Improving the Use of Science in Conservation: Lessons from the Florida Panther

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Abstract

*In a companion article (Beier et al. 2006), we identified 2 sets of unreliable inferences that may compromise efforts to conserve the Florida panther (*Puma concolor coryi*). In spite of serious flaws in methodology and interpretation, these unreliable conclusions have appeared in prominent, peer-refereed scientific journals and have been repeatedly cited and miscited in support of panther conservation. Future editors and referees may reduce these errors by insisting on adherence to an Introduction, Methods, Results, and Discussion (IMRAD) format; checking improbable assertions attributed to earlier papers; and refusing to allow scientific inference in publication formats not subject to scientific peer review (e.g., editorials). We urge conservation biologists to view science as an adaptive process and to use the method of multiple working hypotheses (Chamberlin 1890) that are now a central feature of adaptive resource management (Walters 1986, Williams et al. 2002). We advocate a workshop approach, similar to that used for analysis of data for the northern spotted owl (*Strix occidentalis*; Anderson et al. 1999), to deal with scientific disagreement where, as in the case with panthers, stakeholders have entrenched points of view. Finally, we recommend the creation of an independent Scientific Steering Committee to address long-term issues of future research and monitoring of Florida panthers.*

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Key words

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Wildlife biologists view themselves as the appropriate experts for decision makers to consult on matters of wildlife conservation and management. The extent to which such advice is reliable, and thus useful for decision making, rests, in turn, on the degree to which the advice is based on principles of good science (Murphy and Noon 1991, Nudds and Morrison 1991). Like these authors and others (e.g., Romesburg 1981, Nichols 1991, Sinclair 1991), we consider science to be a *process* for reliable (and thus repeatable) inquiry and not merely an accumulation of facts. Thus, conclusions that are not supported by good science are unreliable, even if they cannot be refuted and might eventually be supported.

In the Endangered Species Act of 1973 (ESA; 16 U.S. Code 1531–1544), the principal legal mechanism for endangered species recovery in the United States, scientific evidence is required for listing decisions (Section 4), designation of critical habitat (Section 4), and biological assessments of the impacts of proposed projects (Section 7). In addition to these explicit references to science, the ESA clearly intended that scientific information and scientific approaches be applied to management of listed species. This intent is apparent from ESA's requirements for many scientific activities, including reporting to Congress on the status of each listed species every 2 years (Section 4-f); monitoring recovered species for 5 years (Section 4-g); using objective, measurable criteria to remove species from the list (Section 4-f); determining whether proposed actions would jeopardize or otherwise adversely affect a listed species (Section 7-a); requiring reasonable and prudent mitigation and enhancement measures for such projects (Section 7-g); quantifying the impact likely to result from a permit to take individuals of listed species (Section 10-a); minimizing and mitigating the impacts of any taking of listed species to the maximum extent possible (Section 10-a); and prohibiting actions that would appreciably reduce the likelihood of survival and recovery of listed species in the wild (Section 10-a). Finally, the Act authorizes the U.S. Fish and Wildlife Service (USFWS) to procure the services of qualified persons (typically scientists) to help develop and implement recovery plans (Section 4-f). To encourage qualified persons to serve on recovery teams, the ESA explicitly exempts them from the requirements of the Federal Advisory Committee Act. In 1994, the federal agencies mandated independent peer-review of proposed listing decisions and draft recovery plans (USFWS and National Marine Fisheries Service, Federal Register, 1 July 1994)

In a companion article (Beier et al. 2006), we identify 2 sets of unreliable inferences that may compromise efforts to conserve the endangered Florida panther. In this article, we identify several ways that these inferences were published, with the result that science was not effectively used for panthers. Our objectives are to 1) suggest improvements to the peer-review process to reduce the risk of publishing unreliable inferences; 2) emphasize the importance of evaluating multiple working hypotheses rather than using research to confirm favored hypotheses; 3) recommend an approach for analyzing data to support conservation in an objective atmosphere devoid of the entrenched, personalized positions of stakeholders; and 4) advocate engaging independent scientists in recovery activities *between* recovery plan revisions so that scientific issues and controversies are promptly addressed and recovery actions are rapidly adapted to new knowledge.

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Four Paths to Publishing Unreliable Inferences—And How Peer-Review Can Respond to Them

Two sets of unreliable inferences about panthers (Beier et al. 2006) made it through the peer-review process via several routes (Table 1). Although authors bear the highest level of responsibility for these inferences, it is useful to ask how the peer-review process could help reduce the prevalence of these unfortunate events. We believe that the first 3 types of problems typically are not what editors and referees look for; therefore, they deserve increased attention. In contrast, referees usually detect inappropriate methods or inferences within an otherwise well-constructed paper.

1. Incorrectly attributing inferences to an earlier article.—In several instances, previous work was inappropriately cited as the authority for assertions made in later papers (Table 1, see also Beier et al. 2006). This sort of unreliable inference is the primary responsibility of the authors because editors and referees cannot be expected to read each cited work to determine whether it was accurately interpreted. In these examples, referees probably assumed that the authors would not misinterpret their own work, and thus they were particularly unlikely to uncover the misattribution.

It is equally inappropriate to cite as fact claims made in editorials, essays, or other nonrigorous sources (Table 1). The problem is particularly insidious when the original inference appeared as an editorial note in a peer-refereed journal. In such cases, a referee probably would not detect the problem because the entry in the Literature Cited section is indistinguishable from the citation for a peer-refereed article. A subtler problem arises in citing inferences found in agency research reports, web sites maintained by researchers, or news stories. Although many of these inferences are reliable, some of them use inappropriate methods, describe methods or results in insufficient detail to evaluate reliability of subsequent inferences, or fail to discuss discrepancies with previous work. Authors considering whether to cite such works have a special responsibility to evaluate these issues and provide appropriate caveats if that work is cited.

Misattribution, especially when repeated in several articles, can make an unreliable inference become accepted as fact in a way that snowballs over time and is resistant to counterevidence. Clearly, the best way to reduce the risk of these events is for authors to scrupulously ensure that they accurately and honestly report inferences of earlier papers. But we believe referees can help. Although they cannot check all inferences attributed to previous literature, referees should make it a habit to verify the 1 or 2 most improbable attributions to previous literature. For example, we believe that a referee familiar with studies of large mammal habitat use and movements would have considered improbable the Maehr and Deason (2002) assertion (attributed to Maehr and Cox 1995) that panthers are reluctant to cross 90 m of nonforested habitat.

2. Presenting inferences within papers not subject to rigorous peer review.—Several articles with unreliable inferences (Table 1) either did not appear in peer-refereed journals, or if they did, did not adhere to the standard Introduction–Methods–Results–AND–Discussion (IMRAD) format (Day 1998). The simple logic of IMRAD has endured and become

Table 1. Types of unreliable inferences identified by the authors in a review of scientific literature supporting Florida panther conservation, and recommendations for how these instances may be reduced by editors and referees.

Type of unreliable inference	Example related to the Florida panther	How editors or referees can reduce this problem
1. Incorrectly attributing an inference to an earlier paper, or citing a non-IMRAD[a] paper as if it were rigorously peer reviewed.	Maehr and Deason (2002) wrongly cited Maehr and Cox (1995) as demonstrating that panthers were “reluctant to cross” 90 m of nonforest. The population viability analysis by Maehr et al. (2002) used estimates of kitten survival provided in an editorial by Maehr and Caddick (1995).	Within the manuscript, identify 1 or 2 improbable attributions to previous literature; verify whether the attribution is justified.
2. Making inferences (that may later be abused) within editorials or other non-IMRAD papers.	Demographic inferences in Maehr and Caddick (1995).	Do not allow authors to insert scientific inferences into editorials, book reviews, and essays. Insist that scientific inferences be presented in IMRAD format.
3. Presenting inferences in the Discussion section of an IMRAD article that 1) were unrelated to the article’s stated objectives, 2) used methods not described in the Methods section, or 3) lacked relevant material in the Results section.	Inferences by Maehr and Cox (1995) that panthers need large forest patches.	Strict adherence to IMRAD format with 1:1:1:1 correspondence among objectives, methods, results, and inferences.
4. Flawed methods or inferences in an otherwise well-constructed IMRAD paper.	Maehr and Cox (1995) ignored location error in stating that 91% of panther locations were within 90 m of forest cover.	Reviewers should insist on additional review by persons with expertise the reviewer lacks. Editors should not ask reviewers for recommendations on whether to publish the paper. Instead, they should ask reviewers to specify the steps needed to make the paper publishable. Journals should publish names of Associate Editor and reviewers for each paper that is accepted and published.

[a] IMRAD = The format of Introduction, Methods, Results, and Discussion in scientific papers.

dominant because it helps "the author organize and write the manuscript, and provides an easy road map for editors, referees, and ultimately readers to follow" in evaluating and reading the paper (Day 1998:7).

We urge authors to resist the urge to make scientific inferences outside of the system of rigorous peer review. Once published, such unreliable inferences can be intentionally or unintentionally abused. Referees and editors of journals of record can reduce this problem by being alert to attempts to insert scientific analyses and inferences into editorials, book reviews, and essays (however, all original syntheses such as book chapters should be peer refereed). Such articles can and should be freewheeling and free of the IMRAD straightjacket; conversely, we must keep these literary forms free of the analyses and inferences that belong in peer-refereed, IMRAD articles.

3. Presenting inferences in the Discussion section without corresponding objectives, methods, and results.—In a conventional IMRAD paper, the Discussion makes inferences from empirical results, which in turn were produced by clearly described methods motivated by stated objectives. In contrast, a key article on habitat use by panthers presented inferences in the Discussion section that bore no obvious connection to the Objectives, Methods, and Results presented earlier in the article (Table 1). This made it difficult for readers to critically evaluate whether the inference had strong, empirical support or to even to notice that the analysis had not been described in the article.

Those involved in the peer-review process can help reduce the prevalence of this problem in the literature. First, editors should insist that Results be presented separately from Discussion. Second, editors and referees must look for tight correspondence between Objectives, Methods, Results, and Discussion topics. A scientific paper should contain no method that is not related to a clearly stated objective, each result should correspond to a clearly described method, and each inference should relate to previously stated results. Authors, referees, readers, and conservation science are ill served by the lack of such tight correspondence. The IMRAD format helps authors and referees notice when the authors leap to an interpretation of results without first stating an objective to make this sort of inference, then describing the analytic methods, and then presenting the unvarnished results from which the inference can be made.

One useful practice is for the referee to make a preliminary assessment of whether a paper should be published based solely on a careful reading of the Introduction and Methods sections (Armstrong 1997). If the paper has not raised important questions and described appropriate methods for each question in these initial sections, rejection or major revision will be required. If the objectives are important and the methods are clear and appropriate, the results (even if statistically nonsignificant) should be at least potentially publishable. This mental exercise will also sensitize the referee to upcoming results or inferences for which no proper groundwork was developed in the first part of the paper.

4. Flawed methods or inferences in a paper free of the defects listed above.—Some papers we reviewed that were free of the defects already mentioned, nevertheless contained technical flaws in methods that lead to erroneous inferences (Table 1). Although referees are generally alert to this type of problem, editors can help reduce such flaws by asking referees not to make decisions on acceptance or rejection but rather to see their role as making each paper as rigorous and reliable as possible (Armstrong 1997). If referees envision their names as coauthors on the article, they would want to tell their colleagues how to make the paper as bullet-proof as possible. Likewise, referees would want to delete or qualify inferences that they would not want associated with their own scientific reputations. By clearly stating major concerns as changes needed to make the paper reliable and publishable, the referees will still provide editors the information they need. At the same time, referees will be encouraged to adopt a constructive tone in their review and will tend to focus on critical issues related to scientific reliability. Finally, because authors would perceive referees as allies rather than barriers to publication, the referees should feel less need for anonymity, a necessary condition for widespread acceptance of open review (next section).

Because of the specialized nature of some disciplines in conservation and management, referees often cannot evaluate whether certain methods are appropriate or whether the corresponding results are clearly presented and properly interpreted. In these cases, the referee should make a clear demand that someone with the appropriate expertise review the paper.

How Editors Can Encourage Better Peer Review

Greater recognition and accountability for peer-reviewing.—Many of our recommendations call for increased effort on the part of referees and editors, most of whom are already overworked and all of whom are underrewarded for their labors. Although serving as an editor may bring prestige and can help scientists in promotion decisions, we think that referees are motivated mainly by a love of science and a desire for management and conservation decisions to be based on sound science. We are fortunate that these motives have provided a peer-review system that works as well as it does. Good referees are people whose habit of striving for quality in their own work leads them to unselfishly help others do so.

We suggest 2 ways that journals can motivate referees to intensify their already-laudable efforts. First, cognizant of the noble motives of referees, editors must show referees that their effort has produced more reliable science. Whenever the referee has raised significant concerns about a paper, the editor should send the revised manuscript to the referees so they can evaluate whether the authors addressed those concerns or provided convincing counterarguments. In our experience as referees for journals, we have been provided the opportunity to review the resubmitted manuscripts in <50% of the cases in which we raised major concerns about the quality of the inferences. In some cases, we have seen articles published in a form that did not address what we had seen as fatal flaws during our reviews. Although the editor may have had good reason to set aside the referee's concern, ignoring the referee in this way is the surest way to deflate the enthusiasm of someone motivated by love of reliable scientific inference.

Second, journals can increase recognition for referees and assigning editors—and make them more accountable—by publishing their names with each published paper. Although this would eliminate referee anonymity, this may be acceptable if the process were explained to the referees at the outset and applied only to articles that are eventually accepted. We suggest listing these names on a separate line near the beginning or end of the article, rather than buried in the Acknowledgments section. Although such recognition is small, the referees' contribution would be noted by readers working in closely related fields and, thus, would add to referees' stature among colleagues. The only recognition offered by journals today (a list on the back pages of the last issue of the year) is nearly invisible to print subscribers and unseen by those who obtain paper or electronic reprints.

Publishing names of referees may motivate them to avoid quick and careless reviews. A referee whose name appears on the article will feel responsible for its quality and will see his or her role as an ally to the authors by trying to make the paper as publishable and rigorous as possible. We further encourage journals to carefully consider—perhaps on an experimental basis at first—moving to a system in which referees are required to sign their reports. Godlee (2002) suggested such systems result in higher-quality, more-constructive reviews with no adverse effects such as ruined careers or physical attacks on referees. By making the best reviews accessible, journals can also let graduate students and other potential new referees learn from the best of their colleagues.

Journal responsibility to set the record straight.—Journals should create procedures to inform the scientific community about major errors in the articles they publish. Publishing corrigenda in a later issue is a rather anemic effort that does not reduce the prevalence with which discredited studies are favorably cited (Pfeiffer and Snodgrass 1990). Because many articles are now accessed via the publisher's web page, the on-line version of journals should provide prominent links to commentaries, retractions, and corrections of each article. The on-line journal *Ecology and Society* has had such a format for 8 years; perusal of their website demonstrates that this does not provoke a flood of needless comments (see for example, Kerkhoff et al. 2000, Marmorek and Peters 2001). Journals should also consider allowing referees to add comments to the on-line articles, indicating whether authors had satisfied the referees' requests for major changes to the original submission.

Science as an adaptive process.—As we indicate in Beier et al. (2006), we emphatically do *not* argue that the conclusions reached in the panther literature are wrong. Indeed, we acknowledge that these conclusions may ultimately be supported by evidence. We *do* contend that the process by which many of these conclusions were reached was flawed (Table 1). Furthermore, we note that some studies that criticize these conclusions may themselves be seriously flawed. However, such flaws in other studies would in no way validate unsupported conclusions about panther biology (Table 1).

Regardless of who is ultimately right, managers usually must make decisions based on the quantity and quality of the science they have now. We argue in the next section that science would be best served by an approach based on multiple alternative hypotheses, integrated into formal adaptive management (Walters 1986, Williams et al. 2002). This approach provides a formal mechanism for decision making when knowledge is imperfect—as it always will be—with incremental improvements in knowledge (and, therefore, decision making) occurring via adaptive feedback from monitoring and research.

The Importance of Multiple Working Hypotheses in Research

More than a century ago Chamberlin (1890) warned against becoming too attached to a favored hypothesis and advocated an alternative method based on multiple working hypotheses. Similarly, Platt (1964) championed statistical testing of multiple working hypotheses as a path to strong inference in biological sciences. There are many examples of the method of multiple working hypotheses in ecology, including studies of factors governing barnacle distribution in the intertidal zone (Miron et al. 1999), wetland loss in the northern Gulf of Mexico (Turner 1997), and the phylogenetic origin of a metazoan taxon (Collins 1998). Recent analyses have advocated or used multiple working hypotheses in management contexts, including evaluating the success of restoration experiments (Michener 1997), assessment of salmon stocks in Snake River basin (Marmorek and Peters 2001), and the issuance of take permits (permits from USFWS allowing activities that harm a listed species) under the ESA (Smallwood et al. 1999). In a particularly lucid example, Sinclair et al. (2000) summarize a decade of experiments and observations that evaluate the level of support for 27 hypotheses about trophic interactions in a boreal ecosystem. The many studies summarized by Sinclair et al. (2000) embody an approach that was markedly different than the series of publications that reasserted the importance of forests to Florida panthers in increasingly strong language over time without new data or analyses.

In contrast to the studies cited above, many works we criticized (Table 1, Beier et al. 2006) seem to us to have in common the premature acceptance of single hypotheses. If true, this phenomenon may have retarded advances in scientific conservation of Florida panthers. Early research on panthers did suggest that panthers preferred forests to open habitats (e.g., Belden et al. 1988, Maehr et al 1991). At about the same time, Roelke (1990) speculated that there could be a geographic health cline in panthers, with body weight and physical condition declining and mercury contamination increasing from north to south. This corresponded with a north–south decrease in the importance of deer as prey, decrease in forest cover, and increase in the genetic contribution of South American pumas (O'Brien et al. 1990). The geographic coincidence of so many clines precluded strong inference about which factors were causes and which were effects, but they provided fertile grounds for generating alternative hypotheses.

However, by the mid-1990s published work seemed focused on a single hypothesis, namely that Florida panthers depended on forests for their existence (Maehr 1997), required large forest patches (Maehr and Cox 1995), and rarely traveled more than 90 m from forest edge (Maehr and Deason 2002). As we described (Beier et al. 2006), selective use of data, biased analyses, and miscitation of earlier work were used to support these conclusions. This trend culminated in 2 papers that provided no new relevant data or analyses (Maehr et al. 2001, Meegan and Maehr 2002), with the assertion that panthers were forest obligates.

We provide 3 illustrations of the lack of alternative hypotheses in these articles. In the first page of his book, Maehr (1997:xi) stated that the panther is, "hampered only by limited space . . . [d]iseases, parasites, highways, hurricanes, inbreeding, and heavy metals . . . [have not] impaired the panther's ability to live and reproduce." However, the book did not provide an even-handed evaluation of these alternatives, but it simply marshaled evidence for the importance of forests. Similarly, the second paragraph of the Introduction in Maehr and Cox (1995:1009) started with the assumption that, "large contiguous forested areas are required to sustain viable populations"—a curious starting premise for an article that has this same inference as its main conclusion. Finally, the Introduction of Kerkhoff et al. (2000) started with the assumption that, "[f]orest cover is a reasonable surrogate for useful habitat," and the article proceeded to develop a model that confirmed this assumption.

Lack of alternative hypotheses can lead to study designs (selective use of data, comparing used habitats to available habitats used by animals excluded from the analyses) biased toward supporting the favored hypothesis. For instance, in response to an inquiry from our Scientific Review Team (SRT) in late 2003, David Maehr stated that Maehr and Cox (1995) had excluded data on 18 of 41 panthers because, ". . . we viewed the southeastern area of occupied range as fundamentally different and not typical of preferred habitat" (Beier et al. 2003:3).

Unsupported inferences about panther demography (Beier et al. 2006) also may have been motivated by a desire to support this same, prematurely accepted idea that panthers were limited only by habitat (forest) area. If the habitat-limitation hypothesis were true, then genetic erosion was not an imminent threat in the early 1990s, and the genetic restoration program of 1994–1995 was misguided.

Testing of competing hypotheses in wildlife science has been forcefully advocated by Romesburg (1981), and it was further discussed by Sinclair (1991), Bauer (1994), and Weiner (1995). As an alternative to null-hypothesis testing, Burnham and Anderson (2002) advocated an information-theoretic approach to evaluate which alternative models are best supported by evidence (see also Guthery et al. 2001). Hilborn and Mangel (1997) and Williams et al. (2002:24–27) show how information theory or Bayesian inference can be used to explicitly place multiple working hypotheses in the context of adaptive management (Walters 1986). We support the use of multiple hypotheses and believe science and the Florida panther would have been better served had it been employed by panther researchers.

Protocols for Reanalysis of Data When Stakeholders Have Entrenched Disagreements

We identified 2 sets of unreliable conclusions about panthers. Here, we suggest ways to avoid the continuance or magnification of these and similar errors. Although science is not about getting it right once and for all, it is about following protocols that tend, over time, to discover and eliminate unreliable inferences (Romesburg 1981, 1991). Our single biggest criticism of previous scientific research on panthers is that rigorous protocols sometimes were not followed, and we are now paying the price in unreliable information.

Some scientists and managers have found constructive responses to previous situations where information was lacking, unreliable, or contested, but where conservation decisions nonetheless had to be made. We are most familiar with the example of the northern spotted owl (*Strix occidentalis caurina*), an endangered species that favors old-growth forested habitats of the Pacific Northwest (Gutiérrez 1996). As with panthers, conservation decisions regarding owls are of interest to diverse, local and regional stakeholders. Much is known about basic biology, habitat affinity, and demographic parameters of owls (Burnham et al. 1996, Gutiérrez 1996, Boyce et al. 2005). Nonetheless, the issue is rife for contention, largely because of the huge stakes on both sides of the issue of harvesting old-growth trees (Marcot and Thomas 1997). Perhaps compared with panthers, the situation is scientifically more clear cut. Nonetheless, even a relatively small degree of scientific uncertainty surrounding, for example, the degree to which owls are dependent on old-growth forest, can be exploited by stakeholders, who sometimes demand that opponents demonstrate scientific certitude. Although attractive in the legal realm, such proof is impossible in science.

Instead, we suggest that scientific uncertainty regarding panthers be considered as part of an adaptive approach to improved understanding and management. Ideally, this approach would evolve to formal adaptive management (Walters 1986, Williams et al. 2002), wherein decisions are made in the face of uncertainty, with feedback from monitoring and research reducing uncertainty through time and, thereby, improving management. It is essential to identify, and where possible, correct, areas of unreliability that can be agreed upon. One approach for doing this is the workshop format, implemented by Anderson et al. (1999). The goals of a workshop for panthers would be 1) to develop protocols that would guide the reanalysis of existing data on habitat selection and demography, and 2) to correct identified areas of unreliability. First, it is important to establish protocols for evaluating data that are viewed as objective by all parties. Parties must then agree on the specific questions to be addressed and methods to quantify uncertainty as objectively as possible, avoiding any attempt to prevail with any one scientific view. Then, and only then, parties should consider the implications of scientific uncertainty to decision making and ways that uncertainty can be reduced (but likely, never eliminated) through future research and monitoring.

Involvement of Independent Scientists in Recovery Actions Other Than Writing Recovery Plans

Our SRT was convened because a crisis simmered for several years until the agencies realized they needed outside advice. Our review took nearly 2 years to complete; we believe the panther would have been better served by continuous involvement of independent scientists to address the scientific uncertainty, new scientific information, and scientific controversies that are an inevitable part of recovering some endangered species. Snyder and Snyder (2000) suggested that scientists and recovery teams should spend less effort writing recovery plans with detailed step-down outlines that are out-of-date as soon as they are printed and more effort responding adaptively to current challenges. Recently, the USFWS has moved toward the use of standing recovery teams, which may be able to function as the scientific steering committee we advocate here.

In the spirit of suggestions by Snyder and Snyder (2000) and Clark et al. (1994), we recommend the creation of an independent Scientific Steering Committee (SSC) to address long-term issues of future research and monitoring of Florida panthers. Many of our recommendations regarding this SSC would also help provide strong, independent recovery teams. The SSC should comprise individuals from universities, agencies, professional scientific organizations, nongovernmental organizations (NGOs), or other stakeholder groups with an interest in conservation, management, and recovery of the listed species. Members should not have current proprietary, legal, or contractual involvement with any aspect of Florida panther recovery, and they should be an advisory rather than decision-making body.

The SSC should be charged 1) to provide advice on research priorities; 2) to review and make recommendations on proposed study plans for research; 3) to call attention to needed scientific tasks, including those that agencies are not eager to undertake; and 4) to review and rank proposals submitted under any competitive request. We recommend that periodically an ad hoc advisory group constituted by one of the professional societies (e.g., The Wildlife Society, Society for Conservation Biology, National Academy of Science, American Society of Mammalogists) be convened to determine whether the SSC should remain in existence or whether it is no longer useful. This ad hoc advisory group should also be convened if there is a substantial demand by stakeholders for a change in membership of the SSC.

Because the SSC may make recommendations that would be unpopular, its independence must be guaranteed. The SSC should be encouraged to summarize important recommendations as written documents and to communicate directly with the public. This idea does not threaten the decision-making authority of the agencies, but it would allow the SSC to signal when the agencies are choosing a course that is not recommended by the SSC. The recovery team, individual members of the recovery team, employees of state and federal agencies working on related issues, and representatives of conservation NGOs should be encouraged to contact the SSC directly to suggest SSC attention to scientific issues without having to route their request through a chain of command.

Management Implications

Modern wildlife management depends on wildlife scientists to provide reliable knowledge. As Romesburg (1981:293) wrote, "Unreliable knowledge is the set of false ideas that are mistaken for knowledge." To this, we add that ideas that are based on unsound methods or flawed logic are themselves unreliable, even if they are eventually supported. Unreliable knowledge leads to poor management decisions and loss of manager and scientist credibility.

Managers accept peer-refereed publication as the gold standard of reliable information. When unreliable knowledge appears in peer-refereed literature, professional credibility is eroded. Our suggestions are offered in the spirit of continuing the advancement of reliable knowledge for wildlife conservation, with hopes that this will benefit the Florida panther and conservation science in general.

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AFTER we reviewed the page proofs, JWM inadvertently deleted an important reference (cited 9 times in this paper). See attached Erratum.

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Associate Editor: Morrison.

ERRATA

CONROY, M. J., P. BEIER, H. QUIGLEY, and M. R. VAUGHAN. 2006. Improving the use of science in conservation: lessons from the Florida panther. Journal of Wildlife Management 70:1–7.

Beier et al. (2006), the companion paper cited throughout the article, is missing from the literature cited. Citations in the online article incorrectly point to Beier et al. (undated). The correct citation is:

BEIER, P., M. R. VAUGHAN, M. J. CONROY, and H. QUIGLEY. 2006. Evaluating scientific inferences about the Florida panther. Journal of Wildlife Management 70:236–245.

Beier et al. (2003) is incorrectly listed in the literature cited as Beier et al. (undated). The correct citation is:

BEIER P., VAUGHAN M. R., CONROY M. J., QUIGLEY H. 2003. Comments and communications received by the Scientific Review Team during its review of literature related to the Florida panther. Final report. Florida Fish and Wildlife Conservation Commission, Tallahassee, USA.

JWM apologizes to the authors for this error.

RHODES, J. R., A. J. TYRE, N. JONZÉN, C. A. MCALPINE, and H. P. POSSINGHAM. 2006. Optimizing presence-absence surveys for detecting population trends. Journal of Wildlife Management 70:8–18.

Equation (1) on page 9 should read:

$$\ln\left(\frac{f_{x,i}}{1-f_{x,i}}\right) = \ln\left(\frac{f_i}{1-f_i}\right) + b\left(\frac{Q_x}{\tilde{\mathrm{Q}}} - 1\right)$$

JWM apologizes to the authors for this error.