Public Comments about the Draft Ozone Integrated Science Assessment

My name is Jennifer Richmond-Bryant. I am an Associate Professor of the Practice in the Department of Forestry and Environmental Resources at North Carolina State University. Between August 3, 2008 and August 9, 2019, I worked for the National Center for Environmental Assessment at the U.S. EPA. I was the principal author of the exposure appendix on the Integrated Science Assessment for Ozone. I am speaking now as a concerned member of the public.

My comments have three parts. First, I will express my concerns regarding Agency efforts to curtail the scientific evidence through false narratives about study quality and transparency. Second, I will discuss concerns about using a narrow interpretation of causality, which is inconsistent with the Agency's historical process of making causality determinations. Third, I will address points raised by the Chair about use of exposure surrogates in epidemiology studies in his comments on the Draft Particulate Matter ISA. His comments during that review are relevant for this one. Many of my concerns are based on identified conflicts of this review with the text of the Clean Air Act

First, regarding the ISA's treatment of the scientific evidence, §108(a)(2) of the Clean Air Act states, "Air quality criteria for an air pollutant shall accurately reflect the latest scientific knowledge useful in indicating the kind and extent of all identifiable effects on public health or welfare which may be expected from the presence of such pollutant in the ambient air, in varying quantities." In *Whitman v. American Trucking Association*, Justice Scalia wrote in the majority opinion, "The EPA, "based on" the information about health effects contained in the technical "criteria" documents compiled under §108(a)(2), 42 U. S. C. §7408(a)(2), is to identify the maximum airborne concentration of a pollutant that the public health can tolerate, decrease the concentration to provide an "adequate" margin of safety, and set the standard at that level. Nowhere are the costs of achieving such a standard made part of that initial calculation." In other words, the court ruled that the NAAQS must be based solely on an accurate portrayal of the science.

In Dr. Cox's comments on the Draft ISA for Particulate Matter, he stated, "We previously proposed the following screening criteria for excluding individual studies from further consideration when their design or analysis precludes valid causal interpretation of their results." Among the criteria are:

- "Study does not distinguish between true exposure values and estimated exposure values in analyzing and presenting information.
- "Study design, data collected, or analyses performed do not permit threats to internal validity to be tested and refuted.
- "Study design, data collected, or analyses performed do not allow external validity to be established and correct generalizations to target populations to be made.
- "Study design or data only address association and do not permit valid inferences about (manipulative) causation."

Dr. Cox is well aware that adopting these criteria creates a risk of removing the vast majority of epidemiology studies, because most use some surrogate for exposure, the definitions of internal and external validity are unclear, and most epidemiology studies are not designed to make determinations about manipulative causation. This will be done in the name of "study quality" with the true goal of discarding valuable scientific evidence that would help the Agency to identify the effects of ozone exposure on public health. Any doubt about that erodes when one reviews Dr. Cox's statement that these are (emphasis added) "***screening criteria for excluding individual studies***." Following the April CASAC letter, EPA staff were directed to embark upon this type of study quality evaluation. This step

has the potential to contravene the mandate of §108(a)(2) to base the ozone criteria on the body of scientific evidence.

Much has already been said in the news about the proposed "Strengthening Transparency in Regulatory Science" Rule, otherwise known as "Secret Science". It is well known that health studies cannot make data publicly available without risking disclosure of study participants. Publication of those datasets would also likely violate IRB terms for those studies. The Agency Leadership knows this. If the Administration prohibits the use of health studies based on this proposed rule, they will purposely discard important studies upon which the NAAQS have been historically based. This is in clear conflict with §108(a)(2) of the Clean Air Act.

Second, regarding the ISA's process of making causality determinations, I would argue that the CASAC Chair's insistence on a narrow test of "manipulative or interventional causation" goes against the Clean Air Act. The EPA has for decades relied upon principles of consistency, coherence, and biological plausibility outlined by Sir Bradford Hill. In his questions to the consultants, Dr. Cox declares, "I emphasize manipulative and interventional causation (rather than predictive (Granger) causation, but-for causation, epidemiological (attributive) causation, mechanistic causation, etc.) because it is most relevant for policy makers." This statement makes it sound like epidemiological studies alone are used to judge causality. Coherence means that the Agency relies on multiple lines of evidence for causality judgments. The strongest evidence for ozone health effects has come from the controlled human exposure literature, and the epidemiological literature base has been used to provide additional support. Moreover, §109(b)(1) of the Clean Air Act calls for creation of NAAQS that are "requisite to protect the public health" with "an adequate margin of safety". This suggests that we are not limited by evidence where a model unequivocally shows an effect with no ambiguity whatsoever. That is why the Agency has adopted a weight-of-evidence approach. When multiple studies, including high-quality studies, from multiple disciplines mostly point towards the same conclusion, the Agency can feel confident in its causal determinations. The standard is intended to be protective of public health.

Third, regarding exposure measurement error, the majority of studies cited in the Draft Ozone ISA showed that exposure measurement error generally led to underestimation of the association between exposure to ozone and a health effect in both short-term and long-term studies. Dr. Cox has in the past pointed to several papers that examine how exposure measurement error tends to "linearize" a concentration-response model that is non-linear (particularly a "hockey stick" shaped threshold model). The result is that the model tends to be biased away from the null in the region below the threshold and biased towards the null above the threshold. These papers were not included in the exposure appendix. Most were published well before the beginning of the search window (January 1, 2011). One (Cox, 2018) was published several months after the cut-off date (March 30, 2018). Review articles, such as Rhomberg et al. (2011), are outside the scope of the ISA and so are not considered. A small number of the epidemiological studies cited in the draft Ozone ISA suggested a threshold below 40 ppb, which is around the 35th percentile 8-hour daily max concentration during summer nationwide. Most other studies do not cite a threshold at all. Even if exposure measurement error leads to bias away from the null within that potential threshold region, it would still lead to underestimation of effect in the majority of cases. Therefore, the threshold argument is overstated and attention to evidence of an effect that may be even larger than estimated across a range of exposures experienced by most people in the country is still warranted.

Thank you for your time and attention.

References

Brauer, M., Brumm, J., Vedal, S., and Petkau, A. J. (2002). Exposure misclassification and threshold concentrations in time series analyses of air pollution health effects. Risk Analysis, 22(6):1183-1193.
Simulated C-R for different assumptions about thresholds to show the bias that results from not including the threshold in the model. The threshold was masked due to bias in the exposure.

Cox, L. A. (2018). Effects of exposure estimation errors on estimated exposure-response relations for PM2.5. Environmental Research, 164:636-646.
Assumed a threshold model instead of published linear C-R model and then discussed biases associated with the published models if the threshold were true.

Lipfert, F.W., Wyzga, R.E. (1996). The effects of exposure error on environmental epidemiology. In RF Phalen; RC Mannix; MC Tonini (Eds.), The second colloquium on particulate air pollution & human mortality & morbidity (pp. 4-295 - 4-302). Sacramento, CA: California Air Resources Board.
The ISA does not cite papers that aren't peer-reviewed.

Rhomberg, L. R., Chandalia, J. K., Long, C. M., and Goodman, J. E. (2011). Measurement error in environmental epidemiology and the shape of exposure-response curves. Critical Reviews in Toxicology, 41(8):651-671.
Found that C-R curves were often biased towards the null by classical error with the exception of the threshold hockey stick model, which was biased away from the null in the region of the threshold. Exposure error tended to linearize nonlinear models.

Watt, M., Godden, D., Cherrie, J., and Seaton, A. (1995). Individual exposure to particulate air pollution and its relevance to thresholds for health effects: a study of traffic wardens. Occupational and Environmental Medicine, 52(12):790-792.
Estimated a hockey-stick shaped C-R model when personal exposure measurements are made, compared with a population model using data from a central site monitor, which produced a more linear C-R curve.

Yoshimura I. (1990). The effect of measurement error on the dose-response curve. Environmental Health Perspectives, 87, 173-8.
Found that D-R curves with exposure error are linearized.