Review of Document
David A. Gay, Ph.D.
November, 2019

Document: *Letter to Senator Dayton & Stratton: Response to LaVere Merritt's Utah Lake deposition study*, by Dr. J. Brahney, Utah State University, Dec. 11, 2017

Overall: I have not read the original paper/report by Dr. Merritt that Dr. Brahney was providing comments on in this memo. Overall, I find Dr. Brahney's comments to be rather straightforward and include reasonable conclusions. Dr. Brahney's main conclusion is that the measurements made and subsequent flux calculations in the original paper result in fluxes to the lake that are much higher than the referenced research. However, I do disagree somewhat with the definitions of contamination, as I detail below. Dr Brahney's conclusions do not mean that the original conclusions by Dr. Merritt are incorrect, but that these fluxes are certainly very large outliers relative to referenced research studies noted by Dr. Brahney.

<u>Specific Comments:</u>

1. Page 1, Para 1: The levels reported here from Dr. Merritt's study are very high deposition flux rates for TP, as noted. These are much higher than the papers referenced, and higher than the Olsen et al., Hydrology V5, 45, 2018 paper which I also reviewed. In that paper, the authors note that TP flux of 350 metric tons TP/year, which the authors conclude is likely at the higher range of TP fluxes (although this paper was published after the paper which Dr. Brahney refers). I would not use the term "impossibly high" as Dr. Brahney added. I think there is certainly a possibility that the flux rates are correct, but I would have to conclude that the likelihood of these being the correct rates are small. Having not read the original study, I am limited as to what comments that I can confidently make. In these types of studies, the calculations can become complicated, and often mistakes can be made in the mathematics. If the Board is concerned about the large flux estimates in the Merritt study, the recommendation that I would make would be to have the mathematics reviewed along with the assumptions made for the calculations.
2. Page 1, Para 2: This is all correct. Dr. Brahney's background and Ph.D. work focused on phosphorus deposition, and she is collaborating with NADP on methods for improving depositional measurements for phosphorus. She is well qualified to

comment on deposition projects, particularly in the western U.S., dry deposition rates, and those focused on phosphorus.

3. Page 1, Para 4: Placement of samplers. According to this memo, the placement of the samplers in question violated the bulleted NADP rules and guidelines leading to "collecting data that may be contaminated by local sources of material". These bulleted points are part of the NADP criteria. However, the NADP criteria are set to limit the impact of local sources on the NADP regional signal/measurements that the NADP is trying to capture. NADP does not want one particular source to unduly bias the regional flux estimates (negatively or positively). As I have mentioned in other document reviews, the NADP sets these criteria so that any local sources are well mixed into a consistent signal that NADP is trying to capture at each of its sites so that the flux measurements NADP reports is reproduceable at other locations in that particular environment (i.e. an agricultural region, forested region, etc.). However, it seems to me that for this particular question of flux to Utah Lake, you want to capture all of the important and significant fluxes that are contributing to deposition flux to the lake. Therefore, the contribution of local sources is important to include in the flux estimation. On the other hand, if all of the samplers are clearly dominated by local sources that only contribute to flux in certain parts of the lake, leaving low flux rates unaccounted for, then the total flux estimates can be overestimated following from the point that Dr. Brahney is making. Therefore, I would not call the local emissions "contamination", but I would caution that site selection in this particular study as it will be very influential in the final deposition flux estimates. It comes down to a balance of accounting for local sources while not allowing them to dominate the flux estimates to the lake. You want an appropriate accounting for all sources.

4. Page 1, Para 4: Bulleted points. These are rules and guidelines from the NADP wet deposition networks. Specific comments on the individual bullets follow.

    - Irrigation sources; an NADP rule. However, the rule is primarily meant to prevent water that is not precipitation from combining with the actual precipitation water, and diluting the regional wet deposition concentrations, and altering the true precipitation chemistry to look more like the chemistry of the irrigation water.

    - >100 meters from roads, which is a rule. Same argument as above.

    - >10 meters from access roads, is a rule.

- Low vegetation within 5 meters, is a rule, so that there is no undue mechanical influence on the sampler or no chemical influence on the sample.
- >5 meters from 1m tall objects, is a rule, and is to reduce precipitation bounce into the sample.

Of these, I would say that the irrigation source and roads would be local sources that can impact the lake. However, an irrigation source contributing pure water to the sample would reduce the concentration of the sample by increasing sample volume, and I would consider this to be contamination of the wet deposition sample. It could also influence the sensor and have the wet deposition sample bucket exposed to what is really a non-precipitation event. Irrigation water could also contribute analytes to the wet deposition sample, driving the chemical profile to look more like the irrigation water than the true precipitation chemical profile. It could also contribute water and analytes to the dry side bucket without actually setting off the wet precipitation sensor.

However, it seems that in this situation, irrigation water (and its TN and TP) could/can reach the lake and add to the overall flux into the lake. Dr. Brahney is correct that this would be contamination relative to the wet deposition calculation, but I agree with Dr. Merritt that this irrigation water could be an important source to the lake flux. It is just a case of what is wet deposition and what is not.

On the final two bullets, I would also consider these potential methods of contamination to the sample. High vegetation could interfere with the mechanics or sensor of the sampler, and could contribute biological material inappropriately to the sample in certain situations. Equipment near the sample collector can pick up dry deposition, and when precipitation starts, it is possible for a rain droplet to hit the equipment, pick up some dry deposited analyte, and "bounce" or spray into the open collector which would be contamination of the wet sample.

5. Page 2, Para 1: "NADP samplers…deposition rates." See discussion above. Additionally, I would say that the issue comes down to a balance of including influential local sources to accurately attribute their impact to the lake deposition flux, but not to bias the deposition rates to the high side by only collecting measurements where influential local sources are present. The lake deposition is likely to be highly variable in each of the surface area meter square "cells". Some of them will be highly influenced by local sources and others likely will have no impact by that that particular local source. This goes to Dr. Brahney's comment of not being "extrapolated to the entire landscape", which I do agree with.

6. Page 2, Para 1: "Most true…spores and pollen." I do generally agree that this is usually the case, but if there is an important source of dust/particulate near the lake, such as a heavily used road, this will be very influential in TP moving into the lake, and contributing significantly to the TP flux.
7. Page 2, Para 2: Sampler height. How high or low were the original samples made? Having reviewed one document with a picture of one site, the sampler main surface appeared to be about 1.5 feet off the ground making the sampler inlet about 2 feet above grade. I do agree that if all samplers were collected at this height, then these samples could easily be contaminated by dust from the immediate area around the sampler. In fact, walking up to the sampler on Tuesday sample collection day may contaminate the sampler. One to 2 meters is the generally accepted height, and higher (and on boardwalks) when heavy snow and wind would become another issue. I would advise against them being much higher than 2 meters, unless blowing snow is expected to be an issue.
8. Page 2, Para 3: Irrigation samples. See the discussion above. Also, it is true that plowed fields tend to require less wind to pick up soils and blow these particulates downwind. However, as mentioned previously, if plowed fields are part of the natural environment around the lake, as they seem to be from satellite images I have seen, then these sources are likely contributing particulate load to the lake along with any TP/TN fluxes. I would consider these appropriate sources to the deposition fluxes and not contamination. Dr. Brahney mentions two sites having significantly higher concentrations of TP. I don't know if these irrigation systems are using river water or groundwater, but the presence of phosphate rock in the area would suggest that irrigated water could be high in TP. I can easily see agricultural soil contributing to high TP deposition.
9. Page 2, Para 4: "None of these…team." Again, I have not read the original report, so I cannot say that no safeguards were used or not used. Previously, I have suggested how I would define contamination, etc. in these situations.
10. Page 2, Para 5: "The NADP…insects (Figure 2)." This is partially true. The dry buckets used for dry deposition estimates (these measurements were discontinued in the late 1980s) did collect many insects and vegetation pieces, in part leading to some contamination. Open buckets are clearly susceptible to these influences. But additionally, dry deposition measurements were discontinued for other reasons, primarily that the plastic buckets did not accurately mimic natural surfaces such as leaves and grasses, etc. particularly for gaseous deposition, and therefore, did not

provide measurements of dry deposition that could easily be used for dry deposition fluxes to the natural environment. Buckets are also very poor collectors of previously collected particles; i.e. particles are collected better on low wind days, and many of the particles can be swept out of the bucket on windy days. Therefore, with all of these problems, NADP stopped measurement dry deposition with the bucket system of the Aerochem sampler. Additionally, the data that was collected is sequestered and is no longer distributed, explaining NADP's lack of confidence in the data.

11. Page 2, Para 5: Insects and vegetation. I am not a biologist, so I have little to add to the phosphorus content of individual biological particles. However, I assume there are both TP and TN in both insects and vegetation. I can easily see that they can be very influential in defining or biasing flux estimates and be considered contamination of a deposition sample. However, I would also add that insects and vegetation bodies/particles are naturally being added to the lake, and therefore, I would consider them appropriate to add to the total flux of TP/TN to the lake. Scientists would argue about whether bugs/vegetation are wet and dry deposition or not, but I would call them deposition, and appropriate to estimate for flux estimates. I do agree with Dr. Brahney's point that the bug contribution can be very influential and add large amounts of TP/TN to the estimate.
12. Page 3, Intro Para: "As a result…contaminants." There are methods available to keep bugs and some vegetation components out of the dry deposition samples. However, this does bring us back to whether these biological particles should be part of the flux to the Lake, and I would say yes, they are. But still in question is are these wet and dry atmospheric deposition.
13. Page 3, Para 1: Bird feces in samples. I would agree here that obvious bird feces in the wet samples (and dry for that matter) is an important issue and question. When a bird uses the sampler as a perch and defecates into the sample collector, I would consider this contamination of the sample. There are methods to try and deter this activity. However, they create other problems by changing the aerodynamics around the sampler opening and providing precipitation bounce surfaces. But I would also argue that bird defecation to the lake, with a large population of birds present, would likely be an important flux of TP/TN to the lake. It just remains on how to estimate this flux. But I would consider this bird feces as contamination of the sample.

14. Page 3, Para 2: total phosphate versus bioavailable phosphate. I am not a biologist or geologist, so I do not have an opinion with any scientific weight. However, some type of decision does need to be made on what is biologically available and what is not. Biologically available TP, at least in the near term, is ultimately what is important to the Lake's condition.
15. Page 3, Para 3: I agree that to extrapolate shoreline concentration/deposition fluxes across the entire lake is not an appropriate method. A shoreline source, for example a gravel pit, will generate a large number of relatively large particles. These larger particles will fall out of the atmosphere rather quickly and not travel over long distances. If this gravel pit is close to the shore, it will likely add a large flux of particles (and likely TP/TN) to the waters near the source, but the flux rates will be reduced quickly and steadily as the plume moves into the open waters. And this flux to the water will be in the downwind direction, which likely will change day to day. Therefore, it is reasonable that local particulate sources will only affect certain areas of the lake rather than the entire lake, at least on a day-to-day basis.
16. Page 4, Para 1: "phosphorus deposition rate of 0.2 g/m^2 yr." In Olsen et al.'s Hydrology article, the reported TP deposition rate was reported as 2.95g/m^2 year, or about 10 times this rate, for comparison.
17. Page 4, Para 1: "Bioavailability of total dust phosphorus (~30%)." This figure is what was sourced in other Brahney documents, 34% to be exact.
18. Page 4, Para 1: IMPROVE estimates. I was able to find a few IMPROVE measurements of atmospheric phosphorus dust concentrations in and near Utah (Canyonlands, Dinosaur NP, Capital Reef, etc.). The reported levels are indeed very low. But the phosphorus data I was able to find was part of the fine particulate fraction, or part of aerosols less than 2.5 microns in diameter. Soil particulate and many other potential sources of particulate would more likely be in the larger particulate size fractions. This is not evidence that the phosphorus concentration is higher in these particulates, but I would expect that the important phosphorus contribution from these types of sources (agriculture soils, gravel pits, etc.) would be in these larger particles not sampled by IMPROVE (or at least those that I found). I do not see any measurements or data in the IMPROVE system of phosphorus in these larger aerosols/particulates diameter ranges.

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Document: *Estimating Total and Bioavailable Phosphorus Loading to Utah Lake Version 3, Version 09-30-19, and Final version**, by Dr. J. Brahney, Utah State University.

Overall, the document is very well written and referenced, as expected. Much of the paper I agree with, and most of Dr. Brahney calculations I was able to reproduce. I note when and where I was unable to do this. I feel that this paper outlines a very reasonable, scientifically valid, and straightforward approach to estimating the deposition of nitrogen and phosphorus to Utah Lake

There are several general points I feel are important to make at the outset in the review. It would seem that Dr. Brahney and I have a somewhat fundamental disagreement on the definition of "contamination". It is certainly possible that I have misunderstood, but I am noting this issue to be clear. First, my way of thinking is that any Nitrogen (N) or Phosphorus (P) that enters Utah Lake could be important flux to the lake, which would include the local sources near the lake, and the only questions are whether these fluxes are counted as wet or dry deposition and what chemical form the N/P is in when it enters the lake. It seems that Dr. Brahney is considering, for example, plants, insects, locally sourced dust, etc. as being contamination to the sample. Specifically, if there is plant debris washed into the wet deposition sample or caught on a dry deposition filter sample, I would generally consider that a valid sample. I say this because precipitation is going to wash the atmosphere clean of all particulates whether they are of biological origin or not. I will argue that this material is being added to the lake, therefore representative of the deposition to the lake from all sources (the point of this sampling). Insects crawling into the sample bucket is one thing (I would say generally contamination), but being washed out of the atmosphere is another issue altogether. Dust can get into a wet deposition sample as contamination, or washed out of the atmosphere as wet deposition, and thus is important to the flux. Another example that comes up is bird feces in a sample. Like Dr. Brahney in this case, I would also consider this situation to be true contamination of a sample and that it should not be used in the study. So, in the overall study to determine the addition of Total Nitrogen and Phosphorus (TN, TP), everyone involved needs to be very clear about what is to be considered contamination to a deposition sample, and what is not.

**Note: I was just about finished reviewing the 9-30-19 version of the document when the final version arrived. So, I transferred over my comments and read through the final version again.*

A second point is that I would not support a totally passive sampling plan as Dr. Brahney proposes as an alternate in Section 5.2. Passive samplers have their uses, but with the focus on TP in precipitation and particulates, I do not think passives are the approach to use in this project (with a possible exception being ammonia gas). I am supportive of the active sampling for this step, as Dr. Brahney suggests in the previous section.

A third point: as Dr. Brahney suggests several times during her discussion, I agree with her that measuring dry deposition is very problematic. Basically, there are several methods for measuring dry deposition, and Dr. Brahney's newer method (baffling within buckets) and other methods have promise, but there is no universally agreed upon method to measure dry deposition that is scientifically valid. In general, the criticism focuses on the issues of the texture of the sampler not resembling the natural environment, inconsistent aerodynamics, loss of sample after collection over time, and other things similar to these. Therefore, if you choose to measure dry deposition directly, you will be opening yourself up to criticism that your method is flawed. NADP gave up on measuring dry deposition in the 1980s due to some of the same issues mentioned here. Clean Air Status and Trends (CASTNET) does not measure dry deposition directly either, due to the inherent problems. Mercury scientists also do not use them, even though several devices have been designed and tested. I would say that the preferred method for dry deposition fluxes is to estimate the atmospheric concentration of analytes and to estimate the deposition velocity to calculate flux. However, a measurement approach is much more viable for TP, since there are no atmospheric phosphorus gases.

Specific comments on the paper

1. Page 5, Para 1: How long has the lake been eutrophic? Does it correlate well with the urbanization or other changes? Maybe this a clue as to the important compounds and sources.
2. Page 5, Para 3: I agree that dry deposition is well correlated to drought (but this is periodic) and urbanization. I am not familiar with these particular papers, but are we convinced that dry deposition dust fluxes are increasing here at Utah Lake?
3. Page 5, Para 3: "3 events to as many as 12 events." I agree, but I would think the important dust events are in the spring with winds, and the summer with dryness, and in both cases, with little or no snow cover.
4. Page 5, Para 4: "Distance will…distribution." Agreed, and very much so.

5. Page 6, Table 1: The all N. America TP deposition flux seems high, at least based on the other noted values from North America. While most continental numbers appear to be averages, the NA value does not; it is higher than all reported values.
6. Page 7, para 2: "gaseous aerosol deposition". I am unclear with this use of words. Does it mean the combined deposition of gases and aerosols? It is just not clear to me.
7. Page 7 Para 3 "On average...environments." I don't follow this enrichment idea. It just isn't clear to me why this would be. Is it an enrichment driven by agricultural related activities?
8. Page 8, Para 3: "local sources...shoreline." I think this is generally true for particulates. I do not really know what the local sources are exactly, but there could local burning source which could clearly impact areas beyond the local area. And if it was a local source of small particulates, these could also be transported regionally or beyond.
9. Page 8, Para 3: Attenuation equations. It would be nice to add the equations to the document so it is clear what exactly was done. They could be perfectly fine, but it would be nice to see them. Dr. Brahney seems to be using the equations in a worst-case manner, which is a very reasonable approach.
10. Page 9, Para 2: Here, I am just noting that this is a huge range of TP deposition from around the globe. I would expect this very large and variable range.
11. Page 9, Para 3 through to Table 3 (Page 10): I know that there are not very many urban TP concentrations available, but there are only three used for this 152.3 mg TP m^2 year (Table 3), and these three are unpublished. Again, this does not mean they are wrong, but only three unpublished values inject some uncertainty into the estimate.
12. Page 10, Table 3: The averages (Utah measured and estimated) are reasonably similar, I would say. The Logan measurement is essentially the same as the estimated average. Therefore, this somewhat supports the estimate of 93.6 mg TP/m^2 year.
13. Page 10, Para 1: The sentence "The exact fraction…dust composition." is very important to note. The soluble portion of TP can be highly variable above or below any average.
14. Page 11, Table 4: I can identify two of these values; the min and the max are two of the three Utah measurements from Table 3. However, the average, I would assume,

would follow to be the 152.3 mg TP/m^2 year value also from Table 3. But the average is listed as 139.3, and I am not sure where this figure comes from. If this is a mistake, then the mean and max water-soluble values from this mean are also likely incorrect.

15. Page 11, Table 5: Many of the references are unpublished articles, and again, less useful with this condition. Noting the Utah average deposition rate here (4.0 mg/m^2 year) and the table note, Dr. Brahney thinks that this average is somewhat high and some level of local contamination can be assumed. I think this is likely correct also; a bit of high bias here for the Utah measures. I also think that many of the study periods listed overlap with a decade-long Southwest drought (roughly 2003-2013) and are therefore potentially biased high from this perspective. See https://earthobservatory.nasa.gov/images/84801/drought-persists-in-the-american-southwest.
16. Page 12, Map: Based on the map, and assuming this is all of the surface/exposed phosphorus rock is in Utah, I think that Utah Lake is generally upwind of the source rock region. However, there could be other areas in Utah that are not on the map, and this could negate this point. If some areas are not represented with this map, it would be better to have a map with potential sources to the west and southwest of Utah Lake also included, and not just a clip from NE Utah.
17. Page 13, Table 6: Typical crustal rock is 1.2 mg TP/g rock (Page 12). The Utah Uinta observations are well above typical rock values, but the Utah/Wasatch Front rock concentrations are all below typical values (mean 0.9 vs 1.2 mg TP/g rock). I find this somewhat surprising, since there is so much phosphorus rich rock around the lake area. This might get to my upwind downwind comment previously. Values in Colorado are also below typical values. The table includes more referenced unpublished studies.
18. Page 13, Table 6: From the table note, it says that Uinta TP rock concentrations are not used in the average concentrations, which I think is likely appropriate. However, there is no asterisk in the table or on the Average Intermountain average, which is what I assume Dr. Brahney was referring to with the table note. However, if I have all of that is correct, then the intermountain average should be more like 0.75 mg TP/g rock rather than the 1.40 mg TP/g as listed. This 1.40 value appears to have the Uinta values averaged into it. This point needs to be clarified.
19. Page 14, Table 7: It seems that the water soluble plus organic column in the has one digit that should be shown? The average is 0.24 mg/g, but the table values are

rounded to second digit. Furthermore, I can only approximate several of the averages; I get very close but not exactly the same. Additionally, (from Para 2 "On average…phosphorus (Table 7).") the values listed are only approximately what they are in Table 7. For example, the average % water soluble + Organic is 33.95% in the table and 31% in the text. Also, 3.7% Water Soluble in the table, and 2.7% in the text.

20. Page 14, Table 7: There are more unpublished and less useful references used in the table.
21. Page 14, Table 8. The final bioavailable column is the first column, or Total P * 34%, so this math makes sense and follows from Table 7. However, in the Table 8 Total P column, I think the mean value of 4.8 should be 5.6 mg/m^2 year. Second, the water-soluble P column values are approximately 0.027 * the Total P value, which follows. However, if the 2.7% value is incorrect, and the actual value is 3.7% as shown in Table 7, then the Water Soluble P values in the table are all lower than they should be.
22. Page 15, Para 2 & Table 9: First, note that the Jersey City and College Ward wet deposition values are very close to the 5.6 mean dry TP deposition value (mg/m^2 yr). Second, the wet deposition range of 5-15 mg/m^2 yr is just an estimate, it would seem. It seems reasonable, based on these few measurements. Third, the last entry from Hyrum is labeled as NADP, and must be from Dr. Brahney's study within the NADP network or with the NADP network. NADP does not measure Total Phosphorus, but ortho-phosphorus (PO_3^{4-}). I have not looked at the NADP ortho-P values lately, but they are very low and generally lower than this average. However, I would have to check in to the ortho-P values to be certain about this value at Hyrum.
23. Page 15, Para 3: "Note that if…separately." I agree with the idea that there could be some double counting of TP when estimating dry deposition and measuring wet deposition. However, it could be separated out also. For example, wet deposition does wash the atmosphere clean of suspended dusts. But these dusts could be in higher layers of the atmosphere rather than in the lower layers where they would dry deposit (these would be smaller suspended solids). Dry deposition would occur during the non-raining periods, which in this region would dominate the year. So, if we were able to measure dry deposition only when no rain occurs, and measure wet-only deposition without any contamination (i.e. not bulk deposition), then we could clearly define total deposition (defined as wet + dry). However, if we are

estimating dry deposition following these procedures, it is unclear how much double counting is occurring, and it would be very difficult to separate out the double counted P and N. For example, most of these estimates are from atmospheric measurement studies which logically would have excluded any wet deposition from the measurement periods and when these dry deposition values were determined. And wet deposition estimates were made, again, presumably with little dry deposition contamination. So, it would seem to me that double counting is a good point, but not necessarily occurring in this particular study.

24. General Comment: Note that there are other types of “deposition” that should at least be mentioned for consideration of determining Utah Lake fluxes. With some analytes and environments, these separate fluxes could be dominant.
    1. Throughfall: Dry deposited analytes may be washed off plant and other surfaces during precipitation, elevating concentrations of these analytes in throughfall over the associated concentrations in precipitation.
    2. Litterfall: Analytes may be held by the leaves/twigs and deposited as litterfall to the lake.
    3. Occult: Deposition of analytes by direct contact with mist or cloud containing droplets of contaminated water.

25. Page 15, Para 3: “Logan was not…locations.” I am not sure yet, but given the proximity of Utah Lake to Provo, the measurements at Logan as urban measurements could indeed be the best estimates and, perhaps, more appropriate.

26. Page 16, intro. Para & Table 10: Using half the detection limit for below detection samples is the standard method for NADP when calculating averages, etc., so this is consistent with NADP practice. This mean value of 2.9 mg ortho-P/m^2 y is very similar to the Table 9 Logan value of 4.7 mg TP/m^2 y, which lends support to the Logan value.

27. Page 16-17, using CMAQ/NADP values, shown in Figure 2. This is a valuable tool to use to determine estimates of both dry and wet deposition of N to Utah Lake, and at least, in my opinion, an alternative method versus doing further measurements. The method is well documented and based on factual science. It also could give estimates for multiple points on the lake. It will not likely capture small, local sources and their fluxes, however. Specific local source modeling approaches could be added to these basic estimates for a more complete picture.

28. Page 18, Para 1. There are a few points to mention in this paragraph. For example, it would be good to see the decay equation. Also, I don’t see the area of the lake, a

figure showing estimates of deposition (which vary over the lake), etc. to be able to check the calculations for the estimate of TP to the lake.

29. Page 18, Para 1: I would agree that the winds show favorable conditions to move N & P to Utah Lake from Provo/Suburbs. I would also say that few of the surface wind rose figure insets suggest deposition from the Southwest. Upper level winds (not shown) could provide some input from the southwest direction, however.
30. Page 19, Para 1 and figures: I am unclear how Dr. Brahney arrived at some of these diagrams and exactly what they mean. I would need further information than is provided to understand and comment on these results. I think that the bootstrap method produced these frequency of deposition results (both figures, TP and bioavailable TP). Also, based on the graphs showing that the two most frequent conditions/results were 1 and 2.5 mT/y, then Dr. Brahney is using these to conclude "add 1 to 2.5 metric tons per year". It seems that several attenuation rates were used (see caption). But again, I would need more specific detail to comment further.
31. Page 20, Para 1 & Table 11: I agree that the N deposition rates as determined by NADP TDEP are a reasonably good estimates. Using the area of the lake of 385 km2, I checked the conversion to metric tons per year in Table 11 (test mistakenly refers to Table 12). I was unable to match the deposition rates reported by summing the four individual nitrate/ammonium wet and dry flux totals (in mg N/m^2 yr) and multiplying through by the area of the lake. However, I was able to approximate the four entries in tons per year from mg N/m^2 yr (some of the values were off a few percentage). It is possible that I just don't understand exactly how these values were calculated.
32. Page 20, Para 2: Seasonal variation. I agree. The seasonal nature of regional variation should vary significantly, as would time of day for the urban deposition. The measurement campaign should be multiyear with several repetitions of seasons, due simply to drier and wetter years.
33. Page 20, Para 3: "Because open bucket…localized contamination". I have mentioned this previously, but again, the term "contamination" must be used very specifically. It could be that some bodies/particles that are collected in a bucket sample would not be contamination, but may be adding TN and TP to the lake, and therefore, would not be considered contamination. However, I do agree that the best method to "measure" dry deposition is to measure atmospheric concentrations and estimate them to the ground using dry deposition modeling, as Dr. Brahney suggests. It is not

yet clear as to how much monitoring would need to occur over space to estimate these values.

34. Page 21, Para 1: I have not read Reynolds et al. 2016, but there are many tools available to measure particulate by size classification. However, I would recommend that the entire range of particulates be measured, since they are all likely to contribute to TP and TN loading (i.e. very small to very large). I would not focus on the health-based size cuts of 2.5 and 10 micron in diameter particles. I do not think these size cutoffs offer anything to this study, given that they are health based particulate size ranges. What is in question is the total P and N flux to the lake, and that could include many size ranges. Therefore, I would suggest measurement of total particulate loading of all size ranges. However, size segregation will need to occur if we are to estimate deposition velocities.
35. Page 21, Point 1: "Obvious point-sources…avoided." Again, I am not sure I agree with this, since obvious point sources could be contributing to the lake flux. I agree that you have to be very careful about how you site near a local point source so that you do not inappropriately bias your estimates for the lake. I agree that you want to mix these (if any) large point sources with the regional non-point sources too.
36. Page 21, Para 2: Sampling cruises on the lake. Although these could contribute to our understanding of the situation, sampling cruises can get very complicated and expensive. I would suggest that this option be considered carefully before undertaking this option.
37. Page 21, Para 3: "Captured particles…unavailable forms." Agreed; I think it would be well worth the effort to analyze the samples for bioavailable forms of TP, since the deposition to Utah Lake is so dependent on this determination. It will provide scientific backing for future calculations as well. I do not have any idea what this would cost from a laboratory perspective, but this can be determined and should at least be considered.
38. Page 21, Para 3: "Organic P...such as apatite." I am not sure I follow Dr. Brahney's idea here. I think what she means is "determining organic P as TP minus Inorganic P". But again, I am not clear exactly what is meant here.
39. Page 21, Para 4: "Standard tipping…methods." I don't necessarily agree with this point. By using tipping bucket gages, you could be allowing the double counting process to occur. Although a tipping bucket precipitation gage is likely not a very good bulk sampler (funnel versus open chamber), it will act as a bulk collector for both wet deposition and dry deposition of larger particles (and potentially other

things) during the periods of no precipitation. That is adding a double counting term which will be difficult to estimate/calculate. There is also a potential for transfer of residual TP to the next sample if not cleaned weekly. There are also other potential issues that could come into play. Therefore, I would not recommend using any precipitation collector for the sample.

40. Page 21, Para 4: I agree that existing spatial data sets, namely the NADP/TDEP model estimates are a good way to proceed for the reasons I have listed previously. They are already prepared (for specific years), detailed in space, and scientifically defensible. It would also allow you to spend your time and funds on measuring/estimating the phosphorus deposition, where no data exists. One issue is that it will be for past years, and therefore, you would have to use average conditions/depositions rather than estimates for specific years. There would also be the issue of somehow adding in local deposition sources to the estimates.

41. Page 22, Para 1: Passive sampling approach. I am a bit unclear about what is meant here. Perhaps I define the term "passives" differently. I think Dr. Brahney is suggesting that option 2 is to use bulk-type bucket collector sampling for wet and dry deposition. If this is correct, then I agree this should only be option 2, and I would suggest not to use it. But she mentions an Aerochem collector here, which I do not consider a passive sampler. She mentions the issues with passive samplers, which I would agree with. But I would consider the dry deposition to a bulk-type sampler to be the contamination in a sense (mixing the wet and dry deposition signals), where she may not be thinking in the same manner. I would need more clarification.

42. Page 22, Para 2: Dr. Brahney's point about sample height of 2 meters does follow NADP protocol and makes sense from the standpoint of a truly wet deposition sample. I do agree that saltating particles are not appropriate for a wet deposition sample. However, saltating particles can be a significant contributor for TP/TN to the lake and should be included in some estimate/measurement of flux, because these particles can end up in the lake, particularly if bare soil goes to the edge of the lake in any significant areal coverage. If so, then these saltating particles could be a significant flux addition. But I would not call it dry deposition, per se. But I think we both would agree that these particles need to be accounted for in the sampling plan somewhere.

43. Page 22, Para 2: Few NADP samplers are located much above 3 meters above the surface. As you increase in height, the windspeeds change, and therefore, the

deposition characteristics would also change. Because of this, I would not recommend that any of the samplers be located with their openings/inputs be much above the 2 meter AGL.

44. Page 22, Para 2: Specific sources and sampling locations. There are two aspects that need to be noted here. If significant local sources are present, then you want to accurately account for their flux to the lake. However, you don't want their signal to dominate the deposition samples and flux estimates, since they only affect certain parts of the lake. So, it is a delicate decision on sampler location to balance these two competing needs. If you want a regional estimate of deposition (wet or dry), then you want to avoid them. But for a flux study to a specific lake, you want to appropriately account for any and all significant sources of TN and TP. So, the choice of sampling location with local sources in mind in particularly influential in the final flux estimates. But it is also particularly hard to do.
45. Page 22: Final bullet point. NADP limits high vegetation in the immediate area of the sampler so it does not interfere with the sample or operation of the sampler. But NADP allows vegetation right up to the sampler; it just must be trimmed to be below 1 meter in height. But again, it is my opinion that vegetation materials moving into the lake are an appropriate source material to be added to the TP/TN flux into the lake. We might not want this in the wet deposition sample, but it should be considered a real input to the total flux.
46. Page 22: Is there an open pit mine near the site? This could be a heavy loading of TP to the lake and emissions from this source need to be carefully addressed.
47. Page 23, Para 1: "It is recommended…and ions." Agreed. It is very uncertain how well (if at all) aerosol and gasses are collected by a plastic bucket. This was the reason that NADP discontinued this dry deposition measurement early in its operation. It is also very uncertain how accurately it captures particles, along with how well it keeps particles from being scoured out by high winds after collection. I further agree that the best manner to estimate dry deposition is with atmospheric concentrations and modelled deposition velocities, as stated previously.
48. Page 23, Para 2: DSU sampler. Although I think this sampler is a good design for capturing particles, it has yet to be vetted through publication. Therefore, it would be prudent to treat it as an unproven method as of this point. It would also not capture aerosols or gases accurately as just described, since it is an operating Aerochem collector. Further, using the screen covering would reduce sample contamination as stated, but it would still be problematic.

49. Page 24, Para 1 & 2, contamination. As I have stated previously, the idea of contamination must be clearly worked through. Again, some "contamination" is generally considered contamination to a sample (bird feces, for example). But others are real fluxes to the lake (vegetation, insects, suspended particulates, etc.). In addition, if there is a large population of birds in and around Utah Lake, which is very likely, you can make a very good argument that bird feces could be a large contributor of both TN and TP to the water body and should be considered in some way, even though it may not be truly wet or dry atmospheric deposition. I would call it closer to "atmospheric litterfall", but it could be important signal from the atmosphere (in a strange sort of way).
50. Page 25-31: The document is certainly well referenced.

Review of Document
David A. Gay, Ph.D.
November, 2019

Document: *Updated Interim Report on Nutrients in Precipitation on Utah Lake* (Utah Lake Precipitation Report 3a), by Dr. Wood Miller, 4 April 2019, 13 pages.

This study strikes me as a generally straightforward and scientifically credible study of bulk deposition to Utah Lake. These are straightforward measurements of bulk deposition to sample sites well distributed around the lake. They give a historical record of these concentrations for two plus years for context and comparisons to other studies of the area. I do have a few issues with the measurements and quality assurance of the data, and with the issue of bulk deposition rather than wet and dry deposition. Bulk deposition is a first order approximation of deposition, but it does not capture all of dry deposition, so there is a bias vs. deposition measurements or estimates. Also, I do not think that the precipitation collectors are the best collectors for wet deposition or dry deposition, or bulk collection. But the study does show a low-cost method for collecting basic deposition information for the area.

General Comments:

First, using the National Weather Service precipitation collectors for wet deposition measurements. Although I think it is a use of available resource, there are some potential problems with using these samples. Therefore, these results should be considered with the following potential issues:

a. These are not wet deposition, but a bulk deposition samples of both wet deposition and some dry deposition. These samplers are not a good collector of dry deposition (i.e. gases, small particles), and it is likely that the samples include some TP and TN from larger particles during dry days. This dry deposition is added to the wet deposition samples and these concentrations/fluxes. So, the samplers are likely to be biased versus wet deposition measurements.
b. Evaporation from the sampler is an issue, as with all precipitation collection, which can bias the concentrations (again toward higher concentrations) as the length of time increases that the sample is left in the collector. During the summer, this effect

could be significant, as it is in some NADP samples which are designed to minimize evaporation.

c. Contamination from one sample to the next. The paper does report that some effort was made to clean the collector between samples. But particularly since TP is being measured at very low levels, I worry about TP remaining in the collector and being recorded in the next sample. NADP goes to considerable expense to ensure that this does not happen. NADP cleans buckets in laboratory conditions and still finds trace level of contaminant. This also brings in the question of the quality of the water used for the cleaning (QA records could be important, gloves used for the sampling? etc.).

d. It is my understanding that loss of TP to the walls of plastic buckets can be an issue. This is part of the reason that NADP is careful about the release of ortho-P values. This could also be the case with stainless steel collectors. However, I do not know that this is the case, but it is a concern for this particular study. This type of thing could lead to low bias of concentrations, and contamination of the next sample with phosphorus.

e. Loss of analyte due to evaporation (N compounds), and bacterial action reducing concentrations (N&P). This is always a concern with wet deposition sampling in any natural setting. With extended periods of samples remaining in the collector, this issue would become more important.

f. Typically, the NWS removes the funnels during the winter months when snow collection is the dominant precipitation type. Were the funnels from the gages removed during the winter for these samples? This will change the sampling conditions between warm and cold precipitation events, and perhaps change the concentrations measured.

g. Typically, NWS rain gages are mounted very low near the ground (sample opening lower than 1 meter). In a dusty local environment, this could lead to dry deposition from the very local area, that I would consider contamination.

h. NWS collectors, depending on the type used here, will sometimes have a funnel at the opening of the collector and sometimes not. It is important to know what type was used for certain. It is my understanding that these samplers used funnels. Funnels should reduce the amount of dry deposition added to the wet deposition collection, but not eliminate it, so it would remain a bulk deposition collection. Without a funnel, I would expect heavier dry deposition in the bulk collection. But I would also suspect added evaporation from the wet precipitation collection. If the funnel was used in the summer and removed during the winter, as NWS sites do with certain collectors (open when snowing), this could add another issue of slight biases between winter/summer collection.

Second, I am not familiar with the Chemtech-Ford Laboratories. They could be quite appropriate for these chemical tests, but there is no quality assurance information reported in this paper. It would be appropriate, in this context, to look into a) basic quality assurance information for the laboratory, b) ensure that long term quality assurance is present, and c) review of basic sample run information associated with these samples and the specific tests (pretesting calibrations, during test calibration checks, post sample calibrations, any independent blind testing protocols that they follow, etc.). I bring up this topic due to my role of review of the results. If this were an academic report/journal article, I would expect to see some type of quality assurance information on the analysis.

Third, the biological side of this particular paper is really out of my area, and I have no professional opinion with any scientific weight. I don't really know how much it takes to make a lake eutrophic. However, another question is, how long has this lake been eutrophic? Is it the case that this has been a long-term problem, or has it become a problem recently as the city has expanded in population? This type of record could provide additional evidence towards the fundamental problem associated with Utah Lake.

Specific Comments:

1. Page 1, Para 4: It would seem that the NWS was pouring off the precipitation collection into sample bottles. It is unclear if the precipitation sample used for depth was moved to a sample bottle, or if the samples were collected in sample bottles inside of a NWS precipitation collector that was not being used for precipitation depth. If they were active precipitation measurements, then contamination between samples is an issue. Regardless, the sample is a bulk sample, as noted previously.
2. Page 1, Para 5: It was noted that an orchard area was sampled. Is it possible that there are phosphorus-containing chemicals used in this orchard?
3. Page 1, Para 5: "southwest storm track." Can this statement be verified with the appropriate wind rose? If so, this would bolster the idea of Severe Lake particulate moving to Utah Lake, and associated high TP from the areas to the southwest. The wind roses that I have seen are all more associated with a northwest to southeast line of dominant wind.

4. Page 2, Para 1: “dust laden”. I would assume that these observations were recorded on the field sheets? Do we know which samples were dust laden? Looking at the results, Elberta (#5) does not appear to have high concentrations of either TP or TN, except for TP with no outliers. And in the average, it is only slightly higher than Mosida (#6). I would expect site 5 to be dust laden also.

5. Page 2, Para 2: “We realize…samples are.” I agree with questing the idea of how representative the samples are, given the dry deposition nutrients entrained in the sampling equipment. Local sources could be very influential at many or all of the sites.

6. Page 2, Para 2: Wet only samples. I agree, I would expect some differences if Olsen & Reidhead used wet only sampling equipment, since these samples are bulk deposition rather than just wet deposition.

7. Page 2, Para 5: Table 2-10 description. I assume these outlier concentration limits are reasonable. Within NADP, the samples that are above detection for ortho-P are very episodic and can be quite high relative to other samples and can appear to be “outliers”. However, in the West, it is possible that these episodic events are accurate and valuable and are not outliers. So, this decision is important, and I think the authors realize this by providing all of the data (with and without outliers).

8. Figure A: Based on my simple analysis, the high TP values are occurring at sample locations 2, 6, and 4, and the high TN values are occurring at sample sites 6, 2, and 3. It seems that the high TN is shifted somewhat to the north, and the high TP to the south. Most of the high values are on the west side of the lake, and not on the East nearer to the city. However, with southeast flow, the southern city could be influencing these samples, and it implies the city or the suburbs are playing some part here.

9. Page 4, Para 2: Outliers. The outliers constitute about 15% of all samples. Outside of the high values, are there any other indicators that they are outliers? Is there evidence of contamination, mishandling, other? Are there any field records by the site operator/sample collector? If there is, this would give us a better idea of the reality of outlier concentrations/samples.

10. Page 4, Para 4: “However...all the samples.” It is good that the author is questioning the outlier status. NADP would remove the large insects and debris, also. But as discussed before, this is considered sample contamination in respect to a purely wet deposition sample. But these insects/dust/debris are also being input to the lake, and are therefore part of the TP/TN flxu to the lake. For removal, it is important to

know—how exactly how did you remove this debris? Did you filter the samples? Did you just let it settle out? This is the likely manner. Exactly how this was done is important to ensure no contamination or analyte loss due to the "filtering" process.

11. Page 4, Para 4: Dirty precipitation samples. The author says that this is a common situation. How common was this? Is it possible that these dirty samples are the samples that provided the very high deposition routinely? This would suggest that large particulates are producing the high concentrations. Do you have field notes that could solidify this connection? The author also mentions wildfires. Large wildfire records are kept by the USDA-Forest Service (and other organizations). With back-trajectory software, a user can estimate the flow of wind (and potential wet/dry deposition) based on date. Were field records kept of the samples with potential indications of wildfires so this could be attempted?
12. Page 5, Para 3: I looked up the surface area of the Utah Lake to check this calculation (Wikipedia). They report an area of 380km2. With this, and using the 0.218 mg TP/L, I calculate a loading of 23.2 mtons/yr and/or 25.5 US tons/yr (it isn't clear if US or metric tons were used). But the loading calculation is generally correct at 20 Tons and 80 Tons (at max concentration, I get 90 Tons). I would assume that the area of the lake can change drastically during wet vs. dry years, summer vs. spring, etc. Are there significant areas of wetlands or perhaps significant areas that are wet during some years and would eventually contribute deposited TP and TN when flooded? Is this occurrence large enough to affect the calculations of deposition to the lake? It would seem possible that in wet years where the lake is significantly larger and submerges areas with previously deposited TP/TN, these fluxes could be an important contributor after the fact of deposition.
13. Page 5, Para 4& 5: Dr. Merritt's comments. If these numbers are all correct, then Dr. Merritt is also likely correct; there is very little that you can do, and the engineering controls to try and get to this level will be very expensive indeed. But I think you can look at other lakes in the area that are not receiving outflow from any sewer district and roughly the same wet and dry deposition (assuming it is near the city) and determine if these are also undergoing eutrophication. If they are eutrophic, without sewer addition, then you can reasonably conclude that the atmospheric deposition is enough to force eutrophication, and that any reduction to the sewer TP (and or TN) will not stop the problem.
14. Page 5, Para 4 & 5: Two ideas come to mind: First, a large source could be runoff from city streets where precipitation is bringing in wet deposition loading and

washed off dry deposition loading from the buildings, parking lots, trees, etc. With mercury, this is a huge amount of Hg added directly to water in one large pulse (Fulkerson et al). Second, there is likely to be a large amount of TP in the lake sediments. If the lake is not deep (which is reported), then wind will create lots of waves, and the waves will continually mix the sediments up and reentrant the TP in the sediments. Is the water typically cloudy? Are there many suspended solid samples?

15. Page 6, Table 1a: At almost all sites (7/9), summer concentrations are greater than winter concentrations by generally three times, as I would expect. So, this would indicate agriculture, or natural/urban dusts, or wildfires as important sources.
16. Page 6, Table 1a: Why are there no outliers at BYU site? And only one at Spanish Fork? This is a very interesting result. Both of these sites are on the extreme part of the city, and are against the mountains. Is this a clue that they are far from the source, or upwind of the source of high concentrations? Does this suggest the city is part of the problem? Or another indication that agriculture is an important input? Is there another source on the west side of the city? It also suggests that the "outliers" are not analytical outliers because you would expect these to be at all sites roughly equally.
17. Page 7, Total averages: This is a small point, but there are two summers, two and a half winters, two falls, but three springs recorded. Therefore, this biases the overall averages slightly toward spring and winter concentrations and away from the other seasons.
18. Page 7, Figure 1a: The two sites with the highest concentrations (all pts, LP, Gen) are very close to each other, suggesting a large source close to the southwest edge of the lake, and leading to high episodic deposition. These sites are near the suburbs, a steep hillside with natural particles, and what appears white in the satellite view (extreme south of Utah Lake, SW of Genoa). Is this a mine, or salt deposit? It is unclear using Google Earth. It is also interesting that Mosida is similarly very high in average concentration, but does not show these extremely high "outliers".
19. Page 8, Summer concentrations: LP, Gen, Mosida are the sites showing generally the highest deposition concentrations, with and without outliers. LP tends to fall away with no outliers. But the other two remain high with no outliers. Mosida is closest to the irrigated ag. fields, and the two sites are closest to the white area from above. Elberta is also close, but generally upwind in the summer, so it shows lower concentrations.

20. Page 9, Winter averages: Concentrations are much lower and are all much closer to each other, although Elb and PP, LP and Mosida are still relatively high. This is suggesting perhaps that in winter, the concentrations reflect more regional sources. Most of the "outliers" go away in winter, suggesting that the outliers are real, and associated with an important summer source (agriculture, bird activity, wildfires, no snow on the ground allowing for natural dusts, etc.).
21. Page 10, Table 1b: For nitrogen, most summer concentrations are greater than winter concentrations (8/9) as expected, and 6/9 without any outliers. Also, the TN concentrations have less nitrogen loss (or more appropriately compound conversion and some evasion) due to bacterial activity. The high values are on the west side of the lake, PP, Mos, LP, and very similar to TP. However, they seem to be a bit farther west and north. This is the case in summer and for all concentrations. In winter, it reverts back to the south two points.
22. Page 11, Figure: Without outliers, all of the sites have pretty much the same concentration, with Spanish Fork having the lowest average concentration. The highest three sites PP, Mos, LP are not far from the hillside, near agriculture and irrigations and downwind of the suburbs.
23. Page 12, Figure: Without outliers, Lehi is the highest average, and has few outliers. This is again on the west side, but farthest north (ag.?). It appears that the wintertime relative TN concentrations are not as low as the TP winter averages become. This would suggest two separate source groups, at least to me. TP is more seasonally dependent, with important summer sources. TN is not so seasonally dependent, which could suggest an anthropogenic source and not an agriculture source.

Review of Document
David A. Gay, Ph.D.
November, 2019

Document: *Comments on the white paper: Estimating total and bioavailable nutrient loading to Utah Lake from the atmosphere*, by Theron Miller, 7 pages.

Overall, I think the Miller document does make some good points on the White Paper by Brahney et al. However, I do not agree that Brahney et al. can be dismissed out of hand as something more than an educated guess. I think it is the case that the White Paper is a well-documented and reasonable estimate of deposition to Utah Lake.

Specific Comments:

1. Page 1, Para 1, etc.: Although I do think that Miller makes some good points on the differences between Utah Lake and Lake Tahoe (used as an analogy by Brahney et al.), I do not think that it is "unfounded". For example, I agree that Lake Tahoe is much deeper, the population around the lake is much different, there are no agriculture areas near, the elevations are different, the valley characteristics, etc. are all very different. Dr. Miller does make a good point that the surrounding lake sediments are significantly different than that in Lake Tahoe, again making the argument that there are significant sources of phosphorus to Utah Lake. I do not think there is an argument against these points.

   Further, Miller notes that the dominant wind direction is from the Southwest. However, based on my limited searching, the Provo Airport does not show a dominant southwest wind, but a clear southeast and northwest wind dominance. This is also shown by the Dugway Proving Ground records, showing a southerly dominant wind followed by northwest and southeast components. Additionally, the local wind patterns shown in Brahney are clearly southeast and northwest dominant with occasional winds from the southwest (I cannot verify these figures, however). These are all surface wind roses, and it could be that upper air is dominantly southwesterly (I am fairly sure that a westerly component is important). But I have not seen evidence of a dominant southwest flow.

   However, I think it is fair to say that Dr. Brahney used the Lake Tahoe analogy to define the rate of particle loss as the plume moves over a lake and an open water surface. Using this analogy seems reasonable to me, particularly since the measurements were done on this basis. I don't think any of the comments about lake differences affect how Dr. Brahney used this information about the loss of particles to a lake surface. She did not use the deposition rates as many of Miller's comments focus on, but instead used the rate of deposition to the lake surface combined with the particulate levels at Utah Lake in her calculations. Miller's point that the southwest winds blow the particulates towards a wall (i.e. restricting dispersion of the plume) which could be the case. But Dr. Brahney was clear about how she took measurements from another study and how she used them. I commented that she could have provided more specifics, but I don't think her use of these measurements was misguided. She chose this study given that few studies exist, and I think it was reasonable to do so.

2. Page 2, Para 1: NADP Sites. I agree that these NADP sites do not properly represent the Utah Lake Valley as stated. However, these sites measure only wet deposition and not dust samples.
3. Page 2, Para 1: I do not think it is reasonable to say that Dr. Brahney ignores local sources. I think the difference is that the NADP rules are designed for a regional wet deposition network, and avoid local sources to keep the regional conditions (the goal of NADP). I do agree that local sources should be accounted for, given the goal of estimating TP/TN flux to the lake. Important local sources need to be accounted for.
4. Page 2, Para 2: Sampler locations. I agree that avoiding open pit mining and housing development sources would be a mistake, given that these are likely to be important sources of TP/TN to the lake. However, I think both parties would agree that these plumes cannot be the only plumes considered, given that they are likely to be large flux contributors. They need to be accounted for, but cannot overly dominate the flux estimates to the lake. I agree they are important to characterize as best as possible. I would not avoid significant local sources.
5. Page 2, Para 2: I agree that dry deposition measurements are still evolving, and that it is likely that specific methods will need to be considered for this particular study. As I have noted before, measurement of dry deposition is a difficult one to make, and fraught with assumptions. Wet measurement methods are much more specific and consistent between measurement projects.
6. Page 2, Para 2: "The only challenge…equations." I do agree with these statements. I think these are the basic issues: how exactly to make the measurements, where to place the samplers, and how to approach the attenuation equations with the

assumption that dry deposition measurement is the approach chosen. Some of the same issues will occur with an atmospheric concentration and modeling estimate approach.

7. Page 3, Para 1: I do not agree that the statement is misleading since "up to two orders" was used to preface the comment and the numbers were referenced. It is clearly a statement of the measurements relative to a recent journal article. It is clear that the measurements are very high. It does not mean they are wrong, but they are high. Even the low end of the measurement range is 15% higher than the article range of concentrations. I cannot comment on the amount of phosphorus in woodsmoke, since I have not worked in this area, nor have I read the Dolislager et al. paper.
8. Page 3, Para 2: I agree that the decay rate can be debated and other methods of estimating urban deposition could be used, but the Brahney method is a valid method based on measurements and referenced articles. However, there is no evidence that this method is the best way to perform this estimate. I agree that urban pollution is moving over the lake on all days. But I read Dr. Brahney's paper as estimating a regional signal (which includes a general urban component when the specific study is located near urban areas), and then adds to this signal an additional urban signal based upon when inversions occur. I believe that Dr. Brahney assumed the entire lake surface during an inversion because higher concentrations during the inversions are present and extensive over the lake, and likely impact the entirety of it. The impact of urban influences extending to 400-600 meters during normal times is appropriate for particulate flux which have relatively high deposition velocities.
9. Page 3, Para 2: "Thus I suggest…of the year." I think multiple people would make different assumptions and could conclude that a more important urban signal is appropriate. But I would then argue that this approach follows the idea that there is a regional flux, and an urban flux, and that they need to be calculated separately. I read Dr. Brahney's paper as we have a general regional signal present, and in the case of inversions, we have a particularly strong signal urban signal to be added to the general regional signal. Dr. Miller argues that his measured values include all of the above, and that they are more appropriate values. This could also be true.
10. Page 3, Para 3: Nitrogen: reading through the estimates from both studies, my thinking is that they are pretty close, between 92 and 529 mT TN, and somewhat support each other. Together, they give a range of values where the actual value is likely to be within. Although the numbers are large and readers would hope for

more specific agreement between the two approaches, I think they are reasonably close. Dr. Miller mentions the insect issue, which I have also mentioned previously. I think insect flux of TP/TN is an important issue to examine and an important flux to add. Part of the issue is that in a traditional definition, insects are typically not thought of as wet or dry deposition.

11. Page 4, first line. I have not seen evidence that the Olsen estimates were deemed to be not credible. Perhaps there is a different document where this occurred, but that is not my understanding of the measurements. After reading his thesis, I think that it is a straightforward and reasonable approach and approximation of the flux. I do have a few issues with the study (detailed elsewhere), but I would not say that the study is not credible. And I do agree with Dr. Miller's contention that the N fluxes are very similar, and therefore do add credibility to their approach. But I would also say that it does lend credibility to Dr. Brahney's approach.
12. Page 4, conclusions. 1) I would agree with this conclusion. I have some issues with the study, but it is my opinion that Olsen's work is a credible estimate for flux to Utah Lake. 3) I don't think it is correct to say the TDep Working Group was part of this modeling effort. The TDep results are completely separate from this effort, and this project/issue was not considered in their work. It is certainly possible that if the TDep deposition rates were used as suggested here that the rates calculated would be closer to the Olsen rates. 4) Agreed.
13. Page 4, Para 2: "It appears…valid." I don't agree that the white paper decisions are questionable, but that appears to be Dr. Miller's opinion. As stated before, I do believe that the Olsen estimates are valid for this discussion.
14. Page 4, Para 3: "…it is far…white paper." I do not think that the NADP objectives have anything to do with the project. The NADP objectives are completely separate from the questions here. I think what Dr. Miller meant is that the data does not meet our quality assurance guidelines, and was not considered.
15. Page 4, Para 4: "…that 90%...is unfounded." I have not read the original Lake Tahoe paper, therefore I cannot comment specifically on this. But as presented here, it is possible that the conditions of the Lake Tahoe study are as stated. But it is not stated this way in the White Paper.
16. Page 4, Para 4: "Whether the dust …across the lake." I agree. Many factors will go into determining the dispersal and deposition of particulates across the lake, including particle size, wind velocity, temperature, and many others. The approach

taken in the White Paper is relatively simple, but it is certainly valid. But other more complicate methods can also be undertaken for other estimates of the same.

17. Page 5, intro. Para: "…are barely…educated guess." My sense is that this statement is a bit strong. See the comment above.
18. Page 5, intro. Para: "Moreover...warrant investigation." The bioavailability of the phosphorus is beyond my specialty, so there is little I can comment in this section. However, it seems reasonable to me that measurements of different conditions could be made to estimate the ability of the lake/sediments to change the bioavailability of TP in the lake.
19. Page 5, Para 1: "In addition…usable results." Although it would take some work to determine exactly what would be required to measure atmospheric concentrations of particles/etc. and estimate the deposition velocity as an alternative method for determining flux to the lake, I think that this approach is a valid one as I have suggested in other document reviews. A good case in point is the NADP and its TDep approach. Also, the Clean Air Status and Trends Network of EPA (https://www.epa.gov/castnet), which is a network making estimates of dry deposition. This is the method that they use; i.e. measuring atmospheric concentrations and modeling them down to the ground. The CASTNET data is used by TDep for their dry deposition estimates. Again, I am not exactly sure what level of effort and cost would be required, but I think it is an alternative method that the board should at least consider.
20. Page 5, Para 1: "Moreover…crew and captain." I would have to agree that making measurements of particulate over a lake would at times be very difficult. These measurements in general would also be very expensive. However, if some measurements of particulate load in the lake are possible, this would begin to narrow down the idea of how far lakeshore sediments travel into the lake.
21. Page 5, Para 2: In short...Utah Lake." I agree that the regional signal of wet deposition captured by the NADP sites in north and east Utah are probably not appropriate for this project. But NADP measurements are regional by design, and in the intermountain West, those regions can be relatively small. However, if helpful, an NADP site could be established in the Utah Lake basin.
22. Page 5, Para 2: "Rather…nothing more". I disagree with this point. It was a relatively straightforward and reasonable approach and was backed with many detailed references, but I do not think Dr. Brahney claimed it was the only approach. Further (next sentence), being a person that makes measurements, I agree that more

measurements can always add to the discussion. The point of a midlake monitoring location seems reasonable to me, if this can be done for a reasonable amount of money. You could also use it to test the idea of urban influences during inversion conditions.

23. Page 6, Para 1: "The guidelines…soils in place)." This is the case. The NADP guideline is that the grass/vegetation around a sample be kept low so that it does not affect the operation or chemistry of the sample being taken. It does not require there be no vegetation around a sampler.
24. Page 6, Para 1: "As well…three years." I think this is a good idea which can be used to test the consistency of older data with more current data.
25. Page 6, Para 2: Insects/contamination. I have mentioned this point many times in these reviews. Insects can be considered contamination in a wet deposition sample (but not always), and are certainly likely to change the chemistry of the sample (Dr. Brahney's point). But I also agree with Dr. Miller in his opinion that the flux of TP/TN in organisms dying and falling into the lake could be an important contributor to the chemical flux to the lake. I think it is important to note that insects found in NADP samples do not disqualify samples, but a flag is assigned to a sample (with insects) to warn the data user that the chemistry of the sample could be affected. It does not mean that it is disqualifying. Finally, this adds to the point that the insect flux is likely important, but likely not considered wet or dry deposition but some other form of chemical flux to the lake.
26. Page 6, Para 3: I agree with the first sentence that both studies can likely be improved, and would likely lead to estimates of flux that will come closer to each other. That seems to be the case in almost all field work—by building on what you learned from the first experiments. I also agree that the suggestions of 1 and 2 are important ways to improve the estimates to date. Both of these seem reasonable ways to improve the estimates. But again, as I have said before, #2 is not my area of expertise and have little to add.

Review of Document
David A. Gay, Ph.D.
November, 2019

Document: Jacob M. Olsen – *Measuring and Calculating Current Atmospheric Deposition*, Thesis, 2018, Brigham Young University.

I reviewed this thesis and the resulting paper from this work (*Measuring and Calculating Current Atmospheric Phosphorous and Nitrogen Loadings to Utah Lake Using Field Samples and Geostatistical Analysis, by Jacob M. Olsen, Williams, Miller and Merritt*). Most of the comments are applicable to both documents, with many of the comments repeated here.

Overall, I think the thesis is a nice bit of work; from measurement through analysis and making sense of the information. In my opinion, it is a nice thesis all around.

First, one of the most important criticisms I have of this study is the lack of information on the correct operation of the sampler, the proper function of the sampler's sensor, and the lack of quality assurance information about cleanliness etc. of the sampling equipment/supplies and analytical work. Although there is no information that these types of issues occurred, there is no record presented that these issues of true contamination did not occur.

Second, I found a significant number of calculational differences (they tended to be small), and these are detailed below.

Third, the location for the Saratoga Springs site, with the largest concentrations and fluxes into the Utah Lake, make the results are particularly sensitive to this site and its sampling location. With a small difference in location of the sampling site, I can see where the flux estimate could increase or decrease drastically as the TP/TN source has a larger or smaller impact upon the wet and dry deposition flux estimates made here. I do agree that all sources need to be accounted for in the flux estimates (the authors say "local deposition"). And I also concur that the definition of contamination is complicated for this study, particularly with the addition of bees and other insects included in the sample. I have shared a few of my concerns on this contamination issue below.

Fourth, there are a few assumptions for the study, noted below, that I either do not understand or that I do not agree with. One in particular is the background value for total nitrogen (TN) used in the lake study. My understanding would lead to a significantly different flux value for Utah Lake. Other points are listed in the specific comments below.

Specific Comments

1. Abstract, Page ii, Para 4: "The results…cyanobacteria." This is an excellent point. Has anyone looked to see if there are other shallow ponds in the area, to see if they are also affected by algal blooms? They would have roughly equivalent atmospheric deposition, and little to no input by treated waters. It could be a good idea to add this into the developing sampling plan.
2. Page 1, Para 1: "atmospheric deposition…budget." Agreed, atmospheric deposition is quite often neglected, and it can often be important.
3. Page 3, Para 2: "NADP…TP in precipitation samples." Just to be clear, NADP does not measure total phosphorus (TP) as stated, but we do measure soluble ortho-phosphate precipitation concentrations.
4. Page 4, Para 2: "There are three main classifications…dry deposition." I don't think of deposition quite like this. The way I view it, there is wet deposition by precipitation and dry deposition through gravitational settling. And bulk deposition is wet deposition plus some dry deposition. Bulk deposition is typically what is measured when power is not available, and in remote locations.
5. Page 5, Para 1: Wet deposition. I have always considered the sampling of wet deposition independent of receptor surface. However, as Olsen mentions, this choice could have error associated with it, such as loss of analytes to the side of the receptor or bucket, loss of volatile compounds over the sample period, correct operation of the sampler, etc.
6. Page 5, Para 2: Dry deposition. Olsen lists many sources of dry deposition, which are all correct (there are also other sources). But with washout of the atmosphere, these can all be seen in wet deposition samples too. I see deposition sources with part of the source signal depositing as wet deposition, and some of the signal depositing as dry deposition. An exception is made for non-water-soluble analytes where little of this mass comes out in wet deposition (gaseous elemental mercury, for example).
7. Page 5, Para 2: "The process of dry deposition…wet deposition." I agree that dry deposition can be much more important than wet deposition under some circumstances, and this is likely to be the case in the western United States where drier conditions prevail. But it is also true that wet deposition can be much more

important than dry deposition, and is likely to occur in areas where precipitation rates are very high.

8. Page 5. In addition to the deposition discussion here, I will note that there are other forms of deposition, including litterfall and throughfall. I have not been to the Utah Lake site personally, but based on the satellite photos that I have seen, these processes of deposition are likely to be unimportant. But with trees and extensive grasslands near the lakeshore, these additional processes could be additional deposition types to consider in the overall flux to Utah Lake.
9. Page 6, Para 1: "Others may only…after the storm." I am glad that this research included a wet dry collector. Using water collected from a precipitation gage can be done, but it brings with it several quality assurance problems and interpretation issues. Some issues could include the mix of wet and dry deposition inputs to bulk collection, unknown cleanliness week-to-week of the collector, and possible contamination between weeks, among other issues. However, using gage-collected water can give approximate values when other samples are unavailable and power availability limits separate wet and dry sampling.
10. Page 6, Para 2: "…dry deposition…difficult." This is absolutely true. Dry deposition is a complicated measurement. It is associated with many assumptions, and also brings with it criticism given that there is no accepted method on exactly how to make these measurements correctly.
11. Page 6, Para 2: "There is a …surface for collection…". Agreed, there is a huge variability in the measurements. I also agree that a wet surface collector is probably better than many other techniques for estimating deposition to a water surface. But, it also suffers from its own limitations. The fetch of the measurement is quite different than the natural lake. Wave action is not present, the water chemistry is different than the natural condition, and most of all, the aerodynamics around the collector of any design are not consistent with the natural environment you are trying to mimic. I agree that a water surface sampler is likely better than others, but again, it is not a perfect sampler either.
12. Page 6, Para 2: "The NADP…plastic surfaces." This is not actually correct, at least not anymore. During the early years of NADP, dry deposition was collected in what is called "the dry bucket" (a plastic surface). However, those measurements were only conducted during the first few years of NADP (ending in 1985 or so), and the measurements were stopped because it was determined that the dry deposition measurements were not representative of anything real in the natural environment;

i.e., they were only representative of dry deposition to a plastic bucket, and therefore, not applicable to the aforementioned natural environment. So, the measurements were discontinued and the data was removed from circulation.

13. Page 7, Para 1: "Contamination…studies." I would say contamination is always an issue, regardless of whether the study acknowledges it or not, based on the NADP's experience. NADP spends quite a bit of time on quality assurance of its measurements and looks for contamination throughout its system. NADP continues to find contamination, even though its measurement technique is relatively simple and has been used for over 40 years.
14. Page 7, Para 2: "There is not…the deposition." I would agree. On the studies that I have read, there is no general agreement on what constitutes contamination, or what is considered important local emissions, etc. And unfortunately, my opinions differ on what I would consider important flux sources and contamination to wet and dry deposition.
15. Page 7, Para 2: "It is important…legitimate." I agree that the goal should be to determine the total flux, and that with local and regional sources likely contributing.
16. Page 8, Para 3: I would add the automobile and emissions from catalytic converters to this list of ammonia sources. Given the proximity to an urban environment, this source could be a very important one in the TN flux.
17. Page 9, Para 3: "TP incorporates…forms." I think Olsen means to say it "all soluble and particulate forms of phosphorus."
18. Page 12, Para 1: "An additional challenge…surface." I agree that local dust sources can elevate the estimate of deposition to Utah Lake, but I would not consider this to be contamination. I see this as an appropriate flux to estimate for Utah Lake. Olsen goes on to discuss dust bouncing along the ground for very short distances and that elevating the sampler is the key to removing this type of contamination. I agree that the appropriate height is at least a meter off the ground. However, the site sampler picture (Figure 3-2) shows that the sampler deck and the bucket openings both appear to be below 1 meter in height. If this is representative of all samplers, then this local area dust could be a contamination issue.
19. Page 13, Para 2: "Locations were…by the NADP." Distributing the samplers roughly evenly around the lake is certainly a reasonable approach. But the location of the samplers is quite influential to this particular project. I can see situations where moving one sampler one direction or the other by a modest distance would increase

or decrease the flux estimates quite significantly. Therefore, sample site selection is a very influential decision. Additionally, I would like to see more information about how well the sampler preformed under field conditions. It is very similar to the Aerochem, but one of the most important pieces of the sampler is the precipitation sensor and how well it worked/opened when precipitation is present. Any wet dry sampler is very dependent on the sensor, and the wet deposition measurements can be significantly changed by a sensor that operates differently than expected.

20. Page 14, Para 1: "Movement of…bucket cover." I have generally the same comment again: how well did the sensor work to open the sampler? I would have hoped to see some quality assurance information about the sensor operation in particular. This speaks to how well the wet and dry deposition measures were separated, and how getting the sampler open quickly during the initial precipitation minutes is very influential (typically, the early part of the precipitation event has very high analyte concentrations). My additional questions would be: "How long did it take after precipitation starts for the sensor to signal that it is raining?" "Is the sensor heated and does it require continued rain to remain open?" "Did dew cause the sampler to open?" and so on.
21. Page 14, Para 1: "activated by rain or snow". This suggests that the sensor is heated if it was used to collect snow, correct? A heated sensor is important in two ways; 1) It melts snow so that a sensor needing liquid to trigger opening will work in snowy conditions, and 2) It will continue to evaporate precipitation so that new precipitation has to occur to keep the sampler open. Was the sensor heated, and how well did it work?
22. Page 15, Figure 3.2: The sampler is very low to the ground in this picture. Assuming that all of the samplers are this height, was ground contamination an issue? i.e. Could particles bouncing along the ground with wind have contaminated the samplers? Typically, wet dry samplers are at least a meter above the surface to prevent this problem. Second, the solar panel is very close to the wet bucket in this picture. Again, if this arrangement is the same for all samplers, precipitation ("bounce") could potentially strike the solar panel, pick up dry deposited contamination from earlier days, and bounce or drip into the wet deposition sample. This would clearly be contamination in the wet deposition sample, leading to double counting of analytes. This arrangement of samplers would not be allowed in NADP because of this issue. I understand that Olsen is using the collector for additional solar panel bracing, but that is problematic for contamination to wet deposition.

Additionally, I don't see the sensor in this picture. I would assume it is on the other side of the buckets?

23. Page 15, Para 1: "…in accordance with NADP protocol." It is not stated, but the thesis implies that the samples were collected every Tuesday morning, resulting in weekly integrated deposition samples. Is this correct? Or were they collected right after a significant precipitation event? If so, how long were the buckets left in place before an event?
24. Page 15, Para 1: "In order to simulate…deposition rates." I understand the need for a compromise of putting a large volume of water into the dry side bucket to simulate the water surface for the flux measurement and keeping the volume low enough to concentrate the analytes so that you can measure the dry deposition in the laboratory. But, if it is an approximately 5-gallon bucket, as I calculated, then it is only 14% full (3L of 21L). This means that the liquid level is well below the sample bucket opening. This makes the surface less like a water surface with wind flowing over the sampler opening; essentially the wind is flowing over a 35cm plastic bucket with a little bit of water at the bottom of the bucket. It will certainly trap the larger particles that fall into the water and keep the wind from sweeping out collected particles. But this is another example, as Olsen has mentioned, of how difficult a dry deposition measurement is.
25. Page 16, Para 1: Supply cleaning. Was any quality assurance done on the cleanliness or the potential contamination over time of the supplies and cleaning procedures? For example, were blanks done on the cleaned buckets, or travel/field blanks of the buckets? Although there may not be any contamination of significance, it would strengthen the argument if this type of information was tracked and reported.
26. Page 16, Para 1: Water used in dry deposition bucket. I have the same comment as above. It would be good to see what the values were that were blank corrected from the concentrations of analytes. Where they significantly large?
27. Page 16, Para 1: "Contamination…than a week." It would be good to see a listing of how often bulk samples occurred due to sampler malfunction. It would also be beneficial to see a listing of how often bird droppings and algal growth in particular occurred. I would consider bird feces in the bucket to be a true analytical contamination that would take that particular sample completely out of the study, since that TP and TN could be quite high in those particular samples. Also, I would consider algal growth in the sample to be an indication of a sample contamination problem, and I think I would have thrown these samples out also due to

contamination. Were overlapping samples a frequent occurrence? This would have an impact on the accurate operation of the sampler and particularly the accuracy operation of the sensor. How often were samples left on the samplers for longer periods than a week? Ammonia loss with long-term samples is a particular worry. In all cases, it would have been good to see an appendix record of these issues.

28. Page 16, Para 2: "TP was analyzed…reduction." In the Olsen et al. Hydrology paper, it says that the samples were digested so that both the soluble and insoluble TP could be analyzed. It isn't clear here that this is the case, but is it safe to assume that it is?
29. Page 17, Para 1: "I calculated…nutrients deposited." I understand why Olsen did the calculations this way. It is simple and straightforward. However, bucket catch is notoriously inconsistent when it comes to a record for precipitation depth as compared to recorded gage measurements. If you have poor bucket catch, you could be missing wet deposition flux or adding precipitation flux that is not likely there. NADP uses an additional precipitation gage at each site to ensure that the sampler worked properly (comparing gage precipitation depth to bucket catch precipitation depth), and to know when the sampler captured the correct amount of precipitation. Although, I also realize that funding can be an issue and limit the amount of equipment one can purchase for a project.
30. Page 17, Para 1: "A step-by-step…Appendix C." I appreciate Olsen reporting the calculations in the appendix. That is good scholarship. But I found some inconsistencies in the tables and calculations. See the comments below.
31. Page 17, Para 2: Lake Area 354 km^2. This is not exactly the same as used in Appendix C, which is 345 km2. See comment below.
32. Page 17, Para 2: Variogram chosen. It would seem to me that the variogram selection is particularly significant to the final flux estimates. Did Olsen do any tests by changing the variogram choice to see how this affected the final flux measurements? Basically, was a sensitivity analysis done?
33. Page 20, intro Para: "These six points…(NADP, 2014)." I am somewhat unclear about how the background values for the six points in the lake were determined. I would assume that the TP value of 0.019 mg TP /m^2 week came from the listed reference. However, the TN value of 0.112 mg TN /m^2 week is referenced to be from NADP. First, the reference is odd, since it is a reference for the NADP Site Selection manual. But assuming that Olsen meant the reference to be a general NADP reference, I compared both the wet (NADP maps, 2018) and dry deposition (from

TDep, 2018) values. I may have done the conversions incorrectly, but I calculate the wet flux in the area of Provo to be about 3.85 mg/m^2 week, and to be between 0.96 and 5.77 mg/m^2 week for dry deposition, again as measured by either NADP or estimated by NADP TDep. (I will admit, I did not pull out the grids, but just picked colors from the site location on the maps.) The value Olsen uses for DIN of 0.112 mg DIN/m^2 week is significantly lower. The NADP values are regional values, and, if used, would significantly increase the estimates of TN to the lake by increasing all of the cells in the provided lake model. I am unclear how Olsen arrived at the 0.112 value, and why they used this value rather than the regional value as measured by NADP?

34. Page 20, Para 2: "For sites…for that week." I have comments below on missing weekly data, particularly data not shown at any of the sites for November and December. See below comments on figures and Appendix C.
35. Page 22, Figure 3-6. It would improve the figure with a color ramp representing the deposition ranges.
36. Page 23, Para 2: "The comparative…Appendix A." I found a few issues with the comparison between the labs. See Table A-2 comment below.
37. Page 23, Para 2: "Analysis on…the analysis." I think the same critique can be made for bioavailable phosphorus. I would assume it would remain in the sample, but a couple of months is worrisome for quality assurance data.
38. Page 23, Table 4-1. The values of samples collected by site do not match the sample numbers provided in the text. I would assume that the difference is that samples were removed for analytical contamination and some samples were not collected due to sampler malfunction. It would add to the discussion if a short explanation for this difference was added in.
39. Page 25, intro Para: "In addition…exceeding the mean." This is fairly normal in wet deposition monitoring, particularly with concentrations measured in dry environments.
40. Page 25, Para 1: "Precipitation data…significant differences." I assume Olsen means that they compared the bucket catch estimates of precipitation depth. As I noted before, these are notoriously variable. But if there were no significant differences in the mean, this is a good result.
41. Page 26, 27, Figures 4-1 and 4-2. These figures all show that no samples (or at least no results) were present from mid-October through mid-December. But there is no

explanation for why these samples not in the graphics. So, I must assume they were not collected or used. However, there are values representing total deposition to the lake for this same time period (Figure 4-3 and 4-4). Where these values substituted in for missing sample weeks? If so, how were these values calculated? It would seem from the summary figures that the deposition was minor each week, but it could be important in the flux to the lake for the eight-month period.

42. Page 28, Para 1: "This is thought…industrial practices." There is likely an important source nearby, correct? Google Earth shows what appears to be gravel pits to the northwest (one of the dominant wind directions), making this site downwind of a major source. This source could be particularly important to the conclusions made. By moving this site 20 meters in either direction would likely significantly affect the fluxes measured, and the flux could even be higher than determined with the sampler located where it is.

43. Page 28, Table 4-4. Given that Olsen added in this analysis, the results presented are clearly showing the importance of the measured values from the Saratoga Springs site. It would seem that the model results are dominated by these measurements, making the location of this site one of the important decisions in the study. I do agree that the sources in this part of the lake are important to include in the flux. But it is also true that a subtle difference in location of this particular site could have a significant effect on the result; potentially making them either higher or lower.

44. Page 28, Para 2: "The higher value…analysis." I agree that this is a reasonable way to bracket the study and determine the low and high estimates of fluxes. But with my earlier comments about bird feces being a real contaminant in a sample, it is possible that Olsen's higher flux estimates could be inflated by feces, etc. and true contamination (at least in my definition).

45. Page 29, Table 4-5: In the weekly loadings table, what do the dashed lines mean for the uncontaminated samples? It isn't clear from the table caption or text. Does it mean that there were no uncontaminated samples? Was nothing used for the weekly total for these seven weeks? Does this bias the flux negatively? Or was another manner used to account for these weeks?

46. Page 30-32, Figures 4-3 to 4-6. As I mentioned previously, the individual site measurement figures show no values after mid-October, but the summary figures for all sites show values representing flux for these weeks. A clear explanation for this discrepancy is important.

47. Page 33, Para 1: "For example…during the summer months." Olsen notes that the contamination by bees could be one reason why the Saratoga Springs values were so high. Earlier, it was also suggested that the local bare soil and industrial operations could be the cause. I think all of these could be true, since much of the high values go away during the summer. A significant part of the conclusions for this study hinge on the Saratoga Springs measurements, with several potential rational reasons for these high values suspected. Further work on these sources would seem appropriate. Were there any samples in the summertime without bee contamination? The high values generally are not present in the fall season, when I would assume that the industrial processes (gravel?) would still likely be operating? Were the bees not present in the fall? This also begs the question of whether the bees are also being deposited into the lake at a significant rate for added TN/TP. It also brings out the question of whether bees in the sample are contamination or not. I think that bees dying and being added to Utah Lake are potentially an important flux to measure/estimate. But they are also not generally considered wet/dry deposition in the usual sense. So, this comes back to these basic questions: What is contamination and what is not contamination? It would seem to me that one "next step" would be to tease out this potential bee signal from the industrial/agriculture signal, and determine the change in the flux rates without bees present. Then, make some estimate of the bee contribution separately from wet and dry deposition.

48. Page 34, Para 1: "The high spatial…sampling sites." I agree that this suggestion of important local TP sources is the case.

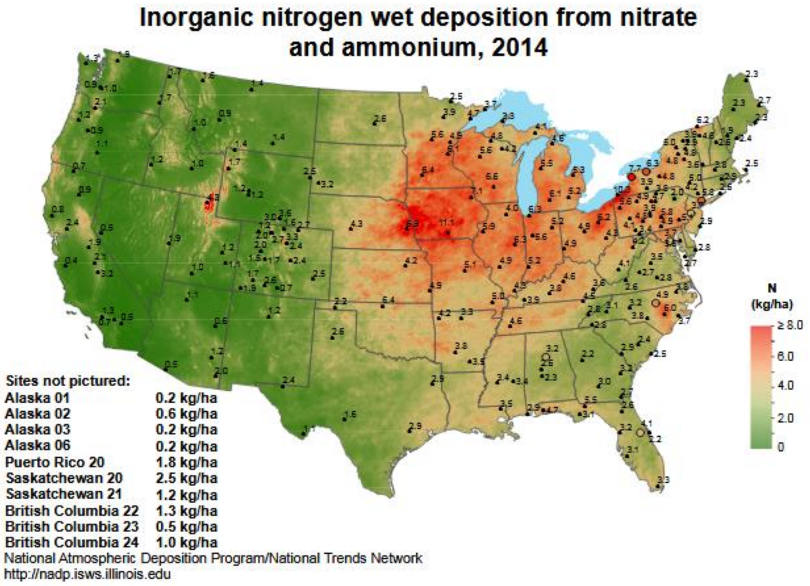
49. Page 34, Para 1: "By taking 1.02…(Table 4.3)." I am not exactly clear what Olsen means by this statement. Does it mean that global/regional sources contribute about 13% of the loading to Utah Lake, making the assumption that the Central Davis site in roughly regional deposition?

50. Page 34, Para 1: "However, due to…deposition." I agree that between year variation will be present, which strongly suggests a multiyear measurement campaign for further information.

51. Page 35, Para 1: "After interpolation…August 2017." It would seem that from what I have read, these weekly values are very high. It does not mean that they are necessarily wrong, but they are very high.

52. Page 35, Para 2: "These rates…(NADP, 2014)." There are some problems here with the comparison to NADP wet deposition rates. I am assuming the author is comparing this to NADP wet deposition map measurements. I looked up the total N

measurements from 2014 (see map , which can be found at http://nadp.slh.wisc.edu/maplib/pdf/2014/N_dep_2014.pdf) where NADP measurements show Nitrogen wet deposition flux rates (as N, rather than molecular NO3-+NH4+) range in Utah from 1 to 4.3 kg/ha year, and are roughly 2.0 kg/ha year in the Utah Lake area. These would convert to approximately 100 to 430 mg/m^2 year, rather than 2.14 to 8.42 mg/m^2 year as stated in the text. Furthermore, using the average flux rate from Table 4-3 of 10.35 mg/m^2 day, that rate for comparison is not 0.04 to 24.82 mg/m^2 year as stated, but more appropriately is 3,777 mg/m^2 year (assuming 365 days of flux, average of 10.35 mg/m^2 day). Alternatively, NADP wet deposition values would become about 0.5 mg/m^2 day, averaged over 365 days, or about 1/20th of your wet+dry estimate. So, the values do not really compare that well to the wet deposition values provided by NADP. Note that NADP is only wet deposition, and this study is total deposition. I also calculated total wet flux for the lake from NADP wet deposition values, assuming 2 kg/ha, which is approximately 78 tons/ full year for N. This compares to your values of TN deposition of 46 to 460 tons/eight-month year. Therefore, the NADP wet deposition estimate is relatively close to the no contamination estimate of 46 tons and an increased somewhat for the four months not sampled.

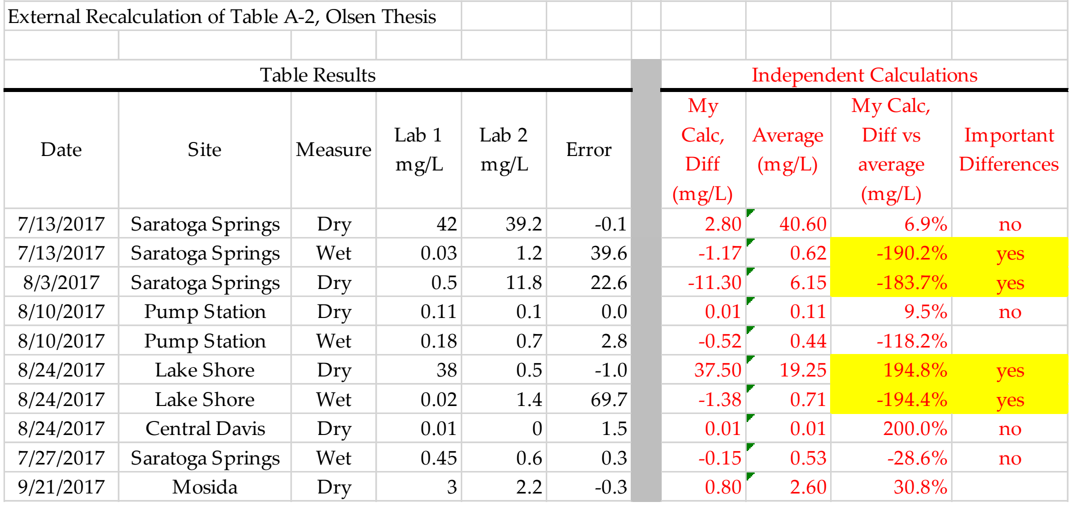


53. Page 35, Para 2: “When compared…reasonable.” The unit issue with the rate of deposition for the TN flux, when compared with a correct unit conversion, resulting in 3,777 mg/m^2 year may no longer compare as favorably to these references.

54. Page 44, Table A-1. Following the picture for the sampler, I think that the solar panel on the sampler likely added contamination to the samples. It presents a surface where dry deposition can deposit/adhere, and future precipitation can wash off of the panel and move dry deposition into the sample collection bucket and the wet

deposition collection. This could result in significant double counting of some dry deposition mass.

55. Page 45, Table A-2: Recalculations of Laboratory Intercomparisons. The laboratory intercomparison for the concentrations for wet and dry deposition were presented in the Appendix Table A.2. I was unclear as to what the error column unit was, and what some of the values meant. So, I calculated some basic comparison statistics (in red, see below). With this calculation, I found some differences between the laboratories that I would consider significant and important (highlighted in yellow), and two other values that some would consider significant. I realize that laboratory intercomparison can be difficult, and wide bounds need to be used (+-25%) for significant concentration differences. I found differences between the laboratories present in both directions (higher and lower). For the comparison, I see four values of 10 that show important differences between the laboratories. I know that projects can be limited by time and funds, but it would be nice to see more QA information in the thesis.



External Recalculation of Table A-2, Olsen Thesis

Table Results						Independent Calculations			
Date	Site	Measure	Lab 1 mg/L	Lab 2 mg/L	Error	My Calc, Diff (mg/L)	Average (mg/L)	My Calc, Diff vs average (mg/L)	Important Differences
7/13/2017	Saratoga Springs	Dry	42	39.2	-0.1	2.80	40.60	6.9%	no
7/13/2017	Saratoga Springs	Wet	0.03	1.2	39.6	-1.17	0.62	-190.2%	yes
8/3/2017	Saratoga Springs	Dry	0.5	11.8	22.6	-11.30	6.15	-183.7%	yes
8/10/2017	Pump Station	Dry	0.11	0.1	0.0	0.01	0.11	9.5%	no
8/10/2017	Pump Station	Wet	0.18	0.7	2.8	-0.52	0.44	-118.2%	
8/24/2017	Lake Shore	Dry	38	0.5	-1.0	37.50	19.25	194.8%	yes
8/24/2017	Lake Shore	Wet	0.02	1.4	69.7	-1.38	0.71	-194.4%	yes
8/24/2017	Central Davis	Dry	0.01	0	1.5	0.01	0.01	200.0%	no
7/27/2017	Saratoga Springs	Wet	0.45	0.6	0.3	-0.15	0.53	-28.6%	no
9/21/2017	Mosida	Dry	3	2.2	-0.3	0.80	2.60	30.8%	

56. Page 47, Supply List. I notice that latex gloves are on the supply list. Were any of the gloves tested for TP or TN? I say this because many glove brands contain powder to make them easier to slip on. This powder is usually talcum powder (magnesium silicate), I believe. Being a mineral, it is possible that it has some phosphorus contamination in it?

57. Page 50, Parts List. It says that the bucket is a 2-gallon bucket, but I calculated that it is roughly a 5-gallon bucket. I bring this up due to the idea of how deep into the

bucket is the water surface. A 2-gallon bucket with 3 L of water in it would be more representative of a natural lake surface than a 5-gallon with 3 L.

58. Page 51: I notice that you have a rain gage on the equipment list. Was there just one used, or was there one gage at each station during the study?
59. Page 52, Sampler Code. Did your code record when the collector was open? As with NADP, we use this information to determine if the wet deposition collector is open when the onsite precipitation gage is recording precipitation. I know there wasn't a digital precipitation gage available, but it would be valuable to have this information to determine if the collector was open during the entire precipitation event and how long the sample collector was open during the week to ensure that it was working correctly.
60. Page 52-62, Sampler Code. I am not familiar with the language used here, so I did not evaluate any of the coding.
61. Page 72, Mathematics. It seems to me that all of the dry and wet deposition calculations shown here are correct.
62. Page 73, TotalDeprate1. I think this rate calculation is just a little bit off. I calculate 6.44 mg/m2 rather than 6.41 as shown. This is likely a minor error in the big scheme of things, but it could be significant at some level.
63. Page 74, Figure C-1. It would be helpful if this graph had a color scale for the flux level. Also, why is there a big green diamond shape in the middle of the lake? Error? Short coming of the method? Mistake in the figure?
64. Page 75, Table C-1. I found two errors in the table. First, for the loading column, I calculated a slightly different value than is present (653,054.149. or about 0.1% off). This could be a rounding error. But, if they are all off by this much, then the total flux for the week is 0.6 kg per week too low. Second, the total area of the lake is a bit different also. Early in the document, Olsen states that the lake is approximately 354 km square, but this number is approximately 2.5% lower (at 345 km2). This difference could also be important.
65. Page 75-76, Table C-2. I have a couple of questions from this table. This table has both uncontaminated and contaminated sample columns. At least in a few sample weeks, the contaminated samples have lower flux values than the noncontaminated samples. I'm sure this is possible, but I would expect contaminated samples to have higher flux rates. Did Olsen investigate this occurrence? Second, as mentioned previously, if a hyphen is present for the uncontaminated samples, does this mean

that there were no uncontaminated samples during that week, or was there no flux that week? Either way, this could have significant impact on your uncontaminated flux for the particular week. I did not see this point clarified in the text. Additionally, it is interesting that there is no contamination after week 22 for TP? I can see that the birds leave for winter and there are no more insects after first frost. But to have no TP contamination after this point? This may go back to the point that the samples after mid-October were handled differently.

Review of Document
David A. Gay, Ph.D.
November, 2019

Document: *<u>Measuring and Calculating Current Atmospheric Phosphorous and Nitrogen Loadings to Utah Lake Using Field Samples and Geostatistical Analysis</u>, by Jacob M. Olsen, Gustavious P. Williams, A. Woodruff Miller and LaVere Merritt* (Hydrology-05-0045 by Olsen and others)

I reviewed this article and the underlying thesis work (Jacob M. Olsen – *Measuring and Calculating Current Atmospheric Deposition*, Thesis, 2018, Brigham Young University). Most of the comments are applicable to both documents, with many comments repeated here.

Overall, I found this to be a nice and straightforward journal article about the topic. I found it to be generally scientifically valid, and had a good approach as well. I have a few comments on some of the methods, but none of them invalidate the study. I do have several questions and comments about the equipment (wet and dry), several questions about the dry measurements themselves, and in particular the method used to estimate deposition fluxes over the lake.

One of the most important criticism I have of this study is the lack of information about the correct operation of the sampler, the operation of the sampler's sensor, and the lack of quality assurance information about cleanliness etc. of the sampling equipment/supplies and analytical work. Although there is no information that these types of issues occurred, there is no record presented that these issues of true contamination did not occur.

Second, the location/siting for the Saratoga Springs site, with the largest concentrations and fluxes into the Utah Lake, and the results are particularly sensitive to this site and its location. With a small difference in location of the sampling site, I can see where the flux estimate could increase drastically or decrease drastically as the TP/TN source has a larger or smaller impact upon the wet and dry deposition flux estimates made here. I do agree that all sources need to be accounted for in the flux estimates (the authors say "local deposition"), and that the definition of contamination is complicated for this study, particularly with insects and bees included in the sample. I have made a few of my concerns on this contamination issue known below.

Specific Comments:

1. Page 2, Para 2: "For example…growth." I would assume that measurements have been made of sediment loads of TP/TN? With high sediment load in the lake, it seems that wave action and the like would continually resuspend the TP/TN in sediment back into the water column. If there is a high concentration of sediment TP/TN, it could be that any reduction by the Sewer District and atmospheric deposition will have no effect at all because of this residual TP/TN. The discussion of sediment and soil concentrations in the next paragraph suggest the same, that TP/TN in the sediment/soils is a very large source, relative to other sources.
2. Page 3, Para 1: "There are three…deposition." I think it is more appropriate to say here that bulk deposition is some combination of wet deposition and dry deposition, and that it is generally used when power is unavailable or no funds are available for wet and dry deposition. I don't think it's necessarily chosen over wet and dry deposition measurements.
3. Page 5, Para 2, into Page 6: As with all studies, site selection can have a very important impact on the results obtained. This is particularly true for a local atmospheric deposition project with important local sources. I am not clear at this point of all of the local sources and source strengths, but this point must always be kept in mind. The stated goal of trying to get a representative sample is the correct one, but the site selection is always difficult and definitely opens the results to criticism. I think an even distribution is a certainly a rational approach for a representative sample around the lake.
4. Page 6, Para 1: Selected samplers. There is little information about the samplers in the paper, but a sampler built to simulate the Aerochem (ACM) is an appropriate start (the ACM is no longer made). The NADP has quite a bit of experience with this collector. As the authors likely know, the ACM has several issues, including several researchers who suggest that precipitation "bounce" is an issue, leading to a high bias for concentrations (a raindrop that hits the collector top, picking up dry deposition and landing in the sample bucket; i.e. bounce). I am unclear what the size of the collector top is, but smaller is better to avoid this issue. There is also little information about the precipitation sensor used. This is a very important, given that the sensor senses precipitation, and the collector then opens the wet sample bucket to the atmosphere, ultimately determining if the deposition will be counted as dry or wet deposition. The NADP has a long history with the ACM collector and sensor,

and it is known that all sensors act somewhat differently. There is also no information about any failures with the sampler (motorbox/drive motor, sensor, electricity loss, etc.). Are these operation records available? Are sampler pictures available?

5. Page 6, Table 1: Most of the siting looks fine, except for a few questionable entries. For all sites, the solar panels are within the 5 m NADP rule. The concern is bounce again. If the panels were near/above the sampler, then dry deposition contamination in the wet sample is possible, and potential double counting of deposition is a worry. Hopefully, the angled face of the panels was oriented away from the collector to minimize this issue. The only other concern was for Lake Shore with a horse corral and farm shed nearer to the collectors. My concern here would be with the organic nitrogen component and soil dust (and TP) being biased high with animal activity. I don't know in which direction these sources are, but some additional deposition could be an issue at this site. This also brings up the contamination issue, as discussed previously. This farm is part of the surrounding environment, and therefore, a normal source of deposition to the lake.
6. Page 6, Para 1: Polyethylene buckets. This is consistent with the NADP, but a still unanswered question is the adhesion of phosphorus to the walls of the bucket. Some think that this could be a significant loss of phosphorus, although this has not been documented by NADP (NADP only measures ortho-phosphate, not TP).
7. Page 6, Para 1: There does not seem to be an independent precipitation collection, as is used in the NADP network. Therefore, I assume the authors used bucket catch to determine the precipitation depth per precipitation event. NADP has long noted the inconsistency between the so-called "bucket catch" and precipitation gage depth. The odd shape of the Aerochem and its lid change the aerodynamics around the sampler enough to affect the collection of precipitation. Sample volume depth is often greater than 10% different from the precipitation gage, and can be much larger. The differences can also be in both directions (above or below gage depth). NADP only uses sample volume to determine precipitation depth if our independent precipitation gage does not work correctly. Therefore, this could be a source of error to the deposition flux calculation if done in this manner.
8. Page 6, Para 1: Open/close of the Aerochem-style sampler. The ACM sampler is a good and reliable collector, but NADP has documented many instances where the collector opens late, or does not open, for example, during light snow and windy conditions. This is also part of the variability of precipitation depth mentioned

previously. There is no discussion of the operation of the sampler, and particularly the operation of the sensor opening the sampler. Also, was the sensor heated--requiring continuous rain to keep the sampler open, and additionally, to melt fallen snow to trigger the sampler? This makes a heated sampler particularly important during the winter.

9. Page 6, Para 2: Sample collection. Sample collection occurred on Tuesday morning, per NADP protocol? I assume that the wet buckets were changed even without any precipitation and or sampler openings? Were sample openings recorded? Is there a way to know if the sensor was reliably opening the collector during precipitation?
10. Page 7, intro. Para: I think using a water-based sample for dry is an appropriate method to approximate dry deposition to a lake's surface. But, I have no personal experience using these types of samplers. I don't know of many references that document studies that have used them, but they certainly could work, or at least be representative. My question is why only 3L of water was used in the sampler in a bucket that holds about 21 L (~5 gal) meaning there is only liquid in the bottom. I realize that analytical concentrations would be lower. But my thinking is that it should work fine to keep particles inside the bucket (and snow) which is good. But, I am not sure if it represents a water surface well to the atmosphere flowing over the sampler. Did anyone run any comparisons of larger water volume to determine if any change was present in the collection of particles/aerosols/gases?
11. Page 7, Para 1: Cleaning procedure. I would assume you checked the cleaned buckets for contamination of TP and TN, but I did not see any results presented. Did you determine that the residual, if any, was minor? Further in the paragraph you note that you blank corrected the deionized water for nutrients. What levels did you note in the water? Where they significant?
12. Page 7, Para 2: Small brushes. I assume you cleaned the brushes between buckets with phosphorus free detergent and acid as well as the buckets themselves? Was there any analysis of the cleanliness to guard against bucket contamination from the brushes and bucket handling?
13. Page 7, Para 4: Contamination. I agree with the thought of contamination being different for this project in contrast with traditional wet deposition definitions. Most "contamination" in samples for this project are inputs of TP and TN to the lake and therefore, important to include in fluxes to Utah Lake. However, for overlapped samples, it is important to know how often this occurred, which speaks to the overall operation of the wet dry sampler. I assume it did not occur very often? I have

the same question of the occurrence rate of bulk samples. Did anyone record the open and close periods to compare to a digital record of precipitation on a short-term basis? How do you know when a bulk sample occurred? Or did not occur? The report says that there was "obvious algal growth" as a contaminant. Did this occur regularly? If so, why would this occur? I am assuming this is not a regular occurrence over a week's time, which could lead to loss of nitrogen from the sample. How often did the sample get collected for longer than one week? Beyond this, were any samples marked contaminated by the laboratory? I think bird feces is one obvious contamination issue in the traditional sense. Although bird feces does reach the lake, I would not consider this a valid sample for wet or dry deposition. My thinking is that this would alter the chemistry, particularly for P and N, and that these samples would be removed from all analyses. Additionally, I think it is good that both of the no-contamination/contamination values in your calculations were used to bracket your results, given the potential disagreement over the definition of contamination and the local focus of the project.

14. Page 7, Para 4: I think your decision to remove the plant matter and insects from samples was the right choice, given the difficulty in keeping these out of wet deposition samples and the analytical issues possible with leaving them in the sample.

15. Page 7, Para 5: Analytical discussion. It is not clear to me if someone did or did not filter the wet deposition sample and measure only TP/TN in solution (I would choose sonication to be appropriate). NADP does not sonicate its samples but filters out suspended solids before analysis, and therefore this would not explicitly follow NADP protocol, correct? I think much of the TP in the wet sample is in the undissolved solids. Furthermore, duplicate samples for TP are mentioned; What were the duplicate results? None were shown in the paper, and it would strengthen your argument to have these results presented.

16. Page 7, Para 6: Calculation of deposition flux. I think this method is generally fine, as long as the sampler accurately collected the volume of sample. However, if the sampler did not open for a significant portion of the week's precipitation, or significant evaporation of the wet sample occurred, then the recorded depth of precipitation is not correct. Also, the concentrations would be different or altered. I don't know if this was a problem, but it certainly could have been. The NADP standard is to measure the concentration and multiply through by the precipitation depth as determined by the rain gage, with the idea that the rain gage is a better

estimate of depth and therefore, a better flux estimate. This also relates back to how well the sampler operated for wet deposition and which records were kept.

17. Page 8, Para 1: I do not know many specifics of the lake, but given that it is very shallow, I would assume that the lake level can change significantly year to year. Are there significant areas of wetlands, or perhaps, significant areas that are wet during some years and would eventually contribute deposited TP and TN while flooded? Is this occurrence large enough to affect the calculations of deposition to the lake? It would seem possible that in wet years when the lake is significantly larger and submerges areas with previously deposited TP/TN could be an important contributor for both.

18. Page 8, Para 2: The presented method of using a using effective background sites through the mid-lake region to force estimates of local dry deposition to background levels seems reasonable to me. And I agree that the deposition is likely to be higher near shore than it is in the lake. With no measurements in the lake, you have to have some approach. But, NADP Site Selection and Installation ("24") was referenced as the source of the dissolved inorganic nitrogen flux of 0.112 mg DIN/m^2 week. I am a bit confused as to the source of this number and how it is used. Assuming that you mean NADP is the general source of this rate, I compared both the wet (NADP maps, 2018) and dry deposition (from TDep, 2018) values. I calculate the wet flux in the area of to be about 6.7 g/m^2 week, and to be between 0.96 and 6.7 mg/m^2 week for dry, again as measured by either NADP or estimated by TDep. (I will admit I did not pull out the grids, but just picked colors from the site location on the maps.) The value you use for DIN of 0.112 mg DIN/m^2 week is significantly lower. The NADP values are regional values, and if used would significantly increase the estimates of TN to the lake by increasing all of the cells in your lake model. I think an explanation of how the 0.112 value came to be would be beneficial. Also, why was this value used rather than the regional value as measured by NADP?

19. Page 8, Para 2: In this modeling approach, it seems that the pseudo-sites are used to interpolate TN and TP to these low background values, which would mean both wet and dry deposition together. If I understand this correctly, then I agree that the dry deposition values should drop drastically as you move toward the center of the lake. However, I am not as convinced that wet deposition will drop off this quickly. My thinking here is that wet deposition scavenges TP and TN from a large vertical column of the atmosphere. I agree that the wet deposition will likely scavenge local and near-shore local sources too, but these would be a lower percentage of the total flux than it would be with the dry deposition. So, it would seem to me that reducing

wet deposition in the middle of the lake in the same manner is reducing wet deposition to a lower flux than it should be.

20. Page 8, Para 2: Additionally, DIN makes sense for wet deposition, and I assume DIN was used when referencing dry deposition rates is the dissolved N in the sample? Was sonication necessary for the dry deposition N measurements in any given week? Or was the total dry deposition sample sonicated, then split for TP and TN measurements? It is at least possible that if no sonication occurred, then some N was left in the solid phase and filtered out of the sample, leading to lower TN estimates.

21. Page 9, Para 1: "We repeated…study period." It is stated that the researchers summed the deposition for every week that there was data over the 31 weeks. I assume that means they summed 31 values and that no week was without data, correct? The estimates would be lower for the eight month period if one or two weeks were missing. Another point is the eight month period, rather than a full year. I understand that sampling studies have limits, but any other study will likely estimate deposition for a calendar year for total flux values. Was this attempted in this study? Were these values reported? This would be a nice addition for comparison purposes.

22. Page 9, Para 2: "From May….each site." The number of samples in the text do not quite match the number of samples in Table 2. Is the difference in sample number due to samples that were discarded because of quality assurance? There seems to be about 25 dry and eight wet samples that are not present. In addition, did anyone do any field blanks, where you take a blank to the field, mount it on the sampler and let it remain onsite through the week? This field check goes a long way to prove there is no systematic contamination.

23. Page 9, Para 3: Results section. In an earlier measurement study around the lake that I have read, the highest TP fluxes (wet + dry) were near Mosida (perhaps at the same location), and definitely in the southeast quadrant of the area. The next highest was near the Saratoga Springs area. With different years and slightly different locations, I understand that one could experience different results. Did you note any reasons why this might be the case? In both studies, the lower deposition rates were at Orem.

24. Page 10, Table 3: The rain value is included for the week, and I believe that it was mentioned that this was from an airport gage. To get the loading rates in this table, did you use this gage times the wet deposition concentration to get loading, or the previously mentioned value of using the bucket flux only?

25. Page 10, Table 4: Average DIN flux rate. I checked the 2017 TDep rate for total wet plus dry N deposition and converted this number to mg/m^2 day, and came up with something between 1.65 and 3.30 mg/m^2 day. This compares to the reported average of 10.23, with site values ranging from 1.59 to 36.06 mg/m^2 day. Outside of the very high value at Saratoga Springs, it would seem the listed values compare favorably (local + regional signals) with the TDep modeling effort values. They are higher than the TDep estimates, but I would expect them to be since little information goes into a TDep estimate from smaller and local sources. So, I would say they compare favorably, given the vastly different manner of methods and without the SS site. The Saratoga Springs value is quite high, however. Looking at the graphics (page 11) for Saratoga Springs, it would seem that the high values are not isolated to one value (like wet deposition for the Pump Station or Orem). The peak wet and dry values for SS do not line up directly, but are about the same period. But these very high values could also include bird feces in samples, as mentioned previously.

26. Page 11, Miscellaneous weekly graphics. I noticed that the reported sampling records are not present for August, September, and October (date is in an odd form with day-month, and I am assuming I read this correctly). It would seem from the graphics that none of the observations at sites actually go through November and December. It is possible that the ending samples (where snow is most likely) were all removed due to contamination issues? But, it would seem that these values were likely to be very low in concentration, as is typical for NADP winter measurements. Had these observations been in the averages, the average concentrations would be lower than stated. It would also seem that the reported measurements are really six month values rather than eight month values, and therefore, the fluxes would likely be higher with an eight month time period.

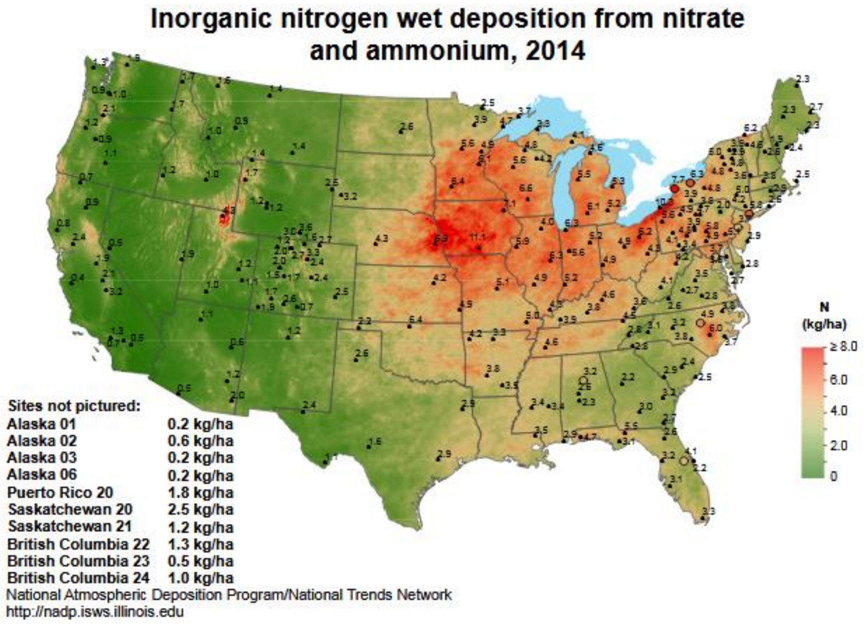
27. Page 12, para 1: Saratoga Springs. I agree with the conclusion of the likely source of very high values here is attributable to the gravel pit. The very high values of TP occur in the summer when activity should be highest here, and the soil surface would be at its driest. But I would expect gravel pit operation to continue into the fall, but no other high values are shown. Were measurements made during this period?

28. Page 12, Para 4: I respect the choice to provide both the low flux and the high flux rates of the measurements. When there are extremely high concentrations, it is always a struggle to determine if an "outlier" is real or not, and whether to include it. It is very clear why these methods were used, and why both flux rates were included.

29. Page 13, Figure 6, 7. In these summary figures, the authors do show deposition values going out through the beginning of December. But again, earlier, there is no data in these site graphs to show that measurements were made. I do not understand why these data points are present. Were they estimated for missing weeks, or were the earlier site graphs not complete? This is also the case for Figures 8 and 9. On the other hand, it is good to see that the dry and wet deposition high flux values line up nicely and occur at the same periods. This supports confidence in the measurements.
30. Page 13, Para 1: "Wet deposition rates…more similar." I would submit that these statements, which I agree with, support my contention made earlier that the wet deposition rates in the middle of the lake are likely lower than they should be when assuming the background wet deposition flux.
31. Page 14, intro. Para: Bees. I will agree with you that the presence of bees in the sample (and there are similar examples in the NADP record of the same) likely alter the chemistry. But I am not sure that they necessarily raise the nutrients in the samples. I would at least speculate that the bee carcasses could adsorb ions to their body hair, and when their carcasses were removed, the adsorbed cations and anions could have been removed as well. I do not know one way or the other, but it is certainly a concern. I would recommend in the future to use some type of widely spaced screen over the top of the dry bucket to try and minimize this issue. However, I would not recommend using a screen on the wet bucket side due to possible contamination issues. I think that if significant number of bees do die in the lake, then this flux of chemicals should be part of the flux calculation. But as far as wet deposition, they are likely affecting the chemistry of the sample, and potentially increasing and/or decreasing both TP and TN. Therefore, they are likely altering the wet deposition flux values.
32. Page 15, Figure 10: Although this method for estimating deposition onto the gridded lake surface seems reasonable, the choices of how this estimation was done affect the answer distinctly. This is critical to the final answers. Some may even argue to use alternate methods. It may be valuable in the future to test some of the assumptions inherent in the method to determine how much the total deposition changes; a model sensitivity, if you will. Others have suggested making measurements either over the lake (moving measurements) or longer term in-lake measurements. These options would shore up these types of unknowns.

33. Page 16, Intro. Para: I agree with most of this argument for contamination, but the NADP doesn't restrict being near local sources because they consider it contamination. NADP is looking for a regional signal, and therefore, wants any local sources to be well mixed into the regional signal. This is the reason that local sources cannot be too close to the NADP collectors.
34. Page 16, intro. Para: "However, since…lake surface." I agree with this reasoning. It is the signal that is important to look for here. All significant sources that contribute to the atmospheric deposition at Utah Lake should be evaluated and accounted for.
35. Page 16, Para 2: "Local sources… storms." Did someone note large soil particles in some of the samples, and note this on the field notes? Are these samples the same samples with high concentrations? This would be further evidence for an important soil dust contribution. Also, were the samples tested for calcium ion? This would be a chemical link to soil sources or gravel operations, if this data is available.
36. Page 16, Para 3: "This indicates…this question." I would agree that this is likely to be a very low estimate of deposition to the lake, because a large part of the lake has near zero deposition per day, minimizing the deposition during these "low estimate" runs. But as you note, this is assuming your rates are correct.
37. Page 16, Para 4: As I have mentioned previously, I would expect variation by year, particularly with dry deposition dominating, as it is mentioned. With an extremely dry drought year, I would expect the deposition to be heavier during the spring and summer. Further, a wet year would likely make the wet deposition flux much more important. Any future studies might want to go for longer periods, and multiple years (or multiple summers, etc.).

38. Page 16, Para 6: “We compared…local sources.” There are some issues here with the comparison to NADP wet deposition rates. I am assuming the authors are comparing this to NADP wet deposition map measurements. I looked up the total N measurements from 2014 (map below, which can be found at http://nadp.slh.wisc.edu/maplib/pdf/2014/N_dep_2014.pdf) where NADP measurements show Nitrogen wet deposition flux rates (as N, rather than molecular NO3-+NH4+) range in Utah from 1 to 4.3 kg/ha year, and are roughly 3.0 kg/ha year in the Utah Lake area. These would convert to approximately 100 to 430 mg/m^2 year, rather than 2.14 to 8.42 mg/m^2 year as stated in the text. Further, using the authors’ average flux rate from Table 4-3 of 10.35 mg/m^2 day, the average flux rate for comparison is not 0.04 to 24.82 mg/m^2 year as stated, but more appropriately is 3,777 mg/m^2 year (assuming 365 days of flux, average of 10.35 mg/m^2 day). Alternatively, NADP wet deposition values would become about 1.0 mg/m^2 day, averaged over 365 days, or about 1/10th of your wet+dry estimate. So, the values do not really compare that well to the wet deposition values provided by NADP. Note that NADP is only wet deposition, and this study is total deposition. I also calculated total wet flux for the lake from NADP wet deposition values, assuming 2 kg/ha, which is approximately 78 tons/ full year for N. This compares to your values of TN deposition of 46 to 460 tons/eight month year. Therefore, the NADP wet deposition estimate is relatively close to your no contamination estimate of 46 tons and an increased somewhat for the four months not sampled.



Inorganic nitrogen wet deposition from nitrate and ammonium, 2014

Sites not pictured:

Alaska 01	0.2 kg/ha
Alaska 02	0.6 kg/ha
Alaska 03	0.2 kg/ha
Alaska 06	0.2 kg/ha
Puerto Rico 20	1.8 kg/ha
Saskatchewan 20	2.5 kg/ha
Saskatchewan 21	1.2 kg/ha
British Columbia 22	1.3 kg/ha
British Columbia 23	0.5 kg/ha
British Columbia 24	1.0 kg/ha

National Atmospheric Deposition Program/National Trends Network
http://nadp.isws.illinois.edu

39. Page 16, Para 7: Bioavailability. This is a distinction from other studies by not estimating bioavailable TP and TN. This is not my area, so I don’t think I can add much to this discussion. It is mentioned that “a majority of the samples only

contained dissolved phosphate". This brings up the topic again of whether the wet deposition samples were filtered, and therefore did not include suspended solids. It is not clear to me which method was used. Also, several suggest that the majority of TP is not in solution, but in the suspended particles that are removed from NADP samples.

40. Page 17, Para 1: Contamination. I agree that part of the disagreement around this flux question is due to a lack of agreement on how to define what is regarded as "contamination". An agreed upon delineation would certainly help, but as it is noted, this is a difficult distinction to make. For this particular situation, I would argue that all contributions of nutrients, including insects, excavation, and agriculture should be included into the flux of TP/TN to Utah Lake, following the statement of "For this study…to the water column." But insects in the sample are not traditionally considered wet deposition, so I would advocate to remove them from the deposition samples, and try to account for the bug/bee flux in some other manner.
41. Page 17, Para 4: This could be one area where we disagree on contamination. I assume that if a sample had bird feces in it, it was not included in the "contaminated" samples. If a bird defecates into the sample because it was sitting on the sampler, I would argue that this sample should be removed from analysis. However, birds defecating into the lake would also be a flux of TP/TN into the lake, and should be incorporated into the flux determination, if it was determined that the flux was an important contributor (in some fashion). But I am not sure how that would be accomplished. I would assume due to the dry Utah climate, a large water source would attract a significant number of birds to the area. Therefore, I would consider it a large source worth an attempt at a flux estimate.
42. Page 17, Para 5: "We feel…center of the lake." I am not sure about the dominant nature of the wind being west to east. This is assumed by several researchers, but the wind roses that I have seen are either northwest or southeast dominant winds. I would assume that wind directions aloft (more important for wet deposition) are likely to have a dominant westerly component. This point needs to be clarified.
43. Page 17, Para 6: "We also expect…longer periods." I agree, the year-to-year flux could be dramatically different, due in part to rainy years versus drought years. This would obviously influence the wet deposition flux, but also the soil dryness and dry deposition flux.

44. Page 18, Conclusions: It might be useful, for comparison of these numbers to other studies, to clear up the issue with samples in November and December (mentioned previously), and try and estimate these flux rates for a full year. It would also be beneficial to estimate bioavailable fractions (or limit other studies to eight months, non-bioavailable). This would allow for further intercomparability of these various estimates/measurement studies.

Review of Document
David A. Gay, Ph.D.
November, 2019

Document: Josh Reidhead, – *Significance of the Rates of Atmospheric Deposition Around Utah Lake*, Thesis, 2019, Brigham Young University.

General Comments

This study focuses on soil analysis while continuing the atmospheric deposition measurements of the previous year made and reported by Olsen, 2018. I think that the measurements are generally valid, but I have some issues associated with the measurement, as noted in my review of the Olsen thesis.

The depositional P fluxes noted here are significantly lower than the Olsen values from the 2017 sampling, which Olsen notes as being the maximum expected. Therefore, the two studies do complement each other scientifically. The N deposition flux noted here is actually larger than previously reported by Olsen, 2018.

Additionally, I found some quality assurance issues with the duplication of soil fractionation analytical results. It is my opinion from these duplicate results that the TP fractionation estimates are somewhat suspect, as I understand them. I think that the TP values measured are more accurate, but they are higher by about 20% than the data suggested. I think the duplicate soil fraction analyses of SWP, LBP, AFBP and CBP make the reported values questionable and must be viewed with a high degree of analytical uncertainty. Again, soil phosphorus and nitrogen are not my scientific specialty, so I provide these comments from a more general science perspective. I am also not directly familiar with the specific soil fractionation tests used here.

Specific Comments

1. Abstract, Estimates of deposition. The atmospheric deposition levels summarized in the abstract are distinctly lower than those reported in Olsen, 2018, and in the Olsen et al., 2018 Hydrology article. This could be a result of more refined calculations, a change of methods, seasonal variability of deposition, or a number of other differences.

2. Page 1, Para 3: "This is particularly…or more." Do you have a reference for this? If so, then this seems to be a clue to the connection between wind, N and P in the water column and algal blooms. This suggests that when the turbidity goes down, the algal blooms are more frequent.
3. Page 2, Para 3: If all of these P levels are correct to support eutrophication, it would seem the atmospheric deposition levels could well support these levels. So, I would think over time that the atmosphere would contribute excessive P to the lake relative to these levels.
4. Page 2, Para 4: This soil fractionation test seems to be a reasonable and straightforward test to determine the individual P components of soil, since biologically available P is what is the most important input to Utah Lake. This is true for current times and does not consider any long-term P conversion to be biologically available P. I would assume that this is a relatively standard test available, and not one that is scientifically questionable or highly problematic.
5. Page 2, Para 4: Residual P (RP). It is unclear to me and is not defined in the document what exactly residual P is. I am assuming residual P is TP minus all of the other measured components of WSP, LBP, AFBP, and CCBP?
6. Page 3, intro. Para: Is it true that water soluble P is roughly equivalent to Soluble reactive P? It is not clear in this text, but I am assuming that this is generally the case.
7. Page 3, Para 1: I think that this idea of P being considered a minor atmospheric component is generally true. There are no known gaseous atmospheric compounds of P, and therefore, atmospheric P deposition is all particulate mass suspended in the atmosphere and deposited. And, yes, it is generally true that given this solid form of P, atmospheric deposition of P should be dominated by short suspension times in the atmosphere and driven by local sources. However, intercontinental movement of soil/particulate has been shown, and therefore, intercontinental deposition of some P compounds should also be occurring.
8. Page 3, Para 2: Olsen AD estimates. To put these values into context, the Olsen TP estimates from the 2017 measurement season were about twice the level of the 2018 measurement season, and a slightly higher TN deposition during 2018. In that study, Olsen gave a range of possible TP and TN deposition values for Utah Lake, and the 2018 TP estimate here are about right in the middle of the Olsen deposition range. The TN estimate here is about 10% greater than the 2017 estimate. In both cases, these values are well in excess of the TP reported by Brahney in previous documents

(5.0±3.0 mTons), and approximately three times the deposition for TN (Brahney 153-288 mtons).

9. Page 4, intro. Para: “Utah Lake is…the lake.” This seems to support the idea that TP and TN in the sediments could be a significant source to the water column and part of the oligotrophic nature of the lake.
10. Page 5, Para 1: “For example, no…the soil.” I agree that if you are sampling the soil of a general area, the soil contained in corrals etc. can be highly disturbed and unusual relative to a general soil survey. However, if corrals and pastureland are a significant percentage of the area surrounding Utah Lake, then I would think that these soils should be sampled too, since they could be significant sources of particulate to Utah Lake.
11. Page 6, Figure 1: Based on the picture alone, it would seem that the soil samples seem reasonably well distributed around the lake. Therefore, the sampling plan would capture a good statistical distribution of the soils around the lake. The study did not mention it specifically, but was a soil sample taken near the gravel pits that have been mentioned several times? I know that this is likely to have little soil in the gravel, but the area at the pit could be a particularly important dust source to Utah Lake.
12. Page 7, Figure 3: I assume these soil sample bottles were cleaned appropriately and quality assurance tests are made? I did not see these records in the appendix.
13. Page 9, AD Sampling methods: Here, the author reports that the sampling methods used previously by Olsen, 2018 and also reported in Olsen et al., 2018 were again used here. Therefore, the comments previously provided about the sampling methods in these referenced documents are applicable here. I note that the methods generally followed the NADP guidelines, but with a sampler very close to the ground, there is potential contamination from the solar panel, and the previously mention question about the operation and quality assurance of the sampler and sensor, etc.
14. Page 9, Para 1: “Due to the…other week.” It seems reasonable to go to a two week sampling period in the wintertime where lower precipitation dominates, etc. However, this also brings in the question of when this sampling shift occurred, whether samples remained on the collector for two weeks (assuming an early period rain/snow event), and whether the quality of these samples was degraded. This document does not report this information.

15. Page 10, Figure 4: Sampling locations. From this figure, the five sampling locations do not appear to have changed significantly, which is appropriate for the study continuation. As mentioned in previous reviews, since much of the flux is thought to be from important local sources, small changes in the location of the samplers could result in very significant changes in deposition flux measurements. However, the author reports that the LS and M sites were moved between sampling seasons (about 100 feet, about 300 feet). It is possible that these site moves had a significant impact on the results. However, without a better sense of the movement and local sources around these sites, I cannot make this judgment. It seems that Reidhead considered the moves not to be significant. The Brown Island site was not operating in 2017 and is a new site. However, it appears that it did not operate for much of the 2018 sampling season.
16. Page 12, Para 1: "The samplers consist of two 2-gallon buckets…" In an earlier review, there was some question about the size of the bucket. I had calculated that the bucket size was approximately 5 gallons (consistent with NADP), but here it is reported that it is smaller. The point is that the size of the bucket defines how low in the bucket the surface of the water is located. I would hope that the bucket size did not change between sampling seasons, and that it is just being reported inconsistently.
17. Page 12, Para 1: Bucket cleaning. This method is consistent with the 2017 sampling season, and consistency is good. However, as previously noted, there are no records of laboratory or field blanks to document the cleanliness of the buckets, tools, etc., or the occurrence of buckets found not to be clean enough.
18. Page 12, Para 1: "Three liters...to retain…" I understand why the author made this change for 2018, since evaporation from Utah Lake should be quite high. Water must be in the sampler to retain the captured particles. However, it makes the analytical determinations a bit harder, since you now have to track which bucket had 3L and which had 4L of water, and then ensure you keep these records straight. This distinction is less important since you are calculating deposition flux, rather than measuring the concentration. Did you ever have the occurrence of a weekly/biweekly sample that was dry? If so, how often did this occur? Did this occurrence cause you to change the amount of water added to the dry deposition bucket?
19. Page 12, Para 1: Representative sample. Given the interest in TP, and several comments from other documents of clearly evident solids in the wet deposition

sample, how did you ensure that these solids were resuspended in the precipitation when you subsampled for the 500 ml representative sample? As I understand the method now, I would have recommended that all of these solids be added to the 500 ml sample bottle (to capture all insoluble) with a well-mixed soluble sample added to the 500 ml sample bottle afterwards. Is this how you made these subsamples?

20. Page 12, Para 2: Filtering of insects. This is a change from the 2017 methods. During 2018, the insects from the samples were filtered through a clean screen to remove them. Were you able to compare samples with filtered insects versus samples without insects? Was there any difference? I assume that you were careful not to remove any undissolved solids? Also, cleaning/rinsing the gloves was a good quality assurance measure.
21. Page 13, Figure 7 caption: What do you mean by smoke? A local forest fire, perhaps? Were you able to discern any differences in the AD samples on that day/week? For the sampler design, I mentioned previously that the sampler design has some issues, including possible local ground contamination due to the very low sampler, contaminating wet deposition from the solar panel, etc.
22. Page 14, Para 2: "…if they appear clean." I assume that you mean if the samples are somewhat cloudy, or turbid? Or do you mean with unidentifiable solids/insects suspended? If I am understanding correctly, I agree that this is correct for the N sampling. The document just needs to be clear about what "appears clean" means.
23. Page 15, Para 1: External influences. Yes, natural variability in the environment is always present. It also brings up the importance of multiyear sampling to account for this natural variability.
24. Page 15, Para 2: The reasons given for the removal of soil samples seems reasonable. The author notes that blanks and duplicates were run. See further comments about the duplicates. The duplicates are of particular interest here, which would support your contention of accuracy in the laboratory.
25. Page 17, Para 1: The Kruskal-Wallis test is an appropriate test to determine if samples are collected from different distributions (i.e. median differences), and are rank tests which do not assume normality of the distributions. However, do we know that dust storms/local fires are the reason that the high concentrations are occurring? I am not convinced that we know this for certain.
26. Page 17, Para 2: This is correct, but only to a certain extent. The deposition samples will collect both the regional signal (i.e. global), plus any local signal combined. I

would suspect that the dry deposition samples will be dominated by the local sources, and the wet deposition will be more balanced between local and regional sources.

27. Page 17, Figure 10 & 11 and 12: You again mention smoke (I assume from a forest fire). You may have records of this particular day and reason to think this, but it seems that it could also be photochemical smog moving down from the urban areas of Salt Lake City. Back-trajectory modelling could resolve this for you. Blowing dusts could be part of it (as mentioned). My point is that with evidence that you know that fires are present and that the atmosphere moved from this region, then this is strong evidence for the source of at least part of the Utah Lake issue. But this evidence it is not presented here.

28. Page 18, Final para: I agree with you that algal growth in the sample can depress both the TP and TN values of the sample. Did you find this in the wet deposition or the dry deposition side? How often did you find these in samples? This was also noted in Olsen, 2018, and is a cause for concern.

29. Page 19, Para 1: Conversion of ammonium to nitrate. I applaud your effort for trying to detect this change, which is well known in nitrogen deposition circles. Although added driving time certainly could be an issue, I think the real issue (and test) would be the time from sample collection until the time of laboratory analysis. Reidhead used weekly or biweekly sampling. If, for example, it rains of the first day that the new bucket is put out for collection, the sample will sit on the sampler for at least six days, then have return travel time added to this. I would think that this is the true test that you want to evaluate. NADP has seen reductions in ammonium in its weekly samples versus daily removal of samples, so the effect is real. Also, the reference to Figure 32 is not correct; the correct figure seems to be Figure 30.

30. Page 19, Para 2: Interpolation to the lake. The method that you have chosen to estimate concentrations to the lake seems reasonable. Did you do any comparisons to determine how influential this method change was to the estimates made in 2017? I ask this to see what your opinion is on the influence of the flux estimates versus the previous year procedure. Was your change of methods made because you thought this was a superior method to the 2017 method? The method seems reasonable to me, but I have not done enough dry deposition modeling to know which type of depositional decrease estimate is best. However, it would seem to me that if both procedures are going to roughly zero in the middle of the lake, the equation would just define where in the lake the deposition is occurring. Additionally, by relating

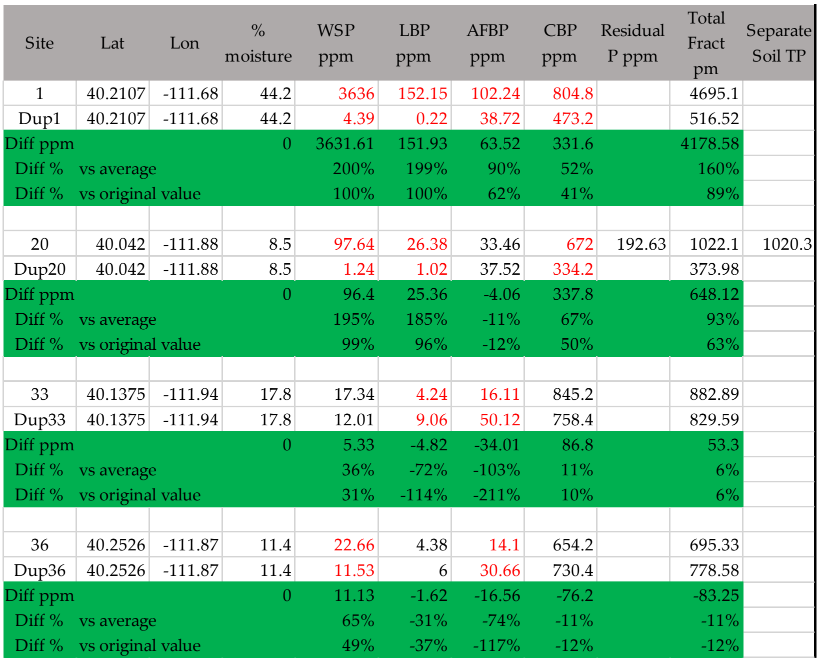
two sampling sites to each other in your rectangle along the shore, does this extend higher concentrations of pollutants along the shore, versus treating the measurement like a plume?

31. Page 19 & 21, interpolation. I am not clear on how deposition between sites is determined. Is it a linear decrease/increase between sampling site 1 and sampling site 2 in this model?

32. Page 21, Para 1 and Figure 14: With the model set to go to zero at the center of the lake, this seems like a reasonable and simple assumption. Also, a linear decrease seems reasonable to me. However, for wet deposition, it would seem that the center location of zero is somewhat low. I would think it would decrease linearly from the shoreline concentration (local + regional signal) down to the regional signal closer to the middle of the lake, or where local sources go to zero. Therefore, for wet deposition, it would seem there could be an area in the middle of the lake that is actually somewhat lower than expected. I think it is fine to go to essentially zero for dry deposition, assuming that it is dominated by local signals (or alternatively, go to a global background as Olsen set his model). I don't think that this idea or point is a large difference, but I would have to somehow calculate this difference to know for sure. Additionally, there is an idea to set up a mid-lake sampling location, which could clear up some of these trailing questions.

33. Page 23, Para 1: Basic soil results, displayed in Table 6 and summarized in Table 1. I recalculated a few of the statistics from the raw data found in Table 6. First, I compared the duplicates to the field values. Since this study is so dependent upon the different TP fractions as calculated here, I would have recommended more than four duplicates (if funds were available).

    Unfortunately, the duplicate values do not compare very well (see nearby table, my



Site	Lat	Lon	% moisture	WSP ppm	LBP ppm	AFBP ppm	CBP ppm	Residual P ppm	Total Fract pm	Separate Soil TP
1	40.2107	-111.68	44.2	3636	152.15	102.24	804.8		4695.1	
Dup1	40.2107	-111.68	44.2	4.39	0.22	38.72	473.2		516.52	
Diff ppm			0	3631.61	151.93	63.52	331.6		4178.58	
Diff %	vs average			200%	199%	90%	52%		160%	
Diff %	vs original value			100%	100%	62%	41%		89%	
20	40.042	-111.88	8.5	97.64	26.38	33.46	672	192.63	1022.1	1020.3
Dup20	40.042	-111.88	8.5	1.24	1.02	37.52	334.2		373.98	
Diff ppm			0	96.4	25.36	-4.06	337.8		648.12	
Diff %	vs average			195%	185%	-11%	67%		93%	
Diff %	vs original value			99%	96%	-12%	50%		63%	
33	40.1375	-111.94	17.8	17.34	4.24	16.11	845.2		882.89	
Dup33	40.1375	-111.94	17.8	12.01	9.06	50.12	758.4		829.59	
Diff ppm			0	5.33	-4.82	-34.01	86.8		53.3	
Diff %	vs average			36%	-72%	-103%	11%		6%	
Diff %	vs original value			31%	-114%	-211%	10%		6%	
36	40.2526	-111.87	11.4	22.66	4.38	14.1	654.2		695.33	
Dup36	40.2526	-111.87	11.4	11.53	6	30.66	730.4		778.58	
Diff ppm			0	11.13	-1.62	-16.56	-76.2		-83.25	
Diff %	vs average			65%	-31%	-74%	-11%		-11%	
Diff %	vs original value			49%	-37%	-117%	-12%		-12%	

calculations in color-filled cells). I understand that duplicate chemistry will never produce 0% difference when a sample is rerun. I also understand that the chemical procedure is relatively difficult with the prolonged shaking and multiple stages of the same. Additionally, I applaud you for discussing the potential differences and what was leading to poorer analytical values.

I colored the measurement values red where I would conclude poor duplication of the original value. Quite often, the values for duplication were not well repeated for WSP (best reproduction was 35% different), for LBP (again best comparison of about 35%), AFBP (where most were 100% different), and the first two duplicates of CBP. The duplicates seem to improve with later runs of the test. But I would have to conclude that the individual P fraction data are highly uncertain.

On the other hand, the one blank run was accurate, with only important contamination for WSP (but only about 3% of the mean value reported).

There appears to be an error for moisture content in sample 9.

The comparison of TP by fractionation versus the independent TP test looks much better, with the average differencing being about 26% (see second table nearby, again my calculations in green fill). Although some of the comparisons were quite different (>30%), many of them were less than this. I would say this is good for two independent methods used to compare the same TP value. So, this gives me more confidence in the TP values.

As compared to Table 1, I was unable to reproduce some of the averages for the entire lake. I did not attempt to recalculate the averages in the bottom two thirds of Table 1, lacking the information about which samples were taken from the west side of the lake. I am unclear as to why Reidhead chose to focus on the west side of the lake for further analysis. I would assume that this decision was made since the west side is less urban and more natural, and generally upwind of the lake.

For the entire lake, the report shows a P-fraction minus residual-P average of 1077 mg/kg. I calculated this average difference to be 882 mg/kg, or about 20% lower than reported in Table 1. I was able to calculate the residual P value of 294 mg/kg exactly.

Meaning of residual P: I was unable to find a specific definition of what residual P means. I am assuming that Residual-P is unaccounted for P in the fractionation test (i.e. where Residual P= (TP-frac – WSP – LBP – AFBP – CBP), or again the unfractionated remainder. As noted, I was able to reproduce this value.

Site	Lat	Lon	% moisture	WSP ppm	LBP ppm	AFBP ppm	CBP ppm	Residual P ppm	Total Fract ppm	Separate Soil TP	TP Diff	TP Diff %
1	40.2107	-111.68	44.2	3636	152.15	102.24	804.8		4695.1			
3	40.2171	-111.7	43.5	2158	64.95	85.78	979.4		3288.1			
6	40.2509	-111.72	34.4	606.8	41.54	39.84	1137.2		1825.3			
7	40.2584	-111.72	41.7	401	30.52	49.96	1018.8		1500.2			
9	40.2759	-111.74	408.6	377.2	45.03	100.02	220.4		742.65			
10	40.2841	-111.74	20	648.8	14.27	42.98	36.68		742.75			
11	40.297	-111.76	22.9	114.38	7.54	30.36	673.6		825.87			
13	40.1671	-111.74	28.4	183.42	24.24	46.68	592.6		846.94			
15	40.1502	-111.75	28	636.6	24.35	30.68	477.4		1169			
171	40.1126	-111.79	24.4	321	31.54	43.52	1228.6		1624.6			
172	40.1126	-111.79	34.4	315.8	8.68	30.04	578.8	549	1482.3	1002.5	479.8	39%
181	40.1448	-111.81	14.3	982.2	28.82	97.88	1149.4		2258.2			
182	40.1448	-111.81	23.5	92.82	2.57	21.22	1081.2		1197.8			
191	40.1328	-111.83	11.9	140.6	5.86	32.68	860.8		1039.9			
192	40.1328	-111.83	19.6	213.4	5.41	23.82	790.2		1032.8			
20	40.042	-111.88	8.5	97.64	26.38	33.46	672	192.63	1022.1	1020.3	1.8	0%
21	40.0583	-111.86	22.8	153.78	105.6	68.34	1063		1390.7			
221	40.1004	-111.8	51.6	40.42	86.02	103.9	386		616.34			
222	40.1004	-111.8	13.6	23.8	25.08	92.36	643.8	187.48	972.52	925.5	47.02	5%
231	39.9586	-111.88	81.7	9.23	0	39.46	350.6	474.75	874.03	659.07	214.96	28%
232	39.9586	-111.88	59.4	82.82	5.78	18.122	243.4		350.12			
24	39.9526	-111.94	5.4	32.9	20.66	11.25	528.2		593			
25	39.9688	-111.97	5	69.56	8.86	34.78	857		970.2			
26	39.977	-111.97	13	61.9	9.78	18.83	585.8	181.45	857.75	711.14	146.61	19%
27	39.9975	-112.02	18.7	29.32	8.52	21.1	481		539.94			
28	40.0016	-111.96	8.1	53.44	12.98	35.42	487.8		589.64			
29	40.0548	-111.97	2.9	7.41	2.32	3.38	324.4		337.5			
30	40.0767	-111.94	26.6	42.38	32.32	104.34	1296.4		1475.4			
31	40.0757	-111.93	22.8	63.94	18.66	56.18	1855.8		1994.5			
32	40.0759	-111.93	27.3	40.22	15.4	121.48	1225.2	395.5	1797.8	1396.8	401	25%
33	40.1375	-111.94	17.8	17.34	4.24	16.11	845.2		882.89			
34	40.1965	-111.9	8.5	9.268	1.94	47.16	1003.6		1061.9			
35	40.2193	-111.88	7.4	20.46	5.12	42.46	684.6		752.64			
36	40.2526	-111.87	11.4	22.66	4.38	14.1	654.2		695.33			
37	40.2667	-111.86	21.3	36.9	4.26	17.58	1343.8		1402.5			
38	40.2821	-111.87	15.6	10.78	6.44	40.2	1027.4	188.39	1273.2	923.02	350.18	32%
39	40.2939	-111.88	10.8	20.66	3.42	7.96	741.4		773.44			
40	40.3059	-111.89	13.8	13.16	2.38	11.56	863.2	183.93	1074.2	574.26	499.94	61%
41	40.3505	-111.92	20.1	35.68	4.82	26.78	653.4		720.68			
42	40.3601	-111.9	11.5	8.69	3.06	20.14	637.4		669.29			
43	40.3625	-111.89	6	13.46	2.92	5.84	663.4		685.62			
44	40.3627	-111.85	9.2	16.64	1.44	15.89	720.4		754.37			
Average Samples			31.4	282.4	21.7	43.0	773.1	294.1	1176.2	901.6	267.7	26.0%
St. Dev			61.7	651.1	30.6	31.9	351.7	153.8	786.9	259.9	192.9	19.1%
Min			2.9	7.4	0.0	3.4	36.7	181.5	337.5	574.3	1.8	0.2%
Median			19.8	57.7	8.8	34.1	702.5	190.5	971.4	924.3	282.6	26.6%
Max			408.6	3636	152.15	121.48	1855.8	549	4695.1	1396.8	499.9	60.7%
average %				24.0%	1.8%	3.7%	65.7%					
% from P-fract - resid P				32.0%	2.5%	4.9%	87.6%					

For the range values in the upper part of Table 1, I would assume these were the 95% confidence limits from the statistical test, rather than the actual data range. If this is the case, they seem about right, although I did not recalculate them.

Again, I was able to calculate the "separate TP" average in Table 1, but not the "TP from Fractionation". For this value, I calculated 1176 mg/kg, or about 15% less.

It is certainly possible that I am not understanding completely how these soil values were calculated after reading through the text, or that I made some mistake in my calculations. However, at this point I would have to conclude that your average TP-residual P values are about 20% high in the average, and that your fractionation values for WSP, LBP, AFBP, and CBP would need to have wider confidence ranges around them, given that the duplicate analytical tests show significant differences for the four values.

34. Page 23, Para 4: Comparing the authors western lake fraction percentages, I was able to reasonably reproduce with the all lake LBP, AFBP, and CBP percentages, but not for WSP. My value for WSP is much higher, influenced heavily by the initial few very high measurements.
35. Page 24 and Figure 15: I agree that the test showing no difference between the TP values from different tests is a good result, suggesting that TP values from each test are not significantly different. In support of my earlier comments that the average TP value seems high by about 20%, the second TP test shows that each of the eight measurements were lower than the TP-fraction measurement, and that the mean difference is 267ppm, or about 26% lower.
36. Page 26, Atmospheric Deposition section, Table 2. As mentioned previously, the TP flux to the lake is a little less than half of the maximum Olsen estimate for 2017. The TN value for 2018 is about 10% more than the maximum value reported for 2017. As shown in Table 3, there are very few differences between the two methods used to estimate the deposition from the sampling sites out into the lake. Both sets of estimates are generally the same, suggesting that the method of distribution is relatively insignificant to the final result. However, I am not all that familiar with the different modeling methodologies available to do this. Additionally, there is little text devoted to the calculation procedure. I would assume Reidhead generally used the same procedure. However, I am unable to check any of the calculations.
37. Page 27, Table 4: I am a bit unclear on the calculation used to move from TP in the right half of the table to SRP in the left half. From Table 8, SRP calculations were made for each measurement. But I did not see these values summarized in the text. From Table 8, I assume that when the SRP measurement was made and it resulted in BDL, then 0.0 mg/m2 was added in for that week? Was quality assurance

information on that test (blanks, duplicates, etc.) also measured for SRP? I did not see this reported in the document.

38. Page 27, Para 1: "The AD outliers…(Figures 10 and 11)." It would be good if Reidhead would have provided field records or local fire records to show that fires line up with the individual high concentration record for TP. This would bolster this statement and provide significant evidence that heavy deposition was coincident with high winds (daily average wind speeds, fire locations, wind directions, etc.). This comparison would pair the two ideas together nicely.

39. Page 28, Figure 16: This figure reports 2017 SRP values from the Olsen study. Did Olsen report 2017 SRP values? Or were these measurements unreported in his thesis? Were these calculated differently somehow? The values for 2018 are clearly lower for almost every site for TP but higher in TN, which is an interesting finding.

40. Page 30, Figure 19: These results show again that the SS site is clearly dominant for TP deposition, and would suggest further investigation at this site for additional reasons why. If it is blowing dust associated with high winds, is there some obvious reason why SS is more susceptible to this deposition than the other sites? I would expect all sites to be very high with a regional dust storm. SS is about three times higher during the high weeks than the other sites. Is there a local source of loose soil here that is not present at the other sites?

41. Page 20, Figure 20: By removing the very high values (>11 mg/kg), was the point to show that the sites are all relatively high together without the outliers?

42. Page 33, Para 1: Although I follow the logic here, I would think that having a significant difference in the medians would suggest that they are not from the same distribution. One conclusion from this is that local sources are the source. I would conclude that the sources are different. This could be saying the same thing, but different sources are a little less stringent. They could be two separate local sources of N perhaps, such as an active gravel pit and agricultural fields, or something a bit more complicated like this.

43. Page 38, Figure 30: Note my earlier comments on this test.

44. Page 39, Para 1: "A complete…" through the middle of page 40. I would agree. As I mentioned previously, I have questions about the repeatability of the fractionation tests. Although these values could certainly be relatively correct (given appropriate ranges of confidence), I do have my questions about the averages and error bars around the values. I must commend you for reporting the QA data (many do not),

and further for discussing the issues that you found and ways to improve the measurement. I think this would be a good next step to this part of the analysis.

45. Page 40, Para 2: I would agree that local sources are likely sources of significant P to Utah Lake. Your TP values suggest that there is significant TP in the soils. It is still unclear (to me at least) which levels are biologically available in these potential TP fluxes. These TP values are significantly larger than those reported in recent Brahney documents (TP=5.0±3 metric tons/year), but significantly lower than the high-end estimates of Olsen (350 tons/year). I do agree that local sources need to be investigated in detail, as you conclude.
46. Page 41, Para 2: Bird baths. NADP records would show that birds will likely use the water in a number of ways, including as baths, sit on the lips of sampling instrumentation, etc. I can see where if there is a large lake nearby, they may be less likely to use the samplers in this manner, but I would say from past records that they will affect the samples, regardless. It is assumed by NADP that bird feces will result in high ortho-P, which is primarily what the NADP uses the ortho-P measurements for.
47. Page 41, Para 3: Insects in samples. I agree that insects likely did have an impact upon the chemistry of the samples. Although NADP does not invalidate samples for insects, we do warn the data user of the occurrence of “debris in the sample”. In the case of a large number of insects in the sample, we would disqualify both the sample and concentrations. It is my opinion that insect deposition to the lake should be treated as a separate flux, with some type of estimate made to account for this flux. Whether this is called wet/dry atmospheric deposition or not is another question. Trying to keep insects out of a wet deposition sample is problematic. Furthermore, insects flying in the atmosphere can be swept out of the atmosphere into a wet deposition sample, and thus be considered truly part of “wet deposition”. A clear agreement on this issue is needed.
48. Page 32, Table 5: I am not clear what the units are in the N and P columns? Does the author think that these values could be a significant source of P and N?
49. Page 43, Para 1. As stated before, I think the soil fractionation TP values show the general high values of TP in the soils. I think there should be additional soil testing with more precise measurements (the author noted ideas) in mind. But I do think you can clearly say that the TP values are significant around the site, and therefore could be a major contributor to the lake flux of TP with significant levels of soil depositing into the lake.

50. Page 43, Para 2: 1 mg TP/g soil. This is very similar to the average Wasatch Front soil concentration as reported by Dr. Brahney, per her 2019 report on the topic. That they both are reporting the same value gives scientific credibility to this value.
51. Page 45, Para 2: The authors report here that they measure approximately 10% of TP is WSP, LP and AFBP fractions. Dr. Brahney (2019) reports normal Colorado and Utah values are approximately 34% water soluble and organic, and available to biological life to use. I don’t know for sure, but assuming that these fractions are roughly equal, the estimates are significantly different.
52. Page 49, Para 1: Rerun of samples for TP fractionation. I would agree with this. More data would be good to have availed for future estimates.
53. Page 50, Para 1: I would agree that study of the lake sediments and the TP/TN in those sediments should be evaluated for future use.
54. Page 50, Para 2: I would agree with all of these recommendations for improved deposition sampling; raising the samplers, moving the solar panel (not mentioned), and a midlake sampler (if possible) would clear up some of the issues with lake deposition measures.
55. Page 50, Para 3: This might be a good idea. An alternative might be to compare other metals in the soils versus samples collected. This may give clues to the identity of the solids in the samples; i.e. a statistical principal components analysis on an array of metals may allow detection of a soil signal.

Review of Document
David A. Gay, Ph.D.
November, 2019

Document: *Proposal to Measure Atmospheric Deposition in Utah Lake (Atmospheric Deposition Workplan – Version 4)*, by Dr. Theron Miller, August 2019 revision.

General Comments

1. I think that this proposal is a good start of a sampling plan that generally continues the sampling of the Olsen/Reidhead work in 2017 and 2018, with suggestions on how to improve the measurements from past comments and suggestions. I gather that this document is in the development stage, and therefore additions will be made since it is labeled as version 4. Consistency is good in this sampling plan.
2. As it stands now, the proposal is somewhat incomplete. The document will need some cleanup in the presentation, it does not include any information about the field methods for sample collection, there is no little information about the laboratory analysis, there is little quality assurance information that will be collected for the project, etc. This information will need to be added to the document before it is released to a review committee or to the public. I also have suggested a few additions to improve the sampling plan (below).
3. Any time that you say that you will measure dry deposition (DD), you are opening yourself up to criticism. The problem is that scientists do not agree on how to do it accurately and well. So, if you plan to measure DD, then someone is not going to agree with how the study was done. Some methods are likely better for some analytes, while other methods would be better for others. But there is no one perfect method. That is one reason why NADP stopped making dry deposition measurements to what is called the dry side bucket.
4. What NADP members have decided to do is to measure the atmospheric concentration of particular analytes, use models to estimate the deposition based on resistances to analyte movement, and estimate the resulting deposition velocity and flux. Although this method has its critics, there are a few advantages to using this method. Essentially, you can make the atmospheric concentration measurements in a scientific and valid manner, which most will generally agree on. You can then provide this valid information to those who do not like the model that you use or the assumptions that you have made, and then they can use the same basic

information to estimate deposition using their preferred method/model. For more information, see http://nadp.slh.wisc.edu/committees/tdep/.

Specific Comments:

1. Page 1, Para 1: "Notably...didn't begin till 2009." This is not actually correct. Ammonium concentration measurements from the NTN Network have been collected from the beginning of operation, which was 1978.
2. Page 1, Para 1: NADP phosphorus data. NADP does measure ortho-phosphate (PO_4^{3-}), and has since the beginning of the network. But ortho-P data is handled in a different way, and is not located with the other data on the website. NADP made the decision early in its operate to look on ortho-phosphate as an indication of contamination (bird feces, etc.). That view has continued, and therefore a high ortho-P value is viewed as a reason to check the sample quality. This ortho-P data is available, but only by request. Additionally, there has been some discussion on adding Total Phosphorus (TP) measurements, but this has yet to be approved.
3. Page 1, Para 2: NTN made "dry deposition" measurements early, but was discontinued in or about 1985. It became clear to NADP that the measurements were just estimating dry deposition of NTN analytes to a plastic bucket, and that the results had little use in estimating dry deposition to real surfaces in the environment (leaves, grasses, lakes, streams, etc.). And following from this paragraph, I agree that measuring dry deposition is fraught with problems, assumptions, etc., and it is very true that dry deposition studies are very specific to the project at hand, rather than a long-term monitoring effort on a regional or national scale. Again, see nadp.slh.wisc.edu/committees/tdep/.
4. Page 2, initial Para: "In addition…than wet deposition samples." I agree. The latest thinking also follows from a current document on phosphorus, which I would recommend. The authors suggest that atmospheric deposition is the key source, and that it is more likely to be dry deposition rather than wet deposition.

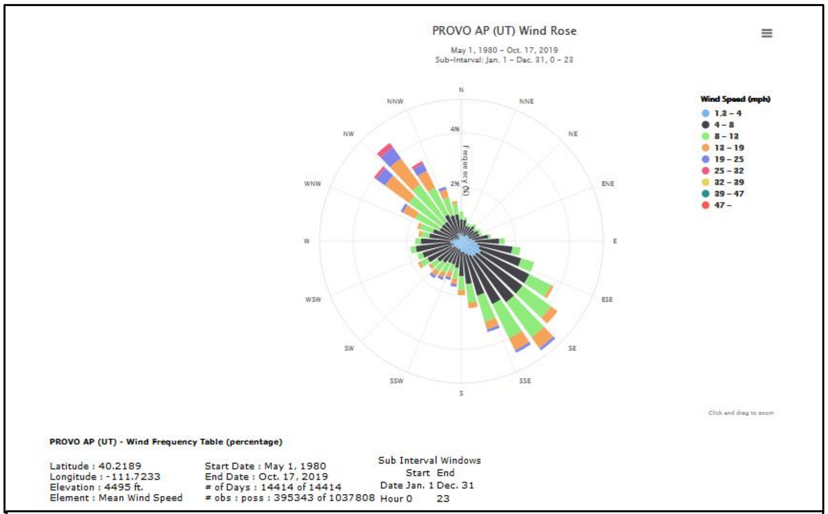
   > *Stoddard, J. L., Van Sickle, J., Herlihy, A. T., Brahney, J., Paulsen, S., Peck, D. V., ... & Pollard, A. I. (2016). Continental-scale increase in lake and stream phosphorus: Are oligotrophic systems disappearing in the United States?. Environmental science & technology, 50(7), 3409-3415.*

   (Note that Dr. Brahney is a coauthor on this paper.)
5. Page 2, initial Para: "Conversely…pollution levels". I generally agree that this is true. But I also think that it is more nuanced than this. If a pollutant is water soluble,

and it is raining or snowing, then a local source will surely be contributing to wet deposition. We see this often with mercury. Sulfate is a regional pollutant because is oxidizes quickly and therefore is generally deposited locally to regionally and is not generally considered to be transported long range. It is not to say that it cannot be transmitted over long distances, but I would say that it is generally not the case. Further, I would say that dry deposition generally dominates in low precipitation areas. Deposition of larger particulates are generally associated with local sources.

6. Page 2, Para 1: "Notably…can be ignored". I don't agree fully with this point. I think this is generally true in certain locations where it rains every day as in the tropics, for example. However, I don't think this is the case in the southeastern USA, in a region that I would consider to be a rainy area. With very high temperatures and soils that dry out quickly, dry deposition can also be an important flux. But in general, I think dry deposition is likely to dominate in the western U.S.
7. Page 2, Para 2: "Furthermore…780 ppm." I agree that intercontinental transport of dust and phosphorus can be important.
8. Page 3, Para 1: I am not familiar with the Lake Bonneville situation, but I would have to think that it is a likely important source of dry deposition to a significant part of Utah, given its size and open fetch, and likely higher windspeeds over the area.
9. Page 3, Para 2: Siting of current NADP sites. I agree with the conclusion that the currently operating NADP sites do not necessarily record the conditions at Sevier Lake. Bryce Canyon, Canyonlands, and Green River sites are all in the southern part of the state. East McKee is generally downwind, but as noted, is separated from the lake by mountainous territory. The Logan site is likely the most closely related to Utah Lake, but it is quite a distance from this lake area and therefore, not a substitute for onsite measurements. However, it is near Salt Lake City, and may offer some clues into the deposition in area of interest.
10. Page 3, Para 2: "Hence…at these sites." I generally agree, but there is a possibility that the Logan NTN site does see a signal from the Salt Flats, at least based on the basic maps. They are quite large, and could move dust up to a high enough level to travel (with a southwest wind) over to Logan. But of course, it would need to then be raining and washing this dust out into the wet deposition sample.
11. Page 3, Para 3: "Alternatively…Salt Lake Desert". I agreed. My guess is that we would likely be able to find satellite evidence of blowing dust from this area moving towards the northeast, if needed.

12. Page 3 & 4, Sevier Lake as a potential source: I would say that the document needs an annual, and possibly monthly wind roses from the Utah Lake area. I downloaded these wind roses (mrcc.illinois.edu/CLIMATE/) for the Provo Airport (below) using hourly data. Now, given the complex terrain in the area, the wind rose could certainly be different from the lake area given the airport's particular location. However, if this is not the case, then this wind rose suggests that the dominant winds are not from the southwest to the northeast, but from the southeast and from the northwest.



PROVO AP (UT) - Wind Frequency Table (percentage)

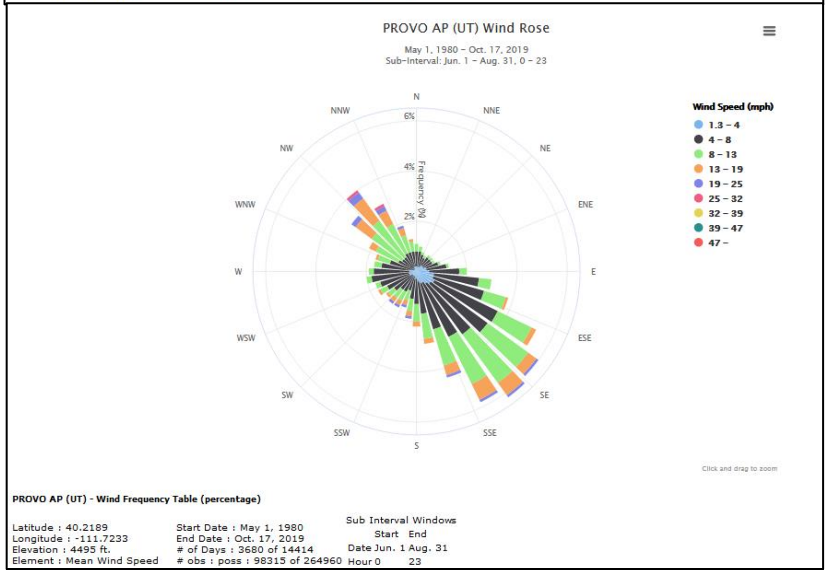
Latitude : 40.2189
Longitude : -111.7233
Elevation : 4495 ft.
Element : Mean Wind Speed

Start Date : May 1, 1980
End Date : Oct. 17, 2019
# of Days : 14414 of 14414
obs : poss : 395343 of 1037808

Sub Interval Windows

	Start	End
Date	Jan. 1	Dec. 31
Hour	0	23

I also downloaded the summer wind rose (June 1-Aug 31) which would be the season I would expect most dust transport to occur. Again, this highlights that the dominate wind direction in Provo is from the Southeast, and not from Sevier Lake. This is true at least most of the time.



PROVO AP (UT) - Wind Frequency Table (percentage)

Latitude : 40.2189
Longitude : -111.7233
Elevation : 4495 ft.
Element : Mean Wind Speed

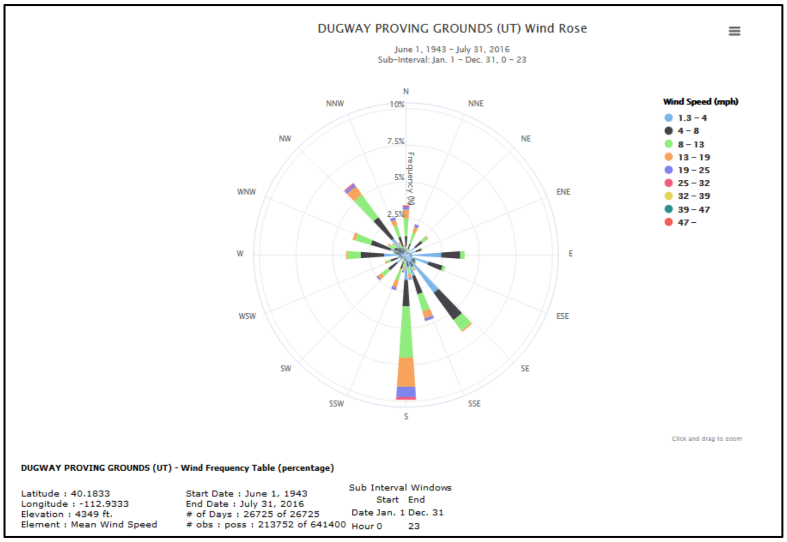
Start Date : May 1, 1980
End Date : Oct. 17, 2019
# of Days : 3680 of 14414
obs : poss : 98315 of 264960

Sub Interval Windows

	Start	End
Date	Jun. 1	Aug. 31
Hour	0	23

From an online map, I see that Provo Airport is at the lake boundary, in relatively open ground. But the mountains generally run southeast to northwest, and could be controlling the local winds.

I also got the annual wind rose from Dugway Proving Grounds, in/near the Salt Flats. The winds at Dugway are a bit more southerly than in Provo, but this site also does not show a distinct SW wind direction over the entire year. These three wind roses show that the wind is not necessarily from the Sevier Lake area, but primarily from the southern region of Utah, and locally from the suburbs of Spanish Fork and Payson.



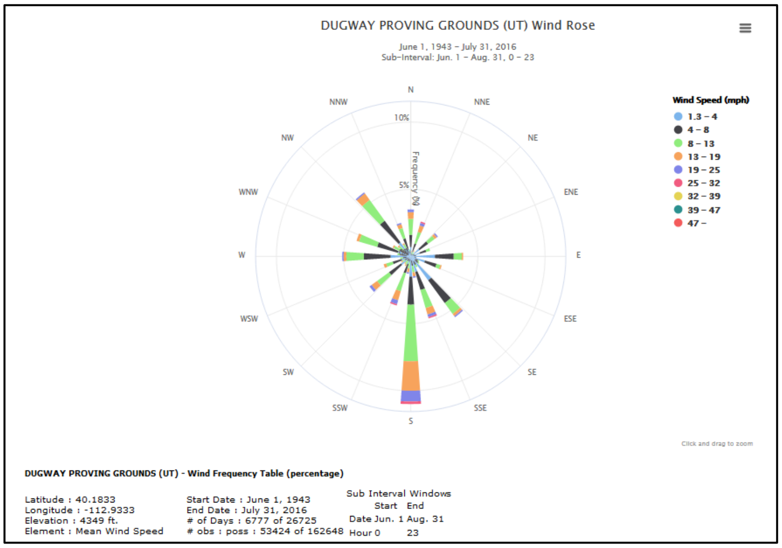
DUGWAY PROVING GROUNDS (UT) - Wind Frequency Table (percentage)

Latitude : 40.1833
Longitude : -112.9333
Elevation : 4349 ft.
Element : Mean Wind Speed

Start Date : June 1, 1943
End Date : July 31, 2016
# of Days : 26725 of 26725
obs : poss : 213752 of 641400

Sub Interval Windows
Start End
Date Jan. 1 Dec. 31
Hour 0 23

It could be true that on certain days you have significant and high winds from the west and southwest creating dust storm conditions as noted in the Deseret News Article (page 3). However, the data that I have found thus far suggests that these conditions would not occur frequently. The summer wind rose from Dugway suggests that a southwest component is a bit more important (below). But based on these simple tools and not having a very good sense of the area, these wind roses suggest that the largest surface and local sources (or perhaps more important sources) would be to the south and the southeast of Utah Lake.



DUGWAY PROVING GROUNDS (UT) - Wind Frequency Table (percentage)

Latitude : 40.1833
Longitude : -112.9333
Elevation : 4349 ft.
Element : Mean Wind Speed

Start Date : June 1, 1943
End Date : July 31, 2016
# of Days : 6777 of 26725
obs : poss : 53424 of 162648

Sub Interval Windows
Start End
Date Jun. 1 Aug. 31
Hour 0 23

Upper level winds could be very different, and should have more of a westerly component. But dust storms tend to be relatively shallow, near-surface features.

13. Page 3 & 4. It occurs to me that we could do a back trajectory analysis from Utah Lake over the entire year (or multiple years) to see how often and when the winds from any potential sources.

14. Page 4, Para 3: "Dr. Wood…2.5 years." I have some concerns about these samples. My concerns do not make them invalid, but I think they could be somewhat problematic.
15. Page 4, Para 3: "it is possible … conservative estimate of deposition." I agree that particulate deposited to the funnel surface can be resuspended with winds blowing after deposition. However, I think these samples would then be biased low versus a no-funnel bulk collector. Additionally, other issues could be present, including nitrogen and phosphorus losses to the sampler walls, bacterial action consuming N and P, cleanliness between samples, etc.
16. Page 4, Para 3: "Unpublished data". I would recommend checking to make sure that the data is still unpublished, and I hope that it is now published. Basing an argument on unpublished and unreviewed data can be problematic.
17. Page 4, Para 4: "The proportion of…scope." But this does present an opportunity. What I am thinking is that if you also analyze for Ca^{2+} and Na^{+} in deposition and the correlation to TP is very high, that provides you with something of a tracer and evidence that the TP is likely to be associated with soil/sediment. It is also possible that these sediments would be high in a particular metal, for example, and you could test for this metal and use it somewhat as a tracer for lakebed sediments.
18. Page 4, Para 4: "We believe…reach the lake." I agree. The urbanization and surrounding agriculture should definitely be considered potential sources of mass flow to Utah Lake.
19. Page 5, Figure 1: Based on Figure 1, I would suspect the ancient Bonneville Lake sediments south of Utah Lake (dominant wind), and any agriculture activity in this region are likely important sources. I did not realize how vast the lake sediments were in the western half of Utah.
20. Page 8: Just a comment from the Owens Lake discussion: You could do particulate sampling based on wind direction too, with the resulting samples used to determine TP and TN. So, for example, you could make total particulate measurements for eight different wind directions (45-degree quadrants). This would allow you to conclude that TP from the south, the southwest, or the west is the dominant wind direction for high concentrations of particulate and TP. Or, it might tell you that all of the wind sectors have important TP deposition.
21. Page 9, Objectives: The primary objective seems to be very valid, and one that can be accomplished with certain assumptions being made (for dry deposition). But it

seems that if source apportionment is beyond the scope of the project, then it will not provide you with the information that you need to potentially control the source, if control is possible.

22. Page 9, Para 1: "Moreover, with…selection". I don't think that this is exactly correct. NADP is focused on regional sources, and our guidelines are designed to avoid collecting samples that are dominated by local sources. This is because people use our data with the assumption (which is correct) that our samples represent regionally consistent samples. However, our equipment and methods can be used at a local site to collect the impact of local samples. You would just need to discount some of our guidelines. So, I think the sample plan for wet deposition of samples is to use NADP methods, with some guideline changes in and around the lake. Wet deposition is relatively easy part of this sampling plan.
23. Page 9, Objective 1: I assume you mean total P and N, correct? I would specify this. But I agree, the objective is to determine the total flux of P and N to the lake, broken into wet deposition and dry deposition contributions. I do not understand why Table 1 is referenced here (NADP site classification).
24. Page 9, Objective 2:
    a) Comparing USGS to Wood; are there observations made with the USGS method at the lake? Are these methods available in SOPs etc.? Could these be directly incorporated into the sampling plan?
    b) Comparing to the Wood measurements; I think this would be a good thing to do. If they compare favorably, you have much more data available for averages, predicting the future, etc. However, I do have my concerns about this sample method (as mentioned previously).
    c) NADP dry design; Officially, there is no NADP design for dry deposition measurements to compare against (except for ammonia gas). However, NADP does measure atmospheric concentrations and estimate dry deposition through models. This approach should be considered for the project design. And this general approach could be used to estimate all dry deposition, as an additional estimate, or as a substitute method for making dry deposition measurements.
25. Page 9, Objective 3:
    a) I agree with the goal of the screen and attempting to keep the bugs out of the dry deposition sample. This will make the results more easily justified and

understood. But if insects/vegetation addition to the lake is substantial, it needs to be included in the flux of TN and TP to the lake.

b) For wet deposition, the screen surface will likely provide a surface for precipitation bounce and contamination to occur. Essentially, the raindrop could hit the screen, be converted into many smaller droplets, and some of these could bounce away from the sample collector. This would cause a loss of some of the precipitation. Also, droplets hitting the screen could wash off dry deposition on the screen into the wet deposition sample. This is why NADP does not use any type of screening over its wet deposition sampler. Additionally, snow will not go into the sample collector without melting, giving it more time to blow over the sample collection system. So, you definitely do not want to use a screening system during the winter for wet deposition.

c) It is at least possible that some particulate will get hung up on the screen and not make it to the sample, unless the screen was washed off. And it could be that the screen will change the aerodynamics of the opening and therefore, change the dry deposition of the sample.

d) Overall, I think you would need to reconsider this option of whether to move forward with it or not.

e) For plant contamination in particular, I would argue that this could be part of the sample because that plant material (and its TP and TN) is real deposition to the lake. For example, if plant matter is washed out of the atmosphere with rain, it would properly be called wet deposition. But regardless, a significant plant matter source of TP/TN is adding/subtracting from the lake flux. Therefore, at the very least it needs to be accounted for in the overall flux estimate to the lake, if not considered part of the wet and dry deposition fluxes.

26. Page 9, Objective 4: I am not clear on the current height of the samplers. However, assuming they are around 0.5 meters or so, the wet deposition component will likely be no different during rainfall. However, if you are currently collecting snow, a lower level sampler is likely picking up more blowing snow and therefore, would show significant differences. For dry deposition, I would think that the lower the bulk sampler is, the more dry particulate it would collect, since bigger particles would be more likely rolling and bouncing along the ground. Also, the windspeed should decrease nearer the ground, allowing larger particles to come out of the atmosphere and drop into the sampler. However, the lower the sampler, the more likely that the sample will be contaminated due to activity on/near the ground. The

standard height for a wet deposition collection is 1.5 to 2.0 meters above grade, and I would suggest that this height be continued unless a specific reason is presented otherwise.

27. Page 9, Objective 6 (there does not seem to be a 5): I think putting a bulk sampler in the middle of the lake to determine a decay constant is a good idea, theoretically. However, making it a reality seems to be very problematic. Given that the lake is relatively shallow, building a platform (or a stable boat) seems fairly straight forward. But you have to visit the site for every sample, there will be no power, etc. So making this site actually work would be difficult, and likely expensive.
28. Page 9: Overall, I agree that putting a range of monitors around the lake, to get different input of dry deposition from different sources is an appropriate method. That should allow you to draw a flux map over the lake, with multiple measurements of deposition at the edge and in the rough center of the lake. But then, then there is the question of the appropriateness of the measurements.
29. Page 9/10: Overall comment on siting: I think in general that the closer to the lake the better for all samplers. Here, they would more likely capture all signals/sources adding to the lake flux.
30. Page 9, Para 3: Orem site: What is in the storage shed? Anything with N or P in it? Ag chemicals? I am wondering because if there is anything being used, going in and out, etc. it could affect the emission rates from the source area.
31. Page 10, JR Pump Station Site, Phragmites: The plant particles are getting into the lake and therefore, will be part of the N & P loading. However, if you want to truly separate the dry deposition from the wet deposition flux numbers, then it really isn't wet deposition in its true sense. However, if the particles were at 1000 meters above grade and they got washed out of the atmosphere, we would call it part of the wet deposition flux. This is a good point on the dust addition to the Phragmites; they will be full of N/P and dust, most likely.
32. Page 10, Specific method A: I generally like this approach of using a water surface, particularly because the surface is more similar to the lake surface and for its collection of particles as noted from the references. However, the overall problem and questions surrounding the methods are still present. Principally, the aerodynamics around and to the sampler are likely very different from the lake surface (waves, temperatures, chemical nature), and there is no data to prove that the flux rate to the sample water/collector is equal to the lake flux rate. Additionally, the samplers will not be at lake surface elevation, so wind differences will be

present. But I think this is better than measuring dry deposition to a stainless steel bucket or receptor. Will there be any evasion of N/P from the sample? Evaporation from the sampler? NADP has noted this with a covered wet deposition sampler and mercury.

33. Page 10, Sp. Method b: I am aware of the marble sampler (Dr. Brahney, Utah State) and this method has been tried in association with NADP sites. But I think in general, the results were less than satisfactory. I do think the marbles will capture and keep large particles, but I would recommend that you consult directly with Dr. Brahney on this issue (or search for journal articles, etc.). This is particularly true if you use water in the marbles. I don't think that the marble design will be particularly effective for gaseous deposition, and therefore, for TN dry deposition.
34. Page 11, Sp. Method c: I am just noting here the particular issues with screens and wet deposition as mentioned previously. The mesh size seems particularly small for allowing particles free movement through the screen and into the sample collector. I agree with the last sentence about direct contact with the water.
35. Page 11, Sp. Method d: Lake sample site. "Wet and dry design". Do you mean that this sampler will be two samples: one wet deposition sample and one dry deposition sample? It is just unclear, but this would be my recommendation of the collections to be made.
36. Page 11, Sp. Method e: I would hope that the solar panels are more than 5 meters away from the sampler, as well as 20 cm below the opening. This would be best. For the NADP galvanized steel recommendation, the recommendation was put in place with the idea that metals analysis might one day be conducted within the network. Therefore, the NADP sites would want to avoid galvanized metals and treated wood. Stainless steel is the best to use for the structures. I am not sure if the QC method will be appropriate since these structures are very likely to collect dry deposition and contaminate the cloth upon wiping the structures.
37. Page 13, Gillette et al.; This could be used perhaps as a guideline for doing a combined wet deposition/atmospheric concentration approach.
38. Page 14: Zobrist et al.; If you choose to go through with the wet surface sampling, we (I) need to read through this paper, and papers that have critiqued it to determine what others say about the approach.
39. Page 15: Caption. This is a draft report, but this caption is just out in the document, or there was a picture missing in my version.

Suggestions for the Sampling Plan

I offer these suggestions, based on the different reports that I have read about the Utah Lake project to date. They are certainly only suggestions at this point. I have given no consideration to funding, or time constraints, etc. which are always present.

Quality Assurance

1. I would consider having the lab join with USGS/others for an intercomparison of the analytes measured. This will give evidence to support the analytical accuracy for the measurements. NADP continues to contribute to this type of interlaboratory comparison.
2. An increase in the number of analytical duplicates and blanks for both wet and dry chemistry.
3. I would suggest a written and detailed quality assurance plan, and releasing all of your quality assurance data with your reporting, again to provide supporting justification for the analytical results.
4. I suggest a straightforward plan for insects in the samples, or a decision to remove them all, and perhaps use screens on the dry deposition samples to try and prevent them from being part of the sample.
5. Make a determination of whether bird feces in the samples will invalidate or not invalidate the sample, and move this idea into the SOPs for field operation.
6. Make a determination of whether algal growth in the samples will invalidate or not invalidate the sample, and move this idea into the SOPs for field operation.
7. Establish an NADP/NTN site at Utah Lake, collocated with one sampling site, where the NADP protocol will be used. This site would be used to bolster confidence in the sampling program and results, by providing an independent site for comparison. This site will automatically come with a separate quality assurance program on all aspects of the laboratory and sampling, and provide support for future measurements. This would be most useful for wet deposition of N, but would also provide ortho-P measurements for comparison. I would recommend that it be placed at the Saratoga Springs site.

Field Operation

8. I would suggest a written field Standard Operating Procedure for Field Sampling, and I would recommend starting and modifying with the NADP Field SOPs (which are available for use). I would also suggest some type of weekly field report for each site and sample, where notes can be kept in an organized fashion. You may also consider sample images of cloudy, or particle field samples
9. I recommend raising the sampler opening to between 1.5 meters to 2.0 meters. Along with this, I also recommend moving the solar panels away from the sample collectors which will improve the measurements for both wet and dry deposition.
10. I recommend establishing an onsite digital rain gage (alternatively, it would be part of an NADP/NTN site) and a digital record of sampler openings and closings, where the current sampler operation would be recorded along with periods of precipitation. A consideration of an onsite meteorological site (likely at Saratoga Spring) would also be very useful.
11. Consistent weekly samples for the full year (Tues to Tues), with no movement to double week samples.

Other

12. Consider including calcium ion measurements, and perhaps other analytes useful for further evidence of the sources of the samples and TN/TP.
13. I would suggest inviting Dr. Brahney to come to Utah Lake and her dry TP sampler side by side with the project samplers, specifically for dry deposition of phosphorus. Her design seems to be well reasoned and scientifically defensible, and I assume that her results have been/will soon should be published. If this can be accomplished, the project would gain scientific credibility, repeatability, and perhaps more consensus.
14. A source identification survey in the immediate area of the lake to identify all potential sources of TP and TN.
15. A sampling review of the Saratoga Springs siting and the nearby sources, and perhaps some type of sensitivity study to the location of the Saratoga Springs sampler due to its influence on the flux to the lake.
16. I would suggest multiple years of consistent sampling, to capture the year to year variation as suggested by the 2017/2018 measurements of Olsen and Reidhead.
17. The NADP/NTN is slowing being moved to bag sampling (a plastic liner in the current bucket), that as clean or cleaner than the current bucket washing procedures.

This change would remove or clarify many of the questions about quality assurance of the sample cleaning and handling, and further bolster the analytical and field results.

18. I would recommend a determination of insect flux to Utah Lake, separate from wet/dry deposition.
19. I would recommend a back-trajectory analysis using the Provo Airport metrological information (and perhaps other stations) to identify high wind conditions, samples associated with these potentially important deposition samples, and evidence of the sources of the same.
20. Consideration should be given to collecting atmospheric particulates and TP for modeling determination of TP dry deposition. This would primarily be based on size selective sampling of atmospheric particles.
21. Careful consideration of the sampling location in/over Utah Lake, as has been proposed. It has merit, I agree, but will be difficult and costly. Results of a continuous sampling routine in the midst of the lake would certainly provide very useful results.
22. Consider a sampling plan using regional NADP measurements of wet and dry deposition of N (TDep estimates), modeling flux estimates of local TN sources, along with wet deposition measurements of TP, and a modeling project for estimating both regional and local sources of TP dry deposition.