Discussion of "Problems with Liquefaction Criteria and Their Application in Australia" by R. Semple, Australian Geomechanics, Vol. 49 No. 1, March 2013

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The author is to be commended for drawing attention to an issue that should be of great concern not only in Australia but worldwide.

There are two broad problems with the use of simplified methods for the evaluation of the potential for liquefaction in the real world including those based on the NCEER report. The first is that that this is just one example of the increasing use of canned spreadsheets both in teaching and in practice that make it difficult for the user to exercise his or her own judgment, rather than encouraging the user to exercise judgment that takes into account the both local geology and local experience with the phenomenon in question. The second is that penetration resistance is an imperfect measure of liquefaction resistance. Because penetration resistance does not fully account for the effects of overconsolidation, previous cyclic shearing, age and the presence of fines, especially clayey fines, on the potential for liquefaction, these simplified methods based on penetration resistance should not be used for site conditions which are outside the ranges of site conditions in the databases of case histories on which they are based. Those case histories consist almost entirely of observations of liquefaction in recent alluvium and hydraulically-placed sand fills. In the case of the case histories which rely on CPT data, this means that the parameter I_c is likely to have been less than 2.05, indicative of a relatively clean sand.

By way of further filling out the excellent historical survey of this subject that is provided by the author, I offer the following brief history of studies of the phenomenon of liquefaction in saturated sandy soils as a result of earthquakes at the University of California, Berkeley. The initial laboratory studies of the phenomenon of liquefaction under cyclic loading were conducted by Harry Bolton Seed and his co-workers Kenneth Lee and Clarence Chang in the mid-nineteen-sixties. Those tests were conducted on reconstituted samples of clean, washed sands. When I later questioned Professor Seed on some of the details of the test procedures he explained that they were so excited that they were able to reproduce the phenomenon in the laboratory that they were not too concerned about the details. In the earliest studies the sands both in the laboratory and in the field were simply characterized by their relative density. However, it quickly became apparent that relative density alone was an inadequate measure of the resistance to liquefaction of a sandy soil. In the early nineteen seventies various students of Professor Seed including myself, John Mulilis and Kenji Mori, showed the importance of soil fabric, and hence the method of specimen preparation in the laboratory, the time under sustained pressure, or age, the previous straining history, overconsolidation and the coefficient of lateral earth pressure on the resistance to liquefaction. These effects were first brought together by Professor Seed in a paper presented at the BOSS '76 conference. The recognition that it was difficult both to prevent the loss of these affects as a result of sampling disturbance and that it was difficult to fully recreate them in the laboratory, along with pressure from Professor Ralph Peck and others to emphasize a more empirical procedure, led Professor Seed in an important paper in 1979 to emphasize the use of empirical methods, at that time focusing on use of the SPT, over the use of laboratory tests because the same factors that tended to increase the resistance to liquefaction also tended to increase the penetration resistance, as measured by the SPT blowcount. However, it was never stated or implied that penetration resistance was a perfect analog for liquefaction resistance, because it is not. For instance, overconsolidation is known to increase the coefficient of lateral earth pressure, but the increase in the resistance to liquefaction caused by overconsolidation is greater than that implied just by the increase in the coefficient of lateral earth pressure.

Unfortunately, after 1979 the emphasis in most academic studies has been directed to refining the empirical evaluation methods based on penetration resistance, rather than quantifying the divergence between penetration resistance and the actual resistance to liquefaction of real soils. This is not a big deal in the case of very recent alluvial deposits and hydraulically placed sand fills, which constitute the bulk of the case histories of liquefaction in earthquakes, but it is very important for all other soils. Had Professor Seed lived longer he surely would have paid more attention to the divergence between the penetration resistance and the resistance to liquefaction of these other soils. It was particularly disappointing that this was not addressed in the NCEER and MCEER workshops which led to the 2001 paper by Youd, Idriss and others. That is why I wrote a discussion on that paper which was published in 2003. My discussion has been widely quoted in practice but has had less impact on academic research and publications than I had hoped. However, Professor Ricardo Dobry recently wrote to me commending my discussion and indicating that he is seeking to further quantify the issues involved. Professor Dobry was in fact an early advocate of the belief that shear wave velocity of soils is a better indicator of the liquefaction resistance of soils than penetration resistance. Critics of that belief argue that under earthquake loading the most sensitive aspects of soil fabric that control the low strain shear modulus and hence the shear wave velocity might be lost, but Professor Dobry and others have shown rather conclusively that the rate of excess pore pressure development is very dependent on small strain behavior and there is no evidence that cyclic loading significantly disrupts these aspects of soil fabric short of complete liquefaction.

In view of this it is perhaps surprising that the use of shear wave velocity as an index for liquefaction resistance, as developed in particular by Andrus and Stokoe, has not been more widely embraced but that has a lot to do with the "not invented here" syndrome. Academics whose experience and interests are tied more closely to other approaches are not inclined to admit that they have been wasting their time on arguing about trivia and have been missing some of the big picture issues that are important in practice. To compound the problem the same academics have been carried away with teaching simplified methods of analysis that can be implemented using a spreadsheet. These simplified methods might be fine as screening tools but in general they should not be used for anything beyond that and in particular they should not be relied on for projects that have significant financial considerations both in terms of the cost of construction and the consequences of failure.