Franz-Josef Ulm, Editor
Journal of Engineering Mechanics
American Society of Civil Engineers
1801 Alexander Bell Drive
Reston, VA 20191

June 15, 2022

Re: Appeal of Decision to Decline Manuscript No. EMENG-1410R1 (Discussion of "Why the Observed Motion History of the World Trade Center Towers is Smooth")

Dear Professor Ulm:

We are writing to you to formally appeal your May 10, 2022, decision on manuscript number EMENG-1410R1, which is the revised manuscript of our Discussion of Jia-Liang Le and Zdeněk Bažant's "Why the Observed Motion History of the World Trade Center Towers is Smooth," published in the *Journal of Engineering Mechanics* in January 2011.

Procedural Matters

Before we explain why we believe your decision was in error, we would first like to address two procedural matters arising from this unique situation:

1. Your decision was not in accordance with the remedy set forth in our ethics complaints against former *JEM* editors Roberto Ballarini and Kaspar Willam.

   The remedy set forth in our ethics complaint — as a preferred alternative to holding a disciplinary hearing for editors Ballarini and Willam — was for the chief editor or an associate of the *JEM* 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.)

   Your decision was not a "technically reasoned decision" because you did not explain why you concurred "on scientific grounds" with the arguments of the reviewer despite our Rebuttal of the reviewer's comments, which you did not address at all in your decision.

   Therefore, while we are formally appealing your first decision, we also hereby reserve the right to appeal your forthcoming decision on this appeal <u>to you</u> before appealing to the Engineering Mechanics Institute Board of Governors (should either appeal become necessary). It would not be fair to deny us the opportunity to appeal the first "technically reasoned decision" on our revised manuscript because we were forced to use up an appeal opportunity appealing a review decision that is not a "technically reasoned decision."

2. We discovered in the "Annual Report to the EMI Membership for FY 2013" that you were serving on the EMI Board of Governors in September 2013, which is when the board reviewed and declined our appeal of the editors' August 2013 decision to reject our revised manuscript as "out of scope." (It later became clear through subsequent communications with then-EMI President Roger Ghanem and Journals Director Angela Cochran that the board did not actually know the revised manuscript was rejected as "out of scope" when it upheld the editors' decision.) President Ghanem's letter to us on September 16, 2013, stated: "[T]he Board feels that you were

treated fairly and all ASCE Publication processes were properly followed." This gives the impression that there was unanimous consensus among the members of the board.

Would you please advise whether or not you participated in the review of our appeal to the EMI Board of Governors in September 2013?

To be clear, we are not claiming that you have a conflict of interest in this matter. We are only seeking full transparency around the handling of our submission.

With that said, if you did participate in the review of our appeal and you would prefer to avoid the *appearance* of a conflict of interest — which is an obligation of editors set forth in "Publishing in ASCE Journals" — we would not oppose reassigning this review to an impartial associate editor (i.e., one with no known professional relationship to Drs. Le, Bažant, Ballarini, or Willam and with no prior involvement in handling our submission).

Technical Rebuttal

The relevant section of your review decision 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 your comments.

First, where you write that the Discussion "circles back to an argument made by Le and Bažant (2008)," you are either citing the wrong paper — our Discussion is of Le and Bažant's 2011 paper — or you are 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, you state 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.)

You justify this 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, as our Discussion attempts to do.

Third, you look into the actual claims we make in our Discussion, and write: "We have read the earlier editorial review and concur with its arguments on scientific grounds." In order to address this statement, let us summarize the reviewer's central criticisms, which all concern simple factual matters. After each claim, we've included a summary of our response to it from the Rebuttal document. (The Rebuttal document is attached hereto, since it is not clear from your comments whether or not you read it.)

1. Reviewer's claim: Le and Bažant's $m_c = 0.627$ 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 $F_y = 0.25$ 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 << b for the upper-story columns of WTC 1.

4. Reviewer's claim: There is no basis for the authors' assertion that $A = 4$m^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.

In your comments, you claim to have "scientific grounds" for accepting the reviewer's claims. If you still have scientific grounds for accepting the reviewer's claims and rejecting our responses after reviewing our responses to the reviewer's claims more closely (or for the first time), it is incumbent upon you, both as a journal editor and the person carrying out the remedy to our ethics complaint, to describe them.

Fourth, and finally, you state that "no major changes" were made in the revised manuscript, and therefore the issues raised in the 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 you have 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 σ_0 = 250 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 of 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 look forward to finally having our Discussion published — and giving the authors of the original paper the opportunity to respond to our critiques, as *they* should be the ones to perform that task. If our Discussion is declined yet again, we expect to receive a "technically reasoned decision" containing *truly* compelling reasons why our Discussion should not be published, since no such reason has been provided to us in the 11 years since our Discussion identifying fatal errors in a *JEM*-published paper was first submitted.

In addition, please advise as to whether you participated in the review of our appeal to the EMI Board of Governors in September 2013.

Sincerely,

Tony Szamboti and Richard Johns

Cc: Michelle English, Executive Editor, ASCE Journals
Tara Hoke, General Counsel, ASCE