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"IN MY OPINION . . ."

PREDICTABLE RESULTS AND PEER REVIEW

While skimming through an issue of the *Wildlife Society Bulletin*, I paused to read an article that caught my attention. The subject pertained to a controversial habitat alteration. Glancing at the name and affiliation of the author, I thought, "I'll bet I know what he concludes." After reading the paper's summary, my suspicions were confirmed. The next article I read addressed a management practice about which there is controversy. I predicted the conclusion, and again my prediction was correct.

My observations formed the basis of a subjective hypothesis—if a person, agency, institution, or organization is conducting, sponsoring, or supporting a study about a subject in which they have a vested interest, the published results will serve that vested interest. For example, if an agency's policy was to control predators by lethal means, its vested interest would be for research results that supported predator control. Conversely, a study funded by a humane group would predictably refute the need for lethal predator control.

To test this skeptical and subjective hypothesis, I examined 8 volumes of the *Bulletin* and selected papers that pertained to the interests, goals, or policies of the organizations or people involved. I read the names of author(s) and the agency or financial supporter, predicted the conclusions, and compared my predictions to the published conclusions. I was correct in 35 of 43 (81%) instances. I repeated this exercise with several other publications, and predicted the findings correctly in 10 of 14 attempts.

All my ideas for explaining these results are unsavoury. The worst interpretation is that investigators are dishonest and "fudge" their data. I doubt that this happens often, because I know the authors of some papers in my sample, and I am certain that they do not cheat intentionally. Less condemning, but still sad, is the possibility that they do not recognize their biases and that their study designs or interpretations are affected as a result.

If individual biases were the only explanation, then the review process mandated by most agencies should catch and correct the problem. Obviously, it does not. Most, if not all, papers submitted for publication in the journals I reviewed were reviewed by the authors' organization for content, clarity, and *policy*. Perhaps I have naively ignored the effect that the threat of losing one's job can have on scientific objectivity or the tenacity to publish unrewarding results. Discouraging the publication of results that tarnish an institution's image and accepting positive results may be partially responsible for the predictability of published results.

The review process of the journals involved probably also contributes to the malady. Peer review is just that—review by peers. Peer reviewers also work for agencies, organizations, and institutions with responsibilities for natural resources and they too have "axes to grind." As an Associate Editor of the *Journal of Wildlife Management*, I am often surprised at the vehemence and vitriol of reviewers. I doubt that their acidity arises only from objective outrage. They are protecting their "turf," either consciously or subconsciously. I have yet to read a comment such as, "This paper refutes

what my organization and I have been doing or believing for years, but the findings are valid and the paper should be published pending minor revisions."

The argument that policies of organizations follow results of the studies and thus explains my hypothesis is attractive, but it does not survive scrutiny. The pertinent policies in most papers I reviewed were long-standing and predated the investigations.

Organizations also may prevent adverse publications by refusing to fund research that tests established policy. This would be a modification of the old saying, "If it ain't broke, don't fix it," to read, "If we don't look too deep, we won't find mud."

The last explanation I propose is that the entire question is a product of my own subjective and biased interpretation of the perceived self-interests of various organizations. I suggest that those who doubt my premise should look at some publications and try the experiment themselves. I hope that someone will find error in my interpretation and restore my waivering faith. I am sorry that I do not have a solution for what I think is a major pothole in our road to respectability as a science.

I suppose the most optimistic way to view my observations is that, of the 57 papers I read, 12 reported results that were not in the self-interest of the sponsoring organization. Also encouraging was that I did not detect a vested interest in many papers, especially those published in the *Journal of Wildlife Management*. This is likely because most papers in the *Journal* described biological phenomena rather than management activities.

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PERSPECTIVES ON PEER REVIEW

The system of peer review used by many journals involves 2 or 3 referees, usually anonymous, who comment on the merit of a manuscript submitted for publication. Referees are seen as established professionals with strong objectivity. Scientists regard the precedure as an effective method of insuring sound contributions to the literature.

While peer review is irreplaceable, it is also imperfect. I contend the system is too often exalted beyond its inherent virtue by referees and editors. My purpose is to present arguments supporting this view and to offer recommendations for improving the process.

My first concern is that few, if any, journals have entrance standards for referees. Quality control is lacking. Most scientists have seen announcements asking for names, addresses, and areas of expertise from potential referees. Anyone privy to the announcement can respond; no credentials are required. Except for a few familiar names, editors have little idea of the capabilities of respondents.

Moreover, few editorial offices *formally* rate the performance of active referees. An incompetent or insensitive referee can receive manuscripts indefinitely if his or her performance is not tracked through the terms of succeeding editors. Widely recognized individuals may be poor referees (Baskett 1985), so reputation alone does not guarantee worthwhile reviews.

My second concern is that referees are subject to all the foibles, prejudices, and emotions of the human condition. Humans "interpret the factual evidence through the filter of [their] own values, beliefs, tastes, desires, and feelings" (Fowler 1986:106). "Rejection can and does occur on grounds that have nothing to do