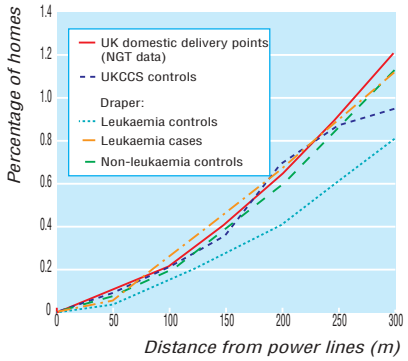
Letters

Childhood cancer and power lines

What do the data mean?

EDITOR—Draper et al used distance of mother's home from high voltage overhead transmission lines (predominantly 275 kV and 400 kV) at the time of her child's birth as a proxy for her child's subsequent exposure to power-frequency magnetic fields (reviewed by Ahlbom et al).[1 2] As they acknowledge, this is a crude estimate since, in contrast to other reports,[2] no household measurements were taken, no data on more prevalent low voltage distribution sources were collected, no information from other time points in the child's life was obtained, variations during the 33 years period studied were not considered, and no validatory home visits were carried out. A recent report into residential exposures to magnetic fields in the United Kingdom estimated that proximity to high voltage lines, 275 kV and above, explained only 9% of those with measurements ≥0.2 microtesla (μT).[3]

National data on the distribution of houses in relation to high voltage lines in the UK were provided (J Swanson, National Grid Transco, personal communication, 2000) to the UK Childhood Cancer Study (UKCCS) Group for its study of power lines and childhood cancer, to assess the representativeness of study subjects.[4] An equivalent comparison using National Grid data spanning the far longer period investigated in the paper by Draper et al was not undertaken. The assessments of distance to power lines in the UKCCS were made for all registered controls, who have been shown to represent the general population.[5]



Study by Draper et al—proportion of subjects living close to power lines. Comparison with national data supplied by National Grid Transco (NGT) and UK childhood cancer study (UKCCS)

A plot of the distributions of the leukaemia and non-leukaemia cases and controls in the study by Draper et al, national populations, and UKCCS populations by distance from high voltage lines (figure) seems to show that the leukaemia controls used in the analysis presented by Draper et al are systematically different. Their positive result over 100 m may therefore be explained not by an excess of cases but by a deficit of controls in the early years of the study.

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Competing interests: None declared.

1 Draper G, Vincent T, Kroll ME, Swanson J. Childhood cancer in relation to distance from high voltage power lines in England and Wales: a case-control study. *BMJ* 2005;330:1290. (4 June.)
2 Ahlbom A, Day N, Feychting M, Roman E, Skinner J, Dockerty J, et al. A pooled analysis of magnetic fields and childhood leukaemia. *Br J Cancer* 2000;83:692-8.
3 Maslanyj MP, Mee TJ, Allen SG, Investigation and identification of sources of residential magnetic field exposures in the United Kingdom childhood cancer study (UKCCS). www.hpa.org.uk/radiation/publications/hpa_rpd_reports/2005/hpa_rpd_005.htm (accessed 28 Aug 2005).
4 Skinner J, Maslanyj M, Mee TJ, Allen SG, Simpson J, Roman E, et al. Childhood cancer and residential proximity to power lines. UK Childhood Cancer Study Investigators. *Br J Cancer* 2000;83:1573-80.
5 UK Childhood Cancer Study Investigators. The United Kingdom childhood cancer study: objectives, materials and methods. *Br J Cancer* 2000;82:1073-102.

Results do not support causal role for electromagnetic fields

EDITOR—Draper et al present findings on the relation between childhood cancer and the distance of birth residence to high voltage power lines.[1] The study's strengths include the large number of case children and unbiased control selection. However, the findings are inconsistent with another UK study, in which neither proximity nor estimates of dose to extremely low frequency magnetic fields from power lines showed any relation with childhood leukaemia.[2]

The strength of the findings is based on trend statistics, with the reference group resident over 600 m from the lines. This has no sound scientific basis for inferring associations with extremely low frequency magnetic fields, as beyond 200 m their contribution to exposure can be considered to be "background."[3] No plausible biological evidence currently links magnetic field exposure to childhood leukaemia. Despite this, the paper quantifies the likely number of cases "associated" with high voltage lines where the main exposure is to magnetic fields.

The significant associations in this geographical analysis lack any adjustment for population characteristics except social class, and how this was done for births before the 1981 census is not described. Crucially, the area distribution of childhood leukaemia varies with population density and population mixing[4]; neither has been considered as potential confounders.

It is of interest that using all controls as the comparison group reduced the risk. Matched analyses may be preferred, but findings can be considered less conclusive if the estimates are noticeably different when matching is broken. All controls were selected to represent the population, and an investigation of why differences were observed is warranted.

The findings of this study point towards geographical correlates of risk for childhood leukaemia but do not support the hypothesis that electromagnetic fields have a causal role.

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1 Draper G, Vincent T, Kroll ME, Swanson J. Childhood cancer in relation to distance from high voltage power lines in England and Wales: case-control study. *BMJ* 2005;330:1290-2. (4 June.)

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Results depend on chosen control group

EDITOR—Given the large size of the study by Draper et al,[1] the risk estimates should be stable. Furthermore, because contact with the subject was not necessary, selection bias due to the differential participation among cases and controls as in previous studies has been avoided.[2]

We were therefore surprised by the dependence of the results on the chosen control group noted by the authors (who used the central nervous system and other cancer controls for leukaemia cases in one of the comparisons). To explore this further, we combined all controls into one group and used it for comparison. We thought this was justified on the basis of theoretical and empirical grounds: exposure at birth among controls chosen for leukaemia, brain tumours, and other cancers should not depend on the cancer subtype; crude odds ratios calculated by us did not differ (beyond the first decimal) from the matched results presented by the authors (data not shown).

Use of the combined control group showed a pattern that was different to the one presented in the original paper (table). As would be expected, results for all cancers combined show no relation to the distance. For leukaemia and brain cancer, results at two distances are noteworthy: for the 50-100 m category we observed an excess of leukaemia and a deficit for brain tumours. For the 500-600 m category we observed a modest excess for both leukaemia and brain tumours. The trend reported in the original paper is not present when the combined control group is used, which indicates that the trend depended on the leukaemia controls rather than on the leukaemia cases.

We agree with Draper et al that the results of this study do not support a possible association with magnetic fields, as has been reported by the International Agency for Research on Cancer.[3] However, distance is known to be a poor predictor of magnetic field exposure, and therefore the results of this material based on calculated magnetic fields, when completed, should be much more informative.

Further insight might be gained by details on the methods used for the control selection and sensitivity analyses by age, sex, and time period.

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1 Draper G, Vincent T, Kroll ME, Swanson J. Childhood cancer in relation to distance from high voltage power lines in England and Wales: a case- control study. *BMJ* 2005; 330:1290-2. (4 June.)
2 Ahlbom A, Day N, Feychting M, Roman E, Skinner J, Dockerty J, et al. A pooled analysis of magnetic fields and childhood leukaemia. *Br J Cancer* 2000;83:692-8.
3 International Agency for Research on Cancer. *IARC monographs on the evaluation of carcinogenic risks to humans. Vol 80: non ionizing radiation. Part 1: static and extremely low-frequency electric and magnetic fields.* Lyons: IARC, 2002. www-cie.iarc.fr/htdocs/monographs/vol80/80.html (accessed 28 Aug 2005).

Study had important omissions

EDITOR—The study by Draper et al omitted most cases near power lines of 132 kV.[1] The United Kingdom has 10 000 circuit km of 400 kV lines and 4000 circuit km of 275 kV lines, but 20 000 circuit km of 132 kV lines and more than 260 000 pole mounted transformers.[2] Should the effect be found near these lower voltage sources, then saying that only five cases per year would result becomes a dramatic understatement. Major residential exposure to electromagnetic fields is not from power lines but from home appliances and wiring, and these could also augment incidence.

At 50 Hz no association exists between strengths of electromagnetic fields since people who are exposed are in the source's near field. Magnetic fields from power lines will attenuate at $1/r^3$, whereas electric fields may attenuate only at the simple reciprocal, thereby still exceeding average levels much further away, accelerated or diminished by metal objects or screening.

Draper et al therefore argue for involvement of electric fields. Henshaw's hypothesis implicating corona discharges cannot be a complete answer since these are unlikely from 50 Hz domestic sources.[3] Our 1996 study found a 4.7-fold incidence of childhood leukaemia when the electric component was on average 20 V/m, with power lines only a minor exposure source.[4]

The UK childhood cancer study also measured bedplace electric fields, finding mildly raised incidences, but reported only spot measurements and 48 hour measurements—neither representing children's nocturnal exposure.[5] Were the study's data reanalysed to reflect only night-time exposure the incidence might be found to be similarly high to ours.

If electric fields are bioactive (and most epidemiological research has been directed only to the magnetic component) then a biological mechanism becomes more plausible, since electric fields are superpositive. Many studies of important life processes (heart beat rate, electroencephalogram, ATP synthesis) are mediated via electric currents. Their fields, or electron transport, report adverse effects from exposure to electromagnetic fields. In vitro studies and animal studies also report adverse electric field effects, particularly on lymphocytes and on melatonin synthesis. The supplementary use of melatonin is proving a useful adjuvant as a radioprotective agent, not only at power but also at radiofrequencies.[w1]

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1 Draper G, Vincent T, Kroll ME, Swanson J. Childhood cancer in relation to distance from high voltage power lines in England and Wales: case-control study. *BMJ* 2005;330: 1290-2. (4 June.)
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Reference w1 is available on bmj.com

Distance of address at birth from nearest National Grid line and estimated odds ratios, using all controls combined

	Leukaemia		Brain tumours		Other tumours		All cancers combined		
Distance (m)	No of cases	Odds ratio (95% CI)	No of cases	Odds ratio (95% CI)	No of cases	Odds ratio (95% CI)	No of cases	Odds ratio (95% CI)	No of controls
0-49	5	0.94 (0.34 to 2.57)	3	0.83 (0.24 to 2.84)	7	1.00 (0.41 to 2.42)	15	0.94 (0.46 to 1.90)	16
50-99	19	1.73 (0.99 to 3.05)	4	0.53 (0.19 to 1.51)	15	1.04 (0.56 to 1.91)	38	1.15 (0.72 to 1.84)	33
100-199	40	1.18 (0.82 to 1.70)	26	1.12 (0.73 to 1.73)	37	0.83 (0.57 to 1.20)	103	1.01 (0.77 to 1.33)	102
200-299	44	0.93 (0.66 to 1.30)	38	1.17 (0.82 to 1.68)	66	1.05 (0.78 to 1.41)	148	1.04 (0.82 to 1.31)	143
300-399	61	1.23 (0.91 to 1.66)	35	1.04 (0.72 to 1.50)	79	1.21 (0.92 to 1.59)	175	1.18 (0.95 to 1.47)	149
400-499	78	1.15 (0.89 to 1.50)	40	0.86 (0.62 to 1.22)	80	0.89 (0.69 to 1.16)	198	0.97 (0.80 to 1.18)	204
500-599	75	1.24 (0.95 to 1.63)	54	1.31 (0.96 to 1.78)	86	1.08 (0.83 to 1.39)	215	1.18 (0.97 to 1.44)	182
≥600	9378	1 (reference)	6405	1 (reference)	12 406	1 (reference)	28 189	1 (reference)	28 252

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Summary of responses

Draper et al reported a higher risk of developing leukaemia among children who lived close to power lines at birth. This apparent risk extends to a greater distance than would have been expected from previous studies. The researchers are clear that no accepted biological mechanism exists that might explain the epidemiological results and that the relation may be due to chance or confounding. Some correspondents, however, present various hypotheses that might support the idea of a causal connection.[1]

Volker Königsbüscher, information technology manager from Switzerland, is not surprised to find the risk extending to such a considerable distance. If this effect exists at all and if children live near the lines—playing or walking or visiting people under or close to them—an effect might reflect the amount of time they spend in the affected zone.

Another Swiss contributor, Thomas Netter, sees air pollution as a possible culprit since power lines are often built alongside main traffic routes. Cars and trains generate and carry pollutants, and the aerodynamic friction may charge the aerosols in the proximity of the lines. Pollution is also the link seen by Robin Poston, a histopathologist from London, in the shape of the power lines themselves. Arcing and corona electrical discharges from power lines create nitrogen oxides and ozone—mutagenic compounds that would pollute the air.

And more pollution is the theory of US engineer Wayne Hunter, who hypothesises that extremely toxic spray herbicides in use in the 1940s and 1950s may have been used in the vicinity of the lines. Many of these chemicals remain toxic for more than 100 years and are still available for inhalation and ingestion. The effect on small children breathing trace amounts of vapour or inhaling dirt with those herbicides might have added substantially to the findings.

Physicist Adrian Gaylard points out that the researchers report association, not causation. He takes issue with the plausibility of any proposed causation. No plausible biological mechanism exists for the induction of leukaemia by the electric or magnetic fields associated with power lines. As the relative risk for tumours of the central nervous system or brain takes values either side of unity (and if we are to understand them as indicators of a real risk), any biological mechanism would also have to protect from such tumours out to 199 m, induce tumours from 200-399 m, and then continue to protect against such between 400 m and 499 m, again causing them from 500-599 m. A similar, if less striking, observation can be made for "other diagnoses."

Alternatively, separate biological mechanisms would be needed for power lines to cause individual types of cancer. Gaylard sees chance as a more likely explanation. Alan Preece, emeritus professor at the University of Bristol, agrees that magnetic fields are unlikely to be associated with the raised leukaemia risk out to 600 m but does not dismiss the possibility of a physical mechanism associated with high voltage. He and Dennis L Henshaw, professor of human radiation effects in Bristol, refer to Henshaw's hypothesis on the possible health effects of corona ion emissions. To test this hypothesis, allowance must be made for wind direction—which this study had done in too simplified a manner and was described by the authors as oversimplified. Given that the study includes only a small fraction of the 132kV lines and the prevailing wind is assumed to be from the southwest for the whole country, this study cannot be said to test the hypothesis. Therefore there may still be a mechanism to be tested.

Biology comes into play in the alternative hypothesis proposed by Professor Henshaw. He suggests a causality that is nothing to do with the effects of pollution or electromagnetic fields—the disruption of the hormone melatonin—an anti-oxidant that acts as a natural anti-cancer agent—in the body as a result of radiation due to proximity to power lines.

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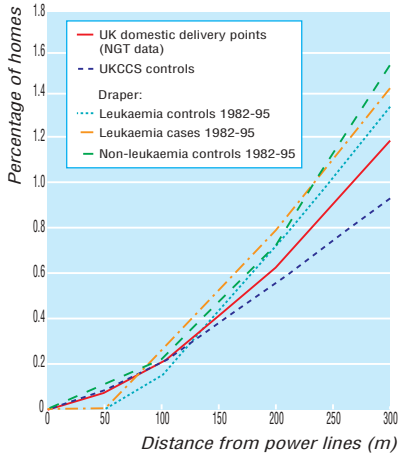
BMJ

Competing interests: None declared.

1 Electronic responses. Childhood cancer in relation to distance from high voltage power lines in England and Wales. http://bmj.bmjjournals.com/cgi/eletters/330/7503/1290 (accessed 8 Sep 2005).

Authors' reply

EDITOR—We thank everyone who has commented on our paper. We have been criticised for publishing alarming results that we cannot explain. We should have preferred to delay publication until we could analyse magnetic field exposure data and, if possible, explain our results. It would have been unethical, however, not to publish results of potential health significance. Moreover, these results had been partially leaked, and the only satisfactory response was to publish.



Percentages of subjects living close to high voltage power lines. Comparisons between cases and controls and between different data sources for roughly comparable periods

Day et al and Hepworth et al discuss our results in relation to those of the UK childhood cancer study (UKCCS).[1] Contrary to the statement by Hepworth et al, the results of the two studies are not inconsistent, and we are puzzled by some of the criticisms in these two letters. In our study the distribution of distances from power lines for the leukaemia controls differs not only from that for the leukaemia cases but also from that for the controls for the other diagnostic groups. We explained the consequent uncertainty about whether the findings provide evidence of a distance related risk or were simply a consequence of a chance selection of unrepresentative leukaemia controls.

However, the comparison with the two sets of data from the UKCCS study shown in the figure given by Day et al is invalid because, as they themselves point out, the various datasets cover different calendar periods. Their two sets of comparison data refer to addresses in the 1990s. Our study extends from 1962 to 1995, during which time the numbers of lines and of houses situated close to lines increased, and the figure shows that when data relating to more closely comparable periods are used we actually have, for most of the distances considered, higher proportions of leukaemia controls living near lines than are found for the two UKCCS comparison groups (values for UKCCS controls from table 1 of [1]). Comparisons between these datasets are in any event questionable because they relate to different subgroups of the population.

The same authors say that we used distance from lines as "crude estimate" (of exposure to power-frequency magnetic fields); we did not. Distance is of interest irrespective of its relation to magnetic field exposure, however; clearly, it will have some relation to exposure from power lines. We shall present our field estimates in a subsequent paper.

We give more details, and discuss some more arcane points of statistics and physics in these and other letters, in our responses on bmj.com.[2]

Kheifets et al also refer to the problem with the leukaemia controls and make comparisons with the complete set of controls including those for other diagnostic groups. They show that the resulting estimates would provide little evidence for a relation between distance and leukaemia risk. However, these estimates do not take account of the matching factors used in selecting the controls. Also, and in our view more importantly, it would be wrong to prefer the results of a re-analysis done simply because the first gives unexpected results.

Coghill and Hepworth et al refer to our calculation that five cases of childhood leukaemia a year in England and Wales would be attributable to high voltage power lines if the association found in our paper is causal. They do not repeat our distinction between (chance) association and causality. Coghill makes suggestions about the numbers of cases attributable to 132 kV lines, but

he goes beyond our data. We agree with Gaylord's suggestion that the pattern of results for central nervous system or brain and other tumours seems to be due to chance.

Our results have several alternative explanations. Henshaw and Preece refer to Henshaw's corona ions hypothesis. We described our test of this hypothesis as "oversimplified" and are analysing our data using a better test. Electric fields, suggested by Coghill, seem no more likely to explain risks at 600 m than magnetic fields. We shall investigate suggested explanations put forward by Königbüscher, Netter, Poston, Coghill, Henshaw, Preece, and Hunter, in cases where data are available.

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Competing interests: GJD, TJV and MEK: none declared. JS is employed by National Grid Transco and worked on this project with their permission.

1 UK Childhood Cancer Study Investigators. Childhood cancer and residential proximity to power lines. *Br J Cancer* 2000;83:1573-80.
2 Electronic responses. Childhood cancer in relation to distance from high voltage power lines in England and Wales. http://bmj.bmjjournals.com/cgi/eletters/330/7503/1290 (accessed 8 Sep 2005).

Using mobile phones in hospitals

Risks were worth running during SARS epidemic

Editor—Highlighting Taffinder's personal view in her Editor's Choice, Godlee asks whether there is evidence that the use of mobile phones in hospitals is dangerous.[1 2] The question of mobile telephones in hospitals, or more importantly in critical clinical areas, remains one of balancing risks with benefits. Porters in my hospital carry walkie-talkies to aid communication, and they just take more care using these when they have to enter the intensive care unit.

The electromagnetic interference of mobile phones and walkie-talkies pales into insignificance in comparison with surgical diathermy, yet we still allow surgeons to use this procedure in operating theatres, even with concurrent, critical electronic monitoring of anaesthetised patients. No mobile phone can hope to compete with this power output (routinely 30 W or more for the surgeons I work with). We just make sure that the relevant precautions and procedures relating to electronic equipment (including, but not limited to, syringe pumps and cardiac pacemakers in patients) are strictly adhered to. If the benefits are great enough the risks are worth running (especially when steps are taken to minimise them).

During SARS, all hospitals had a strict "no visiting" policy, and patients were allowed to use their mobile phones in bed to keep in touch with their family. This was particularly important because the whole community was gripped by the fear of an unknown epidemic. Mobile phone restrictions are applied more rigorously now that the epidemic is over.

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Competing interests: None declared.

1 Taffinder N. The finger, the foot, my bone cancer. *BMJ* 2005;331:463. (20-27 August.)
2 Godlee F. Let's call it cardiac impairment [Editor's choice]. *BMJ* 2005;331 (20-27 August.)

Evidence is lacking, but risk may not be nothing

Editor—Godlee would love to know the evidence that using mobile phones in hospital is dangerous.[1 2] There is not a great deal of evidence to demonstrate one way or the other that using mobile phones is causing any actual harm to patients or to equipment connected to patients. This does not mean that there is no risk. Some time ago I investigated the effects of porters' radios on some infusion pumps and syringe drivers. I found that they interfered with their correct operation. Mobile phones can, and do, cause similar interference to the correct operation of some medical devices.

Guidelines from the Medicines and Healthcare Products Regulatory Agency and regulations from the International Electrotechnical Commission (www.iec.ch) recognise that all electrical and electronic medical devices can potentially be interfered with by any radio source.[3 4 5] Deciding that it is okay to use phones on the basis of little more than private opinion or self interest is arrogant. Just because hospital telecoms management is not up to scratch does not entitle anyone to put others at risk.

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2 Taffinder N. The finger, the foot, my bone cancer. *BMJ* 2005;331:463. (20-27 August.)
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New phones seem not to interfere with new equipment

Editor—I agree with Taffinder that using mobile telephones in hospital makes it much easier and quicker to contact people and reply to them.[1] When I worked in Singapore for a year most staff used mobile phones in hospitals. The quality of care for patients was high, and doctors' response to calls or bleeps quick and efficient.

Current mobile phones are highly advanced digital phones. Older phones were analogue, interfering with medical equipment more than new models do. I have seen senior and junior colleagues use mobile phones in operating theatres and anaesthetic rooms without interference to medical equipment. I have also seen mobile phones being used in intensive care units, again without problems.

The policy of not using mobile phones in hospitals should change, provided that the hospital does not have ancient medical equipment that might be affected by mobile phones.

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1 Taffinder N. The finger, the foot, my bone cancer. *BMJ* 2005;331:463. (20-27 August.)

Review of publication bias in studies on publication bias

Studies on publication bias are probably susceptible to the bias they study

Editor—The empirical evidence about the existence of publication bias is comparatively certain, although the direction and extent of specific types of publication related biases and the consequences of publication bias are much less convincing.[1]

Questioning whether studies on publication bias themselves suffer from the bias they studied is reasonable. Dubben and Beck-Bornholdt used a funnel plot to tackle this issue and found no evidence of publication bias in studies of publication bias.[2] Although their short article is readable and interesting, they acknowledge that the analysis is handicapped by insufficient power (with only 26 included studies) and also by the diverse definitions of publication bias in the primary studies. However, the study has other, more important, limitations.

Firstly, the design may not be appropriate. The ideal and most robust design would be to directly compare the findings of published and unpublished studies on publication bias, although it may be difficult, if not impossible, to identify relevant unpublished studies.

Secondly, the funnel plot, although widely used, is an unreliable tool in

detecting the existence of publication bias. An asymmetrical funnel plot may be due to many possible factors other than publication bias, and the existence of publication bias cannot be safely ruled out even if the funnel plot is symmetrical. Therefore, it is likely, despite Dubben and Beck-Bornholdt's findings, that studies on publication bias are just as susceptible to biased selection for publication as other types of research.

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This response is based on the discussion at a meeting of East Anglia Research Synthesis Group.

Competing interests: None declared.

1 Song F, Eastwood AJ, Gilbody S, Duley L, Sutton AJ. Publication and related biases. *Health Technol Assess* 2000;4:1-115.
2 Dubben HH, Beck-Bornholdt HP. Systematic review of publication bias in studies on publication bias. *BMJ* 2005;331:433-4. (20-27 August.)

Details of the six other authors are available on bmj.com

Meta-research on publication bias does not help transfer research results to patient care

EDITOR—Dubben and Beck-Bornholdt conclude that there is no evidence of publication bias in reports on publication bias.[1] Apart from the fact that funnel plots should be used only as a "tool" and not a "rule" in the evaluation of publication bias,[2] I question whether such "meta-research" really helps to improve patient care or facilitates the applicability of research results.

Systematic reviews help to improve patient care since pooling of appropriate data sometimes enables us to see the results without the noise of the random play of chance. All sources of bias are a potential threat to the credibility of meta-analyses. Despite efforts to ensure that the set of trials used in meta-analyses is a non-biased sample of all existing studies, a recent analysis on studies in the Cochrane database finds that publication bias may be present to some degree in about 50% of meta-analyses and strongly indicated in about 20%.[3]

What are the implications of these findings? When such facts are known, doubts arise on the applicability of such "skewed data." It is even harder to imagine the implications of the meta-research presented in by Dubben and Beck-Bornholdt.

Where do we go from here? Do we need another investigation that focuses on the biases associated with the publication of papers on the "publication bias in studies on publication bias"? What do the results mean other than that publication bias is no fiction: it exists in the real world.

For the clinician it may be reassuring to know that, in most cases, these biases did not affect the conclusions.[3] Therefore, systematic reviews continue to represent a valuable tool to digest huge amounts of research or to find hidden "pearls of evidence" given the time constraints in daily business. This enables clinicians to focus on the "complicated, value laden, rewarding activity that is clinical medicine."[4]

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Competing interests: None declared.

1 Dubben HH, Beck-Bornholdt HP. Systematic review of publication bias in studies on publication bias. *BMJ* 2005;331:433-4. (20-27 August.)
2 Tang JL, Liu JL. Misleading funnel plot for detection of bias in meta-analysis. *J Clin Epidemiol* 2000;53:477-84
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Here's a proposal for editors that may help reduce publication bias

EDITOR—Publication bias is a pervasive problem in biomedical research,[1] Dubben and Beck-Bornholdt providing further evidence on its importance.[2] The preference for publishing papers with significant results may seriously compromise the ability to draw valid conclusions from the published literature. This problem seems particularly relevant to results from epidemiological research.

We offer a solution to this problem that lies at the disposal of journal editors. Preliminary editorial decisions could be based solely on the peer review of the introduction and methods sections of submitted papers. These two sections deal with the key issues on which editorial decisions would ideally be based: the importance of the research question and the potential for the study design and proposed analyses to inform that question.

Blinding reviewers to the results and discussion sections may pose some challenges to the reviewing process because elements of these later sections are also relevant for editorial decisions. However, these difficulties would probably be outweighed by the benefits of reducing publication bias. Peer reviewers might be asked to make a preliminary recommendation to the editor (reject or continue further review) on the basis of the merit of the study design and proposed data analyses—not on the findings themselves.

If manuscripts pass this initial stage then reviewers could be unblinded to the results and discussion sections. Our proposal could have the additional benefit of improving the clarity and detail of methods sections.

Our proposal may be particularly appropriate for papers dealing with topics that are susceptible to publication bias—those in which prior hypotheses are biased strongly in one direction. The usefulness of this proposal could be further evaluated in a randomised trial: submitted manuscripts could be randomly allocated to either a traditional review process or a review process blinded to the results. Editors could then assess whether papers with non-significant results are more likely to be published under the alternative review process.

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1 Sutton AJ, Duval SJ, Tweedie RL, Abrams KR, Jones DR. Empirical assessment of effect of publication bias on meta-analyses. *BMJ* 2000;320:1574-7.
2 Dubben HH, Beck-Bornholdt HP. Systematic review of publication bias in studies on publication bias. *BMJ* 2005;331:433-4. (20-27 August.)

Mandatory publication of data may help

EDITOR—Glymour and Kawachi suggest a modest proposal to reduce publication bias, reviewed by Dubben and Beck-Bornholdt.[1] Their suggestion is indeed modest because many statistically insignificant or negative studies are not necessarily submitted to a medical journal for publication.

The only effective solution would be mandatory registration of all trials in a centralised website before the trial starts, with mandatory publication of the raw data on that centralised website irrespective of whether the trialists submit their results to a medical journal for publication. That solution would allow meta-analysts to collect data from all relevant studies and not only studies published in medical journals.

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Competing interests: None declared.

1 Dubben HH, Beck-Bornholdt HP. Systematic review of publication bias in studies on publication bias. *BMJ* 2005;331:433-4. (20-27 August.)

Systematic review is needed

EDITOR—Dubben and Beck-Bornholdt conclude that studies of publication bias are not themselves subject to publication bias.[1]

This statement is premature. Surely we need a systematic review of systematic reviews of publication bias in studies of publication bias. Any takers?

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Competing interests: None declared.

1 Dubben HH, Beck-Bornholdt HP. Systematic review of publication bias in studies on publication bias. *BMJ* 2005;331:433-4. (20-27 August.)