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August 14, 2015

City Council
City of Moreno Valley
Community Development Department
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cc: Mr. Mark Gross, markg@moval.org
cc: City council members:
Jesse Molina, jessem@moval.org
Jeff Giba, jeffg@moval.org
George Price, georgep@moval.org
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Dear City Council,

I was speaking at the 33rd Annual Meeting of the Doctors for Disaster Preparedness in Ontario, California on 1 August of this year on the subject of statistical errors in particulate matter-morbidity-and-mortality studies. In the questions-and-answers portion of my speech, questions were raised regarding the World Logistics Center Project and the regulatory hurdles they were facing based (in part) on the very kinds of studies I was examining.

I had previously submitted comments to the California Air Resources Board on similar matters, and thought I should bring to your attention concerns I had about the studies used by the air resource agencies as evidence to justify their position. Of particular concern was the baffling and misleading position of California air resource agencies in their interpretation of the conclusions of the Health Effects Institute's report on new technology diesel emission.

Now that I have examined the HEI report, and as a scientist who has spent over 20 years analyzing statistical methodologies used in analyzing data of this nature, I find the opinions of the air resource agencies perplexing and I thought you might benefit from my observation.

HEI

I have reviewed the Health Effects Institute's report "Advanced Collaborative

Emissions Study (ACES): Lifetime Cancer and Non-Cancer Assessment in Rats Exposed to New-Technology Diesel Exhaust”. I find the studies on which this report is based scientifically and statistically sound.

For instance, the study synopsis “Effects of Lifetime Exposure to Inhaled New-Technology Diesel Exhaust in Rats” states that this is the “first study to conduct a comprehensive evaluation of lifetime inhalation exposure to emissions from heavy-duty 2007-compliant engines”. Only those studies which investigate engines of a similar modern type (new-technology diesel exhaust; NTDE) are commensurable. Studies which measured (or claim to have measured) older-style engines, which are understood to produce more emissions, would overstate the effect of emissions in comparison with the HEI report.

The “Rats” study is also to be commended because it measured *actual exposure* to NTDE and traditional-technology diesel exhaust (TDE) diesel emissions, and at realistic rates. This point cannot be stressed too highly because, as I’ll discuss below, most studies only *guess* what exposure was. Guessing leads to over-certainty and wrong conclusions.

The HEI report concluded, in a rigorously *controlled* environment, that realistic exposure to NTDE compared to TDE “did not induce tumors or pre-cancerous changes in the lung and did not increase tumors that were considered to be related to NTDE in any other tissue”, though a “few mild changes were seen in the lungs, consistent with long-term exposure to NO₂.” The statistical methods on which these conclusions are drawn are standard and good and the sample size was adequate, but even more to the point was the way the experiment was conducted.

It is often said that *controlled* trials are the “gold standard” in experimental design, and this is because these are the only kinds of studies that can determine cause. Observational studies, on the other hand, can only determine correlation. This is why observational studies often incorrectly ascribe cause. Most studies on the effects of diesel exhaust or particulate matter (PM) in humans are observational.

It is a mistake to say that the HEI report cannot be relied upon because it is the only study of its kind. Either there is a legitimate criticism to make about the study or there isn’t. If there is, it should be made. That it is the only study of its nature (on NTDE) is not a criticism; it is only a re-statement that the study exists. That its findings disagree with other studies is also not a criticism, but a matter for investigation, a point I take up in this letter below. If no legitimate criticism exists, then the study must be accepted. That is how science is supposed to work.

OTHER STUDIES

The South Coast Air Quality Management District (SCAQMD) and California Air Resource Board (CARB) both commented on the Final Program Environ-

mental Impact Report (Final PEIR) for the Proposed World Logistics Center Project. SCAQMD urged that studies (like HEI) not be “cherry picked” and that the large body of literature of diesel and PM studies should be accorded greater weight. I reviewed the studies recommended and discovered they suffer from a fatal (but very common) flaw.

Studies on which SCAQMD and CARB rely are often predicated on the “epidemiologist fallacy” which consists of saying or implying, “X causes Y” but where X is never measured and a statistical test is improperly used to ascertain cause. These studies greatly exaggerate the certainty with which a cause has been properly identified. The weight accorded to these faulty works should therefore be reduced accordingly.

My 2014 paper (cited below) shows the epidemiologist fallacy consists of two parts. The first is the classical “ecological fallacy”, which is when an author says a potential cause was measured when it was really not. The second part of the fallacy is to assume a statistical model (in the presence of the ecological fallacy) can demonstrate a cause with a stated certainty. Yet no statistical procedure that is based on correlations (like those present in studies which rely on the ecological fallacy) can show cause (Briggs, 2015). The old saying is relevant: “Correlation is not causation”. It is surprising, however, how often it is forgotten that correlations are assumed to have demonstrated causes.

The epidemiologist fallacy is present in the SCAQMD-cited 2006 observational study, “Traffic, Susceptibility, and Childhood Asthma” by McConnell and others. In its abstract, this study states, “we examined the relationship of local traffic-related exposure and asthma and wheeze in southern California school children (57 years of age).” Yet exposure to traffic was never measured. Instead, the “exposure” children had to traffic was based on a *guess* (the guess itself was the result of a statistical model, and the uncertainty inherent in the model was ignored). To emphasize, *where* the children were during the course of this study was never measured, but only approximated. The authors conclude their “results indicate that residence near a major road is associated with asthma.” As noted, it is a statistical mistake to infer, as these authors do, that “associated with” means “caused.”

It might be that living near a roadway causes, in some children, asthma. But are poorer or more well-off children likely to live near a major roadway? Is it the roadway itself that causes the asthma (only in some cases) or it is, say, the poor health or lifestyle of the parents or some other environmental agent? Or is it that more children are being screened for asthma (because of school programs and the like) and that heretofore marginal cases, especially among the poor, went undiagnosed? All these, and many more, unanswered and unanswerable questions are why observational studies cannot be trusted as the sole basis in estimating risk. It is also why observational studies tend to exaggerate risk.

The cited paper by McConnell and others, 2010, “Childhood Incident Asthma and Traffic-Related Air Pollution at Home and School” suffers from these same

shortcomings.

Another frequent mistake is to conflate model-based predictions with reality. The cited paper by Perez and others, 2009, “Global Goods Movement and the Local Burden of Childhood Asthma in Southern California”, claimed “Approximately 1600 (9%) of all childhood asthma cases in Long Beach and 690 (6%) in Riverside were attributed to traffic proximity. Ship emissions accounted for 1400 (21%) bronchitis episodes and, in more modest proportions, health care visits for asthma.”

Now *attributed to* means *caused by*, an inferential mistake, as I have already noted. And the supposed attributions were all the result of guessing using various published statistical sources and by adding another statistical model on top of these. There was no attempt made to ascertain how well the predictions matched reality by, say, measuring the actual causes of asthma in real people. As I show below, not only does statistical risk over-estimate real risk, the technique of Perez is doubled (so to speak) the over-certainty by taking models of models for reality.

One of the statistical models that Perez used as input to their statistical models was based on the The Children’s Health Study, 2004. This study also committed the epidemiologist fallacy, and substituted “community”-based measurements of PM (and other atmospheric elements) as actual exposures. The conclusion is that the Perez study very grossly over-estimates risk.

Lastly, it must be recognized that none of the alternate studies measured NTDE exclusively, as the HEI study did. Even accepting (what is false) that distance to a road accurately measures exposure to PM, the PM generated at roadways is produced by an enormous variety of sources. Given the dates these studies were conducted, only a small portion of the PM could have been from NTDE vehicles. This makes comparisons with the HEI report even sketchier.

REAL RISK

In calculating real risk, there are two points that should be kept in mind. The first is that it is well to ask what is the risk (for morbidity or mortality) for the proposed World Logistics Center Project (WLCP), but it must be remembered that all risk is relative. The proper comparator is not the WLCP or nothing, but the WLCP or that which will happen in its absence. “Nothing” is not a thing. It is not that forbidding the WLCP will cause nothing to happen, but it will cause the goods which would have been moved through the WLCP to be moved elsewhere and by differing means. The people who would work at the WLCP would be doing something else; they would not be doing nothing. It is those differences which must be estimated for any calculation of real risk to have validity.

Comparing the WLCP with “nothing” is bound to exaggerate the risk of the WLCP. The risks also have to be balanced against the potential rewards, some

of which might be improved health. For instance, the people working at the WLCP would presumably have access to medical care which, if they are now unemployed, they currently lack. Excluding these, or any kind of benefit (and there are many), necessarily biases the risk to make it appear to be more dire than it is.

There is a tremendous difference between statistical and real risk. The former is always greater than the latter. Statistical risk that is often taken for real risk (see Briggs, 2015). Explaining these differences must involve a great deal of technicalities, but the essential idea is this. A statistical model which identifies correlation of exhaust and some morbidity or mortality produces an estimate of a “parameter” which approximates risk. That it *is* and approximation is usually quickly forgotten, and so real risk is exaggerated.

But there is a way to account for the approximate nature of the parameter, one which properly accounts for its uncertainty. This technique produces a far superior measure of real risk. I did not see this technique (looking at the posterior predictive distribution of the statistical model) in any of the papers cited by SCAQMD and CARB. As I say, the matter is not simple, but this take away point is: statistical risk *always and necessarily* is larger than real risk. For instance, if the number of increased cancer cases (over some threshold or norm) estimated with statistical risk is some value, the real risk value will always be lower.

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Sincerely,

A handwritten signature in black ink that reads "William M. Briggs". The signature is written in a cursive style with a large, stylized initial "W".

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Statistical Consultant
Adjunct Professor of Statistics
Cornell University

encl: Curriculum vitae

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1. EXPERIENCE

- (1) 2004-PRESENT ADJUNCT PROFESSOR OF STATISTICAL SCIENCE, CORNELL UNIVERSITY, ITHACA, NEW YORK
I teach summer course to people who (rightfully) hate statistics; class focuses on *understanding* not memorization. Research: philosophy of science & probability, epistemology, Bayesian statistics, climatology & meteorology, goodness of forecasts, over-confidence, medicine; public understanding of science, limitations of science, scientism; scholastic metaphysics (as it relates to science).
- (2) 1998-PRESENT. STATISTICAL CONSULTANT, VARIOUS COMPANIES
The rest of my time is spent coaxing people out of their money for telling them they are too sure of themselves. All manner of analyses cheerfully undertaken. Example: I created the *Wall Street Journal's* College Rankings.
- (3) 2003-2010. RESEARCH SCIENTIST, NEW YORK METHODIST HOSPITAL, NEW YORK
Besides the usual, I sit/sat on the Institutional Review Committee to assess the statistics of proposed research. I was an Associate Editor for *Monthly Weather Review*: (through 2011). Also a member of the American Meteorological Society's Probability and Statistics Committee (through 2011).
At a hospital? Yes, sir; at a hospital.
- (4) 2007, 2010 VISITING PROFESSOR OF STATISTICS, DEPARTMENT OF MATHEMATICS, CENTRAL MICHIGAN UNIVERSITY, MT. PLEASANT, MI
Ask me about the difference between "a degree" and "an education."
- (5) 2003-2007, ASSISTANT PROFESSOR STATISTICS, WEILL MEDICAL COLLEGE OF CORNELL UNIVERSITY, NEW YORK, NEW YORK
Working here gave me a sincere appreciation for how government money skews research.
- (6) 2002-2003. GOTHAM RISK MANAGEMENT, NEW YORK
A start up then, after Enron's shenanigans, a start down. We set future weather derivative and weather insurance contract prices that incorporated information from medium- and long-range weather and climate forecasts.
- (7) 1998-2002. DOUBLECLICK, NEW YORK
Lead statistician. Lot of computer this and thats.
- (8) 1993-1998. GRADUATE STUDENT, CORNELL UNIVERSITY
Meteorology, applied climatology, and finally statistics. Was Vice Chair of the graduate student government; elected probably thanks to a miracle.
- (9) 1992-1993. NATIONAL WEATHER SERVICE, SAULT STE. MARIE, MI
Forecast storms o' the day and launched enormous balloons in the name of science. My proudest moment came when I was able to convince an ancient IBM-AT machine to talk to an *analog*, 110 baud, phone-coupled modem, all using BASIC!

- (10) 1989-1992. UNDERGRADUATE STUDENT, CENTRAL MICHIGAN UNIVERSITY
 Meteorology and mathematics. Started the local student meteorology group to chase tornadoes. Who knew Michigan had so few? Spent a summer at UMichigan playing with a science-fiction-sounding lidar.
- (11) 1983-1989. UNITED STATES AIR FORCE
 Cryptography and other secret stuff. Shot things; learned pinochle. I adopted and became proficient with a fascinating and versatile vocabulary. Irritate me for examples. TS/SCI+ security clearance (now inactive).

2. EDUCATION

- (1) Ph.D., 2004, Cornell University. Statistics.
- (2) M.S., 1995, Cornell University. Atmospheric Science.
- (3) B.S., Summa Cum Laude, 1992, Central Michigan University. Meteorology and Math.

3. PUBLICATIONS

3.0.1. *Popular.*

- (1) Op-eds in various newspapers; articles in *Crisis Magazine* (for Catholic laity), *Stream*, *The Remnant*, *Quadrant*, *Quirks*; blog with ~80,000 monthly readers. Various briefs submitted to government agencies, such as California Air Resources Board, Illinois Department of Natural Resources. Talks of all kinds.

3.0.2. *Books.*

- (1) Briggs, WM., 2015 *The Philosophy of Probability*. In preparation! Notice the exclamation point! I'll probably change the title!
- (2) Briggs, WM., 2008 *Breaking the Law of Averages: Real Life Probability and Statistics in Plain English*. LULU Press, New York. Text for undergraduates.
- (3) Briggs, WM., 2006 *So You Think You're Psychic?*. LULU Press, New York. Hint: I'll bet you're not.

3.0.3. *Methods.*

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- (13) Briggs, WM, 2012. Why do statisticians answer questions no one ever asks? *Significance*. Volume 9 Issue 1 Doi: 10.1111/j.1740-9713.2012.00542.x. 30–31.
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3.0.4. Applications.

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