Table 1 Risk of breast cancer with hormone replacement therapy (HRT) in various studies

Study	Category	No of cases	Relative risk (95% CI)
Wingo *et al*, 1987[4]	Ever users:		
	Oestrogen for 75-99.9 months	32	1.9 (1.1 to 3.3)
	Ever users without ovaries:		
	Age 50-54 years	165	2.0 (1.2 to 3.2)
	Oestrogen for 25-49.9 months	41	1.9 (1.1 to 3.4)
	Oestrogen for 75-99.9 months	21	2.4 (1.1 to 5.2)
Hunt *et al*, 1987[5] and 1990[6]	Ever users:		
	HRT	50	1.59 (1.18 to 2.1)*
Bergkvist *et al*, 1989[7]	Ever users:		
	Oestrogen + progestin	10	4.4 (0.9 to 22.4)
	Oestradiol 73-108 months	17	2.3 (1.2 to 4.3)
	Oestrogen ≥109 months	29	2.3 (1.1 to 4.8)
Persson *et al*, 1992[8]	Ever users:		
	Oestrogen + progestogen	14	1.3 (1.1 to 1.6)
	Oestrogen + progestogen 7-11 years	NA	1.6 (1.1 to 2.1)
Stanford *et al*, 1995[9]	Ever users with ovaries removed:		
	Oestrogen + progestin	17	19.0 (1.8 to 199.4)
Folsom *et al*, 1995[10]	Current users:		
	Any HRT	95	1.24 (0.99 to 1.56)
	HRT ≤5 years	NA	1.45 (1.03 to 2.06)
Colditz *et al*, 1995[11]	Current users:		
	Conjugated oestrogens	270	1.32 (1.14 to 1.54)
	Oestrogen + progestin	111	1.41 (1.15 to 1.74)
	Progestin only	12	2.24 (1.26 to 3.98)

NA=Not available.
*Figures for 1987; incidence not given for 1990; mortality doubled between 1987 and 1990.

replacement therapy increases primary venous thromboembolic disease.[3]

Findings of an increased risk of breast cancer in large, well conducted studies are not cancelled out by the existence of apparently negative studies. In fact, all of the studies quoted in the responses found increased relative risks with exposure to hormone replacement therapy, either overall or in subgroups, ranging from 1.3 to 19 (table 1). Any increase in a common and fatal disease affects large numbers of women. There are about 30 500 new cases of breast cancer in England and Wales and 12 500 deaths annually. Even a 30% increase means several thousand extra cases and premature deaths each year. Would these women be happy with the explanation of a "small acceptable risk"?

When exogenous hormones are given as contraceptive pills an increase in breast cancer is no longer denied. Eleni Hemminki's editorial about the reanalysis of 54 combined studies by a collaborative group lists many reasons why the increases (24% for current users and 59% for those starting the pill before age 20) could be underestimates.[12] The median duration of use was only three years and the median starting age 26. Today, women are exposed from much younger ages.

Should not women be warned of the dangers? Warnings about the contraceptive pill in the 1970s were followed by pronounced falls in registrations of breast cancer for a decade among the specific age groups involved. This was against the prevailing trends.[13] Overall, data from the Office for National Statistics show large increases for England and Wales. For women now aged 55-64 the incidence of breast cancer has more than doubled, increasing by 137% between 1962 and 1991. These women were aged 25-34 in the 1960s, when they became the first generation to be exposed to the contraceptive pill.

Doctors must now review whether it is defensible or ethical knowingly to give healthy women contraceptive or menopausal hormones when increases in breast cancer, ovarian cancer,[14] and thrombosis have been shown in reputable studies. Even any long term benefit in the prevention of fractures by hormone replacement therapy is being questioned.[15] These concerns will be discussed at a meeting of DASH (Doctors Against Abuse from Steroid Sex Hormones) in London next June.

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1 Use of hormone replacement therapy [letters]. *BMJ* 1996;313:686-7. (14 September.)
2 McPherson K. Breast cancer and hormonal supplements in postmenopausal women. *BMJ* 1995;311:699-700. (16 September.)
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11 Colditz GA, Hankinson SE, Hunter DJ, Willett WC, Manson JE, Stampfer MJ, *et al*. The use of estrogens and progestins and the risk of breast cancer in postmenopausal women. *N Engl J Med* 1995;332:1589-93.
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Effect of psychogeriatric team on depression in frail elderly people at home

More information is needed on subjects and interventions in study

EDITOR—Sube Banerjee and colleagues suggest that intervention by a psychogeriatric team produces a better outcome than standard care by a general practitioner.[1] Discussion of their paper in our journal club, however, raised several questions.

Firstly, from the methodological description in the paper it is not clear whether the general practitioners of the patients and the controls were told that the patients had fulfilled the criteria for caseness for depression or whether they were simply told that the patients were taking part in a controlled trial, with no further details given. This distinction is important for readers to ascertain whether the study simply compared interventions in a group who had been identified as depressed or also included effects of detection or non-detection.

Secondly, we thought that further data on the interventions in the control group should have been included. It could be argued that it was not the individual interventions themselves but their coordination by a multidisciplinary team that was important in terms of the eventual outcome. It could also be argued that the low rate of use of antidepressants in the control group at follow up was due to the fact that in primary care many patients prescribed antidepressants receive prescriptions of short duration.[2]

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1 Banerjee S, Shamash K, Macdonald AJD, Mann AH. Randomised controlled trial of effect of intervention by psychogeriatric team on depression in frail elderly people at home. *BMJ* 1996; 313:1058-61. (26 October.)
2 MacDonald TM, McMahon AD, Reid IC, Fenton GW, McDevitt DG. Antidepressant drug use in primary care: a record linkage study in Tayside, Scotland. *BMJ* 1996;313:869-1. (5 October.)

Results may have been due to intervention by a specialist

EDITOR—Sube Banerjee and colleagues report the effect of intervention by a psychogeriatric team on the outcome of depression in frail elderly people at home.[1] The overall design of their study was good, particularly as this is a difficult area to research. Our main criticism concerns the methodology, because the intervention used was not the usual team intervention. The subjects were all allocated to a senior psychiatrist as the keyworker, which is not usual practice and is not the team's usual philosophy. Might not the perceived benefits have been a result of medical intervention by a specialist? The keyworker was not blinded and would have been highly motivated and highly skilled. It cannot be concluded that the study showed the effect of the team. The study would have been better designed if the patients had been allocated to a range of keyworkers within the team, as is the team's usual practice.

Although the study suggests that antidepressants did not alone account for the difference between the patients and controls, its design does not allow the degree of effect resulting from any single component of the intervention to be determined. There was a fivefold difference in the rate of prescribing of antidepressants. A similar study investigating intervention by a community psychiatric nurse did not show such a pronounced effect, and, interestingly, the rate of use of antidepressants in that study was low.[2] A more detailed study investigating the effective components of intervention by a team is required. The response shown may reflect lack of access to care, as the usual practice of both primary and secondary services is to take a passive stance rather than be proactive. This was a proactive study, and the benefits produced may reflect this. The result might have been seen with any type of intervention given.

Although the paper concludes that referral to a specialist team is helpful, it also raises the issue of how the most vulnerable people in our population gain access to treatment for depression, whether from a general practitioner or a specialist team. Perhaps psychogeriatric services should be encouraged to target those people identified by social services as being most vulnerable, as the benefits of this have been shown in this paper. How can those with depression be identified and treated? Would more liaison and training with social services be helpful? What are public health measures such as the defeat depression campaign achieving in this area?

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2 Blanchard MR, Waterreus A, Mann AH. The effect of primary care nurse intervention upon older people screened as depressed. *Int J Geriatr Psychiatry* 1995;10:289-98.

Authors' reply

EDITOR—Roger Walters and Hilary Evans ask whether the general practitioners were informed that all the subjects fulfilled the criteria for caseness for depression. They were. Thus the control group received normal general practice care apart from the fact that the general practitioner had been told that the subject was depressed; this suggests that simply providing information is insufficient to change behaviour. Walters and Evans's second point concerns the intervention. The main aim of the research was to investigate whether these people could be successfully treated rather than to evaluate individual components of the management package. We agree that the multidisciplinary approach is likely to have led to optimum efficacy. No control subject was started on an antidepressant and then stopped it during the study. Our observations are unlikely to be a function of the duration of the prescription.

Gillian Pinner and colleagues suggest that the intervention differed in practice and philosophy from normal intervention by the team. We acknowledged that the intervention group had a doctor as their keyworker; this was the only way in which management differed from normal team processes. Around two fifths of the patients managed by the team have doctors as their keyworkers, so such management is not atypical. Pinner and colleagues question whether the effect was due to medical intervention by a specialist. This was not the case. Only one patient in the intervention group was prescribed a drug by the team; the rest were prescribed drugs by their general practitioners and managed jointly with them, as is the team's practice. Drug treatment was reviewed at multidisciplinary team meetings, and physical reviews were completed by general practitioners or appropriate medical teams. The comparison that Pinner and colleagues make is flawed. The cases of depression in the study that they cite were derived from general population screening rather than a disabled population,[1] and the low rate of prescription of antidepressants was due to low take up of advice to prescribe by the general practitioners rather than differing psychiatric practice.

Pinner and colleagues comment on the proactive nature of our study. An important finding was that the team approach was acceptable to depressed disabled elderly people identified through screening rather than through their seeking help; we achieved a high level of compliance with antidepressant treatment. We are glad that Pinner and colleagues agree with the need to develop and evaluate screening and management packages for primary health care and social services so that these patients' mental health needs can be better met.

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Plagiarism in the *BMJ*

Author should have acknowledged his source

EDITOR—In his article on the inadequacy of language Liam Farrell plagiarises an idea without crediting it to its originators[1]: Douglas Adams and John Lloyd have provided a book of definitions of many place names, *The Meaning of Liff*.[2]

"Droitwich" is defined as "A street dance. The two partners approach from opposite directions and try politely to get out of each others way. They step to the left, step to the right, apologise. Step to the left again, apologise again, bump into each other and repeat as often as unnecessary."[2] The suggestion that place names "loaf around on signposts" is taken directly from the introductory page of Adams and Lloyd's book.

I would have hoped that a journal such as the *BMJ* would work to eliminate such plagiarism.

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1 Farrell L. The unbearable inadequacy of language. *BMJ* 1996;313:1660. (21-28 December.)
2 Adams D, Lloyd J. *The meaning of liff*. London: Pan, 1983.

Author's reply

EDITOR—Firstly, I must thank Helen Vecht for the tullywinney. The basic concept of coining new words is not new and has been used by, among others, Lewis Carroll ("to gyre and gimble in the wabe") and James Thurber. The original version of my article acknowledged the debt to *The Meaning of Liff*, but when I requested permission from Douglas Adams's agent about this it was refused. I believe, however, that my article was the first to use this literary device to augment our lamentably limited medical vernacular.

Secondly, I didn't know about the Droitwich; I was quoting from Thurber, so this may be a case of great minds thinking alike. Cogging from one author is indeed plagiarism, but cogging from two counts as research—mandatory for all serious writers.

Thirdly, for loafing about the crossroads I have no defence; the phrase was obviously droitwiching about in my subconscious.

Tullywinney (n): the rush that a columnist gets from proof that somebody out there actually reads his stuff and is inspired enough to complain. He lay back and lit a cigarette; "Darling," he said, "That was almost as good as a tullywinney."

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