H. Russell Bernard and P. D. Killworth 1979. Review of the Small World Literature. *Sociological Symposium* No. 28, Fall.

A REVIEW OF THE SMALL WORLD LITERATURE

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ABSTRACT. Recently, we have been experimenting with the Small-World Technique (SWT), due to Stanley Milgram, in order to generate data on aspects of social structure. We feel that the SWT is a potentially powerful way to study social structure because it generates behavioral data. This review paper summarizes the available literature to date, and presents a program of related experiments which we are currently developing or wish others would do. In general, we hope that others will begin to study social structure experimentally.

(Note: An earlier version of this paper was written as TR#BK-115-78, under contract No. N00014-75-C-0441-P00001, Office of Naval Research, Code 452. The opinions expressed in this paper are those of the authors and do not necessarily reflect the position of the supporting agency. Reproduction in whole or in part is permitted for any purpose of the United States Government.)

Introduction

Recently, we have been experimenting with the Small-World (SW) technique, due to Stanley Milgram (1967, 1969). Considering the initial interest in the SW problem, there isn't a great deal of literature on it. Studies on the SW problem fall into two categories: theoretical papers which treat the problem mathematically, and observational studies which present data collected by the SW technique. In this paper we will review the SW literature to date, and discuss ways to use variants of the SW technique for getting data relevant to a theory of social structure. We hope that this will stimulate others to study social structure experimentally.

Original Problem

The SW problem was formulated in 1958 by Pool and Kochen in what has become a classic underground paper, recently published (1978). Milgram stated the problem in 1967 thus: "starting with any two people in the world, what is the probability that they will know each other?" (p. 62). This particular formulation is, of course, very general; it doesn't mention the existence of social structure. In fact, this formulation of the problem is blatantly independent of social structure. Clearly, if we asked "what is the probability that any two investment brokers will know each other?" the probabilities would be higher than in the earlier question.

Pool and Kochen found that there is one chance in 200,000 that any two Americans ($n = 2 \times 10^8$) taken at random will know each other. They assumed that Americans "know" (on average) 500 others. (This figure had been determined in 1961 by Gurevich in his doctoral dissertation at MIT). Pool and Kochen also reported that more than 50% of the time any two Americans can be linked together if two intermediaries are allowed.

To our knowledge, the SW problem has not yet been empirically tested. There are good logistic reasons for this (how does one ask randomly selected pairs of Americans, or black Americans, or tailors in South Africa if they know one another?), so Milgram reformulated the problem (p.62): "Given any two people in the world, person X and person Z, how many intermediate acquaintance links are needed before X and Z are connected?" By so reworking the problem, Milgram was able (for $680!) to develop his now-famous SW technique. In the technique, a "starter" (S) is given the task of getting a folder to a "target" (T). S is told that he or she can pass the folder to T directly, only if T is a personal acquaintance (i.e. "known to S on a first-name basis" is the usual criterion -- but more on this below). And so the chain grows, until the folder gets to T.

In the original experiment the drop-out rate was very high. Only 44 out of 160 chains were completed. At each remove on each chain, there was a chance that a person would not send the message along. Over the years, the mail has continued to be the preferred mode of message transfer for SW experiments. But there are costs. In 1975 Jean Guiot, a management theorist, published the first SW experiment in which telephones were used. The study was restricted to a single city (Montreal), but there is no reason that long distance calls could not be the vehicle for message transfer. In a current

experiment, we have used the telephone for a SW experiment in Morgantown, West Virginia. Our results corroborate those of Guiot: there is, indeed, a very high rate of success in completing chains (better than 70% at this writing). A few starters demur and we have to go back to our sampling parameter (the Morgantown phone book); but after a chain gets started people respond very well. (Except for S we begin each call with "S (or whoever, one remove back) told us to call you. We're trying to locate T in an experiment we're conducting to see how...").

We feel that it is very important to remove the attrition problem from the SW technique for two reasons. First, it biases the data. Second, and much more important, attrition is at least partly an artifact of the technique. It has yet to be shown that attrition plays any role in any communication process which the SW technique is supposed to be measuring. Is the drop-out rate for SW folders the same as that for a dull rumor? A juicy rumor?

Milgram's Experiments

In the original article by Milgram (1967), two SW chains were reported--one from 145 persons in Wichita, Kansas to the wife of a divinity student in Cambridge, Massachusetts; and the other from 160 persons in Nebraska to a stockbroker who lived in Sharon, Massachusetts, a suburb of Boston where he worked.

In what follows, 'chain length' is used consistently to mean the number of *steps* required to reach a target. SW literature typically reports the number of intermediaries in a chain. If a chain is: Starter to A to B to Target, it seems to us that three steps is the relevant distance involved, rather than 4 people or 2 intermediaries. On a journey, one quotes the number of miles travelled, not the number of milestones passed!

As noted, 44 chains were completed, and to everyone's surprise, the average length was about 6.21 steps, with a mode of 7. White (1970) shows that the chains would be slightly longer if attrition did not occur (i.e., they would rise from 6 to 8), since at each remove from the starter, there is a chance that the chain will die. The dead chains clearly lower the average length of the completed chains.

The Nebraska-Boston study is fully reported in an article by Travers and Milgram (1969). Originally, 196 Nebraska starters were chosen by mail solicitation. 100 were owners of blue-chip stocks; 96 were selected from the population at large. Another 100 volunteers were solicited through an advertisement in a Boston newspaper. The mean length of all chains was 6.2, although the Boston chains were signficantly shorter than the Nebraska chains (5.4 vs. 6.7).

In the Travers and Milgram study the length of chains clearly depended on the distance between S and T -- presumably physical, as well as social distance, whatever this means. So far, there hasn't been any systematic study of social distance, although it is clearly necessary; we'll discuss this later in this article.

Another interesting finding in this study was a "funneling effect." As the chains approached T, they funneled in through only a few penultimate links. 48% of the 64 chains reaching T in the Nebraska-Boston study came in through just three links. Travers and Milgram (1969: 442) noted that "the convergence of communication chains through common individuals is an important feature of small world nets, and it should be accounted for theoretically." As far as we know, this has not been attempted, although in our own work (Killworth and Bernard, 1978a) we examined this from the point of view of outgoing, rather than incoming, networks.

Soon after Travers and Milgram reported on their study, Korte and Milgram (1970) used the SW technique to test racial boundaries. They recruited 18 Ts in New York City, including 9 male whites, and 9 male blacks. Each T was to receive 30 folders. Of the 540 startng chains, 123 (22%) reached their targets. There was a significant difference in the completion rate for whites and blacks (33% vs. 13%), but the chain lengths were not significantly different. Nor were chain lengths in this study radically different from those in the Travers and Milgram study. We will suggest a simple reason for this later.

The difference in completion rate for blacks and whites shows the potential importance of social structure in a theory for the SW problem. (Recall that Pool and

Kochen developed their original theory without social structure as an input). On the other hand, two of the chains in the Korte and Milgram study (and one chain in the Travers and Milgram study) had only one intermediary. This means that all that is required to test the original one-intermediary problem is a sufficiently large number of SW experiments. The reformulation of the SW problem by Milgram was not, therefore, really necessary. It was interesting; and it yielded a brilliant new technique; but it didn't have to be done in order to test the SW problem. As it turns out, Milgram's reformulation is just another question that can be asked about intermediaries beyond 1.

An alternative way of tackling the mutual-acquaintance-problem (and this is an experiment we are now conducting, along with Linton Freeman) is to put random pairs of people in a room (or onto a phone) and ask them a) do you know each other? and b) if not, who do you know in common? Their communication would be monitored, to allow discovery of exactly what features about each other enables a pair to find a common acquaintance. (It would be good to know whether such an acquaintance would in fact ever be used in any traditional SW experiment. How could one test this?) By doing this procedure in increasingly large populations (towns, cities, regions, etc.), we can tell what the chances are of people knowing someone in common. If social factors are of interest, then this can be studied also. Suppose that we want to find out if people who fly to Europe from the United States know someone in common. The best way to find out is to go to Kennedy airport, grab people in random pairs and ask them. This (and other direct tests of the SW problem) is expensive and difficult to do; but it is much less expensive than many experiments in the biological and physical sciences. Of course, much pretesting would be necessary to ensure good data return on the investment. After all, knowing that random pairs of people have, say, a 1 in 10 chance of knowing one another may not by itself be a very useful statistic. We need also to understand how people go about the task of finding their mutual acquaintance(s).

Lin, Dayton, and Greenwald

Some information on this question is presented by Lin, Dayton, and Greenwald (1977, 1978). Their first paper deals primarily with the problem of attrition, while the second presents data on reasons for choices by intermediaries. Data were collected in an "urbanized area" in the Northeast. There were four targets: a white male, and a white female; a black male, and a black female. There were 300 starters, also split into four groups on the basis of how much information they had about Ts (whether or not they knew T's race or occupation). Because of the high (30%) completion rate, and the additional information acquired from intermediaries, these data are probably the richest available in the SW literature.

In the first paper Lin *et al.* examine some causes of attrition. They find, as did Korte and Milgram (1970), that crossing racial boundaries is difficult. Another finding which also agrees with Korte and Milgram was the tendency for a drop in social status between penultimate link and target. An attempt to extend the influence of social status to other links in the chain produced no significant results, but Lin *et al.* nonetheless believe that (p. 116) "the effective strategy ... is one where a participant is relatively high on the occupational status hierarchy, so that a panoramic view of the structure below enhances the search for ... a target relatively low in the status hierarchy."

In their second paper, Lin *et al.* concentrate less on attrition and more on social structure as revealed by their SW experiment (although chains are still divided into successful -- those which reached the target -- and unsuccessful). They found that the occupational prestige of successive links in completed chains "tended to" rise; whereas in unsuccessful chains it stayed approximately constant. Further, participants in successful chains guessed that they knew more people than those in the unsuccessful chains. Lin *et al.* then examined how well each individual in a chain knew his or her next link by answers to the question "how many days ago did you last see this person?" By this expedient measure, they found that participants in successful chains "tended to use" weaker (i.e. less-well-known) ties than participants in unsuccessful chains.

These conclusions are very interesting, but are clouded by several factors. First, Lin *et al.* never quantify the "tendencies" in their data. They report no significance tests. Rather, their conclusions are illustrated by diagrams which display only means. Furthermore, the diagrams often contradict the conclusions stated in the paper. Part of the ambiguity of the results might be removed by allowing for the fact that participants in unsuccessful chains, several removes back from termination, may be making choices in

exactly the same way as those in successful chains. Only the penultimate link may be making the "wrong" choice.

Second, but less important, people really don't know how many people they know, or when they saw anyone last (cf. Bernard and Killworth, 1977; Killworth and Bernard, 1978a). This is not a stringent criticism of Lin *et al.* (1978); correlating chain characteristics with what people think about anything is a perfectly valid thing to do. We stress only that they report no correlations, but merely vague "tendencies."

In summary, the work by Lin *et al.* is full of excellent ideas about how to elucidate social structure from SW data. Many potentially valuable experiments are suggested in these papers.

Hunter and Shotland

Many ideas for SW experiments are generated by an important article by Hunter and Shotland (1974), and Shotland (1976). Data gathered by the SW technique are used to "estimate the distance between social categories, the diffuseness of connection within a category, and the relative isolation of various categories" (Hunter and Shotland, 1974:321). The categories are students, faculty, and administrators at Michigan State University.

They show that the mean "distance" (i.e., the mean number of links between these groups, as measured by the familiar SW technique) is not easily taken at face value for several reasons. First, the criterion used to move the folders is not the same from one SW experiment to the next. Thus, "someone you know on a first-name basis" and "someone who you trust" may produce radically different chains. Even one criterion may produce different results in different cultures or sub-groups. Telling wasps, blacks, and chicanos to give a booklet to a "friend" could result in uninterpretable results.

Second, there is the (by now familiar) problem of attrition, i.e., incomplete chains. As Hunter and Shotland put it (p. 322), if a folder gets lost "it probably doesn't mean that you can't get from A to B, but that some intermediary was careless, hostile, or hopeless." Even if the probability that a person will cooperate is a constant, k, the probability of a folder reaching a target get would be k^n, where n = the number of intermediaries in the chain. For long chains, the likelihood of completion can get very small. The problem is confounded by the fact that we don't know the value of k, or whether such a value exists.

However, assuming a constant loss rate, White (1970) (see also Feinberg and Lee, 1975) showed that the loss rate on each transaction is not a simple function of the percentage of incomplete chains, and the "theoretical average chain length is not a simple function of the observed average chain length" (Hunter and Shotland, 1974: 323). Hunter and Shotland provide an excellent discussion of the mathematics of this problem.

Hunter and Shotland's discussion of attrition, is sociologically interesting. They point out that different subpopulations have different probabilities of folder-loss. Thus, in the Korte and Milgram study, the chains are much more likely to get through the white group than the black group. Hunter and Shotland (p. 323) say "an analysis based solely on completed chains would vastly under-represent the number of passes to and from the high loss group -- i.e., the extent to which members of one category know and are known by people in other categories." They conclude that, if this is done "incompleted chains may lead to a misinterpretation of the structure of the group one is examining. For example, the high loss categories would appear to be much more socially isolated than they actually are." In fact, White (1970) concluded that the black group may be more atomistic than the white group in the Korte and Milgram study.

Hunter and Shotland (p. 329) show how to compare observed chain length with "ideal" length (i.e., the chains that would result if there were no attrition at all). This allows an investigator "to go from his observed results to a good approximation of the results of a 'perfect' experiment."

Perhaps the final difficulty with a Markov model is that the imposed categories may or may not be relevant for the actual structure under investigation. Perhaps other category definitions might produce better fits to the data than the ones used. We have recently (Killworth and Bernard, 1978b) described a Markov model of the Milgram-Korte-Travers experiments (i.e. involving the entire U.S.) in which the

membership of each category (or state) is a function of the target. The transition probabilities between states were obtained partly from data we had gathered in a previous experiment (see the reverse SW discussion, below), and partly from intuition. Unlike Hunter and Shotland, we used no actual SW chain data to obtain the probabilities in our model.

Because the membership of each category changes with target, but transition probabilities vary only weakly (it turns out), the resultant path lengths from most of the U.S. to any T are virtually independent of the T. This agrees with the Milgram-Travers-Korte findings. Indeed, the fit to their data is very good, considering the small number of parameters (3) we used. However, free parameters allow easy fits to data. Clearly such parameters should be both reduced in number as far as possible, and, ideally derived from observations.

Of course, the number and type of categories is essentially another free parameter in both Hunter and Shotland's and our models. We have proposed (Killworth and Bernard, 1978b) a method for deriving the categories for a Markov model (if, indeed, such categories exist). This involves asking Ss to name their choices of first-links to a variety of Ts. Ss are allowed to ask as many questions about each T as they wish, until they feel able to make a choice. At the end of this phase of such an experiment, all the Ss are recontacted and asked to provide information about themselves corresponding to the aggregate of questions asked by all Ss about all Ts.

This procedure gives, for each S-T combination, the following data: information about S; comparable information about T; which questions were asked about T by S (coded as zero or one); characteristics of the choice made; and the reasons S gave for making the choice. Factoring these very rich data should yield various starter categories, choice categories, and a (hopefully short) list of fundamentally different questions which Ss require in order to make a choice.

Some Other Small World Papers

A similar approach to Hunter and Shotland, but purely theoretical, was taken by Stoneham (1977). She created a spatial model (in which spatial distance is the relevant criterion for communication) with many (100) categories, and simulated the Markovian chains by a Monte Carlo method. (Although one could probably generate the required statistics analytically by methods similar to those of Pool and Kochen, the labor would be immense, and the simulation method seems most suited to the problem). She was able, by adjusting only two parameters, to fit the mean and standard deviation of path lengths found by Travers and Milgram. The difficulty with this type of study, of course, lies in its interpretation. As with all models (including our own) fitting two parameters by adjusting two others is hardly a test of a theory, but of equation solving. Yet, lacking a way to obtain either of Stoneham's parameters from other data, there seems no way to confirm or deny her method. Andrews (1977) has questioned the procedure on different grounds. He suggests that percolation rather than diffusion, may be the relevant process to model. Since nobody has applied percolation theory to model the SW process, it is impossible to tell whether Andrews' suggestion is of any value. Andrews reiterates this suggestion in a recent note in this journal (1978).

Two SW experiments are described in articles by Bochner and others. In both experiments the SW technique was modified to allow open-ended chaining. In a high-rise building study in Sydney, Australia (Bochner, Duncan, Kennedy, and Orr, 1976), seventeen residents of a high-rise complex "were given chain-booklets with instructions to advance these through their social networks in the building."

This study was done to test the "proximity hypothesis," i.e., that in impersonal situations like a high-rise city building, proximity of residence is a major determination of interaction. The results were disastrous, but enlightening. Two interviewers went door-to-door, canvassing a random sample of 84 starters; 38 persons were not at home; 29 refused to participate (in a face-to-face request situation!) and 17 renters agreed to start a folder on its way.

Of the 17 started folders, only 6 went beyond the starter. The object was only to move the folder to "another person in this building, whom you know on a first-name basis." There was no target, and this might account for the low participation. Theirs is a preliminary research report, and doesn't add much to the literature except for the potential negative results about participation in social science experiments by urban

high-rise dwellers.

Another study by Bochner, Buker and McLeod (1976) was done at the East-West Center, in Honolulu, Hawaii. They selected two dormitories, one female, the other 80% male. The dormitories housed a total population of 500 residents, from 38 national groups.

Once again, the object was to find chains of acquaintance rather than to reach a target. They wanted to find out what accounts for the particular chains. They chose Ss by national group and by sex, controlling for length of residence. Eighteen Ss were chosen and all agreed to participate. Unlike the Sydney high-rise, these foreign students were thought to form a "community" of sorts, and it was known that many residents knew each other.

Sixteen folders went at least one step beyond the starter. Eight chains were five or more links in length. One chain went 13 links, and one went 15 links (the limit imposed by the number of mail-back cards). Although the data are not rich, Bochner *et al.* were able to demonstrate their main hypothesis, that (not surprisingly) folders in a foreign student dormitory go mainly between people from the same country. They also go mainly between people of the same sex, as in all SW studies.

The SW technique has also been used to study large organizations. Lundberg (1975) campares the data from the Travers and Milgram study (1969) to data he collected on two large companies in Dallas during 1971-72. He used two Ts at the top of each organization, and two at the mid- or bottom-levels. There were 120 Ss for one firm (30 per target) and 342 starters (85 per target--sic) in the second firm.

Only 22% of Travers and Milgram's chains were completed, but 57% of Lundberg's were. Of course, Lundberg's chains were, on average, much shorter (4.36 vs. 6.25 steps for Travers and Milgram), but there are other obvious reasons that the success rate might be high. In Travers and Milgram's study, the 64 chains funneled through 26 links. In Lundberg's study, 263 chains went through 208 penultimate links. While the chain length is longer in Milgram's open society experiments, the convergence rate is much faster than that found by Lundberg for organizations.

In comparing organizations, Lundberg found some interesting differences. He purposely chose two groups with very different organizations, one very bureaucratic (standardized work, strict hierarchy, etc.), and one rather loose organization. Sure enough, chains starting in the loosely organized group had a higher rate of completion than in the tightly controlled bureaucracy. Also, the lower the bureaucratization and the less standardized the work, the higher the number of penultimate links, expecially for executive targets.

The Lundberg study shows the potential value of the SW technique both for the comparative study of organizations, and for the evaluation of organizational communications. Typically, the flow of communications in organizations is assessed by a) asking people who they talk to; b) creating a matrix from such data; c) massaging the matrix with the latest clique-finding algorithm; d) comparing the resulting picture with the formal organizational flow chart and the desired flow chart in the minds of the managers who commissioned the assessment.

The Reverse Small World Technique

One of the many things which worried us about the SW technique as a source of data which could be applied elsewhere, is the lack (except for Hunter and Shotland) of enough wide-ranging data to obtain statistical reliability. No matter how many starters one uses, one obtains -- per target -- essentially three pieces of information: a) how many people comprise his incoming network (assuming an awful lot of starters were used); b) the mean length of chains to that target (and, hopefully, a fit to various SEC indicators of the starters on this); and c) some snippets about intermediaries in the chains.

Apart from the first of these, one is clearly in danger of repeating and extending an experiment in order to obtain more data whose only field of confirmation is the experiment itself: in particular, (b) and (c) are extremely biased data sets.

Clearly, to get any statistical reliability in a SW experiment one needs many starters and many targets to neutralize the attrition problem; whereupon the cost of the

experiment, together with complexity, soars. We attempted a way out of this by removing all intermediate links, and creating a vast list (1267) of mythical targets. Starters were presented with the list, which also contained the town, occupation and race or ethnic background of each target. The Ss were instructed in the SW technique and asked to write down their choice, from among the people they knew, for the first link in a potential chain from them to each of the 1267 Ts. With each choice, Ss provided information on the types of choice made (e.g. mother, cousin, friend, acquaintance, or whatever), together with the sex of the choice and the reason that choice had been made. The reason could be in one or more of four categories: something about the location of T caused S to think of his or her choice; or the occupation of T was responsible for the choice; the ethnicity of T; or some other, unspecified, reason.

Six main conclusions were drawn from the data.

1) A mean of 210 choices per starter account for the "world" (i.e. the 1267 targets). This number is probably an underestimate. However, only 35 choices are necessary to account for half the world. Of the 210 choices, 95 (45%) are chosen most often for location reasons, in preference to the other possible reasons; 99 (47%) are chosen most often for occupation reasons; only 7% of the choices are mainly based on ethnicity or other reasons.

2) Choices are mainly friends and acquaintances, with strong cleavage by sex. For any given T, the type of choice used by the majority of Ss was always a friend or acquaintance, and never a family member. For any given T, the most likely sex of the choice (i.e. over all Ss) can be predicted accurately on 82% of occasions. This sex tends to be male, unless both S and T are female, or if the T has a low-status occupation.

3) Location was the usual reason for choice (out of the four categories), with occupation second most used. For any given T, the reason for choice used by the majority of Ss was *always* location or occupation, never ethnicity or other reasons. This most popular reason for choice may be correctly predicted 81% of the time for any given T.

4) The decision as to which choice was made appears to depend primarily on the occupation of T, and secondly on T's distance (near/far) from Morgantown, West Virginia, where the experiment took place.

5) The expression "having one's man in" can be partially quantified. One can define a choice to "handle" a state (e.g. Alabama) in the U.S. if he or she was chosen for two thirds or more of the targets in that state for which choices were made on the basis of location. Then, for any S, on average, half the states are each "handled" by a single choice.

6) The accuracy of Ss' recall about their networks is low, in the sense that their recall is incorrect more often than it is correct (i.e., their recall could not be put to any other use with any reliability). This confirms previous experiments on informant accuracy.

Reliability of data

Now let us suppose that, at some future time, statistical reliability has been thoroughly obtained. How should we interpret the data? And can we trust it? An example will illustrate the problem.

In 1972 we invented a clique-finder and exported it. We tested it on dozens of organized groups and found it to be very subtle. Invariably, the pictures we drew (including covert relationships, in some cases) appealed to the intuition of managers. Everyone agreed that the technique (called CATIJ -- rhymes with cabbage) was a "useful decision-making tool." It was this last part that worried us. Managers could certainly tell whether the pictures we drew conformed to what they *wished* were true. But we simply could not say that the results of CATIJ were correlated to morale or performance or any other measure of organizational effectiveness.

In 1975 we began to question the premise of this kind of work. Namely, that the input data for studies or organizational communication are accurate. In other words, if we ask people who they talk to, are their answers accurate? In a series of papers we have shown (Killworth and Bernard, 1976; Bernard and Killworth, 1977) that people do not

know, with any accuracy, who they talk to (even in a closed group) over periods of a week or more. We have also shown (Killworth and Bernard, 1978c) that both kinds of data (who people talk to, and who they say they talk to) are highly structured. This leads to the following conclusions: a) data based on recall of communications are not a proxy for the communications; b) if a manager wishes to make a decision on the basis of information about communication, then data on perceived communication are inappropriate inputs into the decision.

Lundberg's study suggests a convenient experimental check on the power of clique-finding instruments. Select several formal organizations of about 100 persons. For each organization do a mapping of perceived communications, (using one's favorite clique-finder, for example) and a SW experiment in getting a message from a line employee to the head of the organization. The message should be critical of policy, and at each remove the respondent must be instructed to give the message to someone whom he or she feels is trustworthy, i.e., won't hurt the respondent for passing a critical message up the line. An alternative criterion might be to "pass the message to someone you know socially, i.e., outside the office."

Over a period of about a year one ought to be able to collect about 20 such data sets; ten would be with the SW experiment before, and ten after the cognitive map test. An appropriate statistic will have to be concocted to evaluate the "accuracy" of the cognitive map vis-a-vis the SW behavioral data.

Lundberg's study comparing two organizations using the SW technique is a good source of ideas for persons interested in further studies of this sort. Suppose the dependent variable were "effectiveness," operationally defined, and suppose the independent variable were "amount of reported or observed interaction" between line staff and mid-level management outside the office. Then, the SW technique could be used to introduce an intervening variable (length of chains between levels of organizations) and might become a useful management tool for evaluating the relationship between organizational communication and measures of productivity.

A preliminary study by Erikson and Kringas (1975) shows the potential of the SW technique for studies of political power and influence. They attempted a study of networks between members of a political constituency and that group's elected representative in government. The idea is very good, because constituencies are rather well-defined, large groups. Furthermore, such studies are of substantive interest; and influence, after all, is of major sociological interest.

Unfortunately, only 16 of 300 chains were completed. They chose 300 persons randomly, and reached 260 of them in their initial attempt to recruit starters. Only 38 agreed to participate (i.e., start a chain). Eventually, 22 of them dropped out (i.e., the chains were incomplete). As preliminary as this study is, however, it raised a number of interesting questions, as well as possibilities for using the SW technique in influence studies. Erikson and Kringas were concerned with "politics as a process embedded in social relationships rather than politics in the usual sense of a static pattern of individual attributes based on artificially disconnected respondents to a sample survey."

This study brings into focus the potential difference between perceived and behavioral networks. It further raises the possibility of comparitive work on political networks, both behavioral and perceived. Suppose the vice president in charge of moving junior executives in General Motors were the target. And suppose that 100 of the most junior executives in General Motors were Ss. And suppose Ss were told that if they could get their request to Mr. Big via one of 6 (unnamed) channels, then they (Ss) would get transferred to an ocean-front office in San Diego. This makes the funneling effect the important artifact of the SW studies so far. We have learned in all SW experiments that it is often possible to get a message from S to T. However, if we ask whether S can influence T via an indeterminate number of others, the a) the question is of greater sociological interest; and b) we don't have any empirical data. We suppose that the results of SW experiments based on influence as a criterion might be different in various cultures; but again, we just don't know.

Aims of Future Research

A difficulty with the SW technique as used up to now is that it provides no direct input into a theory of social structure. To be sure, it supplies figures (mean number

of steps in SW chains, etc.) which serve as stringent tests of statistical models. But, as we have said above, knowing a mean path length alone says nothing about social structure. How, then, can the SW method be modified to yield more relevant data?

First, what do we need to know? A basic description of social structure involves knowing all the people that each person in the structure knows, and the reasons he or she knows them. This is not only unwieldy, it may also be irrelevant. The precise details of each person's networks may be akin to the characteristics of individual molecules in a gas, in which bulk motion (and not molecular details) is the important quantity. In other words, data-reduction may be necessary not only for research purposes, but also for enlightenment. Therefore, the first thing we need to know is how much, and what kinds of data are needed. It seems to us that the members of a social structure ought to be able to tell us this.

Suppose three separate SW experiments are performed, all with the same target, each with, say, 200 starters. The first set of starters and all their intermediaries are given "full" information about the target. What constitutes "full" information is found by the aggregate of all questions asked by starters in the experiment discussed here and in Killworth and Bernard (1978b). To our knowledge (from pretesting) full information may include such esoteric items as the T's hair color, choice of contraceptive method, age of oldest child, and so on (!) The second set of Ss and their intermediaries are given no information but are free to request as much information as they desire. The third set are given nothing but T's name, address and occupation. (Perhaps other sets might be given less than that: just location, or just occupation, as in the work of Lin, Dayton and Greenwald, 1977).

One can then examine whether the SW chains are significantly shorter for Ss armed with a lot of information about T. If this is not the case, then a great deal of data immediately becomes irrelevant--at least for acquaintance chains.

In general, the types of experiments we have described in this paper should provide much of the data one needs to understand social structure. Then there is another, more serious, question to answer.

Second, what should we do with the data? It is embarassing to admit that we really have no idea at this stage how to answer this question, and we suspect that this would be true for most other researchers as well. Just what would a quantitative theory of social structure have in it? As we stated in the beginning of this paper, we hope that future research in social networks will enable us to answer this question directly.

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