Response to 2009 *Lepidium Papilliferum* (LEPA) Status Review Questions

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Prepared 4/8/2009, revised 4/16/2009 to correct grammatical errors and to incorporate inclusion of the Sullivan and Nations "final revised" report dated 4/13/2009 and the Zwartjes (USFWS) report dated 4/9/2009.

Other than question headers, text revisions are presented in underline and original text is presented in strikethrough font. (Note: The bolded underlined percentages in Question 8's reply were bolded underlined in my original 4/8/09 version)

I have thoroughly reviewed all materials provided on the USFWS CD mailed to panel members. Additionally, I have also thoroughly reviewed the following materials: GIS analysis report by Stoner (1/30/09), final revised Sullivan and Nations report ~~(3/31/09)~~ (4/13/09), revised Popovich report (3/14/09, formerly Wells and Popovich), revised Robertson and White report (2/16/09), and certain public comments posted on the comment web site, including the 3/30/2006 Brackett Livestock, Inc. comments (FWS-R1-ES-2008-0096-0015) and those by Western Watershed.

I also compiled six pages of personal notes in reviewing the above data sources.

My reply follows below to questions on USFWS letterhead mailed to panel members dated 2/10/09 and with subject line: *Slickspot peppergrass: Review of Scientific Literature*.

My answers are based on review of the above data, personal knowledge of this taxon, and my best professional judgment in consideration of the data at hand.

Question 1, Trend

I am in agreement that the revised Sullivan and Nations data show a highly statistically significant downward trend for LEPA on OTA both including and excluding the Study 4 Site. I think confidence is high in this determination *as applied to the data at hand*. However, I am not confident that the model data convince me that a true downward trend over longer time periods would occur or is to be anticipated. I feel that it has now been established that annual variation in LEPA is correlated in various ways with precipitation and temperature, and those correlations and differences in correlations found between various reports are adequately summarized in the revised Sullivan and Nations report. Even with this confounding layer of correlation to climatic variables, which is to be rather expected with vernal annuals/biennials, whatever that correlation may be, I do believe that the downward trend model is tracking a real change independent of tracking weather. However, the trend is not strong, and I am not convinced that LEPA numbers ~~may yet increase~~ will continue to decrease in outyears at the study plots. This is based on

the large annual variation in part tied to weather, and that the direct downward trend established is not profoundly downward (i.e., the slope is not steep). I think a few more years of data (2 minimum) would further strengthen the data in that *if* a substantial trend or at least a continued downward trend is produced in the next few years, that would serve to elevate my confidence that a true downward trend is in fact occurring at the OTA monitored sites. If data in outyears showed departure form downward trend, then that would weaken confidence. The above discussion pertains to data *directly* showing downward trend. I believe, however, that other cumulative data must be concluded to *indirectly* show downward trend over the last 50 years. See reply to Question 5 for discussion.

Question 2, Trend applied to different parts of LEPA range

I feel that the OTA "rough census" data are the best *direct* trend data we have for the taxon, and agree with the report's statement (p. 3) that "The long-term nature of the data from OTA makes these data the best available data when attempting to model trends through time." However, I do not feel it is appropriate to extrapolate OTA trend data beyond the local Snake River Plain populations in proximity to OTA. I feel there are too many variables at play, including differences in local climatic drivers (as presented in the Sullivan and Nations report), differing livestock use levels, and differing habitat conditions, to allow confidence in applying ~~OTS~~ OTA trend to populations beyond the local area. I do not agree with their conclusions (p. 3) that "The comparison of the HIP program on the OTA with that off the OTA suggests that the fluctuations observed on the OTA are similar to those seen off the OTA. Although the rangewide monitoring has not been conducted for as long as the monitoring on the OTA, the trend observed on the OTA is likely representative of the trend across the entire range." I feel that those comparisons, though perhaps valid, do not justify extrapolation of trend at OTA to other locations. Rather, trend at other locations should be based on local data. If local data support a downward trend, then those data should be used.

I do not feel there are sufficient data for sufficient numbers of years in areas beyond local OTA to determine those areas' trends *directly*. This does not mean that a downward trend range-wide is *not occurring*, only that the extrapolation of OTA data to infer range-wide direct trend ~~data~~ would not be recommended.

Question 3, Trend correlated with spring precipitation

I feel that it has now been established that annual variation in LEPA is correlated in various ways with precipitation and temperature, and feel those correlations and differences in correlations found between various reports are adequately summarized in the final revised Sullivan and Nations report. There is contradiction and differences between various reports' conclusions. I think what is important is that there ~~is~~ seems to be clear establishment of correlation between fall-winter and spring, or fall-winter or spring, precipitation and temperature and LEPA abundance. I agree with the Sullivan and Nations report (p. 135) statement ~~"Temperature and precipitation in the longest running data is important in predicting LEPA abundance but the nature of importance changes from fall to spring."~~ "The results of the analyses from Chapter 4 indicate that both

peppergrass, but the nature of the importance changes from fall through winter and into spring." I am very confident that correlation exists, but the exact descriptor or best model to use seems to vary depending upon report and data set.

The Zwartjes report shows that regression analysis of precipitation monitoring data from the OTA does not indicate any significant trend in precipitation over the years 1991-2007, yet the Sullivan and Nations data show a decline in the density of LEPA on the rough census monitoring areas of the OTA observed over the years 1990-2008. This is important because it shows that downward trend over time in LEPA density may be occurring *independent* of possible correlation with local weather (also see Nations' comments on pages 16-17 of the Zwartjes report). Thus, the argument could be made that downward trend is not reflecting a causal relationship with or correlation to weather.

Question 4, Data consistency

There are numerous differences in the various data results and reports. I feel most differences are related to comparing different data sets and different data with differing collection methodologies, the comparisons of which may not be appropriate. Some are as follows: Sullivan and Nations states that LEPA abundance is lower in slickspots that had previously burned, and that fire has (with "limited evidence") a negative short-term effect, and "no indication" of a short-term positive effect. The Popovich report contradicts this in one sense by showing that within the first two growing seasons following fire LEPA can be more abundant than in unburned areas (e.g., site S052215G), but supports this by showing that in general over time and with large sample sizes, sites that had burned in the past (i.e., "native bunchgrass sites") have fewer LEPA plants than unburned sites (i.e., "sagebrush sites"). They state that sagebrush abundance had a positive relationship with LEPA abundance only on the Snake River Plain, while Popovich data show that the most abundance is also clearly found with sagebrush sites on the Owyhee plateau population. They state no correlation found with crested wheatgrass presence in Owyhee populations, but Popovich shows a strong decrease there in plant abundance in crested wheatgrass seedings (which also often burned prior to seeding) relative to unseeded sites. They state no relationship exists between cheatgrass and LEPA abundance on the Owyhee plateau but the Popovich report shows lower LEPA abundance with greater weediness, with a principle weed being cheatgrass. They state that there may not be sufficient cheatgrass on the Owyhee plateau to impact LEPA. I feel that cheatgrass presence in the areas studied by Popovich is quite strong and most definitely present enough to be exerting influence to LEPA sites.

They state that cow prints are greatest in the Owyhee sites (I cannot compare these print levels to other populations across its range), but that trampling cover averages <6%. The Popovich data show >6% cover, on average, in sites from study years 2000-2002. They state that there is no "cumulative" livestock impact on LEPA abundance, while Popovich found one year (2000) of significant negative correlation to % livestock trampling cover at alpha = .05 and a negative correlation across all three years of data (2000-2002) significant at alpha = .101. Popovich further finds significant models of cattle trampling

and distance to water, and LEPA abundance decreasing with ~~increased~~ decreased distance to stock water sources, which clearly implies a livestock trampling negative effect.

It should be noted that I feel locations of the HII/HIP study plots in the Owyhee plateau do not adequately capture the range of variation or representation of LEPA site characteristics or of geospatial distribution of LEPA sites across that metapopulation, and that small sample size of those plots is constraining in results repeatability or their extrapolation to other places in the Owyhee plateau.

I feel that the Popovich report and supporting data carry the most confidence in quality and in results interpretation for the factors it addressed in the Owyhee populations; that is, for vegetation type, seeded vs. unseeded sites, rosette contribution to LEPA abundance, site quality, weed presence, Agsp-Stth presence, and livestock correlations to populations in the Owyhee plateau. The reasons are, relative to HII/HIP or other local studies:

- large sample sizes (n) across much of the core range of the Owyhee population
- three sequential years of data that are generally consistent between years even though different populations were assessed between years
- rigor and reliability of data field collection methods
- first-time use of geospatial analyses for linking distance to livestock water sources to LEPA densities (implying livestock use impacts)
- statistical rigor used and general statistical significance at alpha = .05 (in some instances at greater alpha levels)

When comparing the Popovich report results to other reports that have differing results, I feel the Popovich large sample sizes and broadness of sites in terms of adequacy of vegetation type representation and geospatial dispersion, as well as analysis rigor and technique, should be compared to other local studies to arrive at choosing which results may carry more weight.

I feel that there is significant new data and results in the Popovich report. Principle examples are the comparison of LEPA abundance in sagebrush sites vs. native bunchgrass sites (i.e., burned in the past but unseeded) vs. seeded sites; unseeded (sagebrush and burned sites) vs. seeded sites, and livestock use. This is the first paper to my knowledge to report on a relationship between % cover of cattle trampling and distance to livestock water source in context to LEPA abundance, and distance to water source and LEPA abundance. I feel these correlations may show important relationships to livestock use levels that are not presented or presented as fully elsewhere. ~~They~~ These correlations are in concert with the Sullivan and Nations report statement (p. 123) that "there is very limited evidence that slickspot peppergrass abundance declines with increases in cover of livestock footprints and feces, when these relationships are examined within each region and each year of the HIP survey;" except that the relationships in the Popovich report may in fact be stronger evidence than the "very limited" evidence presented in Sullivan and Nations. ~~They~~ The Sullivan and Nations

_____ also noted that the mean cover of livestock disturbance variables was greatest in the Owyhee plateau.

In other reports from other populations, in general I feel that the revised Sullivan and Nations report is probably most reliable in overall conclusions and data confidence. I feel the Menke and Kaye report contains weaknesses in their data selection and conclusions about correlation with spring precipitation as discussed in the Unnasch report (p. 16) and as influenced heavily by the influential precipitation data year 1998. I feel the Unnasch and Sullivan and Nations reports are probably more thorough and current in analyses of precipitation and temperature correlations than the Menke and Kaye report.

I do not place confidence in the Salo paper, as the criteria for selection for "trigger" thresholds and statistical methods and rigor are not addressed. I therefore cannot evaluate the report's results or conclusions.

Question 5, Reliable projection

I think the final revised Sullivan and Nations paper adequately presents a weak downward trend at OTA through 2008. I am not confident that this projects a downward trend into the future of LEPA abundance at OTA or range-wide. I do not feel other data sets such as range-wide HII/HIP plots convincingly *directly* show downward trend. I do not feel the data in the Popovich report *directly* show downward trend.

Having said the above, however, I *do* feel that substantial downward trend, could be and probably is in actuality occurring, and that reliable population trajectory can be confidently projected. It may well be that data collection to date simply does not successfully capture or allow detection of downward trend, or can only hint at it due to clouding and variance by confounding factors.

I believe that cumulative data *must* be concluded to *indirectly* show a downward trend over the last 50 years and a downward projection into the future. This argument is speculative in nature since it is not based on *direct* trend data.

First, the Popovich report found that sagebrush sites and native bunchgrass sites exhibited significantly greater numbers of total plants than sites seeded to crested wheatgrass. This was consistent across all years 2000-2002. Sagebrush sites exhibited about 9X (read "about nine times") the amount of total plants as compared to seeded sites in 2000, while native bunchgrass sites exhibited about 7X as many total plants as seeded sites. Native bunchgrass sites had mostly burned in the past and were not returning at time of survey to sagebrush, and contained fewer plants than sagebrush sites (which could have very old sagebrush plants or also may have burned in the past but were now dominated by adult sagebrush). Sullivan and Nations reported that the weighted number of fires at a LEPA site was significantly correlated to fewer LEPA plants.

Second, in the Popovich report, unseeded sites (includes both sagebrush sites and burned-unseeded sites) exhibited significantly greater numbers of total plants than seeded sites.

This was consistent across all years 2000-2002. Unseeded sites exhibited about 8.4X the amount of total plants as compared to seeded sites in 2000, about 2.9X in 2001, and about 1.8X in 2002.

Third, in the Popovich ~~2009~~ report, the seeded bunchgrass vegetation type was most commonly encountered in the 2002 survey year. However, although it represented 73% of all sites, it represented only 58% of all plants encountered in the survey. The sagebrush vegetation type represented 15% of all sites but 24% of total plants. Similarly, the native bunchgrass vegetation type represented 12% of all sites but 19% of total plants. There is an unequal contribution to total plants found in the entire survey area by each vegetation type, with sagebrush sites and native bunchgrass sites contributing 9% and 7% more plants relative to their number of sites, respectively, while seeded sites contributed minus 15% to total plants relative to its number of sites.

Forth, the Popovich report found that "good" quality sites exhibited about 2.4X the amount of total plants as compared to "poor" quality sites, and about 1.3X the amount of total plants as compared to "fair" quality sites.

Fifth, the Popovich report found that sites with abundant weeds exhibited 74% of the total LEPA plants exhibited by sites with few weeds, or 26% fewer total LEPA plants than sites with few weeds.

Sixth, the Popovich report found that sites with low % cover of cattle trampling averaged across all years 2X more total plants than sites with high trampling.

Seventh, the Popovich report found a significant relationship between total LEPA plants and effective distance to stock water, with plant abundance increasing with increased distance form water. This result has not been documented elsewhere to my knowledge.

Eighth, Roberston and White found increased seed predation in areas dominated by bluegrass relative to areas containing a sagebrush component.

If ~~you~~ one considers all these above patterns across the relatively large sample sizes of the Owyhee plateau sites and across three survey years, and in combination of similar patterns in the other literature reviewed, there is repeatable and irrefutable data that sites with sagebrush and fewer disturbances contain more plants than seeded sites or sites with more disturbances by one or multiple disturbance factors. I feel there is enough evidence to say that the sagebrush sites can serve as "before disturbance" sites and the disturbed sites as "after disturbance" sites. This indirectly can serve as a surrogate for monitoring the same site over time that may undergo a disturbance. One can then confidently conclude that, in general and across the sampled landscape, sites that have been disturbed or seeded support fewer plants currently than they did prior to disturbance. This is a speculative conclusion that cannot be proved, but I believe nonetheless that much confidence can be placed in it.

The great majority of these disturbances occurred within the last 50 years. **Therefore, one can conclude that there has been a marked downward trend in the last 50 years to the many sites that have received these disturbances over pre-disturbance LEPA abundance levels.** The amount of downward trend at a particular site cannot be known with these data, but the average differences in LEPA abundances in the above discussion points between sagebrush sites and disturbed sites can be substantial (for example, up to 9 times more plants in sagebrush sites than sites seeded to crested wheatgrass were observed in 2000, and up to about twice as much in 2002, which had a much greater sample size).

Also, there are about 9 sites that have been documented as being extirpated, which could be a further indirect or alternate indicator of downward trend in areas where those sites occurred. Finally, note that the Sullivan and Nations report states (p. 3) that "No factor showed a consistently positive impact on slickspot peppergrass."

Even though I feel this is compelling albeit indirect evidence of past downward trend, perhaps establishing irrefutable proof of direct downward trend is not the most effective measure of long-term viability or threat projections. I feel that perhaps a more important question in determining long-term viability of this taxon is "Would the observed current and expected reduced levels of plants at a site and across the range due to habitat alterations and grazing management anticipated in the foreseeable future to all or nearly all sites be large enough to maintain minimum viable populations capable of seedbank turnover and resilient to adverse pressures and stochastic events?" An additional component would be "Would these minimum viable populations be interconnected enough to allow sufficient gene flow to maintain viability in the long-term?"

I feel that a very real and substantial reduction in plants or occupied sites over the last 50 years, and thus downward trend, *has occurred* and *is occurring* annually, but I cannot *directly* prove so with the data available, and as my best professional judgment is based on *indirect* cumulative data, it could therefore be construed to be speculative in nature and not directly supported by the data.

I do not rely too heavily on results borne only with "statistical significance." That is only a tool, and alpha = .05 can be hard if not impossible to achieve in biological data containing much variance and confounding unaccountable factors, which is exactly the case with this taxon. It may be more meaningful to focus on repeatability of pattern with favor being given to larger sample sizes, and the congruence or similarities of pattern reported in different studies by different authors in different places of the LEPA range, even if ~~he~~ the patterns are not "statistically significant at alpha = .05."

To me, it is important in the lack of unequivocal data to make conclusions based upon a preponderance of the evidence, the cumulative data and the most supported patterns. In the case of this taxon, I think the above patterns in their summation offer indirect evidence for downward trend over the last 50 years, when most habitat alteration, wildfires, seedings, and weed invasions have occurred, and, for the Owyhee plateau LEPA sites, the great majority of cattle trampling.

Question 6, Primary threats

I feel that wildfire, conversion to non-native bunchgrass seedings, and livestock trampling are the greatest threats to population viability. I feel that weed invasion and persistence is also highly important. Weed increase following fire and with increased livestock use, as well as seed predation by ants as reported in Roberston and White being more common in non-sagebrush sites, may exasperate the situation to a profound degree. I think that the correlation of LEPA abundance in the Popovich report with sagebrush sites being greatest in LEPA abundance followed by burned-unseeded followed by much lower LEPA abundance with seeded rangelands shows a cause-and-effect relationship that burned-unseeded habitats and especially seedings result in lower LEPA abundance. I think the sequential relationships in the Popovich report of trampling increasing with ~~increasing~~ decreasing distance to water source and LEPA abundance ~~also~~ decreasing with ~~increasing~~ decreasing distance to water source show a cause-and-effect negative relationship of cattle trampling to LEPA abundance. I have extremely high confidence in the adverse impacts or causal agent of crested wheatgrass seedings to LEPA abundance and medium to high confidence in cattle trampling as a causal agent. I say "medium to high" for trampling data rather than "certainly high" only because the relationship and Rsquare were not strong, but I feel in reality the relationship is probably quite strong but ~~it~~ is simply difficult to detect in the data due to many other confounding factors that serve to increase variance and mask the relationship. Although I cannot illustrate the exact mechanisms responsible as the causal agents, I think the strength of these patterns goes beyond simple correlation and represents true causality. In other words, "on average, if you do this treatment to the habitat, you will see a reduced number of LEPA over time at the site."

Question 7, Different threats in different places

I can best speak to threats for the Owyhee sites. There, I feel that the dominance of conversion to non-native seedings and heavy localized cattle use on slickspots are relatively high when compared to the reported levels and that I have observed in other LEPA populations in the Snake River Plain and Boise Front. I am confident these are threats and that they are occurring.

Question 8, Predicting time for threats

Based solely on the downward trend model for OTA "rough census" data presented in Sullivan and Nations, one could extrapolate the number of years until LEPA plants reach zero. As previously stated, however, I do not feel that the model should be extrapolated to sites range-wide. In Question 5, I conclude that downward trend is indirectly shown.

But, the more important question than absolute proof of downward trend is "Do the reduced levels of LEPA abundance *currently observed* in altered habitats such as burned-seeded sites (that may also be weedy and trampled by cattle) represent levels that are unviable in the future?"

Assuming that habitat alteration (wildfire, seedings, weed invasions, etc.) and livestock use would continue at current levels, and that the observed results currently experienced may represent LEPA populations that are in undetected downward trend and are in fact unviable in the long-term (i.e., sites in current seedings used by livestock that are weedy = unviable populations in the long-term), then I think it is more informative to assess timelines of when all or near all of the extant sites would reach a state of altered habitat that renders them unviable.

The Stoner report shows (Table 6, p. 48) that 11,441 acres of LEPA area out of 15,749 total acres of LEPA area, or **73%**, burned from 1957 to 2007. It also shows (Table 9, p. 70) that 2,029 of 5,060 burned acres, or **40%** of total acreage burned from 1984 to 2005, had a burn severity of moderate to severe. This severity causes more serious sagebrush-steppe plant mortality and habitat alteration than low severity (commonly accepted statement by fire ecologists). Based on burn return interval and burn size data with the Idaho BLM, one could model how long it would take before all or nearly all of the remaining unburned LEPA sites burn.

In the Popovich 2009 report, the seeded bunchgrass vegetation type was most commonly encountered, and contained the most plants overall, reflecting the dominance (**73%**) of this vegetation type across the surveyed landscape. Map 11 of the Popovich 2002 survey report shows much suitable habitat converted to seedings. Significantly more area has burned in large fires ~~since~~ in the vicinity since the creation of this map. One could similarly model how long it would take for the burned areas to become seeded.

Once these habitat alterations have been realized, then it would be concluded that it is a matter of time before the altered sites reach unviable plant abundances. The amount of time seems less important than the conclusion that the downward path is inevitable and extirpation will occur.

Question 9, Synergistic effects

I feel that is the synergistic effects that collectively pose the greatest threats to this taxon. Wildfires followed by seedings and/or invasion of weedy plants, overlain with livestock trampling, and repeating this scenario over time, cumulatively can lead to a demise in impacted population. I think the degree of observed fire, weeds, seedings, and grazing being among the majority of occupied (and unoccupied) sites rather than these variables being small in scale and isolated, serves to adversely impact plants and sites over time.

Question 10, Strengths or vulnerabilities

I think that the annual/biennial life form and small, delicate stature makes this taxon more susceptible to physical damage to trampling or seeding, and less able to fend off invasion by weedy species. Its life form makes the seedbank the real key to long-term viability, and the seed bank is much more prone to physical disturbances like plow-and-drill seedings and cattle trampling because of the susceptibility in turn to physical disturbance to the seedbank's habitat, which is the slickspot itself. The slickspot habitat by nature is

much more susceptible to adverse physical and chemical disturbances than other soil types. The seedbank is also susceptible to predation. Because the taxon must ~~relay~~ rely on seedbank germination and subsequent successful seed set to replenish the seedbank and turn over in the future, any agent that serves to reduce successful seedbank retention and turnover can harm long-term viability. This is in stark contrast to, for example, a plant that reproduces vegetatively and can form mats or is rhizomatous and that occurs in rock cracks or soil that is more able to tolerate disturbance like trampling or seeding and resist weedy invasion. Due to these vulnerabilities being impacted by current and anticipated on-going maintained or increased levels of disturbances, this increases the projection of a downward trajectory.

Question 11, Factors for maintenance

Maintaining 1) sagebrush steppe habitat with presence of sagebrush, 2) little or no weeds or trampling, and 3) connectivity for gene flow would be optimal for maintaining viability and allowing this taxon to persist into the future. I don't think these situations can be effected in the foreseeable future due to anticipated habitat alteration and fragmentation, and anticipated levels of livestock use.

Question 12, Additional relevant data

I think that other public comments that state that in the Owyhee plateau "the more we look, the more LEPA we find" are misleading in implying that there are many more LEPA plants there. This is very probably not true based on past surveys. The 2002 Popovich surveys examined many areas that contained no LEPA plants; in fact, several large survey blocks contained no plants at all (see for example Map 11 of the 2002 report). The Popovich 2000, 2001 and 2002 reports address the topic of how many more areas may contain plants in the Owyhee plateau, the circumscription of the Owyhee population range, and which soil types account for what proportion of total plants. In those reports, a limited number of soil types have been shown to contain the majority of plants. One can see how much of those soils types remain unsurveyed or have already been surveyed but yielded no plants. These data are available. By far the most promising areas in the Inside Desert and adjacent areas of even marginally-suitable habitat have been intentionally surveyed.

I think that more consideration should be given to the fact that in the Popovich Owyhee plateau surveys, a great many occurrences of unoccupied sites have been surveyed (i.e., there is much negative data) in seeded areas (see for example Map 11 of the 2002 report) that would appear to have at one time been capable of supporting plants by line of evidence and reasoning that adjacent unburned sagebrush islands do contain occupied slickspots. The only difference appears to be treatment (a burn followed by seeding, or a seeding for range improvement). This is speculative in nature but founded on fair observation.

Data for the "second line of evidence" section in the Popovich 2009 report were not presented, but they are available.

Also related to the Popovich report, there are undocumented data concerning site S052215G, which had a large number of rosettes expressed immediately following wildfire. These were heavily trampled later in the same season due to heavy localized cattle use, which was witnessed by Popovich and his crew. The numbers of plants at this site the previous year prior to wildfire (year 1), the number of plants following wildfire in year 2, and the number of plants immediately following the cattle use event have been documented but have not been presented together. These data are available.

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