

Additional Criticism of EPA-452/P-19-001 September 2019 Policy Assessment for the Review of the National Ambient Air Quality Standards for Particulate Matter, External Review Draft

James E. Enstrom, PhD, MPH, FFACE
UCLA and Scientific Integrity Institute
<http://www.scientificintegrityinstitute.org/>
<http://scientificintegrityinstitute.org/biography.html>
jenstrom@ucla.edu

December 11, 2019

This Comment adds additional criticism of the 2018 Draft EPA PM ISA (PM ISA) and the 2019 Draft EPA PM PA (PM PA) to the criticism contained in my detailed October 17, 2019 Comment ([https://yosemite.epa.gov/sab/sabproduct.nsf/F729E7D8E248A2C5852584970009565A/\\$File/Enstrom+Comment+to+CASAC+re+090519+EPA+PM+PA+101719.pdf](https://yosemite.epa.gov/sab/sabproduct.nsf/F729E7D8E248A2C5852584970009565A/$File/Enstrom+Comment+to+CASAC+re+090519+EPA+PM+PA+101719.pdf)) or (<http://scientificintegrityinstitute.org/JEEPMPA102219.pdf>). My October 17 Comment presented strong evidence that 1) there is NO causal relationship between PM_{2.5} and total mortality in the US, 2) the PM PA cites 'positive authors' and omits 'null authors' and their criticism, 3) the PM PA does not address the PM_{2.5} deaths controversy, 4) my analyses of underlying data for four key US cohorts, including H6CS and ACS CPS II, support the need for the proposed EPA Transparency Rule, and 5) the PM PA must be revised to incorporate the CASAC Review and the criticisms by me and others.

My criticism of the PM PA is now supported by the 297-page November 13, 2019 Draft CASAC Review of the PM PA, which contains this summary statement: "Overall, the CASAC finds that the Draft PM PA depends on a Draft Particulate Matter (PM) Integrated Science Assessment (ISA) that, as noted in the April 11, 2019, CASAC Report on the Draft PM ISA, does not provide a sufficiently comprehensive, systematic assessment of the available science relevant to understanding the health impacts of exposure to PM, due largely to lack of a comprehensive, systematic review of relevant scientific literature; inadequate evidence and rationale for altered causal determinations; and a need for clearer discussion of causality and causal biological mechanisms and pathways. Given these limitations in the underlying science basis for policy recommendations, and diverse opinions about what quantitative uncertainty analysis and further analysis of all relevant data using the best available scientific methods would show, most CASAC members conclude that the Draft PM PA does not establish that new scientific evidence and data reasonably call into question the public health protection afforded by the current 2012 PM_{2.5} annual standard." (<https://yosemite.epa.gov/sab/sabproduct.nsf/ea5d9a9b55cc319285256cbd005a472e/0a46bdbe59c86531852584b10077b0f6!OpenDocument>) or (<https://junkscience.com/2019/11/winning-epa-science-advisers-reject-epa-staff-particulate-matter-claims/>).

On November 18, 2019 I sent an email message to those October 22, 2019 public speakers who have criticized and/or do not support the 2019 EPA CASAC Reviews of the PM ISA and PM PA. I asked these speakers to send me their assessment of my criticism of the PM ISA and PM PA or indicate a willingness

to discuss my criticism (<http://www.scientificintegrityinstitute.org/CASACCritics111819.pdf>). Not one of the speakers who received my message has responded to my offer. This nonresponse indicates that it is virtually impossible to have a dialog with PM2.5 investigators who do not agree with me scientifically. No one wants to discuss any of the issues in my October 17 Comment, particularly the fact that my independent reanalysis of CPS II data has revealed severe flaws in the primary study underlying the PM2.5 NAAQS. Five individuals and two organizations who received my November 18 email message made comments to EPA CASAC on December 3 and none of them addressed the points in my October 17 Comment.

Therefore I am so appreciative of the excellent work of the current CASAC, which has produced a Draft Review that identifies serious flaws in the PM ISA and PM PA, consistent with the findings in my October 17 Comment. Rather than specific comments about the details the CASAC Draft Review, I describe three clear examples of serious corruption in the assessment of PM2.5 health effects that are not known to CASAC and that are highly relevant to the PM2.5 NAAQS. These examples illustrate the five types of bias that have led to exaggerated PM2.5 health effects: investigator bias, journal editor bias, journal reviewer bias, EPA funding bias, and EPA assessment staff bias. Given these serious biases and flaws, an entire reassessment of the PM2.5 NAAQS is justified.

1) 2018 Draft EPA PM ISA and 2019 Draft EPA PM PA Violate EPA Principles of Scientific Integrity

On June 12, 2019 I submitted a formal complaint to EPA Scientific Integrity Official (SIO) Francesca T. Grifo, PhD, against Assessment Lead Jason D. Sacks, MPH, regarding the 2018 Draft EPA PM ISA (<http://www.scientificintegrityinstitute.org/SOIGrifo061219.pdf>). I presented strong evidence that Mr. Sacks violated the basic rule for ethical behavior by EPA employees regarding “Interpreting and presenting results” as defined in the EPA Principles of Scientific Integrity. I stated that three sentences in Section 11.2.7 ‘Summary and Causality Determination’ are utterly false regarding US evidence: ‘Recent extended analyses and reanalysis of these cohorts continues to support this relationship, demonstrating consistent positive associations for total (nonaccidental mortality),’ ‘Overall, recent epidemiologic studies build upon and further reaffirm the conclusions of the 2009 PM ISA for total mortality,’ and ‘Collectively, this body of evidence is sufficient to conclude that a causal relationship exists between long-term PM2.5 exposure and total mortality.’

In his September 4, 2019 response to my June 12, 2019 complaint, Deputy SIO Vincent Congliano, PhD, did not address my specific evidence that Mr. Sacks violated the basic rule for ethical behavior by EPA employees regarding “Interpreting and presenting results.” Instead he stated “peer review by a federal advisory committee with the accompanying public comment satisfies the requirements of EPA’s Scientific Integrity Policy” (<http://www.scientificintegrityinstitute.org/SOIGrifo090419.pdf>). His response indicates that the EPA SIO does not require that EPA staff, specifically Assessment Lead Sacks, prepare “a comprehensive, systematic review of relevant scientific literature” in the PM ISA and the PM PA. Instead, the EPA SIO expects “peer review by a federal advisory committee [CASAC] with the accompanying public comment [like mine]” to force EPA staff to hopefully produce “a comprehensive, systematic review of relevant scientific literature.” In my opinion, current key EPA senior staff like Mr. Sacks have indeed violated the EPA Principles of Scientific Integrity regarding “Interpreting and presenting results” and should no longer be involved in preparing the PM ISA and the PM PA.

2) Deliberate Falsification of Research Record re Enstrom Reanalysis of ACS CPS II Cohort Data

Since 2016 I have possessed the original de-identified version of the underlying data for the 1,200,000 US subjects in the 1982 ACS Cancer Prevention Study (CPS II) cohort, which ACS followed for mortality from 1982 to 1988. The positive relationship between PM2.5 and total mortality in the CPS II cohort (Pope 1995) provided the primary epidemiologic evidence that was used to establish the 1997 PM2.5 NAAQS. The reanalysis presented in Enstrom 2017 (<https://doi.org/10.1177/1559325817693345>) and Enstrom 2018 (<https://doi.org/10.1177/1559325818769728>) provides unrefuted evidence that the positive relationship in Pope 1995 is not robust. Specifically, Table 3 of Enstrom 2018 shows substantial variation in the 1982-1988 relative risk (RR and 95% CI) of total mortality associated with PM2.5 defined in two different ways. For CPS II subjects residing in 47 US counties, RR = 1.081 (1.036-1.128) based on the 1979-1983 HEI PM2.5 values used in Pope 1995, but RR = 1.021 (0.984-1.058) based on the 1979-1983 IPN PM2.5 values used in Enstrom 2017 and Enstrom 2018. My reanalysis challenges the validity of the PM2.5 NAAQS and demonstrates the urgent need for the proposed EPA Transparency Rule.

Instead of acknowledging my reanalysis, the Pope 1995 authors and other PM2.5 investigators have simply refused to cite it and are now falsifying the research record regarding the 25-year PM2.5 deaths controversy. ACS Officials Gapstur and Brawley still have not acknowledged that I possess the CPS II data and they have refused to respond to my 2019 emails. In 2017 they implied that Enstrom 2017 was not based on CPS II data, but Enstrom 2018 proves conclusively that Enstrom 2017 is based on CPS II data. Pope has published three recent articles on the relationship between PM2.5 and total mortality using US National Health Interview Survey cohort data: April 1, 2018 *Air Quality, Atmosphere & Health* article (<https://doi.org/10.1007/s11869-017-0535-3>), July 24, 2019 *EHP* article (<https://doi.org/10.1289/EHP4438>), and November 21, 2019 *Environmental Health* article (<https://doi.org/10.1186/s12940-019-0544-9>). All three articles omit reference to Pope 1995, HEI 2000, HEI 2009, Enstrom 2017, and Enstrom 2018, thereby falsifying the research record regarding my peer reviewed evidence challenging the validity of Pope 1995 and related PM2.5 death claims.

The May 3, 2018 *PLoS Medicine* Editorial by Ioannidis “All science should inform policy and regulation” (<https://doi.org/10.1371/journal.pmed.1002576>) praises HEI 2000 with regard to Pope 1995: “importantly, detailed re-analysis of results and assessment of their robustness by entirely independent investigators.” Although Enstrom 2017 found that the Pope 1995 results were not robust, Ioannidis omitted reference to Enstrom 2017. The August 20, 2019 *PLoS Biology* Primer by Ioannidis “Air pollution as cause of mental disease: Appraisal of the evidence” (<https://doi.org/10.1371/journal.pbio.3000370>) omitted reference to Enstrom 2017 and Enstrom 2018 and stated regarding the Pope 1995 findings that the HEI 2000 “reanalyses reached mostly similar conclusions, although there are still some dissenters.”

The December 1, 2019 *AnnalsATS* Focused Review by Schraufnagel and Balmes “Health Benefits of Air Pollution Reduction” (<https://doi.org/10.1513/AnnalsATS.201907-538CME>) claims that total mortality can be reduced by reduction of PM2.5 but does not acknowledge the 2018 Intrepid Insight evidence in my October 17 Comment that there is NO *causal* relationship between PM2.5 and total mortality in nine US cohort studies. This review also omits reference to Enstrom 2017 and Enstrom 2018. Enstrom 2018 contains Figure 3 based on Jerrett 2007 (<http://scientificintegrityinstitute.org/jerrett051707.pdf>), which shows no reduction in PM2.5-related mortality relative risk from 1982 to 2000 in the CPS II cohort.

3) *Science* and AAAS Continue to Aggressively Oppose the Proposed EPA Transparency Rule

On November 26, 2019, Herbert Holden Thorp, Editor-in-Chief of the *Science* family of journals (<https://www.sciencemag.org/news/2019/08/aaas-names-chemist-holden-thorp-editor-chief-science>), along with the editors of *Nature*, *PLoS*, *PNAS*, *Cell Press*, and *Lancet*, issued a “Joint statement on EPA proposed rule and public availability of data (2019),” which was published as a Letter in December 6, 2019 *Science* (<https://doi.org/10.1126/science.aba3197>). Also, *Science* published a December 6 news item about this letter (<https://www.sciencemag.org/news/2019/11/major-journal-editors-blast-epa-s-secret-science-rule-again>). Two key sentences in the letter are “As leaders of peer-reviewed journals, we support open sharing of research data, but we also recognize the validity of scientific studies that, for confidentiality reasons, cannot indiscriminately share absolutely all data” and “We are also concerned about how the agency plans to consider options related to existing regulations.” These sentences are very deceptive because the proposed EPA Transparency Rule does not require that investigators whose research is used as the basis for EPA regulations to “indiscriminately share absolutely all data.” There just needs to be enough access to underlying data to independently assess the validity of implausible and widely disputed environmental claims, such as, the claim that PM2.5 causes premature deaths. Absolute confidentiality can be maintained if the investigators involved in the data sharing are ethical and the data involved are de-identified to the degree necessary to protect subject confidentiality.

In addition, on December 6, 2019, Marie Lynn Miranda, Professor of Statistics and Immediate Past Provost of Rice University, authored a lead *Science* Editorial “Getting the EPA back on track” (<https://doi.org/10.1126/science.aba3769>). This editorial contains inaccurate statements like “The EPA’s proposed transparency rule does not ensure research rigor or improve transparency” and “Work by the Health Effects Institute, in which an industry-government–funded partnership reanalyzed data from the Harvard Six Cities Study and the American Cancer Society Study on the link between particulate matter pollution and mortality, represents an excellent model for evaluating the validity of research pivotal to environmental health regulations without compromising confidentiality or excluding studies.” My independent reanalysis of ACS CPS II data, as described in Enstrom 2017 and Enstrom 2018, clearly demonstrates that data access does improve research rigor and transparency. Furthermore, my reanalysis reveals serious flaws in HEI 2000, the HEI Reanalysis of Pope 1995, and challenges the validity of the PM2.5 NAAQS. Either Miranda was and is not aware of my reanalysis or she deliberately ignored it. In any case, I have strong evidence supporting the value of the proposed EPA Transparency Rule. I have submitted a Letter to *Science* that addresses the inaccuracies in the Joint Statement and the Miranda Editorial (<http://scientificintegrityinstitute.org/Science121119.pdf>).

A major problem is the fact that *Science* and the other cited journals do not publish null findings that challenge the validity of existing EPA NAAQS and EPA regulations. For example, my 2017 reanalysis of the ACS CPS II data identified major flaws in the seminal Pope 1995 article, which provided the primary evidence used to establish the 1997 EPA PM2.5 NAAQS. The way *Science* dismissed my strong evidence that PM2.5 does not cause premature deaths is consistent with their repeated editorial opposition to data transparency and objective assessment of PM2.5 death claims (<http://www.scientificintegrityinstitute.org/CPSIIRej122716.pdf>).

On July 5, 2016 I submitted to *Science* for peer review my Manuscript No. aah4744 “Fine Particulate Matter and Mortality in Cancer Prevention Study Reanalysis.” The Abstract clearly stated the importance of the data access necessary for my reanalysis: “Background. The EPA National Ambient Air Quality Standard (NAAQS) was established in 1997 for fine particulate matter (PM2.5), largely because

of its positive relationship to mortality in the 1982 American Cancer Society (ACS) Cancer Prevention Study (CPS II) cohort. This implausible and contested relationship has been used to justify many costly EPA regulations, most recently the Clean Power Plan. This paper presents the first truly independent examination of the CPS II data. . . . Conclusions. No significant relationship between PM2.5 and total mortality in the CPS II cohort was found when the best available PM2.5 data was properly included. The 1995 analysis and 2000 reanalysis misrepresented and exaggerated this relationship by selective use of CPS II and PM2.5 data. These findings demonstrate the importance of independent analysis of underlying data and raise serious doubts about the epidemiologic evidence supporting the PM2.5 NAAQS.” On July 8, 2016 my manuscript was rejected after initial screening and NO in-depth review. This immediate rejection occurred in spite of the fact that my Reference 2 was the July 25, 1997 “Showdown over clean air science” article in *Science*, which described the PM2.5 deaths controversy. After I appealed the rejection, I was informed on July 11, 2016 that *Science* would not consider ANY resubmission of the manuscript. On July 13, 2016 I submitted to *Science Advances* for peer review the same manuscript, Manuscript No. ScienceAdvances-D-16-01615. On July 30, 2016 the manuscript was rejected after initial screening and NO in-depth review. The manuscript was eventually published on March 28, 2017 in *Dose-Response*, after being rejected by seven major journals. My independent reanalysis was possible because I was able to legally obtain an original version of the 1982-1988 CPS ACS II cohort data and documentation. Although, my reanalysis strongly challenges the validity of Pope 1995 and the epidemiologic justification for the 1997 PM2.5 NAAQS, it has been totally ignored by *Science*.

Science’s repeated opposition to PM2.5 data transparency is further reflected in its extensive collaboration with the Union of Concerned Scientists (UCS), which has NO relevant expertise in PM2.5 science. In Summer 2015 nine accomplished scientists, including myself, responded to the May 29, 2015 *Science* Perspective “Congress’s attacks on science-based rules” written by nine leaders of UCS and six others. *Science* immediately rejected without peer review three versions of our response to the UCS’s misrepresentations regarding the need transparency and reproducibility in order to properly justify EPA regulations. This entire saga is described in the December 15, 2015 National Association of Scholars Blog “Concerns about National Academy of Sciences and Scientific Dissent” by Peter Wood, which describes examples of *Science’s* suppression of minority views on the linear no threshold (LNT) hypothesis, man-made global warming (AGW), and PM2.5 deaths by then *Science* Editor-in-Chief Marcia McNutt (https://www.nas.org/blogs/dicta/nas_letter). The bottom line is that *Science* refuses to publish evidence that supports the need for transparency and reproducibility in the research findings used to justify EPA regulations.

4) *BMJ* Rejects Enstrom CPS II Reanalysis and Deletes Enstrom Review of Harvard PM2.5 Paper

On January 25, 2019 I was asked by Dr. Sophie Cook, *BMJ* UK Research Editor, to review Manuscript BMJ-2018-048424 by Yan Wang and Yaguang Wei, et al. “Air pollution and cause specific risks and costs of hospital admissions.” In a series of thirteen more emails with Dr. Cook and Dr. Elisabeth Loder, *BMJ* Head of Research, up to April 1, 2019, I made several attempts to upload my nine-page January 24, 2019 Review in both PDF and Word format, but I was unsuccessful. My review recommended “Unequivocal Rejection of this Manuscript” and provided extensive details to back up my recommendation. On April 18, 2019, Dr. Loder made the surprising statement “Recently, I asked you to review Manuscript ID BMJ-2018-048424, entitled “Air pollution and cause-specific risks and costs of hospital admissions.” It has since become apparent that I will not need you to review at this time.” On May 6, 2019 I replied in part

"I want to make clear that my review is to remain in your review system and is to be used as part of the final decision on this manuscript." (<http://scientificintegrityinstitute.org/BMJ050619.pdf>). The *BMJ* editors had no further communication with me and I was never able to confirm that my review was actually entered into the *BMJ* Manuscript website." Then on November 27, 2019 the Research Article "Short term exposure to fine particulate matter and hospital admission risks and costs in the Medicare population: time stratified, case crossover study" by Yaguang Wei and Yan Wang, et al. was published as a 13-page *BMJ* Research Paper (*BMJ* 2019;367:l6258 <http://dx.doi.org/10.1136/bmj.l6258>).

BMJ "Peer Review" (<https://www.bmj.com/content/367/bmj.l6258/peer-review>) states "For research papers *The BMJ* has fully open peer review. This means that accepted research papers submitted from September 2014 onwards usually have their prepublication history posted alongside them on thebmj.com." The April 18, 2019 "First decision" by Dr. Loder stated "We sent it for external peer review and discussed it at our manuscript committee meeting. We hope very much that you will be willing and able to revise your paper as explained below in the report from the manuscript meeting. We are looking forward to reading the revised version and reaching a final decision." (https://www.bmj.com/sites/default/files/attachments/bmj-article/pre-pub-history/first_decision_18.4.19_1.pdf). This nine-page document includes reviews by Reviewer 1 "Dr Suzanne Bartington, Clinical Research Fellow, University of Birmingham"; Reviewer 2 "Ka Hung Chan, Research Fellow, University of Oxford"; Reviewer 3 "Cesaroni Giulia, Senior Researcher, Epidemiology Dept. of Lazio Regional Health Service, ASL RM1". There is NO mention of my January 24, 2019 Review. The August 26, 2019 "Second Decision" by Dr. Loder cites the above three reviewers, but makes NO mention of me (https://www.bmj.com/sites/default/files/attachments/bmj-article/pre-pub-history/second_decision_26.8.19_0.pdf). This Research Paper was finally accepted for publication on October 16, 2019 based on the reviews of three Europeans. In summary, the research was conducted at the Harvard TH Chan School of Public Health using several US funding sources and the large US Medicare data base and the lead authors are two Chinese doctoral students, Yaguang Wei and Yan Wang.

Compare the *BMJ* editorial treatment of this Harvard publication with the treatment that I received regarding my manuscript on the only independent reanalysis of the ACS CPS II cohort data identifying major flaws in Pope 1995 (<http://www.scientificintegrityinstitute.org/CPSIIRej122716.pdf>). On August 11, 2016 *BMJ* accepted for review my Manuscript ID BMJ.2016.035002 "Fine Particulate Matter and Mortality in Cancer Prevention Study Cohort Reanalysis." On August 14, 2016 Dr. Elisabeth Loder, *BMJ* Acting Head of Research, emailed me a rejection that read in part "Thank you for sending us your paper. We read it with interest but I am sorry to say that we do not think it is right for the *BMJ*. In comparison with the many other papers we have to consider, this one is a lower priority for us." On August 22, 2016 *BMJ Open* accepted for review my Manuscript ID bmjopen-2016-013986, which was identical to my *BMJ* manuscript. On September 2, 2016 *BMJ Open* Assistant Editor Emma Gray emailed me a rejection that read in part "I am writing to you in regard to manuscript # bmjopen-2016-013986, "Fine Particulate Matter and Mortality in Cancer Prevention Study Cohort Reanalysis", which you submitted to *BMJ Open*. Your manuscript has been evaluated and has been declined for publication in *BMJ Open*."

Thus, within less than a month, my manuscript showing no relationship between PM2.5 and total mortality was rejected without peer review by both *BMJ* and *BMJ Open*. However, after receiving my strongly negative January 24, 2019 peer review, *BMJ* spent ten months working with three reviewers with no experience in US PM2.5 epidemiology. On November 27, 2019 *BMJ* published a manuscript showing a positive relationship between PM2.5 and hospital admissions. The published "Peer Review" does not even acknowledge that existence of my review. This *BMJ* review process provides direct recent evidence of strong editorial bias by a major medical journal on highly controversial PM2.5 health effects.