

# Voice, Control, and Procedural Justice: Instrumental and Noninstrumental Concerns in Fairness Judgments

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One hundred seventy-nine undergraduate Ss took part in a study of the effects of instrumental and noninstrumental participation on distributive and procedural fairness judgments. In a goal-setting procedure, Ss were allowed voice before the goal was set, after the goal was set, or not at all. Ss received information relevant to the task, irrelevant information, or no information. Both pre- and postdecision voice led to higher fairness judgments than no voice, with predecision voice leading to higher fairness judgments than postdecision voice. Relevant information also increased perceived fairness. Mediation analyses showed that perceptions of control account for some, but not all, of the voice-based enhancement of procedural justice. The results show that both instrumental and noninstrumental concerns are involved in voice effects.

Since the earliest studies of the psychology of procedural justice (Thibaut & Walker, 1975; Walker, LaTour, Lind, & Thibaut, 1974), it has been known that the opportunity to present information relevant to a decision enhances judgments of the fairness of the decision-making procedure. This finding, termed the "process control effect" by Thibaut and Walker (1978) and the "voice effect" by Folger, is probably the best-documented phenomenon in procedural justice research.<sup>1</sup> Numerous studies, conducted in natural settings as well as in the laboratory, have made it clear that the voice effect enhances procedural fairness even when the individual making the fairness judgment has no direct control over the decision itself (e.g., Folger, 1977; Kanfer, Sawyer, Earley, & Lind, 1987; LaTour, 1978; Lind, Kurtz, Musante, Walker, & Thibaut, 1980; Lind, Lissak, & Conlon, 1983; Tyler, 1987; Tyler, Rasinski, & Spodick, 1985): Apparently, as long as there is an opportunity to express one's views and opinions before the decision is made, procedural fairness is enhanced.

Given the reliability and strength of the voice effect, it is not surprising that virtually every theory and conceptual analysis of procedural justice processes has attempted to explain the psychological processes that produce the effect. Early theories of procedural justice (Leventhal, 1980; Thibaut & Walker, 1975,

1978) sought to explain procedural justice phenomena with reference to the perceiver's assumptions about the outcomes that various procedures would generate. These theories attributed the voice effect to beliefs about the instrumental consequences of input. The voice effect is explained by presuming that persons given an opportunity to express their views will believe that voice will help them control their outcomes—their arguments might persuade the decision maker to provide a better outcome—and that these expectations lead to higher procedural fairness judgments. In the leading instrumental theories voice is seen as fair because it increases the probability of either a favorable outcome (Leventhal, 1980) or an equitable outcome (Thibaut & Walker, 1978). Instrumental theories of procedural justice and related models are still favored by many procedural justice researchers (e.g., Brett, 1986; Vidmar, in press).

At least two recent theories of procedural justice (Lane, 1988; Lind & Tyler, 1988, pp. 230-240) have sought to explain procedural justice phenomena in terms of the symbolic and informational consequences of procedures, rather than in terms of the procedure's capacity to provide good outcomes. Lind and Tyler proposed a "group-value" model, which argues that the voice effect stems from the implication that those accorded an opportunity to present information are valued, full-fledged members of the group enacting the procedure. People value voice because it suggests that their views are worthy of hearing, according to Lind and Tyler, and procedures that accord people status in this way are viewed favorably, whatever their likely effects on the outcome of the procedure. Lane took a similar position (pp. 179-180) when he described the opportu-

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<sup>1</sup> In this article we will use the term *voice* rather than *process control* because voice seems to best capture the essential condition for the effect.

nity to be heard as a "dignity good" that citizens value in government procedures.

There is a middle ground in the debate between instrumental and noninstrumental theories of procedural justice. Notwithstanding their advancement of noninstrumental group-value theory, Lind and Tyler (1988) ultimately concluded that the field will eventually adopt a position intermediate between instrumental theories of procedural justice and theories that explain procedural justice in terms of the symbolic implications of procedures. After reviewing numerous studies, Lind and Tyler pointed out that the entire range of procedural justice phenomena can be explained only if it is accepted that fairness judgments are driven both by instrumental, informed self-interest concerns and by noninstrumental, group-value concerns (pp. 240–241). They called for new theories that integrate both types of concerns in explaining the psychology of procedural justice.

In the context of these theoretical disagreements about the processes underlying the voice effect, an important empirical question is whether voice effects can be shown to occur in situations where it is clear that the exercise of voice cannot influence the outcome of the procedure. Folger (1986) has pointed out the importance of this question and reviewed the literature relevant to it. He found only three studies that explicitly addressed the question, all reported in an article by Tyler et al. (1985). However, none of the Tyler et al. studies provides a true experimental test of noninstrumental mediation of the voice effect: One study is a correlational study of traffic court defendants' perceptions of control, voice, and fairness; the second is a correlational study of student evaluations of grading procedures; and the third is a hypothetical scenario study in which subjects did not actually experience the procedures and outcomes they rated. The two correlational studies leave unanswered, of course, critical questions concerning the direction of causality. The hypothetical scenario study can only determine whether subjects *believe* that voice would enhance their procedural fairness judgments beyond voice's instrumental value; the study does not prove that such effects actually occur. Previous studies have demonstrated that scenario studies often arrive at conclusions that are at variance to those of true experiments, both in social psychological research in general (e.g., Freedman, 1969) and in procedural justice research in particular (e.g., Lind et al., 1983).

We sought to give the theoretical debate about instrumental and noninstrumental processes in voice effects a stronger empirical basis by testing noninstrumental effects experimentally. Specifically, we tested whether procedural justice judgments were enhanced by an opportunity to voice one's views *after* the decision had been made. If voice effects are attributable solely to the symbolic or status-enhancing consequences of being able to express one's views, as the noninstrumental theories have suggested, then postdecision voice should enhance perceived fairness as much as predecision voice. On the other hand, if voice effects are due solely to the belief that voice will lead to a better decision, as instrumental theories suggest, then postdecision voice will lead to no greater perceived procedural fairness than no voice at all. And if both instrumental and noninstrumental concerns are important in procedural fairness judgments, then postdecision voice should lead to some

enhancement of perceived fairness, but not as much as does predecision voice.

It should be noted that it is certainly conceivable that postdecision voice might lead to *lower* procedural justice judgments than does no voice at all. There is evidence that, under some circumstances, people actively reject procedures that appear to offer process control but that do not provide any real input into the decision-making process. Folger and his colleagues (Folger, 1977; Folger, Rosenfield, Grove, & Corkran, 1979; Folger, Rosenfield, & Robinson, 1983) have conducted a number of studies of what is termed the *frustration effect*. These experiments showed that people react quite negatively to ostensibly high voice procedures when repeated unfavorable outcomes or communications from others focus attention on possible biases that might subvert the impact of their voice. Lind and Tyler (1988, pp. 183–184) argued that frustration effects occur only when the procedure is blatantly biased, but absent further empirical evidence it is certainly possible that unbiased postdecision voice will decrease perceived justice.

We studied the effects of voice in the context of a procedure for setting performance goals. During the past decade, theory and research in the goal-setting literature has focused on the influence of participative procedures (procedures that permit an opportunity for input) on goal acceptance and subsequent task performance. A number of studies have examined whether participation in the goal-setting process enhances goal acceptance and task performance (e.g., Erez, Earley, & Hulin, 1985; Latham, Erez, & Locke, 1988; Latham & Saari, 1979; for reviews see, e.g., Kanfer, in press; Latham & Lee, 1986; Locke & Schweiger, 1979; Locke, Shaw, Saari, & Latham, 1981). Some of the explanations for why participation in goal setting may exert beneficial effects resemble explanations of the voice effect on procedural fairness. For example, participation may provide an opportunity to influence the goal that is ultimately established (an instrumental explanation). Alternatively, even when workers' preferences are not implemented, the participative process may convey to the workers that organizational superiors are concerned with the workers' attitudes (a noninstrumental explanation). Another explanation of the effects of participation in goal setting focuses on the information that workers might gain about the task in the process of discussing goals (Locke, Latham, & Erez, 1988). That is, participation might enhance goal acceptance and performance by providing workers with knowledge about how to approach the task, thereby enhancing task-specific self-efficacy expectations.

In this study we tested the effects of voice across three conditions relevant to this last, information-provided explanation of participative goal setting. We manipulated the information provided to the subject by the experimenter. In one condition, the experimenter provided information on strategies that might improve performance on the task. In another condition, the experimenter provided information that was of interest to the subjects but that was unrelated to the task, and in a third condition the experimenter provided no information at all.

If the provision of task-relevant information during discussion serves to increase self-efficacy expectations, then the increased sense of competence associated with heightened self-efficacy expectations might reduce the need to exert influence on the goal assignment. If instrumental theories of procedural jus-

tice are correct, we would expect that the provision of task-relevant information would enhance subjects' efficacy expectations, enhance perceptions of goal fairness, and diminish the effects of voice compared with the irrelevant- and no-information conditions (cf. Locke & Latham, 1990, pp. 147–148). Thus, instrumental theories of justice predict both a main effect for information and an interaction of information and voice on fairness ratings. In contrast, noninstrumental theories of procedural justice would predict that the provision of interesting information, whether relevant or not, provides individuals with recognition of their involvement and membership in the work group (cf. Bies & Moag, 1986). From the noninstrumental perspective, therefore, both relevant and irrelevant information would be expected to enhance procedural fairness judgments relative to the no-information condition. In contrast to an instrumental explanation, noninstrumental theories of procedural justice would not predict an interaction between the task information provided to the subject and the voice effect.

## Method

### Subjects

One hundred eighty male undergraduate students participated in the experiment in partial fulfillment of a requirement in an introductory course in psychology.<sup>2</sup> Twenty subjects were randomly assigned to each cell of the experimental design. Data from one subject (in the predecision voice/relevant-information condition) were incomplete and were not used in the analyses reported below.

### Experimental Design

The experimental design was a 3 (Procedure: no voice, postdecision voice, and pre-decision voice)  $\times$  3 (Information From Experimenter: no information, irrelevant information, and relevant information) complete factorial design.

### Experimental Procedure

The subjects were told that they were participating in a study of performance using a typical student activity: the construction of course schedules. They were told that the experimenter was interested in how students put together their class schedules and that the findings of the study would be reported to the registrar's office for use in deciding how to schedule classes at the university. It was stressed that the experimenter wanted to learn about students' approaches to course scheduling in order to improve the scheduling process.

Each subject was seated in a separate experimental cubicle and given written instructions on how to perform the course scheduling task. The subjects were then told to perform the scheduling task for 5 min as a practice trial. After the practice trial, the experimenter entered the room and collected the schedules the subjects had completed. The subjects then received instructions for the main, goal-setting portion of the experiment.

The subjects were told that they would be given a goal for the number of schedules to be completed during the second, final phase of the experiment. In the *predecision voice* procedure condition, the experimenter first told the subjects that he was tentatively thinking of setting a goal of 12 schedules during the 15-min time frame allotted for the final work period. The experimenter said that he wanted to take the subjects' views into account, however, and he encouraged them to express their opinions concerning the task and the goal. After listening to the subject's views—which invariably included an expressed desire

for a lower goal—the experimenter assigned a work goal of 10 schedules in the final work period. (On the basis of pilot testing, it was known that the goal of 10 course schedules in 15 min was a very difficult but attainable goal.) The *predecision voice* condition thus mirrors the voice procedures used in most previous research, with the additional characteristic that the voice clearly appears to be successful in influencing the goal. In the *postdecision voice* procedure condition, the experimenter told the subjects that he had already decided on a goal of 10 schedules and that this goal would not be changed regardless of what the subject said. The experimenter said he was nonetheless interested in the subjects' views and encouraged the subjects to express their opinions concerning the task and the goal that had been set. As in the *predecision voice* condition, the subjects in the *postdecision voice* condition always said that they would prefer a lower goal. After listening to the subjects' views, the experimenter reiterated the work goal of 10 schedules. Thus, any perception of control in the *postdecision voice* condition would run contrary to both the experimenter's explicit denial of any influence of the subjects' input and his failure to change the goal. In the *no-voice* condition, the experimenter simply announced the work goal of 10 schedules without soliciting comments either before or after the goal was set. The instructions and the communication from the subjects took place through an intercom system, and the experimenter spoke with a calm and reassuring tone.

The experimenter then enacted the information-from-experimenter manipulation, with the rationale of filling time until the second work session was due to start. In the *relevant-information* condition, he informed the subject of several strategies that would be useful in achieving the goal. For example, the experimenter pointed out that different schedules could be produced by rotating blocks of classes intact while changing only a single class. In the *irrelevant-information* condition, the experimenter addressed the subjects for an equivalent period of time (3 min), providing information on university calendar events. In the *no-information* condition, the experimenter simply asked the subjects to wait a few minutes.

The subjects were then asked to complete a brief questionnaire. After they had completed the questionnaire, they were instructed to begin working on the task. After 15 min the subjects were told to cease working, and the completed schedules were collected. The main questionnaire, which included all of the measures reported below, was administered after the subjects had completed the task but before they were given any feedback about their performance.

### Measures

The experimental questionnaire contained two 5-point rating scales on perceptions of justice: Procedural fairness judgments were solicited in a question asking how fair was the way the goal was set, and outcome fairness judgments were solicited in a question asking how fair the assigned goal was. Perceptions of control were assessed with a 5-point scale asking how much control the subject had over the goal. A 5-point scale asking how much information the subject gave to the experimenter about the goal constituted a check on the manipulation of voice (i.e., the opportunity to present information), and a 9-point scale asking how much information the subject had concerning the goal and task constituted a check on the efficacy of the information-

<sup>2</sup> Of course, our use of only male subjects precludes us from testing for gender-based differences in procedural justice judgments. Recent research (Lind et al., 1989) tested for gender differences in procedural justice judgments in a naturalistic setting and found none. This is not to say that such differences do not occur, but we know of no study that has shown differences between the procedural justice judgments of men and those of women.

from-experimenter manipulation. The subjects were asked to rate, using a 10-point scale, the degree to which they had adopted the goal given them by the experimenter. Ratings of feelings of efficacy with respect to the scheduling task were solicited with a 5-point scale asking how well the subject thought he or she did on the task. The subjects were also asked to rate how much they liked the experimenter, using a 5-point scale. Finally, performance on the task was measured by counting the number of schedules the subject completed during the 15-min work period.

## Results

### *Manipulation Checks*

To gauge the subjects' perceptions of the experimental conditions, we conducted a multivariate analysis of variance (MANOVA) on ratings of the opportunity for voice, the subject's control over the goal assignment, the amount of information the subject had about the task and goal, perceptions of efficacy, and how much the subject liked the experimenter. The analysis showed significant main effects for both manipulations: for the voice manipulation, multivariate  $F(10, 332) = 11.83, p < .001$ ; for the information-from-experimenter manipulation, multivariate  $F(10, 332) = 5.08, p < .001$ . The voice manipulation produced significant univariate main effects on the ratings of the opportunity for voice,  $F(2, 170) = 52.54, p < .001$ ; on the ratings of control over the goal,  $F(2, 170) = 34.00, p < .001$ ; and on ratings of perceived efficacy in performing the task,  $F(2, 170) = 6.22, p < .002$ . Neither the multivariate test of the Voice  $\times$  Information interaction nor any of the univariate tests of the interaction were significant. The subjects' ratings of how much they liked the experimenter were generally in the neutral range of the scale and were not affected by either of the manipulations or by their interaction.

The subjects felt they had given the experimenter more information in the predecision voice condition ( $M = 3.96$ ) than in the postdecision voice condition ( $M = 3.27$ ), and that they had given the experimenter more information in the postdecision condition than in the no-voice condition ( $M = 1.87$ ). Scheffé post hoc pairwise comparisons (Kirk, 1968, pp. 188–189) among the three treatment conditions showed that all three voice conditions differed significantly in the amount of information subjects felt they had given the experimenter (critical  $S_{.05} = .51$ ). The subjects also felt that they had more control over the assigned goal in the predecision voice condition ( $M = 3.26$ ) than in the postdecision voice condition ( $M = 2.52$ ) and more control in the postdecision voice condition than in the no-voice condition ( $M = 1.68$ ). For perceptions of control, the post hoc pairwise comparisons showed that each of the three voice conditions differed from the others (critical  $S_{.05} = .47$ ). For perceptions of efficacy, post hoc Scheffé tests showed that the no-voice condition ( $M = 2.73$ ) differed significantly from the predecision voice condition ( $M = 3.28$ ), but that the postdecision voice condition ( $M = 2.98$ ) did not differ significantly from either of the other conditions (critical  $S_{.05} = .38$ ).

The significant multivariate main effect for the information-from-experimenter manipulation appears to be due to univariate main effects on ratings of the amount of information the subject felt he or she had received about the task and goal,  $F(2, 170) = 14.14, p < .001$ , and on ratings of perceived efficacy,  $F(2,$

$170) = 7.65, p < .001$ . Subjects felt that they had received more information in the relevant-information condition ( $M = 5.45$ ) than in the irrelevant-information condition ( $M = 3.80$ ) or the no-information condition ( $M = 3.15$ ). Scheffé post hoc comparisons on ratings of information received showed that the relevant-information condition differed significantly from the two other conditions and that the irrelevant-information and no-information conditions did not differ significantly (critical  $S_{.05} = 1.23$ ). Post hoc Scheffé tests also showed that the relevant-information condition led to feelings of greater efficacy ( $M = 3.34$ ) than did either the irrelevant-information condition ( $M = 2.78$ ) or the no-information condition ( $M = 2.87$ ); the latter two conditions did not differ from each other (critical  $S_{.05} = .38$ ).

### *Procedural and Outcome Fairness Judgments*

Table 1 shows the cell means for subjects' ratings of the fairness of the way the goal was assigned—procedural fairness—and their ratings of the fairness of the assigned goal itself—outcome fairness. We tested the effects of the voice manipulation with a priori contrasts comparing the two voice conditions with the no-voice condition and comparing the postdecision voice condition with the predecision voice condition. (Recall that the prediction from noninstrumental theories of procedural justice is that only the former contrast will be significant, and the prediction from instrumental theories of procedural justice is that only the latter contrast will be significant.) A MANOVA showed significant multivariate and univariate effects for both variables on both contrasts: The voice conditions produced higher ratings of procedural and outcome fairness than did the no-voice condition, multivariate  $F(2, 169) = 23.15, p < .001$ ; for procedural fairness,  $F(1, 170) = 42.11, p < .001$ ; for outcome fairness,  $F(1, 170) = 32.19, p < .001$ , and the predecision voice condition produced higher ratings of procedural and outcome fairness than did the postdecision voice condition, multivariate  $F(2, 169) = 6.21, p < .002$ ; for procedural fairness,  $F(1, 170) = 9.62, p < .002$ ; for outcome fairness,  $F(1, 170) = 10.56, p < .001$ .<sup>3</sup> Thus, the fairness ratings provided support for both instrumental and noninstrumental theories of procedural justice.

Both fairness variables also showed a significant effect for the information-from-experimenter manipulation, multivariate  $F(4, 338) = 3.64, p < .006$ ; for procedural fairness,  $F(2, 170) = 3.71, p < .03$ ; for outcome fairness,  $F(2, 170) = 4.20, p < .02$ . Scheffé tests of pairwise comparisons among the information-from-experimenter categories showed that relevant information differed significantly from no information in terms of procedural fairness and that relevant information differed from irrelevant information in terms of outcome fairness; all other pairwise comparisons were not significant. Neither fairness variable showed a significant interaction effect, multivariate

<sup>3</sup> It is conceivable that the significant contrast of the voice to the no-voice conditions was due solely to the very high fairness ratings of the predecision voice condition. However, Scheffé post hoc comparisons revealed that for both fairness variables, all pairwise comparisons among the voice conditions were significant (critical  $S_{.05} = .43$  for procedural fairness, and critical  $S_{.05} = .45$  for outcome fairness).

Table 1  
Cell Means: Perceived Fairness

| Information from experimenter | Communication to experimenter |              |             | <i>M</i> |
|-------------------------------|-------------------------------|--------------|-------------|----------|
|                               | None                          | Postdecision | Predecision |          |
| Perceived procedural fairness |                               |              |             |          |
| None                          | 2.15                          | 3.00         | 3.40        | 2.85     |
| Irrelevant                    | 2.55                          | 2.95         | 3.80        | 3.10     |
| Relevant                      | 2.60                          | 3.50         | 3.90        | 3.33     |
| <i>M</i>                      | 2.43                          | 3.15         | 3.70        |          |
| Perceived outcome fairness    |                               |              |             |          |
| None                          | 2.00                          | 2.80         | 3.10        | 2.63     |
| Irrelevant                    | 2.15                          | 2.25         | 3.10        | 2.50     |
| Relevant                      | 2.20                          | 3.10         | 3.74        | 3.01     |
| <i>M</i>                      | 2.11                          | 2.72         | 3.31        |          |

Note. Entries are cell means on 5-point scales; higher values indicate perceived fairness.

$F < 1.0$ ; for procedural fairness,  $F < 1.0$ ; for outcome fairness,  $F(2, 170) = 1.20$ , *ns*.

### Perceptions of Control and the Voice Effect

The patterns of mean fairness ratings appear to be generally supportive of a mixed model of procedural fairness, showing both instrumental and noninstrumental voice effects. However, the finding that postdecision voice led to greater perceived control than no voice at all makes the pattern of means somewhat more ambiguous than would otherwise have been the case. Subjects in the postdecision voice condition appear to have believed they had some control, even though they were told explicitly that their input could not affect the goal the experimenter assigned and even though the goal remained the same after their input. As we note below, these elevated control ratings may have been due to an illusion of control effect in the postdecision voice condition or they may have been the result of generally enhanced feelings of group involvement.

If the enhanced feelings of control in the postdecision voice condition are the result of an illusion of control, however, one could argue that the fairness findings could be accounted for entirely within an instrumental theory of procedural justice judgments, providing that the theory was based on *subjective* rather than *objective* control. Additional analyses are needed to determine whether perceived control mediated all of the voice effect. We conducted mediational analyses (Baron & Kenny, 1986) to test whether the voice effect could be attributed entirely to perceptions of control. Baron and Kenny (p. 1177) pointed out several criteria that must be met to sustain the assertion that a variable mediates an effect: The mediator must be affected by the independent variable, the dependent variable must be affected by the independent variable, and the relationship between the independent variable and the dependent variable must be attenuated or disappear when variance attributable to the mediator is removed. The first criterion is met for perceptions of control by virtue of the significant voice

effect on the control ratings. The second criterion is met by virtue of the voice effects on the fairness ratings.

To test the third Baron and Kenny criterion, we conducted analyses of covariance (ANCOVAs), removing the effects of perceived control prior to testing the contrasts among the three voice conditions. The analyses showed that the contrast between the two voice conditions and the no-voice condition remained significant for ratings of procedural fairness,  $F(1, 169) = 4.65$ ,  $p < .04$ , but the variance accounted for by the contrast was markedly reduced (without removing variance mediated by control,  $\eta^2 = .19$ ; after removing variance mediated by control,  $\eta^2 = .03$ ). For the contrast of the voice conditions to the no-voice conditions on ratings of outcome fairness, the ANCOVA showed no significant effect ( $F < 1.0$ ;  $\eta^2 = .16$  without control ratings removed, and  $\eta^2 = .01$  with control ratings removed). When variance attributable to the control ratings was removed in the ANCOVA, neither fairness variable showed a significant contrast between the postdecision voice and the predecision voice conditions (both  $F$ 's  $< 1.0$ ; for procedural fairness,  $\eta^2 = .05$  without control ratings removed and  $\eta^2 = .004$  with control ratings removed; for outcome fairness,  $\eta^2 = .06$  without control ratings removed and  $\eta^2 = .004$  with control ratings removed). These analyses suggest that the control ratings can account for some, but not all, of the voice effect on procedural fairness ratings.

### Goal Acceptance and Performance

A MANOVA of the number of schedules produced in the test period and of subjects' ratings of the extent to which they accepted the goal they were assigned showed a significant main effect for the voice manipulation, multivariate  $F(4, 338) = 9.83$ ,  $p < .001$ ; for goal acceptance,  $F(2, 170) = 7.61$ ,  $p < .001$ ; for performance,  $F(2, 170) = 19.94$ ,  $p < .001$ . Goal acceptance was higher for both the predecision voice condition ( $M = 5.93$ ) and the postdecision voice condition ( $M = 5.67$ ) than for the no-voice condition ( $M = 4.33$ ; critical  $S_{.05} = 1.08$ ). Similarly, performance was higher in the predecision voice condition ( $M = 6.58$ ) and the postdecision voice condition ( $M = 5.85$ ) than in the no-voice condition ( $M = 4.67$ ; critical  $S_{.05} = .76$ ).

The MANOVA also showed a significant main effect for the information-from-experimenter manipulation, multivariate  $F(4, 338) = 21.38$ ,  $p < .001$ ; for goal acceptance,  $F(2, 170) = 5.43$ ,  $p < .005$ ; for performance,  $F(2, 170) = 46.86$ ,  $p < .001$ . Relevant information led to greater acceptance of the goal ( $M = 6.14$ ) than either irrelevant information ( $M = 4.93$ ) or no information ( $M = 4.85$ ; critical  $S_{.05} = 1.08$ ), and relevant information also led to better performance ( $M = 7.42$ ) than either irrelevant information ( $M = 4.93$ ) or no information ( $M = 4.77$ ; critical  $S_{.05} = .76$ ).

There was a significant multivariate interaction between the voice manipulation and the information-from-experimenter manipulation, multivariate  $F(8, 338) = 2.05$ ,  $p < .001$ ; for goal acceptance,  $F < 1.0$ , *ns*; for performance,  $F(4, 170) = 2.31$ ,  $p < .06$ . The interaction appears to be due to an especially strong difference in performance between the postdecision voice cell and the no-voice cell within the relevant-information condition.

## Discussion

The experiment has a number of important implications for understanding the psychology of procedure. The voice effects on acceptance of the goal and on performance add to a growing body of evidence showing that voice affects a variety of organizational attitudes and behaviors (see, e.g., Earley & Lind, 1987; Greenberg, 1987; Lissak, 1983). By and large, voice appears to promote positive attitudes toward supervisors, tasks, and goals and to lead to better performance. At least one study (Kanfer et al., 1987) has found lower performance when workers are allowed voice, however.

The fairness effects of the information-from-experimenter manipulation show that relevant information, which promotes feelings of efficacy, can also promote the perception that task-related procedures and assignments are fair. As we noted earlier, this finding is congruent with instrumental theories of justice: Relevant information made the subjects feel that they were better at the task, and this may have made the goal seem less extreme and therefore fairer. By rendering the outcome more fair, the relevant information may have made the procedure that produced the outcome seem fairer. We had hypothesized on the basis of group-value theory that even irrelevant information might make the subjects feel more involved and therefore lead them to view the procedures as fairer. The mean procedural fairness rating in the irrelevant-information condition appears to be somewhat higher than that in the no-information condition, but the difference is not statistically significant. It is not possible to determine whether our hypothesis was incorrect or whether the irrelevant-information manipulation was simply not strong enough to produce the hypothesized effect.

The most important implications of the study have to do with the psychological processes involved in the voice effect. The results of the present experiment show these processes to be more complex than is suggested by any of the current theories of procedural justice. The experiment produced relatively strong evidence that issues of control are an important factor in producing voice effects, but it also showed that not all of the effect of voice was due to control.

Consider first the experimental findings—the mean differences we observed on the procedural and outcome fairness variables. It is clear that, at least within the context and subject population we studied, fairness judgments are enhanced by the opportunity to voice opinions even when there is no chance of influencing the decision. At the same time, it is clear that voice *with* the possibility of influence (and in our experiment with evidence of successful influence) leads to even greater perceived fairness. The postdecision voice condition shows the level of procedural fairness that occurs if only symbolic processes are at work, and the predecision voice condition shows the level of procedural fairness that occurs if both instrumental and symbolic processes are at work. Therefore, the difference between procedural justice ratings for the no-voice condition and those for the postdecisional voice condition provides an indication of the strength of symbolic voice processes, whereas the difference between postdecision voice and predecision voice provides an indication of the strength of instrumental

voice processes over and above symbolic processes. The mean values we observed suggest that the symbolic voice effect is at least as strong as the instrumental voice effect: The difference in procedural justice ratings between the no-voice condition and the postdecision voice condition is slightly larger (difference = .72) than the difference between the postdecision voice condition and the predecision voice condition (difference = .55).

The findings with respect to perceived control should also be taken into account. In spite of what seemed to us to be a very strong statement to the subjects about the absence of any possible influence on the decision in the postdecision voice condition and in spite of a clear demonstration of the ineffectiveness of postdecision voice in changing the goal, subjects in this condition reported feeling more control over the outcome than did subjects in the no-voice condition. There are at least two possible explanations for this rather surprising finding. First, the subjects might have felt more control in the postdecision voice condition because the opportunity for voice made them feel more at ease with the experimental setting and its social milieu. The same symbolic factors that were hypothesized to enhance perceived fairness in the postdecision voice condition might also enhance perceived control: To be allowed to express one's views implies that one is a full-fledged member of the experimental group, and this in turn leads to feelings of security and control over the situation (see Lind & Tyler, 1988, pp. 236–237). Alternatively, postdecision voice might have produced an illusion of control (Langer, 1983; Taylor & Brown, 1988).<sup>4</sup> Which ever interpretation is correct, it is interesting to note that similar effects, showing exaggerated perceptions of control in the context of a variety of decision-making procedures, have been seen in other studies (Earley & Lind, 1987; Lind, 1975; Pepitone, 1950).

As we noted earlier, if the high control ratings in the postdecision voice condition are indeed due to an illusion of control—and there is no way of discriminating between the two explanations in the present study—then one could argue that the differences in procedural justice judgments reflect instrumental processes in that they were produced by differences in *subjective* control. Our mediation analyses showed, however, that even under this most favorable interpretation of the data, instrumental models could not account for all of the voice effect. Taken as a whole, then, our study shows that both instrumental and noninstrumental processes are at work. It appears that the early emphasis in procedural justice research on procedure as means to outcomes was in error, but our data make it clear also that it is premature to conclude, as Tyler et al. (1985) did, that

<sup>4</sup> It is possible, of course, that the subjects simply did not understand the statement or that some other aspect of the experimenter's behavior led them to believe that they could influence the goal they were assigned. The former explanation seems to us unlikely in view of the emphasis placed on the procedure manipulation in running the experiment and in light of the fact that the postdecision voice condition did produce lower control ratings than the predecision voice condition. This explanation is difficult to prove or disprove, but seems to us unlikely in light of the absence of any differences in ratings of the likability of the experimenter.

“people seem insensitive to whether their heightened process control is linked to actual control over decisions” (p. 79). It is time for researchers and theorists to become more sophisticated about what drives procedural justice judgments: The answer to the instrumental–noninstrumental debate is that both sides are right, but neither side is entirely right.

The results of the experiment also show that analyses of procedural justice need to be more sophisticated with respect to the psychology of voice. In much of the early research on procedural justice, the major concern was with how procedures that differed in terms of objective power relations might affect fairness judgments (Thibaut & Walker, 1975). It was generally assumed that perceptions of control more or less matched the objective power associated with various procedures. Our data show that voice procedures that are radically different in objective control can produce rather similar perceptions of control and, for that matter, rather similar fairness judgments. In this respect, the present study stresses the subjectivity of justice, a perspective long evident in distributive justice theories (e.g., Adams, 1965; Reis, 1981) but often ignored in procedural justice theory and research. The present study shows, as has other recent research, that what ultimately matters for fairness judgments is not the objective level of outcomes or the objective characteristics of procedures, but instead the subjective evaluations of the variables that underlie fairness judgments (Greenberg & Cohen, 1982; Lind et al., 1989; Paese, Lind, & Kanfer, 1988).

The subjectivity of control and fairness in voice effects also shows a potential for the abuse of voice-based enhancement of procedural fairness judgments. Cohen (1985, 1988) has raised concerns about deceptive uses of the voice effect to promote the appearance of procedural justice. Cohen pointed out that people could be given the appearance of voice—they might be allowed or even encouraged to express their views to the decision maker—when, in reality, the decision maker has no intention of considering the input. Such “false consciousness” of control could lead the individuals in question to believe that the decision-making procedure was fair even though, by objective criteria, it is patently unfair. Cohen noted that the objective unfairness of such procedures is exacerbated if the decision maker uses the appearance of justice to advance his or her own interests at the expense of those subject to the procedure. If the perception of fairness is enhanced even in the face of the relatively straightforward denial of control involved in our postdecision voice condition, voice-enhanced fairness is all the more likely to occur in situations where a decision maker actively hides the ineffectiveness of input—conditions that may well be more common in the real world than is postdecision voice.

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### Call for Nominations for *Developmental Psychology*

The Publications and Communications Board has opened nominations for the editorship of *Developmental Psychology* for the years 1993-1998. Ross D. Parke is the incumbent editor. Candidates must be members of APA and should be available to start receiving manuscripts in early 1992 to prepare for issues published in 1993. Please note that the P&C Board encourages more participation by members of underrepresented groups in the publication process and would particularly welcome such nominees. To nominate candidates, prepare a statement of one page or less in support of each candidate. Submit nominations to

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