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<th>Non-attendance at outpatients departments. More information was needed for non-UK readers</th>
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<td><strong>Author(s)</strong></td>
<td>Castan-Cameo, S; Johnston, JM; McGhee, SM</td>
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<tr>
<td><strong>Citation</strong></td>
<td>BMJ (Clinical Research Ed.), 1999, v. 319 n. 7217, p. 1134; author reply 1135</td>
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<tr>
<td><strong>Issued Date</strong></td>
<td>1999</td>
</tr>
<tr>
<td><strong>URL</strong></td>
<td><a href="http://hdl.handle.net/10722/53527">http://hdl.handle.net/10722/53527</a></td>
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We suggest that using appropriate treatment to prevent iatrogenic exacerbation of a disease that is distressing, disfiguring, and difficult to treat is entirely consistent with Ahlquist’s philosophy of first do no harm.

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Non-attendance at outpatient departments

More information was needed for non-UK readers

Editor—The trial by Hamilton et al1 examining the effect of giving patients a copy of their referral letter on non-attendance at outpatient departments raises several interesting and controversial issues, but it is difficult to assess for an international audience who may not be familiar with the British healthcare system. This issue of being international has been raised by others,2 and surely if the BMJ aspires to be an international journal, the research setting needs to be clarified for international readers.

For example, what is the usual referral procedure in the United Kingdom? In Hong Kong a referral letter is always given to the patient and is required for access to secondary care. Patients make their own appointments. Hamilton et al also fail to establish the justification for the research under discussion.

What was the rationale for this randomised controlled trial? They hypothesise that a lack of communication between the patient and the referring doctor is the cause of non-attendance. If so, how will a copy of the referral letter be expected to improve this communication or guarantee attendance? An explanation of the topic antecedents and justification of the research question are required together with a discussion of the cost implications of this intervention.

Although Hamilton et al quote a national (England or United Kingdom) non-attendance rate of 12%, their own study had a much lower non-attendance rate. The situation in Hong Kong is very different, despite patients receiving a referral letter, and such low rates would be welcomed in Hong Kong. The authors offer no explanation to account for the difference between the study and the national non-attendance rates. This may be due to the study selection criteria. Excluding patients because of severity of disease, previous suboptimal care, or patients’ attitude or lifestyle may have biased the sample and led to an incorrect estimate of the non-attendance rates. The intervention was intended to reduce non-attendance and it did not target the appropriate population, the non-attenders. As the authors have not specified the reasons for patients’ non-attendance, the reader does not know whether the intervention is appropriate.

The BMJ is a widely read journal, and to reach an international audience enough information should be provided to facilitate the assessment of the research and its potential for application elsewhere.

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Key messages did not accurately summarise the study

Editor—We should like to suggest two additional “key points” for the paper by Hamilton et al concerning hospital attend ance rates. Firstly, the setting may affect the ability of a randomised controlled trial to produce valid results, and, secondly, the BMJ key messages boxes may not provide a reliable summary of the data contained in the paper.

Non-attendance at hospital outpatient departments wastes resources, frustrates staff, and may result in unmet health needs. However, chance, human nature, and the complexities of modern life make it unlikely that 100% attendance will ever be achieved, whatever measures are used. The authors quote a national non-attendance rate of 12% and studies showing a range from 9.9 to 29%.3 Yet only 6% of the patients in their pilot study did not keep their hospital appointments; this level is so low that we wonder whether it is possible to reduce it further.

The first key point in the box asserts that copy letters do not reduce non-attendance at hospital outpatient departments. This has been demonstrated in an area where non-attendance was already half the national average, but we know nothing about the effectiveness of the intervention elsewhere. If the underlying hypothesis about the relation between information sharing and non-attendance is true, the low rate in their area may reflect doctor-patient communication that is already optimal. Influences on attendance may also vary as the rates change, and qualitative research to generate further hypotheses is probably needed.

The second key point states that the concept of copying letters to patients is acceptable to doctors and patients. The perceived acceptability of the copy letter was investigated in a questionnaire sent to participating practices, and all were prepared to send copy letters if they were shown to be beneficial and the cost could be offset. Ten of the 13 practices received positive comments from patients. However, we do not know how many patients...
commented, and as the patients were not approached directly these views may not be representative. The third key point, that it may be possible to apply interventions from primary care to reduce non-attendance, is intuitive but cannot be deduced from any of the data presented.

This study used a practical intervention to address an important problem, but we would like to see it repeated in a different setting before the copy letter is dismissed as ineffective.

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1 Hamilton W, Round A, Sharp D. Effect on hospital attend-
ance rates of giving patients a copy of their referral letter: randomised controlled trial. BMJ 1999;318:1392-5. (22 May.)
2 Andrews R, Morgan JD, Addy DF, McNicoll AS. Understanding non-attendance of family and friends. We expected increased attendances, with their general practitioner or family and friends. The patients were not satisfied with the process. Finally, we consider that the trial did establish the feasibility of applying interventions from primary care to reduce non-attendance because this is what we actually did. It is a pity that it did not work.

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Authors’ reply

Dear—Castan-Cameo et al comment that international readers may be unused to the United Kingdom's system of referral. In brief, a referral decision is made between patient and general practitioner; the general practitioner writes to the hospital consultant, and an appointment is sent to the patient by post from the hospital. Our hypothesis was that offering information to patients would perhaps allow them to make a more informed decision on the value of attending. The written summary that the copy letter provided should have allowed patients to reflect on their condition, perhaps increased their understanding, and given an opportunity for further discussion with the general practitioner or family and friends. We expected increased attendances, increased cancellations, and reduced non-attendances.

Both Castan-Cameo et al and Lawlor and Hanratty must have missed our first sentence, which stated that the 12% figure included new and follow-up appointments; follow up appointments have a higher non-attendance rate,1,2 but our study only targeted new appointments. They are the appointments most influenced by the general practitioner. It may prove impossible to reduce new patient non-attendance from 6%, but at £65 per appointment1 even small reductions are worth while. Castan-Cameo et al wonder if the low non-attendance rate was due to exclusions; not so, only 117/2078 (5.6%) were excluded attendance rate was due to exclusions; not only 117/2078 (5.6%) were excluded. Castan-Cameo et al wondered if the low non-attendance rate was due to exclusions; not only 117/2078 (5.6%) were excluded.

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Lawlor and Hanratty consider our key points unrepresentative of the paper. However, it is incorrect to say that the setting affects the ability of a randomised controlled trial to produce valid results. Validity depends on the design and conduct of a trial. Perhaps what they intended to say was that the results of a trial in one setting may not be applicable in another—we agree with this. Our study was representative of patient behaviour in one geographical area. Although the trial was not primarily designed to establish acceptability, this having been tested in the pilot study when patients were directly approached,3 our study confirmed that patients and doctors seemed satisfied with the process. Finally, we consider that the trial did establish the feasibility of applying interventions from primary care to reduce non-attendance because this is what we actually did. It is a pity that it did not work.

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Anaesthetists need consent, but not written consent

Dear—Dobson’s article concerning information and consent for anaesthesia appeared under the headline “Anaesthetists do not need separate consent before surgery,” and stated that “New guidelines on obtaining consent for anaesthesia recommend that consent from patients specifically for a general anaesthetic is not needed.”5 These statements are incorrect, and I believe that they may mislead readers.

The guidelines issued by the Association of Anaesthetists of Great Britain and Ireland state that express consent should be obtained for any procedure which carries a material risk. The working party noted that they may mislead readers. The working party believed that discussing the risks of delivering health care to patients and the safety statistics of airline travel,1 with air travel being over 10 000 times safer for the passenger than medicine for the patient. Although nobody doubts the importance of designing safer healthcare systems that reduce adverse effects, serious drug errors, etc, this comparison is fundamentally flawed. It is not simply because old aeroplanes are grounded before they fall out of the sky. More importantly, if you want to compare health care with aviation then like should be compared with like—that is, care of the patient with the aeroplane itself and not with the individual passenger. If a separate team looked after each patient or, conversely, if the team flying the aeroplane flew twenty or more planes simultaneously, as is the case with patient care in hospitals, safety indicators of these two different fields would be closer. Problems with air controllers over London, a recent hot topic in the media, also illustrate this. On the other hand, in surgery the presence of a well staffed high dependency unit reduces complication rates: where funds are available for an increased number of trained staff to look after patients then complications and presumably the rate of adverse effects are reduced.7

Further research is necessary. In the meantime superficial comparisons worthy of a tabloid newspaper than the BMJ are best avoided; they may harm patient care by obscuring important contributing factors to current difficulties in delivering health care.

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Risks of medicine and air travel

Dear—Berwick and Leape draw parallels between the risks of delivering health care to patients and the safety statistics of airline travel,1 with air travel being over 10 000 times safer for the passenger than medicine for the patient. Although nobody doubts the importance of designing safer healthcare systems that reduce adverse effects, serious drug errors, etc, this comparison is fundamentally flawed. It is not simply because old aeroplanes are grounded before they fall out of the sky. More importantly, if you want to compare health care with aviation then like should be compared with like—that is, care of the patient with the aeroplane itself and not with the individual passenger. If a separate team looked after each patient or, conversely, if the team flying the aeroplane flew twenty or more planes simultaneously, as is the case with patient care in hospitals, safety indicators of these two different fields would be closer. Problems with air controllers over London, a recent hot topic in the media, also illustrate this. On the other hand, in surgery the presence of a well staffed high dependency unit reduces complication rates: where funds are available for an increased number of trained staff to look after patients then complications and presumably the rate of adverse effects are reduced.7

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