A TEST OF THE LAW OF ANTICIPATED REACTIONS*

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ABSTRACT

The "law of anticipated reactions" was tested by experimentally manipulating beliefs about a more powerful individual's "reactions" to attempts to change a centralized communications network that, because of the way a bonus was allocated, was the cause of a gross inequity in the allocation of rewards. S's were informed that past experience showed that individuals in the center of such networks preferred them to any alternative network and "almost always," "about half the time" or "almost never" used a power E had given the center to completely control allocation of rewards to penalize attempts to change the network. This manipulation was imperfect, S's in the "almost always" condition reporting on post-session questionnaires that they actually expected penalties for attempted change only "about half the time." Nevertheless, given a strong incentive to change and a legitimate way to do so, change attempts were inversely proportional to the perceived probability of a penalty even though an experimental confederate at the center never overtly expressed any preferences, demanded any compliance, promised any rewards, or threatened any penalties.

I. Problem.

People often comply with power even when it is not overtly exercised: They comply with what they anticipate will be the reactions of more powerful others without any overt demands, promises of reward for compliance or threats of penalty for noncompliance. This is usually referred to as the "law of anticipated reactions" (LAR). The hypothesis goes back at least to Friedrich (1937) and plays an important role in several contemporary theories of power, including Bachrach and Baratz (1962; 1963; 1970), Nagel (1968; 1975), Pollard and Mitchell (1972), and Wrong (1968; 1979).

The significance of the LAR lies in the importance it gives to the less visible aspects of power. Most of the time sentries do not fire their weapons, nor even warn others off. Nevertheless, most people most of the time obey them. If this much is accepted, then the kind of theory required to explain such behavior must take subjective states of mind into account. Covert compliance with sentries is difficult to understand without taking into account what everyone knows sentries are there to do. It motivates some theorists, like Bachrach and Baratz, to construct entire theories around invisible power (1962, 1963, 1970) and others, like Nagel (1968; 1975), at least to formulate subjective expected utility (SEU), rather than other kinds of theories of power, because SEU theories more readily imply it. (See especially Pollard and Mitchell, 1972 and Tedeschi, et al, 1973.)

The LAR is so intuitively compelling a hypothesis that, so far as we have been able to discover, no one has ever bothered to test it. The purpose of the present paper is therefore to experimentally test the hypothesis. Planning, like others, to organize a theory of power around it, we want to establish whether we are justified in doing so before undertaking the task.

II. Hypothesis.

Of various possible theories of power, it is the SEU theory that most naturally implies the LAR. In turn, it is the SEU theory that most clearly explicates the component elements of the hypothesis, which is useful in designing methods of testing it. We therefore develop the hypothesis here in SEU terms.

We will think about power in terms of one actor A who directs a second actor B to do X. A promises a reward r for compliance and/or threatens a penalty t for noncompliance. B's choice between these alternatives is determined by the subjective value of their outcomes (referred to as their *utility*), B always choosing the alternative that leads to his or her preferred outcome. However, a choice may have more than one outcome. The decision process is therefore probabilistic rather than deterministic. (For example, A may promise a reward but not always deliver.) The utility determining a choice is therefore weighted by the probabilities of its two or more outcomes, and choice is determined by *expected* utility, i.e. the sum of the probability-weighted utilities of the outcomes. But the probabilities employed by B in making such judgments are not necessarily the objective probabilities, they are B's subjective estimate of them. Hence, B's choice is determined by the *subjective* expected utilities of the alternative outcomes.

In theories of power it is usually assumed that, other things being equal, compliance is less valued by B than noncompliance, otherwise it would not require power to cause compliance. Hence, what causes compliance by B is the SEU of rewards for compliance and penalties for noncompliance. Compliance, in fact, occurs at the point at which the sum of the SEU's for rewards for compliance plus penalties for noncompliance exceeds the difference in SEU between unsanctioned noncompliance and compliance.

Note that the concept of "power" employed in this theory is very narrow, corresponding most closely to Festinger's definition of it as public without private compliance (Festinger, 1953). It is "unwilling" compliance caused by extrinsic rewards and penalties, without corresponding internal changes in the actor. It thus differs a good deal from a number of other possible meanings of power. It is unlike Parsons' (1963) or Hawley's (1963) definition of power in focussing only on interpersonal power, on "power over" rather than "power to." It is unlike Dahl's (1957) or Raven and French's (1959) definitions of power in focussing only on reward and and punishment power where Dahl, Raven, and French cover all the ways that A causes a change in B's behavior by the term, including influence, authority, and manipulation as well as rewards and penalties. On the other hand, it is somewhat broader than Blau's focus only on coercive power (1964) or Goode's emphasis on "force threat" (1972). We are not rejecting the Parsons and Hawley definition, which is useful for studies of collective action; we merely assume that interpersonal power is different. On the other hand, we do reject omnibus uses of the term like Dahl's or Raven and French's, which by encompassing so much ends by being useless (as March has shown, 1966). Our use is close to that of Blau's and Goode's, but formal investigation of the meaning of reward and penalty (cf Zelditch and Ford, 1984) convinces us that the difference between reward and penalty, which sometimes depends on the difference between giving and withholding, is difficult to maintain in practice. Our concept is even closer to Emerson's "power-dependence" relation (Emerson, 1962) except for the fact that Emerson describes the structure of relations, while our concept of power is concerned with specific acts of compliance or noncompliance (cf Samuel and Zelditch, 1986).

The variable that distinguishes the LAR from the more general concepts of SEU theory is B's knowledge of past behavior by A, on which B may base anticipations of A's future behavior. This knowledge may derive directly from past experience, but it might also arise more indirectly from (1) observations of others' experience with A, (2) reports by others of their past experience with A, (3) past experience by B with other people in roles similar to that of A, or even (4) general cultural traditions about relations like that of B to A. If by any of these means, B believes s/he is able to predict how A will react to X or not-X, then

(1) <u>The LAR.</u> Given B's knowledge of A's preferences and a given probability that A will reward compliance with them by an amount r and/or

penalize noncompliance with them by an amount t , compliance by B with A's preferences is a monotonically increasing function of the difference in SEU between compliance (plus anticipated rewards) and noncompliance (plus anticipated penalties), independently of any overtly expressed demand for compliance, promise of reward, or threat of penalty by A.

A corollary of the LAR is that A's power will not be visible to an outside observer, because it will not appear to be the consequence of any causal action by A. 1

It should be noted that the LAR, formulated in SEU terms, has three component elements: The amount of power of A over B; A's preferences ; and the subjective probability of A's rewards and penalties. The distinction between A's preferences and the probabilities of various reactions by A, often not made in discussing the LAR, has more far-reaching consequences than might appear at first sight. It is easy to conceive of situations in which B knows the preferences of A but not the probability that A will use his/her power to reward B for compliance or penalize B for noncompliance. Under such conditions, B may comply at a higher rate than one would predict from the actual probability of sanctions because compliance is the "safer" of the two alternatives. That is, under such conditions B may act in terms of what A _could_ do, failing knowledge of what A _would_ do. 2

This same effect may occur even if A's preferences are not known. Without a knowledge of A's preferences there is of course no way that B can know what constitutes compliance or noncompliance. Nevertheless, there is a third option, inaction, which may be the "safe" response until A's preferences are revealed. Inaction is the most general case of what Bachrach and Baratz (1962, 1963, 1970) have called a "nondecision," their term for the suppression of attempts to change a power-prestige order.

The LAR has been used by Bachrach and Baratz as one of the principal tools in explaining nondecisions: In their view, nondecisions are (sometimes) caused by anticipation that attempts to change a power-prestige order will be negatively sanctioned. In such contexts, the LAR plays an important role in collective action, suppressing potential protest.

The LAR is already of interest because it reveals some of the less visible aspects of power, but nondecisions are even less visible than a kind of compliance that requires that B do something. Furthermore, in the case of nondecisions, there are additional factors that are likely to make power-prestige orders persist, adding to the interest in nondecisions. When compliance requires that B do something that A observes, misperceptions are corrigible because of unexpected reactions by A. But inaction is less likely to be a stimulus to any corrective reactions by A, Therefore, misperceptions are likely to persist, and with them the status quo.

Because we are especially interested in the less visible aspects of power, it is in a nondecisionmaking setting that we propose to test the LAR. In the present paper we focus on the case in which B knows both A's preferences and the probability of A's reactions, holding both his/her power and preferences constant. But we study, as a dependent variable, whether B does or does not attempt to change the power-prestige structure of the experiment when given a strong incentive to do so and when change is made possible by legitimate means. In fact, to carry the theme of invisibility even further, we focus only on penalties for noncompliance. Not even A's sanctioning behavior will be visible if B avoids penalty by complying. We thus test one special case of the LAR only:

(2) <u>A Hypothesis Implied by the LAR.</u> Holding the power of A over B and B's perception of A's

preferences constant, compliant inaction by B is directly proportional to the perceived probability of penalty for noncompliance by A.

This hypothesis is given an operational form in the next section.

III. Method.

Our method of testing hypothesis (2) is experimental.

The subjects (S's) of the experiment, all of whom were Stanford male undergraduates between the ages of eighteen and twenty-one, were greeted individually at a predesignated spot by the host experimenter (E) or an assistant. They were then randomly assigned to individual cubicles by choosing a colored token from a container. The color of the token, red, yellow, blue or green, corresponded with the color designating the cubicle to which they were assigned. They were referred to by this color throughout the experiment. One "S," known as "Orange," was actually a confederate of E, so there was no orange token in the container. Each cubicle was equipped with a table and chair, video monitor, a workbook, an instruction book, pencils, and message slips.

In addition, there was a signalling device which was used to communicate with research assistants who acted as messengers between the cubicles and the control room. This was hooked up to a small control panel outside the control room for the convenience of the messengers. These devices, plus the video monitors, were hooked up to a master control panel in the control room which was monitored by E.

After S's were seated, they were given instructions about the nature of the task over the video monitor through the use of a prerecorded videotape. (Instructions were presented by videotape to standardize the presentation and avoid experimenter fatigue.) After the task was described, the S's performed a practice trial, after which there was a short rest period. The S's were then instructed to begin the criterion task trials.

The task required them at each trial to construct a graph consisting of five points connected by varying numbers of lines. Each S was given some of the information necessary to construct the answer graph but no S had all of it and each S was told that he must communicate with every other member of the group in order to complete the task. There was a different answer graph on each of ten criterion trials.

A trial ended when each of the five members of the group submitted his solution to E. Each correct solution was worth

60 cents on each trial. If all 5 solutions were correct, $3.00 would therefore be earned by the group on each trial. S's were told that because the task was a group task, the $3.00 would be awarded to the group as a group. It was part of the role of Orange to decide how to allocate the team earnings to members. (See below the description of the manipulation of Orange's power.)

All communications were written on message slips provided in the cubicles. When a message was completed, S used his signalling device to summon a messenger to transmit the communication. Although S was told that he was sending messages to others in his team, in fact all messages went either to E or the confederate who played the part of Orange.

The confederate occupied the central position in a centralized communication network called a "wheel," devised by Bavelas in 1950. In this structure there is one central position and four peripheral positions communicating directly only with the center. The four peripheral positions were occupied by the S's. S's were told to communicate only with those teammates to whom they were connected by an open channel, hence only with Orange. But they were also told that if they wished to open one or more of the closed channels of communication, they could rent additional channels at 10 cents per channel per problem. Because this fee was to be shared by all members, a change required that a majority of the group approve it. In order to make a change, therefore, an election was required. To hold such an election, any S could move a particular motion to change, specifying which channels to open, which then required a second by one other member. S's were led to believe that if their motion was seconded, ballots would be issued by E to all team members, who would then vote on the motion. If the majority voted for the motion, and the channels were opened, the rental fee was to be deducted from team earnings.

After a practice trial, S's were told that they had performed well but that in order to induce them to work faster a bonus of $3.00 would be awarded, on each problem, to the individual who first completed the correct solution. This was obviously inequitable because a wheel network permits Orange, because of his central position in the network, to complete the problem first, and therefore win the bonus, on every trial. Most S's realized this almost immediately. The inequity was further underlined by informing S's that they were all of the same sex, age, and level of education and reminding them that they had been allocated to their roles in the wheel purely by chance.

In previous experiments in the same experimental setting, S's typically have proposed a change in the communication structure, usually to an all-to-all network, by the third trial of the experiment (Zelditch, et al, 1983). A proposal to change the wheel, referred to as a "change-" or "C-" response, is the dependent variable of the experiment. Because each position communicates only with E or a confederate of E, each S is an independent observation. The experiment ends for each S if S proposes to change from the wheel to any other structure and continues for each S who does not propose a change. If S does not propose to change the wheel, the experiment ends after the tenth trial. The dependent variable of the experiment is therefore the trial at which S individually ends the experiment.

In all conditions of the experiment, S's were informed that Orange, the central position, had the right to allocate team earnings as he saw fit at the end of the experiment. Assuming that all solutions were correct, the amount at stake for each S was $6.00. No effort was made to measure the utility of the $6.00 for individual S's. We simply assumed that it varied randomly across conditions, equalizing them. Orange's control over team earnings was mentioned, though without any particular emphasis, three times in the course of the instructions and once more in the summary of the instructions. This part of the instructions created Orange's "power" over S.

In the course of the instructions, a third of the S's were told that past studies of centralized communication networks had shown that Orange almost always liked that structure and <u>almost always</u> used his control over team earnings to withhold them from team members who attempted to change the structure by renting more channels. Another third were told that past studies had shown that Orange always liked that structure and used his control over team earnings to withhold them from team members who attempted change <u>about half</u> the time. The remaining third of S's were told that past studies had shown that Orange always liked that structure but <u>never</u> had actualy used his control over team earnings to withhold them from team members who attempted to change it. When the bonus was later introduced (after the first practice trial) it was emphasized that the bonus could not be divided and S's were again reminded of Orange's control over team earnings and how Orange behaved when change was attempted, as part of differentiating team earnings from bonus earnings.

These instructions created S's expectations about the probability that Orange would penalize attempts to change the wheel. Thereafter, Orange at no time actually overtly exercised the powers given to him by E. He did not overtly express any preferences, did not overtly demand any compliance, did. not overtly promise any rewards for silence, did not threaten any penalties for attempting to change the communication network.

Given the instructions we have described, the operational form taken by the special case of the LAR given above at (2) is in terms of the probability that S makes a C-response. The hypothesis we actually test, therefore, is that:

> (3) <u>Operational Hypothesis of the Experiment.</u>
> The probability of a C-response is inversely proportional to the perceived probability of a penalty for attempting change without any overt expression of preferences, promises of reward for compliance, or threat of penalty for noncompliance by Orange.

This means that S's will be less likely to change, or make attempts to change later, in the "almost always" condition than in the "about half" condition, in which, in turn, they will be less likely to change, or make attempts to change later, than in the "almost never" condition.

IV. Results.

A. Validity of the Experimental Manipulations.

A proper test of the hypothesis that C-responses are inversely proportional to the perceived probability of negative sanctions requires that the pressure to change be the same in all three conditions while the perceived probability of a penalty for attempting a change should differentiate among them.

As measures of the pressure to change the wheel we used two post-session questionnaire items. The first asked A's how much they approved the wheel (on a 5-point scale ranging from 1="highly approve" to 5="highly disapprove"). The other asked how desirable it would be to change that network in future experiments (scaled similarly).

Neither item differed significantly by condition. For approval of the wheel network, the aggregate mean, across all three conditions, was 3.74. Using two-tailed probabilities, we broke the comparisons down pairwise, finding that between the "almost always" and "about half" conditions t=1.47, n.s.; between the "almost always" and "almost never" conditions t=.59, n.s.; and between the "about half" and "almost never" conditions t=1.01, n.s. The aggregate mean for the desire to change the network in the future was 2.35. Between the "almost always" and "about half" conditions, t=1.43, n.s.; between the "almost always" and "almost never" conditions, t=1.31, n.s.; and between the "about half" and "almost never" conditions, t=0.27, n.s.

We were less successful in differentiating S's perceptions of the probability that Orange would use his power. It was a mistake to use the word "almost" either with "always" or "never." The scale used in asking S's "Did you think that Orange would exercise his power to withhold team earnings?" ranged from 1=almost always to 3=almost never (with 4=don't know). The mean score of S's in the "almost always" condition was only 2.23, i.e. they thought Orange would use his power "about half" the time. The mean score of the "about half" condition was 2.65, the difference between the two conditions being only marginally significant. (Using a one-tailed test, t=1.33, p=.10.) The mean score of the "almost never" condition was 3.10, which differed significantly from both the "almost always" condition (t=3.24, p=.001) and the "about half" condition (t=2.08, p=.02).

We will see in the following section that the weakness of this experimental manipulation created difficulties for our test of the LAR, but that they nevertheless differentiated the C-responses of the S's sufficiently to draw a reasonable conclusion from the experiment.

B. <u>Effects of the Manipulations on the Probability of a Change-Response.</u>

The C-responses of the S's are best represented by a "survival curve" showing the cumulative proportion of S's surviving at the end of trial T of the experiment for each T. Survival curves are a better representation of the data than either the median trial at which S terminates the experiment or the number surviving at the tenth trial because rather different curves can have the same medians and the same number surviving at the end of the experiment. Because the data are ordinal in nature and also because we cannot assume normal distributions, we used the nonparametric logrank test (Peto and Peto, 1972; Peto, et al, 1977) to assess the significance of the differences between survival curves.

TABLE 1 ABOUT HERE.

Table 1 gives the cumulative per cent of S's surviving at the end of each trial. It can be seen from Figure 1, which is based on this table, that there is a difference between the "almost always" and "almost never" curve that begins early and lasts throughout the experiment; and that the "about half" curve is consistently in the middle between the two.

FIGURE 1 ABOUT HERE.

A useful byproduct of the logrank test is a measure not only of levels of significance but also of the magnitude of the effect of each condition. The test calculates the ratio of expected to observed values for each condition. The ratio of any two of these ratios gives the relative rate of change in one condition compared to a second. One minus this relative rate of change measures the extent to which one condition prevents or delays the amount of change taking place in the other. Thus, the notes at the foot of Table 2 show that believing that Orange would "almost always" penalize attempted change prevented or delayed 66% of the amount of change taking place in the "almost never" condition. Believing that Orange would penalize attempted change "about half the time" prevented or delayed about 42% of the change taking place in the "almost never" condition. And believing that Orange would penalize attempted change "almost always" prevents or delays 42% of the change taking place in the "about half" condition.

TABLE 2 ABOUT HERE.

The logrank test, however, shows that only the difference between the "almost always" and "almost never" conditions is statistically significant. (See Table 2.) The logrank test is the most powerful nonparametric test available for this kind of data. Its power derives from the fact that, because the proportion of survivors changes at each trial the test computes expected values at each trial. The cumulated difference between the expected and the observed values is distributed as chi square. Table 2 shows that the probability of the obtained chi squares for the differences between "about half" and either "almost always" or "almost never" are not significant.

However, chi square, and therefore the logrank test, is insensitive to order while our hypothesis specifies the order of the three survival curves. We therefore also tested the hypothesis that the three curves were in the right order by Jonckheere's test for ordered alternatives (Jonckheere, 1954). This more sensitive test shows that the order of the three curves would have been obtained by chance only two and one half per cent of the time (J=817, z=1.96, the one-tailed probability of which is .025). 3

V. <u>Discussion.</u>

One unexpected finding of the experiment is the relatively low rate of change in the "almost never" condition. Other experiments in the same setting have found that about 20% of S's never attempt a change even when nothing stands in the way. But in the present experiment, 38% of S's made no attempt to change even when there was no penalty.

Much of this effect is obviously to be explained by the mistake of using the word "almost" before "never." This is supported by the fact that S's in the "almost always" condition also changed more than expected, 27% of the time, and said, on post-session questionnaires, that they believed Orange would penalize them only "about half the time."

Part of the effect, however, may be more subtle. Even when we told S's that Orange never used his power, we repeated several times that Orange <u>had</u> power, thus substantially increasing its salience. We have found in a companion experiment, in which S's were uncertain about

Orange's preferences and/or did not know the probability of the use of his power, that S's remain silent about their desire to change the wheel network because of what Orange could do if he wished to, even when they do not know what he would do (see Zelditch and Ford, 1984). Thus, the elevated level of quiescence in the "almost never" condition may in part be due to the effect of A's potential, as opposed to actual, power.

But the effect of potential power would of course operate equally in all three conditions. It is therefore not an alternative explanation of our results, which we interpret as confirming the LAR hypothesis.

VI. Summary and Conclusion.

The "law of anticipated reactions" was tested by experimentally manipulating S's beliefs about the probable "reactions" of a more powerful individual to attempts to change a centralized communication network that, because of the way a bonus was allocated, was the cause of a gross inequity in the allocation of rewards in the experiment. S's were informed that past experience showed that individuals in the center of such networks, because of the material advantage of the central position, preferred them to any alternative network and "almost always," "about half the time," or "almost never" used power to control allocation of team earnings to penalize attempts to change the network, which was in principle possible by majority vote. The dependent variable of the experiment was the trial at which the subject attempted to change the structure of the communication network. There were a total of ten trials if S did not attempt to change the network.

The center of the network was occupied by a confederate who, although he controlled allocation of rewards, at no time expressed any preferences, made any demands, promised any rewards, or threatened any penalties.

The manipulation of anticipated reactions was imperfect, S's in the "almost always" condition perceiving the probability of a penalty for attempts at change to be more nearly "about half the time." Nevertheless, change attempts were inversely proportional to the perceived probability of a penalty even though the experimental confederate at the center of the network never overtly exercised power.

We conclude that the law of anticipated reactions is supported by the data of this experiment.

FOOTNOTES

1. A more complete formulation must account for A's behavior by an independent assumption. A must also anticipate B's reactions, otherwise A does not necessarily know that power need not be overtly exercised. A more complete formulation is given in Samuel and Zelditch, 1986, but is not needed to explain the experiment reported here.

2. Of course, B will also have reason to search the immediate situation for clues as to what A's probable reactions will be. If A's preferences are known, the LAR may still hold, if there is enough at stake for B to search out such clues, because A's preferences themselves are a clue to the magnitude of the risks of noncompliance. Probably, the more important B believes compliance is to A, the more likely that B complies because B believes sanctions are more likely. Even if B does not know A's preferences, s/he may still obey a near-relative of the LAR for B may be able to infer A's preferences from the objective interests present in A's situation. At least, Stanford undergraduates seem to do this in some of our previous experiments. In addition, however,

they seem also to play it safe when they cannot make such inferences. That is, they "nondecide" in uncertain situations (see below). These results are reported in a companion experiment, Zelditch and Ford, 1984.

3. Daniel (1978), pp 207-211, is a simpler guide to this test, including a straightforward method of computing it.

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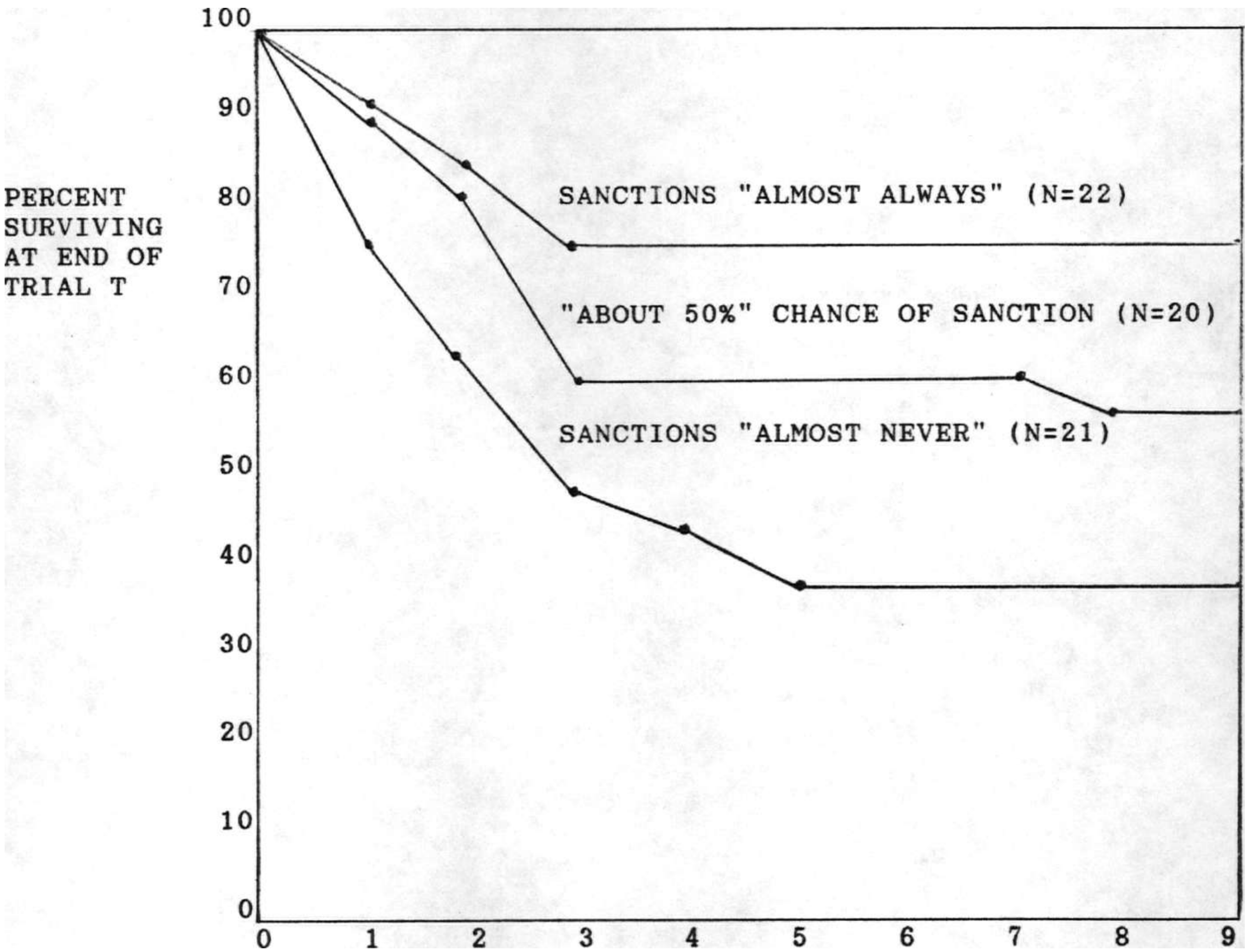
TABLE 1. CUMULATIVE PROPORTION OF SUBJECTS SURVIVING AT END OF TRIAL T BY CONDITION.

		TRIAL NUMBER								
CONDITION	N	1	2	3	4	5	6	7	8	9
Almost Never	21	.76	.62	.48	.43	.38	.38	.38	.38	.38
About Half	20	.90	.80	.60	.60	.60	.60	.60	.55	.55
Amost Always	22	.91	.82	.73	.73	.73	.73	.73	.73	.73

FIGURE 1.

A COMPARISON OF SURVIVAL CURVES FOR
"ALMOST ALWAYS," "ABOUT HALF," AND "ALMOST NEVER" CONDITIONS.



TRIAL NUMBER

TABLE 2. STATISTICS OF THE LOGRANK TEST OF THE SIGNIFICANCE OF DIFFERENCES BETWEEN SURVIVAL CURVES IN THE EXPERIMENT.

CONDITIONS	EVENT RATIO	CHI SQUARE	PROBABILITY
Almost Always/Almost Never	.34*	5.40	P .025
About Half/Almost Never	58**	2.71	N.S
Almost Always/About Half	*58****	1.07	N.S.

* = "Almost always" prevents or delays 66% of rentals that occur in the "almost never" condition.

** = "About half" prevents or delays 42% of rentals that occur in the "almost never" condition.

= "Almost always" prevents or delays 42% of rentals that occur in the "about half" condition.