Letters

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Sea Otter Mortality from the *Exxon Valdez* Oil Spill: Evaluation of an Estimate from Boat-Based Surveys

The *Exxon Valdez* oil spill killed large numbers of sea otters (*Enhydra lutris*) in Prince William Sound (PWS), Alaska, where the spill originated, as well as within the oil's path along the coasts of the Kenai Peninsula and Kodiak Island. Sea otters attracted particular attention after the spill due to their public appeal (Batten 1990) and known vulnerability to oil (Costa and Kooyman 1982, Siniff *et al.* 1982, Davis *et al.* 1988, Williams *et al.* 1988). Extensive efforts were made to rescue and rehabilitate oiled otters at a resulting cost of *ca.* $80,000 per animal (Estes 1991). The argument that otters lost in the spill should be similarly valued made the estimate of mortality a matter of substantial concern to those involved in spill-related litigation or out-of-court settlements.

Lensink (1990) offered the first estimate of spill-related otter mortality: from a count of the recovered carcasses and a guess as to the percentage recovered, he suggested that, at most, 1,500 otters died. In contrast, Garrott *et al.* (1993) used the difference between boat-based counts of otters conducted in 1984–1985 (hereafter called the prespill survey) and counts a few months after the spill in 1989 (hereafter called postspill surveys) to generate a loss estimate of 2,650 for PWS alone and posited that another 2,000 otters died beyond PWS. Because of their more rigorous approach, and publication in a peer-reviewed journal, Garrott *et al.*'s (1993) estimate has been widely regarded as accurate. In a recent compilation of government-sponsored marine mammal studies conducted after the spill, their estimate is cited in four of six papers on sea otters, as well as in the book's foreword and summary chapter (Loughlin 1994). Although the quality of Garrott *et al.*'s (1993) assessment is of little concern to the conservation and management of sea otters, which still abound in the area of the spill and elsewhere, it had important legal and political implications, flavored many biological interpretations of the spill's effects, and set a precedent for similar assessments following future incidents of this nature.

The legitimacy of Garrott *et al.*'s (1993) approach depends on two fundamental assumptions: (1) that the number of animals was accurately estimated both before and after the spill, and (2) that the difference between these values was attributable to spill-induced mortality. Upon examining the data and analyses that Garrott *et al.* (1993) employed to produce their estimate, we became convinced that neither of these assumptions was met. Our purposes here are to specify shortcomings in both the data and treatment of data, and to provide recommendations for future efforts of this sort.

Biases Related to Deficiencies and Misuse of Prespill Data

Postspill sea otter surveys (Burn 1994) were patterned after the methodology of the prespill survey (Irons *et al.* 1988) with regard to type of boat, boat speed, transect width, *etc.*, to ensure comparability of results. However, several deficiencies in the prespill data hampered direct comparison of prespill and postspill counts. (1) The prespill survey was conducted to assess relationships between otter density and nearshore habitat, not to produce a population estimate; thus, counts were directed at otters within 200 m of shore. The number of otters living farther offshore had to be extrapolated from data collected during postspill surveys. (2) Sightability (proportion of otters sighted by observers) was not assessed in the prespill survey and, therefore, also had to be taken from tests done postspill, all of which were limited to the nearshore zone (Udevitz *et al.* 1995). (3) Only one prespill survey was conducted. (4) That survey was done in 1984–1985, 4–5 yr before the spill. These deficiencies do not necessarily bias Garrott *et al.*'s (1993) loss estimate, but they pose numerous irresolvable possibilities for departures from the assumptions implicit in that estimate. In dealing with these deficiencies, however, especially the time lag between the prespill survey and the spill, Garrott *et al.* (1993) introduced an assumption that we believe was not only unnecessary but invalid. Because otter densities in the unoiled portions of PWS increased by 12.7% between the prespill and postspill surveys, they ascribed a similar rate of population change to the oiled area.

Relative rates of population change for oiled and unoiled portions of PWS prior to the spill can be gleaned from historical data. Sea otters in PWS (and elsewhere) were hunted to the verge of extinction during the 18th and 19th centuries (Kenyon 1969). A remnant colony that survived in southwestern PWS (Lensink 1962) with protection, expanded north and eastward during the 1970s and 1980s (Pitcher 1975). Only two sound-wide counts of otters were conducted during the 1970s and 1980s: a helicopter survey by Pitcher (1975) in 1973 and a boat survey by Irons *et al.* (1988) in 1984–1985. These two surveys, although employing different observational platforms, indicated dissimilar population trajectories between the eastern portion of PWS, which was not oiled, and the longer-established western portion of PWS, which became oiled (Table 1). A series of boat-based counts in selected parts of western PWS during the late 1970s and early 1980s, combined with information on foraging, time budgets, and mortality, indicated that otter numbers were stable at or near carrying capacity, due to prolonged occupation of this area (Garshelis *et al.* 1986, Garshelis and Garshelis 1987, Johnson 1987). Conversely, continued population growth during the 1980s in the part of the sound that was not oiled is explainable by the comparatively late recolonization of this area by otters (Pitcher 1975, Garshelis *et al.* 1986, Johnson 1987). Data on foraging and weights of otters indicated that food resources in eastern PWS remained superior to those in western PWS during the early 1990s (National Biological Service, Anchorage, unpublished data). These patterns and interpretation are consistent with information from other sea otter populations (Kenyon 1969, Rotterman and Simon-Jackson 1988, Estes 1990, Riedman and Estes 1990). Garrott *et al.*'s (1993) assumption that population change prior to the spill was uniform throughout PWS thus runs counter to a large body of information on sea otter population dynamics, as well as to empirical data from PWS.

Biases Related to Postspill Sampling

Irons *et al.* (1988) took two full summers (1984–1985) to conduct a single count of about 95% of the shoreline of PWS (718 variable-length, 200-m-wide transects). After the spill, a series of counts were deemed necessary to document subsequent population change. In order to conduct more counts, however, postspill surveys were limited to a subset of randomly selected transects comprising 25% of the nearshore zone and 2.5% of the offshore zone. Random sampling was chosen because this procedure assures that the resulting estimate is unbiased (Cochran 1963). However, random samples are unbiased only in the sense that the average result, when the sampling is repeated many times, will converge on the population parameter being estimated. Garrott *et al.* (1993) did not do this, but instead resurveyed the same set of randomly selected transects. Whereas this approach helps ensure comparability of results, it also retains any discrepancy between the initial sample and the population being estimated.

Table 1. Counts[a] of sea otters and implied annual population growth rates[b] in the part of Prince William Sound, Alaska, that became oiled in the 1989 spill *vs.* the part of the sound that was not oiled.

	Oiled area[c]			Unoiled area		
Year of survey	Otters counted	Count extrapolated to total area	Implied annual population growth	Otters counted	Count extrapolated to total area	Implied annual population growth
1973	1,228	1,228		529	529	
1984	2,191	2,285	5.8%			
1985				1,666	1,754	10.5%

[a] Counts in 1973 were made by Pitcher (1975) from a helicopter. Counts in 1984 and 1985 were made by Irons *et al.* (1988) from a boat. All counts include only otters ≤200m from shore.

[b] Growth rates were calculated without correction for differences in sightability of otters from different survey platforms. However, the purpose here was to show demographic differences between the two areas, not to produce estimates of actual population growth.

[c] Oiled area includes the 5-km-wide buffer zone delineated by Garrott *et al.* (1993).

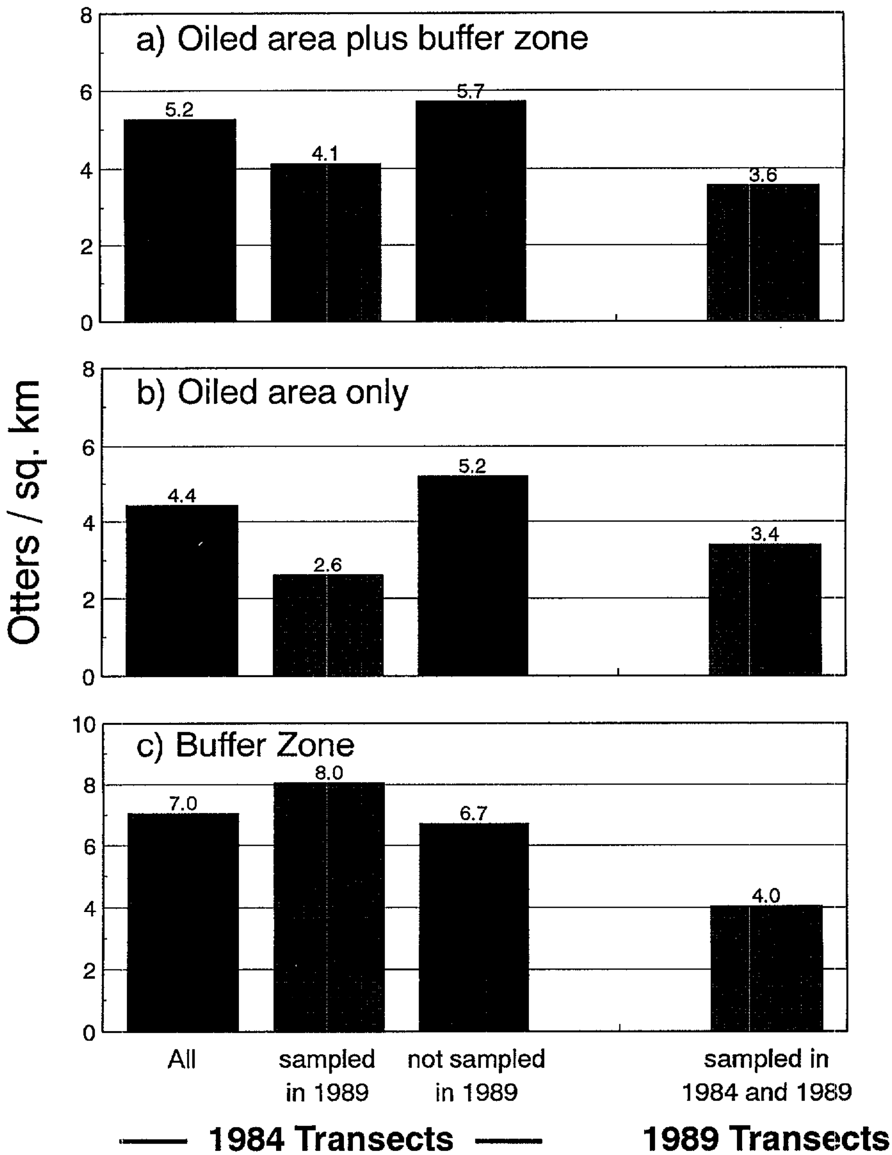

Figure 1. Density of sea otters in the nearshore zone ($\leq$ 200 m from shore) of PWS, before (1984) and after (1989) the *Exxon Valdez* oil spill. Garrott *et al.*'s (1993) mortality estimate was derived from the prespill-postspill difference in the oiled area plus buffer zone (top panel), comparing all transects surveyed in 1984 (leftmost bar) to a subset of transects sampled in 1989 (rightmost bar). No mortality estimate is possible comparing only those transects sampled in both 1984 and 1989 in the area actually oiled (center panel), because the density in 1984 (2.6 otters/km^2) was less than in 1989 (3.4 otters/km^2). The only decline on transects surveyed in both 1984 and 1989 was in the non-oiled buffer zone (bottom panel).

We examined the 1984 survey data from the area subsequently oiled and found that otter density in the transects that were sampled postspill was considerably less than the density in the transects that were not sampled postspill (Fig. 1). In fact, of 23 transects with densities exceeding 20 otters/km^2 in 1984, only one (Axel Lind Island, discussed more later) was surveyed in 1989; that is, the 25% random sample that was chosen postspill happened to include only 4% of the transects that had high prespill otter densities. If this difference in density between the sampled and unsampled transects persisted until the time of the spill, then otter density in the sample selected in 1989 was biased low.

Population surveys and other data from areas as diverse as California and Amchitka Island, Alaska, indicate that although individual otters may travel widely, especially seasonally, their general distribution and hence relative density across a broad range does tend to persist for long periods (Jameson 1989; J. A. Estes, unpublished data). In PWS, Irons *et al.*'s (1988) prespill survey, aimed specifically at characterizing habitats associated with high densities of otters, was based on the premise that relative density corresponds with habitat and thus remains fairly constant. Even following the spill, Johnson and Garshelis (1995, unpublished data) found that otter distribution in a large portion of western Prince William Sound was not noticeably altered: high and low density areas in the early and mid-1980s remained as such in the early 1990s. Consequently, by sampling mainly low density areas after the spill, and comparing that overall density to prespill density throughout the western sound, the prespill-postspill difference that Garrott *et al.* (1993) used to estimate mortality likely became inflated.

This difficulty could have been circumvented in two ways. First, after drawing the random sample of transects (but before conducting the first survey), the corresponding prespill data could have been examined to ensure that the chosen sample was representative of the targeted population. Given the comparatively low prespill density in the transects they chose to sample (Fig. 1), we believe that Garrott *et al.* (1993) should have drawn another sample. Alternatively, they should have derived both the prespill and postspill estimates from the same sample of transects. Using all prespill data, rather than just the transects surveyed postspill, increases sample size and hence boosts the statistical power needed to discern a difference between prespill and postspill densities; however, in this case the issue was not *whether* there was a difference, but how large the difference was. Given that the sample of transects chosen in 1989 was likely not representative of the whole population, a comparison of matched transects would have yielded the best estimate of this prespill-postspill difference.

Biases Related to Delineation of the Spill-Zone Boundary

If Irons *et al.* (1988) had surveyed offshore as well as nearshore areas of PWS, and thus generated a 1984–1985 population estimate for the entire sound, and if data on population growth were available to enable projection of that estimate to 1989, then spill-related mortality could have been estimated by subtraction of the postspill population estimate from the prespill estimate for the whole of PWS. Subtraction of postspill from prespill estimates of abundance for any smaller portion of PWS containing the entire spill zone would theoretically yield the same mortality estimate; that is, the exact boundaries of the spill would not be important. However, Irons *et al.* (1988) did not survey offshore areas, so Garrott *et al.* (1993) had to apply the proportion lost, estimated from nearshore counts, to the area offshore. Thus, the larger the offshore area judged to be within the spill zone, the higher the resulting estimate of mortality.

The spill zone was delineated from a composite of aerial photographs taken shortly after the spill. The region of known oiling was then circumscribed with a 5-km-wide buffer strip, so as to include areas outside the slick wherein otters might have become affected by oil. Because this buffer strip was not actually oiled, otter mortality rates there should have been lower than in the area engulfed by oil. That is, nearshore parts

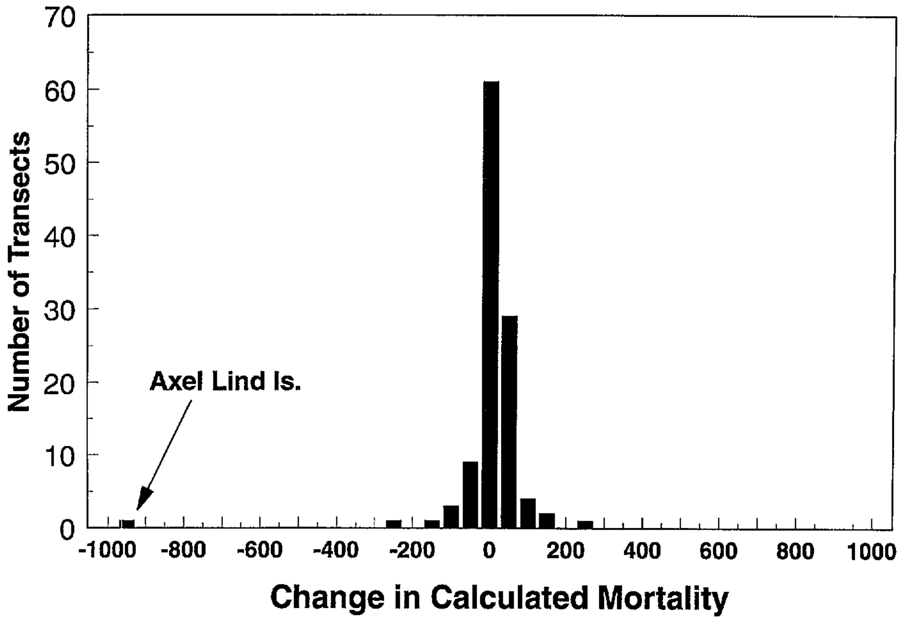

Figure 2. Change in the estimate of sea otter mortality from the *Exxon Valdez* oil spill caused by individually removing both prespill and postspill data for each surveyed nearshore transect. Survey data are from Burn (1994), provided as an electronic database. An unoiled transect on Axel Lind Island (within the buffer zone) is highlighted because of its particularly large effect on the mortality estimate.

of the buffer strip should have exhibited a lower prespill-to-postspill decline, and this lower rate of loss should have been applied to the offshore portion of the buffer strip, thus nullifying the necessarily arbitrary selection of the dimensions of this strip. In actuality, though, the prespill-to-postspill decline in the nearshore zone of the buffer strip was higher than in the oiled area (Fig. 1).

Using Garrott *et al.*'s (1993) database, but restricting the analysis to transects sampled both before and after the spill, we found that postspill density declined in the buffer strip, compared to the 1984 count, whereas it actually increased in the oiled area (Fig. 1). That is, comparing matched prespill-postspill transects, the *only* loss of otters detected after the spill was in the arbitrarily designated buffer strip!

We examined the geographical distribution of the loss of otters more closely by calculating the contribution of each transect within the oiled area and buffer zone to the mortality estimate. Each transect that was sampled both prespill and postspill was individually removed from the analysis, and mortality recalculated as per Garrott *et al.* (1993). Of 112 matched transects, only 6 (5%) affected the mortality estimate by >100 otters (3 positively and 3 negatively). One of these, a 0.8-km^2 transect on Axel Lind Island (along the northern mainland) was particularly anomalous; removal of this single transect reduced the mortality estimate by 940 otters (Fig. 2). The effect of this transect on the mortality estimate is particularly troubling because (1) it was the only transect with a high prespill density that was sampled by Garrott *et al.* (1993); (2) it was located within the buffer strip >3km from the nearest extent of oil; (3) other transects in the same general area, but well beyond the buffer strip, exhibited similar sharp declines in otter abundance since the 1984 survey; (4) actual counts at this site declined from 97 otters in 1984 to 2 in 1989 (*i.e.,* only 95 otters), but the effect on Garrott *et al.*'s (1993) mortality estimate was magnified nearly 10-fold; and (5) comparable, natural declines were witnessed at other small islands in PWS (*e.g.,* Applegate Rock) prior (and thus unrelated) to the spill (Johnson 1987), as groups of otters apparently departed together (Garshelis and Garshelis 1984) (these group movements represent occasional, isolated events, rather than large-scale shifts in distribution). Thus, the decline in otter numbers at Axel Lind Island was quite possibly due to the normal population dynamics of sea otters in PWS, having little or nothing to do with the spill, but it had a profound effect on Garrott *et al.*'s (1993) estimate of spill-related mortality.

Effects of Biases on Mortality Estimate

Garrott *et al.*'s (1993) estimate of sea otter mortality, although sound in concept, was empirically flawed because it violated the underlying fundamental assumptions. As such, we believe the analytical complexity with which Garrott *et al.* (1993) approached the problem was misdirected. While we do not question the conceptual validity of their model, we contend that the inadequate information to which this model was applied made hollow exercises of their apparent rigor.

The broad confidence interval (500–5,000) around Garrott *et al.*'s (1993) loss estimate might seem to diminish the significance of these shortcomings; however, this confidence interval was derived only from variation among transects—it did not take into account the basic problems of their approach and data. Moreover, the lower confidence limit was truncated at the minimum number of animals that were known to have died from the spill (oiled carcasses collected in PWS plus those that died during attempted rehabilitation = 475), and the upper limit approximated the total number of otters in the oiled area before the spill. The actual mortality certainly fell between these limits, as these were essentially the defaults that bounded the span of all possibilities. That Garrott *et al.*'s (1993) confidence limits bracket the real value is thus tautological and gives no credence to their method nor to their point estimate, which is cited far more often than their confidence interval.

Our principal purpose in writing this commentary was not just to argue that Garrott *et al.*'s (1993) loss estimate was unreliable, but to show that a meaningful estimate of sea otter mortality cannot be derived from the available survey data. To demonstrate how misleading this estimate could be, we recalculated mortality using Garrott *et al.*'s (1993) procedure, but treating some of the assumptions differently. We did this *not* to generate more realistic mortality estimates, but rather to examine the sensitivity of their estimate to the tenuous assumptions. We examined combinations of scenarios with and without the presumed 12.7% growth rate between 1984 and 1989, using all transects *vs.* just those in common between the 1984 and 1989 surveys, and including or excluding the buffer strip. All of these resulted in mortality estimates lower than those obtained by Garrott *et al.* (1993), and in two cases mortality estimates became negative (Table 2).

We do not suggest that any of these scenarios produced a more reliable estimate than Garrott *et al.*'s (1993). The negative estimates are clearly impossible, but resulted because within the area actually oiled, on transects sampled both before and after the spill, otter density was higher afterwards. There are at least four explanations for this paradox. One is that the otter population in western PWS increased significantly (more than Garrott *et al.*'s [1993] estimate of 12.7%, *i.e.,* > 2% per year) between the mid-1980s and the time of the spill. Second, many of the otters killed in the spill may have been replaced by otters from unoiled portions of PWS, although this would had to have occurred after the spill moved through the area in April and before the first postspill survey in June and while the area was heavily congested with boats involved in clean-up operations (heavy boat traffic tends to deter otters [Garshelis and Garshelis

Table 2. Estimates of spill-related mortality for sea otters in Prince William Sound, Alaska, generated using the boat-based survey data from Burn (1994) and Irons *et al.* (1988) and the procedure presented by Garrott *et al.* (1993), but with different assumptions.

Assumptions				
Population growth 1984–1989[a]	Transects compared[b]	Treatment of buffer zone[c]	Mortality estimate	Disparity from mortality estimate of Garrott *et al.* (1993)
12.7%	All	Included	2,648[d]	—
0%	All	Included	1,910	738
12.7%	Matched pairs	Included	1,454	1,194
0%	Matched pairs	Included	783	1,865
12.7%	All	Excluded	1,421	1,227
0%	All	Excluded	917	1,731
12.7%	Matched pairs	Excluded	−543	3,191
0%	Matched pairs	Excluded	−936	3,584

[a] Garrott *et al.* (1993) assumed the population grew by 12.7% between the last prespill survey in 1984 and the 1989 spill.

[b] Garrott *et al.* (1993) compared densities on all transects surveyed in 1984 with a 25% random sample of transects surveyed in 1989; mortality estimates using matched pairs of transects include only those that were surveyed in both years.

[c] Garrott *et al.*'s (1993) mortality estimate included a 5-km-wide buffer strip around the area of known oiling; mortality estimates shown as excluding the buffer strip excluded only the portion of the strip nearshore (if the whole strip had been excluded, mortality estimates would have been lower).

[d] Estimate made by Garrott *et al.* (1993).

1984]). Third, since Irons *et al.* (1988) did not estimate probability of sighting, the possibility exists that their prespill survey detected a smaller proportion of the population than had been estimated from postspill data, despite attempts by postspill investigators to replicate prespill methodology. Finally, just as Garrott *et al.*'s (1993) random sample of transects may have provided an unrepresentatively low estimate of postspill otter density, the absence of any apparent decline in otter numbers after the spill could have been the simple consequence of sampling variation, especially given that there was but a single prespill survey. None of these scenarios can be categorically rejected with the available data. Johnson and Garshelis (1995), using their own boat survey data and different sets of prespill data, concluded that the first explanation (*i.e.*, a population increase during the late 1980s of about 5% per year) was most plausible. Accepting that there was a population increase in western PWS during the late 1980s on the basis of these unexpectedly high postspill counts does not, however, lend support to Garrott *et al.*'s (1993) assumption of such an increase based on data from eastern PWS. One cannot logically use postspill counts that exceeded prespill counts within the area of the spill as evidence for a prespill population increase, and then use a different postspill population estimate, smaller than the prespill estimate, to derive an estimate of loss.

Recommendations

We have criticized Garrott *et al.*'s (1993) mortality estimate as being founded on invalid or violated assumptions and have argued further that a reasonable estimate of sea otter losses from the *Exxon Valdez* oil spill cannot be made with the available boat-based survey data. The failure of this method was partly attributable to the way the data were handled and partly to deficiencies in the data. Garrott *et al.* (1993) recognized some of the weaknesses in their data (especially the prespill data) and recommended more comprehensive surveys of spill-vulnerable populations every 2–3 yr. In our view, the large area of potential concern, and the rare and unpredictable nature of oil spills, makes this approach difficult to justify.

We suggest three other strategies to estimate spill-related mortality of sea otters that would be more tenable, because they could be implemented after a spill has occurred: (1) collect carcasses and experimentally assess carcass retrieval rates, (2) compare counts of otters immediately in front of and then behind the advancing oil slick, and (3) monitor the fates of otters captured and radiomarked ahead of the slick. The first strategy was attempted after the *Exxon Valdez* spill but only at Kodiak Island (DeGange *et al.* 1994*a*) with a small sample of carcasses and an unrepresentative carcass collection effort (Garshelis 1997). The second strategy was apparently attempted, at least cursorily in PWS (Zimmerman *et al.* 1994) and somewhat more thoroughly along the Kenai Peninsula, Kodiak Island, and the Alaska Peninsula (DeGange *et al.* 1994*b*), but the effort expended was inadequate to obtain counts with sufficient precision to document sea otter losses. The third strategy was considered, and funding was obtained (C. Monnett and L. M. Rotterman, personal communication), but the work was not implemented because of bureaucratic and political obstacles. These failings point to a lack of prespill planning and preparation, as well as some poor judgments postspill, which precluded investigators from being able to appropriately address the issue of how many otters died. In retrospect, these other strategies were feasible and, if properly done, likely would have yielded a better estimate of spill-induced sea otter mortality than did the boat surveys.

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Response to Critique by Garshelis and Estes of *Exxon Valdez* Sea Otter Mortality Estimate

The introductory paragraphs of the Garshelis and Estes critique of our paper (Garrott *et al.* 1993) suggest that the main motivation for their efforts stems from the fact that our point estimate of sea otter mortality due to the *Exxon Valdez* oil spill has been cited in a modest number of papers and "has been widely regarded as accurate." We are pleased that our paper has been of some utility to others concerned with sea otter conservation and the effects of oil spills on natural resources, but believe few scientists would regard the estimate we provided as accurate. Indeed, we expended nearly as much time in discussions among ourselves and colleagues, and performing analyses and computations to develop confidence limits on the estimate, as was expended in developing the estimate itself. The result of this effort was an estimate of sea otter mortality in Prince William Sound of 2,650 with confidence limits on the estimate of 500–5,000 otters. The wide confidence limits were, in fact, one of the major points of the paper, serving to highlight the lack of adequate data to assess impacts of such catastrophic events as the *Exxon Valdez* oil spill. Given our efforts to emphasize the wide confidence limits on the mortality estimate throughout the paper, from the abstract through the discussion, we believe that Garshelis and Estes' impression that the scientific community has accepted our estimate of spill-related sea otter mortality as "accurate" is mistaken. Nevertheless, we offer a brief response to their critique in an effort to provide additional constructive suggestions for those that may be faced with similar problems in the future.

Although Garrott was directly involved in efforts to evaluate the effects of the oil spill on sea otters during the weeks immediately following the accident, we (Garrott and Eberhardt) were not asked to assess the data on otter mortality until some time after the event. We were not consulted as to survey methodology prior to implementation of the post-spill boat surveys and, thus, had to work with the available data. We did strongly recommend that an effort be made to obtain a correction for otters missed in the surveys, and this led to the study reported by Udevitz *et al.* (1995). Garshelis and Estes have criticized our analytical procedures and results by using some radical departures from accepted sampling methodology. Hopefully, these errors will be apparent to many readers, but we think it worthwhile to call attention to the more important errors, and to mention some other issues.

Garshelis and Estes' first major criticism of our work ("Biases related to deficiencies and misuse of prespill data") challenges our estimate of a 2% annual increase in sea otter numbers between the pre- and postspill boat surveys, arguing that the sea otter population was "stable at or near carrying capacity, due to prolonged occupation of the area." We noted in our paper that there was no satisfactory information on growth of the Prince William Sound sea otter population from 1984–1985 (the time of the prespill boat survey) to the time of the postspill surveys conducted in 1989. The papers cited by Garshelis and Estes provide no direct estimates of population trends and were limited to data collected in a very small portion of the Sound. We used the best information available, the change in otter densities in unoiled areas between the pre- and postspill surveys. The argument is made that the oiled area had been colonized for a long period, resulting in a stable sea otter population, while the unoiled area has been colonized only recently by the expanding sea otter population and, hence, would still be increasing in number. We believe the fundamental premise of this argument is flawed. A comparison of the map of sea otter colonization of Prince William Sound provided by Rotterman and Simon-Jackson (1988, fig. 3, p. 246) and the oil spill map provided in our paper indicates that the Sound was generally colonized from the south to the north. Portions of both oiled and unoiled areas have been occupied by otters since the 1950s, with nearly the entire Sound occupied by the mid 1970s. The portion of the Sound that has experienced the most recent colonization (1980), Orca Inlet near the town of Cordova, was not included in the boat surveys due to the difficulty of surveying the extensive tidal flats in the area. Hence, we think the estimate of modest annual population growth derived from boat survey data from the unoiled portions of the Sound is reasonable given the lack of any other empirical data. What is most puzzling to us about this argument is that Garshelis and Estes assert later in their critique that the sea otter population in the oiled area may have actually been increasing at a rate of 5% per year.

The second criticism ("Biases related to postspill sampling") challenges the sampling protocols used for the boat surveys. We acknowledged some of the shortcomings of the sampling protocol in our paper, but we do not agree with the strategies suggested by Garshelis and Estes. Irons *et al.* (1988) counted all the shoreline areas during the summers of 1984–1985. If their counts were absolute, there would be no sampling error in the shoreline total. However, a variety of other evidence, along with the study of Udevitz *et al.* (1995) in the study area, indicates that not all otters were seen. Nonetheless, the 1984–1985 shoreline counts provided a good relative measure of the population in the shoreline stratum. However, there were no counts in 1984–1985 in the offshore areas, so that the only data on otter numbers in that region came from the postspill surveys. Unfortunately, sample sizes in those strata ("offshore" and "coastal") were too small, yielding estimates of otter numbers with large standard errors (see our Table 1). This is a major problem with the population estimate.

Garshelis and Estes claim that the postspill random sample was biased and that we should have discarded it and drawn another sample. This is the kind of "purposive" sampling that all sampling authorities warn against, and very bad advice indeed. Anyone planning a survey under similar circumstances needs to be very much aware that litigation is an adversarial process and any such error will greatly weaken their case. Given that the local pattern of otter density does not change much, the 1984–1985 data might have been used to design a more efficient scheme (possibly a stratified ratio estimate), but we were not consulted at the planning stage. Garshelis and Estes also recommended that the estimates should have been derived from the same set of transects. This amounts to discarding information from the 1984–1985 survey and is again poor sampling practice.

Several arguments were made under the heading "Biases related to delineation of the spill-zone boundary." The first dealt with the delineation of a 5-km "buffer strip" around the perimeter of the area affected by the spill. Garshelis and Estes discuss the boundary of the spill as if it were known precisely, when in fact it was not. When we initiated the work on estimating sea otter mortality, nearly a year after the spill, a definitive map depicting the extent of the oil spill had not yet been produced, and with each new such effort, boundaries of the oil spill were modified as additional data were incorporated into the maps. Hence, the absolute boundaries of the spill could never be determined, although those areas covered by heavy crude were readily identified. Since the boundary of the spill could not be determined precisely, we decided to include a buffer strip to assure that the "oiled area" delineation used in our analyses encompassed the entire spill-affected area. Otters are quite mobile and did not appear to detect and avoid the oil, so we felt that the impacts of the oil spill would extend beyond the absolute boundaries of the spill. Hence, we believed that the combination of uncertainties of the actual extent of the oil spill and the mobility of otters dictated use of a relatively coarse spatial scale and that the types of fine-scale delineations and calculations suggested by Garshelis and Estes were inappropriate.

In this same section of the critique the authors state that we "had to apply the proportion lost, estimated from the nearshore counts, to the area offshore." This is not the procedure employed, which is given by equation (1) of our paper. Ratios of estimated densities in oiled and unoiled areas were used, with the same areas used throughout. These ratios do not include offshore areas, which were only involved in preparing the 1989 population estimates. Garshelis and Estes also go on to calculate:

> ". . . the contribution of each transect within the oiled area and buffer zone to the mortality estimate. Each transect that was sampled both prespill and postspill was individually removed from the analysis, and mortality recalculated. . . ."

This is the basis for an approach known as jackknifing, used to calculate variances in complex situations. If carried through appropriately it would show that the overall mortality estimate has very wide confidence limits, as we had already determined. However, all sampling texts point out that one cannot legitimately dissect results of a random sample after the fact and use portions of the results separately as done by Garshelis and Estes. As pointed out above, a more efficient sampling design should have been used in the postspill studies.

Under the section "Effects of biases on mortality estimate," the authors comment that our estimate was "empirically flawed because it violated the underlying fundamental assumptions," and "the analytical complexity . . . was misdirected" making "hollow exercises of their apparent rigor." This is largely rhetoric. They go on to attempt further *post facto* fishing expeditions into the data, including various speculative explanations generated by selecting combinations of "scenarios." These efforts lack any semblance of rigor.

In the final section of the critique, Garshelis and Estes make various recommendations for determining spill-related mortality of sea otters, largely in line with those made in our paper but with less detail. The authors dismiss our suggestion of conducting comprehensive surveys of otter populations vulnerable to oil spills at 2–3-yr intervals and as soon after a spill as conditions comparable to prespill surveys are attained. Instead they focus on three strategies that would have to be implemented during the spill event. Although we discussed all three of these possible strategies in our paper, we are less optimistic that they can be effectively implemented. One of us (Garrott) was involved in attempts to execute each of these suggested postspill studies during the *Exxon Valdez* spill, and, as Garshelis and Estes noted, each attempt failed to yield the information needed to estimate sea otter losses. The authors suggest that this failure was due to a lack of prespill planning and preparation and poor judgement. In part this might be true. However, we think that any strategy that depends on intensive data collection during a spill event is quite risky. A major oil spill is always a crisis, and everyone associated with a spill must respond to the most pressing needs of the moment. In the case of the *Exxon Valdez* accident, nearly all logistic support including aircraft, boats, and housing were almost immediately committed to various activities such as equipment and personnel transport, oil containment, monitoring, cleanup, and protection of fish hatcheries, harbors, and other important sites. Even if agencies responsible for natural resource management have dedicated equipment for spill-related research, the operation of boats in oil spill areas, necessary for at least two of the three recommendations, would be problematic at best. The heavy crude floating on the water fouls boat hulls and clogs engine cooling systems. Oiled boats may also be denied access to home ports for refueling and resupply. The U.S. Coast Guard or other authorities may restrict or deny access to critical spill areas for both aircraft and watercraft. All of these limitations were experienced during the *Exxon Valdez* event, and we doubt that such problems can be completely overcome with adequate planning. Thus, it is our opinion that, although data collection activities at the time of the spill would certainly be a worthwhile effort, the primary strategy for assessing spill-related losses of sea otters may need to be maintenance of routine comprehensive population surveys.

We would like to add a recommendation that anyone having responsibility for areas subject to the possibility of an oil spill should have a contingency plan based on consultation with someone experienced in sample survey methodology and well acquainted with the available data on the species and environments likely to be involved. The various possible alternative schemes for estimating loss should be evaluated in more detail than was feasible in our paper and field-tested insofar as possible. The frequency of major oil spills worldwide makes it nearly certain that further spills will occur. The fact that attempts to discredit one of the few quantitative analyses of actual data made thus far are being made eight years after the event indicates the importance of such planning.

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Lone Sociable Bottlenose Dolphin in Brazil: Human Fatality and Management

Lone, wild, and sociable bottlenose dolphins, *Tursiops truncatus,* have been described worldwide for many years (see Lockyer 1990, St. John 1991, Dudzinski *et al.* 1995). It is still not clear why these dolphins spontaneously seek human company (Lockyer 1990).

Beginning in March 1994, a lone dolphin was observed on a daily basis in the vicinity of the ferryboat pier at São Sebastião (23°48′S, 45°24′W), São Paulo State, Brazil. It was nicknamed "Tião." "Tião" was a 2.60-m male bottlenose dolphin with a deep, rounded wound on its lower jaw tip and an easily recognizable dorsal fin. For about five months, it was observed following the ferryboat and other small boats in the area. After this period it moved northwards to Caraguatatuba (23°37′S, 45°23′W). From August to December 1994, it interacted often with bathers. The bathers' behavior varied from simply touching the animal, to grabbing its fins, to hitting it and jumping on it. Some bathers even attempted to put ice-cream sticks into its blowhole. The dolphin reacted aggressively when repeatedly harrassed and injured about 29 bathers, who were sent to the hospital with minor injuries. On 8 December 1994, "Tião" struck a 30-year-old bather who died several hours later from internal bleeding due to a stomach rupture, according to "Casa de Saúde Stella Maris," the local hospital.

After the human fatality, a management program was planned by a team consisting of the author and members of a federal environmental agency (IBAMA), a non-governmental organization (FUNDAMAR), and the Prefecture of São Sebastião. The main goal of this program was to avoid more accidents during the summer season by focusing on (1) public education, (2) media control, and (3) prevention of "harmful" interactions between the dolphin and human beings. Team members worked daily from 0800 to 2000, from mid-December 1994 until mid-February 1995 monitoring "Tião" and the bathers. Brochures providing information about the dolphin and how to behave safely in its presence were distributed. When beaches were crowded and the dolphin appeared, swimmers were asked to leave the water. However, when the dolphin approached bathers before they could leave the water, the educational material warned against touching the dolphin's sensitive areas such as the blowhole, the genitals, and eyes. At night, free lectures about wild dolphins and "Tião" were presented to the public in general.

Perhaps as a result of this educational material, no more accidents were recorded. On January 1995 the dolphin returned to São Sebastião, where it was observed until August. Since then it has not been seen.

Because the sightings of this dolphin were preceded by the release of a captive dolphin about 600 km to the south in 1993, there has been speculation that "Tião's" attraction towards humans was due to a previous history in captivity. However, these dolphins were not the same individual because: (1) the captive dolphin had been freeze-branded with a Brazilian flag, a brand not observed on "Tião"; (2) at the same time that "Tião" was being sighted daily in São Sebastião, the former captive dolphin was being sighted 110 km to the south at São Vicente in May 1994, and 360 km to the south off the Paraná coast from July until October 1994, when it disappeared; (3) the former captive was 2.80 m long in 1993, while "Tião" was measured to be 20 cm shorter than this in 1994; and (4) close-up sightings of both dolphins and comparison of photographs of their dorsal fins by the author indicated that they were not the same individual.

Interactions between humans and wild dolphins may lead to unfortunate consequences for people and for dolphins. Because dolphins sometimes seek out humans, regulations and guidelines to manage such situations are necessary to avoid injuries to humans and to dolphins.

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Stenella attenuata from Curaçao Misidentified as *S. coeruleoalba*

Debrot and Barros (1994) reported the collection of the head of a juvenile striped dolphin, *Stenella coeruleoalba*, found floating at sea off St. Michielsbaai, Curaçao. The accompanying photograph showed a color pattern (no pronounced eye-to-anus stripe and indistinct flipper stripe) more like that of a spotted dolphin, *S. attenuata* or *S. frontalis*, than that of a striped dolphin, and re-examination of the skull (specimen ZMA24.595 at the Zoological Museum of Amsterdam) revealed characters that strengthened this impression (arcuate ramus and absence of palatal grooves—Perrin *et al.* 1981—and tooth counts at or below the known minima for *S. coeruleoalba* but within the ranges for *S. attenuata* and *S. frontalis*—Perrin and Hohn 1994; Perrin *et al.* 1994*a,b*). The two spotted species overlap in all cranial characters in the Caribbean (Perrin *et al.* 1987), precluding further identification of the juvenile specimen from skull measurements or tooth counts alone. Therefore, we decided to attempt an identification based on DNA sequence comparisons.

Using standard protocols, DNA was extracted from a small piece of dried tissue removed from the skull, and a 787-base-pair fragment of the mitochondrial cytochrome B gene was amplified using the polymerase chain reaction (PCR). Both strands of a 396-base-pair fragment in the middle of the gene were sequenced on an automated sequencer and compared to the same gene-segment from known specimens of the three species of *Stenella*: three of *S. coeruleoalba* (two from the Atlantic and one from the Pacific), three of *S. attenuata* (two Atlantic and one Pacific) and two of *S. frontalis*.

Among the nine sequences, there were 32 variable sites out of the 396 bases. Two of these were ambiguous in the sequence of the unknown sample and were not included in the comparisons. For the 30 remaining sites, intraspecific pairwise comparisons for the three species, even between ocean basins, showed seven or fewer differences, while interspecific comparisons showed 7–22 differences. The unknown sample showed much greater similarity to the *S. attenuata* sequences than to any of the sequences from the other two species (3–4 *vs.* 15–19 differences). Further, there were 13 sites where all three of the *S. attenuata* sequences had a fixed difference from the *S. frontalis* sequences, and at all of these sites the unknown sample had the same base as the *S. attenuata* sequences.

We conclude based on these results that the juvenile calf from Curaçao was a pantropical spotted dolphin, *S. attenuata.* This species has not been previously reported from Curaçao.

We thank Mike Henshaw for extracting the DNA and Andy Dizon and Bob Brownell for reading the manuscript and offering suggestions for its improvement. Details of the genetic analysis and results are available from the authors.

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Memories

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Deaths Reported

Keiji Nasu, 28 October 1996, Miyazaki, Japan
Age Jonsgård, 9 January 1997, Oslo, Norway
Stephen Leatherwood, 25 January 1997
Bob Blaylock, 31 January 1997

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