Referent Cognitions Theory: The Role of Closeness of Reference Points in the Psychology of Voice

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The most generally accepted and best documented manipulation in procedural justice experiments is varying whether or not participants are allowed an opportunity to voice their opinion about a decision. To better understand the psychology of voice, the authors focus on Referent Cognitions Theory (RCT). It is argued that thus far RCT has not been used to explain the psychology of voice and that previous RCT research suffers from methodological problems and has been more outcome oriented than necessary. Two experiments resolve these problems and show that people react more strongly to procedures (especially no-voice procedures) when reference points are close as opposed to distant. These findings suggest that closeness of reference points plays an important role in the psychology of voice. The findings expand RCT in significant ways and indicate that insight into the role of reference points is essential for understanding the psychology of justice.

Social justice plays a key role in human life. For example, being treated fairly by one's organization and the people who work in the organization typically leads to higher ratings of positive affect, more positive judgments about one's relationship with the organization and the people involved, higher commitment to the organization, and more extra-role citizenship behavior (e.g., Lind & Tyler, 1988; Tyler & Lind, 1992; Tyler & Smith, 1998). People who experience unfair treatment, on the other hand, are more likely to report higher ratings of negative affect, leave their jobs, start behaving in anti-normative ways, and show lower levels of commitment (Folger & Cropanzano, 1998). These findings show that fairness plays a crucial role in social behavior. It is therefore important to study people's concerns about justice (Lind & Tyler, 1988; Tyler & Lind, 1992). Social psychologists have studied these concerns and have proposed a number of theories that try to explain people's justice concerns.

Early social psychological theories of social justice—such as equity theory (e.g., Adams, 1965) and other related conceptions of justice (e.g., Blau, 1964; Homans, 1961; Stouffer, Suchman, DeVinney, Star, & Williams, 1949)—are commonly referred to as theories of distributive justice because they focus on the outcomes that people receive. Some conceptions of distributive justice state that outcomes are crucial in the fairness judgment process. For example, it has been argued that experiencing unfavorable outcomes is a necessary precondition before people start thinking about social justice issues (Rutte & Messick, 1995; see also Lerner & Whitehead, 1980).

Another conception of justice emerged from the innovative work by Thibaut and Walker (1975). These authors argued that the outcome of a court trial and the manner in which the trial is conducted form two separable aspects of legal settings that can affect litigants' fairness judgments. Thibaut and Walker and their co-researchers showed that social justice concerns indeed include questions about the fairness of procedures as well as questions about the fairness of outcomes. More specifically, the research findings by Thibaut, Walker, and colleagues indicated that having control over the outcome of a court trial (decision control) and having control over the manner in which the trial is conducted (process control) have independent effects on litigants' reactions (see, e.g., Houlden, LaTour, Walker, & Thibaut, 1978; LaTour, Houlden, Walker, & Thibaut, 1976; Lind, Kurtz, Musante, Walker, & Thibaut, 1980; Thibaut & Walker, 1975; Walker, LaTour, Lind, & Thibaut, 1974). Since Thibaut and Walker's (1975) research, numerous articles, books, and research studies have shown that perceived procedural justice can strongly affect people's beliefs, feelings, attitudes, and behaviors (see, e.g., Cropanzano & Greenberg, 1997; Folger & Konovsky, 1989; Lind & Tyler, 1988; Tyler & Lind, 1992).

A crucial concept in Thibaut and Walker's (1975) work was whether or not a procedure allowed people (some) control over the process. In the pioneering research by Folger and colleagues (e.g., Folger, 1977; Folger, Rosenfield, Grove, & Corkran, 1979), the

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focus shifted from the concept of process control toward investigating whether or not people receive an opportunity to voice their opinion in the decision-making process. The concept of voice is very important in the field of procedural justice, partly because research convincingly has shown that voice is one of the key determinants that lead people to judge a particular procedure as fair or unfair (depending on whether voice opportunities are or are not allowed). The concept of voice is also important because studies have revealed that voice incorporates both instrumental and noninstrumental aspects. That is, Lind, Kanfer, and Earley (1990) varied whether participants in a goal-setting situation were allowed an opportunity to voice their opinions before the goal was set, after the goal was set, or not at all. As predicted, both pre- and postdecision voice led to higher fairness judgments than no voice did, with predecision voice leading to higher fairness judgments than postdecision voice. Research on voice effects thus has revealed that not only instrumental considerations are important in the psychology of social justice (cf. Thibaut & Walker, 1975) but noninstrumental concerns are as well (Lind et al., 1990). These and other findings have led to the situation that experiments on procedural justice nowadays typically do not vary process control (cf. Thibaut & Walker, 1975), but instead manipulate whether participants are or are not allowed voice. In fact, varying whether or not people have an opportunity to voice their opinion is now the most generally accepted manipulation of procedure (Brockner et al., 1998; Lind et al., 1990; Lind & Tyler, 1988; Van den Bos, 1999; Van den Bos, Vermunt, & Wilke, 1996). Thus, voice is central to the canon of procedural justice studies and has removed the field of social justice from a purely instrumental orientation to a perspective that incorporates both instrumental and noninstrumental aspects of justice concerns.

A large number of studies, conducted in natural settings as well as in the laboratory, have found that being allowed voice, as opposed to no voice, leads to more positive reactions, such as enhanced perceived procedural justice (Lind & Tyler, 1988) and less negative affect (e.g., anger against officials; Tyler, 1987). Following Folger and colleagues' work (e.g., Folger, 1977; Folger et al., 1979), this finding is usually labeled *the voice effect.* It has been concluded that the voice effect is one of the most important and robust findings in the domain of procedural justice and in the field of social psychology (Lind & Tyler, 1988; Tyler, 1987; Tyler & Lind, 1992; Van den Bos, Lind, Vermunt, & Wilke, 1997). Moreover, research on the voice effect has been so successful that it has been argued that the field is waiting for theories that explain the psychology of this phenomenon (e.g., Lind & Tyler, 1988).

In the present article, we try to offer an explanation by arguing that how people react to voice and no-voice procedures depends on their reference points (Kahneman & Tversky, 1982). This is an issue that, according to our knowledge, has not been explored before and that is important because, from major theoretical treatments of classic social psychology (e.g., Adams, 1965; Blau, 1964; Folger, 1986; Homans, 1961; Stouffer et al., 1949; Thibaut & Kelley, 1959), it can be deduced that when people are forming fairness judgments they do so by referring to reference points. For instance, in the relative deprivation literature (e.g., Stouffer et al., 1949; see also Cropanzano & Randall, 1995; Folger & Martin, 1986; Folger, Rosenfield, Rheaume, & Martin, 1983; Folger, Rosenfield, & Robinson, 1983), it has been argued that outcome improvement may be a source of rising expectations. These rising expectations may serve as a reference point such that when expectations are violated by improvements that fail to rise at the same rate, people may be dissatisfied with their improved outcomes. To give another example, equity theory (e.g., Adams, 1965) argues that outcomes are evaluated in terms of where own outcomes fall relative to the outcomes of others. Outcomes of comparison others constitute reference points in that own outcomes are judged to be more fair when they are equal to, as opposed to different from, outcomes of comparison others.

We argue here that these and other examples (see, e.g., Blau, 1964; Homans, 1961; Thibaut & Kelley, 1959) suggest that insight into the role of reference points is essential for understanding the psychology of voice. In the current article, we try to show that the psychology of voice depends in large part on people's reference points by focusing on Referent Cognitions Theory (RCT; Cropanzano & Folger, 1989; Folger, 1986, 1987, 1993; see also Folger & Cropanzano, 1998, 2001). RCT is an important theory for the current purposes because, within the justice domain, it is probably the most recent and most thorough framework dealing with reference points. We argue, however, that thus far RCT has not been used to explain the psychology of voice. Furthermore, we point out that important methodological issues of the previous RCT studies have to be dealt with, and we try to deal with these issues in the research presented in this article. Below we give a short introduction to RCT, after which we derive the purposes of the current research.

Referent Cognitions Theory

RCT was developed in the 1980s. In the course of time, different versions of RCT have been developed (for overviews, see Cropanzano & Folger, 1989; Folger, 1986, 1987, 1993; see also Folger & Cropanzano, 1998, 2001; Folger & Kass, 2000), but these versions all share an important core principle with respect to the psychology of social justice: Following the work by Kahneman and Tversky (1982), RCT argues that people's reactions to procedural and distributive justice depend largely on their counterfactual thoughts. In essence, RCT reasons that when a distributive or procedural rule is broken people's thinking becomes inherently referential: People use a frame of reference for evaluating what happened that consists of a mental comparison to what might have happened instead (for more elaborate introductions to RCT, see Cropanzano & Folger, 1989; Folger, 1986, 1987, 1993; Folger & Cropanzano, 1998, 2001; Folger & Kass, 2000). Although an elaborate overview of the empirical work on RCT is beyond the scope of this article (for more complete descriptions, see, e.g., Cropanzano & Folger, 1989; Folger, 1986, 1987, 1993; Folger & Cropanzano, 1998, 2001; Folger, Rosenfield, Rheaume, & Martin, 1983), we discuss three classic RCT studies that are most closely related to the psychology of voice: Cropanzano and Folger (1989), Folger and Martin (1986), and Folger, Rosenfield, and Robinson (1983).

In these studies, all participants received an unfavorable outcome (i.e., they all failed to obtain a bonus of extra course credit). In each study, participants were made aware of the outcomes they would have obtained if things would have been different. That is, participants in the high-referent outcome conditions were led to believe that there was an alternative situation in which they would have received the favorable outcome (viz. the bonus). Participants

in the low-referent outcome conditions were informed that they would not have obtained the favorable outcome in either case. The manipulation of procedure in the studies by Folger and Martin (1986) and Folger, Rosenfield, and Robinson (1983) varied whether participants were given good reasons (high-justification conditions) or poor reasons (low-justification conditions) for why they failed to obtain the bonus. The procedure manipulation in the experiment by Cropanzano and Folger (1989) was such that participants were allowed (participant-decision condition) versus not allowed (experimenter-decision condition) to decide which of two types of tasks would be the task with which—depending on their performance on the task—they would have an opportunity to obtain the bonus. Findings of these studies indicated Outcome × Procedure interactions such that participants showed the highest levels of negative reactions (e.g., resentment and discontent) when high-referent outcomes were combined with bad procedures (viz. poor justifications in Folger & Martin, 1986, and Folger, Rosenfield, & Robinson, 1983, and experimenter-decision conditions in Cropanzano & Folger, 1989).

The Current Research

We argue here that RCT can be extended in three important ways. That is, we scrutinize below the operationalizations used in previous RCT studies and reason that by operationalizing (a) the procedure manipulation and (b) the referent manipulation in different ways than was done in previous RCT studies and (c) by *not* giving participants unfavorable outcomes, insights into the psychology of procedural justice and voice will be enlarged considerably.

The first way of extending RCT that we propose has to do with the manipulation of procedure used in previous RCT studies. More specifically, as has been argued by Bies (1987) and acknowledged by Cropanzano and Folger (1989), the justification conditions in the studies by Folger and Martin (1986) and Folger, Rosenfield, and Robinson (1983) differed in terms of the social accounts provided by the experimenter. As a consequence, the results of these studies "do not bear directly on the effects of different decision-making procedures per se but instead on the effects of different degrees of explanation for a decision" (Cropanzano & Folger, 1989, p. 294). In the experiment by Cropanzano and Folger (1989), procedure was operationalized by varying whether participants were allowed to decide which task they could complete. We argue here that it can be disputed whether this is a manipulation of procedure rather than a manipulation of decision control (cf. Thibaut & Walker, 1975): Although the Cropanzano and Folger study did not manipulate whether participants had control over the final decision (viz. whether to give participants the bonus), their procedure manipulation clearly varied the amount of decision control participants experienced (participants or the experimenter decided which task was completed). Furthermore, even if process control as opposed to decision control was manipulated in the Cropanzano and Folger study, it can be concluded that voice procedures have never been studied in previous RCT studies.

Studies conducted by Ambrose, Harland, and Kulik (1991) and Grienberger, Rutte, and Van Knippenberg (1997) manipulated procedure in similar ways as Cropanzano and Folger (1989) did. In addition to this, the Ambrose et al. and Grienberger et al. studies included a referent control manipulation: It was varied whether another participant was or was not allowed to decide which task to complete. Thus, none of the various RCT studies ever manipulated procedure, per se, prior to the studies by Cropanzano and Folger, Ambrose et al., and Grienberger et al. Moreover, even in those cases, the manipulations of procedure involved perceived control as "choice" rather than the procedural element of "voice" that is both more central to the canon of procedural justice studies as well as more removed from a purely instrumental interpretation. This lack of evidence regarding voice procedures is unfortunate given the potential value of RCT for the psychology of the voice effect. As a result, we do not know RCT's contribution to our understanding of one of the most important phenomena, if not the most important phenomenon, in the procedural justice domain: the voice effect. In the studies presented in this article, therefore, we operationalized procedure by varying whether participants were or were not allowed an opportunity to voice their opinion.

The second way in which we extend RCT involves the referent manipulation used in previous RCT studies. This manipulation is a crucial element in RCT's empirical work, making the theory significantly different from other justice theories. It should be noted here, however, that in previous studies this manipulation was induced by varying the ease with which participants could think of having obtained a better outcome than the outcome they actually got (see, e.g., the referent outcome manipulations in the studies by Cropanzano & Folger, 1989; Folger & Martin, 1986; Folger, Rosenfield, & Robinson, 1983) or a better procedure than they actually got (see, e.g., the referent control manipulations in the experiments by Ambrose et al., 1991, and Grienberger et al., 1997). Thus, the referent manipulation was induced by referring to outcome or procedure information. This suggests that referent manipulations should be induced by making reference to justice-related information (e.g., outcome or procedure information). We propose here, however, that reference points can be manipulated without referring to justice-related information. In other words, we argue that referent manipulations do not have to be confounded with justice-related information and that independently varying justice-unrelated reference points and justice-related information may strongly enhance the significance of RCT for the psychology of social justice.

For our manipulation of reference point, we go back to the roots of RCT: the work by Kahneman and Tversky (1982). These authors informed participants that two men, Crane and Tees, both missed their airplane flight. Closeness of reference point was induced by varying the time with which flights were missed: Crane had missed his flight by 30 min whereas Tees's flight took off just 5 min before he arrived at the airport. As predicted, the majority of participants (96%) thought that Tees would be more upset about missing his flight. Kahneman and Tversky argued that this was because the psychological distance between the actual situation of missing the flight and the simulated or referent situation of making the flight was smaller for Tees than for Crane.

In the current study, we varied whether participants were confronted with close or distant reference points. It is important to note here that participants in the Kahneman and Tversky (1982) study were not explicitly told what antecedents should have been different before Tees would have gotten on his flight. Thus, whereas participants in the high-referent conditions of previous RCT studies were explicitly told that if a different decision had been made they would have obtained a better outcome or proce-

dure, the classic manipulation by Kahneman and Tversky suggested that we did not have to explicitly inform participants in our close-reference-point conditions that an alternative situation would have yielded a different procedure or a different outcome. In other words, this implied that we could confidently manipulate closeness of reference points without explicitly referring to justice-related information (e.g., information about procedures or outcomes).

On the basis of the above-presented line of reasoning, we orthogonally manipulated reference point (close vs. distant) and procedure (voice vs. no voice) in the studies presented here. The concrete operationalizations are explained below, but for now we note that this design allowed us to investigate effects of procedure without giving participants information about outcome. This is important, we argue, because the previous empirical RCT work has been strongly outcome oriented. Besides the above-mentioned manipulation of procedure by means of choice (Cropanzano & Folger, 1989; cf. Ambrose et al., 1991; Grienberger et al., 1997) and the confound of referent manipulations and outcome information (e.g., Cropanzano & Folger, 1989; Folger & Martin, 1986; Folger, Rosenfield, & Robinson, 1983), this orientation is illustrated by the fact that all participants in the studies by Cropanzano and Folger (1989), Folger and Martin (1986), and Folger, Rosenfield, and Robinson (1983) received an unfavorable outcome (viz. failed to obtain the bonus).

The third way in which we go beyond the previous RCT studies is by not giving participants unfavorable outcomes. This is important because the fact that participants in all the previous RCT studies were given unfavorable outcomes suggests that in the previous studies it was assumed (perhaps implicitly) that experiencing unfavorable outcomes is a necessary condition before people start thinking about social justice issues. As was noted before, this assumption has been explicitly made in outcome-oriented articles on social justice (e.g., Rutte & Messick, 1995). However, we argue here that this distributive orientation is not an essential part of RCT's theorizing and hence—following Ockham's razor, which dictates that scientists should be as parsimonious as they can—should not constitute a crucial element in RCT's empirical work.

Experiment 1

A central element in RCT is that people compare what actually happened to what might have happened instead. This suggests that when alternative situations are clearly available it should be easier to imagine that something else could have happened than when alternatives are less clearly available. We used this to set up the reference point manipulation of Experiment 1.

Participants in Experiment 1 read and responded to stimulus information manipulated by means of scenarios. In these scenarios, participants were asked to imagine that they were members of a group of people (the blue team). Participants in the voice condition were informed that members of the blue team received an opportunity to voice their opinions about a decision that had to be made, and participants in the no-voice condition were told that members of the blue team did not receive such an opportunity. By means of the reference-point manipulation, we made it easier for participants in the close-reference-point condition to imagine that something else could have happened than for those in the distant-reference-point condition: In the close-reference-point condition, we informed participants that they had until recently been a member of a different group (the red team). In the distant-reference-point condition, participants were not told that they had been members of a different team. Following the above-presented line of reasoning, we argued that when a clear alternative situation is available (still being a member of the red team) it is easier for participants to imagine that something else could have happened (if only they would still have been a member of the red team) than in the distant-reference-point condition (in which there is no alternative previous group membership). As a consequence, we predicted that, following the RCT framework, procedure would more strongly affect reactions of participants in the close-reference-point condition than those in the distant-reference-point condition. More specifically, because RCT argues that particularly when a fairness rule is broken people's thinking becomes inherently referential, and because counterfactual thinking is stronger when people have received negative as opposed to positive information (e.g., Landman, 1987; Zeelenberg, Van den Bos, Van Dijk, & Pieters, in press; see also Folger & Kass, 2000; Olson, Buhrmann, & Roese, 2000), we expected that particularly in the no-voice conditions, participants' reactions would be affected by the manipulation of reference point.

Because it is important to measure affective elements in people's responses (Tyler & Smith, 1998; Van den Bos & Miedema, 2000; Vermunt, Wit, Van den Bos, & Lind, 1996; Weiss, Suckow, & Cropanzano, 1999), and because both Kahneman and Tversky (1982) and RCT studies have focused on negative affect (Folger & Cropanzano, 1998), the main dependent variables in Experiment 1 were participants' negative affective reactions to their procedure.

Method

Participants and design. Sixty students (17 men and 43 women) at Leiden University participated in the experiment and were paid for their participation. Participants were randomly assigned to one of the conditions of the 2 (procedure: voice vs. no voice) × 2 (reference point: close vs. distant) factorial design. The design was balanced with 15 participants assigned to each of the four conditions.

Experimental procedure. Participants read the scenario and answered the questions that constituted the dependent variables after participating in two other, unrelated experiments. The experiments lasted a total of 1 hr, and participants were paid 10 Dutch guilders (1 Dutch guilder equaled approximately $0.50 U.S. at the time the studies in this article were conducted). On arrival at the laboratory, participants were led to separate cubicles, each of which contained a computer with a monitor and a keyboard. The computers were used to present the stimulus information and to measure the dependent variables.

First, participants were asked to imagine the following situation:

> You work in an organization. In this organization, people work together in teams. There are 5 teams within the organization. These teams are described by means of colors: the red team, the blue team, the yellow team, the green team, and the orange team. Because the organization has performed well, it is announced that a bonus of 10,000 Dutch guilders will be distributed among all employees. A certain amount of money has been allocated to your team. The management of the organization has to decide how this amount of money will be distributed.

This was followed by the manipulation of reference point. Participants in the close-reference-point condition read the following sentence: "Yesterday morning you were a member of a different team, the red team, but

since yesterday afternoon you are a member of the blue team." Participants in the distant-reference-point condition read the following: "You are a member of the blue team."

This was followed by the manipulation of procedure.[1] Participants in the voice condition read the following two sentences: "The management has decided to give voice to the members of the blue team: The members of the blue team may give their opinion about how the money should be distributed." Participants in the no-voice condition read the following statement: "The management has decided to give no voice to the members of the blue team: The members of the blue team may not give their opinion about how the money should be distributed."

After participants had read the scenario, they were asked questions pertaining to the dependent variables. Following Van den Bos and Spruijt (in press), we asked participants of Experiment 1 for their negative affective reactions to their procedure by asking them on 7-point scales to what extent they were angry (1 = *not at all angry*, 7 = *very angry*), furious (1 = *not at all furious*, 7 = *very furious*), disappointed (1 = *not at all disappointed*, 7 = *very disappointed*), and sad (1 = *not at all sad*, 7 = *very sad*) about the way they were treated. Participants' answers on these four questions were averaged to form a reliable index of negative procedural affect (α = .89). When the participants had answered these questions, they were thoroughly debriefed, paid, and thanked for their participation.

Results

The means and standard deviations of the negative procedural affect scale of Experiment 1 are presented in Table 1. A 2 (procedure) × 2 (reference point) analysis of variance (ANOVA) on this scale showed only a main effect of procedure, $F(1, 56) = 106.42$, $p < .001$, and an interaction effect, $F(1, 56) = 5.56$, $p < .03$. The main effect of procedure indicated that participants who received an opportunity to voice their opinion were less negative about the procedure than participants who received no such opportunity were. More important, however, the interaction effect showed that the effect of procedure was stronger in the close-reference-point condition, $F(1, 56) = 80.33$, $p < .001$, $\eta^2 = .59$, than in the distant-reference-point condition, $F(1, 56) = 31.66$, $p < .001$, $\eta^2 = .36$. In further correspondence with what was expected, the results show that the difference between the close- and distant-reference-point conditions was stronger in the no-voice condition, $F(1, 56) = 5.25$, $p < .03$, than in the voice condition, $F(1, 56) = 1.09$, *ns*.

Discussion

The findings of Experiment 1 support our line of reasoning: When it is easier for people to imagine that something else could have happened (e.g., because they have until recently been a member of a different group) their reactions are more affected by manipulations of procedure than when it is less easy to imagine something else could have happened (e.g., because there is no previous group). In other words, when people are confronted with a close reference point, they are more affected by variations in procedure than are participants who have been subjected to a distant reference point. In further correspondence with what was predicted, especially strong effects of reference point were found when people did not get an opportunity to voice their opinion. Before strong conclusions could be drawn on the basis of these findings, however, it was important to replicate them in a second experiment.

Table 1
Means and Standard Deviations of Ratings of Negative Procedural Affect as a Function of Procedure and Reference Point (Experiment 1)

	Reference point			
	Close		Distant	
Procedure	*M*	*SD*	*M*	*SD*
Voice	1.6	0.8	2.0	1.0
No voice	4.6	0.9	3.8	0.9

Note. Means are on 7-point scales, with higher values indicating higher ratings of negative procedural affect.

Experiment 2

Participants in Experiment 1 read a scenario and responded to this hypothetical situation. One might wonder whether similar results would be obtained if participants were exposed to a situation in which they directly experienced a procedure and a reference point. As a second test of our predictions, therefore, the same independent variables were manipulated in such an experiment. To rule out alternative explanations for the findings of Experiment 1, we used a different operationalization of reference point in Experiment 2. We derived this manipulation from the time manipulation in the Kahneman and Tversky (1982) study, in which stimulus persons just (close reference point) or clearly (distant reference point) missed their flights.

In Experiment 2, all participants were informed that they were assigned to one of two groups. Furthermore, participants in the voice condition were informed that members of their group received an opportunity to voice their opinions, and participants in the no-voice condition were told that members of their group did not receive such an opportunity. In the close-reference-point condition, participants were assigned to their group just before the procedure manipulation was induced. In the distant-reference-point condition, participants were assigned to their group at the beginning of the experiment. We reasoned that participants in the close-reference-point condition (who just had been assigned to their group) would find it easier to imagine that something else could have happened (if only group assignment would have turned out differently) than participants in the distant-reference-point condition (who had been assigned to their group quite a while ago). Therefore, following the RCT framework, we predicted that procedure would more strongly affect participants' reactions in the close- as opposed to the distant-reference-point conditions. We further expected, following the line of reasoning presented in the introduction of Experiment 1, that participants' reactions would be affected by the manipulation of reference point particularly in the no-voice conditions.

Because we wanted to present data on various human reactions and because justice judgments are typically measured in studies on

[1] In the experiments presented here we did not inform participants about the reasons why events happened. This was done because, as argued earlier, we wanted to manipulate voice versus no-voice procedures rather than justifications, being the manipulation of procedure used in previous RCT studies (Folger & Martin, 1986; Folger, Rosenfield, & Robinson, 1983).

social justice and are important in RCT (Cropanzano & Folger, 1989; Folger & Martin, 1986; Folger, Rosenfield, Rheaume, & Martin, 1983; Folger, Rosenfield, & Robinson, 1983), the main dependent variables in Experiment 2 were participants' judgments of procedural justice.

It has been argued extensively that people use frames of reference when forming justice judgments (e.g., Adams, 1965; Blau, 1964; Folger, 1986; Homans, 1961; Stouffer et al., 1949; Thibaut & Kelley, 1959). The manipulation of reference point was intended to make frames of reference more cognitively available in the close-reference-point condition than in the distant-reference-point condition. By means of the reference-point manipulation, we therefore tried to establish that participants in the close-reference-point condition would find it easier and hence would need less time to form procedure judgments than participants in the distant-reference-point condition would. As a check on the manipulation of reference point, therefore, the time participants needed to answer the procedural justice judgments was measured.

Method

Participants and design. Eighty-eight students (40 men and 48 women) at Leiden University participated in the experiment and were paid for their participation. Participants were randomly assigned to one of the conditions of the 2 (procedure: voice vs. no voice) × 2 (reference point: close vs. distant) factorial design. The design was balanced, with 22 participants assigned to each of the four conditions.

Experimental procedure. Participants were invited to the laboratory to participate in a study on how people perform tasks. On arrival at the laboratory, participants were led to separate cubicles, each of which contained a computer with a monitor and a keyboard. Participants were told that the computers were connected to one another and that the experimenter could communicate with them by means of the computer network. The computers were used to present the stimulus information and collect data on the dependent variables and the manipulation checks. Participants participated in the experiment before participating in another, unrelated experiment. The experiments lasted a total of 1.5 hr, and participants were paid 15 Dutch guilders.

In the first part of the instructions, the experimental procedure was outlined to the participants: After the experimental tasks were explained, participants would practice the tasks for 2 min, after which time they would work on the tasks for 10 min in a first work round and for 20 min in a second work round. All participants were then informed that this study consisted of two substudies, which they were told were different, yet comparable with each other in several aspects. Participants were further told, by means of the computer network, that at some point in the study they would be assigned to one of the two substudies. (In reality, however, all stimulus information was preprogrammed, a procedure to which none of the participants objected on debriefing.) Participants in the distant-reference-point condition were then informed that they were assigned to the second substudy.

The task was then explained to the participants. Figures would be presented on the upper right part of the computer screen. Each figure consisted of 36 squares, and each square showed one of eight distinct patterns. On the upper left side of the computer screen, one of the eight patterns would be presented, and participants had to count the number of squares with this pattern in the figure on the right side of the screen. When participants indicated the correct number of patterns in the figure on the right side of the screen, another figure and another pattern would be presented on the screen. In the practice and work rounds, the number of tasks that the participant had completed (i.e., the number of figures that the participant had counted) in the present round were presented on the lower right side of the screen. On the lower left side of the screen, the time remaining in the present round was shown. The practice round then began, after which the first work round began. After the first work round had ended, participants were told how many tasks they had completed in this round. Participants in the close-reference-point condition were then informed that they were assigned to the second substudy.

The procedure that participants received was then manipulated.[2] In the voice condition, the experimenter told participants (by means of the computer network; cf. Lind et al., 1990; Van den Bos & Spruijt, in press; Van den Bos et al., 1996):

> I have decided that participants of the second substudy will receive an opportunity to voice their opinion about the number of tasks they should perform in the second work round and will be asked to type in their opinion about the number of tasks they should perform in the second work round. So please type in the number of tasks that you think you should perform in the second work round.

In the no-voice condition, the experimenter told participants:

> I have decided that participants of the second substudy will not receive an opportunity to voice their opinion about the number of tasks they should perform in the second work round and that they will not be asked to type their opinion about the number of tasks they should perform in the second work round. So you will not be asked to type in the number of tasks that you think you should perform in the second work round.

Participants were then asked questions pertaining to the dependent variables and the manipulation checks. Following Van den Bos (1999), we solicited procedural justice judgments by asking participants how just (1 = *very unjust*, 7 = *very just*), fair (1 = *very unfair*, 7 = *very fair*), and appropriate (1 = *very inappropriate*, 7 = *very appropriate*) they judged the way in which they were treated. Participants' answers on these three questions were averaged to form a reliable index of procedural justice judgments (α = .94). By means of the manipulation of reference point, we tried to establish that participants in the close-reference-point condition would find it easier and hence would need less time to form their procedure judgments than participants in the distant-reference-point condition did. As a check on the manipulation of reference point, therefore, we measured the time participants needed to answer the three procedure judgments of Experiment 2 with the computers. Because the distributions of response latencies generally were positively skewed, we applied logarithmic transformations. Participants' transformed response latencies were averaged to form a reliable scale of their response latencies (α = .79).[3] To check whether the procedure had been perceived as intended, we asked participants to what extent they agreed with the statement that they had been given an opportunity to voice their opinion about the number of tasks they should perform in the second work round (1 = *strongly disagree*, 7 = *strongly agree*) and to what extent they agreed with the statement that they had not been given an opportunity to voice their opinion about the number of tasks they should perform in the second work round (1 = *strongly disagree*, 7 = *strongly agree*). To further validate the manipulation of procedure (cf. Tyler, 1987), participants' perceptions of due consideration

[2] Reference point was manipulated in Experiment 2 by varying the time of telling people their group membership. It is important to note here that, in all conditions of Experiment 2, reference point was manipulated before the procedure manipulation was induced. Thus, order of manipulations (cf. Van den Bos, Vermunt, & Wilke, 1997) cannot explain the findings reported here.

[3] Analyses of variance (ANOVAs) were conducted on this scale of transformed response latencies. To make interpretation of the ANOVA effects easier and more understandable, we present the relevant means and standard deviations in seconds, and not in logarithmically transformed values.

were solicited: Participants were asked to what extent the experimenter had paid attention to their opinions (1 = *very weak*, 7 = *very strong*). When the participants had answered these questions, they were told that the second work round was cancelled, after which they were thoroughly debriefed, paid, and thanked for their participation.

Results

Manipulation checks. A 2 × 2 multivariate analysis of variance (MANOVA) on the two manipulation checks of procedure (the voice check and the no-voice check) yielded only main effects of procedure at both the multivariate level and the univariate levels: multivariate $F(2, 83) = 136.98$, $p < .001$; for the voice check, $F(1, 84) = 276.55$, $p < .001$; for the no-voice check, $F(1, 84) = 135.84$, $p < .001$. Participants in the voice condition agreed more with the statement that they had received an opportunity to voice their opinion ($M = 6.0$, $SD = 1.3$) than did participants in the no-voice condition ($M = 1.6$, $SD = 1.2$). Participants in the no-voice condition agreed more with the statement that they had not received an opportunity to voice their opinion ($M = 5.8$, $SD = 1.8$) than did participants in the voice condition ($M = 1.8$, $SD = 1.4$). It can be concluded that the procedure manipulation was successfully operationalized and was induced independently of the manipulation of reference point.

A 2 × 2 ANOVA on participants' perceptions of due consideration showed only a main effect of procedure, $F(1, 84) = 82.77$, $p < .001$. Participants in the voice condition believed the experimenter had paid more attention to their opinions ($M = 4.3$, $SD = 1.6$) than participants in the no-voice condition did ($M = 1.5$, $SD = 1.2$). This yields corroborative evidence that the manipulation of procedure was successfully operationalized and was induced independently of the reference-point manipulation.

By means of the manipulation of reference point, we tried to establish that participants in the close-reference-point condition found it easier and hence needed less time to form their procedure judgments than participants in the distant-reference-point condition did. In correspondence with this, a 2 × 2 ANOVA on the response latencies scale showed only a main effect of reference point, $F(1, 84) = 12.57$, $p < .01$. As expected, participants in the close-reference-point condition needed less time to respond to the procedure judgments questions ($M = 7.0$ s, $SD = 4.4$) than participants in the distant-reference-point condition did ($M = 10.3$ s, $SD = 7.7$). It can be concluded that the manipulation of reference point was successful in establishing the effects on participants' response latencies that we intended with this manipulation.

Additional variables. The number of tasks that participants completed in the practice round and the number completed in the first work round were subjected to a 2 × 2 MANOVA. This yielded no significant effects at either the multivariate level or the univariate levels. Independent of conditions, participants completed the same number of tasks in the practice round ($M = 21$, $SD = 6.2$) and completed the same number of tasks in the first work round ($M = 128$, $SD = 27.7$).

Participants who were allowed voice ($n = 44$) typed in their opinion about the number of tasks they should perform in the second work round. As expected, a one-way ANOVA yielded no significant effect of reference point. Independent of reference point, participants thought they should perform the same number of tasks in the second work round ($M = 229$, $SD = 62.0$). It can be concluded that the manipulation of reference point had no unintended effects on participants' performance in the practice and first work rounds and on their voiced opinions.

Procedural justice judgments. The means and standard deviations of the procedural justice scale of Experiment 2 are presented in Table 2. A 2 × 2 ANOVA on this scale yielded only a main effect of procedure, $F(1, 84) = 20.06$, $p < .001$, and an interaction effect, $F(1, 84) = 4.57$, $p < .04$. The main effect of procedure showed that participants who received an opportunity to voice their opinion judged their procedure to be more just than did participants who received no voice opportunity. More interesting, however, was that the interaction effect showed that the effect of procedure was stronger in the close- than in the distant-reference-point conditions. In fact, in the close-reference-point condition, participants who received an opportunity to voice their opinion were significantly more positive about the way they were treated than were participants who received no such opportunity, $F(1, 84) = 21.88$, $p < .001$, whereas in the distant-reference-point condition, participants' judgments showed a similar but not statistically significant effect of procedure, $F(1, 84) = 2.74$, $p < .12$. In further correspondence with what was expected, the results show that the difference between the close and distant conditions was stronger in the no-voice condition, $F(1, 84) = 7.24$, $p < .01$, than in the voice condition, $F < 1$.

Discussion

The findings of Experiment 2 are supportive for our line of reasoning: When it is relatively easy for people to imagine that something else could have happened (e.g., if only group assignment, which has just happened, would have turned out differently) their justice judgments are more affected by manipulations of procedure than when it is less easy to imagine something else could have happened (e.g., because group assignment has happened quite a while ago). This suggests that when people are confronted with close reference points their perceptions of procedural justice are more affected by variations in procedure than when they are subjected to distant reference points. In further correspondence with our hypotheses, especially strong effects of reference point were found when people did not get an opportunity to voice their opinion.

Table 2
Means and Standard Deviations of Procedural Justice Judgments as a Function of Procedure and Reference Point (Experiment 2)

	Reference point			
	Close		Distant	
Procedure	*M*	*SD*	*M*	*SD*
Voice	5.5	1.3	5.4	1.1
No voice	3.8	1.1	4.8	1.4

Note. Means are on 7-point scales, with higher values indicating higher ratings of procedural justice.

General Discussion

Taken together, the findings of our two experiments suggest that people react more strongly to voice versus no-voice procedures when they have close as opposed to distant reference points. These findings were found using different manipulations of reference point. This is important because each manipulation of reference point was designed to rule out alternative explanations of the other reference-point manipulation. The fact that we obtained similar results in both experiments suggests that the effects reported are robust and are not contingent on details of the manipulations used here. Robustness is also indicated by the fact that we obtained similar findings when people reacted to a hypothetical procedure and reference point (Experiment 1) and when they directly experienced a procedure and reference point (Experiment 2), and when judgments of negative procedural affect (Experiment 1) and procedural justice (Experiment 2) served as dependent variables. Furthermore, because RCT argues that particularly when a fairness rule is broken people's thinking becomes inherently referential, and because counterfactual thinking is stronger when people have received negative as opposed to positive information (e.g., Landman, 1987; Zeelenberg et al., in press; see also Folger & Kass, 2000; Olson et al., 2000), we expected that especially in the no-voice conditions participants' reactions would be affected by the manipulation of reference point. The findings of our two experiments are in correspondence with this prediction in that in both experiments the strongest effects of reference point on procedure judgments were found in the no-voice conditions.

Main dependent variables in Experiments 1 and 2 were ratings of negative procedural affect and procedural justice, respectively. These dependent variables were measured because both have a prominent role in the literatures on social justice in general (see, e.g., Tyler & Smith, 1998; Van den Bos & Miedema, 2000; Vermunt et al., 1996; Weiss et al., 1999) and RCT in particular (see, e.g., Cropanzano & Folger, 1989; Folger & Cropanzano, 1998; Folger & Martin, 1986; Folger, Rosenfield, Rheaume, & Martin, 1983; Folger, Rosenfield, & Robinson, 1983). The fact that we have obtained supportive evidence for our hypotheses on both types of dependent variables increases confidence in our findings and indicates that RCT can be used to predict and explain both people's affective reactions and their justice judgments.[4]

By means of the procedure manipulation, we tried to ensure that participants believed they either received or did not receive an opportunity to voice their opinions and that this manipulation of procedure would be independent of the reference-point manipulation. Both manipulation-check findings and perceptions of due consideration that were assessed in Experiment 2 showed that the manipulation of procedure was induced as intended and was manipulated independently of the reference-point manipulation. Thus, participants in both close- and distant-reference-point conditions perceived the procedure manipulation as intended.

By means of the manipulation of reference point, we tried to establish that participants in the close-reference-point conditions would find it easier to form procedure judgments than participants in the distant-reference-point conditions did. We therefore reasoned that participants in the close-reference-point conditions would need less time to construct procedure judgments than would those in the distant-reference-point conditions. The manipulation-check findings presented in Experiment 2 suggested that participants indeed needed less time to give their procedure judgments in the close- as opposed to the distant-reference-point conditions. Thus, the manipulation of reference point was successful in affecting participants' response latencies in ways that were intended with this manipulation. This suggests that the manipulation of reference point was successfully operationalized. These are important findings, we think, because both the justice literature (see, e.g., Van den Bos, Vermunt, & Wilke, 1997) and the literature on counterfactual thinking (see, e.g., Zeelenberg et al., 1998) typically do not provide data on the processes they are talking about. Furthermore, assessing participants' response latencies by means of computers is a nice, inconspicuous way to get at the psychological processes researchers are interested in, without any chance that this will affect these processes (cf. Folger & Cropanzano, 2001).

It should be noted here, however, that it was conceivable that participants' response latencies could be affected by the procedure or the combination of procedure and reference point that participants experienced. Such effects, however, were not observed here, and in fact typically are not found in procedural justice experiments. This may have to do with the difficulty of reliably assessing people's response latencies and/or with the psychological explanations that RCT proposes. What the present findings do show, however, is that response latencies were affected by the manipulation of reference point in ways we intended with this manipulation, and that procedure judgments were affected by the combination of reference point and procedure in ways that were predicted by the RCT framework. We hope that the present findings may serve as a starting point for future justice studies to tap more directly the processes they are interested in.

Whereas participants in the high-referent conditions of previous RCT studies (Cropanzano & Folger, 1989; Folger & Martin, 1986; Folger, Rosenfield, & Robinson, 1983) were explicitly told that if a different decision had been made they would have obtained a favorable outcome, the classic work by Kahneman and Tversky (1982) suggested that we did not have to explicitly inform participants in our close-reference-point conditions that an alternative situation would have yielded a different procedure or a different outcome. This implied that we could confidently manipulate closeness of reference points without explicitly referring to justice-related information (e.g., information about procedures or outcomes). Thus, in our close-reference-point conditions, clear alternative situations were available (viz. still being a member of the previous group in Experiment 1 and having been assigned to a different group in Experiment 2), but we did not explicitly inform participants that the alternative situation would have yielded them a different procedure or a different outcome. Following Kahneman and Tversky (1982), our findings show that operationalizing closeness of reference points in this way yields hypothesized effects on participants' reactions. An interesting implication of the findings

[4] Note that when both types of dependent variables are included in justice experiments, affect and judgments of justice typically are highly correlated. For example, in a recently conducted experiment by Miedema, Van den Bos, and Vermunt (2001), $|rs| > .58$, $p < .001$. In the three experiments by Van Prooijen, Van den Bos, and Wilke (2000), $|rs| > .64$, $ps < .001$. In the two experiments by Van den Bos and Lind (in press), $|rs| > .68$, $ps < .001$.

by Kahneman and Tversky (1982) and those in the current article therefore may be that having the opportunity to think of what "might have been" is sufficient for people to imagine themselves how things could have been better or worse. It is not necessary to inform people explicitly that alternative situations would have yielded them better or worse procedures or outcomes. This suggests that referent thinking may play a broader, more important role in the psychology of justice than has been proposed in previous RCT studies.

In Experiment 2, no statistically significant procedure effects were found in the distant-reference-point condition, whereas significant effects were found in Experiment 1 (albeit weaker than those in the close-reference-point condition). These differential results between experiments may have been caused by the fact that we measured different variables in the two experiments. That is, it has been reported before (see, e.g., Folger, Rosenfield, Rheaume, & Martin, 1983; Van den Bos & Spruijt, in press) that judgments related to fairness and justice (cf. Experiment 2) are less easily affected by experimental manipulations than measures of affect are (cf. Experiment 1). It should be emphasized here, however, that although there are some differences between the results of our two experiments, findings of both Experiments 1 and 2 suggest that the voice effect in close referent situations is one in which being denied voice leads to more negative reactions, whereas the presence of voice does not necessarily lead to more positive reactions. This is a pattern that has been reported before in the research literature (e.g., Van den Bos & Spruijt, in press): If (depending on the levels of a moderator variable) people are significantly affected by voice versus no-voice procedures, no-voice conditions frequently lead to more negative reactions as opposed to voice conditions leading to more positive judgments. This suggests that what typically is labeled as *the voice effect* (suggesting more positive judgments following voice) might actually be thought of more properly as a *no-voice effect* (indicating more negative judgments following no voice).

We think that the no-voice effect may be an interesting and important new concept in the psychological literature on voice and procedural justice. For example, it explains why in Experiment 2 we found that participants who received no voice opportunity in the distant condition judged their procedure to be nearly as positive as those who received voice: This line of reasoning suggests that we found an absence of the no-voice effect. Furthermore, this also may help to explain that significant effects of reference point were found in the no-voice conditions of both experiments reported here, whereas no statistically significant effects were found in the voice conditions. More generally, the concept of the no-voice effect is in correspondence with some notions that have been put forth (cf. Folger, 1984) that, whereas we scientists tend to think and talk about the psychology of justice, unjust events affect lay people's cognitions and reactions more strongly than just events (Brockner & Wiesenfeld, 1996; Van den Bos, Vermunt, & Wilke, 1997), suggesting that injustice plays a more prominent role and that it might be better to talk about the psychology of injustice as opposed to justice (Folger & Cropanzano, 1998). Furthermore, it is worthwhile to note here that there are other areas of research within social psychology (e.g., person perception) in which negative information has been found to have more impact on people's reactions than positive information does (e.g., Skowronski & Carlston, 1989). Future research on social justice and injustice may profit from insights developed in these areas of social psychology.

We reasoned that it made sense to begin our line of research by performing empirical studies that enabled us to study the processes we identified as clearly as possible and to achieve high internal validity and experimental control: that is, experimental studies. In our studies, we tried to achieve acceptable levels of external validity. One way we tried to do this was to use stimulus materials that had real-life characteristics and that were important for our participants (and debriefing interviews indicated that we were successful in this). Care must be taken, however, in generalizing from the experimental findings presented here to real-world settings. Although we are convinced that our analysis of the psychology of voice is generalizable to other social contexts and operationalizations (see, e.g., Van den Bos, 1999), future researchers may want to explore the boundary conditions of the effects reported here. Furthermore, the present experiments showed a confirmation of a pattern of effects of some importance that happened to have been predicted by RCT, but these patterns have implications that hold whether or not this theory holds up in the long run. Other theories may be worthwhile to pursue in future studies. However, what matters most is that the present experiments suggest that particular effects may occur. This may stimulate future researchers to explore other operationalizations, other research methods, and other theories. As research accumulates concerning the limiting conditions of voice and no-voice effects, as it has in this study and in other studies (see, e.g., Folger, 1977; Lind et al., 1990; Tyler, 1987), researchers begin to understand not only when voice and no-voice effects disappear, but also a great deal about why they occur at all and why they are so potent when they do occur. This knowledge in turn promises to advance our understanding of fundamental issues in the social psychology of justice and of the role of justice-related phenomena in basic social relations.

We have argued that previous RCT studies show a distributive orientation. This is indicated by the fact that all participants in previous RCT studies by Cropanzano and Folger (1989), Folger and Martin (1986), and Folger, Rosenfield, and Robinson (1983) received unfavorable outcomes, which suggests that RCT assumed that experiencing unfavorable outcomes is a necessary condition before people start thinking about social justice issues (cf. Rutte & Messick, 1995). Furthermore, previous RCT manipulations of reference point were often induced by referring to outcome information, and manipulations of procedure were less removed from a purely instrumental interpretation than the current manipulations of voice. Our findings show that manipulating outcome-unrelated reference points (instead of outcome-related reference points; Cropanzano & Folger, 1989; Folger & Martin, 1986; Folger, Rosenfield, & Robinson, 1983) independently of voice procedures (instead of procedural justifications; Folger & Martin, 1986; Folger, Rosenfield, & Robinson, 1983; or control; Cropanzano & Folger, 1989) without giving participants unfavorable outcomes is sufficient to yield the effects RCT proposes. This suggests that the implicit distributive focus in previous empirical RCT work is not warranted and hence should not constitute a central element in the RCT framework.

To put it differently, previous RCT studies implicitly suggested that distributive information is essential in the psychology of social justice. This assumption explicitly has been made in outcome-

oriented articles (e.g., Rutte & Messick, 1995). Our findings show, however, that RCT is a theory about not only distributive justice but procedural justice as well. Thus, it can be concluded that RCT is a theory about social justice (including both procedural and distributive justice). This is an important implication of our study because, now that it has been established that RCT is a theory about both procedural and distributive justice, scientists can start using the framework to provide new perspectives on the study of conditions under which one type of justice is more important than the other and vice versa (e.g., Van den Bos, Vermunt, & Wilke, 1997) and how the two types of justice relate to each other (cf. Brockner & Wiesenfeld, 1996). We hope that the current article—together with other important developments (e.g., Brockner & Wiesenfeld, 1996)—may stimulate researchers to focus on these important questions.

But, to return to the present study, the current article fits into a tradition of important social-justice research in which suggestions can be found that when people are forming fairness judgments they do so by referring to reference points (see, e.g., Adams, 1965; Blau, 1964; Homans, 1961; Stouffer et al., 1949; Thibaut & Kelley, 1959). This suggests that insight into the role of reference points is essential for understanding the psychology of justice. In recent years, however, less and less attention has been paid to this issue. In the current article, we try to show that the psychology of procedural justice in general and voice in particular depends in large part on reference points. We have done this by focusing on RCT as the most recent and most thorough framework dealing with reference points in the justice domain. The experiments presented here expand RCT and point to the important role of closeness of reference points in the psychology of voice.

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