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Discussion

A discussion of the paper "Very-high-performance concrete with ultrafine powders" by Guangcheng Long, Xinyou Wang and Youjun Xie☆

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The authors [1] dealt with an interesting subject. However, I would like to comment and discuss some of the points made in the paper [1], especially its format and clarity.

(1) First, I would like to start with its heading. The heading begins with 'very-high-performance concrete', however, the paper deals with mortar and paste in general. In the paper, the term 'concrete', which is used instead of 'mortar', is also mentioned in a few lines. The paste, mortar and concrete are cement-based materials; they differ from each other in terms of the maximum size of aggregate used. Paste consists of only cementitious powder and water and chemical additives if necessary. Mortar consists of paste and sand. Concrete consists of mortar and coarse aggregate. Further, the term 'high performance' is used as an equivalent of 'high strength' in the paper [1]. It is known that the extent of high performance, which covers not only strength but also durability properties (i.e. permeability, porosity, resistance to carbonation, abrasion, alkali–silica reaction, acidic attack, sulfate attack, etc.), and high strength is different because high-performance concrete is defined as concrete which meets special performance and uniformity requirements that cannot always be achieved routinely by using only conventional materials and normal mixing, placing and curing practices. The requirements may involve enhancements of characteristics such as placement and compaction without segregation, long-term mechanical properties, early-age strength, toughness, volume stability or service life in severe environments [2,3]. This definition can be found on the Internet [2]. In the paper [1], the durability properties of the mixtures are not presented. 'High strength' does not always mean 'high-performance', that is, to be verified with durability properties. Therefore, the heading should have started with 'very-high-strength mortar' instead of 'very-high-performance concrete'.

(2) There is also something missing in the chemical oxide composition of fly ash used, because the total amount of oxide composition, which is 63.46%, is much lower than 100% (see Table 1 of Ref. [1]). Moreover, whether the ash used high calcium (Class C of ASTM) or low calcium (Class F of ASTM) was not provided. It would be better if the specific surface area was provided along with the mean diameter of the powders used.

(3) The authors [1] stated that the smaller the diameter of particle results, the better its filling effect. However, one can analyse the experimental results in different ways, such that the ratios of mean diameter fly ash–silica fume and slag–silica fume are 29 and 32.5 (see Table 1 of Ref. [1]). This means that silica fume is much finer than fly ash and slag. However, the improvement in silica fume resulted in relative density of about 2% and 2.5% when compared with fly ash and slag, respectively, of about 20% of the content of powder/mass (see Fig. 1 of Ref. [1]). The improvement in relative density due to lower mean diameter seems to be insignificant. The discusser believes that the reason for the improvement observed when silica fume is used can be present within the particle shape and size distribution of silica fume.

(4) Regarding Fig. 3 of Ref. [1], the authors [1] stated that the reduction in water/binder ratio results in higher relative density. This is not always true. The discusser would like to state that there will always be an optimal water/binder ratio that will give maximum relative density regardless of fluidity [4–6].

(5) The first three rows of Table 2 (see Ref. [1]) show that the introduction of fly ash into paste mixture increases fluidity and relative density. The introduction of silica fume into cement–fly ash paste mixture results in further improvements. A similar conclusion can be made for the mixture made with 0.2 water/binder ratio. However, the last three rows of Table 2 show that the introduction of silica fume into cement–fly ash paste mixture reduces fluidity, and increases relative density. Those results are contradictory. Also, the discusser would like to ask if there are any

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ternary mixture results made with cement–silica fume–slag. Because, one can think that there may be more results present regarding this issue but the authors [1] presented the selected results that was not stated in the paper.

(6) Although the heading of Table 3 is the experimental results of flowability and strength of mortars, the amount of sand content was not provided in the paper [1]. However, the mortars are regarded as concrete in the paper [1] as mentioned previously. The authors [1] used the values in Table 3 for the optimization of VHPC composites, then concluded that the strength of concrete specimen only containing silica fume increases incrementally in the contents of silica fume, and the optimum content of silica fume was about 0.2–0.3 of the weight of the cement. The discusser would like to state that if one looks at Table 3, then he can see that while silica fume content increases, water/binder ratio also decreases (see first four rows of Table 3). Therefore, it is not fair to state that 'increment of silica fume content resulted in an increase in the strength', because it is well known that decreasing water/binder ratio also results in an increase in the strength. The authors [1], using the same table, also stated that the compressive strength of VHPC samples, including silica fume and fly ash, was slightly higher than that of the samples containing silica fume and slag. The discusser's opinion is that the strength difference between the two-mixture system could be explained by the use of the fresh mixtures' relative density, which was not provided in the paper [1].

(7) The authors claimed that with the limitation of a steel tube to VHPC, not only the toughness, but also the compressive strength of VHPC, could be improved. Also, they claimed that the brittleness of VHPC can be overcome by short steel fibers with L/D=60. Discusser's opinion is that it would be better if the stress–strain curve of VHPC was provided. Dimensions of steel fibers other than L/D ratio were not provided in the paper. The discusser thinks that without giving the actual size of steel fiber and stress–strain curve for both compression and tension, the claim would be weak. Assuming that the diameter of the fiber is 0.5 mm, then the length of it will be 30 mm. Furthermore, the dimensions of the casting mould were 40×40×160 mm. The least dimension of prisms is 40 mm, fiber is 30 mm (recall). This may create distribution and dispersion problems for steel fiber in the casting stage. Therefore, size effect and wall effect may be incorporated in the strength measurements, thus, the results obtained may not be accurate. Furthermore, the discusser believes that the broken pieces of prism specimens after obtaining flexural strength were used to obtain compressive strength, since how the compressive strength specimen was prepared is not clear in the paper [1]. Whether it is cube or cylinder and what its size is were not clearly given other than Section 3.5. The sample preparation part of the paper is insufficient and not made very clear.

(8) Finally, there was a syntax error regarding the curing room referred as 'flog room' which should be 'fog room'. Also, Refs. 4 and 5 of the discussed paper [1] are probably mixed because both are supposed to be in the same conference proceedings and on the same pages.

References

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