general speed of the process. There will coexist (with the paper journal) electronic byproducts of the composition system, such as online full text[4] and volumes on CD-ROM disk, which will be increasingly available to the more sophisticated computer-supported user. But the overall system will continue to look much as it does today, while the final product—the printed journal—is to all outward appearances identical with that of a half-century ago.

Suppose, however, that (1) cost-effective technology becomes widely available for digitizing graphics and half-tones and for printing out completely legible, aesthetically pleasing full pages from digitized files, (2) telecommunication costs drop substantially, because of competition and increased carrying capacity, to the point that it becomes actually *less* expensive to telecommunicate the content of the manuscript than to mail a paper copy, and (3) coding and compatibility problems are resolved.

In such a scenario, conditions would be in place for (1) authors to telecommunicate manuscripts to editors, (2) editors to transmit copy electronically to reviewers, (3) reviewers to send comments on manuscripts to editors by electronic mail, (4) editors to telecommunicate manuscripts to copy editorss, and (5) copy editors to edit online and transmit the manuscript to the printer—with no more inputting necessary. Under these circumstances, the existing system would become both less expensive and quicker, with information moving at electronic speed rather than with the gait of the mailman. At the same time, however, one must recognize that conditions would then exist for a parallel—and conceivably competitive—system, in which authors could post their manuscripts on an electronic bulletin board from which anyone with valid access could read or print out a copy.

It seems certain that these technology advances will eventually occur. Some of them have already occurred. But certain questions will need to be answered first before one can confidently predict that an all-electronic primary journal system will (or should) supplant the "mail and paper" system. How will the quality-control function be exercised? How will the archive's permanence be guaranteed? Will such a system conform to the sociological needs of the scientific author?

The author's personal response to these questions is that he has been for many years a publisher of print journals; as such, it is constitutionally difficult for him to envisage a system that does not center on the print journal. Possibly, the main compulsion to think in this fashion is the difficulty he has conceiving of an electronic archive that has the obvious and imposing permanence of a shelf full of bound volumes of JACS! The main attraction of the present, paper-based system is linked to an author's unshakeable conviction that his written ideas leave a legacy in ink that the centuries will not erase. This author, for one, does not believe that sheer ease of transmission of ideas will, in practice, override his subconscious fears of leaving his creative legacy in an archive that head crashes or warped disks could render nonexistent in a picosecond! On the other hand, attitudes toward the electronic handling of information are changing so rapidly that any predictions seem, by the very nature of the subject matter, to be speculative and unlikely to be accurate. The next 10 years will, without doubt, provide strong clues to the real answers.

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Scientometrics with Some Emphasis on Communication at Scientific Meetings and Through the "Invisible College"†

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Scientometrics uses quantitative methods to investigate science as an information process. Studies were made of attendance and speakers at several scientific meeting series. Data from these and other investigations lead to the conclusion that "invisible colleges" exist within science and that advancement is often through interactions within these informal organizations. Studies have also been made of what happens to oral presentations (are they eventually published?) and how journals communicate with other journals. Such investigations aid in understanding the communication process in science.

Science is an information process, developing with time. As such, it can be investigated quantitatively. The term scientometrics was introduced in 1969 by V. Nalimov[1] to stand for those quantitative methods that deal with the investigation of science viewed as an information process. Such studies had been made, of course, for many years before 1969, but the development of computers and computer techniques has made possible rapid search and research that in the past would have taken years to complete. Garfield seized the opportunity provided by computer information processing to found ISI and use of his *Citation Indexes* and complementary publications is now almost de rigueur for any scientometric study. Terms such as citation rate, impact factor, and immediacy index probably coined and certainly popularized by Garfield have become a part of scientometric jargon even as bit, byte, boot, and Basic have come into the literature through the computer terminal.

Whole volumes can be and have been written on scientometric methods; one of the best and most recent is by Braun, and Bujdoso.[2] Scientometrics has its own journal called, not suprisingly, *Scientometrics*, and scientometric papers have appeared in numerous journals including such disparate

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Table I. Probabilities of Attendees Giving Papers at NAA Conferences

meeting year(s)	probability	meeting year(s)	probability
P_{67}	0.28	$P_{72,78}$	0.20
P_{72}	0.49	$P_{67,78}$	0.11
P_{78}	0.39	$P_{67,72,78}$	0.056
$P_{67,72}$	0.14		

publications as *Analytical Chemistry*, *Physics Today*, *New Scientist*, *Science*, *and Nature*. No introduction to the subject would be complete without mention of de Solla Price, whose writings include the classic *Big Science Little Science*,[3] R. K. Merton, e.g., see reference 4, and Thomas Kuhn whose *Structure of Scientific Revolutions*[5] still has the power to precipitate debate. For a broad discussion of many aspects of scientometric chemistry studies the reader is referred to reference 2. This paper concerns itself with investigations of oral communication at meetings. All work discussed is in the literature, and the interested reader can go to the original paper for more detailed information:

ORGANIZATION, ATTENDANCE, SPEAKERS, AND SESSIONS: A STUDY OF FOUR SCIENTIFIC CONFERENCE SERIES

Neutron activation analysis (NAA) is an interesting area of science. It is relatively homogeneous, yet in application it crosses almost the entire spectrum of sciences. NAA is a technique that enables one to determine trace element concentrations in materials by irradiation in a nuclear reactor and measurement of radioactive isotopes produced. The International Atomic Energy Agency (IAEA) has sponsored three conferences on Nuclear Activation Techniques in the Life Sciences: 1967, 1972, and 1978. Invitations to participate and paper selection are through formal channels at the national level. Papers are generally limited to about 50, and a quota system is used to assure representation from many countries. Attendance and speakers at these conferences were compared[6] to those at three other conference series: IAEA Medical Scintigraphy (1964, 1968, and 1973), University of Missouri Trace Substances (1969, 1971, and 1976); Air Cleaning Conference (1968, 1972, and 1976). The probability (P_α) of a person giving a paper at a meeting is

$$P_\alpha = S_\alpha / N_\alpha \quad (1)$$

where S_α = total number of speakers and N_α = total number of attendees.

The probability of a speaker giving a paper at two conferences is

$$P_{\alpha\beta} = \frac{S_\alpha}{N_\beta}\frac{S_\beta}{N_\beta} = P_\alpha P_\beta \quad (2)$$

and at three conferences

$$P_{\alpha\beta\gamma} = \frac{S_\alpha}{N_\alpha}\frac{S_\beta}{N_\beta}\frac{S_\gamma}{N_\gamma} = P_\alpha P_\beta P_\gamma \quad (3)$$

Figure 1 shows total and overlapping attendance and speakers at the different IAEA conferences on Nuclear Activation Techniques in the Life Sciences: 1967, 1972, and 1978. The numbers within overlapping arcs indicate multiple-meeting attendees or speakers. Table I shows the probability of attendees giving papers at NAA conferences.

If one knows the number of multiple attendees at any two meetings, $N_{\alpha\beta}$ (as, for example, shown in Figure 1), one can calculate the expected number of multiple speakers:

$$S_{\alpha\beta} = P_\alpha P_\beta N_{\alpha\beta} \quad (4)$$

This calculation has been made for the three dual combinations shown in Table II as well as for the theoretically expected number giving a paper at all three conferences. From Table II one sees that the number of observed multiple speakers is almost twice the expected number for two meetings and is 3 times the calculated number for all three. This seems clear evidence that there is a group within NAA that is statistically over represented on programs. This may or may not be desirable.

The same calculations were performed for attendees at the IAEA Medical Scintigraphy conferences (Table III), Trace Substances (Table IV), and Air Cleaning Conference (Table V). In every instance, the multiple speakers far exceed the theoretically predicted number.

Price[3] reintroduced the term "invisible college", which was first used in the 17th century to refer to a collection of scientists who later formed the Royal Society. "Invisible" suggested that the group members were dispersed and not easily seen. Price used the term to indicate a collection of scientists that through personal accomplishment dominated publications and conferences in a given field. The data of Tables II–IV tend to support the belief that invisible colleges exist not only in NAA but in other disciplines as well.

Other papers reported on institutional and national representation, speakers, and topics at five Modern Trends in Activation Analysis (MTAA) conferences[7] and predictions of paper subjects based on data in reference 7 compared with

Table II. Calculated and Observed Multiple Speakers at IAEA NAA in Life Sciences[a]

multiple years	P, probability of giving a paper	N, no. of attendees at multiple meetings	multiple speakers: theoretical	multiple speakers: obsd
67, 72	0.14	39	5	12
72, 78	0.20	44	9	13
67, 78	0.11	34	4	8
67, 72, 78	0.056	12	1	3

[a] Total speakers, S = 124.

Table III. Calculated and Observed Multiple Speakers at IAEA Medical Scintigraphy[a]

multiple years	P, probability of giving a paper	N, no. of attendees at multiple meetings	multiple speakers: theoretical	multiple speakers: obsd
64, 68	0.116	55	6	26
68, 73	0.066	103	7	30
64, 73	0.07	41	3	13
64, 68, 73	0.023	33	1	6

[a] Total speakers, S = 206.

Table IV. Calculated and Observed Multiple Speakers at University of Missouri Trace Substances[a]

multiple years	P, probability of giving a paper	N, no. of attendees at multiple meetings	multiple speakers: theoretical	multiple speakers: obsd
69, 71	0.054	38	2	2
71, 76	0.052	35	2	7
69, 76	0.077	27	2	2
69, 71, 76	0.015	19	0.3	1

[a] Total speakers, S = 123.

Table V. Calculated and Observed Multiple Speakers at Air Cleaning Conferences[a]

multiple years	P, probability of giving a paper	N, no. of attendees at multiple meetings	multiple speakers: theoretical	multiple speakers: obsd
68, 72	0.017	50	1	6
72, 76	0.037	69	3	9
68, 76	0.023	33	1	3
68, 72, 76	0.004	21	0.1	3

[a] Total speakers, S = 140.

Table VI. Oral Presentation and Publication Data for ORNL Analytical Chemistry Division

year, *Y*	no. 1st Au talks total	no. 1st Au talks research	research talks/ total talks	no. 1st Au papers total	ratio, 1st Au talks total (year *Y*)/ 1st Au papers (year *Y* + 1)	ratio, 1st Au talks research (year *Y*)/ 1st Au Paper (year *Y* + 1)
1977	66	37	0.56	36	66/61 = 1.08	37/61 = 0.60
1978	60	43	0.72	61	60/71 = 0.85	43/71 = 0.61
1979	84	59	0.70	71	84/90 = 0.93	59/90 = 0.66
1980	78	63	0.81	90	78/67 = 1.16	63/67 = 0.94
1981	92	64	0.70	67		
					$\bar{X}$ = 1.01 ± 0.14	$\bar{X}$ = 0.70 ± 0.16

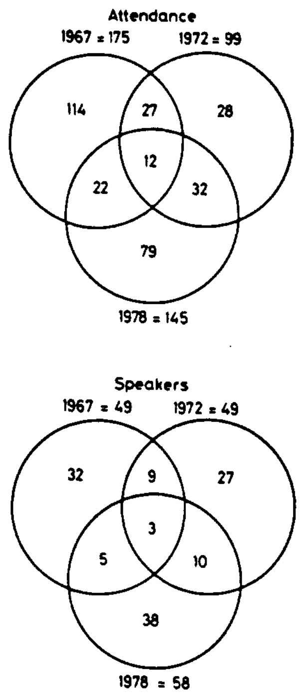

Figure 1. Distribution of attendees and speakers at IAEA NAA meetings.

reality at the 1981 MTAA conference.[8] Data from these conferences strengthen the belief that invisible colleges do indeed exist.

ORAL COMMUNICATION AND INVISIBLE COLLEGES

Another way in which oral communications and a small nucleus of colleagues can advance a field was studied in 1981. The field was resonance ionization spectroscopy (RIS), popularly called one-atom detection. In the early stages of development of RIS, oral communications outnumbered printed ones by a ratio of 5:1.[9] Total oral presentations hit a peak 4–5 years after invention, and self-citations in the literature sometimes ran as high as 80%. This is not too surprising since RIS was developed and exploited by a small group primarily at one institution (but with a gradually increasing number of outside collaborators). The study does show how a field expands through oral communication and collegial collaboration.

A recent paper[10] discusses the beginnings of flow injection analysis, about which there has been some controversy. By use of citation data and a scientometric approach, it was postulated that an invisible college associated with a particular group advanced the approach of one claimant as opposed to that of another. Invisible colleges should not be looked upon as sinister forces but rather instead as loose groups of experts who act as leaders and gatekeepers.

TECHNICAL ORAL PRESENTATIONS: WHAT HAPPENS TO THEM

Technical oral presentations generally fall into the category of research reports or reviews. Research reported is almost

Table VII. ACD Research Papers Given and Later Published

year	total talks (*N*)	no. published in indicated year, fraction per year: *Y*, *Y*/*N*	*Y* + 1, (*U* + 1)/*N*	*Y* + 2, (*Y* + 2)/*N*	*T*, *T*/*N*
1977	37	7, 0.19	8, 0.22	8, 0.22	23, 0.62
1978	43	6, 0.14	14, 0.33	0, 0.21	29, 0.67
1979	59	7, 0.12	27, 0.46	3, 0.05	37, 0.63
1980	63	20, 0.32	21, 0.33	3, 0.05	44, 0.70
1981	78				
					av: 0.66 ± 0.04

VIII. Publishing History of ACD Research Talks

year presented	fraction finally published as: journal article	conf proc or book chapters	report
1977	0.22	0.35	0.05
1978	0.28	0.30	0.09
1979	0.27	0.32	0.03
1980	0.42	0.24	0.03

always ongoing or just completed; reviews survey a field or technique, usually include some mention of the speakers' work, and serve as guides or stimulation to other workers.

Garvey[11] and others maintain that these informal channels of communication represent the research front. Obviously for research reports this is true. But what fraction of oral presentations really represent research (as opposed to reviews)? And what is the ultimate fate of such ephemeral efforts? Are they lost, or are they preserved in the archival records of science?

Titles of all talks and publications cleared through the ORNL Analytical Chemistry Division were reviewed and categorized as either research or review. The ultimate fate of each presentation was found by examining the publication record of the division for the three succeeding years. A total of 380 oral presentations during the period 1977–1981 were studied.[12]

Table VI lists total oral presentations, total research talks, and total publications (on a first author basis) for years 1977–1982. A rapid upsurge in all categories was observed in 1979. The ratios of total first author talks (and research first author talks) to first author publications shown in column 6 (and column 7) were obtained by using publication data for the year following the talk. This was felt to be more realistic than using same year data because of the publication time lag between submission of a paper and publication. As seen in Table VI, the ratio of total talks to papers is about one. The ratio of first author research talks to first author papers that resulted from these talks is much lower, 0.70 ± 0.16. Thus, it appears that oral presentations are indeed a communication and "trying out" process since about one-third of such efforts never reach the archival literature. Table VII traces the publication history of these research talks by year. For example, of the 37 research talks given in 1977, seven were published the same year, eight in 1978, and eight in 1979. The fraction of research papers published is seen to be 0.62 for that year. The picture is fairly consistent with an average of 0.66 ± 0.04 talks finally ending as publications.

Table IX. ACS DAC Research Papers Given and Later Published

year	total talks	no. published in indicated year, fraction per year (Y/N) 1977, Y/N	1978, Y/N	1979, Y/N	T/N
1977	59	7, 0.12	18, 0.30	5, 0.08	0.51

Table X. Publishing History of ACS DAC Research Talks

year presented	fraction finally published as journal article	conf proc or book chapter	report	total
1977	0.41	0.08	0.02	0.51

Table VIII shows the fractions of papers published that resulted in journal articles, chapters in conference proceedings or books, and reports. Surprisingly, no consistent pattern of journal and proceedings publication is apparent, and more surprisingly, the fraction appearing in conference proceedings declined drastically in 1980.

Results from ORNL were compared with those obtained from an actual conference. The 1977 New Orleans, LA, National American Chemical Society (ACS) meeting and papers given in the Division of Analytical Chemistry (DAC) were selected. Every third paper was taken ($N = 70$) and categorized as to research or review, and ultimate publication fate was ascertained by using first author in a computer search of *Chemical Abstracts* data base. Tables IX and X give results. The ratio of research to review papers are somewhat higher at this meeting (0.84) than that from ORNL with the fraction attaining final publication being somewhat lower (0.51).

These data suggest first that research talks *are* frontier communications and that the failure of all such talks to end up in the literature indicates the health and vigor of the informal communication process and second that published conference proceedings are not usurping the journals' functions in analytical chemistry. Results of the DAC presentations confirm the hypothesis that such talks are often a trying out process since the ratio research/review is much higher and the fraction of papers published much lower than those from ORNL, which itself requires a somewhat onerous referee and screening process prior to submission of an abstract for conference presentations.

Finally, it is rather interesting that from ORNL the yearly ratio of first author research talks to total talks (0.70 ± 0.10) is almost identical with the ratio of first author research talks to total papers (0.70 ± 0.16). Apparently, the 30% of discussed research that never gets published is balanced by a 30% research output that goes directly from the laboratory to the journal without conference reporting. It is also possible, of course, that some conference-reported research represents rather inconsequential or perhaps dead-end explorations. In any event, an 70% yield does not seem too unsatisfactory even in these days of expanding journals and relatively easy publication.

COMMUNICATION AMONG JOURNALS

Information obtained from compendia such as *Chemical Abstracts* and *Analytical Abstracts* illustrate how fast certain chemical disciplines are growing, which countries are most active in each field, and how many papers journals publish. Use of *Citation Index* data makes possible additional qualitative and quantitative evaluations. The literature of *Analytical Chemistry*,[13] *Health Physics*,[14] and *Prompt Nuclear Analysis*[15] were examined. Techniques used were rather simple and straightforward. A more sophisticated approach was used, however, in a study of information flow in analytical

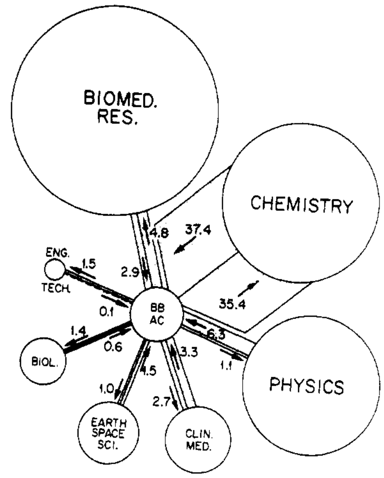

Figure 2. Information flow between analytical chemistry and other fields of science as a percentage of the total flow of analytical information. The areas of the circles representing the fields are proportional to their virtual size.

journals.[16] Data from *Journal Citation Reports*[17] were used to describe information flow (on a macroscale), between two groups of journals and journals in other fields. The two groups of journals were broken down into their components, and the interactions between them described. Finally, the interactions were compared with what might be expected if journals cited themselves as they do others. In this way, one observes how journals communicate with each other. Analytical journals were divided into two groups: (a) 10 broad based (BB) journals, such as *Analytical Chemistry* and *Analyst*, and (b) 12 specialty journals, such as *International Journal of Mass Spectrometry* and *Journal of Chromatography*. Citations to and from these journals vis-ã-vis journals of (a) other chemistry fields and (b) other scientific fields were obtained and information flow diagrams such as that of Figure 2 plotted. The main information sources of analytical chemistry appear to be physics, clinical medicine, and the earth and space sciences; analytical chemistry absorbs information from all these disciplines.

By arranging a group of journals in an $m \times m$ array, where an element c_{ij} indicates both the number of references that journal i gives to journal j and the number of citations that journal j receives from journal i, a reference-citation matrix is obtained. Table XI shows such a matrix for the BB journal. This transaction, or input–output matrix, shows the relationship of a journal to others in the group. For example, the citation ratio for a journal can be a simple indicator of the journal's behavior within its group. This ratio is found by dividing the sum of the ith column by the sum of the ith row. If the citation ratio is greater than 1, the journal is an emitter or exporter of information; if less than 1, the journal is an absorber or importer of information. For example, within the BB group (Table XI) *Analytical Chemistry* is seen to be an exporter (2159/2074 = 1.04); *Talanta* is an importer (696/836 = 0.83). Such matrices can be the starting point for more sophisticated analyses of information flow.

Inspection of the matrix in Table XI reveals an interesting anomaly: most journals cite themselves much more frequently than would be expected. (Self-citations are shown in the diagonal terms.) Such self-citation may well represent a different emphasis, philosophy, or policy from that employed by an author citing articles from other journals. It would be desirable to be able to "normalize" (or correct) this anomalous behavior. Price[18] and Price and Burke[19] have devised a procedure to replace the diagonal terms by those that would be there if each journal referenced itself as it referenced others. Price's method consists of treating the matrix as if each ele-

Table XI. Cross-Citing of Broad-Based Analytical Chemistry Journals in the Year 1978

	citations to										cited
ref from	1	2	3	4	5	6	7	8	9	10	sum
(1) *Anal. Chem.*	[3107]	578	46	229	160	151	216	276	126	292	2074
(2) *Anal. Chim. Acta*	698	[541]	9	179	49	36	90	170	12	37	1280
(3) *Analusis*	103	37	[33]	24	12	<6	8	34	<6	<10	240
(4) *Analyst*	349	162	<3	[235]	31	13	45	66	7	48	724
(5) *Anal. Lett.*	118	41	<3	16	[34]	<6	9	14	<6	<10	223
(6) *Bunseki Kagaku*	200	74	<3	34	6	[<6]	21	50	<6	9	403
(7) *Fresenius' Z. Anal. Chem.*	281	95	<3	46	14	13	[344]	41	<6	9	508
(8) *Talanta*	247	147	10	100	16	74	87	[239]	37	118	836
(9) *Zavod. Lab.*	27	11	<3	7	<6	<6	6	8	[190]	76	150
(10) *Zh. Anal. Khim.*	136	55	<3	23	<6	8	33	37	113	[429]	414
citing sum	2159	1200	83	658	300	313	515	696	319	609	

Table XII. Symmetrized Expectation Matrix for the Main Broad-Based Analytical Journals[a]

	1	2	3	4	5	6	7	8	9	10
(1) *Anal. Chem.*	[1.9]	1.03	1.13	0.90	1.21	1.11	1.20	0.70	0.66	0.90
(2) *Anal. Chim. Acta.*		[2.4]	0.82	*1.44*	1.06	0.91	1.09	1.14	**0.34**	0.58
(3) *Analysis*			[14.4]	0.83	*1.42*	0.73	0.57	1.34	1.37	0.65
(4) *Analyst*				[3.8]	1.03	0.75	1.03	1.14	**0.41**	0.77
(5) *Anal. Lett.*					[4.4]	0.57	0.71	0.59	0.93	**0.51**
(6) *Bunseki Kagaku*							0.76	*1.76*	0.74	**0.40**
(7) *Fresenius' Z. Anal. Chem.*							[10.9]	1.20	**0.48**	0.74
(8) *Talanta*								[2.8]	0.83	1.35
(9) *Zavod. Lab.*									[34.4]	*7.08*
(10) *Zh. Anal. Khim.*										[13.8]

[a] $\bar{e}$ = 1.04; SD = 0.97.

ment is a product of the corresponding row and column coefficients, assuming the diagonal terms are unknown.

If the matrix operations given in eq 5 are performed, one

$$e_{nn} = \frac{\left(\prod_{\substack{i \\ i=n}}^{m} c_{nk}\right)^{1/m-2} \left(\prod_{\substack{j \\ j\neq n}}^{m} c_{jn}\right)^{1/m-2}}{\left(\prod_{j=1}^{m} \prod_{\substack{k=1 \\ k\neq j}}^{m} c_{jk}\right)^{1/[(m-1)(m-2)]}} \qquad (5)$$

can obtain the value of each diagonal term, e_{nn}. This value is the "corrected" or expeccted self-citation for each journal assuming it cites itself as it cites others.

A matrix such as that in Table XI appears to be only a confusion of numbers. Price proposed a procedure to "pick out order and disorder in both rows and columns." To use his method, a simple model is assumed in which each entry in the matrix has an expected value determined by the marginals. Thus, each value depends only on what proportion its row is of the whole and what proportion its column is of the whole. The ratio of actual to expected value is then calculated for each element of the matrix, and a new "expectation matrix" is constructed with these ratios. Inspection then shows which journals have values very much higher or lower than unity; these are the misbehavers—the journals that are either unusually heavily cited or unexplainedly overlooked. By use of this method, the values shown in Table XII, i.e., the symmetrical expectation matrix for the BB journals, were obtained. What conclusions does it suggest?

By looking along the diagonal, it is seen that most journals cite themselves a lot—notably, the Russian and German journals. Only *Analytical Chemistry*, *Analytical Chimica Acta*, and *Talanta* manage to stay below a value of 3.

Second, looking at the expectation values for pairs of journals, some close and some distant relationships are noted. The two Russian journals *Zavodskaya Laboratoriya* and *Zhurnal Analiticheskoi Khimii* reference each other 7 times more often than statistical estimates would predict. The Japanese journal *Bunseki Kagaku* has its closest relationship with *Talanta* (e = 1.76). *Bunseki Kagaku* also has the second most distant relationship of any journal—0.40 with *Zhurnal Analiticheskoi Khimii*. This is only slightly less frigid than that of *Analytica Chimica Acta* and *Zhurnal Analiticheskoi Khimii* (e = 0.34). Other cool relations exist between *Zavodskaya Laboratoriya* and both *Analyst* and *Fresnius' Zeitschrift fuer Analytische Chemie* (in Table XII, high values are italicized and low ones are in bold face).

CONCLUSIONS

From this brief discussion, it is hoped that the reader has glimpsed some of the fascination of studying scientific communication. The work cited covers but a fraction of the possibilities. The great lexicographer Dr. Samuel Johnson, through the writing of his dictionary, might be considered the father of formalized communication. Johnson said, "Knowledge is of two kinds: We know a subject ourselves, or we know where we can find information upon it." Perhaps scientometrics now offers us a third kind of knowledge: how the subject is communicated, and who uses it.

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Two Programs To Further Popular Literacy in Technology†

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The structure and content of two programs that were intended to foster technical literacy are described. The first program, for undergradate seniors, worked with the topic "information encoding and information transfer", in terms of the structure of English, the structures of molecules and materials, encoding information in material, and the structure of computers. The second program was for faculty members and emphasized the human, political, and organizational aspects of large-scale, chemically based projects such as environmental monitoring and medical testing. Both programs were interesting to the participants and moderately effective in a short-term way.

INTRODUCTION

The term *scientific literacy* has come to be associated with the communication to the general population of the results of scientific research and its applications. This is an old problem; for example, June 1871: "During the recent siege of Paris, Henry St. Claire Deville, addressed the Academy of Sciences.... Deville stated what all the world has been uttering before him, that France was conquered by the science of Germany. The very discoveries and inventions of their own men were used to destroy them. In seeking an explanation of this disastrous state of affairs, Deville gave two adequate reasons: first that men of science had been overlooked by the Government, and mere politicians appointed in their places; and second, that the members of the Academy had devoted themselves too exclusively to abstract science and left the world to find out what was going on in the best way it could. He proposed as a security that the Institute should appoint committees to discuss all matters related to the government; and at the same time seek to popularize science, and by well-edited publications to familiarize the public mind with the grand discoveries of the day."[1]

With small changes in wording, that could have been said today. The vision of Deville "to popularize science" and "to familiarize the public mind..." is still supported frequently. For example:

(i) A. W. Trivelpiece, "I have a suggestion on how to improve this situation of public understanding of the role of science and technology in our lives. I believe that it is time for scientists and engineers to take more repsonsibility for explaining science and technology in ways the rest of our citizens can understand and appreciate."[2]

(ii) W. R. Benson, "Chemists are interested in telling nonchemists about what chemists do, if it is easy and organized." Benson goes on to recommend subscription to the ACS's radio program "Dimensions in Science" and its tapes.[3]

(iii) The University of California, Berkeley, has begun a 3-year pilot project to provide the public with accurate information on the uses and hazards of chemicals.[4]

In contrast to the viewpoint described above is the thought that the populace probably cannot be informed well enough to participate in public debate on essentially technical issues. This view was stated nicely with respect to research on recombinant DNA:

(iv) Francis Crick, "I live near a small California township called Del Mar, and when the housewives are spending their time worrying about recombinant DNA, I really think it has gone too far."[5]

A very short list of topics of public debate is the following: transport of chemicals and of chemical waste; storage and disposal of chemical waste; nuclear power generation; nuclear fuel reprocessing; acid rain; long-term changes in the atmosphere—warming, ozone; genetic engineering; pesticide use; medicine development and use; should we drink coffee or not?; should we exercise or not?

These topics and many others are matters of general concern. Some are controlled by laws now, and more will be controlled by laws in the future. In what ways and to what extent can the populace participate in shaping public policy in technical matters? When we recall the difficulty chemists have communicating with each other, the vision of Deville described above seems clearly to be unattainable, but the view expressed by Crick is unacceptable. What, then, is possible; what can we do? Clearly, we must inform people as well and as much as we can about technology. But, equally clearly, it is impossible for large numbers of people to be well informed about any particular technolgoy. We must be sensitive to the kinds and amounts of technical information the public can use. The populace will always have to rely on expert witnesses and must cope with the following questions:

(i) Which experts should be believed?

(ii) Of what is told by the experts, what should be believed?

(iii) What is the larger social framework within which any given technical problem must be seen and for which the technical experts have no special help to offer?

Scientists must continually seek ways of helping people with these questions but should probably not try to teach the technology itself in any particular case. For example, the usual

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