

Research in human geography
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Writing Geographically

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Unpublished research isn't research . . .
Publish or perish . . .

These clichés are widely used in academic circles, and encapsulate two of the major features of academic writing. They are positive statements, indicating how academic society operates. Without an appreciation of the norms that they express it is difficult to appreciate in full the nature of any academic writing.

The first of the statements identifies a major role of the staff members of universities, polytechnics and colleges. They are expected – in many cases required¹ – to undertake research, not simply as a way of indulging themselves and filling in their time, but to contribute to the advancement of knowledge. They are, in the widest sense of that term, scientists, working within communities that share five values, according to Mulkey (1975, p. 510):

- 1 *originality* – science is concerned to advance knowledge through original research (i.e. work that is not simply a full replication of previous research, except in certain conditions that fall under the fifth value);
- 2 *disinterestedness* – science is concerned to advance knowledge and is pursued by scientists for that reason alone, with personal rewards having no influence on the conduct of research;
- 3 *universalism* – scientific judgements are based on criteria which are applied impartially to the research and not at all to the researchers;
- 4 *communality* – all information is shared, and its provenance recognized;²
- 5 *organized scepticism* – all research results are subjected to constructive criticism, in order to evaluate their contributions to the advancement of knowledge.

Research findings must be reported, therefore, in order that they can be evaluated and added to the store of knowledge; researchers who do not report their findings are failing to contribute to scientific goals. The need to report must be met by a reporting mechanism. Research

results can be transmitted by word of mouth, in formal and informal situations; academic conferences provide major arenas for such communication among peers, as do smaller seminars, colloquia and other gatherings; teaching contexts allow researchers to report their findings to students, potential researchers and others. But such transmission is ephemeral, and the message is potentially distorted by the selective memories of the audience (including their note-taking) and the mode of presentation. It is also very partial, since it is extremely unlikely that all those who would like to hear the report can be present; repetition at different times and places is possible but inefficient, and still faces the ephemerality problem. And so publication, the permanent recording of the results, is the acknowledged way of contributing, literally, to the store of knowledge. Research that isn't deposited in that store, therefore, is widely regarded as unpublished research.³

Before turning to the nature of the store, and how material can be deposited in it, it is necessary to take account of the second statement with which I opened this essay. The scientific values set out above suggested an operation organized around very high ideals. As I expressed it elsewhere (Johnston, 1983a, p. 18), it appears that: 'academic work is carried out in a neutral fashion; there is a complete lack of partiality, self-seeking, secrecy and intellectual prejudice. The existence of objective criteria for assessment is assumed, as are high levels of ability and humility on the part of members of the academic community'. But academic communities rarely, if ever, achieve those high ideals, for they are peopled by individuals who, whatever their commitment to science, are pursuing careers: their quality of life, their status, and their power are closely linked to their success as scientists (see Dixon, 1976). This may not present a problem if both universal criteria of quality are recognized and applied, and all people of similar quality receive similar rewards. But neither is the case. Certainly the second is not, for successful scientists compete for a relatively small number of 'top jobs'. One must not only be good, therefore, but must prove that one is better. (Not all academics necessarily aspire to the 'top jobs'. Using the analogy of 'optimizing' and 'satisficing' behaviour – see Pred, 1967, for example – many are satisfied with a certain level of achievement below the top level.)

How, then, does one prove this quality? As already indicated, a major index of completed research is the publication of its findings; thus the nature of a scientist's published work is used to indicate the quality of work done. And, in a general sense, the more that is published, the greater the scientific contribution. (This creates a major problem for those evaluating scientists, since quantity is not necessarily to be equated with quality. One scientist may publish relatively little, for example, yet contribute more to the development of a field of study than another who

publishes a great deal. A major contribution to the history of geography has been made by Clarence Glacken, for example, but he produced little (Glacken, 1983) apart from his magisterial (1967) *Traces on the Rhodian Shore*. An academic's publication record, then, is used as the major index of the research record – within the context of the publishing norms of that individual's field of study, as set out below. It is used to assess both the individual's claims to occupy the various levels of the academic job hierarchy – hence 'publish or perish' – and applications for support to undertake further research.

Writing, then, is a fundamental characteristic of academic life: it provides a record of completed research and a portfolio of material that can be used to assess an individual as an academic. Within academia as a whole, the conventions relating to writing vary somewhat, but in terms of applying the guidelines set out above geography is clearly middle-of-the-road, though not without its peculiarities. Illustrating the nature of that road is the purpose of the remainder of this essay.

Research in Geography: the Norm

There is a stereotype view of the nature of research in geography – frequently equated with *the scientific method* (as in Abler, Adams and Gould, 1971) – which presents it as a clear sequence of stages. A research area is identified, and a general *model* of the subject-matter is constructed, comprising a simplified representation of some aspect of the complex 'real world' – the pattern of land-use in a rural area, perhaps, or the changing shape of a river cross-section as one moves from source to sea. The purpose of that model is to guide empirical investigations, to structure questions that can be answered in an experimental or field situation. Such questions are usually termed *hypotheses*, which are hunches or controlled speculations, statements of what the researcher expects to find – that the intensity of land-use decreases with increasing distance from a market, for example, and that the cross-section becomes flatter and broader with increasing distance from the source. A piece of empirical research is then devised to ~~test~~ the hypothesis.

A report on a piece of research therefore should involve a discussion of the model that guided it, a presentation of the hypothesis and how it was derived, a clear statement of the methodology employed in the test, and an evaluation of the hypothesis in the context of that test. This is a general model of 'the scientific paper' – it is a statement of 'the question I asked, why I asked it, how I addressed it, and the answers I got'. In some disciplines (e.g. psychology: see Spencer and Blades, 1986) the papers are very terse and matter-of-fact; in others (including much geography) they are more discursive, and there is some general discussion

of both the model and the implications of the test.

According to this model of the practice of science, each experiment can be equated with a research report, with a paper published in a scientific journal. As a consequence, one could infer that the design and implementation of a research project should include within it a report-writing stage, and that the whole procedure follows a very stereotyped pattern. Thus, for example, I became interested in the possible impact of campaign spending on British election results. A general model was devised which incorporated a series of relationships between a party's votes at a previous election, how much it spent at the next, how much its opponents spent, and how many votes it won. Specific hypotheses were derived, tests formulated and conducted, and reports of the various tests were published (e.g. Johnston, 1985a, b). To a considerable extent this work followed the model set.

A major question is raised, however. I stated in the previous paragraph that 'I became interested in . . . Why did I develop that interest and, more importantly, why did I translate that general interest into a piece of research? There are, after all, many other topics that I became interested in without these becoming the focus of any research endeavour. The things we choose to do research on are not random topics (or they shouldn't be). They are part of a larger endeavour, of a concerted effort to understand a certain topic or body of subject-matter.

As researchers we are constrained, if not blinkered, by the academic environment into which we are socialized. Our education and training become progressively more specialized – in considerable part because the store of knowledge is now so great that we can only contribute to it by specializing in some small portion. Many of the questions that we might pose in our everyday life (why is it wetter on the west coast? why do many people choose to patronize the nearest pub to their home?) have been answered by others. There is no point in us replicating their work, since that would not contribute to scientific advancement. We need to know all that has been already achieved, so that we can focus on as-yet-unanswered questions. So much has been achieved that we can only master a small proportion of it, and so our education and training focuses us on particular subjects.

During that education and training we discover, for our chosen field: what is already known (i.e. the results of previous research); what questions (further hypotheses) this knowledge raises; and how those questions can be answered. We are educationally socialized into what Thomas Kuhn (1962) termed a *research paradigm*. Our own research is then not a random series of events. Rather it is a sequence of speculating and testing (and reporting) guided by a framework. The particular questions that we ask may be the consequence of our observations of the world we live in – of candidates spending money trying to convince

us to vote for them, perhaps – but those observations themselves are influenced by the frame of reference provided by our research paradigms, as is our incorporation of them into our models.

Although each scientific paper can be equated with a particular piece of research, therefore, it should not be deduced from this that each paper is independent of all others. In the vast majority of cases exactly the opposite is true, and the research reported in a single paper is but one contribution that the researcher involved is making to that subject area. (An excellent index of this is self-citation, the referencing by researchers to other papers that they have written on the same topic. Such self-citation is characteristic of a majority of scientific papers, and indicates the research is perceived, by the researchers, as a cumulative process.)

Research, then, is cumulative, which suggests the need for forms of writing other than the *research report*. In particular there is a need for the *research synthesis and evaluation*. The findings of the individual pieces of research need to be brought together, in syntheses (sometimes known as *theories*) which integrate the various separate pieces. Without these, each individual researcher must maintain a continuing synthesis, reading and evaluating every separate research report. The syntheses allowed some of this work to be avoided, by identifying the key contributions to the area (those that should be read and evaluated and not cited – rather than sighted! – at second hand only) and pointing the way forward for future research, perhaps by proposing models that are improvements on those currently employed. As the volume of research work has increased, so the need for such syntheses has grown. Geographers now have two specialist journals – *Progress in Human Geography* and *Progress in Physical Geography* – oriented entirely to that task, and other specialized, non-serial publications provide periodic reviews. (As in the, now defunct, series that I edited with David Herbert – *Geography and the Urban Environment: progress in research and applications*. Specialist journals – such as *Urban Geography* – now provide reviews of subfields of subdisciplines, indicative of the explosion of research reporting: more is being done, and more people are faced with the 'publish or perish' syndrome.)

A further need is for writing on *research methods*. Many questions have to remain unanswered because we lack the ability – usually either technical or technological, but not necessarily so – to tackle them. Thus some research effort must focus on these methodological issues. The development of procedures should, of course, be set in the context of a particular research problem, and reported accordingly. But many procedures have wider applicability than the particular question to which they were initially addressed, and their separate presentation can more readily bring them to the attention of other researchers. (In some cases

one can identify a 'have method, will travel' syndrome, of people with methods seeking valid applications: this was a charge levelled at some geographers during the 'quantitative revolution', for example. In others the general potential of a procedure is identified, and much effort is then expended realizing this potential: this is the case at present with remote sensing.)

As research proceeds, so more material is provided for the syntheses, and more methods are developed that may be relevant to other applications. The store of knowledge is being built up. New researchers are socialized into that store, into the contents of a particular research paradigm. For their education and training a particular form of synthesis is needed, not one which summarizes and evaluates recent research reports but one which encapsulates an entire field, which is a comprehensive state-of-the-art statement of what is known, of how knowledge is obtained, and of where we should go next. This is the *textbook*, the point of entry for the new, would-be researchers in a field. These could be very stereotyped. Some, especially in the area of research methods, are – as in statistical methods texts. But most are idiosyncratic, reflecting the authors' selection of material and framework for presenting it (compare, for example, Morrill's (1970) *The Spatial Organisation of Society*, Abler, Adams and Gould's (1971) *Spatial Organisation, the geographer's view of the world* and Haggett's (1965) *Locational Analysis in Human Geography*, all of which are syntheses of the same research paradigm. Note that Haggett's book was in two parts, one – 'Locational models' – providing the synthesis, and the other – 'Locational methods' – the procedures.)

Getting it Published

According to what we might call the standard model of scientific research there are several types of publication, therefore, each written in a particular way for a defined audience. How, then, is the process of writing organized? The discussion here is divided into two. In the first part the focus is on the writing of scientific papers – research reports and syntheses and presentations of technical developments. The second is concerned with books.

Scientific Papers and Academic Journals

Most researchers, when they set out on a piece of work, have it in mind that they will produce one or more papers to report their findings. (Not all do. Many are simply attracted by the research activity itself and find writing it up the hardest task. Indeed much research work lies incomplete because the researcher didn't publish a report – because of a 'writing blockage', perhaps, a lack of time, the attractions of new research, or

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Most of these journals operate very similar procedures. Authors submit papers to the editor, who sends them to referees, asking for comments and recommendations regarding the suitability of the paper for publication in that journal. *Environment and Planning A*, for example, asks the referee to rate each paper on the following scale:

- 1 The paper should be published in its present form.
- 2 The paper should be accepted for publication, subject to suggested amendments being made.
- 3 The paper should be returned for revision (along the suggested lines) and then resubmitted (with the possibility of being sent to referees again).
- 4 The paper should be withdrawn and a much shorter version submitted.
- 5 The author should seek publication elsewhere.
- 6 The paper should be rejected without qualification.

In addition, it asks the following questions:

- 1 Is there sufficient original material (results, theories) to warrant publication?
- 2 Are there any obvious faults in empirical basis and/or theoretical reasoning?
- 3 Are there demonstrable errors in mathematics, calculations, units?
- 4 Are the terminology, nomenclature and units correct?
- 5 Is the order of presentation logical (allowing reasonable latitude for individual preferences)?
- 6 Could some parts be condensed and/or others expanded?
- 7 Does the paper read well?
- 8 Are the title and abstract adequate?
- 9 Are the figures and tables satisfactory and correctly labelled, or are some redundant?
- 10 Are the references appropriate and free from obvious omissions?

Editors and referees are important *academic gatekeepers*, therefore, with substantial power over both the development of the discipline(s) they serve and the careers of the individual researchers (if they can't publish, they perish). This power can be exercised partially. An editor may select sympathetic or unsympathetic referees deliberately; referees may be destructive or constructive. (Some editors send papers to referees with no indication of the author(s), although the practice of self-citation frequently destroys the anonymity that this seeks to create. In any case, most referees – supposedly expert in their field – should (and almost

whatever. The published record of completed research should not be equated with the 'unpublished incomplete research' record.) These papers will be sent to relevant academic journals. But which – for there are very many? And will they necessarily be published? Very few journals commission papers – they react to what is submitted to them rather than actively encourage (though an editor may solicit a piece). And extremely few accept all that is submitted to them; they evaluate the pieces very carefully, and many reject 50 per cent or more of what they receive, on a variety of grounds.

The choice of which journal to send a paper to would seem to be a straightforward one. The research is geographical, so it should be sent to a geographical journal. But there are many such journals – even in the English language and published in England. Basically, they are of two types. The first are the *general geographical journals* which cover the entire discipline (such as *Transactions of the Institute of British Geographers*, and *The Geographical Journal* in the UK and *The Geographical Review* and *Annals of the Association of American Geographers* in the USA). Most of these are published by *learned societies*, established to promote the subject as a whole, though some (the Royal Geographical Society and the American Geographical Society, publishers of the *Journal* and the *Review* respectively) serve a much wider, general audience than the more narrowly academic. Authors writing for readers of the latter's journals (i.e. *Transactions* and *Annals*) are thus addressing their academic peers, whereas those writing for the former's are presenting their findings to a wider audience, whose interests may not be the advancement of the frontiers of knowledge *per se*. Some general journals simply publish a wide range of research papers. Others focus on research reports and syntheses written for a certain purpose, such as contributing to materials that can be used in geographical teaching, as in the journal *Geography*.

The second type is the *specialist journal*. Most of these focus on a particular aspect of geography – such as *Economic Geography*, *Urban Geography*, *Journal of Biogeography*, and *Earth Surface Processes and Landforms*. Writing for them clearly involves the presentation of research reports, syntheses and methods to one's immediate academic peers. Most of them are produced by commercial publishers, and are aimed in particular at academic libraries. Few are very narrow, since overspecialization would probably mean a small potential market and relatively few sales. To achieve the needed market size, yet without generalizing the coverage too far (as with the first type) and thereby orienting towards a different type of writing, many such journals are explicitly multidisciplinary, focusing on a variety of approaches to particular subject-matter – as in *Environment and Planning A*, *Society and Space*, *Government and Policy*, and *International Journal of Urban and Regional Research*, all of which serve geographers, among others.

certainly do) recognize the work of a large proportion of 'anonymous' authors.) Even where no partiality is intended by the editor, the choice of referees can have an important influence on whether the paper is accepted. (A good test of this is the fate of rejected papers. The practice of many authors whose work is rejected is to submit it immediately to another journal, perhaps after some revision in the light of the referees' comments. The proportion accepted at the second - or subsequent - submission is not known; I have very few that have not found a home eventually! Of course, occasionally the same referees are used by the second journal. Problems sometimes arise when revisions are requested; authors have been known to resubmit the same paper, to the same journal, in the hope that either it will not be read in detail against the previous version or that a different referee will be used!)

One could deduce from this that there is a status continuum of journals - some have higher standards than others. This is probably a belief held by many writers, and correctly so in the case of some disciplines. But there is little evidence that it is so in geography. Some people do feel that greater prestige is achieved by publishing in some journals (the *Annals*, perhaps) than others, and I have been told by some that they would consider submitting something to such journals 'above them'. But there is nothing in the requests from editors to referees which suggest differentials in standards, and analyses of the 'value' of papers - such as citation counts - provide little evidence to suggest that the 'best' work appears in certain journals.

The differentials between journals are much more likely to reflect their varying markets, identified particularly by their titles. Thus, for example, the journal *Geographical Analysis* (founded in 1969 as a 'journal of theoretical geography': see Gollidge, 1979) is aimed specifically at work with mathematical and statistical strengths (its theory is positivist), and not surprisingly it is a preferred journal for writers of such work. In part such specialized journals are favoured for their 'externality effects'. A paper in *Geographical Analysis* will be published among others of similar orientation, and is more likely to be identified by the casual reader (perhaps an economist looking for such work) than if it were in a general journal, only looked at by such readers when specifically referred to. (As already noted, many general journals are published by learned societies and distributed to all of their members. It is sometimes claimed that many members merely read the contents pages of such journals, find that there is nothing in their specialist field of interest, and put them straight back on the shelves unopened: Clayton and O'Riordan, 1977).

An important influence on the market definition for a journal is the editorial role, particularly with the commercially produced specialist journals. The editors of journals produced by learned societies have less

freedom of action, because they are answerable to the governing bodies of those societies for the conduct of the journals, and members - who may be sore at the treatment of papers submitted - have a channel through which grievances can be expressed. Editors of commercially produced journals are answerable only to the publishers. These will no doubt be concerned that high academic standards are maintained, since a poor reputation is likely to affect sales. And the editors will not be expected to narrow the appeal of the journal and similarly reduce its saleability. But within those commercial constraints it is left to the editors to create an image for the journals, and to decide what sort of work will and will not be accepted. (They usually have unlimited terms in which to do this. Editors of learned society journals are almost all appointed - or 'elected' - for fixed terms.)

The differences between the two types of journal identified here are generalizations, of course, and no rigid classification can be imposed. Many writers can quote their experiences of what they identify as partial treatment by editors and referees, in both types of journal (see, for example, the comments by Brian Berry on the problems of getting the early quantitative work published, in Halvorson and Slave, 1978). Peter Gould (1979, p. 139) summarized such treatment in his analysis of the Angean period, when there were

new paths opening up, new connections being made, and real challenges being met. There was a sense of discovery and forging, of breaking out of the banal, factual boxes erected by the 'old men'.

But those 'old men' controlled the geographical communication system:

These were also the gate-keeping days, when the journals of the field closed ranks and stopped up the chinks to prevent the winds of new ideas from getting through. Some of the letters of rejection were extraordinary for their arrogance, sarcasm, and sheer venom, and we used to circulate the prize examples among ourselves as paper witnesses to the reaction of the current order. Eventually the reactions . . . became so predictable that few bothered to submit their writing thereafter, and in general this pattern has continued, so that even when a less conservative editor takes over, the traditional reputation of a journal lingers on. . . . One editor . . . has been forced to return papers containing mathematical notation to the authors without even sending them out for review (Gould, 1979, p. 142).

Some, at least, of the editors have been prepared to defend their positions. Wilma Fairchild (1979, p. 35) was editor of *The Geographical Review*. On her appointment she found that most of the Fellows of the American Geographical Society were not professional geographers, and so concluded that

balance in content was essential, that each issue must contain articles of general as well as of professional interest – a circumstance that became more difficult to implement with the thrusting wave of the 'quantitative' surge in the 1960s.

But she also realized that the surge could not be denied, and that continued professional support required publication of such work:

Clearly, quantification was a force whose time had come. . . . In accepting manuscripts of this genre, I insisted only that the methodology illuminate a subject that had originality and meaning; too often . . . an author seemed merely to be carried away by his enthusiasm for the techniques (Fairchild, 1979, p. 38).

The editor of the specialist journal *Economic Geography* was less catholic. Raymond Murphy (1979, p. 39) noted that the journal was founded for geographers and others 'who wished to have a part in the intelligent utilization of the world's resources'. As editor (1949–69) he had sole control of the content:

Most journals use referees or an advisory board to help make editorial decisions, but I did not do this. It was too time-consuming. Besides, I believed that authors accepted the decisions of the editor more readily than those of anonymous referees or board members (Murphy, 1979, pp. 41–2).

Thus he fostered a journal which he identified as 'widely used in college and university courses in economic geography and urban geography' (p. 42). The quantitative revolution posed a dilemma, for the new statistically oriented papers could mean little to those users of the magazine who were not trained in modern statistical techniques' (p. 42). Should there be a shift, to a new clientele? The decision was taken by Murphy's successor, but his own position is clear:

the new editor . . . felt that the future of economic and urban geography lay in more, rather than less, use of modern statistical techniques. To some, the change was long overdue; to others the magazine lost much of its interest and usefulness (p. 42).

Editors, working alone or in concert with publishers/sponsors, can determine a journal's style, and thus the pattern of geographical writing that it represents. Most, if not all, display some idiosyncracies. In 1969, a short paper was published in *The Professional Geographer* (a 'forum and journal of the Association of American Geographers'; the Institute of British Geographers publishes a similar quarterly – *Area*) on 'The bicycle as a field aid' (Salter, 1969, p. 369), which concluded that 'For reasons . . . of economy, mobility, independence and rapport with the field country population a bicycle is suggested as an important purchase after arrival in the field.' This intrigued me and some of my colleagues,

and over lunch one day we wrote to the editor (tongue-in-cheek; we presumed that the piece on the bicycle was similarly written and was published as 'light relief') proposing a series of such articles on: boots and their use; the surfboard and coastal research; the value of a dog in fieldwork; the tape recorder; the biro vs the pencil; and the benefits of automatic transmission in field surveys. (Incidentally, we had a departmental bicycle, available for local field work!). The reply we got told us that

The article which you found so objectionable reached us from a thoroughly reputable source – the chairman of one of the largest American geography departments – and we published it deliberately to call attention to the inadequacies of the desk-bound computerized geographer. I judge that you missed the point.

We were duly chastened! But the statement on the provenance of the paper interested us even more, and I responded that

It suggests that it is not what you know but who you know that matters. Over the last few years you have rejected two contributions from me. I was disappointed, but believed this to be on academic grounds, and so did not complain. Am I now to understand that if I had not sent these items to you myself . . . but had forwarded them via a 'big name' in American geography, then they would have been published?

The correspondence closed with me being told that my papers were probably rejected because the journal had a long backlog, and that

My remark that a reputable source recommended the bicycle article came about because I was aware that the article was prepared by a student and not by a faculty member, but the chairman of his department (and it is one of our largest and best), felt that what the boy had written was well done and of value. I concurred . . .

This is not to imply that the short paper should not have been published, nor that this is normal editorial practice; far from it. But it does illustrate the power of the gatekeeper – and of other senior members of the academic community – in the control of geographical writing.

We know very little of the process of writing in detail, and so do not know whether people have a clear end in view (a paper in a particular journal) when they set off on a piece of research. Clearly at some stage they must decide if they are going to write the paper, in what form, and for what journal (the latter is necessary at a pragmatic level because of differences in formatting conventions and because a paper can only be submitted to one journal at a time; some ask for a signed statement that this requirement has been honoured). Choice of journal is influenced by a variety of factors such as the intended audience (local, national, international; specialist, general; etc.). This may be important because,

for example, of what can either be left unsaid or briefly alluded to in references and footnotes: referees and editors for specialist journals are more likely to assume readers know the academic pedigree (the theoretical framework) of a piece of work than are those working for general journals.

One decision faced by many geographers is whether to seek publication in a geographical journal (specialist or general), a multidisciplinary journal, or a journal of another discipline (or indeed whether to publish in more than one of these, using the same material in different ways for the separate audiences). Although geographers draw heavily on the work of researchers in other disciplines, this interdependence is only rarely reciprocated: there are, for example, many more references to the works of political scientists by political geographers than vice-versa (see Laponce, 1980). Some geographers try to place papers in the specialist journals of other disciplines in order to 'display their wares', and some are rebuffed through, it seems, the territorial politics of the other discipline (see Gudgin and Taylor, 1979, Preface). Some see it as more prestigious to publish in that way: a physical geographer may feel that his or her scientific reputation is enhanced further by publishing in, say, *The Journal of Glaciology*, *The Journal of Soil Science*, and *The Journal of Hydrology* than in a geography journal.

There are strong pressures within academic geography to publish. Further, those pressures point in particular towards the publication of papers in reputable journals (i.e. those which use the refereeing system, sometimes grandly known as peer review). Lists of such papers are the key parts of the CV of an application for an academic post, including promotion, and for a research grant, because they indicate a commitment to the completion of research projects at acceptable standards. (In some universities, lists of 'acceptable' and 'unacceptable' journals exist — although these are rarely made public.) Because of the importance of publication, researchers as writers must accept the dictates of the gatekeepers, the editors and referees in particular, and most orient the reporting of their research to the acceptable ways (they must even, some claim, orient their research as a whole in order to meet the publication criteria).

How About a Book?

As already indicated, textbooks play a central role in a research paradigm, although their production is not necessarily considered to be research. They are, however, only one type of book produced by academic writers. The following classification suggests six types.

1 The instructional text, which teaches methodological topics —

statistical applications, computer programming, etc. These can vary in how the material is presented, but little more; they are evaluated on their success in getting the material across (assuming the absence of factual errors).

- 2 *The synthesis text*, which summarizes the substantive material in a discipline or, more probably, some segment of it. These are more likely to vary (as noted above) because of differences in the selection and organization of material; they are evaluated for their structure and coherence, as well as their presentation.
- 3 *The essay*, which is similar to the synthesis text except that the presentation is oriented more to the argument for a particular approach to the subject than to a summary of previous achievements; it is evaluated on the strength of the argument.
- 4 *The research monograph*, which is the report of a major piece of research that cannot be summarized in a single paper or split into several papers; it is evaluated in the same way as a paper.
- 5 *The collection*, either of previously published essays or of specially commissioned pieces, around a particular theme. Such multi-author texts usually reflect the inability/unwillingness of any individual to cover the entire field under consideration, so a group of experts is brought together to do that. Evaluation then is based on the overall coherence of the pieces as well as their individual qualities; coherence is usually harder to obtain if the pieces are reprints, frequently though not invariably of research papers.
- 6 *The reference work*, which in many cases is a particular form of synthesis text, such as a dictionary or encyclopaedia. Geographical reference works of course include atlases.

Four of these types (1, 2, 5 and 6) are clearly aimed at the usual textbook market — i.e. students. Those in the third type are more likely to be aimed at the writer's peers; those in the fourth certainly are.

Leaving aside type 5 for the moment, the size of the potential market for a book will decline from types 1 and 2 and 3 to 4; type 6 should have the largest market of all. Instructional texts are likely to attract most purchasers, and research monographs least. Thus it is not surprising that commercial publishers favour types 1 and 2, and that unsubsidized publishers (many university presses receive hidden, if not direct, subsidies, for example) are very chary of research monographs, especially in a discipline such as geography that has no market outside the academic (i.e. either the 'interested layman' or a profession of graduates).

Publishing a text is the result of negotiations between potential authors and publishers, negotiations which either may commence: an individual

– and many teachers find them of little use as teaching texts. (This is especially so with collections of reprinted research papers, which assume that readers are already socialized into the relevant paradigm.) For this reason other publishers, especially those aiming at big markets, are reluctant to contract for them. Pioneering, purpose-written collections are more favourably viewed, however.

There are three major differences between writing research papers and writing books, therefore. First, the paper is usually written without any prior agreement to publish it, while the reverse is the case with books. Secondly, the audiences differ: most books are written for students; most papers for one's peer group of researchers. And thirdly, the rewards from publishing papers are charisma, status and, perhaps, promotion whereas those from books are additionally financial. Such differences mean that the production processes involved are very separate; a good writer of research papers is not necessarily a good text/book writer, or vice-versa.

But is it all Like That?

All of the discussion in the previous section has presented a particular view of scientific activity, one in which progress is linear as new research builds upon the findings of that which went before. According to this view, geographical writing involves one of: synthesizing what we already know, suggesting the next set of questions to be asked; proposing means of answering those questions; and reporting the results of posing one or more questions. Work of the last type is the most frequent, providing empirical research reports that add to the store of knowledge within a clearly defined framework.

But is all science like that? Is all geographical writing set in that framework? In physical geography the answers are almost certainly yes, but in human geography they are not. In the last two decades there has been considerable reaction to the framework! A full review of the debates and their foundations is not called for here (summaries can be found in Johnston, 1983a, b), but a brief overview is needed for an evaluation of other forms of geographical writing.

Varieties of Science

Definitions of science abound. Most focus on it as a frame of mind, as a means of carefully scrutinizing evidence in order to account for some phenomenon or event. It is, as Dixon (1976, p. 36) tells us, 'no more than scrupulously applied common sense'. But is there only one form of scrupulous application, the hypothesis-testing procedure outlined above? The answer to this question is a focus of much debate. Among human

may wish to write a text, and will sound out publishers; a publisher may identify a lacuna in the supply of texts and seek an author to fill it. A contract is then signed, either before anything is written or, if the publisher is uncertain, after sample chapters have been produced and opinions received (much like referees' opinions on papers, except that saleability is under consideration, as well as quality; most publishers seek expert opinions on book proposals, which are only a few pages long). The author is writing for a specified audience. The publisher will want this to be as large as possible, hence the frequent advertising of British geography books that they are suitable for both introductory undergraduate courses and school sixth-form syllabuses.

Publishers vary in their market orientation. Most British publishers are content to ensure that there is a market for a proposed book and then, unless the delivered manuscript fails to meet the agreed criteria (too long perhaps, or badly written), will go ahead, publish and sell. They would like large sales, and the use of the book in many courses, but rarely undertake any major marketing 'hype'. Of course, outright flops have to be avoided. Some publishers take great care over offering contracts, therefore, whereas others are more liberal – but price their books higher to cover their gambles. The most successful are those able to judge potential authors as likely producers of good books. Many American publishers, on the other hand, are seeking to tap very big markets, especially in the large introductory undergraduate courses. To do this they will spend a lot of money and energy in ensuring that the book is 'right' for the market. The manuscript will be read by several referees, and much rewriting may be asked for. Copyediting will be substantial and, if necessary, a lot of effort will be put into the design of illustrations. They are making a major investment, and the writer may feel little more than a puppet (writing for an established market – perhaps even producing something very similar to another publisher's popular book on that subject rather than branching out into something new and risky, which may or may not create its own market). This can be irksome: as Gunnar Olsson (1975, p. vii) expressed it in the preface to *Bird in Egg*: 'It was then that Philistines offered to publish a book'.

Essays and research monographs are harder to publish than texts, especially in geography which has only a small number of academics plus students as its potential market. (Geographers often bewail their inability to attract an audience among the general reading public in the same way as, say, historians. Few have really tried, however.) Learned societies may publish them, as part of their subsidy to research publication, but few can be produced in that way.

The situation with collections is ambiguous. Some publishers are keen on them, but reviews are rarely favourable. Many collections have little coherence – especially those based on the papers offered at a conference

geographers in recent years, three separate types of science have been identified and promoted.

1 *The empiricist/positivist*, in which the scientist is portrayed as a neutral observer recording and analysing empirical phenomena (the pattern of land-use; choices of shopping centres, etc.) according to the tenets of natural science. The outcome of such work is the explanation of individual phenomena/events as exemplars of general laws, whose applicability is spatially and temporally invariant (e.g. the laws embedded in central place theory).

2 *The humanistic*, in which the scientist is an insider rather than an outsider, seeking to understand how events have occurred not as exemplars of general laws but as the outcomes of individual decisions. The scientific goal is appreciation not explanation.

3 *The realist/structuralist*, in which the scientist argues that the proximate causes of an event (the appreciations provided by humanistic studies) provide insufficient explanations, since they do not also account for the mechanisms that underpin those causes. (That I let go of my pen provides the proximate cause of why it falls to the ground, but does not tell me why my pen must fall if I do not hold it. The answer to that is provided by the law of gravity - which I have a theory of, but which I cannot apprehend; I know it by its consistent outcomes, not its existence. Similarly, industrialists introduce new machinery in order to increase productivity and profits, and we can appreciate their actions. But we can only understand why they do that through our theory of capitalism, which demonstrates the necessity of increasing productivity. The theory of gravity does not predict why I let go of my pen, and the theory of capitalism does not predict the introduction of new machinery - there are other ways of increasing productivity; the theories provide the context for studying the proximate causes.)

All three of these are scientific approaches, since all are concerned to provide rigorous accounts. (Some argue that humanistic accounts are not scientific since they deal with the subjective. This is a misrepresentation. Humanistic studies are concerned with the study of the subjective, but are themselves no more subjective than are studies in the other approaches.) The nature of those accounts varies, however. Empiricist/positivist approaches explain individual events as cases of general laws; humanistic approaches appreciate the reasons for actions, as expressed by the actors; and realist approaches explain individual events as the outcomes of particular cause-effect sequences - which may be unique (though not singular: Johnston, 1984, 1985a) and in no sense realisable.

The Uses of Science

Science is usually portrayed not just as scrupulous analysis but also as the production of 'useful knowledge'. But if there are three approaches to science, are there also three types of 'useful knowledge'?

1 *Science, engineering and control*. The empiricist/positivist approach to science is geared to produce tools that can be used to manipulate - whether matter or society. Physical laws are used to ensure that buildings can withstand certain stresses, for example, and (assumed) economic laws are used to promote the economic health of a society (as in the manipulation of the money supply to influence the rate of inflation). This type of science is linked to environmental and social control, therefore, with the laws suggesting what needs to be manipulated to achieve certain ends and avoid others.

2 *Science and awareness*. The goal of humanistic approaches is not to provide laws of behaviour that can be used to predict and/or modify action, for such approaches deny the existence of any laws and their implications that people lack self-control. Instead the goal is to promote awareness, both self-awareness (increased appreciation of oneself, as in psychoanalysis) and mutual awareness (increased appreciation of others). The outcome of such increased awareness is a greater ability to exercise self-control and to accommodate the variety of others.

3 *Science and emancipation*. Both empiricist/positivist and humanistic approaches deal with events but ignore the mechanisms underlying those events: people appreciate why actions are taken to increase productivity but not why such actions are necessary in a capitalist social formation. Only a theoretical understanding of capitalism can provide the latter appreciation, thereby emancipating people from their ignorance of the forces driving society and hence identifying the possibility of altering those forces.

These brief paragraphs provide only the barest outline of the differences between the various approaches to science (for a fuller discussion, see Johnston, 1986). They are sufficient, however, to indicate that each approach must be associated with a separate style of geographical writing. As already demonstrated, empiricist/positivist science is a cumulative process, and each new piece builds on previous ones. This allows for the development of the textbook-research paper writing framework.

Realist science is also cumulative, in that its goal is to build a coherent theory of the mechanisms underlying action. Individual pieces can be written either as extensions to the existing theory - as illustrated in

journals such as *Society and Space* and the *International Journal of Urban and Regional Research* – or as uses of the theory to illustrate the mechanisms underlying particular empirical events. But since the theory is intended to be coherent, it is often necessary to reconstruct the whole as one tackles a part (as in Harvey, 1982). And individual pieces cannot be made to conform to a set framework.

Humanistic science is not cumulative, however. It is concerned with the individuality of events, people, and places, and with conveying that (though there are difficulties in expressing individuality through the use of language the words of which imply generality). Presentation of such interpretations does not fit the mould of the 'classic' scientific paper, since there are no hypotheses to be tested, no conclusions to be drawn; instead, a method is being applied to assist in the appreciation of a topic. That method involves the scientist addressing the texts (verbal, written, symbolic – including landscape etc.), and putting the material together in a coherent interpretation. Presentation of that interpretation is usually in essay form – which may be of book-length – and evaluation of it involves other scholars asking 'does that interpretation square with mine?', etc. The interpretation is itself personal, and there are no 'objective' criteria to say whether or not it is 'right'. For this reason, editors of journals find it difficult to assess essays sent to them according to their usual criteria: 'Is it telling a good story, which helps me appreciate the time/place described?' is rather different from 'Are there any demonstrable errors in mathematics, calculations, units etc.?' Because humanistic essays are personal interpretations, they are both difficult to evaluate 'objectively' and yet easy to attack. This is well demonstrated in historical research, in which critical evaluation – in book reviews, for example – is frequently much harsher than is the case with reviewing in geography (and frequently much more personal too). This suggests that either the reviewer or the scholar whose work is being reviewed has been unscientific. But this need not be so. If I were to write an essay about, say, Swindon as a place it may be very different from that which somebody else would write, because we have experienced Swindon in different ways (see Fyles, 1985). Neither of us is wrong, for we are both describing the place as we experienced it. In historical research, including historical geography, we don't experience places directly, but through the materials left by others – and if, as is almost certainly the case, two or more people have provided materials then the interpretations we have access to are likely to be different. We must assemble and reinterpret those materials. We cannot approach them in a position of theoretical agnosticism, for to do so would be no more than random empiricism. We have a structure into which we think the material fits, and as we fill out that structure so we test its continued coherence in the face of new material; if it fails, we must restructure. In this, of

course, we are acting very much as people do in their daily lives. They have a coherent structure – a model, if you wish – that provides the framework for living, for interpreting new material; at times the framework cannot accommodate new material, and so it must be altered. As humanistic scientists, therefore, we operate as people do daily, assimilating new material into our interpretative framework. The difference is that we then seek to transmit that framework to others – so that they can better appreciate the worlds that they and others inhabit.

There are many similarities between humanistic and realist writing, since both are aiming to increase awareness. There are two major differences, however. The first is that humanistic work remains at the empirical level of events and actors; realist work is interested in these too, but in people acting within the context of mechanisms. The second relates to the purpose of the writing. Humanistic work is aimed only to increase awareness of those events and actors, and is linked to no larger goal – it is typical of what is often termed 'liberal education', broadening the mind. Realist work, on the other hand, is linked to emancipatory goals, aimed at enabling people to appreciate the mechanisms that constrain their lives, so that they might then seek ways of removing them. The latter is clearly a political goal, linked by many to Marxism as a programme for action as well as a theoretical structure for the social sciences. As such, realist writing is viewed with distaste, if not outright disapproval, by many of the gatekeepers, since it promotes change – it doesn't square with either the claimed objectivity of empiricist/positivist science or the studied non-involvement of humanistic science. The latter is seen as harmless, the former as subversive (of the established order within geography, as well as beyond it).

In Summary

This treatment of 'writing geographically' has been very largely concerned with 'publishing geographically'. The two words – writing and publishing – are not synonyms, but since unpublished geographical writings are inaccessible the only sort that can be discussed are those which are published. And getting work published involves conforming to the norms of the publishers. (There are exceptions, of course. Most learned societies elect publishers, who deliver addresses to plenary sessions of their annual conferences. Those addresses are then published, unrefereed and unedited; the 'refereeing' took place at election time.)

In geography at the present time there is no one set of norms to which all conform. There are several different conceptions (or philosophies) of geography, with separate goals that lead to different forms of writing. Conflict between those various conceptions has led to the establishment

of separate publishing outlets, journals specifically launched for a particular type of writing (as, for example, with *Geographical Analysis* and *Autopode*). To the extent that the discipline is divided within itself, so these would seem to be 'natural splits'. But the discipline also seeks to be united (see Johnston, 1985d, 1986), and its learned societies (which act somewhat like trade unions) in particular seek to promote a coherent geography. Their general geographical journals must reflect this, and so accept – reluctantly, and probably slowly – the various types of writing. It sounds like schizophrenial!

One of the problems of the different styles of writing, especially when they are new, is that they use language differently – and maybe invent some, known often (and pejoratively) as jargon. Thus Spate (1960, p. 388), reacting to the presentation of the 'new quantitative geography' wrote of the 'smoke screen of formulae. The *cognoscenti* have their own radar to see through it: the uninitiate may be completely bluffed'. And 23 years later, focusing on humanistic writing, Billings (1983, p. 400) wrote of 'the Mandarin dialect':

nothing corrupts the geographical literature of our time more than fadishness, verbal trickery and the uncritical employment of unnecessary literary conceit. . . . The sonorous phrase is in, clear expression is out. Ostentatious, nonsensical and artificial by turns, the new form is increasingly *de-rigour*. It has balance and élan, passion and commitment, it has imagery, metaphor, simile and hyperbole, flamboyance and energy – in fact everything save honesty of intention and meaning.

People write in the way they believe that their audience wants them to, and assume that if they pass the gatekeepers then they have got it right. Some write more than others, perhaps because they have a greater dread of perishing: or perhaps they publish more because they simply enjoy it, have some deep need for public recognition, or just know how to please the gatekeepers. To understand fully the contents of the geographical literature we would have to appreciate the writers as well as their writings. As yet, neither autobiographies nor biographies have given us much insight into geographers, and our concern is only with their geography.

It is becoming commonplace these days for writers (not least me) to paraphrase Marx's comment about people making their own history but not in conditions of their own choosing. But it is a very insightful statement, which certainly applies to geographical writing. Academic geographers are socialized into a way of life which sees the conduct of research and the publication of its findings as the main performance indicator. Within that general way of life they are socialized into disciplinary and interdisciplinary frameworks that identify both 'good' research and 'good' writing. In order to succeed within those frameworks they must

accept their norms. Over time the norms may change, and they may have some part to play in bringing about the change. But for most geographers, most of the time, the process of writing is a necessary activity which must be undertaken within the generally accepted rules. As Dixon (1976, p. 39) expressed it (in part quoting Cottrell)

The scientific genius may have his [sic] flashes of inspiration while in solitude, but if he is to be an effective scientist he must be very much part of the community of science. . . . 'He has to take the ideas and problems as they exist among his fellows, transmit them in his own personal way, and then bring them back as offerings to his community. He both takes and gives, in the scientific currency of his time.'

Notes

1. My contract as a Professor of Geography at the University of Sheffield makes me 'responsible for the prosecution and encouragement of original studies in the subject of. . . [my] Chair'.
2. Hence the copious provision of references and footnotes.
3. There is a problem if researchers cannot get their work published in the recognized outlets of their discipline, for reasons discussed in detail below. They may initiate new outlets that will accept their work. Initially, however, they are likely to circulate reports to interested persons in mimeographed form (often known as 'fugitive' or 'underground' publications), so that 'publication' is restricted to those who are members of a network, or linked to it. In addition some authors circulate early drafts of their research reports in similar formats, and many institutions have formalized this through the publication of Departmental Discussion/Seminar Paper series. Some reports circulated in this way never enter the 'formal' literature!
4. Promotions committees in the University of Sheffield, especially that of the Faculty of Pure Science, have been very reluctant to consider the publication of textbooks as an index of research activity.

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