Exhibit B: Studies Cited in the NTT and COT Reports Fail DQA Standards

Aldridge et al. 2008.[1] This study is cited three times between the COT Report and NTT Report in support of the flawed presumption that disturbance leads to extirpation. This study is also mis-cited in support of the 50-70% sagebrush threshold in the NTT Report. The NTT Report claims 50-70% of the range must be adequate in order for GRSG to persist. Aldridge et al. 2008 suggests that "preferably" 65% is necessary, but the results of this study give measurements related to range persistence and anecdotally how that might correlate to extirpation. For example, if occupied habitat was converted to a crop field, the population closest to the converted area was less likely to persist than populations located in suitable habitat farther away from the field. These results do not indicate that 70% or even 65% of the habitat must be suitable--only that fringe populations are more likely to be extirpated. Logistic regression, as used in this study, examines combinations of continuous variables to predict a positive or negative outcome. In this case, that was the presence or absence of GRSG at each point on a 5 km grid. However, the predictions of what affects the presence/absence of points on the grid can be completely unrelated to population persistence where the authors have not examined the totality of population response.

Aldridge, C. L., and M. S. Boyce. 2007.[2] The NTT and COT Reports cite this study for the flawed proposition that limited source habitats appear to be the main reason for poor nest success (39%) and low chick survival (12%). It is questionable that this study of GRSG in Alberta can have utility outside of that limited study area at the northern periphery of the species range. The Alberta population is small and has minimal suitable habitat available irrespective of human influence. Rather, ecology and geologic formations are the primary limiting factors in Alberta. Therefore, any impact on this population will appear heightened compared to other populations.

The authors claim many GRSG populations are at risk of extirpation. (page 509.) However, the authors proffer no citations nor authority for that proposition. In their modeling, the authors chose a subjective suite of variables related to habitat or human disturbance that they felt may be important. Id at 510. Such an analysis is readily subject to bias.

Further, they used a one-km^2 window which may be so large as to be meaningless as it is over 33 times larger than the 30m grid size used by the 2001 National Land Cover Data (NLCD 2001, http://www.epa.gov/mrlc/nlcd-2001.html). The 30m grid size has long been in use to develop qualitative models for endangered species critical habitat because it is the resolution of many digital elevation models (e.g. Turner et al. 2004). Some conservation GAP analyses use data with a resolution of 10m. While data resolution may limit analyses in some regions, a more focused evaluation of sage grouse core areas that

[1] Aldridge, C.L., S.E. Nielsen, H.L. Beyer, M.S. Boyce, J.W. Connelly, S.T. Knick, and M.A. Schroeder. 2008. Range-wide patterns of greater sage-grouse persistence. Diversity and Distributions 14:983–994.

[2] Linking occurrence and fitness to persistence: habitat based approach for endangered Greater Sage-Grouse. Ecological Applications 17:508–526.

utilizes a more informative grid size (e.g. industry standards of 90, 30, or 10 m) would be a more appropriate basis for policy decisions and conservation measures than that offered in this paper.

The authors grossly assume that roads and power lines affect productivity and chick survival saying little more than the generalization that "mortality associated with power lines and roads occurs year-round." The authors cite absolutely no support for this assertion. *See* Messmer Tall Structure Synthesis at 10. This is clearly inappropriate to extrapolate to nest success and chick survival and the NTT reliance upon this proposition should be withdrawn. *See* NTT at 19. Moreover, citations to Aldridge and Boyce 2007 are inappropriate for the proposition that energy development leads to population declines because non actually quantified a population-level demographic response. See NTT at 19.

> "Habitat fragmentation, largely a result of human activities, can result in reductions in lek persistence, lek attendance, population recruitment, yearling and adult annual survival, female nest site selection, nest initiation, and complete loss of leks and winter habitat (Holloran 2005; Aldridge and Boyce 2007; Walker *et al.* 2007; Doherty *et al*. 2008)." COT at 9.

All of the studies cited in support of this were conducted in heavily developed energy fields utilizing older industry technology, thus, these studies are representative only of heavily developed energy fields in Wyoming and Alberta developed several years ago and not more broadly representative of various "human activities," as claimed in the studies. , The study also represents a very small fraction of the range of GRSG, none of which are representative of the Great Basin birds. It can also be argued that the study area/population used by Aldridge and Boyce is not representative of GRSG rangewide. The Alberta population is small and has minimal suitable habitat available irrespective of human influence, but is a result of ecology and geologic formations. Therefore, any impact on this population will appear heightened, then what might happen to a stronghold.

Interestingly, the data in Aldridge and Boyce 2007 suggest the majority of the late brood rearing habitat is already on land that is regulated by BLM.

Blickley et al. (in press). This study reported a population decline in lek attendance when projected sound from recordings at the edges of leks, which were as high as the noise levels occurring within 200m of a busy freeway (as measured across an open field with traffic loads of greater than 50,000 cars per day, or 55-70 decibels as shown in Figure 2 of Reijnen et al. 1995). The subsequent avoidance was then assumed to lead to have a negative effect on the population (i.e. contribute to their decline). Below, is a relevant excerpt from Blickley et al. (in press):

> Drilling-noise recordings were broadcast on experimental leks at an equivalent sound level (L_{eq}) of 71.4±1.7 dBF (unweighted decibels) SPL re 20 μPa (56.1±0.5 dBA [A-weighted decibels]) as measured at 16 meters; on road-noise leks, where

> the amplitude of the noise varied with the simulated passing of vehicles, noise was broadcast at an L_{max} (maximum RMS amplitude) of 67.6±2.0 dBF SPL (51.7±0.8 dBA).

The fact that authors broadcast such high levels of noise in such close proximity to leks biased the results, an error of omission by the authors and the NTT Report that cites them and proposed regulations based upon their recommendations.

The NTT Report cannot have it both ways, claiming a negative effect on sage grouse populations but admitting that there was "low statistical support for a cumulative effect of noise over time" in the study by Blickley et al. (in press). As noted above, there are no data showing a long-term cumulative decline in the sage grouse population in the Pinedale Planning Area.

The cited research was an amateurish attempt to reproduce the sounds of oil and gas development using substandard equipment that was wholly unsuited to the task of accurately recording and playing back traffic and sounds from oil and gas operations. Deficiencies in Blickley et al.'s equipment are detailed below.

Microphone: According to the manufacturer (http://en-us.sennheiser.com/k6-microphone-system), "the ME 62 [microphone used by Blickley et al.] is an omni-directional microphone head suitable for K6 and K6P powering modules. It can be used for reporting, discussions and interviews. The ME 62 is particularly suitable for good reproduction of 'room' ambience and 'spaced omni' stereo recording. Matt black, anodized, scratch-resistant finish."

Recorder: The Marantz model PMD670 used by Blickley et al. does not offer high-resolution (88.2 or 96 KS/s) sampling rates, its metering characteristics are unknown, and it is limited to 16/48 recording and thus is not considered a high-resolution recorder. It retails online for $700.

Playback speakers: The speakers used in the study were standard outdoor speakers camouflaged as rocks and designed for background music playing in home, hotel, and amusement park applications. They were not designed for accurately reproducing industrial sounds. The specifications for the speakers may be found on the manufacturer's website: http://www.ticcorp.com/specifications_tfs14.pdf.

The speakers were powered by 12 volt car batteries rather than 120 volt AC power and a car stereo amplifier of unknown make and model was used to boost the output. Packed into each simulated rock speaker housing was a 10" woofer with an injection molded cone, a 5.5" midrange cone, and 2" soft dome tweeter. The size and quality of the speakers, and the small speaker housing, severely limits the physical capability of the system to accurately reproduce either low or high frequency sound produced by oil and gas operations or traffic.

As a result of substandard equipment and lack of expertise in sound recording and reproduction, Blickley et al. (in press) resorted to placing their speakers at the edge of leks and to playing their systems at high levels in order to elicit a behavioral response. This is a biased approach to obtain a preferred result. The BLM cannot rely on biased research in its decision-making.

The recommended noise levels are not based upon any standardized, repeatable data collection, or accepted methods of sound measurement. The methods used by Blickley et al. (in press), and reported results did not contain any credible, professional analysis of local ambient sound levels or oil and gas noise (e.g. the type, duration, frequencies, sound pressure levels, and power of sound produced by different oil and gas drilling or production operations; equipment being recorded); or employ the use of professionally accepted standards, such as International Organization for Standardization (ISO) standards for quantifying industrial and traffic noise (http://www.iso.org/iso/home/standards.htm).

The standards not followed by the cited studies include, but are not limited to: ISO 1996-1:2003 Acoustics -- Description, measurement and assessment of environmental noise -- Part 1: Basic quantities and assessment procedures; ISO 9613-2:1996 Acoustics -- Attenuation of sound during propagation outdoors -- Part 2: General method of calculation; ISO 4871:1996 Acoustics -- Declaration and verification of noise emission values of machinery and equipment; ISO 532:1975 Acoustics -- Method for calculating loudness level; ISO 7196:1995. Acoustics -- Frequency-weighting characteristic for infrasound measurements; ISO 8297:1994 Acoustics -- Determination of sound power levels of multisource industrial plants for evaluation of sound pressure levels in the environment -- Engineering method; and IEC 61672-1:2002(E) - Electroacoustics, Sound level meters -- Part 1: Specifications).

Blickley et al. did not employ any sound propagation models in their study to quantify the confounding effect of temperature, relative humidity, topography, ground cover and surface porosity, wind direction, the direction noise was generated from, the geographic extent of the noise, its duration, frequency of occurrence, or permanence, (Attenborough 2007). Nor did they provide any correlation of their playbacks compared to the industrial and traffic sources they had attempted to duplicate. Furthermore, no graphic equalizer was used which would have allowed for the adjustment of sound pressures in different frequency ranges (at standardized 1/3 octave band frequencies), and no measurement of sound pressure levels was taken in front of playback speakers, which together would have allowed for the accurate reproduction of the sound at the same frequencies and sound pressure levels as the original noise. Therefore, BLM cannot base regulations upon no data and results based upon arbitrary methods that are not compliant with accepted professional standards in the noise control industry (i.e. Bies and Hansen 2009; ISO).

Carpenter, et al. 2010.[3] This study was cited four times in the NTT Report for the proposition that energy development and disturbance cause negative impacts to GRSG

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[3] Carpenter, J., C. Aldridge, and M.S. Boyce. 2010. Sage-grouse habitat selection during winter in Alberta. Journal of Wildlife Management -74:1806-1814.

through avoidance behavior i.e. loss of functional habitat. The NTT Report incorrectly cites this paper in support of no surface occupancy ("NSOs") and four-mile buffers even though such concepts were not discussed therein, and the maximum distance discussed is well short of four miles. While the authors discussed distances from 328 feet to 1.2 miles and ultimately recommended 1.2 miles, this is far from the excessive four-mile buffers recommended in the NTT Report. Despite the representation in the NTT Report, this paper does not support the proposition that disturbance equals population declines. NTT Report at 19. The NTT Report also recommends, "[D]o not allow new surface occupancy on federal leases within priority habitats, this includes winter concentration areas (Doherty et al. 2008, Carpenter et al. 2010) during any time of the year." NTT at 23. The NSO is not supported. First the recommendations of this study are limited to the population in Alberta, and the authors recommend a 1.2 mile buffer, not a categorical prohibition of development.

An important limitation of this study is that it does not differentiate between habitats that were previously used and those that GRSG now avoid. It assumes subjective factors such as moderate sagebrush cover will always lead to use by GRSG even if there is no evidence that GSG ever used these locations. The authors provide no evidence of ground-truthing for their assertions. They assume that GRSG would use this otherwise suitable habitat but for energy development and anthropogenic features. The authors state that "*…winter habitats may be of greater importance in declining populations*" based upon opinion and conjecture. What little evidence they provide, i.e. Swenson 1987 and Beck 1977, is completely outdated. Moreover, GRSG survival is consistently highest over winter, so any population level impact is likely limited by other variables. Other citations used in the study, such as Doherty et al. 2008, are refuted herein.

The authors admit that they merely assumed GRSG avoid landscapes with anthropogenic disturbances. Further, the authors fail to explain how capture sites were chosen and whether ease of winter access may have impacted their results or why they chose 2003 as a model year. In their modeling exercise, they subjectively chose some 86 variables for input. Table 1. Such a model is only as good as the data entered. This study relies on the quality of the techniques used by Aldridge and Boyce 2007--studies with their own serious shortcomings as critiqued and referenced herein. The statement, "[T]hreats such as oil and gas development or cultivation of native habitats could reduce connectivity and disrupt migratory patterns, possibly causing bottlenecks between seasonal ranges or populations" was not adequately assessed in the paper and seems to be based purely on opinion. The results of this study might inform management in Alberta, but not in other parts of the range. Moreover, this study as well as Aldridge and Boyce, and Doherty et al. 2008 all included study areas that were heavily developed, and not representative of conditions range-wide. Furthermore, as discussed herein, more recent studies have shown that the effects of energy development today are much different and much smaller than the dated scenarios for oil and gas development used by the authors.

Cassaza et al. 2010.[4] This study is mis-cited in the COT Report in support of the proposition that pinon juniper needs to be reduced to no more than 5 percent of the landscape. Given the authors considered brood-rearing habitat, it is inappropriate to rely upon this study for the COT Report's recommendation that this proposed pinon juniper threshold be applied across all GRSG habitat.

Doherty et al. 2008.[5] This paper is cited six times in the NTT Report for support of surface use restrictions. This paper is largely based upon professional judgment rather than hypothesis testing. Even then, the NTT Report mischaracterizes the study as support for its recommended 3% disturbance threshold, four-mile buffers and prohibition on leasing in priority habitat. With regard to the proposed four-mile buffer, Doherty et al. 2008 did not test whether four-mile buffers are necessary, or whether GRSG would respond positively to a four-mile buffer. The authors do say that current management is insufficient to protect winter habitat (i.e. 0.5 mi lek buffer.), but do not suggest a wholesale prohibition on leasing. This study did not look at population trends; rather it examined habitat selection and variables impacting it.

The study examined 24 predictor variables over two years to predict female GRSG winter habitat selection. Table 1. For variables that were strongly correlated the authors chose to keep variables they "felt" were strong predictors, and dismissed the others. After deciding on a habitat model the authors conducted a bootstrap analysis (n=5,000) to quantify change in odds of use with the introduction of coalbed methane (CBM) wells. The bootstrap analysis was repeated to quantify how the amount of sagebrush (four square km) affected the odds of use with or without wells. However, instead of conducting analysis with varying degrees of development, the authors assumed full build out at 12.3 wells/km square versus 0 wells/km square. This only provides insight into a worst-case scenario which is not representative of actual conditions or variables across the vast range of GRSG, or newer technologies undertaken in later phased (POD) developments. Topography and sagebrush cover were the best predictors of GRSG use. Ultimately, the authors found that female sage-grouse are more likely to avoid winter habitats with intensive (full build out) CBM development.

While it is well known that GRSG are positively correlated with the amount of sagebrush cover, and Doherty et al. 2008 found that percent cover was a good predictor of occurrence at a coarse (4km sq) scale, the authors improperly state "[C]onversion of sagebrush negatively influences sage-grouse populations" without supporting data. *Id.* at 193. The negative influence that the authors found was avoidance by GRSG. However, the presumption that avoidance leads to population level impacts has not yet been proven, as discussed elsewhere in this challenge.

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[4] Cassaza M.L., P.S. Coates, and C.T. Overton. 2010. Linking habitat selection to brood success in greater sage-grouse, Pp. 151-167 *in* B.K. Sandercock, K.Martin and G. Segelbacher (eds.). Ecology, conservation and management of grouse. Studies in Avian Biology (no.39). University of California Press, Berkeley, CA.

[5] Doherty, K.E., D.E. Naugle, B.L. Walker, and J.M. Graham. 2008. Greater sage-grouse winter habitat selection and energy development. Journal of Wildlife Management 72:187-195.

Importantly, the authors state "Examination of ecological processes at the landscape scale does not eliminate the need to understand habitat relationships at local scales; rather, it will likely require a combination of scales to completely understand how sage-grouse respond to their environment." *Id.* at 194. This statement undermines the NTT's broad one-size-fits-all landscape approach to the exclusion of local data.

Doherty et al. 2010.[6] NTT Report cited Doherty et al. 2010 twice in support of a 50-70% sagebrush landscape cover. However, the Literature Cited section lists two Doherty et al. 2010 studies and fails to differentiate which one stands for the proposition alleged. Neither Doherty et al. 2010a or Doherty et al. 2010b stand for this proposition. Doherty et al. 2010a developed nesting habitat selection models at multiple scales to evaluate the "relative importance and interpretation of local, landscape, and multiscale models." Doherty et al. 2010a did not test what minimum range is necessary for GRSG to persist. Ultimately, Doherty et al. 2010a found that multiscale models were more predictive than local or landscape scale models alone. They also found that both local and landscape scale features influence nesting site selection and that sagebrush cover alone was not predictive of use. Doherty et al. 2010b used lek count data and bird abundance at varying levels of energy development to develop a method for evaluating offsets. Doherty et al. 2010b found that as energy development increased (ie. density of wells) the likelihood of lek loss increased and bird abundance decreased. Like many similar studies, Doherty et al. 2010b does not account for movement of GRSG away from disturbance.

Fedy et al. 2012.[7] This study is cited two times in the COT Report, once in support that areas outside priority areas of conservation ("PACS") may need to be maintained. This is another example of the mistaken assumption that GRSG are unable to bypass unsuitable habitat during migration or other seasonal movements.

Garton et al. 2011.[8] This study is cited 15 times in the COT Report to allegedly demonstrate population declines, existing conditions, and expected persistence. There are significant issues with this study as discussed in detail in Exhibit A to the Petitioner's DQA Challenge on the NTT Report.

Hagen et al. 2007.[9] This study was cited nine times in the NTT Report in support of the proposition that sagebrush cover must meet certain thresholds (15%) and for reclamation

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[6] Doherty, K.E., D.E. Naugle, and B.L. Walker. 2010a. Greater sage-grouse nesting habitat: The importance of managing at multiple scales. Journal of Wildlife Management 74:1544-1553; or Doherty, K.E., D.E. Naugle, and J.S. Evans. 2010b. A currency for offsetting energy development impacts: Horse-trading sage-grouse on the open market. PLoS One 5:e10339. Accessed 19 September 2011.

[7] Fedy B.C., C.L. Aldridge, K.E Doherty, M. O'Donnell, J.L. Beck, B. Bedrosian, M.J. Holloran,G.D. Johnson, N.W. Kaczor, C.P. Kirol, C.A. Mandich, D. Marshall, G. McKee, C. Olson, C.C. Swanson, and B.L. Walker. 2012. Interseasonal movements of Greater sage-grouse, migratory behavior, and an assessment of the core regions concept in Wyoming. Journal of Wildlife Management 76:1062-1071

[8] Garton, E.O., J.W. Connelly, J.S. Horne, C.A. Hagen, A. Moser, and M. Schroeder. 2011. Greater sage-grouse population dynamics and probability of persistence. Pp. 293-382 *in* S.T. Knick and J.W. Connelly (eds). Greater Sage-Grouse: ecology and conservation of a landscape species and its habitats. Studies in Avian Biology (vol. 38). University of California Press, Berkeley, CA.

[9] Hagen et al 2007 A meta-analysis of greater sage-grouse Centrocercus urophasianus nesting and brood-rearing habitats. Wildl. Biol. 13 (Suppl. 1): 42-50.

bonds, brood-rearing characteristics, habitat parameters and fire treatments. Most of the citations in the NTT Report appear to mischaracterize this study. For example, the NTT fails to distinguish between seasonal habitats or recognize that Hagen et al. 2007 only applied these numbers to nesting habitat. The authors support, "[I]n general, a range of 15-25% sagebrush, 10% forb, 15 % grass canopy cover and, a herbaceous height of 18 cm are needed for **breeding habitats** of greater sage-grouse." (Emphasis added). Hagen et al. 2007 at 43 citing Connelly. However, we know based on other work that these parameters are not applicable across seasonal habitats or range-wide, particularly in the Great Basin, discussed in detail below. Moreover, even the authors recognize that many question, "…the applicability of management guidelines (Connelly et al. 2000) across the range of the greater sage-grouse, as well as the techniques used to derive the earlier estimates of vegetative cover and height." *Id.* citing Bates et al. 2004, Schultz 2004)."

Incredibly, the authors later ignore their own statements on the inapplicability range wide and conclude that they apply "throughout the geographic range of [GRSG]". *Id citing* Connelly and Braun 1977. Not surprisingly, the NTT Report seizes on this improper conclusion in the study to support its flawed one-size-fits-all approach. However, we know based on other studies that "other" shrub cover in some management zones and populations plays a more critical role than sagebrush in nesting habitat, thus the conclusion is wrong.

The authors conducted a meta-analysis of vegetation characteristics at 24 nest sites and 8 brood habitats to determine if there was an overall effect of habitat selection and to estimate average canopy cover of sagebrush grass and forbs and height of grass at nest sites and brood-rearing areas, but these results are quite limited. For example, several researchers[10] have conducted significant work in the Great Basin documenting the characteristics of nesting habitats. These studies conflict with the conclusions of Hagen et al. 2007 that more sagebrush cover is required for nest success. Moreover, the sample size used for this study is very small and could be questioned.

While the authors recognize total shrub cover had a larger effect size than sagebrush only for nesting habitat, their conclusion is directly contrary to this finding. The authors note that methods to measure vegetation characteristics have not always been consistent, which is very important because it diminishes the reliability of older studies, and it also makes it difficult to compare data across studies. Five of the studies analyzed in this study did not differentiate between sagebrush and other shrubs which can bias the results in favor of higher sagebrush cover. However, in a later study Kolada[11] found in the Bi-

[10] Kolada, E. J., Sedinger, J. S. and Cassaza, M. L. (2009), Nest Site Selection by Greater Sage-Grouse in Mono County, California. The Journal of Wildlife Management, 73: 1333–1340. doi: 10.2193/2008-338; Gregg, M., Crawford, M., Drut, M., & DeLong, A. (1994). Vegetational Cover and Predation of Sage-grouse Nests in Oregon. Journal of Wildlife Management, 58:162-166; Coates, P., & Delehanty, D. (2010). Nest Predation of Greater Sage-grouse in Relation to 10 Microhabitat Factors and Predators. Journal of Wildlife Management, 74:240-11 248, Lockyer, Z.B. 2012. Greater sage grouse *(Centrocercus urophasianus)* nest predators, nest survival, and nesting habitat at multiple spatial scales. M.S. thesis. Depratment of Biological Sciences, Idaho State University, Pocatello, ID.

[11] Kolada E.J., J.S. Sedinger, M.L. Casazza. 2009. Ecological Factors Influencing Nest Survival of Greater Sage-Grouse in Mono County, California. The Wildlife Society. DOI: 10.2193/2008-339 See also,

State area that the limiting factor for nesting habitat was not sagebrush cover but rather "other" shrub cover. Hagen et al. 2007 differentiates between early and late brood rearing, and some of the studies analyzed did not, which again can impact the result. For studies that did not differentiate, the authors of this study pooled the effect size.

There are numerous misrepresentations of Hagen et al 2007 in the NTT Report. For example:

"Riparian Areas and Wet Meadows… Within priority and general sage-grouse habitats, manage wet meadows to maintain a component of perennial forbs with diverse species richness relative to site potential (e.g., reference state) to facilitate brood rearing. *Also conserve or enhance these wet meadow complexes to maintain or increase amount of edge and cover within that edge to minimize elevated mortality during the late brood rearing period* (Hagen et al. 2007, Kolada et al. 2009, Atamian et al. 2010)." NTT at 16

- Hagen et al. does not readily support the statement in italics. While the authors report that "[D]uring brood rearing, sagebrush cover decreased from early to late periods, forb cover increased, whereas grass cover and height did not change appreciably," they did not test whether these parameters decreased mortality.

"Within priority sage-grouse habitat, reduce hot season grazing on riparian and meadow complexes to promote recovery or maintenance of appropriate vegetation and water quality. Utilize fencing/herding techniques or seasonal use or livestock distribution changes to reduce pressure on riparian or wet meadow vegetation used by sage-grouse in the hot season (summer) (Aldridge and Brigham 2002, Crawford et al. 2004, Hagen et al. 2007)." *Id.*

- Hagen et al. 2007 does not support the NTT's assertion. This study only looked at selection and vegetative characteristics- other parameters like presence of grazing or other potential pressures were not included in determining use or selection by GRSG.

"Require a full reclamation bond specific to the site. Insure bonds are sufficient for costs relative to reclamation (Connelly et al. 2000, Hagen et al. 2007) that would result in full restoration." *Id.* at 23

- This is clearly not supported by the Hagen et al. study. Perhaps they meant to say restoration to the habitat characteristics described in Connelly and Hagen, but how this sentence is structured implies that the studies support the need for full reclamation, which they do not.

"Fuels treatments…Do not reduce sagebrush canopy cover to less than 15% (Connelly et al. 2000, Hagen et al.2007)" *Id.* at 26.

Kolada E.J., J.S. Sedinger, M.L. Casazza. 2009. Nest site selection by greater sage-grouse in Mono County, California. Management and Conservation Article. DOI: 10.2193/2008-338

- This is only generally supported by Hagen but ONLY for nesting/brooding habitat, not across seasonal habitats.

"Fuels treatments… Do not use fire to treat sagebrush in less than 12-inch precipitation zones (e.g., Wyoming big sagebrush or other xeric sagebrush species; Connelly et al. 2000, Hagen et al. 2007, Beck et al. 2009)." *Id.*

- This is not supported by Hagen et al 2007. The NTT completely misrepresents this study.

Appendix B states, "[B]ecause sage-grouse research has been on-going for over 60 years, managers have access to published literature from several studies (metareplication (Johnson 2002)) that includes different years, study areas, methods, and investigators (Johnson 2002) which leads to more certainty in conclusions (for example see Hagen et al. 2007)." *Id.* at 57.

- However, the authors concede they used different methods than earlier studies and fail to explain how the quality of the data they utilized might influence results. The quality of the lek count and location data used is suspect because those data have been collected by different individuals and agencies using different methods for decades without proper data quality checks and/or data migration and curation. Furthermore, the data are not public and therefore the results are not reproducible. That means that purported "confidence" in the results is without a sound scientific basis.

Holloran 2005.[12] This study is cited 14 times between the two reports (NTT/12. COT/2) in support of several flawed propositions and conservation measures, including alleged population declines associated with energy development and the allegation that fragmentation impacts use and ultimately persistence. Holloran 2005 did not acknowledge that the BLM had intentionally waived stipulations on the Pinedale Anticline in order to facilitate research on impacts without these stipulations. This does not correspond to impacts under stipulations required at the time, nor account for current (and dramatically reduced) impacts under more recent and stringent stipulations. Finally, Holloran's (2005) population scenarios and predictions of population decline have simply failed to come true.

As an initial matter, Holloran 2005 was an unpublished dissertation that did not employ any hypothesis testing. Instead, Holloran 2005 used subjective interpretations of his results, or the equivalent of creating "just so stories" to explain results in light of a particular viewpoint. That is not science, it is subjective opinion. Additionally, the following data quality issues are identified in the study by Holloran 2005 that are relevant to BLM's continued reliance on it as a basis for decision making:

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[12] Holloran, M.J. 2005. Greater sage-grouse (Centrocercus urophasianus) population response to natural gas field development in western Wyoming. Dissertation, Department of Zoology and Physiology, University of Wyoming, Laramie, Wyoming.

Holloran 2005 only speculated on potential causal mechanisms of population decline, as his data and study design were focused only on localized effects. Additionally, Holloran admitted that, *"Identifying causes of population declines has remained elusive."* And the "displacement theory" favored by Holloran (2005) does not provide any test of the hypothesis that local, temporary displacement of yearling sage grouse from areas under intensive development has led to population-level declines.

Holloran 2005 does not provide any data that population declines have occurred, or that density-dependent effects have occurred in nearby areas, only that the results suggest that these *might* occur or have the *potential* to occur. He wrote,

> The results from this study suggest that dispersal from developed areas could be contributing to population declines. Although the proportion of potentially displaced adult and yearling males and yearling females breeding and nesting in areas removed from gas field infrastructure is unknown, offsite populations could be artificially enhanced by gas development. Because of potential density-dependent influences on breeding and nesting success probabilities (LaMontagne et al. 2002, Holloran and Anderson 2005), maintenance of these enhanced populations could require increasing the carrying capacity of offsite habitats.

The author also stated,

> Adult male displacement and low juvenile male recruitment appear to contribute to declines in the number of breeding males on impacted leks. Additionally, avoidance of gas field development by predators could be responsible for decreased male survival probabilities on leks situated near the edges of developing fields (i.e., lightly impacted leks). Although site-tenacious adult females did not engage in breeding dispersal in response to increased levels of gas development, subsequent generations avoided gas fields, as suggested by the temporal shift in nesting habitat selection and differences in habitat selection by yearling and adult females. This suggests that the nesting population response is delayed avoidance of natural gas development. The results suggest that male and female greater sage-grouse displacement from developing natural gas fields contributes to breeding population declines.

As one can readily see, this "strong science" depends upon speculation, hypothetical worst-case scenarios coming true, and creating just-so-stories to explain results. It does not rely on hypothesis testing.

Holloran (2005, page 82, Table 2) actually reported that the probability of survival was predicted to be *higher* (61.5 +6.4%) in disturbed areas than in less impacted areas (29.6 +18.1%) or control areas (48.5 +14.4%). This result is contrary to Holloran's (2005) own assertions regarding supposed population impacts.

Moreover, the author’s predicted population declines (-8.7 to -24-4% annually) have simply failed to come true. Recent analysis of male lek-attendance trends by the State of

Wyoming has instead found that the GRSG population has been increasing since 1990. It is the litmus test of science that when such predictions fail to come true, the hypotheses/theories they are based upon are simply wrong (Platt 1964). BLM cannot rely on studies cited that have been so clearly falsified.

Holloran 2005 did not provide any data that show consistently lower level of fitness for birds that nested father from roads. Further, the author made very specific recommendations regarding one well per section that were not based upon testing of that threshold in the analysis. Holloran wrote, "[M]aintaining well densities of ≤1 well per 283 ha (approximately 1 well per section) within 2 mi of a lek could reduce the negative consequences of gas field development." However, the author did not test impacts at this density versus other well densities. Instead, he reported on leks affected by different numbers of impacts in each of four quadrants in the cardinal directions and predictions based upon correlations at a scale of 3 km. Data, significance tests, and scatterplots of those correlative analyses were not reported, making the scientific rationale for his one-well-per-section not reproducible. BLM cannot rely on unsupported opinion and irreproducible analyses as the basis for recommendations made in the NTT Report.

Five years after the original Holloran study was released (Holloran 2005), Holloran et al. (2010) did not document any population loss--only temporary displacement of sage grouse. We emphasize, even Holloran et al. did not support their own earlier study, yet the NTT Report uses it uncritically. Holloran et al. (2010) wrote the following about their results,

> "Leks that recruited more than the expected number of males were significantly farther from drilling rigs, producing well pads, and main haul roads compared to leks that recruited fewer males than expected (Table 1). Additionally, leks that recruited more males than expected were significantly farther from main haul roads than leks that recruited the same number of males as expected."

In other words, only leks near the drilling rigs were affected and males from those leks tended to move to leks farther from active development. These missing males did not die off and the population did not crash, no negative demographic effect on the population was found. BLM cannot rely on studies that purport to document a negative effect (i.e. Holloran 2005), yet consistently fail to do produce data that show such a negative effect.

There has been no decline in the GRSG population in the Pinedale Planning Area (Upper Green River Basin). Instead, data and analyses performed by the Wyoming Department of Game and Fish reveal that between 1990 and 2012 there has been a consistent increase as measured by male lek attendance and male density per square mile. Wyoming Game and Fish 2012. The information relied upon by the NTT Report is simply wrong.

Holloran et al. 2010.[13] This study is cited four times in the NTT Report for the proposition that avoidance leads to population declines. As discussed extensively above, local avoidance does not, as the authors speculate, equate to population declines.

Johnson et al. 2011.[14] This study is cited three times in the NTT Report in the context of negative impacts being measured great distance from leks, suggesting support of four-mile buffers, and 3 percent disturbance threshold.

Regarding professional judgment and the 3% anthropogenic disturbance threshold, the cited studies (Johnson et al. 2011, and Naugle et al. 2011a, b) are not as definitive as claimed in the NTT Report with regards to susceptibility of sage grouse to either discrete or diffuse disturbance. First, Johnson et al. 2011 utilized extremely weak statistical inference and there are simply not enough years of data to reliably support inferences with single variables, much less multiple variables analyses produced by Johnson et al. 2011 are not reliable statistical inferences and it is hard to imagine that such a weak paper was ever published. The authors examined 62 different predictor variables, using only 11 years of lek count data for the response variable, in seven different sage grouse management zones. Reliability was further compounded by the fact that 37% of the lek counts used by Johnson et al. (2011), had *only four years of data* associated with them. As a result, Johnson et al. 2011 is an example of an extremely weak approach to statistical inference and a poorly planned "data-fishing expedition."

There are simply not enough years of data to support inferences with single variables, much less several variables, and certainly not the 62 variables studied by Johnson et al. (2011). The study only reported Pearson correlation coefficients (r), rather than r2 and its significance, which is not common practice and illustrates the lack of meaningful signal in the data. The scatterplot figures illustrate the main result: that there are no significant correlations between predictor and response variables. Instead, there were random clouds of points. The authors resorted to LOESS smoothing in an attempt to identify potential patterns in the data that did not otherwise have any statistical significance. LOESS smoothing allows one to portray a pattern or trend, where none exists.

Despite the obvious issues (discussed above), the authors reported on "trends" and discussed the potential importance of these. The fact that Johnson is employed by the USGS raises questions about the independence of this paper. Two of the NTT members (D. Naugle and S. Knick) were also authors on USGS GRSGS Monograph (where this study was published) and S. Knick was one of its editors. This raises issues about the lack of independence of the NTT Report and the validity of the scientific information relied upon to formulate its recommendations.

[13] Holloran, M.J., R.C. Kaiser, and W.A. Hubert. 2010. Yearling greater sage-grouse response to energy development in Wyoming. Journal of Wildlife Management 74:65–72.

[14] Johnson, D.H., M.J. Holloran, J.W. Connelly, S.E. Hanser, C.L. Amundson, and S.T. Knick. 2011. Influences of environmental and anthropogenic features on greater sage-grouse populations. Pages 407-450 in S.T. Knick and J.W. Connelly, editors. Greater sage-grouse: ecology and conservation of a landscape species and its habitats. Studies in Avian Biology 38. University of California Press, Berkeley, California, USA.

Kaiser 2006.[15] This study is cited four times between the two reports (NTT/3, COT/1). The NTT Report cites this study in the context that birds avoid habitat that might otherwise be suitable and this thus could lead to lek abandonment. The COT Report wrongly concludes this study proves populations decline as a result of oil fields. Lek avoidance does not equate to population declines.[16] For example, Tack et al. 2011 used satellite global positioning system transmitters to reveal dispersal of GRSG over much greater distances (over 100km and some up to 300 km) and more frequently than previously thought. Accordingly, GRSG may fly over or around areas of disturbance.

Kiesecker et al. 2011.[17] The COT Report mis-cites this study. While Kiesecker et al. mention that ecological zoning is an admission that conservation of all habitat is improbable (See p. 167), this is not what the study is about. Kiesecker et al. propose what they believe to be a better way to implement, track, assess the mitigation hierarchy, which they assert will be more effective at conserving key habitat, while "allowing" continued energy development.

The authors propose that offsets (mitigation) are ecologically equivalent to impacts resulting in net neutral or positive outcomes, but fail to suggest how this would be measured. In addition, the authors fail to recognize the many uncertainties and variables within GRSG habitat. The authors proposed strategy for accounting for offsets will punish those who seek offsets in restoration activities as opposed to protective offsets, by making the cost of restorative offsets more costly. Obviously, this deters any incentive to restore habitat, but it also forces an increase in areas that would be off limits to future development.

As part of the accounting approach when deciding on the appropriate ratio, the probability of success must be determined, as seems reasonable. However, if for example the scientific literature is lacking or uncertain on restoration activities/success, then the cost of offsets will increase. In other words, if a given restoration practice is only marginally successful, this would be favored in the calculation over newer innovative technology because the new methods success odds are not yet proven. This will stifle technological advances in restoration and ultimately would harm habitat by preventing any incentive to try and improve it.

Knick et al. 2003.[18] The very title of this piece evidences extreme bias, "Teetering on the edge or too late?...." This study is cited 15 times between the two reports (NTT/1,

[15] Kaiser, R.C. 2006. Recruitment by greater sage-grouse in association with natural gas development in western Wyoming. M.S. Thesis, University of Wyoming, Laramie, WY. 102pp.

[16] *See* Ramey, Thurley and Ivey 2014.

[17] Kiesecker J.M., H.E. Copeland, B.A. McKenney, A. Pocewicz and K.E. Doherty. 2011. Energy by design: Making mitigation work for conservation and development. Pp. 159-181 *in* D.E. Naugle (ed). Energy Development and Wildlife Conservation in Western North America. Island Press, Washington, D.C. This paper was note cited in the NTT or COT Reports, but has fundamental flaws which weigh against agency reliance upon it in Land Use Plan Amendments or otherwise.

[18] Knick, S.T., D.S. Dobkin, J.T. Rotenberry, M.A. Schroeder, W.M. Vander Haegen, and C. Van Riper III. 2003. Teetering on the edge or too late? Conservation and research issues for avifauna of sagebrush habitats. Condor 105:611-634

COT/14). Both the NTT and COT Reports cite this paper in support of the proposition that sagebrush ecosystems are beyond recovery thresholds and the amount of habitat lost or degraded as a result of human settlement. There is a likely conflict of interest as Schroeder, a co-author is a COT team member, and Knick is an NTT member. While reflected in the NTT and COT Reports for mistaken propositions on hindrances to restoration, this paper represents the opinions of the authors and is not based on actual data or hypothesis testing.

The authors provide no evidence for their assertion that "disruption" leads to the inability to restore habitats, and does not present data to support its assertions that restoration could take decades or centuries. Knick et al.do not indicate whether their assertions are based on passive or active restoration, and most importantly fails to recognize that there are many factors that impact resilience and thus restoration of habitats. Numerous publications suggest that restoration is possible, but it takes active as opposed to passive management.

Furthermore, even if it does take decades or longer to fully recover sagebrush habitats Knick et al. 2003 assumes that GRSG require climax communities in all life stages and seasons, and also assumes that after loss of a patch there is none available for GRSG to disperse to. GRSG can walk and fly and bypass unsuitable habitats for suitable habitat.

Knick et al. 2003 continues to espouse and assume that because not all sagebrush can be effectively restored this equates to an overall lack of effectiveness of restoration efforts. This notion is false. The authors have not quantified the amount of habitat that has reached a change in state that precludes effective restoration, which may be small. There have been numerous studies published as part of the Sagebrush Steppe Treatment Evaluation Project (SageSTEP) that have demonstrated how proper management, and different restoration methods can positively influence resilience in a cost-effective manner.[19] For instance, transplanting sagebrush is significantly more effective and is also

[19] *See* David A. Pyke, Scott E. Shaff, Andrew I. Lindgren, Eugene W. Schupp, Paul S. Doescher, Jeanne C. Chambers, Jeffrey S. Burnham, and Manuela M. Huso (*2014*) Region-Wide Ecological Responses of Arid Wyoming Big Sagebrush Communities to Fuel Treatments. Rangeland Ecology & Management: September 2014, Vol. 67, No. 5, pp. 455-467; Richard F. Miller, Jaime Ratchford, Bruce A. Roundy, Robin J. Tausch, April Hulet, and Jeanne Chambers (*2014*) Response of Conifer-Encroached Shrublands in the Great Basin to Prescribed Fire and Mechanical Treatments. Rangeland Ecology & Management: September 2014, Vol. 67, No. 5, pp. 468-481; Bruce A. Roundy, Richard F. Miller, Robin J. Tausch, Kert Young, April Hulet, Ben Rau, Brad Jessop, Jeanne C. Chambers, and Dennis Eggett (*2014*) Understory Cover Responses to Piñon–Juniper Treatments Across Tree Dominance Gradients in the Great Basin. Rangeland Ecology & Management: September 2014, Vol. 67, No. 5, pp. 482-494; Bruce A. Roundy, Kert Young, Nathan Cline, April Hulet, Richard F. Miller, Robin J. Tausch, Jeanne C. Chambers, and Ben Rau (*2014*) Piñon–Juniper Reduction Increases Soil Water Availability of the Resource Growth Pool. Rangeland Ecology & Management: September 2014, Vol. 67, No. 5, pp. 495-505; Benjamin M. Rau, Jeanne C. Chambers, David A. Pyke, Bruce A. Roundy, Eugene W. Schupp, Paul Doescher, and Todd G. Caldwell (*2014*) Soil Resources Influence Vegetation and Response to Fire and Fire-Surrogate Treatments in Sagebrush-Steppe Ecosystems. Rangeland Ecology & Management: September 2014, Vol. 67, No. 5, pp. 506-521; Richard F. Miller, Jaime Ratchford, Bruce A. Roundy, Robin J. Tausch, April Hulet, and Jeanne Chambers (*2014*) Response of Conifer-Encroached Shrublands in the Great Basin to Prescribed Fire and Mechanical Treatments. Rangeland Ecology & Management: September 2014, Vol.

significantly cheaper than seeding. Others have shown that if seeding is chosen then it is significantly more effective if it is covered with organic matter, and has shown that by seeding with herbs after a fire or pinyon juniper treatment, it can suppress the spread and establishment of cheatgrass.

This paper lacks any useful scientific findings and seems basically a biased call to arms for environmental groups. For example, the authors complain about a lack of political agenda and advocate that public lands be "Protect[ed] from economic use." Other incredibly biased statements include,

> "[O]ur primary challenge, presented over a quarter of a century ago (Braun et al. 1976), may be to convince our society of the intrinsic value of sagebrush ecosystems and their unique biodiversity. This change in mindset will have to be followed by a firm commitment by federal and state agencies to provide the resources necessary to resolve issues presented in this paper. Only with this concerted effort and commitment can we afford to be optimistic about the future of sagebrush ecosystems and their avifauna."

Further, the purpose of this study is said to be to "emphasize the urgency for conservation and research actions, and synthesize existing information…" It is clear that this study was not designed objectively, and it is truly an opinion paper of where the authors perceive there to be gaps in research. There is no hypothesis testing, and no real data presented.

Because this is a review of existing conditions in 2003, the information is outdated. Significant work and conservation efforts have taken place since the publication of this paper which have reduced threats. Throughout the paper the authors make the assertion that habitat has been disturbed or disrupted *"beyond a threshold at which natural recovery is unlikely*" a bold and biased statement which is not supported by data but reflects the opinions of the authors and those they cite, many of which are also outdated.

The authors cite anthropogenic disturbances like mining, grazing, oil and natural gas, and infrastructure as fragmenting and degrading habitat. West and Young 2000 (advocates of listing Gunnison sage) are frequently cited in support of the amount of habitat lost since pre-European settlement and that most of the range is beyond what can be restored. The authors then go on to cite Braun 1997, 1998, a paid consultant to listing proponents and a biased advocate of listing Gunnison and GRSG, as well as Connelly, and Schroeder et al. 2000, in support of their mistaken view of long-term population declines:

- "numbers of sage-grouse (*Centrocercus* spp.) have continued to decline throughout their range (Connelly and Braun 1997, Braun 1998, Connelly, Schroeder, et al. 2000) and individual populations have become increasingly separated (Schroeder,

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67, No. 5, pp. 468-481; Herriman, Kayla R. 2009. Wyoming big sagebrush: Efforts towards development of target plants for restoration. Moscow, ID: University of Idaho. 63 p. Thesis.

Hays, Livingston, et al. 2000, Beck et al. 2003)." This evidences bias and has been refuted by Zink 2014.

- "In addition to the challenge of understanding shrub-steppe bird-habitat dynamics, *conservation of sagebrush landscapes depends on our ability to recognize and communicate their intrinsic value and on our resolve to conserve them*." This shows serious lack of objectivity.

Knick et al. 2011.[20] This lengthy 162-page paper presents another cumulative effects analysis that covers nearly every conceivable deleterious human activity on sagebrush and sage grouse. This study is cited a total of eight times between the two reports (NTT/6, COT/2). The NTT's use of this study in support of the proposition that various anthropogenic disturbances results in population declines is the most problematic. The NTT Report cites it in support of withdrawals, and suggests draconian restrictions are necessary because increased development on private lands is not subject to mitigation and thus there is "greater need for conservation of sage-grouse and sagebrush on public lands (Knick et al. 2011)." NTT Report at 12.

Notably absent from this one-sided analysis is any mention of the effects of hunting harvest, even though this is a major, documented source of sage grouse mortality with 207,430 grouse killed just between 2001 and 2007, and higher annual take in the preceding years. Instead, the authors devote pages of attention to a number of hypothetical effects:

> "Even activities, such as hiking and mountain biking, which often are perceived as low impact or benign, have an influence wildlife (Miller et al. 1998, Taylor and Knight 2003). Any human activity of high frequency along established roads or corridors, whether motorized or non-motorized, can affect wildlife habitats and species negatively through habitat loss and fragmentation, facilitation of exotic plant spread, population displacement or avoidance, establishment of population barriers, or increased human-wildlife encounters that increase wildlife mortality (Gaines et al. 2003). These effects appear to be common across a variety of habitats and species that span the full range of forested to arid terrestrial environments (Gaines et al. 2003, Ouren et al. 2007)."

However, when one looks closely at the cited literature, these supposed population-level effects are speculative. The omission of documented sources of mortality and inclusion of speculative sources, indicate a less than objective analysis.

To quantify the influence of human activities on patterns and processes of sagebrush habitats and sage-grouse populations, the authors rely on the previously designated Sage-Grouse Conservation Area or the pre-settlement distribution of sage-grouse buffered by 50 km (Connelly et al. 200; Schroeder et al. 2004). As noted below in the discussion of

[20] Knick S.T., S.E. Hanser, R.F. Miller, D.A. Pyke, M.J. Wisdom, S.P. Finn, E.T. Rinkes and C.J. Henny. 2011. Ecological Influence and Pathways of Land Use in Sagebrush. Pp. 203-251 in S.T. Knick and J.C. Connelly (editors), Greater Sage-Grouse: ecology and conservation of a landscape species and its habitats. Studies in Avian Biology (vol. 38), University of California Press, Berkley, CA.

Schroeder et al. (2004), the pre-settlement distribution was a subjective assessment of pre-European GRSG distribution that included both habitat and non-habitat, and selectively excluded some areas of documented occupancy. The widening of the pre-settlement range by a 50km "buffer" (by Knick et al.) inflates the size of the area affected by human activities, even though GRSG may have never occurred there.

As with other disturbances in sage grouse habitat, Knick et al. quantify the "effect area" that surrounds any kind of development based on other studies. In the case of oil and natural gas wells, the effect area includes a 3km buffer around each well pad, and the affected area of a pipeline was 3km in total width because of presumed spread of invasive plants (although Table 16 shows in many cases the authors used a higher figure). A 3km effect area was also applied to all transmission lines. These effect areas were applied across the study area, substantially inflating the effects of these activities, even if mitigation, such as conservation offsets, had been implemented. However, the cited paper for oil and natural gas construction (Lyon and Anderson 2003) made no such 3km recommendations. They simply recommended that the BLM regulations in place at the time be "reexamined." Knick also misrepresented cited studies regarding the affected area of roads, pipelines, and transmission lines. For example, the following studies do not support the one-size-fits-all approach as Knick avers:

1) Lyon and Anderson (2003) also reported observations contrary to the one-size-fits-all effect areas used by Knick et al. For example, Lyon and Anderson (2003) (discussed in detail below) reported that:

> “On the Pinedale Mesa, potential disturbances associated with natural gas development were restricted to vehicular traffic on the pre-existing main haul road. All males from the 3 disturbed leks in our study strutted either on or within 15 m of this road. However, the mean number of vehicles using the mesa road in a 24-hour period during spring and summer of 1998 and 1999 was <12.”

2) Instead of reporting a 3km effect area, Bradley and Mustard (2006) reported limited effects from roads and transmission lines:

> “In 2001, cheatgrass was 20% more likely to be found within 3 km of cultivation, 13% more likely to be found within 700 m of a road, and 15% more likely to be found within 1 km of a power line.”

3) Similarly, instead of finding a 3km effect area, Gelbard and Belnap (2003) reported:

> “…we observed anecdotally that sites isolated (1000 m) from roads tended to contain fewer exotic species than sites near (50 m from) road… We found a significant effect of road improvement on both exotic and native species richness in interior communities 50 m beyond the edge of the road cut, suggesting that road improvement influences the distribution of both exotic and native species in lands beyond the influence of roadside disturbance. Exotic species richness tended to be greater and native species richness tended to be lower next to more improved roads,

although we caution that our measurements of richness were a snapshot.”

Knick et al. stated that, “We used an ecological rationale for estimating the area around points, lines, or polygons from which land use potentially influenced land cover or sage-grouse populations. Estimates for effect sizes into surrounding areas were based on foraging movements of human-subsidized predators, distance of exotic plant species spread, or on distribution data relative to land use.” However, because of the misrepresentations detailed above, the other "effect sizes" and "ecological rationale" used by Knick et al. should be closely reexamined.

According to Knick et al. "All nonproprietary and nonsensitive spatial data sets used in our analysis are available for download on the SAGEMAP website http://sagemap.wr.usgs.gov; United States Department of the Interior 2001a). Each data set is accompanied by a metadata record documenting original source and GIS procedures." It is presently unknown how much of the data are proprietary or sensitive.

Further, the NTT cited Knick for the proposition that:

> “Human land use, including tillage agriculture, historic grazing management, energy development, roads and power line infrastructure, and even recreation have contributed both individually and cumulatively to lower numbers of sage grouse across the range (75 FR 13910, Knick et al. 2011).” NTT at 6.

While the above land uses “may” impact GRSG numbers, Knick et al.2011 quantified alleged effects—not population numbers. The NTT Report misrepresents the findings of the study and attempts to tie effect size to population decline which was not tested. Knick et al. do attempt to tie effect size to distribution of GRSG, however, the assumed historical distribution is based on flawed studies which subjectively calculate historic distribution. Other issues include:

- Travel and transportation: “Within the sage grouse range, 95% of the mapped sagebrush habitats are within 2.5 km (1.55 miles) of a mapped road; density of secondary roads exceeds 5 km/km2 (3.1 miles/247 acres) in some regions (Knick et al. 2011)... The effect of roads can be expressed directly through changes in habitat and sage- grouse populations and indirectly through avoidance behavior because of noise created by vehicle traffic (Lyon and Anderson 2003, 75 FR 13910).” NTT at 11. However, the effect size reported/calculated by Knick et al. 2011, is flawed, partially due to their misrepresentation of previous studies.

- Lands and Realty: “In addition, land acquisitions and withdrawals may be important conservation strategies because increased development on private lands, which is not subject to mitigation, will focus greater needs for conservation of sage- grouse and sagebrush on public lands (Knick et al. 2011).” NTT at 12. This is an opinion and is not a tested hypothesis. In addition, it is not accurate to broadly assert private lands are not subject to mitigation. Any activity on private land, for example split-estate lands, or those requiring a Federal permit or approval would be subject to mitigation.

- ROWs: "Sage-grouse may avoid power lines because of increased predation risk (Steenhof et al. 1993, Lammers and Collopy 2007). Power lines effectively influence (direct physical area plus estimated area of effect due to predator movements) at least 39% of the sage-grouse range (Knick et al. 2011)." NTT at 13. However, Knick et al. conceded 50% of GRSG conservation areas are already impacted by power lines. Knick et al. at 213.

- Livestock: "Treatments used to manipulate vegetation ultimately may have far greater effect on sage-grouse through long-term habitat changes rather than direct impacts of grazing itself (Freilich et al. 2003, Knick et al. 2011)." NTT at 14. This is only loosely supported. See discussion at 228-230.

- Urbanization: "Conversion of sagebrush habitats for agriculture, the expanding human populations in the western United States and the resulting urban development in sagebrush habitats." NTT at 14. Neither Knick nor the NTT provide evidence that urbanization impacts but a small fraction of sagebrush habitat nor that it is a current issue.

Knick, S.T. and S.E. Hanser. 2011.[21] This study is cited a total of 14 times between the two documents (NTT/6, COT/8). The NTT cites this study in the context of the importance of connectivity, which wrongly assumes that sage-grouse cannot bypass unsuitable habitat, but rather habitat must be expansive and connected. This notion has been refuted by Tack et al. 2011 and Ramey, Thurley and Ivey 2014. This study wrongly assumes disturbance equates to declines in population persistence.

A fundamental problem with the Knick and Hanser analysis is that lek persistence data are used in lieu of actual population data, and the analysis rests on the critical assumption that population persistence and lek persistence are strongly correlated. For example, if leks had simply moved because of disturbance (e.g. fire) then the analysis would treat the lek as extirpated when the subpopulation birds that comprise it were not extirpated.

Although the data were originally at a 30m resolution, the authors resampled at a 540m resolution, claiming that they "were able to detect relatively fine-scale patterns at this resolution when considered at the spatial extent of the SGCA **[sage grouse conservation area]**." The authors do not acknowledge that this rescaling could be expected to inflate the effects of disturbance.

The authors' belief that "little is known about the connectivity and ability for spatially

[21] Connecting pattern and process in greater sage-grouse populations and sagebrush landscapes. Pp. 383-406 *in* S.T. Knick and J.W. Connelly (eds). Greater Sage-Grouse: ecology and conservation of a landscape species and its habitats. Studies in Avian Biology (vol. 38). University of California Press, Berkeley, CA

structured populations to exchange individuals," is contrary to the abundant field and genetic data showing ongoing long distance dispersal (>18km). (This aspect is discussed extensively in the reviews of Chapter 16 of this monograph, Garton et al.)

The authors were "unable to identify a specific source of human disturbance because the score represented a summed influence of all anthropogenic features." Thus, they concluded that "the cumulative effect of human activities may have a greater influence on persistence of sage-grouse populations than single land uses." This ignores the relative influence (effect size) of specific types of disturbance on sage grouse populations and assumes that they all contribute to sage grouse decline, when in fact some do not. This is not a sound epistemological basis for informed management decisions.

A more robust analysis would include a logistic regression approach to model population presence/absence. If lek presence/absence data were substituted, then the analysis could only refer to factors leading to the extirpation of leks, and that would best be done at a more limited, regional scale (e.g. sage grouse management zone). Results would be compared to a range wide analysis. Ideally, the variables selected for analysis should be winnowed down on the basis of plausible cause and effect mechanisms, and those likely to have the largest effect sizes. In that way, variables can be treated as testable hypotheses.

Leu and Hanser 2011.[22] This study is cited three times in the COT Report in support that fragmentation is the primary cause of population declines/primary threat, and that sage-grouse avoid anthropogenic disturbances opposed to natural disturbances. This is something that Knick 2013 (the study which claims disturbance should be limited to less than 3%) wrongly suggests.

This paper utilizes a complex spatial analysis to predict impact of the "human footprint" on sagebrush habitat (termed "sagebrush landscape" by the authors). This is the same approach used previously to describe the "human footprint" across the west, by two of the same authors as Leu et al. (2008). The third author of Leu et al. (2008), is Knick, also an editor and frequent contributor to this sage grouse monograph.

The paper contains considerable jargon, making a comprehensive read a time-consuming task.

The model used to study the "human footprint" is dependent upon the inputs of other models, but the error associated with these inputs, and their effect on results, were not addressed by Leu and Hanser. Use of the terms "error," "uncertainty," and "confidence interval" are absent from this paper. The authors did not appear to us statistical methods that deal with stochastic variation to estimate the magnitude of the error variance and propagate it through to the confidence intervals.

[22] Leu, M. and S.E. Hanser Influences of the human footprint on the sagebrush landscape patterns: implications for sage-grouse conservation. Pp. 253-272 *in* S.T. Knick and J.W. Connelly (eds). Greater Sage-Grouse: ecology and conservation of a landscape species and its habitats. Studies in Avian Biology (vol. 38). University of California Press, Berkeley, CA.

The significance of this paper lies in its likely utilization by the USFWS for a range wide or regional "cumulative effects analysis" of various human land uses and activities on sage grouse. Therefore, a more in-depth review of this paper may be desirable. The authors describe their approach as: "The cumulative effects of human actions on landscapes, the human footprint, can be delineated as the physical and/or ecological human footprint."

In this paper, as with Leu et al. (2008) no hypotheses are tested. Instead, the authors rely on a *post hoc* interpretation of results and make recommendations derived from their complex spatial analysis. That paper interprets the results using a descriptive, story-telling approach. The authors recommend that certain landscapes in a given human footprint class be "carefully evaluated," although the criteria by which such an evaluation would be objectively conducted is not described. The results are deemed supportive of those obtained by other authors in the monograph, however no criteria were provided that would potentially falsify previous conclusions. The authors believe raven control to be ineffective and suggest that all future transmission lines follow existing high impact corridors, an expensive proposition to be based on surmise.

The size of the affected area surrounding each type of land use was developed from one or few studies, and applied across the range of sage grouse. This is a questionable one-size-fits-all approach to quantifying potential disturbance. For example, the corvid (e.g. raven, crow, and magpie) and domestic cat and dog predator risk models (regressions of probability of occurrance vs. distance from human habitations) were based on extremely limited data (4, 2, and 3 data points respectively) and with no tests of significance or confidence intervals. Such poorly supported inferences cannot be viewed as reliable. (The impact of oil and gas wells is treated as a disturbance area around fixed points and their supporting infrastructure (roads and transmission lines) is quantified separately.) The authors provided a handful of citations including an unpublished masters thesis in support of data used to develop input models.

The authors analysis rests on the use of fractals (as opposed to Euclidean geometry) and modeled artificial landscapes, to summarize aspects of habitat fragmentation, including patch shape, edge, and size in terms of *lacunarity*. A concise definition of *lacunarity* used in ecology may be found in Halley et al. (2004):

> In general terms, however, lacunarity is an index of texture or heterogeneity [of a fractal object]. Highly lacunar objects possess large gaps or low-density holes, while low-lacunarity objects appear homogeneous. Thus, for example, in observations of vegetation cover using quadrats, lacunarity is low if we find very similar levels of cover in every quadrat (Plotnick et al. 1993). More precise definition of lacunarity has been problematic.

Leu and Hanser's rationale for using this method is as follows:

> We analyzed artificial landscapes due to the lack of previous research evaluating

> lacunarity in natural landscapes demarcated by convoluted patch boundaries and to aid interpretation of lacunarity analyses from natural landscapes (Elkie and Rempel 2001).
>
> Lacunarity has several advantages over other more common fixed-scale landscape metrics because it consists of a single metric evaluated at multiple scales, is not influenced by edge effects, nor restricted to landscapes with high occurrence of habitat of interest (Plotnick et al. 1993). Lacunarity metrics can also be used to assess degree of relative fragmentation across diverse landscapes (Wu et al. 2000).
>
> Despite its ease in calculation, lacunarity analyses have been rarely used to study patterns of natural landscapes (but see Wu et al. 2000, Derner and Wu 2001, Elkie and Rempel 2001) perhaps, because interpretation of lacunarity curves can be difficult. However, we found that using lacunarity analyses of simulated landscapes, where degree of fragmentation and proportion of land cover reflect the range of values of landscapes studied, greatly aids in the interpretation of lacunarity functions of landscape patterns.

Other authors have raised issues as to whether these models accurately represent real-world situations, and the conditions under which its use may be questionable. The uses and abuses of fractals in ecology are thoroughly discussed in Halley et al. (2004).

The original paper (Leu et al. 2008), a general description of the approach used in this paper, and data appendicies may be found at the following websites:

- http://www.esapubs.org/archive/appl/A018/039/default.htm

http://sagemap.wr.usgs.gov/HumanFootprint.aspx

Lyon and Anderson 2003.[23] This study is cited seven times in the NTT Report in alleged support of four-mile NSO buffers and 3% surface disturbance thresholds based on the erroneous assumption that a temporary disturbance of sage grouse from a local area under development equates to a population decline.

Lyon and Anderson's (2003) data were inadequate for: 1) achieving statistical significance in comparisons of nest initiation and nest success in disturbed versus undisturbed areas, and 2) demonstrating a population decline. Instead, the presumed biological significance of their statistically insignificant results were based upon belief, as the following excerpt from Lyon and Anderson (2003) shows:

> "Finally, even though nest initiation between disturbed and undisturbed hens was not statistically significant, we believe lower initiation rates for disturbed hens were biologically significant and could result in lower overall sage grouse productivity."

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[23] Lyon, A.G. and S.H. Anderson. 2003. Potential gas development impacts on sage grouse nest initiation and movement. Wildlife Society Bulletin 31: 486-491.

Lyon and Anderson (2003) also stated that,

> "Hens captured on disturbed leks demonstrated greater movements from capture lek to nest than hens from undisturbed leks. Hens from disturbed leks nested approximately twice as far from capture leks as did hens from undisturbed leks. Our random nest vegetation analysis indicated no significant differences in nesting habitat between disturbed and undisturbed areas, suggesting that nest habitat was not influencing sage grouse hen movements."

This is expected, as animals that are disturbed by human activity will sometimes move away from it. However, it does not mean that the result will be a population decline.

Naugle et al. 2011a.[24] This study is one of twelve chapters found in the publication, "Energy development and wildlife conservation in western North America" (Island Press, hereinafter Naugle's Book). This chapter is cited three times in the NTT Report and once in the COT Report for the proposition that cumulative impacts of mineral development leads to landscape level impacts on GRSG. Naugle acted as the editor of his own book and authored or co-authored three of the chapters.

Correspondence between Naugle and BLM official and NTT Team Leader Raul Morales obtained via FOIA also evidence a lack of objectivity on the part of Naugle. Further evidence of bias comes from the introductory chapter of Naugle's book itself: "Everyone has a stake in the future of the West. The *world* expects the historical West to retain its wildness and wildlife, even if only a fraction of those people ever come to see it. The mere knowledge of its existence is a comfort." Naugle and Copland (2011) at 6.[25]

This quote is representative of the uniform and continued bias of the main contributors to the book, whom are also frequently cited in both the NTT and COT Reports. Their personal bias against land use and oil and natural gas has adversely influenced the design, interpretation, and ultimately the conclusions resulting in a lack of objectivity. Moreover, the fact that the BLM is the primary funding source not only for the entire book but specifically for this paper, and the primary author and editor, Naugle, is an NTT member, shows a clear conflict of interest.

The authors conclude: "[F]oregoing development in priority landscapes is the obvious approach to conserve large populations." *Id.* at 70. This notion is legally impossible without an Act of Congress given the multiple-use mandates of BLM and the US Forest Service. Moreover, this policy conclusion is based on opinion rather than science or data. These facts alone should disqualify this incredibly biased and one-sided work.

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[24] Naugle, D.E., K.E. Doherty, B.L. Walker, H.E. Copeland, M.J. Holloran, and J.D. Tack. 2011a. Sage-grouse and cumulative impacts of energy development. Pages 55-70 in D.E. Naugle, editor. Energy development and wildlife conservation in western North America. Island Press, Washington, D.C., USA.

[25] Naugle and Copland. 2011. Introduction to Energy Development in the West. Pp. 3-6 *in* D.E. Naugle (ed). Energy Development and Wildlife Conservation in Western North America. Island Press, Washington, D.C.

Incredibly, the NTT Report appears to adopt pages of this chapter verbatim on pp. 19-21 without any attribution or citation.

This study cherry-picks studies and interprets them to support the authors' pre-ordained conclusions. The authors selectively reviewed and reinterpreted studies, many of which were authored by co-authors on this publication, that they "feel" are important while blatantly excluding others, such as Ramey, Brown and Blackgoat 2011.[26] Their premise that impacts from energy development are "universally negative and typically severe" and cause population declines is fundamentally and deeply flawed.

In this study, the authors wildly surmise that "world demand" for energy will lead to "unprecedented pressure" on wildlife in the western U.S. The authors assume inappropriately that conservation must be equal to the amount of development, which is not based on data or tested to determine if it is reasonable, achievable, or necessary. Instead it is based on the opinion of the authors, not based in science.

Here, the authors "synthesized" 14 studies which looked at the effects of energy development on GRSG using various methods described in Naugle et al 2011b[27] in the GRSG Monograph. This chapter is really just another publishing of the Naugle et al. 2011b. The authors of this paper are the same authors of 2011b monograph chapter with the exception of Tack.

The authors did not conduct an objective review of the literature. Eleven of the studies were conducted either in the Pinedale Anticline, Powder River Basin, or Alberta. None of these study areas are representative of conditions range-wide, and instead represent studies with areas of intensive energy development using technology of yesteryear. Recent publications such as Ramey, Brown and Blackgoat 2011, Kirol et al. in preparation,[28] and Applegate and Owens 2014[29] demonstrate that with improved technological advances, resource management, and best management practices, sage-grouse have responded positively to mitigation and other conservation efforts. Further, eight of the 14 papers are journal articles, dissertations, and theses of the chapter's authors. Of the remaining studies, there appears to be significant misrepresentation.

Of the studies included in this chapter, only eight had data for six or more years. Kaiser 2006 for example had just one year of data and an extremely small sample size. This leads to limited confidence in the findings of this study, such as lack of trend data, discussed elsewhere in this challenge.

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[26] Ramey, R.R., L. Brown, F. Blackgoat 2011.

[27] Naugle, D.E., K.E. Doherty, B.L. Walker, M.J. Holloran, and H.E. Copeland. 2011b. Energy development and greater sage-grouse. Pages. 489-503 in S.T. Knick and J.W. Connelly, editors. Greater sage-grouse: ecology and conservation of a landscape species and its habitats. Studies in Avian Biology 38. University of California Press, Berkeley, California, USA.

[28] Kirol C.P., A.L. Sutphin, L. Bond, M.R. Fuller, T.L. Maechtle. Mitigation effectiveness for improving productivity of greater sage-grouse nesting in natural gas development areas. International Sage-Grouse Forum, Salt Lake City, Utah (November 13-14, 2014).

[29] Applegate D., N. Owens. 2014. Oil and gas impacts on Wyoming's sagegrouse: summarizing the past and predicting the foreseeable future. Human–Wildlife Interactions 8(2):284–290

A key objective of the study is to provide landscape-level recommendations. The authors state, "the size of sage-grouse breeding populations is often used as an indicator of the overall health of sagebrush ecosystems." Naugle et al. 2011a at 56 citing Hanser and Knick 2011. The results of Hanser and Knick 2011 appear to be misrepresented in Naugle et al 2011a. Hanser and Knick evaluated whether treating GRSG as an umbrella species would benefit other species that use sagebrush to varying degrees. They did not estimate whether the GRSG would be an effective umbrella for the other 95% of the species that use sagebrush environments. Thus, to surmise that GRSG health is an indicator of ecosystem health is an overly narrow view in light of the remaining 95% of species present in the sagebrush biome.

One of the most egregious conclusions of this study is the false assumption that "sage-grouse populations decline when birds avoid infrastructure in one or more seasons." *Id.* at 61, *citing* Doherty et al 2008; Carpenter et al 2010. Among other issues, the findings of these studies were misrepresented in Naugle et al 2011a. Doherty and Carpenter examined impacts on winter habitat selection. They did not model or collect data on population decline/persistence. The negative impacts that the authors report are based on the assumption that avoidance leads to population declines. This mistaken concept has never been tested or proven; it is purely speculative.

The authors also assume that loss of a lek near energy development equates to a population decline and that the birds then cease to reproduce all together. Instead it could be hypothesized that the birds simply relocate/move to other leks or a new lek might form somewhere else. *See* Tack et al. 2011. Lek data obtained from Wyoming Game and Fish indicates that populations are stable to increasing, which refutes the notion that GRSG are undergoing population decline as a result of avoidance to energy infrastructure.

Citations for the false assumption of population declines of Holloran and Anderson 2005, Aldridge and Boyce 2007 and Holloran et al. 2010 are misplaced as described elsewhere herein. *Id.* at 62. And the notion that site fidelity combined with disturbance leads to declines based upon Yoder et al. 2004 is also suspect. The Yoder study was conducted on ruffed grouse and addressed how predation is a function of movement or dispersal. Reliance on Holloran 2005 at pp. 62-63 is also misplaced. As discussed herein, the alarmist population predictions made in Holloran 2005 failed to come true.

Notwithstanding the above, at least the authors recognize, "[T]ools to manage sage-grouse populations will vary across the species range with biotic and abiotic characteristics of different landscapes and local constraints to populations." *Id.* at 68. This statement undermines the NTT and COT Reports' one-size-fits-all approach.

Patricelli et al. 2010. This study was one of the first of its kind in attempting to discern potential effects of noise on GRSG. However, it was fraught with errors in documentation of methods, lack of data, assumptions, and erroneous interpretation of results. Clearly lacking was any involvement by professional acousticians, or use of professional data collection and reporting standards in the industry. As a result, the study cannot be viewed as anything more than preliminary and cannot be used as the basis of regulations.

The cited studies provided no evidence of GRSG population declines as the result of anthropogenic sound produced by the oil and gas industry.

Only a transient period of disturbance to GRSG at leks where the playbacks occurred was observed. There was no data reported that the levels of fecal corticosteroid metabolites in male GRSG at the affected leks had resulted in reduced fitness (e.g. decreased reproductive capabilities and/or decreased survivorship that have led to any detectable population decline in the study area). Rather, population trends in male lek attendance and density in the study area (Upper Green River Basin portion of the Wyoming basin population) have been consistently above state average and increasing since 1990 (data from Wyoming Game and Fish 2013).

The data used are not public and results are not reproducible. No data were reported from: 1) objectively-measured noise generated during various phases of drilling activities, 2) noise generated during production, 3) road noise, or 4) the occurrence of these over a 24 hour period. No data were reported on the environmental parameters under which any data were collected, or the ambient sound levels in the study area based upon professional standards (which include wind). Instead, the authors cited "unpublished data" and speculation about the accuracy of their playback noise levels, in support of their claims (emphasis in **bold** below):

> "We played drilling noise and road noise on leks at 70 dB(F) sound pressure level (unweighted decibels) measured 16 m directly in front of the speakers (Fig. 1 & Supporting Information). **This is similar to** noise levels measured approximately 400 m from drilling rigs and main access roads in Pinedale, Wyoming (J.L.B and G.L.P., **unpublished data**).
>
> "To minimize disturbance, we took propagation measurements during the day. Daytime ambient noise levels are typically 5-10 dBA higher than those in the early morning (J.L.B and G.L.P., **unpublished data**) and **are likely higher** than those heard by birds at a lek."
>
> "For leks treated with drilling noise, recordings from 3 drilling sites were spliced into a 13-minute mp3 file that played on continuous repeat. On leks treated with road noise we randomly interspersed mp3 recordings of 56 semi trailers and 61 light trucks with 170 30-second silent files to simulate average levels of traffic on an access road (Holloran 2005). Noise playback on experimental leks continued throughout April in 2006, from mid February or early March through late April in 2007, and from late February through late April in 2008. **We played back noise on leks 24 hours/day because noise from deep natural-gas drilling and vehicular traffic is present at all times."**

(emphasis added). There was no data presented that the playback sound was an accurate rendition of actual frequencies and sound pressure levels from oil and gas operations as measured at set-back distances required by the BLM, or that it occurred at the same levels 24 hours a day. Instead, the authors relied upon "unpublished data" or speculation.

While a 0.25 mile buffer has been the minimum set back distance required by the BLM, most oil and gas operations are found at far greater distances from leks (Wyoming Oil and Gas Conservation Commission well data and Wyoming Game and Fish lek count and location data). Thus, the reported effects on GRSG were biased in the cited studies to achieve a negative response rather than measure responses from sound pressure levels as they would occur at the required set back distances.

Pyke, D.A. 2011.[30] This study is cited four times in the NTT Report and five times in the COT Report. The use of this study is more concerning in the COT Report as the COT Report mis-cites Pyke for the misplaced assumption that restoration is very difficult across the landscape and that 4,000ha are required to sustain a population. The mistaken notion that restoration is too difficult and thus habitat should be left undisturbed is addressed elsewhere herein.

Pyke discusses both the pros and cons of grazing, and discusses when grazing might be a benefit and when reduction or removal would be better. Pyke discusses appropriately that grazing is not black and white and that appropriate grazing during certain times of the year may maximize the benefits in reducing invasive species. Pyke 2011 at 538-539.

Pyke is quite clear that several factors must be considered prior to making any decision on how or whether to restore/rehabilitate. To make the above assertion is premature. Pyke also indicates that depending on the habitat component that needs restoring, it may only take 3-5 years, in some instances.

There is extensive research being conducted on restoration and treatment of degraded sagebrush habitats.[31] Several factors influence the efficacy of restoration including soil moisture, elevation, seed mix, type pre-treatment/disturbance conditions, distance between stands, to name a few. To say that restoration is impossible is inaccurate because there is very little long-term data available on sites where active restoration activities exceeded the typical three year emergency stabilization and rehabilitation (ES&R) policies of land management agencies. Moreover, it has been common practice to defer to passive management if the first re-seeding event failed. Moreover, GRSG habitat parameters at these older ES&R sites were not necessarily considered. Thus, what long-term data is available is not representative of newer restoration techniques and technologies.[32] On the other hand, short-term studies show that with the right

[30] Restoring and rehabilitating sagebrush habitats. Pp. 531-548 *in* S.T. Knick and J.W. Connelly (eds). Greater Sage-Grouse: ecology and conservation of a landscape species and its habitats. Studies in Avian Biology (vol. 38). University of California Press, Berkeley, CA.
[31] *See* footnote 25, *infra*.
[32] Id.

type/mixture of management of habitats, tailored to the specific needs of the plot undergoing restoration, habitats do respond positively.[33]

Finally, with regards to restoration generally and after fire, Pyke describes the reason for difficulty seems to be land manager choices in seed mix. It follows that it is inaccurate to claim that restoration is impossible as suggested in the COT Report. The primary recommendation made in Pyke is that native seed mixes should be used whenever possible, with the use of non-native seeds when there is limited native seed mix available. If land managers simply shifted their seed mixture to that of natives implemented known BMPs that increases seed establishment, and the land was managed to control invasives, then it could be hypothesized that the restoration would be more successful. The conclusion to draw from Pyke is that current restoration practices are flawed, not that restoration is not possible and therefore, habitat must be left undisturbed.

Schroeder et al. 2004.[34] This study was cited once in the NTT Report, six (6) times in the COT Report and 18 times in the FWS' 2010 listing decision. Contrary to the position presented in Schroder et al. 2004, the pre-European distribution of GRSG is far more uncertain—particularly where the historic record is an incomplete estimate at best. The author's estimate of the potential habitat of sage grouse from pre-European settlement through the present based on historic distribution maps, museum records, published accounts, and other information was neither well explained nor objective. Beyond the limitations of an incomplete historic record, GRSG have a much broader habitat tolerance than asserted in this study. GRSG can and have lived in riparian meadows, agricultural land, steppe dominated by native grasses and forbs, shrub willow, and sagebrush habitat with conifer and aspen trees. This study subjectively excludes observations and specimens outside of sagebrush habitat to claim that GRSG are confined to and wholly dependent on sagebrush territory. The author failed to acknowledge the inherent uncertainty associated with such blanket statements after reviewing limited data along with a fragmented and questionable historic record. These serious flaws make reproducibility nearly impossible.

The pre-settlement habitat extends from 1400 to 1850 A.D., and encompasses the Little Ice Age (which averaged .5-.9 degrees Celsius lower than current temperatures). Since the weather was colder, and more arid, vegetation and climate were simply different than in modern times. Natural fluctuations in climate have a profound impact on sagebrush habitat.

Reliance on Kuchler's (1964) PNV for historic climate estimates is similarly problematic as they are qualitative, generalized descriptions of vegetations communities which are not suitable for extrapolation. Kuchler's PNV models could be characterized as "informed guesswork," and a summary of prevalent opinion as to the likely ecological status of many different types of American vegetation, but not as "a reliable predictive tool." Schroder also failed to account for the effect of Native Americans on sage grouse and their habitat prior to 1800. Native American populations are estimated between 40 and

[33]Arkle et al. 2014.
[34] Schroeder et al. 2004, Distribution of sage-grouse in North America, Condor 106:363-376.

112 million people prior to European contact, most of whom lived in temperate regions. Native Americans hunted sage-grouse, and started range fires to improve edible forage and game. These and other highly disruptive activities had an unquantifiable but assured impact on GRSG populations.

Walker, et al. 2007.[35] This paper is cited eight times in the NTT Report and two times in the COT Report in support of lek buffers, impacts to habitat selection, and the proposition that energy development leads to declines in population numbers. However, neither the NTT nor the COT Report mentions the methodological issues with these studies or the fact that none reported a population-level decline in sage grouse rather than a localized effect on rates of male lek attendance near the disturbance.[36]

The authors concede speculation as the premise for their alleged conclusions even in the abstract of the paper. The paper suffers from subjective interpretation of results where no hypothesis testing was used. Avoidance of disturbance is not uniform among locations as the authors suggest. Rather, it can be site-dependent for factors such as density of development and age of the oil and natural gas field impact of oil and natural gas operations on GRSG is not as clear-cut nor as negative as the authors of this paper and the NTT report claims.[37]

In addition, data show that GRSG behavior can be affected by certain types of anthropogenic disturbance more than others, which can result in localized avoidance, but the effect of any of these disturbances or development on migration rates is unknown. Data from Lyon (2000), Bush (2009), Tack et al. (2011), and more recent papers, all reveal that GRSG traverse (fly) over or around roads, agricultural areas, and oil and natural gas development, at distances up to 300 km from their natal leks.

However, the author advocates for disturbance caps, yet the authors did not <u>test</u> any percent disturbance caps. Instead they modeled response in lek attendance in terms of <u>distance(s)</u> from potential sources of disturbance. Therefore, Walker et al.'s (2007) support for a 3% disturbance cap, represents nothing more than the opinions of the authors.

While citing this paper for support for its proposed four-mile buffers and 3% disturbance caps, the NTT Report fails to recognize any of the methodological issues with Walker et al, or the fact that none reported a population-level decline in GRSG rather than a localized effect on rates of male lek attendance near the disturbance.
Walker et al. (2007) used model selection procedures that were not statistically reliable because they used nine predictor variables, with just nine years of data, to compare 19 models, in an attempt to identify combinations of predictor variables that would potentially explain patterns in the data. However, for model selection to work properly,

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[35] Greater sage-grouse population response to energy development and habitat loss. Journal of Wildlife Management 71:2644-2654**.**
[36] *See* Ramey, Thurley and Ivey 2014.
[37] *See* Harju et al. 2010; Taylor et al. 2010; Ramey, Brown and Blackgoat 2011. The

the number of predictor variables must be smaller in comparison to the number of observations, in this case, the number of years of data.

Additionally, for model selection to be scientifically defensible, the predictor variables are best narrowed down in advance based on plausible cause and effect mechanisms and tests for independence among variables, procedures that Walker et al. (2007) did not employ. Finally, the results of Walker et al. (2007) were confounded by the obvious location of at least nout of 35 inactive leks immediately adjacent to Highway 14, Highway16, and Interstate 90. Walker et al. (2007) is not a scientifically sound basis for precise predictions about GRSG population responses.