

WHY ARE THERE NO SOCIAL PHYSICS?

by

H. Russell Bernard
West Virginia University

and

Peter D. Killworth
Department of Applied Mathematics
Cambridge, England

INTRODUCTION

It is, or ought to be the business of anthropology to formulate laws which govern the formation and evolution of human groups. This is precisely what appears to have been the motivating force behind the work of the classic evolutionists. It was certainly Boas' goal when he sought to withdraw anthropology from armchair theorizing and to lead it in the direction of inductive studies. It was obviously Steward's goal when he wrote "Cultural Causality and Law" (1949), and when he developed his ideas of cultural ecology (1936). And it is spoken for today most eloquently by Harris, in his constant plea, since 1964, for transforming anthropology from a descriptive to what he calls a "nomothetic science."

The classic evolutionists did try to formulate universal laws to account for the evolution of human groups.¹ Having had no scientific basis on which to develop such laws, they made them up, and they had the misfortune to have made up nonsense. When this was discovered (by Boas, most importantly) the nonsense was thrown out--and so, in large measure, was anthropology's

association with the search for laws. Since the mid-1940's, there has been a resurgence of interest in the search for universally applicable laws of culture and culture change. Given nearly 40 years of interest and work, we may now ask, with no lack of charity, why the effort has been so singularly unsuccessful. In this paper, we will discuss this problem, and we will offer some suggestions for how we might proceed.

REASONS FOR LACK OF SUCCESS

As we see it, there are two reasons for the failure of anthropology to formulate what Comte called "social physics" over 140 years ago.

The first reason is the familiar "forest and trees" problem, or the concentration on detail so that global generalities are not considered. In most physical phenomena there is turbulence on all length and time scales. In meteorology, for example, there are small eddying (ie., turbulence) motions in the atmosphere, almost on molecular scales, which occur in fractions of seconds. There are also eddying motions on larger scales which we see as thunderstorms or hurricanes, with time scales of hours or days. And from paleoclimactic records, there are strong indications of atmospheric turbulence on much longer scales (thousands, and even millions of years). At this stage of understanding in physics, it is not clear that we can even determine what the "average" atmospheric circulation is--over what time scale should one average in order to find the mean circulation?

We must expect a similar kind of turbulence to affect social groups and human behavior. Indeed, much of what we see in our daily lives is plausibly describable as turbulence. How much of what anthropologists observe is, in fact, turbulence? How much is a time signal of some "mean state," which may or may not exist? And how can we tell the difference? For example, is the distinction between patrilineal and matrilineal descent important? Perhaps such differences in kinship activity are

just small eddies on some larger mean than we usually assume? And what is the larger mean?

The trouble is that, by definition, what we observe in field research is detail, not mean behavior. In fact, there is nothing else to observe but details. At the risk of great oversimplification, the structuralists have argued that the goal of social anthropology is to find the structure of human relations, so that the turbulence of the behavior itself (i.e., people) may be filtered out. The typical American response has been to argue in favor of the study of culture, (that is, the information people carry in their heads, their behavior--presumably generated by the information--and the material results of behavior) in order to find the structural signals.

Consider, however, that both of these pursuits may be wrong. Both details and structure are surely straightforward manifestations of the results of the laws we are all trying to uncover. There tends to be an assumption in the social sciences that human behavior is complicated, while physical processes are simple. But all that can really be said is that the laws of physics are devastatingly simple, though their application may produce remarkably complex outcomes.

It is difficult to imagine a simpler law than Newton's second law (force equals mass times acceleration). Yet its application produces coastal upwelling in the ocean (and concomitant fishing industries); it also produces tornadoes, and wide variations in oceanic and atmospheric climate in regions which seem to be very similar; on Jupiter, the Great Red Spot, which has existed since at least the 1600's, is predicted by several subsets of physics based on this simple law; and so on. These physical events are very, very complicated; and it is impossible to derive Newton's second law inductively from observation of these messy phenomena. At the structural level, the so-called general law that "storms exist in the atmosphere everywhere" still yields no information which would derive Newton's second law. In fact, this law was derived hundreds

of years before it was applied to geophysical phenomena. This leads to the peculiar suggestion that, in anthropology, we might be better off making a shrewd guess about laws, and seeing what their application produces in a given situation (ie., a culture), than to attempt to induce the laws of society directly from raw data if we don't know what is meaningful to measure, then in other words, modeling a system and comparing the predictions of the model to data might be as productive as attempts to interpret data directly.

The second reason we have failed to develop a social physics involves the fact that most processes--and certainly social processes--are dynamic, rather than steady-state.² It is notoriously difficult to guess a dynamic process from a snapshot of it; but even on the largest (intercultural) scale, our observations are at best a snapshot of a constantly changing situation. The problem of how to measure meaningful phenomena is exacerbated if the snapshot is blurred, as is the case with data obtained by nonquantifiable measurements, such as participant observation.

Of course, a concern with culture dynamics has been a cornerstone of anthropology; repeated observations on a culture at least have extended our view to a series of very blurred snapshots. But, even if the data are error-free, these snapshots may have been taken at the wrong time intervals to capture what is actually occurring. Two blatant examples of this are given in Fig. 1. In both cases the time signal is a sinusoid in time superimposed on a mean. In Fig. 1a, regularly spaced observations yield the inescapable, but incorrect, conclusion that the signal is steady. More frequent observations (on a different process), as in Fig. 1b, would show that the signal was a slowly varying sinusoid, whereas in fact the true signal oscillates very rapidly.

Such incorrect conclusions can occur in any branch of physics--and this applies even if the observations are precise, quantified, and reproducible. If none of these conditions is

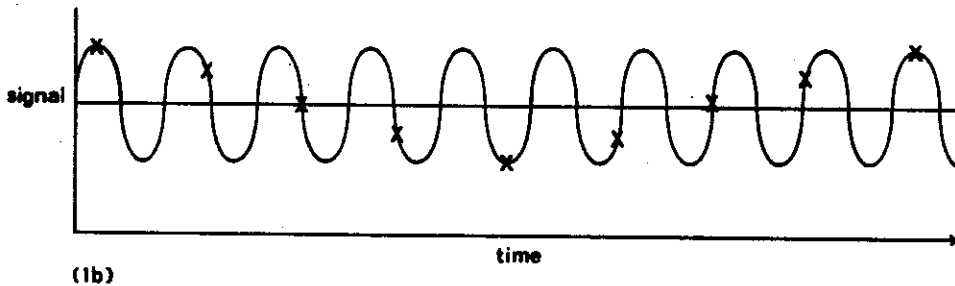
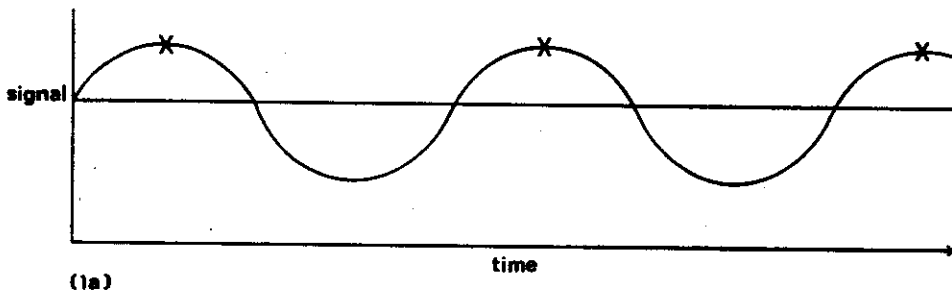


Figure 1. Regularly spaced observations, over time, of two different signals. X represents an observation, and the firm line the signal. The signal mean is represented by the horizontal line.

satisfied (as in blurred ethnographic observations) the opportunity for incorrect deductions is magnified. Thus, a shrewd guess as to what any social law might be must be phrased in quantitative terms, or it simply can not be tested against reality. Moreover, the manner in which reality is measured can itself generate a false signal--as Fig. 1 demonstrates.

We do not claim that all data must be collected in quantifiable form, only that laws must be expressed in such a manner. Good descriptive research is clearly vital if we are to test social laws, and precise measurements of unimportant, meaningless phenomena are inimical to such tests.

SOME ATTEMPTS TO GET AT STRUCTURE AND LAWS

We have approached these problems--what is meaningful to measure, and how should one measure it?--in a series of studies since 1972. We have assumed (and continue to assume) that networks of relations amongst people are a meaningful object of study. That is, there is a structure (with describable properties) inherent in the networks of relations. By "network" all we mean is a set of lines (which represent relations) connecting a set of points (usually people).

We began by rejecting the sociometric bias towards affective relations. That is, most sociometric networks are based on asking people who they like. Instead, we asked people about their communications with others in natural groups (task forces, housing units, bureaucracies, and so on). We obtained data by asking people some form of the question "who do you talk to, and how much?" because this seemed to yield firm, quantified data. It was also, historically, the instrument most often used by researchers in gathering sociometric or network data. Even at this stage of naive empiricism, we assumed that there might be some form of error in the data, i.e., people giving us wrong answers to our questions, and the inevitable copying and coding errors. This led to the creation of a descriptive tool, called CATIJ (Bernard and Killworth,

1973; Killworth and Bernard, 1974), which remains the only such algorithm which has been shown to filter out some forms of random error in network data. Unfortunately, we have since learned that error in network data is both nonrandom and unknown; thus, filtering random error from such data (a plausible thing to do on statistical grounds) is somewhat pointless.

In spite of this, algorithms like CATIJ (usually called "clique-finders" in the literature) do seem to yield practical results. For example, when managers are confronted with clique-structure pictures based on CATIJ (and other algorithms), they usually react enthusiastically. They have no difficulty interpreting the results; or in explaining, to their own satisfaction, why some groups seem isolated, why some people appear to be brokers, and so on. In other words, people who know intimately and intuitively about the communication relations in their own group have no difficulty relating to the results of clique-finder analysis.

Nonetheless, the nagging doubt as to what we were describing with CATIJ remained. We thought that we were measuring how much people talked to each other, because that is what we asked them. It was natural to assume, therefore, that we were describing the communications structure of groups. It seemed sensible to test this assumption.

In a series of papers (Killworth and Bernard, 1976; Bernard and Killworth, 1977; Killworth and Bernard 1979a; Bernard, Killworth and Sailer, 1979) we have studied a variety of naturally occurring groups whose communication was either automatically monitored or could be easily monitored by an observer. We wanted to find out if people could remember, with any accuracy, who they talked to.³ For if they could not remember, then taking recall data about communications was worthless as a means of studying communications and its structure.

At the simplest and most basic level, person *i* is accurate if, when he says he talked to person *j* by some amount,

then he did. We examined this possibility and found (not surprisingly) that he didn't. Over many data sets, people reported their dyadic communications inaccurately more than half the time. In fact, this was the best they could do.

Of course, we should be analyzing network data for structure, so we examined triadic structures⁴ and clique structures. We felt it was possible that if i, j, k, l, and m formed a structural unit, or clique, then i might state that he talked to j and k when, in fact, he talked to l and m. If, indeed, people think of themselves as members of such structural groups, then this would clearly be less inaccurate than our findings at the dyadic level. In other words, there might be a structure to inaccuracy of cognition itself.

Unfortunately, the cliques formed in cognitive and behavioral data were quite different. They differed from one another, on average, by 160%. This left us in a difficult position; CATIJ (and algorithms like it) had not been describing what network theorists had assumed they were describing.⁵

Perhaps the links between individuals in a network were simply too "turbulent" for structural signals to be evident. In 1976 we began to feel that a more global investigation was more likely to yield useful results about the laws of social behavior, even though this would mean leaving the relatively neat confines of closed social groups and confronting the outside world as a whole.

Of the large-scale, and therefore relatively uncontrolled, studies which had been performed to that date, we were most impressed by Milgram's "small world" technique (Milgram, 1967). This had as its goal the discovery of pathways between any pair of individuals in the U.S., where these pathways were prescribed to be only between people who knew each other (usually on a first-name basis). The technique, which involved the mailing of packets across the country, had great aesthetic appeal (it is neat--and cheap!). It also yielded some very interesting (even spectacular) facts about the (globally defined) social/communication structure of the U.S. Because of Milgram's

work, and repeats of his experiment by others (Hunter and Shotland, 1974; Lin, Dayton and Greenwald, 1978; and see Bernard and Killworth, 1978 for a review of all the small world literature to date), it is now known, for example, that any two randomly-chosen white people in the U.S. are connected, on average, by 5.25 intermediaries. (The average distance between any white and any black person is about one intermediary longer).

However, the number of links between any two people (or even the routes between pairs in a group of specific people) is really very limited information about the structure of the links and the rules which generate that structure. The structure is clearly a "pattern" of relationships between people. Each person in an ego-centered network has some (unknown but probably large) number of links to the world. By "links" we mean people a person knows and can use for some purpose--like sending a folder or packet to a target in a small world experiment. Thus, a small world experiment tells us about one link for each person along a chain to a target. If many people are asked to start folders on their way to a specific target (ie., what Milgram did), then we find out a lot more information about the target, because of the large number of incoming links involved.

Although clearly pioneering work, the small world technique has two disadvantages: 1) it yields very little information about the circle of acquaintances around a randomly chosen person. A very large number of small world experiments would be needed in order to acquire this type of information about a large number of targets. 2) The small world technique tells us nothing about the rules which govern the structure. These rules determine, amongst other things, to whom an individual decides to send the folder in a small world experiment. In an initial attempt to discover some of these rules, we carried out a similar, but inverted, procedure we term the "reverse small world" experiment (Bernard and Killworth, 1978).

This involved asking many individuals to tell us, for each of over 1,000 mythical people in a list (each provided with a short biographical summary), to whom they would send an (equally mythical) folder in a small world experiment, and the reasons for their choice.

The data so created proved very fruitful, and it was possible to formulate an initial set of rules which appear to govern how an individual selects one of his acquaintances to be the next link in a small world chain (and by unproved extension, how and why he knows the people in his network). Some of these rules proved very powerful: on 81% of occasions, it is possible to predict the most likely reason for an informant to choose an intermediary from a simple linear function of certain target characteristics (location, occupation, and gender).

An attempt to model the decision-making process (see section on models) revealed some remaining gaps in what we need to know if explicit rules are to be created. For example, we had provided our own list of reasons for choices of an intermediary, and we had chosen which pieces of information about targets to give to our informants. A more ethnoscientific approach would be to ask informants what they need to know about a target, and to let them tell us why they chose a particular intermediary for any given target.

This experiment is currently underway. Informants are now told nothing a priori about the targets (not even the name of each target), but instead are allowed to ask as many questions about each target as they like before they make their choice. Once a choice is made, informants tell us which question(s) that they asked enabled them to make their choice, and why. These data provide the basis for numerical models of both the decision-making procedure (probably experiment-dependent) and the rules which govern how and why people know each other (hopefully not experiment-dependent).

MODELS

Suppose that at some stage we have some prototype laws for social structure. How can we test them in any stringent fashion? As noted above, the first requirement is that these laws be quantitative. It is clearly important to make predictions about structure from the proposed laws in order to see if this matches what is observed in actual groups. If the laws remain qualitative, then only qualitative comparisons are possible, and hence no stringent test can be made.

However, quantitative laws do not simply allow an extension to quantitative comparisons. If such laws are expressed as mathematical equations (now possible because they are quantitative) then one can make far-reaching deductions about the solution of such equations which were not obvious from their formulation. A good example from physics would be the appearance in the solution to a simple oceanographic circulation problem (Stommel, 1948) of a strong boundary current identifiable as the Gulf Stream--this in spite of the fact that such a current was not put into the circulation problem to begin with. An example in social science (Killworth and Bernard, 1975) is the prediction of unlikely and non-obvious possible steady-state solutions to equations purporting to represent how the relations in a closed group changed over time. Whether or not these solutions (ie., predictions) occur in the real world is a test of the model. Furthermore, such a test could not be made if the model had not been expressed mathematically.

As an example of what we mean here, consider the quantification of the rules deduced in the reverse small world experiment (Killworth and Bernard 1979b). At the qualitative level we formulated a plausible flowchart which represented our intuition about how choices were made by each informant in our experiment. Since we used the data to create this flowchart, we could hardly test it with the data. (Testing at this level is the point of our current experiment.)

However, a direct test exists once the flowchart is converted to quantified form. This yields a Markov process with states representing subsets of the U.S., and transition probabilities for the folder to pass from state to state. This enables immediate computation of statistics relating to path length between individuals--and these are directly comparable to data generated by Milgram and others in small world experiments.

The classic evolutionists of the 19th century began by making some plausible assumptions about the global phenomenon of social evolution. By working on such a grand scale, they neatly avoided the problem of lower scale-level turbulence (but note that all of human evolution--physical as well as cultural--may be a form of turbulence in an even longer scale than we can deal with here); and they formulated some reasonable hypotheses about how social evolution might have proceeded. As we see it, the problem faced by Tylor, Morgan, Bachofen and the others of that time was not that they came up with fanciful schemes. They did propose some rather outrageous courses of social evolution, and they were properly chastized by Boas and his followers. The real problem, however (and one which Boas no doubt appreciated as a physicist), was that there was no way to test the schemes of the 19th Century anthropologists; they simply provided no quantified form of their ideas. Steward's brilliant paper on "Cultural Causality and Law" (1949) was a step in the right direction. He, too, made up some plausible inputs to a model of social evolution, including the fact that evolution might not have proceeded in a single (ie., unilineal) manner.⁶ Steward also assumed that there was a relationship (not quantified) between technology and the environment, and that larger populations required higher levels of "sociocultural integration" to hold them together. This scheme, of course, is very much like Durkheim's mechanical-organic solidarity; Tonnie's *gemeinschaft-gesellschaft*; and Redfield's folk-urban continuum.

Given the various plausible inputs of these qualitative schemes, let us see if we can develop a quantitative model of social evolution. Such a model should be informed by available knowledge, or by intuition; and the results (i.e., the predictions) should be testable against data. In this case, the data for testing must come from archeology and from comparative studies of current societies as proxies for what might have adhered in the past. This, of course, is an advantage we have over the theorists who tried to model evolution 100 years ago. We have far more data on the temporal and spatial variation of human groups than they had. Therefore, anything we now propose off the top of our heads will surely run right into some facts which contradict our model's predictions. But this is precisely what we need in order to improve on a model.

We must stress very strongly that the "thought" model of social evolution which we shall now propose is not meant to be a reflection of objective reality. It is intended as an example of how one might proceed in formulating and testing laws. If the reader feels that our rules are unrealistic (as indeed, some are) then it is easy to modify them.

The model proceeds from two very simple hypotheses. First, we assume a limit on people's ability to comprehend too large an unstructured group, and that everybody must feel that they are "connected to the rest of the known world" in some culturally appropriate ways (i.e., kinship, friendship, collegueship, etc.). A theory of random groups (Bernard and Killworth, 1973) showed that applying the known psychological limit of "the magic number seven" (Miller, 1956), produced two limits on comprehension of human groups. It turned out that only populations under 140 could remain totally random and still be comprehended by members of that population. Above this number, splitting into structured subgroups was necessary, until a population of 2,460 was attained. Above this, some central government or similar structure was required. This

agrees well with the various commonly understood limits on populations such as hunting and gathering bands, peasant villages, neighborhoods in cities, etc.⁷

The second hypothesis is that the ability of a group to form any kind of internal structure depends solely on whether that group has yet developed the necessary "technology." Which "technology" would of course vary from area to area; we are using the word in the widest sense, to include agriculture, organized warfare, bureaucracies, abacuses, stirrups, etc. For simplicity, we refer here, then, to "technology" in the generic sense.

Let us now quantify our two hypotheses. We shall assume that a group with a successful hunting and gathering subsistence technology experiences population growth at a steady rate, until its population exceeds a limit Q_1 (here taken as 140). If the group possesses no other, more sophisticated technology, as we have defined it (such as hoe agriculture), then it cannot restructure itself, and so must split; we assume it divides exactly in two for simplicity. Of course, the splitting would in all likelihood have occurred much earlier, lacking agriculture; but this quantization permits a straightforward simulation, so we retain it. If there is sufficient technology, then the group (and those known to it in other groups) restructure into a larger social unit; in Steward's terms, they achieve a new level of sociocultural integration.

When the second limit on population, Q_2 (here 2,460) is reached, (implying the availability of technology) a central government must be formed, so that each individual can comprehend his own place in the world. This is necessary, because each individual must be able to use his network to access all parts of the new social unit; without a central structure, the limitations on size of an individual's comprehensible network would make this impossible.

For demonstration purposes, consider a land area with 25 unstructured groups (bands) of 30 people each, as shown in

Fig. 2. The bands are distributed orthogonally purely for numerical convenience, not to represent reality. In the real world, the groups would possess sizes between, say, 15 and 140 and would be distributed randomly in a spatial sense.

The process will occur in stages. We do not specify how long a stage lasts; it may be thousands of years. At every stage, the population of each group doubles (again, this is for simplicity; we neglect disasters and other random external events). We assume a probability at any stage of 20% of any group making contact (through trade, marriage, etc.) with any orthogonally situated neighboring group; a 5% chance of some new technology being discovered or invented by any group; and a 10% chance of that technology diffusing between neighboring groups who already know each other.

This may seem rather probabalistic. However, we regard this as a subsuming of all the other, neglected laws to a simple degree of randomness.

Figure 2. Initial conditions for models consisting of 25 groups each of population 30.

30	30	30	30	30
30	30	30	30	30
30	30	30	30	30
30	30	30	30	30
30	30	30	30	30

Figures 3 to 6 show a Monte Carlo simulation of the evolution of the system. After one stage (Fig. 3) two groups have developed new technology, and various groups have made contact. The population size is still smaller than 140, so no splitting or structuring has yet occurred. One stage later (Fig. 4) the population size is approaching 140; more technology has appeared, and more groups know each other.

At the next stage (Fig. 5) the situation changes dramatically. All populations are now 240. Seven groups do not possess access to the appropriate technology for restructuring, and so subdivide to groups of 120. Two pairs of groups, who know each other, have the technology and form organizations of size 480. The remainder, who know each other through a complex network, exceed 2,460 and so form a centralized structure, with population 3,360.

The next stage (Fig. 6) finds the small groups exceeding 140 people. However, contact has been made with either the small organization, or the central structure, and these groups are subsumed. This, incidentally, shifts one small organization, by force of population, into a central structure. One small organization remains.

At the last stage, contact between the central structure is made, and the entire population becomes one large structure.

Perhaps the clearest point to emerge from this crude simulation is the almost accidental creation of multilinear evolution. Some groups pass through all stages of structure; some skip stages and form central structures quite abruptly. In fact, the accelerating progress from stage to stage is simulated quite well by this model. So from very simple hypotheses (two) a variety of social structures has been produced which were not obvious from the original hypotheses.

These particular results derive from a single Monte Carlo simulation with one set of parameters. However, it is straightforward to see how the results are affected by changes

Figure 3. After one stage, all populations reach 60. Populations marked T have technology; a firm line shows connection between groups which know each other.

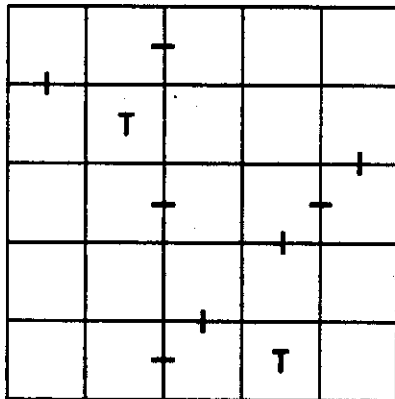


Figure 4. After second stage, all populations reach 120.

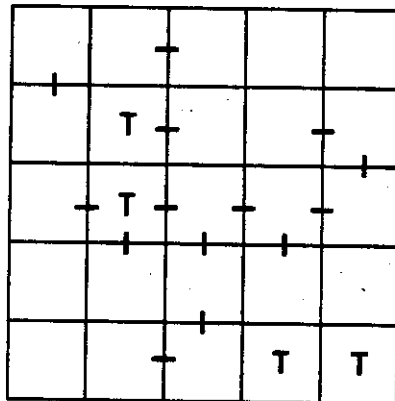


Figure 5a. After third stage population reaches 240. This cannot remain, so Figure 5b forms.

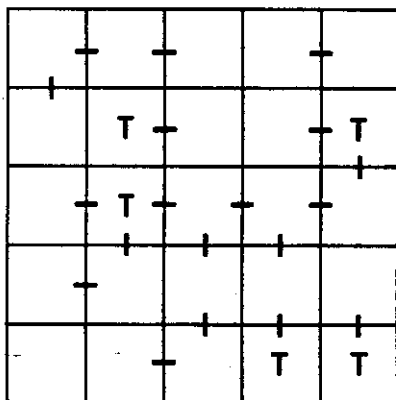


Figure 5b. The result after splitting and amalgamation. Seven groups have split to pairs of 120 (and remain hunters and gatherers). Two small organizations of 480 have appeared, and a large central structure of 3360.

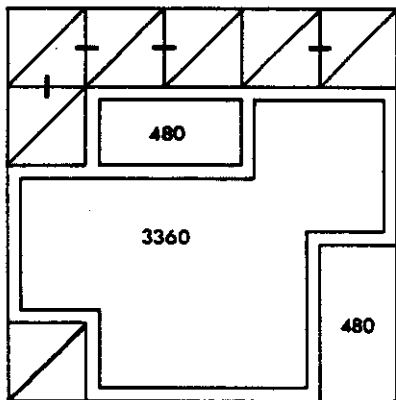


Figure 6a. After fourth stage. Hunting-gathering groups reach 240, but each makes contact with a larger group. This leads to Figure 6b.

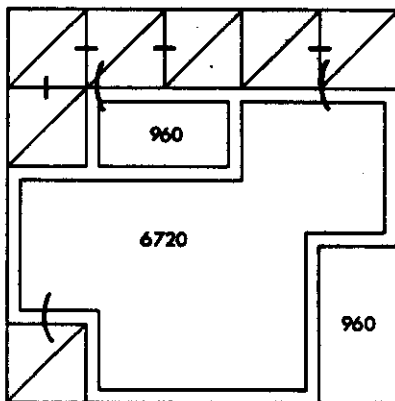
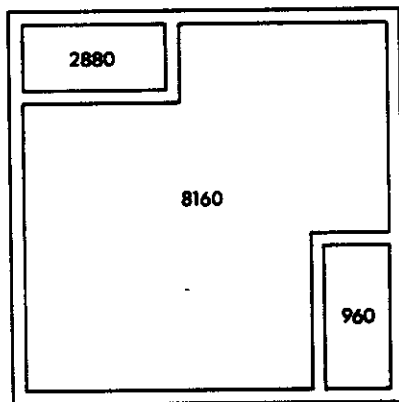


Figure 6b. The subsuming of groups by the 960 small organization moves it over the 2460 limit, and it becomes a central structure. The other 960 remains.



in parameters. If Q_1 or Q_2 are altered, little occurs save an adjustment in time scale of the problem (i.e., if Q_1 or Q_2 increase, there is more time for a band or tribe to discover technology before splitting becomes necessary). If the probability of making contact is made very unlikely (e.g., bands or tribes are geographically well segregated) then continual splitting occurs until technology arrives. If, conversely, contact becomes very likely, complex organizations occur everywhere very rapidly.

If technology becomes very unlikely then small organizations keep collapsing (increased unrest, crime, etc.) as Q_2 is exceeded without technology. Conversely, technology becoming very likely forces centralized structures into being as soon as population levels exceed Q_2 anywhere. Finally, technology diffusion has only a quantitative effect on the results.

Thus, the variety of behavior possible from the model is fairly large with respect to parameter variation--but the same is true for any individual tribe in any case. The reproduction of the various social structures above is not, therefore, due to parsimonious choice of parameters.

DISCUSSION

In 1824, Auguste Comte wrote "I believe that I shall succeed in having it recognized ... that there are laws as well defined for the development of the human species as for the fall of a stone" (see Sarton, 1935, p. 10; also quoted, in translation, in Stimson, 1962).. His enthusiasm was shared by the Belgian astronomer and statistician, Adolphe Quetelet, whose "Treatise on Man" in 1835 carried the audacious and hopeful subtitle "Social Physics." "Would it not be an absurdity," asked Quetelet "to suppose that, whilst all is regulated by such admirable laws [of nature], man's existence alone should be capricious, and possessed of no conservative principle?" (1842:9).

Comte never troubled himself with the detailed empirical research required to uncover the Newtonian laws of social

evolution which he believed existed. Comte was content instead to deduce the social laws and to leave "the verification and development of them to the public" (1875-77, III:xi; quoted in Harris, 1968). This would have been perfectly acceptable had only Comte formulated his laws in such a way that others might test them. Comte, however, could not be bothered with numbers.

Not so Quetelet. In painstaking detail, he presented aggregate statistics on crime and mortality in Europe; and he extracted from those data some very strong signals. For example, Quetelet concluded that, at least for Paris of his day, crime proceeded with greater regularity by age than did mortality so that "each age [cohort] paid a more uniform and constant tribute to the jail than to the tomb" (1942; viii).

Quetelet also recognized the limitations of the inductive approach to the discovery of social physics. While he was clearly more at home with statistics and inductive studies, if data were not available on a question of scientific interest, Quetelet was prepared to be the arch-deductivist. In the absence of time-series data from the past, "we must do as astronomers have done in the theory of arbitrary constants--make an abstraction at first of the disturbing force, and return to it afterwards when a long series of documents permits us to do so (1842:8)." It seems to us that the 19th century social evolutionists, following Comte, made plenty of abstractions, but (again like Comte) failed to quantify them so that they might be falsified. The historical particularists tried to provide the "long series of documents" spoken of by Quetelet. But the particularists had neither an overall abstraction to work with, nor quantified data. What they did have was empathy, developed by sustained phenomenological fieldwork. Empathy and insight are useful tools in divining laws⁸ of social evolution, or any thing else. To be testable, all such laws must be quantified, of course. For testing models of social evolution, however, there is one more requirement, namely, the assumption that the array of present societies will serve as a

proxy for the varied societies which have existed in the past: a change from spatial to temporal evolution, in other words. This assumption may be unwarranted; but it must be made. It is made all the time in the physical sciences; laws which are derived from data on current phenomena are presumed to be the same laws which governed those phenomena in the remote past. That is, the phenomena may change, but the laws don't. If we make this assumption in anthropology, then the laws will hold over many realizations of current phenomena--such as social groups. If we deduce a law which holds for an array of present cultures, then it is legitimate to assume that the law holds for an array of cultures through time. At the surface level, of course, there are good reasons to suppose that the details of life in surviving band and tribal level societies are not representative of such detail in the past. This should not prevent us from modeling social structural evolution formally, and testing our models against extant data.

We believe that network studies are a good way to examine social structures. There may, of course, be perfectly good alternative ways to investigate social structures and, by extension, social evolution. However, for now, we envision a series of network studies (such as the reverse small world study described above) which elucidate the structural properties of an array of cultures. This may be difficult: perhaps, in a very large group, informants are inherently incapable of giving accurate information to a researcher about their networks of interaction. This would account for the inaccuracies we have found in our behavior-cognition studies, and would lead us to conclude that it is the very limitation of human information processing ability (a testable quantity or set of quantities) which is the foundation of social evolution. Progressively higher level mechanisms of sociocultural integration may be seen as the soft technologies which allow us to feel that we are part of larger wholes than we can comprehend.

ACKNOWLEDGMENT

This work was supported under Office of Naval Research Contract No. N00014-75-C-0441-P00001, Code 452. The opinions expressed in the 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. Distribution of this article is unlimited. We are grateful to Ann Paterson and Ronald Althouse for criticisms of an earlier draft.

NOTES

1. See Carneiro, 1973 a and b, and Harris, 1968 for reviews of social evolutionary thinking in anthropology.
2. Steady-state should not be confused with static; a steady-state process may be in motion, but no quantity in the motion varies over time.
3. For example, we surreptitiously recorded all the conversations amongst a group of radio hams over a period of a month. Then we simply asked each of them how much he had talked to each of the others. A great variety of tests was done on a number of different kinds of groups, in order to see what might account for differences between individuals and groups in accuracy of recall. The interested reader is referred to the series of papers in the bibliography.
4. The reason for examining triadic structures is historical. In a series of papers, Holland and Leinhardt have examined triadic structures, invented a measure of structures and produced relevant statistics on such structures. The interested reader is referred to Holland and Leinhardt, 1975, for a review of their procedures, and to Killworth and Bernard, 1979a for our use of their procedure and our findings on matched sets of recall and behavioral data.
5. This might lead some people to argue that cognition, rather than behavior, is the sensible thing to study. Behavior such as communication has the merit of being obviously correlated with other observables, ie., other behaviors. For example, innovation diffusion is clearly a behavioral phenomenon. Ideas pass amongst people only when they communicate. We maintain that if cognition about a behavior does not relate to the behavior, then it is unlikely to relate to anything else, either--except perhaps to other cognition data.

6. Carniero (1973a) has argued very persuasively that a) the classic evolutionists were not so "unilineal" as everyone supposed in this century; and b) the formulation that Steward offered was really quite "unilineal," but Steward could never have associated himself with this.
7. This does not account for structure within one's acquaintances, like friends, family, participants in hobbies, etc. Extension of our arguments to this case produced a figure of 24-27 close acquaintances for a typical individual; this has been confirmed experimentally by Pattison (1977) and his associates in their work on network therapy.
8. See Scriven, 1969, for an excellent discussion of the role of empathy in logical positivism and the search for law in the social sciences.

REFERENCES CITED

- Bernard, H. Russell and P. D. Killworth
 1973 On the Social Structure of an Ocean-Going Research Vessel and Other Important Things. *Social Science Research* 2(2):145-184.
- 1977 Informant Accuracy in Social Network Data II. *Human Communication Research* 4(1):3-18.
- 1978 A Review of the Small World Literature. *Connections* 2(1):15-24.
- Bernard, H. Russell, P. D. Killworth and Lee Sailer
 1979 Informant Accuracy in Social Network Data IV. *The Journal of Social Network*, in press.
- Carneiro, Robert
 1973a The Four Faces of Evolution: Unilinear, Universal, Multilinear, and Differential. In *Handbook of Social and Cultural Anthropology*, John J. Honigmann, ed. Pp. 89-110. Chicago: Rand McNally and Co.
- 1973b Classical Evolution. In *Main Currents in Cultural Anthropology*, Raoul Naroll and Frada Naroll, eds. Pp. 57-121. New York: Appleton-Century Crofts.
- Comte, Auguste
 1975-77 *System of Positive Polity*. London: Longmans, Green.
- Harris, Marvin
 1964 *The Nature of Cultural Things*. New York: Random House
- 1968 *The Rise of Anthropological Theory*. New York: Thomas Crowell Co.

- Holland, Paul and Samuel Leinhardt
 1975 The Statistical Analysis of Local Structure in Social Networks. In Sociological Methodology, David Heise, ed. San Francisco: Josey-Bass.
- Hunter, John and R. L. Shotland
 1974 Treating Data Collected by the "Small World" Method as a Markov Process. Social Forces 52: 321-32.
- Killworth, P. D. and H. Russell Bernard
 1974 CATIJ: A New Sociometric and Its Applications to a Prison Living Unit. Human Organization 33(4):335-50.
 1975 A Model of Human Group Dynamics. Social Science Research 5:173-224.
 1976 Informant Accuracy in Social Network Data. Human Organization 35(3):269-96.
 1979a Informant Accuracy in Social Network Data III. Journal of Social Network, in press.
 1979b A Pseudomodel of the Small World Problem. Social Forces, in press.
- Lin, Nan, P. Dayton, and P. Greenwald
 1978 Analyzing the Instrumental Use of Relations in the Context of Social Structure. Sociological Methods and Research, in press.
- Milgram, Stanley
 1967 The Small World Problem. Psychology Today 1:61-67.
- Miller, George
 1956 The Magical Number Seven Plus or Minus Two: Some Limits on Our Capacity for Processing Information. Psychological Review 63:81-97.
- Pattison, Mansell
 1977 A Theoretical-Empirical Base for Social System Therapy. In Current Perspectives in Cultural Psychiatry, J. Westermeyer and A. R. Favazza, eds. Pp. 217-253. New York: Spectrum Publications.
- Quetelet, Adolphe
 1842 A Treatise on Man. Burt Franklin, Research Source Works Series, #247. New York.
- Sarton, George
 1935 Quetelet (1796-1874). Isis 23:6-24.

- Scriven, Michael
1969 Logical Positivism and the Behavioral Sciences. In The Legacy of Logical Positivism, P. Achinstein and S. F. Barker, eds. Pp. 195-209. Baltimore: Johns Hopkins Press.
- Steward, Julian
1936 The Economic and Social Basis of Primitive Bands. In Essays Presented to A. L. Kroeber, Robert Lowie, ed. Pp. 331-345. Berkeley: University of California Press.
1949 Cultural Causality and Law; A Trial Formulation of Early Civilization. American Anthropologist 51:1-27.
- Stimson, Dorothy, ed.
1962 Sarton on the History of Science. Harvard University Press, Cambridge.
- Stommel, H.
1948 The Westward Intensification of Wind-driven Ocean Currents. Trans. Am. Geophys. Union 29:202-06.