CORPS OF ENGINEERS, U. S. ARMY

MISSISSIPPI RIVER COMMISSION

POTAMOLOGY INVESTIGATIONS

Report no. 11-4

REPORT OF SECOND POTAMOLOGY CONFERENCE WITH HYDRAULICS CONSULTANTS

WATERWAYS EXPERIMENT STATION

VICKSBURG, MISSISSIPPI

23-24 MAY 1949

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General

1. The second potamology conference with special hydraulics consultants was held at the Waterways Experiment Station on 23-24 May 1949. The conference was concerned principally with the laboratory or model investigations of river meandering and revetment; however, results of a field investigation of turbulence forces in the Mississippi River and a review of the recent bank failure and levee crevasse at Free Nigger Point, just upstream from Baton Rouge, Louisiana, were also presented.

2. There were three sessions of the conference. Those present at one or more of the sessions were:

Consultants

Dr. Boris A. Bakhmeteff, Columbia University
Dr. Hunter Rouse, Iowa University
Dr. Lorenz G. Straub, University of Minnesota

Waterways Experiment Station

Lt. Col. Ralph D. King, Director
Mr. Joseph B. Tiffany, Technical Executive Assistant
Mr. Eugene P. Fortson, Jr., Hydraulics Division
Mr. George B. Fenwick, Rivers and Harbors Branch
Mr. Ernest B. Lipscomb, Potamology Section
Mr. William L. McInnis, Potamology Section
Mr. Robert G. Cox, Potamology Section
Mr. John F. Hand, Potamology Section
Mr. Robert G. Fletcher, Potamology Section
Mr. Eugene H. Woodman, Instrumentation Branch
Mr. Leiland M. Duke, Instrumentation Branch
Mr. Leo F. Ingram, Instrumentation Branch

Program

3. The conference program was as follows:

23 May

9:00 - 9:10 A.M.	Orientation - Mr. Fortson
9:10 - 9:30 A.M.	Introductory remarks - Lt. Col. King and Mr. Tiffany
9:30 - 10:30 A.M.	Inspection of potamology models - Mr. Lipscomb
10:30 - 12:00 Noon	Morning session on conference agenda - Mr. Fenwick
2:00 - 4:00 P.M.	Afternoon session on conference agenda - Mr. Fenwick

24 May

3:00 - 4:00 P.M.	Afternoon session on conference agenda - Mr. Fenwick

Agenda

4. The following items for discussion were on the conference agenda:

a. Model investigation of meandering. Development of erodible bank materials; correlation of model and prototype materials; model-prototype scale relationships; effects of model scale distortion; development of a meander model and its operating technique.

b. Model investigation of revetments. Model bed material; model-prototype scale relationships; correlation of model and prototype turbulence; factors affecting revetment stability; possible new or improved methods of stabilizing river banks; development of a revetment model and its operating technique.

c. Field investigation of turbulence forces. Methods used in measuring hydraulic forces; adequacy, interpretation, and application of basic data; adequacy of instruments; desirable future instrumentation program.

d. Bank failure at Free Nigger Point. Explanation of failure on basis of hydraulic concepts involving turbulence forces.

Resume of Conference

Orientation

5. The conference was opened at 9:00 a.m. on 23 May by Mr. Fortson who, after making the necessary introductions, explained the various arrangements and functions that had been planned for the visiting consultants.

6. Colonel King, the Director, extended a general welcome to the visiting consultants and expressed the interest that both General Feringa, President of the Mississippi River Commission, and he had in the potamology investigations. He stated that considerable preliminary work on these investigations had been accomplished in the past two years and he now thought the time was approaching when some definite results should be obtained. He mentioned that the recent bank failure and levee crevasse at Free Nigger Point would be discussed in the conference.

7. Mr. Tiffany then reviewed the history of the potamology investigations and stated that they were connected to one of the most important problems now under study by the Mississippi River Commission, that is, the stabilization of the banks of the Mississippi River. He said that three revetments currently were being investigated in the field, and that the model investigation program underway at the Waterways Experiment Station had progressed to the extent that construction of a meander model and a bank-stabilization or revetment flume now had been completed and were ready for operation. An inspection of these model facilities was made at the conclusion of Mr. Tiffany's presentation.

Discussion - Model investigation of meandering

8. Discussions on the potamology investigations were undertaken during the morning session of 23 May after the inspection of model facilities had been completed. The first session, which was confined to a discussion of the model investigation of meandering, was opened by Mr. Lipscomb who stated that nearly all work to date on this phase of the investigation had been directed toward the development of a material or materials which would be subject to erosion and transportation in models similar to those phenomena in the prototype river. He explained further that a crushed bituminous coal, graded to pass an 8-mesh sieve and be retained on a 16-mesh sieve, had been tentatively selected as the basic movable-bed material. Mr. Lipscomb pointed out that the crux of the present problem was the selection of a binder material which could be mixed with the crushed coal and placed in the model banks for the purpose of reproducing the rates of erosion of the nonhomogeneous prototype banks. For this purpose two small erodibility flumes are being used to determine the rates of bed movement and bank caving of various materials and binders from which factors of erodibility can be determined. Each flume is approximately 15 ft long and 1 ft wide and includes a 90-degree bend of uniform curvature, with provisions for molding the concave bank in erodible material. Mr. Lipscomb pointed out that some 30 to 40 different binder materials have been investigated in the flumes and the four which showed possibilities for use as a binder were cottonseed meal, shellac, flour, and castor oil. Tests conducted thus far indicated that the

cottonseed meal showed the best possibilities for future use. Mr. Lipscomb mentioned that cottonseed meal was now being used as the binder material in the banks of the meander model.

9. In discussing the meander model, which is built to linear-scale ratios of 1 to 400 horizontally and 1 to 100 vertically, Mr. Lipscomb stated that the Concordia-Scrubgrass Bend reach of the Mississippi River was selected for initial investigation inasmuch as it is an unstable reach which has had considerable bank changes in the last few years. Also, the banks in this reach of the river are composed of various types of materials and the development of the reach has not been affected by revetment or other man-made structures. The 1941 and 1948 surveys were the only complete surveys available at the time model operation was started, and therefore tests are being conducted to verify in the model changes occurring in the prototype during that period. Mr. Lipscomb then pointed out the difficulties encountered in trial-verification tests conducted thus far in the model. In each test an aggraded and braided channel had obtained because of the inability of the model to transport the bank material as fast as it was eroded. In addition, difficulty had been experienced in eroding the large convex bar on the main bend in the model. Mr. Lipscomb stated that, in order to overcome some of these difficulties, the water-surface slopes, discharges, and resistances of the banks at several locations (shown to be more resistant by soil borings) had been varied in the model and that the specific gravity of the model fluid had been increased in an effort to increase bed movement.

10. Dr. Straub pointed out that the banks in the river probably were composed largely of very fine material which when eroded would contribute a high percentage to be carried along in suspension. He stated that in

many rivers such as the Missouri and Mississippi there are conditions where the bank material that is eroded contains a large percentage of material deposited initially from suspension; as an example he mentioned that on the Missouri River the total load was of the order of 90 per cent in suspension and 10 per cent moving along the river bed. Thus it was possible that a large percentage of material being carried in the flume and along the model bed should be carried in suspension. Mr. Tiffany pointed out that this proportion of suspended load to bed load might be much lower on the middle and lower reaches of the Mississippi River. Dr. Straub estimated that even on the Mississippi River over 50 per cent of the total load that is transported continuously would be carried in suspension. Thus he thought that the percentage of caved material going into suspension in the flume and in the model probably did not correspond with the percentage of caved material that went into suspension in the prototype. Dr. Straub expressed his feeling that consideration should be given to the possibility of adding something like sawdust to the coal or basic material which would go off in suspension as the banks caved and thus avoid aggrading the model bed. He stated that the upper part of a caving bank is usually a fine material probably deposited by suspension, and when the bank caves a large percentage of this fine material is not carried along as bed load but as suspended load. He thought that this bulk material as simulated in the model banks might be accumulating in and aggrading the model channel. He suggested that it might be worthwhile to make a mechanical composition of the materials in the prototype banks. He believed that this would give an indication of the magnitude of the amount of material which would be transported along the stream bed.

11. Dr. Rouse stated that the slope might be increased to accelerate movement of material along the bottom, but Mr. Fenwick pointed out that the attack on the caving banks would probably increase also. Mr. Tiffany stated that it would be coincidence in the tests conducted thus far if the caving of the banks and movement of material along the bottom were in the same proportion. He thought that probably the caving of the banks in the model would increase more rapidly than the ability of the model flow to carry the eroded material away. Mr. Tiffany and Mr. Fenwick both were of the opinion that the model banks would have to be made more resistant to erosion.

12. Mr. Tiffany stated that the bars which build up in the river are almost entirely sand, but it was only at a cut-off, either natural or artificial, that any fine-grained material appeared. He thought that a natural river bank might have some cohesive quality which would tend to cause the overbank materials to be more resistant to erosion. Mr. Maxwell stated that the top 40 ft of overbank material usually was a very fine-grained clay which was quite hard and was the most erosion-resistant material along the river. Underlying this clay was sand, and Mr. Maxwell was of the opinion that this sand would tend to have a more rigid structure than the loose sands along the bottom of the river channel. He did not think that the presence of silt in sand would provide any additional strength against sloughing.

13. In reply to a question by Dr. Rouse, Mr. Maxwell stated that erodibility factors for the various prototype bank materials at Concordia-Scrubgrass Bend had not been established from the soil borings; however, other than the clay plugs, he did not think there were any significant variations in the bank materials. Mr. Maxwell also stated that the

mechanical composition of the materials on the large convex point bar, which Mr. Tiffany believed to be composed of sand, had not been determined. Dr. Straub stated that he thought the bar material was not the kind of sand ordinarily transported along the river but rather was a material deposited primarily from suspension. He thought the bar was in a region of low turbulence where deposition from suspension would take place and that, in the model, more difficulty would be encountered in simulating conditions of suspension than in simulating movement by so-called tractive force. He stated further that there was always a migration of suspended material in the river from points of high turbulence to points of low turbulence, and he thought the point bar was in a region of low turbulence. Thus in the meander model, one of the difficulties was that the bed material, not being in suspension, could not get over to the places of low turbulence and be deposited. Dr. Straub suggested that it might be interesting to determine the mechanical composition of the bar during a low-water stage or after the river has been in a condition of equilibrium for some time, so that the nature of the bottom would be mostly the result of the immediate preceding history. He thought that there would be quite a variation in composition of the material not only as to mean size but also as to size distribution out in the river channel. In addition, he thought it probable that material that moved in suspension exerted considerable influence upon the conformation of the river in that locality. In reply to a question from Mr. Fenwick relative to the procedure to use in determining the mechanical composition of the river materials, Dr. Straub suggested taking bottom samples about every 100 ft across the channel.

14. Dr. Bakhmeteff stated, that all through the discussion he was giving thought to the very impressive fact, that the model was unable to erode the bar on the outward bank of the curve. There cannot be any question, that the natural forces in the model are operating different from the way they act in the prototype. No matter how important otherwise, the composition of the bar material could not be the cause of this fundamental difference. He felt that there was a basic hydraulic process involved, which so far has not been fully understood, and a proper grasp of which may probably furnish the answer to the difficulties encountered in the Concordia-Scrubgrass Bend model and throw light on the problem of meander studies in general. For the moment, Dr. Bakhmeteff could not be more specific but hoped to return to the question later.

15. Dr. Rouse mentioned that the uniformity of the model material (coal) disturbed him, inasmuch as he did not believe that it was possible for the model to simulate the actual possibilities of the river to move material from one side to another under such conditions. He thought that there should be a gradation of materials from coarse to fine, and that the sorting or grading of material in operation of the model would then more or less follow the prototype pattern. He believed that to overcome some of these difficulties it would be necessary to have an undistorted model. Dr. Bakhmeteff also agreed that this process of selective deposition could not be reproduced in the model without using a graded material. He felt, however, that the most important feature to reproduce was a similar hydraulic pattern. Dr. Straub stated that the use of an ungraded material meant that movement would occur under certain conditions but that as soon as a certain critical point was reached movement would cease; such a condition

would not be true in the prototype since there is a movement taking place in all areas consisting of deposition in some places and erosion in other locations.

16. Colonel King stated that before developing a model technique he thought it necessary to arrive at a better understanding of what takes place in the river. He described his understanding of the caving of banks and building of bars in the river as a general movement of material along and immediately adjacent to the bottom as influenced by the velocity. He visualized the building up of a bar on the inside of a bend also as a process where material was moved along the bottom by the current, and he did not believe that any considerable gradation would result therefrom. At this point Dr. Straub stated that he thought the conformation of a river section resulted both from movement of materials along the bottom and deposition of material from suspension. He thought that there would be a gradation of material in the river, and that around a bend such a process would exert a very important effect. Depending on the flow pattern and kind of material, he thought that a concentration of coarse sizes could be obtained several feet above the river bottom.

17. Mr. Fenwick inquired as to the possibility of adding a percentage of lighter weight material to the coal without actually adding any fines. Dr. Rouse replied that he thought the model bed material should have the same specific gravity throughout from coarse to fine. Dr. Straub stated that control of the gradation process would be rather difficult. This in turn, of course, would influence the transportability characteristics of the material since the addition of fines or other graded sizes would result in different types of movement. The addition of fines would tend to provide a stabilizing influence to the bed material.

18. Dr. Straub thought that there are two problems involved, namely, the bulk problem and the sorting problem. The bulk problem is caused by the caving banks in the model adding more material to the model stream than the model can transport, resulting in a braided river condition. The sorting problem is caused by the fact that sedimentation is not occurring in the model in areas of low turbulence or velocity as it probably does in the prototype. He believed that the bulk problem might be solved by using sawdust in the caving banks; as the banks eroded the sawdust would float away probably leaving a very small residue. He suggested determining the percentage of prototype bank material finer than some arbitrary value such as 1/16 to 1/20 mm and replacing this percentage in the model banks with sawdust. Thus, he thought, the bank material which normally moves by suspension in the prototype might be removed from the model banks during operation.

19. Mr. Tiffany brought up the question as to just what proportion of the total river load was contributed by a locally caving bank. He thought that if this proportion was rather small it might not be necessary to attempt to reproduce the local caving in the model by means of a bulk material such as sawdust. Dr. Straub suggested making some quantitative studies to determine how much volume goes into the river as a result of bank caving and then making an analytical investigation as to the relationship between the amount of suspended material carried in the river and the amount of material carried along the bed. He thought that within certain ranges the order of magnitude of material carried along the bed could be measured. The combination of the two then could be compared to the volume of bank caving. In reply to a question by Mr. Tiffany, Dr. Straub estimated that in the

Mississippi River there were on the average 1,500,000 tons of suspended material per day being transported; he estimated the material moving along the bed to be about 20 to 30 per cent (300,000 - 450,000 tons per day) of the suspended load. Mr. Tiffany thought that a figure of 200,000 to 300,000 tons per day of bed material might be very great in comparison with the amount of material contributed by a locally caving bank.

20. Dr. Straub suggested that it might be desirable to determine the ratio of the rate of bank caving at a particular locality to a rational estimate of the total bed load of the stream and then compare with the corresponding figures obtained in the model. He thought that there should be a reasonable relationship between the volume of bank caving and total bed load obtained, respectively, in the model and in the prototype, and he believed that a quantitative calculation of the order of magnitude would help clarify these points. This would tie in with the development of a model technique. Mr. Tiffany explained that it was planned to make the model banks more resistant to erosion in order to get a proper ratio between the rate of bank caving and the rate of movement of material along the bed.

21. Colonel King remarked that he did not think the relation of the amount of bank caving to the total bed load was too important. He pictured normal bank caving as being a very gradual process confined to fairly small sections maybe 3 or 4 ft wide and 10 to 20 ft long, and he thought the effect of the caving extended over a very limited area without affecting appreciably the bed load along the bottom of the stream. Colonel King also thought that there should be a greater variation in sizes of the model material not to reproduce particularly the silt, but to reproduce any cementing action in the prototype sand that might be due to greater

distribution of grain sizes. He suggested that possibly one reason for obtaining a braided model channel was because of the inability of the coal to stand on the required slope. Mr. Lipscomb pointed out that because of the model distortion the bank slopes were much steeper than the natural angle of repose of the coal and that, as soon as operation of the model was started, sloughing of the banks occurred thereby overloading the channel.

22. Mr. Fenwick brought up the question of the effects of distortion on the model other than producing steeper bank slopes. He desired to know if some distortion was permissible in a model of this type. Dr. Bakhmeteff stated that centrifugal forces were greatly altered by distortion, and therefore the correct patterns for the distribution of velocities and secondary currents would not obtain. He suggested making a field investigation of the distribution of surface velocities both on the inner and outer banks in order to determine the general features of the flow pattern. Knowing the distribution of these prototype surface velocities, he stated that he would then try to vary the slope and other possible agencies until he got a similar velocity distribution in the model. Dr. Rouse thought that a change in the model slope would produce equal changes in the velocity components.

23. Mr. Fenwick then inquired as to the correctness of using the verification procedure as a basis for similitude in this type of model. Also, Mr. Tiffany inquired about accepting a general overall verification for projecting model indications into the future. As stated by him, the problem was one of trying to predict where and at what rate banks might cave or bars build, whether in effect a revetment was needed or whether the river would

change its course in 6 months or a year and cause the revetment to become inactive, and whether the bank caving was at such a rate as to require a levee set-back. Mr. Fortson mentioned the Greenville Bridge model as being indicative of another type of study engaged in by the Waterways Experiment Station in which the future effects of changes in the river were of paramount importance.

24. Dr. Rouse replied that if the verification was generally accurate he thought it could be reasonably accepted. Dr. Bakhmeteff emphasized the significance of Mr. Tiffany's statement in regard to the general aim of model work and verification procedure. The task, as now envisaged, seems to extend far beyond the overall verification previously required of model studies. If he grasped the meaning correctly, the aim at present is to work out a model technique and procedure by which the operation of natural forces in a watercourse could be reproduced in greater detail, so that their effect on different aspects of the river regimen could be reliably foretold and practical questions answered in advance. In this light verification means checking similitude in the various principal prototype occurrences. Naturally if model technique were able to reasonably reproduce and verify a prototype regimen under certain conditions, then essentially comparable results could be expected to obtain for another situation of the same type. Colonel King stated that he thought a model that had been verified could be depended upon to predict the effects of minor changes but could not be relied upon to indicate effects of major alterations much greater than any which occurred during the verification. It was generally agreed, however, that such indications of the effects of major changes in the river would be more dependable than mere judgment.

25. This concluded the discussion on the model investigation of meandering. The next item presented for discussion was the model investigation of revetment.

Discussion - Model investigation of revetment

26. Mr. Lipscomb then presented a description of the bank-stabilization flume in which it was planned to conduct the model investigation of revetment. He stated that the flume was approximately 600 ft long, 125 ft wide, and from 2.5 to 3.5 ft deep and that it was planned to install the Reid Bedford Bend reach of the Mississippi River therein to an undistorted linear-scale ratio of 1 to 50. It was pointed out by Mr. Fenwick that the study was not concerned primarily with any specific problem at Reid Bedford Bend, but that this reach had been selected as being most suitable for testing various types of revetment in the flume. Mr. Lipscomb stated that revetment was placed in this reach (prototype) in October and November of 1946 and two major failures occurred during the first two months of 1947. The revetment consisted of articulated concrete mattresses along the subaqueous portion and mass sand-asphalt mixture on the upper-bank paving. He explained that actual testing in the model had not been undertaken, but that it was planned to verify the revetment flume for the period of one year (August 1945 to August 1946) preceding the laying of the revetment before undertaking a study of the revetment failures. Mr. Lipscomb then presented the results of some preliminary tests which had been conducted in a small experimental revetment flume with articulated concrete mattresses, concrete blocks, and mass sand-asphalt mixtures. Colonel King explained that the mass sand-asphalt mixture now being used in the prototype contains a maximum of about 11 per cent asphalt with the remainder being river sand. He stated that it has been

used mainly for maintenance and repair of revetment, but that it would be desirable if practicable to develop it to replace the articulated concrete mattress.

27. Inasmuch as the model testing program for the revetment investigation was in the preliminary stages and therefore no operational difficulties had as yet been encountered, there was no further discussion of this phase of the potamology investigations.

Discussion - Field investigation

28. The next item on the agenda presented for discussion was river turbulence data obtained at the Reid Bedford Bend revetment during the period March-April 1949. Mr. Cox of the Potamology Section opened the discussion by describing the equipment and methods used during the test period. Various lantern slides illustrating test conditions were projected on a screen to familiarize the consultants with the conditions under which these data were obtained. There followed a graphical presentation of data, which Mr. Cox interpreted and explained. The following pertinent points affecting the data were noted:

a. That these data were obtained during a falling river stage toward the end of a high-water period.

b. That the relatively low turbulence values and pressure fluctuations obtained would have been much higher had the measurements been made on a rising stage.

c. That physical conditions limited measurement of the forces of turbulence to comparatively shallow depths (20 to 25 ft) with distribution of the equipment parallel rather than transverse to the line of flow.

d. That interpretation of the velocity data in terms of turbulence was accomplished through statistical analysis in which 30-second average velocities were used.

e. That pressure fluctuations were the maximum and minimum values occurring within 15-second time periods.

f. That in most cases the duration of each test was two hours.

g. That all data showed pulsations of the river which were in alternating cycles of 15 to 30 minutes, the shorter pulsation lasting 15 to 20 minutes and the longer pulsation lasting 25 to 30 minutes.

h. That day-by-day observations at the site showed wide variations in the flow conditions, one day being relatively calm and the following day being relatively turbulent with the location of main current varying some hundreds of feet transversely.

i. That mean velocities measured were about 4.5 ft per second with extremes from 3.0 to 7.5 ft per second; standard deviations varied from 0.416 to 0.756; ratios of turbulence intensity varied from 9.3 to 13.7 per cent; and pressure fluctuations based on velocity measurements varied from 0.09 to 0.20 lbs per square inch.

j. That the uplift pressure on the cell blocks reached a maximum of 0.11 lbs per square inch, which was within 0.04 lbs per square inch of that required to lift revetment, and that in no instance did the pressure tend to aid in stabilization of the revetment.

k. That pressure fluctuations on the surface of pressure-variation cell blocks were found to be as high as 0.15 lbs per square inch.

29. A general discussion of these data followed in which the consultants expressed their ideas and opinions. Dr. Bakhmeteff expressed surprise that the observed values of turbulence were so relatively low. Mr. Tiffany explained that while the observed velocities were low, 4 to 5 ft per second, that during higher stages velocities of 9 to 12 ft per second magnitude were known to exist along the Reid Bedford Bend revetment and that therefore had the measurements been taken during a higher stage undoubtedly the pressure fluctuations would have been considerably greater. In connection with this thought Dr. Bakhmeteff remarked that if the velocities doubled and the standard deviation value remained the same the pressure fluctuations could be expected to increase four times. He further felt that the observed values were extremely significant in as much as they were obtained during a period of known low turbulence. In discussing the long and short turbulence cycles noted in the previous paragraph, Dr. Bakhmeteff remarked that the correlation length of turbulence is commensurate with the depth of the stream and that the vortical units responsible for the observed periods of turbulence or pulsations were engendered some distance upstream and passed down river in periodic surges. Dr. Rouse questioned the use of the word turbulence to describe this type of flow. Dr. Bakhmeteff said that was a matter of definition. The source of all turbulent phenomena were the eddies, engendered in the boundary zones, and the long period pulsations are set up by very large eddies having zones of influence commensurate to the depth of the stream. In reply to a question by Dr. Straub concerning the possible correlation of velocity meters in tandem horizontally, Dr. Bakhmeteff said that the features of this were rather complex and that the mechanics of the situation were not well known.

30. In the discussion on direct measurement of pressure fluctuation by means of pressure cells cast in concrete blocks, which in turn were placed on the submerged revetment, Colonel King raised the question as to why the pressure on the bottom of the revetment should vary. It was explained that, whereas long-period pressure fluctuations may permit equalization of pressures under the revetment, very short-period pressure fluctuations would set up differential pressures between the top and underside of the revetment, depending upon the medium of communication between the two surfaces. Dr. Bakhmeteff stated that if the underside of the slab had little or no communication with the outside flow, the under pressure would be fairly constant. On the other hand if a cell block were to rest more or less freely on the revetment the pressure transfer to the underside was easily accomplished particularly for fairly long period pressure changes. However, even in this case for very short cycles of pressure changes the medium of communication would not be able to transfer pressure changes rapidly enough to reduce instantaneous pressure differentials. In regard to a revetment which rests flat against a bank where the pressure transfer is not readily accomplished, the lifting effect may be annulled by the suction which develops in conjunction with the vertical displacement of the revetment prompted by the lift. However, should a pocket develop under the revetment which communicates with a constant-pressure area, the lifting effect would become fully operative and a dangerous condition would obtain.

31. Concerning the fact that the observed pressure differentials showed a force always operating against revetment stability, Dr. Straub felt that this was due to the fact that the cell block was not an integral part of the revetment, that a surface of discontinuity existed which created a

zone of influence caused by the streamline over the block, and that this zone of influence would always be in the direction of lower pressure. He felt that, while this zone of influence would prevail over all the cell blocks, it would only be effectively operative in terms of differential pressures between the two surfaces. The zone of influence would exist over a pressure variation cell block, but it would be noneffective due to the fact that an absolute rather than differential pressure was measured.

32. In the discussion concerning the value of the results of the data obtained to date and the advisability of extending the investigation to cover a period when more critical stages would prevail, Dr. Straub stated that such information would greatly contribute towards understanding what influence hydraulics have on revetments, either positively or negatively, and to what degree turbulence influences the failure of revetments. He also felt that some very useful factual data would be obtained by taking sediment samples in conjunction with the velocity measurements to tie in on a large scale the theory of suspended sediment transportation with the whole picture. Concerning future operations, Dr. Rouse stated that the longitudinal zone of pressure influence could be determined from a single meter, but that it would be necessary to space meters laterally to determine the transverse extent of the zone of influence. It was generally agreed that lateral dispersion and the obtainment of measurement at the thalweg were most desirable. In reference to a question by Mr. Lipscomb concerning the desirability of using both pressure cells and velocity meters in future operations, Dr. Bakhmeteff stated that if a choice between the two methods were to be made, the pressure cells were more important from a practical point of view as they were a more direct method of answering the practical questions raised. He also felt that

the use of a pressure variation cell was more significant than the use of a pressure differential cell because they measured absolute values. Dr. Rouse contended that the velocity data could be used in many different ways and that they should be obtained. It was generally agreed that all three types of measurements should be made, as each has its own specific values and purposes. It was also agreed that future plans and equipment should be flexible enough to permit movement from one location to another. Dr. Bakhmeteff pointed out that local acceleration will reduce the standard deviation whereas local divergence of flow would probably result in an increase, and that selection of test sites on this basis could be most important.

33. In connection with future operations Dr. Bakhmeteff felt that the size of the pressure cell blocks should be increased and the edges curved in order to eliminate the effect of surface separation. He pointed out that although malalignment existed in the revetment, the expanse was such that the effect was uncomparable to the angular edge of the short cell block. Dr. Rouse was also of the opinion that the effect of surface separation must be eliminated. Dr. Straub felt that in the case of a differential cell block that effects of the zone of separation could easily be greater than the effects of turbulence.

34. To investigate the magnitude of the effects of surface separation on differential pressures obtained during the test period, Dr. Straub suggested theoretical calculations for this effect and the reduction of the observed values by the computed quantities to correlate the differential and pressure variation cell blocks. Dr. Rouse felt that such a computation would be difficult, and Mr. Tiffany suggested putting the cell block in a flume for physical determination of the effects of the surface separation and then

applying this correction factor to the test data. Dr. Straub felt that a reasonable answer could be obtained by such a method.

35. Dr. Bakhmeteff was of the opinion that the zone of influence over which the uplift pressures acted simultaneously could be determined only by trial; that is, one should set down a number of cells at predetermined distances and then change their positions until the zone of influence was defined. In connection with the possible zone of influence of underpressure, a discussion of the size, extent, and the mechanics of fabrication and placement of revetment mattresses ensued. Dr. Straub felt that if a zone of pressure influence simultaneously affected a transverse area of 30 ft or more over a mattress, it could have considerable influence in pulling the mattress off the bank.

36. Mr. Fortson brought the afternoon session to a close by summarizing the deficiencies of the present turbulence investigation as follows:

a. The magnitudes of turbulence values in the test area were surprisingly low.

b. Currently available data are insufficient to establish the entire picture.

c. Transverse data are desirable as well as longitudinal data.

d. The shape and size of the cell blocks resulted in a zone of separation.

Dr. Straub added one more item to the above list: failure to obtain measurements in deep water. Mr. Fortson adjourned the meeting with the remark that the discussion would be continued at the next session.

37. Discussion of the forces of turbulence as determined by field measurements was resumed at the afternoon session of 24 May to obtain the specific recommendations of the consultants relative to future operations in this field. Dr. Bakhmeteff reviewed the plans developed at the 1948 December conference and set up four important factors to be considered in future operations.

a. Based on results obtained to date the obtaining of velocity measurements at the toe of the revetment is mandatory. Since turbulence energy is proportionate to the energy absorbed, and since the latter is directly proportional to the depth, the energy actuating turbulent manifestations at a 90-ft depth would be about three times the energy revealed at a 30-ft depth. Therefore, the measurements made at a 24-ft depth were not indicative of maximum values, not withstanding the fact that fairly critical pressure differentials were obtained. The difficulty involved in putting pressure cells in extreme depths could necessitate resorting to velocity meters.

b. The elimination of the surface discontinuity of the cell blocks by curving the edges and enlarging the blocks to 36 inches with 18 inches of flat surface on the top sides is desirable. Also, the rapid transition of pressure from the surface to the underside of the blocks resting on the revetment decreases the importance of the differential pressures measured by this means. Therefore, the use of pressure-variation cell blocks is more important inasmuch as they reflect absolute pressure variations. Furthermore, every effort should be made to get a cell block down to the thalweg, with additional cells spaced transversely up the bank with at least 3 in 100 ft. The latter is important due to the dimensions and shape of the mattress. Longitudinal pressure variations could be determined from the time graphs of transversely spaced cell blocks or velocity meters.

c. Although the long-phase motion of the river causes pressure fluctuations over the revetment, the higher-frequency pressures appear to be of greater possible influence than originally thought.

d. The continuation of velocity measurements is important as a main source of information. The correlation of pressure cells and velocities at one point could provide a basis for interpretation of velocities in terms of pressures at other points.

38. Dr. Straub emphasized the fact that lateral in-phase and out-of-phase fluctuations were most important, and that while it would be desirable to obtain this information by use of pressure cells, the use of velocity meters for this purpose would be more expedient. He therefore suggested the use of velocity meters transversely over the revetment from the thalweg shoreward up the revetment at intervals of 30-50 ft. At this point Dr. Bakhmeteff pointed out that velocity meters in the vertical at the thalweg would be most desirable in conjunction with lateral dispersion. Dr. Straub continued that, in his opinion, the long-cycle variations were not important, that perhaps data obtained from such accessible places as the Vicksburg Bridge could be transferred to critical areas, and that the effects of the shape of cell blocks on differential pressures might be determined experimentally through a flume study. Dr. Rouse felt that the effects of the surface discontinuity resulting from the shape of the cell block would be relatively unimportant when viewed in light of the possible tilt of the blocks when actually laid on the revetment. However, he favored

the idea of point-supporting the lower edge of one cell block to simulate the free edge of the revetment. Mr. Tiffany favored continuation of the use of differential cell blocks as these resulted in data more easily understood by individuals not familiar with the mechanics of turbulence.

39. It was the unanimous opinion of Drs. Bakhmeteff, Rouse, and Straub that the present method of laying mattresses where surface discontinuities are established at the thalweg gives rise to dynamic forces which may reach, under certain conditions, dangerous proportions. It is necessary therefore to make the mattress heavy enough to resist such forces. To appraise the order of magnitudes of these forces, and consequently the required weight of mattress, velocity and pressure measurements at the toe of the mattress should be made, at least during the lower stages. Mr. Tiffany explained that during low stages the revetment toe is covered with sand and becomes exposed on early rising stages. He further remarked that scour in a river bend after the placement of revetment is not due entirely to the revetment, but also to natural stages and currents. Dr. Bakhmeteff supported Mr. Tiffany's statement with the remark that all river experimental work has shown that rapid changes take place during rising stages.

40. In summary, it was concluded that instantaneous velocity measurements over one-hour periods were necessary, that measurement of velocities as close to the thalweg as possible was of paramount importance, that a time correlation on the present velocity data was desirable to indicate the longitudinal scale, and that the development of a pressure-cell velocity meter was desirable even if not indispensable.

41. Mr. Tiffany brought the discussion of river turbulence data to a close by stating that the instrumentation program for Fiscal Year 1950 would be prepared and submitted to the consultants for their consideration.

Discussion - Free Nigger Point failure

42. The next item on the agenda was a discussion of the recent bank failure on the right bank of the river at Free Nigger Point one mile above Baton Rouge, Louisiana. The purpose of the discussion was to obtain the opinions of the consultants relative to the hydraulic aspects of the failure and to get ideas paramount to the prevention of such failures. Mr. Tiffany opened the discussion by describing bank and river conditions preceding the failure and those found to exist immediately after the failure. He stated that the failure occurred in a very sharp bend in the river which had been fairly stable for many years, that a very deep hole had been known to exist in the bed of the river at the point of failure for a number of years, and that very large twin eddies had existed, one on each side of the river, for the past few years just upstream from the point of failure. It was noted that the top bank line began to show an indentation to the right adjacent to the upstream limits of the failure and that the bottom contours receded much more sharply to the right than the top bank line, making almost a 90-degree angle at that point. Thus, although the top bank line did not indicate a particularly sharp break in alignment nor an appreciable protuberance into the river, the bottom contours indicated a very marked subsurface protuberance. The bank slope at the point of protuberance prior to failure was about 1 on 0.87. During the night of 24 March, 1100 ft of foreshore suddenly and without warning slid into the river, breaching the main line levee for a width of

about 200 ft. Computations made from before and after surveys of the failure site showed that approximately 9,000,000 cu yds of material moved into the river during the failure. At the time of failure, the river was about four feet out of banks and was on a slowly falling stage, having peaked several feet higher. Closure of the crevasse was effected 67 hours after the failure. A soils investigation made after the failure indicated that the slide may have been of the flow type and that a slide of this magnitude would probably not have occurred but for the geological fact that a sharp line of demarkation between the resistant backswamp deposits and the easily erodible point-bar deposits existed approximately along the upstream limits of the failure. Mr. Tiffany then presented a hypothetical analysis of the hydraulic conditions believed to have existed prior to the failure which probably resulted in conditions conducive to the slide. He stated that, from past experience gained by observation of turbulence in models resulting from similar underwater topography, he believed that a vigorous clockwise eddy with erosive velocities must have existed, before the failure, at and near the bottom of the river at the upstream limits of the failure. He also believed that this subsurface eddy in the horizontal plane probably was accompanied and complicated by an equally intensive counter-clockwise (looking downstream) roller, with very high upward velocities existing along the steep bank. The net result of these conditions probably was an intense scouring of material from the toe of the steep slope, movement of material up the steep bank, and additional scouring of material from the bank itself by the upward-directed currents. He presented a map and cross section showing the hypothetical hydraulic conditions, and observed that this hypothesis was supported by observations made on a distorted (20 to 1) model of that reach of the river.

43. Drs. Rouse, Bakhmeteff, and Straub concurred with Mr. Tiffany in the probability of his hypothetical analysis of the hydraulic conditions existing in the river prior to the failure. Dr. Rouse cited three hydraulic conditions which undoubtedly existed at the same time: (1) a topographical pocket separation producing a discontinuity in surface, (2) an eddy, resulting in a means of moving material, and (3) a tendency of the upper surface water to travel out, with the lower surface water moving in, thus producing most scour at the bottom and up the side of the bank. Dr. Bakhmeteff stated that in comparatively wide, shallow rivers like the Mississippi River the velocity distribution is considerably different from that in rivers of greater depth-width ratios, but that very little is known on the mechanics of the former and that a general investigation was needed to supply information on that matter. He pointed out that Free Nigger Point was a case where the main direction of current and distribution of velocity of a comparatively wide and shallow river would result in a tendency for the inner bend to collapse, that this case showed how the relative depth or changes therein would change the flow pattern in the section, and that in view of the configuration and the great depth, the flow pattern in the section was abnormal. He recommended a model study to determine how the flow pattern and velocity distribution changes with change in configuration and suggested a fixed bed study followed by a movable-bed study to determine redistribution of velocity on changing parameter of depth to width.* Dr. Straub concurred with Drs. Rouse

* In reviewing a draft of the report Dr. Bakhmeteff elaborated his thoughts on the foregoing. This dissertation is included as an addendum to this report.

and Bakhmeteff and suggested the possibility that the failure was of the shear type. Mr. Maxwell of the Soils Division explained the characteristics of the slide and pointed out that the slope was too flat and the extent too great to result from shear failure. He explained the theory that the slide was caused primarily by a trapped pocket of more or less fluid material being set free by erosion and, or, a minor shear slide. From the general discussion it was concluded that the prevailing hydraulic forces set up the conditions which produced the slide either by erosion followed by a minor shear slide which exposed the fluid pocket or directly by shock resulting from rapid hydraulic pressure changes. It was further concluded that by studying similar critical locations by the use of undistorted models the hydraulic factors affecting similar types of failure could be evaluated, and by such a process the probability of such future occurrences could be determined through qualitative analyses.

Addendum

Following a review of the initial draft of the report of the second conference on potamology, Dr. Bakhmeteff wrote a dissertation on his thoughts as set forth in paragraph 43 of the report. This dissertation follows:

"The occurrences at the Free Nigger Point brought to a conclusion thoughts that had been maturing in my mind all through the conference and which bear directly on the Concordia-Scrubgrass case as well as on meandering studies in general. The outstanding fact at Free Nigger Point is that the erosive powers of the river are concentrated on the inside projecting bend of the curve, contrary to the customary rule where caving takes place along the outward bank of a curve. The cause should be sought in the extremely sharp curvature of the inner bank and in the comparatively reduced width of the river in the bend in relation to the depth.

"As I see things at present, the fundamental issue in most of the problems considered at the potamology conferences is the flow regimen in the curved portions of a natural watercourse. The theoretical solution in an ideal fluid is a pattern, having the highest local velocity at the inner part of the curve and the lowest velocity at the outer part, and with the velocities in between interrelated with the radii of curvature through $ur =$ constant. Experiment shows that such theoretical distribution is more or less approximated in the inner portions of curved pipes and closed ducts, whenever the so called 'aspect ratio', or the quotient of the height of the cross section at right angles to the plane of curvature to its width in the plane of curvature, is large, so that the flow in the inner regions is more or less two-dimensional. Experiments indicate at the same time, that the theoretical regimen is greatly influenced and eventually totally deformed by friction. In fact friction exercises a potent effect not only in the boundary layer zone near the solid confines, but also throughout the central regions in the plane of curvature, it being known that as the aspect ratio of a cross section is reduced, the high velocity filaments are displaced from the inner towards the outer parts of the curve.

"Indeed, together with 'relative curvature' or the ratio of the width of the conduit to the mean radius of curvature, the aspect ratio of a cross section appears to play a decisive role in the shaping of the pattern. Thus the intensity and the 'spread' of the secondary transverse currents, which lend the spiral features to the flow are apparently strongly influenced by the shape of a cross section. The same may be said about 'separation', which occurs usually on the downstream part of the inward curve, but occasionally may unfold also in the upstream range of the outer boundary of a bend. Separation, as a source of intense turbulence, is not only principally responsible for the energy losses, but is instrumental also in shaping the overall flow pattern in a bend. Thus separation causes flow in the downstream portion of the curve to be shifted over towards the outer side of the bend, and this even in ducts with a comparatively high aspect ratio, where initially the velocity distribution tends to approximate the ideal velocity pattern.

"Notwithstanding the abundance of experimental data on flow in closed curved conduits, the complexity of the case has prevented so far reaching a wholly clear and satisfactory understanding of the phenomena. Thus even such assumedly simple features as the overall resistance coefficients in a curved pipe, appear to vary in different observation series by nearly 500 per cent.

" By comparison to closed conduits, flow in bends of open channels and particularly in natural watercourses, is far more complicated, and factual data, as reported in literature and based on reliable observations, are very meager. Moreover most of the experimental evidence was obtained in laboratories with relatively deep and narrow channels. I am not aware of any systematic and comprehensive investigations relating to flow in curved portions of large rivers.

"Obviously in a curved open channel the centrifugal forces cause a rising of level on the outer side of a curve and a lowering on the inner border. It is generally accepted further that this difference of levels produces a secondary transverse current along the bottom of the channel from the outer towards the inner parts of the bend. There is sufficient evidence, however, to assume that in a very wide river, where the depth is but a small part of the width, these cross currents and the ensuing spiral flow are negligible.

"A principal object of interest in curved open surface flow is the velocity distribution in the plane of curvature. Similar to the case of closed conduits, if the channel is relatively deep and narrow so that the aspect ratio is high, the pattern tends toward the theoretical distribution with the high velocities on the inward side of the bend. On the other hand, in rivers with a low depth-width ratio, the current is known to be drawn towards the outer bend, which explains the customary configuration of the thalweg and the usual caving of the outer banks. The resulting pattern is obviously a matter of interreaction between the frictional agencies and the inertial forces, engendered by curvature. One may in the present state of knowledge, cite examples typifying that or other extreme cases. However, reliable knowledge, enabling quantitative appraisals and rational forecasting, is not available.

"In regard to the cases considered during the potamology conferences, it would seem, on the face of reported facts, that the Free Nigger Point with the caving of the inner bank, and the Concordia-Scrubgrass Bend with the customary working of the river on the outward side of the curve, offer two opposite possible examples of the overall pattern. It may be further surmised that the scale distortion in the Concordia-Scrubgrass model caused a change in the width to depth quotient (aspect ratio) which was sufficient to shift the current away from the outer bank, so that the intense caving action taking place in the prototype ceased to be reproduced in the model.

"Meandering phenomena in general and in most practical cases, where model techniques are resorted to for predicting with reasonable detail the workings of a river, are clearly related to the flow regimen in curves and bends. A comprehensive and promising solution of such problems is scarcely possible therefore, before the basic facts concerning curved flow in watercourses are known and clearly understood.

"I recommended, therefore, that systematic observation on curvilinear motion be carried out first in a fixed bed channel, and subsequently in a flume with a movable bed. It is essential to study both cases as the eventual deformation of a movable bed may substantially contribute to the final established regimen. The principal parameters to be varied within suitable limits should be the relative curvature and the aspect ratio. An important factor within limits, is relative roughness. Another feature suggested by observations on closed conduits, is the influence on the flow pattern in curves of the velocity distribution in the approaching streaming. The ideal theoretical pattern is based in fact on the premise of equal energy content in the different filaments. The role of friction in causing radical departures from the theoretical outlines may be attributed to an extent, to the grossly uneven distribution of energy in the flow as it approaches a bend.

"It would be most useful finally to have some sufficiently detailed observations of the actual velocity distribution in a few selected typical bends of the Mississippi River, especially if such observations would be repeated at various stages. The observed distributions could be then compared with the flow regimen in models, built on variously distorted scales."