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The effect of data analysis strategies in density estimation of mountain ungulates using distance sampling

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Abstract

Distance sampling is being extensively used to estimate the abundance of animal populations. Nevertheless, the great variety of ways in which data can be analyzed may limit comparisons due to the lack of standardization of such protocols. In this study, the influence of analytical procedures for distance sampling data on density estimates and their precision was assessed. We have used data from 21 surveys of mountain ungulates in the Iberian Peninsula, France and the Italian Alps. Data from such surveys were analyzed with the program Distance 6.0. Our analyses show that estimated density can be higher for higher levels of data truncation. We also confirm that the estimates tend to be more precise when data are analyzed without binning and without truncating. We found no evidence of size biased sampling as group size and distances were uncorrelated in most of our surveys. Despite distance sampling being a fairly robust methodology, it can be sensitive to some data analysis strategies.

Keywords: *Distance sampling, mountain ungulates, data analysis, population monitoring, precision*

Introduction

Estimating the abundance of wild animal populations is a basic management action. Unbiased and precise population estimates and related demographic parameters are often needed to monitor and manage wild populations. Caprids, like other big game species, are not an exception (Shackleton 1997). Currently, different methods are available for estimating animal abundance. They could be direct (based on the observation of individuals) or indirect (based on the record of calls, dung, tracks, etc.) methods; some of them are simple and others complex, e.g. taking into account the heterogeneity in detection probability, or combining simple and independent methods (Krebs 2001; Borchers et al. 2002; Williams et al. 2002). Among this variety of methodologies, distance sampling (DS hereafter) has

become a popular and versatile technique. It can be applied as a direct or an indirect method, and does not necessarily involve the capture and handling of animals (Williams et al. 2002). If the methodological assumptions are met, it allows obtaining robust and precise estimates of population density with a good balance between cost and results (Southwell & Weaver 1993; Buckland et al. 2001, 2004; Fewster et al. 2005). During the last two decades, DS has been used for estimating numbers of ungulate populations in a wide variety of habitats (Trenkel et al. 1997; Marques et al. 2001; Focardi et al. 2002; Ward et al. 2004; Ellis & Bernard 2005; Olson et al. 2005; Franzetti & Focardi 2006; Ogutu et al. 2006; Acevedo et al. 2008), and its efficiency is often compared to that of other methods such as total counts, block counts, capture–recapture, and

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derived population estimates (Seddon et al. 2003; Herrero et al. 2011). This methodology has also been used for monitoring mountain ungulate populations (Escos & Alados 1988; Palomares & Ruiz-Martínez 1993; Pérez et al. 1994; Liu et al. 2008; Carranza 2010; Herrero et al. 2011; Wingard et al. 2011; Schmidt et al. 2012).

DS methodology uses a count of observations (i.e. animals or groups, dung, nests, songs, among others) and their distance from the observer (radial distance if observations are made from a point, or perpendicular, when using line transects). Importantly, DS relaxes the critical assumption that detection probability in a sampling unit is equal to 1. Statistical inference based on DS depends on the validity of different assumptions (Burnham et al. 1980; Barry & Welsh 2001; Buckland et al. 2001; Borchers et al. 2002): (1) objects (animals) located precisely on the point or on the transect line are always detected. This means that $g(0) = 1$, where $g(x)$ is a decreasing function of distance and denotes the probability to detect an object, given that it is at distance x; (2) animals are detected at their initial location, prior to any movement or, in other words, the animals are detected by the observer before the observer has any influence on their position; (3) distances and angles, if applicable, are accurately measured, or objects are included in the appropriate distance bin (or distance range) if grouped data are used; and (4) observations are independent events.

Otto and Pollock (1990) emphasized the problem of size bias: bigger groups should have a higher probability of being detected than the smaller ones, especially at larger distances, which could contribute to overestimation of the mean cluster size of the population and, by consequence, the real abundance as well. Buckland et al. (2001, sections 3.5, 4.8) provide several recommendations to account for size-biased issues in DS. On the other hand, Southwell and Weaver (1993) remarked on the great number of arbitrary decisions we have to make when working with DS (e.g. which estimator to use, whether data must be binned in distance categories or not, truncated or not, or if we have to use observations as individuals or groups). Nevertheless, the way in which data are analyzed in each survey is rarely described with this level of detail in the published literature. A few exceptions might be found for other taxonomic groups; see for instance Stanbury and Gregory (2009) on breeding bird abundance.

In this work, we compare the most common strategies described in the specialized literature about truncating and binning data (Buckland et al. 2001). Our main objective is to assess the effects of different analytical procedures on the density estimates and their precision for several populations of Iberian ibex (*Capra pyrenaica*), Alpine ibex (*Capra ibex*), Pyrenean chamois (*Rupicapra pyrenaica*) and Alpine chamois (*Rupicapra rupicapra*) in order to improve data analysis protocols, and to make comparable estimations carried out in different years and locations, as indicated in Morellet et al. (2011).

Materials and methods

We used data from 21 field surveys carried out between 1995 and 2012 in the Parc National des Pyrénées (PNP), France, for Pyrenean chamois; Parque Nacional de Peneda-Gerês – Baixa Limia and Serra do Xurés Natural Park (PNPG), two adjacent and contiguous areas in northern Portugal and northwestern Spain, respectively, Sierra de Loja (SL), Sierra Sur de Antequera (SSA), Sierra de Tejeda y Almijara (STA), Sierra Mágina (SM) and Sierra Nevada Natural Space (SN), southern Spain, for Iberian ibex, and Parco Naturale Val Troncea (PNVT), western Italian Alps, for Alpine chamois and Alpine ibex (Figure 1, Table I). These surveys accounted for a total of 1919.7 km of walked transect. The surveys were designed to sample at least 10% of the study surface area and transects were placed using a stratified sampling design (regarding orientation, slope, height and type of vegetation). For details in survey design, see Palomares and Ruiz-Martínez (1993) and Pérez et al. (1994). In Table I, we indicate the date (month and year) in which each survey was performed. Transects were sampled from 07:00 to 10:00 and

Figure 1. Distribution of the study sites. 1: Sierra Nevada Natural Space for *Capra pyrenaica hispanica*; 2: Sierra Mágina for *C. p. hispanica*; 3: Sierras de Tejeda y Almijara for *C. p. hispanica*; 4: Sierra de Loja for *C. p. hispanica*; 5: Sierra Sur de Antequera for *C. p. hispanica*; 6: Parque Nacional de Peneda-Gerês – Baixa Limia and Serra do Xurés Natural Park for *Capra pyrenaica victoriae*; 7: Parc National des Pyrénées for *Rupicapra pyrenaica*; 8: Parco Naturale Val Troncea for *Rupicapra rupicapra* and *Capra ibex*.

Table I. Data describing surveys used in this study: location and date (month and year) of each survey, species studied (Cp: *Capra pyrenaica*, Rp: *Rupicapra pyrenaica*, Rr: *Rupicapra rupicapra*, Ci: *Capra ibex*), K: number of transects, L: total length (in km), Md: maximum distance of observation, Td (5%): distance used for truncation of 5% of observations, Td (10%): distance used for truncation of 10% of observations (all above-mentioned distances in m). Superscript numbers 1: 20%, 2: 33.3%, 3: 15%, 4: 20% and 5: 30% of observations truncated. SN: Sierra Nevada Natural Space; STA: Sierra de Tejeda y Almijara; SL: Sierra de Loja; SSA: Sierra Sur de Antequera; SM: Sierra Mágina; PNPG: Parque Nacional de Peneda-Gerês–Baixa Limia and Serra do Xurés Natural Park.

Location	Date	Species	K	L	Md	Td (5%)	Td (10%)
SN	Jun 1995	Cp	19	269.5	800	550	300
SN	Nov 2001	Cp	47	283.7	500	500	475
SN	Jul 2002	Cp	40	319.5	800	500	400
SN	Jul 2003	Cp	40	374.7	800	500	420
SN	Oct 2009	Cp	26	155.5	420	237	197
STA	Oct 2009	Cp	20	139.5	623	471	375
SL	Jun 2005	Cp	8	33.4	850	550	440
SL	Jun 2007	Cp	8	33.9	480	450	400 [1]
SSA	Oct 2009	Cp	5	32.5	313	274	221
SM	Nov 2012	Cp	13	83.1	750	600	600
PNPG	Jul 2005	Cp	9	39.3	554	554	300
PNPG	Feb 2006	Cp	9	39.3	534	534	450 [2]
PNPG	Mar 2007	Cp	9	41.0	541	479	447 [3]
PNP	Oct 2002	Rp	1	2.8	150	147 [4]	140 [5]
PNP	Oct 2005	Rp	1	2.6	600	530	500
PNP	Oct 2007	Rp	1	4.5	265	252	247
VT	Oct 2006	Rr	8	29.2	593	527	390
VT	Oct 2007	Rr	8	18.4	600	370	300
VT	Oct 2008	Rr	5	17.3	633	490	413
VT	Jul 2009	Rr	15	51.4	811	574	455
VT	Jul 2009	Ci	10	31.7	418	391	300

from 17:00 to 21:00, coinciding with daily activity periods of animals.

Each transect was walked by two or three observers who recorded the observed number of groups, group sizes and their perpendicular distances to the transect line. We used a compass, laser telemeter and global positioning system (GPS) to navigate, measure distances and note locations of species occurrences.

We used Distance 6.0 release 2 (Thomas et al. 2010) to obtain density estimates and corresponding coefficient of variation. To model the detection functions we used half-normal with hermite polynomial expansion, uniform with cosine expansion and hazard-rate with cosine expansion (Buckland et al. 2001; Franzetti & Focardi 2006). A maximum of two adjustment terms was used in order to balance between bias and precision, and the lowest AIC (Akaike's Information Criterion) value was used for model selection. Encounter rate and mean group size were obtained for each survey. Variance of encounter rate was estimated empirically. The coefficient of variation (CV) of density estimates and their associated 95% confidence intervals were also obtained.

In the absence of group size bias, the following estimator of density can be used to account for the effect of group size:

$$\hat{D} = \frac{n.\hat{f}(0).\bar{s}}{2L} \tag{1}$$

where n is the number of observed groups, $\hat{f}(0)$ is an estimator of the probability density function of perpendicular distances evaluated at distance 0, $\bar{s}$ is the observed mean group size and L is the transect length. The estimation of f(0) plays a very important role. For a reliable estimate of D, it is recommended that the histogram of observed perpendicular distances presents a shoulder near the line or point (i.e. near distance zero). Also, it is recommended that detections at great distances from the transect, that are considered outliers, should be discarded to facilitate modelling of the data since they provide little information to estimate f(0) (Buckland et al. 2001).

The estimated variance is given by:

$$\hat{var}(\hat{D}) = \hat{D}^2 \left\{ [\text{cv}(\text{n})]^2 + \left[cv\left\{ \hat{f}(0) \right\} \right]^2 + [cv(\bar{s})]^2 \right\} \tag{2}$$

where cv denotes the coefficient of variation of each variance component (Buckland et al. 2001).

We used 21 different strategies of data analysis: three right truncation levels (0%, 5% and 10% of observations) and seven data binning categories

(using unbinned distances – no binning – and binning distance data into five to 10 equal intervals). After careful inspection of the distance histograms for the surveys, we did not find evidence of heaping, which might justify the analysis of binned data in distance categories. We performed analyses with the conventional distance sampling (CDS) engine. For each strategy, we calculated density estimates with the corresponding coefficient of variation. We used Friedman's χ^2 tests (Siegel & Castellan 1988) to compare the ranks of density estimates and their associated CVs, and to find possible significant differences among strategies, since the data from the same surveys are repeatedly used for each analysis. We used the post-hoc Dunn test to check for pairwise comparisons when significant differences were detected among strategies. Statistical analyses were performed using SPSS 18.0 (SPSS Inc. 2009). For each survey, we also calculated the correlation between group size and perpendicular distance.

Results

In Table II, we present a summary statistics of each survey: number of observations (individuals and groups), encounter rate for groups, mean group size and its associated coefficient of variation, and

the observed correlation coefficients between group size and distance of observation, with the corresponding p-values. Evidence of correlation between group size and distance of observations was almost always absent. Only in the survey of PNP carried out in 2007 for *R. pyrenaica* were group size and perpendicular distance positively correlated: $r = 0.52$; df = 24, $p = 0.003$.

Overall, the 21 surveys detected 5852 individuals and 1274 groups. Nevertheless, only six of the surveys presented adequate sample sizes (> 60 groups), and nine surveys presented questionable sample sizes for analyses (from 29 to 58 groups). The remaining six had inadequate sample sizes (from three to 26 groups) to perform DS estimation.

Thus, only surveys with adequate and questionable sample sizes were used for comparing density estimates and associated CVs under the different analysis strategies to perform DS estimation. These surveys also have an adequate number of replicates ($K \geq 15$), as can be observed in Table I.

In Table III, we present a summary of the statistical tests resulting from the comparison of strategies for surveys with adequate sample sizes. We notice that for unbinned data, density estimates differed significantly with truncation ($p = 0.015$). The post-hoc Dunn test shows that densities obtained after

Table II. Data describing observations from each survey: location and year (in brackets) of each survey, n_i: number of individuals detected, n_c: number of clusters detected, ER_c: encounter rate (clusters), mcs: mean cluster size obtained for each sample and its respective coefficient of variation (CV, %), cor: correlation between cluster size and distance of observation (x_i), p: p-value associated to the correlation coefficient, a: Alpine chamois survey, b: Alpine ibex survey. SN: Sierra Nevada Natural Space; STA: Sierra de Tejeda y Almijara; SL: Sierra de Loja; SSA: Sierra Sur de Antequera; SM: Sierra Mágina; PNPG: Parque Nacional de Peneda-Gerês–Baixa Limia and Serra do Xurés Natural Park.

Survey	n_i	n_c	ER_c	mcs	CV (%)	cor	p
SN (1995)	521	138	0.51	3.78	11.47	−0.06	0.750
SN (2001)	966	119	0.42	8.12	8.88	0.14	0.073
SN (2002)	830	241	0.75	3.44	5.89	−0.04	0.726
SN (2003)	540	169	0.43	3.38	16.04	0.01	0.460
SN (2009)	489	79	0.51	6.19	10.16	−0.02	0.573
STA (2009)	196	58	0.42	3.38	13.06	−0.08	0.719
SL (2005)	298	46	1.38	6.46	16.89	0.12	0.206
SL (2007)	264	30	0.88	8.80	21.83	−0.19	0.848
SSA (2009)	87	29	0.89	3.00	13.54	−0.24	0.900
SM (2012)	240	43	0.52	5.63	11.36	−0.11	0.753
PNPG (2005)	74	6	0.20	12.33	49.66	−0.17	0.625
PNPG (2006)	109	3	0.08	36.33	36.73	−0.87	0.838
PNPG (2007)	137	13	0.32	10.46	31.08	−0.10	0.604
PNP (2002)	21	10	3.57	2.10	36.51	−0.39	0.868
PNP (2005)	63	21	8.08	3.00	21.82	−0.12	0.695
PNP (2007)	72	26	5.82	2.77	13.33	0.52	0.003
VT (2006)	189	43	1.47	4.40	15.07	0.09	0.280
VT (2007)	171	42	2.28	4.07	22.96	−0.11	0.760
VT (2008)	97	33	1.91	2.94	32.84	−1.47	0.920
VT (2009 a)	409	96	1.87	4.26	12.40	0.001	0.500
VT (2009 b)	79	29	0.91	2.72	15.77	0.19	0.840

Table III. Results from Friedman's test to compare densities and their coefficients of variation (CV) by strategies – Friedman's χ^2: observed value for the test statistics; df: degrees of freedom; p: p-value; $\star$: significant at 0.05.

Strategies	Density Friedman's χ^2	df
No binning: without truncating; truncating 5%; truncating 10%	8.435	2
No binning: without truncating; binning 5–10 classes	16.316	6
No binning: without truncating; binning in 5–10 classes and truncating 5%	4.639	6
No binning: without truncating; binning in 5–10 classes and truncating 10%	3.521	6
No binning: truncating 5%; binning in 5–10 classes and truncating 5%	5.856	6
No binning: truncating 10%; binning in 5–10 classes and truncating 10%	1.153	6
Binning in 5–10 classes: truncating 5%; binning in 5–10 classes: truncating 10%	8.845	11

truncating 10% of data were significantly higher than those obtained when using all the information from the surveys (i.e. 0% right truncation) ($p = 0.018$). Similarly, binning untruncated data into distance intervals produced significantly different density estimates ($p = 0.012$). Nevertheless, truncation did not affect the precision of density estimates, i.e. associated CVs ($p = 0.200$), nor did binning untruncated data into distance intervals ($p = 0.069$). Also, we did not find significant differences when comparing densities or CVs using different binning strategies if data were truncated at 5% or 10%.

Density estimates did not differ when comparing untruncated–unbinned data and other data binning strategies for truncation at 5% or at 10% ($p = 0.591$ and $p = 0.741$, respectively). Associated CVs for density estimates did not significantly increase when data were binned and truncated at 5% ($p = 0.079$) nor when truncated at 10% ($p = 0.331$).

No significant differences in either estimated densities or their associated CVs were observed when
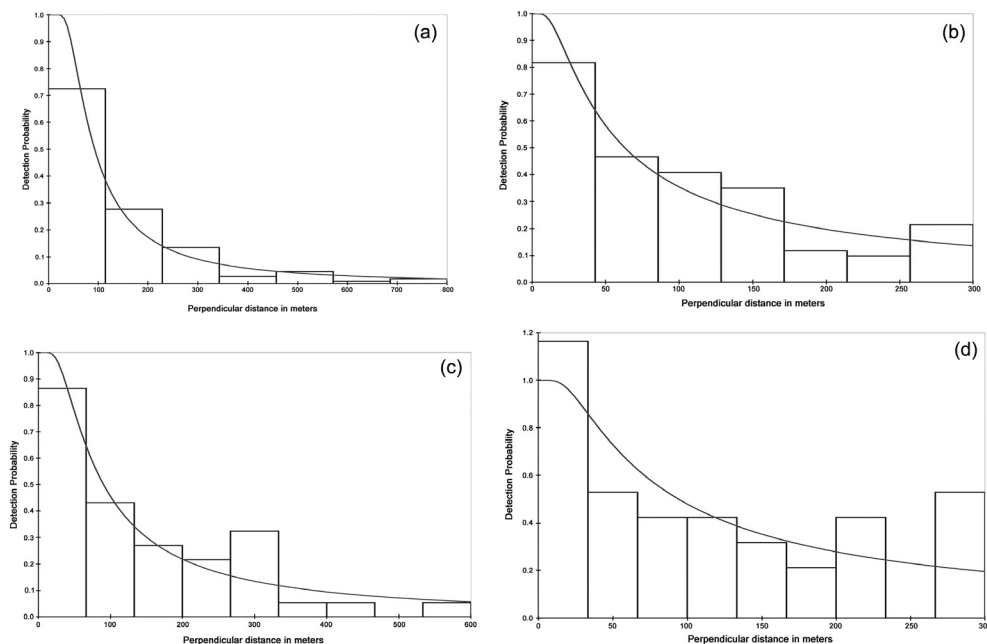

Figure 2. The effect of truncation of 10% of observations on the adjustment of the detection functions to distance data on the density estimate and its associated CV illustrated with surveys from two different locations; df = degrees of freedom; P = p value for GOF (goodness of fit) test; K = number of parameters of the detection function; D = estimated density; CV = estimated coefficient of variation associated to D; CI = confidence interval. (a) Sierra Nevada (1995), *Capra pyrenaica*; unbinned distance data; total Chi-square value = 5.7630; df = 4.00; $P = 0.21756$; hazard/cosine; $K = 2.0$; $D = 6.85$, CV = 46.88, 95% CI = 2.73–17.20; number of observed clusters = 138. (b) Sierra Nevada (1995), *Capra pyrenaica*; distance data truncated at 10%; total Chi-square value = 7.9731; df = 4.00; $P = 0.09257$; hazard/cosine; $K = 2.0$; $D = 8.42$, CV = 51.62, 95% CI = 3.12–22.72; number of observed clusters = 124. (c) Val Troncea (2007), *Rupicapra rupicapra*; unbinned distance data; total Chi-square value = 7.0644; df = 6.00; $P = 0.31494$; hazard/cosine; $K = 2.0$; $D = 30.74$, CV = 39.13, 95% CI = 14.51–65.13; number of observed clusters = 42. (d) Val Troncea (2007), *Rupicapra rupicapra*; without binning and truncating 10% of data; total Chi-square value = 2.6380; df = 5.00; $P = 0.75559$; hazard/cosine; $K = 2.0$; $D = 32.44$, CV = 46.46, 95% CI = 13.43–78.34; number of observed clusters = 38.

using binned data truncated at 5% and 10% ($p = 0.636$ and $p = 0.609$, respectively). For the sake of completeness, we also analyzed surveys with questionable sample size ($26 \leq n \leq 60$), and similar results were found (results not presented).

Figure 2 illustrates the effect of data truncation on model fitting, density estimates and their associated CV. In these two selected surveys, we can observe a slight tendency of increasing density and decreasing precision after truncating 10% of observations.

Discussion

Almost any analyst interested in the application of DS methodology is confronted with some technical and often arbitrary decisions that sometimes induce misleading results. Should data analyses involve truncation and, if so, how much should we truncate? Should distance categories be used to bin the data? How many bins should we use? The effect of truncating, binning the data into distance categories and choosing an "optimum" number of classes is rarely discussed when presenting results in the published literature. Buckland et al. (2001) advocate truncating about 5% or 10% of the data in order to eliminate extreme observations or outliers. There is a trade-off between the amount of data available for density estimation and data binning or truncation.

When data are truncated and/or binned there is a reduction in the amount of information available for estimation, which could lead to a decrease in precision. In the case of binned data, DS uses the count and the mid-point of each interval class for estimation purposes, rather than individual distance observations. Our results suggest some evidence that binning untruncated data has a significant effect on density estimates. However, there is no really good reason to bin distance data unless there is evidence of problems such as measurement error, heaping or movement. Although assumption violations in our studies were kept to a minimum, we argue that when sampling mountainous ungulate populations, these types of problems are more likely to occur due to the particular features of the land topography. The effect of binning might actually be different depending on what are the underlying problems that led to the need for binning.

One of the main findings of the current study is that density estimates may increase with increasing level of truncation. This might seem counterintuitive and in disagreement with previous work (see for instance, Southwell & Weaver 1993; Stanbury & Gregory 2009).

We could consider that the use of truncated data can severely underestimate the effective strip width and, consequently, overestimate density. However, for the species in consideration, DS data often are clustered, and truncation sometimes involves removing a significant amount of data at larger distances (either clusters or individuals) in mountainous terrain without necessarily impacting the probability of detection, but seriously affecting density estimates due to the trade-off between the number of detections ignored after truncation and the estimated effective strip width. This may be particular important in data collected from mountainous terrain, where the topography plays an important role in detectability, and the detectability function may not be strictly monotonically decreasing.

Our results, however, agree with those obtained by Southwell and Weaver (1993) regarding the loss of precision in estimates when working with truncated and/or binned data. This work also suggests that DS methodology likely is fairly sensitive to the choice of data analysis strategy. However, we cannot assess the accuracy of our estimates, as the real density remains unknown in the majority of cases. Therefore, further research involving simulations, artificial or marked populations and/or combined methodologies (e.g. DS and capture–recapture) would be necessary for assessing whether DS methodology is appropriate to monitor mountain ungulate populations (Pérez et al. 2002).

In order to increase the precision of density estimates, some users of DS methodology may use single animals as individual observation units, even in the presence of grouped populations. This approach is completely inadequate and has been widely discussed in the literature. Grouped animals are not independent observations; therefore, we recommend treating groups as the units of observation to avoid possible violation of the assumption that every observation must be an independent event (Burnham et al. 1980; Barry & Welsh 2001; Buckland et al. 2001; Borchers et al. 2002). The variance associated with mean group size is an added component of the overall variance of the estimated density, as shown by Equation 2 (Leatherwood et al. 1978; Southwell & Weaver 1993; Buckland et al. 2001).

Different authors have pointed out the possibility that group size and perpendicular distance of observation are dependent variables (Drummer & McDonald 1987; Drummer et al. 1990; Otto & Pollock 1990; Drummer 1991; Chen 1996; Chen & Cowling 2001), but, in general, this was not the case in our study. A possible explanation for the lack of dependency between group size and detection distance might be related to the topographic features of the terrain, the vegetation type or some other factors, but further research is needed to investigate this finding. Nevertheless, additional work using multiple

covariate distance sampling (MCDS) would help us to determine if group size or other covariates (e.g. habitat type, sex and age of animals) are needed to better model detectability.

Our study provides evidence that estimating the density of mountain ungulates with high precision and accuracy is difficult. A detailed description of the way in which data are analyzed is fundamental to better understand and make comparable results from different surveys.

There is often unaccounted variance in DS estimates, as these are conditional on some essentially ad hoc decisions. However, if there are no issues in the data, and for adequate sample sizes, the impact of these choices may be negligible. Often, differences observed among specific data strategies are the result of the joint effect of possible problems in the data sets, common to ungulate data, and an eventual effect of the strategy itself. Therefore, these strategies are mostly important and needed when dealing with data for which some assumptions are violated. Then the analysis chosen cannot be taken from a recipe; it must be carefully chosen to minimize the violation of the assumptions. For the surveys that we analyzed, assumption violations were kept to a minimum because strict sampling protocols were followed during data collection.

One may suspect that animals can react to the presence of the observers prior to their detection. Therefore, the assumption that $g(0) = 1$ could be violated. This assumption was assessed with the use of Bayesian statistics (Markov's chains; see Monteiro 2010). For this purpose, Monteiro used the observations of the surveys from PNP (*Rupicapra pyrenaica*) since most of the individuals of this population are marked (Alpízar-Jara & Pollock 1996). The results demonstrated that $g(0)$ did not significantly differ from 1.

The distance to the animal or to a group was measured to their initial location. For this purpose, if the animals moved, any referential object (e.g. a tree or a stone) can be very useful. When we work under optimal light conditions (this implies an adequate sampling design regarding the hours used to carry out the survey, and avoiding sampling under bad weather conditions) accurate measures of distances and angles can be obtained.

Finally, the use of the cluster (or group) as the observation unit guarantees that each observation is an independent event.

Even when the assumptions of the DS methodology are met, obviously the results must also be correctly interpreted and/or discussed. For instance, a high CV value can indicate a great variability of local density within the study area, even when a high number of observations have been recorded, or else

it must be due to a great variability of cluster sizes (see surveys from the PNPG, Table II).

Estimating year-to-year changes in density is not a realistic goal for most species, and population density *per se* provides no information on the relationship between the population and its habitat (Morellet et al. 2007). For these reasons, these authors propose the use of a set of indicators of animal performance, population abundance, habitat quality and/or herbivore habitat impact within the context of management programs.

The reliability of density estimates by means of DS, particularly in those areas in which a small number of observations are recorded, might be reached after implementing repeated surveys over a year (see Loison et al. 2006).

Sampling free-ranging populations, like those in the wild, is a difficult task, despite the methodology used. In any case, our results highlight that DS works also in mountainous habitats.

Acknowledgements

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