CORPS OF ENGINEERS, U. S. ARMY

POTAMOLOGY INVESTIGATIONS

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MINUTES OF CONFERENCE ON SOIL STUDIES

POTAMOLOGY INVESTIGATION

18 APRIL 1949

WATERWAYS EXPERIMENT STATION

VICKSBURG, MISSISSIPPI

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MINUTES OF THE CONFERENCE ON SOILS STUDIES

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1. The conference on Soils Studies, Potamology Investigations was convened at 9:00 A. M. 18 April 1949. The following personnel were present:

Dr. A. Casagrande, Consultant, Harvard University
Dr. M. Juul Hvorslev, Consultant, Soils Division, WES
Mr. R. A. Latimer, Chief, Construction Division, MRC
Mr. R. H. Haas, Chief, Bank Protection, MRC
Mr. J. B. Tiffany, Technical Executive Assistant, WES
Mr. E. P. Fortson, Chief, Hydraulics Division, WES
Mr. G. B. Fenwick, Hydraulics Division, WES
Mr. E. B. Lipscomb, Hydraulics Division, WES
Mr. S. J. Johnson, Soils Division, WES
Mr. W. G. Shockley, Soils Division, WES
Mr. A. A. Maxwell, Soils Division, WES
Mr. P. K. Garber, Soils Division, WES

2. The first topic was a discussion of the bank failures at Reid-Bedford Bend. Mr. Johnson briefly reviewed the general problem. A movie of the site was shown after which the slides were described in some detail and models of the slides were shown and discussed. Mr. Johnson discussed scour and fill in the revetted area during the periods between construction and each failure. Mr. Latimer commented that the problem probably would not have occurred or at least not have been as severe if sufficient funds had been available to place the required amount of revetment. Mr. Haas commented that he had always thought of the failures as "connection" failures, since he had often observed depressions in the lower bank just below zero MLW which were not connected to the thalweg. He thought the trough or throat of the

failure developed after the upper depression had been formed by scour. Mr. Latimer asked if any studies had been made of the sand waves that have been observed (by sounding) moving down the thalweg against the revetment. Mr. Johnson said that no such investigations had been made by the Soils Division, it being basically a hydraulic problem.

3. Mr. Maxwell discussed the piezometer installations and the data obtained from these installations. He pointed out that the readings agreed very well with the theoretical piezometric surface and were therefore probably reliable. He further stated that the seepage gradients were so low that the forces due to seepage, if truly reflected by the piezometer data, were of no importance at any river stage, and may have been zero at the time of failure or actually directed into the bank and thereby increasing its stability.

4. Mr. Maxwell discussed the split spoon penetration resistance. He stated that the conclusions drawn from this investigation were that some account must be made for the increase in penetration resistance with depth when correlating it with density, and on the basis of a rough method of analysis which took this into account he believed that the deep sands behind the failure on Range 48 were less dense than elsewhere. He further stated that inflow of material had a considerable effect on the penetration resistance at this site, but the mere fact that the material would flow in so readily might be an indication of its low density. He stated that the elevation of the ground water affects the penetration resistance, in that it changes the effective overburden pressures and that additional borings would be made at Reid-Bedford at river stages corresponding to those at failure.

5. Mr. Johnson asked for discussions on the possible types of failures by Dr. Casagrande and Dr. Hvorslev. Mr. Tiffany asked for the floor previous to their discussion so that he might present the hydraulic aspects of the problem. He discussed the failure at Free Nigger Point and commented that tremendous forces were at work in the river which might add to the general instability of any portion of the bank. He pointed out the hydraulic aspects of the problem and that the failures at Reid-Bedford were initiated by scour, regardless of what type they might have been; i. e., flow, internal erosion, or external erosion. Mr. Johnson concurred with him on this latter belief but pointed out that intensive scouring has occurred where there have been no failures, and that while scour at the toe is probably the basic factor it is not the only factor.

6. Dr. Casagrande stated that he felt that the failures could not be either shear or direct scour, but that internal erosion might have been responsible, since three-dimensional concentration of flow due largely to soil irregularities could cause erosion even under low total differentials. It would be in the form of progressive backward and upward, fan-shaped erosion paths. Vertical columns might be left between channels which would temporarily support the overburden, and which would collapse after high water stages. Once the overburden was disturbed, it would have little or no shear strength and would appear to fail by flow. Dr. Casagrande then stated that the clue to the matter might be found in searching the failure area for traces of cohesive overburden. Mr. Johnson replied that this had been done and such materials were found.

Dr. Casagrande stated that the narrow neck of the failure makes him suspicious that a typical flow failure did not occur; the shape being more typical of internal erosion. Mr. Johnson stated that no traces of erosion failures had been observed at low stages and reviewed the sounding data available, according to which the failure of necessity had to occur rapidly; in the space of less than two weeks in one instance. He questioned the likelihood of internal erosion as a basic cause but readily agreed that this possibility should be carefully investigated. Dr. Casagrande stated that they might have been occurring under water for years. Mr. Tiffany stated that this was a combined problem of soils and hydraulics and illustrated the forces and how they were acting in a sketch of the hydraulic conditions at Free Nigger Point. Dr. Casagrande asked if it had ever been observed that these forces caused vibrations in the bank. Mr. Haas stated that he thought it was possible in some bank materials. Dr. Casagrande asked if the bank was subjected to pounding similar to wave action. Mr. Tiffany said that pressure fluctuations had been measured, in terms of velocities, in the order of 3 to 6 feet per second during low water stages but might be doubled during high water; scour and shock may be very significant contributing factors. Mr. Johnson stated that Free Nigger Point has always been turbulent — many tows have been turned around and broken up at that point. If these forces have been acting for 50 years without affecting the bank, how is it that they suddenly removed so much material overnight? Mr. Tiffany replied that they might have been acting on more resistant materials and had broken through into weaker materials. Mr. Latimer stated that, in

connection with possible flow failures, it might be well to find out if a very heavy freight had gone over the bridge about a mile below on the night of the failure.

7. Mr. Johnson asked Dr. Casagrande if he could describe how internal erosion might have taken place in such short periods as those before the failures for which we have no hydrographic surveys of the area — or if the erosion existed for longer periods of time, how it went undetected by the hydrographic surveys. Dr. Casagrande asked if a hole 3- to 10-ft wide might be detected in the hydrographic survey. Mr. Fenwick said it could not. Dr. Casagrande said that the erosion could be taking place through an opening no larger than 3 to 10 ft in diameter. The overburden would be supported by arching and the final failures set off by the powerful river forces of which Mr. Tiffany spoke. A lot of these failures may start and be stopped by soil falling into the opening; these would show up as only minor surface depressions. Mr. Tiffany stated that the stage was set for failure to take place by scour. Dr. Casagrande asked if the forces acting in the failure pocket are violent. Mr. Tiffany replied that they were in the upstream portion below the surface. Mr. Johnson replied that they could not be too violent and that scour could not be too effective since the shoulders of the failure on Range 48 remained undisturbed for one month without protection. Mr. Maxwell asked if the forces of which Mr. Tiffany spoke actually removed the soils or if they set up the conditions. Mr. Tiffany said it was combination of both which ultimately became a slide.

8. The conference was adjourned for lunch at 12:30 P. M. and reconvened at 1:00 P.M.

9. Mr. Johnson asked Dr. Hvorslev for his ideas on the failure. Dr. Hvorslev stated that he thought there were two possibilities — flow slide or internal erosion. He also thought the neck of the failure could be either cause or effect. The neck might be the cause in an erosion failure; i. e., all of the material was eroding through the neck. The neck might be the effect in a flow slide; i. e., the neck material is removed by the weaker materials sliding out. Mr. Haas then asked if the pounding action of the mat might not have some effect upon the stability of the bank. He said that the failures generally occur in the downstream portion of the revetment where the hydraulic forces and consequent deepening may be in the order of 25 to 50 per cent of the original depth.

10. Mr. Johnson briefly reviewed the laboratory tests. He said that the purpose of the triaxial tests was to reproduce flow failures and to study critical void ratios. He then discussed the types of tests which were performed — the drained shear tests and the constant volume shear tests. Mr. Garber presented the test results and the characteristics of each type of test and failures obtained therein. He concluded that it was possible to reproduce flow failures and that there may be critical sands rather than critical void ratios. The conference was adjourned to the laboratory where all members witnessed a constant volume shear test on sand. Although a flow failure did not result, the lateral effective stress was reduced with increasing strain to about 5 per cent of its initial value. Dr. Casagrande stated that the test results were

very interesting and that the 5.6-in. sample may be the solution to the problem of eliminating test errors due to membrane restraint, etc. He did not think that volume change control was too important — the hand method could be improved but he would not attempt to refine the procedure to any greater extent.

11. Dr. Casagrande then discussed the laboratory and field conditions in a comparative sense. He said that, at the time of publication of the triaxial review, his opinion was that the triaxial tests were only a very rough approximation of field conditions and that laboratory results will always be on the unsafe side. In his opinion, liquefaction is not as likely in the laboratory as in the field, since the laboratory specimen will develop more resistance than it would in the center of a uniform mass because of nonuniform strain and because of the required slowness of the test. The specimen will also have a different critical void ratio than the mass, because the specimen has been mixed; the mass is always more or less subdivided into separate strata, each more uniform in grain size than the laboratory sample. In a great number of cases, the field void ratio has been less than the critical void ratio from laboratory tests, but still flow failures occurred. At the present time he feels laboratory tests, at least those performed prior to this investigation, can not be directly applied and that more research on this is required. Dr. Casagrande concluded that he preferred a 5-in. sampler to a 3-in. sampler for obtaining samples of sand with a frozen plug sampler, but that he had no evidence to support his preference.

12. Mr. Johnson then summarized the topics which had been discussed

in the conference and asked Dr. Casagrande and Dr. Hvorslev if they were prepared to give an opinion on the relative possibility of each type of failure. They were not. Mr. Johnson then asked if they could say on which type of failure would further development be most justified. Dr. Casagrande said that all three causes were justified. He was in favor of full-scale tests as the best investigational method to prove flow slides. He said sand samples might be taken by hand in Gow caissons in the field at low river stages. Laboratory tests are an important part of the investigation but cannot be depended upon to give precise answers. Mr. Johnson questioned Dr. Casagrande as to how to investigate the possibility of internal erosion and Dr. Casagrande suggested model tests. Dr. Hvorslev thought erosion might just remove fines (as in filter failures) and thereby reduce the critical void ratio. Dr. Casagrande thought that this effect would only take place on the surface and would not be important. Mr. Latimer stated that Browns Point would be a good spot to use for the field tests but questioned how it would be possible to determine what type of failure occurred. It was concluded that from the soils viewpoint the investigation of the possibility of a flow failure should be continued and in addition the possibility of failure due to internal erosion should be investigated.