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# Estimating the Narcotic Effect of Public Sector Impasse Procedures

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# Estimating the Narcotic Effect of Public Sector Impasse Procedures

## Abstract

This paper first describes in a relatively nontechnical fashion several econometric techniques that the authors believe should be useful to industrial relations researchers. Those techniques are then applied to an analysis of whether public sector impasse procedures create a "narcotic effect," that is, a tendency for the bargaining parties, once they use the procedures, to become increasingly reliant on them in future negotiations. The authors reanalyze data from Thomas Kochan and Joan Baderschneider's study of the impasse experience of police and firefighters under New York State's Taylor Law during the 1968-76 period and find that while a narcotic effect did exist, as Kochan and Baderschneider argued, that relationship lasted only during the early years of the period and was actually reversed in later years. The authors conclude by explaining how such a pattern of results could occur, tracing the implications of their findings for public policy, and suggesting; other research questions that could be analyzed by the econometric methods they have described. (A comment by Kochan and Baderschneider follows this article.)

## Keywords

industrial relations, public sector, econometrics, bargaining procedures, "narcotic effect", fire fighters

## Disciplines

Econometrics | Labor Economics | Labor Relations | Public Economics

## Comments

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# ESTIMATING THE NARCOTIC EFFECT: CHOOSING TECHNIQUES THAT FIT THE PROBLEM

THOMAS A. KOCHAN and JEAN BADERSCHNEIDER\*

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This paper compares the Butler and Ehrenberg analysis of the narcotic effects of impasse procedures, presented in the preceding article, with the purposes, methods, and empirical results of the authors' earlier paper on the same subject. The authors use the differences in the two papers to argue that a need exists to achieve a better blending in industrial relations research of model building and testing, of quantitative and qualitative data, and of simple and complex statistical tests. The differences in results presented in the two papers are shown to reflect, in part, differences in the definition of the problems examined. These authors stress the importance of choosing statistical techniques that fit the theoretical and policy problems of interest to industrial relations researchers and practitioners, and the power gained from mixing qualitative and quantitative methods.

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**A** FUNDAMENTAL problem for researchers in the fields of industrial relations and labor economics is the selection of appropriate statistical techniques with which to analyze a given body of data. Clearly, if results vary according to the techniques used, researchers must choose a method of analysis only after they have developed (1) an appreciation of the nature and definition of the underlying problem being studied, (2) a sound theory that provides the conceptual guidance for deciding which hypotheses to test, and (3) an understanding

of the data available. This requires thorough consideration of the institutional and historical context of the substantive issue being studied and transformation of "raw" information provided by the institutional context into a set of theoretical concepts, testable propositions, and empirical measures appropriate for the particular body of data. Only then can appropriate statistical techniques be fruitfully applied

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\*Thomas Kochan is a professor in the Industrial Relations Section, Sloan School of Management, Massachusetts Institute of Technology and Jean Baderschneider is an assistant professor in the School of Business, University of Kansas. They would like to thank the following individuals for their helpful comments on an earlier draft of this paper: Katharine Abraham, David Bloom, Henry Farber, Harry Katz, Robert McKersie, and Joseph Shedd.

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Richard Butler and Ronald Ehrenberg have had the opportunity to read and comment on earlier versions of the Kochan-Baderschneider reply. While they still have some substantive disagreements with Kochan and Baderschneider, in particular over the latter's interpretation of what is happening in the fixed-effects model, they feel that devoting further space to these disagreements would be unproductive and would detract from the fact that the two sets of authors do agree on many points. They believe that, read together, the two papers complement each other and suggest important improvements in research methodology in industrial relations.—EDITOR

and their results realistically interpreted. The preceding paper by Richard J. Butler and Ronald G. Ehrenberg (B & E) illustrates the pitfalls of using a set of advanced statistical techniques without adequate consideration of the substantive problem, the theoretical model, or the data used in their analysis.

B & E argue that a Bernoulli runs test and a regression model that attempts to account for both "observed and unobserved heterogeneity" are the most appropriate techniques for analyzing whether or not a narcotic effect is present in the patterns of usage of impasse procedures. They argue further that their application of these techniques to data from New York State alters some of the conclusions reached in our paper published in this *Review* in 1978.<sup>1</sup> In our discussion we will argue that (1) a properly specified Bernoulli runs test may be better than the conditional probability tests we used for describing the patterns of dependence on impasse procedures (although we were not able to replicate their results), and (2) their regression tests for unobserved heterogeneity lack a sound theoretical rationale and produce misleading conclusions. On the other hand, the B & E paper makes a major contribution by highlighting the theoretical ambiguity of the concept that we and others have traditionally referred to as the narcotic effect. B & E also show that a fixed-effects model can be a useful supplement to ordinary least squares regression techniques and that it should be used when the assumptions of the test fit the substantive industrial relations problem. Unfortunately, they chose a poor example and set of data to make their point.

We hope that nothing contained in this discussion discourages specialists in industrial relations, economics, or the behavioral sciences from applying the advances of model building and statistical testing to important industrial relations problems. Instead, we hope that this discussion serves to stimulate critical evalua-

tion and selective use of advanced techniques. As we have argued elsewhere, progress on problems in our field can best be served by mixing quantitative and qualitative methods to develop and test theories that are well grounded in an understanding of the essence of the problem at hand.<sup>2</sup>

### The Purpose of Our Original Paper

Before taking up the empirical and methodological issues raised by B & E, we need to emphasize that the purpose of the original paper was to present and test a theory of the determinants of impasses and to use that theory to test for the effects of the change in the Taylor Law from factfinding to arbitration on the rate of impasses that occurred under that law. This was clearly stated in our paper:

Since impasses can be caused by a wide array of factors in addition to the nature of the impasse procedure, these other causes must be controlled before the effects of the procedures can be estimated. A theory that identifies these other causal forces must therefore be developed, and since the effects of a procedure may change over time, any specific estimates of the effects of a procedure must be placed in their historical context. This paper will present a theory of impasses in public employee bargaining and use it to estimate the effects of a change in impasse procedures for police and firefighters in the state of New York.<sup>3</sup>

We were concerned with assessing the narcotic effect only in the first part of our paper, where we attempted to show why it was important to develop a theory of impasses in order to place the empirical analysis of the change in the law in its proper theoretical and historical context.<sup>4</sup> The

<sup>2</sup>For a summary of the overall project, its results, and its policy recommendations, see Thomas A. Kochan, Mordehai Mironi, Ronald G. Ehrenberg, Jean Baderschneider, and Todd Jick, *Dispute Resolution Under Fact-Finding and Arbitration: An Empirical Analysis*, (New York: The American Arbitration Association, 1979), pp. 177–82.

<sup>3</sup>Kochan and Baderschneider, "Dependence on Impasse Procedures," p. 432.

<sup>4</sup>In fact, the original title of the manuscript we submitted to the *Review* was: "Determinants of Collective Bargaining Impasses: Theoretical and Policy Analysis." The editor and a referee suggested we change the title to capture the issue of the narcotic effect. We

<sup>1</sup>Thomas A. Kochan and Jean Baderschneider, "Dependence on Impasse Procedures: Police and Firefighters in New York State," *Industrial and Labor Relations Review*, Vol. 31, No. 4 (July 1978), pp. 431–49.

regression analysis that followed did not incorporate a test for the narcotic effect because we do not see the narcotic effect as a useful theoretical explanation for *why* some units are more dependent on procedures than others.

The purpose of our paper and our use of the term narcotic effect are repeated here to point out the fundamental difference between the way we used this term and the way it is used in the B & E paper. In contrast to our broad use of the term for describing the overall trends in impasse usage across units and over several rounds of bargaining, they interpret the term in a more strict fashion and seek to sort out a "true narcotic effect"—that increase in the probability of going to impasse that occurs because a unit went to impasse in the past, net of the other factors (such as heterogeneity) that influence the probability of an impasse. We will show that the difference in definition and use of this term explains part, but not all, of the differences in the results obtained and the conclusions drawn from our respective analyses. We should also note that by introducing this term in our paper as it has traditionally been defined in the literature, we may have perpetuated its conceptual ambiguity and thereby led B & E to seek a more precise operational definition and statistical test for its presence.

#### Differences in Empirical Analyses and Results

Our empirical analysis started with a general overview of the impasse histories of police and firefighters in New York State. The descriptive data clearly showed that the rate of usage of the impasse procedure had increased over each successive round of bargaining under the Taylor Law between 1968 and 1975. Specifically, the percentages of units going to impasse across the first five rounds were, in order, 41, 47, 59, 61, and 65 percent. We also reported that the largest cities were the heaviest users. For example, the largest city in our sample, Buffalo, went

to impasse 100 percent of the time it bargained with police and firefighters, and the five largest cities in the sample—Buffalo, Yonkers, Rochester, Syracuse, and Albany—together went to impasse in 90 percent of their police and fire negotiations during this period and to factfinding or beyond in 70 percent. In contrast, 15 percent of the police units and 8 percent of the firefighter units in the state never used the procedures over the rounds of bargaining during this period.

It was that overall pattern of usage that we sought to describe more precisely with the conditional probability tests presented in our paper. Any redefinition of the narcotic effect that requires the exclusion of the information drawn from those units that always or never went to impasse, or limits the information drawn from those units, produces a misleading description of this impasse history. Yet this is essentially what happens in the B & E Bernoulli and regression tests.

#### The B & E Empirical Tests

B & E use two sets of empirical tests to assess whether a narcotic effect was present: a Bernoulli runs test and a fixed effects regression model that controls for both "observed" heterogeneity (that portion of the variation in the probability of using a procedure that is accounted for by the theory and the variables measured in our model) and "unobserved" heterogeneity that is constant over the rounds of bargaining examined and unique to each bargaining relationship included in the analysis. The latter test is presented as *the* most appropriate technique in this context, and therefore we will concentrate most of our discussion on that test and will only touch briefly on the Bernoulli test.

*The Bernoulli runs test.* To test for a narcotic effect we applied a conditional probability test to *the first five* rounds of bargaining. B & E, in contrast, applied a runs test to two sets of data: the first three and then the last three rounds of bargaining included in our impasse history profiles. Their last three rounds generally span the time period from 1970 to 1974 and include some observations under factfinding and

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compromised by using the term "Dependence on Impasse Procedures."

Table. Replication of the Runs Test with Complete Set of Observations.

(1)	(2)		(3)	(4)	(5)	(6)	
	B & E	K & B				B & E	K & B
000	25	20					
001	13	17	13.33	1	3	2	1
010	13	11	13.33	2	2	2	2
100	14	11	13.33	3	1	1	2
011	7	10	13.67	1	3	3	3
101	11	19	13.67	2	2	2	1
110	23	11	13.67	3	1	1	2
111	16	37					

where:

- (1) Pattern (010, for example, indicates that impasse occurred only in round 2.)
- (2) Actual number of times that pattern occurred using B & E subsample and K & B sample.
- (3) Expected number of times that pattern would occur if a simple Bernoulli process were present.
- (4) Expected rank in the group if a positive narcotic effect existed.
- (5) Expected rank in the group if a negative narcotic effect existed.
- (6) Actual rank in the group for B & E subsample and K & B sample.

some under arbitration.<sup>5</sup> Their tests showed that a positive narcotic effect existed in the first three rounds and a negative narcotic effect was present in the latter three. From this they conclude that their results contradict ours. However, examine carefully what we reported in our article based on our conditional probability tests:

The results shown in Table 2 indicate that the probability of going to impasse increased in subsequent rounds of bargaining for those units that had previously gone to impasse. Only in the second and third rounds of negotiations, however, was the difference in the probability of going to impasse significantly greater for those units that

went to impasse in prior rounds . . . . By the fourth and fifth rounds, the increasing usage of the procedures was more a general phenomenon than a function of the specific bargaining unit's impasse experience.<sup>6</sup>

Thus, our results from the conditional probability test are similar to B & E's results using the runs test for the first three rounds of bargaining. A positive and significant narcotic effect existed in rounds two and three.

There is, however, a difference in the findings for the three later rounds. We stated that the rate of usage continued to be higher for earlier users but that the difference was not significant because new units were also beginning to use the procedure. B & E state that a *negative* narcotic effect was present in the later rounds. A major reason for this conflict in findings is the difference between the studies in their definition of the narcotic effect: B & E exclude from their runs test all units that either used the procedures each time or never used the procedure, whereas we include these units in our conditional probability tests.

A second reason for the difference in re-

<sup>5</sup>The impasse history profiles should not be confused with the data used in our regressions to test for the effects of the change in the law nor in the B & E regressions used to test for a narcotic effect. These historical profiles were collected from the records of the Public Employment Relations Board and verified in our *first* set of field interviews with the parties. They do not attempt to mirror the data on the stage of settlement for the last round of bargaining under fact-finding and the first round under arbitration since not all of the units had negotiated a first contract under arbitration when the profiles were collected and some of the units did not complete their first negotiations under arbitration during the time period available to our study. Thus, the profiles contain a slightly larger number of bargaining units than were available for our regressions under arbitration.

<sup>6</sup>Kochan and Baderschneider, "Dependence on Impasse Procedures," p. 438.

sults is that we also differ over other decision rules for defining the sample used in the runs tests. The table shows the results of our effort to replicate B & E's runs tests on the cases included in our original analysis. Our results, using their methodology, show a larger number of impasses in the "last" round and do not display a pattern that is consistent with what B & E argue would be a negative narcotic effect. Our replication of their test, and our original conditional probability tests, used all of the units in the sample for which impasse history data were collected and verified during our first field interviews with the parties. B & E excluded several of these units from their runs test, some because of the difference in their definition of the narcotic effect and others for other reasons we do not find persuasive.<sup>7</sup>

Regardless of these differences in definition and empirical results, two things should be noted here. First, a runs test is superior to the conditional probability test for describing these data because it provides more detailed information concerning the patterns of impasses and forces the researcher to specify more precisely what type of pattern would indicate support for a positive, negative, or no narcotic effect. In this sense B & E's test is an improvement over ours, even though we believe the constant users and settlers should *not* be excluded and the complete set of observations should be used. Second, neither their test nor ours explains *why* these patterns occur because the causes of variations in dependence

on the procedures are uncontrolled. To provide such control, one needs a theory of impasses and some type of multivariate analysis.

*The B & E regression tests.* As stated in our first paper, the purposes of our regression equations were: (1) to test the theoretical propositions advanced for explaining variations in the probability of an impasse occurring and (2) to test for the effects of the change in the law from factfinding to arbitration on the probability of an impasse. Therefore, when B & E state that "K & B did *not* actually test for the presence of narcotic effects with the regression estimates they presented in their paper," their comment is correct but pointless, since this was not the purpose of these estimates. Therefore, our discussion will focus on the interesting and important research question their analysis raises, namely, how well does the fixed effects model fit this problem and these data?

The fixed effects model attempts to test whether our theoretical model omitted some characteristic that is fixed over the time period of the last three rounds of bargaining under factfinding and the first round of bargaining under arbitration and that is correlated with one or more of the variables included in our model. B & E do not offer any a priori hypothesis for what this phantom effect might be or the reason that this type of effect is more theoretically plausible than some other type of omitted characteristic (such as one that is not fixed over time but is also correlated with an included variable). We will not, however, engage in a debate over whether or not such a variable was omitted from our model. Undoubtedly there are a number of determinants of impasses that our theory missed and there are others that we captured in our theory but that are measured imperfectly.

No social science model ever claims to capture or perfectly measure *all* causal variables. The relevant question, then, is whether one should test for a fixed effect in the absence of a theoretical rationale as a check on the completeness of a model that offered an explicit rationale for the variables included. There is no "correct" answer to this question since one's choice depends

<sup>7</sup>B & E deleted cases from the sample analyzed in our original analysis if any one of the following occurred: (1) no contract was negotiated in 1975 or 1976 because the parties were in the midst of a multiyear agreement or had not settled their contract by June 1976 when data collection was terminated, (2) no contract was negotiated in 1968 or 1969, (3) the parties signed a three-year contract during the 1968–76 period so that there were two consecutive years during which no bargaining occurred, or (4) a contract reopener rather than a full successor agreement was negotiated. The contract reopeners were excluded because the code book we gave to B & E did not correctly show a code for reopeners; they deleted five cases because of this error. We believe the first three rules are also inappropriate, since they eliminated a number of units with negotiation and impasse histories that are as relevant to the analysis as the histories of those units included.

largely on how much confidence one has in the theoretical model and how much additional potential for error is added to the analysis by conducting the more complex statistical test. In this case, for example, testing for the fixed effect requires B & E to add lagged measures of the dependent variable (impasse experience) to the right-hand side of the equation and, in some cases, as many as fifty dummy variables (one for each city) to their equations. Obviously, these are not costless procedures, for they have a potential for muddling the effects of the variables included in the equation originally for their theoretical and policy relevance. Our preference, given the limitations of the sample size and of the measures available, would be to demand a stronger theoretical rationale than B & E provide for complicating the equation in this way. Clearly, however, this is a judgement over which researchers can reasonably disagree. Thus, let us examine more specifically what happens when this model is tested on these data.

B & E used two techniques to attempt to disentangle the narcotic effect from the fixed effect. The first is a complex instrumental variable model using first differences. The second uses the more conventional dummy variable technique: essentially a dummy variable is included for each city to capture the unique within-city fixed effects. Both procedures produce negative and significant coefficients on the lagged impasse variables that are included to capture whatever narcotic effect is left over after the fixed effects are partialled out. This negative result surprises us, given our knowledge of the general rise in impasse rates that occurred across these rounds and the knowledge that the heaviest users, such as Buffalo and Syracuse, tended to keep going to impasse over these rounds of bargaining.<sup>8</sup>

<sup>8</sup>Note that based on the regression results discussed in our paper, we would not have been surprised by an insignificant effect for the lagged impasse variables, since we also found that new units began to go to impasse during this time period. Also, when we added a measure of the percentage of times the units went to impasse in previous rounds, the coefficient was generally not significant. We chose not to dwell heavily on this finding, however, since we were concerned about the potential confusion that could result in

The key to explaining that finding by B & E is again to be found in the lack of effect that the constant users and constant settlers have on their coefficients. In the case of constant use across these rounds,  $y_{t-1}$ ,  $y_{t-2}$ , and  $y_{t-3}$  all take on the value of one. Similarly, in the case of constant settlement without impasse across these rounds,  $y_{t-1}$ ,  $y_{t-2}$ , and  $y_{t-3}$  all take on the value of zero. *In either case the first-difference terms always take a value of zero.* Therefore, the lagged-impasse variables that B & E use to test for the narcotic effect are perfectly collinear with the fixed-effects terms for these observations. Thus, there is no way to determine if the effects of the constant users or constant settlers are captured in the previous-impasse variables or in the fixed-effects term. At best, any information gained from the constant users and constant settlers is spurious.<sup>9</sup>

In short, one cannot conclude from B & E's equations that a negative narcotic effect was present across the entire range of units in the sample. The fact that they get the same results when they delete constant users and settlers from their regressions reinforces our view that their equations are driven not by all the units but only by those that changed their behavior across the rounds examined.

The bottom line of all this is that part of B & E's evidence for the narcotic effect as traditionally defined, and as we used it in the first part of our original paper, is measured by what B & E call fixed effects due to unobserved heterogeneity. What all this suggests

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adding measures of lagged dependent variables to the equation.

<sup>9</sup>Along with the collinearity problem there is another technical reason why no useful information is gained in these regressions from the constant users or the constant settlers. The preferred specification for this type of zero-one dependent variable would be a logit model rather than an ordinary least squares model. If the logit model had been used in the B & E equation, the coefficients on those observations that are perfectly collinear with both the dependent variable and the lagged impasse variables would attempt to go to infinity and the program would not work. Thus, whatever results were obtained from these observations in the ordinary least squares equation would not have been obtainable had the more technically preferred model been used.

is that the difference between their findings and ours again comes down to a definition of narcotic effect. A broad definition, and the one that generally fits the popular usage of the term in collective bargaining, is "repeated or heavy reliance on an impasse procedure." Those using this definition make no effort to disentangle variations in the initial probability of using the procedure from variations in the probability of using the procedure later. A stricter and narrower definition of the narcotic effect, and one that is tested implicitly in the B & E regressions, is "the portion of a unit's reliance on impasse procedures that is solely due to the unit's having once used the procedure in the past." That definition excludes that portion of a unit's reliance resulting from variations in the "natural" tendency of bargaining units to go to impasse regardless of the nature of the procedure. That narrow definition also implies that it may be incorrect to view Buffalo, for example, as addicted to impasse procedures, even though Buffalo used the procedures each time, because the city and its unions may simply be impasse-prone due to "unobserved heterogeneity." In contrast, we would (and did) explain the fact that Buffalo is a heavy user by noting that Buffalo scores high on most of the critical variables contained in our model of the causes of impasses.

While the B & E definition may be more technically precise, given the way economists analyze the problem of "state dependence versus heterogeneity," its use changes the substantive and theoretical problem of greatest concern to us and to policy makers. Thus, in the course of their analysis, B & E inadvertently changed the substantive problem in a way that loses sight of the underlying theoretical and policy problem that we addressed. They end up addressing the question, "Does going to impasse per se increase the probability of usage of the procedure in the future?" That question is also of potential interest to policy makers, but attempting to answer it as B & E do produces misleading conclusions because those observations of greatest concern to policy makers (those units that were repeated users) are excluded from their analysis. By discount-

ing the effects of observations at both tails of the distribution of responses on the dependent variable, B & E's technique also limits the power of the regression model to test the propositions included in the theory for explaining the causes of variations in dependence on the procedure. Finally, this data restriction makes it inappropriate for B & E to extrapolate the results obtained in their regressions to the entire range of observations in this sample or in future samples, as they do in their concluding section when they discuss potential theoretical arguments for the reasons a negative narcotic effect might occur.

### Implications for Research

Now that we have explained why the two studies obtained different results and have shown that the B & E results *do not* alter any of our substantive findings or conclusions, let us return to the larger question of which approach offers a better model for industrial relations research. Our answer is that a combination of both approaches is needed. B & E demonstrate the potential applicability of the fixed effects regression model and thereby show that under the correct conditions this model can and should be used to test for the robustness of ordinary least squares regression results in studies that employ a combination of time-series and cross-sectional data. They further illustrate how the term "narcotic effect" is misleading as a *theoretical* explanation of an observed heavy pattern of dependence on impasse procedures. We believe, however, that the fundamental question of why some units use procedures more than others cannot be explained outside of an explicit theory that states and then measures the underlying causal forces. To rely on an equation that "explains" current use of impasse procedures with measures of previous use plus a set of dummy variables for each city in the sample begs the question and muddles the impact of the theoretical and policy-relevant variables already included in our model.<sup>10</sup>

<sup>10</sup>Examine, for example, how the coefficients on the explanatory variables contained in our model and used as the x variables in the B & E regressions change as

Finally, we believe that much of the confusion introduced by B & E would have been avoided if they had given more weight to the simple descriptive trends in our data and had tested their quantitative results against the larger set of quantitative and qualitative data obtained in our field interviews with the parties involved in these negotiations. As we noted, the results reported in the original paper were only one part of a much broader project.<sup>11</sup> The data were obtained through personal interviews with the labor, management, and neutral representatives involved in these negotiations. We followed the evolution of the change in the law for a two and one half year period and used retrospective interviews and impasse history data to place those current experiences in perspective.<sup>12</sup> The quantitative results reported in the paper and in other contexts were constantly subjected, first, to our own critiques of whether they were consistent

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they experiment with different models that include lagged impasse usage, instrumental variables, first-difference terms, and fixed or random effects terms. Note also that B & E pay almost no attention to these changes in discussing their results. Their primary interest is not in the adequacy or stability of the theoretical explanation but in the phantom effects that are omitted from the theory.

<sup>11</sup>Kochan, Mironi, Ehrenberg, Baderschneider, and Jick, *Dispute Resolution Under Fact-Finding and Arbitration: An Empirical Analysis*.

<sup>12</sup>The time period for the study was dictated by our commitment to the Public Employment Relations Board, and to the labor and management representatives who assisted in the data collection, to prepare a report summarizing our findings, conclusions, and recommendations in time for them to use (or abuse) the results in the legislative debate over whether or not to extend, modify, or repeal the arbitration provisions in the law that were due to expire in 1977.

with the qualitative data and "insights" obtained in the interviews, second, to comparisons with data independently collected by the Public Employment Relations Board, and, finally, to the critical eyes of the parties themselves.<sup>13</sup> Had our regression analyses produced the surprising finding that a strong "negative narcotic effect" existed over this time period in the cities we visited, we would have had a problem of reconciling that result with our interview data and with the basic descriptive statistics collected earlier in the project.

Not all industrial relations research problems can be addressed through a mix of theory construction, direct data collection from the field, qualitative and quantitative analysis, and direct feedback to the parties. Yet we are fearful that in the absence of this type of close interaction across problem and setting, theory, data, analysis, and reporting, researchers can easily focus on tangents that are of limited relevance to either practical events or public policy development and end up choosing statistical techniques that lose sight of the key theoretical and policy problems. If the latter strategy prevails, we will get less of the scientific and public policy advancement that both we and Professors Butler and Ehrenberg are in search of and more of what someone reading this reanalysis of our data described as "mystical empiricism."

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<sup>13</sup>The Public Employment Relations Board held a symposium to discuss the results of the research and to hear the views of labor and management and state officials on the arbitration amendment. See *Symposium on Police and Firefighter Arbitration in New York State* (Albany, New York: The Public Employment Relations Board, 1977).