

When Do We Need Procedural Fairness? The Role of Trust in Authority

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On the basis of fairness heuristic theory, the authors argue that when information about whether an authority can be trusted is not available, people will resolve the question of how they should interpret the decisions of the authority by relying on perceived procedural fairness. As a consequence, people who do not have information about authority's trustworthiness react more positively toward the outcomes of authority's decisions if the authority is using fair as opposed to unfair procedures. However, when people know that the authority either can or cannot be trusted, they are less in need of procedural fairness information, yielding less strong effects of procedural fairness on people's reactions. The findings of 2 experiments support the authors' line of reasoning. It is concluded that people especially need procedural fairness when information about an authority's trustworthiness is lacking.

A crucial precondition for authorities to function effectively and efficiently is to establish positive attitudes toward and voluntary acceptance of their decisions (Tyler & DeGoey, 1996). Social psychologists have recognized that if authorities must continually explain and justify their decisions, their ability to manage effectively is diminished (Gamson, 1968; Tyler, in press; Tyler & Lind, 1992). In this article, we try to further our knowledge of the psychology of people's perceptions and voluntary acceptance of decisions made by authorities. More specifically, because it has been suggested that people who experience procedural justice are more likely to voluntarily accept authorities' decisions (Tyler, Boeckmann, Smith, & Huo, 1997; Tyler & Lind, 1992), we focus on the importance of procedural justice concerns in determining when the outcomes of authorities' decisions will be accepted. We present studies in which the main dependent variables were how satisfied people were with an outcome they received from an authority and how fair they judged this outcome to be. We base our line of reasoning on recent insights in the domain of social justice research.

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One of the most striking contributions of the work on social justice, and one of the most frequently replicated findings in the area of social psychology, has been the discovery that perceived procedural fairness positively affects how people react to the outcomes they receive from authorities (Folger, 1977; Folger, Rosenfield, Grove, & Corkran, 1979; Tyler, 1990; Walker, Latour, Lind, & Thibaut, 1974). Following Folger et al., several authors have labeled this as an instance of the fair process effect (Greenberg & Folger, 1983; Van den Bos, Lind, Vermunt, & Wilke, 1997; Van den Bos, Lind, & Wilke, in press; Van den Bos, Vermunt, & Wilke, 1997; Van den Bos, Wilke, Lind, & Vermunt, 1998). Fair process effects have been found in people's reactions to outcomes they receive from various authorities, such as organizational authorities (Folger & Konovsky, 1989), police authorities (Tyler & Folger, 1980), political authorities (Tyler & DeGoey, 1995), and authorities in court trials (Lind, Kulik, Ambrose, & De Vera Park, 1993). An illustration of this effect can be found in Lind, Kanfer, and Earley's (1990) research. In the experiment presented in that article, the researchers manipulated whether participants were or were not allowed an opportunity to voice their opinion about an outcome they would receive. As expected, participants judged the voice procedure to be more fair than the no-voice procedure. More important, the findings also revealed a fair process effect: Participants who were allowed voice judged their outcome as more fair than those who were not allowed voice.

The frequently replicated fair process effect suggests that if authorities allocate outcomes by means of fair procedures, recipients are more willing to voluntarily accept the outcomes the authorities give to them, and as a consequence will judge the outcomes that result from the authority's decisions more positively, as compared with when authorities use unfair procedures in the allocation process (Tyler & DeGoey, 1996). The primary aim of the current article is to try to enhance the insights in the psychology of the fair process effect. To achieve this purpose we expand on fairness heuristic theory, a theory that we have recently developed. Compared with explanations by others as well as by our own previous work, the analysis of the

psychology of the fair process effect that we put forward in the present article may yield an as yet unidentified and unexplored explanation of this effect (for an overview of other explanations, see Greenberg & Folger, 1983; Lind & Tyler, 1988; Van den Bos, Lind, et al., 1997; Van den Bos, Vermunt, & Wilke, 1997; Van den Bos et al., 1998).

Fairness Heuristic Theory

Fairness heuristic theory makes a number of novel predictions about the psychology of procedural and distributive justice (for overviews, see Lind, in press; Lind et al., 1993; Van den Bos, Lind, et al., 1997; Van den Bos, Vermunt, & Wilke, 1997; Van den Bos et al., 1998). As Van den Bos et al. (in press) have noted, one element of the theory that has not been directly investigated thus far is when fairness is important for people. Answering this question is of crucial importance for any theory about justice (Leventhal, 1980), and therefore we focus on this question in the present article.

Fairness heuristic theory recognizes that in several situations fairness is a salient issue. More specifically, the theory proposes that people especially need fairness judgments when they are concerned about potential problems associated with social interdependence and socially based identity processes. These problems are related to what has been termed the *fundamental social dilemma* (e.g., Lind, 1995). In essence, this dilemma is concerned with the question of whether one can trust others not to exploit or exclude one from important relationships and groups (cf. Huo, Smith, Tyler, & Lind, 1996; Lind & Tyler, 1988; Smith, Tyler, Huo, Ortiz, & Lind, 1997; Tyler & Lind, 1992).

An important subgroup of social relations are authority processes. Based on the work by Tyler and Lind (1992; Tyler & DeGoe, 1996), fairness heuristic theory argues that, because ceding authority to another person raises the possibility of exploitation and exclusion, people frequently feel uneasy about their relationship with authorities and about the outcomes they receive from the authority. We propose here that this line of reasoning suggests that when people are trying to find out how to react to an outcome they received from an authority, they want to have information about whether they can trust the authority.

However, one may also ask: Do people often have such direct information about an authority's trustworthiness? We suggest that they frequently do not. Furthermore, we propose here that if people do not have information about authority's trustworthiness, they still are interested in trying to find out how to judge the outcome they received from the authority. How do they do this? We suggest that in such situations—in which information about authority's trustworthiness is missing—people refer to the fairness of authority's procedures to decide how to react to the outcome they received from the authority. In other words, in situations in which definitive trust information is lacking, procedural fairness serves as a heuristic substitute in the process of deciding how to judge an outcome one has received from the authority. As a consequence, we expect that when people do not have information about authority's trustworthiness, they react more positively toward the outcome they received from the authority when the authority has been using a fair as opposed to an unfair procedure.

On the other hand, this line of thought also suggests that when people do have direct, explicit information about authority's trustworthiness, they may be less in need of procedural fairness as a heuristic substitute. We therefore expect that if people do not have information about authority's trustworthiness, recipients react more positively toward the outcome when the authority has been using a fair as opposed to an unfair procedure, but that less strong fair process effects will be found if people have received direct information about authority's trustworthiness (Hypothesis 1).

Besides the above-mentioned reasons, there are at least two other reasons why it may be interesting to investigate this hypothesis. One reason is that in previous research on trust and procedural and distributive justice (for overviews, see Tyler & Lind, 1992; Brockner, Siegel, Daly, Tyler, & Martin, 1997) independent and dependent variables were measured by means of self-reports at the same point in time. This makes it difficult to determine the causal impact of the independent variables on the dependent variables. In the current article, therefore, we present findings of laboratory experiments in which the independent variables were manipulated before the measurement of the dependent variables.

A second reason for conducting the research was stimulated by Tyler and Lind's work on trust and procedural justice (for an overview see Tyler & Lind, 1992). In the appendix of their 1992 article, Tyler and Lind presented two studies in which trust was operationalized. The first study was conducted by MacCoun, Lind, Hensler, Bryant, and Ebener (1988), who measured trust by asking respondents to indicate their opinions to the statements whether "the arbitrator(s) were very ethical in their handling of this case" and whether "the arbitrator(s) gave thorough consideration to all the evidence." The second study that Tyler and Lind discussed was the Tyler (1990) research, in which trust was measured by the question "how hard did your supervisor try to be fair to you?" We argue here that ethicality, consideration, and even the intention to be fair are important and are certainly factors that might enhance trust, as they almost certainly enhance perceived fairness. However, none of the measures contain the word "trust" or any direct reference to the concept of trust. This suggests that these questions are an operationalization of trust that is not as direct as is possible. In the present article, we try to enhance insights about trust and justice by manipulating trust in a very direct way: Participants were informed that other persons hold the opinion that the authority can be trusted (positive-trust condition), cannot be trusted (negative-trust condition), or participants did not receive explicit information about authority's trustworthiness (trust-unknown condition).

Experiment 1

Participants in Experiment 1 read and responded to stimulus information manipulated by means of scenarios. In the scenarios, we varied whether participants knew that they could trust an authority, whether they knew that they could not trust the authority, or whether they had not received any information about whether or not they could trust the authority. This was followed with a fair versus unfair procedure manipulation, by

varying whether or not participants were allowed an opportunity to voice their opinion (cf. Lind et al., 1990). Participants' outcome and procedural satisfaction and fairness judgments were the dependent variables.

Method

Participants and design. One hundred thirty-two students (41 men and 91 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) \times 3 (trust: unknown vs. positive vs. negative) factorial design. The design was balanced with 22 participants assigned to each of the six conditions.

Experimental procedure. Participants read the scenario and answered the questions that constituted the dependent variables either before or after participating in other, unrelated experiments. The experiments lasted a total of 1 hour or 1.5 hrs, and participants were paid 10 or 15 Dutch guilders, depending on whether they participated for 1 hour or 1.5 hours (1 Dutch guilder equaled approximately \$0.60 U.S. at the time the studies presented 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 would like to spend 6 months in California to conduct research for your Master's thesis. The university you would like to attend is highly recommended. You will work together with highly esteemed professors. All of this offers you good career opportunities. Furthermore, California has some other advantages as well: for example, sun, sea, beach, and studying under palm trees. To pay for your stay and research in California, you apply for a grant at "Students Around the World" (SAW). To decide whether they will award you the grant, you will have to appear before the grant committee of SAW.

This was followed by the manipulation of trust. Participants in the trust-positive and trust-negative conditions read the following sentences (manipulated information in italics):

The evening before you go to SAW, a good friend of yours tells you that you *can/cannot* trust the members of the grant committee: He has had *good/bad* experiences with the members of the committee.

These sentences were not presented to the participants in the trust-unknown condition. This was followed by the manipulation of procedure. Participants read the following sentences (manipulated information in italics):

You appear before the committee. The committee gives you *voice/no voice*: The committee *asks you/does not ask you* to voice your opinion about the amount of money you think you need for your stay and research in California.

Then, all participants read the following:

A week after this you are informed about the amount of money you are allowed: You will receive 5,000 Dutch guilders.¹

After participants read the scenario, they were asked questions pertaining to the dependent variables. All ratings were made on 7-point scales. Outcome satisfaction was assessed by asking participants how

satisfied they were with the 5000 Dutch guilders that they received (1 = *very dissatisfied*, 7 = *very satisfied*). Outcome fairness judgments were solicited by asking participants how fair they considered the 5,000 Dutch guilders that they received (1 = *very unfair*, 7 = *very fair*). Procedural satisfaction was assessed by asking participants how satisfied they were with the procedure used to decide about the 5,000 Dutch guilders that they received (1 = *very dissatisfied*, 7 = *very satisfied*). Procedural fairness judgments were measured by asking participants how fair they considered the procedure used to decide about the 5,000 Dutch guilders that they received (1 = *very unfair*, 7 = *very fair*). When the participants had answered these questions, and had completed the other experiments in which they would participate, they were thoroughly debriefed and paid for their participation.

Results

Procedure judgments. To analyze participants' procedural judgments we conducted a 2 (procedure) \times 3 (trust) multivariate analysis of variance (MANOVA) on the two procedure judgments (satisfaction and fairness). This analysis yielded only main effects of procedure at both the multivariate level and the univariate levels: multivariate $F(2, 125) = 158.51, p < .001$; for procedural satisfaction, $F(1, 126) = 315.24, p < .001$; for procedural fairness, $F(1, 126) = 215.01, p < .001$. As was expected, participants who received an opportunity to voice their opinion were more satisfied with the procedure ($M = 5.8$), and judged the procedure as more fair ($M = 5.9$) than participants who did not receive a voice opportunity ($M_s = 2.6$ and 2.9, respectively).

Outcome judgments. The means of the outcome judgments in Experiment 1 are presented in Table 1. We performed three types of analyses on these judgments. The first two types of analyses were preliminary in nature, and in the third type of analyses we directly tested whether the data provided supportive evidence for our hypothesis.

First, we tested for overall effects in a 2 (procedure) \times 3 (trust) MANOVA. This MANOVA showed main effects of procedure at both the multivariate level and the univariate levels: multivariate $F(2, 125) = 5.98, p < .01$; for outcome satisfaction, $F(1, 126) = 4.90, p < .03$; for outcome fairness, $F(1, 126) = 11.93, p < .01$. These effects were qualified by significant interactions between procedure and trust at both the multivariate level and the univariate levels: multivariate $F(4, 250) = 3.60, p < .01$; for outcome satisfaction, $F(2, 126) = 6.68, p < .01$; for outcome fairness, $F(2, 126) = 5.72, p < .01$.

¹ Because some theorists (e.g., Brockner & Wiesenfeld, 1996) have argued that the valence or magnitude of the outcome affects procedural effects, it is worth noting that the 5,000 Dutch guilder outcome was intended to be neither unqualified good nor unqualified bad. The participants' appeared to have perceived it so: Participants were asked whether they thought 5,000 guilders was sufficient or insufficient to fund their stay and research in California (1 = *very insufficient*, 7 = *very sufficient*), and whether they considered 5,000 guilders to be a large or small amount of money to pay for their stay and research in California (1 = *very small*, 7 = *very large*). A 2 (procedure) \times 3 (trust) multivariate analysis of variance (MANOVA) on these two questions yielded no multivariate or univariate effects. Participants judged the 5,000 guilders to be somewhat insufficient ($M = 3.1$) and a somewhat small amount of money ($M = 3.7$) to pay for their stay and research in California.

Table 1
Mean Outcome Judgments as a Function of Procedure and Authority's Trustworthiness (Experiment 1)

Dependent variable	Procedure	Trust		
		Unknown	Positive	Negative
Outcome satisfaction	Voice	5.5 _a	4.8 _a	4.9 _a
	No voice	3.7 _b	4.9 _a	4.9 _a
Outcome fairness	Voice	5.7 _a	5.1 _{a,b}	5.0 _{a,b}
	No voice	3.8 _c	5.0 _{a,b}	4.5 _{b,c}

Note. Entries are means on 7-point scales; higher values indicate more positive ratings of the dependent variable in question. For each dependent variable, means with no subscripts in common differ significantly, as indicated by a least significant difference test for multiple comparisons between means ($p < .05$).

Second, before we conducted the analysis that was pertinent to Hypothesis 1, we checked whether participants' outcome judgments in the trust-positive condition were differently affected by procedure than were participants' outcome judgments in the trust-negative condition. The reason for this was that although our hypothesis focuses on the moderating effects of trust information (absent vs. present) on the relationship between procedural justice and outcome judgments, one may wonder whether positive trust information affects the relationship between procedural justice and outcome judgments differently than negative trust information does. Therefore, in the second type of analyses, we tested whether the interaction between procedure (voice vs. no voice) and a "trust valence contrast" (trust positive vs. trust negative) was significant. We analyzed this interaction contrast in a 2×3 MANOVA. This analysis only yielded main effects of procedure (as indicated above with the overall analysis), and no other significant effects. Thus, we found that procedure did not affect participants' outcome judgments differently in the trust-positive condition than in the trust-negative condition.

Third, and more important, we then tested the interaction contrast that was pertinent to Hypothesis 1. Hypothesis 1 was the prediction that procedure would affect outcome judgments more strongly when participants did not have information about authority's trustworthiness than when they did have trust information. In other words, Hypothesis 1 led us to expect an interaction between procedure and a knowledge-of-trust contrast that contrasted the trust-unknown condition with the mean of the trust-positive and trust-negative conditions (note that this contrast was orthogonal to the trust valence contrast we used in the second analysis). We analyzed this interaction contrast in a 2×3 MANOVA. This analysis showed main effects of procedure (as noted above with the overall analysis). More important, however, these effects were qualified by significant interactions between procedure and the knowledge of trust contrast: multivariate $F(2, 125) = 7.16, p < .01$; for outcome satisfaction, $F(1, 126) = 13.34, p < .001$; for outcome fairness, $F(1, 126) = 11.12, p < .01$. As predicted by Hypothesis 1, variations in procedure affected outcome judgments more strongly in the trust-unknown condition than in the trust-positive and the trust-

negative conditions. In fact, results of least significant difference tests for multiple comparisons between means ($p < .05$; see Table 1 for results) revealed that participants who did not know whether they could trust the authority judged their outcome as significantly more satisfying and more fair when they had received an opportunity to voice their opinion than when they had not received such an opportunity, but that the outcome judgments of participants who knew that the authority could be trusted or who knew that the authority could not be trusted did not differ significantly as a function of whether participants were or were not allowed a voice.

Discussion

The findings of Experiment 1 support our line of reasoning: As predicted by fairness heuristic theory, our findings indicate that people's reactions to an outcome they received from an authority are strongly affected by procedure fairness information when they do not know whether the authority can be trusted. This suggests that recipients who do not have information about authority's trustworthiness are more willing to accept the outcomes the authority gives to them if the authority allocates outcomes by means of fair instead of unfair procedures. However, when people know that the authority can or cannot be trusted they are less in need of procedural fairness information, yielding less strong fair process effects on the outcome judgments of these persons. This suggests that when direct information about an authority's trustworthiness is missing, people may resolve the question how they should interpret the outcome they have received from the authority by relying on perceived procedural fairness.

Before we draw strong conclusions on the basis of these findings, however, it is important to replicate them. In Experiment 1, participants read a scenario in which they were asked to imagine that they were involved in a situation and to judge how satisfying and fair the procedure and outcome were in this hypothetical situation. One might wonder whether similar results would have been obtained when people were exposed to a situation in which they directly experienced the satisfaction and fairness of a procedure and an outcome. In the experimental situation developed by Van den Bos, Lind, et al. (1997, Experiment 2; Van den Bos et al., 1998; Experiment 2), persons directly experience the satisfaction and fairness of a procedure and an outcome. As a second test of our predictions, therefore, the same independent variables were manipulated in a study using this experimental paradigm.

An additional aim of Experiment 2 was to find out whether participants perceived the positive and negative trust information as intended. That is, although informing participants that other persons hold the opinion that the authority can or cannot be trusted seems a very direct manipulation of positive and negative trust, one may argue that the results of Experiment 1 do not show whether the participants perceived the positive and negative trust information that is essential to our theory. In Experiment 2, therefore, we asked participants how they perceived the positive and negative trust information we gave them: We asked participants who received positive or negative trust information whether they trusted the authority in Experiment 2. Besides

measuring trust perceptions, we sought additional evidence that our operationalization of positive and negative trust information was successful: Because it has been argued that positive and negative trust affects people's voluntary compliance with authority's decisions (e.g., Tyler & DeGoey, 1996; Tyler & Lind, 1992), we were interested whether the positive and negative trust information we gave our participants would affect their voluntary compliance with the authority's decisions. This would yield corroborative evidence for our operationalization of the positive and negative trust information we gave our participants.

Experiment 2

Method

Participants and design. One hundred and thirty-eight students (43 men and 95 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) \times 3 (trust: unknown vs. positive vs. negative) factorial design. The design was balanced with 23 participants assigned to each of the six 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. Next to the monitor, participants found a piece of paper and a pencil. Participants were told that the computers were connected to one another and that persons in the cubicles could communicate with each other by means of the computer network. The computers were used to present the stimulus information and to collect data on the dependent variables and the manipulation checks. Participants took part in the experiment and answered the questions that constituted the dependent variables and the manipulation checks either before or after participating in other, unrelated experiments. The experiments lasted a total of 1 hr, and participants were paid 10 Dutch guilders.

In the first part of the instructions, participants were informed that they participated in the study with two other persons. One of them would be appointed to the position of supervisor, referred to as position A. The other two persons would be assigned to the positions of work performers, labeled as positions B and C. After this, positions were assigned. Participants were appointed to position B. Participants were informed that the person appointed to the position of A was given that appointment because of this person's experience in another, comparable study, and that the persons appointed to positions B and C were assigned to these positions because they had not had this experience. (In reality, however, all stimulus information was preprogrammed; a procedure to which none of the participants objected on debriefing.)

The experimental procedure was then 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. Furthermore, it was communicated to the participants that, after they had all taken part, a lottery would be held among all participants. The winner of this lottery would receive 100 Dutch guilders. (Actually, after all participants had completed the experiment, the 100 Dutch guilders were randomly given to 1 participant; a procedure to which none of the participants objected on debriefing.) Participants were informed that a total of 200 lottery tickets would be divided among all participants. Participants were further told that after the work round the person appointed to position A would divide some lottery tickets between them and C. Six practice questions were posed to ensure comprehension of the lottery. If participants gave a wrong answer to a question, the correct

answer was disclosed, and main characteristics of the lottery were repeated.

The task was then explained to the participants. Figures would be presented on the upper right 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 part of the screen. When participants had indicated the correct number of patterns in the figure on the right part of the screen, another figure and another pattern would be presented on the screen. In both the practice round and the work round, the number of tasks that the participant had completed (i.e., the number of figures that the participant had counted) in the present round would be 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 trust information that participants received was then manipulated. In the trust-positive and trust-negative conditions, participants were presented with fictitious survey results that persons who had participated with A in a former study allegedly had filled in. In the trust-positive condition, participants were informed that A had been rated as very trustworthy. A had received a mean score of 6.9 on a 7-point scale ranging from "not to be trusted at all" (1) to "to be trusted completely" (7). The majority of the scores had been 7, and the lowest score that A had received was 6. In the trust-negative condition, participants were informed that A has been rated as very untrustworthy. A had received a mean score of 1.1 on the 7-point scale. The majority of the scores had been 1, and the highest score given to A was 2. Two practice questions were posed to ensure comprehension of the trust information.² If participants gave a wrong answer to a question, the correct answer was disclosed, and main characteristics of the trust information were repeated. In the trust-unknown condition, participants did not receive information and did not answer the practice questions about A's trustworthiness.

The practice round then began, after which the work round began. After the work round had ended, participants were told how many tasks they had completed in the work round and, to ensure that participants compared themselves to C and to make this study comparable with our previous experiments (Van den Bos, Lind, et al., 1997; Van den Bos et al., 1998), it was communicated to the participant that C had completed an equivalent number of tasks. Participants were then told that A would divide the lottery tickets between them and C. After this, participants were asked to think for 1 min about the percentage of lottery tickets that they should receive relative to C and to write down this percentage on the piece of paper next to the computer. Participants were informed that at the end of the experiment the pieces of paper would be thrown away.

The procedure that participants received was then manipulated (cf. Van den Bos, Lind, et al., 1997; Van den Bos et al., 1998). In the voice condition, A allegedly asked participants, by means of the computer network, to type in their opinion about the percentage of tickets that they should receive relative to C. (In reality, however, all stimulus information was preprogrammed.) Participants in the no-voice condition were informed that they would not be asked to type their opinion about the percentage of tickets that they should receive relative to C.

It was then communicated to the participants that they had received

² Participants were asked whether the persons who had participated with A in the former study trusted A (1 = yes, 2 = no), and whether they themselves could trust A (1 = yes, 2 = no). Only one participant typed in a wrong answer to the first question, and all participants answered the second question correctly.

3 lottery tickets.³ After this, participants were asked questions pertaining to the dependent variables and manipulation checks. All ratings were made on 7-point scales. Outcome satisfaction was assessed by asking participants how satisfied they were with the 3 lottery tickets that they received (1 = *very dissatisfied*, 7 = *very satisfied*). Outcome fairness judgments were measured by asking participants how fair they considered the 3 lottery tickets that they received (1 = *very unfair*, 7 = *very fair*). Procedure satisfaction was measured by asking participants how satisfied they were with the procedure used to assess the number of tickets that they received (1 = *very dissatisfied*, 7 = *very satisfied*). Procedural fairness judgments were solicited by asking participants how fair they considered the procedure used to assess the number of tickets that they received (1 = *very unfair*, 7 = *very fair*). As was explained in the *Discussion* section of Experiment 1, we measured manipulation checks of the positive and negative trust information we gave our participants: We asked participants who received positive or negative trust information whether they trusted A (1 = *I definitely do not trust A*, 7 = *I definitely trust A*), to what extent they were willing to go along with the decisions made by A (1 = *I definitely am not willing to go along with the decisions made by A*, 7 = *I definitely am willing to go along with the decisions made by A*), to what extent they were willing to comply with the decisions made by A (1 = *I definitely am not willing to comply with the decisions made by A*, 7 = *I definitely am willing to comply with the decisions made by A*), and to what extent they were willing to help A (1 = *I definitely am not willing to help A*, 7 = *I definitely am willing to help A*). When the participants had answered these questions, and had completed the other experiments in which they would participate, they were thoroughly debriefed and paid for their participation.

Results

Trust-valence checks. The manipulation checks of the positive and negative trust information we gave our participants showed only main effects of valenced trust at the multivariate level and the univariate levels: multivariate $F(4, 85) = 3.18, p < .02$; for perceptions of trust, $F(1, 88) = 12.16, p < .01$; for going along with A's decisions, $F(1, 88) = 7.36, p < .01$; for compliance with A's decisions, $F(1, 88) = 7.33, p < .01$; for helping A, $F(1, 88) = 9.18, p < .01$. Main effects of procedure and interactions between procedure and valenced trust were not significant. As was expected, participants who received positive trust information trusted A more ($M = 4.3$) and were more willing to go along with A's decisions ($M = 4.3$), comply with A's decisions ($M = 4.2$), and help A ($M = 4.3$) than participants who received negative trust information ($M_s = 3.2, 3.5, 3.5$, and 3.3 , respectively). These results show that participants perceived the positive and negative trust information as intended. Furthermore, the findings show that the positive and negative trust information we gave our participants was able to affect voluntary compliance with the authority's decisions, and hence provide corroborative evidence that our positive and negative trust information was successfully operationalized.

Percentage findings. A 2×3 analysis of variance (ANOVA) was conducted on the percentages of lottery tickets that participants believed that they should get relative to the other participant and which they wrote down on the pieces of paper. This ANOVA yielded no significant effects. One hundred thirty (out of 138) participants indicated that they should get 50% of the tickets. The grand mean percentage was 50.3%.

Participants who were allowed voice ($n = 69$) also typed in

their opinion about the percentage tickets that they should receive relative to the other participant. We checked whether participants typed in a different percentage than the percentage which they wrote down on the pieces of paper. None of the participants did so. An ANOVA indicated that independent of the trust information that participants received, participants typed in that the lottery tickets should be divided equally between themselves and the other participant: Sixty-eight of the participants answered that they should get 50% of the tickets, and the mean percentage was 50.7%. Thus, these findings are supportive of equity theory: Participants preferred to divide outcomes equally between themselves and the other participant (who contributed an equal amount of inputs, and who hence deserved, according to equity theory, to receive the same amount of outputs as the participants themselves).

Procedure judgments. A 2×3 MANOVA on the two procedural judgments (satisfaction and fairness) showed only main effects of procedure at the multivariate level and the univariate levels: multivariate $F(2, 131) = 25.07, p < .001$; for procedural satisfaction, $F(1, 132) = 45.12, p < .001$; for procedural fairness, $F(1, 132) = 40.08, p < .001$. As was expected, participants who received an opportunity to voice their opinion were more satisfied with the procedure ($M = 4.7$) and judged the procedure as more fair ($M = 4.7$) than participants who did not receive such an opportunity ($M_s = 2.8$ and 3.0 , respectively).

Outcome judgments. The means of the outcome judgments in Experiment 2 are presented in Table 2. We performed the same three types of analyses we conducted in Experiment 1: First, we tested for overall effects in a 2×3 MANOVA. This analysis only showed significant interactions between procedure and trust at both the multivariate level and the univariate levels, $F(4, 262) = 3.00, p < .02$; for outcome satisfaction, $F(2, 132) = 5.01, p < .01$; for outcome fairness, $F(2, 132) = 6.05, p < .01$.

Second, we checked whether the interaction between procedure (voice vs. no voice) and the trust valence contrast (trust positive vs. trust negative) was significant. In correspondence with the findings of Experiment 1, this analysis yielded no significant effects.

Third, and most important, we tested the interaction contrast that was relevant to Hypothesis 1: Procedure (voice vs. no voice) \times The knowledge of trust contrast (trust unknown vs. trust positive–trust negative). This contrast analysis only yielded significant interactions between procedure and the knowledge of trust contrast: multivariate $F(2, 131) = 6.09, p < .01$; for outcome satisfaction, $F(1, 132) = 9.92, p < .01$; for outcome fairness, $F(1, 132) = 11.99, p < .01$. In correspondence with Hypothesis 1, variations in procedure affected outcome judgments more strongly in the trust-unknown condition than in the trust-positive and the trust-negative conditions. In fact, results of least significant difference tests (see Table 2 for results) showed that participants who did not know whether they could trust the authority judged their outcome as signifi-

³ As in Experiment 1, the participants all received an outcome that was neither extremely positive nor extremely negative: The expected value of the 3 lottery tickets was quite modest ($3/200 \times$ Dutch guilders $100 = 1.50$ Dutch guilders \approx U.S. \$0.90).

Table 2
Mean Outcome Judgments as a Function of Procedure and Authority's Trustworthiness (Experiment 2)

Dependent variable	Procedure	Trust		
		Unknown	Positive	Negative
Outcome satisfaction	Voice	4.9 _a	3.9 _{b,c}	3.4 _{b,c}
	No voice	3.2 _c	4.2 _{a,b}	3.5 _{b,c}
Outcome fairness	Voice	4.7 _a	3.9 _{a,b,c}	3.3 _{b,c}
	No voice	3.0 _c	4.2 _{a,b}	3.5 _{b,c}

Note. Entries are means on 7-point scales; higher values indicate more positive ratings of the dependent variable in question. For each dependent variable, means with no subscripts in common differ significantly, as indicated by a least significant difference test for multiple comparisons between means ($p < .05$).

cantly more satisfying and more fair when they had received an opportunity to voice their opinion than when they had not received voice, but that the outcome judgments (both satisfaction and fairness) of participants who knew that the authority could be trusted or who knew that the authority could not be trusted did not differ significantly as a function of whether participants were or were not allowed a voice.

General Discussion

Taken together, the findings of our two studies provide strong support for our line of reasoning. The results show that people's reactions to an outcome they received from an authority are strongly affected by procedural fairness information when they do not know whether the authority can be trusted. This suggests that recipients who do not have information about authority's trustworthiness are more willing to accept the outcomes the authority gives to them if the authority allocates outcomes by means of fair instead of unfair procedures. However, when people have been informed about authority's trustworthiness they are less in need of procedural fairness information, yielding less strong—in both our experiments, even nonexistent—fair process effects on the outcome judgments of these persons. We found this both when people judged a hypothetical procedure and outcome (Experiment 1) and when they directly experienced the procedure and outcome (Experiment 2). In both our experiments we found that positive and negative trust information did not yield different fair process effects. This suggests that the psychological explanation of the fair process effect that we have put forward in the present article does not only hold for positive trust conditions, but for negative trust as well. The findings suggest that people especially need procedural fairness in the process of deciding how to react to the outcomes they received from an authority when they do not know whether the authority can be trusted. In other words, the current findings suggest that people may resolve the question how one can trust others not to exploit them by starting to form fairness judgments—as heuristic substitutes—when direct information about authority's trustworthiness is missing.

It should be noted here that perceived procedural fairness

cannot be equated with perceptions of trust. Although we have shown in the current article that procedural fairness can substitute for missing trust information, this should not lead one to conclude that procedures and trust are the same thing. For example, many studies have shown that perceptions of procedure are more directly related to outcome perceptions than that trust valence is related to judgments of outcome (for an overview, see, e.g., Tyler & Lind, 1992). The fact that, compared with valenced trust, procedures more frequently affect outcome judgments is in correspondence with the present findings. It is important to emphasize here that although positive and negative trust did not affect outcome judgments this does not imply that manipulations failed. We think that there is good reason to argue that positive and negative trust should not necessarily affect outcome judgments. Tyler et al. (1997; Tyler & DeGoey, 1996; Tyler & Lind, 1992), for example, have shown that valenced trust is more important for attitudes and behaviors that have a longer term horizon (e.g., willingness to comply) than variables such as perceived outcome satisfaction or fairness. In correspondence with this, the manipulation-check findings from Experiment 2 reveal that our positive and negative trust information was successful in yielding effects on participants' trust perceptions as well as on their voluntary compliance with authority's decisions. This shows that our positive and negative trust information was perceived as intended and was successful in affecting some well-specified variables. These are important findings because they show that our positive and negative trust information was manipulated successfully. Even more important, however, we think that in the current article we have revealed that it may be more worthwhile to focus on the distinction between trust known versus unknown than on trust positive versus negative. The present study shows that focusing on the former distinction may reveal insights in the psychology of social justice and trust that previous research, which has focused on the latter distinction, has missed.

At the outset of this article, we drew a sharp distinction between situations in which people do versus do not have information about authority's trustworthiness. We reasoned that it made sense to begin our line of research by performing empirical studies that enabled us to study the distinction as clearly as possible and to achieve high internal validity and experimental control: experimental studies. In fact, according to our knowledge, these studies are the first in the procedural justice domain to directly manipulate trust (for overviews, see Brockner et al., 1997; Tyler & Lind, 1992). 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). It should be noted here, however, that our manipulations of trust were extreme. This made our experiments very good at testing the fairness heuristic theory prediction, but care must be taken in applying the results directly to real-world settings where less extreme levels of trust and distrust may be the norm, and were different elements of the concept of trust may play an important role (for overviews, see Brockner & Siegel, 1996; Brockner et al., 1997; Kramer & Tyler, 1996).

There is recent evidence, however, that corroborates our line

of reasoning: In a survey among parents who were interviewed about an organization that was responsible for their child's day care, Van den Bos and Van Schie (1998) found that when respondents were uncertain about the organization's trustworthiness, their judgments of the organization's reliability were more strongly affected by their perceptions of the organization's procedures than when respondents indicated that they had favorable information about the organization's trustworthiness. These results are in correspondence with those of the present article, and suggest that our line of reasoning may also hold in important domains of everyday life. Future research should investigate other domains of social behavior and different operationalizations of trust to find out the generalizability of the effects reported in the present article, but we think that it is safe to conclude that our findings show new refined insights in the psychology of the fair process effect.

One reason why our results are interesting, we believe, is because they can be contrasted with some suggestions that have been made in the social justice literature (cf. Tyler & Lind, 1992), suggesting that trust is an antecedent of procedural justice judgments, not a moderator of procedural justice effects. We argue that the apparent inconsistency arises because trust is operationalized quite differently here and in the Tyler and Lind (1992) model. As explained in the introduction, we reasoned that it made sense to manipulate trust as directly as possible, and we therefore informed our participants that the authority could be trusted, could not be trusted, or gave them no explicit information about authority's trustworthiness. Future studies may pay attention to other operationalizations of trust, and, more important, may want to try to integrate the various research domains in which trust has been studied, such as trust in organizations (e.g., Brockner & Siegel, 1996; Brockner et al., 1997), social dilemmas (Messick, 1998; Yamagishi, 1995), experimental games (Güth, Ockenfels, & Wendel, 1997), negotiations (Ross & LaCroix, 1996), and managerial decision making (Korsgaard, Schweiger, & Sapienza, 1995; Zand, 1972).

Future research may want to vary information about outcome (see, e.g., Brockner et al., 1997; Van den Bos, Lind, et al., 1997; Van den Bos et al., 1998). This may be an important issue because Brockner and Wiesenfeld (1996) have argued that outcome favorability interacts with procedural fairness. The focus of the present article, however, was not on Procedure \times Outcome interactions, but on Trust (unknown versus positive-negative) \times Procedure interactions—an issue on which Brockner and Wiesenfeld did not focus. Because we did not focus on Procedure \times Outcome interactions, we decided to fix our outcome information in such a way that participants judged it to be neither unqualified favorable nor unqualified unfavorable (and in Footnotes 1 and 3 we explained that we were successful in doing so). More important, it should be noted here that Brockner and Siegel (1996) argued that because trust valence (positive vs. negative) is central to the conception of procedural justice, the Trust Valence \times Outcome interaction supersedes the Procedure \times Outcome interaction. However, Brockner and Siegel's work focused on valenced trust, but did not focus on the distinction between unknown trust and valenced trust, as we do in the present article. We think, therefore, that the present article goes

beyond the issues Brockner and Siegel (1996) and Brockner and Wiesenfeld (1996) have identified.

As explained at the outset, fairness heuristic theory posits that people will not have much need for fairness-relevant information if they already trust an authority. Therefore, we decided to manipulate trust before the other independent variable in our experiments (procedure) and before we gave participants an outcome. Future research might want to manipulate the order in which independent variables are presented to the participants (cf. Van den Bos, Vermunt, & Wilke 1997). It seems reasonable to conclude on the basis of our experiments, however, that we have identified at least some conditions in which the psychological processes put forward in this article hold.

The robustness of the fair process effect has led some procedural justice researchers to conclude that the formation of overall justice judgments is more strongly affected by procedures than by outcomes (see, e.g., Lind & Tyler, 1988, p. 1). This led to the present situation in which procedural justice research tends to focus on only one aspect of the cognitive process leading to fairness and other judgments, procedures. Distributive justice researchers, on the other hand, also tend to focus on one aspect of the fairness judgment process, outcomes. Some researchers have even suggested that outcomes may be more important for people's fairness judgments than procedures (e.g., Lerner & Whitehead, 1980). Thus, procedural and distributive justice research each tend to focus on only one aspect of the fairness judgment process, at the expense of other important concepts. As several authors have pointed out, however, it is now time to integrate the procedural and distributive justice domains (Brockner & Wiesenfeld, 1996; Cropanzano & Folger, 1991; Greenberg, 1990; Sweeney & McFarlin, 1993; Tyler, 1994; Van den Bos, Lind, et al., 1997; Van den Bos et al., in press; Van den Bos, Vermunt, & Wilke, 1997). In the present article we have tried to integrate the procedural and distributive justice domains by paying attention to the psychology of the frequently replicated fair process effect, and our findings show new refined insights in the psychology of this effect. The present studies have focused on an element of fairness heuristic theory that has not been directly investigated thus far, and have pointed at the role of knowledge about trust (present vs. absent) in answering the question when procedural fairness is important for people, and hence when fair process effects are more likely.

Fairness heuristic theory argues, among other things, that to explain the fair process effect, it is necessary to know what information is (vs. is not) available to people when they are forming outcome judgments (Van den Bos et al., in press). In previous studies, we have explored what effects variations in (social comparison-based) equity information have on the outcome judgment process (see Van den Bos, Lind, et al., 1997; Van den Bos et al., 1998). In these studies, we have identified that when people have been informed that a comparable other person (comparable in the amount of input he or she has provided) receives an outcome that is equal to, better than, or worse than the outcome of people themselves, they are not affected by variations in procedure (voice vs. no-voice). However, when people are not informed about the outcome of the other person (Van den Bos, Lind, et al., 1997) or have a less strong outcome reference point than this social comparison-based equity infor-

mation (Van den Bos et al., 1998) they have to rely more strongly on procedure information in the outcome judgment process, and hence their outcome judgments are more strongly affected by procedure information.

In previous studies (Van den Bos, Lind, et al., 1997; Van den Bos et al., 1998) we have focused on the contents of outcome information and on when procedural fairness acts as a heuristic substitute when outcome information is missing or weak. The current results have expanded the line of reasoning from these previous studies to other, more socially oriented information: trust. It can now be concluded that people especially need procedural fairness when social comparison-based equity information (Van den Bos, Lind, et al., 1997; Van den Bos et al., 1998) or information about authority's trustworthiness (the present article) is lacking. What is especially interesting about the current findings is that they provide strong evidence for one answer to the question of why people care about justice: People do so because they want to find out whether they can trust others not to exploit or exclude them from important relationships and groups. The present findings imply that an answer to the question of when people care about justice—and especially, procedural justice—may be that people are especially in need of fairness information when they do not have direct, explicit information regarding whether they can trust others.

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