a department head more likely than other senior faculty to be selected as an honorary author? What role does a department head play in the whole process of a radiologic study? What makes the department head likely to be an honorary author? What recommendations may further decrease the prevalence of honorary authorship in the radiologic literature?

As we ponder these issues, I hope we can develop a better understanding of how to set up a fairer environment for authors that will reduce the negative effects of honorary authorship.

Disclosures of Potential Conflicts of Interest: No potential conflicts of interest to disclose.

Reference

1. Eisenberg RL, Ngo L, Boiselle PM, Bankier AA. Honorary authorship in radiologic research articles: assessment of frequency and associated factors. Radiology 2011;259(2):479–486.

Response

From
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We thank Dr Liang for his interesting comments concerning our work (1). The use of the adjective "perceived" to describe honorary authorship simply reflects the subjective nature of the survey, which was sent to only one author of each published manuscript. Based on our methodology, we are relying on the perception of the survey respondent and do not have "scientific proof" of individual author contributions (or lack of contributions).

Our work was descriptive in nature and represents the first of several iterative steps in the analysis of honorary authorship. As such, it cannot (and was never designed to) answer the questions raised by Dr Liang. We acknowledge the importance of these questions and have offered several potential answers in our article. As was noted in a previous study cited in our article, honorary authorship was "primarily attributed to individuals who had some control over the first author by virtue of either fear or obligation" (2). Consequently, it is not surprising that junior, nontenured investigators often give honorary authorship to their section and department heads, who control work assignments, allocate research time, and are needed to support any promotion to higher academic rank. Evidence of this assumption is provided by Bhandari et al (3), who concluded that the increased number of authors of original articles primarily reflected the proliferation of authorship among professors and department chairs. In manuscripts submitted to journals that do not use a double-blind reviewing system, junior investigators may offer honorary authorship to senior staff members in the hope that their names may give weight to the article and increase the likelihood that it will be accepted, even though this practice has been described as among "acts generally considered to be instances of serious misconduct" (4).

Finally, we agree with Dr Liang that new recommendations to reduce the prevalence of honorary authorship may be required. According to the results of our study, one simple solution would be for section and department heads of academic departments to agree to serve as contributing authors only for studies for which they have made substantial contributions in accordance with International Committee of Medical Journal Editors (ICJME) guidelines. In this way, these leaders would not only avoid the potential personal perils of honorary authorship but would also serve as role models for compliance with ICJME guidelines for more junior department members under their supervision and guidance.

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Biliary Atresia in Neonates and Infants

From
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Editor:

We read with interest the article titled "Biliary Atresia: Color Doppler US Findings in Neonates and Infants" by Lee and colleagues (1) in the July 2009 issue of *Radiology*. However, we noticed a few discrepancies that can leave the readers baffled. To ensure that the contents of this article can be used for future research, it is desirable to have clarification on these issues.

First, in the last paragraph of the section describing patients in Materials and Methods, consecutive sentences were contradictory to each other, with the first mentioning that bilirubin levels had not been measured in the control group and the very next sentence stating that bilirubin values were measured in 10 of 19 control subjects. It appears from table 1 that the bilirubin values were indeed measured in the control group in a few babies.

Second, in another paragraph in Materials and Methods it was stated that the 35 patients with idiopathic hyperbilirubinemia, neonatal hepatitis, total parenteral nutrition, nonsyndromic interlobular bile duct paucity, Alagille syndrome, and portal vein thrombosis received a diagnosis of biliary atresia (BA). It should probably have been "non-BA."

Third, it was also mentioned that images from color Doppler ultrasonography (US) were not included for four patients. The same paragraph mentions that US was performed in 64 patients with neonatal cholestasis. It was not clarified if US examinations were actually performed in 68 patients with complete exclusion of these four patients from the study or if the gray-scale US findings in these four patients were actually included, with exclusion of only the images from color Doppler US.

Discrepancies like these need serious looking into. Some might have been due to the printing error (the second issue, probably), whereas others might have crept in at various stages during original writing, proofreading, or manuscript revision. These incidents underscore the need for careful scrutiny of galley proofs by the authors.

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Reference

1. Lee MS, Kim MJ, Lee MJ, et al. Biliary atresia: color Doppler US findings in neonates and infants. Radiology 2009;252(1):282–289. [Published correction appears in Radiology 2011;261(3):1003.]

Response

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We thank Dr Krishna and colleagues for their interest in our article (1). As they mention, it is important to check the articles before they are published. However, no matter how many times manuscripts are reviewed, there is the possibility for errors to occur.

In the submitted version of our article, we wrote "we did not check total and direct bilirubin levels in all neonates and infants in control group because they did not have biliary tree or liver diseases. Among 19 neonates and infants in control group, we checked total and direct bilirubin levels in eight and two neonates and infants, respectively." We meant that some of patients had a test of bilirubin levels and others did not. This was changed during the copyediting process to "We did not check total or direct bilirubin levels in any neonate or infant in the control group…." This change was not exactly what we meant.

For the second comment, we agree with Krishna and colleagues that the descriptor used should have been "non-BA." In our submitted manuscript, we wrote "the remaining 35 patients were diagnosed by clinical, imaging, and laboratory studies and pathologic evaluation, as follows: idiopathic hyperbilirubinemia, neonatal hepatitis, total parenteral nutrition–induced cholestasis, nonsyndromic paucity of interlobular bile duct, Alagille syndrome, and portal vein thrombosis." This sentence was also altered during the copyediting process to read: "The remaining 35 patients (26 boys, nine girls; mean age, 48 days ± 32) received a diagnosis of BA on the basis of the results of clinical, imaging, and laboratory studies (n = 25) and pathologic examination (n = 10), as follows…." We did not notice this error in the final version of the manuscript.

For your third comment, we do describe in our article Materials and Methods the four patients who were excluded due to inability to cooperate for the Doppler US examination. These four patients were diagnosed as having BA finally. But we could not perform color Doppler US in these patients. The purpose of our study was to describe color Doppler US findings in neonates. Thus, we decided to exclude them. Sixty-four patients in whom we performed US and color Doppler US were enrolled in our study.

Thank you for your thoughtful review and comments on our article. We are pleased to be able to clarify the confusing sentences in our paper.

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Reference

1. Lee MS, Kim MJ, Lee MJ, et al. Biliary atresia: color Doppler US findings in neonates and infants. Radiology 2009;252(1):282–289. [Published correction appears in Radiology 2011;261(3):1003.]

Editors' Response

From
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We thank Dr Krishna and colleagues for their careful reading and comments regarding "Biliary Atresia: Color Doppler US Findings in Neonates and Infants." Letters to the Editor are an important part of peer review (in this case post-publication peer review) that can lead to improved understanding of scientific results and correction of inadvertent errors. To correct the errors mentioned in the preceding letter, we have issued an erratum in this issue of our journal.

Currently, more than 63% of articles submitted to *Radiology* are from outside of the United States. When authors are non-English speakers, review and editing of these articles is often difficult because the authors' meaning can be obscured by issues of language and semantics. We try to address the scientific content to the best of our ability. During the revision process, when we suggest changes in the manuscript, we track changes in written documents and ask authors to ensure that their meaning has been maintained. In the case of the article "Biliary Atresia: Color Doppler US Findings in Neonates and Infants," two main issues arose that Dr Krishna and colleagues mention in their letter, at least one of which was not recognized by the authors as changing their intended meaning, likely due to problems with translation from English to their native language.

This letter raises several important issues for our authors.

First, we encourage authors submitting works to *Radiology* who are nonnative English speakers to have someone familiar with their work who is a native English speaker carefully check their final draft and revisions. Second, all authors should carefully check revisions suggested by the editor and copy editors for accuracy. Third, we encourage our readers to let us know of any errors in Letters to the Editor so that corrections can be appropriately addressed.

$CTDI_{vol}$, DLP, and Effective Dose Are Excellent Measures for Use in CT Quality Improvement

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Editor:

McCollough and colleagues, in their recent editorial in the May 2011 issue of *Radiology* (1), have dismissed volume computed tomography (CT) dose index ($CTDI_{vol}$) and dose-length product (DLP) as useful measures of patient radiation dose, arguing that they do not measure the dose the patient absorbs. However, these indexes quantify the radiation dose to which a patient is exposed and thus dictate the dose absorbed by the patient. Although absorbed doses vary by patient size, they are primarily determined by the doses that come out of the machine and the region imaged. These types of readily available and controllable measures are critically needed to understand and improve the safety of imaging. Although these measures may vary by as much as twofold across patient size for the same type of examination to get images of similar quality, we found 10- to 100-fold differences in DLP for CT scans obtained for the same clinical indication among thousands of examinations we have reviewed, reflecting far more variation than could possibly occur owing to patient size. In fact, after accounting for patient weight and body mass index, a profound—and unacceptable—variation in these measures remained. Most of the variation in dose is due to variation in the adoption of multiphase protocols, larger scanning regions, or higher dose settings without awareness of the resulting dose burden these choices create. Thus, without even considering patient weight, we could greatly improve how we are conducting CT simply by assessing $CTDI_{vol}$ and DLP.

The authors also dismiss the use of effective dose (1), which can be calculated from DLP by using age- and sex-specific conversion factors, or more complicated methods that take into account patient size, arguing that it is too imprecise. Effective dose is a useful measure for identifying patients who receive unnecessarily high doses, tracking doses over time, and assessing facility performance and is easy to understand because it puts doses from scans of different body regions on an equitable scale. Furthermore, it is a useful measure for epidemiologic studies, where standard statistical methods exist for analyzing imprecise variables and can account for effect modification according to patient size, sex, and region imaged.

No measurement we use in medicine or research is perfect. The important question is whether a particular measurement is useful given its limitations. In this case, the answer is a resounding yes. $CTDI_{vol}$, DLP, and effective dose are excellent measures of radiation dose from CT and could be used immediately to improve the safety of CT by identifying when doses may be higher than necessary and standardizing how we conduct CT examinations.

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1. McCollough CH, Leng S, Yu L, Cody DD, Boone JM, McNitt-Gray MF. CT dose index and patient dose: they are *not* the same thing. Radiology 2011;259(2):311–316.

Response

From
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