

Μεθοδολογία της Έρευνας

Παντελής Μπάγκος

Καθηγητής

Πανεπιστήμιο Θεσσαλίας, 2020

Σύνοψη

- Αξιολόγηση μιας εργασίας με το σύστημα κριτών.
- Το σύστημα peer-review και παραλλαγές του.
- Εμπειρικά δεδομένα
- Κριτική στο σύστημα
- Πως απαντάμε στα σχόλια των κριτών.
- Παραδείγματα

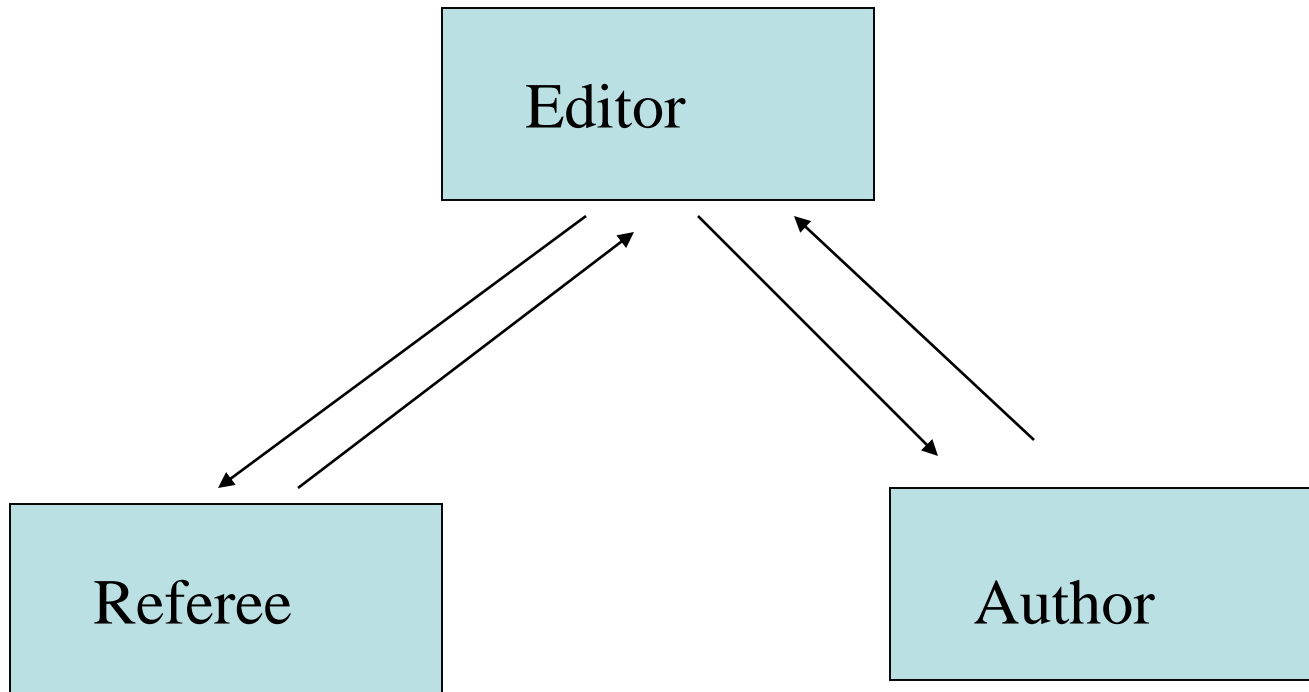
Peer Review System

- Για Δημοσίευση Άρθρων, βιβλίων κλπ
- Για χρηματοδότηση ερευνητικών προγραμμάτων
- Για την κατάληψη μιας θέσης
- Για βραβεία, διακρίσεις κλπ

Peer Review

- Double-blind
- Author blind
- Open (signed) peer review

Peer review



<http://www.bmj.com/sites/default/files/attachments/resources/2011/07/wager.pdf>

1ο Στάδιο

- 1st Editorial decision
 - Reject (out of scope, limited interest)
 - Send to Referees

2ο Στάδιο

- Response of the Referees
 - Accept as is
 - Accept with revisions
 - Resubmit (major revisions)
 - Reject

3ο Στάδιο

- Απάντηση στα σχόλια των Referees
- Διορθώσεις στο κείμενο
- Resubmission

4ο Στάδιο

- Final Submission
- Proof Checking
- Publication (κεράσματα, σαμπάνιες κλπ)

5ο Στάδιο

- Επιλογή άλλου περιοδικού
- Η ίδια διαδικασία από την αρχή

Κριτική στο σύστημα

JOURNAL OF THE ROYAL SOCIETY OF MEDICINE Volume 99 April 2006

Peer review: a flawed process at the heart of science and journals

Richard Smith

J R Soc Med 2006;**99**:178–182

- Peer review is impossible to define in operational terms (an operational definition is one whereby if 50 of us looked at the same process we could all agree most of the time whether or not it was peer review).
- Peer review is thus like poetry, love, or justice. But it is something to do with a grant application or a paper being scrutinized by a third party—who is neither the author nor the person making a judgement on whether a grant should be given or a paper published.
- But who is a peer? Somebody doing exactly the same kind of research (in which case he or she is probably a direct competitor)? Somebody in the same discipline? Somebody who is an expert on methodology?
- And what is review? Somebody saying ‘The paper looks all right to me’, which is sadly what peer review sometimes seems to be. Or somebody pouring all over the paper, asking for raw data, repeating analyses, checking all the references, and making detailed suggestions for improvement? Such a review is vanishingly rare.

- There may even be some journals using the following classic system.
- The editor looks at the title of the paper and sends it to two friends whom the editor thinks know something about the subject.
- If both advise publication the editor sends it to the printers.
- If both advise against publication the editor rejects the paper.
- If the reviewers disagree the editor sends it to a third reviewer and does whatever he or she advises.
- This pastiche—which is not far from systems I have seen used—is little better than tossing a coin, because the level of agreement between reviewers on whether a paper should be published is little better than you'd expect by chance

What is peer review for?

- One answer is that it is a method to select the best grant applications for funding and the best papers to publish in a journal. It is hard to test this aim because there is no agreed definition of what constitutes a good paper or a good research proposal. Plus what is peer review to be tested against? Chance? Or a much simpler process? Stephen Lock when editor of the BMJ conducted a study in which he alone decided which of a consecutive series of papers submitted to the journal he would publish. He then let the papers go through the usual process. There was little difference between the papers he chose and those selected after the full process of peer review.¹ This small study suggests that perhaps you do not need an elaborate process. Maybe a lone editor, thoroughly familiar with what the journal wants and knowledgeable about research methods, would be enough.

What is peer review for?

- Another answer to the question of what is peer review for is that it is to improve the quality of papers published or research proposals that are funded. The systematic review found little evidence to support this, but again such studies are hampered by the lack of an agreed definition of a good study or a good research proposal

What is peer review for?

- Peer review might also be useful for detecting errors or fraud. At the BMJ we did several studies where we inserted major errors into papers that we then sent to many reviewers. Nobody ever spotted all of the errors. Some reviewers did not spot any, and most reviewers spotted only about a quarter. Peer review sometimes picks up fraud by chance, but generally it is not a reliable method for detecting fraud because it works on trust. A major question, which I will return to, is whether peer review and journals should cease to work on trust

THE DEFECTS OF PEER REVIEW

- Slow and expensive
 - Editors, reviewers, managers + publication costs
- Inconsistent
 - Often the reviewers disagree
- Bias
 - Due to institution, name, outcome etc
- Abuse of peer review

Target Article

Peer-review practices of psychological journals: The fate of published articles, submitted again

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Abstract

A growing interest in and concern about the adequacy and fairness of modern peer-review practices in publication and funding are apparent across a wide range of scientific disciplines. Although questions about reliability, accountability, reviewer bias, and competence have been raised, there has been very little direct research on these variables.

The present investigation was an attempt to study the peer-review process directly, in the natural setting of actual journal referee evaluations of submitted manuscripts. As test materials we selected 12 already published research articles by investigators from prestigious and highly productive American psychology departments, one article from each of 12 highly regarded and widely read American psychology journals with high rejection rates (80%) and nonblind refereeing practices.

With fictitious names and institutions substituted for the original ones (e.g., Tri-Valley Center for Human Potential), the altered manuscripts were formally resubmitted to the journals that had originally refereed and published them 18 to 32 months earlier. Of the sample of 38 editors and reviewers, only three (8%) detected the resubmissions. This result allowed nine of the 12 articles to continue through the review process to receive an actual evaluation: eight of the nine were rejected. Sixteen of the 18 referees (89%) recommended against publication and the editors concurred. The grounds for rejection were in many cases described as "serious methodological flaws." A number of possible interpretations of these data are reviewed and evaluated.

Coping with peer rejection

Accounts of rejected Nobel-winning discoveries highlight the conservatism in science. Despite their historical misjudgements, journal editors can help, but above all, visionaries will need sheer persistence.

Not many people spend tens of thousands of dollars to tell the world that they were robbed. But that is what Raymond Damadian and his company did last week when he discovered that he hadn't won the Nobel Prize in Physiology or Medicine, and complained in full-page advertisements in *The New York Times* and other prominent newspapers (see page 648). He claims in his advertisement that he should have shared the prize won by Paul Lauterbur and Peter Mansfield for their work on magnetic resonance imaging.

Whatever the merits of Damadian's case, the episode highlights

fact. But we can take comfort, however dubious, from the fact that our unmitigated embarrassments are but a minority in a substantial list of journals' historical misjudgements.

We can take more respectable comfort from a little-celebrated positive accomplishment of editors, which is to champion submitted papers in the teeth of referees' (and sometimes colleagues') resistance. One such submission, according to his Nobel lecture, came from Thomas Cech. The three referees ("outraged enzymologists", as Cech described them) all opposed the idea that self-splicing RNA could be a catalyst, but *Nature* published it nevertheless.

Rejecting and resisting Nobel class discoveries: accounts by Nobel Laureates

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I review and discuss instances in which 19 future Nobel Laureates encountered resistance on the part of the scientific community towards their discoveries, and instances in which 24 future Nobel Laureates encountered resistance on the part of scientific journal editors or referees to manuscripts that dealt with discoveries that later would earn them the Nobel Prize.

Lack of progress in science is never so much due to any scarcity of factual information as it is to the fixed mindsets of scientists themselves.

[SCHRAM, 1992, P. 357]

A new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it.

[PLANCK, 1949, PP. 33–34]

Journal of Universal Rejection



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- You can send your manuscript here without suffering waves of anxiety regarding the eventual fate of your submission. You know with 100% certainty that it will not be accepted for publication.
- There are no page-fees.
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- The JofUR is one-of-a-kind. Merely submitting work to it may be considered a badge of honor.
- You retain complete rights to your work, and are free to resubmit to other journals *even before our review process is complete*.
- Decisions are often (though not always) rendered within hours of submission.

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Founder and Editor-in-Chief

[Caleb Emmons](#), (Mathematics and Poetry).

Instructions for Authors

The JofUR solicits any and all types of manuscript: poetry, prose, visual art, and research articles. You name it, we take it, and reject it. Your manuscript may be formatted however you wish. Frankly, we don't care.

After submitting your work, the decision process varies. Often the Editor-in-Chief will reject your work out-of-hand, without even reading it! However, he might read it. Probably he'll skim. At other times your manuscript may be sent to anonymous referees. Unless they are the Editor-in-Chief's wife or graduate school buddies, it is unlikely that the referees will even understand what is going on. Rejection will follow as swiftly as a bird dropping from a great height after being struck by a stone. At other times, rejection may languish like your email buried in the Editor-in-Chief's inbox. But it will come, swift or slow, as surely as death. Rejection.

Submissions should be emailed to j.universal.rejection@gmail.com. Small files only, please. Why not just send the first couple pages if it is long? If you are lucky, your eventual rejection letter will appear on the Journal's [blog](#). Please let us know in your cover letter if you would not mind being identified, otherwise most identifying information will be redacted.

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Archives

- March 2009 (Vol 1, No 1) contents:
(empty)
- June 2009 (Vol 1, No 2) contents:
(empty)
- September 2009 (Vol 1, No 3) contents:
(empty)
- December 2009 (Vol 1, No 4) contents:
(empty - because we were on holiday)
- March 2010 (Vol 2, No 1) contents:
(empty)
- June 2010 (Vol 2, No 2) contents:
(empty)
- September 2010 (Vol 2, No 3) contents:
(empty)
- December 2010 (Vol 2, No 4) contents:
(lost when server crashed - presumed empty)

HOW TO IMPROVE PEER REVIEW?

- standardizing procedures
- opening up the process
- Blinding reviewers to the identity of authors
- reviewing protocols
- training reviewers
- being more rigorous in selecting and deselecting reviewers
- using electronic review
- Rewarding reviewers
- providing detailed feedback to reviewers
- Using more checklists
- or creating professional review agencies

Εμπειρικά δεδομένα

[JAMA](#). 1998 Jul 15;280(3):231-3.

What makes a good reviewer and a good review for a general medical journal?

[Black N¹](#), [van Rooyen S](#), [Godlee F](#), [Smith R](#), [Evans S](#).

+ Author information

Abstract

CONTEXT: Selecting peer reviewers who will provide high-quality reviews is a central task of editors of biomedical journals.

OBJECTIVES: To determine the characteristics of reviewers for a general medical journal who produce high-quality reviews and to describe the characteristics of a good review, particularly in terms of the time spent reviewing and turnaround time.

DESIGN, SETTING, AND PARTICIPANTS: Surveys of reviewers of the 420 manuscripts submitted to BMJ between January and June 1997.

MAIN OUTCOME MEASURES: Review quality was assessed independently by 2 editors and by the corresponding author using a newly developed 7-item review quality instrument.

RESULTS: Of the 420 manuscripts, 345 (82%) had 2 reviews completed, for a total of 690 reviews. Authors' assessments of review quality were available for 507 reviews. The characteristics of reviewers had little association with the quality of the reviews they produced (explaining only 8% of the variation), regardless of whether editors or authors defined the quality of the review. In a logistic regression analysis, the only significant factor associated with higher-quality ratings by both editors and authors was reviewers trained in epidemiology or statistics. Younger age also was an independent predictor for editors' quality assessments, while reviews performed by reviewers who were members of an editorial board were rated of poorer quality by authors. Review quality increased with time spent on a review, up to 3 hours but not beyond.

CONCLUSIONS: The characteristics of reviewers we studied did not identify those who performed high-quality reviews. Reviewers might be advised that spending longer than 3 hours on a review on average did not appear to increase review quality as rated by editors and authors.

PMID: 9676665 [PubMed - indexed for MEDLINE]

Open review

BRITISH JOURNAL OF PSYCHIATRY (2000), 176, 47-51

Open peer review: a randomised controlled trial

ELIZABETH WALSH, MAEVE ROONEY, LOUIS APPLEBY
and GREG WILKINSON

- Τυχαιοποιημένη δοκιμή με reviewers του BJP
- Οι reviewers που υπέγραψαν την αναφορά, έκαναν πιο ποιοτική δουλειά
- Έκαναν περισσότερο χρόνο
- Ήταν πιο πιθανό να προτείνουν δημοσίευση

Table 1 Review quality ratings by item in signed and unsigned groups

Quality item	Quality rating (mean (s.d.))		Difference (95% CI)
	Signed group	Unsigned group	
Importance of research question	3.02 (1.07)	2.85 (1.12)	0.16 (−0.06 to 0.39)
Originality	2.85 (1.16)	2.71 (1.18)	0.15 (−0.09 to 0.39)
Methodology	3.63 (1.00)	3.40 (0.93)	0.23 (0.02 to 0.43)*
Presentation	3.30 (1.03)	3.04 (1.01)	0.26 (0.05 to 0.48)**
Constructiveness of comments	3.75 (0.94)	3.48 (0.93)	0.27 (0.07 to 0.47)**
Substantiation of comments	3.45 (1.05)	3.25 (1.03)	0.20 (−0.01 to 0.42)
Interpretation of results	4.43 (1.16)	3.25 (1.14)	0.18 (−0.06 to 0.41)
Mean score	3.35 (0.86)	3.14 (0.86)	0.21 (0.03 to 0.39)*
Tone of review	4.51 (0.65)	4.27 (0.91)	0.25 (0.09 to 0.42)**
Time taken	2.05 (1.25)	1.65 (1.33)	−0.39 (−0.74 to 0.06)*

* $P < 0.05$, ** $P < 0.01$.

Table 2 Recommendation on publication in signed and unsigned groups

Recommendation for publication	Signed group (n (%))	Unsigned group (n (%))	Total (n (%))
Accept without revision	41 (25%)	36 (24%)	77 (24%)
Accept with revision	53 (31%)	35 (23%)	88 (28%)
Resubmit with revision	43 (26%)	31 (20%)	74 (23%)
Reject	30 (18%)	51 (33%)	81 (25%)
Total	167 (100%)	153 (100%)	320 (100%)

CLINICAL IMPLICATIONS

- A sufficient number of reviewers will agree to sign their names, making an open process feasible.
- Signed reviews are at least as good as unsigned reviews, and may be of better quality.
- Signed reviews take longer to complete, leading to a greater workload for reviewers.

LIMITATIONS

- The review quality instrument used, although validated, is open to subjective interpretation.
- Reasons for some reviewers not wishing to sign were not sought.
- We do not know why some randomised reviews were not returned.

Effect of open peer review on quality of reviews and on reviewers' recommendations: a randomised trial

Susan van Rooyen, Fiona Godlee, Stephen Evans, Nick Black, Richard Smith

- Reviewers randomised to be asked to be identified were 12% (95% confidence interval 0.2% to 24%) more likely to decline to review than reviewers randomised to remain anonymous (35% v 23%).
- There was no significant difference in quality (scored on a scale of 1 to 5) between anonymous reviewers (3.06 (SD 0.72)) and identified reviewers (3.09 (0.68)) ($P = 0.68$, 95% confidence interval for difference -0.19 to 0.12), and no significant difference in the recommendation regarding publication or time taken to review the paper.
- The editors' quality score for reviews (3.05 (SD 0.70)) was significantly higher than that of authors (2.90 (0.87)) ($P < 0.005$, 95% confidence interval for difference -0.26 to -0.03). Most authors were in favour of open peer review
- Asking reviewers to consent to being identified to the author had no important effect on the quality of the review, the recommendation regarding publication, or the time taken to review, but it significantly increased the likelihood of reviewers declining to review

Table 1 Characteristics of 125 reviewers randomised to remain anonymous (control) and 125 randomised to be identified (intervention). Values are numbers (percentages) unless stated otherwise

Characteristic	Anonymous reviewers	Identified reviewers
Mean (SD) age (years)	51.1 (9.0) (n=120)	51.2 (8.4) (n=120)
Place of residence:		
United Kingdom	106/124 (85)	106/123 (86)
North America	8/124 (6)	6/123 (5)
Other	10/124 (8)	11/123 (9)
Postgraduate training in epidemiology or statistics	67/120 (56)	62/119 (52)
Involved in medical research	103/120 (86)	103/121 (85)

Totals less than 125 are because of missing data.

Table 2 Effect of reviewers being randomised to be identified on the quality of their review (editors' assessments) and time taken to review. Values are means (standard deviations) unless stated otherwise

Item	Anonymous reviewers (n=113)	Identified reviewers (n=113)	Difference (95% CI)
Item of quality*:			
Importance	2.77 (0.96)	2.86 (0.87)	-0.09 (-0.31 to 0.12)
Originality	2.46 (1.18)	2.47 (1.21)	-0.01 (-0.29 to 0.28)
Method	3.38 (0.99)	3.28 (1.00)	0.10 (-0.13 to 0.34)
Presentation	2.88 (1.05)	2.91 (0.96)	-0.03 (-0.25 to 0.19)
Constructiveness of comments	3.51 (0.89)	3.56 (0.79)	-0.04 (-0.23 to 0.14)
Substantiation of comments	3.16 (0.92)	3.36 (0.90)	-0.19 (-0.40 to 0.01)
Interpretation of results	3.22 (0.94)	3.18 (0.95)	0.04 (-0.18 to 0.26)
Mean total score	3.06 (0.72)	3.09 (0.68)	-0.03 (-0.19 to 0.12)
Time taken to review (hours)	2.25 (1.46)	2.20 (1.76)	0.05 (-0.33 to 0.43)

*Items scored on a five-point scale (1=poor, 5=excellent).

Table 3 Effect of reviewers being randomised to be identified on their recommendation for publication of reviewed manuscript. Values are numbers (percentages)

Recommendation	Anonymous reviewers (n=114)	Identified reviewers (n=114)
Publish without revision	1 (1)	2 (2)
Publish after minor revision	33 (29)	41 (36)
Publish after major revision	19 (17)	20 (18)
Reject	55 (48)	46 (40)
Other	1 (1)	1 (1)
Missing data	5 (4)	4 (4)

Key messages

- Arguments in favour of open peer review include increased accountability, fairness, and transparency.
- Preliminary evidence suggests that open peer review leads to better quality reviews
- We conducted a randomised controlled trial to examine the feasibility and impact of asking *BMJ* reviewers to sign their reviews
- There were no differences in the quality of reviews between those who were randomised to be identified and those who were not
- Most reviewers agreed to be identified to authors, and most of the authors surveyed were in favour of open peer review

Effect of blinding and unmasking on the quality of peer review: a randomized trial.

[van Rooyen S¹](#), [Godlee F](#), [Evans S](#), [Smith R](#), [Black N](#).

⊕ Author information

Abstract

CONTEXT: Little research has been conducted into the quality of peer review and, in particular, the effects of blinding peer reviewers to authors' identities or masking peer reviewers' identities.

OBJECTIVE: To determine whether concealing authors' identities from reviewers (blinding) and/or revealing the reviewer's identity to a coreviewer (unmasking) affects the quality of reviews, the time taken to carry out reviews, and the recommendation regarding publication.

DESIGN AND SETTING: Randomized trial of 527 consecutive manuscripts submitted to BMJ, which were randomized and each sent to 2 peer reviewers.

INTERVENTIONS: Manuscripts were randomized as to whether the reviewers were unmasked, masked, or uninformed that a study was taking place. Two reviewers for each manuscript were randomized to receive either a blinded or an unblinded version.

MAIN OUTCOME MEASURES: Mean total quality score, time taken to carry out the review, and recommendation regarding publication.

RESULTS: Of the 527 manuscripts entered into the study, 467 (89%) were successfully randomized and followed up. The mean total quality score was 2.87. There was little or no difference in review quality between the masked and unmasked groups (scores of 2.82 and 2.96, respectively) and between the blinded and unblinded groups (scores of 2.87 and 2.90, respectively). There was no apparent Hawthorne effect. There was also no significant difference between groups in the recommendations regarding publication or time taken to review.

CONCLUSIONS: Blinding and unmasking made no editorially significant difference to review quality, reviewers' recommendations, or time taken to review. Other considerations should guide decisions as to the form of peer review adopted by a journal, and improvements in the quality of peer review should be sought via other means.

Working double-blind

Should there be author anonymity in peer review?

Double-blind peer review, in which both authors and referees are anonymous, is apparently much revered, if not much practised. The Publishing Research Consortium (PRC) has assessed attitudes towards peer review among 3,000 academics in an international survey across the sciences and humanities. The results, released last month¹, strongly affirm the value of peer review. They

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Is there evidence that double-blind peer review presents a better alternative? It would do so if it generated more constructive comments in the minds of editors and authors, or if the identity of authors were truly protected, or if biases were reduced. So far, the jury is out. Although at least one study in the biomedical literature has suggested that double-blind peer review increases the quality of reviews, a larger study of seven medical journals^{2,3} indicated that neither authors nor editors found significant difference in the quality of comments when both referees and authors were blinded. Referees could identify at least one of the authors on about 40% of the papers, undermining the *raison d'être* for double-blinding. The editors at the Public Library of Science abandoned double-blind peer review because too few requested it and authors were too readily identified.

The one bright light in favour of double-blind peer review is the measured reduction in bias against authors with female first names (shown in numerous studies, such as ref. 4). This suggests that authors submitting papers to traditionally minded journals should include the given names of authors only on the final, published version.

also highlight that 71% have confidence in double-blind peer review and that 56% prefer it to other forms of review. Support is highest with those who have experienced it (the humanities and social sciences) or where it is perceived to do the most good (among female authors). The least enthusiastic group is editors. So is it time for editors, and those at *Nature* in particular, to reconsider their position?

If referees know the authors' identities, it may leave the latter vulnerable to biases about them or their previous work, their gender, their nationality or their being new to an area of research. But the PRC survey supports the contention of *Nature* and others that identifying authors stimulates referees to ask appropriate questions

in the life sciences. But some physical sciences, such as high-energy physics, share preprints extensively through arXiv, an online repository. Thus, double-blind peer review is at odds with another 'force for good' in the academic world: the open sharing of information. The PRC survey found that highly competitive fields (such as neuroscience) or those with larger commercial or applied interests (such as materials science and chemical engineering) were the most enthusiastic about double-blinding, whereas fields with more of a tradition for openness (astronomy and mathematics) were decidedly less supportive.

Who is afraid of reviewers' comments? Or, why anything can be published and anything can be cited

John P. A. Ioannidis^{*,†,‡}, Athina Tatsioni^{*,†} and Fotini B. Karassa^{*}

^{*}University of Ioannina School of Medicine, Ioannina, Greece, [†]Tufts University School of Medicine and Institute for Clinical Research and Health Policy Studies, Boston, MA, USA, [‡]Harvard School of Public Health, Boston, MA, USA

- Συνεχείς υποβολές του ίδιου άρθρου, ακόμα και στο ίδιο περιοδικό
- Τα σχόλια των reviewers συνήθως αγνοούνται
- Το σύστημα ανταμείβει την παραγωγικότητα αλλά δεν βάζει ποινή στην προσπάθεια
- Τα πάντα δημοσιεύονται, τα πάντα αναφέρονται
- Μήπως τα σχόλια των reviewers πρέπει να παραμένουν διαθέσιμα και να ακολουθούν την εργασία;

Springer, IEEE withdrawing more than 120 nonsense papers

with 31 comments

Two major publishers will remove more than 120 papers created with random paper generator [SClgen](#), according to *Nature*.

[Richard van Noorden](#), who has the scoop, reports:

“ Over the past two years, computer scientist [Cyril Labbé](#) of Joseph Fourier University in Grenoble, France, has catalogued computer-generated papers that made it into more than 30 published conference proceedings between 2008 and 2013. Sixteen appeared in publications by Springer, which is headquartered in Heidelberg, Germany, and more than 100 were published by the Institute of Electrical and Electronic Engineers (IEEE), based in New York. Both publishers, which were privately informed by Labbé, say that they are now removing the papers.

Although it's unclear who submitted the papers, or why, it's hard not to see the revelations as as the ying to the yang of John Bohannon's [string of open access publishers](#) that appeared in *Science* in October. Bohannon, posing as a fake academic, got half of a group of more than 300 journals to accept fake papers. [From today's *Nature* story:](#)

“ Labbé emphasizes that the nonsense computer science papers all appeared in subscription offerings. In his view, there is little evidence that open-access publishers — which charge fees to publish manuscripts — necessarily have less stringent peer review than subscription publishers.

Indeed, as we and many others [pointed out at the time](#), Bohannon didn't include any traditional journals. As we noted:

“ ...Retraction Watch readers may recall that it was *Applied Mathematics Letters* — a non-open-access journal published by Elsevier — that published a [string of bizarre papers](#), including one that was



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Potential, possible, or probable predatory scholarly open-access publishers

This is a list of questionable, scholarly open-access publishers. We recommend that scholars read the available reviews, assessments and descriptions provided here, and then decide for themselves whether they want to submit articles, serve as editors or on editorial boards. The criteria for determining predatory publishers are [here](#).

We hope that tenure and promotion committees can also decide for themselves how importantly or not to rate articles published in these journals in the context of their own institutional standards and/or geocultural locus. We emphasize that journal publishers and journals change in their business and editorial practices over time. This list is kept up-to-date to the best extent possible but may not reflect sudden, unreported, or unknown enhancements.

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Who's Afraid of Peer Review?

A spoof paper concocted by *Science* reveals little or no scrutiny at many open-access journals

On 4 July, good news arrived in the inbox of Ocorrafoo Cobange, a biologist at the Wasse Institute of Medicine in Asmara. It was the official letter of acceptance for a paper he had submitted 2 months earlier to the *Journal of Natural Pharmaceuticals*, describing the anticancer properties of a chemical that Cobange had extracted from a lichen.

In fact, it should have been promptly rejected. Any reviewer with more than a high-school knowledge of chemistry and the ability to understand a basic data plot should have spotted the paper's shortcomings immediately. Its experiments are so hopelessly flawed that the results are meaningless.

I know because I wrote the paper. Ocorrafoo Cobange does not exist, nor does the Wasse Institute of Medicine. Over the past 10 months, I have submitted 304 versions of the wonder drug paper to open-access journals. More than half of the journals accepted the paper, failing to notice its fatal flaws. Beyond that headline result, the data from this sting operation reveal the contours of an emerging Wild West in academic publishing.

From humble and idealistic beginnings a decade ago, open-access scientific journals have mushroomed into a global industry, driven by author publication fees rather than traditional

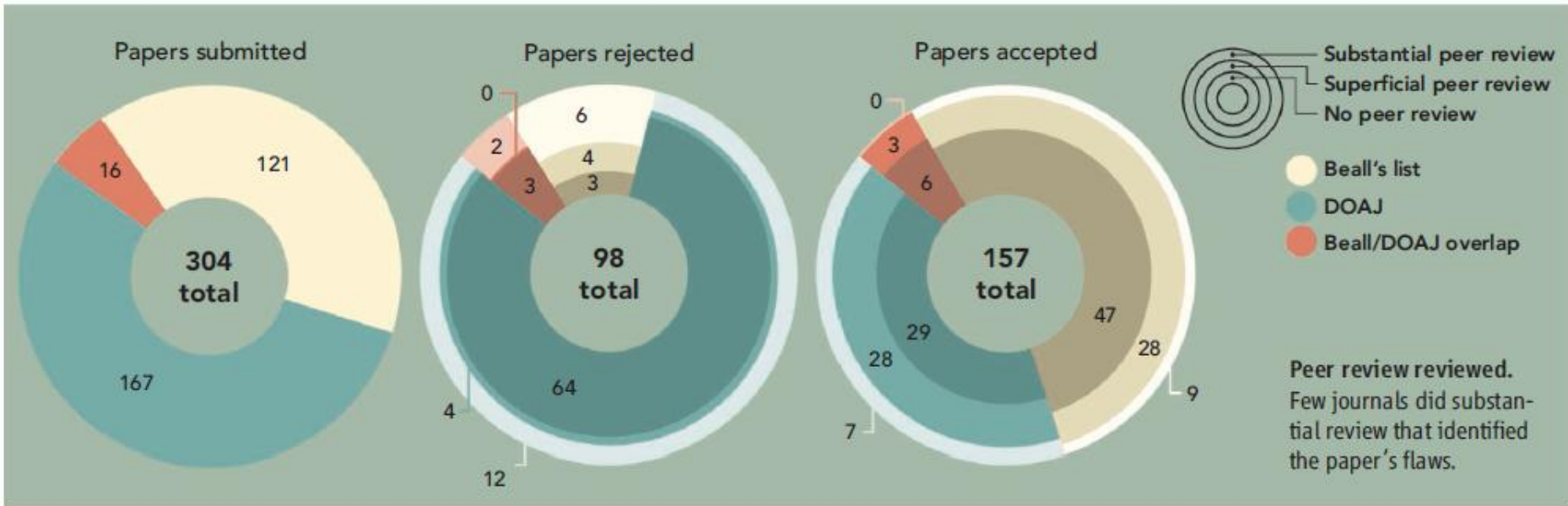
subscriptions. Most of the players are murky. The identity and location of the journals' editors, as well as the financial workings of their publishers, are often purposefully obscured. But *Science's* investigation casts a powerful light. Internet Protocol (IP) address traces within the raw headers of e-mails sent by journal editors betray their locations. Invoices for publication fees reveal a network of bank accounts based mostly in the developing world. And the acceptances and rejections of the paper provide the first global snapshot of peer review across the open-access scientific enterprise.

One might have expected credible peer review at the *Journal of Natural Pharmaceuticals*. It describes itself as "a peer reviewed journal aiming to communicate high quality research articles, short communications, and reviews in the field of natural products with desired pharmacological activities." The editors and advisory board members are pharmaceutical science professors at universities around the world.

The journal is one of more than 270 published by Medknow, a company based in Mumbai, India, and one of the largest open-access publishers. According to Medknow's website, more than

ILLUSTRATION: DAVID FLUNKERT





- 157 of the journals had accepted the paper and 98 had rejected it. Of the remaining 49 journals, 29 seem to be derelict: websites abandoned by their creators. Editors from the other 20 had e-mailed the fictitious corresponding authors stating that the paper was still under review; those, too, are excluded from this analysis. Acceptance took 40 days on average, compared to 24 days to elicit a rejection. Of the 255 papers that underwent the entire editing process to acceptance or rejection, about 60% of the final decisions occurred with no sign of peer review.

- Of the 106 journals that discernibly performed any review, 70% ultimately accepted the paper. Most reviews focused exclusively on the paper's layout, formatting, and language. This sting did not waste the time of many legitimate peer reviewers. Only 36 of the 304 submissions generated review comments recognizing any of the paper's scientific problems. And 16 of those papers were accepted by the editors despite the damning reviews.

- The results show that Beall is good at spotting publishers with poor quality control: For the publishers on his list that completed the review process, 82% accepted the paper. Of course that also means that almost one in five on his list did the right thing—at least with my submission.

- A bigger surprise is that for DOAJ publishers that completed the review process, 45% accepted the bogus paper.

SCIgen - An Automatic CS Paper Generator

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About

SCIgen is a program that generates random Computer Science research papers, including graphs, figures, and citations. It uses a hand-written **context-free grammar** to form all elements of the papers. Our aim here is to maximize amusement, rather than coherence.

One useful purpose for such a program is to auto-generate submissions to conferences that you suspect might have very low submission standards. A prime example, which you may recognize from spam in your inbox, is SCI/IIIS and its dozens of co-located conferences (check out the very broad conference description on the [WMSCI 2005](#) website). There's also a list of [known bogus conferences](#). Using SCIgen to generate submissions for conferences like this gives us pleasure to no end. In fact, one of our papers was accepted to SCI 2005! See [Examples](#) for more details.

We went to WMSCI 2005. Check out the [talks and video](#). You can find more details in our [blog](#).

Also, check out our 10th anniversary celebration project: [SCIpher!](#)

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Author 3:
Author 4:
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Generate

Επαναφορά

Examples

Here are two papers we submitted to [WMSCI 2005](#):

- **Router: A Methodology for the Typical Unification of Access Points and Redundancy** ([PS](#), [PDF](#))

Jeremy Stribling, Daniel Aguayo and Maxwell Krohn

This paper was accepted as a "non-reviewed" paper!

- [Acceptance e-mail](#)
- A strange [follow-up email](#), along with our [response](#)
- [Anthony Liekens](#) sent an [inquiry](#) to WMSCI about this situation, and received [this response](#), with an amazing letter ([PS](#), [PDF](#)) attached. (Also check out Jeff Erickson's [in-depth deconstruction](#) of this letter.)
- With the many generous [donations](#) we received, we [paid](#) one conference [registration](#) fee of \$390.
- Our registration fee was [refunded](#). See [above](#) for the next phase of our plan.

We received many [donations](#) to send us to the conference, so that we can give a randomly-generated talk.

- **The Influence of Probabilistic Methodologies on Networking** ([PS](#), [PDF](#))

Thomer M. Gil

For some reason, this paper was [rejected](#). We [asked for reviews](#), and got [this response](#).

Talks

Thanks to the generous donations of 165 people, we went to WMSCI 2005 in Orlando and held our own "technical" session in the same hotel. The (randomly-generated) title of the session was **The 6th Annual North American Symposium on Methodologies, Theory, and Information**. The session included three randomly-generated talks:

- **Harnessing Byzantine Fault Tolerance Using Classical Theory**
Dr. Thaddeus Westerson, Institute for Human Understanding (Max)
- **Synthesizing Checksums and Lambda Calculus using Jog**
Dr. Mark Zarqawi, American Freedom University (Jeremy)
- **On the Study of the Ethernet**
Franz T. Shenkrishnan, PhD, Network Analysis Laboratories (Dan)

As promised, we videotaped the whole thing. You can download the resulting movie, titled *Near Science*, below. Movie length: 13:15.

- **High quality (AVI: 88 MB, RealMedia: 65 MB):**
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- [Some books of interest](#) **NEW**
- [Other useful Web sites on the "Social Text Affair"](#)
 - [Jason Walsh site](#) **NOTE NEW LOCATION!!!**
 - [Gen Kuroki site](#)
 - ["The Science Wars Homepage"](#) Please note that I do *not* endorse the title of this page; in my opinion this is an intellectual debate, not a war.
 - [Sokal et Bricmont dans la presse francophone](#) (bibliographie, en français) **NOTE NEW LOCATION!!!**
 - [Vittorio Bertolini site](#) (sito in italiano) **NOTE NEW LOCATION!!!**
 - [Jukka-Pekka Takala site](#) (site in Finnish and Scandinavian languages)

<http://www.physics.nyu.edu/faculty/sokal/>

Transgressing the Boundaries:
Towards a Transformative Hermeneutics
of Quantum Gravity

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http://www.physics.nyu.edu/sokal/transgress_v2_noafterword.pdf

Heisenberg,

the different intuitive pictures which we use to describe atomic systems, although fully adequate for given experiments, are nevertheless mutually exclusive. Thus, for instance, the Bohr atom can be described as a small-scale planetary system, having a central atomic nucleus about which the external electrons revolve. For other experiments, however, it might be more convenient to imagine that the atomic nucleus is surrounded by a system of stationary waves whose frequency is characteristic of the radiation emanating from the atom. Finally, we can consider the atom chemically. . . . Each picture is legitimate when used in the right place, but the different pictures are contradictory and therefore we call them mutually complementary.¹⁵

And once again Bohr:

A complete elucidation of one and the same object may require diverse points of view which defy a unique description. Indeed, strictly speaking, the conscious analysis of any concept stands in a relation of exclusion to its immediate application.¹⁶

This foreshadowing of postmodernist epistemology is by no means coincidental. The profound connections between complementarity and deconstruction have recently

The differences within physics between wave and particle theories of matter, the indeterminacy principle discovered by Heisenberg, Einstein's relativity theory, all are accommodations to the impossibility of arriving at a unified field theory, one in which the "anomaly" of difference for a theory which posits identity may be resolved without challenging the presuppositions of science itself.

For further development of these ideas, see Aronowitz (1988a, 524–525, 533).

¹⁵Heisenberg (1958, 40–41).

¹⁶Bohr (1934), cited in Jammer (1974, 102). Bohr's analysis of the complementarity principle also led him to a social outlook which was, for its time and place, notably progressive. Consider the following excerpt from a 1938 lecture (Bohr 1958, 30):

What?

- In the second paragraph I declare, without the slightest evidence or argument, that "physical reality" [note the scare quotes] ... is at bottom a social and linguistic construct." Not our theories of physical reality, mind you, but the reality itself. Fair enough: anyone who believes that the laws of physics are mere social conventions is invited to try transgressing those conventions from the windows of my apartment. (I live on the twenty-first floor.)
- Throughout the article, I employ scientific and mathematical concepts in ways that few scientists or mathematicians could possibly take seriously. For example, I suggest that the "morphogenetic field" -- a bizarre New Age idea due to Rupert Sheldrake -- constitutes a cutting-edge theory of quantum gravity. This connection is pure invention; even Sheldrake makes no such claim. I assert that Lacan's psychoanalytic speculations have been confirmed by recent work in quantum field theory. Even nonscientist readers might well wonder what in heavens' name quantum field theory has to do with psychoanalysis; certainly my article gives no reasoned argument to support such a link.
- Later in the article I propose that the axiom of equality in mathematical set theory is somehow analogous to the homonymous concept in feminist politics. In reality, all the axiom of equality states is that two sets are identical if and only if they have the same elements. Even readers without mathematical training might well be suspicious of the claim that the axiom of equality reflects set theory's "nineteenth-century liberal origins."
- In sum, I intentionally wrote the article so that any competent physicist or mathematician (or undergraduate physics or math major) would realize that it is a spoof. Evidently the editors of *Social Text* felt comfortable publishing an article on quantum physics without bothering to consult anyone knowledgeable in the subject.

Why?

- Politically, I'm angered because most (though not all) of this silliness is emanating from the self-proclaimed Left. We're witnessing here a profound historical *volte-face*. For most of the past two centuries, the Left has been identified with science and against obscurantism; we have believed that rational thought and the fearless analysis of objective reality (both natural and social) are incisive tools for combating the mystifications promoted by the powerful -- not to mention being desirable human ends in their own right. The recent turn of many ``progressive'' or ``leftist'' academic humanists and social scientists toward one or another form of epistemic relativism betrays this worthy heritage and undermines the already fragile prospects for progressive social critique. Theorizing about ``the social construction of reality'' won't help us find an effective treatment for AIDS or devise strategies for preventing global warming. Nor can we combat false ideas in history, sociology, economics and politics if we reject the notions of truth and falsity

Why?

- The results of my little experiment demonstrate, at the very least, that some fashionable sectors of the American academic Left have been getting intellectually lazy. The editors of *Social Text* liked my article because they liked its *conclusion*: that "the content and methodology of postmodern science provide powerful intellectual support for the progressive political project." They apparently felt no need to analyze the quality of the evidence, the cogency of the arguments, or even the relevance of the arguments to the purported conclusion.

Why?

- Of course, I'm not oblivious to the ethical issues involved in my rather unorthodox experiment. Professional communities operate largely on trust; deception undercuts that trust. But it is important to understand exactly what I did. My article is a theoretical essay based entirely on publicly available sources, all of which I have meticulously footnoted. All works cited are real, and all quotations are rigorously accurate; none are invented. Now, it's true that the author doesn't believe his own argument. But why should that matter? The editors' duty as scholars is to judge the validity and interest of ideas, without regard for their provenance. (That is why many scholarly journals practice blind refereeing.) If the *Social Text* editors find my arguments convincing, then why should they be disconcerted simply because I don't? Or are they more deferent to the so-called "cultural authority of technoscience" than they would care to admit?

Why?

- In the end, I resorted to parody for a simple pragmatic reason. The targets of my critique have by now become a self-perpetuating academic subculture that typically ignores (or disdains) reasoned criticism from the outside. In such a situation, a more direct demonstration of the subculture's intellectual standards was required. But how can one show that the emperor has no clothes? Satire is by far the best weapon; and the blow that can't be brushed off is the one that's self-inflicted. I offered the *Social Text* editors an opportunity to demonstrate their intellectual rigor. Did they meet the test? I don't think so.
- I say this not in glee but in sadness. After all, I'm a leftist too (under the Sandinista government I taught mathematics at the National University of Nicaragua). On nearly all practical political issues -- including many concerning science and technology -- I'm on the same side as the *Social Text* editors. But I'm a leftist (and feminist) *because of* evidence and logic, not in spite of it. Why should the right wing be allowed to monopolize the intellectual high ground?

- "Ποιος ασχολείται σήμερα με υπαρξισμό; Με δομισμό – ή αν θέλετε στρουκτουραλισμό; Και ήρθε μετά ο Άλαν Σοκάλ και γελοιοποίησε όλη την Γαλλική φιλοσοφία με τα κείμενά του που παράγονταν από προγράμματα υπολογιστών"

Fund people not projects

John P. A. Ioannidis proposes ways to save scientists from spending all their time writing grants.

The research funding system is broken: scientists don't have time for science any more. Because they are judged on the amount of money they bring to their institutions, writing, reviewing and administering grants absorb their efforts¹. The requirement that they promise taxpayers specific results to justify research tends to invite either exaggeration or boringly predictable projects. Yet the research behind 30% of the pivotal papers from Nobel laureates in medicine, physics and chemistry was done without direct funding².

Every scientist recognizes this problem

and hopes for a solution. Although detailed proposals may be indispensable for some projects, such as rigorous clinical trials and large-scale collaborative research, ideas abound for more efficient ways to fund general research. Some organizations are already experimenting. Multiple options could co-exist, with portions of the budget earmarked for different schemes.

Here are some of the most promising proposals to reduce the amount of time scientists spend trying to fund their research, and the pros and cons of each (see table). Definitive fixes would require major

system overhauls, which are likely to make some scientists justifiably nervous. But smaller, pilot efforts that enable us to evaluate what works could begin right away.

FUND EVERYBODY (OR A LUCKY FEW)

Some — or all — of the research budget could be allocated to eligible scientists in equal shares, or given to a few lucky ones at random. With egalitarian sharing, each scientist would receive only a small amount, which could quickly evaporate without returns when research costs are high. But scientists in some fields — mathematics, ►

OPTIONS FOR REVAMPING THE FUNDING SYSTEM

Option	Pros	Cons	Example	Who would be funded?
Egalitarian (fund everybody)	Avoids peer-review biases Gives sufficient amounts to scientists doing low-cost research Small administrative burden	Does not support large research efforts Does not recognize exceptional scientists	Some universities fund the salaries of all their faculty	All
Aleatoric (fund at random)	Avoids peer-review biases Small administrative burden	Will not capture all deserving scientists	Foundational Questions Institute	Flexible
Assessment of career	Captures career trajectory Has gold-standard status	Is vulnerable to favouritism Inappropriate for young researchers Is labour-intensive	MacArthur Fellows Program	Few elite scientists (or else administratively burdensome)
Automated impact indices	Eliminates favouritism Evaluates many applicants with ease Approaches objectivity	There are many indices, all with flaws; no consensus about best one to use Indices can be gamed Databases have shortcomings (such as imperfect citation coverage, entry errors, name disambiguation problems)	UK Research Excellence Framework	Flexible
Scientific citizenship	May improve science, if good practices are rewarded and bad ones penalized	Automation is not yet possible for data gathering, and is difficult for some citizenship practices Has peer-review biases	Financial incentives to peer reviewers	Could be extended to many scientists only for aspects that can be automated
Projects with broad goals	Proposals are easy to write and review Formulating work can be flexible Permits targeted innovation	Does not eliminate project proposals Is vulnerable to favouritism Holds potential for exaggerated promises and claims	NIH Director's Pioneer Awards Howard Hughes Medical Institute	Few elite scientists

Two or more options can also be combined (for example, automated impact indices plus evaluation of scientific citizenship).

Rules of the Game

- Confidentiality
- Disclosure of competing interests
- Morality
- Responsibility
- Integrity, etc

Side-effects

- Editorial misconduct
- Referee misconduct

Writing an Article

- Αξίζει να γραφτεί?
- Που πρέπει να δημοσιευτεί?
- Σε ποιο κοινό απευθύνεται?

<http://www.bmj.com/sites/default/files/attachments/resources/2011/07/wager.pdf>

Επιλογή περιοδικού

- Αντικείμενο περιοδικού
- Impact Factor/Αναγνωσιμότητα
- Συμμόρφωση με τις οδηγίες (Instructions for authors)
- Cover letter

ΕΠΙΣΤΗΜΟΝΙΚΗ ΑΠΑΤΗ

- Fraud-fabrication of data
- Duplicate publication
- Copyright violation
- Plagiarism

Πραγματικά παραδείγματα

Publication Misdemeanour

Two Articles for Comparison

Article A

APACHE II score in massive upper gastrointestinal haemorrhage from peptic ulcer: prognostic value and potential clinical applications

This study examined the prognostic value of the APACHE II scoring system in patients undergoing emergency operations for bleeding peptic ulcer. There were 96 operations for gastric ulcers and 58 for duodenal ulcers. The mean scores in survivors and in patients who died were 10.8 and 17.5 respectively. None of the 66 patients with an APACHE II score <11 died, while the mortality rate in those scored >10 was 22%. In patients scored >10 non-resective procedures carried less risk of mortality than gastrectomy. The APACHE II score is useful when measuring the severity of the acute disease and predicting the outcome in these patients. If used in daily practice it may assist the surgeon in stratifying patients into a low-risk group (score <11) in which major operations are well tolerated and outcome is favourable and a high-risk group (score >10) in which the risk of mortality is high and the performance of procedures of lesser magnitude is probably more likely to improve survival.

Key Words: APACHE II, outcome prediction, upper gastrointestinal haemorrhage, emergency gastroduodenal operations

The procedure of choice for the surgical control of a bleeding peptic ulcer is controversial. While some authors advocate the use of truncal vagotomy and antrectomy (TV+A) for a bleeding duodenal ulcer, others claim that truncal vagotomy and a drainage procedure (TV+D) is safer. The same argument involves the management of bleeding gastric ulcers. Whilst partial gastrectomy continues to be the procedure of choice in many major centres, others attribute lower mortality rates to more conservative operations such as TV+D or

Article B

APACHE II score in massive upper gastrointestinal haemorrhage from peptic ulcer

Abstract

This retrospective study examined the prognostic value of the APACHE II scoring system in patients undergoing emergency operations for bleeding peptic ulcer. Between 1980–1997 there were 228 patients who had operated emergency for bleeding peptic ulcer. There were 57 operations (25%) for gastric ulcers and 171 (75%) for duodenal ulcers. There were 48 (21.2%) female, 180 (78.9%) male and the median age was 46.0 ± 16.4 (range 15–81). In the group who had operated for gastric ulcer male/female ratio was 47/10 and the median age was 49.4 ± 16.4 . In the group who had operated for duodenal ulcer male/female ratio was 133/38 and the median age was 44.8 ± 16.3 . The mean APACHE II scores in survivors and in patients who died were 5.29 and 9.27 respectively. In patients who had high APACHE II scores, vagotomy + drainage procedures carried less risk of mortality than vagotomy + resection.

If used in daily practice it may assist the surgeon in stratifying patients into a low-risk group in which major operations are well tolerated and outcome is favourable and high-risk group in which the risk of mortality is high and the performance of procedures of lesser magnitude is probably more likely to improve survival.

Key Words: APACHE II, upper gastrointestinal haemorrhage, emergency gastroduodenal operations

Introduction

The procedure of choice for the surgical control of a bleeding peptic ulcer is controversial. While some authors advocate the use of truncal vagotomy and antrectomy (TV+A) for a bleeding duodenal ulcer, others claim that truncal vagotomy and a drainage procedure (TV+D) is safer. The same argument involves the management of bleeding gastric ulcers. Whilst partial gastrectomy continues to be the

