TESTING FOR RESOURCE USE AND SELECTION BY MARINE BIRDS: A REPLY TO AEBISCHER AND ROBERTSON

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Abstract.—Aebischer and Robertson (1993) suggest compositional analysis as a preferred substitute for pre-existing, alternative techniques that examine resource use and selection. It is revealed how their proposed use violates basic prerequisites of statistical inference because information "packets" are based on fallacious sample units with no known statistical or biological target population. Compositional analysis is neither suitable nor necessary for sample variables (whole integers) obtained from enumerated data, the performance of this technique (under different conditions and relative to alternatives) has not yet been evaluated, and the sample sizes used to advocate it were, by the authors' own admission, "borderline." A distinction is made between inherent assumptions of statistical techniques *per se* and weaknesses of biological data employed in those techniques; the latter do not automatically invalidate the former. Attention is drawn to sources that will enable the field ornithologist and statistical practitioner to choose among several tests that address resource selection in the widest sense, thereby finding the best match to his/her data structure, biological hypothesis of interest, the logistical constraints of field studies and the behavior of the animal population being studied.

PRUEBAS RELATIVAS AL USO Y SELECCIÓN DE RECURSOS POR AVES MARINAS: UNA CONTESTACIÓN A AEBISCHER Y ROBERTSON

Sinopsis.—Aebischer y Robertson (1993) sugieren el análisis de composición para sustituir correctamente las técnicas alternas preexistentes de examinar la selección y uso de recursos. Se muestra como el uso propuesto por ellos viola prerequisitos básicos de inferencia estadística, ya que los paquetes de información se basan en unidades de muestreo falsas sin poblaciones estadísticas o biológicas conocidas bajo estudio. Entre las deficiencias propuestas se encuentran: el que el análisis composicional no es apropiado ni necesario para variables de muestreo (integrales completos) obtenidos de datos enumerados, que la acción de esta técnica (bajo condiciones diferentes y relacionadas a alternativas) no se ha evaluado, y que los tamaños de muestra usados para la recomendación eran "mínimos" por admisión de los autores. Se hace una distinción entre aseveraciones inherentes a las técnicas estadísticas y deficiencias en los datos biológicos usados para esas técnicas; los últimos no invalidan a los primeros. Se llama la atención a fuentes que permitirán tanto al ornitólogo de campo como al estadístico práctico seleccionar dentro de varias pruebas que evalúan la selección de recursos en la forma más amplia, permitiéndole la mejor prueba para analizar la estructura de sus datos, las limitaciones logísticas de estudios de campo, hipótesis biológicas de interés y el comportamiento de la población animal estudiada.

Currently, there is extraordinary interest among researchers concerning resource selection by wildlife (see Alldredge and Ratti 1992). Given a staggering array of methods available for analyzing results from field and laboratory investigations generally, it is worthwhile to ask whether an additional if not entirely new test lives up to claims, especially if those claims call for abandoning alternatives. As compositional analysis may be unfamiliar to many field ornithologists, further discussion of its merits and drawbacks is appropriate from this standpoint as well. Presumably,

ornithologists want to be aware of statistical pitfalls while at the same time avoiding the paralysis of statistical paranoia. Fortunately, these goals need not be mutually exclusive.

Aebischer and Robertson (1993) suggest compositional analysis for examining resource use and selection. Several points seem to be interesting and provocative, but their proposed use violates basic preconditions of statistical inference. And contrary to their assertions, the technique is neither suitable nor necessary for sample variables consisting of enumerated data. Not surprisingly, the reader is left completely uninformed about several pertinent issues. Speaking about their own sample sizes, Aebischer et al. (1993:1321) admit that "the examples presented in this paper are therefore borderline in this respect" (cf. Morrison 1988). Still later we find that "the performance of the technique [compositional analysis] remains to be evaluated" (Aebischer et al. 1993:1322).

Unlike Aebischer and Robertson (1993), I make a clear distinction between inherent assumptions of statistical techniques *per se* and weaknesses of biological data employed in those techniques. I conclude by referring the reader to sources that will enable field ornithologists and statistical practitioners to choose among several tests that address resource selection, thereby finding the best match to his/her data structure, the logistical constraints of field work, and the behavior of the target population under study.

In their attempt to justify use of compositional analysis, Aebischer and Robertson (1993) repeatedly mistake necessary assumptions common to all statistical tests for "problems." The most fundamental topic in their comments (point 3 = data structure) is addressed first; additional points are subsidiary to point 3 and are not unique to any single technique.

Statistical inference involves drawing subsamples in representative fashion (e.g., random, systematic), calculating a test statistic, comparing the observed to the expected test statistic at some pre-determined probability level, and finally extending results to the entire target population from which the subsamples were drawn. Aebischer and Robertson (1993) implore us to use "packets" of information, such as the number of birds per transect, integrated into sample units. Such sample units are entirely the construct of the investigator, with absolutely no connection to real units or variables existing in nature (e.g., individual birds, genetic alleles, nests, clutch size, number of fledged young). As variability among "packets" results solely from the investigator's arbitrary arrangement, differences among packets have neither statistical nor biological meaning.

To illustrate the consequences of this practice, we should ask: from what target population are such "packets" drawn? Is it all possible "packets" and "packets" (=transects) consisting of what size, shape, and spacing? What biological population is to be evaluated with this ambiguous statistical population? These questions are of course unanswerable. When statistical and biological populations are ill-defined or unknown (see Tacha et al. 1982 for safeguards), statistical inference should not be conducted at all (James and McCulloch 1985:31). No statistical technique

can ever salvage results from such ill-conceived sampling designs. By
advocating arbitrary sample units, Aebischer and Robertson (1993) iron-
ically repeat mistakes that can be circumvented easily by using any of
several techniques expressly designed for enumerated data (e.g., the con-
fidence-interval method described by Neu et al. [1974] and Byers et al.
[1984], recommended in Haney and Solow [1992a]).

Besides inability to use statistical inference, there are at least two other
undesirable results from this practice. First, when individual birds are
grouped into artificial sample units, sample sizes are reduced and the risk
of Type II error greatly increases (Haney and Solow 1992b). Second,
Aebischer and Robertson (1993) fail to recognize fundamental differences
between statistical techniques appropriate for measured, continuously-
distributed variables versus counted, enumerated variables that consist
entirely of whole integers. Precautions against this common oversight
have been voiced previously in ornithology (see Rice 1987). Large numbers
of consistently small integers (often zero) in count data typically preclude
normal distributions (Haney and Solow 1992b). Not only is univariate
normality an assumption to compositional analysis (Aebischer et al. 1993),
residuals must follow multivariate normality as well if hypothesis testing
is conducted!

Most ornithological investigators will recognize these distributional
assumptions as far from inconsequential when multivariate testing is
considered (Johnson 1981). At least Aebischer and Robertson (1993) seem
dimly aware of this drawback because they acknowledge that random-
ization tests are likely to be needed given inevitable failures in distribu-
tional assumptions. Unfortunately, they do not demonstrate whether such
permutation tests can successfully redeem results from compositional anal-
ysis under various conditions (e.g., covariables; see below), nor do they
indicate costs in programming or computer time associated with the ex-
ecution of such tests. Practitioners should be skeptical of unsupported
pleas to join statistical bandwagons on purely technical bases (see Green
1979 for examples of unforeseen consequences following the "flight to
nonparametrics").

With respect to their point 1, and its assertions notwithstanding, the
confidence-interval method (Byers et al. 1984, Neu et al. 1974) conforms
to the unit-sum constraint, i.e., resource availability sums to an overall
probability of one. Indeed, this was specifically addressed by the statement
"the most critical assumption for the procedure is that resource types
must be truly accessible" (Haney and Solow 1992a:46). The number of
resource categories should be limited, preferably with few or no unused
categories (Alldredge and Ratti 1986). The two-resource example offered
by Aebischer and Robertson (1993) to illustrate a supposed problem is
a red herring. Confidence-interval methods are only necessary if resource
categories number three or more (Haney and Solow 1992a). If enumerated
data are being compared across only two resource categories, the inves-
tigator can rely upon standard categorical procedures such as chi-squared,
G-tests, etc.

Their point 4 states that the confidence-interval method is not sufficiently flexible to accommodate for covariables such as age and sex. Again, this statement is specious. Suppose that one wished to know whether selection across oceanographic habitats was equivalent for young-of-the-year and adult Short-tailed Shearwaters (*Puffinus tenuirostris*) in the northwest Bering Sea (Haney and Solow 1992a). In this case, observed frequencies would be compared to confidence intervals on expected probabilities derived from one or more cells in a 2 × 4 contingency table. Unless sample sizes for all cells were quite large (i.e., no sparse cells ≤5 in order to adhere to conditions of the bionomial distribution), this test *can* lose its sensitivity (Alldredge and Ratti 1986). This is by no means equivalent to invalidating the test, however. Whenever covariables are present (and they will *not* be in all biological hypotheses of interest), investigators can use either multi-way contingency tables (Agresti 1990) or log-linear models (Fienberg 1980). Each technique is expressly designed for enumerated data, and log-linear modeling allows addition of covariables, weighted terms, and testing for both main and interaction effects (e.g., Wilkinson 1989).

The rationale for composition analysis presented by Aebischer and Robertson (1993) fails to differentiate those behavioral, social and ecological characteristics that impact the sampling of disparate species groups, i.e., radio-tagged, relatively sedentary terrestrial birds versus line transect counts of highly-mobile marine birds (albatrosses travel up to 16,000 km in a single foraging trip; Jouventin and Weimerskirch 1990). In point 2, Aebischer and Robertson (1993) opine that conditions of binomial experiments (an assumption to the confidence interval method; see Haney and Solow [1992a]) are violated if seabirds form flocks. Here Aebischer and Robertson confuse independence of animal location with independence in sample units. Separate birds are distinct sample units, with straightforward connections to a target population, regardless of their spatial proximity. It is true that spacing requirements of territorial species, such as the pheasants studied by Aebischer et al. (1993), influence access to and thus use of available habitats by each individual. Their concern is rightly applied to such species and circumstances, yet we are not told if or how such concerns were addressed during compositional analysis of those data (cf. Haney and Solow 1992a).

Only if persistent social units other than individual birds are present (i.e., pairs, adults with brood, etc.) can behavior consistently influence assumptions of independence for population counts. Notably, seabirds are not territorial at sea, inhabiting ambits (expanding and contracting spheres of action) rather than home ranges (Haney and Solow 1992b). And unlike terrestrial birds, which form persistent foraging flocks (Morse 1970), seabirds do not forage in or as groups (Hoffman et al. 1981). Rather, seabird aggregations form briefly during localized, temporary feeding bouts. Experimental prompting of temporal and spatial parameters for this process indicate maximum limits of minutes and about 5–10 km, respectively (Haney et al. 1992). The importance of even this temporary

process may be over-appreciated. Recent studies of radio-tagged Black-legged Kittiwakes (*Rissa tridactyla*) indicate that foragers, even mated pairs, actually ignore flocks and commute to individually-selected feeding sites (Irons 1992).

Due to hierarchical habitat selection at different spatial scales (Hunt and Schneider 1987), flocking will never compromise independence assumptions if spatial boundaries on designated habitats exceed flocking dimensions (10–50 km dimensions were used in the habitat resource example provided in Haney and Solow 1992a). Even if habitat dimensions are approximately equivalent to distances of individual attraction, the investigator can safeguard against this potential departure from the independence assumption by collecting count data under different conditions (time of day, location, etc.), thereby achieving greater dispersion in observational (as opposed to manipulative) study designs (see Eberhardt and Thomas 1991, Haney and Solow 1992b, Hurlbert 1984).

In perhaps the most delusive assertion, Aebischer and Robertson (1993) insist that testing for equal use of six prey items by Crested Auklets (*Aethia cristatella*) is biologically meaningless. Such preliminary analysis showed that *Thysanoessa* euphausiids dominate dietary intake for the species in this region during this period. Crested Auklets preferred euphausiids, *whatever their availability*. Unarmed with such information, testing the more specific hypothesis of prey selection would be futile because determining availability in marine settings depends upon prey species-specific gear deployment and a complementary sampling protocol. As the non-random prevalence of this prey type has been determined, we could justify trawl design to specifically measure availability of *Thysanoessa*. Practical outcomes might involve constructing gear with net mesh size for excluding items smaller than *Thysanoessa* (thus cutting down on net drag during tows) along with a net opening large enough to overcome gear avoidance by this rapidly swimming plankter.

Rather than attempt to justify a single method by searching for appropriate examples, statistical techniques should fit each particular field problem. As Aebischer et al. (1993:1322) did "not pretend that compositional analysis is the ultimate solution to analyzing habitat use," it is somewhat puzzling why Aebischer and Robertson (1993) do so here when questioning the confidence-interval method. In over 50,000 simulations of four different techniques for resource selection, including the confidence-interval method (Byers et al. 1984, Neu et al. 1974), no method was best in all cases purely on statistical grounds (Alldredge and Ratti 1986). From a practical perspective, the confidence-interval method is still the only method for resource selection that does not require individually-marked animals. For other situations, including radio-tracking data, many methods are available (Aebischer et al. 1993; Alldredge and Ratti 1986, 1992; Johnson 1980; Kincaid and Bryant 1983; Quade 1979; Thomas and Taylor 1990). Practitioners can search for those methods that most closely approximate the biological hypothesis of interest in their study (Alldredge and Ratti 1992), the animal population investigated,

and the data structure itself. These are the best criteria for choosing any methodology.

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