

의학논문의 전문가심사 Peer Review

홍성태

서울의대 기생충학교실

대한의학회 간행이사

대한의학학술지편집인협의회 부회장

학술지 발간과 논문발표의 목적

- **학술지:** 편집인과 발행인을 중심으로 공동 관심 학문주제를 가지는 학자들이 모여서 수행하는 학술활동의 매개체로 인쇄물 또는 웹자료 형태
- **논문:** 각 연구자들이 수행한 연구결과물을 일정한 형식에 맞추어 작성한 문서로 심사를 거쳐서 학술지를 통하여 공개하는 전문가 집단의 공동 재산
- **논문의 목적:** 개인의 창의적인 학술활동 결과물을 인류공동의 지식으로 바꾸어 기여하는 것

학술지 발간 참여자와 그 역할

- **발행인**: 발행의 비용을 부담하는 사람 (학회장, 이사장, 사장, 학장), 편집인 임명
- **편집인, 편집위원**: 학술지의 내용과 대외 평판에 대하여 책임을 지는 사람. 성격을 정함
- **인쇄인**: 학술지의 인쇄와 제본을 책임지는 사람
- **전문가심사위원**: 학술지에 게재할 논문을 사전에 심사하여 게재여부와 수정요구 의견을 내는 사람
- **저자**: 논문의 생산자, 인용자
- **독자**: 학술지의 소비자, 논문의 인용자

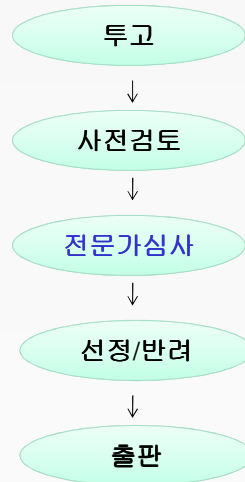
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학술지 논문심사 Peer Review

- **전문가심사(Peer Review)**: 학술지의 질적 수준 유지의 필수요소. 인쇄 전에 원고를 검토함.
- “**학술지를 통하여 공개하여도 좋은 논문인가?**”를 검토
- **Peer**: 배심원
- **의의**: 학술지 게재논문의 학문적 검증 단계. 학술지의 정보원으로서의 수준을 결정
- 특정 논문의 출판에 대한 편집인의 **결정**에 대하여 해당 분야 전문가 조언의견 제시
- **근거**: 편집 또는 투고규정에 따라 편집인이 권위를 부여

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논문출판 과정



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전문가심사 절차와 제도 운영

- **전문가**란?
 - 해당 연구내용의 최근 지경까지 정확하게 파악하고 있는 학자
- 통상 2-3인을 선정하여 의뢰
- 심사자는 익명으로 처리
- 편집인은 심사 의견을 존중하나 결정은 편집인이 한다
- 투고된 논문의 출판 여부 결정에서 가장 중요
- 표절, 중복출판의 스크린에서 핵심적 역할

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전문가심사의 속성

- **전문성**: 편집인이 심사자를 전문성을 고려하여 선정. **적합하지 않을 경우 반환해야 함. 철저하게 전문가답게 시행함.**
- **공정성**: 학술적인 내용만 객관적으로 평가
- **신뢰성**: 심사를 통하여 논문의 학술적인 객관성을 부여. 독자에 대한 신뢰를 구축
- **주관성**: 심사자의 주관에 영향을 받음.
- **윤리성**: 편집인과 저자의 신뢰에 근거하여 원고를 미리 보는 권한을 가짐. 평가에만 활용. 이해관계에 해당하면 심사하지 말아야

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전문가심사 편집인 윤리

- 투고원고의 비밀유지: 철저하게 원고의 내용을 보호 (ICMJE Recommendation 2013: **Requests from third parties to use manuscripts and reviews for legal proceedings should be politely refused, and editors should do their best not to provide such confidential material should it be subpoenaed.**)
- 심사자의 이해관계 고지의무 확인
- 편집진의 이해관계 여부 확인

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전문가심사자 윤리

- 심사대상 논문에 대한 편견 또는 경쟁심리에 의한 결정
- 논문내용의 개인적 활용
- 이해관계 저촉에 유의
- 고의적 지연
- 익명성 훼손

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전문가심사의 유형

- **Single Blind (Conventional) Review:** 심사자명만 가림. 저자명을 공개. 60%
- **Double Blind (Masked) Review:** 심사자명, 저자명 모두 가림. 40%
- **Open Review:** 심사자명, 저자명 노출

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원고심사의 실제

1. 심사수용 회신
2. 일차 읽기
3. 이차 읽기
4. 의견서 작성
5. Down to Earth Method

Hoppin FG. Am J Respir Crit Care Med 2002

전문가심사의 평가항목

- Importance of research questions
- Originality: 논문의 원저성
- Structure: 논문 구성요소와 완성도
 - Title, Abstract, Introduction, Methodology, Results, Discussion, References, Figure/Tables
- Language
- Previous Research
- Ethical Issues

전문가심사의 기본원칙

- 독자의 입장에서 논문을 검토
- 학술지의 학문적 정보전달에 도움
- 편집인의 등재여부 결정에 필요한 분명한 의견 제시
- 지적 또는 거부시 이에 대한 근거 제시
- 의사결정에 필요한 시점까지 회신
- 저자에게 도움이 되는 수정 의견 개진

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전문가심사자의 유형

- 무반응형: 회신하지 않음
- 무성의형: 읽지도 않고 판정
- 기본형: 형식적으로 읽고 최소한 의견 제시
- 과잉형: 지나치게 많은 수정을 요구하거나 논문을 새로 써주는 유형
- 적절형: 내용과 표현을 모두 적절하게 검토하여 신속하게 의견제시

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우수한 심사자의 자세

- 편집인과 팀웍을 형성, 역할 분담
- 사심없이 학문발전에 기여
- 독자의 안목으로 읽고 평가
- 저자의 입장을 고려한 의견의 예의바른 표현
- 논문에 도움이 되도록 의견을 제시
- 나에게도 도움이 되는 기회로 활용
- 의견을 자세하게, 분명하게 개진
- 최근 중요한 문헌 인용을 점검
- 특히 같은, 유사한 내용의 논문 정보 제공
- 주어진 기간 내에 회신

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심사의견 통보의 유의점

- 편집인이 보낸 양식을 사용
- 학생을 지도하는 것이 아니라 동료의 연구결과물 평가임을 인식하여 표현하여야 함
- ‘실험을 다시 하라, 논문을 다시 작성하라’는 표현은 삼가해야
- 학술적인 문제점을 분명하게 지적
- 대안을 제시하면서 지적해야
- 심사자도 심사결과 의견서에 의하여 평가를 받는다는 사실을 인식

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편집인 입장에서 보는 전문가심사 시행의 문제점

- 우수한 심사자 수의 부족
- 논문의 부족 또는 과다
- 유명, 거물 전문가의 심사 부실
- 심사자에 따라 과도한 시간 소요
- 저자의 반발
- 심사자와 저자 간의 갈등
- 편집인과 저자의 갈등
- 전문편집인의 부재
- 전문가심사 문화 정착

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전문가심사 제도의 보완책

- 우수한 심사자의 적극 발굴
- 젊은 인력의 교육과 훈련을 통한 심사자 풀을 확대
- 다른 학회 인력의 심사 활용
- 영문논문의 경우 외국인 전문가 활용
- 심사자에 대한 적절한 제도적인 되먹이기
- 심사의견에 대한 저자의 반론제기를 인정
- 심사자에 감사 표시 등 예우

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심사의견에 대한 저자의 입장

- 내용을 잘 모르면서 잘못된 심사의견을 준다
- 내 논문에 대해 감히 누가 뭐라고 ...
- 시험보고 채점한 답안지를 보는 기분이다
- 남의 논문을 열심히 읽고 좋은 의견을 주어 도움이 크다
- 경쟁자에게 원고가 넘어가서 자칫 불이익이?
- 논문출판의 가장 큰 고개를 넘었다!

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국내 원저 논문 심사 주요 지적사항1

- 창의성이 없는 추시 논문이다
- 결론이 없거나 주장이 없다
- 목적과 결론이 다르다
- 논리가 빈약하거나 비약이 있다
- 중복된 기술이 많이 있다
 - 서론과 고찰
 - 도표와 본문
- 문장이 길다
- 문장을 읽기 어렵다

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국내 원저 논문 심사 주요 지적사항2

- 주제와 관련없는 기술
- 교과서적 내용이 기술
- 자료의 제시가 부적절
- 문헌의 인용이 과다
- 오래된 문헌 인용
- 문헌 인용의 오류
- 중요한 문헌을 인용하지 않음
- 문헌의 간접인용이 많다

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게재불가 판정 원인

- 원저의 학문적 독창성 결여
- 중복출판, 표절
- 결과의 해석오류 : 대조군 미설정
- 논리의 비약
- 논문의 근본적인 작성오류
- 논문의 학문 분야(scope) 부적합
- 증례의 희귀성 결여

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학술지가 전문가심사를 잘 하려면

- 편집인이 주문을 분명하게 해야 하고 심사자가 이를 숙지해야
- 학회 회원들 (심사자, 저자, 독자)의 이해와 동의가 필요함
- 심의자 풀을 확대하는 노력이 필요(외국인 활용)
- 시스템으로 운영함
- 젊은 회원의 훈련: 논문 작성, 논문 비평, 전문가 심사 등
- 최근 지건을 확인하여 반영해야
- 주제어 검색을 통한 점검

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전문가심사 결과판정의 고려요소

- 독자에게 흥미있는 주제인가?
- 창의성있는 원저나 증례보고인가?
- 처음 출판되는 추시논문인가?
- 창의성은 없어도 기록물로의 가치가 있는가?
- 외국에서도 인용할만한 내용인가?
- 게재거부의 경우 근거가 무엇인가?
- 수정하여 출판할만한 가치는 무엇인가?

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Peering into review

The peer review process can be frustrating to researchers eager to get their work published. Changes to the process might be warranted—but only if they are based in fact, not conjecture.

Confidential peer review is a cornerstone of the publication process in science, but is not without its drawbacks. A recent open letter (<http://www.eurostemcell.org/commentanalysis/peer-review/>) signed by 14 stem cell researchers to the editors of major scientific journals cites “unreasonable or obstructive reviews” and the fact that “publication of truly original findings may be delayed or rejected” as reasons to refine our current practices. To increase transparency, the signatories suggest appending reviewers’ comments and editorial correspondence alongside published manuscripts.

This is not the first time scientists have voiced concerns over the peer review process, and, consequently, at *Nature Medicine* a number of mechanisms are in place to prevent bias. The editorial team discusses every paper before referees are chosen and articles are sent out for external review. In a given year, we draw upon hundreds of reviewers to assess manuscripts. Including both established researchers and young investigators, our pool of reviewers is in constant flux, preventing a too-small number

and drawbacks of the studies and to outline standards of excellence for publication in the field. This discussion allows us to refine our editorial guidelines and identify what to expect from future submissions (both in technical and conceptual terms).

The authors of the open letter advocate for more profound changes, including publication of supplementary files containing anonymous referees’ comments and relevant editorial communication. These measures might allow readers to appreciate how the review process has shaped a paper and might provide insight into the peer review process to younger investigators. These benefits notwithstanding, a number of lingering concerns prevent us from endorsing this strategy.

Publication of referees’ comments in full may affect the quality of the reviews, leading to more cautious and restrained comments. It is difficult to ascertain how much the quality of reviews would be compromised by adopting these measures; however, previous attempts with open peer review suggest that referees are less likely to provide a direct and detailed evaluation of the report. Authors may also be reluctant to adopt this

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Reviewing the Reviewers: Comparison of Review Quality and Reviewer Characteristics at the *American Journal of Roentgenology*

**AJR 2005; 184:
1731**

**Kliwer MA et
al.**

OBJECTIVE. The purpose of our study was to determine which manuscript reviewer characteristics are most strongly associated with reviewer performance as judged by editors of the *American Journal of Roentgenology* (*AJR*).

MATERIALS AND METHODS. At the *AJR*, manuscript reviews are rated by the journal editors on a subjective scale from 1 (lowest) to 4, on the basis of the value, thoroughness, and punctuality of the critique. We obtained all scores for *AJR* reviewers and determined the average score for each reviewer. We also sent a questionnaire to 989 reviewers requesting specific information regarding the age, sex, radiology subspecialty, number of years serving as a reviewer, academic rank, and practice type of the reviewer. The demographic profiles were correlated with the average quality score for each reviewer. Statistical analysis included correlation analysis and analysis of variance modeling. Reviewer quality scores were also correlated with the scoring of individual reviews and ultimate disposition of 196 manuscripts sent to the *AJR* during the same period.

RESULTS. Responses to the questionnaire were obtained from 821 reviewers (83.0%), for whom quality scores were available for 714 (87.0%). Correlation analysis shows that the quality score of reviewers strongly correlated with younger age ($p = 0.001$). A statistically significant correlation between quality score and practice type was seen ($p = 0.008$), with reviewers from academic institutions receiving higher scores. No significant correlation was found between quality score and sex ($p = 0.72$), years of reviewing ($p = 0.26$), academic rank ($p = 0.10$), or the ultimate disposition of the manuscript ($p = 0.40$). The quality score of the reviewers showed no variation by subspecialty ($p = 0.99$).

CONCLUSION. The highest-rated *AJR* reviewers tended to be young and from academic institutions. The quality of peer review did not correlate with the sex, academic rank, or subspecialty of the reviewer.

Fake Reviewer

Retraction Watch

Tracking retractions as a window into the scientific process

South Korean plant compound researcher faked email addresses so he could review his own studies

with 48 comments

Scientists frustrated by the so-called "third reviewer" — the one always asking for additional experiments before recommending acceptance — might be forgiven for having fantasies of being able to review their own papers.



Hyung-In Moon

But one Korean scientist, [Hyung-In Moon](#), managed to do just that, through what must have seemed like clever subterfuge at the time. And he got away with it for a while — until he didn't, as witnessed by this retraction [notice](#) for "Larvicidal activity of 4-hydroxycoumarin derivatives against *Aedes aegypti*," published in *Pharmaceutical Biology*, an Informa Healthcare title:

“ The peer-review process for the above article was found to have been compromised and inappropriately influenced by the corresponding author, Professor HI Moon. As a result the findings and conclusions of these articles cannot be relied upon.

The corresponding author and the publisher wish to retract these papers to preserve the integrity of material published in the journal. The publisher acknowledges that the integrity of the peer review process should have been subject to more rigorous verification to ensure the reviews provided were genuine and impartial. The publisher apologizes for any inconvenience

Pages

- [About Adam Marcus](#)
- [About Ivan Oransky](#)
- [The Retraction Watch FAQ, including comments policy](#)
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28건은 지인이 심사 7건은 실형 조작

기사 **매경닷컴 창사 14주년 감사이벤트**

국제 학술지에 투고한 논문을 문인과 지인이 심사했다는 이유로 28건의 논문이 철회된 지방대 교수가 이번에는 수년 전 실형 조작으로 7건의 논문을 철회했던 것으로 밝혀졌다. 당시자는 적절한 절차를 거쳐 철회된 논문이라고 해명했지만 학계에서는 한 교수의 논문 35건이 윤리적 문제에 연루된 것은 이해할 수 없다는 반응이다. 학교 측은 적절한 조사를 통해 처벌하겠다는 뜻을 밝혔다.

17일 논문 철회 표절 감시 사이트인 "리트랙션 왓치"에는 동아대 의약생명공학과 문형인 교수의 논문 7건이 실형 조작으로 철회했다는 글이 게재됐다. 지난해 24일 리트랙션 왓치는 "문 교수가 e메일 주소록 조작해 자신의 논문을 검토한 것이 밝혀져 28건의 논문이 철회됐다"고 밝힌 바 있다. 이로써 문 교수가 철회한 논문은 총 35건으로 늘어났다. 문 교수는 동아대에서 한의학초학과 의약품분석학실형 등을 가르치고 있다.

이번에 철회된 논문은 학술지 '효소저해와 의학화학'지에 게재된 7편이다. 문 교수는 "실형 오류를 결과로 착각해 썼던 논문을 철회했을 뿐"이라며 "리트랙션 왓치가 마치 새롭게 발표된 것처럼 글을 게재했지만 이미 수년 전에 공지된 내용"이라고 해명했다. 문 교수는 논문의 심사과정에서 발생한 부정행위에 대해서도 "실프 심사는 하지 않았다"며 "연구자 수가 적은 문이라 친분이 있는 지인을 심사자로 추천한 것이 잘못일 뿐"이라고 덧붙였다.

하지만 학계는 한 연구자가 쓴 논문 35건이 무더기로 철회된 것은 이해할 수 없다는 반응이다. 한 사람대 교수는 "교수가 쓴 논문 중 35건이 철회했다면 실수라고 보기 힘들다"며 "철저한 조사가 있어야 한다"고 지적했다.

동아대 수학과 권영철 교수(교무처장)는 "현재 당과대 학과

인기뉴스

- 1 29년간 성안계 하지 않은 남편, 아내 못잡고 ..
- 2 나체출발삼 - 신동 성매매 적발
- 3 "너 오늘 너무 예쁘다" 직장 동료 향한 속내 ..
- 4 유니폼의 사회적, 세련미 '교과합성' vs 직 ..
- 5 불면제 희극화, 학생교적 '성취'에 이 말 ..
- 6 내안음모 혐의 아역기 의결 첫 공판-법원 앞 ..
- 7 여직원에 골대없는 카톡도 성희롱
- 8 30대 주부 '혹로' 남편 있는 마보 시어머니 ..
- 9 妻 끼어들기 괴리물기 29일부터 과태료
- 10 '내안음모' vs '근거없다'... 첫 격돌

GiC
Gyeonggi International Ceramic Biennale
2013 경기세계도자비엔날레



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 off- subscriptions. Most of the players are murky. The identity and location of the journals' editors, as well as the financial work-

Downloaded from www.sciencemag.org on October 9, 2013

SCIENCE 4 October 2013 vol 342: 60-65.

Fake Journal?

우수한 전문가심사의견서

03-Oct-2008

Dear Prof. Hong:

Manuscript ID PR-2008-0457 entitled

"Identification of two β -tubulin isotypes of *Clonorchis sinensis*" which you submitted to the *Parasitology Research*, has been reviewed. The comments of the reviewer(s) are included at the bottom of this letter.

The reviewer(s) have recommended publication, but also suggest some minor revisions to your manuscript. Therefore, I invite you to respond to the reviewer(s)' comments and revise your manuscript.

대한의학회 간행위원장 홍 성 태

To revise your manuscript, log into <http://mc.manuscriptcentral.com/parasite> and enter your Author Center, where you will find your manuscript title listed under "Manuscripts with Decisions." Under "Actions," click on "Create a Revision." Your manuscript number has been appended to denote a revision. You will be unable to make your revisions on the originally submitted version of the manuscript. Instead, revise your manuscript using a word processing program and save it on your computer. Please also highlight the changes to your manuscript within the document by using the track changes mode in MS Word or by using bold or colored text. Once the revised manuscript is prepared, you can upload it and submit it through your Author Center. When submitting your revised manuscript, you will be able to respond to the comments made by the reviewer(s) in the space provided. You can use this space to document any changes you make to the original manuscript. In order to expedite the processing of the revised manuscript, please be as specific as possible in your response to the reviewer(s). IMPORTANT: Your original files are available to you when you upload your revised manuscript. Please delete any redundant files before completing the submission. Because we are trying to facilitate timely publication of manuscripts submitted to the Parasitology Research, your revised manuscript should be uploaded as soon as possible. If it is not possible for you to submit your revision in a reasonable amount of time, we may have to consider your paper as a new submission. Once again, thank you for submitting your manuscript to the Parasitology Research and I look forward to receiving your revision.

Sincerely,
Prof. Bill Chobotar
Managing Editor, Parasitology Research
chob@andrews.edu

COMMENT FROM THE EDITOR:
ONE ADDITIONAL MINOR ITEM NEEDS TO BE ADDRESSED. THE PUNCTUATION WITH REGARD TO THE AUTHOR CITATION IN THE BODY OF THE MANUSCRIPT NEEDS TO BE CHANGED TO CONFORM TO THE PARASITOLOGY RESEARCH FORMAT. FOR THIS I WILL SEND, IN A SEPARATE EMAIL, A PAGE FROM PARASITOLOGY RESEARCH FOR AN EXAMPLE. PLEASE MAKE THE CHANGES THROUGHOUT THE MANUSCRIPT

Reviewer(s)' Comments to Author:

Reviewer: 1

Comments to the Author

Identification of two beta-tubulin isotypes of *Clonorchis sinensis*
Shunyu Li, Sung-Jong Hong, Min-Ho Choi and Sung-Tae Hong

This article reports on two beta-tubulin isotypes expressed in adult *Clonorchis sinensis*. As yet few trematode beta-tubulin sequences have been reported and thus their work will be of interest to some parasitologists, although the new data presented is limited. There are a substantial number of queries and issues that need to be addressed before the work can be published.

General comments.

In some parts of the article CstB1 and CstB2 refer to the cDNA nucleotide sequence (eg in the abstract) in others the the tubulin isotypes that are encoded by them (eg page 7, line 16). This should be clarified and corrected throughout the article

Queries and Comments

Abstract

Page 2

L2; Was it not a PCR product synthesized using degenerate primers that was used for screening the cDNA library and identified clones that encoded the two tubulin isotypes, CstB1 and CstB2?. In addition the fact that the cDNA library was generated from adult *C. sinensis* should be included

L10; phylogenetic analysis of selected sequences.

L11; are contained within one group

L12-L14; This last sentence is not necessary and could be omitted.

Introduction

Page 4

L5; have been multiple isotypes expressed within some species
L8 -10; To my knowledge, it has not been shown that the various *Echinococcus multilocularis* isotypes result in the benzimidazole (BZ) susceptibility or resistance status rather than they have some characteristics of other BZ-susceptible or resistant beta-tubulins.

Materials and Methods

Page 5

L11; (Title to section) cDNA cloning: should this not be Identification of full-length *Clonorchis sinensis* cDNA clones?

L12; The sequence of the degenerate primer set should be given

Page 6.

L9; Stringency condition used during filter washing should be given since this may affect the number of beta-tubulin-coding clones that were revealed eg would variant tubulin coding sequences be detected under the conditions used? Also, the number of plaques screened should be included to give an indication of the probability of detecting clones encoding weakly expressed beta-tubulins.

L11; Helper phage not Heperphage.

Results

Page 7

L17; (end of line): were silent, resulting in no amino acid changes

L18; Does the figure (78% identity in nucleotides) refer to the coding sequence or to the entire cDNA transcript including the non-coding regions? The authors state on page 8 that the coding region has the same level of nucleotide identity. This would imply that the non-coding regions have 78% sequence similarity.

Page 8

L12-16; Most of the parasites used in the comparison express more than one tubulin isotype. The authors should state which one is being used in their analysis or refer to the figure legends if included here (see comments under Figure legends). They should also indicate that their alignments and phylogenetic tree apply only to those sequences that have been selected for study. Some of the parasites that have been included encode variant isotypes which, if used, may give rise to a different tree.

L16; As far as I am aware the GTP binding domain is not Met~~Arg~~-Glu-Ile (see Lowe et al. J. Mol. Biol. 2001, 313, 1045-1057). This should be corrected or a reference put in for the statement.

Page 9

L1; In Fig. 2 the full C-terminal end of the *F. hepatica* sequence has not been given. This has recently been published and the authors have referred to the paper later in the text. This should be corrected.

L3; The beta-tubulin gene lacked the mutation giving rise to tyrosine at position 200 of the encoded protein. In addition only one *E. multilocularis* isotype was included in the alignment in Figure 2 although three are discussed.

Phylogenetic analyses section (starting at Line 11)

General comment: I found this section confusing to read. It would help if the authors indicated the groupings that they are referring to by drawing boxes round them on the tree presented in Fig. 4. They do not describe where the *C. sinensis* isotypes (the topic of the paper) are grouped in the tree. This is important and the whole point of the phylogenetic analysis. In Fig. 4, the bootstrap analysis figures should also be given (at least those that provide confidence for the branch points).

Discussion

Page 10

L5; The C-terminal ends, although similar, are not completely conserved

L7; The sentence beginning with The two clones should be removed. This information has already been given in sentence two of the discussion.

L8; UTR has not been defined

L9; perhaps This should be inserted before the beginning of the sentence Most of the additional nucleotides ----. Also the sentence beginning All of the cDNA sequences -- could be removed.

L13; The sentence beginning This difference between amino acids-- is incorrect and should be removed

Page 11

L2; the major target of the drug is thought to be beta-tubulin. As far as I am aware no conclusive proof has been obtained for this. It would be of interest to know if triclabendazole is effective against *C. sinensis* since they comment on the high similarity of their isotypes with two of the *Fasciola* isotypes. Also, somewhere in the manuscript they should indicate if these two trematode tubulins have the same C-terminal ends.

L14-L16; The authors indicate that the *C. sinensis* isotypes group with the trematodes and vertebrates in the tree because their C-terminal ends are conserved. The alignments and phylogenetic trees rely on comparison of all of the sequence not just their C-terminii.

Presumably the phylogenetic trees were largely based on the sequences shown in the alignments. The data presented in Fig 2 show substantial variability between the C-terminii of trematode (eg *Fasciola*) and the human sequence that have been included. These factors need to be addressed.

Page 13

L8-10; Although the authors have identified two isotypes of beta-tubulin that are expressed in adult *C. sinensis* there may well be others that they have not identified by screening their cDNA library (depending on the number of plaques examined, their expression levels and their screening conditions). In addition others may be expressed at other stages of the life-cycle. They may wish to comment on these points either here or elsewhere in the article.

Figures and Legends

Fig 1; Indicate what type of size markers are included in the figure and give an indication of the sizes of some of them on the diagram

Fig 2; The authors should indicate the full identity of the isotypes used in the analysis and give their accession numbers. They should also indicate what the boxed residues represent in the alignment.

Fig 3; Again the full identity of the residues should be given. There are at least four boxes shown in the diagram. The authors should indicate what all of them represent.

Fig 4; The authors do not give the accession number of the *Fasciola hepatica* or *Schistosoma japonicum* sequences in the legend. These need to be included.

Reviewer: 2

Comments to the Author

Parasitology Research: Li et al. manuscript (PR-2008-0457).

Comments to authors.

This was a useful paper, extending our appreciation of the β -tubulin isotypes in trematodes. The technical aspects of the study seem to have been carried out carefully, but the significance and appreciation of the data are somewhat confused. The authors need to address some of the issues outlined below before the manuscript can be accepted for publication.

I have a number of specific points:

p.4, lines 8-10. How do we know the drug susceptibility of the β -tubulins in *E. multilocularis*? This point needs to be clarified.

Purpose of study was not made very clearly.

Fig. 2. Which *F. hepatica* isotype was used? This needs to be made clear.

Fig. 3. What was the purpose of this Fig.? ie Why were the Clonorchis sequences compared to nematodes?

Fig. 4, phylogenetic analysis. What are the authors trying to show? It would have been better to have carried out an analysis along the lines of Fig. 2 in the paper by Ryan et al. (2008). That would have shown where the Clonorchis sequences fitted in, in comparison with other platyhelminths and other helminths, etc.

p.11, line 2. Comment about β -tubulin being a drug target in *F. hepatica*. The data covering this idea needs to be discussed.

p.11, lines 3-4. Comment about the relationship between resistance and isotype variation being uncertain?needs to be clarified.

p.12, second paragraph. There appears to be some confusion about the link between amino acids at positions 167 and 200 and BZ resistance in *F. hepatica* and *C. sinensis*. In *F. hepatica*, there seems to be no difference between the β -tubulins of TCBZ-resistant and TCBZ-susceptible flukes. This may explain why *F. hepatica* is naturally refractory to common BZs and why such BZs have little activity against the fluke. I suggest that the authors read the paper by Robinson, M.W. et al. 2004. J. Mol. Graph. Model. 23, 275-284 for a better appreciation of the data.

Review Comments on 2011-0000 1

Major comments 1 Reject

The authors studied epidemiology and clinical characteristics of *C. difficile* infection in a Korean tertiary hospital. It is meaningful as a prospective study, but the **major drawback of this manuscript is contents are not new**. There were also other articles about epidemiology, clinical features, prognostic factors, results of treatments, recurrence rate, toxin gene typing and antibiotic resistance patterns with Korean cases (Gut and Liver, 2010;332-337, KJ of Gastroenterology 2010;175-182, Infection and Chemotherapy 2007;71-77, KJ of Gastroenterology 2010;169-174, KJ of Gastroenterology 2009;13-19, KJLM 2010;491-7, etc.). Even the incidence of CDI in Korea showed increasing tendency was revealed in several studies, although they were retrospective studies.

Treatment outcomes were described according to treatment regimens (metronidazole vs. vancomycin) and numbers of episodes. After recurrence or metronidazole failure, therapy regimens are usually changed. Therefore the cases should be re-classified according to the results of therapy responses (Table 4).

AAD was defined when patients had diarrhea and there was no reason for diarrhea except antibiotic use (by ref. and authors). However, it's not easy to rule out the cases because many pathogens causing diarrhea are not routinely detected in the Lab. How can authors define or rule out AAD? Please describe it in detail.

Review Comments on 2011-0000 2

Major comments 2 Reject

The authors wrote they performed toxinotyping method (line 98-105). However, no results were found about it and remarks about 027 strains were briefly mentioned in line 186-187 and line 257-258 without data. Explain data and results about toxinotyping, or delete them.

.

Minor comments

The paper would benefit from a clearer explanation of the data.

Line 15-16; toxin A-toxin B+ or binary toxin ---15.4% or—is not propriately described because binary toxin is a different toxin from toxin A and B .

The combined expression of toxin A,toxin B+, and/or binary toxin should be written.

Ex; toxin A+toxinB+CDT+, toxinA-toxinB+CDT-

Line 32-34; few data? . There were actually more than 20 articles associated CDI in Korea.

Line 51; tcdA, tcdB cdtA or cdtB by multiplex PCR—is inappropriately described. As mentioned above, binary toxin is another toxin.

Review Comments on 2011-0000 5

Major comments 3 Publish as it is

This is a very interesting paper and important paper about the clinical characteristics of *C. difficile* associated colitis in Korea.

I have no major points

Author Reply 1

Dear Prof. Sung-Tae Hong
Editor-in-Chief, Journal of Korean Medical Science

We are pleased that our manuscript is being considered for publication in JOURNAL OF KOREAN MEDICAL SCIENCE. We thank the reviewer for the constructive and helpful comments concerning the manuscript. We have addressed the reviewer's concerns by revising the manuscript. The point-by-point replies are given in this letter. We hope that we have addressed satisfactorily all concerns raised by the reviewer, and that this manuscript is now suitable for publication.

Thank you again for the comment.

Sincerely yours,
Correspondence to Dr. OOOO

Author Reply 2

<Response to reviewer>

(Comment 1) The introduction is lengthy and redundant. It may be abbreviated with core contents. For example, the second paragraph may be deleted.

(Response 1) We absolutely agree with the referee's comment. As the referee pointed out, we changed the "Introduction" section as the referee suggested.

(Comment 2) The authors tried to show difference between RA patients and controls with different formulas. However, current golden standard of the periarticular osteoporosis is the intuitive reading by an expert radiologist. Therefore, it may be more appropriate to compare the calculated values with the golden standard which was determined by the expert radiologists.

(Response 2) We think the referee pointed out an important issue. We added new figures (Figure 2 in revised manuscript) and also another paragraph (on page 9 in revised manuscript). We highlighted the changes within the manuscript using a yellow band.

Author Reply 3

(Comment 3) The control group is very heterogeneous. It may be more reasonable to compare RA group with healthy control groups and disease control groups separately.

(Response 3) We absolutely agree with the referee's comment. As the referee pointed out, we divided overall controls (n = 106) into two groups (healthy control (n = 32) and disease control (n = 74)). As showed in revised table 2 and revised result section on page 9, our equation values representing periarticular osteoporosis were not different between the two groups. We highlighted the changes within the manuscript using a yellow band.

(Comment 4) Page 5. Evaluation of Hand X-ray Images by Physicians. The conditions with which radiograph taken should be described in more detail. For example, the name of radiographic machine, exposure time etc.

(Response 4) As the referee pointed out, we added the detailed information of the radiograph and associated condition in the "Materials and Methods" section. We highlighted the changes within the manuscript using a yellow band.

대한의학회 간행위원장 홍성태

Good Reviews

1. Did the reviewer **discuss the importance** of the research question?
2. Did the reviewer **discuss the originality** of the paper?
3. Did the reviewer clearly **identify the strengths and weaknesses of the method** (study design, data collection and data analysis)?

대한의학회 간행위원장 홍성태

Good Reviews

4. Did the reviewer make specific **useful comments on the writing, organization, tables and figures** of the manuscript?
5. Were the reviewer's **comments constructive**?
6. Did the reviewer **supply appropriate evidence** using examples to substantiate their comments?

대한의학회 간행위원장 홍성태

Good Reviews

7. Did the reviewer comment on the **author's interpretation of the results**?
8. How would you **rate the quality** of this review overall?

대한의학회 간행위원장 홍성태

Problems of Peer Review

- Costly & time-consuming
- Inconsistent, low transparency
- Biased by individuals
- Open Access publication: poor peer review

대한의학회 간행위원장 홍성태

Cascading Peer Review

- Review model by redirecting peer-reviewed paper
- From top journal to lower-tier or spin-off journal
- Automated manuscript transfer: **peer review consortium**
- Author's agreement

By Barroga EF. Cascading peer review for open-access publishing.
Eur Sci Editing 2013; 39(4): 90-1.

대한의학회 간행위원장 홍성태

전문가심사의 평가

- Scoring of Reviewers by ICMJE/WAME
 - 1 unacceptable effort and content
 - 2 unacceptable effort or content
 - 3 acceptable
 - 4 commendable
 - 5 exceptional, hard to improve

대한의학회 간행위원장 홍 성 태

Results of PR in JKMS

- In 2009
- a reviewer pool (n = 1889)
- substantial variability in quality

대한의학회 간행위원장 홍 성 태

2009년도

- Among 1136 manuscripts, 84/1136 (7.4%) were rejected on the Editorial desk
- Remaining 1052 (92.6% of 1136), reviewed by expert peers
- 870 (82.7% of 1052) manuscripts:
rejected after considering the peer reviews
- Overall reject rate : $954 (84 + 870) / 1136, 84.0\%$

대한의학회 간행위원장 홍 성 태

The total of peer reviews : 1905

- A total of 1905 peer reviewers,
to review the 1052 manuscripts
(1.81 reviewer / manuscript)

대한의학회 간행위원장 홍 성 태

736 experts denied participating

- Among the 1905 invitations, 736 experts denied participating in the review (736/1905, 38.6%).
 - disagreement to review : 395 (395/1905, 20.7%)
 - no response within 2 weeks : 341 (341/1905, 17.9%)

대한의학회 간행위원장 홍 성 태

The time to receive the review results

- Analysis of resulting 1169 peer reviews
- 16.05 ± 17.8 (mean \pm s.d.) days

대한의학회 간행위원장 홍 성 태

Scores of Review Quality

N = 1009, 3.07 ± 0.67

- 1 : unacceptable effort and content, 21
- 2 : unacceptable effort or content, 130
- 3 : acceptable, 616
- 4 : commendable, 240
- 5 : exceptional, hard to improve, 2

대한의학회 간행위원장 홍성태

Response Rate

- A tendency to decrease with age
- No difference in the response rate, delay time and score of review quality
 - between genders, affiliations or specialties

대한의학회 간행위원장 홍성태

Conclusion

- Quality of peer reviews in JKMS
: an acceptable degree
- Young age of peer reviewers
: a better response rate

대한의학회 간행위원장 홍 성 태

Major Concerns

- High rate of denial after reviewer invitation
- Systematic process for assessing peer reviewers
 - using response rate, delay time and scoring of review quality

대한의학회 간행위원장 홍 성 태

Suggestions

- Feedback to peer reviewers
- Education programs for “good review”
- Art of review
- Publication ethics

대한의학회 간행위원장 홍성태

Elsevier 학술지의 편집인 Advice

- **Be critical**
- Justify all criticisms by references
- Check ‘Aims and Scope’
- Clear recommendation
- Number your comments
- Be specific: page, line
- Be careful not to identify yourself

대한의학회 간행위원장 홍성태

From Editor of the Am J Med 1

- **Professional honor**
- Ensure the subject is within your purview of expertise
- Read the abstract first
- Original? Reproduced?
- Examine tables and figures
- Statistical analysis: sufficient number
- Methods: reliable laboratory supports
- Discussion makes sense: unnecessary conjecture, redundant statement, unfounded conclusion

대한의학회 간행위원장 홍성태

From Editor of the Am J Med 2

- Manuscript: concise, well-organized
- See quality of figures or photos
- Following the 'Instructions to Authors'
- Not blinded: reputation of institution, potential conflicts of interest
- Documentation of IRB & informed consent, IACUC
- Typographical errors / mistakes in references?
- Believe or suspect? Any scientific fraud or duplicated?

대한의학회 간행위원장 홍성태



Sung-Tae Hong
Seoul National University,
College of Medical Sciences,
Department of Parasitology and Tropical Medicine,
Seoul 151-747
South Korea

Dear Dr Sung-Tae

I am delighted to recognise the contribution you have made to "Parasitology International" and enclose your certificate as a **Top Reviewer in 2011**

Reviewers play an essential part in science and in scholarly publishing. For more than 300 years, scientists and scholars have relied upon peer review to validate research, engage other specialists in the support of submitted work, and increase networking possibilities within specific specialist communities.

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Yours sincerely,

Dale Seaton,
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