VI. THE IMPACTS OF ALTERNATIVE DISPUTE RESOLUTION ON WORKPLACE OUTCOMES

Discussion

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It is my pleasure and privilege to offer a few comments on the three excellent papers that have been prepared for this session. The use of alternative dispute resolution (ADR) to resolve workplace disputes has been one of the most important developments in employment relations in the past twenty-five years. There has been a considerable amount of useful and valuable research published on ADR in recent years. In that regard, I want to recommend an article that provides a thorough review of the research on employment dispute resolution written by my fellow panelist and friend Lisa Bingham, which has just been published in the Conflict Resolution Quarterly. Anyone intending to do research on employment ADR who is not already familiar with the literature should start by reading Bingham's excellent article.

But as Bingham points out, despite the vast literature already produced on ADR, "We do not have adequate quantitative, multivariate research on what factors best predict the adoption, design, and function of dispute resolution systems and what designs produce the best outcomes. We cannot answer questions on the impact of these private systems on public justice" (Bingham 2004, 168). I might add that researchers have only begun the difficult task of developing the theories and concepts needed to construct the models required to conduct the quantitative, multivariate research that Bingham calls for. The good news today is that the authors

of the papers we have just heard are all on the frontier of advancing ADR research in exactly the fashion that Bingham and I believe is necessary.

For nearly a decade Alex Colvin has been conducting research on dispute resolution in employment relations, and his research is rightfully considered to be required reading for serious scholars. In his current paper, Colvin emphasizes the variation across U.S. workplaces in employment dispute resolution. He makes very useful distinctions regarding union workplaces and nonunion workplaces that have what he terms "complex nonunion procedures," "simple nonunion procedures," and no apparent dispute resolution procedures at all. The first interesting empirical finding is that in his sample of 475 telecommunication establishments more than half had either simple or complex nonunion dispute resolution procedures, whereas only a little more than one quarter of the establishments had collectively bargained grievance procedures. I believe this finding is consistent with other evidence, some of it collected by Ron Seeber, Dick Fincher, and me, that suggests that a majority of employees in the United States are now covered by employer-promulgated ADR procedures and only a minority by collective bargaining agreements (Lipsky, Seeber, and Fincher 2003, 75-115).

Second, Colvin shows that the type of dispute resolution procedure used in an establishment is related to factors such as the propensity of employees to use such procedures and the proportion of cases won by employees. (In his study, Colvin focuses only on discipline cases.) He finds that the most significant differences are between union and nonunion workplaces, but it is also noteworthy that in nonunion workplaces the employee win rate is more than twice as high in establishments with so-called complex procedures as it is in establishments with so-called simple procedures. Colvin's definition of a complex procedure is somewhat similar to the definition of a conflict management system that Seeber, Fincher, and I used in our recent book (Lipsky, Seeber, and Fincher 2003, 11-19). Thus, Colvin's findings suggest the possibility that in nonunion settings systems provide more protections for employees than simple procedures. If this result can be replicated in other studies, it would be a most important finding. Colvin concludes his paper by suggesting a number of useful approaches that might encourage the adoption of "more substantial, effective grievance procedures" that improve the access that nonunion

employees have to workplace justice.

It is my job to offer some constructive criticism of the papers we have heard, and so I cannot avoid that responsibility. In Colvin's case I have a problem with his terminology: he uses the term "grievance procedure" to cover all types of nonunion ADR procedures. In our research, we discovered that the vast majority of large U.S. corporations now use some form of ADR to resolve employment disputes but most resist using the term "grievance procedure" because they want to avoid using a term they associate with collective bargaining. For example, we discovered that somewhere between 80 and 90 percent of the Fortune 1000 use arbitration or mediation to resolve employment disputes, but only about one-third of these firms had what they termed "grievance procedures" for their nonunion employees (Lipsky, Seeber, and Fincher 2003, 80-82). If one were to focus literally on nonunion grievance procedures, which I do not believe Colvin has done, one could lose sight of the policies and techniques used by a majority of nonunion employers to resolve employment disputes. Nevertheless, as he has in previous research papers, Colvin has pointed researchers in the direction they need to move if we are going to understand how ADR procedures affect workplace disputes.

At the heart of the paper by Ariel Avgar and Hyunji Kwon is an exceptionally important idea: namely, that the rise of what they call "the non-bureaucratic workplace" and particularly the increasing use of so-called high-performance work systems, have transformed the nature of workplace conflict. Other scholars have recognized the significance of the development of high-performance work systems for dispute resolution, but they have not done the conceptual work needed to understand the effects of the so-called new workplace on the nature of conflict (Stone 2001; Lipsky, Seeber, and Fincher 2003, 65-69). I very much like a number of other features of Avgar and Kwon paper. For example, their integration of Pondy's three types of conflict into their model is a valuable idea. Also, their conceptual model has the strength of incorporating testable and refutable hypotheses (or propositions) that can be readily tested with the right data set, and I understand that Avgar and Kwon have already embarked on that particular task.

Avgar and Kwon propose that the shift from bureaucratic to non-bureaucratic

workplaces has resulted in a shift from "conflict over adherence to existing rules to conflict over the definition of rules." As an aging industrial relations scholar, it is probably inevitable that I interpret their proposition in the terms that have traditionally been used in U.S. industrial relations, that is, I believe they are making a distinction in their paper between disputes over interests and disputes over rights. If my interpretation is correct, I read their hypothesis to say that in the new workplace, as compared to the traditional workplace, there will be more interest disputes and fewer rights disputes. So when they turn to the question of promotion and advancement, it seems to me they are proposing that the use of a high-performance work system will be associated with a higher level of interest disputes regarding the rules governing promotion and advancement, compared to the traditional workplace. This is a very clear-cut hypothesis that can be tested with the appropriate data, but frankly I am skeptical that empirical testing will support the validity of the proposition. It is my strong impression that conflict over promotion and advancement, which in unionized settings usually involves the development of seniority systems and rules, is a common occurrence in U.S. collective bargaining, whereas in nonunion settings employers continue to control the rule-making process on promotions and most other matters, and conflict principally exists (in the form of rights disputes) over the application of those rules. Apart from this example of nit-picking, however, the model developed by Avgar and Kwon represents the kind of analytical thinking that we need in the study of employment dispute resolution.

Mahoney, Klaas, and Wheeler have prepared a valuable paper on an important aspect of workplace dispute resolution. They address the question of whether the outcomes of employee termination cases depend on who exactly makes the key decision in such cases. They hope their research will cast light on the tendencies of three types of key decision makers: human resource managers, peer review panels, and line managers. In their research, however, they use students in a graduate management program as surrogates for line managers. They also analyze how the facts and evidence in employee termination cases influence the decisions made in these cases. They do this by having the subjects in their sample make their hypothetical decisions using thirty-two different fictional, if nonetheless realistic, scenarios. They find that generally human resource managers and peer review panelists do not differ significantly in how they decide these cases, but line managers

(i.e., students) are more inclined than either peers or human resource managers to favor the employer in these termination cases. Another interesting finding is that peer review panelists place "more weight on an employee's work record than either HR managers or students."

Fundamentally, the model posited by the authors holds that the outcome of termination cases depends on two factors: (1) the nature of the termination cases and (2) the identity of the decision maker. This is a perfectly reasonable model, and it is clear that their empirical results largely support the validity of the model. Nevertheless, I worry about the possible influence of other factors on the outcomes of termination cases. For example, we know nothing about the experience of either the human resource managers or the peer panelists in the authors' sample, either generally or specifically in the handling of termination cases. In her research, Lisa Bingham has focused our attention on the so-called repeat player effect, that is, on the likelihood that experience with ADR procedures influences decision making. In the paper by Mahoney and his colleagues, it would be reassuring to know that variables representing the experience of the subjects in their sample do not influence decision making. It is a fair guess that the graduate students who serve as surrogates for line managers probably do not have as much real-life experience as the other subjects in the authors' sample. If that is true, the fact that graduate students were much tougher on these fictional employees than either human resource managers or peer panelists might simply reflect the students' lack of appreciation for the complexities of the workplace and not represent an inclination associated with line managers. But, again, the research conducted by Mahoney, Klaas, and Wheeler is exactly the type of research we need at this point in the evolution of ADR scholarship.

In another paper, Ariel Avgar and I suggested that research on employment dispute resolution had traveled through three distinct generations. The first generation of ADR research, dating to the 1970s, focused largely on legal questions and the implications of ADR for our legal system and social justice. The second generation of ADR research, conducted largely by industrial relations and human resource scholars, focused on dispute resolution at the macro-organizational level. The third generation of ADR research, which began to bloom in the 1980s, focused

on dispute resolution at the micro-organizational level. We maintain that there is an emerging generation of ADR researchers who are attempting to integrate societal concerns with macro- and micro-organizational perspectives. The newest generation of researchers is doing a better job of bridging the gap between practice and research and of building and testing empirical models based on sound theory. The papers we have heard at this session represent advances in ADR research that fulfill the hopes and expectations that Avgar and I expressed in our earlier paper.

References

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