> results of your work as it was reported in the local newspaper. The person has realized their answers to your questions are clearly out of line with the majority of the hunting community as represented by your data. The person tells you they are emotionally devastated by what your work has revealed to them. What plans do you have in place to help this person with the emotional problem your research may have contributed to? Is it a review-committee approved plan?

Clearly, no person or review committee can foresee all of the problems that may surface while conducting a research project. However, each of the above problems are fictional wildlife variations of actual problems that I, or close colleagues, have had while conducting human research projects. I have been pleased on more than one occasion when members of institutional review committees made recommendations that at the time seemed to be designed to cope with the trivial or improbable. I was comforted by having my protocols reviewed even when neither I nor the committee saw in advance a potential problem, and by having an identified group of colleagues I could turn to for help in resolving unseen difficulty.

As the use of questionnaires has become more popular and has proliferated rapidly beyond common use within departments of psychology, education, public health, sociology, political science, etc., the problems of compliance with review have grown dramatically. As a scientist and psychologist, I am very skeptical of the increased use of questionnaires and surveys. I seriously doubt the validity, reliability, and even utility of much of the information obtained. A large body of historical literature indicates that self-report information is usually quite incongruent with historical fact, contemporary personal actions, or future behavior. With great effort researchers can at times diminish (not eliminate) some of the problems associated with self-report information. Self-report information even fails to be consistent with other self-report information. Since 1989, I have authored, with other colleagues, 5 peer reviewed publications and 7 scientific convention presentations in which we reported experimental results clearly and importantly showing reliable inconsistency between a verbal and non-verbal measure of body image across genders and cultures, irrespective of psychiatric status. I have seen or reviewed far more published or presented works recently that fail to address or account for these problems than works that address these serious difficulties.

I can have little effect on the over-reliance upon questionnaires as research or clinical tools, but feel it very important to contribute to the assurance that their use causes no individual harm, including harm to my own future research opportunities. In the present research climate, the insensitivity, ignorance or misbehavior of an individual researcher jeopardizes, complicates, and may prevent future access to subjects (human and non-human). If these negative consequences are not brought about by institutional regulation, they will certainly be brought about by the less controllable perceptions of the public. I encourage you, as the Editor of the *Wildlife Society Bulletin,* to set into place policies and procedures that effectively assure that material published in the *Bulletin* complies with all aspects of concern in the use of human subjects.

Carl R. Gustavson, Ph.D.
Research Professor, Arizona State University
Tempe, Arizona

Where are biologists on the world scene?

I am becoming increasingly concerned about the fate of wildlife and natural ecosystems in the developing world and about the lack of action of American wildlife biologists in the matter. In fact, this was again clearly indicated to me when reading in the *Bulletin* (22:507) that [the importance of] international wildlife conservation has only a rating of 2.59 (1 = high, 5 = low).

Although I am aware of an increasing interest among American wildlife biologists to try to do something about this development, considering the number of biologists and resources available in North America, I feel that far more should be done and that American wildlife biologists should assume more of a leadership role and not leave action mainly in the hands of WWF [World Wildlife Fund], IUCN [International Union for the Conservation of Nature and Natural Resources], and Greenpeace.

Tony de Vos
Former Chief, Wildlife and Protected Areas Section,
FAO, United Nations
Queensland, Australia

(Editors note: At Dr. de Vos' offering, I have asked him to prepare a more detailed statement of his views and concerns for a future issue of the *Bulletin.* See opinion by D. Crowe and J. Shryer in this issue.)

Issues on small populations

I am writing to point out 4 errors in an article that appeared in a recent *Wildlife Society Bulletin* (Belovsky et al., 1994, Management of small populations, *WSB* 22:307–316). The first 2 errors directly concern a paper that I wrote (Beier, 1993, *Cons. Biol.* 7:94–108).

- [Beier's model] "was based on population persistence times." In fact the model was based on risk of extinction over a 100-year period, not population persistence times. Indeed the term "persistence time" does not appear in my paper. This is a relatively trivial error, but disturbing because it is a misstatement of fact.
- "Beier's (1993) claim that these models [clearly referring to *theoretical models with a ceiling at carrying capacity K*] frequently produce populations larger than carrying capacity is not possible." I made no such claim. My very clear statement (Beier 1993:98 and 105) was that *my simulation model* produced such results when survival rates were density independent. This is a serious error, because it falsely attributes to me a claim that I clearly did not make.
- In the same paragraphs, Belovsky et al. (1994:314) muddy the waters by misapplying the term "density independent" to those theoretical models that abruptly stop population growth when $N \geq K$. Density independence means that there is "*no tendency*" for mortality rates to increase or fecundity rates to decrease as density increases (M. Begon and M. Mortimer, 1981, *Population ecology*, p.17, emphasis in original). Density dependent models thus *include* models with an abrupt threshold (such as those that abruptly stop population growth at K). Indeed, the *exact* model that Belovsky et al. (1994) labeled as "density independent" was correctly labeled "density dependent" by R. Landé (1993, *Am. Nat.* 142:911-927) and was presented as "one obvious example" of "our usual formulations for density dependent population growth" by D. Goodman (1987, Pages 11-34 *in* Soulé, ed., *Viable populations for conservation*, Cambridge Univ. Press). Clearly Belovsky et al. used "density independent" in a way that flies in the face of traditional use, a fact driven home by the next point.
- Belovsky et al. (1994) incorrectly attribute this misdefinition of "density independent" to M. S. Boyce (1992, *Annu. Rev. Ecol. and Systematics* 23:481-506), who offers no such support, and P. B. Stacey and M. Taper (1992, *Ecol. Appl.* 2:18-29), who clearly acknowledge (p.24, first full paragraph) that a "density independent model with a reflecting boundary" is a form of density dependence. The existence of the 2 citations suggests that Belovsky et al. searched the literature for a supporting definition of "density dependent" but were unable to find any.

Paul Beier
Department of Forestry,
Northern Arizona University
Flagstaff, Arizona

Belovsky responds:

First, in our paper, there was no intent to be negative about Beier's work; in fact, I thought that we were complimentary when we referred to it as "innovative". However, we did believe that Beier's paper potentially could mislead professionals who were not versed in the intricacies of MVP [minimum viable population] and PVA [population viability analysis].

Our differences are largely semantic and I would still differ with many of Beier's definitions; these are trivial for us, but may not be for others, especially managers. For example, risk of extinction and persistence times are just two faces of the same coin from a mathematical perspective, and I believe that a model with carrying capacity is density dependent whether birth and death rates change continuously or abruptly. Our main differences rest on 2 points. First, I see Beier's model giving basic demographic stochasticity results (approx. 20 individuals required for persistence); therefore, is a simulation necessary or could the same prediction have been made in a more straightforward fashion with existing analytical models (which Beier acknowledges)? Second, if Beier's model strictly reflects demographic stochasticity, why contrast it critically with my predictions based upon environmental and demographic stochasticity in his paper?

Finally, the important issue that our paper raises is: what do the available data really reflect and is it adequate for MVP/PVA analyses? We praised Beier's paper, but used it to illustrate how and why managers must carefully consider these concepts. This is an important area for wildlife managers, but one fraught with misunderstandings and problems for those less versed than ourselves, especially when we might disagree on points.

Gary E. Belovsky
Professor and Coordinator of the Conservation Biology Program,
Utah State University
Logan, Utah