

R-1500

21 January 2004

MEMORANDUM

From: David S. Maehr

To: Florida Panther Recovery Team

Subject: Review of "An analysis of scientific publications related to the Florida Panther"

PART I

Introduction

Clearly, the review by Beier et al. (2003) (hereafter referred to as the SRT) represents a great effort to synthesize a voluminous and often perplexing body of information. In general, the document includes relevant publications. However, it ignores several that are critically related to Florida panther conservation, misinterprets other papers, and is inaccurate in its portrayal of several of my views on panther conservation. Further, the SRT involves a selective scrutiny. This scrutiny ranges from scathing critiques on many papers to benign coverage or total lack of criticism on other papers with obvious deficiencies. This is problematic with regard to the message that this document sends recovery agencies, conservation organizations, and the public. Whereas excellent, well-known models of species-specific literature reviews are a part of the many tools available to researchers and managers of large carnivores (e.g., Anderson 1983, Anderson 1987), the SRT chose to develop a document that went beyond its given charge by using the apparent policy implications of a particular paper to warrant a differential level of criticism. This will be expanded upon below.

The comments that follow are a response to some of the conclusions of the SRT. They are based on nearly a decade of scientific experience with the Florida panther while supervising research for the state of Florida, and more recent experiences as an invited participant in U.S. Fish & Wildlife Service (FWS) – directed recovery activities, as a private consultant, and as an independent academic. The progress made in understanding the ecology and conservation needs of this animal since the early 1980s is due to the efforts of many people from diverse backgrounds. At the start of my involvement in panther research (1985), radio-collared panthers had dropped to 2 individuals, monitoring and field activities were at a standstill, and little work had been done to examine the status of the population outside of readily accessible public lands. Persistence in obtaining permission to access private lands for capture and monitoring work in the late 1980s lead to the discovery of panthers that did not fit the initial perception of this population as nutritionally compromised and close to extinction. The continued success in panther conservation and research in south Florida is largely a product of the accomplishments in those early years. One of the attributes that I brought with me to south Florida was a commitment to document the successes and failures of our work in the published literature – this was strongly reinforced by well-known cougar expert Maurice Hornocker who encouraged us not to delay the publication of our work because the conservation needs of the panther were so great. Thus, we embraced the philosophy of conservation biology as a crisis discipline before the journal of the same name had finished its first year of distribution (1987). This resulted in the production of many papers over the next few years – some that may have been rushed to print as much

through my efforts to write them as the journals' desires to get them out. Such research productivity is unusual in state and federal wildlife agencies. The downside to much of this work was that sample sizes were always small. On the other hand, so was the population – a fact that is still true today. This point is important when considering the SRT review. While shortcomings may be apparent in many of the papers critiqued, the proportion of individuals included in any paper on the panther is high relative to studies of other wildlife populations. In other words, knowledge of even an individual's movements (e.g., panther #24 in Highlands County during 1988; Maehr et al. 1992) is an important contribution to understanding the ecology and conservation needs of a population that numbers well below 100. Although the total number of panthers that has been studied increases each year, the number under study at a given point in time is relatively low.

Another point unacknowledged by the SRT is that much of the research that has been criticized was done in the late 1980s and early 1990s, a time when personal computers were just beginning their rise (i.e., no one in the Naples office of the Florida Game & Fresh Water Fish Commission had a computer assigned personally to them, a "gigabyte" was unheard of, and analytical capabilities were limited), and when digital land cover data were bulky and difficult to come by. This resulted in a limited ability to manage very large data sets such as land cover. Thus, we relied on agencies such as the Florida Department of Transportation for digital maps that were not yet completed for our study area (i.e., we had only partial coverage). This is the explanation for the limited number of panthers examined in the earliest habitat use analyses. Further, the analytical approaches chosen to process the data were products of the time.

I will restrict my comments to those sections of the SRT report with which my views differ or where it seemed to me that the report was in error. The order of sections is intended to follow the organization of the SRT report. Literature citations for those papers that are not already included in the SRT are listed at the end of this document.

Habitat

→ It is clear that my view of panther habitat in much of the Big Cypress Swamp and Everglades has disturbed a handful of people who very much want to believe otherwise. Nonetheless, the low prey densities that have been documented in these areas, occasionally high water levels, the sparse distribution of forest, and the demographic performance of panthers prior to the introduction of Texas cougars are compelling reasons to suggest that southeastern Florida is primarily a population sink. The Smith et al. (1996) study of white-tailed deer in the Everglades concluded that deer and panthers existed at low densities, but that these low numbers probably represented the carrying capacity of this area for both species. This paper, cursorily reviewed by the SRT, also was clear in suggesting that the amount of forest edge in an area was important in explaining panther presence in an otherwise open landscape. This may have changed as a result of the coincidence of recently benign environmental conditions, possibly higher deer numbers, and the presence of translocated adult cougars, but little has changed in this landscape since the last genetically invigorated population (the descendents of the Piper stock) became effectively extinct more than a decade ago. To conclude that the recent success of the current population is the result of the better genetic vigor of local residents and that this proves that southeastern Florida is excellent habitat, ignores a

How do you know this?

longer history of temporary occupation and extinction. This is the only part of panther range where home range sizes have changed dramatically in the last 20 years. The cause for this change may be a combination of prey density and panther demographics, but this experiment is too young to conclude that current occupation in southeastern Florida means that this is excellent panther habitat. The SRT also ignores the implications of the early biomedical studies – that a condition gradient of declining panther health and reproduction from northwest to southeast (Roelke 1990, Roelke and Glass 1992) coincides with a pattern of decreasing forest cover and increasing forest patchiness in the landscape (Maehr 1997a). The paper arguing most energetically that panthers are now thriving in southeastern Florida and that the amount of available forest seems unimportant to population persistence, is the recent one by Comiskey et al. (2002). I will begin by addressing this paper and the relatively cursory review provided of it by the SRT.

Some months ago Darrell Land, David Shindle and I prepared a response to this paper – one that we intended to submit to *Conservation Ecology*. We were concerned that the methodology employed was unrepeatable, it promised innovative methods that were not explained (nor incorporated in any obvious way), and the new “paradigm” that was offered was based on the very daytime data that Comiskey et al. (2002) criticized with regard to their suitability for examining panther habitat use. It is surprising that the final draft of the SRT claimed that a more in depth review of this paper was unnecessary because it had not been widely “miscited” (sic SRT: 15) nor used in setting conservation policy. It is my understanding that no such restrictions were placed on the terms of the review requested of the SRT. Further, had this *post facto* rule change been applied to the entire body of literature, the final report would have been an order of magnitude shorter. And in fact, as will be shown in more detail later, the Comiskey et al. (2002) paper has been used by the U.S. Fish & Wildlife Service (FWS) for permitting purposes for at least a year now. The following section is from a draft of the response that we wrote but did not submit because we believed that the paper was so obviously fraught with errors that it would not be necessary to point them out, and because we were unaware of its use as a regulatory tool:

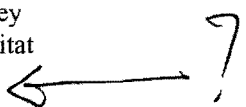
The decades-old view of the Florida panther (*Puma concolor coryi*) as a creature of the landscape (Maehr 1990, Land 1994, Maehr and Cox 1995, Maehr 1997) has been ascribed to in the recent effort by Comiskey et al. (2002). We agree, as these earlier works have demonstrated, that “panthers move freely within large home ranges, using and benefiting from the full spectrum of natural habitats available to them. The primary needs of panthers are an extensive area with a high degree of connectivity and minimal disturbance, the availability of large prey, and cover for denning and resting” Comiskey et al. (2002:27). For every panther that has been studied since the inception of radio telemetry studies in 1981, these key features of the landscape are either provided by or are found within a forested landscape. Indeed, with the exception of the Everglades, Florida was nearly entirely forested before European settlement (Brown 1909, Harlow 1965, Davis 1967). The Florida panther is, thus, a forest obligate (Maehr 1998), meaning that without this cover type the landscape would be absent of the only population of puma east of the Mississippi River. Clearly, other cover types besides forest contribute to individual home ranges, support potential prey and movements, but some of these habitats (i.e., agriculture, suburbs) have largely replaced much of the original forests in which the subspecies evolved. As Maehr and Cox (1995:1017-1018)

observed, “The high percentage of agricultural lands in occupied range suggests that the Florida panther is not strictly a wilderness animal. Thus, even in areas where improved pastures, farm fields, and other agricultural activities exist, panthers can remain part of the native fauna...But the context and pattern of these land uses will determine whether or not panthers are capable of persisting in denatured areas. Uninterrupted expanses of agriculture alone will not support breeding panthers...There is no reason to suspect that the Florida panther cannot continue to persist in south Florida if the landscape continues to provide all of its life-history requirements.” In struggling to offer a new view of the panther’s landscape, however, Comiskey et al. (2002) may have inadvertently helped to swing the development pendulum further against this endangered subspecies.

South Florida forests are not extensive tracts of one dominant canopy species that abut other extensive tracts of other species. Rather, a complex mosaic exists such that a person could seldom travel more than 100 meters without crossing several habitat types, including many non-forested habitats. We fully recognize that these mosaics are not comprised solely of forest vegetation, but the importance of the associated forest cover should not be underemphasized because of the proximity of non-forested habitats. Understory vegetation provides essential panther-level cover and it is not coincidental that south Florida forests have intact understories and panther use. Forest cover without understory vegetation, removed either mechanically or through intensive cattle grazing, would have limited utility to panthers.

Past analyses (e.g., Maehr and Cox 1995) as well as current work (i.e., Florida Panther Subteam of MERIT, U.S. Fish and Wildlife Service, Vero Beach, FL; Kautz et al., *in review*) shows clearly the relationship between 20 years of panther relocations and forest cover. This work has demonstrated that over 90% of all panther telemetry relocations are within 100 m of forest and that forest patches >392 ha contain 89% of all relocations. The conclusion by Comiskey et al. (2002) that panther telemetry data are useful in describing the distribution pattern of panthers in Florida is in agreement with a large body of work. However, the unsupported twist that panthers can “thrive” in non-forested landscapes should not be allowed to diminish the focus on Florida’s remaining forests as conservation priorities. In fact, landscapes dominated by non-forested habitats have been shown to be insufficient to support sustained occupation (Bass and Maehr 1991).

Comiskey et al. (2002) imply that a new panther habitat paradigm is needed, one that shifts away from a “forest-centered view.” This view is based primarily on their observations concerning panthers in Everglades National Park – a predominantly non-forested landscape. We feel that panther conservation needs throughout the currently occupied range would not be best served by focusing on panthers in areas where periodic extinctions have occurred (Bass and Maehr 1991). In fact, the panther population in ENP was reduced to a single male panther by 1995 and the release of 2 female Texas cougars as a component of Florida Panther Genetic Restoration is the only reason panthers exist in ENP today. Yet, Comiskey et al. (2002) refer to these panthers as “thriving.” The Everglades ecosystem is restricted to the extreme southeast portion of occupied panther range and there is a sharp demarcation between the extensive publicly-owned marshlands and the sprawling urban centers of Miami-Ft. Lauderdale. These vast wetlands and urbanized areas severely limit dispersal opportunities thereby exacerbating problems associated with isolation and small population size (Maehr et al. 2002). A case can be made, however, that had Comiskey et al. (2002) focused strictly on the impacts of Everglades restoration on panther habitat and conservation in southeastern Florida, future replumbing of this system might be done with less negative consequences to the panther. In contrast, conservation strategies in the northern fringes of occupied panther range are critical to ensuring the



long-term stability of panthers. The importance of forests in these areas should be above all debate. If the Comiskey et al. (2002) paradigm of less than 15% forest cover as suitable panther habitat is accepted as a conservation strategy, potential panther range could be effectively eliminated north of the current public land boundaries because of the lower threshold of forest cover derived from an Everglades-centered view of panther ecology. As Maehr et al. (2001) observed, fluctuating panther populations typify areas that offer <50% forest cover. Conversely, the Comiskey et al. (2002) paradigm shift could also identify as important panther habitat extensive tracts of human-altered landscapes (agricultural and urban) that, in our opinion, have limited to no benefits to the population and would serve only to complicate conservation initiatives by turning panthers into liabilities for private landowners.

Unfortunately, the "new paradigm" offered by Comiskey et al. (2002) is neither new nor an objective assessment of panther use in south Florida. Instead, it is a confusing and convoluted effort that attempts to discredit previous published work with the very data they claim are biased. Instead of adding to a body of knowledge that will become increasingly important for local, regional, and interstate planning, this paper creates an argument that appears more based in a fanciful view of panther ecology than in an objective attempt to help understand the over-arching patterns of panther abundance and distribution. In the contentious world of development permitting in south Florida, a view of the panther as not only tolerant of but "thriving" in landscapes with less than 15% forest (Comiskey et al. 2002:18) is an invitation for developers to push for more fragmentation of panther habitat. To suggest that such areas supporting as little as 10-22% forest cover are important and typical panther habitat and that this should represent some threshold that is characteristic of excellent panther habitat is indefensible. The creation of a contentious atmosphere, as this paper has done within Florida panther recovery efforts, will compromise otherwise positivistic efforts to manage and conserve this endangered subspecies.

Comiskey et al. (2002) continually criticize all previous work and offer their own analyses as the "new" paradigm for panther conservation. They acknowledge the utility of previously collected panther telemetry data in determining overall distribution, delineating home ranges, documenting dispersal, and describing natal den cover. These daytime telemetry data, despite their repeatedly acknowledged limitations (Land 1994, Maehr 1997), have been essential in determining the spatial dynamics of the panther and in facilitating conservation planning (Hector et al. 2000, Maehr et al. 2002). However, the authors belabor the fact that previous telemetry locations were collected during daylight hours and caution against their application to all aspects of panther habitat use. Surprisingly, Comiskey et al. (2002) use these very data to develop their "new" paradigm. The abstract promises that "innovative telemetry mapping" and "information from field observations" would be used to address the absence of nighttime telemetry data and to describe habitat use during "active hours". What ensues is an analysis that is fraught with all of the data biases that Comiskey et al. (2002) are strident in condemning in previous work. Attempts at "creating" nocturnal location data by point buffering, as suggested by Comiskey et al. (2002), is no better than speculation. Further, at no point in the paper did the authors describe how "scats, tracks, urine markers, and kill sites" were used as dependable indicators of temporal habitat use and activity. We, at least, are unaware of any quantitative and repeatable forensic methodology that has been applied to such "data" and Comiskey et al. (2002) provided no assurances that any standard methodology whatsoever was used. These deficiencies reinforce the view of this entire paper as an exercise in speculation based on complex models, phantom "data" analyses, incomplete methodology, and groping to reveal behavioral and landscape patterns that may or may not exist.

In spite of all their criticisms concerning what constitutes panther habitat, the authors did not utilize their paradigm to delineate areas critical to panther conservation. Several of the Comiskey et al. (2002) authors also serve on the panther sub-team of MERIT, a group assembled by the USFWS that is working on just such a map. Do Comiskey et al. (2002) agree with the boundaries of this map? This map was generated by looking at the extents of panther telemetry data and refining those extents based on existing forest cover. This approach would seem to be completely counter to the beliefs Comiskey et al. (2002) espouse yet this sub-team has apparently completed its charge. It would appear that the main objective of Comiskey et al. (2002) was simply to discredit all past panther research instead of producing a positive contribution that would benefit panther conservation efforts in Florida.

While it is possible that Comiskey et al. (2002) are sincere in their efforts to relate some aspect of panther behavior to the landscape, it fails to add to the body of knowledge of this endangered subspecies due to insufficiently described methodology and ungrounded speculation. Editorial and review processes should have insisted upon a more concise treatment of this subject, more details on methodology and should have tempered the indictments by the authors of previous analytical work based solely upon their unrepeatable approach.

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The concern about the Comiskey et al. (2002) paper was not restricted to the review above. Dr. Thomas Hootor, University of Florida, shared his concerns with the SRT. A portion of his comments follow:

I also want to briefly address both the "Habitat Relations" and "Genetics" sections of the draft review. On page 4, the SRT claims that "yet a clear consensus of panther-habitat relationships has not emerged". However, after being involved in the MERIT panther subteam I am convinced that the primary argument about Florida panther habitat use, the so-called forest "debate" focused on by Comiskey et al. (2002) and the SRT, is actually centered on the conviction of some that the southern half of Big Cypress National Preserve (BCNP) and Everglades National Park (ENP) are good panther habitat.

Though there is certainly debate about the long-term value of southern BCNP and ENP as habitat important for helping to support a viable population of Florida panther in south Florida, I disagree that a consensus about panther habitat use has not been reached. A consensus, which involved both parties from the "debate", is found within the report of the MERIT panther subteam. It states clearly that forest is an important habitat component, that panthers use other land cover types including open natural lands and agricultural lands, and that large areas containing forest and other compatible land cover and land use types with very low levels of intensive development (subdivisions, commercial development, and large high-traffic highways) are critical for effective conservation of a viable panther population in south Florida. This consensus conclusion was also supported by a compositional analysis of land cover within panther home ranges in the MERIT panther subteam work, which strongly demonstrated the importance of forest cover.

In my opinion, the SRT spends way too much time in the habitat relations section deconstructing the "forest-centered view" coined as a "straw-man" argument in

Comiskey et al. (2002). Comiskey et al. (2002), based on their paltry analysis and much questionable conjecture, argued that panthers “thrive” in areas with little forest in southern BCNP and ENP. I agree with the sentiment expressed by the SRT in their Recommendation #8 in the Habitat Relations section regarding the fact that the debate about forest polarizes opinion, with the caveat that the consensus in the MERIT panther subteam report recognized the importance of forest cover for quality panther habitat. The significance of forest is also recognized by the SRT on page 11: “. . . the available data clearly point to the importance of forest and forest cover to panthers . . .”

In addition, the SRT needs to balance their review on this subject. Comiskey et al. (2002) purports to develop a new paradigm for Florida panther conservation. Yet their analysis, if you accept their analytical methods, only shows that there may not be a strong relationship between percent forest cover and home range size when analyzing both Florida panthers and puma hybrids and that forests are an important habitat component (at least during the daytime according to them). No new data, nor innovative analyses (as promised in the abstract), are presented that indicate that large blocks of forest are not needed to support a viable panther population. They then suggest a “new paradigm” that focuses on maintaining the integrity of “panther breeding range” and requires a “landscape-based perspective”. Neither of the ideas is even close to being new for the Florida panther or the conservation of large carnivores in general (Noss et al. 1996). In fact, Maehr (1990), which is not cited in the draft SRT review, sets the basis for the necessity of a landscape-extensive strategy for conserving the Florida panther. This fact is also mentioned specifically in Maehr and Cox (1995), which both the SRT and Comiskey et al. (2002) fail to acknowledge after criticizing other aspects of the paper in detail. Finally, the MERIT panther subteam, which included Dave Maehr and authors from Comiskey et al. (2002), developed a consensus view of panther habitat conservation that acknowledged the essentialness of forest cover and regional landscape-level protection. In addition, the work by Dave Maehr and the Florida Fish and Wildlife Conservation Commission has been the basis for identifying and protecting hundreds of thousands of acres of Florida panther habitat since 1990, well before Comiskey et al. (2002) claimed to develop a “new paradigm” that landscape-level conservation was important for protecting a viable Florida panther population.

However, the key remaining argument lies with the future role of the southern half of BCNP and ENP for helping to support a viable population of the panther in south Florida. Comiskey et al. (2002) use the current occupation of these areas by puma hybrids as evidence that these areas are good panther habitat and even claim that these areas support panthers that are “thriving”. However, it is well known that these areas had never supported a source population since panthers have been intensively studied, and the very small population has been extirpated in ENP more than once. Comiskey et al. (2002) also speculate that the only reason this was the case was due to a lack of fitness in the panther population due to inbreeding prior to the genetic introgression program. Though this could be true, there is absolutely no evidence for it. In addition, if it were true, one could argue that this even more clearly demonstrates the importance of the existing population core in the northern half of BCNP, the Florida Panther National Wildlife Refuge, and on adjacent private lands since a “genetically inferior” population would likely persist only in the highest quality habitat remaining in its range.

The wet-dry cycles and fluctuating prey populations of southern BCNP and ENP and the function of these areas as population sinks before introgression demands

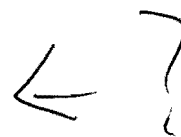
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that the now argued importance of these areas as habitat by Comiskey et al. (2002) should be treated with extreme caution. The precautionary principle applies here, and all past evidence suggests that southern BCNP and ENP cannot support stable panther populations. Though introgression may result in a permanent change in the status of these areas as important habitat, the prudent course is to be cautious and skeptical about this until much longer term trends in habitat use and fecundity of panthers and puma hybrids in southern BCNP and ENP are analyzed. This is extremely important because it is very relevant to the carrying capacity of south Florida for panthers. If population expansion in these areas turns out to be an ephemeral trend, it could mean that the existing population may significantly exceed the long-term carrying capacity of south Florida, which has important implications for maintaining essential efforts to conserve habitat on private lands. The SRT review does acknowledge this issue on page 10:

“The SRT is not persuaded by the arguments that the 2 federally managed areas are poor panther habitat (**my note—this should be amended to say the southern half of BCNP because the importance of BCNP north of I-75 is not in contention**). On the other hand the post-1995 population increase in these areas does not convince us that these areas will continue to support a dense panther population. Arguments over whether ENP and BCNP (**my note—this, again, should be amended to say southern BCNP**), or any other land outside the ‘core area,’ are suitable panther habitat will be resolved as we observe panther survival and reproduction in these areas over the next few years.”

I do not think the SRT is giving this issue nearly the weight it deserves, especially given the fact that it is not specifically mentioned in the recommendations for the Habitat section. The SRT also fails to link this issue to a discussion of carrying capacity in the Demography section or to the north-south habitat quality cline (Roelke 1990) mentioned in the Prey Relationships section and discussed in more detail in the Biomedical Issues section. Clearly these issues are related, and the SRT should reconsider their significance and potentially develop recommendations that would serve as a cautionary note on the potential misinterpretation of short-term habitat use and population trends after introgression, and research and monitoring that might elucidate future trends.

I want to mention a few more things about genetic introgression. I think that it is very preliminary and unscientific for the SRT to echo Comiskey et al. (2002) by concluding that the “genetic restoration program has been so successful.” Initial data and short-term trends may suggest evidence that this is true and, if so, this is indeed very good news. However, I do not share in the SRT’s optimism that an objective view of any perceived differences between Florida panther fitness and puma hybrid fitness can be clearly elucidated because spatial and temporal differences will severely complicate meaningful analysis and conclusions. In addition, genetic introgression will not be successful if it results in a puma population dominated by genes from *Puma concolor stanleyana* or outbreeding depression. The apparent success may also be tempered by potential future population failures and overall population decline if environmental and prey trends result in significantly reduced habitat quality in southern BCNP and ENP, as history suggests. I strongly urge caution on the part of the SRT in regards to bold assertions regarding the “near miracle” of genetic introgression in keeping with an objective scientific review that could result in either the recovery or extinction of Florida’s flagship species.



In addition to the problems outlined in the reviews above, the Comiskey et al. (2002) paper suffers other serious flaws. Perhaps the most egregious problem, and one not recognized by the SRT, is that the paper is incapable of even addressing habitat use issues because there is no measure included that indicates what type of habitat the panther was in when a pair of UTM coordinates was collected. This is important because an approach that examines only the content of a home range, as is done by Comiskey et al. (2002), cannot elucidate how the individual animal was using the available habitat. Thus, the paper cannot live up to its claim to have described some measure of panther "functionality." The approach that was used by Maehr et al. (1991) and Maehr (1997a) for the panthers that had corresponding land cover data in southwest Florida compared the habitat type in which the cat was located with the availability of cover in the home range. In these cases "Habitat use was determined by identifying the vegetative cover types associated with each radio-location. While radio-locations were fixed points defined as Universal Transverse Mercator (UTM) coordinates, error associated with observer variance and map accuracy precluded the determination of habitat type solely from this cartographic system. Using the aerial tracking methodology of Mech (1983), vegetative cover types were recorded as independent variables that were associated with a pair of UTM coordinates. Thus, even though some coordinate pairs may have imprecisely pinpointed a two-dimensional location, the habitat type was not affected by as many potential sources of error" (Maehr 1997a: 36). While this approach followed the now fairly antiquated methodology of Johnson (1980), such methods have been widely used to examine radio telemetry data (Erickson et al. 2001). Basically, the method used by Comiskey et al. (2002) is suitable for describing the composition of a home range, but not how a panther uses or perceives it. Simply placing a buffer around a location is not sufficient to determine the importance of associated land uses, and it is misleading to suggest that panthers use this larger area around each point at night. By using such a large buffer (up to 6.6 km) and by utilizing all available telemetry data, the results of any home range analysis could not help but include more non-forest cover in each home range than would be indicated by daytime data alone. This is because for those panthers living at the periphery of large forests, extensive areas of non-forest will be added to what the data would otherwise indicate, and because so many more locations were collected per individual panther in Everglades National Park (i.e., daily flights v 3 days per week in southwest Florida). Thus, the data set employed by Comiskey et al. (2002) is biased toward an Everglades landscape, and the gigantic 6.6 km buffer around radio locations is bound to include many denatured habitats (roads, residential areas, etc.) and treeless expanses that panthers typically avoid. Basically, what Comiskey et al. (2002) have confirmed is what has been known for about a decade – that is, the panther lives in a landscape that can often be dominated by non-forested habitat. As was observed almost a decade ago, "The high percentage of agricultural lands in occupied range suggests that the Florida panther is not strictly a wilderness animal. Thus even areas where improved pastures, farm fields, and other agricultural activities exist, panthers can remain part of the native fauna" (Maehr and Cox 1995: 1017). However, it does not make a convincing argument that forest is a relatively unimportant component of Florida panther habitat.

I am not alone in pinpointing forest as the single most important component of panther habitat. In another seminal paper ignored by the SRT, Pearlstine et al. (1995: 1024) concluded that "Spearman correlation coefficients of cover types from our land-

cover map extracted from under the home-range polygons confirmed ($p < 0.0001$) that forested areas (hydric hammock, pine-cypress, cypress, pine with narrow-leaf emergents, and upland pine) are the most important cover types within a panther's home range..." This analysis, conducted in southwest Florida (primarily Hendry and Collier counties) using a GIS-modeling approach, independently confirmed the findings of other studies (that used more conventional methods) that concluded forests in southwest Florida are the key to panther occurrence in the area. Because no one has, as yet, completed a habitat use/preference analysis for the panther in the Everglades, the relative importance of forest in this landscape is unclear. It would seem that because of the importance of thick cover as stalking and denning habitat (normally associated with forest), that forest (or what would otherwise substitute for a forest understory) is likely even more important to the Everglades population than elsewhere. Whereas Comiskey et al. (2002) include a discussion of habitat use, there does not appear to be a section of the paper that actually quantifies how panthers used or avoided available cover. Hootor's point from above is relevant here: if genetically pure (and arguably inferior) Florida panthers could survive in the forests of southwest Florida without the benefits of introgression for generations, then such landscapes must be superior panther habitat. I recommend that future analyses of panther habitat use focus on specific geographic areas that can be differentiated based on common vegetative, soil, and hydrologic characteristics. While some universal patterns should emerge in an analysis such as this, it would be prudent to treat areas such as the Everglades, Big Cypress Swamp south of I-75 and east of SR 29, southwest Florida (the area often referred to by me as the core), and potential colonization areas (e.g., Highlands and Glades counties) as unique zones of panther distribution (the findings from the north Florida cougar experiments represent yet another set of conditions for the panther – and where forests were the primary habitats used). To do otherwise would be akin to assuming that patterns of habitat use among desert-dwelling cougars in New Mexico should be the same for mountain lions living along the Snake River in Idaho. Perhaps such an approach has the potential to generate a more logical view of the panther in a region with tremendous land cover variation.

The Peer-review Process

As the SRT has repeatedly pointed out, I cannot claim perfection in explaining the details of methodology, especially in Maehr and Cox (1995) – we are talking about a sampling of writing and analyses that were done a decade or more ago. There was no intent to mislead or be vague. Had the SRT observed the same rigor in examining the Comiskey et al. (2002) paper as was given several of my publications, I would have expected an equally critical review. The criticism of the peer-review process is out of place in the SRT review – there is not much that can be done about this – scientists are at the mercy of our anonymous reviewers. Ultimately, the responsibility is with the authors – but certainly the peer review process is not perfect. I am reminded of The Wildlife Society meeting in Reno, Nevada 2 years ago where one of the associate editors of *Conservation Ecology* volunteered to me that they were handling a recent submission that was going to "take you out" (referring to me). I was too startled to even respond appropriately, i.e., with a comment to the effect that such a breach of confidentiality was unethical at best. At the very least we should expect a fair and balanced treatment of each paper rather than a false claim that it "has not been widely cited or miscited, nor has

it formed the basis for conservation policy” (SRT: 15). Researchers of endangered species have the profound burden of examining small amounts of data, analyzing the information to the best of their abilities, and sharing it as quickly as possible with the scientific community in peer-reviewed forums. Hindsight is 20-20, of course, and I would like nothing better than to go back in time and correct some of the deficiencies in a few of my papers. Nonetheless, I stand by what I have written, and the SRT itself has confirmed the importance of these contributions.

Relationships among Soil, Hydrology, Prey Density, and Panthers

I have no doubt that ENP panthers have lived in a landscape with <15% forest cover. The question is, for how long will they be able to do so? History suggests that the possibility of such occupation is absolutely dependent upon supplementation from elsewhere (since the Everglades introductions of the 1960s through the 1995 introductions). Indeed, the SRT (pg. 41) goes as far as recommending supplementation of the existing hybrid population in Everglades National Park. This was the subject of the Maehr and Lacy (2002) opinion piece, a paper that simply contained cautionary thoughts relative to what might be expected with the kind of climate and hydrology that exist in SE Florida. The purpose for offering the deer data was simply to demonstrate that in non-forested systems such as are found in SE Florida, deer numbers should be expected to fluctuate wildly – as they apparently have. The SRT was incorrect in linking a cause-and-effect relation between the deer numbers graphed in Figure 2a of Maehr and Lacy (2002) and panther numbers (SRT, pg. 33), and, thus, “inadvertently” providing “another striking line of evidence for the superiority of hybrids” (referring to hybrid panthers). To my knowledge there are no resident panthers in this unit (Conservation Area 3A North), a fact that is likely explained by low deer numbers and lack of forest cover. In addition, the SRT (pg. 33) claims that Figure 2b “depicts a more prolonged and 3-fold increase in deer numbers during the late 1980s.” Such a figure does not exist in the paper, and I am curious how this misstatement could have occurred (although increases occurred in 1988 and 1989, this was followed by a drop to the previous low levels – no prolonged increase is apparent). Nor did we suggest a mechanism to explain the link between fluctuating deer numbers and fluctuating water levels.

It has been well established that prey abundance is an important factor in explaining panther occurrence and reproductive potential – a notion attributed by the SRT to the paper by Dees et al. (2001)(SRT: 17), but that has been discussed elsewhere (Roelke 1990, Smith and Bass 1994, Smith et al. 1996, Maehr and Lacy 2002) and about carnivores in general (e.g., Andrewartha and Birch 1984). It has also been suggested that deer, panthers, and agriculture can coexist where soils are of sufficient quality (Maehr 1997b: 112). I remain convinced that time will demonstrate that the panthers currently occupying southeast Florida will experience sharp declines as deer populations experience what I believe will be inevitable crashes, and that their demographic performance has much less to do with genetic superiority. There is no doubt that deer respond negatively to high water events, as past massive die-offs attest. Our intent in Lacy and Maehr (2002) was not to demonstrate cause and effect (there are no data available to do this) but simply to demonstrate the tremendous variation in deer numbers that occur in areas dominated by poor soils and herbaceous marshes (some of which do not support resident panthers at all). Such fluctuations should be expected, as should

panther population responses if residents occur there. The Maehr and Lacy (2002) paper, again, an opinion piece, makes a compelling argument. Let the future of panther and deer numbers dictate the extent to which this was much ado about nothing.

Here, as in other instances, the SRT appeared to be seeking disagreement with alternative explanations for phenomena that will likely always be somewhat mysterious even after many more decades of study. For example, even if hybrid panthers responded to an increase in deer numbers in the Stairsteps Unit, is there any evidence that non-hybrid panthers would not have done the same thing had they been translocated there instead? In other words, might it simply have been the numerical jumpstart of introduced Texas cougars that allowed the population to grow so quickly in this area, and not a new-found genetic capacity to reproduce? There is no doubt that the introduction of Texas cougars into south Florida changed the demographics of the population including its population size and the magnitude of its dispersal characteristics. I find it interesting, however, that among the long-distance dispersal events that followed the population increase, the first and longest was exhibited by a non-hybridized panther. While one could argue that all of the changes could be attributed to increased genetic vigor, this event suggests that native Florida stock is fully capable of responding to the changes imposed by Texas cougars in dramatically unexpected fashion, and perhaps in responding to changing habitat conditions in a manner similar to the Texas hybrids.

Competition

The first draft of the SRT claimed that no work had been done to examine the potential for competition among south Florida's large carnivores ("Competition between panthers and other predators for what may be a limited resource has not been explored, but is fertile ground for future research" pg. 17). After this oversight was brought to its attention, the SRT acknowledged that Maehr (1997a) had examined spatial and dietary overlap among bobcat, black bear, Florida panther, and coyote (little potential competition among the first 3, but potentially great between these and the coyote). The new discussion of this subject in the final draft should then have incorporated the findings of Labisky and his colleagues (in 2 prominent refereed journals) with regard to hunter effects on white-tailed deer in potential panther reintroduction zones (Kilgo et al. 1998), and the capacity for the bobcat to be a formidable predator of deer in southeastern Florida (Labisky and Boulay 1998). These studies suggest the potential for some very interesting future work on this subject.

Genetics

file → I am not, nor have I ever been an opponent of genetic introgression – a presumption that suggests a cursory review of the literature at best, and another inaccurate portrayal of my writing. My problem with introgression, as I have clearly stated, is not that it is happening or needed, but the rate at which it occurred and has been allowed to escalate. As to my assertion about a higher level of inbreeding in the Florida panther even before European settlement – how could it be any other way? A one-way flow of genes into south Florida (and very limited east to west potential) demands such a result, especially in light of the large home ranges of males – it would take just a handful to span the width of the peninsula. I disagree with the SRT that 10,000 years is a short time in an evolutionary sense. Elsewhere we are witnessing evolution before our eyes

with species such as the golden-winged and blue-winged warblers (zones of colonization and hybridization in the northeastern U.S.), and that it has happened in the last few hundred years with the red wolf, and the transcontinental range expansion of the coyote. Further, the high rate of endemism in peninsular Florida (e.g., Florida scrub-jay [recently recognized as its own distinct species], gopher tortoise, and a variety of other animals and plants) attests to the selective power of this island-like peninsula. I believe that the comparison with Vancouver Island is an unfair one – I suspect that there is regular interchange with mainland individuals through one mechanism or another, and neighboring populations of cougars there are closer than any others are to south Florida. There are no populations from which cougars could be reasonably expected to colonize south Florida, but if there were they would arrive there via the peninsula and from nowhere else. The foundation of the SRT opinion on genetic variability may be compelling, but it is no more defensible than any that I have set forth. Mostly, this is now a moot argument from a management sense – but it may be an important consideration for future management decisions. It remains an academic curiosity and will continue to be a great experiment. The only questions now are how many and how often do new cats need to be reintroduced if the population is not encouraged to expand on its own.

The importance of maintaining genetic variation is not simply a conservation biology construct – one only needs to examine *A Sand County Almanac* to see how important a force Leopold (1949) believed evolution and natural selection to be. I believe that the SRT is overly harsh in my portrayal of the apparently one-sided debate regarding genetic fitness. The sole purpose of Maehr and Caddick (1995) and any other mention of genetic variability and outbreeding depression was that recovery agencies need not panic in their rush to genetically augment the Florida panther (again, a rather moot point today). At no time did we “downplay” the need for some level of introgression. Outbreeding depression may not be a likely scenario in south Florida panthers, but the loss of unique genetic material that made the Florida panther distinctly different from any other North American population should be of concern. As Ernst Mayer (1976: 10) has repeatedly reminded us, natural selection operates on the phenotype – not the genotype. That is, it really matters what these animals look like and how they are put together (it is these morphological and behavioral differences between populations that are the taxonomic foundation of species and subspecies – not their genetic makeup). Simply examining the genetic blueprints of populations (especially given the limited amount that we can learn through state-of-the-art genetic analyses) has the potential to promote biotic impoverishment through homogenization. Whereas traditional wildlife managers may see no problem in the continued blending of the Earth’s biota, I view it as one of the biggest issues facing humankind. The management of the Florida panther has hastened this homogenization process (that’s my *opinion*). Finally, if a panther with an atrial-septal defect dies before it reproduces, or its offspring die before they reproduce, the responsible lethal alleles should eventually be expunged from the population. Certainly, this would lead to exacerbating the losses of genetic variability, but it should not become a population-wide, nor population-threatening phenomenon. Certainly, if other adaptive traits are lost in the process, the consequences may be quite negative. My arguments regarding panther genetic issues have always been intended to promote the integrity of the subspecies, and to approach the management of this small population in a way that

does the least harm to the population – arguably a unique genome. Such an approach was also suggested in a case study on the Florida panther (Shrader-Frechette and McCoy (1993:261-262)(another publication not examined by the SRT).

In the SRT's reference to Maehr and Caddick (1995) it is suggested that our point was to argue for a lack of demographic impact from inbreeding. I do not believe that this was said. The point of the graphs was simply to suggest that (based on the most raw field data, and based on what we could ascertain for the 1st 12 months of a panther's life) that the panther population had the capacity to grow – not that it was, in fact, growing (and yet we had witnessed range expansion by individual panthers such as female #52 in the early 1990s [Maehr 1997a:37]). In other words we were talking about 1st year survival whereas the SRT claims we meant recruitment. As we suggested, there appeared to be something that was holding back *rapid* population growth once these kittens became independent – perhaps mortality that was going unnoticed and that may have been density dependent (just as the tremendous change in dispersal patterns appears to have been). This was expanded upon in Maehr and Lacy (2002: 973-974):

“It is likely that panther numbers throughout south Florida, as with the cougar elsewhere in its range (Seidensticker et al. 1973, Logan and Sweanor 2001), reflect the relative abundance and stability of local prey populations. This might explain the relatively high juvenile (1 – 12 months of age) survival (Maehr and Caddick 1995), but comparatively low adult recruitment that typifies south Florida panthers. High survival rates in both the juvenile and resident adult age classes suggest that, without frequent home range vacancies, newly independent subadults leave the population permanently through dispersal (Maehr et al. 2001), or their circular dispersal movements return them to occupied range where they are vulnerable to intraspecific mortality caused by resident adults (Maehr 1997a, Maehr et al. 2002a).

Recent introductions may have allowed Texas adult females to bypass the density dependent mechanisms (i.e., intraspecific aggression, juvenile mortality, disease) that normally operate more frequently on younger animals to limit panther population growth. Whereas panther kittens are protected from predation for at least one year by their mother, and mature adults live in a land tenure system (Seidensticker et al. 1973) with the psychological and physical attributes to successfully defend home ranges (Morse 1980), independent subadults are more vulnerable to both intraspecific aggression and anthropogenic mortality (Maehr et al. 1991a). Such a process appears to govern home range establishment in other solitary carnivores such as female tiger (*Panthera tigris*; Smith et al. 1987), and leopard (*Panthera pardus*; Bailey 1993), and is likely a limiting factor in these populations. Some of the introduced Texas cougars may have experienced an additional advantage by being placed in areas devoid of resident females. This would eliminate the liabilities of entering the population (as do native-born subadult panthers) at the bottom of the social hierarchy (Morse 1980).

The fact that Texas cougars introduced into south Florida were mature adults may have allowed the panther population to temporarily exceed its environmentally constrained limits. This is especially true if recent conditions in the Stairsteps Unit exemplify other habitat in the eastern and southern portion of the panther's occupied range. In an ecologically variable landscape such as south Florida, habitat conditions can change quickly and dramatically with single events such as a tropical storm (Henry et al. 1994). Deer populations can decrease rapidly following high rainfall periods (Figure 2) whereas populations typically increase gradually during times of moderate to low rainfall. The latter situation characterizes habitat conditions and deer herd size at the time of Texas cougar introduction. Increases in southeastern Florida deer numbers,

however, have always been temporary. Massive, weather-related mortality events have been a part of this largely aquatic system for many decades, and annual reductions of as much as 30% have been recorded in Everglades deer herds (Loveless 1959, Forrester 1992)."

I am unaware of anyone who is closely involved with panther recovery who believes that some sort of genetic management is not needed. In fact, I suggested that "immigration into the South Florida panther population is an important consideration for long-term management. However, the rates of managed introgression should be sensitive to the demographics of the target population. For every 10 breeding adult female panthers, 2 will die in a 10-year period (based on observed mortality in Southwest Florida from 1986-1994), so a reasonable rate of infusion from outside the known South Florida population would be one adult female every five years. Higher rates have the potential to promote genetic swamping." (Maehr 1997a:125). This latter point was further reinforced by Maehr and Lacy (2002). Therefore, it is inaccurate to portray the authors of cautionary writing pertaining to genetic restoration as "skeptics" (SRT: 26).

Demography

The evidence is rather compelling that the panther population was slowly growing from the mid-1980s up to the Texas introductions. Some of this evidence includes the discovery of breeding female panthers in areas that had previously been surveyed and found without them, and the documentation of successful colonization and reproduction by females in peripheral areas. I believe that the Texas introductions jump-started a process that was already underway, and that growth was happening gradually and in balance with environmental conditions. It should be no surprise that such a rapid and artificial infusion of adults into a population that may have been governed by long-term averages in prey density, and intraspecific aggression and mortality, resulted in rapid population growth. Thus, the Florida panther's failure to exploit an expanding deer herd may have had nothing to do with reproductive performance but with behavior. Due to the philopatric nature of female panthers, we would not expect an immediate colonization of vacant or low density range as has often prevailed in the Everglades and eastern Big Cypress Swamp. Nonetheless, I agree with the SRT that vigorous data collection for the purpose of answering these questions is important – I hope that the foundation to do so has been laid.

Survival rates – I believe that if one restricts the examination of kitten survival to one year (when they are still dependent on the mother, and that reflects the approach of Maehr and Caddick 1995), that it is possible to demonstrate a survival rate that is higher than adults. What happens after this 12-month period is up for speculation, but this tends to be the beginning of a period of higher mortality, especially for males. I believe that any PVA that used the 80% kitten survival rate restricted the use of such a number to the first year, and that mortality went up considerably, especially for males in the subsequent 2-3 years. I have been surprised at the difficulty of many people to accept this as a possibility – at least for the time period from which these data derived. Certainly, more recent data may suggest a different rate of survival for kittens (especially now that the population is living at such high relative density), but for the period that we examined, the known kitten production was outpacing the known mortality. Certainly there are some biases in this more expedient approach. However, the discovery of mortality was biased in the

sense that uncollared adults were easy to find if they were killed on highways, whereas many natal dens may have gone undiscovered (however, we were usually confident that most of the females in the areas accessible to us were collared, thus, the kitten survival data in the first year were more likely to represent reality than were the adult mortality data).

The SRT has not accurately portrayed how the survival data were interpreted by Maehr and Caddick (1995). First and foremost we considered survival only through 12 months-of-age, a period of time for which there was reasonably good data. I believe this quote from our paper is instructive: “Although these data may not cover sufficient time to prove that the population is growing, they suggest that demographic stability is at least a defensible hypothesis...That significant population growth has *not* (emphasis added) been observed is likely a function of limited space for the accommodation of dispersers” (Maehr and Caddick 1995: 1297).

While the SRT (pg. 47) acknowledged that “The study of panther dispersal (Maehr et al. 2002) is one of a handful of papers to provide meaningful insight into dispersal dynamics of *Puma concolor*”, the discussion on this topic is exceedingly brief given its importance to the demographics of vertebrate demography and to the panther in particular. Not included in this discussion is the dispersal model of Meegan and Maehr (2002). Although this paper is criticized for having erroneously considered the 90 m distance suggested by Maehr and Cox (1995) as a meaningful landscape metric, the dispersal model would be little influenced by the elimination or increase of this distance as a buffer to the forested areas examined. Linkages in the modeled pathway include forest patches that are as small as 2 ha. Regardless, the least cost path that derived from this exercise overlapped almost exactly with the Caloosahatchee River crossing points for the 3 radio-collared males that have crossed this landscape filter, and it follows a path that is close to what these animals used. This pathway, thus, represents a reasonable conservation target as a managed dispersal zone (a circuitous route would be fiscally irresponsible especially if it incorporated areas of current or future development). Meegan and Maehr (2002), regardless of using 90 m buffers associated with forest fragments, is important for demonstrating the importance of small forest patches in facilitating the movements of dispersing panthers through the southwest Florida landscape. It is interesting to note that the results if the MERIT panther subteam’s dispersal exercise had a very similar result while using different cost surface rules. While we do not understand how a panther perceives the landscape, it is clear that the arrangement of forest (or lack thereof) is important in determining movement direction and choices for negotiating landscape filters such as dredged river channels.

Conclusions of Review of the SRT Report

I have attempted to be even-handed and objective in my responses, even though I feel that an inordinate and often inaccurate examination of some of my work has occurred. The inaccurate interpretation of some of this work is concerning to me for many reasons. When I and several of my former co-authors learned of the pending outside review of panther literature, we were confident that an objective and unbiased product would be produced. At this point I am not so sure that this has happened. Again, I do not claim perfection in my publication record, but there seemed to be a very negative

spin put on much of my work, and there was an admission that not all papers were equally scrutinized if read at all.

Finally, I believe that a healthy, professional debate is essential in the evolution of our understanding of Florida panther ecology and conservation needs. This has been my aim throughout my nearly 20 years of involvement in panther recovery, and I believe that the content and tone of the papers that I have authored or co-authored clearly communicate this. I would be the first to celebrate the cessation of human population growth in south Florida and the creation of an atmosphere that promoted the restoration of biodiversity including the panther. All of my papers have been written with the intent to get us closer to that goal. To suggest that I oppose genetic restoration of the panther is false – I have merely taken advantage of my independence as an academic to point out potential dangers in the approach as it has been implemented – I am certainly not alone in this concern. Several of the papers that received harsh criticism by the SRT were published as essays or opinions – treatments that were not intended to set scientific baselines or present new data, but to offer cautions regarding irreversible management actions. Whereas my ideas may not represent mainstream thinking, and therefore are not viewed as politically correct by some, these facts do not diminish the gravity of my message: yes, we may have a fortified panther living now in south Florida, but is this the animal that has the greatest likelihood of reseeding vast tracts of vacant range throughout the southeastern U.S. in a manner that will be acceptable to human residents? Given the recent advent of genetic restoration efforts in south Florida, it is disturbing to hear the claims that this action has solved the problem of low panther numbers in south Florida. Until the eastern Big Cypress and Everglades panthers experience and survive the severe conditions that can characterize south Florida almost overnight (i.e., high water for long periods, deer die-offs, and related ills), managers should continue to view these predominantly wetland and sparsely forested areas as questionable long-term good habitat for the panther.

The now pitched debate over the importance of forest to the panther has the potential to result in the elimination of panther habitat throughout occupied range on private lands. While the unscrupulous developer might have pre-emptively eliminated upland forests to avoid mitigating for impacts to panther habitat, the most responsible and law-abiding land-owner can now readily justify the elimination of 85% of an area's upland forest because a recent analysis that is now used by the FWS in its permit review (Comiskey et al. 2002) claims that the panther can "thrive" in areas with less than 15% forest. The SRT's admonishment of Comiskey et al. (2002) as "not intelligible" (pg. 105) is correct, but it was quite in error to suggest that because this paper has not "formed the basis for conservation policy" (pg. 15), it does not warrant the same scrutiny as was applied to Maehr and Cox (1995). Unlike any other paper on panther habitat use of which I am aware, the FWS now directs development permit applicants to apply the findings of Comiskey et al. (2002) to the permitting process (see as one example of several the 27 January 2003 letter, pg. 4, from Linda S. Ferrell, FWS, Assistant Field Supervisor, South Florida Ecological Services Office, Vero beach, FL; to John R. Hall, U.S. Army Corps of Engineers, Ft. Myers, FL regarding the *Walnut Lakes* development [application #200003795 (IP-DY)] in Collier County). The SRT's decision to waive further criticism of Comiskey et al. (2002) sends a telltale message regarding the

thoroughness of its review and suggests ominous consequences for the panther and the forests that it clearly prefers – especially in southwestern Florida. If we accept that non-forested landscapes can provide the same benefits to the panther as do large forests, the next step in logic is that panthers do not need forest. On the contrary, it is abundantly clear that the panther could do quite well without sawgrass marshes, wet prairies, tomato fields, and improved pastures if such areas were replaced with forests. The opposite is not the case. The preponderance of evidence suggests that without forest (or in a landscape that is only 15% forest) the panther would eventually perish (as past history has also shown). I believe that this very simple notion places the role of forest versus non-forest in proper perspective. At the very least, and as the SRT observed (pg. 12), “forests are the most important habitat for diurnal locations of panthers.” Beyond that, it appears that the distrust of previous habitat analyses puts the inferences based on them in so much doubt that we can say very little at all about the subject. Regardless, I believe that time will bear out that forests are the single most important landscape feature in explaining the presence and fecundity of the panther. This conclusion was borne out during the discussions of the panther subteam of MERIT. In addition, it is interesting to note that among the many wildlife underpasses that were built to ensure the safe travel of panthers, only those that were incorporated into densely forested systems (such as the Fakahatchee Strand) were used regularly by panthers, and that virtually all of the crossing events occurred after nightfall. Thus, although these panthers were traveling, they remained under cover – whether forest or concrete. Panther use of underpasses built in the predominantly herbaceous habitats that occur to the east of SR 29 has been much less frequent than use of underpasses incorporated into the Fakahatchee Strand. It is the same highway, with the same traffic loads, but the landscape settings are completely different – the more open landscape to the east is certainly a contributing factor in the lower use of these underpasses. Perhaps ongoing studies will resolve this issue soon. In the meantime, the advice of Comiskey et al. (2002) will likely prove detrimental to the panther if it continues to be offered as the standard against which suitable habitat is judged, and for which mitigation is required.

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