

**SINGLE-SITE AND REGIONAL SURVEYS OF CROCODILIANS OR  
CROCODILIAN SIGN: DESIGN AND INFERENCE CONSIDERATIONS  
FOR ESTIMATION OF POPULATION TREND<sup>1</sup>**

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Counting individuals directly or indirectly (by nests, tracks, or other signs) in crocodilian population surveys is a popular tool for monitoring population status and trend. Relative to surveys of many other species, individuals are easy and inexpensive to count. Patterns observed in the survey data are thought to be representative of patterns in the population. Thus, survey programs are an attractive option for resource agencies desiring to monitor crocodilian populations.

In typical settings, counts (taken here to mean either a true count or some proportionate index of population size) are collected under some prescribed conditions at regular time intervals at one or more survey areas. Information on environmental conditions or survey characteristics may also be recorded. Translating the data into useful inferences on population trend relies on (a) correctly identifying a statistical model for counts, so that population trend can be quantified and judged for significance; (b) successfully removing outside sources of variability through survey design or analytical adjustment; and (c) adequately replicating the survey either in time or space, so that the survey has power to distinguish a real trend from chance patterns. Ways to meet these requirements are hardly ever obvious, but gaining experience in monitoring the population and stating the scope and objectives of the survey are the best means of narrowing the options.

I will illustrate how controllable and uncontrollable factors can impair the usefulness of the data, what assumptions about the data are necessary for making inferences on population change, and how a good survey design can alleviate difficult situations. Most of the discussion pertains to the collection and analysis of data at a single site. When appropriate, however, I will extend the discussion to the problem of regional population inference based on samples at multiple sites. Most examples derive from personal experience with crocodilian data, but some I borrow from the field of avian monitoring, where practitioners have long struggled with the same issues.

A detailed treatment of all issues pertaining to design and analysis of crocodilian population surveys is beyond the limits of a single manuscript. My purpose is to organize, describe, and provide bibliographic reference to problems likely to be encountered in crocodilian monitoring. I hope to provide enough detail to allow the reader to pursue further topics independently and to communicate design and analysis concerns effectively with a statistician.

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<sup>1</sup>Contribution to 12 Working Meeting of the IUCN/SSC Crocodile Specialist Group, Pattaya, Thailand, May 1994.

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J. R. Sauer and A. R. Woodward kindly reviewed drafts of this manuscript and provided many improvements.

## SOURCES OF VARIABILITY IN THE COUNT

Even in a constant-size crocodilian population, counts of individuals or their sign are not censuses, and hence will generally vary in successive surveys of the population. A reasonable way to think of the counting process is as follows. Each individual or item in the survey area has some probability, including probabilities 0 and 1, of being detected. Probabilities may vary among individuals in the same survey and within individuals in different surveys. Whether an individual is counted or not is a random event determined by its detection probability. Thus, variability of the count is due partly to chance and partly to how severe detection probabilities vary between individuals or items within and among surveys. While the chance component is beyond any kind of survey control, good and poor population surveys are distinguished by how well the investigator exerts control over factors that cause detection probability to vary (Seber 1982:53).

Detection probability of individuals or their sign may vary according to factors both within and beyond the control of the investigator. Detectability effects due to season, time of day, location, meteorological conditions, habitat structure, water attributes, animal physiology and behavior, counting procedure, observer characteristics, and equipment used have been described elsewhere (Magnusson 1982, Bayliss 1987, Woodward and Moore 1990). The investigator can control some of these factors. Others are not controllable, yet the investigator can prescribe favorable conditions under which surveys can be run. However, simultaneous ideal conditions for all factors rarely prevail, so conditions among samples must vary to some degree even in the most rigorous surveys.

By counting crocodilian sign, one can avoid some sources of detection variability associated with counting individuals. For example, nests (the most commonly surveyed form of crocodilian sign) are fixed and generally uniform in appearance, so detection probabilities associated with animal behavior, animal size, time of day, and many other factors may not be of concern. However, such surveys introduce new sources of variability that may limit their usefulness (Seber 1982:54). Whether or not an adult female builds a nest is a random occurrence with unknown probability, and the probability may vary among females. Further, each nest in the survey area has some probability of not being unique to an individual, that is, false or "sibling" nests may be available for inclusion in the count. Nest "creation" and "sibling" probabilities operate in opposite ways with regard to the population count, but because the probabilities may vary among individuals, there is no assurance that the probabilities completely offset one another.

## ASSUMPTIONS FOR ESTIMATING POPULATION TREND

### Constancy of count bias

If we could count every member of the population at each survey period, we could measure trend exactly. Even lacking a complete count, we could obtain an exact measure of trend as long as the number of individuals or items counted each time was a strictly constant proportion of the population. In other words, the count bias, the difference between population size and the population index, must be a constant proportion of population size at each survey occasion. If bias varies from count to count, then a trend in counts measures something other than a trend in population size (Seber 1982:53).

Because detection is a random occurrence, a count of individuals or items is almost certainly a biased estimate of population size. Because detection probabilities vary among individuals or items at each sample, there generally is high risk that the assumption of constancy of count bias is not met.

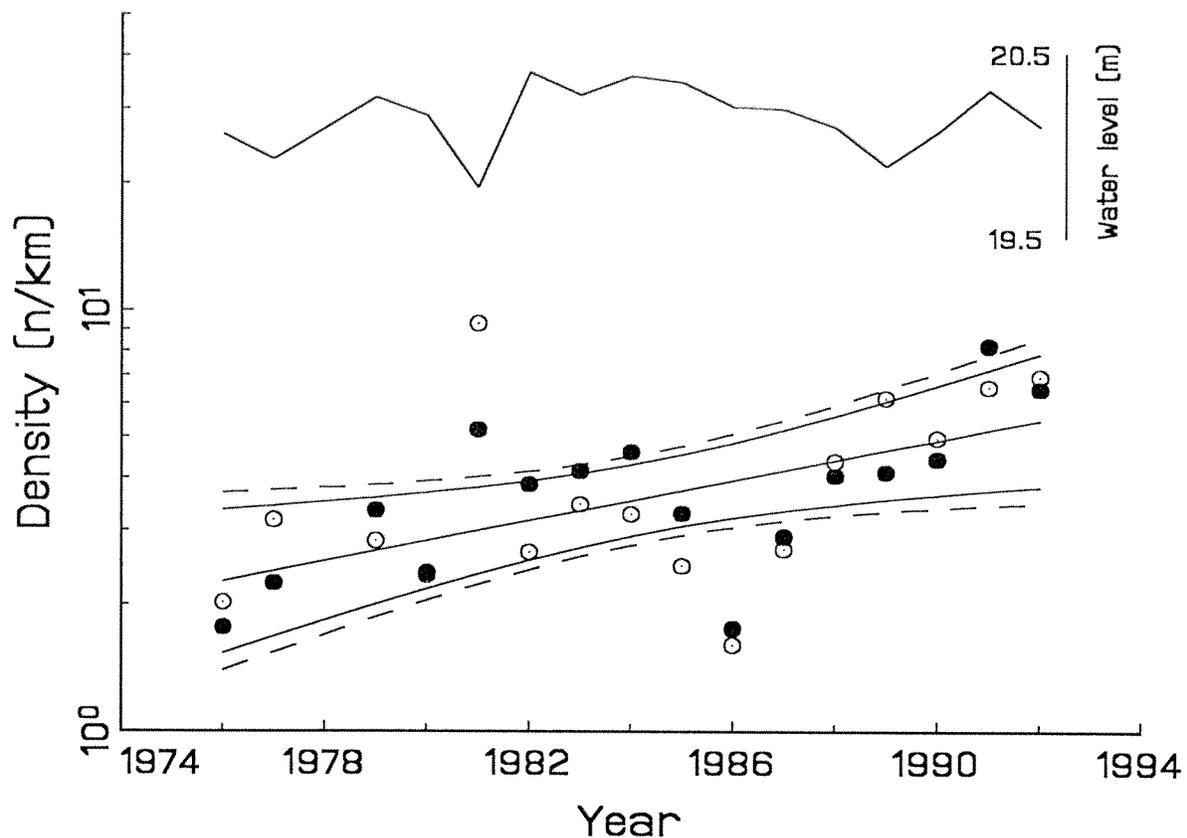
### Effects on count bias variability are known and controllable

The investigator should know all the effects that cause count bias to vary and should control these effects through survey design. Control can be achieved either through standardization of survey methods (Seber 1982:53) or through post-hoc analytic adjustment of the counts for measured values of the effects.

Neither part of this assumption is likely to be completely satisfied in practice. In any survey, we can never enumerate all factors that cause count bias to vary, but we hope to at least list those that we can confidently presume to have the greatest effect. However, of factors that we can list, we are unlikely to be able to design a survey or concoct an analysis that will entirely remove all of their effects.

For example, season, water temperature, and water level influence the population distribution of the American alligator (*Alligator mississippiensis*) on wetlands in Florida, and variability in population distribution was believed to greatly affect detection probability during night population surveys (Woodward and Moore 1990). The season and water temperature effects were controlled by conducting counts in a narrow annual time frame. Water level could neither be manipulated nor anticipated. However, a measurement of water level was obtained in each survey for later covariate adjustment. Adjustment for water level smoothed wild swings in the count and improved precision for the estimate of trend (Fig. 1). Surface coverage by hydrilla (*Hydrilla verticillata*) also likely influenced detection probability, but its influence was not as effectively removed.

In the North American Breeding Bird Survey (BBS), a spring-season roadside count of all breeding birds detected through sight or song (Robbins et al. 1986, Droege and Sauer 1990), changes in observers on a survey route affect the between-year detection probability of



**Figure 1.** Mean summer (June, July, August) water level (m) on Newnans Lake, Florida, USA, and observed (○) and adjusted (●) densities of alligators 31-122 cm on Newnans Lake. Direction and magnitude of adjustment was determined by relative water level value. Estimated trend line and 95% confidence band for trend line are displayed (solid lines). The wider confidence band (broken lines) approximately expresses the cost paid in increased variability by ignoring variability in water level.

individuals (Sauer et al. 1994). Over the 27-year history of the BBS, new observers have typically been more proficient at counting birds than their predecessors, perhaps due to increased popularity of bird watching and due to improved quality screening of new observers. Accordingly, the proportion of birds detected has systematically increased over time. On average, ignoring the observer effect misleadingly portrays population increases that are too rapid or population declines that are too slow (Sauer et al. 1994). Thus, observer changes on a route are noted, and effects of the changes are removed analytically (Geissler and Sauer 1990) (Fig. 2). Within-observer changes in counting proficiency (W. E. Kendall, U.S. Fish and Wildl. Serv., unpubl. data), changes in traffic volume or roadside land-use patterns, and annual differences in proportion of males that sing are examples of many largely unknown and uncontrolled factors that affect detection probability in the BBS (Bystrak 1981). Factors that affect detection probability of crocodilians are likely just as varied.

### Statistical model for counts

We assume that counts obey a statistical model that prescribes (a) the pattern in counts over time, (b) the relationship between counts and nuisance covariates, and (c) the probability distribution of model errors. Each element of this assumption is considered below.

*Pattern in counts over time.* We assume that some type of model describes the data pattern (Fig. 3). The most widely used are linear and exponential models of population growth (Harris 1986). Such models are easily estimated via standard linear regression methodology. However, because these models presume that the population grows or diminishes without limit, they may be unsuitable for rapidly changing populations. Models of asymptotic or size-limited population growth (e.g., logistic, von Bertalanffy, and Richards curves) are available, but these models are fit by nonlinear regression, a somewhat difficult procedure that is sensitive to absences of data (Seber and Wild 1989). Both linear and nonlinear growth models describe only monotonic growth, that is, they do not accommodate both positive and negative growth periods over the survey interval, and this limitation may be unrealistic for many populations. To model counts that rise and fall over time, one may augment linear models with curvature parameters to create polynomial, sinusoidal, and other "curvilinear" regression models, or one can use nonparametric smoothers (e.g., lowess, cubic spline, and kernel smooths) (Härdle 1990). Because these models may contain peaks and troughs, inference on population trend is conditional on specified periods of time within the survey interval. Finally, one can fit "jointed" or piecewise linear models (Draper and Smith 1981:252-257) to model interventions (abrupt population crashes or surges) that may result from management actions or extreme environmental conditions.

*Relationship between counts and nuisance covariates.* In situations where control over a detectability factor cannot be achieved sufficiently through survey standardization, it may be necessary to adjust the count by modelling the effect as a nuisance covariate (so called when the only purpose of the covariate is to isolate variability from a covariate of primary interest). The decision whether to use the covariate is rarely easy, and the consequences of an improper decision may be quite damaging to the analysis. If an important covariate is ignored in the

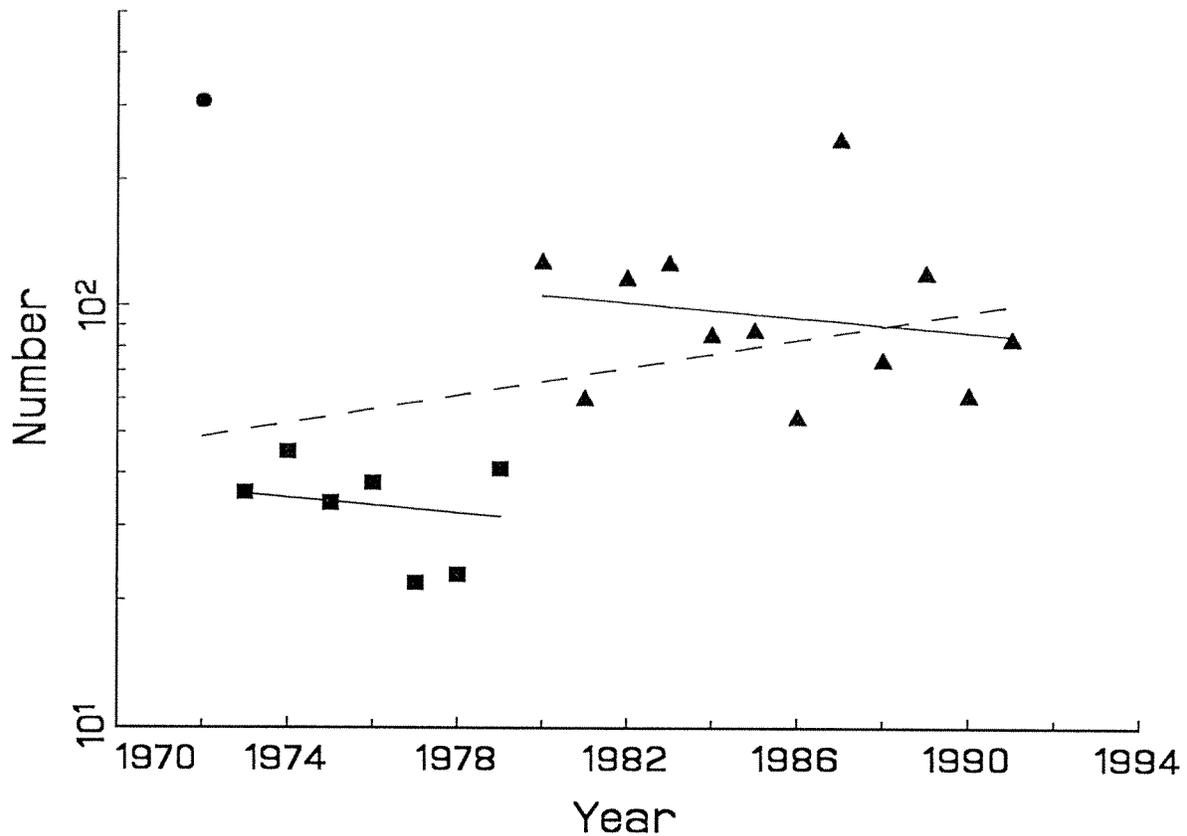
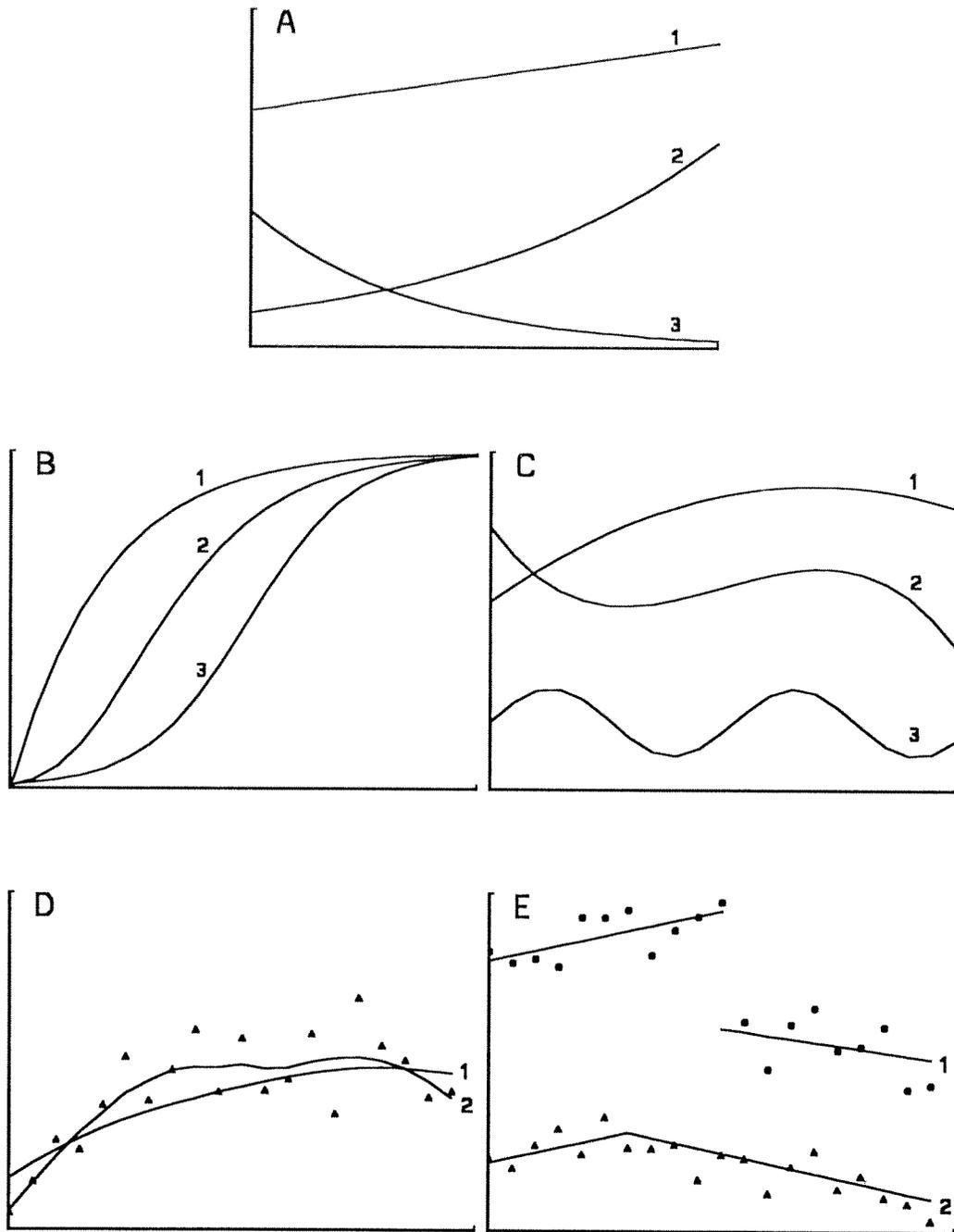


Figure 2. House sparrows (*Passer domesticus domesticus*) detected annually, 1966-1992, on Breeding Bird Survey route 14203, California, USA. Different observers are distinguished by unique symbols. Increasing trend (broken line) is suggested when observers differ in detection proficiency but the difference is ignored; within an observer series, a downward trend (solid lines) is obvious.



**Figure 3.** Examples of regression curves: (A) Linear (1) and exponential (2, 3) curves; (B) nonlinear Von Bertalanffy (1), Gompertz (2), and logistic (3) curves; (C) quadratic (1), cubic (2), and sinusoidal curves; (D) nonparametric lowess smooth with tension = 0.8 (1) and tension = 0.2 (2); and (E) discontinuous (1) and continuous (2) piecewise linear regressions.

analysis, the estimate of population trend can be biased (e.g., ignoring observer differences in counts of birds) or highly variable (e.g., ignoring water level in counts of alligators) (Rawlings 1988:243). An unimportant covariate that is included in the analysis "drains" information available for estimating trend, thus the trend estimate may be imprecise (Rawlings 1988:171).

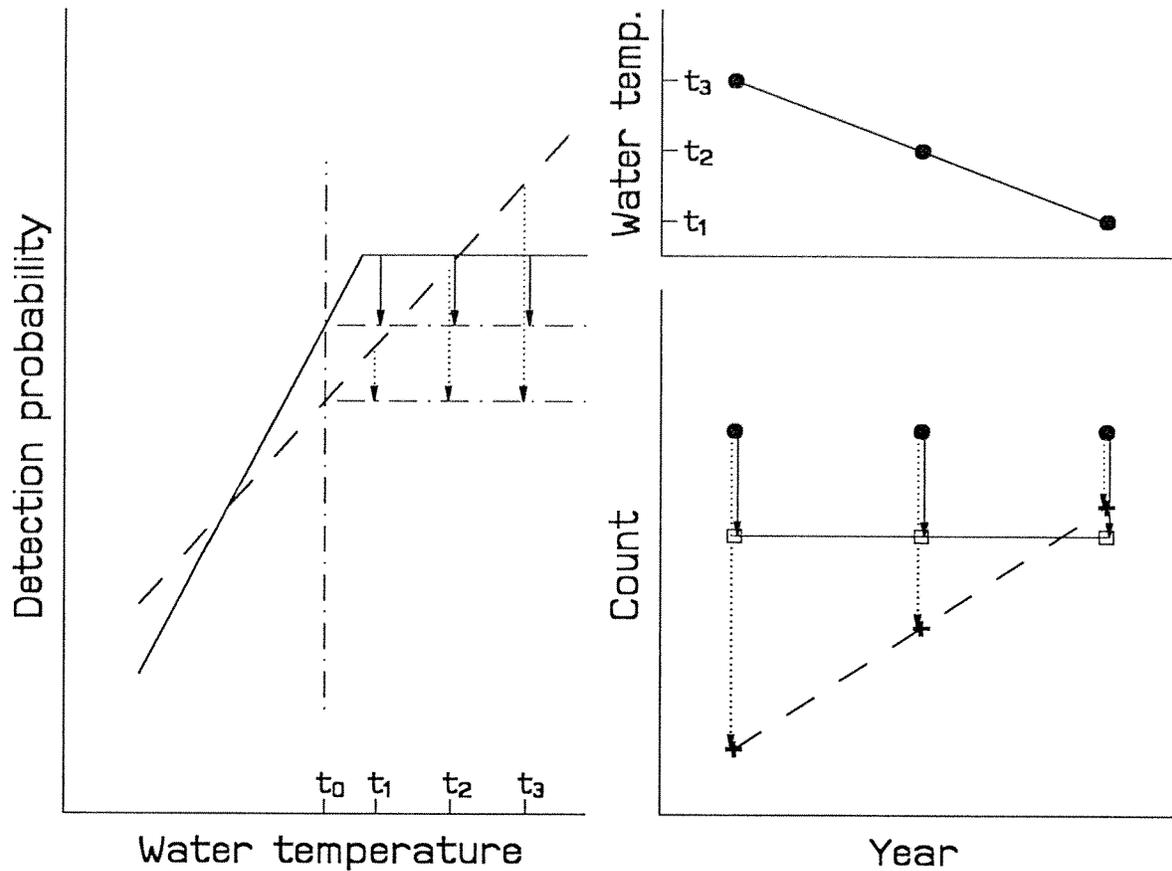
The manner in which the covariate enters the statistical model may not be trivial. Usually, one assumes that the covariate affects the response variable (count, or a transformed value of count) in a linear, additive fashion. That is, if all other covariates are held constant, we expect a change in the covariate to elicit a proportionate change in the response. However, many biological phenomena operate nonlinearly or interactively.

Water temperature may affect detection of many crocodilians nonlinearly. Suppose the chance that a crocodilian is on the water surface increases with water temperature, up to a limit; beyond this limit, no association exists. If this nonlinear relationship is modelled linearly, one runs the risk of providing too much negative adjustment to counts at high water temperatures and too little positive adjustment at low temperatures. The linear model may cast a series of stable counts as an upward population trend if the counts were observed during a span of declining yet relatively warm water temperatures. Contrarily, the correct model makes little or no adjustment to counts in this temperature range, and no population increase is portrayed (Fig. 4).

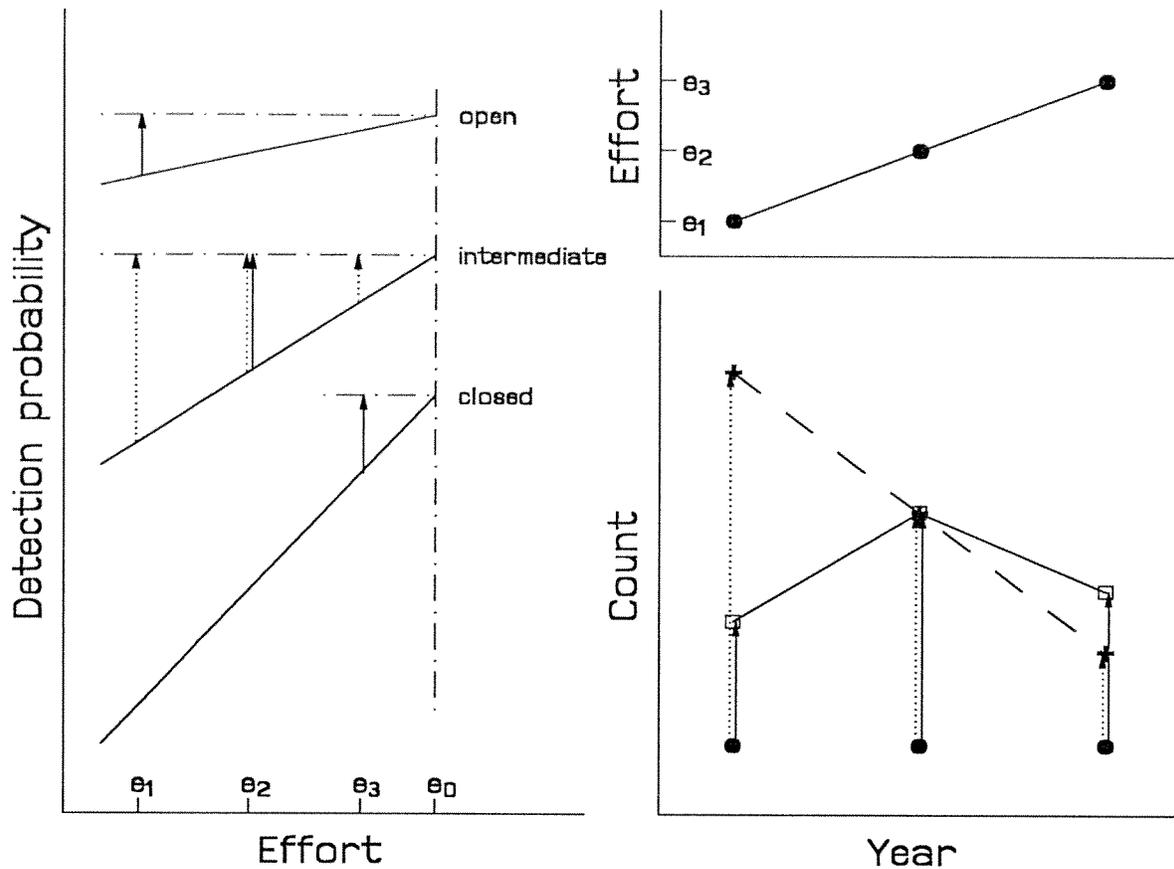
Interactivity occurs when the relationship between a response variable and a covariate depends on the level of another covariate. In a hypothetical but realistic example of interaction, counts of crocodilian nests on a survey area may be positively related to observer effort. However, this relationship is not as obvious in open canopy conditions as in closed canopy conditions because all nests under the open canopy are easy to count regardless of amount of survey effort expended. Suppose that over the course of the survey, more of the area suitable for nesting is obscured by an encroaching canopy. Time therefore interacts with effort because the relationship between count and effort changes over time. If the interaction is ignored in this illustration, counts obtained in earlier years of the survey are overadjusted for effort, and later counts are underadjusted. The resulting trend estimate may be imprecise or biased (Fig. 5).

Covariates may be employed in all forms of linear, nonlinear, and nonparametric models though estimation procedures can be much more difficult than for simple linear regression. However, just as with simple regression, the methods are usually far simpler than the task of determining which covariates to model and their functional relationships to counts.

One common way to incorporate a covariate is through proportionate scaling. That is, one divides counts by corresponding covariate values and analyzes values of the resulting index (individuals per covariate unit). For example, counts of individuals are often divided by some expression of counting effort to obtain an effort-adjusted abundance index. Such scaling implies that the relationship between count and the covariate is represented by a straight line passing through the origin (i.e., a zero-valued covariate implies a zero count) (Packard and Boardman 1988, Butcher and McCulloch 1990). Any other form of the relationship makes the use of



**Figure 4.** Effect of misspecification of the relationship between detection probability and a covariate. The true (solid line) and misspecified (broken line) hypothetical relationships between surfacing probability and water temperature are displayed (left panel). Water temperature decreases with time, but counts ( $\bullet$ ) are constant (right panel). Direction and magnitude of covariate adjustment for each temperature  $t_1$ ,  $t_2$ , and  $t_3$  to standard temperature  $t_0$  are indicated with arrows for both surfacing probability relationships (left panel). Adjustments applied to the counts result in an increasing trend ( $+$ , broken line) for the misspecified relationship and a stable trend ( $\square$ , solid line) for the true relationship.



**Figure 5.** Effect of ignoring interaction between time effect and a covariate. In the hypothetical example, nest detection probability increases with effort, but at a rate dependent on canopy cover (left panel). Further, because the canopy over the survey area closes over time, time and effort interact. Suppose that effort increases over time, but counts ( $\bullet$ ) remain stable (right panel). Adjustment of survey efforts  $e_1$ ,  $e_2$ , and  $e_3$  to the reference level  $e_0$  varies according to canopy cover (solid arrows, left panel), and the adjusted counts show a generally increasing pattern (right panel,  $\square$ , solid line). However, if the interaction between effort and canopy closure (or time) is ignored, covariate adjustment for effort occurs at an intermediate fixed level of canopy closure (broken arrows, left panel), and the adjusted counts appear to decline (right panel,  $+$ , broken line).

proportionate scaling suspect. It is easy to imagine a relationship in which individuals counted increases very rapidly with increasing effort but increases slowly beyond some threshold of effort. In this circumstance, neither proportionate scaling nor linear covariate adjustment adequately model this relationship, but the latter method is more likely to fit at least a part of the data (Rawlings 1988:243). Butcher and McCulloch (1990) used data from the Audubon Christmas Bird Count, an annual survey characterized by varying observer effort, to illustrate other failures of this model for several bird species. The authors noted that failing to model an important interaction between effort and location resulted in a spurious negative relationship between effort and count for several extreme cases.

Colinearity, the existence of high correlation among 2 or more covariates, presents difficulties in linear and nonlinear regression situations. Highly correlated covariates introduce instabilities in the least-squares estimation procedure, and as a result, mean and variance estimates for effects involved in the correlation are unreliable (Rawlings 1988:244). With regard to crocodilian monitoring, colinearity among the time effect and any other single covariate or combination of covariates can prove treacherous for making inferences on trend. For example, if survey transect length is used as a covariate, trend estimates may be unreliable if transect length changed consistently over time. Remedies for colinearity include dropping offending covariates from the analysis or using complicated methods (ridge regression, principal components regression) that obtain estimate stability by permitting a small degree of estimate bias (Rawlings 1988:245).

With these problems and uncertainties, the use of covariates in population monitoring is almost never a straightforward issue. Even if a covariate is known to offer increased accuracy and precision for the trend estimate, its advantage may still be offset by two disadvantages. First, the benefit obtained may not be worth the added complexity of the model and the estimation procedure. Second, establishing only an associative relationship between the covariate and count will always leave the covariate's applicability to future data in doubt; alternatively, an experimentally-confirmed causative relationship would clarify the role of the covariate now and in the future (Rawlings 1988:170-171).

*Probability distribution of model errors.* Inference in most regression problems requires that model errors (disagreements between actual counts and counts predicted by the model) are independent and normally distributed with constant variance; otherwise, effect variances may be poorly estimated and tests of significance may be invalid. One would not expect this requirement to be often met for typical crocodilian monitoring data. In most situations, error variance is not constant but instead increases with the mean count (Eberhardt 1978). That is, counts become more variable as the average count increases. Further, counts centered to their means often exhibit a right-tailed skew. Accordingly, the lognormal, poisson, and negative binomial distributions are better suited to count data than is the normal distribution (Eberhardt 1978).

Fitting the lognormal is accomplished by log-transforming the counts and applying regression methods to the transformed values, now assumed to be normally distributed. This

popular approach carries some caveats. First, estimates of trend or mean count back-transformed to their original scale by simple exponentiation suffer a bias that increases with the variance of the estimate. A simple bias-correcting adjustment may be made to the exponentiated value (Bradu and Mundlak 1970). Second, because a zero count value cannot be log-transformed, analysts often add a small constant to all counts prior to transformation. This practice biases the trend estimate, and the magnitude of the bias depends on the size of the constant, and most importantly, on the prevalence of zero or small ( $< 3$ ) count values over the survey period (Geissler and Link 1988, Collins 1990). Maximum likelihood estimation is a means of fitting the lognormal distribution directly without resorting to data transformation, but the method is iterative, requires familiarity with nonlinear optimization, and may be fraught with other difficulties (McCullagh and Nelder 1983, Seber and Wild 1989). For fitting models to data following poisson and negative binomial distributions, maximum likelihood estimation must be used (McCullagh and Nelder 1983).

Data collected in any time series potentially suffer serial correlation, a condition that violates the assumption of model error independence. Count data collected closely together in time often have positive serial correlation, meaning that the prediction error of the model at time  $t+1$  is generally of similar direction and magnitude as the prediction error at time  $t$  (Draper and Smith 1981). In such situations, the variance estimate for trend is biased low and results in rejection of the null hypothesis of no trend more often than expected (type I error greater than desired) (Rawlings 1988:241). In Florida, counts 1-7 days apart were serially correlated unless water level changed substantially ( $\geq 2$  standard deviations) during the time period (Woodward and Moore 1990). At stable water levels, no evidence of serial correlation occurred in counts taken  $\geq 40$  days apart, but unfortunately, counts separated by such a time span could not be considered replicates within a single season. Because serial correlation occurred in most counts taken more than once per year, replicated counts in Florida were often unusable for variance estimation by standard procedures. Methods exist for detecting and modelling serial correlation (Draper and Smith 1981, Rawlings 1988).

### Site-specific patterns of population change in regional surveys

Multiple sites distributed within a region may be sampled for inferences on the regional population trend. We assume that sites are located at random within suitable habitat of the region and that site-specific patterns of population change can be accurately depicted with a small, consistent set of summary statistics.

*Sites are located randomly within suitable habitat of region.* Unbiased estimation of a regional population pattern depends on a random sample of sites. Rarely is strict randomization used in survey design. Rather, logistics of the survey, accessibility of sites, and prejudices and politics are too often the criteria by which sites are selected. At best, one may be able to randomly choose from a set of sites deemed in some way to be "surveyable" and representative of the region. However, this approach risks systematic exclusion of sites harboring important segments of the regional population, e.g., inaccessible forested wetlands that provide nesting habitat to females. At worst, one chooses the sites haphazardly with emphasis on human use

status, convenience, and access. The importance of stating the limitations of the sampling frame in any presentation of the data cannot be overemphasized.

Random sampling of sites in 2 or more strata within the region is desirable if within-stratum site variability is appreciably less than regional site variability (Eberhardt 1978, Seber 1982:20, 451). For example, one may obtain variances for mean trend at sites both within riverine and palustrine systems, then pool the 2 values. If the pooled variance for mean trend is smaller than the variance obtained by ignoring ecosystem status of sites, then greater efficiency can be obtained by randomly sampling sites within riverine and palustrine systems than by sampling without regard to ecosystem.

Many estimation procedures exist for data taken repeatedly on a sample of individuals (Crowder and Hand 1990). The split-plot ANOVA design, or its variations (Rowell and Walters 1976), can be used when all sites are sampled in all time periods, or very nearly so. If sites are often skipped or if covariates are to be modelled, other procedures are required. One such procedure, route-regression (Geissler and Sauer 1990), was developed and is now routinely used for estimation in the multi-site BBS. In short, estimates of site-specific exponential population trend are weighted by relative abundance of the species at the site and by a quantity inversely related to the estimated trend variance for the site. Weighted site estimates within a physiographic stratum are then "bootstrapped" (intensively resampled by computer) to obtain stratum estimates of mean trend and variance. Stratum estimates, weighted by stratum area, are finally averaged into summaries for larger regions (e.g., states and provinces).

*Pattern representable in summary statistics.* Whereas single-site surveys may permit elaborate modelling of counts, multi-site surveys must emphasize models that are sufficiently flexible to capture gross population patterns yet provide enough reduction of the data for easy summarization over all sites. If straight lines are used to model count data for sites that may contain either straight or curved patterns, then the regional summary will take the form of a misleadingly straight and relatively precise trend. Better approaches are fitting models that include a parameter for curvature or determining whether count patterns correspond to an identifiable stratification of sites and estimating trends separately by stratum.

## CONSIDERATIONS IN DESIGNING CROCODILIAN SURVEYS

### Identifying and controlling survey conditions

The conditions that cause count bias to vary among years must be identified. A more difficult task is to exert control over these conditions so that relationships observed in count patterns can be inferred to the population.

One controls survey conditions either through standardization of sampling techniques or through post hoc analytic adjustment for covariates. Given that the complexity and the potential for problems multiply with the addition of covariates, survey standardization should almost

always be preferred to covariate adjustment. A standardized survey should prescribe how sampling is done with respect to time, location, meteorological conditions, habitat conditions, observer characteristics, counting procedure, and equipment used. Generally, survey costs increase and magnitude of counts decrease with standardization, so the temptation to forego standardization in favor of sampling under varying conditions is strong. However, increasing reliance on covariate adjustment decreases the credibility of survey inferences. When covariates must be measured, they should correspond to conditions proximate to the time and location of the survey, and their values should be measured accurately.

The same recommendations for standardization apply to individual sites within a multi-site regional survey. However, careful consideration to selecting sites and determining the need for stratification are additional concerns in the design of regional surveys.

### **Experimental studies**

A frequent objective in crocodilian monitoring is concluding if a management action causes a population response. The survey design determines whether the question can be successfully addressed. Carrying out the action at a single site constitutes only one replication of an experiment. A population change occurring at the time of the action cannot be attributed to an effect of the action or to a coincident unrelated effect. Therefore, a single-site survey is not a sufficient design for answering this question (Hurlbert 1984). Replicated experimental and control sites, perhaps paired with each other, should be established.

### **Modeling and inference**

In single-site surveys, emphasis is placed on model identification and detection of rapid, short-term population change. The gamut of standard regression diagnostic tools (Draper and Smith 1981, Rawlings 1988) can be used to identify models that successfully fit the data pattern and to diagnose problems that can interfere with assessing population change. Replicated independent counts taken at each time period provide the best tool for identifying a model, because a candidate model can be tested using a variance term computed independently of the model (Rawlings 1988:122-126). Such counts need to be sufficiently close in time to assure that the same population is being sampled, but counts too close in time only provide information on measurement error, not sampling error.

In contrast, emphasis in regional surveys is on coarse estimation of population change at many sites. Under this strategy, estimates from fairly simple models are summarized over several sites, and the use of many sites reduces the influence of poor estimation at a minority of sites. Detection of a regional population trend depends in part on the success of defining appropriate strata for pooling sites with similar trend characteristics.

Index or covariate values which are missing either in isolated cases or in entire series usually increase the need for elaborate estimation procedures and decrease the quality of

inferences that can be made. Consequently, every attempt should be made to acquire a complete set of data, balanced with respect to site and time period.

### Power

The ability to detect trends of given magnitude with reasonable confidence is a primary need for biologists who monitor natural systems (Toft and Shea 1983). Unfortunately, methods of estimating power for a design are not in the statistical training of most biologists.

For data collected under a given design, statistical power is the likelihood of rejecting the null hypothesis when an effect is present. For trend estimation, power is the chance of rejecting the null hypothesis of zero trend when in fact a real trend does exist. A smaller threshold value of trend is detectable in a design with high power than one with low power. Therefore, one should be able to decide the smallest trend that is consistent with population "stability" and determine what design configuration yields a great likelihood of detecting a trend at least as large, given that the trend really does exist. Then, declaring "no statistical significance" for a test of trend carries more relevance to the notion of population stability than would be possible in a low power test. Historically though, no assessment of power accompanies the data analysis, so failure to detect a significant trend is uninformative. When power is not reported, there is no basis to conclude that a nonsignificant trend estimate suggests population stability.

In the usual application of power analysis for population monitoring, one wants to determine those designs most likely to reveal a significant population trend if the trend exists at a specified size or larger. Number and frequency of surveys, use of covariates, and all other aspects of the design influence power and the likelihood of detecting population trend. Therefore, for a list of designs that are feasible given the available resources, the biologist can use power analysis to find the design yielding the highest power for the monitoring objective.

Power analysis for single-site surveys is described in the literature (Gerrodette 1987, Link and Hatfield 1990, Gerrodette 1991), and software is available for performing the calculations (Gerrodette 1994). The use of covariates can quickly complicate matters in power analysis, but power may still be estimated (C. T. Moore, U.S. Fish and Wildl. Serv., unpubl. rep.). Power analysis for regional surveys is not well-documented, but some tools are available (J. R. Sauer, Nat. Biol. Surv., unpubl. rep.; J. P. Gibbs, Yale Univ., unpubl. software). For either type of survey, data from a pilot study should be available to provide an estimate of variance. Such data may be collected in a single year if replicate, independent observations can be obtained. For regional surveys, pilot data must also be available at a sample of sites. In single-site surveys of fixed time span, more power for detecting trends is available through within-year replication and increased standardization of the design. For regional surveys, power is increased primarily through site replication and proper selection of strata.

### Auxiliary information

Because the assumption of constant count bias is critical for inferring population trend from counts, concurrent collection of auxiliary data that allow direct estimation of the count bias may be a worthwhile endeavor. Many variations of line transect or mark-recapture techniques (Seber 1982, Nichols 1987) permit the direct estimation of population size each year. Population size estimates can be used to investigate annual variability in the count bias, to corroborate the survey estimate of trend, and to search for possible failure of trend estimation assumptions.

### SUMMARY

The discussion presented here only scratches the surface of the vast collection of issues relating to design and inference in crocodilian monitoring studies. Because it is so difficult to satisfy key assumptions with any certainty and to implement appropriate analysis methods, trend estimation based on counts is rarely as simple as it appears. Covariates may present more problems than they offer to resolve. The realism of modelling curved trends has to be weighed against the relative simplicity of straight-line models. One can easily determine the monetary cost of random site selection and survey standardization, but a much more difficult and usually overlooked determination is the cost in scientific credibility of opting for the alternatives.

Although the issues are difficult, steps can be taken prior to the survey that will help narrow the design and analysis options. First, a clear statement of survey scope and objectives must be made. Second, gaining as much knowledge as is practical on the population, the habitat, and likely limitations of the survey method is fundamental to assuring the success of the survey. Collecting data from small-scale pilot surveys is an invaluable component of this step. With such knowledge, the biologist and the statistician should be able to determine if the survey objectives can be achieved through the resources available, and if so, design a robust, efficient survey program and analysis plan that will meet the objectives.

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