AD-A056 527 WEST VIRGINIA UNIV MORGANTOWN DEPT OF SOCIOLOGY AND --ETC F/G 5/11
A REVIEW OF THE SMALL WORLD LITERATURE.(U)
JUL 78 H R BERNARD, P D KILLWORTH N00014-75-C-0441
UNCLASSIFIED TR-115-78 NL

END
DATE FILMED
9-78
DDC

LEVEL II

SECURITY CLASSIFICATION OF THIS PAGE (When Data Entered)

REPORT DOCUMENTATION PAGE

READ INSTRUCTIONS BEFORE COMPLETING FORM

1. REPORT NUMBER: BK-115-78	2. GOVT ACCESSION NO.
3. RECIPIENT'S CATALOG NUMBER	
4. TITLE (and Subtitle): A Review of the Small World Literature	5. TYPE OF REPORT & PERIOD COVERED: Interim rept.
	6. PERFORMING ORG. REPORT NUMBER
7. AUTHOR(s): H. Russell Bernard (West Virginia University); Peter D. Killworth (University of Cambridge)	8. CONTRACT OR GRANT NUMBER(s): N00014-75-C-0441-P00001
9. PERFORMING ORGANIZATION NAME AND ADDRESS: H. Russell Bernard, Dept. of Sociology and Anthropology, West Virginia University, Morgantown, WV 26506	10. PROGRAM ELEMENT, PROJECT, TASK AREA & WORK UNIT NUMBERS
11. CONTROLLING OFFICE NAME AND ADDRESS: ONR-Code 452, Arlington, VA 22217	12. REPORT DATE: July 1978
	13. NUMBER OF PAGES: 14 p.
14. MONITORING AGENCY NAME & ADDRESS (if different from Controlling Office): West Virginia University, Morgantown, WV 26506	15. SECURITY CLASS. (of this report): Unclassified
	15a. DECLASSIFICATION/DOWNGRADING SCHEDULE

16. DISTRIBUTION STATEMENT (of this Report)

Approved for public release, distribution unlimited

17. DISTRIBUTION STATEMENT (of the abstract entered in Block 20, if different from Report)

DDC RECEIVED JUL 20 1978 E

18. SUPPLEMENTARY NOTES

19. KEY WORDS (Continue on reverse side if necessary and identify by block number)

small-world, social networks, sociometrics

HAS BEEN CARRIED OUT

20. ABSTRACT (Continue on reverse side if necessary and identify by block number)

Recently, ~~we have been experimenting~~ AN EXPERIMENT with the Small-World Technique (SWT), due to Stanley Milgram, in order to generate data on aspects of social structure. ~~We feel~~ IT IS FELT 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~~ IT IS hope that others will begin to study social structure experimentally.

DD FORM 1 JAN 73 1473 EDITION OF 1 NOV 65 IS OBSOLETE S/N 0102-014-6601

SECURITY CLASSIFICATION OF THIS PAGE (When Data Entered)

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. Whether the data generated by the SWT are meaningful is a problem which we'll discuss later.

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 empirical studies which present data collected by some variation of the SWT. This review paper will summarize the available literature to date, and will present 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.

The SW problem was formulated by Pool and Kochen in what has become a classic underground paper (soon to be published in JSN). Stanley 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. 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.

In Pool and Kochen's paper (given that social categories are unimportant) they 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 Gurevitch in his doctoral dissertation at MIT--where Pool was. 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: "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 SWT. 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 (ie. "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 in SW experiments. But there are costs. In 1975 Jean Guiot, a management theorist, published the first SW experiment in which phones 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, WV. Our results corroborate Guiot's: 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...").

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.

Of the 160 chains that started in Nebraska, 44 were completed, and to everyone's surprise, the average length of each chain was about 5.21 links, with a mode of 6. White (1970) shows that the chains would be slightly longer if attrition did not occur (ie., they would rise from 5 to 7), 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 ad in a Boston newspaper. The mean length of all chains was 5.2, though the Boston chains were significantly shorter than the Nebraska chains (4.6 vs. 6.1).

The length of chains clearly depended on the distance between T and S -- presumably physical, as well as social distance. So far, there hasn't been any systematic study of this, 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 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" (p. 442). As far as we know, this has not been attempted, although in or own work (Killworth and Bernard, 1978) 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 SWT 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 starting 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.

The difference in completion rate for blacks and whites shows the importance of social structure in a theory for the SWP. (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 had only one intermediary. Thus, the reformulation of the SWP (Pool and Kochen's original notion) by Milgram was not 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 SWP. As it turns out, Milgram's reformulation is just another question that can be asked about intermediaries beyond 1. If two of Korte and Milgram's chains had one intermediary (indeed, Travers and Milgram had one such chain also) then all that is required to test the one-intermediary problem is a sufficiently large number of SWT tries.

Alternatively, (and this is an experiment we are now conducting, along with Lin Freeman) we could lock 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 SWT experiment. How could one test this?) By doing this 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. Say 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 SWP) are expensive and difficult to do; but they are 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.

Many ideas for SW experiments are generated by an important article by Hunter and Shotland (1974). 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." The categories are students, faculty, and administrators at Michigan State University.

They show that the mean "distance" (ie., the mean number of links between these groups, as measured by the familiar SWT) are not easily taken at face value for several reasons. First, the criterion used to move the folders is not the 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 uniterpretable results.

Second, there is the (by now familiar) problem of attrition, ie., incomplete chains. As Hunter and Shotland put it, 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 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, Harrison White 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, p. 323). Hunter and Shotland provide an excellent discussion of the mathematics of this problem.

Hunter and Shotland also point out that attrition is a critical problem. However, were a juicy rumor -- rather than a

folder -- circulating, it seems to us more likely that "incomplete chains" would disappear, or at least be drastically reduced. Thus, it might be more accurate to modify the experiment to remove incomplete chains (either by altering the details, or by simply increasing starter numbers by a factor of 100) than to interpret the data as they stand. Obviously, one can't tell--but surely we should try to find out in future experiments.

Hunter and Shotland's point about attrition, is sociologically interesting. They point out that different sub-populations 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 say "an analysis based solely on completed chains would vastly under-represent the number of passes to and from the high loss group -- ie., 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" (323). 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 present a model which compares observed chain length with "ideal" length (ie., 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 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. In many areas of research it would be natural to let the categories be found from the data (clique-finders, factor analysis, and so on). We suggest that research on these lines, combined with the attractive Markovian hypothesis, could well be fruitful.

A similar approach to Hunter and Shotland, but purely theoretical, was taken by by Stoneham (1977). She created a spatial model 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 Poole and Kochen, the labor would be immense, and the simulation method seems most suited to the problem). She was able, by adjusting two parameters, to fit the mean and standard deviation of path lengths found by Travers and Milgram. The dificulty with this type of study, of course lies in its interpretation. 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. Clearly, more data are necessary to feed into her theory.

In both of Bochner's published experiments the SWT was modified to allow open-ended chaining. In the 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," ie., 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. Bochner et al conclude that this may be evidence for the idea that urban high-rises are not communities, and that folks really may not have known anyone on a first-name basis. They admit that low participation may also have resulted from low motivation. This is a preliminary research report, and doesn't add much to the literature except for the potential negative results on participation in urban high- rises.

Another study by Bochner, Buker and McLeod (1976) was done at the East-West Center, in Honolulu, Hawaii. They selected two dorms, one female, the other 80% male - 20% female. The dorms 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 starters by national group and by sex, controlling for length of residence. 18 starters were chosen and all agreed to participate. Unlike the Sydney high-rise, these foreign students were understood to be a "community" of sorts, and it was known that many residents knew each other.

16 folders went at least one step beyond the starter. 8 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 dorm go mainly between people from the same country. They also go mainly between people of the same sex, as in all SW studies.

Lundberg (1975) compares the data from the Travers and Milgram study (1969) to data he collected on two lare organizations in Dallas during 1971-72. He used 4 targets at the top of each organization, and 120 starters for one firm (30 per target), and 342 starters (85 per target) in the second firm.

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 (3.36 vs. 5.25 links for Travers and Milgram), but there are other, obvious reasons the success rate might be high. In any event, the SWT seems very useful for the comparative study of organizations, especially if one wanted to see how well communication flowed between line staff and top level management.

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 the open society experiment, the convergence rate is much faster than 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, especially for executive targets.

The Lundberg study shows the potential value of the SWT for 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.

One of the many things which worried us about the SWT 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 biassed data sets.

Clearly, to get any statistical reliability one needs many starters and many targets; 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 starters were instructed in the SWT 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 targets. With each choice, starters 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 the target caused the starter to think of his or her choice; or the occupation of the target was responsible for the choice; the ethnicity of the target; 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. Only 35 choices are necessary to account for half the world, however. 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; and 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 target, the type of choice used by the majority of starters was always a friend or acquaintance, and never family. For any given target, the most likely sex of the choice (i.e. over all starters) can be predicted accurately on 82% of occasions. This sex tends to be male, unless both starter and target are female, or if the target has a low-status occupation. Additionally, any given starter was most likely to pick a male choice for any target, except for the female starter-female target combination, when female choices were more likely. This was correct on 64% of occasions.

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

4) The decision as to which choice was made appears to depend primarily on the occupation of the target, and secondly on the distance (near/far) from Morgantown, West Virginia, where the experiment took place.

5) The expression "having one's man in" can be partially quantified. Define a choice to "handle" a state 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 starter, on average, half the states are each handled by a single choice.

6) The accuracy of starters' 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.

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, 1976a; Bernard and Killworth, 1977) that people do not know, with any accuracy, who they talk to in an organization over periods of a week or more. We have also shown (KB, 1976b) 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 any clique-finder, such as CATIJ, block models, etc.) 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, ie., 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, ie., outside the office." (We have not finished the experimental design and we would appreciate suggestions from readers on this).

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 CATIJ or other 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 SWT 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 staf and mid-level management outside the office." Then, the SWT could be used to introduce an intervening variable (length of chains between levels of organizations) and might become a potential evaluation tool.

A preliminary study by Erikson and Kringas shows the potential of the SWT 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 (ie., start a chain). Eventually, 22 of them dropped out (ie., the chains were incomplete). As preliminar as this

study is, however, it raised a number of interesting questions, as well as possibilities for using the SWT 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 comparative work on political networks, both behavioral and perceived. Suppose the vice-president-in-charge-of-moving-junior-executives in GM were the target. And suppose that 100 of the most junior executives in GM were starters. And suppose the starters 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 A to B. However, if we ask whether A can influence B via an indeterminate number of others, then 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. We feel it is vital to make network analysis a behavioral as well as a cognitive science. The SWT, and variants thereof, seem to us to be good experimental first-tries.

References Cited

Bochner, Stephan, Eloise A. Buker and Beverly M. McLeod
1976 Communication patterns in an international student dormitory: a modification of the "small world" method. Journal of applied social psychology. 6:275-290.

Bochner, Stephan, Robert Duncan, Elizabeth Kennedy and Fred Orr
1976 Acquaintance links between residents of a high rise building: an application of the "small world" method. Journal of social psychology. 100:277-284.

Erikson, Bonnie with Paul R. L. Kringas
1975 The small world of politics or, seeking elites from the bottom up. Canadian review of sociology and anthropology. 12:585-593.

Feinberg, Stephen E. and S. Keith Lee
1975 Small world statistics. Psychometrika and supplements. 40:219-228.

Guiot, Jean M.
1976 A modification of Milgram's small world method. European Journal of Social Psychology 6:503-507.

Guiot, J. M. and Cannon, S. R.
1975 The small world method: A modified approach. Working paper, Department of Organizational Behavior, SMG Boston University.

Gurevitch, N.
1961 The social structure of acquaintanceship networks. Ph.D. dissertation: MIT, Cambridge, Massachuttes.

Hunter, John E. and R. Lance Shotland
1974 Treating data collected by the "small world" method as a markov process. Social forces. 52:321-332.

Killworth, Peter and H. Russell Bernard
1978 The reverse small-world experiment. Journal of Social Networks (in press).

Korte, Charles and Stanley Milgram
1970 Acquaintance links between white and negro populations: application of the small world method. Journal of personality and social psychology . 15:101-118.

Lundberg, Craig C.
1975 Patterns of acquaintanceship in society and complex organization: a comparative study of the small world problem. Pacific sociological review. 18:206-222.

Milgram, Stanley
1967 The small world problem. Psychology Today. 1:61-67.

1969 Interdisciplinary thinking and the small world problem. In Interdisciplinary relationship in the social sciences, Muzafer Sherif and Carolyn W. Sherif, eds. Chicago: Aldine.

de Sola Pool, Ithiel and M. Kochen
1978 Contacts and influence. Journal of Social Networks (in press).

Stoneham, A.K.M.
1977 The small world problem in a spatial context. Environment and planning A. 9:185-95.

Travers, Jeffrey and Stanley Milgram
1969 An experimental study of the small world problem. Sociometry 32:425-43.

White, Harrison C.
1970 Search parameters for the small world problem. Social forces. 49:259-64.