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Inspecting the Emperor's Clothes: Evidence That Random Selection of Leaders Can Enhance Group Performance

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This article tests the hypothesis that group performance might be superior when leaders are randomly rather than systematically selected. In Experiment 1 groups with randomly selected leaders performed a survival task better than groups whose leaders were systematically selected. This effect was replicated in Experiment 2: Groups with a random leader also performed better than groups with no appointed leader and followers adhered more strongly to the group decision. In Experiment 3, naive participants' experimental expectations confirmed the counterintuitive nature of these findings. Results suggest that systematically selected leaders can undermine group goals and group maintenance. The possibility that this occurs because leaders assert their personal superiority at the expense of shared social identity is discussed.

A large part of the research conducted by organizational psychologists over the last 40 or so years has been oriented toward the practical goal of seeking to improve individuals' performance in the workplace. At an initial stage in this research process, considerable effort went into trying to improve the selection procedures used to recruit employees, with a view to identifying personnel best suited to particular positions. This is particularly true with positions of authority and leadership. The underlying logic here, which can be traced back to some of the very first writings on management (e.g., Taylor's, 1911, second principle of scientific management; see also Locke, 1982), is that the process of scientifically endeavoring to identify the best leaders should produce dividends for the organization as a whole (e.g., in terms of group harmony and productivity).

That this logic is implicit in management theory and practice is suggested by the fact that although a number of studies have sought to compare the efficacy of different selection procedures (e.g., Levinson, 1994; Walters, Miller, & Ree, 1993), to the best of our knowledge, none has sought to examine whether the process of leadership selection is always beneficial in itself. Although they make a slightly different point, Emler and Hogan (1991) appealed to common sense rather than empirical evidence when they stated, "you cannot randomly allocate leadership responsibility and expect the interests of justice or society to be well-served" (p. 86).

In fact though, there are a number of good reasons for believing that under certain conditions the process of systematic leadership selection might be deleterious to group performance. In this vein, pioneering empirical studies in the 1950s and 1960s suggested that group productivity was improved to the extent that leaders showed consideration for their group (Cartwright & Zander, 1960; Shartle, 1956) and might diminish to the extent that leaders sought only to satisfy their own objectives (e.g., self-promotion; Fouriezos, Hutt, & Guetzkow, 1950). Yet clearly in the process of striving to become a leader individuals may display considerable concern for self-advancement and a commensurate lack of consideration for their fellow group members (e.g., Kipnis, 1972; Mulder, 1977). Work conducted around this time also suggested that groups (in particular

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small groups) may be more likely to reach their goals when they (a) are composed of members who all believe that their contribution is valued (Pepitone, 1952, as cited in Cartwright & Zander, 1960), (b) are composed of members who are cooperating rather than competing (Deutsch, 1960), (c) are cohesive (Schachter, Ellerston, McBride, & Gregory, 1960), and (d) have the offices of leadership distributed between all members rather than concentrated in one person (White & Lippitt, 1960). On the basis of this evidence (as summarized by Cartwright & Zander, 1960, p. 506), it seems that there are grounds for anticipating that, to the extent that the process of leadership selection sets the leader apart from—or in opposition to—the other members of his or her group, group performance as a whole might suffer.

This analysis is also consistent with Hollander's (1995) more recent analysis of followership, which argues convincingly that the success of a work team is highly dependent on positive and cohesive leader–follower relations. Hollander suggests that for groups to function as effectively as possible "the leader needs to be attuned to the needs of followers, their perceptions and expectancies" (1995, p. 75). In essence, the point here is that if a group is to function as a group rather than just an aggregate of individuals, its leader must represent (and be representative of) the interests of the collective as a whole rather than just him- or herself (see also Von Cranach, 1986). In this sense, then, leadership is intimately bound up with the concerns of followers—a point expressed succinctly in the remark attributed to Ledru-Rollin in remarking of his political supporters "I must follow them, I am their leader" (see Cohen & Cohen, 1971, p. 45) (a statement also attributed to Bonar Law).

Moreover, the same basic analysis can also be derived from both social identity theory (Tajfel & Turner, 1986) and self-categorization theory (Turner, 1985; Turner, Hogg, Oakes, Reicher, & Wetherell, 1987). Both theories suggest that leaders and followers are more likely to act in terms of a shared group membership (and hence orient themselves toward the achievement of common goals), to the extent that they perceive themselves to share a common social identity (for applications of these ideas to organizational issues, see Ashforth & Mael, 1989; Brown, 1988; Dutton, Dukerich, & Harquail, 1994; Turner & Haslam, in press). Along these lines, Turner (1991) and Hogg (1996) have argued that, in part, a successful leader is someone who embodies a group prototype—representing the perspective and qualities of the group that both (a) define it as a whole and (b) distinguish it from other groups.

Self-categorization theory's principle of comparative fit (Turner & Oakes, 1989; Turner, Oakes, Haslam, & McGarty, 1994) also predicts that the salience of a social identity (and hence the achievement of group-level goals) will tend to be undermined to the extent that the context of social interaction is characterized more by interpersonal differences than by intragroup similarity. So, if the selection process draws attention to the former (by explicitly focusing on interpersonal differences in competence or suitability), then this may undermine the group's inherent "groupness." This in turn might be expected to negatively affect the two primary indexes of group productivity identified by Cartwright and Zander (1960), namely "(a) the achievement of some specific group goal [and] (b) the maintenance or strengthening of the group itself" (p. 496).

Although they did not examine issues of leader selection or workers' own perceptions, some data that bear upon this analysis were recently reported by Jackson et al. (1991). In a correlational study of, among other things, the relationship between group homogeneity and organizational turnover, they found that individuals were more likely to stay in organizations to the extent that they were similar to other group members (e.g., in age, level of education, work experience, and college background). Because the relationship between similarity and attraction is complex (Hogg, 1992), it is hard to establish causality here. We also concur with Jackson (1992, p. 156) that groups are simultaneously both heterogeneous and homogeneous and that it is therefore misleading to suggest that there might be some absolute level of intragroup homogeneity. However, it nonetheless seems plausible that there is an association between awareness of intragroup homogeneity and members' commitment to the group because both can contribute to, and derive from, a salient social identity (Haslam, Oakes, Turner, & McGarty, 1995; Morrison, 1998). Social groups are not only composed of people who are (or perceive themselves to be) more similar to each other than to members of other groups, but so too

those similarities and differences are accentuated through processes of stereotyping and social influence (Haslam, 1997; Haslam, Turner, Oakes, McGarty, & Reynolds, 1998; Oakes, Haslam, & Turner, 1994). Among other things, these processes bring group members—including leaders—back in to the fold through punishment (or nonreward) of deviance or aprototypicality (Platow, Hoar, Reid, Harley, & Morrison, 1997).

A closely related analysis of the dynamic between perceived intragroup homogeneity, social identity salience, and group functioning was presented by Worchel (1994; Worchel, Coutant-Sassic, & Grossman, 1992) in a model of group formation and development that is consistent with (and draws upon) social identity theory. Part of this describes a process whereby group identification (Stage 3) leads to group productivity (Stage 4) but is in turn followed by individuation (Stage 5) and ultimately group decay (Stage 6). Worchel describes this process as follows:

> As the group achieves its goals and gains resources, members turn their attention from the group needs to their individual needs.... Members magnify the differences between themselves and other group members.... Solutions to social dilemmas become individually based, and, consequently, less group oriented. The group remains a focal point, but the nature of this focus now involves the individual's relation with the group. The group next enters a period of decay as increasing attention is paid to personal needs and the group becomes less salient. (Worchel, 1994, p. 213)

Again, to the extent that a process such as leadership selection draws attention away from the group as a whole and toward its individual constituents, it follows from this analysis that this process is likely to precipitate a shift from group-based productivity to group disintegration.

On the basis of this empirical and theoretical analysis, we conducted two experiments to test the hypothesis that the process of seeking to identify the best leader for a small group task might actually undermine rather than enhance group performance. It is important to note that our objective was not to demonstrate that the process of systematically selecting group leaders is generally counterproductive. Instead, we only tested the hypothesis that this would be the case in conditions where, in the absence of a leader being chosen, the group is already oriented to a well-defined shared goal.

Experiment 1

It follows from the above analysis that systematic leader selection might be counterproductive in a situation in which a group task clearly needs to be dealt with by means of consensual group-based action. On the basis of previous research (e.g., as reviewed by Brown, 1988; Worchel et al., 1992) we would expect that on its own this need for group-based action is likely to draw group members together, making them define themselves and behave in terms of a shared identity. To this end, we conducted a study in which small groups of participants had to imagine that they were stranded in an inhospitable environment and had to rank in order of importance a number of items that they would need to survive. This is a standard group task developed and commonly used to address issues of group performance and leadership (Johnson & Johnson, 1991).

Our two key dependent measures corresponded to the goal-achievement and group-maintenance functions identified by Cartwright and Zander (1960). The first was simply the quality of the survival strategy groups decided on (as measured relative to expert ratings). The second measure was obtained by looking at the extent to which the strategy chosen by individuals deviated from that on which their group had previously agreed. Less deviation is evidence of greater group maintenance, suggesting that individuals were more bound to the group and its original decision.

The main independent variable in the study was the manner of leadership selection, and this had three levels—random, informal, and formal. In the random condition the leader was simply the person whose last name was first in alphabetical order.[1] In the informal condition groups decided among themselves who should be group leader. In the formal condition the leader was selected by means of a relatively simple selection procedure. This involved group members completing a short leadership selection inventory in which they rated their own

[1] It is worth noting that, strictly speaking, this selection procedure was not random. In fact it would be more accurate to describe it as arbitrary. However, we refer to the procedure as random rather than arbitrary, because suggesting that leaders were selected arbitrarily might be more misleading in that it may suggest that selection was capricious and a matter of personal taste or prejudice.

ability in areas that Ritchie and Moses (1983) identified as being positively correlated with long-term managerial success. It is important to note that we are not claiming that this measure corresponds to formal procedures typically used to identify leaders in the workplace. Instead, the inventory was seen simply as a tool that measured apparently plausible standards on which basis a leader could be externally imposed on the group (though this may, incidentally, also have tapped leadership-relevant attributes such as confidence and desire to lead).

Our primary prediction was that on both group-goal and group-maintenance measures, the responses of participants in groups in which the leader was randomly chosen would differ from those of participants in groups that used a systematic (i.e., nonrandom) leadership selection process. These predictions were based on the argument that the process of systematic leader selection would tend to undermine group solidarity and a sense of shared social identity—and hence group functioning—by drawing attention to individual differences among group members rather than to those things they had in common.

As well as varying the method of leadership selection, the task that the groups performed was also varied between groups. Half the groups performed a winter survival task in which their plane had supposedly crashed in a frozen wilderness; the other half performed a stranded in the desert task in which their minibus has overturned in a desert (see Johnson & Johnson, 1991). This manipulation was not expected to have any impact on the primary dependent measures, but was included to ensure that any effects observed on these were not attributable to the specific content of the group task.

Method

Participants and Design

One hundred eighty-eight students enrolled in an advanced social psychology course participated in the study as part of a laboratory exercise. Fifty-five of the participants were men and 133 were women; the median age of participants was 21 (range = 19 to 51). Whole classes (containing between 12 and 16 students) were randomly assigned to one of the three leadership selection procedure conditions (formal, informal, random). Each class was randomly divided into between three and four groups, most of which contained 4 members (range = 3 to 5 members). There were 15 groups in each of the three leadership selection conditions. The study was conducted over a 2-year period with a different task performed each year. In each of the three conditions 8 groups performed the winter survival task and 7 performed the stranded in the desert task.

Materials and Procedure

At the start of the class, participants were assigned to groups and told they were "about to take part in a group problem-solving exercise in which you have to decide about appropriate behavior [after your plane has crash-landed in a frozen wilderness area/after your minibus has overturned in a remote desert]. Your first job is to elect a group leader to lead you through this task."

According to the experimental condition to which they had been assigned, groups were instructed to select a leader by one of the following methods:

Random selection. The group member whose surname was first in the alphabet was appointed leader.

Informal selection. Participants were instructed to identify a leader for their group by "whatever means you see fit."

Formal selection. Participants completed a leadership skills inventory in which they responded to the following 10 questions on a scale ranging from *not at all* (1) to *extremely* (7). Each item related to a personality dimension identified by Ritchie and Moses (1983) as a predictor of managerial success.

1. How well do you communicate verbally?
2. How lazy are you when you work with a group of other people?
3. How objective are you about your own performance?
4. How aware are you of your social environment?
5. How rigid are you in your approach to tasks?
6. How high are the personal standards you set yourself?
7. How resistant are you to stress?
8. How intolerant are you of uncertainty?

9. How broad-ranging are your interests?

10. How good are your organizational and planning skills?

Questions 2, 5, and 8 were reverse-scored, and the participants' scores on all items were aggregated. The person who obtained the highest score was appointed group leader.

After each selection had taken place, the experimenter took all the leaders aside and gave them a leadership package. This contained all the materials for the group survival task as well as instructions relating to the leader's role in the group. Leaders were told that the experiment was partly concerned with the issue of whether or not it was possible to train leaders. It was emphasized that they must act in accordance with certain instructions that they then had to go back and read aloud to the other members of their group. These instructions were as follows:

> In this study, we are about to take part in a group problem-solving task. As a leader, my job is to ensure that we all agree with all the suggestions we make. However, as a leader, I am also going to try to encourage everyone to participate so that we can all generate ideas and each make a contribution to the group. At the end of the experiment, the experimenters will look at how good we have been at directing ourselves to a decision with which we all agree.

Having been read these instructions, the groups then performed either the winter survival task or the stranded in the desert task (as developed by Johnson & Johnson, 1991, pp. 224–228; 267–268). Both exercises were adapted slightly to make them relevant to an Australian audience. Briefly, the first of these exercises informed participants that the group was on a plane that had crashed onto a frozen lake in a wilderness area and was faced with a decision about which items to rescue from the plane before it sank. The group's task was to rank 12 items (e.g., a compass, a detailed air map, a loaded pistol, newspapers, a cigarette lighter without fuel) in terms of their importance for the whole group's survival. The second exercise informed participants that they were on a field trip where their bus had overturned in the desert and that they needed to rank 12 items (e.g., a magnetic compass, a rear-view mirror, a map) in terms of their importance for survival.

Groups were given as much time as they needed to perform each task. After they had finished, the groups disbanded and participants were asked to complete a posttest questionnaire individually. As well as asking them to provide their own ranking of the 12 items, participants responded to the seven questions below on seven-point rating scales. The labels attached to scale anchors are indicated in parentheses.

1. How much confidence do you have in the final decision your group reached? (1 = *very little*, 7 = *a great deal*)

2. How much did you identify with the other members of your group? (1 = *very little*, 7 = *a great deal*)

3. To what extent was the group leader representative of the group? (1 = *not very*, 7 = *extremely*)

4. How effective was the process by which your group reached its decision? (1 = *extremely ineffective*, 7 = *extremely effective*)

5. How involved did you feel in making the decision? (1 = *extremely uninvolved*, 7 = *extremely involved*)

6. How effective was the leadership of your group? (1 = *extremely ineffective*, 7 = *extremely effective*)

7. How much did you enjoy taking part in this study? (1 = *very little*, 7 = *a great deal*)

Dependent Measures

The primary measure of performance was the degree to which each group's ranking of items deviated from that of survival experts (as detailed by Johnson & Johnson, 1991, pp. 495, 498). This score potentially varies between 0 (if the ranking was identical to the experts') and 72 (if the ranking was the exact opposite of the experts'). Broadly speaking, a better quality solution was one that emphasized the usefulness of items that allowed the group to stay together until help arrived rather than attempt to walk to the nearest town (e.g., favoring the selection of fire-making or water-collecting items rather than navigational ones).

We also computed the extent to which individual nonleaders' rankings deviated from those of their group as a whole. As already noted, these measures correspond to the group-goal and group-maintenance functions identified by Cartwright and Zander (1960).

Results

Quality of Group Decision

The quality of groups' decisions was initially examined by using a planned (two-tailed) contrast between the performance of groups in

conditions with randomly and nonrandomly (i.e., formally and informally) selected leaders. A second orthogonal contrast compared the performance of groups with formally and informally selected leaders. Consistent with predictions, the first of these contrasts indicated that the quality of decisions was superior in groups in which the leader was randomly rather than nonrandomly selected (random $M = 34.13$, nonrandom $M = 43.29$), $t(42) = 2.42$, $p < .05$. There was no difference in the performance of groups with formally and informally selected leaders (formal $M = 44.73$, informal $M = 41.85$), $t(42) = 0.66$, *ns*. Means for this and all other measures are presented in Table 1.

Individual Measures

Leaders' responses. We compared the ratings made by group leaders on all posttest items across the three experimental conditions by using orthogonal planned contrasts. No effects approached significance (all $ts < 1$).

Nonleaders' responses. Ratings made by group members who were not leaders were examined by using orthogonal planned contrasts (the first comparing groups with random and nonrandom leaders, the second comparing groups with formal and informal leaders). These indicated that there was no difference between individuals from groups with nonrandomly and randomly selected leaders on the group-maintenance measure (nonrandom $M = 5.77$, random $M = 5.63$), $t(140) = 0.08$, *ns*. However, followers from groups with formally selected leaders showed more group maintenance than did those from groups with informal leaders (formal $M = 4.15$, informal $M = 7.29$), $t(140) = 2.19$, $p < .05$.

The only other effects to emerge from these contrasts indicated that individuals from groups with randomly selected leaders perceived that they were more involved in the decision-making process than those from groups with nonrandomly selected leaders (random $M = 6.02$, nonrandom $M = 5.62$), $t(140) = 2.08$, $p < .05$, and that the leadership of their group was less effective (random $M = 4.94$, nonrandom $M = 5.34$), $t(140) = 1.97$, $p < .05$.

Discussion

The results of this study provided some support for our predictions. Most notably, groups with a randomly elected leader made higher quality decisions than those where the leader had been nonrandomly chosen. Clearly, this finding is counterintuitive in light of the body of organizational research that suggests that processes of systematic leader selection should generally enhance group performance (e.g., Sarros & Butchatsky, 1996). On the other hand, the observed pattern is consistent with the idea that under certain circumstances systematic selection of leaders may have a deleterious impact on group performance because it can serve to undermine group solidarity and a sense of shared identity (Hollander, 1995; Turner & Haslam, in press).

Table 1
Means for Experiment 1: Performance and Posttest Measures

Measure	Leadership selection procedure			Significant effects
	Formal (F)	Informal (I)	Random (R)	
n (no. of groups)	61 (15)	64 (15)	63 (15)	
Group measures				
Group decision (deviation from expert rankings)	44.73	41.85	34.13	FI vs. R*
Individual follower measures				
Deviation from group decision	4.15	7.29	5.63	F vs. I*
Confidence in group decision	5.70	5.76	5.88	
Identification with group	5.22	5.39	5.23	
Representativeness of leader	5.24	5.02	4.96	
Effectiveness of decision process	5.48	5.47	5.77	
Involvement in decision	5.57	5.67	6.02	FI vs. R*
Effectiveness of leadership	5.50	5.18	4.94	FI vs. R*
Enjoyed participation	5.39	5.14	5.40	

* $p < .05$.

However, although results on the main measure were in line with our predictions, there was no evidence of greater group maintenance in groups with randomly rather than nonrandomly selected leaders. Indeed, the only effect to emerge on this measure indicated that followers in groups with formally selected leaders showed more group maintenance than those from groups in which the leader was informally selected—a finding that would tend to support arguments about the superiority of systematic leader selection (see Ritchie & Moses, 1983; Sarros & Butchatsky, 1996; M. Smith & George, 1994). Moreover, it is apparent that there was no evidence from additional measures that the main effect for quality of group decision was associated with variance in the amount of expressed identification with in-group members or variance in the perceived efficacy of the decision-making process.

Interestingly though, there was evidence that participants in the random conditions perceived the leadership of their group to have been less effective than did participants in groups in which the leader was nonrandomly chosen. The precise meaning of this finding is not clear, because on the surface it appears to contradict the observation that randomly selected leaders actually elicited better performance from their subordinates. At another level though, the perception of participants in random groups that their leaders were relatively ineffective may arise from their subjective expectations about how a leader ought to be selected and behave—expectations that were almost certainly violated by the process of randomly selecting the leader. In this vein the effect could be interpreted as supporting the idea that to enhance group performance on this particular task there was actually very little need for someone to be conspicuously seen as filling the stereotypic leader role (e.g., being perceived by other group members to be extremely competent and effective; cf. Lord & Maher, 1991).

Indeed, if the leader role was relatively superfluous in this situation, then what may in fact have been required was what would in Australian parlance be referred to as a *Clayton's leader* (the leader you have when you don't have a leader).[2] This point thus goes to the heart of the apparent paradox that the present research sought to explore: that under some circumstances there can be significant drawbacks associated with the process of seeking to select the best leaders and having those leaders behave as good leaders should. Again, this observation is consistent with the theoretical argument that leadership is less a property of the individual as an individual and more a quality of the group-in-context that finds expression in individual group members (Hollander, 1995; P. M. Smith & Fritz, 1987).

However, looking at the results of this study as a whole, it is clear that support for our predictions was not as complete or compelling as it might have been. In particular, this is because it is possible to propose two alternative accounts of the present findings. The first would suggest that the relatively poor performance of groups with nonrandomly selected leaders arose from the fact that in these conditions those participants who did not become leaders were more liable to disengage from the group and participate in social loafing (cf. N. L. Kerr & Stanfel, 1993). In effect, their thoughts about the leader may have been of the form "if you're so wonderful, you can get on with it." On the other hand, followers in the condition in which the leader was randomly selected may have been more inclined to compensate for the perceived shortcomings of the leader (Williams & Karau, 1991). This would also explain why they reported feeling more involved in the decision-making process. In fact, there are parallels between this analysis and the one we developed above, largely because social loafing could be the process through which intragroup differentiation and a lack of shared social identification affect group performance (cf. Brown, 1988; James & Greenberg, 1989; Karau & Williams, 1993).

Another analysis of these results would suggest, however, that the superiority of randomly selected leaders arose from the fact that the role of leader was relatively superfluous in this study. In other words, random leaders may have been successful not because they posed no threat to group cohesiveness, but because they simply did not "stick their oar in" where it was not needed. On the other hand, nonrandomly selected leaders may have felt compelled to prove their leadership potential, when leader-

[2] Clayton's is a nonalcoholic drink marketed in Australia with the slogan "the drink you have when you're not having a drink."

ship really was not called for. Again, there are some commonalities here with the analysis we have proposed, but nonetheless the issue of whether an appointed leader was really necessary to perform the task in this study is quite an important one.

To address these and other issues we decided to conduct a second experiment. This was conducted with the objective of replicating the major finding in this study while eliminating some of the ambiguity surrounding its interpretation. In particular, we sought to do this by (a) incorporating a control condition in which groups had no appointed leader and (b) incorporating posttest measures of social loafing.

Experiment 2

The design of this experiment was similar to that of Experiment 1, but it incorporated a number of procedural changes. Most trivially, the person who was chosen to be leader in the random selection condition was now the person whose last name came last in alphabetical order. This alteration was imposed in case the effects obtained in the previous study reflected some (hitherto undocumented) leadership capacity of people whose last names are alphabetically superior (e.g., based on ethnicity or prior experience of appearing at the top of lists).

The group task was also changed such that all participants completed Johnson and Johnson's (1991) fallout shelter task. In this task the groups were presented with a scenario in which nuclear war had broken out; groups were asked to rank items in terms of their importance for survival in a fallout shelter. As with the tasks used in the previous experiment, this exercise was intended to provide the groups with a clear goal that all members would be motivated to achieve.

In addition, the posttest phase of this study included new items designed to tap processes relevant to the group decision-making process more directly. Specifically, it included new items designed to gauge (a) the sense of common identity within the group, (b) the perceived legitimacy and (c) leadership of the person who was selected (or emerged) as leader, and (d) the extent of leaders' and followers' effort. The latter measure was included to see whether variation in the leader selection procedure was associated with any social loafing on the part of nonleaders—a factor that could have contributed to some of the effects that emerged in Experiment 1.

Most important though, this study replaced the informal leader selection condition of Experiment 1 with a control condition in which no group leader was appointed. Along the lines of arguments presented in the Discussion of Experiment 1, there were two reasons for making this modification. The first was that in the previous study we had exerted no control over the exact methods that group members used to choose a leader in the informal leader selection condition. From a theoretical point of view this was clearly unsatisfactory, as the method was imprecise and offered no clear insight into the selection process. Indeed, it is reasonable to assume that in this condition different groups may have resorted to radically different selection strategies. In addition, within the previous design it was not clear whether the effects obtained on the group performance measure actually reflected the fact that there was no leadership in the condition in which the leader was randomly selected. This interpretative ambiguity is problematic in light of previous research that has suggested that groups do not need leaders to perform certain tasks well (e.g., Howell, Bowen, Dorfman, & Kerr, 1990; Watson & Behnke, 1990). We hoped that the addition of this control condition would enable us to ensure that the effects of selecting leaders randomly did not arise from the fact that those selected by this means actually displayed no leadership at all.

Despite these modifications, our predictions were similar to those of the previous study. Specifically, on the basis of the arguments developed earlier, we expected that groups with randomly selected leaders might perform better than those with either a formally elected leader or no leader. As before, these effects were anticipated on both group-goal and group-maintenance measures.

Method

Participants and Design

One hundred sixty-seven students enrolled in an introductory psychology course participated in the study as part of a laboratory exercise. Fifty of the participants were men and 117 were

women; the median age of participants was 19 (range = 17 to 55). Whole classes (containing between 19 and 27 students) were randomly assigned to one of the three leadership selection procedure conditions (formal, control, random). Each class was randomly divided into between three and four groups, most of which contained 4 members (range = 3 to 5 members). There were 19 groups in the formal condition, 14 in the control condition, and 13 in the random condition.

Materials and Procedure

At the start of the class all participants completed the leadership skills inventory developed in Experiment 1. Participants all scored their own responses on this test and were then assigned to groups. Each group was led away to a separate cubicle to await instructions from the experimenter. According to the experimental condition to which they had been assigned, the experimenter then gave each group a set of instructions about how to proceed.

In conditions in which a leader was chosen the experimenter either identified the person whose name came last in the alphabet (in the random leader condition) or the person who had scored highest on the leadership skills inventory (in the formal leader condition). In both cases the leader was then handed the materials necessary for the study and had to commence by reading the following instructions to the rest of the group:

> In this study, we are about to take part in a group problem-solving task. As your leader, my job is to ensure that we all agree with all the suggestions we make. However, as a leader, I am also going to try to encourage everyone to participate so that we can all generate ideas and each make a contribution to the group. At the end of the experiment, others will look at how good we have been at directing ourselves to a decision with which we all agree.

In the control condition, in which no leader was identified, the following instructions for the task were simply placed on a table inside the cubicle and the experimenter invited the group as a whole to read them and proceed with the task:

> In this study, we are about to take part in a group problem-solving task. At the end of the experiment, others will look at how good we have been at directing ourselves to a decision with which we all agree.

These instructions were thus similar to those for the other two conditions, but the opening rubric made no mention of a group leader.

Having been read these instructions, the groups then performed a modified version of the fallout shelter exercise developed by Johnson and Johnson (1991, pp. 273–274). This exercise presented information suggesting that in the build-up to the 2000 Olympic games Australia had developed increasingly tense relations with one of its neighbors (nation X) and that as a result "the possibility of nuclear attack has been widely discussed and preparations are underway in earnest." In preparation for such an attack, the group's task was to rank 15 items (e.g., garbage cans, soap and towels, a Geiger counter) in terms of their importance for the whole group's survival in an underground bunker.

Groups were given as much time as they needed to perform each task. After they had finished, the groups disbanded and participants were asked to complete a posttest questionnaire individually. As well as asking them to provide their own ranking of the 15 items, participants responded to the following seven questions on 7-point rating scales (with scale anchors labeled as indicated in parentheses):

1. How much did you identify with the other members of your group? (1 = *very little*, 7 = *a great deal*)
2. How similar did you perceive yourself to be to other members of your group? (1 = *not at all*, 7 = *extremely*)
3. How effective was the process by which your group reached its decision? (1 = *extremely ineffective*, 7 = *extremely effective*)
4. How much effort did you personally put into making the decision? (1 = *very little*, 7 = *a great deal*)
5. Think of the person who showed most leadership in your group. How much leadership did they exhibit? (1 = *very little*, 7 = *a great deal*)
6. How legitimate was the process by which your leader was selected? (1 = *not at all*, 7 = *extremely*)
7. How much did you enjoy taking part in this study? (1 = *very little*, 7 = *a great deal*)

It is worth noting that the referent in the fifth question was not specified precisely (i.e., participants need not have been rating the group's appointed leader). This ambiguity ensured that this item made sense to participants in the control condition.

Dependent Measures

These were the same as those in Experiment 1, corresponding to the group-goal and group-

maintenance functions identified by Cartwright and Zander (1960). The primary measure was still group performance on the survival task.

Results

Quality of Group Decision

The quality of groups' decisions (deviation from expert ranking) was analyzed using orthogonal planned contrasts. The first of these compared the performance of groups with randomly selected leaders with the performance of groups with formally selected or no appointed leaders. A second contrast compared the performance of groups with formally selected and no appointed leaders. Consistent with predictions, these contrasts indicated that the quality of group decisions was superior in groups in which the leader was randomly selected (random leader $M = 46.15$, formal–no leader $M = 51.94$), $t(43) = 2.22$, $p < .05$, but that there was no difference in the quality of decisions across the other two conditions (formal $M = 51.71$, no leader $M = 52.11$), $t(43) = 0.14$, *ns*. Means for this and all other measures are presented in Table 2.

Individual Measures

Leaders' responses. Because there were no group leaders in the control condition, ratings made by group leaders on posttest items could only be compared across randomly selected and formally selected leader conditions. Three effects emerged from this analysis. These indicated that leaders who were formally chosen enjoyed the task more (formal $M = 4.83$, random $M = 3.82$), $t(30) = 2.29$, $p < .05$, perceived themselves to be more effective (formal $M = 5.72$, random $M = 5.00$), $t(30) = 2.21$, $p < .05$, and to have made more effort (formal $M = 5.56$, random $M = 4.00$), $t(30) = 3.60$, $p < .01$.

Nonleaders' responses. Ratings made by group members who were not leaders were again examined by using orthogonal planned contrasts to compare (a) the responses of individuals from groups with randomly selected leaders to those of individuals from groups with formally selected or no appointed leaders and (b) the responses of individuals from groups with formally selected and no appointed leaders. Consistent with predictions, these contrasts indicated that individuals from groups with randomly selected leaders deviated less from the group decision than those in the other two conditions (random $M = 4.38$, nonrandom $M = 6.75$), $t(136) = 1.97$, $p < .05$. There was no difference in the amount of group maintenance shown by followers in groups with formally selected and no leaders (formal $M = 6.09$, no leader $M = 7.21$), $t(136) = 0.92$, *ns*.

Additional effects indicated that individuals from groups with randomly selected leaders perceived the decision-making process to be less effective than those in the other two conditions (random $M = 4.83$, nonrandom $M = 5.41$),

Table 2
Means for Experiment 2: Performance and Posttest Measures

Measure	Leadership selection procedure			Significant effects
	Control (C)	Formal (F)	Random (R)	
n (no. of groups)	63 (14)	62 (19)	42 (13)	
Group measures				
Group decision (deviation from expert rankings)	52.11	51.71	46.15	CF vs. R*
Individual follower measures				
Deviation from group decision	7.21	6.09	4.38	CF vs. R*
Identification with group	5.00	5.35	4.93	
Similarity to group	4.81	4.65	4.66	
Effectiveness of decision process	5.30	5.58	4.86	CF vs. R*
Effort made	5.30	5.35	5.10	
Amount of leader's leadership	4.91	4.91	5.52	CF vs. R**
Legitimacy of leader	3.35	3.58	2.24	CF vs. R**
Enjoyed participation	4.98	5.12	4.38	CF vs. R**

* $p < .05$. ** $p < .01$.

$t(136) = 2.44$, $p < .02$, that they thought their leaders showed more leadership (random $M = 5.52$, nonrandom–no leader $M = 4.91$), $t(136) = 2.59$, $p = .01$, that they thought the leader selection process was less legitimate (random $M = 2.24$, nonrandom–no leader $M = 3.44$), $t(136) = 4.00$, $p < .001$, and that they enjoyed the experiment less (random $M = 4.38$, nonrandom–no leader $M = 5.04$), $t(136) = 2.44$, $p < .01$.

Discussion

The results of this experiment replicated and extended those of Experiment 1 and lend further support to the idea that there are conditions under which groups with randomly selected leaders may perform better than those with leaders selected by more formal means. Moreover, because the design of this experiment incorporated a control condition in which groups had no leader, we can be more confident that these results do not reflect the fact that leadership was altogether superfluous in this situation. The findings of this study also go beyond those of the previous experiment in demonstrating that random leadership served a group maintenance as well as a group goal function. That is, groups with random leaders not only performed the survival task better than groups with a formal or no leader, but their individual members were also more likely to abide by the group's decision at the posttest phase.

As well as leading to effects on these primary measures this study also produced an array of effects on posttest measures. Most straightforward, there was evidence that participants from groups in which the leader was randomly chosen perceived the leader selection process to be relatively illegitimate compared with participants in other conditions. This effect is not especially surprising and can be seen to reflect widespread beliefs about the inappropriateness of choosing leaders on a random basis—beliefs that are common among leadership researchers and in society at large. Nonetheless, leaders in groups with random leaders were also seen by their followers to display more leadership than leaders in the other two conditions. This pattern is consistent with results obtained on primary measures, but it is important to acknowledge that there is some ambiguity associated with this measure as followers' responses did not necessarily refer to their perception of the appointed leader. Indeed, it is likely that in these conditions someone other than the appointed leader may have taken over the leadership role (a factor that may also contribute to the group's leadership being perceived as illegitimate).

Despite the above effects, it is worth noting that posttest measures did not reveal any variation in the amount of effort that followers in different conditions reported making. There is thus no direct support for the argument that followers in groups with randomly selected leaders were counteracting a tendency to loaf in light of the shortcomings they perceived in those leaders. Nonetheless, as we noted above, it seems likely that in this condition nonleaders stepped into the breach to perform some leadership roles—thereby displaying some compensation for the leader's limitations (Williams & Karau, 1991). It is also important to note that previous research suggests that loafing measures of the form used here are not especially sensitive (Karau & Williams, 1993). The exact contribution of social loafing to the present findings therefore remains to be clarified in future research.

It is interesting though that posttest measures also indicated that participants in groups with randomly selected leaders were generally less satisfied with the decision-making process and enjoyed participating in the study less than participants in the other two conditions. These effects are consistent with the fact that leadership selection was perceived to be less legitimate when leaders were randomly chosen and also mirror the pattern of perceived leader effectiveness produced in Experiment 1. Nonetheless, they are clearly intriguing in light of evidence that groups with randomly appointed leaders actually made superior decisions.

However, as we argued in discussing the results of the previous study, it is possible that these effects reflect participants' beliefs about how groups ought to make decisions and about how leaders should be selected and behave. To the extent that the random selection of group leaders clearly violated these expectations, it may not be surprising that group members were relatively uncomfortable with the process as a whole and were also unaccepting of it.

In effect, then, findings on these additional measures serve to highlight both the unorthodox nature of the present manipulation and the counterintuitive nature of our findings. Gener-

ally speaking, in Western societies people now expect leaders to be selected on the basis of personal excellence and to bolster group performance accordingly. To illustrate this point, Sarros and Butchatsky (1996) quoted the view of Elizabeth Proust (formerly CEO for the City of Melbourne): "if [we] get the leadership right we can get everything else right" (p. 130). Indeed, this analysis is customarily used (as it is by Sarros & Butchatsky, 1996) both to explain why certain groups do well and to justify the high remuneration that senior executives receive for the leadership roles they fulfill.

In this study there was also evidence to suggest that the behavior of the leaders themselves conformed to similar stereotypic expectations. That is, leaders who were formally selected reported making more effort, enjoying the study more, and being more effective than those who were randomly chosen. Of course, these effects may also reflect the relative immodesty of those who emerged as leaders in the formal condition (in which their selection was based on a willingness to describe themselves as capable, objective, communicative, and so on). Nonetheless, the clear irony here is that these leaders were actually less effective than those who were randomly chosen.

At a purely empirical level, this study therefore questions conventional wisdom in two important ways and invites speculation that stereotypic beliefs about the nature of leadership may have a self-fulfilling role both for those who assume the mantle of leader and for those who justify the system within which particular selection processes operate. First, it makes it clear that this analysis is actually an article of faith rather than one that has been subjected to empirical verification. It is not necessarily the case that systematic selection of leaders produces better group outcomes. Second, it suggests that just because people believe that systematic leader selection is a superior process that improves group performance, this need not actually be the case. The conditions that participants in this experiment thought were delivering better outcomes were not the ones that actually did.

Experiment 3

In an attempt to make sense of the various findings obtained on posttest measures in the previous study, it was suggested that participants had implicit expectations about the inherent superiority of systematically selected leaders. We argued that these expectations dictated their feelings about the group decision-making process in spite of the fact that they were actually at odds with the obtained results. Clearly though, there is no direct evidence to support this analysis. Indeed, it is possible to develop arguments suggesting that people might actually expect groups with no leader or a randomly selected leader to perform well on these tasks.

To obtain evidence that bears directly on this issue, this final study asked people who had not participated in (and who had no knowledge of) our research program to indicate how they would expect groups in the various conditions of the previous study to perform. Participants were presented with a synopsis of the group task and given details of the three independent conditions (no appointed leader, random leader, systematically selected leader). They were then asked to indicate how well they thought each group would perform the task and how nonleaders would react to the group decision on measures modeled on those included in the previous study's posttest measure.

On the basis of arguments developed in the above discussion, our primary expectation was that respondents would expect group performance to be inferior when leader selection was random relative to the other two conditions but that they might also expect performance to be better when the leader was systematically selected than when no leader was appointed. These hypotheses were examined in relation to estimates of group performance, as well as measures of participants' expectations of group maintenance, identification, effort, legitimacy, and enjoyment.

Method

Participants

Thirty-two people who were naive to the present research participated in this study. None were psychologists or psychology students; all were acquaintances of the authors from a range of social and professional backgrounds who were contacted informally on an ad hoc basis.

All participated voluntarily and received no payment or other reward.

Materials and Procedure

Participants were given a two-sided response sheet to complete. The introduction to this provided the following information relevant to the task and design of the previous study: Three groups of four people perform a task in which they decide how useful each of a range of items will be in a survival situation [brief details of which were provided]. The task is quite difficult and takes about 30 min to perform. Each group performs the task under a different set of conditions, so that *Group A* has no leader; *Group B* has a randomly selected leader (the person whose name comes first in the alphabet); *Group C* has a systematically selected leader (the person who is identified as having most leadership skills).

Having been given this information, participants then had to respond to the seven questions below. They made a separate response to indicate the performance of each group (so that each question was answered three times) and did so using 5-point rating scales. The anchors of each scale are indicated in parentheses.

1. How well do you think each group will perform the task? (1 = *very poorly*, 5 = *very well*)

2. How much will the nonleaders in each group want to stick with the group's decision? (1 = *very little*, 5 = *very much*)

3. How much will the nonleaders in each group identify with the group? (1 = *not at all*, 5 = *extremely*)

4. How similar will the nonleaders in each group think they are to other group members? (1 = *not at all*, 5 = *extremely*)

5. How much effort will the nonleaders in each group make? (1 = *very little*, 5 = *very much*)

6. How legitimate will the nonleaders in each group think the leadership of their group is? (1 = *very little*, 5 = *very much*)

7. How much will the nonleaders in each group enjoy performing the group task? (1 = *very little*, 5 = *very much*)

Results

Responses to the seven items were analyzed by using orthogonal planned contrasts. The first of these compared the expected performance and reaction of groups with randomly selected leaders with those of groups with formally selected or no appointed leaders. A second contrast compared the expected performance and reaction of groups with formally selected and no appointed leaders. Means on all measures are presented in Table 3.

The first of these two sets of contrasts revealed that on all but one measure (followers' perceived similarity to the group) the performance and reaction of the group with a randomly selected leader was expected to be significantly less positive than that of a group with a systematically selected or no leader. Thus the group with a randomly selected leader was expected to perform the survival task worse than the other two groups (random $M = 2.56$, others $M = 3.42$), $t(31) = 4.67, p < .001$. Its followers were also expected to adhere less strongly to the

Table 3
Means for Experiment 3: Naive Participants' Expectations of Performance and Posttest Reaction

Measure	Leadership selection procedure			Significant effects
	Control (C)	Formal (F)	Random (R)	
Group performance	2.69	4.16	2.56	CF vs. R**, C vs. F**
Adherence of nonleaders to group decision	2.97	3.75	2.38	CF vs. R**, C vs. F*
Identification of nonleaders with group	3.44	3.47	2.72	CF vs. R**
Similarity of nonleaders to group	3.72	3.31	3.25	
Effort of nonleaders	3.78	3.50	2.94	CF vs. R**
Legitimacy of leadership	2.56	4.16	2.16	CF vs. R**, C vs. F**
Enjoyment of nonleaders	2.88	3.22	2.50	CF vs. R**

* $p < .05$. ** $p < .01$.

group's decision (random $M = 2.38$, others $M = 3.36$), $t(31) = 4.95, p < .001$, to identify less with the group (random $M = 2.72$, others $M = 3.45$), $t(31) = 3.15, p < .01$, to make less effort (random $M = 2.94$, others $M = 3.64$), $t(31) = 3.75, p < .01$, to perceive the leadership to be less legitimate (random $M = 2.16$, others $M = 3.36$), $t(31) = 4.83, p < .001$, and to enjoy the task less (random $M = 2.50$, others $M = 3.05$), $t(31) = 2.90, p < .01$.

The second set of contrasts also indicated that there were some differences in the perceived performance and reaction of groups with a systematically chosen leader and no leader. Specifically, the group with a systematically selected leader was expected to perform better on the survival task than the group with no leader (systematic $M = 4.16$, no leader $M = 2.69$), $t(31) = 6.18, p < .001$. Its followers were also expected to adhere more strongly to the group's decision (systematic $M = 3.75$, no leader $M = 2.97$), $t(31) = 2.60, p < .02$, and to perceive the group's leadership to be more legitimate (systematic $M = 4.16$, no leader $M = 2.56$), $t(31) = 6.01, p < .001$.

Discussion

These results provide very clear evidence to support our previous assumptions that groups with randomly selected leaders would be expected to perform a survival task less well than groups with either a systematically elected or no appointed leader. Indeed, not one single participant believed that the group with the randomly selected leader would perform the survival task better than the group with the systematically appointed leader (and only two respondents thought the groups would perform the task equally well). The study also provides evidence to support suggestions that followers in groups with random leaders would be expected to show less group maintenance, to identify less strongly with the group, to make less effort, to perceive their group leadership to be less legitimate, and to enjoy the group experience less.

In revealing these effects, the study serves two important functions. First, it confirms that the findings of our first two experiments are genuinely counterintuitive insofar as they reveal effects on primary measures of group performance that are directly counter to these expectations. Second, though, the present results also indicate that stereotypic views about the dysfunctional nature of groups with randomly selected leaders were likely to have been available as a resource that could inform participants' responses on posttest measures in those earlier studies. In the previous study in particular, we had made an assumption of this form to account for the clear discrepancy between the actual performance of groups and participants' reflections on that performance.

Elaborating on the above point, it seems clear that stereotypes about the role of group leaders in contributing to group performance are clearly defined and widely shared. We can be particularly confident of the latter point in view of the fact that the participants in this experiment were drawn quite widely from the general community. These beliefs therefore appear to possess many of the key properties of other stereotypes, not least because they can be seen to serve a range of social functions (Tajfel, 1981). So, among other things, they serve (a) to differentiate between supposedly expert leaders and their followers, (b) to explain the differential treatment and respect accorded to leaders, and (c) to justify that special treatment. Like other stereotypes, it also seems likely that they exert a powerful grip on those who hold them, while at the same time being highly contestable at an empirical level (Oakes et al., 1994).

However, it is also clear from the present data that stereotypic views about the appropriateness of particular leadership selection strategies are not confined to a belief that randomly selected leaders will be dysfunctional. Consistent with much of the implicit theory in organizational psychology (e.g., Ritchie & Moses, 1983; Sarros & Butchatsky, 1996; M. Smith & George, 1994), it is apparent that groups with systematically selected leaders are generally expected to perform better than those with no leader. Again though, the data from Experiment 2 provide no support for this expectation. It is also worth adding that this finding strengthens our earlier claim that the tasks used in this study were not ones for which a leader would generally be perceived to be superfluous (cf. Howell et al., 1990; Watson & Behnke, 1990).

General Discussion

Considered together, our first two experiments provide convergent evidence to support the suggestion that randomly chosen leaders can produce superior group performance than leaders chosen by other means. In both experiments groups with randomly selected leaders arrived at solutions to a group task that were, by established criteria, superior to those made by groups whose leader had been nonrandomly chosen, or (in Experiment 2) that had no appointed leader. In addition, evidence in the second experiment suggests that random leader selection was associated with greater commitment to the group and its decision.

These findings thus serve to question the widespread (and rarely challenged) belief that the process of systematic leadership selection is always in the interests of enhanced group performance. This assumption is often taken for granted, and the results of Experiment 3 suggest that it is widely shared and strongly held. However, as we noted at the outset, there are in fact good theoretical grounds for believing that systematic leader selection may at times be counterproductive. If a group can legitimately claim that "united we stand, divided we fall," then it is possible to account for findings like those we have obtained by suggesting that leadership selection can undermine group performance because it brings to light and engenders intragroup division (cf. Hollander, 1995; Jackson, 1992; Worchel, 1994).

It needs to be emphasized, however, that the present experiments did not produce evidence that conclusively supports any one theoretical analysis of the above effects—even though the second experiment ruled out a number of plausible explanations. Accordingly, further work to clarify the exact mechanism underpinning the present findings is clearly necessary. This fact, however, does not diminish the empirical and applied significance of the research. It merely means that important questions have been raised to which all the answers have not yet been provided. Indeed, we have argued elsewhere that the process of creating uncertainty through research is just as important for practical and theoretical progress as the counterdemand to reduce uncertainty by providing methodological and theoretical closure (Haslam & McGarty, 1998).

It should also be clear that we do not think it is appropriate to conclude from the present research that the random selection of leaders will always enhance group performance. Broadly speaking, we would suggest that random leader selection might only ever be advantageous when the group (a) has a clearly defined shared goal, (b) is disposed or able to behave in a relatively democratic manner (e.g., involving shared decision making and division of labor and responsibility), and therefore (c) in the absence of a leader being appointed might tend to have a reasonably strong sense of shared social identity anyway. Clearly these circumstances are not ubiquitous and will typically prevail when small groups perform well-defined tasks (cf. Howell, Dorfman, & Kerr, 1986; Kerr & Jermier, 1978). Having said that, many important groups in workplaces and other settings have exactly these qualities and in such situations it may often be the case that leadership is sought purely in the interests of personal self-advancement (Kanter, 1979; Mulder, 1977).

In this regard, this article suggests a number of avenues for researchers to explore further and points to a number of new hypotheses to be tested. Two priorities are (a) identifying boundary conditions for the effects uncovered in this article, and (b) providing evidence to support a process-based explanation of these effects. It is important to note that there appears to be every possibility that this work, and the approach we have taken, will continue to generate counterintuitive predictions and findings. We therefore hope that future research will play a useful role in questioning and revising implicit assumptions about the impact of routine organizational strategies on group performance.

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