Letters

Authorship

See editorial by Richard Smith

Changing authorship system might be counterproductive

EDITOR—The problem of authorship in science[1] should be set in the wider context of debates that have raged in literary criticism since the early 1970s. In the work of Derrida most notably, the traditional concept of authorship with its implications of individualism and authority over the interpretation of textual meaning has been overthrown in theory, if not entirely in practice.[2] Authorship is a political problem; it involves staking and maintaining territorial rights, colonisation, and empire building. In this it fairly accurately reflects the power game that is involved in the conduct of science itself. The sociology of scientific knowledge shows the actual practice of science to be remote from the received image of selfless dedication to the pursuit of disinterested knowledge.[3 4] It is, rather, a highly politicised sector of the economy in which the first priority is to obtain the funds necessary to establishing and maintaining those research programmes on which so many scientists' careers, at all levels of seniority, depend.

This is not a criticism, merely an observation. Hence I think it would be a mistake to conclude that the authorship system has broken down and needs radical revision.[5] If the present system reflects the structural and dynamic power relations that constitute scientific communities, and if these relations are the inevitable basis of the institutionalisation and conduct of science, then the only reasonable justification for change would be to represent accurately a radically revised basis of science itself. As far as I can tell, this revision is impracticable because power relations will always be the essence of the generation of scientific knowledge. A depoliticised authorship system implies a depoliticised science, which implies its total detachment from the state, industry, and society. Even if such a science were possible it would lose what tenuous significance it has for the world outside its own institutions.

Would it not be better, therefore, to leave the present authorship system to continue its present slow evolution in so far as it accurately reflects the real power relations in science? What is really required, I suggest, is to educate scientific and wider communities into the art of reading scientific literature from a different perspective which more accurately reflects the power paradigm of its production. I do not think that by manipulating the authorship system one could effect any important change in the conduct of science itself. On the contrary: the most likely outcome would be to add another layer of obscurity to conceal its essentially political nature.

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1 Bhopal R, Rankin J, McColl E, Thomas L, Kaner E, Stacy R, et al. The vexed question of authorship: views of researchers in a British medical faculty. *BMJ* 1997;314: 1009-12. (5 April.)
2 Derrida J. *Of grammatology.* Baltimore: Johns Hopkins University Press, 1976. (Trans Gayatri Chakravorti Spivak.)
3 Mulkay M. *Sociology of science.* Buckingham: Open University Press, 1991.
4 Knorr-Cetina KD. *The manufacture of knowledge.* Oxford: Pergamon Press, 1981.
5 Smith R. Authorship: time for a paradigm shift? *BMJ* 1997;314:992. (5 April.)

System acknowledging roles of contributors is best

EDITOR—Richard Smith has invited comment on the issue of authorship.[1] I think it would be an excellent idea to scrap the concept of authorship as we now know it and to opt for a system in which the role of various contributors is acknowledged. I do not believe that the issue of ultimate responsibility is a major problem. Indeed, the descriptive system would facilitate the identification of the person ultimately responsible. In the great majority of my nearly 200 papers in refereed journals I have acted as initiator, raiser of funds, supervisor, more often than not writer, and certainly the person accepting ultimate responsibility. For more than 20 years, however, I have had the responsibility of supervising research students, who have invariably been the first author. I frequently encounter senior colleagues who have not been aware of my involvement with such work. Our studies relating to the health consequences of vegetarianism and diet and diabetes are particularly good examples. I would regard these two issues as being among my main contributions during my entire research career, yet my name has appeared second or last on the papers. There has been no clear distinction between my role and that of, say, the statistician or dietitian, whose role may have been important but certainly was not instrumental in the study.

I appreciate that such a fundamental change would undermine citation indexes, but perhaps these would then be replaced by more appropriate systems of academic credit. From an immediate practical point of view it would simply be necessary to obtain agreement relating to descriptors, which I hope would go beyond "contributors" and "guarantors" as suggested in Smith's editorial.

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1 Smith R. Authorship: time for a paradigm shift? *BMJ* 1997;314:992. (5 April.)

Question of authorship concerns everyone in training grades

EDITOR—While many benefits make up the carrot of having work published, the sticks held up before potential authors should be public accountability for "their" words and the availability of accurately produced data.[1] Recent episodes have highlighted the seriousness of failure to uphold these standards.[2] Perhaps the number of authors on a work should be limited, although this would bring its own problems. Certainly some journals' practice of listing only the first three authors in the references goes some way towards this. Another alternative, which

Advice to authors

We receive more letters than we can publish: we can currently accept only about one third. We prefer short letters that relate to articles published within the past four weeks. We also publish some "out of the blue" letters, which usually relate to matters of public policy.

When deciding which letters to publish we favour originality, assertions supported by data or by citation, and a clear prose style. Letters should have fewer than 400 words (please give a word count) and no more than five references (including one to the BMJ article to which they relate); references should be in the Vancouver style. We welcome pictures.

Letters, whether typed or sent by email, should give each author's current appointment and full address. Letters sent by email should give a telephone and fax number when possible. We encourage you to declare any conflict of interest. Please send a stamped addressed envelope if you would like to know whether your letter has been accepted or rejected.

We may post some letters submitted to us on the world wide web before we decide on publication in the paper version. We will assume that correspondents consent to this unless they specifically say no.

Letters will be edited and may be shortened.

was not particularly favoured in Raj Bhopal and colleagues' study,[3] is the limiting of citations on one's curriculum vitae, yet this would probably have attractions in a different group, such as trainees. For many or perhaps all of us in the training grades, appearing in the literature and thus authorship is a game we have to play to get on. There are no other ways of distinguishing between equally well qualified people on paper, and personal recommendation is now perceived as politically incorrect. The "vexed question of authorship" is of concern to all of us in the training grades, but one question remains: was having nine authors for this study[3] deliberate?

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1 Smith R. Authorship: time for a paradigm shift? *BMJ* 1997;314:992. (5 April.)
2 Dyer O. Consultant struck off for fraudulent claims. *BMJ* 1995;310:1554.
3 Bhopal R, Rankin J, McColl E, Thomas L, Kaner E, Stacy R, et al. The vexed question of authorship: views of researchers in a British medical faculty. *BMJ* 1997;314: 1009-12. (5 April.)

Coworkers should be named

EDITOR—Medical and scientific research papers are always the fruit of collaboration of many workers at different levels, and even when eight or more authors are named, many remain unsung. Before publication some papers have been revised by anonymous referees or partly rewritten by medical editors for clarity, relevance, logic, and conciseness. Librarians, pharmacists, statisticians, etc may have given helpful advice. The work of data collection may have occupied nurses, interviewers, technicians in special departments (radiology, biochemistry, etc)—work for which they were routinely employed or did as an extra. Backing it will have been finance officers, secretaries, porters. In cinema and television the credits roll to name not only the creative artists, the sound and lighting technicians, and the set carpenters but even the location caterers, the accountant, and the insurers. Why not do the same in journals?[1]

Some team members want authorship for five minutes' personal glory, or to further their careers, because bureaucrats find "authorship counting" an easy assessment of individuals and departments. But readers (and publishers) do not want a space-occupying meaningless list of names—they want to know who chiefly is responsible for the planning, coordination, analysis, and presentation of the work; who is leader of the group or department; and what part of the work other specialists (named) have contributed. These are separate matters.

Authorship should be strictly limited to a maximum of four people, who take responsibility for everything said and done—that is, they are satisfied with the validity of the data provided by their specialist coworkers. In the abstract of the paper, after the conclusions, there should be an additional section, headed "Coworkers." In this section will appear the coworkers' names and specialisms and what they contributed to the project, and the journal's editors will decide where to draw the line in accepting some or all for publication. The head of the research group can be named in parentheses after the name of the clinic from which the work comes (as has been the practice in German journals), or in the coworkers section. People who do extra work of the sort for which they are already employed might get monetary reward or the equivalent of Air Miles for their pains, but that is not a matter for journals.

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1 Smith R. Authorship: time for a paradigm shift? *BMJ* 1997;314:992. (5 April.)

Work done by junior researchers gives rise to problems

EDITOR—We think that Richard Smith's ideas for crediting work done and for having guarantors of integrity fit nicely with the way that we do things, because our work is necessarily done by a team.[1] Each member brings different talents and skills.

It is the recognition of the necessary spadework (often done by junior researchers learning their trade) that vexes us most. The "diggers" are often in a position neither to give a formal presentation nor to conceive or design a study, because of lack of experience. Despite making an enormous contribution they might fall short of the criteria for authorship if the Vancouver guidelines were followed to the letter. To disenfranchise them would be unfair; credits would get round this problem.

We now hold a "public" read through of the final versions of papers leaving this unit, in the presence of all involved. This ensures not only that everyone involved approves the final version but also (we hope) a more readable and understandable paper. We also believe that it helps us to comply with the spirit of the Vancouver guidelines.

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Bhopal and colleagues' suggested method of ordering authors wouldn't work

EDITOR—Although admirable in intent, R S Bhopal and colleagues' suggestion that the order of authors on research papers should be decided by aggregating individual preferences is unworkable in practice.[1]

Consider five authors—*a*, *b*, *c*, *d*, and *e*—who respectively think that the order of their colleagues' names should be (*b*, *c*, *e*, *d*); (*d*, *e*, *c*, *a*); (*a*, *b*, *d*, *e*); (*e*, *a*, *b*, *c*); and (*b*, *d*, *c*, *a*). Following the suggested method—in which first place is awarded four points, second place three points, and so on—yields: first *b* (13 points), then *d* and *e* (both with 10 points), then *a* (9 points), and finally *c* (8 points), so that the "democratic" ordering would be *b*, *d*=*e*, *a*, *c*.

Even if the problem of what to do about the equal scores of *d* and *e* is ignored, the result is problematic because, according to the individual preferences, majorities exist for *a* being before *b* (both *c* and *d* are in favour of this, and only *e* favours the reverse) and *c* being before *a* (this is favoured by both *b* and *e* whereas *d* is against it); neither of these preferences is reflected in the group ordering. As the voting scheme gives no indication of the intensity of the authors' opinions about their choice of ranking, the group ordering is little more than arbitrary.

This is not a special case—political economists have been troubled by such "voting paradoxes" for over 200 years.[2] The economist Kenneth Arrow won the 1972 Nobel prize in economics largely for proving that, in general, there is no fair and logical way of aggregating individual preferences into a single, collective ordering.[3] This makes any sort of voting scheme for deciding sequences of authors impracticable. Fortunately, no such difficulties were encountered in deciding the order of authorship for this letter.

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2 Black D. *The theory of committees and elections.* Cambridge: Cambridge University Press, 1958.
3 Arrow KJ. *Social choice and individual values.* 2nd ed. London: Yale University, 1963.

New authorship practices are needed in developing countries

EDITOR—Richard Smith expresses his dissatisfaction with the authorship paradigm and supports a radical solution: abolish the concept of authorship and replace it by a descriptive system (like film credits).[1] A study by Raj Bhopal and colleagues showed that, among researchers in a British medical faculty, guidelines on authorship were inadequate and often not adhered to.[2]

Hitherto, the debate has focused on the publication of research from developed countries. The issue, however, is of equal interest to the developing world. After reading Bhopal and colleagues' paper, I solicited views about the rules concerning authorship from 10 Tanzanian researchers at the National Institute for Medical Research in Mwanza. As in Britain,[2] a range of practical and intellectual contributions were considered grounds for authorship. Eight respondents considered gift authorship to be a problem; four thought it was common, and the others had no idea how common it was. Seven researchers said that gift authorship should be banned. Problems with or conflicts about authorship were commonly reported, such as exclusion from the list of authors when authorship was thought to be deserved (6) and inclusion when it was not merited (3). Eight researchers liked the idea of a film credit system, mostly because of its

transparency and frankness. The two others did not like it because they thought that it was too cumbersome and would not support existing reference systems and curriculum vitae.

Current systems of authorship in large studies in developing countries vary. Gift authorship or inappropriate ranking of authors seems to be common and to be part of development aid in some projects. The researchers themselves, however, prefer to be acknowledged for what they have actually done rather than be part of a politically correct process of allocating authorship.

I agree with Smith that it is time for a paradigm shift. In the context of developing countries the current methods of allocating authorship are obscure and paternalistic. Even though the name of the researcher in the host country ranks high on the list of authors or appears in many publications, he or she may not be taken seriously, simply because senior international researchers are also listed or because the researcher is one of several fellow researchers listed from that country who barely contributed to the study. Current authorship practices may contribute to further underdevelopment of research and researchers in developing countries and fail to acknowledge the researchers appropriately for their actual and essential contributions. As a first step, all articles should have a footnote specifying the contributions of every author.

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1 Smith R. Authorship: time for a paradigm shift? *BMJ* 1997;314:992. (5 April.)
2 Bhopal R, Rankin J, McColl E, Thomas L, Kaner E, Stacy R, et al. The vexed question of authorship: views of researchers in a British medical faculty. *BMJ* 1997;314: 1009-12. (5 April.)

Authorship is influenced by power and departmental politics

EDITOR—As an undergraduate, I researched and analysed 33 000 postmortem records in a university institute. For the study I won a university award. The institute wanted to use and publish my findings without my name, arguing that the original materials were the property of the institute and that it paid for my work by giving a cheque with the award. I was so incensed that I refused to take the award (and the money) and wrote to the rector of the university, forbidding it to use my research findings. The reply was swift. The university too forbade me to publish my findings because the original material belonged to a university institute which did not consent to it. Seventeen years on, the study has still not been published.

In clinical research, data derived from patients belong to consultant X or Professor Y, on the grounds that the doctor is in overall charge of the patients' treatment. The custom of this "possession" is so ingrained that nobody dares question it. Thus it is not possible to use clinical data without the explicit consent of the consultant in charge of the patient. Requesting this usually brings a request from the consultant to see the draft of the paper. Because he or she usually makes some suggestions one can expect the explicit or implicit request for the consultant to be listed as a coauthor. Junior staff, who are in a vulnerable position because they depend on their seniors, will willingly or grudgingly offer them at least coauthorship before they formally request it. Very few consultants will turn down such an offer, saying that they did not participate enough to deserve it. How many of us are brave enough to tell our consultant, "Your participation in this paper does not fulfil the Vancouver criteria, but would you be kind enough to support my application for that post?" I know a consultant who encouraged his juniors to write separate papers putting each other's names on them, and of course his—all in the spirit of the Vancouver recommendations.

A further problem in Britain is that juniors in clinical medicine stay in one place for only a short time. By the time all the data have been collected, analysed, and written up to be published they have probably already moved on. Their contribution is hardly mentioned, if mentioned at all.

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Fierce disputes about order of authors sometimes occur in China

EDITOR—I agree with Richard Smith that the issue of authorship of biomedical papers needs reappraising.[1] In China only the first author is eligible for promotion, no matter what the nature of his or her position (academic, professional, or technical). So it is not surprising that many unpleasant (sometimes fierce) disputes regarding the order of the authors arise between senior and junior doctors and between doctors and technical assistants. I agree with Smith that the concept of "contributorship" is a good solution because it can reflect the concrete contribution of every participant. Accordingly, the "designer" of the paper or the "coordinator" of the "contributor team" should be indicated, to show who will be responsible for the scientific integrity of the whole paper.

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1 Smith R. Authorship: time for a paradigm shift? *BMJ* 1997;314:992. (5 April.)

Knowing who did what in studies is important

EDITOR—I am glad to see the topic of authorship being discussed.[1] In my work with trainees in public health medicine and on continuing medical education for public health doctors in New Zealand, publications are important in the presentation of projects and in providing points for recertification. It may be difficult to establish what role a person had.

I look forward to seeing the development of a practical and realistic way of recognising and authenticating the work that is done. Its acceptance will require a shift in employers' and academic attitudes to the tasks involved in research and publication.

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Without a putative contributor, would the integrity of the work change?

EDITOR—Richard Smith highlights the difference between novelists and scientific writers and examines the problems of crediting authorship to scientific writers.[1] The concept of authorship is, I think, satisfactory. Three elements merge inseparably in the formulation of a novel: inspiration, or ideas, which are developed into characters and a plot; research; and writing. Although scientific papers are quite different, the Vancouver criteria for authorship are comparable: conception and design of a study (inspiration) or analysis and interpretation of data (research) and drafting or revising critically (writing). The problem is not the concept of authorship but that multiple "authors" may be involved in producing scientific papers and may not meet all of the criteria.

The solution is not to redefine authorship but to recognise in some other way important contributions by those who do not qualify as authors. The concept of contributorship—analogous to film credits—has the advantage of defining relative roles in collaborative, including multicentre, ventures. It would, for example, acknowledge the efforts of those whose contribution to research has been important yet whose diluted involvement or non-involvement in the conception of the study or writing the paper has precluded them from qualifying as authors. It may allow more eloquent dissection of contributions to a paper and clarify accountability. It should not, I think, necessarily replace authorship. Many genuine authors remain.

Whether authorship or contributorship is used, the struggle for inclusion remains: almost everyone would like the "glory." Writing is never completely original. Ideas are drawn from experience, from endless encounters. Research examines the work of others. Even the process of writing evolves through various admired styles. No achievement is entirely one's own, and potential acknowledgements are limitless.

How far, then, should we go in acknowledging putative contributors? Clearly, this is not easy. We could be too dogmatic in our limits or too nebulous. It would be reassuring to think that common sense would prevail, but there is no definition of common sense. A reasonable way to decide whether a contribution is important could be to consider whether, without the putative contributor, the integrity of the work would essentially change. Potential non-

contributors may become obvious and may include departmental heads, nepotists, and inquisitive colleagues.

There is, I suspect, a place for authorship and contributorship. There is doubtless a place for non-contributorship, and that is in readership.

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1 Smith R. Authorship: time for a paradigm shift? *BMJ* 1997;314:992. (5 April.)

Assessment of authorship depends on culture

EDITOR—Strict adherence to objectivity and high ethical standards are defining virtues of science. Though scientists share a common language and culture of science, they are also influenced by values and norms in their societies.[1] Our perception that there were more Japanese than non-Japanese authors listed on research articles prompted us to investigate how culture might influence authorship criteria. We identified two qualitatively similar medical journals (criteria available on request): *Circulation Research*, which is published in the United States, and the *Japanese Circulation Journal*. We compared the number of Japanese and non-Japanese authors per article in the 1983, 1993, and 1996 issues of these journals. In each year there were 2-3 more Japanese authors per original article in the *Japanese Circulation Journal* than in *Circulation Research* (table). These trends were similar for other contributions too: there were 296 authors on 45 case reports in the *Japanese Circulation Journal* (mean 6.6 (range 2-12) authors/report) and 225 authors on 59 communications in *Circulation Research* (3.8 (1-11)).

Though these data are from a limited sample, they suggest intercultural variations in crediting authorship. The Japanese penchant for "groupism"[2] and limited individual funding probably lead them to involve more people in research endeavours. Research groups in Japan possess a cohesive sense of unity and mutual reliance on the group and senior leader,[3] who typically is included on every paper from the laboratory. A professor who orchestrates funding and consensus building qualifies as an author because these contributions are critical for conducting[4] and publishing research. According to the requirements of the International Committee of Medical Journal Editors, such contributions qualify simply as an acknowledgement,[5] but such discrimination would be likely to disrupt group harmony. Firstly, greater value seems to be placed on the act of contributing than on the value of the contribution. Secondly, scrutinising the value of contributions may strain relationships and prove counterproductive to cohesiveness in the laboratory.

These data and analysis imply that publication of research manifests the interface between the scientific method and the culture of contributing investigators. Scientific investigation is neither devoid of its own cultural milieu nor immune to the influence of the values and beliefs of investigators using scientific methods. While the criteria of the International Committee of Medical Journal Editors do not accommodate cultural variation in crediting authorship,[5] they reflect the cultural background of the majority of the members and not codification of undisputed scientific principles. The movement to credit only those who deserve authorship is noble, though the assessment of legitimate authorship is a cultural, not a scientific, judgment.

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2 Befu H. The group model of Japanese society and an alternative. *Rice University Studies* 1980;66:169-87.
3 Nakane C. *Japanese society.* Berkeley: University of California Press, 1970.
4 Fetters MD. Nemawashi essential for conducting research in Japan. *Soc Sci Med* 1995;41:375-81.
5 International Committee of Medical Journal Editors. Uniform requirements for manuscripts submitted to biomedical journals. *JAMA* 1997;277:927-34.

Comparison of number of authors per original article by Japanese and non-Japanese groups of authors in two similar medical journals

	Circulation Research		*Japanese Circulation Journal*	
	Japanese	**Non-Japanese**	**Japanese**	**Non-Japanese**
1983:				
No of authors	24	423	446	0
No of articles	7	115	69	0
Mean No of authors/article (range)	3.4 (2-5)	3.7 (1-10)	6.5 (1-15)	0
1993:				
No of authors	138	702	632	20
No of articles	23	163	104	4
Mean No of authors/article (range)	6.0 (2-11)	4.3 (1-13)	6.1 (1-14)	5.0 (2-9)
1996:				
No of authors	116	849	512	19
No of articles	18	173	73	3
Mean No of authors/article (range)	6.4 (3-11)	4.9 (1-13)	7.0 (3-16)	6.3 (4-10)

Researchers' objective is to get the job done

EDITOR—Perhaps one explanation for the differences in how authorship is perceived by editors and researchers is these groups' different objectives. Editors, encouraged by a frisson of puritan zeal, wish to allocate responsibility for published work. Researchers, on the other hand, ultimately want to get the job done and push back frontiers. As Richard Smith indicates, modern research needs large multidisciplinary teams, and authorship, like the honours system, is a cost free way of getting many busy people to participate.[1]

In clinical areas the person with the drive and ideas may not have the patients or the necessary skills in molecular biology, radiology, pathology, etc; and vice versa. Students of human relationships will recognise immediately their day to day contrariness; offering authorship slices through these difficulties and permits better and more innovative research.

Surely editors should agree with the researchers' objective, which is to get the job done; all else, though important, is secondary.

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Author saw fraud, misconduct, and unfairness to more junior staff

EDITOR—As a junior researcher who graduated six years ago, I find the issue of authorship the most frustrating of all the problems within the medical research community.[1] It is frustrating because it is remediable, and the main culprits are often experienced researchers. Two of the most interesting books that I have read are *Follies and Fallacies in Medicine* and *Fraud and Misconduct in Medical Research*.[2 3] Both of these books document cases of unacceptable practice.

During my career I have personally experienced events that constituted fraud, misconduct, or simple unfairness to more junior staff. These were notably:

- Plagiarism: I have witnessed a senior person submit a dissertation for a higher degree in which an appreciable part of the introduction had been copied verbatim from a published paper
- Important data were manufactured on two occasions
- Junior staff were omitted from published work
- A junior researcher conceived a study and wrote the first draft and much of the modification to the proposal, but was excluded from the final submission, which received funding of £100 000. This exclusion was considered to improve the chance of funding
- Token authorship
- Token supervision of higher degrees
- Destruction of computerised data when a funding body decided to scrutinise the work more closely

• Multiple applications for funding of essentially the same proposed work.

Of the solutions to the problem of gift authorship that Richard Smith offered, the radical solution of a list of "credits" appeals most.[4] Editors should require authors to describe their input and demand justification if there is any doubt about someone's inclusion. I would go further and support the idea of an agency to police research.[5] Research and development of drugs undergoes this process because of the potential consequences of poor or bad research practice in the pharmaceutical industry and the large financial incentives involved. The difference between research and development of drugs and other clinical research may be a medicolegal one in that drugs can be fairly easily identified as a root problem whereas a deceptive paper that has influenced a doctor's clinical judgment is more difficult to identify.

The vast majority of people whom I have encountered and practice that I have experienced have been perfectly acceptable. The remainder should be weeded out.

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1 Bhopal R, Rankin J, McColl E, Thomas L, Kaner E, Stacy R, et al. The vexed question of authorship: views of researchers in a British medical faculty. *BMJ* 1997;314: 1009-12. (5 April.)
2 Skrabanek P, McCormack J. *Follies and fallacies in medicine.* 2nd ed. Chippenham: Tarragon, 1992.
3 Lock S, Wells F. *Fraud and misconduct in medical research.* London: BMJ Publishing, 1993.
4 Smith R. Authorship: time for a paradigm shift? *BMJ* 1997;314:992. (5 April.)
5 Klein DF. Should the government assure scientific integrity? *Acad Med* 1993;68:S56-9.

Excluding authors may be impossible

EDITOR—I am glad that the definition of authorship is finally being addressed.[1 2] The publication of original scientific articles may be a key to some recognition, but whether it necessarily leads to academic success is far from clear.

As a trainee, I was able to do a few pieces of original research, most of which got published in professional journals, including the *BMJ*. During these years of training I came across some people who never hesitated to give help and support for any sort of research and some who had hardly been involved in the research but demanded to be listed as an author. I am sure that my experience was not unique. I remember a particular instance, when the editor of the *BMJ* had a major criticism—that there were too many authors for a paper of the size that this paper was. On further analysis, according to the *BMJ*'s recommended criteria, one could exclude four coauthors, but was it possible? No, because any such attempt could possibly lead to a misunderstanding and cracks in relationships. Instead, a path of least resistance was taken and the article was published in another, less esteemed, journal.

This reflects one side of the coin. The other side is when one gets intimately involved with all the practicalities of a piece of research but does not even get an acknowledgement in the paper, let alone inclusion in the list of authors. This experience again is perhaps not unique to me.

It is important to have a clear policy on who should be included as an author, even before the start of the research. Researchers and editors need to develop a unanimously acceptable strategy for authorship. It would be appreciated by most researchers around the world.

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1 Smith R. Authorship: time for a paradigm shift? *BMJ* 1997;314:992. (5 April.)
2 Bhopal R, Rankin J, McColl E, Thomas L, Kaner E, Stacy R, et al. The vexed question of authorship: views of researchers in a British medical faculty. *BMJ* 1997;314: 1009-12. (5 April.)

Number of publications given on curricula vitae should be limited

EDITOR—Authorship will continue to be a problem[1-3] while the curricula vitae of both senior and junior doctors are judged on the number of publications rather than the quality of a limited number of them. Only once in my career have I been asked to limit the number of publications on an application form, and even then it was to too high a number.

I suggest that applicants for posts for preregistration house officers, senior house officers, and specialist registrars should be asked to choose their best two publications for inclusion in their curriculum vitae and to be prepared to discuss them at the interview. This would also allow interview panels to have copies of the publications available at the interview, so long as this was not at the expense of the candidate. Applicants for consultant posts should be asked to choose their best five publications and be prepared to discuss each paper, their personal contribution to it, and the impact that the paper has or will have on their specialty.

For their annual assessments, specialist registrars should be asked to discuss one of two publications that they have submitted in the previous year. For consultants, discretionary points, merit awards, and continuing medical education points should be related more to quality and relevance than to number of publications.

Multicentre trials give only a few people "authorship" but depend on the participation of many and lead to important steps forward in treatment of a wide range of conditions. Participation in such trials should be recognised.

Authorship given to a member of the team for participating in only one aspect of a project (for example, collecting data or retrieving and reviewing case notes) has some advantages, in that it can be used as an incentive to get work done quickly and finish off a project started by others. This, in some cases, is useful as it allows publication of an important finding, which may otherwise be delayed or not submitted at all. Publication counting is rife and ingrained into medical thinking; thus any changes would take a long time to filter through to medical practice.

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1 Smith R. Authorship: time for a paradigm shift? *BMJ* 1997;314:992. (5 April.)
2 Bhopal R, Rankin J, McColl E, Thomas L, Kaner E, Stacy R, et al. The vexed question of authorship: views of researchers in a British medical faculty. *BMJ* 1997;314: 1009-12. (5 April.)
3 Authorship [letters]. *BMJ* 1997;314:1046-7. (5 April.)

General practice fundholding and health care costs

Fundholding has curbed increases in prescribing costs

EDITOR—I was surprised to see such an ill considered editorial as Duncan Keeley's on general practice fundholding and healthcare costs.[1] In the same issue, Thérèse Rafferty and colleagues' study of fundholders' prescribing patterns in Northern Ireland identified some interesting facts.[2] Although prescribing costs and frequency of prescribing increased for both fundholders and non-fundholders, the rate of increase in costs for fundholders was significantly lower than that for non-fundholders.

In Northern Ireland, fundholders' prescribing budgets are set roughly on the previous year's actual expenditure—that is, fundholders are immediately penalised for efforts to reduce prescribing costs. This arrangement pertained for one year in England and was subsequently reversed because it was seen as demotivating. As early as 1993, Bradlow and Coulter identified that fundholding had curbed increases in prescribing costs, including those of dispensing general practitioners, for whom the incentives are different.[3] Indicative prescribing budgets for non-fundholders did not have the same effect.

Interestingly, commissioning general practitioners are now expressing great reluctance to accept responsibility for cash limited prescribing budgets, unlike their fundholding colleagues. This raises doubts about their acceptance of what, ultimately, is inevitable—cash limited budgets in general practice.

Concern has been expressed that fundholding would lead to a shift away from specialist care, because the fixed budget scheme meant that, where referrals were not made, fundholders would save money. Because of this concern, Surrender et al carried out a further study.[4] This indicated that although fundholders' use of specialist consultations had increased by 7.5%, the referral rates of non-fundholders had increased by 26.6%. So while the trend to refer to specialists is increasing, the increase among fundholders is considerably lower. Also, the rate of fundholders' referrals to private clinics decreased by 8.8% during the study, while non-fundholders' referrals increased by 12.2%.

Although emergency admissions are outside the fundholding scheme, Keeley and others suspect that fundholders have a vested interest in admitting patients to hospitals as emergencies, to save on their budgets. In a study for the London School of Tropical Medicine, Nigel Edwards (a health economist) has been investigating in depth the reasons for the rise in emergency admissions. Despite extensive efforts to identify differences in the rate of increase for patients of fundholders and non-fundholders, he was unable to do so.

It is necessary for intelligent professionals, as well as the public, to treat with caution the words of men with bees in their bonnets.

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1 Keeley D. General practice fundholding and health care costs. *BMJ* 1997;315:139. (19 July.)
2 Rafferty T, Wilson-Davis K, McGavock H. How has fundholding in Northern Ireland affected prescribing patterns? A longitudinal study. *BMJ* 1997;315:166-70. (19 July.)
3 Bradlow J, Coulter A. Effect of fundholding and indicative prescribing schemes on general practitioners' prescribing costs. *BMJ* 1993;307:1186-9.
4 Surender R, Bradlow J, Coulter A, Doll H, Stewart Brown S. Prospective study of trends in referral patterns in fundholding and non-fundholding practices in the Oxford region, 1990-4. *BMJ* 1995;311:1205-8.

Fundholding seems not to be implicated in rise in emergency admissions

EDITOR—In his critical editorial on the effects of general practice fundholding, Duncan Keeley stated that there is no evidence on whether fundholding and non-fundholding practices differ in their contribution to the steady rise in emergency admissions to hospital.[1] He implies that since emergency admissions are not a charge against fundholders' budgets, fundholders may have less of an incentive than non-fundholders to seek to reduce emergency admissions. This was an early hypothetical concern raised against fundholding.[2]

Fortunately, at least one recent study contains pertinent evidence. Toth et al tested the hypothesis that general practice fundholding was associated with a change in the proportion of emergency admissions to hospital. They compared fundholding and non-fundholding practices over the first two years of the scheme in the South Western region, looking at causes of admission where emergency admissions might be substituted for elective admissions.[3] They found no evidence that fundholding had had any impact on the proportion of emergency admissions for the causes of admission studied. Thus, at least as far as one region was concerned, it seems that fundholding cannot be implicated in the worrying rise in emergency admissions.

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1 Keeley D. General practice fundholding and health care costs. *BMJ* 1997;315:139. (19 July.)
2 Bevan G, Holland W, Mays N. Working for which patients and at what cost? *Lancet* 1989;i:947-9.
3 Toth B, Harvey I, Peters T. Did the introduction of general practice fundholding change patterns of emergency admission to hospital? *J Health Services Res Policy* 1997;2:71-4.

Fundholding gives choice of alternatives if local service is poor

EDITOR—Duncan Keeley's editorial condemning fundholding cannot go unchallenged.[1] Keeley implies that savings in prescribing costs are not sustained. Yet, in the same issue, research shows real and sustained cost savings with dramatic increases in generic prescribing achieved by fundholding practices.[2] Keeley also suggests that low cost prescribers may be poor prescribers. I know of no published evidence to support this idea, and most of my anecdotal evidence suggests the opposite to be true.

Keeley suggests that fundholding may be responsible for the rise in emergency admissions and again does not supply any evidence to support this. Some fundholders have undoubtedly abused emergency access to minimise their own costs in individual cases, but this should not allow critics to dismiss the benefits of fundholding. Extension of schemes to total purchasing should remove any incentive to abuse emergency admissions and might encourage general practitioners to refer "emergencies" more selectively.

Keeley cites a lack of differential referral rates as showing lack of efficacy for fundholding. However, general practitioners did not enter fundholding to refer fewer patients to secondary care. The attraction of fundholding is that it gives general practitioners and NHS patients the chance to use alternatives if the service provided by the local district general hospital is poor. Because the choice exists, many hospitals have responded by dramatically improving efficiency.

I will counter Keeley's opinions with two realities. Walk into any fundholding practice and you will find real, concrete improvements in services offered to patients, whose general practitioners are motivated and have intimate knowledge about the quality of local services. Secondly, in my own area, fundholding practices have set up two "one stop" clinics, which have been of benefit to all local patients.

Some commissioning groups have improved local services, but many general practitioners will find such groups too large and cumbersome. What alternative incentives will the government provide to encourage general practitioners to prescribe and refer thoughtfully?

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1 Keeley D. General practice fundholding and health care costs. *BMJ* 1997;315:139. (19 July.)
2 Rafferty T, Wilson-Davis K, McGavock H. How has fundholding in Northern Ireland affected prescribing patterns? A longitudinal study. *BMJ* 1997;315:166-70. (19 July.)

Author's reply

EDITOR—Maggie Marum accuses me of writing an "ill considered editorial" but is highly selective in the evidence she cites. In the study in Northern Ireland the prescribing costs of first wave fundholders were increasing at the same rate as those of non-fundholders within three years of them entering the scheme.[1] This is confirmed by the studies in the Oxford region, in which early containment of prescribing costs by fundholders[2] was not found to be sustained in a follow up study three years after the scheme's inception.[3] In the Oxford region's study of referrals there was a surprisingly high increase in the referral rate of the control non-fundholding practices.[4] But most of these control practices were preparing for fundholding, and there was evidence in three practices of a significant increase in referrals in the preparatory year: this could represent a deliberate increase in referral rate before fundholding or, more probably, be an artefact of improved data collection.

Nicholas Mays cites the published study from the South Western region which failed to find evidence of a difference in rates of emergency admission between fundholding and non-fundholding practices.[5] The problem is that both types of practice may have reasons to refer increasing numbers of patients as emergencies. R V Millard accepts that some fundholders may have abused emergency access to reduce costs to their funds.

I agree with Millard's anecdotal impression that practices with low prescribing costs and high rates of generic prescribing—such as the one I work in—may also be prescribing "well" in terms of patient outcomes. But anecdotal impressions may be unreliable, and good evidence on this is, as yet, hard to find. For referrals, a choice of alternative hospital provider within the NHS existed before the reforms. The wisdom, fairness, and cost effectiveness of using NHS funds to allow a subgroup of the population to access private sector surgery needs to be openly debated.

It is possible to see the fact that NHS hospitals are having to do more work for less money as evidence of "dramatically improving efficiency," but general practitioners generally—and rightly—take a different view if asked to do this themselves. With lengthening waiting lists and a continuing financial crisis in the acute hospital sector, those who wish to defend the continuation of fundholding must find better evidence to justify the large sums of public money being spent to maintain it.

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1 Rafferty T, Wilson-Davis K, McGavock H. How has fundholding in Northern Ireland affected prescribing patterns? A longitudinal study. *BMJ* 1997;315:166-70. (19 July.)
2 Bradlow J, Coulter A. Effect of fundholding and indicative prescribing schemes on general practitioners' prescribing costs. *BMJ* 1993;307:1186-9.
3 Stewart-Brown S, Surender R, Bradlow J, Coulter A, Doll H. The effects of fundholding in general practice on prescribing habits three years after the introduction of the scheme. *BMJ* 1995;311:1543-7.
4 Surender R, Bradlow J, Coulter A, Doll H, Stewart Brown S. Prospective study of trends in referral patterns in fundholding and non-fundholding practices in the Oxford region, 1990-4. *BMJ* 1995;311:1205-8.
5 Toth B, Harvey I, Peters T. Did the introduction of general practice fundholding change patterns of emergency admission to hospital? *J Health Services Res Policy* 1997; 2:71-4.

Guidelines on circumcision

Legal position is unclear

EDITOR—In the news item by Linda Beecham the chairman of the General Medical Council standards committee, Professor Sir Cyril Chantler, assures readers that male circumcision is legal.[1] The position is not, however, as clear as he claims. There is no doubt that parents have the power to give proxy consent for removal of an incompetent child's foreskin or any other tissue when removal is strictly necessary for therapeutic reasons. When the removal of tissue is not necessary for treating or diagnosing disease a parent only has the legal power of consent to a procedure which causes negligible risk and minimal burden.[2] Male circumcision causes at least a 2% risk of clinically important complications,[3] removes specialised tissue,[4] and may be later regretted by the patient.[5] It therefore meets neither the requirement for negligible risk nor that for minimal burden.

It is difficult to see how the GMC could escape the conclusion that it is impossible for doctors to obtain valid consent for the non-therapeutic circumcision of healthy infants. The claim that parents would turn to people "who lacked the skills to perform the procedure competently" is superficially attractive, but the therapeutic context does not render ethical surgery which has no therapeutic intent and which is performed without the consent of the patient. No matter how great the benefits of a procedure it is bad medicine if it is performed without consent.

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1 Beecham L. GMC issues guidelines on circumcision. *BMJ* 1997;314:1573. (31 May.)
2 Nuffield Council on Bioethics. *Human tissue: ethical and legal issues*. London: Nuffield Council on Bioethics, 1995.
3 Williams N, Kapila L. Complications of circumcision. *Br J Surg* 1993;80:1231-6.
4 Taylor JR, Lockwood AP, Taylor AJ. The prepuce: specialised mucosa of the penis and its loss to circumcision. *Br J Urol* 1996;77:291-5.
5 Warren JP, David Smith P, Dalton JD, Edwards GR, Foden M, Preston R, et al. Circumcision of children. *BMJ* 1996;312:377.

No longer recommended routinely in North America

EDITOR—I am concerned that the General Medical Council's guidelines on circumcision, as described in the news article by Linda Beecham, do not address the efficacy of this procedure when it is not clinically indicated.[1] Since there are clinical indications for circumcision few would argue that it should be outlawed, but circumcision is also widely practised for religious, social, and pseudomedical reasons. Doctors and parents need guidance on the clinical and psychological implications of circumcision performed for non-medical reasons. The GMC's guidelines do not give this type of guidance or indicate where it can be found.

In North America, where circumcision is common, this question has been addressed by many organisations. Most recently, the Canadian Paediatric Society addressed the issue of whether neonatal circumcision of newborn male infants should be recommended routinely.[2] Its discussions and conclusions are relevant to the question of male circumcision for non-medical reasons. Although the authors indicated that the position was "evenly balanced," they recommended that "circumcision of newborns should not be routinely performed." This is the official position of the Canadian Paediatric Society, and it is in keeping with previous statements and statements by the American Academy of Pediatrics. I understand that the Royal College of Paediatrics and Child Health is considering the matter. Both doctors and patients should be aware of current recommendations in North America.

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1 Beecham L. GMC issues guidelines on circumcision. *BMJ* 1997;314:1573 (31 May.)
2 Fetus and Newborn Committee, Canadian Paediatric Society. Neonatal circumcision revisited. *Can Med Assoc J* 1996;154:769-80.

Warfarin use in patients with atrial fibrillation

May increase risk of haemorrhage in elderly patients

EDITOR—In their study on the use of anticoagulants in patients with atrial fibrillation Mark Sudlow and colleagues do not seem to appreciate that patients may not have been prescribed warfarin because treatment was not indicated as well as because it was contraindicated.[1]

There is clear evidence that warfarin reduces the risk of stroke by about two thirds in patients with atrial fibrillation—from 12% to 4% per year in secondary prevention[2] and from 4% to 1.5% per year in primary prevention. However, only 40-50% of these strokes are major disabling or fatal strokes,[3] the reduction in their incidence being of the order of 4% per year in secondary and 1.1% per year in primary prevention.

This must be set against the risks inherent in treatment with warfarin. Large trials of treatment with warfarin in patients with atrial fibrillation have suggested that the incidence of major haemorrhage is about 2.8% per year[1]; this compares with a typical control figure of 0.7% per year. The figures become more worrying when older patients are considered. One stroke prevention trial reported a rate of major haemorrhage of 4.2% per year in patients over 75.[4] A retrospective and prospective study of 2376 patients receiving warfarin for a variety of indications reported an incidence of life threatening or fatal haemorrhage of 3.38% per year in those over 80.[5] These excess risks of treatment exceed the reduction in the rate of major disabling or fatal stroke in primary prevention and approach the reduction rate in secondary prevention.

There cannot be a recommendation to treat all patients with atrial fibrillation with anticoagulants. Only by considering fully the indications for treatment (including factors such as hypertension, cardiac failure, and appropriate echocardiographic data) for each individual patient can a correct decision be reached. In many elderly patients the correct decision is to withhold anticoagulant treatment.

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1 Sudlow M, Rodgers H, Kenny RA, Thomson R. Population based study of use of anticoagulants among patients with atrial fibrillation in the community. *BMJ* 1997;314:1529-30. (24 May.)
2 European Atrial Fibrillation Trial Study Group. Secondary prevention in non-rheumatic atrial fibrillation after transient ischaemic attack or minor stroke. *Lancet* 1993;342:1255-62.
3 Stroke Prevention in Atrial Fibrillation Investigators. Predictors of thromboembolism in atrial fibrillation. I. Clinical features of patients at risk. *Ann Intern Med* 1992;116:1-6.
4 Stroke Prevention in Atrial Fibrillation Investigators. Bleeding during antithrombotic therapy in patients with atrial fibrillation. *Arch Intern Med* 1996;156:409-16.
5 Fihn SD, Callahan CM, Martin DC, McDonell MB, Henikoff JG, White RH. The risk for and severity of bleeding complications in elderly patients treated with warfarin: the national consortium of anticoagulation. *Ann Intern Med* 1996;124:970-9.

Better surveillance is needed

EDITOR—Mark Sudlow and colleagues' community based study on the use of anticoagulants in patients with atrial fibrillation, sponsored by the Stroke Association, has accumulated a large amount of epidemiological support for the authors' conclusion that "there is an imperative to improve and expand the current use of warfarin."[1] In their report, 75% of the patients who were known by their general practitioners to have atrial fibrillation and who had no contraindications to anticoagulant treatment were not prescribed warfarin. There was, however, no mention of the causes of and risks associated with atrial fibrillation in this group of patients. Valvar abnormalities, diabetes mellitus, ischaemic heart disease, cardiac failure, hypertension, and echocardiographic evidence of large left atrial size or left ventricular dysfunction all increase the risks of thromboembolism when associated with atrial fibrillation. It would be particularly interesting to know how many of the patients with atrial fibrillation who were not treated with warfarin had undergone a full cardiological work up—including cardiac imaging—so that an informed decision about the risks of anticoagulant treatment could be measured against the probability of preventing a major thromboembolic event. The ratio of benefits to risks is particularly relevant when making decisions on whether to give anticoagulants to patients older than 75, whose chance of having a major bleed is estimated to be between 2% and 4% annually. Furthermore, the perceived therapeutic superiority of warfarin over aspirin in the treatment of low risk, non-rheumatic atrial fibrillation is far from clear and has yet to be decisively shown.[2 3]

Sudlow and colleagues' study does not mention whether any of the patients with atrial fibrillation who were not treated with

warfarin were taking a therapeutic dose of aspirin; if they were not were they all subsequently treated with anticoagulants as the authors recommended? The logistical implications of this for overburdened, hospital based, anticoagulation clinics or fledgling community based services are a cause for concern.[4] Although the evidence is overwhelming that warfarin is effective in preventing ischaemic strokes in patients with atrial fibrillation, the "imperative to expand" its use will have the desired effect only if it is matched by improved risk stratification and better surveillance of patients taking anticoagulants.

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1 Sudlow M, Rodgers H, Kenny RA, Thomson R. Population based study of use of anticoagulants among patients with atrial fibrillation in the community. *BMJ* 1997;314:1529-30. (24 May.)
2 Stroke Prevention in Atrial Fibrillation Investigators. Warfarin versus aspirin for prevention of thromboembolism in atrial fibrillation: stroke prevention in atrial fibrillation II study. *Lancet* 1994;343:687-91.
3 Cleland JG, Cowburn PJ, Falk RH. Should all patients with atrial fibrillation receive warfarin? Evidence from anticoagulation trials. *Eur Heart J* 1996;17:674-81.
4 Fitzmaurice DA, Hobbs FDR, Murray JA. Monitoring oral anticoagulation in primary care. *BMJ* 1996;312:1431-2.

Authors' reply

Editor—We agree with David Sulch and Stephen Bridger that the decision to treat a patient with anticoagulants must be made after considering the patient's risks of both stroke and bleeding. The risks of anticoagulant related bleeding discussed by Sulch may be exaggerated by the high intensity of anticoagulation used in some subjects in the study quoted. Additionally, the clinics considered in the study did not use the international normalised ratio which is likely to make control of anticoagulation safer.[1] A study of equivalent size that used the international normalised ratio suggested that bleeding occurs much less frequently (rate of major or fatal bleeding 1.35%).[2]

Both correspondents asked about additional risk factors for stroke in the population we studied. The cohort has enlarged since our report and the analysis of risk factors is continuing. We were, however, able to collect clinical data and perform echocardiography on 71 of the 100 subjects in the article; we found that 66 (93%) of them had either clinical or echocardiographic risk factors. These preliminary results suggest that the rates of stroke quoted by Sulch may underestimate the risk in the community and hence the potential benefits of anticoagulant treatment. The results suggest that the balance of risk and benefit is in favour of anticoagulation for most patients over 65 who do not have contraindications to warfarin treatment.

This balance of risks and benefits is clearly of particular concern in elderly people. The risk of stroke increases with age. Whether the risk of bleeding increases remains controversial,[3] but even if it does the relative rate of increase in the risk of bleeding is no higher than the rate of increase in the risk of stroke, which leaves the overall balance more strongly in favour of anticoagulant treatment in older patients.

Bridger suggests that aspirin may be an effective alternative to anticoagulants in patients with atrial fibrillation. In contrast to the evidence showing a benefit for warfarin, that for aspirin is sparse, with two out of three studies comparing aspirin with placebo and showing no benefit for this treatment.[4 5] We believe that the place of aspirin is currently limited to patients who cannot take anticoagulants.

We agree with Bridger that wider use of anticoagulants will increase the pressure on those who supply anticoagulant services. We believe that the solution to this should be an expansion of supply rather than an inappropriate restriction in demand.

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Helen Rodgers *Senior lecturer in stroke medicine and services*
Rose Anne Kenny *Professor of geriatric medicine*
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1 Fihn SD, Callahan CM, Martin DC, McDonell MB, Henikoff JG, White RH. The risk for and severity of bleeding complications in elderly patients treated with warfarin: the national consortium of anticoagulation. *Ann Intern Med* 1996;124:970-9.
2 Palareti G, Leali N, Coccheri S, Poggi M, Manotti C, D'Angelo A, et al. Bleeding complications of oral anticoagulant treatment: an inception-cohort, prospective collaborative study (ISCOAT). Italian study on complications of oral anticoagulant therapy. *Lancet* 1996;348:423-8.
3 Landefeld CS, Beyth RJ. Anticoagulant-related bleeding: clinical epidemiology, prediction, and prevention. *Am J Med* 1993;95:315-28.
4 Petersen P, Boysen G, Godtfredsen J, Andersen ED, Andersen B. Placebo-controlled, randomised trial of warfarin and aspirin for prevention of thromboembolic complications in chronic atrial fibrillation. The Copenhagen AFASAK study. *Lancet* 1989;i:175-9.
5 European Atrial Fibrillation Trial Study Group. Secondary prevention in non-rheumatic atrial fibrillation after transient ischaemic attack or minor stroke. *Lancet* 1993;342:1255-62.

Title of news item on stillbirths was inaccurate

Editor—Benjamin Hope reports in News that more than half the stillbirths and deaths before the age of 1 month in Britain could have been prevented.[1] If only this were so.

Perinatal mortality has fallen dramatically in recent years, from 32.8/1000 births in 1960 to 7.6/1000 in 1993. Nowadays the vast majority of these perinatal deaths are due to prematurity and congenital abnormality. With current knowledge, many of these might be regarded as unpreventable.

The Confidential Enquiry into Stillbirths and Infant Deaths, which monitors perinatal mortality, excludes premature infants and those with congenital abnormality from its reports and thus is left with a small subgroup of perinatal deaths. It is in this subgroup that half the deaths may be avoidable. Although Hope goes on to clarify this point, he fails to put it into perspective.

The title of Hope's piece—"Half the deaths of young infants may be avoidable"—may be sensational to a journalist, but it is inaccurate, and also distressing to those who look after the health of women and their babies before, during, and after labour and to those whose baby died in the perinatal period.

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1 Hope B. Half the deaths of young infants may be avoidable. *BMJ* 1997;315:144. (19 July.)

Bitten—by taxonomy

Editor—Tim Horton's informative letter on the establishment of Australia's redback spider in Japan was of great interest, not least because my wife (an Australian) is currently working in Japan and as a confirmed arachnophobe I am in fear of being bitten by the redback when holidaying in Australia or Japan.[1] However, Horton misclassified *Latrodectus mactans hasselti* as an insect. Both insects and spiders belong to the phylum Arthropoda. Six legged insects are grouped within the class Insecta, but eight legged spiders belong to a separate arthropod taxon, namely the subphylum Chelicerata, class Arachnida.

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1 Horton P. Redback spider is now established in Japan: bites can be recognised by a unique sign. *BMJ* 1997;314:1481. (17 May.)

⁂We received three other letters pointing out the misclassification.—Editor

New logo

Is this a new use for intrauterine contraceptive devices?

Editor—I am pleased that Trisha Greenhalgh likes her new image as displayed in the Views and Reviews section of the journal.[1] But could someone please explain why she uses an intrauterine contraceptive device as a hair ornament?

Mary Jane Platt *Senior lecturer*
Department of Public Health, University of Liverpool, Liverpool L69 3GB

1 Greenhalgh T. New logo. *BMJ* 1997;314:1559. (24 May.)

Columnist's reply

Editor—I congratulate Mary Jane Platt for her perceptive observation. The illustration was in fact originally intended for inclusion in the forthcoming title "Guidelines on the more imaginative uses of medical implements and instruments," to be published shortly by the Alternative Surgical Procedures Group. As this is the silly season, perhaps readers would like to offer their contribution.

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