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Drs. Kroll, Alt, and Guynn
Deer trustees

RE: Comments on the deer trustees' preliminary report

Drs. Kroll, Alt, and Guynn,

It's a little unclear to me what the purpose of the preliminary report released online (hereafter: "the report") was. The press release from the Department of Administration (3 October 2011), Dr. Kroll's quote within the DOA press release, the report itself, and the Governor's executive order all promised a review of Wisconsin's deer management practices that was "independent"," objective", and "scientifically based". The report promises a "more comprehensive" report due in June and there were public meetings scheduled to "gather ideas and solutions from citizens, sportsmen, and sportswomen to forge a new age for Wisconsin's deer management practices." My confusion is this. While I find that the report is obviously independent, the findings and conclusions drawn in the report are appear to me to be significantly lacking in the scientific content and objective analysis one would need to "forge a new age" which I take to mean something similar to making big changes in Wisconsin's deer management. My hope is that you are all actively at work putting the findings of the report into a rigorous scientific context for a more comprehensive June report. In that spirit, I offer the following observations on issues of science and objectivity raised by the findings and conclusions included in the preliminary report.

1) The preliminary report relies too heavily on self-motivated reporting by stakeholder as a means of characterizing how stakeholders view management.

As I pointed out to you in my e-mail of 16 January 2012, there is a reason why the science of human dimensions of wildlife is highly developed in its own right. Researchers in human dimensions have established that the stakeholders who volunteer their opinions at public meetings or in online forums are not representative of the larger stakeholder population (Johnson et al. 1993, Duda and Nobile 2010, Hunt et al. 2010, Alessi and Miller 2012). In terms of hunters, these volunteers tend to be different demographically (Johnson et al. 1993, Cornecelli and Grund 2011) and different in terms of their success rates and commitment to hunting (Johnson et al. 1993). But most important for the purposes of reviewing Wisconsin's deer management, sportspersons who volunteer in these forums view management more negatively and are more extreme in their views and sometimes want different things

relative to the larger sportsperson population (Johnson et al. 1993, Hunt et al 2010). The magnitude of these differences varies with the circumstances and the questions and some researchers have concluded that the volunteer submissions were still useful if done in conjunction with a structured presentation on a narrow question (Cornecelli and Grund 2011) while one of the most prominent human dimensions researchers in our field concluded that "(o)btaining representative, unbiased, scientifically valid results from online surveys is not possible at this time" (Duda and Nobile 2010:63).

Public meetings and online forums are useful for engaging stakeholders and might be useful to identify the range of issues that management might need to consider but as a body of knowledge subjected to quantification and analysis, it is not objective. It is biased. Hence, attempts to draw inferences without giving adequate attention to the range and direction of that bias is unscientific. The report mentions that your information gathering through public meetings was not scientific in a couple of places (e.g. Page 4) and you also say that your findings "mirror" those of Holsman (2006, 2007; Page 4). But then you immediately follow up with quantification and inference from the volunteer data including the following statement: "Interestingly enough, the only issue receiving less than 200 comments was 'No change needed'." Presented this way you leave the reader to draw the conclusion that relatively few stakeholders are satisfied with deer management as it is. That may be true but I think it's a predictable outcome and likely an artifact of the way the information was gathered. You have no way of knowing this one way or another. My point is that your goals of being objective and scientific are poorly served because you do not provide the readers (most of whom will not be scientists) the proper context for interpreting your findings and your conclusions ignore the limitations of your sampling (Duda and Nobile 2010).

A similar problem is the over-representation of hunter voices. The WI DNR is charged with managing the deer population on behalf of all of Wisconsin's citizens, deer hunters or not. This is why deer impacts on other resources needs to be a focal point (among others) of deer management. While the report says that no group was intentionally excluded, it also make no mention of soliciting the opinions of other groups (Sierra club, Audubon Society, Woodland owners association, botany enthusiasts) who also have an important stake in management but who are likely to have views on deer management that are different than the self-selected hunters your heard from. I think it's particularly unfortunate that you did not interview Professor Don Waller (UW Madison Botany Department) who, in addition to being an active deer hunter, is one of the nation's foremost authorities on the impacts of deer on native vegetation and who has done most of his research right here in Wisconsin. Solicitation of these groups is particularly important because I have been told anecdotally that many have stopped participating in the public meetings over Wisconsin's persistent deer controversies because of their frustration with being shouted down by the hunters.

2. The report naively and selectively quotes Millspaugh et al. (2006, SAK Audit report) to describe the precision of Wisconsin's use of SAK as having confidence intervals of "…±122%".

Millspaugh et al. (2006) says clearly that the precision of the SAK-based DMU-level population estimates actually generated from field data in Wisconsin could not be estimated because the variances of key input parameters are unknown and because the effect of professional judgment in filtering input

variables and pooling across space and time to tame variance could not be quantified (pooling data effectively increases the sample size). In fact, summary point number 8 states this explicitly: "Precision expressions for SAK estimates are currently unattainable given the data input used in the model. Without empirical estimates of all inputs, ***it is not possible to calculate confidence intervals***." (Millspaugh et al. 2006, p. 3. Emphasis added). The "…±122%" figure comes from a Monte Carlo simulation of SAK wherein the authors introduced demographic variance in their matrix model of deer population dynamics and created sampling variances for input parameters drawn from that simulation on the basis of statistical theory (page 40). This was done without attempting to simulate ***the effects of professional judgment and experience in applying SAK to field-generated data in Wisconsin*** (page 34). The point of this part of Millspaugh et al's (2006) analysis was to study how SAK might be vulnerable to poor precision because of structural issues and sample sizes at the DMU-level and the Millspaugh et al. (2009) publication is an outcome of this analysis not an evaluation of the empirical performance of SAK in Wisconsin. Admittedly, the distinction between the actual SAK estimates in Wisconsin and the theoretical simulation of SAK estimates in the summary comments (page 2) of Millspaugh et al. (2006) is not as clear as it is in section 11.3 (page 40) but the pattern of actual SAK estimates is demonstrably better than "…±122%" and the"…±122%" should not have passed the sniff test for you because there are no reports of 0 deer estimated using SAK in any DMU in harvest database. My understanding is that you were provided with the database thus it puzzles me that you did not check this out. In fact, the "…±122%" assertion is a testable hypothesis.

Millspaugh et al. (2006) based the "…±122%" confidence interval statement by assuming a normal distribution for the shape of a distribution of simulated SAK estimates whose the coefficient of variation (CV) was 0.62 (when the theoretical sources of demographic and sampling variance were simulated) and whose mean 10,000 deer. An SAK estimate that underestimates the true population size by ≥100% would return an estimate of ≤0 deer whatever the actual true population size, thus counts of SAK estimates in the DNR database ≤0 can be used as something of a test statistic in a total collection of SAK-based estimates coming from DMUs with differing population sizes. The cumulative probability distribution for a normal curve with a CV of 0.62 indicates that the probability of observing an SAK value ≤0 is 0.054 (I calculated this using Excel's NORMDIST function), conversely the probability of observing a SAK value >0 is 1-0.045 = 0.945. Hence under the null hypothesis of a CV=0.62 (aka confidence intervals of "…±122%"), one would expect the Wisconsin SAK to return a population estimate of ≤0 in 5.4% of all cases. I know there are no 0 or negative SAK values in the database but if I were to approach this as an experiment designed to test the hypothesis of CV = 0.62, I might draw a sample of 100 representative SAK-based estimates. If so, I would expect about five of them to be ≤0. If none of them were, I would calculate the probability of observing no estimates ≤0 in 100 samples under the null hypothesis as $0.945^{100} = 0.003$. If I drew a sample of 1000 SAK estimates and found no estimates ≤0, the probability under the null hypothesis would be $0.945^{1000} = 2.27*10^{-25}$. In wildlife science we commonly reject null hypotheses when probabilities associated with test statistics (p-values) are below 0.05. By convention in most biological and ecological sciences, these tests based on these values would be highly significant evidence against the null hypothesis. A more sophisticated test could be built on the basis of year-to-year differences within DMUs but I will leave that to you. What Millspaugh et al. (2006, 2009) report is the potential lack of precision given 1) assumptions made about demographic variance on their matrix

model simulation 2) assumptions made about additive sampling variance on the input parameters drawn from their matrix model, and 3) a *de facto* assumption (because they did not address it) that professional experience in the use of SAK was unimportant. What they did not report and indeed what they said is unquantifiable because of lack of data is the actual precision of SAK estimates reported for DMUs in Wisconsin. To claim otherwise is dishonest.

Similarly, you highlight in boldface the following statement: " ***Alternative models, such as statistical age-at-harvest models, which require similar data types, might allow for more robust, broad-scale demographic assessments"*** (Millspaugh et al. 2009). This quote comes from the abstract (Millspaugh et al. 2009:442) and this is important because the issue of using statistical age-at-harvest models as alternates to SAK needs to be understood in terms of the context of the body of the paper. The proper context for this issue is Millspaugh et al. (2006:4, 2006:50, and 2009:448). The best example is to quote Millspaugh et al. (2006:4) directly (*emphasis added*).

> Unrealistic assumptions required in the SAK model might be eliminated ***if auxiliary data were collected to estimate age- and sex-specific harvest rates***. However, ***these data also could be used*** in alternative estimation methods, such as the statistical age-at-harvest approach (e.g. Gove et al. 2002), which might hold promise for deer estimation in Wisconsin.

In other words, the authors are saying that SAK could be improved if managers had independent demographic data to add to it but then they could also use statistical age-at-harvest. The context for this issue is the use and availability of the "auxiliary data". Typically, the kind of auxiliary data at issue comes from something like radiotelemetry and needs to have "the same relevant time and spatial dimensions as the population reconstruction" (Skalski et al. 2005:535). Your report mischaracterizes the potential for using statistical age-at-harvest as a replacement for SAK estimates (page 11) because you don't tell the readers that yearly DMU-level statistical age-at-harvest estimates likely require the expense of generating yearly DMU-level auxiliary data with yearly DMU-level radio-telemetry research (often estimated as a rule of thumb at $100,000/study area/year). Are you aware of any state that does this?

> 3. The report hints cryptically at the need for additional attention to habitat management to improve herd productivity (page 17) without any scientific support.

Where's the evidence that deer production is a problem? According to QDMA, Wisconsin has some of the most productive deer range on the continent (second in the nation in terms of fawn recruitment in 2010 [the most recent date reported], Adams et al. 2012:18).

My concern is that increasing the productivity of Wisconsin's deer populations (if possible) will only exacerbate problems of overabundance and herbivory. Of all the science available to you to inform the a discussion about deer management in Wisconsin, the effects of deer browsing on native vegetation has got to be the biggest in terms of published research and the most specific in terms of its sourcing from and application to Wisconsin. Don Waller's (Botany department, UW-Madison) research is particularly important because he has demonstrated over and over again using multiple field techniques that the native plants of Wisconsin are at risk from the cumulative effects of a chronically overabundant deer herd. Don's publications are available at his website (http://www.botany.wisc.edu/waller/publications/publications.html). In terms of the "boots on the

ground" perspectives you are so fond of calling for, from around Wisconsin there are likely few people anywhere who have invested as much time out in the woods in understanding deer ecology as Waller and his associates.

4. The report ignores independent sources that suggest that Wisconsin is actually a comparatively good place to hunt for deer.

QDMA's 2012 Whitetail report lists Wisconsin as 4rth in the nation in terms of harvests of antlered bucks in 2010 and 3rd in the nation in terms of bucks harvested per square mile (Adams et al. 2012: 5). Similarly, Wisconsin was the 2nd highest among 13 Midwestern states in terms of harvests of antlerless deer per square mile (Adams et al. 2012:9). In addition, QDMA's now 10-yr old analysis of trophies listed by Boone and Crockett and Pope and Young trophies (1991 – 2001) listed Wisconsin as the top state in the nation. In fact, the Boone and Crockett database is searchable online (http://www.boone-crockett.org/community/trophyDB/index.asp?com=TROPHYDB&cpe=1) so I did a query of typical white-tailed deer trophies (B&C points ≥160) reported for Wisconsin and it neighboring states since 2001. Wisconsin is at the top having harvested 6.4 B&C typical trophies per 1000 square miles of land area. Illinois, a notable white-tailed deer trophy state, came in second at 5.2. In terms of latitude, weather, vegetation, and landuse, Wisconsin is more similar to Minnesota and Michigan. Hence it is especially notable that Wisconsin's neighbors to the east and west only produced 1.7 trophies per 1000 square miles of land area each. That's less than a third of those reported for Wisconsin. Just for fun and to round out the comparison to your home states, Texas came in a 0.5, Pennsylvania at 0.5, and South Carolina registered only 3 trophies *total* (that's 0.09 per 1000 square miles of land area).

Taken together these statistics suggest that Wisconsin is a comparatively good place to be a deer hunter whether your interest is in the opportunity to harvest any deer or the opportunity to harvest a trophy buck. These statistics also beg the question about whether the deer controversies are driven by unrealistic expectations.

5. The report raises the alarm about predator impacts without any scientific basis.

The reports summary contains a boldface sentence with that begins: **The predation issue also should be addressed immediately**… (page 21). What's the scientific rationale for what looks (to me anyway) as an alarmist way of raising the issue? The report claims that "Concerns by landowners, hunters and non-hunters about predators have grown over the last decade" (page 17), but there is no citation and I am aware of no longitudinal study that demonstrates this. Michigan and Minnesota have the same predator communities as Wisconsin and yet Wisconsin evidently seems to be doing better in terms of fawn recruitment and fawn recruitment and deer harvests (Adams et al. 2012). The fact that Wisconsin's wolf population is "at least three times higher than the goal" (page 19) is a red herring because the goal you are referring to (350 wolves outside of Indian reservations) is an arbitrary number established without regard to population-level impacts on deer. "Three times higher than goal" means roughly 1000 wolves occupying roughly 30,000 square miles of area with hundreds of thousands of deer.

It's completely intuitive draw a connection between the presence of predators and a reduction in deer numbers (as Leopold himself so famously wrote) but (as Leopold also found out) the science of predator

impacts can be counter-intuitive. I have no doubt that some Wisconsin deer hunters are concerned about the impact of predators. I also have no doubt that the level of concern may be increasing given high profile public debates about wolves and our recently increased estimate of bear numbers (MacFarland 2009). My concern is that the report fans the flames of this controversy without offering any new science or objective analysis of the available science.

An "objective" and "scientifically based review" would need to recognize that the extent to which wolves limit or regulate (*sensu* Messier 1991) ungulate populations is controversial among scientists (Gasaway et al 1983, Ballard et al. 2001, Hayes et al 2003, Barber-Mayer et al. 2008) and varies with ecological context (Ballard et al. 2001, Patterson and Power 2002, Hayes et al 2003, Berger and Conner 2008, Rooney and Anderson 2009), spatial scale (Barber-Mayer et al 2008), and analytical models (Vucetich et. al 2005). Ungulate populations are limited by a variety of intrinsic and extrinsic factors (Messier 1991, Godfrey and Reese 2002, Hebblewhite et al. 2005) through mechanisms that separately or jointly (i.e. because of compensatory relationships) influence one or more age-specific reproductive rates and cause-specific mortality rates (Patterson and Power 2002, Hayes et al. 2003). Thus, growth rate, because it integrates changes in reproduction and mortality (Vucetich et al. 2005) is the most natural response variable for studying factors that influence population dynamics (Godfrey and Rees 2002). I showed you an analysis suggesting that DMU-level growth rates are relatively insensitive to variation in black bear occupancy and overlap with known wolf pack (Van Deelen In prep.). This same analysis suggests that DMU level growth rates have a negative association with density – consistent with an interpretation that the deer herds are at high density relative to carrying capacity and suggesting that additional predation mortality from wolves should elicit a largely compensatory numerical response (Sinclair and Pech 1996, Ballard et al. 2001).

Coyotes are also predators of white-tailed deer in the Great Lakes region (Van Deelen et al 1997) and eastern Canada (Patterson and Messier 2000), however predation by wolves is almost certainly compensatory with predation by coyotes because wolves kill and displace coyotes (Arjo and Pletscher 1999, Berger and Conner 2008).

The point is, Wisconsin has a healthy deer population as evidenced by sustained high harvests and strong recruitment (Adams et al. 2012). Controversy over predators is as old as the field of wildlife management and an objective look at the available science (theoretical and empirical) would find little corroboration for the view that Wisconsin is experiencing or about to experience a predation emergency in terms of impacts to the deer population. Objectivity is badly needed here.

I am not meaning to dismiss hunter concerns about predation and neither is the DNR. Our telemetry research on deer is one of the largest wildlife research projects ever undertaken in Wisconsin and it was motivated in part by concerns over predation. I respect that. The telemetry project demonstrates the DNR's commitment to understanding key mortality rates including those occurring as a result of predation.

I wish you every success in your efforts to bring science and objectivity to bear on these contentious debates.

Sincerely

Tim Van Deelen

Associate Professor

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