

Tara Hoke
General Counsel
American Society of Civil Engineers
1801 Alexander Bell Drive
Reston, VA 20191

May 19, 2022

Re: Request for further action in the matter of Tony Szamboti and Richard Johns' Ethics Complaint against ASCE members Roberto Ballarini and Kaspar Willam

Dear Ms. Hoke:

On May 10, 2022, we received a decision letter from ASCE's *Journal of Engineering Mechanics* declining our Discussion, first submitted in May 2011, of Jia-Liang Le and Zdeněk Bažant's "Why the Observed Motion History of the World Trade Center Towers is Smooth," published in the *JEM* in January 2011.

In the Ethics Complaint we submitted in September 2018 against ASCE members and former *JEM* editors Roberto Ballarini and Kaspar Willam for rejecting our duly submitted Discussion as "out of scope," our proposed remedy was for the chief editor or an associate editor of the journal to "perform an editorial review of the revised manuscript of the Discussion Paper and subsequently **publish the Discussion Paper or provide a technically reasoned decision not to publish the Discussion Paper**, consistent with the procedure currently set forth in 'Publishing in ASCE Journals' for review of discussion papers and appeals." (Emphasis added.)

To our disappointment, the new chief editor, Dr. Franz-Josef Ulm, who last month agreed to review the Discussion, rejected it without providing a "technically reasoned decision." The relevant section of Dr. Ulm's comments is as follows:

"JEM welcomes discussion of engineering-scientific issues related to papers published in JEM after peer review. Such discussions advance our understanding of mechanics principles, provide us with new insight into strength and limitations of previously proposed theories or models, and generally contribute to an open discussion using the scientific method.

Unfortunately, this is not the case of the presented discussion paper, which circles back to an argument made by Le and Bazant (2008), without ever diving into the very essence of Bazant and Bazant and [sic] Le's modeling approach and actual results backed up by observations. Instead, the authors claim from the onset that the Bazant and Le approach was based upon unjustifiable assumptions. This means that they are not discussing the paper, which should be the focus of the discussion in the first place.

We are thus left with looking into the claims made by the authors. These claims were addressed in a previous editorial review of an earlier version of the discussion and were found to be without merit. We have read the earlier editorial review and concur with its arguments on scientific grounds. To my understanding, there have been no major changes made in the revised manuscript, which means that the scientific reasons upon which the manuscript was rejected in an earlier editorial review have not been addressed by the authors and thus still stand."

We have four points to make in response to Dr. Ulm's comments.

First, where Dr. Ulm writes that the Discussion “circles back to an argument made by Le and Bažant (2008),” he is either citing the wrong paper — our Discussion is of Le and Bažant’s 2011 paper — or he is mischaracterizing the extent to which the Discussion focuses on Bažant and Le’s 2008 paper. The Discussion does briefly cite Bažant and Le’s 2008 paper, but only as a source for two input values used in Le and Bažant’s 2011 paper. It does not “circle back to an argument” in Bažant and Le’s 2008 paper.

Second, Dr. Ulm bizarrely claims that “they are not discussing the paper.” In fact, we cite Le and Bažant’s 2011 paper *thirteen times* in the body of the Discussion, describing their assumptions, formulas used and results obtained. (We also cite Bažant and Le (2008) twice, and Bažant and Zhou (2002) twice as well.)

Dr. Ulm justifies his bizarre claim by saying that we didn’t discuss

“the very essence of Bazant and Bazant and [sic] Le’s modeling approach and actual results backed up by observations. Instead, the authors claim from the onset that the Bazant and Le approach was based upon unjustifiable assumptions.”

It is true that, in our Discussion, we did not criticize the basic model of Le and Bažant, but only their input assumptions. However, as every engineer knows, the results of a calculation cannot be more reliable than its input numbers. A sound calculation based on faulty data is a case of “garbage in, garbage out.” Moreover, Le and Bažant’s paper reached conclusions about the collapse of *a specific building*, WTC 1, and was not a purely theoretical analysis of progressive collapse in general. Therefore, the assumptions made about this building are *essential* to the conclusions they reached. Also, as we have pointed out many times, a key responsibility of *JEM* editors is to publish discussion papers and/or errata when mistakes in published papers are brought to their attention. Is Dr. Ulm really suggesting that a paper analyzing the failure of a real structure, based on false data about that structure, would not be in need of correction?

It is also worth noting that Dr. Ulm’s claim that “they are not discussing the paper” is in effect saying that a discussion paper like ours that focuses on errors in real-world data is not suitable for publication in the *JEM*. In other words, his claim is very similar to the “out of scope” decision that led to our Ethics Complaint in the first place.

Third, Dr. Ulm looks into the actual claims we make in our Discussion, and writes, “We have read the earlier editorial review and concur with its arguments on scientific grounds.” In order to address this statement, let me summarize the reviewer’s central criticisms, which all concern simple factual matters. After each claim I’ve included a summary of our response to it from the Rebuttal document.

1. Reviewer’s claim: Le and Bažant’s $m_c = 0.627 \text{ Mkg}$, the “mass of one floor slab,” is actually the mass of the entire floor assembly and contents, not just the 11cm-thick concrete slab.
 - We calculated that the concrete slab itself could not weigh more than 0.7 Mkg. Also, the reviewer doesn’t address the fact that Bažant and Le (2008) give the mass of one story as 3.87 Mkg.
2. Reviewer’s claim: Le and Bažant do not assume that $F_y = 0.25 \text{ GN/m}^2$.
 - We showed that this assumption is implicit in their Equation (3), and also that Bažant and Le explicitly gave this value in their 2008 closure to G. Szuladzinski’s discussion.
3. Reviewer’s claim: The formula used $M_p = 1.5 b^2 t F_y$ is not the usual formula for M_p .
 - We derived this formula from one in a standard textbook, using the fact that $t=w$ and $t \ll b$ for the upper-story columns of WTC 1.

4. Reviewer's claim: There is no basis for the authors' assertion that $A = 4m^2$.
 - We showed clearly how this number was calculated.
5. Reviewer's claim: The authors don't provide sufficient evidence that the mass of the descending portion of the building was 33 Mkg, rather than the 54.18 Mkg assumed by Le and Bažant.
 - We pointed out that NIST gives the weight of the descending portion as 73,143 kips, which converts to 33.18 Mkg. Also, Le and Bažant give no source for their value.

Dr. Ulm claims to have "scientific grounds" to accept the reviewer's claims and reject our responses. We would dearly like to know what these grounds are. That would, in fact, satisfy our demand for a "technically reasoned decision." As it stands, however, Dr. Ulm has not provided such a decision, which was a necessary feature of our proposed remedy for resolving the Ethics Complaint and avoiding a disciplinary proceeding against Dr. Ballarini and Dr. Willam.

Fourth, and finally, Dr. Ulm states that "no major changes" were made in the revised manuscript, and therefore the issues raised in reviewer's comments have not been addressed and still stand. This is an unfair statement, since the main objections (summarized as 1-5 above) were thoroughly addressed in our Rebuttal, and Dr. Ulm has not addressed these responses. Also, our revised manuscript EMENG-1410R1 did include five changes in response to objections 2, 3, 4 and 5 above. Specifically:

2. Lines 40-41 we added, "from Equation (3) we can infer that the yield stress $\sigma_0 = 250 \text{ MPa}$ "
3. Lines 46-50 we added the standard formula for M_p , citing Gaylord et al, and at lines 66-68 we explained the derivation of our simplified formula for M_p .
4. We removed the assertion that $A = 4m^2$, as it wasn't necessary for the argument (even though it is correct).
5. Lines 98-99 we added a reference to a NIST report in support of our mass value 33.18 Mkg.

We also made the following three changes in response to minor criticisms of the reviewer:

- Lines 45-46 we explained that 0.3556m is the width and breadth of a perimeter column.
- Lines 79-84 we changed the effective column length from 2.3 to 3.7m, i.e., we ignored the effect of the spandrel plates.
- Lines 123-26 we added a paragraph showing that the general form of our graph (generated by a computer simulation) can be derived easily by hand calculations.

In closing, we request that the Committee on Professional Conduct and the ASCE Journals Division approach Dr. Ulm again to ask that he (or an impartial associate editor) review the Discussion and either accept it for publication or provide a "technically reasoned decision" not to publish it. If Dr. Ulm does not agree to a review where a "technically reasoned decision" to decline must be provided, we will resume the Ethics Complaint against Dr. Ballarini and Dr. Willam.

In the interest of "open discussion using the scientific method," we assume that Dr. Ulm will be perfectly willing to review the changes made to the manuscript and our rebuttals of the reviewer's comments, and either publish the Discussion or provide a "technically reasoned decision" not to. Please ensure that Dr. Ulm receives this letter.

Sincerely,

Tony Szamboti and Richard Johns