Responses to Reviewer Comments

Stepwise regression modeling for compressive strength of alkali-activated concrete

R.J. Thomas^a, Sulapha Peethamparan^{b,*}

^a*Dept. of Civil and Environmental Engr., Utah State University, Logan, UT, USA*

^b*Dept. of Civil and Environmental Engr., Clarkson University, Potsdam, NY, USA*

The authors extend their sincere gratitude to the editors and reviewers for their time and consideration. We have considered each of the reviewer comments and have responded to them in turn. Each reviewer comment is transcribed here verbatim and is followed by the authors' response. We have revised our manuscript accordingly. We are confident that our efforts in revising this manuscript will be satisfactory and look forward to the final results of the review process.

1. Reviewer 1

It is an interesting paper including comprehensive range of experimental results. In my opinion, this work is sufficiently novel to deserve publication. I recommend to accept the paper after fixing the below corrections and clarifications.

1. Page 3 line 28, reference 17 "Bernard et al. [17]" needs correction

*Corresponding author Tel: +1-315-268-4435

Email addresses: thmsrj@gmail.com (R.J. Thomas), speetham@clarkson.edu
(Sulapha Peethamparan)

This reference has been corrected.

2. It is always beneficial for readers to know the source of the raw materials used. I suggest adding a sentence saying from where these materials were sourced.

We have added this information to section 2.

3. Page 4, it is beneficial to have the aggregates grinding curves.

We appreciate the suggestion but we don't feel that this addition would improve the paper.

4. On page 5, lines 78-80, it is said that "This heat curing regime was selected because it has been commonly used throughout the literature" but no reference has been mentioned here. It would be more reliable to see few references when saying "commonly used". From my experience, the heat curing at 75-80 C for a period of 18-24 hours results in the optimal mechanical strength, especially for fly ash based GPCs. Please see the below references Noushini A, Aslani F, Castel A, Gilbert RI, Uy B, Foster S. Compressive stress-strain model for low-calcium fly ash-based geopolymer and heat-cured Portland cement concrete. *Cem Concr Compos* 2016 73:13646. doi:10.1016/j.cemconcomp.2016.07.004.

We have added a few relevant references.

5. Page 6, The mix designs are not presented clearly. The amount of aggregate, binders, alkaline solution, etc are to be presented in a way (kg/m³) that it is easily understandable by engineers.

We have used several hundred unique mixture proportions and it is therefore impossible to report all of them. Instead, we have provided all of the necessary information and references required to replicate the

mixture proportions based on the absolute volume method. Any practitioners or engineers familiar with concrete mixture design should be able to easily replicate these mixture if required.

6. Page 7, why 76.2 mm cylinders were used. If it is based on a standard please specify the standard in here.

The size of cylinders was limited due to the expense of reagent-grade activators. Reagent-grade activators were used so that the activator composition could be controlled with exacting precision. We have updated section 2 to reflect this, and have added a recent reference that discusses the size effect in alkali-activated concrete.

2. Reviewer 2

The paper is well written and presents valuable results. There are several points that the authors are asked to consider to improve the manuscript:

1. Please note that 'slag cement' and 'slag' have different meaning and international audience might be confused by several places where these seem to be used in an exchangeable manner (e.g. page 2 line 5).

The term "slag cement" is used consistently throughout the manuscript. There are no instances of "slag." The acronym "AASC" is defined as "alkali-activated slag cement."

2. Please provide explanation: why was Class C rather than Class F fly ash used to prepare the alkaliactivated mixtures? Class F is more commonly used and more widely available.

We don't feel that our use of class C fly ash instead of the more commonly used class F requires justification. We and others have performed

extensive research on alkali-activated class C fly ashed with great success.

3. It is recommended that the authors perform a statistical analysis to see whether the effects of all the parameters included in the models are "statistically significant" to the strength. It can be done by calculating and reporting the pvalue in the statistical models.

Determination of "statistical significance" using traditional statistical methods (i.e., p-values) is flawed and most modern statisticians agree that p-values should not be reported. This becomes increasingly important when the number of data is large. In fact, several leading journals have banned the use of p-values as statistical evidence. Our approach uses the BIC number as the main identifier of significant parameters with arguably good success.

4. In the conclusion section, it is suggested that the authors provide a recommendation on which parameters should be included in the future modeling, while which one is less significant.

Our conclusion is that all of the parameters listed are significant and necessary to accurately predict the compressive strength. We do present simplified (lower-order) models, but our intent was to report the best model based on the reported modeling procedure.

5. Line 375, the authors stated that the quadratic model performed very well with or without the interaction term. It means that it is UN-NECESSARY to include the interaction term. The authors should, again, perform the statistical analysis to see whether these terms should be included or not.

Again, we have intentionally refrained from the use of p-values and significance testing due to the aforementioned issues. We have presented the models based on the reported modeling procedure. Please note that, although readers may not be as comfortable with our method as they would be with p-values and null value hypothesis testing (p-values), the reported procedure is well established in the field of Bayesian data analysis.

6. It should be noted that a higher value of coefficient of correlation does not indicate a better model. A comprehensive statistical calculation and diagnosis should be conducted.

The correlation coefficient is not used as a primary measure of goodness of fit; instead the BIC number is used. We have discussed the merits of using BIC as a measure of goodness of fit in section 3. Nevertheless, R^2 is worthy of inclusion due to its familiarity with most readers.

7. The title should be changed to reflect the 'statistical analysis'.

We have updated the title to more clearly reflect the stepwise regression modeling used.

8. It would also strengthen the paper if the authors could conduct any validation of their proposed models. Otherwise, only the approach is useful but the implementation of proposed equations will be risky for other researchers.

We agree with this statement, and have clearly made this point in the manuscript.