OR NEWSLETTER

Operational Research Society of New Zealand (Inc.), PO Box 904, Wellington, New Zealand **January 1996**

GUEST EDITORIAL

Dr Andy Philpott, President, ORSNZ; Department of Engineering Science, University of Auckland, Auckland NZ

Last November, the Council of the ORSNZ received 46 responses to a survey on the future direction of the Society, distributed to members in the October Newsletter. Reading the large number of negative comments on these survey returns leaves one with the impression that our society is in a crisis. The intention of this editorial is to present my view of this perceived crisis, and propose some courses of action.

So what are the problems, and how should they be fixed? First of all I contend that OR/MS is alive and well in New Zealand. The criticisms of academic OR/MS expressed in some recent editorials in this Newsletter cannot on the whole be levelled at the New Zealand OR/MS academics that I know. Most OR/MS academics in New Zealand are not preoccupied with publishing theoretical articles of dubious merit in international journals. On the contrary, much of their work, which is on real problems, remains (regrettably) unpublished. This is often because, once an OR/MS project has been completed and implemented, in the words of Hans Daellenbach "the next exciting adventure already beckons."

Are our academic institutions providing the right OR/MS education for our graduates? Although we should not be complacent, I think that most academic OR/MS programmes in New Zealand are doing well. There are increasing numbers of OR/MS graduates emerging from New Zealand universities, and they all seem to be finding employment, in jobs where they have opportunities to apply their OR/MS education in some way (and even if they never use the simplex method, it should still be taught!). [I leave room here for abuse, etc. from Ed.]

What about OR/MS outside the academic institutions? In my view, quantitative problem solving in management is prospering in New Zealand, driven mainly by the information revolution, but also by a political and economic climate which, whether you agree with it or not, makes it imperative for organizations to remain competitive, and seek and preserve value. But does all quantitative problem solving count as OR/MS? My answer is yes. Some would say we should lament a perceived invasion of our subject by other fields such as management information systems, but I see this appropriation an inevitable cross-fertilization of ideas that we should encourage. However there is more to OR/MS than this, and much more that we can contribute. There remain many complex problems in the New Zealand public and private sector, which are amenable to an OR/MS approach. It is our responsibility to pursue them.

So OR/MS in New Zealand is not sick. On the contrary, she is more like an unruly adolescent; enthusiastic, energetic, and perhaps a little immature. Her guardian, the ORSNZ, is also well, but needs some parenting advice to create an environment which will make the teenager into a worthwhile adult, while making sure she doesn't run away from home. The purpose of belonging to ORSNZ, like any society, is to communicate with people with similar interests and exchange ideas with them. What are these ideas? Well, the subject of OR/MS is by its nature vast in its scope, and none of us finds all of it fascinating. I will generally find a talk on mathematical programming interesting but I do not expect an expert in

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organisational theory to enjoy it. But we should all be interested when we meet someone who says "I thought about this company's processes, saw that the central problems were X and Y, and came up with the following methods to solve X and Y, which are now being used by the company". Much of the appeal of a sentence like this, at least to those of us who consider themselves practitioners, comes from thinking "...neat idea, I could do something like that...". I am pleased to say that many of the papers presented at the 1995 ORSNZ Conference gave me such inspiration.

Most of the papers which appear in the *Asia Pacific Journal of Operational Research* (*APJOR*) do not. It is not my intention to interfere here with the editorial policy of APJOR, but some comment on the suitability of *APJOR* as a journal for ORSNZ readers is warranted. In the survey we conducted, respondents voted overwhelmingly for more applied papers to be published in *APJOR* as opposed to theory. Even good theoretical papers are narrow in their focus, and *APJOR* is not published sufficiently often to get enough good theoretical papers to induce us to read it regularly. There are plenty of other international OR/MS journals (some would say too many) which will publish good theoretical papers to which readers will turn for the latest theoretical advances. I believe that we should attempt to improve and support the *APJOR* by encouraging the submission of more applied papers to it. A recent improvement along these lines may already be detectable. However I do not believe that the current level of interest in the journal warrants compulsory subscriptions for ORSNZ members, and I would advocate separating the *APJOR* subscription from ORSNZ membership. The survey respondents also voted overwhelmingly (31-15) for separation (as they did n the previous survey reported in the April 1995 Newsletter. The score then was 38-8 in favour of separation).

What, if anything, should replace APJOR? Many people advocate bundling *OR/MS Today*, but I feel that the Newsletter is admirably fulfilling this role. Moreover, since the common field of interest for ORSNZ members is really about OR/MS being put into practice in New Zealand, members want to know about this. Most of this work is presented at the annual conference, which along with the Newsletter is widely recognised as the most valuable contribution of the Society to OR/MS in New Zealand. Therefore, I propose that even though they may not attend the conference, all ORSNZ members should receive, instead of *APJOR*, a copy of the conference proceedings, the cost of which should be bundled with their subscriptions. The conference fees for ORSNZ members should then be reduced by the cost of the conference proceedings. I intend to place this proposal on the agenda of the next Council meeting.

To prosper, the ORSNZ must promote OR/MS more widely than just to its members. The ideal medium for this is the World Wide Web. The survey respondents (apart from one or two Luddites) voted overwhelmingly for an ORSNZ presence on the Web. We have responded and created the ORSNZ Home Page. This will be administered by computer staff of the Department of Engineering Science, and will eventually have links established to the home pages of all members of ORSNZ. Links have already been arranged with the Australian Society of Operations Research Home Page, and other OR/MS locations. An orchestrated presence on the Web will be of enormous benefit to ORSNZ and its members, as a means of communicating ideas, seeking assistance with OR/MS problems, providing mentors, and placing and seeking job advertisements. Details of the ORSNZ Home Page, and how to establish links with it are provided elsewhere in this Newsletter.

Following the Annual General Meeting of the Society in August, I must confess to feeling some disquiet about being elected President of an organization with so many dissatisfied members. But as we embark on a new year, I feel very positive about the Society, I think it has a lot to offer its members and the community as a whole, and when our teenager comes of age, I believe we will be able to congratulate ourselves on her upbringing.

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The following comment was sent to the editor in response to the October 1995 editorial, reprinted in the November issue of the OR newsletter UK OR Society.

FAULTY ANALYSIS — THE VALUE OF ACADEMIC RESEARCH IN OPERATIONS RESEARCH

Robert Fildes, Vice-President, UK OR Society, University of Lancaster, UK

In the Success and Survival project one of the questions asked of OR managers was how important they regarded technical expertise for their group. For most it was not a priority. It ranked 17 out of 18. And yet, what is the fundamental contribution that OR brings to an organisation? The founding fathers of the discipline were quite clear: it was the synthesis and solution of model based approaches applied within a

broad organisational setting (to avoid what Hitch called sub-optimization, *Operations Research*, 1953). Some of the problems faced in those early years proved soluble when formulated in (what at the time seemed) complex mathematics. Methods were developed to solve such formulations. But there were decreasing returns: after many of the important problems were solved researchers were left to plow furrows that were unproductive, either because they were unimportant to organisations, or their formulation, omitted key elements so that any OR solution was viewed as irrelevant, or perhaps they required mathematical techniques or computer power beyond the resources of the researcher (Wagner, 1971, The ABCs of OR, *Operations Research*).

At the same time as these developments, organisations were becoming increasingly cost-conscious. Organisational slack diminished; time scales for finding solutions lessened. For many, OR was turned into an effective weapon of cost reduction. Little time was available for researching novel solutions, even when the payoff could be high. For the majority of problems, new methods were left to the academics to discover. What practitioners needed were new methods whose potential value was clearly established.

Hans Daellenbach (October 95 *Newsletter of the UK OR Society*) suggests that the academics have failed: failed as developers of useful models and methods and failed as the editorial gatekeepers to the professional journals. Instead, so the argument goes, researchers supported by journal editorial policy, pursue essentially trivial mathematical solutions to non-problems. It is a common complaint made through letters to the Newsletter and discussed in my own editorial (August, 1995) as well as in, for example, the recent article by Reisman and Kirschnick (The devolution of OR/MS, *Operations Research*, 1994). Daellenbach unfortunately ignored these earlier contributions. Nor did he provide evidence of either the preponderance of trivial mathematics or the lack of good case studies — surely a responsibility when initiating such a vigorous critique of academic journal practice. But he still might be correct in his criticisms of the trivial, asinine papers which clutter our journals.

Do we accept his arguments? Well, on the one hand, yes, but on the other, no. To concentrate on the issue of journal publications for the moment, whether you are satisfied with a journal or dissatisfied depends on what you expect to read. As I argued in the August issue, the majority of OR articles contribute little either to theory or practice. Few are read in their entirety. But is this a sign of failure? The majority of new products fail, the majority of advertising is wasted. The issue for the profession is not that identified by Daellenbach unless he can seriously argue that the OR journals year-on-year publish nothing of value. As it happens, the Awards Committee of the OR Society was completing its somewhat protracted deliberations to decide on those papers suitable for the Society s medals at the same time as the Daellenbach polemic hit my desk. In contrast to his views, between the three of us (Ivor Langley from Littlewoods, Paul Williams from Southampton and me) we had identified some twenty articles in *JORS* that we thought were potentially suitable for an award. There was little consensus between us although we happily honed in on a long-short list. But the diversity in our ratings is a strength, not a weakness. It is not that we disagreed (much), with one stating a particular article was worthless while the other thought it a major contribution; different features in an article appealed more to one reader rather than the others. At least in 1994, *JORS* strength lay in case studies, where the mathematics were secondary to the requirements of the problem being modeled. The editor and many of the authors in *JORS* are, we think, to be congratulated. In each of the issues there are usually one or two articles that are interesting (we might not agree which ones of course) and a smaller number directly relevant to current interests. If Daellenbach is unimpressed by one year's publications in JORS, he can examine the interesting case studies considered for the Edelman award and described in Interfaces. My own impression of *JORS* in recent years is that more successful cast studies are being published.

From my perspective, the problem with the mix of papers published in *JORS* and in the other major OR journals is rather different to that described by Daellenbach. It is that too few papers make major contributions to theory. Case studies are interesting; case studies are what OR is about: we can warm ourselves with such thoughts, basking in the mythology of the subject. But the strength of our field and its rapid development arose from our ability to abstract. It is its mathematical and logical core that gives us a set of unique skills that cannot easily be replicated by other groups of management consultants. The problem with case studies is that while we may enjoy reading them, too few researchers and too few practitioners find them relevant to their business and for the obvious reason, they describe a unique problem.

Bob Miles, using his editorial prerogative, headed Hans Daellenbach's article Debate. Well, let me challenge the readership to identify those technical developments in the last ten years which have made OR potentially more effective in solving real organisational problems. What are the areas that OR managers should be boning up on or are they right to put technical developments down towards the bottom of their list in the factors that make OR successful? Do heuristics or neural nets help us? Have soft OR methods let us reach the parts of organisations previously inaccessible? Where do we expect the new developments to arise that will give us the unique skill set to keep us happily employed into the next century?

Addendum

The long-short list of articles we rated most highly were as follows:

K.Y.K. Ng and M.N. Lam [1994]. "Composition of the Canadian Total Force: Issues and findings for the air components", *JORS*, February, 151-164.

A.K. Shahani, N. Korve, K.P. Jones & D.J. Paynton [1994]. "Towards an operational model for prevention and treatment of asthma attacks", *JORS*, August, 916-926.

S.A. Malcolm & S.A. Zenios [1994]. "Robust optimization for power systems capacity expansion under uncertainty", *JORS*, September, 1040-1049.

P. Wark & J. Holt [1994]. "A repeated matching heuristic for the vehicle routeing problem", *JORS*, October, 1156-1167.

G.M. Campbell & C.F. Davis [1994]. "Consolidating facilities for the Connecticut Department of Transportation: a case-study", *JORS*, April, 419-430.

The Goodeve medal has been awarded to the last of these papers. Only one of these papers focuses on making a theoretical contribution alone.

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FAULTY ANALYSIS SQUARED — PUBLISHED ACADEMIC RESEARCH IN OR

Hans G. Daellenbach, Dept. of Management, University of Canterbury, Christchurch, NZ

Robert Fildes invited me to respond to his comments. Although it is fun, this will definitely be the last time I intend to address this controversial subject, where people are bound to disagree.

One of the first things I tell students tackling their first OR project is to approach a problem with an open mind, to avoid firming up ideas before they comprehend the problem situation in which it is embedded. The risk of looking at a problem with preconceived ideas is that one tends to filter out anything that does not fit in with these preconceptions, ending up solving the wrong problem. Fildes's comments struck me very much as a case of selective filtering out. I did not generalize, as he seems to imply, that academics have failed as developers of useful models and failed as editorial gatekeepers. In fact, I gave credit to the ground-breaking work done and still being done. To repeat my words: 'I was talking about the trivial, asinine papers cluttering our journals . . .' I challenge Fildes to tell us how these contribute to the promotion and image of OR as 'the synthesis and solution of model based approaches applied within a broad organizational setting', to use his words. With their restrictive assumptions, these models and solutions algorithms are prime examples of 'suboptimization.' (As an aside, the concept of suboptimization is rather meaningless. Each system is embedded in a wider system. Hence, any optimization is only optimal within that setting, but may be suboptimal within a wider system definition. The real art in OR modeling is to choose the appropriate boundaries of the narrow system within the wider system, so as to cover all significant aspects, where significant is not a hard concept, but one that depends on the resources available for the analysis and the potential benefits. I am sure that Hitch would agree with that.) I also wish to add that Grahan Rand does a marvellous job for *JORS* and has to rely on his referees to do the same. The fault lies in the editorial policy and the instructions to the referees which state that papers should not be rejected because the referee disagrees with the assumptions.

Naturally, what I see as trivial and asinine may be of intense interest to somebody else. I doubt though that this includes more than a handful of real OR practitioners. My evidence for this? Purely anecdotal! It would be a very enlightening research project for a young academic, desperate for a publication, to do a survey on the number and background of the people who read such papers.

The way Fildes argues about my criticism of such papers ('Well, on the one hand, yes, but on the other, no.') reminds me of that vacant job advertisement for a one-armed operations researcher. When the manager of the firm was asked why a one-armed operations researcher, he responded that he was sick and tired of being told by his OR staff that 'one the one hand he could to this, one the other that!'

In the August issue (I would have referred to it, but seamail postage takes three months to reach these shores!) of the UK society OR newsletter, Fildes argues that the majority of papers contribute little either to theory or practice. My statement was much less drastic than that. But what is the purpose then of publishing such papers? When I was a struggling assistant professor in the US, Kim, a former colleague of mine and practicing Zen Buddhist one morning told me that the previous night in his dream he talked to Buddha. From memory, his dreams was something like this:

"Kim: I have a BA magna cum laude from the University of Tokyo, an MA from Princeton, and a

Ph.D. from Harward. And yet, no world-shattering idea has come from me. My hopes of being a second Gauss or even a second Keynes are fading fast. Where did I go wrong? Am I suffering from bad karma?

Buddha: Listen to me, son. Out of how many scholars does a Gauss emerge? or even a Keynes?

Kim: Well, perhaps one out of every 10,000.

Buddha: As a statistician you know about Bernoulli trials?

Kim: Yes.

Buddha: If I toss a loaded coin — so loaded that the chance of getting a head is one in 10,000, how many tosses may be needed for the first head to turn up?

Kim: Well, maybe only one, maybe a thousand, ten thousand, maybe even a hundred thousand tosses and even then I cannot be sure that a head will turn up.

Buddha: But you agree that each one of those tosses contributes to ultimately produce a head?

Kim: Yes.

Buddha: Listen to me and awaken. For one Gauss, hundreds, thousands, even tens of thousands of aspiring scholars must be tried, but each one contributes to produce a scholar ultimately. As it is not up to the coin to determine whether the result of a toss should be heads or tails, it is not up to you to see whether you are a second Gauss or a Keynes. So just continue working hard so that you will join my Bernoulli trials — trials for another Gauss who will benefit humanity."

(As an aside, I encouraged Kim to submit an account of this dream to a mathematical journal. He did and got a publication from it!)

The wisdom in this dream may put a completely new light on the production of such papers. But is this reason enough to pad our journals with them and if so, why not create LEAPs, specializing in them?

Fildes also ignores my earlier contributions to this subject, where I give specific credit to the efforts of the editors of *Operations Research, Interfaces,* and *JORS* for publishing real case studies. I though completely disagree with him about the value of such case studies. He claims (where is his evidence? *Two can play that game!*) that too few researchers and too few practitioners find them relevant because they describe unique problems. If this is so, why do operations researchers persist in wasting their efforts to devise general purpose models for classes of problems? My view on case studies is rather different. Many demonstrate how difficulties caused by unique features can be dealt with within the framework of general purpose models. Others show how a sequence of models will do the trick. Others may trigger research into devising new approaches to tackle such problems. Most will find readers — practitioners as well as researchers, who will recognize analogies with problems and difficulties they face and may get ideas of how to deal with them. They provide essential learning experiences for students in OR. Let us not kid ourselves! OR is not a science, but a collection of models and techniques and a mode of thinking. Although the models and techniques can be learned from textbooks, the mode of thinking is largely learned from seeing experienced operations researcher do it and from doing it ourselves. Case studies are one source of such learning.

I would like to see a change in the editorial policy on case study refereeing. Having submitted case studies myself, I am always amazed that referees ask for revisions, or turn down a case, study because it has not done certain things. (Is it a case to show off their own prowess?) The work has been done. It will not be redone. The referee's role changes. He or she should insist that the case study reports not only on the technical aspects and the solution found, but also on the procedural aspects, on the assumptions made and their impact or lack of it, on the problems encountered and how they were dealt with, on the reasons for choosing a given approach rather than another one, on what the analysts and the client learned from the process, and what would be done differently if the study were to be redone. Even a partial failure can be a valuable learning experience for the reader.

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ORSNZ WORLD WIDE WEB PAGE

Dr A. Philpott, Dept. of Engineering Science, University of Auckland, Auckland NZ

The ORSNZ World Wide Web Home Page has been established on the server in the Department of Engineering Science at the University of Auckland. The Society initially will contribute to the funding of downloading ORSNZ items from our server to external sites. This will be closely monitored by the ORSNZ Treasurer, who will institute a user-pays scheme if excessive expense can be attributed to single users.

From late January, the address for the ORSNZ Home Page is:

http://www.esc.auckland.ac.nz/Organisations/ORSNZ/

The Home Page will link to other national OR Societies, the Royal Society of New Zealand, and a directory of individual members. We are seeking to establish links from this directory to all ORSNZ

members.

If you already have a Home Page on your local computer, and would like this linked to your name in the ORSNZ directory then please email your full name and title, and the address of your home page to

a.philpott@auckland.ac.nz

(Members belonging to university OR/MS groups might like to link the ORSNZ page to their group home page.)

If you currently do not have a Home Page, or a computer, and would like to be included in the directory then please write to *Dr M. Ronnqvist, Secretary, ORSNZ, Dept. of Engineering Science, University of Auckland, Private Bag 92019, Auckland, NZ*, giving your full name and title. We shall construct a Home Page for you on our Web site. Any information you want included on this page should be supplied, preferably in some computer readable form.

SEARCH HEURISTICS FOR SINGLE MACHINE EARLY/TARDY SCHEDULING

Abstract: University of Waikato D.Phil Thesis

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Just-In-Time manufacturing aims to reduce waste within an organisation, by focusing on completing orders at the time requested by customers. Failure to complete orders at the time specified causes the organisation to incur additional costs. The Just-In-Time environment has introduced complexities for scheduling which have been modelled by the Single Machine, Early/Tardy Machine Scheduling problem. Research to date has concentrated on models with assumptions such as the use of a single due date for all jobs or restrictions on early/tardy penalties. This research uses a generalised model which makes no such assumptions. The use of three popular 'intelligent' search techniques: Simulated Annealing, Genetic Algorithms and Tabu Search are investigated as possible solution techniques.

The properties of the common due date, early/tardy scheduling problem enable schedules to be defined from early/tardy job specifications - which state whether individual jobs are scheduled earlier or later than requested. Search techniques can then be applied using an early/tardy job specification solution space rather than the sequence of jobs solution space. Experiments found that the former was more efficient.

The properties of the common due date problem are generalised for the distinct due date problem so that a distinct due date schedule can also be defined from early/tardy job specifications, however, this is only possible with the use of a heuristic. By incorporating early/tardy heuristic into a search, the early/tardy job specification solution space can be used in a search technique. Two early/tardy heuristics are developed, one being a construction based heuristics, the other based on a principle of conflict resolution. Both of these heuristics, unlike any other heuristic for this problem, determine the sequence and timing of each job simultaneously.

Experiments demonstrated that the early/tardy heuristics could find good quality solutions, however, search techniques based on the standard sequence of jobs solution space found better solutions. On larger sized problems, searches based on the early/tardy heuristic made large initial gains but little subsequent progress. Hybrid techniques which used both the heuristic solution space and the sequence of jobs solution space were found to produce the best results. Of the search techniques, Tabu Search was found to consistently produce better results than Simulated Annealing and Genetic Algorithms. Experiments also found that a Tabu Search with an early/tardy heuristic-based diversification strategy was the least sensitive to the starting point used by the search.

A final experiment found that if an error in the earliness and tardiness penalties is within the range of -25% and 20%, there is unlikely to be a significant difference in the solution quality.

ADVERTISEMENT

Graduate Research in Industry Fellowship

An optimization model for the telecommunication and video network

Supervisor: Dr A. B. Philpott, University of Auckland

We are seeking applicants for a Graduate Research in Industry Fellowship which has been awarded jointly to Telecom NZ and the Department of Engineering Science, for study towards a Masters degree in Operations Research at the University of Auckland. The successful applicant will be expected to spend 50% of the time in the offices of Telecom Network Modelling Group in Auckland and the balance at the University. The stipend for this award is $18,943 per annum.

The proposed research project is focused on the development of models for deploying fibre-optic cable,

coax video cable and copper cable in local access networks. The different technologies require slightly different approaches owing to the different possibilities of splitting and/or multiplexing signals. Locating the splitting or multiplexing devices and determining the layout of cable is a challenging integer linear programming problem, although some progress has been made in solving this problem using Lagrangean Relaxation. If they can be solved efficiently then such models are a valuable tool since they can be used to explore the optimal capital investment strategy for many different technology options.

This project will involve the development of a computer program based on Lagrangean Relaxation to solve capital provisioning problems for telecommunications networks. A student contemplating this project will enrol for a Masters degree in Engineering Science and so should therefore have an undergraduate degree in Engineering Science or Operations Research, with expertise in linear and integer programming. The student will be expected to take three advanced papers in OR, with at least one in mathematical programming, and will be expected to write code in Fortran or C, and so he/she should be proficient in either of these languages.

Students interested in applying for this award should forward a copy of a curriculum vitae and academic transcript, with the name of one academic referee, to

Dr A. B. Philpott
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so as to reach him no later than February 9, 1995. Any questions about this award can be addressed to Dr Philpott at the above address, or email *<a.philpott@auckland.ac.nz>*, or phone: 64 9 3737599, ext. 8394, or Fax: 64 9 3737468.

MEETINGS CALENDAR

CALL FOR PAPERS
ORSNZ 32nd Annual Conference
Thursday/Friday 29-30 August 1996
University of Canterbury, Christchurch, NZ
Deadline for abstracts (maximum one page A4 10-pt): **1 May 1996**
Deadline for summary paper (maximum six pages A4 10-pt, incl. tables, figures, and references [*APORS style*]) in Word or WordPerfect electronic file (not e-mail): **1 July 1996**
Note: Send abstracts and summary papers to the conference secretary. Pre-conference proceedings will include all summary papers submitted by 1 July or abstracts. Please follow the page format and style of the 1995 proceedings for both the abstract and the summary paper.
Conference secretary: Dr Ross James, Dept. of Management, University of Canterbury, Private Bag 4800, Christchurch NZ
Fax: 64 3 3642020; e-mail: *r.james@mang.canterbury.ac.nz*

INFORMS Spring Meeting
5 - 8 May 1996
Washington D.C.: Washington Hilton and Towers
Abstracts: 7/10/1995
Information: T. R. Gulledge Jr., George Mason University, Fairfax VA 22030-4444
FAX 001 703 764 4692
e-mail: *gulledge@gmu.edu*
Note: same venue/time for Joint Conference of Information Systems and Technology (abstracts 1/9/95) **and Analysis to Support Public Sector Decision Making**

1996 Asia Pacific Decision Sciences Institute Conference
21-22 June 1996
Hong Kong: The Hong Kong University of Science and Technology, East Kowloon
Abstracts: 4-5 double-spaced pages of full paper, plus a title page with authors, affiliation, complete addresses, phone/fax/e-mail of corresponding author, by 1 February 1996; notification of acceptance by 1/4/96; full paper by 1/5/96
Information: Kar Yan Tam, Information and Systems Management
HKUST, Hong Kong
e-mail: *apdsi@usthk.ust.hk*

4th European Conference on Information Systems
2-4 July 1996
Lisbon, Portugal
Complete papers: in the style of European J. of Information Systems of no more than 5000 words, by 30 November 1995
Information: Prof. J. Dias Coelho, ISEGI, New University of Lisbon,
Tv. Estevao Pinto, 1070 Lisboa, Portugal
Fax: 351 1 387 2140
e-mail: *ECIS96@EPSYLON.ISEGI.UNL.PT*

1996 IFORS Conference in Vancouver, B.C.
8 - 12 July 1996
Venue: Hyatt Regency, Vancouver
Conference theme: OR bridging the theory and practice of decision making
Deadline for abstracts: 31 October 1995
Format: submit three copies, single space, paper title, 50 word abstract limit, author's name(s), full mailing address, presenter. Include abstract fee of CAD$100, payable to IFORS 96, by cheque, VISA, or Mastercard.
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OR 38, Warwick
3 - 5 September 1996
Warwick Business School, University of Warwick, Coventry CV4 7AL
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e-mail: *orsrd@warwick.ac.uk*

CONTINUED PUBLICATION OF ORSNZ NEWSLETTER IN JEOPARDY

You will have noticed that this issue has only **eight** pages! **Why?** Because **YOU** — yes, I mean you who is reading this (there is nobody else behind you!) — have failed to contribute to it. You have not sent me your reasoned comments, opinions, or analysis of issues that concern the OR profession, such as the practice or the teaching of OR. You were too slack to write up that interesting piece of consulting, suitably disguised to hide your client, that you recently completed. You were afraid to share with us some bright new idea for a theoretical or applied research project that got you all excited. You did not feel it important enough to let your colleagues in on your experience with some useful or useless software package you played with. You neglected to organize your impressions on a recent OR/MS related text that you liked or that you advise others not to waste their time on. So, come May you may suddenly discover that the April issue did not arrive and that all you received was a short obituary, lamenting the passing of the Newsletter. Maybe in due time some other sucker will try to squeeze some blood out of a seemingly lifeless membership. Did I make you mad? Good! Send me an e-mail with your comments. But make it at least 200 words long, please! You will see yourself in print!

Hans Daellenbach, (current) editor