

Editorial

## Editorial

I B Pless

### From the editor's desk

#### PAGE CHARGES: AN UNWELCOME NECESSITY

It is with considerable regret—and not without some disagreement among the Editorial Board—that we announce the initiation of page charges for authors. Beginning 1 January 2004, authors will be asked to pay \$150US for each complete printed page. This decision was only reached after months of deliberation and is a reflection of the economical realities of journal publishing in the 21st century. In this case, it reflects in part our well intentioned but, in retrospect, misguided venture to make the complete journal accessible online at no cost. Not surprisingly, many previous subscribers succumbed to simple economics. The result was a loss of vital income for a journal that does not attract advertisers or other sources of income.

The financial consequences were such that, quite understandably, our publishers reassessed the situation. Among the various scenarios, one element that held promise for balancing the books was page charges. This was agreed with two important exceptions: the first is an exemption for readers in low income countries. They will continue to receive

this and all other BMJ journals online at no cost. The second, perhaps more controversial, is a waiver of charges for authors who subscribe to the print or online editions.

This makes sense for two reasons. The goal of any contributor is to see the product of their work in print, preferably in respected and widely read journals. Whether they themselves read such journals regularly, if at all, is seemingly secondary. Yet, when I teach students how to write for publication, I stress the immense value of being totally familiar with their target journals. At the very least I insist they strictly abide by the instructions for authors. Little alienates editors more than receiving a paper that has been prepared for another journal without the necessary adaptations for the journal to which it is eventually submitted. The benefits of familiarity with the journal of choice, preferably as a regular reader (that is, subscriber), should be evident. Hence this concession may ensure submissions better tailored to our readership as well as our structural and stylistic requirements, and

#### BRIDGING FROM RESEARCH TO PRACTICE

This topic has arisen repeatedly over the past 10 years and with good reason. It is one of the great unresolved issues, not just for injury prevention researchers but for almost everyone in applied research. I will not, therefore, apologize for raising the question again: what needs to be done to ensure that solid research findings are put into practice? In a guest editorial Moller (p 2) suggests that one step is for journals such as this one to become more flexible in its interpretation of scientific standards so that more community based interventions, flawed though they may be, can be published. He makes some important points and in fairness to ourselves, we have moved far in the direction in which Moller is trying to push us. We certainly agree that one of the reasons why it is so difficult to cross the bridge is because

essential planks are missing. What works in the artificial settings where efficacy studies are conducted will not necessarily work equally well, if at all, in the real world where effectiveness studies are performed. Moller's point is that those studies, especially if the unit of intervention is a community, are likely to be so flawed that reviewers will object strongly to their publication. This is what makes being an editor so much fun!

These issues aside, there are other matters to consider. I mentioned previously a set of papers in the *American Journal of Public Health* that caught my attention and admiration. One of these is highly pertinent to the question of "translational research" (a term used differently by investigators in different fields). This paper, by Glasgow, Lichtenstein, and Marcus asks: Why don't we see more translation of health promotion research to practice? Rethinking

simultaneously attract more subscribers because it is by far the more appealing financial option.

The page charge decision was not made lightly and we wish we did not have to make it at all. But it is now widely acknowledged that the business of publishing journals has become increasingly challenging.<sup>1</sup> Publishers are not charities and ours has been more charitable than most. There are limits, however, and the loss resulting from the free online meal could not be sustained indefinitely. The economics of the choice for authors should be compelling; even a subscription to the print edition of \$150 per year for six issues is much less costly than the \$600 a four page paper would entail. The web-only subscription at \$40 per year is an unparalleled bargain; we urge aspiring authors and all readers to take advantage of it.

By way of a benchmark of sorts, Potter notes that most Elsevier journals charge between \$1000 to \$6900 for one year institutional subscriptions and the organic chemistry journal, *Tetrahedron*, is a snip at \$20 763. The Public Library of Science intends to charge authors \$2000 to cover reviewing and editing of articles. John Hoey, the editor of the *Canadian Medical Association Journal* estimates it costs about \$5000 "to edit and illustrate an article and get it online".<sup>1</sup> Put that in your pipe and smoke it, and while you do, ponder our generous proposal!

#### REFERENCE

- 1 Potter S. Steep subscriptions. *Medical Post* 23 September 2003.

the efficacy-to-effectiveness transition.<sup>1</sup> I urge readers concerned with this topic (and most should be), to read the original because my précis will fail to do justice to the authors' ideas.

In brief, they make some of the same points as Moller, but add others that they believe lie at the heart of the matter: "the logic and assumptions behind the design of efficacy and effectiveness research trials". They note that the widely held belief that it is the products of successful efficacy studies that are the best candidates for effectiveness and dissemination studies may be entirely wrong. Their argument is situated in a framework of five phases of intervention research proposed by Greenwald and Cullen.<sup>2</sup> It focuses on the nature of phase 4—effectiveness studies intended "to measure the impact of an intervention when it is tested within a population that is

representative of the intended target audience". One key element is generalizability to intended program users, followed by large scale implementation.

As the commentary notes, however, this seemingly logical sequence often fails to materialize and truly successful effectiveness trials are scarce. Thus, the *American Journal of Public Health* paper argues that the model is flawed in part at least because researchers engaged in each phase have distinctively different values and methods. They suggest a new model to give "balanced emphasis to internal and external validity". For details of the model, RE-AIM, see the original paper.<sup>1</sup> It concludes with four recommendations aimed at researchers, editors, and funding bodies, and I quote these verbatim, as follows:

1. Researchers should pay increased attention to moderating factors in both efficacy and effectiveness research.
2. Realize that public health impact involves more than just efficacy.
3. Include external validity reporting criteria in author guidelines.
4. Increase funding for research focused on moderating variables, external validity, and robustness.

Each of the recommendations includes a detailed list of concrete suggestions. We all need to consider these fully and move rapidly to adopt those we agree with. This is required reading for everyone who shares the growing concern that too many measures that

have been shown to be capable of reducing injuries fail to reach the bedside or the communities for which they are intended.

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- 2 Greenwald P, Cullen JW. The new emphasis in cancer control. *J Natl Cancer Inst* 1985;74:543–51.

## Community based interventions

# Reconsidering community based interventions

J Moller

## Balancing rigor and the real world

The systematic review of the literature "Community based programs to prevent poisoning in children 0–15 years" that is published in this issue prompted a debate between reviewers and authors (see p 43). This process revealed a number of important issues that warrant further analysis and comment. In this editorial I try to identify these issues to provide a foundation for further action.

The reviewers raised two basic questions. One suggested that the paper be rejected because it only found four articles of adequate scientific merit. The authors replied that a paper's merit should be judged on the technical competence of the review and not on the results. They argued that a decision based on the number of articles found would amount to a publication bias against new or poorly funded fields.

The second issue concerned the notion of community based intervention. It was suggested that this field did not have sufficient coherence or precision to make it possible to generalise interventions. In response, the authors noted that the characteristics of community based models are well known. They include shared ownership of a

problem and its solution by community members as well as experts. They pointed to the need for "a distinction between efficacy trials of specific counter-measures where the individual is the unit of study vs effectiveness trials involving community interventions". While locked cabinets may be efficacious, community based programs that aim to reduce poisoning by encouraging widespread use of lockable cabinets are not necessarily effective. Because making a difference at the population level is the focus of injury prevention efforts we need to move from "what works" to "how to make it work on a large scale".

The to and fro between reviewers and authors raised a number of issues. Two are at the tip of an iceberg of debate about theory and method. As is true of many previous debates in public health, the arguments are coloured by values and preconceptions; in this case, about what constitutes evidence and what constitutes an intervention. Until this is resolved research involving more recently established fields of study that do not fit classical research paradigms may be barred from publication.

Rather than becoming a third party in the original discussion I want to try to

clarify some of the underlying issues. In 1991 I wrote an article to define the characteristics of community based injury prevention.<sup>1</sup> Nixon *et al* used my definition: "The community-based model for injury prevention is an explicit approach to achieving reductions in the incidence of injury at the population level by application of multiple countermeasures, and multiple strategies in the context of community-defined problems, and community-owned solutions". In other words, a community based strategy is not a specific single intervention, but a set of processes to facilitate effective implementation of one or more interventions. In each community setting the intervention may differ because communities differ, as does the nature of the problem.

The focus of community based injury prevention is to develop a systems response that matches prevention strategies to the cultural, social, and political setting. A population outcome is the goal. Such a broad focus means that strict control of intervention, subject, and analysis required for a true experiment or clinical trial, is impossible. This sends shudders down the spines of those brought up in the empirical tradition and it is tempting to write off community based interventions as too unstable, too difficult to evaluate, and too hard to replicate.

Other public health systems with more traditional professional affiliations might be considered in a similar vein but are less questioned because of their connections to core clinical fields. It appears that we are not yet ready to deal with the outcomes of complex systems as a subject of our research and that provenance rather than science may be

the basis of choosing what is questioned and what is not.

The reviewer's response to Nixon *et al* implied politely but scathingly that community based programmes have neither form nor substance. Their contribution is also questioned because of their diversity, because they lack rigorous evidence of effectiveness, or because it is more difficult to generalise from the results.

The fundamental question for community based injury prevention is "does it work in the real world?" If the answer is "yes" then we must develop evidence about what factors are necessary for success to be replicated. Unlike clinical treatments, replication is based on principles rather than rigid prescriptions.

- How do we classify, define, and compare the different approaches used?
- How do we define outcomes and at what level should these outcomes be measured?
- How do we choose problem identification, solution selection, and implementation strategies?
- How do we identify and measure any general or synergistic effects between multiple interventions in the same community?

Nixon *et al*'s paper only addresses the first broad question but opens the way to other issues. The authors found few high quality studies linking community based programs to the desired outcomes. This is different from evidence of no effect, and their conclusion, quite correctly, is that more research is needed. In contrast, the reviewers raised questions about clarity of definition but instead of considering how they might be addressed, dismissed the field as unworthy. We must be clear about the different levels and types of evidence for community based interventions and understand the reasons for both the lack of clarity and of evidence. These are threefold: (1) An agreed language and concept structure has yet to emerge in this complex environment. (2) The settings in which community based prevention take place are not easily

controlled. (3) Community based interventions are rarely funded to a level that supports rigorous evaluation.

Evaluation of community programs moves out of the comfort zone of traditional research designs. The research and evaluation questions that need to be asked are different and the methods are not those that have become the gold standard for the evaluation of individually based interventions. Currently acceptable methods focus on experimental, quasiexperimental, or case control methods. The requirements for these methods are hard to meet for community based programs. A true experiment, in which the unit of intervention is the population, would require randomisation of populations. This is financially impractical but not theoretically impossible. A community intervention with a matched community control is far more feasible but still challenging because, unlike individual, communities vary widely in characteristics related to exposure to risk. Even where matching populations are found the final comparison comes down to a single case with control design and a critical reviewer can easily dismiss results. The use of time series designs has also been used. These are useful, providing there are long term stable patterns of incidence in more than one community.

It is rare to find a situation where there is sufficient funding, expertise, and multiple communities available to use accepted methods with sufficient rigour to produce high quality results. This means that reviewers are likely to advise journals not to publish the results. But failure to meet rigorous standards is not due to an inherent lack of expertise or professionalism; it simply reflects the nature of the field and the state of play of research methods. The rare circumstances where the criteria for rigorous evaluation can be met tend to occur in affluent communities and where the interventions are limited and carefully controlled. The Holy Grail is sufficient understanding of the dynamics to be able to effectively apply the methods in any setting, including those with few material resources. The methods used in community based

evaluations could change so that these studies can result in publications that do not encounter the usual criticisms. Unfortunately, this would mean that the principles of cooperation, multiple intervention, and adaptation that lie at the heart of these interventions would be undermined.

Research methods are required that allow us to better judge what mix of strategies work with which populations so that communities can select effective prevention strategies. A broad based child poisoning prevention strategy needs to be fine tuned to the range of exposures in the community, to the education of parents and professionals, the attitudes of government, and local service delivery networks. Research needs to address all these issues.

The literature must stimulate discussion on the definitions, values, and principles of community based prevention. It should promote debate on the definition of boundaries and scope and the measurement of efficacy and effectiveness. It should promote the development and testing of new methods and report on the progress made. Above all, it should report on best available practice with critical and insightful comment on problems with methods and conclusions.

This means that at times the literature will need to include reports with insufficient evidence and reports of the results of studies where theory suggests that sample sizes, control groups, and data integrity are less than ideal, but are the best possible. Learning in public health is best promoted by the critical sharing of evidence, not by the censorship of evidence that is less than perfect.

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