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Informal Contacts in Science: A Probabilistic Model for Communication Processes

Abstract. *Significant contacts among scientists within research specialties are generally infrequent and are distributed as an essentially random process, the pattern of most contacts conforming to a Poisson distribution. Extremely productive persons in a specialty, however, seem to form a separate distribution; they have a considerably higher number of contacts.*

The systematic study of informal communication within science dates from Price's postulation in 1961 of the "new invisible colleges" as a social mechanism critical to the continued functioning of science as a large, diffuse, international activity (*1*). In the course of planning and initiating a program of research on communication among medical researchers, we reexamined two questions regarding invisible colleges implicit in Price's formulation, namely: (i) Do "Invisible Colleges," in the sense of small, active elites, function critically in the organization and communication network of science? (ii) Are there quantitative bases for postulating their existence?

These questions seemed largely lost from view because of the incompatibility of data from recent studies that focused on elites (*2*) with data from studies based upon exhaustive lists of the membership of individual specialties (*3, 4*). In the first type of study, the investigators principally contacted central persons in the field, and discarded other respondents. In the second studies, very general measures were taken on all persons who can be identified with a specialty, and the data showed continuous functions relating productivity, "centrality," and communication. To provide a means of reconsidering these problems, the present report proposes a model for informal contacts and communications within science that relates the concept of elites to the membership of specialties and is in line with the scale of science and the number of relatively productive persons within disciplines.

There is substantial evidence that much of science is loosely organized; Mullins' dissertation concluded that normal science operates as a loose network and that the common impression among researchers that invisible colleges exist is no more than a somewhat egocentric view of the individual scientist that persons who relate to him are a group *who, in turn, relate to one another* (the italicized portion being erroneously presumed by the scientist) (*5*). The full published report of the data on informal communication and organization in psychology strongly emphasized the wide range in degree to which specialties are organized and the relatively small size of the group of highly productive scientists, about 2000 to 4000 for even major disciplines; this report postulated that it would be no great task for an experienced researcher to get to know most of the active researchers in any one specialty within a discipline (*6*). In the recent Nelson and Pollack conference volume were chapters by Hagstrom and by Griffith and Miller speculating that the normal size of most scientific specialties is extremely small (*2, 7*). Further considerations for developing a model for network structure were recently furnished by Crawford and Crane, who established strong relationships among position in communication networks, scientific productivity, and direction of information flow (*3, 4*). More central persons were found to be more productive; are more frequently sought out by others, less central, who wish to obtain information; and directed most of their self-initiated communication activities to other central persons.

These static pictures of specialties have taken little account of the relatively fast rate at which the personnel of science renews itself, principally through new persons completing graduate training, or of certain intellectual attributes on which scientific specialties differ (*8*). Thus, there are always "unknowns" entering an active research specialty, who are probably aware of the more productive members of the specialty but who can only become "known" after some period of productivity. In addition to the recruitment of younger researchers, there is a continual movement of researchers among specialties as a function of the transferability of skills and knowledge. The Poisson distribution is given by:

$$p = e^{-\lambda}\frac{\lambda^k}{k!}$$

where p is the probability of k successes per observation where the overall average number of successes is λ (*9*). The published distribution of contacts in Crane's data (see *3*) suggested the Poisson, and since the probability of trials yielding zero successes is given by $e^{-\lambda}$, we used the number of persons receiving zero nominations to solve for λ. The obtained value of λ was .78, and the resultant fit was at least suggestive. This value of λ for the average number of contacts per researcher seemed to us to be no more than we would expect throughout the active

Table 1. Comparison of Poisson model with obtained data. Members of two specialties (one within sociology and a second within mathematics) nominated persons as having impact on their work through informal communication, current collaboration, thesis direction, thesis influence, and influence on the selection of problems and techniques. All predictions of the proportion of persons receiving each number of nominations were based on $\lambda = 0.65$, with 0.82 of rural sociologists (inside) and 0.65 of finite group theorists (inside) conforming to the Poisson distribution; 1.00 (all) of "outsiders" are predicted to conform.

Nominations received	Proportions of persons receiving N nominations							
	Rural sociology				Finite groups (mathematics)			
	Members ($N = 221$)		Outsiders ($N = 389$)		Members ($N = 102$)		Outsiders ($N = 93$)	
	Obtained	Predicted	Obtained	Predicted	Obtained	Predicted	Obtained	Predicted
0	0.46	0.43	———0.52*———		0.34	0.34	———0.52*———	
1	.23	.28	0.31	0.34	.20	.22	0.36	0.34
2	.09	.07	.10	.11	.16	.07	.09	.11
3–5	.10	.02	.06	.03	.14	.02	.02	.03
6–10	.05	< .01	.01	< .01	.11	< .01		< .01
11–20	.04		< .01		.03			
≥ 21	.03				.03			

* No obtained data. Data only for exist for values of 1 and above regarding outsiders. See text for explanation.

members of an entire discipline, *not just* among those persons making up a single specialty. Accordingly, we again examined Crane's data for persons nominated *outside* the specialty (rural sociology) which was the focus of her study (see *3*).

While the data for persons nominated outside the field can furnish no frequency for persons receiving zero contacts, this is not an empty category conceptually. Contacts with persons outside this field would be drawn from a population larger than that of rural sociologists but smaller than that of all sociologists and far smaller than that of all social scientists. In line with our view of contacts within a science, we made a working assumption that the value of λ would be approximately constant for contacts with persons both *inside* the field of rural sociology (as enumerated from a bibliography) and *outside*. Since the proportion of persons at the zero point in a Poisson distribution equals $e^{-\lambda}$, this assumption meant that we regarded the summed distribution of persons outside the field as representing $1 + e^{-\lambda}$ of the persons within the community from which contacts were made by rural sociologists (*10*). The outcome of these preliminary attempts to use a single value of λ to fit contacts both inside and outside a specialty resulted in (i) an estimation of the distribution of contacts inside the specialty which predicts fewer persons having high numbers of contacts than obtained in the data; and (ii) virtually perfect estimation of the frequency of persons outside the specialty having a given number of contacts.

At this point, the idea of trying to estimate the absolute size of the community that is functional in terms of providing contacts was considered, and our first estimate of 1000 (221 rural sociologists plus approximately 800 total outside, including the estimated number of persons having zero contacts) was in line with a reasonable estimate of the total number of active sociologists working during the years in which the rural sociologists were publishing. These preliminary findings suggest that contacts are rare and are distributed as a random process through large segments of a discipline, including most, but not all, of the persons identified with specific specialties. However, the most productive and prominent persons in any single specialty seem to form a separate distribution receiving a considerably higher mean number of contacts from persons within the specialty, and are less closely fit by the Poisson.

Table 1 compares data with estimated scores generated with λ equal to 0.65. The data were furnished by Crane (*11*), and the estimates have been made under the assumption that only portions of the specialty memberships (82 percent of rural sociologists and 65 percent of finite group theorists) have a number of contacts that conforms to the Poisson distribution. The difference in these percentages is in line with the procedures for sampling the two specialties, since the first sample was based upon a sample selected through a complete bibliography of the specialty and may be more inclusive than the second, an enumeration by a key researcher supplemented by a literature search. Thus, the fit afforded by the Poisson distinguishes, within each specialty, between two groups: a majority having a low average rate of contact (0.65) fitting the Poisson, and an elite, as represented by the residual lying above the region predicted by the Poisson. The elite is nominated on the average by approximately five to six persons, a rate about eight times higher than the majority. All outside contacts for each specialty conform to a pattern predicted by the Poisson [see (*12*)].

Much of the data on the characteristics of informal communication in science are in line with the view that high scientific productivity is such a relatively rare commodity that active researchers in any general area are usually acquainted with one another. The probabilistic model presented here affords a quantitive basis for identifying such persons and for conceptualizing the communication process. For the mass of researchers, there is a low, but constant, average number of contacts resulting in substantial effects upon investigators' work and distributed randomly within a general area of research activity (*13*). Within an active research area, there is, in contrast, an intensification of scientific communication focused upon a small elite, whose activities as individuals attract other researchers and students and create a highly structured pattern of exchanges imposed upon the basic pattern of essentially random contacts. The persons nominally identified with a research area, as, for example, through report authorship, include with the elite a larger majority who conform to the random low rate of contact.

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10. We are aware of the great potential for error in estimating the number of persons at the zero value of the Poisson distribution [see L. Mantell, *Am. Doc.* **17**, 8 (January 1966)]. In addition to the logical error in the Mantell report (assuming authors publish in only one journal) subsequently discussed by H. A. Simon in a letter [*ibid.* **18**. 113 (April 1967)], substantial errors may result from obtained distributions differing from Poisson.
11. We thank Diana Crane for access to data from the manuscript of her unpublished book *Diffusion of Innovation in Science*. The obtained data for outsiders have been recast by supplying the proportion having zero contacts that would be predicted by a λ of 0.65.
12. The reader may question the choice of the Poisson as opposed to a variety of other distribution including Paretian distributions, the Whitworth distribution, and so forth. The data do not give a linear plot on log-log paper, which clearly eliminates hyperbolic functions. The data could be approximately fit by an exponential function or the Whitworth distribution; however, any negligible gain in goodness of fit would be offset by a necessary elaboration in assumptions.
13. The reader should be warned against interpreting persons whose contacts conform to the Poisson as being nonproductive scientists. The structure is being examined from the point of view of a single specialty and the outsiders were found by Crane (*11*) to include distinguished researchers in other fields. Further, one should bear in mind that a node of high communication does not always develop at the center of many specialties, a fact most clearly demonstrated in Griffith and Miller (*2*).
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Radio Echo Records Cannot Be Used as Evidence for Convection in the Antarctic Ice Sheet

In a report on convection in the Antarctic ice sheet (*1*), T. Hughes used some samples of radio echo records obtained in Antarctica in 1967 by a team from the Scott Polar Research Institute (SPRI), Cambridge, England, led by G. de Q. Robin (*2*) with logistic support through the U.S. National Science Foundation. They were presented as evidence for convection plumes in the Antarctic ice sheet.

It is misleading to publish radio echo records without a statement of some of the parameters of the system and an explanation of their effects on the film record. For instance, the radio echo system used by SPRI has a very broad beam fore and aft (in the line of flight) and a narrow beam from side to side; therefore, it is dangerous to interpret a strong echo on the film as a reflecting point at the appropriate range vertically below the observer. In fact, a point reflector gives a hyperbolic echo trace similar to the traces seen on the bottom of Hughes' figure 2 in the area of the Gamburtsev Mountains. The echo profile given by various shapes of surfaces and a method for computing the original reflecting surface from radio echo records are described by Harrison (*3*).

The radar system is carried in an aircraft at an altitude of about 1 km above the ice, so that the ray path through the ice can never be inclined to the normal to the ice-air interface at greater than the critical angle for refraction (34 degrees), and a specular reflection cannot be received from a surface whose slope within the ice is greater than 34 degrees. Furthermore, the rate of change of the total range of a specular reflector (whether there is refraction or not) can never be greater than the aircraft's velocity. Thus, the near-vertical lines seen in Hughes' figure 2 cannot be interpreted as vertical reflectors formed by either shear planes or convection plumes, and some alternative explanation must be given.

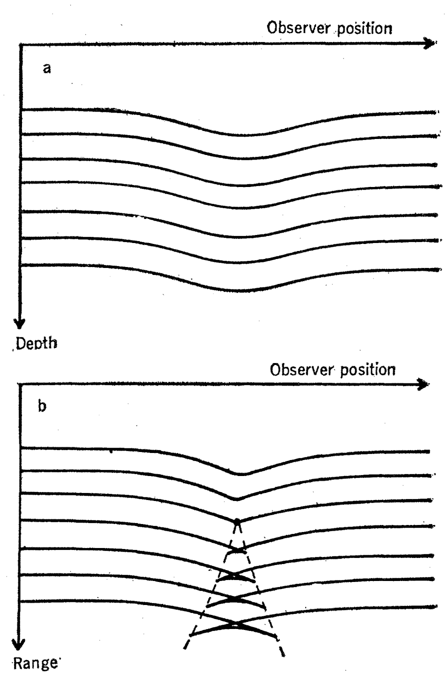

Fig. 1. (a) A number of partially reflecting layers arranged in a simple stack, and (b) echo range for the same layers plotted against observer position.

From an appraisal of all the Antarctic radio echo records at the SPRI, it has been found that the near-vertical patterns always appear in association with layers of large horizontal extent, some of which may be seen in Hughes' figure 2. The near-vertical patterns are explained by considering the geometry of specular reflections from a set of gently waving layers. In Fig. 1a, consider the set of partially reflecting layers and the observer to be within one medium of propagation. With Harrison's method (*3*) we can deduce the approximate range plots shown in Fig. 1b. Within the area enclosed by the broken lines there is the possibility of receiving three specular echoes from any one layer if the center of curvature of part of the reflecting surface lies below the observer's path. It can be shown that the echoes are generally strong above the triangular patch, since they arise from the concave surfaces; they are stronger still near the broken line, where cusps form, because here two echoes, each partially focused, are reinforcing one another; and they are strongest of all at the apex, where the three possible echoes are focused on the observer position. The shape of the patch of strong echoes depends on the shape of the layers and, particularly, on the way in which they are stacked one above another; it is further affected by refraction at the ice-air interface. It is also possible to take account of interference effects between two or more echoes in the triangular patch.

It must be pointed out that these features are not very common. In 1967 they were seen along only 700 km of flight line out of a total of 35,000 km. They occurred mainly near the Trans-Antarctic Mountains, the Gamburtsev Mountains, and a part of Marie Byrd Land where the terrain is rough. (The 2-minute section of film at Byrd Station in Hughes' figure 2, which shows a smooth bottom, is surrounded by evidence of rough bedrock, and the sloping tails of hyperbolic echoes from surface crevassing may be seen on the left.) Nearly all the records away from mountainous zones over thick ice sheet show unperturbed horizontal layers.

Although the instability of the Antarctic ice sheet suggested by Hughes may be theoretically possible, there is