

multaneously giving no such absolute or relative guarantee to the lower-cost areas, is a recipe for mediocrity and intra-institutional conflict. It makes any attempt to redress past erosion difficult, any attempt to upgrade equipment or teaching styles in accounting or economics or law nearly impossible. It protects the Commonwealth from having to take responsibility for its past mistakes and is likely to create serious divisions among academics, more easily led to fight for relatively distributed resources than to unite to demand aggregate sufficiency.

Finally, there is a major silence in the formula about what the future system will be, as well as concerning the student load that will be taken into account. Will all students taught be paid for or will past (or future) "over-enrolled students be discounted or taught without funding? How much freedom and autonomy will institutions in fact have if the cannot be assured of a fixed and predictable price for new students (based on real discipline mix costs), as well as being paid for all of their base students?

The precise effects of the Relative Funding Model are not yet easily assessable. The process of implementation will take at least three years and even then the relativities between institutions with the same weighted load may be up to 6% discrepant because of the 3% permissible variation above or below the formula amounts. The amounts of new money available (\$30 million over three years) is less than one-third of one per cent of the total annual costs of the system, and institutions with accumulated deficits will be unlikely to overcome those losses any time soon. In particular, the "established" universities gain via the Relative Funding Model while the newer universities remain handicapped.

The new funding formula may make things more clearer, but how will it deal with inevitable variations in the over- and under-enrolment of students in particular institutions or fields? Will there still be unfunded students on a major scale? Will discipline mixes and real costs be recognised after 1993? What rational incentives will remain to guide and motivate longer term planning? Finally, if total student demand declines, as it may, what happens to funding? Will it go down too, or will quality and improved ratios and equipment be more abundant? And if demand remains high, who can meet it?

The DEET/Dawkins revolution is not yet complete. Despite some positive aspects, already confirmed are some of CTEC's and the critics' worst fears, although it is too soon to say if the net overall results will be ultimately beneficial. A truly fair and rational system will eventually have to treat differential real costs, quality, and enrolment targeting more openly and equitably. The alternative, a hidden agenda to push students through regardless of quality at lowest cost, would surely be a recipe for decay and disaster, whether in Universities or in Technical and Further Education (TAFE) institutions. While growth remains so strong, consolidation of the system is problematic - regardless of who controls the system after the 1993 parliamentary elections.

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4. "While it is true that there has been a decrease in per capita funding since 1983, this reflects an increasing base student load due to institutional over-enrolment and new intakes funded at marginal rates. Over the 1989-92 period, there will be a massive injection of funds. The fall in the \$/EFTSU rates of funding has been arrested." Dawkins, J.S., cited in *FAUSA News*, p.2, 13 March 1990. This statement is true only for the pre-1989 period.

5. In 1990, \$25 million was divided, circa \$15m to ex-19, pre-1989 universities and \$10m as development infrastructure grants, to ex-CAEs, especially technology CAE. In 1991 a third type of grant for cooperative research infrastructure projects will come to stream, constituting, by 1992, 11m to 16m for ex-CAEs and 19m for ex-universities; some diversion is taking place away from traditional universities, but some is received back, competitively, for specific projects. Out of the ex-universities' infrastructure monies, totalling \$15m, the infrastructure block grants were allocated proportionately to past success in obtaining outside grants: with over 1 million dollars going to Sydney, Melbourne, NSW, Queensland, Adelaide, UWA, and Monash. In the next tier were Flinders, UNE, ANU, La Trobe, Macquarie, Newcastle with between 400,000 and 700,000 dollars each; lowest were Deakin, Griffith, James Cook, Murdoch, Tasmania, and Wollongong. See Attachment A, Australian Research Council, "Funding for Research Infrastructure, Guidelines for 1990", November 1989.

6. See AHES, for the end of 1989, detailing how the National Party in Western Australia's Parliament was unconvinced of the benefits of Murdoch-UWA merger, thus killing the necessary state legislation. Ironically, the planned, (assuming merger), data for UWA-Murdoch suggest this, unique, university-university amalgamation would have hurt these institutions financially much less than a university-CAE merger. If financial advantage were the only criterion, a university under pressure to merge would find an amalgamation with another university much more advantageous.

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Some Dynamics of Authorship

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In a study of career advancement at The University of Sydney (Dunkin, 1991), I found, without surprise, that publication record was a major determinant of initial status as a tenured or probationary lecturer and of the speed with which promotion to senior lecturer was secured. In both cases, those with higher publication scores had more success. I also found, with concern, that gender was involved by virtue of its association with publication record, so that women, who tended to score lower on publication record than men, took longer to obtain tenure if initially probationary, and took longer to receive promotion to senior lecturer. Of course, these results were not necessary to establish the importance of publication in academe, but they were the stimuli that provoked me to undertake the study of the dynamics of authorship reported here.

I began by looking in greater detail at the list of publications contained in the annual Research Reports of the University and found myself focusing upon authorship. It did not take long for me to discover what I probably already knew, that is, that scientists' publications were much more likely to be multiple-authored than those in the humanities and the social sciences. I was much more intrigued by the discovery that women were less frequently involved in multiple authorship than men. At first it occurred to me that this was an artefact of there being many fewer women in the sciences. As my enquiry progressed, however, that explanation did not appear to be sufficient, for I noticed a similar trend even when I compared men and women in the same humanities and social science departments. The upshot of all this was the conclusion that I had better do a more carefully designed study that would allow scientists to be compared with others, with gender being controlled, and that would allow men to be compared with women, with discipline-type controlled.

The literature on authorship

The literature on authorship informs us that it was in France during the 17th and 18th centuries that scholars first collaborated (Beaver & Rosen, 1978, 1979a). However, it seems that it was only after World War II that multiple authorship increased rapidly enough for it now to be the norm in many fields (Price, 1963; Beaver & Rosen, 1979b). Bayer and Smart (1988) wrote that the greatest degree of collaboration occurs in fields which receive considerable financial support, require sophisticated instrumentation and facilities, and which are more "mature" disciplines with "strong paradigm development" (p.1). Thus, such fields as the biomedical disciplines and "hard" sciences, except mathematics, are highly collaborative, while the "soft" social sciences, including educational research and the humanities, have a much lower incidence of collaborative scholarship.

Beaver and Rosen (1979b) pointed out that the more prestigious journals in a discipline contain a higher proportion of multiple authorship than other publications. Gordon (1980) and Presser (1980) both showed that there was a positive correlation between probability of acceptance for publication and the number of authors of a paper. However, Bayer and Smart (1988) concluded that research does not show differences in impact or quality between solo and multiple authored papers in various disciplines. Bayer and Smart (1988) referred to problems for administrators in higher education emerging from collaborative scholarship. Among these problems are disputes about the order of authors' names, and about the names to be included in the list of authors. Complaints about delays and even threats to research projects themselves have arisen

from collaboration. Bayer and Smart asked, "Who among the authors is responsible for fraud in collaboratively authored research?" (p.2).

Authorship "profiles" involving differential weights applied to order of authors' names, and so on, have been used in personnel decisions (Braxton & Bayer, 1986; Creswell, 1985). Frequent solo authors have been described as "uncooperative loners" while frequent collaborators have been suspected of being unable independently to fulfil all the tasks required of a publishing scholar (Bayer & Smart, 1988). Fox and Faver (1984) claimed that junior scholars can lose out through collaboration at the expense of independent publications, while Diamond (1985) argued that multiple authorship is a sign of collegiality and is rewarded in salary decisions.

In their investigation of authorship styles among chemists in American universities, Bayer and Smart (1988) developed a typology of collaborative styles which included seven types as follows: low producers, burnouts, singletons, team leaders, team players, doubletons, and rank-and-files. They also found that significant differences among the types existed on three variables, the quality of their current department, the number of job moves since obtaining their PhD, and the total number of publications since receiving the PhD. Doubletons and team players tended to be employed in more reputable departments than burnouts, low producers, singletons and rank-and-files. Team leaders had held significantly more jobs than team players, rank-and-files, low producers and burnouts. Team leaders, team players and doubletons had significantly more publications than the other types. Unfortunately, by focusing upon a single discipline, Bayer and Smart (1988) were unable to make cross-discipline comparisons. Neither did they explore differences according to such factors as gender and age.

In all, the literature surveyed suggested many potentially important dynamics of authoring that might be expected to emerge in the present study. However, there was an absence of information about the experiences and perceptions of authors themselves and a surprising lack of attempts to relate those experiences and perceptions to the characteristics and work contexts of authors.

I was interested in the perceptions of authors themselves and their experiences as authors. Did the type of substance of their writings affect authoring patterns? For example, are theoreticians more or less likely to write with colleagues than empiricists? What roles are performed by authors engaged in joint publications? How personally rewarding do authors of different backgrounds find multiple authorship and solo authorship? What advantages and disadvantages are perceived in solo authorship and multiple authorship? How do co-authors decide on the order in which their names will appear? How is it determined who will appear in the list of authors? What changes would those who have experienced multiple authorship like to make for future co-operative efforts? Clearly, for a first approach, interviews would be indispensable.

Fortunately, a small research grant became available and a small interview study became feasible.

Method

Sampling

The subject population chosen was the same group of lecturers involved in the earlier study (Dunkin, 1991). They were all the probationary and tenured lecturers who took up duty at The University of Sydney between 1 January 1981 and 1 January 1985.

There were 85 such lecturers.

In an attempt to control for gender and discipline-type, all the women lecturers in science departments were identified. The intention then was to try to match them with men in the same departments and then to form a parallel matched group for the social sciences and the humanities. However, only five of the twenty-one women had been appointed to science departments and for three of them there was no possible male counterpart. A different strategy had to be adopted.

The two women scientists and their male counterparts were selected. Then an attempt was made to find matching males for the sixteen women in the humanities and social sciences. Success was had for seven so that a total of nine matched pairs was identified. These allowed comparisons to be made across a range of discipline-types between men and women who had taken up duty as lecturers at approximately the same time. However, they did not allow comparisons to be made between the sciences and other discipline types. The solution to this problem was approached by randomly selecting from among the thirty-two male scientists a group of nine to equal in number the males in the matched groups. It would now be possible to compare male scientists with a group of males from a range of discipline types. Table 1 shows the disciplines from which all twenty-seven members of the final sample were drawn. Although the matchings by departments of the matched sample were not perfect, they were considered close enough to permit valid comparisons between men and women, with discipline controlled. Furthermore, although Law was over-represented in the matched sample, it was considered that the range of discipline types was sufficient to permit worthwhile comparisons between the male scientists and the others.

Matched Samples		
Male Scientists	Male Matched	Female Matched
Histology & Embryology	Psychology	Education
Organic Chemistry	Industrial Relations	Human Geography
Architectural Science	German Studies	Italian
Electrical Engineering	Jurisprudence	Law
Physics	Public Health	Dentistry
Animal Science	Accounting	Accounting
Civil & Mining Engineering	Law	Law
Applied Mathematics	Fine Arts	Fine Arts
Econometrics	Law	Law

The interviews

Several draft versions of the interview schedule were trialed on colleagues. Finally, questions in three main sections were agreed upon: multiple authorship, solo authorship, and career advancement, with a maximum number of questions of 20 and an actual number for any single respondent ranging from 16 to 20 depending upon whether affirmative or negative responses were made to some questions.

The interviews were audiotape recorded, took on average 50 minutes, and were all conducted by two research assistants, who were ignorant of the sampling strategy, during June, July and August, 1988. They were subsequently analysed by a third research assistant who also did not know the sampling strategy of the study. The data as analysed were then collated by the author to yield

several quantitative measures reported on below, but mainly to arrive at qualitative issues raised in the interviews.

Background and promotion information

Information concerning the lecturers' ages, academic qualifications, teaching experience, departments, genders and times taken to be promoted to senior lecturer were obtained from university papers reporting such matters. Initial publication records were obtained from the same sources including the annual Research Reports of the university.

Table 2 shows that the only statistically significant differences across the three groups in these variables concerned publications

Table 2: Comparisons Among Three Groups on Background, Publication and Promotion Variables

Variables	Group 1 Male Scientist (n=9)		Group 2 Matched Males (n=9)		Group 3 Matched Females (n=9)		F	P
	\bar{x}	SD.	\bar{x}	SD	\bar{x}	SD		
Age	33.22	2.91	31.89	4.3	34.11	5.7	0.56	0.58
PhD	1.89	9.33	1.78	0.4	1.56	0.5	1.33	0.28
Teaching Experience	6.00	2.06	4.67	4.3	6.44	3.6	0.63	0.54
Total Publication Rate	4.29*	3.16	2.44	1.1	1.74	1.3	3.56	0.04
Solo Publication Rate	1.05	1.34	1.77	1.0	1.29	1.3	0.79	0.47
Joint Publication Rate	3.24**	2.36	0.67	0.4	0.48	0.7	3.58	0.04
Time to Promotion	44.44***	16.9	51.78	11.	69.89	25.	4.29	0.03

Notes:
 * Group 1 > Group 3, p<.05
 ** Group 1 > Group 2, p<.05; Group 1 > Group 3, p>.05
 *** Group 1 < Group 3, p<.05

and promotion. Analyses of variance and Tukey tests revealed that there were statistically significant variations among the three groups in the mean total number of publications per year and that the male scientists had significantly higher rates than the women (p<.05). The same techniques revealed that there was a statistically significant variation among the three groups in the rate of multiple authored publications, that the male scientists had significantly higher rates than each of the two other groups (p<.05) and that the latter did not differ significantly from each other. Finally, Table 2 reveals that the women lecturers took, on average, a statistically significant 25+ months longer to receive promotion than the male scientists (p<.05). The 18 months longer it took the average woman in the matched group than her male counterpart was not statistically significant.

It seems clear that gender, discipline-type, authorship and promotion speed were connected so that male scientists who engaged in higher rates of publication, especially by way of multiple authorship, than female colleagues in a broad range of disciplines, received promotion much sooner than them.

Given these findings and the impressions reported at the beginning of this paper, it seems appropriate to conclude that, for the sample of lecturers included in this study, publications and, in particular, authoring practices were highly significant influences upon career advancement. That having been established, the next matter to explore was experiences in authoring.

Authoring perceptions and experiences

The interviews began with a series of questions designed to elicit the respondents' perceptions of characteristics of their fields of

study that might condition authoring practices. They were asked whether people writing in their disciplines were more likely to focus on empirical or theoretical matters, whether they perceived anything distinctive about the ways in which publications were produced in their field, whether collaboration was common in that field, and whether joint authorship was as common as solo authorship in the discipline. These questions tended to produce dichotomous responses, frequently reducible to "yes" or "no". The only one of them to reveal statistically significant variations among the three groups concerned whether multiple authorship was as common as solo authorship. The male scientists responded positively to that item significantly (p<.01) more often than each of the other two groups.

If a systematic difference is, then, taken to exist, at least in the perceptions of frequency of multiple and solo authorship among the three groups, attributed reasons for differing authorship practices seem all the more worth pursuing. In cases in which multiple authorship was said to be more common, reasons given were as follows:

1. The complex nature of the projects calls for specialised skills, techniques, equipment or materials which can not be supplied by a single researcher. A different laboratory containing specialised equipment might have to be used and so personnel from that laboratory are included in the list of authors, even though they might have nothing more to do with the project than run a test or grant access to the laboratory.
2. The complexity and volume of work is so great that more than one person is required to complete it within a reasonable time. Research assistants and postgraduate students might be involved in specific aspects of the work. Division of labour occurs with authoring tasks distributed among a number of people according to special interests and expertise.
3. It is a new field of activity with few experts and so the sharing and testing of ideas among two or more people is essential to make progress.
4. A funding agency is reluctant to support single investigator projects. Solo authorship is viewed with suspicion. Multiple authorship is regarded as one way of "keeping them honest".
5. Multiple authorship frequently arises out of supervision of postgraduate students' research. The supervisor's contribution is acknowledged by having his or her name included in publications that result. In some cases postgraduate students collaborate with each other and respective supervisors to produce multiple authored publications.

When solo authorship was seen more to be the norm, the following reasons were given:

1. Solo authorship increases one's visibility.
2. The university and others value solo authorship more highly.
3. Solo authorship allows one to maintain one's independence.
4. One's creative or best work is done alone.
5. There are fewer logistical problems in solo authorship. One does not have to arrange and attend meetings with co-authors.
6. There are few others with similar interests/expertise in the university. Co-authorship is not possible without compatibility in thinking.
7. Tasks are completed more quickly by single authors. In some areas of study there is a need for rapid reactions to current events.
8. British/Australian traditions in postgraduate education are more supportive of solo authorship.
9. The research is library-based and so discourages joint activity.
10. Some individuals are selfish or secretive and are reluctant to share their ideas with others.

In an effort to elicit more information about multiple author-

ship, respondents who reported such involvement were asked about the roles they performed. While in some cases collaborative roles were clearly discernible in the form of cooperation in the conceptualisation, planning, data gathering and processing and writing-up phases, there were several instances when division of labour along the lines of specialisms were in evidence. For example, one might do the calculations while another did the writing, or one might write the theoretical section while another contributed the empirical section. Variations were also reported regarding leadership roles. In one case the respondent played the role of writer and non-writing authors' names were added in acknowledgment of their contributions. One respondent saw himself as a "senior collaborator" with students, interacting with them at every stage and level. However, when co-authoring with staff colleagues he formulated his own section of the report and then discussed matters such as cohesiveness with the co-authors. Another interviewee saw himself as the "driver" in some projects, taking on the "managerial" role, while in other projects he played a more collaborative role. One insight of particular interest provided by him was that if all the authors are equal, the project tends to be given less priority by all. One clear "driver" reported that he ran the laboratory, initiated the projects, did the literature searches, wrote the proposals for funding and, if funds eventuated, employed assistants.

In two cases development in authorship roles over time was traced, first with the respondent having begun as a research assistant who then enrolled as a doctoral candidate whose supervisor made him a co-author. Since having graduated he has become the originator and now works with his own students. The other reported that she began as a graduate student in an overseas university where she formed associations with another female graduate student and with two male staff members all from the host country. The two young women students worked with the two older male staff members who acted as mentors. The relationship with the other graduate student continued after the respondent returned to Australia, to such an extent that collaborative authorship has continued for several years with the costs of overseas travel each year to meet being met from personal funds.

A variety of conventions for deciding the order in which authors are listed in joint publications was reported. While the degree of contribution was a common criterion, there were other common ones. Chief among these was alphabetical order of family name. Others were as follows:

1. Co-authors take turns at being first and second.
2. The order is determined on the basis of leadership with the leader being first.
3. The originator of the idea of the publication is listed first.
4. The order is determined on the basis of job need with the author of greatest need listed first.
5. The team of authors decides.
6. The most senior member in the hierarchy of university positions is listed first.
7. The most dominant member of the team decides.
8. Student-authors are named first.
9. The person who writes the first draft is named first.
10. The person who edited the final draft is named first.
11. A coin is tossed.
12. The publisher decides.
13. The principal investigator is listed last by custom.
14. The authors adopt a team name.

With such a wide range of criteria, judgements about relative contributions on the basis of order in the list of authors are clearly hazardous. Committees charged with responsibility for personnel decisions would not be justified in concluding that even the first named author was the main contributor in many of these cases.

All those who had had experience of multiple authorship considered it to be worthwhile but a few had reservations. The most commonly reported advantage was that of stimulation but there were several others. Multiple authorship was seen to be intrinsically valuable. Moreover, through it, contacts were established and cemented with colleagues. Problems were solved; new ideas and new projects emerged; improvements to one's own writing were made; one's work pace increased; self-confidence was built; mutual criticism improved the quality of the work; projects that would not otherwise have been undertaken were completed; much learning resulted; helping relationships were formed; personal satisfaction was experienced; jobs were obtained and promotion was secured.

However, multiple authorship was seen to have some disadvantages. One respondent experienced difficulty in negotiations with other authors and thought his contribution had been wasted because of such problems as co-authors' delays. Another thought the experience had been worthwhile but thought of it as "a bit of drudgery". A third found it time consuming and that it produced a lower standard of writing.

The types of problems enumerated were as follows:

1. Research assistants reworking the respondent's work without the latter's knowledge;
2. Inequities in the division of topics and workloads;
3. Disagreement over types of analyses to be conducted;
4. High personal financial costs;
5. Frequent arguments about content;
6. Irreconcilable differences in theoretical views;
7. Co-author's work below expected standard;
8. Suppression of one's ideas;
9. One's ideas being left out or under-represented;
10. Disagreements in interpreting findings;
11. Disagreements in the emphasis given to subsidiary ideas;
12. Personality clashes;
13. Disagreement about the importance of a particular role, such as editing; and
14. Disagreement about who should be the first named author.

The majority of respondents (nineteen) saw one problem or another attendant upon multiple authorship and some offered solutions. One recommended "assertive" methods, for example, refusing to submit to a more powerful co-author. Three, however, thought the solution was to give way. Others recommended discussing the problem, perhaps with mediation by another. A problem of order of names was solved by alternating the names in a series of publications. The most extreme solution, suggested by one respondent, was to leave the institution, while another simply saw no solution.

The respondents were divided as to whether their publication records would have been seriously affected if they had not been involved in multiple authorship. Scientists were much more likely than others to see such a consequence. Some respondents saw that there would have been a lowering in the quantity and quality of publications while a few thought the reduction in quantity would not have been accompanied by a lessening in quality. However, not all the effects were seen to be negative. Two respondents claimed that multiple authorship had taken too much time which had been taken from solo publications.

Most (sixteen) of the respondents said that they would like some conditions to be met before any future involvement in joint publications. However, this majority was due mainly to the fact that seven of the nine male scientist group wanted such conditions. The other two groups were about evenly divided. The most commonly stipulated condition was that there should be early clarification about final responsibility for the authors, and clarification of individuals' roles at the outset. Other requirements mentioned were as

follows:

1. One should be able to "get along" with co-workers.
2. Samples of co-author's work should be inspected before a commitment is made.
3. The co-author's punctuality record should be checked first.
4. All authors should have the right to veto the final product.
5. One should make sure one is in control.
6. Genuine involvement of all concerned must be assured.
7. Care must be taken about having one's name included in list of authors if one's involvement is minor.
8. Research assistants and postgraduate students must be assured of full recognition.
9. There must be more money and resources.
10. There must be more time available.
11. There must be a realistic timetable for completion of tasks.
12. There must be an established right to comment on each other's work.
13. There must be equal responsibility and acknowledgment.

Apart from those requirements, respondents also specified changes in work habits, life-style and domestic arrangements that would need to be made for them to engage in more collaborative authoring with colleagues. Most (seventeen) claimed that no such changes would be required but four of those said they were fully committed already, suggesting that any further increase would, in fact, require a major change. Those who explicitly saw a need for change said that the following were needed:

1. Study leave.
2. Clear, regular research time during work hours.
3. Arrangements to suit child rearing.
4. Less time with the family.
5. More weekend/night work.
6. More use of a facsimile machine.
7. More uninterrupted time.

While most of the questions reported so far have concerned multiple authorship, there were two which invited comment on solo authorship. The first asked for a comparison of the value of solo versus multiple authored publications. The most common reaction to this question was that both types of publications can be valuable. However, among those who did choose one over the other, most voted for joint publications (eight) in preference to solo publications (four). The rest (six) did not express an opinion but commented on the strengths and shortcomings of both. A few said that the issue was not how many authors there are but the quality of the journal in which an article appears.

Joint authorship was seen to be valuable because it stimulates new thinking, provides evidence of ability to cooperate and increases output dramatically. On the other hand, solo authorship shows what an individual can do and is especially suited to small, well-defined topics.

On the question of clarifying individual contributions to joint publications, one respondent claimed that referees need to play a strong role in guiding committees in giving credit for publication records. It was argued that if solo authorship were emphasised by personnel committees, staff would get around it by major co-workers alternating their names on joint efforts and by excluding more junior contributors, such as research assistants.

While authors who only publish "solo" might arouse suspicion, one respondent argued that "solidified", stable, co-author teams are also suspicious.

The second question concerning solo authorship sought respondents' experiences with it. All but two of the respondents had had experience of solo authorship while six (all in the humanities or social sciences) said that most of their publications were solo-

authored. Various factors were mentioned as having occasioned solo publications. These factors included own initiative, ideas suggested by other academics, commissioned work, individual interpretations or "spin-offs" of team-work, work arising from doctoral or post-doctoral research, writing arising from teaching, the results of theoretical work, solo authorship by default of a co-worker, and work arising out of interest, not initially intended for publication.

Feelings regarding solo authorship were mixed with some saying that they enjoyed it, that they write easily alone, that initiating work alone is easy, that one can set one's own deadlines and that solo-authorship is quicker for producing articles but slower for books. On the other hand, it was said that solo-authorship was less enjoyable than joint authorship, that working alone is too isolated, more difficult than joint authorship because problems must be solved alone, and that it can be tedious and frustrating.

Authoring and career advancement

The last few questions of the interviews were concerned with the relationship between authoring and career advancement. All but four of the interviewees agreed that publication record had had an influence upon their careers and most (fifteen) said it was a strong influence. Apart from influence upon such matters as tenure and promotions, the interviewees claimed that publication record affected their reputations, their satisfaction, support for their research and the reactions of others. From among those who thought the influence was weak, unknown, or non-existent were statements that promotion was dependent upon whether one was on the top of the salary scale for one's present position rather than on publications and that one did not know what went on in promotions committees.

Only half (five) of those (ten) who had applied unsuccessfully for promotion in the past thought that publications had been important in that decision. Stronger reasons were said to be internal politics, not having a doctorate and not being on the top of the salary scale for the present position (seniority).

Almost half (twelve) of the interviewees said that there was nothing they might have done in publishing that would have enhanced their prospects of promotion. Several said they were already producing as much as was feasible.

Those who thought something could be done indicated that it was mainly a matter of doing more but some stipulated specific directions such as a solo book, more articles in refereed journals and a textbook. One of those, who was pessimistic about the effectiveness of attempts to enhance promotion prospects, said that a much larger number of publications might have helped but that a marginal increase probably would not have. She reported that she had been told her lack of success in promotion was a matter of seniority and commented that in economically difficult times the hierarchy was inflexible concerning seniority unless it was someone who was "quite brilliant".

Finally, interviewees were asked whether the university could have helped them more in their roles as authors. Most respondents (seventeen) considered that more support could have come from the university and most of those (eleven) saw a possibility of more support from the departments as well. The most commonly expressed ways in which more support might be provided by both the university and the departments was a reduction in teaching and administrative loads, followed closely by the university's providing more funds for research and other resources such as library materials and research equipment. A number of specific criticisms were made as follows: inadequate child care facilities; insufficient funds for overseas travel; inappropriate value given to exceptional types of publications, such as art catalogues and exhibitions; page charges for publishing in journals, which the university would not meet; lack of information on publishing priorities; insufficient support staff, such as clerical and technical assistants and typists; too many ad hoc financial arrangements; insufficient numbers of postgraduate students; and lack of access to senior academics.

Not all were dissatisfied with the level of support they had

Table 3: Quantifiable Responses of Three Groups to Interview Questions

	Group 1 Male Scientists	Group 2 Matched Males	Group 3 Matched Females	Statistical Significance
1. Are the matters about which you write mainly empirical or theoretical? Per cent Empirical.	55.60	44.40	33.30	Not Significant (NS)
2. Are you aware of anything distinctive about the ways in which academics in your area of interest go about producing publications? Percentage Yes.	33.30	44.40	66.70	NS
3. Do scholars in your area often collaborate in the authorship of publications? Mean scale score (2).	4.00	3.33	3.44	NS
4. Is joint authorship as common as solo authorship in your field? Mean scale score (3).	4.11**	2.44	2.11	p<.01
5. Have you ever been involved in jointly authored publications? Mean scale score (4).	2.78	2.11	2.00	NS
6. Did any problems or disagreements arise from working together? Mean scale score (4).	0.56	1.11	1.33	NS
7. If you had not been involved in this/these projects would your publication record have been seriously affected? Per cent Yes.	100.00*	57.00	11.00	p<.001
8. Are there any conditions you would like for any future involvement by you in joint publications? Mean scale score (5).	1.22	1.33	1.44	NS
9. What are your views on the relative value of solo authored vs jointly authored publications? Per cent favouring solo authoring.	11.00	33.00	56.00	NS
10. Has publication had an important influence upon your career? Mean scale score (6).	2.67	2.38	2.33	NS
11. Have you ever applied unsuccessfully for promotion? Mean scale score (7).	1.22	1.33	1.56	NS
12. Do you think your publication record was important in the decision not to promote you? Per cent Yes.	50.00	33.00	80.00	NS

Numbers in brackets (), see Notes at end of article.

*Group 1 > Group 3, p<.01

** Group 1 > Group 3, p<.001; Group 1 > Group 2, p<.01

received, for seven took the view that the university could not have been more helpful. They expressed such opinions as: satisfaction with current publications output; real progress rests on oneself; the university had been supportive and generous; the university had neither helped nor hindered; the institutional environment is more important than materials.

Some of those who saw little or no scope for more help did, however, go on to say that teaching loads interfered with their authoring activity.

Other issues mentioned included the policies of some journals which imposed barriers, and family commitments which prevented study leave from being taken.

Discipline-type, gender and authoring

With such small numbers as were in each of the three groups interviewed in this study it was difficult to discern group trends in responses to the interview questions, especially as most of the material communicated was qualitative rather than quantitative. There were several questions, however, which did elicit responses that could be categorised simply and they permitted numbers of responses of different types to be counted. Of course, even with these, differences among the three groups would have had to be extreme to reach statistical significance. This did occur, however, in relation to two variables, the perception of the relative frequency of multiple and solo authorship in the respondent's field of study, and the perception that non-involvement in collaborative projects would have had negative effects on the respondent's publication record.

Male scientists were significantly more likely than each of the other two groups to report that multiple authorship was common in their fields. They were also significantly more likely than the women to perceive that their publication records would be negatively affected by non-involvement in jointly authored publications. In terms of the sampling design of the study, the first of the above differences can probably be attributed to the influence of discipline-type such that authoring in the sciences is different from that in other fields. However, the second finding of specific difference is not so easy to interpret. As the two groups of men did not differ significantly, it can not be seen simply as a discipline-type effect and as the matched groups of men and women did not differ significantly it can not be seen simply as a gender effect. The possibility exists that it is a joint effect of both discipline-type and gender, given that the male matched group was more likely to see negative effects of non-involvement in joint publication projects than its female counterpart.

Apart from these two, specific differences tended to be small and statistically non-significant. One surprising trend did stand out, nevertheless. It was the number of occasions when women were at one end of the distribution of scores in the three groups and the male scientists were at the other end, with the matched group of males in the middle. In 10 of the 12 such comparisons this was the case, as shown in Table 3. The consistency ($p < .001$) of the order of the three groups suggests that both discipline-type and gender were important in the experiences of multiple and solo authorship and matters related to it. If discipline-type were not important the order of the two male groups would have been expected to vary more often by chance. If gender were unimportant the order of the male and female matched groups would similarly have been expected to change more often. On the basis of this finding, it seems that, in general, the perceptions and experiences of the three groups regarding authoring varied significantly, and that, in the main, discipline-type and gender may have operated jointly to produce the difference.

Conclusions

The study has revealed some dynamics of authorship that have not often been documented before. For example, it has provided further evidence that judgments made about publication record on the basis of multiple versus solo authorship and order of authors' names are hazardous. Authors are apparently able to manipulate these to meet the criteria adopted by authorities such as promotions committees. This means that the frequent assumption that publication record is a more accessible and more reliable criterion of academic performance than evidence of

teaching effectiveness is questionable. Perhaps ground rules across disciplines in such matters as the inclusion of names in lists of authors and for indicating relative contributions of authors need to be established.

There do seem to be differences that can be attributed to discipline-type and it is to be hoped that decisions about career advancement already take these into account. However, the possibility that discipline-type and gender are jointly responsible for some differences poses more intricate problems upon which this study has been able to shed little light. It does appear, however, that if women could be assisted or encouraged to participate more in multiple authorship their publication records would be enhanced and their career advancement expedited.

Notes

1. Fisher's exact test was used to test the probability that the male matched group would occupy the middle of three positions in 10 of 12 trials.
2. The mean scale scores reported for this variable were based upon the following scale derived from respondents' replies: 4 = very often; 3 = sometimes; 2 = rarely; 1 = never.
3. The mean scale scores reported for this variable were based upon the following scale derived from respondents' replies: 5 = much more common; 4 = more common; 3 = as common; 2 = less common; 1 = much less common.
4. The mean scale scores reported for these variables were based upon the following scale derived from respondents' replies: 3 = often; 2 = sometimes; 1 = never.
5. The mean scale scores reported for this variable were based upon the following scale derived from respondents' replies: 3 = more than two conditions; 2 = one or two conditions; 1 = no conditions.
6. The mean scale scores reported for this variable were based upon the following scale derived from respondents' replies: 4 = a very strong influence; 3 = a strong influence; 2 = a moderate influence; 1 = little influence.
7. The mean scale scores reported for this variable were based upon the following scale derived from respondents' replies: 3 = more than once; 2 = once; 1 = never.

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Reviews

Young People's Participation in Post-Compulsory Education and Training

Report of the Australian Education Council Review Committee (Brian Finn, Chair) Canberra, AGPS, July 1991, pp xxvi + 188.

The review of post-compulsory education and training, conducted through a committee set up by the Australian Education Council (AEC) and chaired by Brian Finn of IBM, aroused intense interest, one might almost say hysteria, during its existence in the first half of 1991. The concern was justified.

For a start, the terms of reference ranged wide and were a rather alarming grab-bag. They were drawn up hastily, in the dying minutes of an AEC meeting late in 1991, to broaden a limited enquiry on early school leavers into a more general investigation of educational provision for 16 to 19 year olds. Issues for review included participation targets, national curriculum principles, the roles of different post-compulsory institutions and the links between them, related career education matters, resourcing of post-compulsory education, and strategies for participation in post-compulsory education by 'disadvantaged youth - the original focus.

An additional cause for concern, alongside the wide and multi-focussed terms of reference, was the composition of the enquiry Committee. It left out important stakeholders, for example, universities and non-government schools. It also left out any professional education expertise, for example, on curriculum design. The membership was, in fact, comically managerial, consisting of a pair of state TAFE managers, and a DEET manager who had, as his Commonwealth "pair", the Chair of the Employment and Skills Formation Council of NBEET, Laurie Carmichael.

Anxiety was exacerbated further by the Committee's haste and secrecy, totally inappropriate as procedures for the governmental enquiry in a democratic society but part of the mystique of the competitive, corporate private sector which has thoughtlessly accompanied the transfer of corporate managerialism into the public sector.

As if all this were not enough to arouse concern, many aspects of the Committee's hastily patched-together mandate has already been parcelled out to other working parties, enquiries, and implementation teams under other agreements both in and between States. Thus the Finn Review had the potential to undermine, or even to overturn, much that was already in process. There was considerable treading water in TAFEs, in school systems, and in State boards of secondary school studies, while the enquiry was in process.

The publication in July 1991 of the Finn Report, as it was called even before publication, was something of an anti-climax after this build up. In my view, the Report gives little grounds for alarm to schools, TAFEs or universities. While perhaps it stands at the crossroads of educational history in Australia, in itself it will not determine along which of the alternative routes we shall proceed. It shows all the hallmarks of a multi-authored report written by a group of cautious managers: different chapters say somewhat different things and all are circumspect. Indeed, the background hum of the assumptions it makes about the roles and relations between the economy, business, schools, TAFE, universities, and citizens (dare I use the word) - assumptions with which we have all become too familiar over the past decade - are louder than its specific words. It is indicative of the Report that the two most difficult issues it was asked to address, on national curriculum principles and on resources needs and cunning, are, wisely, dealt with in fairly general terms and then passed on to further, more expert, working parties which it recommends

be set up.

There are a number of good things to be said about its interpretation of the hastily conceived - and poorly drafted - brief. I will mention four of these positives.

First, and at last, Australia has some forward projections of the resource requirements for post-compulsory education. The Finn Report has not only made an attempt to map out desirable enrolment targets for upper secondary schooling, for TAFE and for universities, but it has also attempted to cost them. The huge expansion in post-compulsory schooling in the 80s happened without any national resource planning. Until Finn, the Commonwealth had escaped the obligations it so obviously has to finance its own deliberate educational expansion policies. Whether or not one accepts Finn's suggested rhyolitic institutional growth targets, or even more problematic - accepts the bases on which cost projections are calculated, it is a step forward just to have, out on the negotiating table, a serious attempt at costing future educational growth. The wrangles in late 1991 between the States and the Commonwealth over the bill for TAFE were long overdue, and a direct outcome of this particular aspect of the Finn Review.

Second, the Finn Report is a sign of determination by governments at both Commonwealth and State levels to fit TAFE back into a clear post-compulsory education strategy. This is an important step.

Recommendations in Finn concerning TAFE cover several issues. The most welcome, in my view, is a concern with articulation between school and TAFE on the one hand and between TAFE and universities on the other. Students must be able to transfer, along agreed pathways, between all three components of the post-compulsory system, taking credit with them for what has already been learned. An insistence on the reform and clarification of pathways in and out of TAFE is another overdue reform.

Perhaps the major recommendation concerning TAFE is to increase its current relative size compared with universities and upper secondary schooling. Finn's preferred option is for TAFE numbers to rise by more than three times the rise in higher education numbers over the next ten years (p 172). TAFE was the sector which stagnated in the 80s because State educational funds were stretched by the growth of upper secondary schools. (Retention to the end of Year 12 doubled across Australia in the decade 1981 to 1991). Some have argued that the plan to increase TAFE sector funding in order to finance TAFE expansion is something the universities should refer. I do not share the alarm. The argument for TAFE expansion, apart from the mandatory rhetoric about skills for the economy, is largely concerned with the provision of a cheaper, mass, post-school educational alternative. Such reasoning shows a commitment to maintaining the distinction between the status and funding bases of university and TAFE sectors. This may turn out to be unfortunate as Australian needs instead to heighten the status of technical education. But those in universities who have been raising the alarm because they fear a new relativity with TAFE, can be assured that Finn has made no proposal in that direction. Rather the Report appears to seek to expand the provision of a cheaper, alternative form of post-school education for those below the university cut-off line who are increasingly, for obvious reasons, applying for further education.

Third, the Report recommends, and the AEC has approved in principle, the concept of an Education Guarantee: all young people, it argues, have the right to two years full-time or three years part-time education in some post-compulsory institutional combination beyond Year 10. There is some ambivalence over whether this right will be made real rather than just romantic by guaranteeing that such education will be free. The free education logic is put forward in the body of the Report (p 183) but, with managerial circumspection, is not carried through into the recommendations.

As a final positive note, I cannot resist mentioning, on behalf of those who bore the heat of the battle to keep the ACT secondary college system afloat in the 70s and 80s against much State-based parliamentary torpedoing, that Finn acknowledges the considerable merits of separate