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8 November 2021

Jon M. O'Brien, Environmental Program Manager
Off-Highway Motor Vehicle Recreation Division
California Department of Parks and Recreation
1725 23rd Street, Suite 200
Sacramento, CA 95816

Dear Mr. O'Brien,

Please find attached my specific responses to the comments provided by the San Luis Obispo Air Pollution Control District (APCD) on 28 October 2021 regarding my 1 September 2021 Interim Report, as well as an Appendix B to my Interim Report to provide the additional detail needed to address the questions raised here.

The main APCD criticism is that our flow configuration allowed for particle losses that the APCD speculates can explain not only the difference between their BAM and our gravimetric measurements but also the difference between their mineral dust fraction and ours. Our particle loss calculations show that their explanation is unlikely given the sampling configuration, and our gravimetric measurements show many samples when there are no losses at all. If anything, as detailed in the Appendix B of the Interim Report, our method should over-sample coarse particles in the PM10 fraction, which provides a conservative upper bound determination of mineral dust content rather than an under-sampling as the APCD has claimed.

In their comments, the APCD states that it is unlikely that differences in mass between our gravimetric measurements and their BAM measurements are due to evaporative loss, but Appendix B counters the APCD claim by showing the temperature dependence of the ratio of gravimetric to BAM measurements. The additional weak correlation to the difference between the ambient and instrument room temperatures provides further evidence for the role of semivolatile uptake and evaporation. They have also ignored the large body of peer-reviewed, scientific literature cited in the Interim Report that have documented problems with comparisons of BAM and gravimetric methods, especially in places like coastal California. And the APCD says Scripps speciation data should be interpreted cautiously, but it has not yet released any speciated data that support a substantially different result.

To further address APCD's criticism of our methodology, I note the APCD's reference to their own similar findings in Attachment 14 to State Park's 2 August 2021 Annual Report and Work Plan (https://storage.googleapis.com/slocleanair-org/images/cms/upload/files/DRAFT%202021%20ODSVRA%20ARWP_w%20attachments.pdf). Here, the APCD notes the

same low bias that in our data they attribute to our sampling methodology. The APCD shared with me the 2020 PM10 and mineral dust measurements that underlie that statement, which I analyzed in the Supplemental Report that I submitted to State Parks on 25 October 2021. This Supplemental Report shows that APCD's own 2020 gravimetric and mineral dust results are consistent with our results – not contradictory – when compared on a similar basis (*i.e.* to BAM not gravimetric) and with similar crustal component allocations (*i.e.* specific elemental calculation not regional approximation) for high-wind, high-PM10 days. This Supplemental Report is being withheld by State Parks at the request of APCD, and its release is expected in January 2022 when APCD has planned to release their report. When released, my Supplemental Report will show that the APCD questions about our methodology are without foundation, since their own methodology produces similar results (but for 24-hr rather than 7-hr sampling).

Finally, it is warranted to place the APCD's critique of my work on the mineral dust content of the PM10 on the Nipomo Mesa in the broader context of the APCD's regulatory authority: The APCD had many years prior to the start of the Scripps grant to determine and monitor the mineral dust contribution to PM10 but failed to do so, without explanation. In fact, in their role of providing air quality information and expertise to the SAG and State Parks, it seems they failed to advise those bodies of the relevance of such measurements to the justification for the Stipulated Order of Abatement. The Scripps findings now highlight this omission and so call into to question more than a decade of funding and programmatic decisions that have impacted State Parks. Instead of recognizing the contribution of our work and joining with us (as we have invited them to do) in compiling the available information to advise Parks with due diligence, the APCD's comments reflect defensiveness and avoidance of the facts.

Please do not hesitate to contact me if you have any questions.

Best regards,

Lynn M. Russell
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APCD Comments with Scripps Responses on Scripps/UCSD Interim Report 2021

APCD Description or Context	APCD Comment	Scripps Response (all references are provided in Interim Report)
However, the Scripps samplers also incorporated flow splitters between their size separators and filter holders. The District believes these flow splitters likely resulted in biased sample collection at the filter, since they do not appear to have been engineered to ensure laminar, isokinetic flow. In contrast to the Scripps devices, EPA-approved particulate samplers—including those used by the District—either direct their entire flow to the filter/detector or use flow splitters that are engineered to ensure isokinetic flow.	The report provides no data and cites no references to demonstrate that these novel devices perform as assumed.	To address these details, which were not of interest to APCD when we shared our planned sampling prior to last May, we provide here Appendix B to our report showing the detailed, state-of-the-art calculations that were used to determine appropriate sampling configuration. These calculations show that losses for both PM10 and PM2.5 are negligible for the configuration used. Moreover, there is no evidence that the enhancement expected theoretically was significant. In addition, we show multiple pieces of evidence that indicate the role of evaporation by temperature dependence.
These comments are specifically on the version dated September 30, 2021, provided to the District on about October 1, 2021...This is the fourth in a series of reports by Scripps/UCSD on State Parks-funded research at the Oceano Dunes.		Here the APCD notes that their comments are specific to this report, but succeeding comments by the APCD regard earlier reports about different investigative years. This is an expansion of scope of the review process for this report and a contradiction of the APCD's own statement. This is inappropriate given the State Parks' protocol, as well as surprising since they have previously argued that our past reports were not relevant.
The first report in the series, "Marine Contributions to Aerosol Particulates in a Coastal Environment," dated March 6, 2018, described the results of DNA analysis of E-BAM filter tapes.1 The report was touted in some circles as evidence that vehicle activity at the ODSVRA is not the cause of the PM10 issue, but the District did not find the study to be relevant to the issue, as we described in a June 2019 FAQ2 and a comment letter to State Parks.3	We also offered suggestions for how future investigations could be improved.	I was not provided any suggestions from APCD on how future investigations could be improved. The results in this report led by Palenik clearly showed a contribution of marine aerosol at a dune site, which contradicted the SLOAPCD 2007 report's assertion that coarse aerosol could be assumed to be mineral dust.
The next report was released in early 2020. "First Year (2019) Summary Report: Investigation of Aerosol Particulates in a Coastal Setting, South San Luis Obispo County, California" described the analysis of air samples collected by Scripps at the District's CDF monitoring site.4	Reviews of the report by the District and members of the Scientific Advisory Group (SAG) noted several methodological and other issues with the study and its findings.5 The District also provided suggestions for improving future sampling campaigns.	This report was summarized at submission on 2/21/20 as "As stated in the report, we found that during high wind conditions natural sea salt and inert mineral dust measured in the PM2.5 fraction at the CDF location together accounts for only 30% to 50% of the PM2.5 measured at the same location by BAM instrumentation operated by the local air district (SLOAPCD)." The range reported is completely consistent with this report. The suggested changes in methodology did not significantly change the results.
The third report, "UCSD Supplemental Report 2020: Preliminary Results from May 2020 Aerosol Measurements," was released in September 2020.6 As with the previous reports, the District and SAG were critical of it, noting problems with the study design, sampling methodology, and data presentation, and again offering suggestion for improving future work.7,8	As with the previous reports, the District and SAG were critical of it, noting problems with the study design, sampling methodology, and data presentation, and again offering suggestion for improving future work.7,8	This report was summarized on 9/19/20 as "On average, the measured mineral dust mass is 20% of the BAM's PM2.5 values on high PM10 days." This mineral dust contribution was slightly lower than the prior year, which is not unexpected given the differences in meteorology and sampling. Such year-to-year differences are clearly discussed in the report. Again, the suggested changes in methodology did not significantly change the results.
The current report describes work conducted earlier this year. The field work incorporates some of the suggestions made by the SAG and the District in our previous reviews, including adding PM10 sampling to the study and collecting PM2.5 samples using a VSCC size separator.	Nonetheless, the District has identified deficiencies in the study and has serious concerns about the report's conclusions.	This report includes the first and to date only measurements of mineral dust in PM10 at APCD sites on Nipomo Mesa that are publicly available. PM10 was not the target of the Scripps research contract because our work was intended to focus on the more health-relevant fine particles and specifically the role of sea spray. The objectives were modified to include PM10 not because of the suggestion of APCD to do so, but because it had only then become clear that there was a need for PM10 chemical measurements because APCD had no such evidence of their own. This lack of PM10 quantification of mineral dust was surprising given that APCD had initiated the SOA for ODSVRA years earlier. Yet on careful review, the SLOAPCD reports from Phase 1 (2007) and Phase 2 (2010) did not include any measurements of mineral dust fraction of PM10 at APCD sites on the Nipomo Mesa. An explanation for this omission has not yet been proferred. Further, below and in the attachments cited, we show that the what the APCD characterized as "deficiencies" actually have negligible impact on the main objective, namely quantifying the mineral dust fraction of BAM.
Like the previous Scripps report,6 the current one discusses samples collected at the District's CDF monitoring station during the spring windy season. The previous report found the mass concentrations of their PM2.5 samples to be an average of 26% lower than the masses recorded by the District's PM2.5 BAM instrument. On high PM10 days—defined as days when hourly PM10 exceeds 140 ug/m3 in the afternoon—the difference was even larger, with the Scripps samples 38% lower than the District's measurements.		In this discussion, APCD fails to clarify that the method for mass measurements was different between the District, which used BAM, and Scripps, which used gravimetric. There are several known differences between these methods (Tao and Harley, 2014; Chow and Watson, 2008). In APCD comments on the prior Scripps study, this difference was attributed by the APCD to the difference between SCC and VSCC samplers. We argued that this could only explain the differences if the dust mode peak was smaller than the PM2.5 cutoff, which was not expected -- but had been noted by internal APCD reports previously (Craig, 2011). For this reason, we agreed to compare the two in the 2021 study, even though other factors like sampling times, sampling days, wind conditions, and relative humidity were also likely to contribute to differences between BAM and gravimetric methods.
The results of Scripps's 2021 sampling are also lower than the District's BAM measurements. For PM2.5, Scripps collected two sets of samples: one using a VSCC operated at 16.7 L/min (on loan from the District), and a second using an SCC operated at 7.5 L/min, the same method used in their 2020 sampling.	The VSCC samples are reported to be 13% lower than the District's BAM measurements on average, and 18% lower on high wind days. The SCC samples were 32% lower on average and 39% lower on high PM10 days. For PM10, the Scripps masses are 29% lower than the District's BAM measurements on average, and 35% lower on high PM10 days.	As noted above, while the VSCC did provide results closer to the BAM, there was still a difference between BAM and gravimetric that was likely due to water and semivolatiles (Tao and Harley, 2014; Chow and Watson, 2008).
The District has also collected PM10 and PM2.5 filters samples for gravimetric analysis at CDF.12 As shown in Table 1, below, the mass concentrations for these samples compare well to the measurements from the District's collocated BAMs, with all R2 values greater than 0.9 and most greater than 0.95. However, for PM10, the Scripps masses are only about 65% of the District's, as indicated by the slope of 0.65 for the regression of gravimetric mass versus BAM concentration, with the linear fit forced through the origin.	For the District's sampling campaigns, the slopes are much closer to 1—0.979 to 1.044. The same is true of the PM2.5 data.	The APCD results summarized here are not publicly available, but some of the data for 2019 and 2021 have been provided to me. (The 2018 report does not include data for individual days nor does it provide BAM and gravimetric results for each day.) I find they cover a different concentration range, different sample duration, and different dates than our sampling. This table shows APCD 2019 FAL data sets for PM10 with the unforced slope of ~0.88 for an unspecified number of samples during unknown date. The direction of the bias is that gravimetric is lower than BAM, in line with the Scripps results. Moreover, the 24-hr sampling of the APCD results is expected to give better agreement than the afternoon sampling carried out by Scripps, because of compensating effect of positive and negative biases.
		APCD says evaporative loss is unlikely, but peer-reviewed literature shows it is not only likely but frequent in California coastal regions, and the temperature and water dependence shown in Appendix B provide direct evidence of this here.
Scripps's 2020 report argued that "[i]t is likely that the 38% difference in mass on high PM10 days is due to water evaporating, although other semi-volatile components (ammonium nitrate and organic mass) could also be included in the BAM method and not in the gravimetric method."6 Their current report argues similarly that "it [is] likely that the difference in mass on high-PM10 days is due to adsorbed water and other semi-volatile components (ammonium nitrate and organic mass) evaporating less in the BAM method and more in the gravimetric method."	The District finds this explanation unlikely. We note, as we have previously, that while gravimetric methods are known to be subject to losses of water and semi-volatiles, the design of the BAM 1020 instrument includes a sample heater to mimic this effect and thus produce comparable results. If significantly more water and semi-volatiles were lost from the gravimetric method than from the BAM, then this ought to also affect the gravimetric samples collected by the District, but as seen in Table 1, the this is not the case.	APCD speculates that this effect of semivolatiles is unlikely, but the peer-reviewed literature show it is common. The fact that some data sets agree better than others is illustrative of the dependence of the agreement on variations in relative humidity and source composition. As noted above, the APCD PM10 data for 2019 (FAL) show gravimetric is ~12% below BAM, on average, which does not meet the federal standard of within 5%. Moreover, the APCD measurements are collected over 24 hr, during which there is time for compensation of low and high relative humidity and temeprature times in the diurnal cycle.
The report further speculates that "[a]nother possibility is that the BAM calibration does not apply well to the composition and concentration conditions that are relevant to this site." Prior to receiving FEM designations for PM10 and PM2.5 from the EPA, the BAM 1020's manufacturer had to conduct trials in diverse locations under diverse conditions to demonstrate equivalence to gravimetric, FRM methods.	Given the BAM's extensive track record since receiving EPA approval, it is unlikely that it would fail to generate comparable data in this location. Furthermore, as shown in Table 1, gravimetric samples collected by the District do indeed compare well to BAM measurements at this site.	APCD speculates such approval by EPA is unlikely without consistent results but does not provide a specific citation showing data that make this case. Certainly, had the EPA had such data in support of their decision, it would be publicly available. Moreover, since the BAM prior to 2008 did not have a heating function to correct for relative humidity, any results would have been subject to the well-known errors from adsorbed and evaporated water. As noted above, the PM10 data for 2019 (FAL) show gravimetric is ~12% below BAM, on average, which does not meet the federal standard of within 5%.
The Scripps reports also speculates that sample duration might play a role in explaining the discrepancy. Their samples were mid-day samples collected over 7 hours, while the District comparisons noted in Table 1 are for 24-hour samples. The report states that "errors often vary with time of day, with water adsorption in the BAM affecting afternoon readings and desorption affecting readings after midnight, so that hourly BAM concentrations may have biases of ~20 µg m-3 even when 24 hour averages include cancelling errors. [Kiss et al., 2017]" Again, the District finds this explanation unlikely. As shown in Figure A3 of the Scripps report, the Scripps samples were collected during the lowest humidity part of the day, when any positive bias in the BAM due to water adsorption would be at its minimum. Furthermore, Kiss et al., 2017, concludes that "Positive and negative apparent readings [of the BAM] are observed with increasing and decreasing relative humidities, respectively."13	As shown in Figure A3, generally humidity was decreasing during the beginning of Scripps's 7-hour sampling period and increasing at the end. While there was typically a net increase in relative humidity across the 7-hour period, the change was generally small (<10%), making it unlikely that humidity effects could account for the large discrepancy between Scripps's and the District's measurements.	First, it seems APCD has misread Figure A3, which shows a minimum in ambient relative humidity between 1100 and 1300 (standard time, start hour), reaching nearly maximum values above 90% at 1800. This means the relative humidity is increasing during most if not all of the sample duration, making the positive bias of the BAM consistent with what was reported (Kiss et al., 2017). Second, APCD speculation about the change being small, is both unsupported and not relevant to the point that by taking a 24-hr average the increasing afternoons and decreasing mornings cancel each other out, with the positive bias of the former offsetting the negative bias of the latter. While we understand that the 24-hr sampling is the federal standard, the point is that it is likely to have offsetting biases that make the result agree better for 24-hr than for shorter times -- which explains why the APCD results (all of which are 24 hr) agree better than the Scripps results (all of which are less than 24 hr). Third, other studies have shown a seasonal difference in whether BAM is biased high in summer and biased low in winter (Takahashi et al., 2008), so the comparison to APCD results for which the sampling dates are not specified may also cancel out high biases with low. This effect is likely due to the higher saturation pressures of water at higher temperatures resulting in higher amounts of water at the same relative humidity (see Figure B3). This may also play a role in the afternoon sampling, as the hotter part of the day was frequently in the afternoons.
		APCD says the Scripps "sampling methodology" causes particle losses that reduced the gravimetric masses, but state-of-the-art, peer-reviewed calculations of particle flow show that not to be the case and that if anything it would result in over-sampling (see Appendix B to Interim Report).

The District's BAM sampling was and is conducted in full accord with the BAM FEM designation and all federal quality control and assurance requirements of 40 CFR 58. For the gravimetric sampling summarized in Table 1, the District used Partisol FRM samplers to collect the filters,12 and gravimetry was performed according to either FRM methods (SCAQMD, BAAQMD, and DRI labs) or NIOSH Method 500/600 (FAL). For PM2.5, a VSCC was used as the size separator.	These are standard methods, used around the world for collecting regulatory data. The PM10 BAM method carries a weight of evidence of more than 20 years of legally defensible data collection.	As stated in the report, BAM is an FEM method. The statement about legally defensible implies a limited and specific context that is not specified, so there is no appropriate response.
Based on the description provided in the Scripps report, the gravimetric analysis of their samples seems to have followed the FRM method or a procedure very close to it.	The key difference between the methods employed by Scripps and the District is Scripps's use of non-standard sample collection devices.	The Scripps results were not designed as FRM, but to answer the scientific question of what sources contribute to the afternoon high-PM10 concentrations. Designed specifically for this purpose within the time and budget constraints afforded by Parks, the measurements were optimized to address the stated objectives. While all sampling devices have some particle losses and some limitations, the most accurate measurements are those where the design is suited to the question posed. As described in the Appendix B in the Interim Report, the design was optimized to provide an upper bound on the mineral dust contribution to PM10 BAM. While the comparison of BAM and Gravimetric is scientifically interesting, the differences in volatility between BAM and Gravimetric do not change the mineral dust contribution to BAM nor do they imply a problem in Scripps sampling.
In 2020, Scripps used an SCC size separator operated at 7.5 L/min to collect their PM2.5 samples. The District and SAG were critical of this setup, with the District noting that the SCC was not part of any EPA-approved FRM or FEM PM2.5 method. We are not aware of studies in the academic literature using it for PM2.5 sampling. In our critique of Scripps's 2019 sampling, we wrote: "The District suspects that Scripps's method is under sampling particulates from the ambient air, particularly when winds are high, and that this effect is much more important than evaporative loss in explaining why the gravimetric masses are consistently lower than the BAM masses."	We suggested that Scripps use a VSCC size separator in future PM2.5 sampling.	APCD is correct that they noted the possibility of them providing a VSCC sampler that allowed Scripps sampling by VSCC without unnecessary expenditures. Scripps used this opportunity to evaluate the difference between SCC and VSCC (albeit in a different year than 2020).
This year, Scripps employed a louvered PM10 sampling head for PM10 sampling and a VSCC separator (preceded by a PM10 head) for PM2.5 sampling. These are the same inlet configurations used by the District with our BAMs and gravimetric samplers. However, as shown in Figure 1, below, Scripps's PM2.5 setup also incorporates what appears to be a Swagelok T to split the flow after the VSCC. While the flow through the VSCC was 16.7 L/min and a regulatory PM2.5 sample was likely exiting the bottom of it, an unspecified portion of the sample flow was then diverted, and less than 16.7 L/min of flow was directed to the filter.	The District believes the use of a non-engineered—and likely non-laminar and non-isokinetic—flow splitter likely caused a non-representative sample to be collected by the filter.	APCD is incorrect in assuming that the flow was not engineered (designed) and not laminar. The split was non-isokinetic, by design, in order to enhance the sampling of larger particles. As described in the Appendix B of the report, there are standard corrections that allow one to estimate the magnitude of each of these effects (as well as that of gravity and bends). We used these calculations in designing the system to over-sample PM10 and VSCC measurements by direct downward sampling. SCC was a supplemental measurement and so included two bends, although calculations showed the effects were less than 2%, far too small to explain the discrepancies in sampled masses or to affect the measured mineral dust contributions.
Scripps employed a similar setup for their PM10 sampling (Figure 2). Here, 16.7 L/min enters the inlet and flows through the PM10 size separator and is then split using a T, with an unspecified portion of the flow continuing to the PM10 sample filter. The remainder of the flow is diverted to another T which further splits the flow into bypass and PM2.5 SCC sample streams.	This setup thus collected both a PM10 sample and an SCC PM10 sample from the same 16.7 L/min sample inlet stream.	As above, APCD has failed to note the intentional, bespoke nature of the flow configuration that was designed to provide an upper bound for PM10 and PM2.5 VSCC sampling (with SCC as a secondary measurement for comparison located after some minor losses due to bends). The flow configuration and splitting were specified in the sampling plan provided, and was not repeated here. Given the limited resources and sample duration range and targets, flow splitting was required. This is standard practice. In addition, we refer to the Appendix B in the report (attached), which shows the model results from the plumbing used for this sampling.
Had the Scripps's devices simply directed the entire 16.7 L/min flow through the size separators and directly to the sample filters, then their setup would have been analogous to the sampling streams of the District's BAMs and FRM samplers. Unfortunately, splitting the sample flow as done by Scripps likely renders the samples invalid. In the absence of laminar, isokinetic flow splitting, the sample stream impacting the filters cannot be assumed to be representative. It is well known that subisokinetic sampling of particulates will result in under-sampling of sample mass, and that is likely what is occurring here. Turbulence introduced by the T fitting may also cause particles to be deposited on the sides of the downtube, rather than traveling down to the filter.	The report provides no data and cites no references to demonstrate that these novel devices perform as they assume.	As above, detailed modeling of the well-known effects of the laminar flow configuration used here is now provided in the Appendix B in the report (attached). While APCD is correct in noting that these effects are well known, they fail to mention that (1) for the PM10 and PM2.5 VSCC configuration used, there are gains rather than losses of particles expected and (2) that for PM2.5 both gains (for VSCC) and losses (for SCC) are negligible (1-2%). Further, given a standard calculation of the Reynolds number for all flows, all meet the criteria of laminar (Re<2300), making claims of unmeasured turbulence effects incorrect.
In most EPA-approved particulate samplers—including the BAM 1020 and the Partisol samplers used by the District—the sample flows straight down from the size separator to the filter, and no flow is diverted. There are some EPA-approved FEM samplers that do split the sample flow.	In these instruments, 16.7 L/min flows through the inlet and size separators before a portion of that flow is drawn off and bypasses the detector. For these instruments—and in contrast to Scripps's devices—the flow splitter is an engineered component of the sample path, designed to maintain isokinetic, laminar flow. For example and as shown in Figure 3, below, the TEOM 1405 (EPA Method EQPM-1090-079) incorporates an isokinetic tube-within-a-tube flow splitter to reduce sample flow to its detector to 3 L/min.14 The T640x (EPA Method EQPM-0516-239) incorporates a similar tube-within-a-tube flow splitter to reduce the flow from 16.7 L/min to 5 L/min for the optical chamber of the instrument.15	APCD is correct that flow splitters with a less than 90-degree bends have fewer, but non-zero, losses in the bent flow. The downward flow, which is the relevant one for PM10 and PM2.5 VSCC, remains the same geometrically. While a 30-degree bend would have meant lower losses for the PM2.5 SCC flow, the negligible size of the losses from the two 90-degree bends (combined <1%) meant that such custom-machined splitters were not needed. The tube-within-tube design is one we have used, but for this application was not available at the time of the study. As discussed in the report, had such losses been dominant, they would have been consistent in all samples. Yet there is no such consistent bias in the results.
In summary, the District believes that Scripps's use of non-engineered, non-isokinetic flow splitters has resulted in the collection of non-representative PM10 and PM2.5 samples, which are likely biased low.	This likely also explains the poor correlation, apparent in Figure 2 of the report, between Scripps's VSCC and SCC PM2.5 samples.	Rather than speculation, we provide actual calculations that show that the effect of flow configuration is negligible for PM2.5 and in the opposite direction needed to explain the low bias of the PM10 gravimetric measurements, leading us to reject the APCD hypothesis that flow splitting explains the difference between BAM and gravimetric.
		APCD says Scripps speciation data should be interpreted cautiously, but it has not yet released any speciated data that support a substantially different result.
Given the problems with sample collection noted above, the District is hesitant to interpret Scripps's speciation results.	Without representative sampling of the ambient air, it cannot be assumed that the speciation results are representative of particulates impacting the sampling site.	Scripps notes that APCD recognizes that all of their criticisms are directed at the BAM-gravimetric comparison, which was not the objective of the study. They have not released any data showing that our samples were not representative for the sizes and times studied, yet to avoid the implications of the results they urge a generalized and quite speculative caution that is without merit.
Regarding the speciation results, the District notes the following unusual findings: □ The fraction of mineral dust reported in the PM2.5 samples is higher than the fraction in the PM10 samples. Typically, crustal materials are more enriched in PM10 versus PM2.5. □ For PM2.5, mineral dust is enriched on high PM10 days, as expected for wind-blown dust, but for PM10, the fraction mineral dust is the same on high versus low PM10 days.	It is the opinion of the District that these results are artifacts resulting from non-representative sampling caused by the flow splitters.	There are two significant errors in this comment: (1) They state that the fraction of mineral dust being higher in PM2.5 than PM10 is not typical, and yet just this circumstance has been reported and commented on for CDF, as noted in APCD correspondence (Craig, 2011). So while it may generally be atypical at dust locations worldwide, it is perfectly expected and well-known and expected by SLOAPCD for CDF. Moreover, this comment may imply to some that the amount of PM2.5 is higher than that of PM10, which would be impossible; it is relevant to note that this comment addresses the fraction not the concentration of mneral dust, and were concentrations to be compared then the PM10 concentration substantially exceeds the PM2.5 concentration by a factor of 2 or more on average for all cases. (2) This aspect would be interesting if there were a larger and statistically significant number of high-PM10 days. However, the low number of sampling days precludes such conclusions.
Finally, high PM10 at CDF is correlated with strong winds from the direction of the ODSVRA.16 It is well known that dust is generated by saltation when strong winds blow across sand dunes, and this has been documented in numerous studies conducted at the Oceano Dunes. It is therefore reasonable to expect that mineral dust from this ODSVRA makes up a large fraction of the PM10 impacting CDF on wind event days, and this consistent with the results of the Phase 1 and other studies. In contrast, here Scripps concludes that mineral dust and sea salt constitute only 14% and 4%, respectively, of the PM10 measured on high PM10 days, with the balance composed of water, organic materials, inorganic aerosols, and other semi-volatiles.	The report offers no hypothesis as to why these other species would be correlated with high onshore winds at this site.	Here APCD starts with an undisputed fact and then extrapolates to speculations that lack foundation. Scripps has never disputed that PM10 is high when wind is high; in fact, we have built our analysis of "high-PM10 days" on that since those are typically high-wind days. However, they seem to be assuming that dust must be a majority to cause a correlation, which is a fallacy. The correlation is caused by the amount that varies, which needs to be measurable (10%, i.e. not a "large" fraction) but does not need to be a majority, especially if the other sources do not vary. Thus, even if dust is not the majority, it can cause a correlation, so this argument is misleading. This is one aspect of the frequently quoted aphorism "Correlation is not causation." Moreover, APCD cites Phase 1 (and unnamed and uncited "other studies") as if it provides evidence of mineral dust providing a large fraction of PM10, but Phase 1 actually provided not a single measurement of mineral dust fraction of PM10. "In contrast, the Scripps study provides those measurements for May 2021, and the results are contrary to what the APCD had assumed in the absence of their own testing for mineral dust content in PM10 on the Mesa.
With regard specifically to water, the District notes that at CDF humidity and BAM PM10 are negatively correlated (r = -0.52), as shown in Figure 4, below.		First, the BAM concentration is controlled both by particle sources and humidity, so humidity alone is not expected to explain BAM concentration. Second, as described in detail by Takahashi et al. (2008), it is the bias between BAM and gravimetric (not the BAM itself) that is expected to be explained by relative humidity on high temperature (>16C) days. For other days, specific (absolute) humidity is a better metric as it represents the amount of water in air rather than the amount relative to saturation. In any case, the plot shown by the APCD is not relevant.
Similarly, the spatial pattern of PM10 concentration on wind event days is consistent with the disturbed area of the ODSVRA being the source of particulates, with concentrations at CDF being the highest, followed by Mesa2, and with much lower levels at the Oso Flaco monitoring site. If the source of particulates on high wind days was sea spray or an offshore source, much more homogenous impacts on these sites would be expected.		First, our analysis does not claim that dunes are not a source of PM10 at CDF and nearby sites. We do claim it is not the only source, and in fact typically not the majority source of PM10 at CDF. Second, the report does not attribute the majority of PM10 to sea spray or offshore sources, making this comment irrelevant.

Page 2: "However, the lack of difference between weekday and weekend coarse particle emissions supports natural rather than anthropogenic sources [Li et al., 2013]" This statement fails to consider how vehicular activity causes elevated PM10 levels downwind of the ODSVRA. If tailpipe emissions or "rooster tails" kicked up by active off-roading caused the degraded air quality, then a day-of-week effect would be expected. But these are not major contributors to the issue. As the District has noted elsewhere, "it is the secondary effects to vegetation and dune shapes that lead to greater wind erosion and more dust when the wind blows."7,17 The SAG has noted that "decades of OHV activity have fundamentally altered the natural beach-dune landscape, making the dunes significantly more susceptible to PM emissions than they would be in a natural state."18 Most recently, a DRI study commissioned by State Parks found that the emissivity of bare sand within ODSVRA steadily decreased while the ODSVRA was closed to riding due in 2020 to the COVID-19 pandemic.19	Most recently, a DRI study commissioned by State Parks found that the emissivity of bare sand within ODSVRA steadily decreased while the ODSVRA was closed to riding due in 2020 to the COVID-19 pandemic.19	The statement by Scripps is a correct representation of the cited peer-reviewed publication. The references cited by APCD do not include any peer-reviewed publications. In addition, none of them show a direct measurement of effects on PM10 at CDF (or Mesa2).
Page 3: "the California 24-hr PM10 standard of 50 µg m-3 is exceeded 25% of the time [Motallebi et al., 2003]." This sentence could imply that the situation downwind of the ODSVRA is typical, which it is not. The cited source is almost 20 years old and reviews data from more than 20 years ago.	The cited source is almost 20 years old and reviews data from more than 20 years ago.	The statement is a correct representation of the cited peer-reviewed publication. APCD has not provided a more recent reference.
Page 3: "Since the association of PM2.5 with toxics is likely responsible for the association of PM2.5 with health effects, the use of PM2.5 as a health indicator assumes it co-occurs with toxics."	In the context of this report, this statement may imply that PM2.5 is driving the District's regulation of the ODSVRA, which is not the case.	The statement does not say what APCD says it says.
Page 3: "These standards were developed based on measurements completed by federal reference methods (FRM) … Since then, BAM has been approved as a federal equivalent method (FEM) … Those test locations typically include concentrations below 100 µg m-3 and frequently below 30 µg m-3 [Chung et al., 2001; Gobeli et al., 2008; Hafkenscheid and Vonk, 2014; Hart, 2009], as these conditions were more typical of areas of concern for PM2.5." In the context of this paragraph, "these standards" seems to include the federal and California PM10 standards, but all the studies cited to support this statement are of PM2.5. Some readers may understand this paragraph to mean that the BAM was given its PM10 FEM designation without being tested at the high concentrations observed here at the CDF monitoring station. This impression would be incorrect. The instrument was designated a PM10 FEM in 1998,20 and at the time the EPA testing requirements of 40 CFR 53 were that at least 3 of the 10 trial days had concentrations above 80 µg/m3.21	Some readers may understand this paragraph to mean that the BAM was given its PM10 FEM designation without being tested at the high concentrations observed here at the CDF monitoring station. This impression would be incorrect. The instrument was designated a PM10 FEM in 1998,20 and at the time the EPA testing requirements of 40 CFR 53 were that at least 3 of the 10 trial days had concentrations above 80 µg/m3.21	The statement does not say what APCD says it says. The information provided by APCD does not contradict the information provided by the report.
Page 5: "…seven one-hr measurements reported for PST start times of 1100 through 1800 to provide comparison points..." (Emphasis added.)	This is likely a typo, and "1800" should be replaced by "1700". Scripps collected 7-hour samples, so if the sampling period began at 11:00 and ended at 18:00, then the start time of the final hourly BAM measurement would be 17:00.	APCD is correct, this is a typo.
Page 5: "At high relative humidity (>70%, such as those at CDF in May 2021, see Appendix, Figure A3), hourly measurements will report higher mass concentrations than multi-hour filter measurements [Schweizer et al., 2016]. Comparisons at other sites between gravimetric and BAM PM2.5 mass concentrations have shown correlation coefficients (R2) that varied between 0.65 and 0.99 and slopes that differed by as much as 30% depending on season and chemical composition [Hauck et al. 2004]" (Emphasis added.)	These statements are not supported by the cited references. The Schweizer study compared BAM and EBAM measurements, not BAM and filter samples, as stated in the report. Furthermore, it is well known that the EBAM over-reports due to insufficient sample drying; the EBAM is not an EPA-approved FEM, and it was not used to collect any of the data discussed in the Scripps report or this review. The Hauck study was of an older, non-FEM BAM, so it is a stretch to assume that its results apply to the modern, FEM-designated BAM used by the District.	Both the results are accurately described. The Schweizer et al. (2016) study looks at effects of hourly and daily measurements, and here the filter refers to the filter in the BAM, but we have deleted the word for clarity. The Hauck et al. (2004) results described are accurate and the year of publication makes it clear that they could not have used a 2008 model in a 2004 publication.
Page 6: "XRF analysis provided trace metal concentrations for elements heavier than Na. Atmospheric ambient sea-salt concentrations were calculated using measured Cl- and 1.47*Na+ concentrations…" There seem be to two typos in this statement.	The first sentence states that elements heavier than Na were measured (but not Na itself), but the next sentence mentions measurements of Na. The second sentence should likely say "Cl" and "Na" instead of referencing ions, since the ions were not measured directly (according to the Methods section).	Since the ions and elements have the same mass, the distinction between ions and elements is unnecessary. Na is included, which is clear from the citation, but the word "and" has been added for clarity.
Page 7: "The offline gravimetric method is lower on average than the online BAM instrument for most samples at CDF for both VSCC and SCC cyclones (Figure 1)." This appears to be a typo, as this sentence is in the PM10 section, and there is no VSCC or SCC data in Figure 1.		APCD is correct, this is a typo.
Page 10: "Corrections for BAM to gravimetric have been developed for some regions in order to use BAM to determine if air quality standards are exceeded [Le et al., 2020]." This statement could imply that the District could or should apply a correction to our BAM data; however, even if we believed a correction was warranted, we are not allowed to apply one under CARB and EPA regulations.	If this statement is retained, it should be note that the "regions" where this may be happening are outside of the U.S.	The statement does not say what APCD says it says. It is correct as written, and is true whether or not APCD is allowed to apply corrections.
Page 11: "Another possibility is that the BAM calibration does not apply well to the composition and concentration conditions that are relevant to this site. EPA approval of BAM relied on testing conditions that were typically limited to concentrations lower than 100 µg m-3 and that were 24-hr average measurements [Chung et al., 2001; Gobeli et al., 2008; Hafkenscheid and Vonk, 2014; Hart, 2009]. At PM10 concentrations exceeding 30 µg m-3, BAM and gravimetric methods were not found to be equivalent using consistency criteria [Gebicki and Szymanska, 2012]."	As already noted for the similar statement on page 3, the Chung, Gobeli, Hajkensheid, and Hart papers are specifically about PM2.5, not PM10. The Gebicki and Szymanska paper is about a non-FEM BAM, not the BAM 1020, so does not apply.	The statements are correct as written, and the citations to references makes clear the relevance to PM2.5 and PM10 appropriately. However, it is also worth noting that the BAM technology is the same for both, so the results of PM2.5 are still relevant.
Page 13: "While prior results did not report the mineral dust fraction of BAM or gravimetric PM10 [SLOAPCD, 2007], the reported mineral dust (crustal) fraction of gravimetric PM2.5 reported by the San Luis Obispo Air Pollution Control District for its Nipomo Mesa Particulate Study (Phase 1) for the Mesa2 annual 24-hr average was 20% [SLOAPCD, 2007]. This value is similar to the 7-hr afternoon average in May 2021 for above detection samples reported here (23% of gravimetric) …"	While factually accurate, this statement is misleading because it compares an annual average to a short-term average covering a portion of the windy season. The contribution of mineral dust to ambient PM2.5 is expected to be highest on wind event days, and those occur most frequently in April and May, i.e., the time of year when Scripps collected their samples. In the late fall and winter, mineral dust is not expected to contribute much to PM2.5 mass, and this is indeed what the Phase 2 Study found. So, while the Phase 1 Study found that mineral dust contributed only 20% to annual PM2.5 average at Mesa2, it is likely that it was a much greater fraction of the PM2.5 mass during April and May. Furthermore, of the 7 PM2.5 samples from Mesa2 that were fully speciated in the Phase 1 Study (Figure 17), only one sample, May 9, 2014, is from a day when PM10 exceeded 50 µg/m3 (as determined by cross-referencing the sample dates in Figure 17 with the PM10 data in Figure 7). For that sample, mineral dust comprised more than 60% of the PM2.5 mass.	As noted by APCD, the statement is accurate. Readers are provided with the citation to investigate more details. The differences in sampling conditions (and years) are explicitly noted.
The conclusions of the Phase 1 Study note that "The study results clearly identify wind blown crustal particles as the single largest cause of the high particulate concentrations measured on the Mesa … Elemental analysis of the PM2.5 samples further confirm that on these high particulate days, the largest fraction of particles are composed of the crustal elements of silicon, iron, aluminum, and calcium."		This statement is not relevant, nor do the results of that sudy support the statement listed.
Page 14, "The association of high PM10 and PM2.5 with high wind conditions, even when recreational vehicles were limited at Oceano Dunes compared to prior years, indicates that dune-derived mineral dust is more likely to be primarily caused by natural forces (i.e. wind) rather than human activities."	Like the statement on page 2 noted above, this statement fails to consider how vehicular activity causes elevated PM10 levels downwind of the ODSVRA. While high winds are natural forces, the surface of the dunes has been unnaturally disturbed by the long history of vehicular activity. Thus, more dust is generated when high winds blow across the ODSVRA then would be from undisturbed dunes.	As noted above, there is a lack of peer-reviewed publications supporting the APCD assertions, and the data presented to date do not show impacts on PM10 at CDF.
Page 14: "There is no evidence of mineral dust contributing all or even the majority of BAM PM10, as has apparently been assumed in past reporting [SLOAPCD, 2007]."	This implies that the District once assumed that all PM10 was mineral dust, which is not the case.	Page 40 from the Phase 1 report (SLOAPCD, 2007) states "While there was no elemental analysis performed on the PM10 samples, it is generally accepted in the scientific community that the coarse PM10 fraction is composed mostly of crustal elements." This assertion, without citation, is clearly an assumption by APCD (at the time), making the report statement accurate. In addition, the SLOAPCD web site (https://www.slocleanair.org/air-quality/south-county/more-info, accessed 10/30/21) states "Several studies performed by the APCD in the Nipomo Mesa area have shown the source of the elevated particulate matter (PM) pollution to be windblown dust from the open sand areas of the Oceano Dunes State Vehicular Recreation Area (SVRA)." The syntax here that "the source" (not "a source" or "one source") is dust from the ODSVRA shows a clear assertion that dust is the primary if not only source of PM10.
Page 24, "The SCC method has demonstrated size cut sharpness of 1.25 [Cauda et al., 2014]. The VSCC method has a reported sharpness of 1.16 under clean conditions [Kenny and Thorpe, 2000]…" (Emphasis added.)	"Demonstrated" versus "reported" implies that the sharpness parameter of SCC is more proven or accepted than that of the VSCC.	The wording reflects the information available in the references cited.

Page 25, "The low bias of SCC relative to VSCC could only be explained by the larger sharpness value of 1.25 compared to 1.16 if there are higher mass concentrations just below 2.5 µm than above the 2.5 µm …"	Another explanation is the novel sampling apparatuses noted above, namely the use of nonengineered, non-isokinetic sample flow splitters resulting in non-representative sample deposition on the filters. In addition to that issue, we note that for Scripps's SCC sampling, particles must travel in a zig-zag to reach the filter (Figure 2), while for the VSCC sampling the sample path is straight down (Figure 1). Because of the straighter path and the effect of gravity, more sample likely reaches the VSCC filter than the SCC.	As described in the Appendix to the report provided with these responses, we provide the detailed calculations showing the losses for PM2.5. While the VSCC may be enhanced by 2% and the SCC decreased by 1%, the cumulative 3% difference is not sufficient to explain the observed difference in the absence of a difference in cutoff sharpness.
Page 26, Figure A3. The Scripps samples were collected for 7 hours, but the green box spans 8 hours of data.		While the green box was intended more as a guide to the eye than a representation of the exact timing, we have corrected the box boundaries to only include 7 start-hour times.