SOCIAL CHOICE BEHAVIOR: A THEORETICAL AND EMPIRICAL STUDY

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ABSTRACT

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By

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Over the past thirty years, one of the most intensively investigated fields of social science has been decision making. As a recent review article noted, ". . . the literature of decision theory is vast, and its boundaries are ill-defined. Even a very restrictive definition of the limits of that literature would produce several thousand titles . . ." (Edwards and Tversky, 1967: 396). The sociological relevance of behavioral decision making theory is unmistakable: the axioms of decision making, whatever they may be, describe a procedure for combining and using information, a procedure that unquestionably plays an important part in buying behavior, voting behavior, interpersonal bargaining behavior, and so on. The basic principle of decision making is presumably at the heart of many sociological phenomena. But what is that basic principle?

Perhaps two candidates are most prominent. The first is what might be called a "balancing" principle. According to it, the chooser, given a repetitive choice situation, selects alternatives in proportion to their associated gains--that is, if the gain from A_1 is twice the gain from A_2, then A_1 will be selected twice as often as A_2. The second is what might be called a "utility maximizing" principle. According to it, the chooser selects the strategy that maximizes his total gain, which is conceived as the sum of the gains from the alternatives plus the gain from variability--that is, from avoiding monotony and the like. This dissertation seeks to determine which principle more adequately accounts for observable social choice behavior, particularly social influence decisions.

We select from the literature two mathematical models of behavioral decision making, one that takes the balancing principle as a basic postulate, the other that takes the utility maximizing principle as a basic postulate. The balancing model is by Camilleri and Berger (1967), and the utility maximizing model is by Siegel et al. (1964). The two are comparable in both scope of application and degree of empirical confirmation.

After setting forth some concepts and notation in terms of which both models can be stated, we proceed to explicate both models somewhat more completely than they were explicated by their original authors. In the course of this, we derive some conflicting implications that are amenable to empirical testing. Specializing these to social influence decisions, we arrive at the concrete empirical hypotheses of this dissertation.

By way of indicating these hypotheses, let us describe the experiment that was conducted. Basically, each experimental session entailed a sequence of twenty-five two-step decision episodes having these characteristics: (1) each of two persons makes an initial task-relevant decision, there being two possible responses; (2) each person subsequently receives information that the other person has made the opposite judgment; and (3) each person, after receiving this information, makes a final decision. We are not interested in a subject's actual responses; rather, we are interested in whether, for his final decision, he stays, changes, or (in half the experimental conditions) withholds judgment. Decisions in this sense generate the primary data. The dependent variable is the "response vector" (n_s, n_c, n_w), the vector of frequencies with which the focal actor stays, changes, and withholds judgment. In half the conditions, by definition, n_w equals zero, since the subjects are not offered the "withhold judgment" alternative. In the usual research terminology, ours is a 2 x 2 x 2 factorial experiment, the three factors (independent variables) being a subject's ability compared to his partner's (high or low), a subject's number of response options (two or three), and a subject's sex (male or female). The central hypotheses concern the relationship between the two-alternative decisions and the corresponding three-alternative decisions. The Camilleri model implies that the third alternative will act as a progressive tax on the other two, taking at least proportional amounts of probability mass from them. The Siegel model, in contrast, implies that the third alternative will act as a regressive, or flat-rate, tax, taking equal amounts of probability mass from the other two. Our $2^3 = 8$ experimental conditions allow for four separate comparisons that bear upon these conflicting implications. Other hypotheses are also tested, using a variety of statistical and analytical techniques. But these conflicting implications are the central focus.

To extract a meaningful conclusion from the collateral analysis, we turn to a procedure based upon Bayes' Theorem. Under the assumption that one model or the other is true--or at least is a good approximation to the truth--the question becomes: what is the probability, in the sense of rational belief, that the true model is the Camilleri model? That it is the Siegel model? Before the present research, we assumed that the two models' credibilities were 1/2 and 1/2. After the present research, however, based on a Bayesian analysis, the credibility of the Camilleri model is 3/8, that of the Siegel model is 5/8.

But what about the two underlying principles? This is the initial concern of the final chapter. There, two new models, developed by the author, are set forth. The first, Model A, which is a utility maximizing model, is shown to yield as a theorem schema the balancing principle. The implication of this is that balancing and utility maximization are not separate basic principles. The second new model, Model B, which is based on the principles of utility maximization and diminishing marginal utility, is shown to have the very same implications for our research as the Siegel model, although the two models are substantively very different. The existence of Model A and Model B inevitably forces us to ruminate on what is required for decision making research to bear the sociological fruits we seek. Clearly, more research designed to test implications shared by virtually every model in existence is not required. What we need, now more than ever, is research addressed to matters where one credible model takes issue with another. And research must be judged by its potential to alter the balance of credibility. This, we submit, is the straightest, if not the only, road to further scientific progress.

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J. W. Balkwell

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A document that uses logically tight arguments expressed via mathematical notation tends to be difficult to read, partly because logically tight arguments are inherently demanding, but perhaps even more because the system of mathematical notation employed is apt to be unfamiliar to everyone except the author. To mitigate the latter obstacle, so far as this dissertation is concerned, we have collected here the most important symbols employed in the text, together with brief indications of their meanings.

$A = \{A_1, A_2, \ldots, A_m\}$ is the set of alternatives available to the focal actor in an arbitrary decision making situation.

A_k = The kth member of A, where k is an arbitrary integer, $1 \leq k \leq m$.

$Pr(A_k)$ = The probability of the event that the focal actor selects alternative A_k, given that his potential choices are the members of A.

o_i = An arbitrary elementary outcome, an event that the focal actor believes will occur with a probability that depends upon the alternative he selects.

$u_i = u(o_i)$ is the ratio-scale utility to the focal actor of o_i's occurrence; that is, if o_i occurs, the focal actor receives an amount of satisfaction having the value $u(o_i)$, a real-valued magnitude. If $u_i > 0$, u_i is called an <u>elementary gain</u>; if $u_i < 0$, u_i is called an <u>elementary loss</u>; if $u_i = 0$, the elementary outcome o_i is irrelevant--the focal actor is indifferent to its occurrence.

For any alternative $A_k \varepsilon A$,

p_k = The sum of the elementary gains that are possible if A_k is selected, each weighted by its subjective probability of occurrence.

$-q_k$ = The sum of the elementary losses that are possible if A_k is selected, each weighted by its subjective probability of occurrence.

$g_k = p_k - q_k$ is the expected <u>net gain</u> for the focal actor if he selects A_k.

$Q_m = \sum_{i=1}^{m} q_i$ is the absolute value of the sum of the the expected losses from $A_1, A_2, \ldots, A_m$.

At one very important point in our argument, we compare the probabilities of choices from an m-alternative choice-set with the probabilities of the respective choices from this m-alternative choice-set augmented by one additional alternative. In that context,

$T_m = \{A_1, A_2, \ldots, A_m\}$ is the m-alternative choice-set.

$T_{m+1} = \{A_1, A_2, \ldots, A_m, A_{m+1}\}$ is the augmented (m+1)-alternative choice-set.

$Pr_m(A_k)$ = The probability of the event that the focal actor selects alternative A_k, given that his potential choices are the members of T_m.

$Pr_{m+1}(A_k)$ = The probability of the event that the focal actor selects alternative A_k, given that his potential choices are the members of T_{m+1}.

For research purposes, we consider a particular application of the general development. The specializations of T_m and T_{m+1} to what we call "the social influence application" employ the following notation.

$T_2 = \{C,S\}$ and $T_3 = \{C,S,W\}$, where

C = The change-response;

S = The stay-response;

W = The withhold judgment response.

At two points, one in connection with each model under investigation, we demonstrate that the model in question yields an explicit social influence proposition that is consistent with the known experimental evidence, reviewed in Chapter I. In the context of these "plausibility arguments," the following notation is employed.

u_s, u_o, and u_t are the particular elementary gains associated, by hypothesis, with social influence

decisions. The first, u_s, is the utility to the focal actor of approval for choosing in accordance with his own initial judgment; u_o is the utility of approval for choosing in accordance with his partner's initial judgment; and u_t is the utility of approval for choosing in accordance with objective truth.

Z = The focal actor's subjective probability that his initial choice is correct when he and his partner disagree on their initial choices. Z is also interpreted as a standardized measure of the focal actor's competence-status relative to his partner's.

For both theoretical and methodological reasons, sequences of decisions are often of interest. In discussing such sequences, we employ the following notation.

$n = (n_1, n_2, \ldots, n_m)$ is the "response vector," the vector of frequencies with which the focal actor selects the members of a given m-alternative choice-set. The expected value of the kth component, $E(n_k)$, is the number of responses in the sequence multiplied by $Pr(A_k)$.

(n_s, n_c, n_w) is the particular instance of n that pertains to the research of this dissertation. It is the vector of frequencies with which the focal actor stays, changes, and withholds judgment. Given twenty-five decisions, an example would be (13,07,05).

Three of the four models presented, including both of those invented by the present author, are utility maximizing models. In discussing utility maximizing models,

T = The total utility accruing to the focal actor from his sequence of choices. T depends upon the response vector n, the assumption being that the focal actor's choices tend to produce the unique value of n that maximizes T.

Finally, in connection with our statistical analysis, we introduce the following notation. In this context, we are comparing Model 1, the Siegel et al. model, with Model 2, the Camilleri et al. model.

L_1 = A linear combination of sample means whose expected value, $E(L_1)$, is at issue. According to Model 1, $E(L_1) = 0$; according to Model 2, $E(L_1) > 0$.

L_2 = A linear combination of sample means that, in a sense, is the "mirror image" of L_1; that is, according to Model 1, $E(L_2) > 0$; according to Model 2, $E(L_2) \leq 0$.

$Cr(M_1)$ and $Cr(M_2)$ are the prior credibilities of Model 1 and Model 2, respectively, relative to each other. $Cr'(M_1)$ and $Cr'(M_2)$ are the posterior credibilities, the revised credibilities after new evidence has been analyzed.

There are many other symbols employed for various special purposes--this list is by no means exhaustive. Those presented here, however, are perhaps the most important and most frequently recurring ones.

CHAPTER I

STATEMENT OF THE PROBLEM

Decision making is incontrovertibly ubiquitous. Indeed, viewed abstractly, social life consists of little else. Not surprisingly, therefore, decisional phenomena are increasingly becoming recognized as the junction where sociology, political science, psychology, and economics come together. Currently, a number of scholars in these fields are investigating the possibility that ". . . all decision making is guided by an abstract set of axioms which vary only in the manner in which they are operationalized in specified situations . . ." (Ofshe and Ofshe, 1969: 337). Should this turn out to be true, the implications for social science would be great indeed. Without attempting to prejudge this bold possibility, the present dissertation will investigate whether certain kinds of social choice behavior are guided by a "balancing" or a "utility maximizing" principle. This distinction arises in the context of the theory and research to which we will now turn our attention.

Behavioral Decision Making Theory

Since 1944, when J. von Neumann and O. Morgenstern published their seminal treatise, Theory of Games and Economic Behavior, the accumulated work in this area has become immense. As a recent review article noted, ". . . the literature of decision theory is vast, and its boundaries are ill-defined. Even a very restrictive definition of the limits of that literature would produce several thousand titles . . ." (Edwards and Tversky, 1967: 396). Yet the accomplishments of that literature are less vast. Indeed, if an earlier review paper by Edwards (1954) is accurately indicative, then the first decade after von Neumann and Morgenstern's treatise is important mainly for having dispelled certain common-sense notions. It was learned, for example, that persons do not choose so as to maximize objective utility, as a perfectly rational "economic man" would. Thus, although game theory may describe how persons ought to behave, it does not accurately describe how they do behave. It was also learned that a person's preferences are frequently not transitive. Thus, a person may prefer A to B, B to C, yet C to A. Furthermore, it became clear that a person's preferences are frequently not even consistent: he may prefer A to B sometimes, B to A other times. These difficulties, especially the last two, apparently dashed the sense of euphoria that many felt about the applicability of game theory to social science, for the second decade after von Neumann and Morgenstern's treatise witnessed a pronounced shift in interest toward stochastic conceptions of decision making. In retrospect, this was probably inevitable, for it does circumvent the problems arising from the undeniable intransitivities and inconsistencies found in almost all empirical data.

An especially notable theory of stochastic choice behavior appeared in 1959. There were other theories presented before this, of course, and there have been still others presented since; but the theory of R. D. Luce must be considered a milestone in the "stochastic era." Luce's well known Axiom 1 can be stated as follows: Suppose that T is a set of alternatives, that S is a subset of T, and that R is a subset of S. Then the probability of selecting a member of R, given that the members of T are presented, equals the product of the following two quantities: (1) the probability of selecting a member of R, given that the members of S are presented, and (2) the probability of selecting a member of S, given that the members of T are presented. A direct test of this axiom would require two separate experimental conditions. To illustrate the axiom's meaning, Luce asks us to ". . . suppose that T is the set of entrees on a certain menue, S is some proper subset of T that includes roast beef, and R is the single element set of roast beef. The heart of the axiom is that when, for whatever reason, the restaurant has only the entrees S, the probability of selecting roast beef is the same as the conditional probability of selecting it from S when the whole menue is available" (Luce, 1959: 7-8). A surprising variety of consequences follow from this axiom. One is that the value, or attractiveness, of each member of T, the choice-set, is measureable on a ratio scale of utility; and, assuming a fixed scale factor, the utility of an alternative depends only upon the characteristics of that alternative itself, not upon inter-alternative relationships. This is a strong assumption about the way in which persons combine and use information.

Supporting the axiom is a certain amount of empirical evidence (see, for example, Atkinson, Bower, and Crothers, 1965: 143-150). On the other side of the ledger, ". . . abundant evidence . . . shows that Axiom 1 is not always correct. Many . . . decision theorists consider it too strong . . ." (Edwards, 1961: 484). As the present author sees it, however, a convincing test would have to involve a credible alternative theory and a situation such that this alternative theory and the Luce theory implied clearly distinguishable outcomes. Insofar as we can determine, there have been no tests of this kind.

Becker et al. (1963) use Luce's Axiom 1 for classifying models of decision making. Either a given model satisfies the "strict utility" property derivable from the axiom or else it does not; hence, it can be classified as "Luce" or "non-Luce," based upon that criterion.

Whether Luce or non-Luce, stochastic conceptions of decision making are now dominant. One discomfitting thing about this is that anyone can think of choice behavior that is transparently not stochastic. A satisfactory theory must be able to predict this behavior too. It must be able to specify the conditions under which decision making will be apparently non-stochastic. This problem is not quite as easily solved as one might imagine--in fact, it is not clear that it has been solved. Naturally, after the fact, one can always find ways in which particular stochastic choice situations differ from particular non-stochastic choice situations; and there is an understandable inclination to couch one's observations in an abstruse nomenclature and offer them as general principles, which clearly define the necessary and sufficient conditions for choice behavior to be stochastic (or non-stochastic). To a greater or lesser degree, most discussions of the issue carry this impression.

An exception, however, can be found in the paper by Becker et al. (1963). They postulate that most--though not all--alternatives can be compared on more than one dimension. For example, the prospective car buyer can compare Fords and Chevrolets on the dimensions of styling, performance, gasoline mileage, comfort, workmanship, price, "status value," and so on. The gist of their explanation is that, if the chooser focuses upon a fixed set of dimensions, his choice will be fixed. But the attending process is probabilistic. Therefore, in a second-order sense, the decision making process is probabilistic. But how, one might ask, does this solve the problem of specifying the conditions under which decision making will be apparently non-stochastic? Becker et al. would reply that if the alternatives rank the same on each dimension--a trivial case being where there is only one dimension--the ensuing choice behavior will be deterministic. Likewise, if the chooser focuses upon the entire set of dimensions. But the assumption is that, typically, he cannot "consider everything," so he randomly focuses upon some subset of "everything."

The notion of one alternative ranking higher than another on all dimensions brings to mind the concept of dominance from game theory. But the two are not the same. Suppose that one were offered the choice between a Ford with probability one and a Chevrolet with probability one. According to the game theory perspective, either the two would be equivalent or else the preferred one would dominate the other. In either case, the prediction would differ from that of the Becker et al. theory. And this is not an exceptional case. Think about almost any mundane decision--say, the choice between Wheaties and Oaties for breakfast, both available with probability one if selected. If he acted in accordance with the Becker et al. theory, the chooser might focus upon taste and crunchiness, hence choosing the one, or he might focus upon iron and riboflavin content, hence, choosing the other. The dominance prediction would be that the chooser would select the one with greater utility all the time.

The Becker et al. theory is not without its own weaknesses, however. Indeed, it glosses over one of the most important questions. The idea of multidimensionality suffices to explain why some choice behavior is deterministic but not why the rest is probabilistic. Simply postulating that the attention process is stochastic seems a facile answer. "The method of 'postulating' what we want has many advantages; they are the same as the advantages of theft over honest toil" (Russell, 1919: 71). That any human behavior should be stochastic cries for an explanation. After all, few of us feel that we are stochastic automata. One possible explanation is that our feelings of free will are delusions--that the attending process is isomorphic with electrical activity within the brain, which is wholly explicable in terms of the probabilistic laws of physics. It is noteworthy that a prominent psychologist likens the belief in free will to primitive beliefs about the solar system (see Skinner, 1953: 6-7). But an equally prominent astronomer disagrees, claiming that free will is one of the most self-evident facts of nature: "If I can be deluded over such a matter of immediate knowledge--the very nature of the being that I myself am--it is hard to see where any trustworthy beginning of knowledge is to be found" (Eddington, 1957: 260). A second possible explanation is that persons often behave "quasi-randomly on purpose," so to speak, due either to personal or to normative considerations. It has been suggested that a chooser ". . . has some utility of avoiding monotony, of maintaining variability" (Siegel *et al.*, 1964: 29). Along similar lines, it has also been suggested that a justice principle may give rise to the appearance of randomness: "There is evidence that an equity norm causes a utility of choice variability" (Ofshe and Ofshe, 1969: 339). An equity norm could easily be operative in social situations, whereas a desire to avoid monotony could easily be operative in both social and non-social situations. This second explanation is compatible with--although it does not depend upon--the doctrine of free will, of individual responsibility, and so on. The practical consequence of subscribing to the first explanation is that the scientist will be inclined to view a *sequence* of decisions as a *stochastic process*, seeing the dynamic aspect as primary, the stable-state, or static, aspect as secondary. The practical consequence of suscribing to the second explanation is that the scientist will be inclined to view a sequence of decisions as a goal-directed process, seeing the stable-state aspect as primary, the dynamic aspect as unimportant.

Sequences of decisions are, from a sociological standpoint, without question the most interesting decisional phenomena. Traditionally, sociologists have been concerned more with recurrent behavior than with that which is unique. When a person makes recurrent choices, his behavior can be conveniently described in terms of a "strategy," in terms of a vector $(Pr(A_1), Pr(A_2), \ldots, Pr(A_m))$. As this suggests, a strategy is defined as a probability distribution over alternatives. To illustrate, if, for breakfast, a person ate Wheaties three days per week and Oaties four days per week, then his strategy, defined over Wheaties and Oaties, would be (3/7,4/7). To study decision making in terms of strategies, however, requires one further distinction: when a repetitive choice process commences, two separate phenomena arise. First, there is a transitory learning period; then, a relatively stable pattern develops. Ofshe and Ofshe explain as follows: "Stable-state choice strategy refers to an individual's decision strategy after he has 'adjusted' to the situation. In choice experiments, . . . it is typically observed that subjects adopt a stable choice strategy after [such] a learning period . . ." (Ofshe and Ofshe, 1969: 333). Sociologists have been principally concerned with the stable-state period, whereas learning theorists (for example, Estes and Burke, 1955) have been principally concerned with the learning period.

In bringing this discussion to a close, we should disclaim any intention to discuss every important issue or to give an exhaustive review of the literature. Our aims have been more focused. We have tried to indicate the social scientific importance of analyzing decisional phenomena in the abstract, some of the more salient impediments to such analyses, and some of the more insightful responses to those impediments. There is one additional issue, to which we heretofore only alluded, that will be our point of departure. To this issue we now turn.

An Unsolved Theoretical Problem

Two themes seem to run through the published theoretical studies of decision making. One is that the chooser, given a repetitive choice situation, selects alternatives in proportion to the respective gains associated with those alternatives--that is, if the gain from choosing A_1 is double that from choosing A_2, then the former will be selected twice as often as the latter. The second theme is that the chooser selects a strategy so as to maximize his total subjective gain, which is conceived as the sum of the subjective net gains from the alternatives, plus the subjective gain from variability--that is, from avoiding monotony and the like. On the face of it, these principles are different. If they are in fact different, then at least one of them must be false: "Nature does nothing in vain, and more is in vain when less will serve; for Nature is pleased with simplicity and affects not the pomp of superfluous causes" (Sir Isaac Newton, 1686: 398).

The objective of this dissertation can be viewed broadly or narrowly. At the broad level, we will attempt to pinpoint which of these two principles is the "superfluous cause." We will investigate whether man is essentially a "balancing" or a "utility maximizing" organism. Does he balance his choices vis-a-vis the gains from the respective alternatives, or does he select a strategy so as to maximize his total gain? A more fundamental social scientific question is difficult to imagine. At the more narrow level, we will attempt to assess the relative merits of two decision making models--models formalizing the "balancing" and "utility maximizing" principles.

Perhaps it is now time to relate decision making to the sociological context within which our work will take place, to the more concrete sociological problem.

Decision Making and Social Influence

Decision making is always embedded in a sociological context. The axioms of decision making, whatever they may be, describe a procedure for combining and using information, a procedure that presumably plays an important part in buying behavior, voting behavior, interpersonal bargaining behavior,

and so on. The basic principle of decision making is presumably at the heart of many sociological phenomena. To study it, of course, one must study one of these phenomena. The research context of the present dissertation is social influence in small groups, which, in the author's judgment, is an important sociological phenomenon in its own right.

In particular, we shall investigate the following proposition: In a social interaction situation, the greater a person's competence-status, the greater his influence--that is, the more often his advice is taken, the more often he wins arguments, and so on. But having stated this assertion, it is clear that some elaboration is required. What precisely does it mean? And, given that meaning, what reason is there for believing it is true? These questions are perhaps best answered by pointing to some appropriate research studies. This will define by extension (Carnap, 1958) the concepts entailed, and it will also indicate the proposition's degree of empirical confirmation. So let us do this now, following which we can draw together this substantive problem and the abstract theoretical problem described in the preceding section.

The first study to which we would like to point is by Camilleri and Berger (1967).* They reported results from

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*The purpose of Camilleri and Berger's study was to test a formal model of decision making; however, it is the strictly empirical aspect of their work that concerns us at this time. Their model is obviously relevant to our overall objective, but we shall postpone discussion of it until Chapter II.

a controlled experiment in which competence-status and influence figured prominently. Let us first dexcribe the design of their research and then relate that design to these two variables.

Their research design was such as to produce, in each experiment, a sequence of twenty to twenty-two episodes having the following characteristics: (1) each of two persons makes an initial task-relevant decision, there being two possible responses; (2) each person subsequently receives information that the other person has chosen the opposite response; and (3) after receiving that information, each person makes a final decision. Their dependent variable was the number of times a subject stayed with his initial decision--that is, the number of times his initial and final decisions were in agreement. This will henceforth be referred to as a subject's number of "stay-responses." Their research entailed twelve experimental conditions, obtained by crossing four "expectation states" with three control arrangements. By a manipulation, each subject was induced to think that he had high or low task ability, and he was also induced to think that his partner had high or low ability. Thus the four ability combinations or, from a subject's perspective, "expectation states." The subjects worked together as a team, trying to maximize their "team score," which was a weighted sum of their individual numbers of "correct" final choices, each person's number "correct" weighted by his degree of control. A subject could have full control, half control, or no control, as prearranged by the researchers. To sum up, then, Camilleri and Berger's was a 4 x 3 factorial experiment, the two factors being expectation state and control, the dependent variable being number of stay-responses. They reported their data in the form of relative numbers of stay-responses, aggregated over subjects and trials, for each of the twelve conditions. Now let us relate the Camilleri-Berger experimental design to the two variables in which we are interested: competence-status and influence.

Competence-status in this case is relative task ability as induced by the researchers. Let the symbol "(H,L)" denote the expectation state where the referent actor was induced to think that he had high ability, his partner low ability. Similarly, let "(H,H)" denote "high self, high other," and so on. Then the four levels of the expectation state factor--vis., (H,L), (H,H), (L,L), and (L,H)--represent three values of competence-status: (H,L) represents a large value, (H,H) and (L,L) represent medium values, and (L,H) represents a small value. Now let us consider the second variable.

The referent actor's influence is manifested by--and can therefore be indexed by--his partner's proportion of change-responses, a change-response being a final decision that differs from the antecedent initial decision. Aggregate proportions of changes-responses can be computed from Camilleri and Berger's reported data, since these are simply one minus the corresponding proportions of stay-responses.

Now that competence-status and influence have been operationalized in terms of Camilleri and Berger's experimental design, let us examine their data. Three separate cases must be examined, so as to avoid confounding competence-status with control. The data are presented in Table 1.

Table 1. Competence-status and influence: Camilleri-Berger data.

Level 1 of Control		Level 2 of Control		Level 3 of Control	
Com-Sta	Influ	Com-Sta	Influ	Com-Sta	Influ
High	.57	High	.56	High	.76
Medium	.27 & .29	Medium	.35 & .33	Medium	.48 & .40
Low	.18	Low	.22	Low	.27

It can be seen that, within each level of control, the relationship between competence-status and influence is in agreement with our social influence proposition.

By competence-status, however, we have in mind more than simply task ability. A second facet of this concept can be seen in a study by Moore (1968). The notable difference between Moore's research and Camilleri and Berger's was that Moore manipulated prestige instead of ability. His subjects were recruited from a junior college. In half of his experimental conditions, the subjects were told that their partners were from a local high school; in the other half, they were told that their partners were from Stanford University. The first experimental factor, then, was a subject's "prestige-by-association" relative to his partner's. The second factor was called "relevance," the levels being "implicit relevance" and "explicit relevance." In the former conditions, the subjects were left to decide for themselves whether prestige was a relevant clue to ability; in the latter conditions, they were told point-blank that ". . . ____ students consistently do much better than ____ students" (Moore, 1968: 52). The names of schools were inserted so as to indicate a congruence between prestige and ability. In sum, then, Moore's was a 2 x 2 factorial experiment, the two factors being prestige and relevance, the dependent variable being number of stay-responses. This experimental design can also be related to our two variables.

Competence-status in this case is relative prestige; and, as before, influence is indexed by the referent actor's partner's proportion of change-responses. Moore's data are presented in Table 2. The numbers in parentheses are the proportions after Moore eliminated the data from subjects who did not perceive a status discrepancy. It can be seen that, within each level of relevance, the relationship between competence-status and influence is in agreement with

Table 2. Competence-status and influence: Moore data.

Level 1 of Relevance		Level 2 of Relevance	
Com-Sta	Influence	Com-Sta	Influence
High	.37 (.37)	High	.41 (.41)
Low	.30 (.25)	Low	.31 (.25)

our social influence proposition. The relationship within the first level, "implicit relevance," is especially noteworthy, showing that a relatively weak prestige manipulation has, by itself, much the same effect on influence as an ability manipulation.

In real life, of course, persons generally have more than one piece of competence-status information about the others with whom they interact. A study by Berger and Fisek (1970) sheds light upon the question of how such pieces of competence-status information are combined in a social influence situation. Using essentially the same experimental procedures as those of the studies already discussed, Berger and Fisek showed that two pieces of information will be reconciled by averaging the levels of competence-status that would have been inferred from the pieces individually.

The author believes that the Camilleri-Berger, Moore, and Berger-Fisek studies, and others related to these three, suggest a simple, compact conceptualization; and this conceptualization constitutes the meaning of our original social influence proposition. Let a <u>status characteristic</u> be defined as a characteristic of a group member, the states of which are differentially evaluated by the group in question (cf. Berger <u>et al</u>., 1972: 242). For example, age might be a status characteristic in a certain group, older members being evaluated more highly than younger members. This would be a "diffuse" status characteristic. An ability clearly related to the group's purpose, on the other hand, would be a "specific" status characteristic. Let I = the referent actor's degree of influence within a given social interaction situation; Z = the referent actor's relative competence-status, as perceived by the group; $S_1, S_2, \ldots, S_n$ be status characteristics (all diffuse, all specific, or whatever combination applies); and <u>a</u> and <u>b</u> be empirical parameters that are constant over group members, the first being positive. Then the conceptualization suggested by the studies cited can be summarized as follows: $Z = \emptyset(S_1, S_2, \ldots, S_m)$, where $\emptyset$ is some function whose form is probably roughly linear; and $I \approx a\,Z + b$. In words, all the competence-status information that is available in a given situation is combined into a single evaluation, which then guides the social influence process. The greater a person's competence-status, the greater his influence.

That the social influence proposition we have been discussing can be conceived as an application of behavioral decision making theory is intuitively clear enough. And that some authors have so conceived it is also clear (see, for example, Camilleri and Berger, 1967). Nevertheless, the precise coordinating statements have not been explicated to the present author's satisfaction; and, since these statements are crucial links between the theoretical ideas discussed in the previous section and the research to be conducted by us and reported in this dissertation, it is only prudent that such an explication be made. To this important undertaking we now turn.

Coordinating Analysis

At the outset, we must state more carefully the relationship between competence-status and influence. To this end, let I_{xy} = the influence of person x over person y; Z_{xy} = x's competence-status relative to y's; and e_{xy} = a random disturbance of x's influence over y such that E $(e_{xy}) = 0$ and $Var(e_{xy}) = \sigma^2$. As above, let _a_ and _b_ be empirical parameters that are constant over group members, _a_ being positive. Then

$$I_{xy} = aZ_{xy} + b + e_{xy}. \qquad (1)$$

Essentially, what we must do is transform equation (1) into an equivalent form that is more favorable to analysis in terms of behavioral decision making theory.

First, let us propose that, for each x and y, $Z_{xy} + Z_{yx} \overset{\text{def}}{=} 1.0$, where $0.0 \leq Z_{xy} \leq 1.0$. This definition is consistent with our interpretation in the foregoing section that competence-status is a relational concept; moreover, it puts Z_{xy} on a standardized numerical scale, which is easily interpreted. Second, recall that we have operationalized I_{xy} as y's proportion of change-responses. Observed values of I_{xy} are assumed to be the observed values of an underlying random variable. For heuristic purposes, we might imagine that I_{xy} has a scaled binomial distribution (but, of course, assuming a particular distribution is peripheral to the argument). Now, let C_y = the event that y makes a change-response on an arbitrary trial. Then the <u>probability of</u> C_y is the first moment of the theoretical distribution of I_{xy}. That is, $Pr(C_y) \overset{\text{def}}{=} E(I_{xy})$. Parenthetically, it is worth noting that <u>if</u> the distribution of I_{xy} arises from Markov dependent trials, as some authors have posited (see, for example, Cohen and Lee, forthcoming), then $Pr(C_y)$ is the unconditional probability of the event that y makes a change-response on an arbitrary trial.

But let us return to the problem of transforming equation (1) into an equivalent but more convenient form. With the definitions now at our disposal, the conversion is straightforward:

$$\begin{aligned}Pr(C_y) &= E(I_{xy}); \\ &= E(aZ_{xy} + b + r_{xy}) ; \\ &= aE(Z_{xy}) + E(b) + E(e_{xy}) ; \\ &= aZ_{xy} + b ; \\ &= a(1 - Z_{yx}) + b ; \\ &= -aZ_{yx} + (a + b) .\end{aligned}$$

If we now switch perspectives, letting y instead of x be the referent actor, we have an equation that relates the (new) referent actor's theoretically expected behavior to his own competence-status. Since the focus is now upon a single actor, there should be no confusion if we suppress subscripts, writing simply

$$Pr(C) = -aZ + d, \qquad (2)$$

where d = a + b, and where it is understood that the reference is to a given actor in a social influence situation such as that of the Camilleri-Berger, Moore, and Berger-Fisek studies.

Verbally, this proposition states that, in a social interaction situation, the greater a person's competence-status, the *less* his probability of making a change-response. The important point, however, is that this proposition is readily amenable to analysis as an application of behavioral decision making theory. In the coming chapter, and also at other points where our focus is upon abstract decision making theory, equation (2) will be referred to as "the social influence application." Let us file this equation in our minds, so to speak, along with the concomitant discussion of this section and the preceding section. We will return to social influence and the social influence application of behavioral decision making theory at numerous subsequent points, most notably in connection with our own empirical research, to be reported at a later point.

This brings to a close the work of this chapter. In summary, the problem, stated succinctly, is to determine whether decision making is guided by a utility maximizing principle or by a balancing principle. For research purposes, we shall focus upon social influence decisions, which, as we have just seen, can be conceived as a subclass of all behavioral decisions. The next segment of our work will involve the presentation of two decision making models--models that formalize the utility maximizing and balancing themes. Once these models have been presented, and once they have been suitably specialized to the social influence application, the outlines of an empirical test will begin to emerge, and such a test will then become our paramount concern.

CHAPTER II

ALTERNATIVE MODELS

The relatively complex business of this chapter will be tackled in five steps. First, we will set forth some concepts and notation in terms of which behavioral decision making can be further discussed and analyzed. These concepts and notation are similar--but not identical--to those used by Camilleri et al. (1972). Second, we will present, in terms of our conceptual scheme, a model by Siegel et al. (1964), a model that incorporates the utility maximizing principle. Third, we will present, also in terms of this scheme, a model by Camilleri et al. (1967; 1972), which incorporates the balancing principle. Fourth, we will identify some logical consequences of these two models that disagree. And finally, specializing and coordinating these disagreeing consequences to a concrete empirical situation, we will arrive at and state the concrete empirical hypotheses of this dissertation. With this plan in mind, let us begin.

A Basic Conceptual Scheme

Let $A = \left\{A_1, A_2, \ldots, A_m\right\}$ be a well defined choice-set. By well defined, we mean that the alternatives are mutually exclusive, exhaustive, and relevant. A relevant alternative is one the chooser accepts as viable, one that is not dominated. Put negatively, an alternative the chooser would never seriously consider is not relevant. For example, given the choice between Wheaties, Oaties, and raw fish for breakfast, many persons would consider at least one offering to be irrelevant.

Generally, the choice situations of interest are characterized by imperfect information. That is, certain information upon which the choice outcomes are contingent may be known only within limits. To be more precise, for the choice situations of interest, we posit the existence of a set of "contingencies" $C = \{C_1, C_2, \ldots, C_n\}$ that differentially affect the attractiveness of the alternatives; one and only one is the true state of affairs at the time of any given choice; and the chooser has only probabilistic information about which one that is. We can think of the contingencies as Nature's alternatives, the chooser's probabilistic information as his conception of Nature's strategy.

Let AxC be the cartesian product of A and C. Associated with each member of AxC, by hypothesis, is a well defined outcome-set. Let O_{ij} denote the outcome-set associated with alternative A_i and contingency C_j. Then O_{ij} is the set of social outcomes for the chooser if he selects A_i and Nature "selects" C_j. The relationships among these basic concepts are illustrated in Figure 1.

	C_1 (z_1)	C_2 (z_2)	...	C_n (z_n)
A_1	O_{11}	O_{12}	...	O_{1n}
A_2	O_{21}	O_{22}	...	O_{2n}
⋮	⋮			
A_m	O_{m1}	O_{m2}		O_{mn}

A_i = the ith alternative; C_j = the jth contingency; and z_j = the probability of the event that C_j is operative.

Figure 1. The matrix of outcome-sets.

The array of O_{ij}'s will be called the matrix of outcome-sets, and, as suggested, it will enter into both models.

We assume that the elementary outcomes, members of the O_{ij}'s, are differentially valued by the chooser--that is, that some are more attractive to him than others. Given an elementary outcome o, let u(o) denote the ratio-scale utility to the chooser if that outcome occurs. That is, if o occurs, the referent actor receives u(o). Now u(o) may be either an "elementary gain" or an "elementary loss." For any elementary outcome o, u(o) is an <u>elemetary gain</u> iff $u(o) > 0$; and u(o) is an <u>elementary loss</u> iff $u(o) < 0$. Whenever $u(o) = 0$, of course, o is an irrelevant outcome--that is, the referent actor is indifferent to it.

There is a class of outcomes having diametrical opposites. For example, gaining five dollars is the diametrical opposite of losing five dollars, and social approval is the diametrical opposite of social disapproval. More abstractly, "generalized reinforcers," pecuniary or otherwise, have diametrical opposites. In contrast, such outcomes as pellets of food or electric shocks do not. The principle about to be stated applies only to the class of outcomes having diametrical opposites.

OPPOSITES RULE: Let u(o) be an elementary gain, and let $\bar{o}$ be the diametrical opposite of o. Then $u(\bar{o}) = -ru(o)$, where r is a positive real number that does not depend on o.

The gist of this rule is that, subjectively, avoiding the loss of five dollars has r times as much utility for the chooser as getting five dollars. Moreover, it does not matter who or what gives or takes the money. In other words, more generally, the incentive value of <u>not losing</u> is r times the incentive value of <u>gaining</u>, irrespective of the particular outcomes or from where they come, so long as the pairs in question are diametrical opposites. In general, $r \neq 1$.

It will be possible to simply our subsequent exposition enormously by employing some shorthand notation. It should be emphasized, however, that these definitions do not affect the arguments to be made in any way--they are merely conveniences. For future reference, let us call them our auxiliary definitions.

AUXILIARY DEFINITIONS: For any alternative $A_k \varepsilon A$,

$p_k \overset{def}{\equiv}$ the sum of the elementary gains that are possible if A_k is selected, each weighted by its subjective probability of occurrence;

$-q_k \overset{def}{\equiv}$ the sum of the elementary losses that are possible if A_k is selected, each weighted by its subjective probability of occurrence;

$g_k \overset{def}{\equiv} p_k - q_k \overset{def}{\equiv}$ the expected net gain from A_k; and

$Q_m \overset{def}{\equiv}$ the absolute value of the sum of the expected losses from $A_1, A_2, \ldots, A_m \overset{def}{\equiv} \sum_{i=1}^{m} q_i$.

These definitions will be very useful, simplifying certain parts of our presentation considerably.

In conclusion, we have presented a conceptual scheme that, in many ways, resembles that of one-person game theory. In addition, we have presented one substantive principle--the Opposites Rule--and four auxiliary definitions. With these various results available for future reference, let us turn to the two models themselves, presenting the Siegel *et al*. model first.

The Siegel et al. Model

The general case.--Although the exact scope of the model is not known, it is assumed to apply to a fairly broad class of choice behavior. We assume a repetitive choice context. Moreover, we assume that the decisional process has settled down to a stable-state. The model is intended to predict a single actor's stable-state strategy; it is not concerned with the transitory adjustment process, the learning period. This focus of concern has been succinctly expressed as follows: "Our interest . . . is not in learning, but rather in stable-state strategy behavior after the subject has learned . . ." (Siegel et al., 1964: 151).

Let GV be the chooser's gain from variability, and let T be his total gain from the strategy $(Pr(A_1), Pr(A_2), . . ., Pr(A_m))$. Then the following utility function is posited.

AXIOM S1: $T = \sum_{i-1}^{m} g_i Pr(A_1) + GV$

Note that g_i is the expected net gain from A_i, which was defined in the preceding section. The following is also posited.

AXIOM S2: $GV = b \sum_{i=1}^{m} Pr(A_i)[1 - Pr(A_i)]$

The intuitive rationale for Axiom S2 is this: Let X_i be an indicator random variable, equaling one if the chooser selects the ith alternative, zero otherwise. Then the gain from choice variability is seen as being proportional to the sum of the variances of the indicator variables X_i, $(i=1,2, . . ., m)$. The greater the sum of the variances, the greater the gain from variability (cf. Siegel et al., 1964: 36). It is easily shown that GV is maximal when, for all i, $Pr(A_i) = 1/m$.

Now, the distinctive principle of this model is that the chooser selects $(Pr(A_1), Pr(A_2), . . ., Pr(A_m))$ so as to maximize T. Mathematically, this can be stated as follows.

AXIOM S3: $Pr(A_k)$ equals the kth component of the solution vector of the set of m-1 linear equations in m-1 unknowns determined by $\Sigma\ Pr(A_i) = 1$ and $\partial T/\partial Pr(A_i) = 0$, $(i=1,2, \ldots, m)$.

That is, when, for each A_i, the instantaneous rate of change of T with respect to $Pr(A_i)$ equals zero, given the constraint that $\Sigma\ Pr(A_i)$ equals one, there is a unique strategy, a unique set of probability values. At the point determined by these values, T is maximized. For a rigorous statement of the mathematical principles that justify this conclusion, see Cronin-Scanlon (1967: 119-124, 264-267).

With some mathematical ingenuity, it is possible to derive an analytic expression of $Pr(A_k)$ from Axioms S1, S2, and S3. This we will state as a theorem schema.

THM SCHEMA S1: $$Pr(A_k) = \frac{1}{m} + \frac{m\, g_k - \sum_{i=1}^{m} g_i}{2\, b\, m}, \quad (k=1,2, \ldots, m)$$

This completes our presentation of the Siegel <u>et al</u>. model in its general form. Now let us turn to the social influence application, discussed in Chapter I. At the outset, we should note that applying the model requires specifying the alternatives, contingencies, and outcomes associated with each alternative-contingency pair. In addition, for those elementary outcomes having diametrical opposites, it requires specifying which are to be regarded as the elementary gains and which the elementary losses. Typically, certain substantive assumptions will be required before a detailed application will be possible.

The social influence application.--The objective here is to derive a theorem schema that gives an explicit theoretical characterization to equation (2) of Chapter I, that specifies the parameters *a* and *d* in terms of theoretically meaningful quantities. Recall that in the social influence experiments described in Chapter I, each subject made a sequence of two-step choices. On each trial, he made an initial choice; he then received information that the other subject had made the opposite initial choice; and, after receiving that information, he made a final choice. For his final choice, he could stay or change; and it was the decision to stay or change that was of interest. Equation (2) involved the probability of the event that the referent actor would change on an arbitrary trial.

Adopting the standpoint of a focal actor, we can coordinate this situation to the two-alternative, two-contingency case of our basic conceptual scheme. Let

C_1 = the contingency that the referent actor's initial choice was correct, hence that his partner's was incorrect;

C_2 = the contingency that the referent actor's initial choice was incorrect, hence that his partner's was correct;

z_1 = the referent actor's subjective probability that C_1 is the true state of affairs (henceforth to be denoted "Z");

z_2 = the referent actor's subjective probability that C_2 is the true state of affairs (henceforth to be denoted "1-Z");

A_1 = the change-response (henceforth to be denoted "C"); and

A_2 = the stay-response (henceforth to be denoted "S").

Notice that $z_1 = Z$ is a measure of the referent actor's competence-status, a measure that satisfies the definition of Chapter I.

Now we must specify the elementary outcomes associated with this situation. The immediate outcomes are approval and disapproval, coming from various sources for various reasons. Interestingly, for most purposes, the <u>sources</u> are wholly irrelevant. They would probably include the actor himself, the other subject, the experimenter, and perhaps even physically absent "significant others" (cf. Israel, 1963). They could even include "posterity" or God. But none of these potential sources needs to be explicitly acknowledged in deriving a general theorem schema.

The <u>reasons</u> for getting approval or disapproval fall into three categories: (1) acting in accordance or disaccordance with one's own initial choice, (2) acting in accordance or disaccordance with one's partner's initial choice, and (3) acting in accordance or disaccordance with objective truth. Altogether, then, we can, at this level, distinguish six elementary outcomes:

o_s = approval for acting in accordance with one's own initial choice;

$\bar{o}_s$ = disapproval for acting in disaccordance with one's own initial choice;

o_o = approval for acting in accordance with one's partner's initial choice;

$\bar{o}_o$ = disapproval for acting in disaccordance with one's partner's initial choice;

o_t = approval for acting in accordance with objective truth; and

$\bar{o}_t$ = disapproval for acting in disaccordance with objective truth.

The matrix of outcome-sets is constituted as follows:

	Initial Choice Correct Z	Initial Choice Incorrect 1 - Z
Change-response	$\{\bar{o}_s, o_o, \bar{o}_t\}$	$\{\bar{o}_s, o_o, o_t\}$
Stay-response	$\{o_s, \bar{o}_o, o_t\}$	$\{o_s, \bar{o}_o, \bar{o}_t\}$

Now the quantities defined by our auxiliary definitions can be computed. They are given below. Notice that the Opposites Rule has been applied.

$p_c = u_o Z + (u_o + u_t)(1 - Z) = u_o + u_t(1 - Z);$

$p_s = (u_s + u_t) Z + u_s(1 - Z) = u_s + u_t Z;$

$-q_c = (-ru_s - ru_t)Z + (-ru_s)(1 - Z) = -r(u_s + u_t Z)'$

$-q_s = (-ru_o)Z + (-ru_o - ru_t)(1 - Z) = -r(u_o + u_t(1 - Z));$

$g_c = p_c - q_c = -ru_s + u_o + (1 - Z - rZ)u_t;$ and

$g_s = p_s - q_s = u_s - ru_o + (Z - r + rZ)u_t.$

From this point, it is only necessary to substitute the expressions for g_c and g_s into Theorem Schema S1. Writing the result in a suggestive form, we have:

$$\text{THM SCHEMA S2:}\quad Pr(C) = -\frac{(1+r)u_t}{2b} Z + \frac{(1+r)(u_t+u_o-u_s) + 2b}{4b}$$

It is worth pausing to reflect upon what the result of this analysis has been. By applying the Siegel *et al*. model of behavioral decision making, we have specified the parameters *a* and *d* of equation (2) in terms of theoretically and substantively meaningful quantities. Now obviously we still have some unspecified parameters; but the point is that we have derived a proposition from the Siegel *et al*. model that is consistent with what is known about empirical reality. This lends credibility to the Siegel *et al*. model and, therefore, to the utility maximizing principle that it represents. It would be imprudent, however, to draw further conclusions until we have presented the Camilleri *et al*. model, which represents the balancing principle. Let us do that now.

The Camilleri et al. Model

The general case.--The exact scope of this model is unknown, but it too is assumed to apply to a fairly broad class of choice behavior. Again we assume a repetitive choice situation, and again we assume that the stable-state period has been reached. Like the first model, this one is intended to predict a single actor's stable-state choice strategy, his strategy after he has adjusted to the situation.

Camilleri *et al*. note that their model ". . . has many features in common with Luce's" (Camilleri *et al*., 1972: 27). Nevertheless, they depart from Luce's work in an important respect; namely, they define the attractiveness of an alternative--which they call the "expected positive utility" of an alternative--in a novel way:

> The expected positive utility of a response is the sum of the values of the utilities of the positive elementary outcomes associated with the response minus the values of the utilities of the negative elementary outcomes associated with the remaining responses, each weighted by its subjective probability of occurrence (Camilleri et al., 1972: 24).

The authors do not attempt to state this formally, but there is clear advantage in doing so. Let $G(A_k)$ denote the expected positive utility of alternative A_k. Then

$$G(A_k) = p_k - \sum_{i \neq k} (-q_i) \; ; \quad \text{[this is the direct translation]}$$

$$= p_k + \sum_{i \neq k} q_i = p_k - q_k + q_k + \sum_{i \neq k} q_i ;$$

$$= p_k - q_k + \sum_{i=1}^{m} q_i = g_k + Q_m .$$

From this formalization, it can be seen that the expected positive utility of an alternative equals the expected net gain from that alternative plus the absolute value of the sum of the expected losses from all alternatives. Let us state this as axiom one.

AXIOM C1: $G(A_k) = g_k + Q_m, \; (k=1,2, \ldots ,m)$

Expected positive utility is related to choice behavior according to the balancing principle--that is, the probability of choosing a given alternative is proportional to the expected positive utility of that alternative. Since the probabilities of the alternatives must sum to unity, this is equivalent to the following.

AXIOM C2: $Pr(A_k) = G(A_k) \, / \sum_{i=1}^{m} G(A_i)$, $(k=1,2, \ldots, m)$

It is a relatively simple matter to derive an analytic expression of $Pr(A_k)$ from Axioms C1 and C2. In order to preserve a parallel with our presentation of the Siegel et al. model, we will state this as Theorem Schema C1.

THM SCHEMA C1: $$Pr(A_k) = \frac{g_k = Q_m}{\sum_{i=1}^{m} g_i + mQ_m} \text{ , } (k=1,2, \ldots, m)$$

This completes our presentation of the Camilleri et al. model in its general form. Now let us turn to the social influence application. Applying this model requires, as does applying the first model, that we specify the alternatives, contingencies, and elementary outcomes associated with each alternative-contingency pair. Moreover, for each elementary outcome, it requires specifying whether the utility from that outcome is to be regarded as an elementary gain or an elementary loss.

The social influence application.--The objective here, as in the corresponding subsection earlier, is to derive a theorem schema that gives an explicit theoretical characterization to equation (2) of Chapter I. The preliminary analysis presented there applies here as well, of course. Recall that we had the following:

$$g_c = -ru_s + u_o + (1-Z-rZ)u_t \text{ and } g_s = u_s - ru_o + (Z-r+rZ)u_t.$$

It is easily verified that Q_m (Q_2 in this case) is the following: $Q_2 = r(u_s + u_o + u_t)$. From this point, it is only necessary to substitute these expressions in Theorem Schema C1. Writing the result in a suggestive form, we have:

$$\text{THM SCHEMA C2:} \quad Pr(C) = -\frac{u_t}{u_s+u_o+u_t} z + \frac{u_o+u_t}{u_s+u_o+u_t}$$

Notice that again we have partially specified the unspecified parameters of equation (2), this time by applying the Camilleri et al. model of behavioral decision making. Once again the parameters a and d have been given explicit characterizations in terms of substantively meaningful quantities. Just as the corresponding earlier derivation lent credibility to the Siegel et al. model, this derivation lends credibility to the Camilleri et al. model. Before pursuing this matter further, however, it is appropriate to remark upon an unobvious but nonetheless important point.

When the sources of approval and disapproval are explicity stated, let us call the formula that results a "variant" of the theorem schema in question. Then the theorem schemata of the Siegel et al. model and the Camilleri et al. model represent quite an array of separate theorems. Let n = the number of possible sources, and let V = the number of possible variants. Then it can be shown that $V = 8^n$ (eight to the nth power). For n = 3, V = 512; and for n = 5, V = 32,768. One reason for dealing with theorem schemata instead of particular theorems, therefore, is obvious. Still, there is an even more compelling reason. Let us take the perspective of a subject in a social influence experiment of the kind described in Chapter I. Let u_1 = the utility of approval from oneself; u_2 = the utility of approval from one's partner; and u_3 = the utility of approval from the experimenter. Now consider the following "source-analysis" of u_s, u_o, and u_t. Suppose $u_s = u_1$; u_o = zero; and $u_t = u_2 + u_3$. Making these substitutions in Theorem Schema C2, and subtracting the result from unity, we get the equation for Pr(S) that Camilleri and Berger tested via the research described in Chapter I. Out of 512 possible variants, they selected this one and set out to test it. Now it is easily shown that a large number of alternative variants produce exactly the same predictions as those they tested. But the important point is _not_ that they selected an unreasonable variant--in fact, they may have selected the most reasonable one of all 512. The point, however, is that a much stronger test could have been obtained if the authors had designed their experiment to focus upon the distinguishing features _of the model_ as opposed to the unexacting features _of a variant_. This distinction is of fundamental importance.

Empirical Consequences that Disagree

As we have seen, both the Siegel _et al_. model and the Camilleri _et al_. model yield equations that are consistent with the known empirical evidence from social influence experiments. Although these equations are superficially different, they are, we believe, empirically indistinguishable. Camilleri and Berger, in evaluating their model, state: " . . . the ordering implications of our model are consistent with the data" (Camilleri and Berger, 1967: 376). It so happens, however, that the ordering implications of the Siegel *et al*. model are equally consistent with the data. But the decisive point is this. Suppose that the parameter "b" of the Siegel *et al*. model were to have the following value: $b = (1/2)(1 + r)(u_s + u_o + u_t)$. Then Theorem Schema S2 would reduce to Theorem Schema C2. And to prove that b does not have this value would probably be impossible.

This state of affairs is disconcerting, for the two models do represent different substantive principles. The question naturally arises: is it *ever* possible to empirically distinguish between the two models? There does not exist an algorithm for finding the answer to this question; however, one possible road to an answer might be to explore the deviations from Luce's Axiom 1 that each model implies. In particular, let us inquire about what happens to the probability that an alternative will be selected if the choice-set is enlarged by adding one alternative. Let $T_m = \{A_1, A_2, \ldots, A_m\}$ and $T_{m+1} = \{A_1, A_2, \ldots, A_m, A_{m+1}\}$. We will indicate which choice-set a given selection is from by subscripting the probability in question with "m" or "m+1." Notice that T_{m+1} is identical to T_m except that the former has an added alternative. Now, the Luce axiom implies the following:

$$Pr_m(A_k) = Pr_{m+1}(A_k) \; / \; [1 - Pr_{m+1}(A_{m+1})], \quad (k=1,2, \ldots, m).$$

The analogous statements implied by the Siegel et al. model and the Camilleri et al. model are given below.

THM SCHEMA S3: $Pr_m(A_k) = Pr_{m+1}(A_k) + \frac{1}{m} Pr_{m+1}(A_{m+1})$, $(k=1,2, \ldots, m)$

THM SCHEMA C3: $Pr_m(A_k) = Pr_{m+1}(A_k)/[1 - Pr_{m+1}(A_{m+1})] + \partial_k$, $(k=1,2, \ldots, m)$, where

(a) $\delta_k \geq 0$ if $Pr_m(A_k) > 1/m$;

(b) $\delta_k = 0$ if $Pr_m(A_k) = 1/m$;

(c) $\delta_k \leq 0$ if $Pr_m(A_k) < 1/m$.

These theoretical relationships can be verified by direct substitution, using Theorem Schema S1 and Theorem Schema C1, respectively. In Theorem Schema C3 above, it should be noted that whenever $Q_{m+1} > Q_m$, the inequalities involving δ_k are strict.

Whenever $Pr_m(A_k) \neq 1/m$, the predictions of the two models disagree. Moreover, the direction of the disagreement depends upon the direction of $Pr_m(A_k)$'s departure from $1/m$. Herein lies the basis of a strong empirical test.

Research designed to test models based on a balancing principle has produced a fair amount of evidence that has been interpreted as supporting this principle (see, for example, Olin, 1966; Camilleri and Berger, 1967; Balkwell,

1969; McMahon, 1970). At the same time, research designed to test models based on a utility maximizing principle has also produced a fair amount of evidence, but this latter evidence has been interpreted as supporting this second principle (see, for example, Siegel and Goldstein, 1959; Siegel et al., 1964; Ofshe and Ofshe, 1970a, 1970b). It is not clear that any of this evidence is more compatible with one principle than with the other. Thus, on evidential grounds, neither principle can be considered more credible than the other. "How and why," asks the philosopher Karl Popper, "do we accept one theory in preference to others? . . . We choose the theory which best holds its own in competition with other theories" (Popper, 1959: 168). We would add only that someone must arrange for the theories to "compete."

The Hypotheses of this Dissertation

Let us specialize these disagreeing empirical consequences to the social influence application. Recall that the choice-set of that application is $\{C,S\}$. The subject must make either a change-response or a stay-response. To apply Theorem Schemata S3 and C3 to social influence decisions, it is necessary to find a second choice-set that includes C and S and one additional option. The author's experience in conducting social influence experiments suggested that a natural addition would be a "withhold judgment" option, for it was clear that subjects often decide to change or stay only with the greatest reluctance. The chance to say, in effect, "it is too close to call," seemed like one that might have some appeal. The details of this option will be presented in Chapter III; for now, let us just note that in half our experimental conditions, the offered choice-set is $\{C,S\}$, and in the other half, the offered choice-set is $\{C,S,W\}$. The concrete hypotheses of this dissertation are straightforward applications to these choice-sets of Theorem Schemata S3 and C3. In terms of the notation introduced in the preceding section, the first set is a particular instance of T_2, the second set is the corresponding T_3. For future reference, let us call the social influence applications of Theorem Schemata S3 and C3 our "basic hypotheses," and let us record them.

BASIC HYPOTHESES

Model 1: $Pr_2(C) = Pr_3(C) + 1/2\ PR_3(W)$.

Model 2: $Pr_2(C) = Pr_3(C) / [1 - Pr_3(W)] + \delta_c$, where

(a) $\delta_c \geq o$ if $Pr_2(C) > 1/2$;

(b) $\delta_c \leq 0$ if $Pr_2(C) < 1/2$.

Model One, of course, is the Siegel <u>et al</u>. model, and Model Two is the Camilleri <u>et al</u>. model. Regarding Model Two, the case where $\delta_c = 0$ is irrelevant, because research must be designed so that $Pr_2(C) \neq 1/2$ if it is to have any chance of distinguishing between the two models. Finally, these hypotheses could be stated in terms of Pr(S) instead of Pr(C).

From a research standpoint, the choice is entirely arbitrary--the two statements lead to the same tests and the same results. But stating the hypotheses in terms of Pr(C) makes better sense vis-a-vis our original social influence proposition.

Summary and Conclusion

The aim of this chapter has been to present alternative models of behavioral decision making, one representing the utility maximizing principle, the other representing the balancing principle. We began by setting forth a basic conceptual apparatus in terms of which the two models could be stated. Then, in succession, we presented the Siegel *et al*. model and the Camilleri *et al*. model, which rest upon the respective principles. Specializing these models to the social influence application, we observed that both yield propositions that specify, in terms of substantively meaningful quantities, the previously unspecified parameters of Chapter I's equation (2). But to our dismay, we discovered that the two models' specifications were empirically indistinguishable. Turning to the published experimental evidence, we found that all the tests that have heretofore been conducted have been somewhat feeble--that none have borne directly upon the two models' distinguishing properties. As if to prove that consternation can be constructively channeled, we commenced a search for empirical implications of the models that disagree. This effort bore fruit. Finding such implications, we proceeded to specialize them to social influence decisions; and this is where our presentation now stands.

Until now, our concern has been chiefly theoretical; however, we have arrived at a point where further empirical knowledge is sorely needed. So let us turn to some original research that may be able to answer some of the questions that have been raised, some research designed to test our "basic hypotheses," stated above.

CHAPTER III

THE RESEARCH: DESIGN AND PROCEDURES

Various aspects of the experimental design and procedures have already been mentioned, but until now they have not been described fully and systematically. This is the aim of the present chapter. We shall begin with a brief overview; then we shall describe the experimental procedures in some detail; and, finally, we shall describe the statistical analysis, the ways in which the data are to be analyzed, relating these ways to our basic hypotheses. Let us commence.

A Brief Overview

Basically, each experimental session entails a sequence of twenty-five two-step decision episodes that have the following characteristics: (1) each of two persons makes an *initial* task-relevant decision, there being two possible responses; (2) each person subsequently receives information that the other person has made the opposite judgment; and (3) each person, after receiving that information, makes a *final* decision. It should be emphasized that we are not interested in a subject's actual responses; rather, we are interested in whether he stays, changes, or (in half the experimental conditions) withholds judgment. Thus, decisions <u>in this sense</u> are the primary data. An example of a datum from a single subject might be: s,s,w,w,s,c,c,w,s,s,s,c, w,c,c,s,s,s,c,s,s,w,s,c,s. Here the subject stayed 13 times, changed 7 times, and withheld judgment 5 times. These frequencies are the components of our dependent variable, which can be thought of as the "response vector" (n_s, n_c, n_w). For each subject in each condition, we get one value of this vector; and, theoretically, that value depends upon the values of the independent variables to be described.

Using standard research terminology, ours is a 2^3 factorial experiment, the three factors (independent variables) being a subject's competence-status relative to his partner's (high or low), a subject's number of response options (two or three), and a subject's sex (male or female). The first two factors relate to our two models, but sex does not. The reason for adding sex as an independent variable is to obtain insight into the degree to which the other variables operate invariantly across groups with different socialization experiences. Needless to say, "socialization invariant" findings are of greater scientific importance than findings that apply only to certain groups--only to males, only to Chinese, or whatever.

As noted, we have $2^3 = 8$ experimental conditions. The mean values of the response vector for these eight conditions should be related to each other in accordance with the implications of Model One if Model One is correct or the implications of Model Two if Model Two is correct. In a nutshell, our analysis will involve examining the relationships among cell means and drawing conclusions, based on this examination, about the relative merits of Model One and Model Two. The precise statistical and analytical procedures will be described in the third section of this chapter; but, before getting into this somewhat technical presentation, let us describe the experimental procedures that were employed, the concrete details of an individual experimental session.

The Experimental Procedures

Each experiment begins with two subjects being seated in a small groups laboratory. The arrangement of the experimental room is illustrated in Figure 2 (next page). The two subjects are seated side by side, but they are separated by an opaque partition. There is a large screen at the front of the room, behind which is a Carousel slide projector. The experimenter operates mainly from an adjacent room, observing the subjects through a one-way mirror, entering and exiting the laboratory as required. This is explained to the subjects with the statement that "the controls for the slide projector are in this room back here," which seems to be all the explanation that is necessary. When he is in the adjacent room, the experimenter communicates with the subjects through an intercom system.

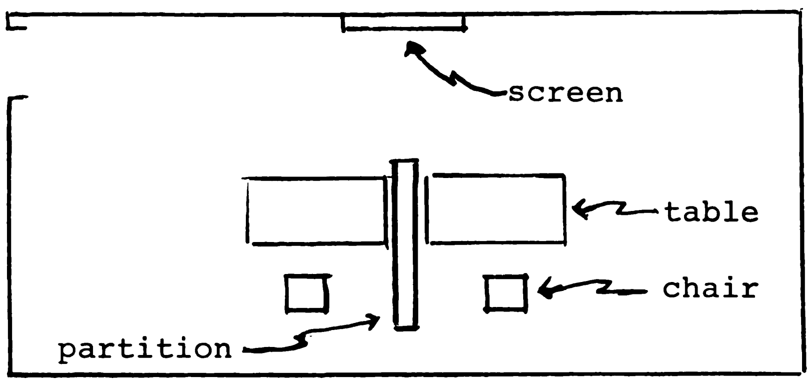

Figure 2. The experimental room.

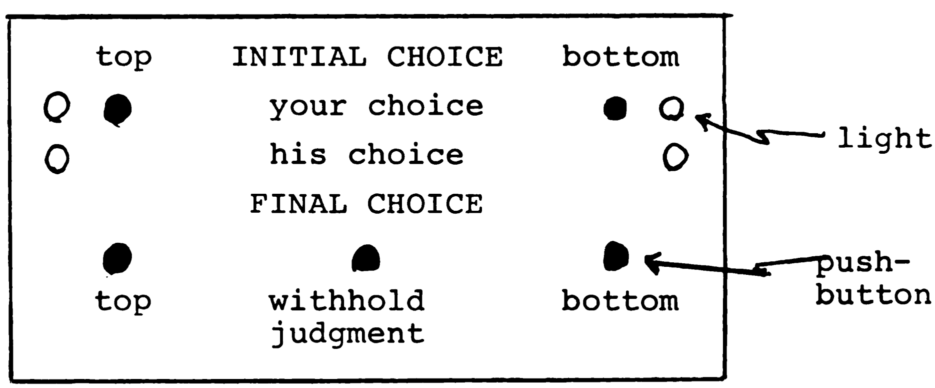

Figure 3. A subject panel

All communication between the two subjects themselves takes place by means of panels, which are part of an ICOM (interaction control machine) system. Unknown to the subjects, the experimenter is able to manipulate the content of this communication (cf. Berger et al. 1972: 249). One of the subject panels is illustrated in Figure 3. (It should be noted, of course, that the withhold judgment button is not present in the two-alternative conditions). As indicated earlier, what are purported to be the subjects' initial decisions are communicated to their partners; however, their final decisions are private. All decisions, both initial and final, are recorded by an Esterline Angus event recorder, which is wired into the ICOM system.

With this as background, let us indicate the organization of an experimental session. This organization can be outlined as follows:

I. Part I (competence manipulation)
  a. Part I instructions
  b. Part I test
  c. Part I questionnaire
II. Part II (social influence decisions)
  a. Part II instructions
  b. Part II test
  c. Part II questionnaire
III. Part III (interview and debriefing)

We will describe the main features of each part in order.

The part one instructions have two purposes, one being to explain how the equipment works, the other being to provide an explanation of what the subjects will be asked to do and why we are interested in such activity. The ostensible purpose of part one is to study something referred to as "contrast sensitivity." Let us note at this time that copies of all the experimental materials, including the part one instructions, can be found in Appendix A.

Following the presentation of the part one instructions, the subjects are given a "test," which purportedly measures their respective levels of contrast sensitivity. Twenty trials make up the part one test. On each trial, the subjects are asked to judge which of two rectangular, "scrambled checkerboard" patterns projected on the front screen is "slightly more black than white." The patterns are arranged vertically; thus, the possible answers are "the top one" and "the bottom one." The subjects are told that one pattern is 50 percent black and 50 percent white, but that the other is 53 percent black and 47 percent white. (Actually, both patterns are 50-50). After the twenty trials have elapsed, the experimenter purports to add up each subject's correct answers, and then he presents fictitious scores. On the wall are posted scoring standards, comprised of intervals labeled "high," "high average," "low average," and "low." It purportedly turns out that one subject got 17 correct, which is "high," the other subject 8 correct, which is "low." Showing a faint touch of practiced embarrassment, the experimenter remarks upon how "unusual" this is, and then he passes out the part one questionnaire.

The part one questionnaire is designed both to check the success of the competence-status manipulation and to reinforce it, the latter by inducing the subject to rehearse its results. He is asked to estimate how well he and his "partner" (by this time we have begun to refer to the two subjects as "partners") would do if they were to take "a second Contrast Sensitivity test like the one given today." The questionnaire has two Likert-type items and one open-ended item, the latter designed to elicit possible suspicion. When the questionnaire has been completed, it is collected, and we procede to part two.

Part two, introduced as "the more sociological part of our work today," is of course the heart of the experiment. In the part two instructions, the subjects are told that we are interested in their abilities to make correct decisions in a "cooperative choice situation." To explain what a "cooperative choice situation" is, we draw an analogy with a physician making a difficult medical diagnosis, who asks the advice of a second doctor before making his final decision. It is emphasized that the doctor ". . . shouldn't care whether he himself thinks of the right answer, or whether he recognizes the right answer after this consultation process. The only thing that's important is that he <u>make the correct final decision</u>" (experimental instructions).

At the completion of the part two instructions, the second test is begun. This test involves 25 critical trials and 5 non-critical trials. On each critical trial, the sequence of events is as follows: (1) the experimenter presents a contrast sensitivity problem, (2) each subject is asked to make an initial choice, (3) each subject is given ostensibly accurate feedback that the other subject has made the opposite initial choice, and (4) each subject is asked to make a final choice. On each non-critical trial, the sequence of events is the same except for the third item. Instead of being led to believe that his partner made the opposite initial choice, he is led to believe that his partner made, on these trials, the same initial choice. For purposes of analysis, these trials--numbers 4, 11, 15, 19, and 26--are simply disregarded. Their sole function is to lend plausibility to the experiment. The crux of each critical trial, as we have indicated before, is what the subject chooses to do with his final choice. In the two-alternative conditions, he may stay or change; in the three-alternative conditions, he may stay, change, or withhold judgment. His pay for participating ostensibly depends upon his final decisions, a "correct" choice being most rewarding, an "incorrect" choice being least rewarding, and (in the three-alternative conditions) a "withhold judgment" choice being intermediate in reward value. To add effectiveness to this pay-off structure, the subjects are told that they are working together as a team. The "team score" is explained to them as follows:

> Each time a person makes a correct final choice, the team will gain two points; each time a person selects the 'Withhold Judgment' alternative, the team will gain one point; and each time a person makes an incorrect final choice, the team will gain nothing. Or to put it another way, each final choice you make will be scored +2, +1, or 0, depending upon whether you're correct, whether you withhold judgment, or whether you're incorrect, in that order. . . . In accordance with our standard policy, each of you will be paid the same amount for taking part in today's study, and that amount will depend on your final team score (experimental instructions).

All mention of the "withhold judgment" option is of course deleted from the instructions of the two-alternative conditions.

Before leaving the part two test, there are three miscellaneous points that should be mentioned. First, the <u>ostensible</u> pay range is \$1.00 to \$2.20, each subject's pay depending upon the team score; <u>actually</u>, each subject receives \$2.00 whatever his choices. Second, it was mentioned earlier that the subjects' final choices are private. This is true, yet it does not mean that the subjects are completely unaccountable for their behavior: they are told that, at the end of the test, each person's contribution to the team score will be announced. Finally, it should be emphasized that, in designing part two, our number one priority was to make the two-alternative and three-alternative conditions identical except for the presense or absense of the withhold judgment option and the instructions pertaining specifically to that option.

Following this second test, each subject is asked to fill out a second questionnaire. This questionnaire asks him to indicate his concern with getting correct answers (one Likert-type item), his concern with avoiding incorrect answers (one Likert-type item), his degree of nervousness (one Likert-type item), his conception of his final choice performance (one Likert-type item), and his general thoughts about the test (two open-ended items). The purposes behind these questions are to elicit possible suspicion and to provide leads for the post-session interview. While the subjects are filling out this questionnaire, the experimenter is purportedly totaling up the team score. As soon as the questionnaires are completed, the experimenter collects them and announces that he would like to speak with each subject individually for a few minutes. He then leads them to separate interview rooms and commences to interview and debrief the first subject.

We have indicated in passing that the experiment entails two deceptions: the fictitious scores of part one and the fictitious disagreements of part two. From a practical standpoint, it is important to learn whether these deceptions were successful; from an ethical standpoint, it is important to undo the injury to truth that is unfortunately entailed. The concluding interview and debriefing is addressed to both these matters. Suggestions by Aronson and Carlsmith (1968: 70-73) were relied upon heavily in designing this third and final part of the experiment.

These, then, are the essential features of an individual experimental session. As noted earlier, it is a subject's final choices in relation to his initial choices that make up the primary data. The example of a datum from a single subject given before was: s.s.w.w.s,c,c,w,s,s,s,c, w,c,c,s,s,s,c,s,w,s,s,c,s. Here the response vector $(n_s, n_c, n_w) = (13,07,05)$. For each subject--there were 80 in all--we obtain one value such as this, where $n_w \overset{def}{=} 0$ in the two-alternative conditions. Thus, the raw material for our analysis is eighty values of the response vector (n_s, n_c, n_w). The question becomes, what are we to do with eighty values of a random vector? How are the data to be analyzed? How are conclusions about the two models to be reached? It is to such questions that we now turn.

Statistical Analysis

Perhaps the prime virtues of a statistical analysis are thoroughness and systematization. The object is to summarize and evaluate every aspect of one's data that bears upon the hypotheses being tested. Given a data-set, there is a finite collection of non-overlapping questions that can be asked. A subset of these is relevant to a preselected group of hypotheses, and the complementary subset is irrelevant, although it may be relevant to other purposes. So the problem is to identify and concentrate upon the first subset while, at the same time, avoiding irrelevant forays into the second.

The place to begin is with a conception of just what it is one wishes to find out. Once that is decided, the rest practically falls in place. In the present case, we should first be concerned with whether <u>either</u> model is consistent with the gross features of the data, whether either model predicts correctly in those ways in which their predictions are parallel. The models' parallel predictions are of course what Camilleri and Berger (1967) referred to as the "ordering implications" of their model, which were restricted to relatively gross ordering implications. Now assuming that such ordering implications are borne out--that both models are "in the right ballpark," so to speak--we should become concerned with whether the data conform better to the implications of Model One or Model Two <u>where those implications diverge</u>. And finally, we should become concerned with the degree of rational belief one can have that the better fitting model is in fact the more valid model. After all, we are examining but one data-set, and no one would deny that there is statistical variation in the data. Thus, the question becomes: what is the degree of rational belief that the observed pattern of results, insofar as that pattern supports one model over the other, represents a true empirical regularity and not just a statistical anomaly? These three concerns define the three stages of our statistical analysis; let us consider each in turn.

First stage.--The first concern, in the present author's judgment, is best dealt with in conjunction with the usual, well known procedures of statistical inference. Since ours is a 2^3 factorial experiment, it is natural and appropriate to ask what might be learned by performing an ordinary three-way analysis of variance on the change-response component and the stay response component, respectively. Let us list the seven pairs of hypotheses that each ANOVA entails. Then we can consider how they bear upon matters of substantive significance. With regard to the change-response component, these hypotheses are the following.

$H1_o$: There is no difference in frequency of changing between high competence-status subjects and low competence-status subjects.
*$H1_a$: There is at least some difference.

$H2_o$: There is no difference in frequency of changing between subjects allowed to withhold judgment and subjects not allowed to withhold judgment.
*$H2_a$: There is at least some difference.

$H3_o$: There is no difference in frequency of changing between male subjects and female subjects.
$H3_a$: There is at least some difference.

$H4_o$: There is no 2-way interaction between competence-status and number of alternatives.
$H4_a$: There is at least some such 2-way interaction.

$H5_o$: There is no 2-way interaction between competence-status and sex.
$H5_a$: There is at least some such 2-way interaction.

$H6_o$: There is no 2-way interaction between number of alternatives and sex.
$H6_a$: There is at least some such 2-way interaction.

$H7_o$: There is no 3-way interaction among competence-status, number of alternatives, and sex.
$H7_a$: There is at least some such 3-way interaction.

The hypotheses marked with asterisks represent predictions of both models. Hypothesis $H1_a$, of course, is our original social influence proposition; hence, we would expect this hypothesis to be strongly confirmed; and, indeed, were such confirmation not in evidence, serious doubt would arise about the efficacy of the experiment. Hypothesis $H2_a$ is also an implication of both models under the assumption that the withhold judgment option attracts any responses at all. Both models imply that an added alternative will take probability mass from each other alternative. This is not, of course, logically necessary: if it is true, it is an empirical truth. But the important point for us is that if it is false, then neither model is predicting correctly--and we should know this before proceeding to what would then be a dubious question of which model is best.

The other hypotheses are theoretically relevant, but they are relevant in a different way. They pertain to "the nature of the givens," so to speak. Consider $H3_o$ and $H3_a$. Neither model says whether we should expect to find a difference between males and females. If there is a difference, however, then the models must be applied to each group separately, for both models assume homogeneity of the subject samples. "Homogeneity," of course, means sameness with respect to pertinent characteristics, not with respect to every conceivable characteristic. So the question becomes: is sex a pertinent characteristic? Similar questions could be asked about age, race, cultural background, and so on. As science seeks general, widely applicable propositions, it is clear that a model is more valuable if it does not depend upon the ascribed and achieved characteristics of the groups to which it is applied. But if a model is dependent upon such characteristics, this only makes it more complicated, not false.

A similar point should be noted in regard to the interaction hypotheses entailing sex. An "interaction" means that the rate of change of the dependent variable with respect to an independent variable is non-constant over different values of the other independent variable(s). For example, if our basic social influence proposition were stronger for males than for females, this would imply an "interaction" between competence-status and sex--it would imply that the parameter a of equation (2) was different for males than for females. Again, however, the issue is whether the models' unspecified parameters depend upon the ascribed and achieved characteristics of the persons to whom they applied, or whether the models' unspecified parameters are independent of such characteristics. This is a scientifically important issue, to be sure, but it should not be confused with the issue of whether a model is true.

The interaction hypothesis about competence-status and number of alternatives also concerns the "nature of the givens." In this case, however, the issue has to do with how the matrix of outcome-sets, discussed in Chapter II, should be constituted, with the nature of the elementary outcomes associated with the withhold judgment alternative. Were we to find an interaction in this case, it would suggest that Pr(W), the probability of withholding judgment, is a function of competence-status, which would place constraints upon the two outcome-sets associated with this alternative.

In sum, then, this first stage of our analysis should yield three kinds of useful information. Most basically, it should indicate whether either model is worth pursuing further; it should also indicate, or at least give insight into, the extent to which our main findings are what the author likes to call "socialization invariant," the extent to which they are independent of the ascribed and achieved characteristics of the subjects. And finally, it should indicate some things about how the withhold judgment option is perceived by the subjects who take part in this study, what the "pros" and "cons" of this option are. At this point, let us consider the second state.

Second stage.--The second question raised at the outset--that pertaining to the models' divergent predictions--is amenable to the technique of planned comparisons, applied to theoretically meaningful functions of the response vector (for discussions of this technique, see Hays, 1963: 459-483; Guenther, 1964: 50-54; Blalock, 1972: 330-334). Consider first the following function, which we will call $\emptyset_1$.

$$\emptyset_1(n_s, n_c, n_w) = n_s + 1/2\ n_w$$

In the two-alternative conditions, the value of this function is simply the value of n_s, since $n_w \overset{def}{=} 0$. Now, according to Model One, the mean value of $\emptyset_1$ in each two-alternative condition should equal the mean value of $\emptyset_1$ in the analogous three-alternative condition. This is easily established by means of our basic hypotheses, stated in Chapter II. But according to Model Two, no such equality should exist. Moreover, the inequalities implied by the second model become reversed as we move from the (H,L) conditions to the corresponding (L,H) conditions. To simplify our exposition, let us disregard sex and diagram the second model's predictions as follows:

	2 alternatives		3 alternatives
(H,L)	$\bar{\emptyset}_1$	>	$\bar{\emptyset}_1$
(L,H)	$\bar{\emptyset}_1$	<	$\bar{\emptyset}_1$

Let L_1 = the sum of the two larger values in this diagram (upper left and lower right) minus the sum of the two smaller values (upper right and lower left). Then Model One's implication is $L_1 = 0$, whereas Model Two's implication is

$L_1 > 0$. This is a planned comparison, and it can be evaluated using standard techniques based upon the t-distribution. The null hypothesis represents Model One.

But a partisan of Model Two might object that such a test favors Model One, since beta, the size of the Type II error, would almost certainly be larger than alpha, the size of the Type I error. Granted. So let us supplement this comparison with one that reverses the roles of the two models. Consider the following function, which we will call $\emptyset_2$. In this expression, N is the number of trials.

$$\emptyset_2(n_s,n_c,n_w) = N\ n_s/(n_s+n_c)$$

In the two-alternative conditions, the value of $\emptyset_2$ is simply the value of n_s, since $N = n_s+n_c$ in this case, which cancels with the denominator. Model Two implies that $\emptyset_2$ of the mean value of the response vector in the two-alternative conditions compares with $\emptyset_2$ of the mean value in the three-alternative conditions as follows: in the (H,L) case, the first is greater than or equal to the second; and, in the (L,H) case, the first is less than or equal to the second. Model One implies strict inequalities in the opposite directions. For all practical purposes, the two models' implications are equivalent to the situations diagrammed below.*

*This is extrapolating from $\emptyset$ of the response vector's mean values to the mean values of $\emptyset$. In the two-alternative conditions, the two are the same, since $\emptyset$ is linear; however, in the three-alternative conditions, the two differ slightly. Looking ahead to the data, in the (H,L) case, $\emptyset$ of the mean values is 20.15, the mean value

	Model One		Model Two	
	2 Options	3 Options	2 Options	3 Options
(H,L)	$\bar{\emptyset}_2$ <	$\bar{\emptyset}_2$	$\bar{\emptyset}_2$ $\geq$	$\bar{\emptyset}_2$
(L,H)	$\bar{\emptyset}_2$ >	$\bar{\emptyset}_2$	$\bar{\emptyset}_2$ $\leq$	$\bar{\emptyset}_2$

Paralleling the approach taken before, let L_2 = the sum of the two larger values minus the sum of the two smaller values according to Model One. Then Model Two's implication is $L_2 \leq 0$, whereas Model One's implication is $L_2 > 0$. Again, this is a planned comparison that can be evaluated using standard techniques. Here the null hypothesis represents Model Two.

These "mirror image" planned comparisons cut right to the center of the differences between Model One and Model Two. Just as an additional check, however, we will also do an ordinary chi-square goodness-of-fit test of each model. Although the goodness-of-fit technique is not as robust with respect to underlying statistical assumptions as the planned comparisons technique, it should nevertheless provide useful supplementary information. In particular, if the two approaches should produce contradictory conclusions, this would alert us to the need for additional analysis. In all probability, however, by the time this second stage of the analysis is completed, one model or the other will have a

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of $\emptyset$ is 19.95; in the (L,H) case, $\emptyset$ of the mean values is 7.32, the mean value of $\emptyset$ is 7.38. It should be noted that this slight bias favors the null hypothesis of the test to be employed.

clear edge in apparent tenability; and this brings us to the third question raised at the outset: Given the result, whatever it may turn out to be, what is the strongest *justifiable* conclusion about the relative merits of the two models that can be extracted?

Third stage.--The problem can be viewed as that of meaningfully translating Neyman-Pearson type conclusions into the language that working scientists actually speak. That a given model has, let us say, been rejected at the .10 level of significance is *evidence to be considered* in drawing a scientifically meaningful conclusion, but the statement of such a result is not itself a scientifically meaningful conclusion (cf. Camilleri, 1962). For the record, the present author favors the use of "significance tests." But it must be conceded that the conclusions from such tests, to a considerable degree, "beat around the bush," as it were. They do not cut to the center of what the scientist really wants to know, which is the credibility of his hypothesis in light of the new evidence he has just examined. Not that they are irrelevant to this, mind you--their fault is perhaps more non-trenchency than irrelevance. It is to circumventing this fault that the third stage of our analysis is directed.

The technique to be used is not new, although it may be unfamiliar to some sociologists. It is based upon Bayes' Theorem, which has long been recognized as an aid to the derivation of unobvious but correct conclusions. Consider a non-scientific example. Suppose one percent of the United States population has tuberculosis. A certain tuberculin test comes out positive ninety-five percent of the time if the person actually has tuberculosis and three percent of the time if the person is in fact free from the disease. Suppose person x takes this test and it comes out positive. What are the chances that x has tuberculosis? The answer is a surprisingly low twenty-four chances out of a hundred. Now, the scientific uses of the theorem are directly analogous to this: given certain probabilistic information, one computes the chances that a certain conclusion is valid. Now let us be careful--in fact, a scientific conclusion is either true or false, just as, in fact, x either has or does not have tuberculosis. But the point is that, on the basis of given evidence, a certain degree of confidence in a conclusion is optimally rational. More confidence is unwarranted, and less confidence is equally unwarranted.

An analysis based upon Bayes' Theorem is very well suited to "crucial experiments" involving stochastic models. To see why, let us review the features of a so-called "crucial experiment." Suppose there are two models, M_1 and M_2, that yield empirically indistinguishable predictions in most cases, that account for the relevant, known evidence equally well. For example, several variant models of "learning theory" yield empirically indistinguishable "learning curves." Suppose that, at a given date, there is no solid basis for preferring either model to the other. In time, however, someone discovers discrepant implications. An experiment is designed whose outcome can be compatible with M_1 or M_2 but not with both. Such an experiment would be "crucial." In principle, its execution would lead to the rejection of one model, the acceptance of the other.

Unfortunately, research is never quite this swift and clean: "In sober fact the crucial experiment does not conclusively establish one alternative while making the [second] absolutely untenable; at most it only alters the balance of probabilities" (Kaplan, 1964: 152). Going into a crucial experiment, the two models must be considered equiprobable, since they have always tied in head-to-head competition, so to speak. But coming out, one or the other must ordinarily be accorded the edge, for remaining aloof to the evidence is "a form of insanity in which truth is by fiat," to borrow an apt phrase from R. D. Luce. The problem is how to fairly express that edge, and this is where Bayes' Theorem enters.

A Bayesian analysis eschews categorical conclusions--for example, that M_1 is now tenable and M_2 is not, or vice versa. Instead, its conclusions relate how the "balance of probabilities" has changed--from .5-.5 to .3-.7 or whatever. This is clearly in the spirit of Kaplan's remarks: a crucial experiment only alters the balance of probabilities.

Of course, given a sequence of tests, covering the range of the two models' divergent predictions, this balance will indeed converge to 0.0-1.0 or 1.0-0.0; but the point is that we are not put in the indefensible position of deciding between models on the basis of a single test.

Let $Cr(M_1)$ and $Cr(M_2)$ denote the credibilities of Model One and Model Two relative to each other. $Cr(M_1) + Cr(M_2) \overset{def}{=} 1.0$. Now Bayesians distinguish between prior and posterior credibility, the temporal reference being to a given consideration of new evidence--say, to the analysis of new experimental data. To distinguish between credibilities of the prior and posterior varieties, let us prime the latter. That is, let $Cr(M_i)$ and $Cr'(M_i)$ denote prior and posterior credibilities, respectively. Finally, let $Pr(D: M_i)$ denote the probability of the event that the focal data--the new evidence--will have the observed characteristics given the event that M_i is true. Then the relationship between prior and posterior credibilities can be expressed as follows:

$$Cr'(M_i) = \frac{Pr(D : M_i)Cr(M_i)}{Pr(D : M_1)Cr(M_1) + Pr(D : M_2)Cr(M_2)}, \quad (i=1,2) \qquad (3)$$

For a classical crucial experiment, $Cr(M_1) = Cr(M_2) = 1/2$; thus, the only problem in using this application of Bayes' Theorem is to compute $Pr(D : M_1)$ and $Pr(D : M_2)$.

Needless to say, computing exact probabilities requires exact knowledge of the pertinent distributions. In the present case, we would have to know the exact distribution of the response vector (n_s, n_c, n_w). We do not. One alternative would be to attribute to this vector a certain distribution and proceed from there. For instance, we could attribute to it a multinomial distribution--or the distribution arising from a specified summed Markov process. But this would have the disadvantage of confounding our calculations with a rather strong auxiliary assumption. There is a better way. Recall L_1 and L_2, defined in the preceding section. Both are asymptotically normally distributed, since both are functions of sample means (see Wilks, 1962; 259). Thus, whatever the response vector's distribution, these quantities computed from it have distributions that are more or less known. It is instructive to recall in this connection that, in the second stage of our analysis, we put to use the fact that L_1 and L_2 conform closely to the t-distribution with 76 degrees of freedom. Now, save far out in the tails, the t density function, given 76 degrees of freedom, is virtually indistinguishable from the normal density function with the same mean and variance. Their corresponding values might differ at, say, the tenth decimal place, but, for all practical purposes, they are the same. This confers plausibility upon the following result, although this result draws it primary justification from normal approximation theory:

$$f_i(x) \simeq \frac{1}{s_i} \phi \left[\frac{x - E(L_i)}{s_i} \right] ,$$

where f_i is the density function of L_i, ϕ is the standard normal density function, and s_i is the estimated standard deviation of L_i. Under M_1, $E(L_1) = 0$; and, under M_2, $E(L_2) \approx 0$. In the latter case, if we are willing to tolerate a slight bias in favor of the Camilleri et al. model, Model Two, we can assume that equality holds. Now it is clear that there is some connection between $f_i(x)$ and $Pr(D : M_i)$. Let D_a and D_b be two data-sets, and let x_a and x_b be corresponding values of L_i (i = 1 or 2, arbitrarily). Then it is intuitively clear that $f_i(x_a)/f_i(x_b) = Pr(D_a : M_i)/Pr(D_b : M_i)$. That is, the probability of getting the observed data-set is proportional to the height of the density function. In making comparisons across different normal density functions, however, it must be remembered that such functions differ in average height. For example, a normal density whose standard deviation equals ten will be much shorter than one whose standard deviation equals one. More precisely, if c_1 and c_2 are the upper .05 critical values of the respective distributions, then the former density at c_1 will be one-tenth as tall as the latter density at c_2. (The choice of .05 is, of course, immaterial.) So to make the comparisons valid, it is necessary to multiply each density by its standard deviation. The principle this leads to, in the present case, is the following:

$$\frac{Pr(D : M_1)}{Pr(D : M_2)} \approx \frac{\phi(x_1/s_1)}{\phi(s_2/s_2)} , \qquad (4)$$

where x_1 and x_2 are the observed values of L_1 and L_2. We might note that if Model One and Model Two were to "compete to a standoff" in the second stage of our analysis, then the right hand ratio would equal one; and this is certainly what we would expect the left hand ratio to equal in such a case.

Although the value of this ratio does not tell us the values of $Pr(D : M_1)$ and $PR(D : M_2)$, it does tell us all we need to know to use equation (3). Therefore, we have at our disposal a way to circumvent the seemingly intractable problem of computing the quantities necessary to use a Bayesian analysis.

It is instructive to apply equations (3) and (4) to a hypothetical case, as this gives some understanding of the relationship between the second and third stages of our analysis. Suppose that, in the planned comparisons of the second stage, one model were to mispredict its L (L_1 for Model One, L_2 for Model Two) badly enough that it could be rejected at the .05 level of significance; and suppose that the other model were to be right on target in predicting its L. Then the calculated probability ratio of equation (4) would be .2565 or 3.899, depending upon which model were which; and the posterior balance of credibilities would be .2-.8 or .8-.2, again depending upon which model were which. Thus, even in this relatively extreme case, we would be cautioned not to be more than eighty percent confident that the seemingly better model were in fact the model of greater promise. Perhaps the most important function served by a Bayesian calculation of the kind indicated here is to restrain the investigator from making either too little or too much of his results, to restrain him from weighing the evidence either too lightly or too heavily in assessing the relative merits of the opposing models.

This concludes a reasonably detailed description of the present research: its design, its experimental procedures, and its statistical procedures. This description has been merely preparation, of course, for what is still to come. In Chapter IV, to which we shall now turn, the actual data will be examined, the actual performance of each model will be scrutinized. Without further delay, let us now proceed to this climactic chapter.

CHAPTER IV

THE RESEARCH: RESULTS AND DISCUSSION

Preliminary Notes

The experiment, for the record, was completed on May 31, 1973. A total of forty-eight sessions had been conducted, in which ninety-five naive subjects and one confederate had taken part. Immediately following each post-session interview, the author decided whether the subject in question should be included in, or excluded from, the sample. Since we were aiming for ten subjects in each experimental condition, eighty subjects in all, it can be seen that, for one reason or another, fifteen subjects were excluded. Before analyzing the data, it might be well to indicate the criteria upon which judgments to exclude subjects were based.

First, a subject was excluded if he volunteered being suspicious about either the part one competence-status manipulation or the part two initial choice disagreements--that is, about the fictitious aspects of the experiment. Eight persons fell into this category, all of whom had had prior experience with studies they perceived to be similar to the present one. The exclusion of these subjects' data seemed advisable, since, strictly speaking, these persons were not naive subjects.

Second, a subject was excluded if his frame of reference, or definition of the situation, proved to be markedly atypical. In seven cases, the author decided that the persons in question just did not meet the assumptions that are necessary if the data analysis is to be valid. This requires some explanation. Both models, as we know, are utility formulations. Each asserts that a subject's behavior will depend upon what he values and disvalues, upon what has utility for him. Now, in an empirical test such as the present one, it does not matter what a subject values or disvalues--our basic hypotheses are completely general in this respect. But it matters very much, indeed it is crucial, that a subject's orientation, determined by what he values and disvalues, be shared with his fellow subjects. Whenever group data are used to test an explicit utility formulation, homogeneity of orientation, both within and across groups, is an inescapable condition of the analysis. If a subject has a clearly atypical orientation, then he simply does not meet this essential condition. In the present case, it was posited that the focal actor values the approval of his partner, and that he perceives his partner's ability as differing from his own. It will be recalled that there were other assumptions of the social influence application as well, but these two are relatively easy to check in conjunction with the post-session interview. In three cases, subjects were eliminated because they thought the part one test was "pure luck." (In fact, these subjects were correct, albeit not in quite the way they imagined: their scores came about with the help of a random number table.) In the other four cases, there was serious doubt about whether the persons in question placed any value whatsoever upon their partners' approval. In these cases, there is obviously a fine line between one decision and the other; yet such judgments must be, and were, made.

Our paramount concern, of course, was that these judgments not affect the data preferentially in regard to the two models. To this end, these judgments were made before the data were analyzed--in most cases, weeks before the graph paper of the Esterline Angus event recorder, which unwinds and winds up again like a typewriter ribbon, was even unwound. This should have ensured that there would be no systematic benefit to either model relative to the other; however, just to make sure, the author did recompute the chi-square values to be presented shortly with the suspect data included. It turned out that, as might have been anticipated, neither model fitted quite as well as it did without the suspect data thrown in; but the worse fitting model lost additional ground to the better fitting model, dispelling any lingering suspicion that the better fitting model might owe its better fit to the selection of the subject groups.

We mentioned earlier that the goal was to have exactly ten subjects in each experimental condition. This requires only very brief comment. The number ten was based mainly on practical considerations. Each additional subject per condition would have required a minimum of four additional experiments. Life being short and these experimental sessions being quite lengthy, ten seemed like an appropriate number. As we know, the power of the F and t tests goes up as the square root of the sample size. The desire for *equal* sample sizes is much less arbitrary: although the robustness of the statistical procedures described in Chapter III is highly gratifying when the sample sizes are equal, it attenuates rapidly when they are not. After discussing such considerations, Hays offers the following succinct advice: ". . . *use samples of the same size*" (Hays, 1963: 322, italics in the original). Although one hesitates to let methodological considerations dominate his thinking, to dismiss them too lightly is to seriously undermine the integrity of the research process.

The issues we have been discussing are not often discussed in research reports. Lest the reader be given the impression that these matters were more troublesome in the present research than in most other research, we wish to reassure him that they were not. In a study of this type that is representative, thirty-four percent of the data were eliminated (Berger *et al.*, 1969: 491). For all reasons combined, sixteen percent were eliminated in the present study, including at least one from each of the eight conditions and no more than three from any one condition.

The eighty observed values of the response vector are presented in Table 3.

The Models' Parallel Implications

Our initial concern, as indicated in Chapter III, was with whether either model adequately accounts for the gross features of the data, whether the gross, common "ordering implications" of the two models are borne out. Based on the models, we would expect, first of all, that subjects in the (H,L) conditions would change less often than subjects in the corresponding (L,H) conditions. Second, we would expect that subjects in the two-alternative conditions would change more often than subjects in the corresponding three-alternative conditions. Third, we would expect that subjects in the (H,L) conditions would stay more often than subjects in the corresponding (L,H) conditions. And fourth, we would expect that subjects in the two-alternative conditions would stay more often than subjects in the corresponding three-alternative conditions. These four predictions are implications of both models. In each case, we tested the null hypothesis, the hypothesis of no difference, and in each case we had to reject this hypothesis, thus accepting as confirmed the predictions of the two models.

Table 3. Observed values of the response vector (n_s, n_c, n_w) by condition.

	2 alternatives		3 alternatives	
	men	women	men	women
(H,L)	(20,05,00)	(20,05,00)	(18,03,04)	(15,07,03)
	(19,06,00)	(21,04,00)	(19,03,03)	(21,01,03)
	(20,05,00)	(19,06,00)	(21,04,00)	(23,02,00)
	(18,07,00)	(23,02,00)	(10,08,07)	(12,07,06)
	(21,04,00)	(18,07,00)	(16,03,06)	(21,04,00)
	(21,04,00)	(22,03,00)	(21,03,01)	(15,04,06)
	(19,06,00)	(20,05,00)	(20,05,00)	(21,03,01)
	(17,08,00)	(16,09,00)	(21,02,02)	(17,04,04)
	(16,09,00)	(22,03,00)	(13,06,06)	(09,10,06)
	(22,03,00)	(20,05,00)	(21,00,04)	(15,05,05)
(L,H)	(14,11,00)	(15,10,00)	(07,13,05)	(04,11,10)
	(06,19,00)	(06,19,00)	(03,19,03)	(09,14,02)
	(05,20,00)	(04,21,00)	(11,08,06)	(09,15,01)
	(10,15,00)	(10,15,00)	(05,18,02)	(05,20,00)
	(15,10,00)	(07,18,00)	(12,11,02)	(03,16,06)
	(16,09,00)	(09,16,00)	(01,13,11)	(01,21,03)
	(07,18,00)	(07,18,00)	(08,12,05)	(05,17,03)
	(02,23,00)	(12,13,00)	(06,11,08)	(06,09,10)
	(12,13,00)	(08,17,00)	(00,24,01)	(07,10,08)
	(01,24,00)	(09,16,00)	(05,13,07)	(11,10,04)

The ANOVA summaries are presented in Table 4 and Table 5. The effect of competence-status is significant far beyond the normally reported levels; and the "number of alternatives" effect, although less potent, is still clearly present.

The various questions about "the nature of the givens" raised in Chapter III also appear to be answered. Sex does not appear to make any difference, and there does not appear to be any interaction among the main effects. Indeed, the F ratios for sex and the interactions are lower than might be expected by chance alone. As an aside, the direct meaning of a low F ratio is that the mean squared error is larger than would be expected on the basis of the between group comparisons. The substantive implication is that there may have been some unanticipated variance causing factor whose effect was somehow equalized across groups. Time of day is one possibility. Another is time of the academic term--perhaps the pressures of pending exams and the like would exert some effect. As with time of day, this would have been more or less equalized across groups but would have been variable within groups, thus increasing the mean squared error disproportionately. There may be other possibilities as well; however, what is less speculative is that the predicted effects did occur, whereas the sex and interaction effects did not occur. This suggests that social influence, as a phenomenon, probably has a relatively simple

Table 4. ANOVA of the change-response component.

Source	SS	df	MS	F
(A) Competence-Status	2205.000	1	2205.000	177.46**
(B) # of Alternatives	48.050	1	48.050	3.87*
(C) Sex	0.200	1	0.200	0.02
AxB	4.050	1	4.050	0.33
AxC	0.000	1	0.000	0.00
BxC	4.050	1	4.050	0.33
AxBxC	4.050	1	4.050	0.33
Error	894.600	72	12.425	
TOTAL	3160.000	79		

* Significant at the .10 level ($p \simeq .06$)
** Significant at the .001 level

Table 5. ANOVA of the stay-response component.

Source	SS	df	MS	F
(A) Competence-Status	2531.250	1	2531.250	189.61**
(B) # of Alternatives	130.050	1	130.050	9.74*
(C) Sex	0.050	1	0.050	0.00
AxB	1.800	1	1.800	0.13
AxC	0.200	1	0.200	0.01
BxC	3.200	1	3.200	0.24
AxBxC	6.050	1	6.050	0.45
Error	961.200	72	13.350	
TOTAL	3633.800	79		

* Significant at the .005 level
** Significant at the .001 level

theoretical structure with some remarkable invariant properties. It is clear that the well recognized socialization differences between males and females in contemporary American society have little or no effect upon the influence phenomenon; and this makes it at least tenable that social influence may have some other "socialization invariant" properties as well.

The fact that the models' parallel predictions are borne out lends a certain amount of credibility to each model; however, it does nothing to distinguish between the two models. Let us now turn to some findings that bear upon the relative merits of the two models.

The Models' Divergent Implications

This brings us to what was described in Chapter III as the second stage of our analysis. Before performing the calculations described there, however, it is instructive to examine the observed response proportions by experimental condition. These proportions are presented in Table 6. Since sex was apparently not an important factor in the experiment, we have collapsed the eight experimental conditions down to just four, combining the comparable male and female groups. The symbol "(H,L)-2" refers to the focal actor having high competence-status, his partner having low competence-status, and there being two alternatives. The similar symbols have analogous meanings. Now, the estimated

Table 6. Observed response proportions by experimental condition.

	Condition 1 (H,L)-2	Condition 2 (H,L)-3	Condition 3 (L,H)-2	Condition 4 (L,H)-3
n_s/N	.788	.698	.350	.236
n_c/N	.212	.168	.650	.570
n_w/N	---	.134	---	.194

sizes of the competence-status effect and the "number of alternatives" effect can be determined from this table. To see the former, compare Condition 1 with Condition 3, Condition 2 with Condition 4. To see the latter, compare Condition 1 with Condition 2, Condition 3 with Condition 4.

At this point, let us consider the relative fits of the two models. The most easily interpreted measure of fit, on the face of it, is the chi-square goodness-of-fit value. Estimates of the expected cell frequencies under each model must be computed separately for the (H,L) and (L,H) conditions. Although chi-square values and degrees of freedom are additive, it is more instructive to see the two values for each model uncombined. After one degree of freedom is subtracted for each linear constraint on the data (the cell frequencies for each condition must add to 500) and for each parameter estimated (there must be two parameters estimated in each case), we are left with one degree of freedom in each case. Summaries of the calculations are presented in Table 7. It is found that both models fit quite well

Table 7. Chi-square goodness-of-fit values.

	(H,L) Conditions	(L,H) Conditions
Model 1	0.8968	0.3728
Model 2	0.4643	3.3290*

*Significant at the .10 level ($p \approx .07$)

in the (H,L) conditions--the empirical frequencies fall between the models' predictions--but that the Siegel et al. model fits much better than the Camilleri et al. model in the (L,H) conditions. Although the one departure from the Camilleri et al. model's prediction appears to be significant at the .10 level, this should not be accepted uncritically, for the goodness-of-fit test assumes that the underlying distribution is multinomial and that there is perfect homogeneity of the parameter for each cell. At best, this is only approximately true in the present case (for a good theoretical discussion of this test, see Hoel et al., 1971: 91-99). Therefore, the precise level of significance should be viewed with a healthy skepticism, although the chi-square values are nonetheless comparable with each other.

Let us now consider the planned comparisons described in Chapter III. It will be recalled that these comparisons were based on divergent inequalities derived from the two models. These comparisons can be regarded as tests of the models' "ordering implications," but these ordering implications are much more trenchant than those considered in the preceding section. They focus *all* the data from *all* the experimental conditions upon the prime area of disagreement between the two models; therefore, if a coup de grace is possible, these comparisons are the ones to provide it.

Carrying out the calculations, we arrive at the results that are summarized in Table 8. The Siegel *et al*. model appears to account for the experimental data very well

Table 8. Theoretical and observed values of L_1 and L_2.

	Theoretical Value	Other Model's Implication	Observed Value	
Model 1	$L_1 = 0$	$L_1 > 0$	0.1500	0.00
Model 2	$L_2 \leq 0$	$L_2 > 0$	1.6145	1.02*

*$p \simeq .15$

indeed; the Camilleri *et al*. model appears to account for the data somewhat less well. To compute t in the latter case, we took the liberty of assuming that $E(L_2) = 0$, a conservative assumption to be sure. It is found that neither model is rejectable at the customary levels of significance.

What to conclude? That is the question.

An A Posteriori Assessment

The objective of this dissertation, as stated in Chapter I, is, at the broad level, to shed as much light as possible on whether man is a "balancing" or a "utility maximizing" organism. How does he combine and use the information available to him? Does he balance his choices vis-a-vis the gains from the respective alternatives, or does he select a strategy so as to maximize his total gain? At the more narrow level, the objective is to assess the relative merits of two mathematical models representing these respective principles. These abstract models of behavioral decision making have now been applied to a concrete situation, empirical evidence about this situation has been gathered, and this evidence has been analyzed. At this time, it is incumbent upon us to extract a conclusion, to summarize the meaning of what we have found for the questions with which we began, to distill from our work its significance.

Let us carry out the Bayesian computation summarized by equations (3) and (4) of Chapter III. This computation goes as follows. Let PR denote the probability ratio of equation (4). Then

$$PR = \frac{\phi(0.10)}{\phi(1.02)} = \frac{.3970}{.2371} = 1.674;$$

$$Cr'(M_1) = \frac{(1.674)\ (1/2)}{(1.674)\ (1/2) + (1.000)\ (1/2)} = 0.626; \text{ and}$$

$$Cr'(M_2) = 1 - Cr'(M_1) = 0.374.$$

According to this computation, the odds, which were assumed to be even prior to this research, are now five to three in favor of the Siegel _et al_. model. On the basis of the evidence now available, the Siegel _et al_. model must be considered more credible than the Camilleri _et al_. model. Correspondingly, the utility maximizing principle must be considered more credible than the balancing principle.

To put this conclusion in perspective, however, let us ask what change in status would occur if this study were to be repeated, and if the result the second time were to be (1) exactly the same, or (2) exactly the reverse. In the first case, we would have

$$Cr''(M_1) = \frac{(1.674)(.626)}{(1.624)(.626) + (1.000)\ (.374)} = 0.737; \text{ and}$$

$$Cr''(M_2) = 1 - Cr''(M_1) = 0.263.$$

Thus, the odds would become roughly eight to three in favor of the Siegel _et al_. model. In the second case, PR would equal .597, and we would have

$$Cr''(M_1) = \frac{(0.597)(.626)}{(0.597)(.626) + (1.000)(.374)} = 0.500; \text{ and}$$

$$Cr''(M_2) = 1 - Cr''(M_1) = 0.500.$$

Thus, the odds would become even again. These computations underscore the fact that our conclusions, like all scientific conclusions, are tentative and are always subject to revision on the basis of further evidence.

It is perhaps appropriate at this time to indicate a few of the directions that further research could take. The most obvious one, of course, is the one taken by the present research. Although the reward structure of sociology does not favor replicational research, there would nevertheless be clear *scientific* value in repeating the present study. Perhaps one of Camilleri's graduate students looking for an MA project could take on such a task. This would give a clearer idea of whether the trends present in our data are stable, thus making it clearer whether other directions are required. We are thinking in terms of the Camilleri *et al*. model at this point, since, as things now stand, the Siegel *et al*. model is relatively unscathed. If it became clear that the trends apparent in our data are stable, another possible direction would be to investigate the possibilities of "loosening up" some of the assumptions of the present analysis, most notably the assumption that the utilities u_s, u_o, and u_t remain constant when a third alternative is added to the choice-set. This line of inquiry produced, in a related context, the best showing of the Camilleri *et al*. model to date (see Balkwell, 1969). A third possible direction would be to consider applications of one or both models to phenomena other than social influence. Although the social influence application has considerable sociological significance, it is not clear that it is the most representative application of behavioral decision making theory. And although it has been suggested that ". . . all decision making is guided by an abstract set of axioms which vary only in the manner in which they are operationalized in specified situations . . ." (Ofshe and Ofshe, 1969: 337), this is hardly a proven fact. Perhaps the Siegel et al. model is more valid for some kinds of decision making, the Camilleri et al. model for others. Perhaps different axioms apply to social choice behavior than to non-social choice behavior. Perhaps there are as yet unknown conditioning factors that determine what set of axioms will be operative, even within well defined classes of decision making situations. The possibilities are almost endless, and no one knows many of the answers. But, through research, it should be possible to find some of them. Research combined with hard theoretical work, that is.

In conclusion, let us merely resubmit the question around which this entire dissertation, our entire theoretical and empirical effort, has been structured: Does the Camilleri et al. model or the Siegel et al. model more adequately account for observable social influence decisions? Accordingly, are such decisions more adequately accounted for by a balancing principle or by a utility maximizing principle? On the basis of present evidence, the Siegel et al. model and the utility maximizing principle are the better bet. The odds are five to three.

CHAPTER V

EPILOGUE ON POST-1971 DEVELOPMENTS

Although references have been updated and a few of the arguments have been refined in minor ways, the theoretical analysis of this dissertation is essentially as it was in December, 1971. A finished dissertation proposal was distributed to the Faculty in early 1972, and that proposal was officially approved on February 29, 1972. Chapters one through three of the present document are practically word for word from that proposal. Twenty months have now passed, and, during this period, the author has done some additional theoretical work that is sufficiently important and germane to warrant inclusion in some form or other. We have chosen to present this work in an epilogue. Essentially, we have developed two new models of behavioral decision making; and the implications of these put both the accomplishments and the limitations of our previous work in a truer light. Moreover, they raise questions that must be faced by the present author, by Camilleri, by Siegel, and by anyone else who wishes to do future creditable work in this area.

Let us begin by reconsidering the distinction between "balance" and "utility maximization," first discussed in Chapter I.

The Demise of a Distinction

A major enphasis of the original dissertation proposal was that the proposed research would shed light upon whether man strives for balance or for utility maximization when he make choices. It is now clear that the latter principle is much more general than the former, and, indeed, that it includes the former as a special case. To demonstrate this, it suffices to show that the balancing notion can be derived from a utility maximizing formulation. Before such a demonstration is made, however, it should be emphasized that this does not in any way undermine our previous work insofar as that work is understood as a comparison of the Siegel et al. and Camilleri et al. models. It only undermines the more abstract comparison of balancing and utility maximization as separate basic principles.

The first model to be presented, which we will call Model A, assumes a repetitive choice situation like that of a typical decision making experiment. Our concern, as in previous chapters, is with a focal actor's stable-state choice strategy. We assume a choice-set $A = \{A_1, A_2, \ldots, A_m\}$ and N trials. Model A has five axioms which can be stated as follows.

AXIOM A1: $T = G(1) + G(2) + \ldots + G(N) + GV$, where T is the focal actor's total gain, G(t) is his gain from the t th decision, and GV is his gain from variability.

AXIOM A2: $G_k = g_k + Q_m$, where G_k is the expected positive utility of alternative A_k, and where g_k and Q_m are as given by the auxiliary definitions of Chapter II.

AXIOM A3: $G(t) = \log(G_k)$ iff $A_k \varepsilon A$ is selected on the $\underline{t}$ th trial.

AXIOM A4: $GV = (1/b)[N \log(N) - \Sigma n_i \log(n_i)]$, where the summation is over $i=1,2, \ldots, m$, n_i is the number of times out of N that A_i is selected, and b is a scale factor.

AXIOM A5: Let $n = (n_1,n_2, \ldots,n_m)$ be the vector of frequencies with which the alternatives $A_1,A_2, \ldots,A_m$ are selected. Then $E(n) = n^*$, where $n^* = (n_1^*,n_2^*, \ldots,n_m^*)$ is the unique solution to the m-1 linearly independent equations in m-1 unknowns determined by

$$\frac{\partial T}{\partial n_i} = 0, \; i=1,2, \ldots,m; \text{ and } \sum_{i=1}^{m} n_i = N.$$

Although it is generally agreed that the axioms of a scientific system do not require intuitive justification, we would like to point out that Axiom A4 admits of an interesting rationale. The function given by the axiom is a variant of the "uncertainty" function of information theory; and its verbal translation can be put as follows: the focal actor's gain from variability is proportional to the uncertainty of his choices to an observer. That is, what an onlooker experiences as uncertainty is experienced by the actor himself as variability, as the absence of monotony. This function and rationale should not be confused with the corresponding function and rationale of the Siegel et al. model, although the broad notion that the focal actor values variability is the same.

Before proceeding, let us note two things. First, Model A is a utility maximizing model by virtue of Axiom A5. It can be shown that the solution to the equations indicated by the axiom does in fact maximize T. Second, if p is the focal actor's strategy, then p = (1/N)n*; that is, for i=1,2, . . .,m, $Pr(A_i) = (1/N)n_i^*$. Drawing this connection is equating p with the vector of expected relative frequencies, which is congruent with the usual conception of a strategy found in the decision making literature. Now let us turn our attention to deriving the balancing principle from Model A.

For convenience, let T_i be the subtotal of T that results from selecting alternative A_i, (i=1,2, . . .,m). Then

$$T_i = \log(G_i) + \log(G_i) + \ldots + \log(G_i); \quad [n_i \text{ terms}]$$

$$= n_i \log(G_i). \text{ T itself is as follows.}$$

$$T = \sum_{i=1}^{m} T_i + GV = \sum_{i=1}^{m} n_i \log(G_i) + GV;$$

$$= \sum_{i=1}^{m} n_i \log(G_i) + (1/b)[N \log(N) - \sum_{i=1}^{m} n_i \log(n_i)]$$

This is Model A's utility function. The next step is to maximize it and thereby determine the vector n*. Unfortunately, the usual calculus techniques for maximizing a function cannot be directly applied here, because we have only m-1 independent variables, due to the restriction that $\Sigma n_i = N$. This problem can be circumvented by algebraically eliminating an arbitrary variable or, equivalently, by introducing a Lagrange multiplier λ and then applying the usual techniques to the Lagrangian function, which is

$$L = \sum_{i=1}^{m} n_i \log(G_i) + (1/b)[N \log(N) - \sum_{i=1}^{m} n_i \log(n_i)] + \lambda \left(\sum_{i=1}^{m} n_i - N\right).$$

The unrestricted maximum of L will coincide with the restricted maximum of T; therefore, it suffices to find the former. Differentiating L with respect to each n_i and setting the partial derivative equal to zero, we get the following:

$$0 = \frac{\partial L}{\partial n_i} = \log(G_i) - \frac{1}{b}[1 + \log(n_i)] + \lambda, \quad (i=1,2, \ldots, m).$$

This holds for each n_i; therefore, for arbitrary i and k, $\log(G_i) - (1/b)\log(n_i) = \log(G_k) - (1/b)\log(n_k)$. This is equivalent to $n_k(G_i)^b = n_i(G_k)^b$. Holding k constant and summing with respect to i yields $n_k \sum_{i=1}^{m} (G_i)^b = N(G_k)^b$. Therefore, $n_k^* = N(G_k)^b / \sum_{i=1}^{m} (G_i)^b$. Now, since $Pr(A_k) = (1/N)n_k^*$, we arrive at the following:

THM SCHEMA A1: $$Pr(A_k) = \frac{(G_k)^b}{\sum_{i=1}^{m} (G_k)^b}, \quad (k=1,2, \ldots, m).$$

Notice that, in the special case of b = 1, Theorem Schema A1 is the balancing principle; and this theorem schema has been derived from a utility maximizing formulation.

To summarize this first point, then, the distinction between a "balancing" and a "utility maximizing" principle is not a distinction between separate basic principles. Balance is a special case of utility maximization. Whether the other "balance" concepts employed in social science are similarly reduceable remains to be seen, but the feasibility of this merits exploration.

There is also a second point that emerges from considering Model A. Note that, from Axiom A2 and Theorem Schema A1, the following straightforward consequence can be obtained.

THM SCHEMA A2: $$\Pr(A_k) = \frac{(g_k+Q_m)^b}{\sum_{i=1}^{m} (g_i+Q_m)^b}, \quad (k=1,2,\ .\ .\ .,m).$$

If this theorem schema is compared to Theorem Schema C1, presented in Chapter II, the following conclusion seems inescapable: there is no conceivable evidence that could refute Model A while, at the same time, confirming the Camilleri et al. model; however, evidence that would refute the Camilleri et al. model would not necessarily refute Model A. Model A, therefore, subsumes the Camilleri et al. model.

The relationship between Model A and the Camilleri et al. model is most noteworthy, however, in regard to their respective highest-level hypotheses. Model A yields as a *theorem schema* the Camilleri *et al.* model's so-called "basic postulate" (Camilleri *et al.*, 1972: 25). In other words, Model A yields as a derivation what the Camilleri *et al.* model merely asserts--viz., the balancing principle.

An Alternative to the Siegel et al. Model

The results of our research, which were reported in Chapter IV, clearly lend credibility to the Siegel *et al.* model. This research provided a stiffer test than others that have been reported in the literature, and the Siegel *et al.* model met this test quite satisfactorily. This situation is clouded somewhat, however, by the fact that there is a model, developed by the author, that is very different from the Siegel *et al.* model, that contradicts the Siegel *et al.* model on substantive points of some importance, but that implies the same empirical consequences with respect to our study.

The assumptions of this model, which we shall call Model B, are the same as those of Model A. Model B has four axioms.

AXIOM B1: $T = G(L) + G(2) + \ldots + G(N)$, where T is the focal actor's total gain, and G(t) is his gain from the *t* th decision.

AXIOM B2: $G(t) = (1-d)^r \exp(g_k)$ iff $A_k \in A$ is selected on the *t* th trial and A_k has been selected exactly r times prior to the *t* th trial. The quantity d is the discount rate.

AXIOM B3: $d = 1 - e^{-b/N}$, where b is an empirical parameter. Notice that $d \to 0$ as $N \to \infty$; and $d \to 1$ as $N \to 0$.

AXIOM B4: Let $n = (n_1, n_2, \ldots, n_m)$ be the vector of frequencies with which the respective alternatives are selected. Then $E(n) = n^*$, where n^* is the unique solution to the m-1 linearly independent equations in m-1 unknowns determined by

$$\frac{\partial T}{\partial n_i} = 0,\ i=1,2,\ldots,m;\ \text{and} \sum_{i=1}^{m} n_i = N.$$

A closed form expression of $Pr(A_k)$ can be derived from these axioms, the derivation being similar to the one presented in the preceding section. To ease the notational burden somewhat, let $E_i = \exp(g_i)$; also, let T_i be the subtotal of T that results from selecting A_i, $(i=1,2,\ldots,m)$. Then

$$T_i = E_i + (1-d)E_i + (1-d)^2 E_i + \ldots + (1-d)^{n_i - 1} E_i;$$

$$= (1/d)[1 - (1-d)^{n_i}]E_i.$$ T itself, then, is this:

$$T = \sum_{i=1}^{m} T_i = (1/d) \sum_{i=1}^{m} E_i - (1/d) \sum_{i=1}^{m} (1-d)^{n_i} E_i.$$

It is clear that T attains its maximun when the vector n is such that $\Sigma\ (1-d)^{n_i}E_i$ is minimized. Let us define the Lagrangian function

$$L = \sum_{i=1}^{m} (1-d)^{n_i}E_i + \lambda(\sum_{i=1}^{m} n_i - N).$$

It can be shown that the unrestricted minimum of L coincides with the restricted maximum of T; therefore, it suffices to find the former. Carrying out the usual procedure, we get

$$0 = \frac{\partial L}{\partial n_i} = (1-d)^{n_i}E_i \log(1-d) + \lambda\ ,\quad (i=1,2,\ldots,m).$$

This holds for arbitrary n_i; hence, for any i and k,

$(1-d)^{n_i}E_i = (1-d)^{n_k} E_k$; and taking logarithms of both sides,

$n_i \log(1-d) + g_i = n_k \log(1-d) + g_k$. Holding k constant and summing both sides with respect to i yields

$N \log(1-d) + \sum_{i=1}^{m} g_i = mn_k \log(1-d) + mg_k$. Solving for n_k and, by Axiom B3, substituting $1 - e^{-b/N}$ for d, we get

$n_k^* = N/m + (N/mb)(mg_k - \sum_{i=1}^{m} g_i)$, $(k=1,2, . . ., m)$. Dividing through by N brings us home.

THM SCHEMA B1: $\Pr(A_k) = \frac{1}{m} + \frac{mg_k - \sum_{i=1}^{m} g_i}{b\ m}$, $(k=1,2, . . ., m)$.

Now this is an interesting result. If it is compared to Theorem Schema S1, presented in Chapter II, it can be seen that the two are alike except for the form of an empirical constant. This minor difference makes no difference in the derivation of the "basic hypotheses," presented at the end of Chapter II; and this means that the results of our research support Model B to exactly the same degree that they support the Siegel _et al_. model.

This fact is not trivial. The two models are based upon very different substantive ideas. For example, according to Model B, as the number of times the focal actor chooses a given alternative increases, the marginal utility of that alternative decreases; but, according to the Siegel _et al_. model, the marginal utility of each alternative is constant. The two models also entail quite different conceptions of the utility function--in fact, they share only one thing in common: by virtue of Axiom S3 and Axiom B4, they are both utility maximizing models.

The full implications of Model B's existence have not been explored at this time. Quite clearly, however, it clouds the interpretation of the present research, to say nothing of the interpretations of several previous investigations.

Conclusion

In this chapter, we have presented two new models of behavioral decision making. These models unsettle some things that seemed to have been settled when the author's dissertation proposal was accepted by the Faculty. Originally, we proposed a comparison between the Camilleri *et al*. model and the Siegel *et al*. model, and this comparison was carried out as planned. But we suggested that this was to be more than just a comparison between two particular models: it was to be a comparison between "balancing" and "utility maximizing" as basic principles of human choice behavior. We need not have done any empirical research to settle this--the formal argument presented in this chapter vitiates the assumed distinction. Our Model A is a utility maximizing model from which the balancing principle can be derived.

The development of Model A opened Pandora's box. Might there not also be other models with interesting implications? Pursuing a hunch, we developed a second model,

Model B, that implies exactly the same empirical implications--at least insofar as the research of this dissertation is concerned--as the Siegel *et al*. model. That this muddies the water need not be labored.

All in all, what are we to conclude from the work of this chapter? We have, of course, presented two new models of behavioral decision making, one of which subsumes the Camilleri *et al*. model as a special case. Both Model A and Model B merit continued investigation, both theoretical and empirical. But this chapter also carries a broader and deeper implication: new standards of discrimination are required if further scientific progress is to be possible. Studies designed to test a single model of behavioral decision making no longer meet important scientific needs. That is to say, research not bearing upon *conflicting* predictions of at least two credible models can add nothing but chaff to the fund of knowledge. Such research should be discouraged. What we need, now more than ever, is research focusing on matters where one credible model or theory takes issue with another; and research results must be judged by the degree to which they alter the balance of credibility. *This*, we submit, is the broader and deeper implication of the present chapter.

APPENDICES

APPENDIX A

THE EXPERIMENTAL MATERIALS

Part One Instructions: All Conditions

First of all, let me introduce myself. I'm Jim Balkwell, and I'll be your host in today's study. On behalf of the sociologists who are conducting this study, I'd like to thank you for joining us. We think you'll find this to be an interesting as well as a rewarding experience.

What we're doing is studying the ways persons solve certain kinds of problems. More precisely, we're studying the ways persons solve those problems under two different conditions. In keeping with this, our work today will be divided into two separate parts, which we call "phase 1" and "phase 2". In each part, you'll be asked to solve problems, but the conditions under which you'll work will differ. I'll explain more about both the problems and the conditions as we go along. We'll now turn to phase 1 of our work.

Sociologists have known for many years that the ability of persons to accurately perceive the relationships within figures or patterns is quite limited. It's known, for example, that two figures or patterns can differ by as much as five percent without this being noticeable to most persons. Differences this small simply can't be seen by most persons. Recently, though, it's been discovered that the variation among persons in this ability is much greater than we once thought. It has become clear that there are persons who can detect differences that are very slight eighty or ninety percent of the time. We call the ability to correctly distinguish two figures or patterns that differ slightly "contrast sensitivity."

As of now, no one knows for sure why some persons have more contrast sensitivity than others. Dozens of studies have been done, and it's been found that contrast sensitivity is completely unrelated to such things as artistic ability, mathematical ability, and general intelligence. You might think that it would be related to one or more of these other abilities; however, it doesn't seem to be. In spite of that, though, there are certain sociological reasons why we want to learn more about it.

What we're going to do today, in phase 1, is administer a test that measures this ability. The test has been given to thousands of college students in the past and is known to be a very reliable, valid test of how much contrast sensitivity a person has.

The test has twenty items, and for each item we'll proceed as follows: I'll present to you, on the screen in in front, a slide containing a contrast sensitivity problem.

(PUT DEMONSTRATION SLIDE #1 ON THE SCREEN.) Your job will be to figure out which rectangle, top or bottom, is slightly more black than white. On each item, one rectangle will be exactly fifty percent black and fifty percent white; the other will be fifty-three percent black and forty-seven percent white. So let me repeat: your job will be to figure out which rectangle, top or bottom, is the one that's slightly more black than white. For about half the items, the correct answer is "top", and, for the rest, the correct answer is "bottom"; and I should add that the slides were put into the projector in a random order. After presenting a slide such as this, I'll give you five seconds to study it. Then I'll ask you to indicate your <u>initial choice</u>, which you'll do by pressing either the button labeled "top" or the button labeled "bottom" in the upper row of buttons on your panel, just below the words "initial choice." (REMOVE SLIDE.)

When you've made your initial choice, it'll be registered on my panel back here. I'll compare your answer with the correct one, which I have here, and then give you feedback on whether you were right or wrong. I'll also keep track of your score.

Since there will be twenty rounds in all, you'll be asked to make twenty initial choices. Needless to say, practically everyone finds these choices difficult, since the two rectangles are so nearly alike. Still, our previous studies have shown that some persons can make correct decisions on the basis of slight, maybe even subconscious, cues and feelings.

When the test is over, I'll tell you your score. To help you interpret that score, we've prepared the standards that you can see on the board. Those standards are based upon previous studies in which more than one-thousand college students participated. As you can see, a score of 15-16 correct is high average, and a score of 13-14 correct is low average. The most common score seems to be 14 or 15. A score of 17-20 correct is relatively rare, representing very high ability; and a score of 0-12 correct is also relatively rare, representing little or no ability.

What I'd like to do at this point, before we start the actual test, is go through a practice round to get you used to our procedure. This round won't count toward your score. The way it works is like this: first, I'll present a slide containing a contrast sensitivity problem. Second, after five seconds, I'll ask you to choose the rectangle, top or bottom, that, in your judgment, is slightly more black than white. You'll indicate your choice by pressing the appropriate button by the words "initial choice."

(1) This is Demonstration Slide #2.

(PUT SLIDE ON, ALLOW FIVE SECONDS.)

(2) All right, now make your initial choice.

(WAIT UNTIL S's HAVE CHOSEN, THEN REMOVE SLIDE.)

Your choices are now registered on my panel. Number One chose _____, and Number Two chose _____. During the actual test, as soon as I've announced and recorded your scores, I'll press this button on my panel (CLEAR PANELS), and the lights on all the panels will go off. Then we'll be ready for the next round, and, as I said, there'll be twenty rounds in all.

During the test, you shouldn't communicate with each other in any way. Otherwise, the scores won't be valid. Is everything clear? All right, now we'll begin the test.

(1) This is slide number _____.

(PUT SLIDE ON, ALLOW FIVE SECONDS.)

(2) All right, now make your initial choice,

(WAIT UNTIL S's HAVE CHOSEN.)

(3) Number One is _____; Number Two is _____.

(REMOVE SLIDE, CLEAR PANELS.)

CONTINUE FOR TWENTY TRIALS ACCORDING TO THE FOLLOWING SCHEDULE, WHERE THE HIGH AND LOW SCORES ARE ASSIGNED BY A RANDOM PROCEDURE.

SLIDE NUMBER	HIGH SUBJECT	LOW SUBJECT
1	RIGHT	RIGHT
2	RIGHT	WRONG
3	RIGHT	WRONG
4	WRONG	RIGHT
5	RIGHT	WRONG
6	RIGHT	RIGHT
7	RIGHT	WRONG
8	WRONG	RIGHT

9	RIGHT	RIGHT
10	RIGHT	WRONG
11	RIGHT	WRONG
12	RIGHT	RIGHT
13	RIGHT	WRONG
14	RIGHT	WRONG
15	WRONG	RIGHT
16	RIGHT	WRONG
17	RIGHT	RIGHT
18	RIGHT	WRONG
19	RIGHT	WRONG
20	RIGHT	WRONG

The test is now completed. I'll add up your scores and, as soon as I finish, I'll report them to you.

(WAIT TEN SECONDS, THEN ENTER THE EXPERIMENTAL ROOM AND FINISH THE REMAINDER OF THIS SCRIPT BY MEMORY.)

Let's see, Number One got _____ correct and _____ incorrect; and Number Two got _____ correct and _____ incorrect. (WRITE SCORES ON THE BLACKBOARD.) You can get some idea of how you did by comparing your scores with the standards here. This is a little bit unusual.

Well, what I would like for you to do now is fill out a short questionnaire, which I'll now pass out.

(PASS OUT THE PART ONE QUESTIONNAIRE; THEN TAKE DOWN THE INDIVIDUAL SCORING STANDARDS, REVEALING THE CHART THAT RELATES THE TEAM SCORE TO EACH PERSON'S PAY.)

(COLLECT THE PART ONE QUESTIONNAIRE AND RETURN TO THE CONTROL ROOM.)

Part Two Instructions: Conditions 1 and 3

(TURN ON INITIAL CHOICE FEEDBACK.) We're now ready to begin Phase 2 of our work. As you may possibly know, psychologists are primarily interested in studying individual behavior. Sociologists, on the other hand, are primarily interested in studying behavior as it relates to a group situation. In terms of this distinction, Phase 1 would be considered the more psychological part of our work today, and Phase 2 would be considered the more sociological part.

In Phase 2, to make a long story short, we'll be interested in your ability at teamwork. Accordingly, we're going to allow you to exchange information before making your final choices. Let's see how this works. Would you, Number Two, press one of the buttons on the initial choice part of your panel? Thank you. Now, only after you've made your initial choice, Number One, will you be able to see what Number Two chose. And since you've already made your initial choice, Number Two, you'll be able to see Number One's initial choice as soon as he (she) makes it. Would you now make an anitial choice, Number One? All right, as you can see, Number One chose _____, and Number Two chose _____. Do you see that? (CHECK TO MAKE SURE THEY DO.)

Your panels are wired so that both persons' buttons must be pressed before the lights will go on. That is, the exchange of information will take place only after both persons have chosen. Needless to add, the exchange of information lights were disconnected during Phase 1.

Now let me tell you a little bit more about our work today in Phase 2. The type of situation we wish to study is a very common one. It's also a very sociologically important one. Sociologists have a name for it: it's called a "cooperative choice situation." The most important thing for persons in a cooperative choice situation is to make the correct final decision. Let me clarify what I mean with an example. When a doctor at Sparrow Hospital has to make a difficult diagnosis, he knows in the back of his mind that the patient may die if he makes a wrong final decision. Yet he also knows that, at first, it may be very hard to recognize what the right final decision is; and, as I'm sure you'll agree, this presents a very serious problem. Medical schools teach that, when faced with this kind of situation, the doctor should first study the case very carefully and arrive at an initial decision. Then, before taking action, he should get the decision of a second doctor. After that, he should carefully weigh all his information and only then make his final decision. It goes without saying that he shouldn't care whether he himself thinks of the right answer, or whether he recognizes the right answer after this consultation process. The only thing that's important is that he make the correct final decision.

At this point, I'd like to direct your attention to our Phase 2 procedure. At the beginning of each round, I'll present a slide containing a contrast sensitivity problem, just as in Phase 1. After five seconds, I'll ask you to make your initial choice, and this will also be just as in Phase 1. Then, after five more seconds, I'll ask you to make your final choice, using the lower row of buttons on your panel. Your final choice may be either the same as, or different from, your initial choice.

We're using contrast sensitivity problems in Phase 2 because, in certain key respects, these problems are like many of the problems faced by persons in decision making positions in society. They have correct answers, but those answers may be unclear at first, becoming recognizable only after a process of consultation.

I mentioned a few minutes ago that you'll be working together as a team. In accordance with our standard policy, each of you will be paid the same amount for participating in today's study, and that amount will depend upon your final team score. Since this team score determines your pay, let me explain exactly how we compute it. Each time a person makes a correct final choice, the team will gain two points; and each time a person makes an incorrect final choice, the team will gain nothing. Or to put it another way, each final choice you make will be scored +2 or 0, depending upon whether you're correct or whether you're incorrect, in that order.

There'll be thirty rounds in this phase. Therefore, each person's largest possible contribution to the team score is 60 points, and each person's smallest possible contribution is 0 points. The team score itself, since there are two of you, has a largest possible value of 120 points and a smallest possible value of 0 points. On the board we have a table that relates each person's pay for participating in today's study to the final team score. You can see, looking at the table, that if your team were to wind up with a full 120 points, you'd each receive $2.20; on the other hand, if your team were to wind up with 0 points, you'd each receive $1.00. The amount you'll actually earn will probably fall somewhere between these two extremes, and it will depend entirely upon your team's final score, which we'll compute when Phase 2 is over.

Before we start the test, I'd like to go through a practice round to get you used to our Phase 2 procedure. This round won't count toward your team score.

(1) This is Demonstration Slide #3.

(PUT SLIDE ON, ALLOW FIVE SECONDS.)

(2) All right, now make your initial choice.

(ALLOW FIVE SECONDS.)

(3) All right, now make your final choice.

(REMOVE SLIDE, CLEAR PANELS.)

We'll do this thirty times, and the procedure will be exactly as we've just done each time. In order to help your partner as much as possible, you should make your initial choices carefully; however, I want to emphasize that <u>only your final choices</u> will count toward the team score.

There is also one more procedural matter that I should call to your attention. Your choices are being machine scored in this phase. Five seconds after I have asked for your final choice, if you haven't responded, the machine will automatically credit you with an incorrect choice. So if you don't know an answer, it would be in your interests to guess rather than to let the time run out and automatically be credited with a wrong answer.

Finally, one last thing I should mention is that, although your pay for participating depends solely upon the team score, I'll let you know how many points you as an individual contributed to the team score when the test is over. Is everything clear at this point? (CHECK TO MAKE SURE EVERYTHING IS CLEAR; THEN CHANGE THE ICOM FROM "VERIDICAL" TO "NORMAL" AND TURN ON THE ESTERLINE ANGUS.)

Okay, now we'll begin.

(1) This is slide number _____.

(PUT SLIDE ON, ALLOW FIVE SECONDS.)

(2) All right, now make your initial choice.

(ALLOW FIVE SECONDS.)

(3) All right, now make your final choice.

(REMOVE SLIDE, CLEAR PANELS.)

(CONTINUE FOR THIRTY TRIALS.)

The Phase 2 test is now over. What I would like for you to do now is fill out another short questionnaire, which I'll now pass out.

(PASS OUT THE PART TWO QUESTIONNAIRE; THEN LEAVE THE EXPERIMENTAL ROOM FOR ABOUT TWO MINUTES; THEN RETURN WITH POST-SESSION INTERVIEW MATERIALS, WAIT FOR SUBJECTS TO FINISH, AND COLLECT THEIR COMPLETED QUESTIONNAIRES.)

Before I tell you your team score, I would like to talk with each of you individually for a few minutes. Would you come this way, please?

(LEAD THE SUBJECTS TO SEPARATE INTERVIEW ROOMS; BEGIN THE POST-SESSION INTERVIEW AND DEBRIEFING.)

<u>Part Two Instructions: Conditions 2 and 4</u>

(TURN ON INITIAL CHOICE FEEDBACK.) We're now ready to begin Phase 2 of our work. As you may possibly know, psychologists are primarily interested in studying individual behavior. Sociologists, on the other hand, are primarily interested in studying behavior as it relates to a group situation. In terms of this distinction, Phase 1 would be considered the more psychological part of our work today, and Phase 2 would be considered the more sociological part.

In Phase 2, to make a long story short, we'll be interested in your ability at teamwork. Accordingly, we're going to allow you to exchange information before making your final choices. Let's see how this works. Would you, Number Two, press one of the buttons on the initial choice part of your panel? Thank you. Now, only after you've made your initial choice, Number One, will you be able to see what Number Two chose. And since you've already made your initial choice, Number Two, you'll be able to see Number One's initial choice as soon as he (she) makes it.

Would you now make an initial choice, Number One? All right, as you can see, Number One chose _____, and Number Two chose _____. Do you see that? (CHECK TO MAKE SURE THEY DO.)

Your panels are wired so that both persons' buttons must be pressed before the lights will go on. That is, the exchange of information will take place only after both persons have chosen. Needless to add, the exchange of information lights were disconnected during Phase 1.

Now let me tell you a little bit more about our work today in Phase 2. The type of situation we wish to study is a very common one. It's also a very sociologically important one. Sociologists have a name for it: it's called a "cooperative choice situation." The most important thing for persons in a cooperative choice situation is to make the correct final decision. Let me clarify what I mean with an example. When a doctor at Sparrow Hospital has to make a difficult diagnosis, he knows in the back of his mind that the patient may die if he makes a wrong final decision. Yet he also knows that, at first, it may be very hard to recognize what the right final decision is; and, as I'm sure you'll agree, this presents a very serious problem. Medical schools teach that, when faced with this kind of situation, the doctor should first study the case very carefully and arrive at an initial decision. Then, before taking action, he should get the decision of a second doctor. After that, he should carefully weigh all his information and only then make his final decision. It goes without saying that he shouldn't care whether he himself thinks of the right answer, or whether he recognizes the right answer after this consultation process. The only thing that's important is that he make the correct final decision.

At this point, I'd like to direct your attention to our Phase 2 procedure. At the beginning of each round, I'll present a slide containing a contrast sensitivity problem, just as in Phase 1. After five seconds, I'll ask you to make your initial choice, and this will also be just as in Phase 1. Then, after five more seconds, I'll ask you to make your final choice, using the lower row of buttons on your panel. For your final choice, you may select "top,' "bottom," or "withhold judgment."

Since the "withhold judgment" alternative was not available in Phase 1, let me explain a little bit about it. In a cooperative choice situation, withholding judgment has both advantages and disadvantages. To see what these are, think about our previous example. When a doctor has to make a difficult diagnosis, it's best, naturally, if he makes the correct final choice and begins the correct treatment. On the other hand, it's very unfortunate if he makes an incorrect final choice and begins an incorrect treatment, because an incorrect treatment could harm, or even kill, the patient. To avoid making an unfortunate mistake, a doctor will sometimes withhold judgment, which is without question the second best alternative there is. Many other examples could also be given, but let me just summarize the general principle: In any cooperative choice situation, the best choice involves being right, the second best choice involves withholding judgment, and the worst choice involves being wrong.

We're using contrast sensitivity problems in Phase 2 because, in certain key respects, these problems are like many of the problems faced by persons in decision making positions in society. They have correct answers, but those answers may be unclear at first, becoming recognizable only after a process of consultation.

I mentioned a few minutes ago that you'll be working together as a team. In accordance with our standard policy, each of you will be paid the same amount for participating in today's study, and that amount will depend upon your final team score. Since this team score determines your pay, let me explain exactly how we compute it. Each time a person makes a correct final choice, the team will gain two points; each time a person selects the "withhold Judgment" alternative, the team will gain one point; and each time a person makes an incorrect final choice, the team will gain nothing. Or to put it another way, each final choice you make will be scored +2, +1, or 0, depending upon whether you're correct, whether you withhold judgment, or whether you're incorrect, in that order.

There'll be thirty rounds in this phase. Therefore, each person's largest possible contribution to the team score is 60 points, and each person's smallest possible contribution is 0 points. The team score itself, since there are two of you, has a largest possible value of 120 points and a smallest possible value of 0 points. On the board we have a table that relates each person's pay for participating in today's study to the final team score. You can see, looking at the table, that if your team were to wind up with a full 120 points, you'd each receive $2.20; on the other hand, if your team were to wind up with 0 points, you'd each receive $1.00. The amount you'll actually earn will probably fall somewhere between these two extremes, and it will depend entirely upon your team's final score, which we'll compute when Phase 2 is over.

Before we start the test, I'd like to go through a practice round to get you used to our Phase 2 procedure. This round won't count toward your team score.

(1) This is Demonstration Slide #3.

(PUT SLIDE ON, ALLOW FIVE SECONDS.)

(2) All right, now make your initial choice.

(ALLOW FIVE SECONDS.)

(3) All right, now make your final choice.

(REMOVE SLIDE, CLEAR PANELS.)

We'll do this thirty times, and the procedure will be exactly as we've just done each time. In order to help your partner as much as possible, you should make your initial choices carefully; however, I want to emphasize that only your final choices will count toward the team score.

There is also one more procedural matter that I should call to your attention. Your choices are being machine scored in this phase. Five seconds after I have asked for your final choice, if you haven't responded, the machine will automatically credit you with an incorrect choice. So if you don't know an answer, it would be in your interests to guess rather than to let the time run out and automatically be credited with a wrong answer.

Finally, one last thing I should mention is that, although your pay for participating depends solely upon the team score, I'll let you know how many points you as an individual contributed to the team score when the test is over. Is everything clear at this point?

(CHECK TO MAKE SURE EVERYTHING IS CLEAR; THEN CHANGE THE ICOM FROM "VERIDICAL" TO "NORMAL" AND TURN ON THE ESTERLINE ANGUS.)

Okay, now we'll begin.

(1) This is slide number _____.

(PUT SLIDE ON, ALLOW FIVE SECONDS.)

(2) All right, now make your initial choice.

(ALLOW FIVE SECONDS.)

(3) All right, now make your final choice.

(REMOVE SLIDE, CLEAR PANELS.)

(CONTINUE FOR THIRTY TRIALS.)

The Phase 2 test is now over. What I would like for you to do now is fill out another short questionnaire, which I'll now pass out.

(PASS OUT THE PART TWO QUESTIONNAIRE; THEN LEAVE THE EXPERIMENTAL ROOM FOR ABOUT TWO MINUTES; THEN RETURN WITH POST-SESSION INTERVIEW MATERIALS, WAIT FOR SUBJECTS TO FINISH, AND COLLECT THEIR COMPLETED QUESTIONNAIRES.)

Before I tell you your team score, I would like to talk with each of you individually for a few minutes. Would you come this way, please?

(LEAD THE SUBJECTS TO SEPARATE INTERVIEW ROOMS; BEGIN THE POST-SESSION INTERVIEW AND DEBRIEFING.)

Questionnaires

(Note: The part one and part two questionnaires are presented in order, just as they appeared to the subjects, beginning on the next page.)

QUESTIONNAIRE--STRICTLY CONFIDENTIAL

Name ____________

Your number _____

1. Suppose that you were given a second Contrast Sensitivity Test like the one given today. Please make an "X" on the scale below at the point representing your <u>best estimate</u> of how many answers you would get correct.

<u>Your Number Correct</u>

0 2 4 6 8 10 12 14 16 18 20

2. Suppose that your partner were given a second Contrast Sensitivity Test like the one given today. Please make an "X" on the scale below at the point representing your <u>best estimate</u> of how many answers he would get correct.

<u>Partner's Number Correct</u>

0 2 4 6 8 10 12 14 16 18 20

3. If the estimates that you made in answering Question 1 and Question 2 differed by more than one or two points from the actual numbers that you and your partner got correct today, please explain your reasoning in the space below.

THANK YOU

QUESTIONNAIRE--STRICTLY CONFIDENTIAL

Name ______________

Your number ________

1. In our previous studies, we have found that, during Phase 2, some persons are primarily concerned with avoiding wrong answers. Other persons are primarily concerned with getting right answers. The two are not necessarily the same. Please make an "X" on the scale below at the point that most nearly represents your own degree of concern about avoiding wrong answers. Then do the same for the scale that involves getting right answers.

Avoiding Wrong Answers

0	1	2	3	4	5	6	7	8	9	10

Not concerned — Moderately concerned — Extremely concerned

Getting Right Answers

0	1	2	3	4	5	6	7	8	9	10

Not concerned — Moderately concerned — Extremely concerned

2. Many persons who have participated in our previous studies have reported feeling some degree of nervousness during the Phase 2 test. Please make an "X" on the scale below at the point that most nearly represents your own degree of nervousness during this test.

Degree of Nervousness

0	1	2	3	4	5	6	7	8	9	10

Not nervous — Moderately nervous — Extremely nervous

3. Now we'd like to get your impression of how well you did on the Phase 2 test. Please make an "X" on the scale below at the point that corresponds to your best estimate of <u>your own</u> contribution to your team's final score.

<u>Estimated Contribution to Team Score</u>

0	10	20	30	40	50	60
points	points	points	points	points	points	points

4. Please explain why you answered Question 3 as you did. That is, what factors did you take into consideration in arriving at your judgment?

5. If you have any additional remarks that you think might be helpful to us, please indicate them below.

THANK YOU FOR YOUR COOPERATION

Interview Schedule

Before I tell you the results of today's study, I'd like to talk to you to find out some of your thoughts and feelings about it. There are many things that might affect the results of a study such as this, and it's these things that I have in mind. Your name is _____, is that correct? And what is your age, _____? And your major field of study here at Michigan State? (RECORD THESE.)

1. To start off, do you have any general, or overall, feelings about the study?
2. Have you ever taken part in any kind of social science research before? (IF YES, PROBE FOR ITS EXACT NATURE.)
3. Have you ever read about studies like the one today? (IF YES, FIND OUT WHAT HE KNOWS ABOUT THEM.) Have you ever heard of the Asch experiment?
4. When the Contrast Sensitivity test was first described to you, how well did you expect to do on it? Why is that? How well did you expect your partner to do? (IF HE SAYS ANYTHING EXCEPT "I DIDN'T KNOW," ASK WHY.)
5. Had you ever met your partner before today? What impression did you get of him before we started?
6. In phase 1 and the initial choice part of phase 2, how did you go about figuring out the answers? Did you rely on your intuitive feelings, or did you use some kind of a system?

7. When I explained phase 2, and you found out that you'd be exchanging information with your partner, did you anticipate that this would be helpful? Why is that? Was it helpful, do you think?
8. In phase 2, do you happen to remember about how many times you and your partner disagreed on your initial choices? (HE WILL PROBABLY SAY "ALMOST ALWAYS" OR SOMETHING OF THE KIND.) Do you have any idea why? How did you react when you found him disagreeing with you? Whom did you think was right?
9. How confident were you of your final choices during phase 2? Did your confidence change at all as the test went along? (IF YES, FIND OUT HOW.)

(AT THIS POINT, IF IT IS CLEAR THAT THE SUBJECT IS NOT SUSPICIOUS AND THAT HE MEETS THE ASSUMPTIONS OF THE ANALYSIS, THE INTERVIEW CAN BE TERMINATED. OTHERWISE, PROBE IN THE APPROPRIATE AREAS UNTIL IT IS CLEAR ONE WAY OR THE OTHER.)

Substance of the Debriefing

(Note: The debriefing will vary somewhat from one subject to the next. It is important that the debriefing seem like a talk, not a recitation. The following is not a script, it is the essential content of a typical debriefing.)

Now I'd like to explain more about what we were trying to study in today's tests. The purpose of our research is to find out the relationship between a person's ability and the decisions he makes. In particular, we are trying to find out how a person's ability is related to whether or not he will change his decision if another person with either more or less ability disagrees with him. We are not interested in Contrast Sensitivity as such.

To set up a situation in which this problem can be easily studied, there are two things we need to arrange: your ability and your disagreement with your partner. Concerning your ability, the test we gave you in phase 1 didn't really measure your contrast sensitivity. On each slide, each of the two rectangles was exactly fifty percent black and fifty percent white, so there were no right or wrong answers. Your seventeen (or eight) right and the other person's eight (or seventeen) right were both fictitious scores that were prearranged and assigned to you and the other person at random. If you had happened to sit in the other chair today, you would have gotten the other score. We hoped that you would naturally assume that the scores were valid and that you had more (or less) ability than your partner. You might wonder why we gave you a fictitious test--why we didn't give you a real test instead. There are basically two reasons. First, if we gave you a real test, there would be no guarantee that one person would score high, the other low. In fact, the odds are against that happening very often. We could, of course, give a real test to a large number of persons and then select the high and low scorers, pair them up, and bring them to the laboratory. But that would mean that, of all the persons who were interested in participating, only about twenty percent could do so--only the very high scorers and the very low scorers. This would be rather wasteful. Second, and perhaps more important, if we gave you a real test, some of the low scorers would feel bad about it. With a fictitious test, the persons who are told that they got low scores may feel bad about it, but they feel much better again when they learn the true nature of the test and the reasons behind it. Does this make sense?

As I mentioned, we also had to arrange your disagreements with your partner. You said that you and he disagreed on your initial choices most of the time. Actually you probably agreed with him about half the time. The panels were wired so that it would look like you were disagreeing much more often than you really were. The reason for this is that we were interested only in disagreements--we wanted to see how you would resolve the conflict. Since we weren't interested in the times when you agreed, we arranged not to have very many of them. The alternative would have been to have a much longer experiment. To get the twenty-five disagreements that we want, we might have to run through sixty or seventy rounds in some cases. On the average, we would need about fifty rounds to get twenty-five disagreements, since, by the law of averages, you would disagree about half the time.

Briefly, then, this is what the study is about, these are the things we had to arrange, and these are the reasons we had to arrange them. We would appreciate your cooperation is not telling anyone about the two fictitious aspects of the experiment. As I'm sure you understand, if persons knew about these things, they couldn't take part in the study, and word spreads pretty rapidly. So could I have your word that you won't disclose this information? Thank you. I'm glad you were able to participate in our research. (AT THIS POINT, THE SUBJECT IS PAID $2.00 AND IS EXCUSED.)

APPENDIX B

THE LOGIC OF THIS DISSERTATION

As noted earlier, a document that rests upon logically tight arguments is unavoidably difficult to read. Sometimes it may be hard to tell what is central to the main undertaking and what is peripheral. Recognizing that this may well be true in the present case, we shall now attempt to explicate the "bare bones" logical structure of Chapters I through IV, which are the heart of this dissertation. In retrospect, based upon the work of Chapter V, it is clear that our original concern with balance versus utility maximization was misguided. Chapters I through IV, the author now believes, should be viewed as neither more nor less than a comparative empirical test of two abstract behavioral decision making models: the Siegel *et al*. model and the Camilleri *et al*. model.

Behavioral decision making is our main focus. For theoretical purposes, we concern ourselves with the *general case*; for research purposes, we concern ourselves with a *particular case*. Just as a theory about all ducks might be studied by studying Canadian mallards, so a theory about all behavioral decision making might be studied by studying social influence decision making; and this is the particular case we select. The essential logical structure of our work consists of two parallel chains of implication, which can be described as follows. The utility maximizing principle conjoined with two other premises (viz., the Siegel *et al*. model) implies Theorem Schema S1, which implies Theorem Schema S3, which in turn, by the applicative principle, implies our first "basic hypothesis" (page 41). This basic hypothesis implies the null hypothesis of the statistical test regarding L_1 and the alternate hypothesis of the test regarding L_2 in the second stage of our analysis. In a parallel fashion, the balancing principle conjoined with one other premise (viz., the Camilleri *et al*. model) implies Theorem Schema C1, which implies Theorem Schema C3, which in turn, by the applicative principle, implies our second "basic hypothesis." This hypothesis implies the alternate hypothesis of the statistical test regarding L_1 and the null hypothesis of the test regarding L_2 in the second stage of our analysis. Now, the results of the third stage of our statistical analysis are determined by the results of the second stage; therefore, these Bayesian conclusions, by the statistical analogue of *modus tollens*, feed back through the logical chains just indicated to the two models.

In these chains of implication, notice that Theorem Schema S2 and Theorem Schema C2 play no part. It might be asked, what is *their* logical status? Before answering this question, let us indicate the purposes of these theorem schemata and the fairly extensive analyses that lead up to them. Those purposes are: (1) to give credence to the claim that either model could account for the social influence data examined in Chapter I, and (2) to provide the rationale for the subsequent, related claim that $Cr(M_1) = Cr(M_2) = 1/2$. Regarding this second purpose, we argue that the two models are indistinguishable on the basis of previous research, hence that there is no scientifically justifiable basis for assuming that either model is more credible than the other. To answer the question raised, then, Theorem Schema S2 and Theorem Schema C2 are <u>tangential</u> to the basic logical structure of our work, although, as indicated, they do serve useful collatoral purposes.

In this dissertation, as the above discussion indicates, the linkages between abstract theory and concrete research are <u>not</u> mediated by <u>any</u> auxiliary substantive assumptions whatsoever, save the assumption that "the social influence application" is a legitimate application of behavioral decision making theory. That is to say, we have short-circuited the complex, loophole-ridden linkages that some authors seem to believe will necessarily confound any substantive tests of these models (see, for example, Camilleri <u>et al</u>., 1972: 29-30). The methodological significance of this accomplishment should not be overlooked when and if, after an appropriate interval, the argument of this dissertation is dispassionately analyzed, and the scientific contribution of our work is dispassionately assessed.

In closing, we might remark that the logical structure of this dissertation is best understood in conjunction with the broad scientific strategy that underlies it. Science progresses most expeditiously, we believe, through comparative empirical research, through research designed in such a way that rival models or theories make different predictions. Now we grant that, in many areas of sociology, rival models do not exist, and that, in such areas, this strategy would be inappropriate; however, whenever rival models do exist, the first research priority should be to eliminate at least one of them. Thus, when contemplating a particular study, the researcher should ask himself: "What predictions does each model make with regard to my proposed research?" If the answer turns out to be that they both make the same prediction, he should "go back to the drawing board," as it were, and alter his research design in such a way that they make different predictions. This is precisely what we did with respect to the research reported in this dissertation; and, in view of the recent additions of Model A and Model B to the list of rival models (see Chapter V), this general approach seems almost obligatory for the future.

Such, at any rate, has been the logic and broad underlying strategy of the present document.

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