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UNION REPRESENTATION ELECTIONS:
LAW AND REALITY. By Julius G. Getman,^{*}
Stephen B. Goldberg,^{**} and Jeanne B. Herman.^{***}
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BOOK REVIEWS

UNION REPRESENTATION ELECTIONS: LAW AND REALITY. By Julius G. Getman,* Stephen B. Goldberg,** and Jeanne B. Herman.*** New York: Russell Sage Foundation. 1976. Pp. xvii, 218. \$7.50.

Reviewed by Donald L. Martin†

It is truly rare when independent scholarship in the social sciences has a dramatic and rapid effect on the formation of public policy. Yet it is difficult to avoid the suspicion that the recent decision by the National Labor Relations Board (Board) to reverse its paternalistic policies towards employees involved in union representation elections was strongly influenced by the empirical efforts of Getman, Goldberg, and Herman.

Between 1962 and the spring of 1977 the Board followed a policy of protecting employees from campaign rhetoric, including both promises of benefits and threats of reprisals, that might prevent employees from forming an uninhibited opinion in deciding for or against union representation. The Board sought to maintain "laboratory conditions"¹ in order to conduct experiments in industrial democracy. In the interest of maintaining election conditions "as nearly ideal as possible,"² the Board assumed the power to issue cease and desist orders concerning unacceptable pre-election campaign activities, to set aside elections, and even to order an offending employer to recognize a union that had *lost* its representation election. This power derives from the Board's assumed ability to determine which campaigning tactics are likely to interfere with employee freedom of choice.

It is particularly remarkable that the Board has never attempted to examine empirically whether any of the forbidden campaign conduct had a significant effect on the outcome of representation elections. In fact, the Board had refused even to consider empirical evidence whenever attempts were made to introduce it. Instead, it was content to operate on a priori conclusions based on a set of

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1. J. GETMAN, S. GOLDBERG & J. HERMAN, *UNION REPRESENTATION ELECTIONS: LAW & REALITY* 7 (1976).

2. *Id.* at 2.

untested explicit and implicit assumptions. That is, the Board concerned itself only with whether it was *reasonable* to conclude that the conduct in question *tended* to prevent the free formation of political choice by employees. The authors correctly see this as unsound methodology in that the questions with which the Board deals cannot be answered from reasoned assumptions alone. They are empirical questions, and in *UNION REPRESENTATION ELECTIONS: LAW AND REALITY* the authors take on the task of formulating and testing them against the evidence.

The book contains seven chapters and six appendices organized around surveys of employee attitudes, knowledge, and perceptions about the prospect of unionization at their respective places of employment, and how, if at all, these were affected by employer and union campaign activities.

Two surveys or interviews of employees were conducted in each of the thirty-one representation elections sampled by the authors. The first, or *wave I*, was conducted after a representation election had been directed by the Board but before an election campaign had begun. The purpose of *wave I* was to record the predisposition of employees toward union representation. The second survey, or *wave II*, of the same employees was conducted immediately after an election in order to record employee impressions of campaign content, as well as how and why they voted as they did. The data from *wave II* were then matched against the authors' election "predictions" derived from employee pre-election predispositions to unionization. Particular care was taken to select a sample of elections that promised to contain unlawful campaign activities of the kind that the Board had previously found sufficient to set aside an election or to direct that an employer recognize, as the exclusive bargaining agent, a union that had actually lost the representation election. If the outcome of the vote in the employee sample could be predicted from voting intentions as recorded in *wave I*, the Board's assumptions about the effect of campaign content, especially of unlawful conduct, on the voting behavior of employees would be highly suspect, and its decisions disallowing election outcomes on the ground that they resulted from biases in the campaign would be unjustified.

The authors found that employee voting is significantly correlated with measures of predisposition towards unionization for 81% of the voters interviewed. This means that 19% of those interviewed did not vote in accordance with their revealed intentions as surveyed prior to the election. This group is certainly large enough to have an effect on the outcome of an election, and in nine of thirty-one elections these "switchers" were decisive. Were they influenced by the campaign, and, if so, were they influenced by unlawful cam-

paign tactics? Given its commitment to "ideal election conditions,"³ an affirmative answer might justify the Board's control over campaign speech and actions even though it could expect that only a minority of employees would be influenced by campaign content.

The difficulty lies in determining whether "switchers" were truly influenced by campaign content. The authors employ a test that is not altogether satisfying. They assume that a voter's ability to recall campaign issues after an election is an index of his familiarity with the issues covered. The authors contend that voters who switched from their intended choice because of campaign content were perhaps more familiar with the issues at stake than voters who did not switch, since based on the foregoing assumption, the latter registered very little familiarity with the substance of the campaign. More particularly, the authors determined that those voters who revealed in pre-campaign interviews an intent to choose the company, but who switched instead and voted for the union, registered relatively greater familiarity with campaign issues than did voters whose intent to choose the company was not altered. On the other hand, the authors found that those voters who switched to the company side were no more familiar with campaign issues than those who pursued their original intent to vote union. Since a relatively larger number of switchers voted for the company, this may suggest that the recall index of campaign familiarity which was employed by the authors may not be a reliable indicator of campaign familiarity. This unhappy interpretation, however, is ignored by the authors. They argue instead that switchers to the company were impressed not by specific campaign content, but rather by the fact that employers made some campaign effort. The authors apparently believe that merely reminding employees that the company has good working conditions and competitive salaries is sufficient to retrieve the sentiments of workers who intend to vote for a change, that is, to vote for the union. They conclude that switching to the union is caused by campaign content but switching to the company is not. This asymmetry in criteria to predict switching is less than convincing. It is certainly no more persuasive than the alternative interpretation that one's ability to recall campaign issues after the election is no measure of the impact which the discussion of those issues may have had on employees at the time of voting.

Switching may be caused by a fear of reprisals. Employees intending to vote union may be "persuaded" to switch in the face of reprisals by a firm that may lay off more workers, close down

3. *Id.* at 4.

entirely, provoke more strikes, or otherwise harass union sympathizers. The authors, however, could find no significant difference between switchers and nonswitchers in their perception of unlawful campaign tactics that might cause the former to behave differently from the latter. This result is a disturbing feature of their analysis. The distinct impression is made that employee voting behavior, including the voting behavior of switchers, is not influenced to any significant degree by the campaign issues and content presented by companies and unions. Why then would these adversaries devote resources to persuasion, misrepresentation, threats, and reprisals? This question is not explicitly addressed by the authors, and it should have been. The union may mount several costly campaigns. Firms may face several costly representation elections. If little influence is to be gained by campaigning, why do it? It would be cheaper to hold elections without campaigning. It is more likely that the authors have not been able to measure effectively the effects of campaign content on switching.

The only Board assumption that was left standing after an intensive empirical examination by the authors was that which postulates a relationship between the demand to be represented by a union, as evidenced by signed employee authorization cards, and the voting behavior of card signers. The authors report that 72% of card signers voted for union representation. This is a relatively important finding since many critics believe that card signing may occur under duress and may not be an indication of employee desire for union representation.

The authors recommend, on the basis of what they believe to be impressive evidence, that several changes be made in Board policy with respect to representation elections. Since voting behavior in elections where the Board believes there have been unlawful campaign tactics is not significantly different from voting behavior under "laboratory conditions" as defined by the Board, the authors see no reason for the continued regulation of campaign content, the setting aside of elections, or bargaining orders directing a firm to recognize a union despite the election outcome. Much of what the Board has recently changed in its rules for conducting representation elections is consistent with the conclusions of this book.

In this review I have studiously avoided trifling attacks on technical statistical issues. Yet it is impossible to ignore what must be judged sloppy and misleading statistical exposition. Throughout the book, the authors are concerned with the relationship between voting predispositions, campaign familiarity, voter perception of unlawful campaign tactics, and actual voting behavior. To test these relationships, individual and multiple correlations were run be-

tween actual voting behavior and these variables. The results of these tests were then reported and certain conclusions were established. For example, in chapter three the results of a multiple linear regression revealed a significant relationship between employee pre-campaign attitudes toward job and union, and employee choice in representation elections. However, from Table 3-4,⁴ it is difficult to determine whether this significant relationship has causal implications. The authors report an *adjusted multiple correlation coefficient* of .67. Does this mean that 67% of the observed variation in voting behavior can be *explained* or *predicted* by differences in employee attitudes toward job and union, or does it mean that the three variables are *correlated*, but not necessarily in a causal way, 67% of the time?

The important distinction between a multiple coefficient of determination, conventionally symbolized by r^2 (or what the authors refer to as the "power of the correlation"⁵) and the multiple correlation coefficient itself, the square root of that "power" statistic, is left unclear throughout the entire text. As a consequence the reader is never sure if the authors' claim that voting predisposition variables have "high predictive power" is correct or not. In reference to the contents of Table 3-4, the authors write: "[T]he regression equation may be used to predict how each individual employee will vote."⁶ In the next paragraph they state: "Overall we could correctly predict 81% of the employees' votes from their pre-campaign attitudes."⁷ But when reference is made to Table 3-4 this 81% figure is not apparent. Nor is it derived from any *explicit* calculation. How can one not be confused by such exposition?

As it happens, 81% is approximately the square root of 67%, the adjusted multiple correlation coefficient found in Table 3-4. If this is a correct interpretation, it means that 81% is a measure of *correlation* not *prediction* (.67 is a measure of prediction or the *power of correlation*, i.e., r^2). The confusion between r and r^2 as measures of prediction permeates the entire manuscript. The authors state: "Intent was the single best predictor of vote."⁸ They then cite a correlation coefficient of $r = .73$. The predictor statistic is the square of this correlation statistic. Thus, through "intent," they were able to explain less than 50% of the variation in vote, not 73%. The latter statistic simply means that intent and actual vote

4. *Id.* at 61.

5. *Id.* at 55 n. 5.

6. *Id.* at 61.

7. *Id.* at 62.

8. *Id.* at 64.

moved in the same direction 73% of the time, or that 73% of the employees interviewed voted as they intended. To see the difference between prediction and correlation one need only imagine two variables that are highly correlated with a third variable that is not stated explicitly. As this third variable moves, the other two variables might move with it. Thus, they are correlated. This third variable may be *causally* related to the first two. If the third variable were held constant, variations in one of the explicitly stated variables may have no effect on the other explicitly stated variable, so that one may not *predict* movements in one from changes in the other. Thus, correlation and prediction statistics give very different information, and it is extremely dangerous to confuse them in presentation.

Moreover, the authors fail to inform us of the statistical significance of the coefficients associated with the independent variables employed in their multiple regression. Consequently, we have no way of knowing if attitudes toward jobs or unions are independent sources of voting outcomes. This is a very serious omission.

The failure of the authors to keep these distinctions straight has severely damaged an otherwise commendable effort to bring some evidence to bear on important policy questions. Nevertheless, for the student of labor relations and labor law, it is a book worth reading, if only as a demonstration that esoteric policy questions in law and industrial relations are amenable to empirical analysis as conducted in the social sciences.

TAX DESK BOOK FOR THE CLOSELY HELD CORPORATION. By Albert M. Lehrman, Professor of Law, Texas Southern University, Houston, Texas. Institute for Business Planning, Englewood Cliffs, New Jersey. 1978. Pp. xviii, 408. \$29.95.

*Reviewed by Hugh L. Sowards**

Even the most casual observer cannot fail to notice the staggering complexity of current tax law. The exhaustive number of decisions and IRS rulings and the ever-changing statutes, rules and regulations serve only as a starting point in the search for a satisfactory solution. Applicability and interpretation may present equally formidable and time-consuming obstacles, especially in the fast-moving area of corporate tax law. Frequently, too, the explanatory

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