Effects of a Legitimate Authority's Justification of Inequality
on the Mobilization of Revolutionary Coalitions*

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## I. Introduction

Nothing very interesting seems to go on in the equity condition of inequity experiments.  Such experiments are largely concerned with reactions to inequity. Equity theories[1] typically assume that there are no reactions to equity if outcomes are proportional to inputs, hence there are no fruitful questions to ask about equity.  The present paper describes the results of a fairly typical equity experiment concerned with revolutionary coalitions[2], i.e. coalitions of individually less powerful members of a group who combine their resources for the purpose of reallocating resources and rewards.  It departs from the typical revolutionary-coalitions experiment only in focusing on the equity rather than the inequity condition.

Our experiment manipulated equity by informing S's in a centralized communication network that the person in the central position, who was the only member of the group with an opportunity to win a a sizeable bonus, had exceptional ability at the task.  It was found, as other such experiments have found, that this manipulation produced significantly fewer revolutionary coalitions than an inequity condition.

Routine checks on the success with which the independent variables were manipulated showed that we did not produce beliefs in individual S's that the differential opportunity for rewards was equitable.  This essentially means that the result produced by the experiment is what Bachrach and Baratz (1962, 1963, 1970) call a "nondecision," i.e. a suppressed issue.  The question with which the paper is concerned is therefore:  Why did S's in the "equity" condition not make an issue of an inequity perceived at the individual level?  We believe the answer to the question will shed some light on an important feature of legitimacy as a factor in collective action.

II.  Statement of the Problem

The reason why equity theories do not find questions about equity itself fruitful is that they tacitly assume that the meaning of "inputs," "outcomes," and "proportionality" are given in the nature of things.  They take them for granted without questioning how they come to be defined as inputs, outcomes, and proportional.  Equity is therefore an inert state, a system at rest, in which nothing much seems to be going on.

But inputs, outcomes, and proportionality are not self-evident. The central idea of any kind of equity theory (and this holds also for status value theories, or any other possible theory of justice) is that similar contributions ought to be similarly rewarded.  But in itself this is not very useful empirically.  What has to be given is the precise features of the similarity relation (Cf Goode, 1978, Ch. 13).  But these are quite arbitrary social products, depending on how states intrinsically given in nature (which tend to shade off into infinite variety) are grouped and related by social usage.  In our view, task-outcomes, inputs, reward-outcomes, and proportionality are all intrinsically indeterminate. Hence, they are somehow "constructed" objects and relations.  The question to ask about "equity" therefore is how equity is constructed.

We do not propose to answer this question in any very general way. In some sense we give the quite obvious answer that "equity" is "socially" constructed.  But the case that we consider in this paper is concerned only with the conditions under which collective action emerges, i.e. the case in which a revolutionary coalition is necessary (and sufficient) to create or maintain equity.  The problem for the actor(s) in this case is whether to join in collective action with others and to mobilize the resources of the members for the pursuit of joint goals.

The mobilization of coalitions is problematic.  Individuals will often prefer other tactics to restore equity, particularly those not requiring reliance

on allies. Even when coalition formation is the only reasonable tactic, individuals sometimes prefer to do nothing rather than form an alliance. (See Michener and Lawler, 1971; Michener and Lyons, 1972; Lawler, 1975.) Hence, it does not follow from the existence of a common interest in "equity" between two actors that they will coalesce.

Under what conditions, then, does collective action in the interest of "equity" emerge? Both the natural and the experimental literature support the idea that, as Michener puts it, revolutionary coalitions are the interactive product (in the statistical sense) of individual-level discontent and organizational factors. Organizational factors here refer to organizational capacities--existing levels of solidarity and communication, conditions of assembly, etc.--and inter-group power-dependence relations, having to do with the strength of a coalition relative to that of a target (e.g. the state) and the readiness on both sides to use this strength.

Both of these organizational factors (as well as the individual-level social psychology with which they interact) depend, in turn, in part on moral factors, i.e. "legitimation". In the experimental literature, where it is easier to disentangle what is going on, the idea that inequity is something more than just relative deprivation is strongly supported: It is the unfairness, or illegitimacy, of a system that promotes revolutionary coalitions (cf. Hoffman, Festinger, and Lawrence, 1954; Lawler, 1975; Webster and Smith, 1978). Moral justification is a factor both in organizational capacity and relative strength of a coalition. What "equity" does for organizational capacity is create legitimacy: It defines existing systems as wrong, actions against the system as right. This becomes the foundation of a revolutionary coalition's claims on the resources of its members, on which its organizational capacity (in part) depends. It does not affect the given resources of individual members, but it does affect both the capacity of the collective to mobilize these resources and its readiness to use them. Hence

it is also important to inter-group power-dependence relations. The study of
naturally occurring revolutionary coalitions supports the same conclusion:
legitimacy increases the strength and readiness of revolutionary coalitions to
act and decreases the readiness of targets like the state to use forceful methods
of social control.[3]

Over and above effects on organizational capacity, the strength of a
coalition, and the strength of the state, legitimacy is also an important
factor affecting exogenous resources available to revolutionary coalitions.
Leites and Wolf (1970) and McCarthy and Zald (1973; 1977) have directed the
attention of resource mobilization theories to resources mobilized from sources
outside of social movements, i.e. from people who are not directly benefited by
them. McCarthy and Zald, in particular, have implicitly recognized how important
moral factors are to this process, calling these nonbeneficiaries "conscience"
adherents of movements (1977).

Both organizational capacity and (collective) power-dependence relations
are clearly external factors in the formation of revolutionary coalitions, i.e.
factors outside the individual. Moral justification, on the other hand, may
appear at first sight to be a factor internal to the individual. We do not
believe this to be true. In fact, in the present paper we want to argue that
personal beliefs in the moral justification of protest are neither necessary nor
sufficient to mobilize a revolutionary coalition. In taking this position, we
reason from two kinds of theory: First, from justice theory itself; second, from
more general theories of legitimation.

Recent evidence offers good reason for preferring the theory of "distributive
justice" to "equity theory" (Webster and Smith, 1978). The labels, of course are
arbitrary and both theories are oriented to the same kinds of phenomena, but an
essential feature of justice theory that differentiates it from equity theory is

that "reactions to injustice" depend on "referential" comparisons, i.e. comparisons with an externally created and maintained frame of reference.  In this theory no individual can create a referential structure alone; referential structures are made up of generalized others and are external to any particular interaction.

The operation of such structures, furthermore, does not depend on any one individual's acceptance of their legitimacy. Many contemporary uses of Weber's theory of legitimacy tend to emphasize the voluntaristic features of his theory, hence the importance of consent; and consent is tacitly often taken to mean every particular individual's consent.  But Weber himself held a much more complex and useful view.  He defined legitimacy as the validity of a normative order (Weber, 1978, Ch. 1, Sec. 5).  Validity he defined as the "binding" quality of the order. One consequence of validity, in this sense, he held to be the probability that a normative order would be supported.  Given a valid normative order, motives of any particular individual for compliance might therefore be habit, expediency, or legitimacy (in the sense of personal acceptance of the normative order as right). More recently, Dornbusch and Scott (1968) have made more room for the attitudes of the powerless towards an order but like Weber, they continue to emphasize the importance of validity.  They distinguish validity, the existence of a binding rule in a group, from propriety, the attitudes of any particular individual to that rule.  As in Weber, the stability of a normative order does not require that every individual accept its legitimacy, reject the legitimacy of actions violating that order, accept the legitimacy of the system of authority enabling the order to mobilize member resources for purposes of enforcing it, or reject the legitimacy of a revolutionary coalition's claims to these resources.  Validity creates support.  Support provides resources required to enforce compliance with a normative order, hence creates expedient motives for compliance.  The regularity of conduct thus produced may also induce habitual compliance.

Because not all individuals necessarily hold the same values and validity depends for its effect in part on support, Dornbusch and Scott also distinguish who initiates and supports a normative order by their resources and their relation to the individual taken as a point of reference. They make a point of the importance of authorization, i.e. of the values of centers of power that assure resources backing an order (Cf. Stinchombe, 1968, Ch. 4). But they also give a significant place to endorsement, i.e. the values and support of powerless individuals.

It follows from this theory that propriety alone is not sufficient to create a legitimate social order and in the absence of propriety a legitimate social order will still compel compliance. Hence, we must think of the legitimation of revolutionary coalitions as a collective, not an individual, process. If, for the moment, we think in terms of a simple Dornbusch-Scott model in which the allocative behavior of an authority, A, is authorized by those who have resources, A', and endorsed by subordinate actors (B's peers), B', conditions do not favor forming a revolutionary coalition no matter how strong B's own sense of the inequity of the system. If, on the other hand, A' does not authorize A's conduct and B' does not endorse it, it is doubtful that B's personal sense that the behavior of A is right is sufficient to make it "legitimate". In other words, "legitimacy" might best be thought of as "right in the eyes of others." Looked at from the point of view of any particular individual, it is something that cannot be created alone.

The case in which A' authorizes A's conduct and this conduct is endorsed by B' almost guarantees the collapse of a revolutionary coalition, and it is this condition that has attracted the attention of Bacharach and Baratz, who describe its consequences as "nondecisionmaking." A nondecision is essentially a suppressed issue. It describes the nonacts of individuals who do nothing about an issue,

however deeply they may feel it. According to Bachrach and Baratz (1962; 1963; 1970) nondecisions are the product of the "mobilization of bias" (an expression taken from Schattschneider, 1960), i.e. of invoking the existing values, beliefs, rules, practices and procedures of a system which operate to delegitimate actors, issues, and tactics that threaten it. The effect of the mobilization of bias is to reduce the organizational capacity of a movement, deauthorizing its rights to make claims on individual-level resources, and to increase the readiness of authorized agents of the system to use organizational resources to repress the movement. Bachrach and Baratz's central position is that existing values, beliefs, rules, practices and procedures, hence existing institutional structures, shape what issues emerge and the form they take. (See Zelditch, el al, 1982, for a critical assessment of Bachrach and Baratz's theory.)

In part the effects of legitimacy are direct: What people see as proper, hence what alternatives are thinkable, is determined by the existing system's way of thought. In part the effects are indirect: Even if individual B does not believe what he sees and hears, the fact that others do lends it a facticity that makes it difficult to question, and an authorization and endorsement that makes questioning it both useless and inexpedient. There is also, of course, the effect this support has in legitimating the authority of A, which directly affects the conduct of B who anticipates how A will react if B tries to change an accepted system of values and norms. The effects of a moral order are more visible earlier in the policy process because legitimacy determines which issures emerge into the open in a group. Legitimacy, of course, is a factor in political struggles at every stage of policy formation. Propaganda routinely is directed at labelling issues and actors at the decision stage of the process in such a way that support for one side is increased or decreased. But the variance in legitimacy is nevertheless less for issues already in the political arena, because legitimacy is an important factor in

determining which issues emerge in the first place. There is less variance

in the legitimacy of the contending actors, less variance in the legitimacy

of the tactics employed, and less variance in the legitimacy of the issues

contested. In a sense, both sides of an issue acceptable to a polity's agenda

are more or less legitimate. Hence, the place to observe how legitimacy oper-

ates--according, still, to Bachrach and Baratz--is before agendas are set.

III. The Problem of a Method.

But "nondecisions" pose a nasty problem of method. They are the undecided

issues of a polity, but there are an infinite number of things a polity does not

decide, only some of which are suppressed issues. The others were never issues

to begin with. The problem is to distinguish the nonissue from the suppressed

issue. Bachrach and Baratz provided no independent test of issueness, i.e., of

the intensity and/or scope of a population's preferences for change, hence were

not able to empirically identify nondecisions. (See Frey, 1971; McFarland, 1969;

Merelman, 1968; Wolfinger, 1971.) Nondecisions are essentially counter-factual

agendas--issues that, in the absence of some countervailing factor would have

been on a polity's agenda. What has to be shown is that, other things being

equal, an issue would have emerged. The comparative method is the conventional

solution to this problem. Comparing otherwise similar polities, a variable $X$ can

be said to cause a nondecision if and only if the issue is present when $X$ is

absent. McFarland (1969) recognized this fairly quickly and his solution was

quickly and brilliantly applied by Crenson (1971). But the complexity and multi-

colinearity of the variables correlated with issueness (Polsby, 1980, ch. 11)

together with the difficulties of cross-level inferences about subjective per-

ceptions from aggregate-level objective measures (Snyder, 1979) have left serious

problems of internal validity unsolved.

A method by which a more strict standard of internal validity can be achieved

is experimentation. By this method we can produce groups which are alike in the conditions believed to create an issue--which in this case is an inequality in the opportunity to win a bonus. We can randomize other factors relevant to the subjective definition of this issue. We can precisely isolate the effects of legitimation--in this case, the justification of inequality by reference to subject differences in ability. And we are in a good position to precisely measure both the behavior of the S's and their perceptions of the issues.

Before describing the design of this experiment, however, we want the reader to understand that, although it was designed with the above theoretical ideas in mind, we fully expected to create equity not only in the aggregate but also in each individual. That is, the experiment was in fact designed to make inequality proper as well as valid. The idea was to fit this case into a larger program of other experiments in which validity was created but propriety was not (Walker, et al, 1980; Thomas, et al, 1980). Checks on the manipulation of the independent variable revealed that S's did not accept the equity of the reward system. Nevertheless, they formed significantly fewer revolutionary coalitions in the equity condition than in the inequity condition (which was originally designed merely as a baseline criterion that in fact an issue would have emerged had we not legitimated inequality). Hence, we created what Bachrach and Baratz mean by a nondecision without intending to do so. Most S's felt the situation to be improper but they did not act on that perception. Our problem is to find out, ex post facto, why they did not.

This gives a somewhat irregular organization to the paper: First we will demonstrate that, behaviorally, inequity produced significant differences in the frequency of revolutionary coalitions. Only then will we deal with the routine checks on the manipulation of the independent variable. Studying these checks, built into the post-session questionnaire, we will show that whatever produced

the behavior of the S's it was not the successful manipulation of equity. Third, we will study, in turn, three possible explanations of our result: We will show that it was not because we legitimated the other basic features of the structure of the group, which therefore resisted change. Although there are strong correlations in the post-session questionnaire data between approval of the group's structure and resistance to change, these are post-hoc. Nor was the result due to a conventionally conceived experimenter effect, i.e. by E directly signalling his hypothesis to S who, out of cooperative motives, confirmed E's hypothesis for him. Postexperimental inquiry (of the kind described by Orne, 1962) showed that either S's did not perceive E's hypothesis or were not reluctant to refute it to E's face. The effect, it will turn out, was due to a demand characteristic of the experimental setting, but of a somewhat special kind—a kind, we will argue, that is an example of the legitimation by a system of values, rules, and beliefs rather than of compliance to persons. We then use this finding to speculate about the collective nature of the process of legitimation, i.e. about how validity works in the face of impropriety.

IV. Design of the Experiment.

Four subjects (S's) are seated in individual cubicles and told that they are part of a five-person group which must solve a series of ten problems. Each problem constitutes one trial of the experiment. Each S has some but no S has all of the information necessary to solve the problem. To get the answer each S exchanges information with each other member of the group by means of written memoranda. This exchange is limited in two ways: (1) only task-relevant information can be exchanged, and (2) each S can communicate only with a limited number of others. Specifically, there is one central person, C (a confederate), who can communicate with everyone else; but all others can communicate only with C. (This is known as the "wheel" network—see Bavelas, 1950). By exchanging

information with C, S's can get all the necessary information and solve each problem. The team as a whole is credited with 60c for each correct answer it submits to E's office. Team earnings are divided equally among the members at the end of the experiment.

After a practice problem subjects are told that in order to get them to work faster a bonus of $3.00 will be paid on each problem to the team member with the first correct answer. Because the communication system is centralized, the bonus will always go to C, the confederate, but a team is allowed to add more channels of communication if it is willing to pay a small cost, to be shared by all members (Mackenzie, 1976). An all-to-all network, for example, would equalize opportunity to win a bonus. Because the cost must be borne by all members, to add more channels a majority of the group must agree. S's are told that an "election" to decide the issue will be held by E if any member of the group (1) proposes a specific agenda and (2) obtains a second by one other member. Thus, from the point of view of the peripheral members, the problem is to mobilize the resources of his peers to accomplish a change that restores equity. No election is actually held: each S is stopped and interviewed at the point at which a proposal to change the structure is first made.

In the baseline, or inequity condition, the inequity of the bonus was underlined by telling S's early in their instructions that all of them were alike, had been allocated to positions in the communication network by chance, and would be given exactly the same amount of information to start with. In the equity condition, although S's were again told what they would all be given exactly the same amount of information, the center was differentiated from all other members. S's were told that:

"The members of your team are...similar in several ways.
For example, you are all similar in age, attend the same
school, and are of the same sex. The position you occupy

in the group, with the exception of the (center)[4] position was determined by chance. You will recall that when you came in you were asked to select one of four colored tokens. The color of the token you selected determined whether your code color would be red, yellow, blue or green. For your group, however, we have specially selected the person who occupies the orange position. We have chosen a person for the orange position who is both experienced at the problems on which your group will be working and is very adept at the solution to these problems. In fact, the person who is orange in your group has demonstrated a performance level in the top 1% of all the people who have worked on these problems over the past year and a half."

The dependent variable of the experiment consists of the trial at which a message is sent by S to any other member of the group proposing a change in the communication network. A message proposing to rent one or more channels of communication is called a C-response (for "change-response").

V. Results.

(1) Behavioral Results. A total of 52 paid undergraduate volunteers were randomly allocated to the equity or inequity conditions. Of these, 12 did not meet the initial conditions established before the experiment as minimally required to test its hypotheses. Four S's were suspicious--3 of these in the inequity and 1 in the equity condition. They did not believe what they were told and showed in post-session interviews that they in fact acted on their suspicions early in the experiment. Four did not understand the instructions--3 in the inequity and 1 in the equity condition. They either did not understand how to rent more channels or other significant features of the instructions. Finally, four of the remaining 44 S's made individualistic rather collective change-responses. They tried to directly negotiate with the center, or withhold information from the others, or send false information in order to make a change in the allocation of the bonus. For other purposes this would be an important behavior to analyze, but 3 of the four were in the inequity condition and hence do not bear on the hypothesis with which we are concerned in the present experiment.[5]

All four were therefore excluded from the analysis of the results. All these
decisions had been pre-planned before the experiment was run, were common to
all experiments in the same program, hence were made during the course of
running the experiment before any hypotheses were tested or results examined. E
continued to run S's until 20 good S's were obtained for each condition. The
experiment was run at the same time as another 3-condition experiment on potential
power, and in fact the inequity condition was also the baseline (or control)
condition for the second of these experiments. S's were randomly allocated to
one of the four conditions of these two experiments by E, and the two experiments
shared the same staff—which, because one E was required to interview each S and
often 4 S's were run at one time, was often quite large.

In the baseline condition of the experiment, 95% of the S's made a C-response
at some time during the experiment (see table 1). From the perspective of the

TABLE 1 ABOUT HERE

present experiment, in which the real interest is in the equity condition, what
this baseline measures is the magnitude of the pressure created by inequalities
in the allocation of the bonus to change the structure of the communication
network. Other things being equal, almost half the S's in the baseline condition
have tried to get another member of the group to endorse a proposal to change the
structure of the communication network by the end of the first trial of the
experiment. By the end of the second trial, 70% have made such a C-response. By
the tenth trial of the experiment, only one S had not made a C-response.

In the equity condition, on the other hand, only 60% of S's had made a
C-response by the tenth trial of the experiment.

One way to measure the magnitude of the effect of making an inequality
legitimate under the particular conditions created by this experiment is to
compare the per cent of S's surviving at the tenth trial of the experiment. By

this method, something like a third of the pressure to change the communication

network in the baseline condition was reduced by telling S's that the center was

especially able.  The observed per cent of C-responses, however, is a seriously

misleading statistic because it does not distinguish faster from slower rates of

change.  Two curves can have the same value at the tenth trial yet differ substan-

tially earlier in the experiment.  Nor will the mean or median trial of the

distribution of C-responses adequately represent the differences in the experience

of the two conditions.  Quite different curves can have the same mean and

even the same median.  The method recommended by medical researchers, who have

the most experience with such curves, is therefore to display and compare the

entire trial x trial experience of the S's.  Figure 1 shows the distribution of

such responses in the form of a survival curve, i.e. the per cent surviving at

the end of each trial of the experiment (which is simply one minus the cumulative

percentage of S's making a C-response at each trial, based on table 1).

<div align="center">FIGURE 1 ABOUT HERE</div>

A statistic that more precisely reflects the differences in shape of

these two survival curves is based on the ratio of the relative rates of change

in the equity treatment compared to its baseline condition, a statistic used to

evaluate clinical trials in medical experiments. The essential idea in analyzing

survival curves is that observed change at each trial is compared to expected

change at each trial, which in turn depends only on the number of S's exposed to

risk at the beginning of each trial.  If $\underline{m}$ S's make a C-response at trial $\underline{t}$ and a

proportion $\underline{p}$ of all S's were in condition $\underline{i}$ when trial $\underline{t}$ began, the expected

number of C-responses in condition $\underline{i}$ is $\underline{p_i m}$ (assuming there is no true diff-

erence between conditions).  The quantity $\underline{O_i/E_i}$, the ratio of the observed to

the expected number of change-responses, gives the relative rate of change in the

$\underline{i}$th condition, i.e, the rate of change in the $\underline{i}$th condition compared to that in

the population as a whole.  The quantity $\underline{CR}^6 = (\underline{O}_i/\underline{E}_i)/(\underline{O}_j/\underline{E}_j)$ gives

the ratio of the relative rate of change in the $\underline{i}$th condition to that in the

$\underline{j}$th condition—reflecting the shape of the two curves because the expected

values are computed trial by trial and are based on the numbers surviving

up to the time each trial begins.  The quantity $\underline{SR}^7 = 1 - \underline{CR}$ provides essen-

tially the same information, but has a more natural interpretation in the present

case as the "suppression" rate, i.e., the rate at which change in the baseline is

delayed or prevented by the equity treatment, and can be simply read "the percent

of change in the baseline condition that is delayed or prevented by the treatment

in the experimental condition."  By this measure, the justification of inequity

by E delayed or prevented 66% of the change found in the baseline condition

(see Table 2).

The statistic $(\underline{O} - \underline{E})^2/\underline{E}$, furthermore, is distributed as chi square

with (in this case) 1 df, which is the basis for the significance levels in table

2.  A suppression ratio of 66% would have occurred by chance less than once in

200 experiments.  (For a comprehensive survey of methods of analyzing survival

curves, see Elandt-Johnson and Johnson, 1980.  An especially clear and nontechnical

treatment, based on the nonparametric "logrank" statistic which is used here, can

be found in Peto, et al., 1977.)

<center>TABLE 2 ABOUT HERE</center>

(2)  Some Post-Session Questionnaire Results.  Whatever it was that produced

this effect, it was not our successful manipulation of equity.  Table 3 shows the

responses to five post-session questionnaire items that were used to check the

manipulation.  Four of the five items do not differ significantly by condition:

Orange was thought about equally able in both conditions, both thought team

earnings should be equally divided, both thought the bonus too high, and both

thought E should change the communication network in future experiments.  The

only item differentiating the two conditions is the third, appropriateness of the bonus. But even this item differs only marginally between conditions, the significant difference depending on just four S's in the equity condition who thought the bonus appropriate (i.e. who said either that the bonus was very appropriate or appropriate). In the post-session interview, S's explained their responses by (often spontaneously) offering that, although orange was very able the task was so simple that ability was irrelevant. Forty per cent of the S's said this before they were directly asked; 75% felt this to be true when asked directly. It is clear from these results that we did not establish the propriety of the experiment's rewards.

<div align="center">TABLE 3 ABOUT HERE</div>

## VI. Discussion

Although our manipulation of equity did not legitimate the experiment's reward system, it did legitimate the behavior of "orange" in the experiment, and hence the centrality of the communication network. S's in the equity condition were more likely than S's in the inequity condition to say that orange was "cooperative" ($\eta$ = .31; t = 1.997; P <.05 (one-tailed). In both conditions, orange took charge of coordinating the team's performance and therefore dominated interaction. In the equity condition this was made legitimate by orange's experience and ability. In a sense, E changed the meaning of orange's behavior by establishing these facts and backing them with E's authority.

For this and perhaps other reasons, S's in the equity condition were more likely also to approve the centralized structure of the communication network. Approval of the network was significantly correlated with how cooperative S felt orange was (Pearson's r = .36; F = 5.66; P <.05) and significantly differentiated the two conditions ($\eta$ = .30; t = 1.95; P <.05 (one-tailed)). This

may explain the differences in the survival curves of the two conditions. In the inequity condition, inequalities in the opportunity to win the bonus were clearly seen as causally linked to the inequity of the reward system and the illegitimacy of the bonus spread to the structure which S's changed in order to restore equity. In the equity condition, some S's at least evidently separated their disapproval of the bonus from their disapproval of the communication network. Separating the two parts of the inequitable structure meant essentially that S's faced a legitimation conflict. Part of the system (taken as a whole) was legitimate and part was illegitimate. If in fact they felt such a conflict, we might expect them to be in three states at any given stage of the process: they could have resolved the conflict in favor of the communication network, resolved it in favor of a change, or they could have not yet resolved the conflict. We might assume that the observable behavior of the S's correlates with these three states: If S has resolved the conflict in favor of change S should make a C-response, and otherwise not.

The shape of the survival curve in the equity condition suggests that if there is such a conflict it is resolved fairly early. One way to describe this curve is to divide it at the point of inflection, which is at the fifth trial, into two populations. One of the two populations makes a C-response quite early in the experiment (or at least within its first half). The other never makes a C-response. Both the cooperativeness of orange and approval of the communication network are correlated with these responses. Believing orange is very cooperative or cooperative delays or prevents 56% of the change taking place among all other subjects ($\chi^2$ = 5.52; P <.05). Approving the communication network (i.e. the 1 and 2 responses) delays or prevents 71% of the changing taking place among all other subjects ($\chi^2$ = 6.26; P <.05). (Both these computations are based on all 40 S's.)

There is, however, one obvious difficulty with this explanation of the differences between the two survival curves. The post-session questionnaire data is obtained after the behavior it is supposed to describe. The entire interpretation could well be post hoc. It is based on assuming that if approval and C-responses are correlated, the causal order is initiated by approval. In the case of the equity condition (but not in the inequity condition), it is possible to check this assumption because S's were also asked their opinions of the communication network after a practice trial but before the bonus manipulation. Measures of approval taken before the critical trials do not correlate with approval measured in the post-session questionnaire (Pearson's $r = .24$; $F = 0.94$; n.s.) and delay or prevent only 11% of the change among those who do not approve the communication network ($\chi^2 = 0.03$; n.s.). This led us to doubt that legitimation conflict explains our results. A later experiment by Thomas, et al (1981) directly tested the hypothesis that post-session approval of the communication network is after the fact of the behavior it justifies by measuring half of the S's before and half of the S's after the bonus manipulation and correlating the two measures with the post-session responses. Where change is prevented, S's initially disapprove the communication network (quite strongly) but by the end of ten trials they approve about as strongly as they had approved before the bonus manipulation. Presumably they are justifying to themselves the behavior they exhibited in the experiment.

A plausible alternative hypothesis is that E somehow signalled his or her hypothesis to S who, desiring to be cooperative, confirmed it for E. The search for such a signal should be simplified by the fact that the only sources of variation in the experiment are the equity manipulation and the short questionnaire given after the practice trial. Everything else was standardized, videotaped, and common to both conditions. Even though the manipulation of the

independent variable did not produce its intended effect, it must still (with the short questionnaire that is associated with it) be the cause of the unintended experimenter effect.

However, it is well-known that the cues given off by E are often so subtle that they cannot be detected by an outside observer even though they can be detected by S. S is an active, hypothesis-forming animal, using the totality of cues in the experimental setting in forming a hypothesis about what is going on, and is not content to use only E's instructions. These cues range from rumors about the experiment before S enters the laboratory to the social attributes of E (race, sex, age, ...), E's personal characteristics (warmth, excitement, nervousness, ...), paralinguistic cues (pitch, emphasis, tone, ...), kinesic cues (posture, expression, ...), and the procedures of the experiment (tests, intervals between tests, strength of the treatments, ...). (Cf Orne, 1962, Adair, 1973; Rosenthal, 1966; Rosenberg, 1965). Hence, one cannot assume from a study of the questionnaire or the instructions of the experiment that there were no experimenter effects. To try to discover whether our result was an experimenter effect, therefore, we must resort to some other method. Although we did not fully carry out the post-experimental inquiry recommended by Orne (1962), we were able to infer something from our post-session interviews about (a) what S's motives in the experiment were and (b) what S thought E's hypothesis was.

The literature on the social psychology of the experiment suggests two common motives: the desire to cooperate, to help E (Orne, 1962; Rosenthal, 1966) and the desire to look good, i.e. to look intelligent, honest, and autonomous to E (Rosenberg, 1965; see also Schulman, 1967). If S is cooperative, S should be more likely to confirm E's hypothesis if S knows what the hypothesis is, hence one method of detecting an experimenter effect is to correlate post-session statements about E's hypothesis with behavior in the experiment. If S is

apprehensive about E's evaluation—for example, if the effect of the short
questionnaire in the equity condition was to differentially signal S that E was
"testing" S—then his conduct in the experiment depends a great deal on the
nature of E's hypothesis, but in general it has been found that S will try to
look honest, intelligent, and autonomous even if this disconfirms E's hypothesis.
(Cf the discussion in Adair, 1973, ch 3)

The evidence of the post-session interviews suggests that S's were more
apprehensive about E's evaluations than cooperative. Asked in post-session
interviews how E's mention of orange's ability affected them when they answered
the post-session questionnaire item about the appropriateness of the bonus, 70%
of the S's in the equity conditioned denied that orange's ability was relevant to
them. Asked how E's mention of orange's ability affected their decision to rent
or not to rent more channels of communication, 70% denied they had taken orange's
ability into account. Either they did not suspect what E's hypothesis was or
they were not cooperative at this stage of the experiment. There was no diffe-
rence between those who did and those who did not deny the relevance of orange's
ability in actual behavior during the experiment $(\chi^2 = 1.92$; n.s.$)$.

These responses, of course, are consistent with the idea that S's wanted to
look autonomous and independent in E's eyes, and the fact that the short question-
naire given after the practice trial was given in the equity but not the inequity
condition is consistent with the hypothesis that it raised apprehensions about
evaluation differentially in the two conditions. The difficulty with this
hypothesis is that if the short questionnaire had had this effect during the
experiment it would have increased, not decreased the rate of C-responses in the
equity condition. In any case, there was no correlation between the strength of
S's expressed autonomy during the post-session interview and his behavior in the
experiment. Every S in the interview (except for two for whom the tapes were

blank) either denied the relevance of E's hypothesis when directly asked or spontaneously offered that orange's ability was irrelevant because the task was so simple. But some did both while others did only one of the two. If we assume that doing both is stronger than doing only one or the other, a logrank analysis of the C-responses of the two groups shows no difference between them ($\chi^2$ = 0.30, n.s.). On the whole, we do not think the result of our experiment was due to S's efforts to confirm E's hypothesis.

But S's obviously did detect something in E's behavior that differentiated their own behavior in the experiment. The only question, really, is what? One alternative to experimenter bias is that we obtained something more like a control effect produced by the demand characteristics of the experiment. In the literature on demand characteristics, it is important to distinguish the effects of performance cues, which signal E's hypothesis, from role definitions, which define for S the proper role of a subject in an experiment and the relation the subject is expected to have to others in the experiment, in particular to E. The more striking instances of a demand characteristic have to do not with experimenter effects in the sense of performance cues but rather with the very high degree of control an E is capable of exerting in an experimental setting. Orne, trying to find a task that S's would refuse to do because it was either painful or meaningless, was never able to find one. S's worked more than five hours on such meaningless tasks as adding columns of figures from a pile of 2000 sheets, even when instructed to tear each sheet up after the additions had been completed. Much earlier, Frank had already shown that S's would perform such equally meaningless tasks as balancing a marble on a steel ball or eating a dozen unsalted soda crackers for no discernible reason. In these cases, what E is doing has nothing to do with cueing E's hypothesis. What is happening is simply that E's authority, accepted by S as legitimate, leads S to do anything S is

asked to do, however pointless it appears to S. Orne uses the expression
"demand characteristics" to refer to both performance cues and role defini-
tions (1962). But the bulk of the literature on the subject is about performance
cues only (Rosenthal, 1966; Rosenberg, 1965). In the present experiment there is
little evidence of bias due to performance cues. But it is nevertheless possible
that we have found a control result instead.

S's enter the experimental situation usually with cooperative motives
and the desire to look good to E (although occasionally negativistic motives are
found—see Adair, 1973 ch 3, for a discussion of these). Even where, as in the
present experiment, we do not find S's "cooperating" in the sense of trying to
confirm E's hypothesis, they still almost invariably want to be a "good S" in the
larger sense of playing the role as it is supposed to be played. Demand charac-
teristics, i.e. the totality of cues the experimental situation presents to S,
create a framework of social knowledge and interpretation that provide S with
some ideas of how they are expected to act in an experiment and how others will
act in an experiment (e.g. E, E's assistants, other S's). But S actually comes
to the experiment with some rules already formed, particularly rules defining the
authority of E, that are understood by S before any demand characteristics begin
to function. On the basis of these preexisting conceptions of the role of an S,
together with demand characteristics in the experimental setting, more concrete
and specific rules emerge in the particular experiment. These emergent rules are
different in character from E's hypothesis. In a sense, they are not about
what the experiment is about, its purpose, E's cognitive assumptions, etc.
They are about what the proper conduct of an S is about. But, like performance
cues, it is to E that S looks to provide cues about the norms governing what an S
is to do in what is to S a novel situation. E is the only source available for
such information. To the extent that S cares to be "a good S", it is E who molds

S's conception of the S-role in the experiment. Any act is understood by both E and S to be proper if S does the task E requires in the way E requires him/her to do it.

The pre-existing rules that define the structure of the E-S relation are based on the beliefs S's who volunteer for experiments have about science. Despite the experience of the sixties, all the S's in this experiment believed the goals of science to be legitimate. They believed, furthermore, that E believed in these goals. What S does in an experiment is justified by the joint commitment of E and S to these goals and by the relevance of what is done in the experiment to them. These goals are taken for granted by E and S in the same way that both take for granted E's superior, expert knowledge, the goodness of his intentions, and the inferior, naive state of S's own knowledge of whatever it is E is doing. It is out of this relation that the emergent rules of the situation arise. But what E is creating, in this instance, is not knowledge of E's hypothesis, but rather the validity of the social structure created by the experiment.

This analysis, incidentally, is not as post hoc as it sounds. It was, in fact, made before the experiment and was the basis for designing and carrying out several other experiments on the effects of legitimacy on collective action (see particularly Thomas, et al, 1980). Throughout, demand characteristics were used to create miniaturized legitimation processes in the laboratory. But the question nevertheless arises whether this analysis has any real application in the present experiment.

Our interpretation of what happened in this experiment is that E created a valid framework of interpretation (in the Dornbusch-Scott Sense) for what S could expect in the experiment, S used this framework to interpret what went on, and the validity of this framework operated to legitimate some kinds of actions and deligitimate other kinds of actions whether or not S personally felt them to

be proper. Any particular act is legitimate if and only if it is in accord

with rules that are in some sense accepted or acceptable. The legitimacy

of acts, of course, is something distinct from the legitimacy of the rules

themselves (cf Rawls, 1955). In any concrete case, saying that an act is or is

not legitimate in fact makes a judgment about both acts and rules. The rules

themselves are legitimate either if they follow from other accepted rules or they

are deducible from a combination of values and beliefs that are accepted or

acceptable. The vagueness with which we say "accepted or acceptable" is intention-

al: If values, rules, and beliefs are accepted by a particular individual, say

p , i.e. if they are just, moral, and right in that particular individual's own

view, we assume that there is no pressure from p to change them. However, if

they are improper, if p does not accept them as right, and the situation in

which p is located is one that requires concerted, collective action, while p

may personally feel some pressure to change the rules, whether p does or does

not in fact act on this feeling depends on the validity of the rules, not their

propriety. If the rules p feels should be changed are valid, and/or if the

rules p believes are proper are invalid, collective action will collapse

even if the strength of the prospective change-oriented coalition is sufficient

to accomplish the change p desires (cf Michener and Lyons, 1972).

In the present experiment, S believes that E believes that the system

of rewards is equitable in the equity condition but not in the inequitable

condition. This is communicated quite clearly in the instructions. Even

if the equity manipulation does not succeed in persuading S of the propriety of

the system, it nevertheless tells S that E believes in it. And E is the chief

source of the emergent values, beliefs, rules, practices and procedures that

define S's conduct in experiments. We may therefore say that the reward system

is valid in the equity condition and invalid in the inequity condition. We

assume that any act of S's is legitimate (to S) if and only if it accords with valid values, beliefs, rules, practices and procedures in the situation of S's action, i.e. the experimental situation. But what we are concerned with in the present experiment is collective action. Our reasoning may not hold for discretionary actions of individuals (for example, reallocating rewards when in a position to do so), but to the extent that what S is required to do requires joint action with others, we assume that legitimacy means that the acts accord with valid values, beliefs, rules, practices and procedures. Put another way, they do not depend on propriety. Propriety by itself cannot create validity, which is an aggregate, a collective property. Personal acceptance by p is in an obvious sense different from acceptance by a group. It is in this sense that we want to argue that validity is a collective, not an individual-level process.

Can we say more? We have so far asserted that if S believes that E believes that the reward system is equitable in the equity condition, then the reward system is valid, even if not proper. In the inequity condition it is neither valid nor proper. The experiment, given these terms of reference, shows that C-responses depend on validity, independent of propriety. But how exactly does validity produce this effect? Here we admit to being on very tenuous ground. Our conclusion will depend on the reader agreeing with the following starting point: We can think of four ways in which validity might produce compliance. These are (1) validity may directly increase voluntary compliance by S because it provides the only available publicly acceptable account for action in the experiment. (Scott and Lyman, 1968), (2) Validity may indirectly increase voluntary compliance by increasing the likelihood that S feels a rule to be proper. If propriety is consistent with validity, compliance should be increasingly likely and increasingly likely to be voluntary. (3) Validity may indirectly increase involuntary compliance by increasing S's anticipation that other S's will endorse

the reward system and, hence, will not join in collective action to change it. Efforts to change the system will therefore appear futile. (4) Finally, validity may also indirectly increase involuntary compliance by increasing S's anticipation that E, because E is authorizing the system, will back it by sanctions.[8]

Only the first of these seems to us plausible in the case of the present experiment. Any effect through propriety is ruled out by the evidence of the checks on the manipulation. These show that few S's believed in the propriety of the reward system. Endorsement is a very plausible hypothesis, and Michener and Lyons (1972) showed that one of the chief effects of their equity manipulation was on the expectations of S that others would support revolutionary coalitions. In the present experiment, S's expectation that others approve or do not approve the bonus is very highly correlated with their own approval (Pearson's $r$ = .79; $F$ = 60.37; $P <.01$), making it difficult to distinguish the effects of the two. Employing only other's approval, however, we find no significant effect on the distribution of C-responses (Logrank $\chi^2$ = .75; n.s.). Thus, the externality at work in the experiment is located in E, not other S's. (This, of course, is due largely to the restricted communication network.)

But the post-session interview analysis shows that it is not really E's sanctions that, anticipated by S, determine S's responses. Or, to the extent that S is apprehensive about evaluations by E what this does is make S want to appear more autonomous. While we concede that the argument is highly inferential, if we must choose one of the four ways in which validity operated to produce a nondecision in the present experiment we conclude that collective action collapsed for lack of a publicly acceptable account of what S would be doing if S tried to initiate it.

FOOTNOTES

1. "Equity theory" refers here to the theories of Adams (1963, 1965), Blau (1964), Homans (1974), and Walster, Walster and Berscheid (1978), i.e. to exchange theories of distributive justice.

2. See particularly Hoffman, et al, 1954; Lawler, 1975; Messe, et al 1975; Michener and Lawler, 1971; Michener and Lyons, 1972; and Webster and Smith, 1978.

3. It may seem far-fetched to support the idea that legitimacy is important in natural revolutionary coalitions by appeal to a literature in which, although the importance of legitimacy may be intuitively obvious, it is not treated as an analytically separable factor in the process of mobilizing movement organizations. Tilly (1978), Obershall (1973), and McCarthy and Zald (1977) either do not treat legitimacy as theoretically significant at all or treat it merely as one kind of resource. Nevertheless, the recent literature on social movements, which has tended to be dominated by resource mobilization theory, amply justifies giving a central place in the analysis to legitimacy (of which equity is one kind). In of this work, and particularly Tilly (1969; 1970; 1975; 1978) and Gamson (1975), it is the legitimacy of actors and tactics that distinguishes outsiders' politics from insiders' politics, hence "social movements" from "interest groups" and violent from peaceful politics. The literature on natural revolutionary coalitions does suggest the importance of more kinds of legitimation than the equity literature does: it turns on the legitimation of actors, tactics, and the jurisdictions claimed by groups, as well as issues (where equity is more directly relevant). But legitimacy is repeatedly at issue in this literature, and essentially it is what the political struggle described by Tilly is about. Tangential to this literature, but also significant for our purposes, is the evidence, from the literature on revolutionary coalitions in natural situations, that an important part of the dynamic of "retaliation" against revolutionary coalitions depends on the perceived legitimacy or illegitimacy of "protest" (Turner, 1969, Parker & Lauderdale, 1980).

4. All positions in the communication network were color-coded. The central position was coded "orange" in all groups. The word "orange" appeared at this point in the instructions.

5. These individualistic responses are included, however, in Zelditch, et al, 1981, which is concerned with both individual and collective responses.

6. C.R. stands for "change ratio."

7. S.R. stands for "suppression ratio."

8. We omit the direct effect of E's authority because E does not directly command S's to change the structure in the inequity condition nor/to change it in the equity condition of this experiment.

Table 1.   Percent of subjects making a change-response by trial and condition

|  | | | | | | Trial Number | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Condition | N | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 | Total |
| Inequity | 20 | 45 | 25 | 10 | 10 | 0 | 0 | 0 | 5 | 0 | 0 | 95% |
| Equity | 20 | 5 | 5 | 30 | 15 | 0 | 5 | 0 | 0 | 0 | 0 | 60% |

Table 2. Logrank statistic of the relative rate of change in the equity condi-
tion compared to the inequity consition

| Condition | N | Observed No. of change-Responses (O) | Expected No. of change-Responses (E) | Relative Rate of change (O/E) * |
|---|---|---|---|---|
| Inequity | 20 | 19 | 10.83 | 1.75 |
| Equity | 20 | 12 | 20.17 | 0.60 |
| All | 40 | 31 | 31.00 | 1.00 |

*The ratio of the relative rates of change, 34%, indicates that the equity
manipulation delayed or prevented 66% of the change taking place in the
inequity condition. ($\chi^2$ = 9.47; p < .005).

Table 3 Post session questionnaire responses used to check the Manipulation of equity. ($N_i$ in parentheses)

|  | Item | Mean | | t | P* |
|---|---|---|---|---|---|
|  |  | Equity | Inequity |  |  |
| (1) | How would you rate the ability of Orange at solving problems? (1 = very high) | 1.35 (20) | 1.47 (17) | 0.49 | n.s. |
| (2) | How appropriate do you believe it is to divide team earnings equally? (1 = very appropriate) | 1.35 (20) | 1.25 (20) | −0.38 | n.s. |
| (3) | How appropriate do you believe it is to award a bonus to the first team member who submits the correct answer? (1 = very appropriate) | 4.25 (20) | 4.84 (19) | 1.86 | .04 |
| (4) | In general, how do you feel about the amount of the bonuses? (1 = much too low) | 3.50 (20) | 3.79 (19) | −0.83 | n.s. |
| (5) | How desirable do you think it is to change the communication network for future studies? (1 = very desirable) | 2.00 (20) | 1.95 (20) | −0.12 | n.s. |

*One-tailed probabilities (that Orange is more able in the equity condition, that equal division is less appropriate, that the bonus is more appropriate, the amount is not too high, and it is less desirable to change the communication network for future studies).
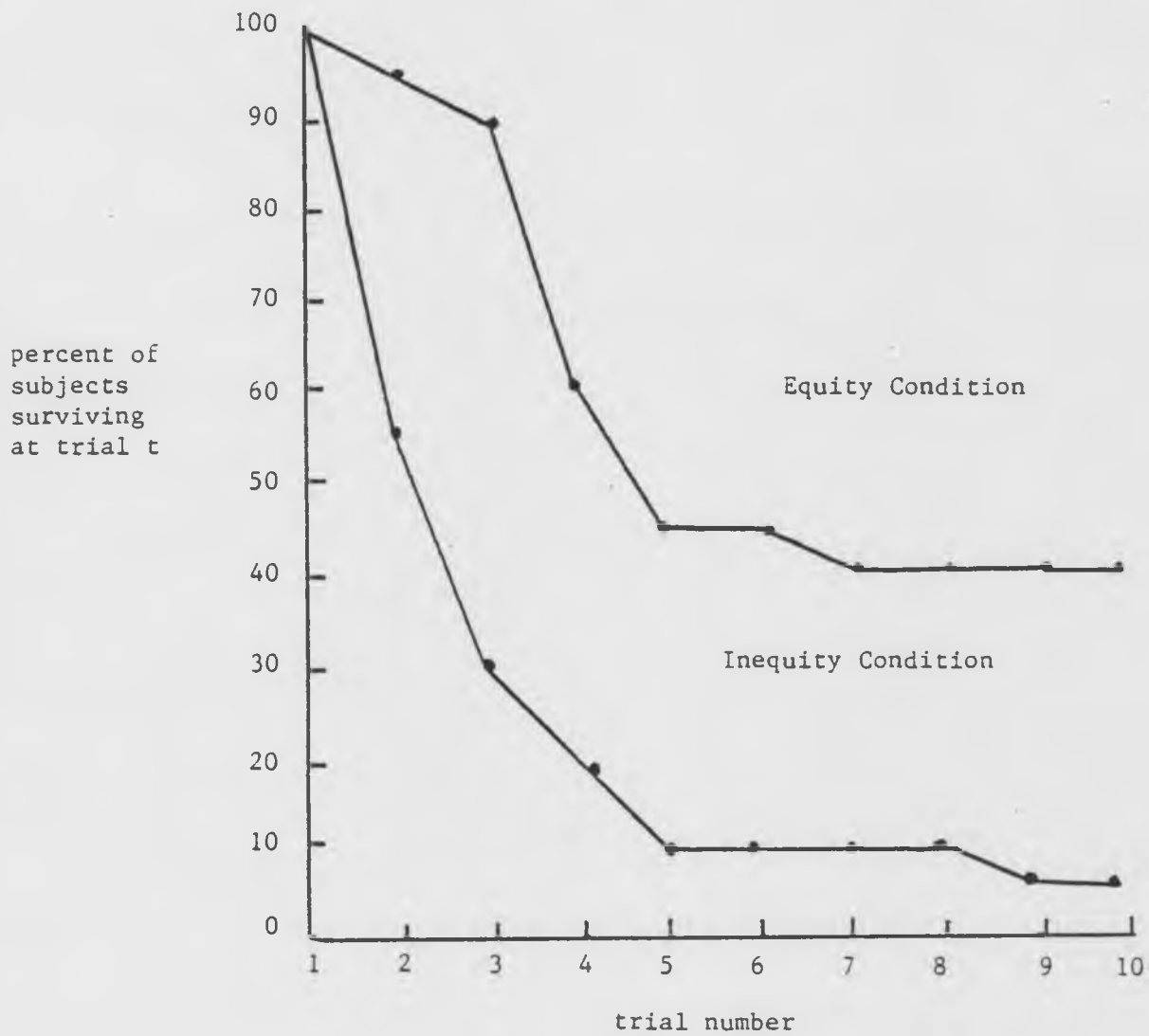
Figure 1.  Percent of Subjects surviving at trial t by condition

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