

July 22, 2019

Comments on the Brahney atmospheric nutrient loading white paper

1. This is a very nice compilation of the relevant field observations for the Great Basin and Utah Lake regions. The white paper goes into quite a bit of detail on the important points and it is very well written. I applaud the author for completing this white paper so quickly.

Thank you.

2. It would be helpful if the author included a table with summary data providing a more global context to atmospheric nutrient loading. At last week's ULWQS panel meeting, Janice Brahney mentioned that there are several recent review papers on atmospheric nutrient loading for other regions of the world. It would be quite useful if the white paper included a table that summarized the results of these reviews for other regions of the world.

I agree with the reviewer and have included a table summarizing atmospheric nutrient loading in other regions, please see Table 1 on page 6.

3. The white paper should also include the raw data summarized in Table 4-5 of the Olsen (2018) Masters thesis. The "uncontaminated" Olsen samples have similar outcomes as summarized in the Brahney white paper. The contaminated Olsen samples are ≈ 50 times higher than the uncontaminated Olsen samples. The Brahney white paper should briefly discuss (e.g., one paragraph) this contamination issue (adult chironomids?) and place this in the context of other atmospheric nutrient loading estimates for the Great Basin/Utah Lake and typical QA/QC procedures for estimating atmospheric nutrient loading.

I agree that the uncontaminated samples from Olsen et al 2018 provide a "more reasonable" estimate (13.4 annual tonnes to Utah Lake as calculated from 8.01 tons in 31 weeks, provided), however I cannot include their data in our tables because they neither provide a dust deposition rate nor a dust P concentrations. They only provide estimated weekly loading rates in tons to Utah Lake *after* kriging, as such I included a reference to the Olsen result on page 18 where we present analogous data. I agree contamination issues should be discussed and included more detail in the Recommendations section. However, I elected to remove the paragraph discussing the Olsen contamination issue in detail because I do not wish to single out a student who was not advised by credentialed atmospheric deposition experts, the limitations in his study were not his fault. Further, I believe even though the chironomid samples were removed that the Olsen samples are still contaminated, as Scott Daly points out, chironomids were not the only source of contamination to the Olsen samplers. The Olsen samplers were placed near roads, open pit mining operations, housing developments, and central pivot irrigation systems that used ditch water. While these may, to some extent, produce dust that is delivered to the lake, the study design did not allow for a reasonable extrapolation of these sources, which may be limited to less than a few 100 m's of influence. To address all reviewer concerns (including comments below)

Commented [SD1]: During the meeting we discussed the need for sampling to be conducted using approved methods and sampling design. The Olsen samples were collected w/ samplers that do not meet the requirements discussed by Janice in her paper and the SP during the meeting. Is it appropriate to reference the uncontaminated samples in context of her other regional results given how the samples were collected?

This point does highlight the fact that more attention should be given to the methods used for calculating attenuation for regional and local sources. For example, if Janice calculates loading using the Olsen number, but her result is 10x lower, I assume the difference is in how dust is attenuated to the lake. Perhaps she could provide a review of relevant approaches for calculating loading to a lake of this size for both regional (attenuation of 1) and local sources.

- I added some more sentences on the need to avoid midge contamination in the recommendations section.
- I make it more explicit that Recommendation 1 would allow for the accurate determination of point sources and their area of influence
- I include the Olsen estimate in our recommended value.

I agree with Scott Daly's comment. I believe the approach taken by the Lake Tahoe study is the most appropriate method to determine attenuation across a lake in various atmospheric conditions. However, as Dr. Miller points out below – this is largely uncharted territory in the atmospheric deposition world, otherwise we would have many more studies on large lakes to draw from. I have included more details within the Recommendations section of the white paper and hope to clarify concerns about the length of such a study (it would collect data year round from multiple points AND provide attenuation estimates). It seems the main point of contention between myself and Dr. Miller's team is the determination of attenuation. Olsen used a kriging method, which is also a potentially viable method - however, both methods are only estimates of what could be empirically measured using the methods outlined in Recommendation 1 only as this is the only way to directly measure the phenomena. Any other method is only an estimate.

4. The white paper should also include a summary table for the different Great Basin/Utah Lake atmospheric loading estimates. I counted nine different observations of dust deposition in Tables 1 and 4 (e.g., Goodman et al. in review, Scholz et al. 2019, Munroe 2014, Brahney Uinta East unpublished, etc.).

This request is unclear to me. Utah Lake (urban) dust loading rates are provided in Table 1. Great Basin (regional loading rates) are provided in Table 4. I include Colorado, because it receives dust from the Great Basin. Similarly Urban P deposition rates are provided in Table 3. Regional P deposition rates were calculated as described in the text and there is not enough data to make a table. It is not necessarily possible to link every deposition estimate with a phosphorus estimates since often these two parameters are not provided in tandem and estimate is based on all data, not some data. There are many estimates of dust deposition locally and regionally and many measurements of P concentrations in regional dusts, but these data were not necessarily linked.

5. These dust deposition rates should be combined with their most likely TP and TN content to generate an analogous summary table of nutrient deposition for the Great Basin/Utah Lake region. This table should include the uncontaminated results from Olsen (2018). This compilation of atmospheric nutrient loading estimates should for now be used to characterize the boundary conditions for the Utah Lake biogeochemistry model being developed by the University of Utah.

As above, this data is not necessarily available to put in table form. In particular the Olsen data, which only provides loading rates in tonnes *post* kriging. The two tables presented provide dust

loading rates and their citation and then P concentrations and their citation, I'm not sure I see the value in combing them but perhaps I have misunderstood the reviewer.

6. I carefully read the two recommended atmospheric deposition sampling schemes. Given that we all agree that these estimates should cover the entire annual period I found the first sampling scheme to be impractical since one cannot be continuously on Utah Lake.

I apologize for the confusion here. The first would in fact provide continuous shorelines measurements but also provide attenuation estimates during different times of day (land-breezes for example) as well as seasonal differences. They can be both portable and stationary thus providing similar, but superior data to option 2, as well as estimates of attenuation.

7. The passive strategy described in the second recommendation makes much more sense to me because that can be left out in the field continuously for a year or more and samples collected every few weeks. This passive strategy should include a few well-defined constraints and be as simple as possible. E.g.,

- a) Be a certain distance off the ground (e.g., 2 m?) so that it does not collect large particles that are only transported short distances.
- b) Place a mesh (e.g., 150 μ m) above the sampler to keep bugs out.
- c) Do not sample close to obvious point source.
- d) Sample at least 5 (?) different locations.
- e) Conduct the sampling for at least one full year (or more?).

As above

8. Table 5 (Phosphate concentrations from regional.....)

It would be great to include the %P of the samples for context. For example, the Average at the bottom is 0.078% P which is less than the Earth's crust average of 0.12%. There should be a sentence stating the Earth's crust average for context (0.12% of dry mass is P). Typical organic matter is 1% P, therefore numbers higher than 0.12% beg the question of organic matter enrichment or contamination (or mineral rock P which can be easily measured in the regional context).

This is a great point. I included this information and that for phosphate rock in Utah on page 12.

9. It would feel less personal if the word "I" was removed from the document. This will require slight reworking of a few sentences.

All references to "I" Have been removed

Commented [FA2]: It is my understanding that the vacuum sampler, or boat sampling, would be utilized to measure the 'regional' dust deposition and to calculate an extinction coefficient for the urban, or 'local', dust load to the lake.

My understanding is that we already have the regional dust estimates based off AD in the Wasatch Mountains and potentially snow samples.

Commented [FA3]: Janice addressed these issues in the 'WFWQC workplan memo' sent out yesterday.

This comment should be included in the workplan memo.

Other changes made

Table 5 – Originally used an average Great Basin and Colorado dust P concentration for regional P concentration estimates (0.78). However, upon reflection it made more sense to just use the values from the Wasatch Front, where concentrations range from 0.57 to 1.15 mg/g with a mean of 0.9mg/g. This increases our median estimate for P loading, altering estimates in tons in Table 11, which now has median values at 3.5, 5.9, and 6.7 tons.

N Deposition.

Instead of only providing a range of estimates for the last few years of data, I provided the total loading estimates for the full year of 2017.

Reviewer 4

Given that this is an unconventional review format, the responses will be given equally within an unconventional format. Responses are provided throughout the text, highlighted in blue.

The use of Lake Tahoe as a reasonable analogue to Utah Lake by the author of the white paper is problematic and unfounded. While it is about 20% larger than Utah Lake, this is the only comparable metric. At 1600+ ft deep it is the 2nd deepest lake in the US. Utah Lake is the shallowest lake of anything near its size in the US, with a mean depth at 6 ft or less.

The reviewer misunderstands the analogy. The depth of the two lakes is irrelevant given that this is an atmospheric study, the Lake Tahoe study remains the only comparable study examining particulate attenuation rates across a large lake. The fact that the lakes have large surface areas is the only comparison being made in the white paper.

The population surrounding Lake Tahoe is about 30,000. The population of Utah Valley is about 575,000. There are no agricultural areas around Lake Tahoe while about 30% of the surrounding land use of Utah Lake is agricultural.

As above. The use of the Lake Tahoe analogue is strictly related to air mass movement across a large lake, we do not use loading rates determined at Lake Tahoe, which would be inappropriate, in fact the larger population of Utah Valley likely accounts for the larger P deposition rates that we did use in this study, making this a moot point by the reviewer.

The Lake Tahoe surface elevation is at about 6225 ft and occupies the entirety of its alpine valley. The surface elevation of Utah Lake is about 4490 ft and occupies a tiny portion of the ancient Lake Bonneville footprint.

This is a fair point in that geographical (not historical) differences may influence wind speeds and direction across the lake surface, which may in turn influence the transport distances. One might hypothesize then that wind speeds are different around each lake, however a quick internet search indicates that average winds speeds around both lakes are similar. This uncertainty, however, is explicitly stated in the manuscript and exactly why the manuscript discusses the limitation of using Lake Tahoe, and instead uses the results found at Lake Tahoe and multiplies them by 2 and 4 “just in case”. Please see Page 18 “*Because we only had one study to draw from*

and local winds surrounding Utah Lake are likely different than those observed at Lake Tahoe (Figure 3), we included two more estimates at 2 and 4 times the radius of influence observed in Lake Tahoe.”

The only other analogue is that the prevailing wind comes out of the Southwest. In addition, one of the most important differences between Lake Tahoe and Utah Lake is that directly upwind of Lake Tahoe is the >9000-ft backbone of the high Sierra mountains that separate it from the agricultural areas of California’s Central Valley, about 100 miles away with an average elevation of about 100 ft above sea level. Thus, any dust deposition on Lake Tahoe has to surmount mountains that are 9,000 feet higher than the dust source.

The prevailing winds around Utah Lake are not from the southwest. This would be a valid point IF we used deposition rates associated with Lake Tahoe, however, we did not, we used deposition rates local to Utah Lake.

Moreover, temperature inversions aloft inhibit vertical mixing between air that may transport material over the Sierra Nevada and the surface air within the Tahoe Basin (Dolislager et al. 2014). In contrast, the prevailing southwesterly winds cross Utah Lake and hit the “wall” of the Wasatch mountains that are 9,000 to >11,000 ft high. This may serve to trap and circulate airborne material within Utah and Salt Lake valleys, facilitating particle deposition.

While I appreciate the imagination here and these processes may in fact occur, there are no published sources to draw from. The reviewer has appeared to ‘miss the point’ of the white paper, which is to use existing published data to make a reasonable educated estimate, identify gaps, and provide recommendations on how to sample in this situation. The paper is not to definitively say what the deposition rate is, which is why it provides instead a gigantic range of possibilities, nor does it make any claims about having the final answer on the loading rates. If it did, what would be the point of a recommendations on sampling section? The purpose of the paper is explicitly stated in the abstract and in the background and scope section.

In addition, at only 200 ft above the elevation of Great Salt Lake, Utah Lake is a tiny remnant of prehistoric Lake Bonneville, which was also a terminal lake for several thousand years – enriching the sediments with nutrients throughout its 20,000 square mile (52,000 square km) surface area. Moreover, much of the geology in the watershed of lake Bonneville/Utah Lake/Great Salt Lake was and is sedimentary, rich in P deposits, much of which now sits in the sediments of the Lake Bonneville footprint. This area was also an ancient shallow sea several million years prior to Lake Bonneville with sea bed sediments rich in nutrients accumulating several hundreds of meters, if not thicker. Presently, for thousands of square hectares and 50 miles directly west of Utah Lake, lies the Great Salt Lake Desert. This vast playa, along with several other large P-rich playas, still remain in the lake Bonneville footprint and are experiencing active wind erosion, one of which is Severe Lake, which is 1.5 times the size of Utah Lake and located only 100 miles upwind and 200 ft above the elevation of Utah Lake. For comparison, Lake Tahoe sits in a granitic basin, very poor in adjacent geologic phosphorus and other than relatively minor urban development around the lake, the surrounding landscape of Lake Tahoe is forested with no agricultural disturbances.

The point the reviewer is trying to make is not clear. The entire paper is about calculating the dust mediated transfer of P to Utah Lake, it explicitly considers dust events and P enriched sediments. Again, the reviewer appears to have missed the point of the analogy and ignores the explicated statement in the paper that there is no 'true' analogy and thus we considered multiples of 2 and 4 based on what was observed in Lake Tahoe. Please see Page 18 "*Because we only had one study to draw from and local winds surrounding Utah Lake are different than those observed at Lake Tahoe (Figure 3), we included two more estimates at 2 and 4 times the radius of influence observed in Lake Tahoe.*"

Total P in recent soil samples from the Severe Lake are near or above 2000 mg/kg. These are near the P concentrations in soils around Utah Lake and Great Salt Lake as well as in sediment core samples (unpublished data; see Abu-Hmeidan et al. 2018). While we are currently confirming P concentrations and solubility in various chemical extractions in these samples, they are likely to be much greater than the 10.9 ug of other dust samples collected in the wet depositional samples cited in the white paper.

This assertion is incorrect. The P content of soils of Severe Lake are not disputed and given the high phosphate content of rock around the Uinta Basin (19 000 mg/kg), the values from this region are especially relevant. Dusts from around the Uinta Region range in concentration from 2800 to 5000 mg/kg as they are of mixed origin and not pure phosphate rock are still much higher than Sevier Lake soils, yet wet P deposition rates in the region are only 13.5 ug L. Nevertheless, our average northern Utah estimate of 10.9 ug/L still represents the only source of peer-reviewed citable data for wet P deposition in northern Utah and give the rationale I just provided, seems more than reasonable.

The four NADP sites identified in Utah in the white paper are all downwind and up-elevation from Utah Lake and the Great Basin in general and do not represent dust samples entering or being produced in Utah Valley. This ignores or contradicts introductory information in the white paper that states: "*At a global scale, dust deposition rates range from 0 to 450 g m⁻² y⁻¹ with the highest deposition rates influenced by proximal (<10 km) sources (Lawrence and Neff 2009)*". Thus, to ignore local sources, particularly when it is recognized that soil disturbance from agriculture and urban development are key sources of local AD is likely ignoring important sources to Utah Lake

Incorrect, Bryce Canyon and Canyonlands are not 'downwind' of Utah Lake. In addition, NADP samplers are not meant to represent dust produced in Utah Valley nor are they treated that way in the white paper, which is why Urban (Utah Valley) and Regional (NADP) samples were kept independent in the white paper. As discussed in the paper, regional deposition rates and urban deposition rates (as well as concentrations of nutrients) should be kept separate because there zone of influence is inherently different. Again, it is well noted in the white paper that the zone of influence for particulates generated in Utah Valley are not known, and thus we multiply the only example we have by 2 and 4 times. We welcome the reviewer to take reasonable steps to determine the zone of influence, lest these arguments can easily get turned around when additional effort fail to include actual empirical measurements of point source attenuation.

The reviewer has miss-read the publication, there is no "contradiction". We use and cite much higher deposition rates in Utah urban locations (Utah Valley) as compared to the regional

deposition rates. To repeat, we use local urban deposition rates in our estimates. Please see Table 3, the determined rates are comparable to other Urban centers – even those in deserts (See Soudi Arabia).

For reference, the 450 g m⁻² yr⁻¹ sample was taken from a glacial outwash plain, where abundant unconsolidated material are available for wind entrainment. This is substantively different than the semi-arid soils of Utah.

The white paper states:

Samplers should not be located near point sources of atmospheric pollution including but not limited to open pit mining and housing developments on the western shores of Utah Lake (Figure 2), agricultural areas where either soils (Figure 4b) or irrigation spray can reach the sampler, construction sites or roads in urban areas (Figure 2, Figure 4c,d). Point sampling methods do not allow one to determine the zone of influence and thus contamination of samples by point sources can lead to erroneous extrapolation and inflation of actual deposition rates.

None of these points made below are disputed by the white paper and it appears as if the reviewer is conflating multiple ideas in an attempt to be contradictory.

As cited above (Lawrence and Neff 2009), excluding these proximal (<10 km) are ignoring potentially important sources to Utah Lake.

Well then it is a good thing we are explicitly included local sources in our estimates.

For example, the NADP Guidelines are focused exclusively on region-wide deposition. As such, guideline methods are only specific to wet deposition and the objective is to evaluate regional/national deposition – not local targets such as Utah Lake. As such, there are no guidelines for dry deposition other than that for the AMoN (atmospheric ammonia) samplers and there are no guidelines for phosphorus AD sampling.

The reviewer contradicts himself, the dry guidelines for dry deposition of Ammonia are dry deposition guidelines, and therefore NADP does have explicit guidelines for dry deposition. The NADP does not YET have guidelines for P deposition, however, they are currently under development by this author.

Yes, NADP is concerned with region deposition. Local contamination is avoided so as not to over extrapolate the effect of a point source. Similarly, this should not be done with Utah Lake.

This is why we present a solution with an extremely high resolution that could capture each point source and determine the attenuation coefficient. Again, if the reviewer wishes to produce a study that would improve estimates of nutrient loading it must include an empirical measurement of attenuation. This answer largely applies to the rest of the paragraph.

This suggests that dry deposition methods are in their infancy. This further suggests that there are additional variables or questions or even study objectives encountered in dry deposition sampling projects that were not dealt with in the previous methods developed for wet deposition, mercury or ammonia deposition. More specifically, this indicates that specific questions, such as deposition on single targeted sites, such as Utah Lake need to be uniquely developed to answer these specific questions. Such questions revolve around local/near-field vs far-field/regional vs global sources. Moreover, there may be near-field important sources such as continued development as well as emissions from existing urban zones, intense agriculture or a local gravel quarry. Because these anthropogenic activities disturb high P soils immediately adjacent to Utah Lake, they need to be considered, not ignored. The only challenge that we face is agreement on

methods used and selection of placement sites, including sites out on the lake to measure transport distance and attenuation equations.

I think we can agree that site placement is critical and that attenuation rates of different points sources should be determined.

The white paper also states:

“I note that Olsen et al. 2019 measured TP deposition on the shores of Utah Lake and found deposition rates between 1.26 and 31.38 (average 8.1) mg P m⁻² day⁻¹, up to two orders of magnitude higher than has been measured anywhere else in the world (Tipping et al. 2014, Brahney et al. 2015b)”.

This is highly misleading. While the high estimate of Olsen is approximately 2 orders of magnitude higher than the lowest worldwide estimate, the lower (and uncontaminated) estimate reported by Olsen was only 15% higher than the highest estimate reported by Brahney et al. (2015). Because the entire range of worldwide estimates is 2 orders of magnitude, a 15% difference from a measured value does NOT invalidate an estimate of AD on Utah Lake. Moreover, the white paper claims that the 400 mg/m²/yr estimate was from an area near biomass burning. However, this burning may not be the source of the high P as Dolislager et al. (2012) noted that wood smoke near Lake Tahoe was very low in phosphorus content.

Actually, it isn't misleading and I apologize for making an error - I mistyped in the original draft. Please forgive me, I had limited time to produce the report and was travelling and read the wrong line in the spreadsheet. In any case, Brahney et al. 2015 in fact reports 260 mg m⁻² yr⁻¹ as the maximum amount, which makes the Olsen estimate 77% greater. I corrected this error in the document, and again apologize. The data are downloadable from the publisher's website if you wish to check. 77% is substantial given the following sniff test (section 3.1, Olsen 2018, Hydrology,

- 1.26 mg m² day is equivalent to 460 mg m⁻² yr⁻², at the stated soil concentration this is 2,300 g m⁻² yr⁻¹ of dust deposition (note this is almost 4 times higher than Kuwait, the highest published deposition rate), or 1 mm of accumulation given mean sedimentary soil density.
- **Mean of all sites: 8.1 mg m⁻² day⁻¹ is equivalent to 2,956 mg P m⁻² yr⁻¹, or 14,783 g m⁻² yr⁻¹ of dust, or 6 mm of accumulation**
- 31.38 mg m⁻² day⁻¹ is equivalent to 11,453 mg m⁻² yr⁻², or 57,269 g m⁻² yr⁻¹ of dust, or 22 mm of accumulation

It is worth noting here that the accumulation rate of sediments in the highly productive Utah Lake that has a catchment area to lake area ratio of 26, is only 2 mm. Moreover, if these values were remotely correct, SLC and environs would have major infrastructural problems.

The point the reviewer is trying to make about biomass burning is unclear. Wood smoke and biomass burning are not the same phenomena. Biomass burning has been shown to contribute extremely high loads of phosphorus to nearby ecosystems, though neither would have 'high P contents'. Leaves have approximately 1.1 mg/g P whereas wood may be closer to 0.5mg/g or less. The measured deposition rate of 260 mg m⁻² yr⁻¹ occurred during a period of intense vegetation clearing by burning near the collection site.

The white paper also states:

“I calculate a range of bioavailable P deposition between 1.1 and 7.9 metric tons per year (Table 10). If we assume an urban deposition rate throughout the entire lake during inversions, which may occur 10 to 25 days of the year, this may add 1 to 2.5 metric tons per year. Option 1 below in the recommendations provides a mechanism for determining the actual potential deposition associated with an inversion”.

I raise the question as why it was assumed that the deposition of urban sources of P only goes 400 to 600 m from shore whether or not the wind is blowing, but when the wind is calm (during an inversion), the atmospheric deposition values are extended across the entire lake.

This is a good point and the total atmospheric deposition load was assumed for two important reasons. 1) If an inversion is rained out, the atmosphere will be scrubbed clean regardless of particle size, and 2) to demonstrate that the argument that ‘inversions’ could contribute ‘large amounts of P’ to the lake are unfounded. This estimate was provided for these two reasons only and were not in fact included in our total estimate loading.

Common sense suggests that urban sources emit similar loads of nutrients continuously throughout the year (and probably higher during the more active summer months), regardless of whether the wind is calm or not. In reality, in view of the wind roses in Figure 3 in the white paper, I suggest that urban emissions are drifting directly over the lake 365 days of the year. Moreover, the turbulence of both wind and surface water exponentially increases exposure and surface transfer of gasses and aerosols. Thus, I suggest that urban sources and deposition of these contaminants be applied for 365 days of the year.

Again, this argument comes down to the effective measurement of attenuation across the lake. I invite the reviewer to measure this.

Table 1. of the white paper estimates the range of nitrogen deposition on Utah Lake. Total N (nitrate plus ammonium) ranges from 400 to 700 mg N m⁻² yr⁻¹ or 153 to 288 metric tons per year on the lake.

For comparison, nitrate and ammonium have also been measured for more than 2 years of weekly sampling during our Utah Lake AD program. Using his total (including contaminated) data set, Olsen reported 529 metric tons N per year depositing on the lake and, discarding the contaminated data set, Olsen reported 97 metric tons of total N. Notably, the white paper estimate falls midway between Olsen’s contaminated and uncontaminated data sets. In a more recent 2018 data set where insects were removed from the dry deposition buckets, the average of weekly samples = 928 mg N m⁻² yr⁻¹ for total nitrogen deposition or 382 metric tons per year on Utah Lake. Notably, this is only 33% higher than the higher modelled estimates reported in the white paper - even when the bugs were sitting in the water for up to 7 days. However we may have disagreed with treatment of bug contamination, considering that the white paper only applied urban source estimates to the first 400 to 600 meters extending from the shoreline needs to be equally scrutinized. Moreover, with such similar estimates of N deposition between the

Olsen data set and the white paper calculations, equal credibility should be granted to Olsen's phosphorus estimates.

The reviewer is conflating phosphorus and nitrogen biogeochemistry and atmospheric transport pathways. As noted in the introduction, the path of these nutrients to lakes from the atmosphere are quite different. The reviewer is also incorrectly assumes that the treatment of urban particles (attenuation) was applied to nitrogen deposition. For nitrogen deposition we used spatial data based on measurements and atmospheric models combined.

There are four conclusions here:

- 1) the P estimates in the Olsen thesis are clearly comparable to the white paper estimates and, because they are based on empirical data, should be considered at least as valid as the white paper estimates;

I would consider including the Olsen data in my estimates if he had published either a dust loading rate or a dust P concentration, which would fit with the rest of the data presented in this paper. He provided neither, only a P loading rate based on contaminated samples. Since this data would not be publishable in a credible atmospheric or biogeochemistry journal I see no reason to include it here. The entire white paper is based off empirical estimates.

- 2) the N estimates of both the Olsen data set and the other BYU measurements are clearly in the range of the white paper estimates – even though the white paper estimates may have significantly underestimated urban-sourced deposition for about 95% of the year;

As above

- 3) If Dr. Brahney and the TDep workgroup had applied their modelled number over the entire surface for the whole year, as they did for inversion periods, the estimate for N would have been much higher – perhaps greater than Olsen's estimate that included the contaminated samples and;

The TDep working group has applied estimates for the entire year, which is what the white paper is using. The methods are published and can be found on the NADP site. The data layers are also freely available. I'm inclined to go with the results as determined by leading atmospheric deposition experts credentialed in the field. Please see Page 16, which reads "*As with phosphorus, I could estimate reactive nitrogen deposition fluxes based on available data for urban and regional sites. However, considerably more effort has been allocated towards the estimation of reactive nitrogen fluxes to ecosystems with the United States. As a result, maps of reactive N deposition data are available at high spatial resolution. The maps are produced using both wet deposition measurements as well as dry deposition estimates that are based on ambient atmospheric concentrations and the output from the Community Multiscale Air Quality (CMAQ) model (TDep 2019). The model combines monitoring data from the Clean Air Status and Trends Network (CASTNET), National Atmospheric Deposition Program (NADP), Atmospheric Integrated Research Monitoring Network (AirMoN) and the NADP National Trends Network (NTN). More details on this modeling effort including caveats can be found in the Total Nitrogen Documentation produced annually by TDep, more information can be found on the NADP TDep website <http://nadp.slh.wisc.edu/committees/tdep/reports/nrDepWhite-Paper.aspx>*

Atmospheric inputs of reactive nitrogen (Nr) occur via wet or dry deposition. Wet deposition includes aerosols and particulates dissolved in or carried by precipitation to an ecosystem. Dry deposition includes the deposition of gases as well as particulates that are deposited to ecosystems directly. The CMAQ model estimates the deposition of common aerosols measured in both wet and dry deposition including nitrate (NO₃⁻), nitrite (NO₂⁻), and ammonium (NH₄⁺). In addition, the CMAQ model estimates the deposition of nitrogen species that are rarely measured but are likely to contribute Nr to ecosystems including peroxyacetylnitrate (PAN), dinitrogen pentoxide (N₂O₅), nitric oxide (NO), nitrogen dioxide (NO₂), nitrous acid (HONO), and organic forms of nitrogen (ON). Note, unlike organic phosphorus, organic nitrogen is not completely bioavailable. Wet deposition of nitrate ions is estimated to be between 200 and 300 mg m⁻² yr⁻¹ and dry deposition of aerosols between 200 and 400 mg m⁻² yr⁻¹. Total Nitrogen deposition, all forms, wet and dry is estimated to be between 400 and 750 mg m⁻² yr⁻¹ (Table 11, Figure 2). “

4) These data suggest that P loading is somewhere between Olsen’s high estimate, including contaminated samples (373 metric tons total P per year) and Olsen’s low estimates, discarding contaminated data (including excluding samples with any single piece of debris; 17 metric tons per year).

I don’t think any ‘data’ suggest this. As above, you appear to be conflating N and P biogeochemistry as well as the two different methods applied to estimate loading.

It appears that the modeling decisions used in the white paper are questionable, and the Olsen and other BYU data are certainly useful and indeed, valid. I also suggest that empirical evidence (i.e. Olsen and other BYU data) is almost always more valid than modeled results, especially when model assumptions are questionable.

This is not really worth responding to, the modeling efforts are published by leading experts in their field and accepted by the deposition community at large. The models are also based on empirical data.

Due to questionable model assumptions and the exclusions of data or sites that are proximal to the lake, the white paper, with its recommendations, is far too narrow and is oversimplifying and may well lead to a severe underestimation of AD values.

I maintain that the estimates are not ‘questionable’ (unless questioned by a credentialed expert in the field). I invite you to contact the authors of the TDep model and ensuing publications with your concerns.

While there is a paucity of local data, and which is controversial, it is far too early to ignore or exclude the data, simply because it does not meet current objectives of the national NADP program or the modeling assumptions used in the white paper.

I actually think there is a fair amount of credible data in the area... please look closely at the tables provided in the paper. In particular table 2, which includes 8 measurements of dust loading

to urban areas of northern Utah (2 near Utah Lake), and table 6, which includes 12 published estimates of P content in regional dusts to the Wasatch Front.

Undoubtedly, this is one of the biggest disputes in this discussion. I continue to postulate (presented as a testable hypothesis) that Utah Lake, with its surrounding landscape, is far too different physically, geographically and ecologically from lakes currently described in the white paper. To extrapolate, from a single dirt road or highway winding through the forest next to Lake Tahoe or from plant and animal debris from an adjacent hardwood forest surrounding 15-ha Mirror Lake, New Hampshire (Cole 1999), that 90% of dust or debris only travels 200 m, which was undoubtedly measured on a calm day; that dust or debris reaching Utah Lake behaves similarly, is unfounded. [For the 10th time, we don't assume this, we actually double and quadruple the estimate and suggest a method for determining the actual attenuation coefficient, which is something that appears to be craftily ignored. Again, I invite you to measure this using acceptable methods.](#)

In addition, as acknowledged in the white paper the local agricultural and urban sources (such as building on and continually disturbing Lake Bonneville sediments), can be important sources and, depending on particle size and wind velocity, they may very well spread across a large portion or most of Utah Lake as there is no wind protection incurred from adjacent forest.

[But there is a mountain range on the western side.](#)

Whether the dust settles within 200 m or 200 miles is a function of particle size and wind velocity and ([exactly, which is what the white paper is asking you to consider with the 'recommendations'](#)), on calm days, the influence of diffusion physics and temperature stratification on dispersal characteristics and particularly across the lake. ([great, can you incorporate this into your study?](#)) Utah Lake's location, obviously in the bottom of Utah valley, attracts urban emissions through the effects of gravity on colder, denser masses of air. I suggest the notion that while we may conjecture and hypothesize how far particles will travel and what the elemental composition is, all current estimates, whether local or regional extrapolations, are barely one step above an educated guess

[An educated guess was the point of the paper. Again, recommendations provide a credible means for collecting empirical evidence. I hope you do this!](#)

Moreover, to think one can specify what SRP is from these different sources, is also merely an educated guess—[*actually these data are based on measurements*](#) particularly with regard to Fe and Al-bound P. While oxidized forms, or high pH forms of these minerals are insoluble, when they hit the bottom of the lake “all bets of off” as everything from bioturbation (or lack thereof), (e.g. Molot et al. 2014; Holker et al. 2015), to redox chemistry, to pH will dictate how much P is soluble; we simply don't know. [**I think Mitch Hogsett studied this in detail. Also, I think 'we do know' that the lake likely does not stratify for meaningful periods of time. Please note that the dissolution rate of iron oxides is several times slower than the precipitation rate of the same, thus extended periods of anoxia would be required to move Fe-P meaningfully into the lake**](#) But certainly, these are testable hypotheses that warrant investigation. [*This is not disputed by anyone*](#)

In addition, the white paper references “Deposition Velocity models”, that, if used in concert with more-easily sampled dust concentrations using HVAC followed by digestions for nutrient content, can be used to accurately predict AD on Utah Lake or anywhere. If such is the case, the white paper should describe the functionality and data requirements for calibration so that the

Science Panel can determine whether such a model can or should be used on Utah Lake. In the absence of data, however, there may be too many assumptions to deliver usable results (e.g. see Raymond et al. 2004).

We have included more information on the High Volume atmospheric aerosol samplers that should be used. The point being made by the reviewer with respect to the Raymond paper is unclear. The Raymond et al paper is not an analogous method?

Moreover, while sampling boat cruises appear to be useful for HVAC sample collection, the author of the white paper has clearly not been on Utah Lake when the wind is blowing 20 to 50 miles per hour, creating waves 2-4 feet high and visibility is less than a half mile because of the sediment-derived dust blowing across the lake. During these all too often events, the enhanced deposition events cannot safely and carefully be sampled by even the most experienced boat crew and captain.

During heavy winds at times when regional dust are being delivered to the lake the methods outlined for regional dust sampling are more appropriate and do not require being on a boat in the lake.

In short, I propose that the regional data from 4 NADP sites, using only wet deposition samples, have far too few data points, in too far proximity from Utah Lake and which may only estimate a small percentage of the AD sourced in arid central Utah to make any accurate predictions of AD on Utah Lake.

This is not actually disputed by any one. This is why we use local data, at the edge of Utah Lake. Did you read the paper?

Rather, the white paper serves as an exercise in hypothesis development, nothing more, that clearly needs to be tested *And it is presented as such*, This is why we give recommendations* with local proximal data that can be collected and verified with carefully constructed control practices and sample collection that includes at least one mid-lake location.

The white paper states the following guidelines (as cited from the NADP installation manual) Samples must not be placed

- Where there is potential for contamination from irrigation;
- Where there is potential for contamination by fugitive dusts;
- <100 m from roads, paved or unpaved;
- <10 meters from access roads, though in Utah I recommend this be increased to at least 30 m (Figure 4c,d);
- <5 m from objects greater than 1 m tall (including a solar panel);
- Within a 5 m radius of vegetation.

The guidelines specify that the vegetation within the 5 m radius should be less than 0.6 m in height (NOT that there should be no vegetation within 5 m: This vegetation should actually help secure soils in place). Nevertheless, we are currently making adjustments to comply with the current guidelines. As well, we are maintaining three samplers in current configuration (1.5 m above ground level), to provide a side by side comparison with the configuration used during the last three years. In addition, we are inserting 250 um screens to prevent plant and animal parts from

contaminating the sample. In addition, we are developing an alternative regression analysis method, similar to Blake and Downing (2009) to determine the influence of contaminated samples.

Good.

In addition, while the potential for contamination of some of the BYU samples has received criticism from the Utah Lake Science Panel, ironically, this contamination reveals the importance of nutrient recycling from insect carcasses that fall back on the lake surface (e.g. after egg laying; as described in the Miller AD proposal and workplan). [Actually, the science panel felt that midges reflected a loss of phosphorus from Utah Lake, which is craftily ignored by the reviewer here. Midges accumulate all their biomass in the lake sediments and then no longer feed once they emerge. Given that not all midge biomass will return to the lake, this is effectively a loss.](#)

Clearly, this “contamination” is significant. Blake and Downing (2009) found high levels of insect contamination (soaking as many as 185 mayfly carcasses in 2 L of distilled water) and decomposition can increase atmospheric deposition estimates by an order of magnitude. But to be clear, this still constitutes a considerable degree of recycling of nutrients – similar to recycling of sediment nutrients by diagenesis and redox chemistry. [Eh, sort of, regardless is not relevant to the question of atmospheric loading. The recycling of nutrients represents an entirely different ‘loop’ if you will, in a biogeochemical cycle.](#)

For example, it has been estimated that only about 10% of aquatic insect carcasses actually fall back on the lake surface, but peak deposition occurred in very close proximity (20-25 m) inland from the shoreline (Dreyer et al. 2015). Yet, of the up to 6000 tons of midges (estimated from quantitative sampling by Dr. David Richards) that are generated annually (some species may be multivoltine), the conservative 10% still constitutes 600 tons per year [\(but this is a loss of 5400 tons\)](#). At 1% P in body mass, this still constitutes 6 tons of dissolved P recycled into the water column [\(and a loss of 54 tons of P\)](#). But this may still be a conservative estimate. For example, if the high end of Olsen’s estimates of P AD (373 tons per year) are primarily due to insect contamination, then reducing this by an order of magnitude still results in an estimate of 37 tons per year. For comparison, this is nearly an order of magnitude higher than the white paper estimates.

The take-home message from this exercise is that no current estimates, either from current BYU estimates, nor from the white paper are ready for “prime time.” [This is a white paper, it is by definition not “prime time”](#). What is clear is that while sampling methods may vary, phosphorus AD may be a substantial contribution to Utah Lake’s nutrient budget, or it may not. The two greatest variables that need investigation are: 1) continued and improved methods to estimate local contributions (agricultural disturbance, urban emissions and mobilization from nearby Severe Lake and Great Salt Lake playas and the degree to which they deposit across the lake (i.e. what is the proper decay equation; and 2) the phosphorus and nitrogen content of mobilized particles, including total P and serial extractions to more accurately describe to degree to which the P content is soluble and biologically available in the Utah Lake environment. This process needs to recognize and include the lake variables such as pH, calcium carbonate species and concentrations, as well as sediment transformations as a result of redox chemistry and bioturbation.

[At last we agree.](#)