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The TEA Set: Tacit Knowledge and Scientific Networks

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INTRODUCTION: METHODOLOGICAL AND THEORETICAL ARGUMENT

Thomas Kuhn's concept of 'paradigm'[1] has attracted a lot of attention from sociologists and historians of science. In particular, some recent work has involved the search for groups of scientists which are taken to be the social analogue of this idea. I will argue here that the boundaries of those 'social circles' of scientists which *have* been found are not likely to correspond with the boundaries of groups sharing a paradigm unless the term be construed in a restricted sense. This is because the research methods used in most cases are unsuitable for the investigation of *cognitive* specificity and discontinuity. But it is precisely because paradigm groups are seen as conceptually homogeneous and bounded that the idea has excited interest in the sociology of science as a branch of the sociology of knowledge.

An impressive number of studies[2] have been published which try to show

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[1] T. S. Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1962). The concept of 'paradigm' is notoriously difficult to define, but perhaps this accounts for its fruitfulness. Kuhn himself writes:

> Given a set of necessary and sufficient conditions for identifying a theoretical entity, that entity can be eliminated from the ontology of a theory by substitution. In the absence of such rules, however, these entities are not eliminable; the theory then demands their existence. (page 197, footnote)

A complete definition of a concept is surely not a necessary prerequisite to its heuristic value.

[2] Diana Crane, 'Social Structure of a Group of Scientists: A Test of the Invisible College Hypothesis', *American Sociological Review*, **34** (1969), 335–52, and *Invisible Colleges* (Chicago: University of Chicago Press, 1972); S. Crawford, 'Internal Communication Among Scientists in Sleep Research', *Journal of American Society of Information Science*, **22** (1971), 301–10; J. Gaston, 'Big Science in Britain: A Sociological Study of the High Energy Physics Community' (Doctoral Dissertation, Yale University, 1969), and 'Communication and the Reward System of Science: A Study of a National Invisible College', *Sociological Review Monographs*, **18** (September 1972), 25–41.

that workers in scientific specialties are organized in a 'social circle'.[3] Such groups have been identified with Price's notion of 'Invisible College',[4] and have also been seen as the social location of distinctive sets of 'technical and cognitive norms'.[5] The claim that a set of actors exists as a social 'group' is, of course, partly a matter of definition. A 'social circle' is distinguished by the greater density of relations between its members than between members and non-members, and this depends on the definition of the relations which are held to be significant. In turn, the discovery of such patterns of relations depends in part on the research methods used to locate them. In the last resort, empirical research can only discover the existence of operationalizations of a relation, not the relation itself. In the case of the recent work by Crane and others, these operationalizations have been based on ideas of the nature of scientific information and influence which are philosophically undeveloped and likely to be sociologically uninteresting. This seems to be the result of a conflation of 'information science' and the 'sociology of science'[6]—a conflation which is well illustrated by a sentence on the first page of Crane's recent book. She writes:

The growth of scientific knowledge like that of most natural phenomena takes the form of the logistic curve.[7]

But this growth pattern has been shown, at best, only for what Price has called 'any reasonable definition of science',[8] not for 'scientific knowledge'.[9] The term 'Invisible College' itself was used by Price for hypothesized groups of scientists whose interest in meeting together would be to overcome the problems of the 'Information Explosion'. Within this context, research

[3] A 'social circle' is a 'fuzzy edged' group whose members associate more with each other than with outsiders in respect of one or more social relation. See C. Kadushin: 'On Social Circles in Urban Life', *American Sociological Review*, **31** (1966), 786–802, and 'Power, Influence and Social Circles', *American Sociological Review*, **33** (1968), 685–99.

[4] D. J. de Solla Price, *Little Science, Big Science* (New York: Columbia University Press, 1963).

[5] The term is due to Mulkay. See M. Mulkay, *The Social Process of Innovation* (London: Macmillan, 1972). It is cited by Crane in *Invisible Colleges, op. cit.* note 2, 26.

[6] I use the term 'sociology of science' as distinct from 'sociology of scientists'. Whitley's recent article is important here. See R. Whitley, 'Black Boxism and the Sociology of Science: A Discussion of the Major Developments in the Field', *Sociological Review Monograph*, **18** (September 1972), 61–92.

[7] *Invisible Colleges, op. cit.* note 2, 1–2.

[8] *Op. cit.* note 4, 1, 4, 5.

[9] May's article showing the relative uselessness of much of the mathematical literature in one field is instructive, and many scientists will make similar comments about the literature in their own fields. See K. O. May: 'Growth and Quality of the Mathematical Literature', *Isis*, **59** (1968), 363–71.

techniques such as the use of interval scale bibliometric indices and questionnaires are quite appropriate, for 'Information Science' treats information as though it can be contained in discrete visible packages of roughly equal value. However, in spite of the operational attractiveness of these techniques, the sociologist has to be concerned, at least at first, with the actor's interpretation of these items, before he can treat them as sociologically relevant. It follows that the 'groups' defined by relations based upon the information scientist's research techniques (that is, groups constructed around relations based upon *their own* perceptions of information flow) may not be groups which have any particular interest for the sociologist concerned with the boundaries of networks whose members share a common conceptual map.

Crane and others have missed the point that learning to become part of, or helping in the conceptual development of, a particular paradigm group, is 'doing' something, in the same sense that absorbing the conceptual structure that makes, say, logical inference 'natural' is learning 'to do' something.[a] What is more, there is no reason to suppose that it is possible to formulate the knowledge required to do the activity in question, any more than it is possible to formulate the knowledge required in order to infer logically. Achilles had this problem in his conversation with the tortoise.[10] Because cognitive influences are often intangible it is unlikely that the associations between scientists discovered through the correlation of questionnaire responses or in bibliographic interconnections will reflect them. This has been appreciated in studies of the transfer of technology, where it is more clear that knowledge consists of the ability to do something; thus Burns has written that technological knowledge is the property of people rather than documents.[11] I am suggesting (with Ravetz)[12] that this element is present in all knowledge, however pure, but is perhaps less noticeable elsewhere.

I will repeat the above point, for it is the most important element of the theoretical argument of the paper.

All types of knowledge, however pure, consist, in part, of tacit rules which may be impossible to formulate in principle. For instance, the ability to solve an algebraic equation includes such normally non-articulated knowledge as that the symbol 'x' usually means the same whether it is written in ball point, chalk, or print, or if spoken, irrespective of the day of the week, or the

[10] Winch, *op. cit.* note *a*, below, 55–7.

[11] T. Burns, 'Models, Images and Myths', in E. G. Marquis and W. H. Gruber (eds.), *Factors in the Transfer of Technology* (Cambridge, Mass: M.I.T. Press, 1969).

[12] J. Ravetz, *Scientific Knowledge and its Social Problems* (Oxford: Oxford University Press, 1971).

[a] Lettered annotations can be found at the end of the paper, pp. 184–85.

temperature of the air. But in another sense, x stands for anything at all and may only mean the same—*exactly* (e.g. 2·75 gms; 27 inches, etc.)—on coincidental and unimportant occasions. Again, sometimes a capital X or an italicized *x* may have a distinctive meaning. X in the equation $x = 5y$ is the *same* as x in the equation $5y = x$, but is not the same as x in $x = 5z$, unless $y = z$: but, on the other hand, 'x' is being used in the same way in all the equations. This list of tacit rules as it is extended becomes more confusing, and comes to resemble a list of all the examples of the uses of x which have ever been made. But such a list cannot serve at all as a guide for the use of x in the future.[13] Learning algebra consists of more than the memorization of sets of formal rules; it involves also knowing how to *do* things (e.g. use 'x' correctly; use logical inference) which may have been learned long before. This means that while it is quite sensible to say 'Mr Jones taught me to solve equations in the third week of January 1947' (he taught me: 'change side change sign'; 'get all the x's on one side'; 'add all the x's up'; 'divide throughout by the number in front of the x'; etc.), it is not sensible to say, 'Mr Jones taught me to use symbols' during a particular week. Now, an important difference between members of different paradigm groups (as I am using the idea) lies in the contents of their tacit understandings of the things that they may legitimately do with a symbol or a word or a piece of apparatus. Because the process of learning, or building up tacit understandings, is not like learning items of information, but is more like learning a language, or a skill, it must be investigated differently. To ask respondents directly for the sources of their tacit knowledge, or to assume that sociograms based on responses to questions about articulated knowledge necessarily picture the diffusion or development of tacit knowledge, is to confuse concept formation with information exchange.

There is a second type of criticism to be made of sociometric studies of scientific groups: this concerns the appropriateness of questionnaire research, even where articulated knowledge is concerned. Crane, reporting her survey of agricultural innovation diffusion researches, writes that some respondents seemed to have difficulty (or were unwilling) to distinguish the influences upon them of other scientists. Gaston quotes a theoretical physicist:

> There are many people who are not very much aware of who they get their ideas from, and six months after they hear the idea they forget who suggested it.

These reservations are serious, but might be less so were it the case that only the most immediately obvious sources of influence and information were significant. Crane writes:

[13] Bloor and Winch have clear and extended discussions of this type of point, drawn from Wittgenstein's 'Remarks on the Foundation of Mathematics'. See note *a*, below.

The use of a questionnaire to elicit some of this information probably has the advantage of obtaining the most important influences, rather than a complete list of major and minor influences.

But there is serious doubt that the most important contributors to the ideas of a scientist are necessarily the most obvious contributors. In fact, a recent article seems to imply that the opposite might be the case.

Mark S. Granovetter[14] argues that weak sociometric links are likely to be more important than strong ones for the transmission of influences over long (sociometric) distances and between groups which are not densely connected. If this is true, those sociometric ties which are furthest from the forefront of a respondent's mind could, for some innovatory networks, be the most important. Granovetter argues, from a premise drawn from the theory of structural balance, that where two groups are connected by only one link, or where there is one link between the two which is part of a much shorter sociometric path than any alternative, that link will be weak. It follows (and he has some empirical evidence to back this up) that the tracing out of sequences of weak links from a node in a social network will define a far larger area of the network than will the tracing out of strong links. Sequences of strong links will soon return to the original node and thus define local groups only.[15] Granovetter suggests that this may explain why marginal individuals can be particularly effective diffusers of ideas. It also seems to follow that where a postulated mechanism of innovation is the transfer of ideas from one scientific field (social group) to another, weak sociometric links may form an essential part of the diffusion path. (In commonsensical terms, if ideas brought into one field from another are genuinely new, there can have been little contact, and therefore no strong links, between the fields previously.) It seems probable that Granovetter's ideas will be important in future sociometric studies of the diffusion of innovations. In particular, they throw further serious doubt on the validity of the questionnaire response as a direct indicator of the flow of real scientific innovatory influence.

In order not to be unfairly critical of the authors in question, it must be pointed out that they do show some sensitivity to these issues. Crane concerns herself with student-teacher relationships, as well as with sociometric choice

[14] 'The Strength of Weak Ties', *American Journal of Sociology*, 78 (1973), 1360–80.

[15] The argument is not that strong links and weak links define a larger network than strong links only—that is common sense. The argument is that weak links only define a larger network than strong links only, which is a considerable advance on common sense. In retrospect it is precisely this property of social relations which allows Kadushin's original use of the idea of 'social circle'. See Kadushin, *op. cit.* note 3, and its subsequent adoption by Crane. It is concentration on strong links which allows Crane to keep her social circles of scientists within manageable dimensions.

and published collaboration relations, and of course the former relation may be the seat of the transmission and development of much tacit understanding. She writes:

> From each of these types of ties among scientists is obtained a somewhat different picture of the extent to which members of a research area are linked to one another. Nevertheless, if one uses the various indicators of linkage separately and then in combination, one is provided with a fairly complete picture of the amount of relatedness that exists.

Unfortunately, we are never shown the networks which are defined by these different relations, nor are expected differences systematically discussed. Without a clear understanding of the social relations defining the network, it is difficult to see what is meant by the phrase '. . . a fairly complete picture of the amount of relatedness that exists.' Relatedness cannot exist unqualified, any more than can equality. Earlier, Crane writes:

> The use of citation linkages between scientific papers is an approximate rather than an exact measure of intellectual debts. . . . Sociometric choices can also be criticized as unreliable indicators of relationships between scientists since it is obvious that the scientists may not recall all such contacts and may be biassed toward reporting contacts with more prestigious individuals and ignoring those with less prestigious individuals. In the absence of other equally good measures, however, I will proceed on the assumption that such approximate measures can be used to provide some indication of the actual nature of relationships among scientists in a research area.[16]

However, even 'these approximate measures' cannot be taken as indicators of anything other than the flow of articulated and therefore visible information.[b]

I will now attempt to illustrate some of the theoretical points made above about the diffusion of tacit knowledge by reporting on a study of a set of experimental physicists. I make no statement about the paradigm of which their practices are a part, and I have not attempted to delineate it. I have picked on experimental physicists because the aspect of their knowledge pertaining to their doing things is more evident than would be the case for theoretical physicists. But, for the reasons touched on above, I want to treat the study as an illustration which is also relevant for theoretical sciences. Perhaps more refined observational techniques could allow direct study of the processes pertaining to the development of theoretical concepts, but I do not know how to do this at present. Nevertheless, it is important that theoretical concepts (such as 'paradigm') do not become eroded by the assumption that what cannot be operationalized cannot be discussed.

[16] Crane, *Invisible Colleges*, *op. cit.* note 2, 20.

It has of course been stated many times before (for instance in the work of Menzel)[17] that scientists tend to claim (on questionnaires) that they use informal means (and even unplanned sources) for the transmission of 'technical' information. Sociologists have treated communication as though it could be exhaustively divided into the two categories 'formal' and 'informal'. Informal communication has then been treated like a more flexibly packaged version of formal communication. I want to go beyond this and stress not only the informality of some information exchange, but also its necessary capriciousness—a symptom of the lack of organization of inarticulated knowledge into visible, discrete, and measurable units.

The study I report here is of a set of scientists in different laboratories engaged for one reason or another on the same problem—namely, the building of an operating TEA laser. There is no claim that this set represents a 'group' of scientists in any sense other than a definitional one. It is true that a few members of the set saw themselves as members of a 'club', and were linked biographically and occasionally bibliographically, but the choice of a set of scientists working on the same narrowly defined problem is not supposed to imply that they communicated more with each other than with others, or were bounded in any way other than by this particular research interest. Such a set, however, has particular methodological advantages for careful research into communication among scientists. These advantages depend upon the small size of the set involved, the contemporaneity of the scientific research, and the visibility of many of the physical parameters of the technique. Thus members of every laboratory involved could be interviewed in depth, about interactions which were comparatively recent. Visible technical variations in the laser itself could be used as a lever for investigating, at a deeper level than is usually possible, the content and quality, as well as the quantity of communications among the members. In addition, a criterion of successful transfer of the technique was readily identifiable—name, the operation of the laser.

THE TEA LASER; THE DESIGN OF THE STUDY; THE PARAMETERS OF THE LABORATORIES INVOLVED[18]

In the late nineteen-sixties, many laboratories throughout the world were attempting to increase the power output of gas lasers by increasing their

[17] e.g. Herbert Menzel, 'Planned & Unplanned Scientific Communication', in B. Barber and W. Hirsch (eds.), *The Sociology of Science* (Glencoe: The Free Press, 1962), 417–47.

[18] Many non-sociological details are given so that the reader may judge how typical an area of science is dealt with here.

operating pressure. Early in 1970, when no-one else had achieved successful operation at pressures above about half an atmosphere, a Canadian defence research laboratory (which I will call 'Origin') announced the 'Transversely Excited Atmospheric Pressure CO_2 laser', which soon became known as the 'TEA laser'. In fact, the device had first been operated early in 1968, and a more sophisticated version had been built by the autumn of that year; but both generations of laser were classified for two years. Since 1970, a variety of models differing in design detail have been produced by many laboratories, and two or three firms are now marketing versions of the device.

The difficulty involved in constructing high pressure gas lasers is that of producing a uniform 'pumping' discharge in the gas, rather than the arc breakdowns that usually occur in electrical discharges in gases above a few torr.[19] The solutions employed in the TEA laser involve pulsed operation, with pulse times too short to allow an arc to build up, and a discharge transverse to the lasing axis which can then be of manageable voltage. The early design solution is known as the 'Pin-Bar' laser from its electrode structure, and second generation devices are known as 'Double Discharge' lasers because the main pulse is triggered by a pre-ionization discharge. The laser is, when used for most purposes, small enough so that several can be set up in the same laboratory room. The cost of the equipment might be between £500 and £2,000, mostly for mirrors and ancillary equipment such as oscilloscopes and detectors which would be found in any laser laboratory. A pin-bar laser could be built in a few days by anybody who had built one before, but a double discharge laser might take several weeks because of the complex machining operations which can be involved. Building a TEA laser is not 'Big Science'.

In the summer of 1971, I located seven British laboratories who had built, or were building, TEA lasers. This was eighteen months after the first news of the device from Origin, and some months before any device became available commercially. The laboratories were found by a snowball technique: I asked at each location for the names of others making TEA lasers. Three other methods were used to test the efficacy of this process and to search for isolates: a questionnaire sent to every physics department in the country; a check on research applications to the Science Research Council; and a search of literature citing the original papers in the field. No isolates were found, and the three methods located only three, four, and one laboratory respectively; all of these had been picked up by the snowball sample. Members of

[19] 1 torr = 1 mm. of mercury.

these laboratories were then interviewed in some depth. Interviews generally involved considerable semi-technical discussion and a tour of the laboratory. Subsequently, it has been possible to interview at the five North American laboratories which were involved in the transfer of the technique into Britain.[20] Interviews with the British laboratories continue, from time to time, and occasionally I have found myself playing an active role in the communication network.

The seven British laboratories involved consist of one government defence research laboratory (referred to as 'Grimbledon'), one other government-run laboratory ('Whitehall'), and five university departments of physics or applied physics ('Seawich', important as a gatekeeper; 'Baird', the only Scottish university; and 'A', 'B' and 'C'). The research of four of the laboratories involves, though not exclusively, development and improvement of the laser, while in the case of the other three concern is overridingly with the provision of a beam of radiation such as the TEA laser produces, for use in other experiments[21]. The North American laboratories (in Canada and the USA) consist of two government-run establishments ('Origin' and 'X'), one university department ('Y'), and one industrial firm's research laboratories ('Z', treated as one unit on the diagram below because they interact in a complex manner with regard to their overall external information flows.)

Patent rights to some variants of the laser have been sold to two Canadian firms by Origin and X. These firms are now manufacturing the laser, and one at least is having some success in selling the device as a scientific instrument. To date, however, only one has been sold to a British laboratory, in response to a failure in a programme of construction. In spite of their commercial interest, members of both Origin and X have published papers giving details of their lasers, read papers at conferences, and received visitors from all over the world, including Britain. For their licensees they have also provided engineers' drawings, and continuing consultancy. There is no evidence (and because the Canadians are far ahead, no rationale) for industrially- or militarily-motivated secrecy regarding TEA laser design in Britain—though this may not be the case with regard to some of the applications for which the laser is being used. Devices which may be classed as 'spin-off' from laser research have in one or two cases been commercially exploited. All the

[20] These interviews took place in the USA and Canada, and included an interview at Origin.

[21] Projects involve the use of laser radiation for damaging surfaces, producing and examining plasmas, construction of a tunable source of laser radiation, and possibly weapons and radar research.

laboratories involved in this study are concerned to publish articles in the scientific journals, and at the time of writing at least two of them have publications citing the original papers in the field. Others of the group have published results using the laser, but citing papers in other fields.

Though the prime concern of this paper is not to elucidate the social structure of a group of scientists, but rather to discuss the modes of transfer of real, useable knowledge among a set of scientists, it may be worthwhile to discuss the set briefly in terms of other criteria. Firstly, where they are linked bibliographically, it is into more than one subset. A bibliographical analysis would discover separate networks centred on, for instance, gas lasers, plasma physics, and spectroscopy. These networks would themselves be likely to be much larger than the TEA laser set, and would contain members with no interest in TEA lasers. I have no information about the frequency of contact between members of the TEA set compared to that between members of these other networks. Secondly, in a discussion of types of scientific specialty, Law[22] distinguishes between 'technique- or methods-based' specialties, 'theory-based' specialties and 'subject matter' specialties. It is not appropriate to assign the TEA set into one of these classes: for one thing, it is not large enough to be termed a specialty; and the TEA set seems to contain some members whose prime concern is laser building technique, and others who are part of subject matter specialties. For instance, the plasma diagnosticians are concerned with the subject of nuclear fusion. The TEA set is a set of scientists with one problem in common, and other problems not in common.

THE INVENTION AND CONSTRUCTION OF THE TEA LASER; THE DIFFUSION OF KNOWLEDGE

The process of development and construction of TEA lasers does not consist of the logical accumulation of packages of knowledge. The construction of the first device involved trial-and-error pragmatism in the face of written and verbal assurances that the principle could not possibly work. Because of this its construction seems to have taken most people by surprise. In retrospect, however, the idea of the pin-bar laser is very simple.

Serendipity was involved in the development of the double discharge laser, which grew in a quite unforeseen way out of attempts to improve the pin-bar system. This development was described as follows:

[22] J. Law, 'The Development of Specialties in Science: The Case of X-ray Protein Crystallography', *Science Studies*, **3** (1973), 275–303.

First of all we had rows of fins instead of pins, but this didn't work too well. We thought this might be because the field uniformity was too great so we put a row of trigger wires near the fins to disturb the field uniformity. Then we started finding out things. It did improve the discharge but there were delays involved. It definitely worked differently to the rationale we had when we first made it. . . .
Even today there is no clear idea about how to get this thing working properly. We are even now discovering things about how to control the performance of these devices, which are unknown. . . .
I have four theories (for how they work) which contradict each other. . . .
The crucial part (in getting a device to operate) is in the mechanical arrangements, and how you get the things all integrated together.
In the electrical characteristics of the mechanical structures. . . .
This is all the black art that goes into building radar transmitters. . . .

Further pointers to the non-systematic element in TEA laser development can be seen in the different details of design of electrode structures in different laboratories, and in the rival claims to the efficiency and power of these. Two of the laboratories which are part of the industrial firm 'Z' are copying quite different types of double discharge laser, each with a rationale for the superiority of its own model. Sometimes there are total failures in construction. One British group (not included in my sample because they have only recently entered the field) report that they built what was, so far as they could see, an exact copy of a laser successfully operated elsewhere, which failed to work. They were at a loss to explain this, and had simply given up. Another respondent reports being particularly impressed by one design of laser because he had actually seen it working. Normally, he reports, laboratories don't demonstrate their big lasers, 'because they don't work'. They will work perhaps one day in five, unpredictably.

The Role of the Literature in the TEA Laser Group

The first article submitted to a journal from Origin in late 1969 surprisingly was rejected, because it was, according to the referee's report, not particularly interesting. Subsequently, a news conference was held at Origin, and reports that the device had been built appeared in the scientific 'news' press, in such journals as *Canadian Electronics*, *Laser Focus* and *New Scientist*. Most British scientists first heard that the laser had been successfully operated from a small 'note' in *New Scientist*[23] referring to a 'Plywood Laser'. (One version had been constructed using a plywood box as the lasing cavity.) The first article in the formal journals appeared six months later in *Applied Physics Letters* of 15th

[23] 22nd January, 1970.

June 1970. This article provided more detail, but, as events proved, insufficient to enable anyone to build a TEA laser. In fact, to date, no-one to whom I have spoken has succeeded in building a TEA laser using written sources (including preprints and internal reports) as the sole source of information, though several unsuccessful attempts have been made, and there is now a considerable literature on the subject.[24] Here again, it is important to distinguish between the existence of publications and their contents,[25] for there is no doubt that at least the early articles in this field were intended as priority claims and little else.[26] Some respondents stated that the early articles would have been more misleading than helpful; another commented upon the growing trend of producing 'pseudo publications', which appear to be full scientific articles, but in reality will keep back a certain amount. A member of Origin commented:

What you publish in an article is always enough to show that you've done it, but never enough to enable anyone else to do it. If they can do it then they know as much as you do.

It cannot be stated categorically that no laboratory could ever build a TEA laser using published sources only, for it is at least possible for the device to be re-invented without even reference to the literature.[27] It was the case, however, that in the diffusion network, as far as I have explored it, written means of communication served at best to keep scientists informed of what had been done and by whom. In most cases, this information would already be known from the conference grapevine, but in the one instance where journals were really important (first informing British workers that the Canadians had succeeded in producing the TEA laser) it was a rapid publication in the 'news' press, rather than in the formal journals, which was the

[24] Papers citing the original article have appeared at a steady rate of about thirteen per quarter.

[25] See M. J. Mulkay, 'Conformity and Innovation in Science', *Sociological Review Monographs*, **18** (September 1972), 5–24, and F. Rief, 'The Competitive World of the Pure Scientist', in N. Kaplan (ed.), *Science and Society* (New York: Rand McNally & Co., 1965), 133–45.

[26] This seems not to be true of more recent articles. A recent article in the *Review of Scientific Instruments* is most unusually detailed. It gives instructions for the construction of a double discharge laser which include cross sectional scale drawings, mechanical and electrical layouts, geometric coordinates, photographs and instructions for making a tool to perform the complex machining processes, and even lists of manufacturers' part numbers for the electronics, as well as for the laser body itself. One American university laboratory is attempting to build a laser following this article, but even in this case their first act was to phone the authors to make certain that the tolerances given were really correct, and as a result of this call have decided to machine the electrodes from graphite rather than aluminium, and to contract this work out. The outcome of this attempt should be interesting.

[27] Grimbledon and a French laboratory were no more than six months behind Origin in the construction of a double discharge laser, but it seems that the simpler idea of the pin-bar device had not occurred to them.

source. The major point is that the transmission of skills is not done through the medium of the written word.

The Actual Process of Knowledge Diffusion

The laboratories studied here (other than Origin) actually learned to build working models of TEA lasers by contact with a source laboratory either by personal visits and telephone calls or by transfer of personnel.[28] The number of visits required depended to some extent upon the degree to which appropriate expertise already existed within the learning laboratory, but many capricious elements were also involved. Attempts at building which followed visits to successful laboratories often met with failure, and were backed up with more visits and phone calls. These failures were inevitable simply because the parameters of the device were not understood by the source laboratories themselves,[29] so that even when there was a close examination of the laser in a non-secretive atmosphere, many crucial elements might be missed. For instance, a spokesman at Origin reports that it was only previous experience that enabled him to see that the success of a laser built by another laboratory depended on the inductance of their transformer, at that time thought to be a quite insignificant element. A more remarkable example concerns the machining of the electrodes of one model of double discharge laser. Here the source laboratory provided information in the form of a set of equations for the so-called 'Rogowski profiles', along with the impression that machining tolerances must be small. The difficulties involved in making the electrodes were found by one laboratory to be insuperable. In the meantime, another British laboratory had produced the shapes roughly from templates and a filing operation, and an American laboratory had simply used lengths of aluminium banister rail, both with complete success.

Network Shape

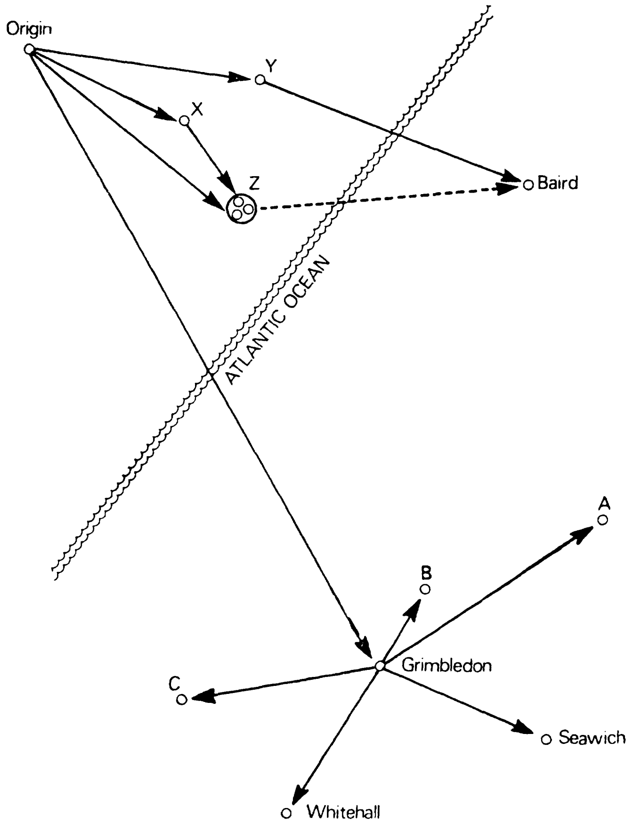
Figure 1 shows the main transfers of information which laboratories used in building their early TEA lasers. Visits between one laboratory and another are not included where they were unproductive. The dominant feature of the diffusion network is the lack of co-operation between British laboratories, other than Grimbledon. Initially a partial explanation of this might be thought to be that the six other labs did not know of each other's interest in the TEA laser. Figure 2, however, shows which British laboratories had heard of each other in connection with TEA laser building at the time of my initial

[28] Of the twelve links on Figure 1 below, four involved long-term movement of persons.

[29] Some versions seem to work because the trigger pulse pre-ionises the gas with a stream of electrons, and some with ultra-violet radiation.

survey in the summer of 1971.[30] Eight of the possible twenty-one links in Figure 2 are missing; that is, eight of the possible fifteen links between the six laboratories remaining if Grimbledon is removed. In many cases, then,

FIGURE 1.



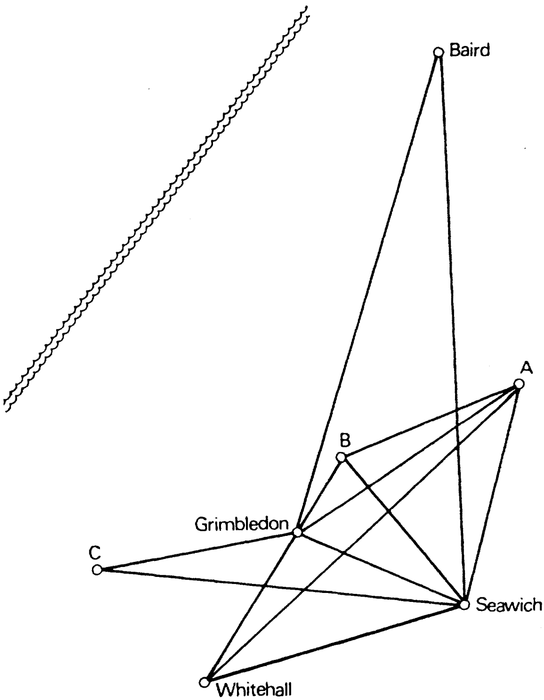
universities were ignorant that others were struggling with, or had solved, the same problems as they themselves. But even when they learned the names of the others as a result of my interview, no new contacts were arranged.[31]

[30] Current developments have included a substantial flow of information back across the Atlantic, particularly from Grimbledon, who have perfected their own distinctive and quite successful design. In addition, the French laboratory involved in this work has had a considerable influence on Grimbledon, one other British laboratory (A) and several Transatlantic laboratories. Figure 1 shows the initial flows of information only, but in any case the situation holding between British laboratories has not changed substantially.

[31] Later I deliberately fed information about useable progress in one laboratory to another. But they did not respond by trying to set up any information link.

It is also apparent that, in their contacts with Grimbledon, the question of the names of other laboratories doing this work was not a high priority; for Grimbledon could have supplied the complete list to any of them. Hence the factor of ignorance does not seem to have much value as an explanation of the lack of cooperation discovered.

FIGURE 2.



Another factor might be simply that when a laboratory has developed a useful link with another, technically senior to itself (and Grimbledon was well in advance of all the others) there may be nothing to be gained from a collaborative link with a research peer. That this was by no means the whole explanation can be shown in three ways. Firstly, there was the case of the machining of the Rogowski profile electrodes mentioned above. A loss of perhaps six months and £2,000 could have been avoided by collaboration here. There is no reason to suppose that this situation is untypical, and members of most laboratories stated in the interview that they would in general be able to learn from research peers. Secondly, there is some evidence that Grimbledon became less accessible as time went on because the number of enquiries was proving to be a burden.[32] Thirdly, in some cases, universities which were late into the field did approach laboratories less advanced than Grimbledon, but were not received in a completely open manner. Hence there is evidence that cooperation between laboratories other than Grimbledon would have been useful, that Grimbledon would have welcomed relief from the pressure of enquiries upon itself, and that the universities were aware of these factors, both in the cases of the seekers of help and the potential sources.

Nobody reported that they were ever subject to an outright refusal of permission to visit another laboratory, and nearly everybody reported at the outset of my interview that they were 'open for anybody to look around whenever they wanted'. Tactics for maintaining secrecy—where there is no military or contractual obligation—are less forthright. Sometimes only parts of the set-up will be demonstrated. Thus one scientist reports of a visit to another laboratory:

> They showed me roughly what it looked like but they wouldn't show me anything as to how they managed to damage mirrors. I had not a rebuff, but they were very cautious.[33]

A more subtle tactic is that of answering questions, but not actually volunteering information. This maintains the appearance of openness while many important items of information can be withheld because their importance will not occur to the questioner.[34] One scientist put i :

> If someone comes here to look at the laser the normal approach is to answer their questions, but . . . although it's in our interests to answer their questions in an information exchange, we don't give our liberty.

Another remarked succinctly:

> Let's say I've always told the truth, nothing but the truth, but not the whole truth.[35]

The major explanation for the lack of productive cooperation between the universities is to be found in their sense of competition with one another. Although this was sometimes direct competition (as in one case where an application for a research grant was turned down because of the similarity in

[32] One of the inventors of the device reports that this development programme was halted for a full year as a result of demands for his appearance at conferences and lectures, etc. He calculates that in the year following the announcement of the device he travelled 86,000 miles.

[33] I was allowed to see the apparatus in question at this laboratory. Presumably this was because of my technical non-competitiveness.

[34] Only detailed technical probing elicited the admission that this strategy was used.

[35] There is probably an etiquette which governs the type of detail a visitor may ask for, and the type of record he may make of it. Memory is allowed, but photographs, for instance, are not.

the proposed project to that in another university), usually the feeling was more diffuse and extended to scientists working with different research goals. Several laboratories seemed to fear that all possible directions of research using the laser might be monopolized by the larger organizations. A typical comment was:

A small laboratory like this has to be fairly careful what we say to other people who have got larger laboratories and more facilities, because they might pick up our ideas and be able to go ahead faster and we do find this kind of barrier operating. There's nothing I would like more than to be able to tell everyone everything.

The explanation of Grimbledon's unique willingness to act as the 'national gatekeeper', as it were, appears to be that it was sufficiently advanced and with sufficient resources, to be quite secure from attacks upon its prestige from British universities. Thus they were not only the laboratory best equipped to supply laser-building skills, but also the one with least to fear from doing so. This seems to account best for the persistence of the predominant star shape of the diffusion graph.

Extra-scientific Factors Involved in Information Links

As stated, I have no evidence for or against any closure or boundedness of the set or sets of communicators of which these laboratories might be construed as being part, and as the notion of social group depends on some such element,[36] the assignation is not appropriate here. Nevertheless, it is the case that all but one of the links shown on Figure 1 involved more than simply the provision of laser-building information. Of the twelve links shown, six were preceded by a history of information exchanges—about, for instance, other types of laser, electronics, or laser output detection devices. Nearly every laboratory expressed a preference for giving information only to those who had something to return. Of these six links, three also involved elements of formal organization (between government laboratories) and one (between Grimbledon and Seawich) an earlier period of colleagueship and co-authorship (at another government laboratory). Of the five remaining links all included an element of direct or indirect acquaintanceship. Seawich played an important role here as a gateway to Grimbledon.[37] One of the relations within North America was set up as a result of a member of the seeking laboratory having been best man at the wedding of a member of a different

[36] Some of the confusion over this idea is discussed in Gaston, 'Communication and the Reward System . . .', *op. cit.* note 2, 26.
[37] Seawich also introduced me to this security-conscious laboratory.

department of the source laboratory. The one relationship set up simply by the expedient of writing a letter to the source laboratory threatened to be very brittle at first, but has since been reinforced by student placements at the source.

Four of the twelve transfers of information have involved a personnel transfer—on visiting fellowships, student placements, or just periods of work at the source laboratory. Of course, the investigation of such transfers is quite outside the scope of research which depends upon frequency of contact to define relations, such as some of the work criticized earlier in the paper.

The importance of friendship relations explains in part the isolation of Baird, the Scottish laboratory, from the other British laboratories, for while they had no special friendship links across the border one of their members remarked of the American scientists from whom they obtained their information, that:

> . . . these people of course had got a Ph.D. working in the same group under the same supervisor, albeit eight or ten years ago. But they're still one of our family.

In this group, then, extra-scientific factors played an important part in the setting up of scientific communication relations, and to this extent some of the findings of the researches on social groups of scientists are corroborated.

DISCUSSION AND CONCLUSION

My argument is that the nature of much scientific knowledge is such as to make it difficult to organize and hence difficult to investigate accurately with conventional sociological techniques. This argument is meant to apply to all scientific fields, but for methodological reasons one particular set (rather than a random sample) of scientists was chosen to illustrate the argument. *Ipso facto* this set is untypical of scientists in general and their area of science is untypical of science as a whole. What is more, this area is probably untypically suitable for illustrating my thesis. This is because the field is new and little of the corresponding tacit knowledge will have been learned during the scientists' apprenticeships; and because experimental work is involved, so that much tacit knowledge is embodied in visible rather than abstract objects. It may then have been difficult to produce the appropriate demonstrations using an established field, or a theoretical field, as an exemplar. Indeed, many studies, using conventional techniques[38], have suggested thatt heoreticians are not conscious of gaining much information from other than formal channels. Nevertheless, unless there is a serious mistake in the philosophical underpinnings of the argument, the *caveat* regarding the use of investigative techniques which treat all knowledge as homogeneously measurable must apply to theoretical science as well as experimental science,[39] for, as has been argued, a lack of consciousness of a source of knowledge is not the same as a lack of importance. The point is, as summed up by Ravetz:

in every one of its aspects, scientific inquiry is a craft activity depending on a body of knowledge which is informal and partly tacit.[40]

I have attempted to show the complexities and uncertainties involved in the transmission of one scientific craft. These include the overt concealment of information by scientists who feel that they are in competition with others, and the effect on information transmission of personal and biographical factors which have no relation with the scientific subject matter in question, as well as the less tangible barriers which are the main concern of this paper. In the process described, publications have appeared frequently in the journals, but seemed to have had no significant information content. Even in the case of informal communication, scientists have often made use of techniques which allow an appearance of openness alongside an underlying secrecy. In cases where there has been no deliberate secrecy, a systematic transfer of knowledge has sometimes been impossible, for the source scientist has not been aware of all the relevant parameters. Thus one or both scientists involved in an interaction can be unaware of whether or not useable knowledge is being transferred. Significantly, all the laboratories which acted as sources of knowledge had completed the task of building an operating TEA laser; no-one could act as a middle man unless he was himself practised in the skill. This suggests that a participant in the flow of knowledge here was not simply a carrier of packages of information but a part of a small scientific culture.[41] In short, scientific knowledge is heterogeneous and its transmission is often capricious. This suggests that there might be a disjunction between the real

[38] e.g. Gaston, 'Big Science in Britain . . .', *op. cit.* note 2.

[39] I find the often-made distinction between science and technology too untidy to be useful. For an interesting discussion of this point see L. Sklair, *Organised Knowledge* (London: Hart-Davis MacGibbon, 1973), ch. 2.

[40] Ravetz, *op. cit.* note 12, 103.

[41] Respondents twice described failed attempts to learn to build the laser from someone who 'had all the particulars', but had not themselves built one. The point is that the unit of knowledge cannot be abstracted from the 'carrier'. The scientist, his culture and skill are an integral part of what is known.

sociological significance of interactions among scientists, and any data which can be gathered on this topic by the methods which have been conventionally used by sociologists and information scientists.[42]

I have not attempted to demonstrate that scientific specialties are not organized in social circles—though I have methodological reservations—but to suggest that the research methods used by others to demonstrate this have not yielded any structure with a significance for a sociological theory of scientific knowledge. A prerequisite of any such study must be an acknowledgment of the importance of all the elements and uses of scientific knowledge, not only the formal and informal elements, but the political, persuasive, and emotive, and even the intangible and unspeakable. It is possible to speak *about* that which cannot be spoken.

NOTES

[a] The phrase is taken from Peter Winch, *The Idea of a Social Science* (London: Routledge & Kegan Paul, 1958). Both the notion of 'tacit knowledge' (Polanyi) and the work of Kuhn are immanent in the philosophy of Wittgenstein, particularly *Philosophical Investigations* and *Remarks on the Foundations of Mathematics*. This is nicely brought out in the writings of Winch, a follower of Wittgenstein. For instance he writes:

> Imagine a biochemist making certain observations and experiments as a result of which he discovers a new germ . . . assuming that the germ theory of disease is already well established in the scientific language he speaks. Now compare with this discovery the impact made by the first . . . introduction of a concept of germ into the language of medicine. This was a much more radically new departure, involving not merely a new factual discovery within an existing way of looking at things, but a completely new way of looking at the whole problem of the causation of diseases, the adoption of new diagnostic techniques, the asking of new kinds of questions about illnesses, and so on. In short, it involved the adoption of new ways of doing things by people involved, in one way or another, in medical practice. An account of the way in which social relations in the medical profession had been influenced by this new concept would include an account of what that concept was. Conversely, the concept itself is unintelligible apart from its relation to medical practice. (*The Idea of a Social Science*, 57.)

and

> 'Time' in relativity theory does not mean what it did in classical mechanics . . . (and) . . . we shall obscure the nature of this development if we think in terms of the building of new and better theories to explain one and the same set of facts; not because the facts themselves change independently, but because the scientists' criteria of relevance change. And this does not mean that earlier scientists had a wrong idea of what the facts were, they had the idea appropriate to the investigation they were conducting. ('Nature & Convention', *Proceedings of the Aristotelian Society*, **60** [May 1960], 231–52.)

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[42] I am sure this disjunction explains conclusions such as Gaston's that in a relatively slow moving scientific field, formal channels of communication would be sufficient if there were immediate publication when communications were received. Gaston, 'Big Science in Britain . . .', *op. cit.* note 2. See also Garvey and Griffith, who seem to misunderstand the significance of interpersonal contact when they write: 'Too often the only informal channel available is the inefficient and expensive one of persons seeking out a source, discovering its originator, and arranging to meet him face to face.' W. D. Garvey and B. C. Griffith, 'Scientific Communication as a Social System', *Science*, **157** (1967), 1011–6.

After writing this passage I received a pre-print of an article from David Bloor entitled 'Wittgenstein and Mannheim on the Sociology of Mathematics', since published in *Studies in the History and Philosophy of Science*, **4** (1973), 173–91. Here Bloor discusses the implications for sociology of Wittgenstein's *Remarks on the Foundations of Mathematics* in a far more clear and complete way than I could hope to. Bloor also cites Winch, and is careful to disown the overall Winchian position that sociology is misbegotten philosophy; rather, he suggests that Winch unwittingly shows that the social sciences are required to illuminate philosophical problems. Ernest Gellner makes the same point in 'The New Idealism—Cause and Meaning in the Social Sciences', in Lakatos and Musgrove (eds.), *Philosophy of Science* (Amsterdam: North Holland, 1968).
Gellner writes:

> If what matters is cultures, and these are the objects of the studies of social scientists, it follows that philosophy and social sciences have the same subject matter, and the correct method in the one field is also the correct method in the other. From this he [Winch] tries—quite mistakenly in my view—to inform social scientists of the correct method in their field, by deduction from what he considers the correct method in philosophy; while the proper procedure is, it seems to me, to argue the other way, and conclude to the mistaken nature of the method in philosophy, from its inapplicability to the concrete objects of the social sciences. (page 400.)

[b] It being the case that some sociometric measures are sensitive to even slight inaccuracies in data collection, there are grounds for suspicion of the results of studies like Crawford's on sleep researchers. Crawford's network is defined by the question 'Who have you contacted at least three times during the last year ?' Part of her analysis is in terms of cliques and isolates, though cliques may be joined or split by the addition or subtraction of only one link. Further, the choice of the number of three contacts seems entirely arbitrary. It would be interesting to know the extent to which the network topology would be changed by the substitution of, say, two or four contacts per year. The same comments apply to her analysis of the network 'core' defined by scientists who have at least three contacts per year with at least six others. See, for instance, J. C. Mitchell, 'The Concept and Use of Social Networks', in J. C. Mitchell, *Social Networks in Urban Situations* (Manchester: Manchester University Press, 1969); and P. Abell and P. Doriean, *On the Concept of Structure in Sociology* (University of Essex, 1970, unpublished mimeograph.)