/Worker, Manufacturing Plants

A. Individual Work Practices

	1977-1993 panel		1977-1996 panel		Full 1993 cross section.
	1993	Differenced	1996	Differenced	with additional controls
	(1)	(2)	(3)	(4)	(5)
1. COMPMNG/100	-.076	-.067	.059	.002	-.003
	(.085)	(.083)	(.076)	(.080)	(.066)
COMPWKR/100	.133	.085	.179**	.124**	-.001
	(.086)	(.084)	(.073)	(.074)	(.074)
2. BENCH	.053	-.032	.008	-.041	.035
	(.048)	(.048)	(.042)	(.043)	(.041)
3. MEET/100	.070	.046	.075	.074	.051
	(.057)	(.056)	(.047)	(.049)	(.047)
4. JOBROT/100	-.095	-.000	-.096	-.127**	.005
	(.086)	(.086)	(.062)	(.065)	(.067)
5. SELFMNG/100	.026	-.051	.027	.005	-.015
	(.097)	(.098)	(.071)	(.075)	(.077)
6. TQM	.035	.030			.043
	(.050)	(.048)			(.040)
7. TRNCROS	.034	.038			.010
	(.056)	(.056)			(.044)
8. TRNTEAM	.080	.027			.029
	(.057)	(.054)			(.044)
9. PAYSKIL/100	.064	.067			-.039
	(.081)	(.080)			(.062)
10. PSHR	.072	-.016			.058
	(.047)	(.048)			(.038)

B. Work Practice Bundles

11. JOBROT	-.068	.051	-.194**	-.187**	.019
	(.098)	(.098)	(.074)	(.078)	(.079)
SELFMNG	.097	.022	-.097	-.057	-.000
	(.123)	(.123)	(.094)	(.099)	(.102)
JOBROT×	-.200	-.273	.463**	.286	-.053
SELFMNG	(.269)	(.272)	(.197)	(.210)	(.199)
12. TRNCROS	.000	.024			-.009
	(.062)	(.062)			(.049)
JOBROT	-.290	-.078			-.099
	(.161)	(.165)			(.138)
TRNCROS×	.269	.104			.134
JOBROT	(.190)	(.194)			(.156)
13. TRNTEAM	.091	.029			.023
	(.072)	(.071)			(.057)
MEET	.067	.029			.047
	(.123)	(.123)			(.086)
SELFMNG	.205	.145			.000
	(.210)	(.216)			(.179)
TRNTEAM×	-.017	.030			.003
MEET	(.140)	(.142)			(.102)
TRNTEAM×	-.270	-.280			-.040
SELFMNG	(.239)	(.244)			(.198)

Table 3 (continued)

B. Work Practices Bundled (continued)

	1977-1993 panel		1977-1996 panel		Full 1993 cross section.
	1993	Differenced	1996	Differenced	with additional controls
	(1)	(2)	(3)	(4)	(5)
14. PAYSKIL	-.056	-.103			-.096
	(.125)	(.122)			(.100)
MEET	.037	-.007			.035
	(.067)	(.066)			(.054)
JOBROT	-.059	.029			.048
	(.101)	(.101)			(.079)
SELFMNG	-.113	-.171			-.150
	(.122)	(.123)			(.099)
PAYSKIL×	.159	.334*			.054
MEET	(.198)	(.199)			(.149)
PAYSKIL×	-.264	-.133			-.268
JOBROT	(.328)	(.328)			(.221)
PAYSKIL×	.436	.236			.489**
SELFMNG	(.295)	(.300)			(.227)
15. PSHR	.099	-.034			.073
	(.064)	(.065)			(.051)
TQM	.091	.017			.131**
	(.073)	(.073)			(.059)
MEET	.058	.092			-.000
	(.089)	(.089)			(.074)
SELFMNG	-.282	-.467**			-.259**
	(.181)	(.182)			(.126)
PSHR×	-.134	.018			-.188**
TQM	(.098)	(.099)			(.079)
PSHR×	.007	-.091			.069
MEET	(.122)	(.124)			(.098)
PSHR×	.417*	.615**			.381**
SELFMNG	(.237)	(.239)			(.172)
N	433	433	660	660	663

Each numbered set of work practice variables was entered separately. In columns (1)-(4), the other control variables included are production function inputs, establishment age (for 1993 only), and dummy variables for two-digit industries, whether the plant belongs to a multi-plant firm, and whether in the 1993 data the labor cost and employment data came from the 1992 LRD. In column (5) additional controls for age of the capital stock and numerous workforce characteristics are added. Descriptive statistics for these variables are reported in Appendix Tables A2 and A3, and estimated coefficients of these variables for the specifications corresponding to those in this table and Tables 4 and 5, but excluding the work practice variables, are reported in Appendix Table A4. In Panel B, the '/100' is omitted from the variable name, although these variables (MEET, JOBROT, SELFMNG, and PAYSKIL) are always divided by 100. '*' indicates statistical significance at the ten-percent level, and '**' statistical significance at the five-percent level, in two-sided tests.

Table 4: Work Practice OLS Regressions for Log of Labor Costs/Worker, Manufacturing Plants

A. Individual Work Practices

	1977-1993 panel		1977-1996 panel		Full 1993 cross section, with additional controls
	1993	Differenced	1996	Differenced	
	(1)	(2)	(3)	(4)	(5)
1. COMPMNG/100	.019	.040	-.010	.002	.027
	(.051)	(.043)	(.104)	(.099)	(.036)
COMPWKR/100	.183**	.020	.147	.154*	.114**
	(.051)	(.043)	(.099)	(.092)	(.041)
2. BENCH	.103**	.067**	.038	.041	.076**
	(.029)	(.024)	(.056)	(.053)	(.022)
3. MEET/100	.044	.014	.108*	.137**	.001
	(.034)	(.029)	(.063)	(.060)	(.026)
4. JOBROT/100	-.060	.018	-.092	-.107	-.044
	(.052)	(.044)	(.085)	(.081)	(.037)
5. SELFMNG/100	.070	-.033	-.007	-.034	.024
	(.058)	(.050)	(.096)	(.093)	(.043)
6. TQM	.094**	.064**			.086**
	(.030)	(.024)			(.022)
7. TRNCROS	.005	-.008			.015
	(.034)	(.028)			(.025)
8. TRNTEAM	.092**	.048*			.046*
	(.034)	(.028)			(.024)
9. PAYSKIL/100	.005	-.033			-.015
	(.049)	(.041)			(.034)
10. PSHR	.063**	.021			.063**
	(.029)	(.024)			(.021)

B. Work Practice Bundles

11. JOBROT	-.021	.071	-.153	-.196**	-.035
	(.059)	(.050)	(.101)	(.097)	(.044)
SELFMNG	.167**	.044	-.082	-.158	.064
	(.074)	(.063)	(.127)	(.123)	(.057)
JOBROT× SELFMNG	-.307*	-.297**	.299	.471*	-.086
	(.162)	(.139)	(.267)	(.261)	(.111)
12. TRNCROS	-.013	-.014			.001
	(.037)	(.032)			(.027)
JOBROT	-.094	-.009			-.133*
	(.098)	(.084)			(.077)
TRNCROS× JOBROT	.047	.038			.113
	(.115)	(.099)			(.087)
13. TRNTEAM	.055	.043			.003
	(.043)	(.036)			(.032)
MEET	-.040	.004			-.045
	(.073)	(.063)			(.048)
SELFMNG	-.063	-.138			-.160
	(.126)	(.110)			(.099)
TRNTEAM× MEET	.068	-.003			.050
	(.084)	(.072)			(.057)
TRNTEAM× SELFMNG	.145	.118			.217**
	(.143)	(.125)			(.110)

Table 4 (continued)

B. Work Practices Bundled (continued)

	1977-1993 panel		1977-1996 panel		Full 1993 cross section.
	1993	Differenced	1996	Differenced	with additional controls
	(1)	(2)	(3)	(4)	(5)
14. PAYSKIL	-.020	-.054			.007
	(.075)	(.063)			(.056)
MEET	.049	.015			.025
	(.041)	(.034)			(.030)
JOBROT	-.118*	-.014			-.080*
	(.061)	(.052)			(.044)
SELFMNG	.095	-.010			.029
	(.074)	(.063)			(.055)
PAYSKIL×	-.016	.041			-.124
MEET	(.120)	(.103)			(.083)
PAYSKIL×	.259	.222			.147
JOBROT	(.198)	(.169)			(.123)
PAYSKIL×	-.114	-.143			.052
SELFMNG	(.178)	(.154)			(.127)
15. PSHR	.044	.019			.048*
	(.038)	(.033)			(.028)
TQM	.112**	.100**			.103**
	(.044)	(.037)			(.033)
MEET	-.044	-.026			-.062
	(.053)	(.045)			(.041)
SELFMNG	.086	-.161*			-.030
	(.108)	(.093)			(.070)
PSHR×	-.050	-.059			-.036
TQM	(.059)	(.051)			(.044)
PSHR×	.117	.049			.065
MEET	(.073)	(.063)			(.054)
PSHR×	-.099	.152			.056
SELFMNG	(.142)	(.122)			(.095)
N	433	433	660	660	663

See notes to Table 3.

Table 5: Work Practice OLS Regressions for (Log of Sales/Worker - Log Labor Costs/Worker), Manufacturing Plants

A. Individual Work Practices

	1977-1993 panel			1977-1996 panel			Full 1993 cross section, with additional controls	
	SW/LC	1993	Differenced	SW/LC	1996	Differenced	SW/LC	1993
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1. COMPMNG/100		-.095	-.106		.069	-.000		-.030
		(.081)	(.083)		(.110)	(.108)		(.065)
COMPWKR/100	+/+	-.050	.065	+/+	.032	-.030	?/?	-.115
		(.082)	(.084)		(.104)	(.100)		(.074)
2. ***BENCH***	+/+	-.050	-.098**		-.030	-.082	?/+	-.041
		(.046)	(.048)		(.060)	(.058)		(.040)
3. ***MEET/100***		.026	.033	+/+	-.033	-.063		.051
		(.055)	(.056)		(.067)	(.066)		(.047)
4. ***JOBROT/100***		-.035	-.018	-/-	-.003	-.020		.050
		(.083)	(.086)		(.090)	(.089)		(.067)
5. SELFMNG/100		-.045	-.018		.034	.039		-.039
		(.093)	(.098)		(.102)	(.101)		(.077)
6. ***TQM***	?/+	-.059	-.035				+/+	-.044
		(.048)	(.048)					(.040)
7. TRNCROS		.029	.046					-.005
		(.053)	(.056)					(.044)
8. ***TRNTEAM***	+/+	-.012	-.022				?/+	-.017
		(.054)	(.055)					(.043)
9. PAYSKIL/100		.060	.100					-.024
		(.077)	(.080)					(.062)
10. ***PSHR***	+/+	.010	-.037				+/+	-.006
		(.045)	(.048)					(.038)
B. Work Practice Bundles								
11. ***JOBROT***		-.047	-.020	-/-	-.041	.010		.053
		(.093)	(.098)		(.107)	(.105)		(.079)
SELFMNG	?/+	-.069	-.022		-.015	.101	?/+	-.064
		(.118)	(.124)		(.135)	(.134)		(.102)
JOBROT × ***SELFMNG***	-/-	.107	.024	+/+	.164	-.186	?/?	.033
		(.258)	(.274)		(.283)	(.284)		(.198)
12. TRNCROS		.000	.037					-.010
		(.059)	(.062)					(.049)
JOBROT		-.196	-.069					.034
		(.154)	(.165)					(.138)
TRNCROS× JOBROT		.222	.066					.021
		(.182)	(.195)					(.156)
13. TRNTEAM		.036	-.014					.020
		(.068)	(.071)					(.057)
MEET		.107	.026					.092
		(.117)	(.124)					(.085)
SELFMNG		.268	.283					.160
		(.200)	(.217)					(.178)
TRNTEAM× MEET		-.085	.033					-.047
		(.133)	(.142)					(.102)
TRNTEAM × ***SELFMNG***		-.415*	-.398				?/+	-.257
		(.228)	(.245)					(.197)

Table 5 (continued)

B. Work Practices Bundled (continued)

	1977-1993 panel			1977-1996 panel			Full 1993 cross section, with additional controls	
	SW/LC	1993	Differenced	SW/LC	1996	Differenced	SW/LC	1993
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
14. PAYSKIL		-.036	-.049					-.104
		(.119)	(.123)					(.100)
MEET		-.012	-.022					.011
		(.064)	(.066)					(.054)
JOBROT	?/–	.059	.043				?/–	.128
		(.096)	(.101)					(.079)
SELFMNG		-.209*	-.161					-.179*
		(.116)	(.123)					(.098)
PAYSKIL × ***MEET***	+/?	.175	.293				?/–	.178
		(.189)	(.200)					(.149)
PAYSKIL× JOBROT		-.523*	-.355					-.415*
		(.312)	(.329)					(.220)
PAYSKIL × ***SELFMNG***		.550*	.379				+/?	.437*
		(.281)	(.301)					(.226)
15. PSHR		.055	-.053					.025
		(.061)	(.065)					(.051)
TQM	+/+	-.021	-.083				+/+	.028
		(.070)	(.073)					(.059)
MEET		.102	.119					.062
		(.084)	(.089)					(.074)
SELFMNG	–/–	-.368**	-.306*				–/?	-.230*
		(.172)	(.183)					(.126)
PSHR× TQM		-.084	.077				–/?	-.151*
		(.094)	(.100)					(.079)
PSHR× MEET		-.110	-.140					.004
		(.117)	(.125)					(.098)
PSHR × ***SELFMNG***	+/+	.516**	.463*				+/?	.325*
		(.227)	(.241)					(.172)
N		433	433		660	660		663

See notes to Table 3. Variables for which there were significant effects of work practices (at the ten-percent level or better) in Tables 3 and 4 are highlighted. In columns (1) and (4), for these highlighted variables the signs of the effects on sales/worker (SW) or labor costs/worker (LC) from the cross-section and differenced estimates are indicated if they can be established based on t-statistics exceeding one; a '?' is reported if either the sign cannot be established or there are both positive and negative estimates with t-values exceeding one (otherwise the sign of the coefficient estimate with the t-value exceeding one is reported). In column (7) the same is done, based only on the cross-sectional estimates in column (5) of Tables 3 and 4.

Table 6: Statistical Tests for Effects of Work Practice Bundles on (Log of Sales/Worker - Log Labor Costs/Worker), Manufacturing Plants

	1977-1993 panel				Full 1993 cross section	
	Estimated effect		P-value (null: effect = 0)		Estimated effect	P-value (null: effect = 0)
	1993	Differenced	1993	Differenced	1993	1993
	(1)	(2)	(3)	(4)	(5)	(6)
Specification 14:						
PAYSKIL and SELFMNG						
Evaluated at sample means, conditional on use of both practices	.015	-.009	.86	.91	-.032	.66
Evaluated at 100% implementation	.306	.169	.25	.55	.154	.47
Specification 15:						
PSHR and SELFMNG						
Evaluated at sample means, conditional on use of both practices	.123	.019	.14	.83	.064	.33
Evaluated at 100% implementation	.203	.104	.15	.49	.121	.30

Tests are based on estimates reported in Table 5. Specification numbers refer to that table. For the first entry for each interaction, the estimated effect is the sum of level and interaction effects, evaluated at sample means of work practice variables conditional on these being greater than zero, with means treated as fixed. For example, in the first row, the estimated effect is ($b_{PAYSKIL}$·PAYSKIL' + $b_{SELFMNG}$·SELFMNG' + $b_{PAYSKIL:SELFMNG}$·PAYSKIL'·SELFMNG'), where the b's are the regression estimates, and a prime indicates a conditional sample mean. The p-value is from the Wald statistic of the null hypothesis that this linear combination of the regression coefficients equals zero. In the second row, 100% implementation is assumed.

Appendix Table A1: NES Manufacturing Analysis Samples

	1994 NES (1)	1997 NES (2)
Full NES sample of interviews	3165	4397
Manufacturing subsample of NES	1727	2536
Delete bad/missing data:		
Workforce characteristics	1342	1956
Establishment characteristics and performance	748	1229
Computer use work practices	738	1216
Benchmark, job rotation, and self-managed teams work practices	725	1160
Meeting work practices	719	1142
1994 NES only:		
Pay for skill or knowledge and profit/gain-sharing work practices	703	
Team and cross training and TQM work practices	701	
Obtain labor costs from 1992/1993 LRDs	663	
Match to 1977 LRD	**433**	**660**

Table shows sample sizes available after imposing sample restrictions or deleting observations with bad or missing data.

Appendix Table A2: Descriptive Statistics and Variable Descriptions for Establishment Characteristics, Manufacturing Plants

	1977-1993 panel		1977-1996 panel		
	1977	1993	1977	1996	Variable description
	(1)	(2)	(3)	(4)	(5)
ESTAGE	...	33.28 (22.79)	...	...	In what year did you start operations in this location (1994 NES only)? Constructed variable: ESTAGE = establishment age.
MULTI	.35	.40	.31	.37	Is this the only establishment in your enterprise, or are there others?
MATER	5137 (25153)	18713 (67310)	5014 (29189)	15078 (59376)	During calendar year 1993, what was the cost of goods and services (1000s) used in the production of your 1993 sales (for example, energy costs, raw materials, and intermediate goods)? 1997: What was the total costs of goods and services (1000s) used in the production of your 1996 sales? (... this excludes labor costs from the last question.)
Median	~881	~5000	~749	~3000	
BKCAP	4036 (19395)	10326 (107591)	2146 (12574)	11698 (76347)	At the end of calendar year ____, what was the total book value (1000s) of the fixed capital stock in your establishment?
Median	~618	~2000	~301	~2000	
CAPINV	444 (3583)	935 (5277)	245 (1445)	1520 (11222)	During calendar year 1993, how much did our establishment spend (1000s) on new equipment and machinery? 1997: In 1997, how much ...
Median	~33	~200	~25	~170	
CAPLT1	...	8.23 (14.41)	...	7.41 (11.38)	Approximately what percentage of your machinery or equipment used in production is: 1997: What percent ... Less than 1-year old?
CAP14	...	20.14 (17.46)	...	22.96 (18.32)	Between 1 and 4 years old?
CAP510	...	31.71 (22.18)	...	30.66 (20.93)	Between 5 and 10 years old?
CAPGT10	...	39.93 (30.61)	...	38.98 (30.32)	More than 10 years old?
N	433	433	660	660	

See notes to Tables 1 and 2.

Appendix Table A3: Descriptive Statistics and Variable Descriptions for Workforce Characteristics, Manufacturing Plants

	1977-1993 panel		1977-1996 panel		
	1977	1993	1977	1996	Variable description
	(1)	(2)	(3)	(4)	(5)
UNION	...	.26	...	.21	Is your establishment represented by a union or unions? 1997: Are any of your employees represented by a union or unions?
TOTEMP	140.41	152.56	122.89	154.86	How many employees were on your payroll at the end of 1993? 1997: Please tell me the number of employees on your payroll at the end of 1996. (Exclude ... agency workers ....) Permanent full-time? Permanent part-time? Temporary or seasonal workers on your payroll? How many contract, leased, or temporary agency workers did you have in 1996? Constructed variable: TOTEMP for 1996 = sum.
	(451.54)	(328.80)	(480.88)	(334.69)	
Median	~47	~67	~34	~55	
OCCM	...	10.01	...	11.56	Of your total workforce at this location at the end of 1993, what percentage were: 1997: Of the permanent employees at this location at the end of 1996, how many, or what percent were in each of these categories: Managers/professionals?
		(8.87)		(10.11)	
OCCS	...	8.13	...	6.76	Supervisors?
		(5.17)		(4.43)	
OCCT	...	5.93	...	5.91	Technical/technical support?
		(7.87)		(9.73)	
OCCO	...	12.36	...	12.74	Office/clerical/sales/customer service?
		(11.83)		(10.61)	
OCCP	...	63.57	...	63.03	Production?
		(19.30)		(19.10)	
EDUC	...	12.52	...	12.41	What is the average number of years of completed schooling for the following categories of employees in your establishment? 1997: Excludes "in your establishment." Managers/professionals? Supervisors? Technical/technical support? Office/clerical/sales/customer service? Production? Constructed variable: EDUC = occupation-weighted average.
		(1.01)		(.83)	
PWOMEN	...	29.34	...	27.38	What percentage of full-time employees are: 1997: What percent, or how many, or your permanent employees are: Women?
		(23.89)		(21.18)	
PMIN	...	22.94	...	25.09	Minorities?
		(25.21)		(26.43)	
PLOWTEN	...	10.83	...	12.17	What percentage of your currently employed workers have been with the firm for less than one year? 1997: What percent of, or how many, current permanent employees have been with you for less than one year?
		(11.41)		(12.62)	
N	433	433	660	660	

See notes to Tables 1 and 2.

Appendix Table A4: Selected Cross-Sectional and Panel Estimates of Coefficients of Control Variables, Manufacturing Plants

	Log of Sales/Worker					Log of Labor Costs/Worker					(Log of Sales/Worker - Log Labor Costs/Worker)				
	1977-1993 panel		1977-1996 panel		1993 cross section,	1977-1993 panel		1977-1996 panel		1993 cross section,	1977-1993 panel		1977-1996 panel		1993 cross section,
	1993	Differenced	1996	Differenced	with controls	1993	Differenced	1996	Differenced	with controls	1993	Differenced	1996	Differenced	with controls
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Log(MATER/	.235	.196	.446	.373	.268	.052	.020	.172	.127	.050	.182	.176	.274	.246	.218
TOTEMP)	(.022)	(.020)	(.021)	(.020)	(.018)	(.013)	(.010)	(.029)	(.025)	(.010)	(.021)	(.020)	(.030)	(.027)	(.018)
Log(CAPINV/	.037	.021	.028	.024	.017	.022	.014	.018	.013	.013	.015	.007	.010	.011	.004
TOTEMP)	(.013)	(.011)	(.010)	(.009)	(.010)	(.008)	(.005)	(.014)	(.011)	(.005)	(.013)	(.011)	(.015)	(.012)	(.010)
Log(BKCAP/	.126	.045	.128	.046	.139	.064	.007	.129	.064	.056	.062	.039	-.000	-.017	.083
TOTEMP)	(.021)	(.017)	(.016)	(.013)	(.017)	(.013)	(.009)	(.022)	(.016)	(.010)	(.020)	(.017)	(.023)	(.018)	(.017)
EDUC					.013					.059					-.046
					(.028)					(.016)					(.028)
PWOMEN/100					-.304					-.349					.045
					(.101)					(.056)					(.101)
PMIN/100					-.005					.050					-.055
					(.079)					(.044)					(.078)
PLOWTEN/100					-.505					-.466					-.039
					(.156)					(.087)					(.156)
UNION					.067					.128					-.060
					(.045)					(.025)					(.045)
ESTAGE/100	-.132				-.205	.158				.073	-.290				-.278
	(.091)				(.081)	(.055)				(.045)	(.087)				(.081)
MULTI	.098	.043	.160	.055	.145	.012	.001	.170	-.059	.038	.086	.042	-.011	.114	.107
	(.053)	(.046)	(.044)	(.058)	(.042)	(.032)	(.024)	(.060)	(.072)	(.023)	(.050)	(.047)	(.064)	(.078)	(.042)
Adjusted-R^2	.528	.240	.636	.392	.586	.429	.049	.213	.073	.554	.371	.183	.239	.117	.398
N	433	433	660	660	663	433	433	660	660	663	433	433	660	660	663

Estimates are reported for specifications including the variables in the table, but no work practice variables. Samples correspond to Tables 3-5. Other variables included but not listed in this table include occupation controls (OCCM, OCCS, etc.), age of capital controls (CAP1T1, CAP14, etc.), two-digit industry controls, and a dummy variable for whether the data on labor costs and employment came from the 1992 LRD. See Table 2 and Appendix Tables A2 and A3 for details. These same controls are included in the main tables for work practices. In the full cross section for 1996, an additional year of education boosts both sales/worker and labor costs/worker by about five percent.

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