

JONATHAN BORAK & COMPANY, INC.

Specialists in Occupational & Environmental Health

April 17, 2006

Stephen L. Johnson, Administrator
USEPA Headquarters
Ariel Rios Building
1200 Pennsylvania Avenue NW
Mail Code: 1101A
Washington, DC 20460

William Wehrum, Acting Assistant Administrator
for Air and Radiation Matters
USEPA Headquarters
Ariel Rios Building
1200 Pennsylvania Avenue NW
Mail Code: 6101A
Washington, DC 20460

***Re: Proposed Coarse PM NAAQS
(Federal Register 71:2620-2708)***

Dear Administrator Johnson and Acting Administrator Wehrum:

I have prepared the following comments at the request of the National Cattlemen's Beef Association and the National Mining Association in order to share my concerns about the scientific basis for the proposed NAAQS for coarse particulate matter (PM_{10-2.5}) that was recently published in the *Federal Register* (*Fed Reg* 71:2620, 01/17/06).

To introduce myself, I have attached a short summary of my experience and qualifications in medicine, epidemiology, toxicology and occupational health science. I am Associate Clinical Professor of Medicine and Epidemiology & Public Health at Yale University. I teach required graduate courses in both Toxicology and Risk Assessment. I also served for 10 years as a founding member of EPA's National Advisory Committee on Acute Exposure Guideline Levels for Hazardous Substances (NAC/AEGL).

This is the third set of comments that I have submitted on this issue. Twice during the past year, in May and August, I submitted comments to the Clean Air Scientific Advisory Committee. Then as now, my principal concern is the general lack of scientific support for a coarse particulate matter standard and the failure of EPA to appropriately address that deficiency.

Those limitations were acknowledged in various versions of the Staff Paper (e.g., EPA-452/R-05-005, June 2005):

“a growing, but still limited, body of evidence on health effects associated with thoracic coarse particles from studies that use $PM_{10-2.5}$ as a measure of thoracic coarse particles.” (Final Staff Paper: 5-47)

That concern is restated word-for-word in the Proposed Coarse PM NAAQS:

“In developing this rationale, EPA has taken into account the information available from a growing, but still limited body of evidence on health effects associated with thoracic coarse particles from studies that use $PM_{10-2.5}$ as a measure of thoracic coarse particles.” (*Fed Reg* 71:2653)

The Preamble also acknowledges that similar concerns were expressed by the CASAC, which noted:

“... significant uncertainties resulting from the limited number of studies to date in which $PM_{10-2.5}$ has been measured and the potentially large exposure measurement errors in such studies”. (*Fed Reg* 71:2671)

As discussed below, the actual evidence available to support the Proposed Coarse PM NAAQS is substantially more limited than is acknowledged by EPA. The relative insufficiency of evidence linking coarse particulates to human health effects is repeatedly acknowledged in the Proposed Coarse PM NAAQS. For example, consider Section III.A, which describes the Evidence of Health Effects Related to Thoracic Coarse Particle Exposure. In that very important, but relatively short section (it comprises only 10 pages of the Preamble), EPA reiterates 24 times that the evidence linking coarse particulate to health effects is either “limited” or “very limited”. By contrast, EPA does not once describe any of the evidence as “sufficient” or “adequate”.

Despite that apparent recognition of these limitations, however, EPA presents the actual data from cited studies in a manner that overstates their informational value. More worrisome is the possibility that the Preamble has been constructed in a manner intended to obscure the deficiencies and to minimize objections that might be raised about the lack of scientific justification for the Proposed Coarse PM NAAQS.

1). For example, the Preamble discusses the most important limitations of evidence in a brief section (*Fed Reg* 71:2671-2), distanced from the primary presentation of the cited studies and their data, and relegated mainly to discussion of an “alternative

interpretation” that is strikingly devoid of specific details. Thus, notwithstanding repeated statements about data limitations, many readers will fail to appreciate the actual magnitude of the deficiency of scientific evidence.

a) My concerns are illustrated by the following example. The centerpiece of the presentation of the Evidence of Health Effects is Figure 2 (page 2656), which summarizes the “Effect estimates for associations between short term exposure to PM_{10-2.5} and mortality or morbidity health outcomes ...” The legend to Figure 2 states:

“for consistency across studies, effect estimates are from single-pollutant, general linear models”.

The decision that only data from single-pollutant models would be presented in this centerpiece graphic is at least curious because the compiled literature provides good evidence that single-pollutant models overestimate the effects of coarse particulate. The possibility that such an approach should not be taken, however, is only discussed in the context of the “alternative interpretation”:

“... an alternative interpretation of the available health evidence presented in the Criteria Document and the Staff Paper ... suggests that that the results from one-pollutant PM_{10-2.5} models are confounded by fine particles and gaseous co-pollutants... Taken as a whole, evidence from PM_{10-2.5} epidemiologic studies could be interpreted to suggest that one-pollutant PM_{10-2.5} models suffer from bias due to omitting co-pollutants in the statistical model...” (*Fed Reg* 71: 2671-2)

But even that statement does not reasonably represent the scientific evidence. It implies that this is solely a matter of “interpretation”, as though reflective of a philosophical debate. In fact, numerous studies cited in the Proposed Coarse PM NAAQS provide evidence that single-pollutant models overstate the apparent risks of coarse particulate. I am aware of none that proposes the contrary.

For example, consider the Toronto study by Burnett et al. (1), which is cited eight times in the Preamble. That study found that positive associations noted in a single-pollutant model “disappeared after adjustment for O₃, NO₂, and SO₂” (1). But Figure 2 presents only the results of a single-pollutant model from that study, thus wrongly indicating a statistically significant effect of coarse particulate ^[1]. Or, consider that use of two-

¹ More surprising is footnote #52 (p. 2657), which disclaims the analytical findings of the multiple-pollutant model in Burnett et al. The footnote states that their results “show relatively consistent effects estimates ... except for the models including NO₂ and all four gaseous pollutants”. This footnote implies a preference for relying on an incomplete analysis of a complex dataset. Of greater concern is that EPA has apparently discounted the authors’ finding that the association “could be completely explained by NO₂, a risk factor not as widely considered in North American locales as the other criteria pollutants” (1). Similar findings and cautionary advice are found in Thurston et al. (12); significant associations with coarse particulates “were merely a statistical by-product of inter-pollutant confounding ... This points out the importance of considering as many pollutants as possible in such analyses, in order to diminish the chances of being misled...”

pollutant models including both PM_{2.5} and PM_{10-2.5} reduced or eliminated the estimated effects of coarse particulate in the Six Cities study (2) and in studies of Detroit (3,4), Los Angeles (5) and eight Canadian cities (6) ^[2].

I am not alone in pointing to the need to consider multiple-pollutant models in order to correctly understand the effects of coarse particulate. That approach was stressed by the Research Committee of the Health Effects Institute in comments on the Detroit study conducted by Lippmann et al. (3):

“In order to determine the relative effects of several risk factors on a health outcome, ideally all variables under considerations would be included in a single model.”

In the apparent pursuit of “consistency”, EPA has selectively presented the least rigorous of the available evidence, thereby minimizing its informational value. Even for those studies which provided results from dual- and/or multiple-pollutant models, EPA has emphasized single-pollutant analyses while discounting the data from more rigorous multi-pollutant analyses. In so doing, EPA has systematically overstated the apparent effects of coarse particulate.

Had EPA correctly acknowledged that the results of single-pollutant models generally overestimate the effects of coarse particulate and that most of the cited studies provided only results of such models, then the even more limited nature of evidence here would have been readily appreciated.

2). Following is another example of the failure to describe and respond to limitations of the evidence presented as justification of the Proposed Coarse PM NAAQS.

In the Final PM Staff Paper (EPA-452/R-05-005, June 2005), EPA Staff described a criterion for deciding whether studies effects data were sufficiently precise to be used in quantitative estimates of exposure-response relationships. In so doing, Staff correctly recognized that some studies are better than others (because of their size or presumably for other reasons) and that studies of lesser quality should not be relied upon as one might rely on studies of higher quality. In particular, the Staff Paper described a “rough indicator of ... precision” that was used for this purpose:

“The natural logarithm of the mortality-days (a product of each city’s daily mortality rate and the number of days for which PM data were available) can be

The literature cited by EPA is dominated by studies with analytical models that failed to consider other pollutants and risk factors. Thus the conclusions of Burnett et al. and Thurston et al. give added reasons to view the EPA evidence with caution. I do not agree with the Preamble statement that “effect estimates for associations between PM, including PM_{10-2.5}, and health endpoints are generally robust to confounding by gaseous co-pollutants” (*Fed Reg* 71:2660).

² These studies are all cited in the Preamble. Results of the Los Angeles and the eight Canadian cities studies were excluded from Figure 2 because they used GAM, rather than GLM analytical models.

used as a rough indicator of the degree of precision of effect estimates ... staff chose to consider only those urban areas in which studies with relatively greater precision were conducted, specifically including studies that have a natural log of mortality-days greater than or equal to 9.0 (i.e., approximately 8,000 deaths) for total non-accidental mortality.” (SP, p. 4-6)

That approach (both specifically and generally) has been deleted from the Proposed Coarse PM NAAQS. As a result, EPA has deleted its Staff’s criterion for objectively distinguishing between individual studies. It is interesting to note that if EPA had accepted this criterion, then it would have had to acknowledge that results from the Coachella Valley studies (7-9) and the Six Cities study results from Steubenville (10,11) had been judged to be of “lesser quality”.

But the Preamble relies on those two studies repeatedly: The Coachella Valley studies are cited 19 times, while the Steubenville data are cited eight times. At no point does the Preamble indicate that EPA Staff had objectively determined that both data were too imprecise to be used for quantitative assessments and thus their conclusion should be viewed with caution. I am concerned that the failure to indicate those Staff determinations serves mainly to conceal the limitations of those studies ^[3].

3). A third example of the failure to describe and respond to limitations of the evidence relates to the adequacy of the exposure assessments that underlie each of the individual studies. Concerns about the precision and accuracy exposure assessment can not be separated from concerns about the precision and accuracy of the studies themselves.

a) One aspect of my concern involves the spatial location(s) of monitors used to describe the exposures of study populations. It is generally accepted that coarse PM (e.g., PM_{10-2.5}) deposits more rapidly and more locally than does fine particulate. Likewise, it is generally accepted that local sources are of greater importance in determining concentrations of coarse particulate (6). Accordingly, it can be expected that measurements from centrally located monitors will less accurately represent regional exposures to coarse particulate than fine particulate (i.e., PM_{2.5}). For that reason, measurements of coarse PM obtained at relatively distant monitoring stations should be viewed with caution, and so should studies that rely on coarse PM measurements obtained relatively far from target populations. When such distant measures are used as the basis for epidemiological studies, efforts should be made to demonstrate that the distant measures do accurately reflect the exposures of target populations.

For example, in an analysis and comments submitted separately, Gale Hoffnagle describes marked spatial variation of fugitive coarse PM emitted by ground level sources such as those characteristic of agricultural and mining activities. His analysis indicates that even when levels at such sources reach several hundred mg/m³, corresponding levels

³ It is also notable, and perhaps related, that despite a statement in footnote 50 (*Fed Reg* 71:2655) that two subsequent reanalyses of the Steubenville data found essentially no significant associations, the Preamble persists in referring to the original Steubenville data as showing “a statistically significant mortality association”.

at a distance of 1000 meters are *de minimis* (i.e., they approach zero mg/m³). Thus PM monitors located at a distance of 1000 meters or more reflect little or no contribution from such sources.

However, a number of the studies cited in the Preamble depended on coarse PM measurements from distant monitors and were apparently not accurate predictors of target population exposures. In the Detroit study by Lippmann et al. (3,4), particulate matter data were obtained from ambient monitors in Windsor, Ontario, several miles from central Detroit. The Staff Paper and the Proposed Coarse PM NAAQS document that levels measured in Windsor were not representative of those in Detroit:

“In recent years, based on available Windsor and Detroit data from 1999 to 2003, the Windsor monitors used in this study typically have recorded PM_{10-2.5} levels that are generally less than half the levels recorded at urban-center Detroit monitors...” (Staff Paper: 5-68)

Accordingly, on the basis of that exposure concern, the Detroit study must be regarded as providing only limited informative value.

In the Coachella Valley studies (8,9), particulate measures were obtained in Indio, approximately 25 miles from older population centered in the Palm Springs area at the western end of the Valley⁴.

b) A second exposure assessment concern is the manner in which coarse particulate levels are determined. The Preamble notes that PM_{10-2.5} measurements are prone to greater exposure errors than are measurements of PM_{2.5} (*Fed Reg* 71:2660). In addition, PM_{10-2.5} levels calculated by the difference method (i.e., subtracting PM_{2.5} from PM₁₀) can be expected to have larger errors than PM_{10-2.5} levels directly measured using dichotomous samplers; the difference method is impacted by two measurement errors, while the direct measurement method has only one. And when the difference method is performed using data from monitors that are not physically co-located, additional exposure assessment errors result because of non-homogeneous spatial distributions of particulate matter.

Finally, estimation of coarse particulate exposures derived from only PM₁₀ measurement in areas where measured PM levels are “dominated” by coarse particulate are by far the most uncertain and least accurate. Because of such uncertainty, the findings of epidemiological studies that rely on those exposure assessments should be viewed as the

⁴ The population of the Indio area, which is on the Northern rim of the Valley, differs from that of many of the other Valley communities. For example, according to the 2000 Census, 15.2% of the population was in the 45-64 year age group and 9.1% were over 65 years. By contrast, the corresponding proportions were 26.4% and 26.2% for Palm Springs, 30% and 43% for Ranch Mirage and 26.3% and 27.6% for Palm Desert. Those cities, with significantly older populations more prone to cardiorespiratory diseases, are located approximately 10-25 miles away toward the Western end of the Valley.

least informative, analogous to ecological studies that are suitable for generating, but not testing hypotheses.

Consider the effect of categorizing the studies cited in the Preamble on the basis of their exposure assessments.

a) The highest quality PM_{10-2.5} exposure assessments are those in studies that employed dichotomous samplers.

Dichotomous particulate samplers were used in two Toronto studies (1,12) that considered hospital admissions and two reports from the Harvard Six Cities study, one considering mortality effects (10,11) and other peak flow and asthma in children (2). All four of those studies found no significant effects associated with exposure to PM_{10-2.5}.

b) Second tier studies calculated PM_{10-2.5} by the difference method, subtracting PM_{2.5} from levels of PM₁₀. Among the co-location studies cited in the Proposed Coarse PM NAAQS, most suffered important data limitations or deficiencies.

No association of respiratory symptoms and childhood asthma were found for coarse particulate calculated by the difference method in Uniontown and State College (2). There were only marginal associations ($0.05 < p < 0.10$) between coarse particulate calculated by the difference method and mortality in the Phoenix study (13,14). The Detroit study found small positive associations for coarse particulate but as discussed above, particulate data were obtained miles from the study population and were significantly inaccurate (3,4). The HEI Health Review Committee concluded that data from the Detroit study were inconclusive:

“...the data do not clearly support a greater effect of one pollutant over another, nor do they establish which pollutants are most likely to cause adverse health effects...” (3) (HEI Synopsis)

The Coachella Valley study (8,9) reported positive associations with mortality and the Seattle study (15,16) reported positive associations with hospital admissions for asthma in non-elderly patients. But both studies suffered from large data gaps that were filled by imputation and arbitrary calculations; in both studies, exposure data were missing for 75% or more of the PM_{2.5} values and, therefore, they were also missing for coarse particulate exposure measures ^[5].

⁵ In the Coachella Valley study, PM₁₀ data were available for a 10-year period, but PM_{2.5} data for only 2.5 years. The missing PM_{2.5} and PM_{10-2.5} were imputed using a predictive function that estimated PM_{10-2.5} as a cubic function of PM₁₀. The predictive function was such a poor fit for PM_{2.5} data that the authors concluded that “predictive models could not be successfully estimated” (9). Accordingly, the calculated values, which represented 75% of the PM_{10-2.5} data, can not be viewed as reliable.

The extent of missing data in the Seattle study is no less extreme. The authors observed: “Numerous missing PM measurements potentially limit our analysis” (15). For the three monitoring

c) The lowest tier studies measured only PM₁₀ in areas thought to be dominated by coarse particulate and thereby inferred associations with coarse particulate. The lowest tier studies included “positive” studies in Anchorage (17,18,18), Reno (19), Tucson (20), and the Coachella Valley (7). Each also suffered from additional methodological concerns.

In Anchorage (17,18), the health effects were measured in terms of outpatient visits, not episodes of illness, and included events likely to be primarily infectious (e.g., “sore throat, ear aches”). Repeated visits by the same individual (e.g., emergency visits and follow-up office visits) would result in temporal dependence among outcomes that would effectively underestimate variance and overestimate the significance of associations, perhaps leading to inappropriate rejection of the null hypothesis of no effect of particulate exposures. In addition, outcome measures were not associated with the highest levels of exposure, only with lesser exposure levels.

The Reno study (19) provided no evidence that PM₁₀ was dominated by coarse particulate. However, two facts suggest that PM₁₀ was dominated by fine particulate, not coarse particulate. First, PM₁₀ levels were inversely related to wind speeds, suggesting that those levels reflected not wind-blown crustal particulate, but decreased dispersion of suspended fine particulate. Also, the authors noted that:

“Higher peaks occurred during the winter season. This may be as a result of increased residential combustion due to cold weather in the study area.”
(19)

If the authors are correct, then those peaks would have represented fine, not coarse particulate. Accordingly, the relevance of the Reno study to coarse particulate exposures is uncertain at best.

The Tucson study, which evaluated cardiovascular hospitalizations, used data from a monitoring station that was “located in a neighborhood site likely to be representative of population exposure”, rather than at a site that would have been “subject to windblown dust” (20). Therefore, it is likely that the Tucson PM₁₀ exposure data derived from samples that were actually dominated by fine

stations considered, no PM_{2.5} data were available for 72-100% of days. The authors “imputed” the missing data. The imputation methods were not described, but the authors indicate that six different imputation methods were used and the results of those six methods were averaged. In addition, the “exposure” data were then “weighted” to favor residential areas, but no justification for that arbitrary weighting scheme was provided. Thus, the “exposure data” in this study were mainly synthetic, rather than empirical, and had been transformed in ways that can not be understood and have not been justified. It is difficult to regard this as a valid observational study.

particulate. Unfortunately, there were no PM_{2.5} data available to validate the underlying assumptions of this exposure assessment.

The Coachella study (7) utilized PM₁₀ data from monitors located in Indio, 25 miles from the major population center around Palm Springs, where on average PM₁₀ levels were 21% greater than in Indio. Given the higher population and vehicular density in Palm Springs, it seems likely that the higher levels reported during the study period in Palm Springs reflected mainly fine combustion particulate, rather than windblown crust.

By means of such a categorization scheme, it can be seen that most of the evidence in support of the Proposed Coarse PM NAAQS is derived from studies with the lowest quality of exposure assessments, while those with highest quality exposure assessments lend no support.

If EPA had used such a ‘quality of exposure assessment’ approach to prioritize the evidence available ^[6], it would have been apparent that support for the Proposed Coarse PM NAAQS is mainly found in the least robust studies. Thus, such an approach would have further emphasized the limitations of supporting evidence.

4). In summary, EPA has systematically presented the results of cited studies in a manner that overstates the evidence linking coarse particulate and health effects.

Data from inferior single-pollutant models have been presented in the centerpiece graphic of the Preamble, while more rigorous analytical results have been relegated to afterthoughts and footnotes.

Studies that EPA Staff deemed to be of inferior quality have been presented as supportive without appropriate qualification.

No apparent effort has been made to distinguish high-quality from lesser-quality studies with respect to the adequacy of their exposure assessments.

One might infer that failure to distinguish between strong and weak studies is motivated by the wish to avoid the exclusion of those positive findings that derive mainly from weaker studies.

5) The Preamble also misleads by its repeated statements that effects associated with coarse PM exposure were not affected by confounding by gaseous co-pollutants:

“effect estimates ...are generally robust to confounding by gaseous co-pollutants”
(*Fed Reg* 71:2660);

⁶ EPA has utilized such an approach for other risk assessments, such as in evaluating evidence of the carcinogenicity of trichloroethylene (e.g., “We divided the cohort studies into three tiers based on the specificity of the exposure information” (25)).

“associations ... were largely unchanged in most cases when gaseous co-pollutants were included in the models” (*Fed Reg* 71:2657);

“effect estimates ... are largely unchanged with the addition of gaseous co-pollutants to the models” (*Fed Reg* 71:2657).

But whether confounding is demonstrated depends on whether the correct co-pollutants have been included in the analytical model. Burnett et al. (1), for example, emphasized this concern: the apparent effects of particulates “could be completely explained by NO₂, a risk factor not as widely considered in North American locales as the other criteria pollutants.” Similar conclusions were reached by Thurston et al. (12). Accordingly, the appropriate concern is not whether the effects of particulates are “generally robust”, but whether potentially significant confounding has been properly evaluated. Such evaluations should consider “as many pollutants as possible” (12).

EPA has apparently not performed such evaluations. Instead, the Preamble relies on studies that incompletely evaluated possible confounding as evidence that such confounding is insignificant. However, the evidence provided by more rigorous studies indicates that confounding by gaseous co-pollutants can not be disregarded.

6) The limitations of the underlying evidence and the failure of EPA to adequately address and respond to those limitations are illustrated in the manner in which the Preamble argues that PM_{10-2.5} is significantly associated with asthma. As described below, that argument is composed of hypothetical propositions and incorrect descriptions of cited studies.

a) The Preamble first proposes that because PM_{10-2.5} might deposit in the tracheobronchial region, therefore it has the potential to aggravate asthma at the levels of exposure considered in the NAAQS. Following are examples of that proposition:

“Deposition of particles to the tracheobronchial region is of particular concern with respect to aggravation of asthma” (*Fed Reg* 71:2654);

“...has the potential to affect lung function and aggravate symptoms, particularly in asthmatics” (*Fed Reg* 71:2655);

“The fractional deposition of elevated coarse particle concentrations is significant in the tracheobronchial region, which is particularly sensitive in asthmatic individuals.” (*Fed Reg* 71:2661);

“... the expectation that deposition of thoracic coarse particles in the respiratory system could aggravate effects in individuals with asthma” (*Fed Reg* 71:2668).

The hypothesis (or expectation) that PM_{10-2.5} might aggravate asthma is not necessarily wrongheaded, but its repeated assertion provides neither support nor evidence that such a “potential” effect actually occurs.

b) The Preamble sometimes treats asthma (or “aggravation of asthma”) as a distinct disease process, but more generally treats it as merely one of a number of more-or-less generic respiratory diseases.

The following statements, for example, suggest that EPA regards “asthma” as a distinct entity:

“Evidence available in the last review suggested that aggravation of asthma ...”
(*Fed Reg* 71:2656);

“...limited epidemiologic evidence suggesting that aggravation of asthma...” (*Fed Reg* 71:2668);

“The authors conclude that for acute asthma related responses...” (*Fed Reg* 71:2657).

In most places, however, the Preamble does not differentiate between asthma and a variety of acute respiratory diseases (e.g., respiratory infections, pneumonia) and chronic respiratory diseases (e.g., COPD). This is reflected by the following statements:

“... respiratory morbidity effects, such as aggravation of asthma, increases in respiratory symptoms and respiratory infections...” (*Fed Reg* 71:2655);

“...associations between short-term exposure to PM_{10-2.5} with hospital admissions for respiratory diseases, including asthma, pneumonia and COPD...” (*Fed Reg* 71:2657);

“...respiratory morbidity, such as increased respiratory symptoms and hospitalization for respiratory diseases such as asthma or COPD...” (*Fed Reg* 71:2661).

I suspect that the failure to distinguish asthma from those other acute and chronic diseases mainly reflects the paucity of published data specifically linking PM_{10-2.5} and asthma.

c) The striking paucity of evidence linking PM_{10-2.5} and asthma is made clear by the very few studies cited to support that association. Moreover, most of the cited studies provide less support than is implied in the Preamble.

The Preamble cites two studies^[7], Hefflin et al. from southeast Washington and Gordian et al. from Anchorage, in which:

⁷ The Preamble actually cites “the last review” - 62 FR 38679 - in which these two studies are specifically identified.

“...aggravation of asthma and respiratory infections and symptoms were associated with PM₁₀ in areas where thoracic coarse particulate were a much greater fraction of PM₁₀ than were fine particles” (*Fed Reg* 71:2657).

But, contrary to statements in the Preamble, Hefflin et al. (21) found no association between high-level exposure to PM₁₀ and aggravation of asthma, even at 24-hour PM₁₀ levels of 1035-1689 µg/m³. To the contrary, those authors report:

“... it is surprising that we not only found no significant association between PM₁₀ and asthma, but we found relatively few emergency room admissions for asthma in a community that would be expected to have 4800 persons with asthma.” (21)

The Gordian studies (17) suffer from potentially important flaws that limit its informational value. As discussed above, health effects were measured as doctors' visits, not episodes of illness, which may have led to overestimating the significance of associations. In addition, associations were noted for asthma and upper respiratory infections (URI) with a temporal relationship suggesting that onset of URI preceded the onset of asthma attacks^[8]. Also, visits were not increased during the peak exposure days, when PM₁₀ levels averaged 565 µg/m³ and peak levels exceeded 3000 µg/m³.

Notably, URI itself has been associated with asthma attacks in asthmatic children (22) and lower airway effects in normal children (23). Because of the apparent cause-and-effect relationship between URI and asthma attacks generally and the apparent correlation between URI and asthma visits in the Gordian study, it is not possible to determine whether those asthma visits reflected PM₁₀ exposure vs. URI. In short, it is not possible to determine the extent to which the Gordian data might reflect the adverse effects of PM₁₀.

The Preamble also states that three “new US and Canadian epidemiologic studies” reported associations between short-term exposures to PM_{10-2.5} with hospital admissions for “respiratory diseases, including asthma” (*Fed Reg* 71:2657). The three cited studies are from Toronto, Detroit and Seattle. However, the Preamble statement is incorrect and misleading.

The Toronto study by Burnett et al. (1) did not find such an association. A multi-pollutant analysis found that any apparent association “was eliminated”, with a relative risk of 1.007. It would be improper (and, perhaps, absurd) if EPA regarded relative risks of 1.007 as indicative of meaningful associations.

⁸ Doctors' visits for URI were most closely associated with same-day PM₁₀ levels, while asthma visits were most closely associated with prior day PM₁₀ levels. That suggests that URI preceded asthmatic symptoms. My personal experience (as an internist, emergency physician, parent and patient) is that visits for acute asthmatic attacks are more likely to occur shortly after the onset of symptoms, whereas URI visits occur after a longer delay, when symptoms and signs seem unusually persistent or severe. Thus, it seems likely that the onset of URI actually preceded asthma by several days.

The cited Detroit study (16) was not a “new study”, but a reanalysis of data from the older Lippmann et al. study (3). As discussed above, the Detroit study relied on particulate levels measured miles away in Windsor, Ontario and the HEI Health Review Committee concluded that data from the Detroit study were inconclusive.

Likewise, the Seattle study (16) was not a “new study”, but a reanalysis of Sheppard et al (15). As discussed above, exposure data were so lacking that 75% of coarse particulate data were “imputed”. Also, the authors noted that wood burning was a “major contributor to PM”, that vehicular exhaust was the second largest source of PM, and that the pollutant most closely associated with asthma was carbon monoxide, “an important environmental indicator of incomplete combustion, particularly from mobile sources” (15).

Given the lack of measured coarse particulate data and the evidence that combustion-related fine particulate was an important pollutant in Seattle, there is essentially no basis to conclude that coarse PM in Seattle caused asthma-related hospitalizations.

The Preamble also mischaracterizes the findings of Schwartz and Neas (2) with respect to asthma. The Preamble states:

“The authors conclude that for acute asthma related responses as well as daily mortality, fine particles are a stronger predictor of health response than are thoracic coarse particles.” (*Fed Reg* 71:2657)

That statement implies that in addition to the large association seen with fine particles, Schwartz and Neas also found an association between coarse particles and asthma. That is not correct, as reflected by the authors’ actual statements:

“For lower respiratory symptoms, the association was stronger for all of the fine-particle measures than for CM [coarse particle mass] in single pollutant regressions. A model including both CM and PM_{2.5} resulted in a substantial reduction in the effect of CM, with little evidence that the remaining effect was different from zero.” (4)

EPA has incorrectly presented these negative findings as though Schwartz and Neas provided support for the Proposed Coarse PM NAAQS.

It is surprising to realize that the above studies reflect the totality of epidemiological data cited in the Preamble as support for the proposition that PM_{10-2.5} aggravates asthma. These studies provide no such support, either individually or as a group.

d) There are other relevant studies that have been ignored in the Preamble discussion of asthma, perhaps because their findings showed no association of coarse particulate and asthma. Consider, for example, the three-year study by Rabinovitch et al. (24) that

specifically considered the effects of wintertime air pollutants on urban minority children at “highest risk for asthma morbidity”. The children were students at a special school, operated at the National Jewish Hospital in Denver, which specifically enrolled children with chronic diseases including asthma. The school was located in a community where PM₁₀ is dominated by coarse particulate; during the study period, coarse particulate on average comprised 61.2% of PM₁₀.

For two years, exposure data (including PM₁₀ and PM_{2.5}) were obtained from EPA monitors located 100 meters from the school. During the third year, particulate data were obtained from a community monitoring station located 2.8 miles from the school. Children were monitored for asthma symptoms, asthma exacerbations, twice-daily FEV₁ and peak flows, use of asthma medications, and URI events. School activities were not modified in response to pollution alerts “so as not to bias any potential pollution effects”.

Associations between air pollutants and asthma outcomes were found in simple models, but not in complex modeling that included all pollutants and time-dependent covariates such as URI events. Using the more complex model, no significant associations were observed between pulmonary function and PM₁₀. Asthma symptoms were significantly associated with ozone levels, but not PM₁₀ and no significant associations were noted between asthma exacerbations and PM₁₀. By contrast, URI symptoms were strongly associated with decreased pulmonary function, increased medication usage, asthma symptoms, and asthma exacerbations.

These findings suggest that exposure to coarse particulate does not provoke asthma symptoms, does not adversely impact pulmonary function and does not induce asthma attacks. The strong associations seen between URI, pulmonary function and asthma lend support to the view that the results of the Gordian studies reflect URI events, rather than coarse particulate exposures.

e) In summary, it should be clear from the very few, very limited, and uncertain studies cited in the Preamble that there is no sound basis for concluding that coarse particulates aggravate asthma or provokes asthma symptoms, even at exposure levels considerably higher than those considered in the Proposed Coarse PM NAAQS. EPA arguments in favor of that association are composed of hypothetical propositions and incorrect or incomplete descriptions of the cited studies.

Conclusion

There is significant paucity of scientific support for the Proposed Coarse PM NAAQS and the scientific studies cited by EPA in support of the NAAQS suffer from significant methodological limitations.

Although EPA repeatedly acknowledges that the database suffers such limitations, it persists in presenting the accumulated data as sufficient to justify the Proposed NAAQS. But in addition to those acknowledged by the Agency, a detailed review of the cited

studies reveals numerous deficiencies that EPA has either not recognized or chosen to ignore.

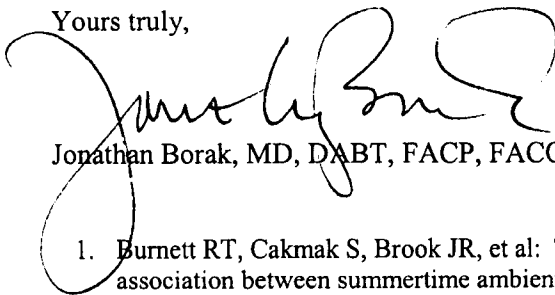
Unlike many other EPA risk assessments that thoughtfully sorted strong from weak studies, emphasizing evidence from the former and discounting that from the latter, EPA in this case seems unwilling to discard any “finding” that might somehow be construed as supporting its NAAQS. That leads to important inadequacies in the justification and support of its proposed policy.

The majority of findings presented as supporting evidence derive from the methodologically weakest studies, while the methodologically most robust studies yield essentially no support. EPA relies on the least rigorous of analytical approaches (e.g., single pollutant models vs. multi-pollutant models), minimizes or ignores potential confounding (e.g., URI events inducing asthma attacks, gaseous co-pollutants) and, as discussed above, by misrepresenting study findings.

A detailed, balanced reading of the evidence indicates no basis to justify regulating of PM_{10-2.5}, only arguments and hypotheses that mainly reflect biological plausibility rather than empirical findings. The general lack of evidence persists even at exposure levels substantially higher than those considered health relevant in the Proposed Coarse PM NAAQS.

I find insufficient scientific justification for the adoption of the Proposed Coarse PM NAAQS.

Yours truly,



Jonathan Borak, MD, DABT, FACP, FACOEM

1. Burnett RT, Cakmak S, Brook JR, et al: The role of particulate size and chemistry in the association between summertime ambient air pollution and hospitalization for cardiorespiratory. Environ Health Perspect 105:614-620, 1997.
2. Schwartz J, Neas LM: Fine particles are more strongly associated than coarse particles with acute respiratory health effects in schoolchildren. Epidemiol 11:6-10, 2000.
3. Lippmann M, Ito K, Nadas A, et al: Association of particulate matter components with daily mortality and morbidity in Urban populations. Cambridge: Health Effects Institute, 2000.
4. Ito K. Associations of particulate matter components with daily mortality and morbidity in Detroit, Michigan. In: Revised Analyses of Time-series Studies of Air Pollution and Health, Boston: Health Effects Institute, pp. 143-156, 2003.

5. Moolgavkar SH: Air pollution and hospital admissions for chronic obstructive pulmonary disease in three metropolitan areas of the United States. Inhal Toxicol 12 (suppl 4):75-90, 2000.
6. Burnett RT, Brook J, Dann T, et al: Association between particulate- and gas-phase components of urban air pollution and daily mortality in eight Canadian cities. Inhal Toxicol 12 (suppl 4):15-39, 2000.
7. Ostro BD, Hurley S, Lipsett MJ: Air pollution and daily mortality in the Coachella Valley, California: A study of PM10 dominated by coarse particles. Environ Res 81:231-238, 1999.
8. Ostro BD, Broadwin R, Lipsett MJ: Coarse and fine particles and daily mortality in the Coachella Valley, California: A follow-up study. J Exp Analysis Environ Epidemiol 10:412-419, 2000.
9. Ostro BD, Broadwin R, Lipsett MJ. Coarse particles and daily mortality in Coachella Valley, California. In: Revised Analyses of Time-series Studies of Air Pollution and Health. Boston: Health Effects Institute, pp. 199-204, 2003.
10. Schwartz J, Dockery DW, Neas LM: Is daily mortality associated specifically with fine particles? J Air Waste Manage Assoc 46:927-939, 1996.
11. Klemm RJ, Mason R. Replication of reanalysis of Harvard Six-City Mortality Study. In: Revised Analyses of Time-series Studies of Air Pollution and Health. Boston: Health Effects Institute, 2003.
12. Thurston GD, Ito K, Hayes CG, et al: Respiratory hospital admissions and summertime haze air pollution in Toronto, Ontario: Consideration of the role of acid aerosols. Environ Res 65:271-290, 1994.
13. Mar TF, Norris GA, Koenig JQ, et al: Association between air pollution and mortality in Phoenix, 1995-1997. Environ Health Perspect 108:347-353, 2000.
14. Mar TF, Norris GA, Larson TV, et al. Air pollution and cardiovascular mortality in Phoenix, 1995-1997. In: Revised Analyses of Time-series Studies of Air Pollution and Health. Boston: Health Effects Institute, pp. 177-182, 2003.
15. Sheppard L, Levy D, Norris G, et al: Effects of ambient air pollution on nonelderly asthma hospital admissions in Seattle, Washington, 1987-1994. Epidemiol 10:23-30, 1999.
16. Sheppard L. Ambient air pollution and nonelderly asthma hospital admissions in Seattle, Washington. In: Revised Analyses of Time-series Studies of Air Pollution and Health, Boston: Health Effects Institute, pp. 227-230, 2003.
17. Gordian ME, Ozkaynak H, Xue J, et al: Particulate air pollution and respiratory disease in Anchorage, Alaska. Environ Health Perspect 104:290-297, 1996.
18. Choudhury AH, Gordian ME, Morris SS: Associations between respiratory illness and PM10 air pollution. Arch Environ Health 52:113-117, 1997.
19. Chen L, Yang W, Jennison BL, et al: Air particulate pollution and hospital admissions for chronic obstructive pulmonary disease in Reno, Nevada. Inhal Toxicol 12:281-298, 2000.
20. Schwartz J: Air pollution and hospital admissions for cardiovascular disease in Tucson. Epidemiol 8:371-377, 1997.
21. Hefflin BJ, Jalaludin B, McCure E, et al: Surveillance for dust storms and respiratory diseases in Washington State, 1991. Arch Environ Health 49:170-174, 1994.

22. Johnston SL, Pattermore PK, Sanderson G, et al: Community study of role of viral infections in exacerbations of asthma in 9-11 year old children. Br Med J 310:1225-1229, 1995.
23. Collier AM, Pimmel RL, Hasselblad V, et al: Spirometric changes in normal children with upper respiratory infections. Am Rev Respir Dis 117:47-53, 1978.
24. Rabinovitch N, Zhang L, Murphy JR, et al: Effects of wintertime ambient air pollutants on asthma exacerbations in urban minority children with moderate to severe disease. J Allergy Clin Immunol 114:1131-1137, 2004.
25. Wartenberg D, Reyner D, Siegel-Scott C: Trichloroethylene and cancer: Epidemiologic evidence. Environ Health Perspect 108 (suppl 2):161-176, 2000.