CORPS OF ENGINEERS, U. S. ARMY

POTAMOLOGY INVESTIGATIONS

REPORT NO. 11-7

MINUTES OF CONFERENCE ON SOIL ASPECTS OF POTAMOLOGY PROGRAM

17-18 JUNE 1950

WATERWAYS EXPERIMENT STATION

VICKSBURG, MISSISSIPPI

OCTOBER 1950

POTAMOLOGY INVESTIGATIONS REPORTS

Issued Prior To and Including This Report

Report No.	Title	Date
1-1	Instructions and Outline for Potamology Investigations	November 1947
1-2	Outline of Plans for the Potamology Investigations	December 1947
2-1	Preliminary Flume Tests of Mississippi River Revetment (1st Interim Report)	October 1947
2-2	Preliminary Tests of Mississippi River Dikes, Bank Stabilization Model	June 1950
3-1	Preliminary Laboratory Tests of Sand-Asphalt Revetment	July 1948
4-1	Investigation of 110-Volt Echo Sounder	July 1948 (Revised May 1950)
5-1	Geological Investigation of Reid Bedford Bend Caving Banks, Mississippi River	July 1947
5-2	Field Investigation of Reid Bedford Bend Revetment, Mississippi River (3 volumes)	June 1948
5-3	Reid Bedford Bend, Mississippi River, Triaxial Tests on Sands	May 1950
5-4	Piezometer Observations at Reid Bedford Bend and Indicated Seepage Forces	May 1950
5-5	Standard Penetration Tests, Reid Bedford Bend, Mississippi River	May 1950
8-1	Hardscrabble Bend, Mississippi River, Revetted Bank Failure, Soils Investigation	June 1950
10-1	Preliminary Development of Instruments for the Measurement of Hydraulic Forces Acting in a Turbulent Stream	June 1948
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11-3	Minutes of the Conference on Soil Studies, Potamology Investigation	18 April 1949
11-4	Report of Second Potamology Conference With Hydraulics Consultants	23-24 May 1949
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MINUTES OF

CONFERENCE ON SOIL ASPECTS OF POTAMOLOGY PROGRAM

17-18 June 1950

PART I: PRESENTATION OF DATA

Introduction

1. A conference was held at the Waterways Experiment Station on 17 and 18 June 1950 on studies being conducted by the Embankment and Foundation Branch of the Soils Division in connection with the Potamology Investigations. Prior to the conference Professors A. Casagrande and D. W. Taylor, consultants, were apprised by memorandum of recent developments in the soils phase of the potamology investigations. A copy of this memorandum and of Professors Casagrande's and Taylor's replies are appended. The agenda which was followed at this conference is also appended. Those attending the conference were as follows:

Professor A. Casagrande, Harvard University
Professor D. W. Taylor, Massachusetts Institute of Technology
Major G. L. C. Scott, Executive Officer
Mr. J. B. Tiffany, Special Assistant
Mr. W. J. Turnbull, Chief, Soils Division
Mr. G. B. Fenwick, Chief, Rivers and Harbors Branch, Hydraulics Division
Mr. W. L. McInnis, Hydraulics Division
Mr. C. R. Foster, Chief, Flexible Pavement Branch, Soils Division
Mr. S. J. Johnson, Chief, Embankment and Foundation Branch, Soils Division
Mr. W. G. Shockley, Asst. Chief, Embankment and Foundation Branch
Mr. A. A. Maxwell, Embankment and Foundation Branch
Mr. J. E. Mitchell, Embankment and Foundation Branch
Mr. R. F. Reuss, Jr., Embankment and Foundation Branch
Mr. P. K. Garber, Embankment and Foundation Branch

The meeting was called to order by Major Scott at 9:15 a.m. on 17 June 1950.

He welcomed the consultants to the Waterways Experiment Station and then turned the meeting over to Mr. Turnbull, who made a few brief statements concerning the nature of the problem at hand and then asked Mr. Johnson to conduct the technical aspects of the meeting.

Review of approach to study of bank stability

2. Mr. Johnson presented a general outline of the soils phase of the potamology program to date. He pointed out that any study such as that concerned with bank stability could be approached in two ways: (1) the empirical approach, and (2) the rational approach. He pointed out that the Embankment and Foundation Branch was using both approaches in the Potamology Investigation. The empirical approach consists of field studies of locations where failures had occurred and studies at stable locations. From comparisons of stable and nonstable areas, attempts are being made to establish distinguishing characteristics of each type of area. The rational approach includes the empirical approach and in addition involves analyses made on a given bank to evaluate its stability on the basis of laboratory strength tests performed on the soils which compose the major portion of the bank.

Purpose of the conference

3. Mr. Johnson stated that the purpose of the conference was to review the work which had been done on the study since the last conference (October 1949) and on the basis of the results to obtain the opinion of the consultants as to the most logical and efficient course to follow in the future.

Empirical Approach

Review of investigational procedures

4. Mr. Johnson then asked Mr. Shockley to review the methods used in the empirical approach. Mr. Shockley stated that the general tools used in the empirical approach were the undisturbed sand sampling technique, the cone penetrometer, and geological surveys of the area. He pointed out that the first attempt at an empirical approach consisted of comparisons of the results obtained with these tools in areas where failures had occurred in the past and areas that apparently were stable. Attempts would be made to correlate the results of the cone penetrometer test with the densities of the sands obtained in the undisturbed sand sampling method, so that use of the latter could be reduced. If this were not possible, an effort would be made to calibrate the cone by means of simple tests, such as testing the cone in a tank of sand at a known density. The density and the grain-size distribution of the sand could be varied to obtain penetration resistances for a wide range of each. There is also a statistical approach to the problem; that is to say, the work which has been done in an attempt to evaluate the results on a statistical basis would be continued and expanded to include analyses by methods of multiple correlation.

5. Development of the geology of an area. Mr. Maxwell reviewed the manner in which the geology of a given area is investigated. He stated that the first step was to obtain aerial photographs of the area and examine them closely. Mr. Maxwell showed aerial photographs of typical point bar deposits and pointed out the general features of each. He also

showed aerial photographs of the other basic features in the valley. The failures that have been studied are confined to point bar deposits, but this is not a very limiting condition since a majority of the banks of the lower Mississippi River are in point bar deposits. However, in a number of cases, such as Morville and Free Nigger Point, large failures have occurred in the area prior to the time they became of interest. He then showed an aerial photograph wherein a large failure had occurred in a point bar deposit and stated that when aerial photographs of an area showed large scallops occurring in point bar deposits it was felt that the stability of any revetment placed in such an area would be very questionable.

6. Review of recent cone and density data. Mr. Maxwell then reviewed the recent work at Reid Bedford and Bauxippi-Wyanoke bends and presented results from cone penetration and density tests. He pointed out that as yet sufficient data had not been obtained to permit any correlation between the cone resistance and the natural density. He pointed out that no distinct difference had been found between the two areas, as far as density and/or penetration resistance was concerned. There were differences between the results of the individual penetration tests; in some cases there was a pronounced increase in penetration resistance with depth, while in other cases there was no apparent increase for a considerable depth. He stated that the cone penetrometer had been a very useful tool in determining the exact elevation at which it would be most desirable to take undisturbed sand samples. By making a cone penetration test prior to the undisturbed sample boring it had been possible to cut down the number of samples required in a boring from about 20 to 6 or 8. In spite of the fact that it was necessary to make a penetration test with the

boring, he believed this procedure would permit some reduction in the cost of the field program and would yield better information.

Proposed methods of
evaluating cone and density data

7. Mr. Shockley reviewed the proposed methods of evaluating cone and density data in some detail. He covered the comparison of results for stable and unstable areas as to characteristics of the sand, as far as grain size, shape, and other factors are concerned, along with the densities and penetration resistance. He pointed out that it was believed necessary to correlate the cone penetration resistance with depth (that is, with the effective overburden stresses), the properties of the sand, and some measure of density, either natural or relative.

Other investigational methods

8. Mr. Maxwell reviewed the possible geophysical methods which might be used to determine whether an area is stable or not, and pointed out the difficulties which would probably be encountered in the use of these methods as the reason for their not having been employed during this investigation.

Rational Approach

Review of field methods

9. A general review of the work accomplished on the rational approach since the last meeting was given by Mr. Johnson. He stated application of any rational analysis depended upon the use of data obtained in the field by the undisturbed sand sampling technique, and therefore field

methods compose a part of the rational approach. The remainder of the rational approach comprised the laboratory investigations of the sands in which the failures occur.

Review of laboratory work

10. Mr. Maxwell, in reviewing the laboratory work performed since the last meeting, pointed out that the original plan of the program had been to determine the critical density of several typical Mississippi River sands by means of triaxial, direct, and torsion shear tests. In order that the results of these tests might be as accurate as possible, it was decided that first an investigation should be made of the factors which influence the test results, as reviewed below:

a. Relative density tests. Since the comparative looseness or denseness of a sand at a given density can be expressed numerically by relative density (a relationship between maximum, minimum, and the given density) it is important that the maximum and minimum densities be determined by a method which is accurate and easily reproducible. At the time of the last conference there appeared to be some question as to the accuracy of the methods being used to determine maximum and minimum density. Accordingly, an investigation of several different methods was made which included variation in mold size, compactive effort, and a comparison of dry and saturated methods. From the results of this investigation it was concluded that the method which had been standard at the Waterways Experiment Station for some time was the most desirable. The minimum density test consists of pouring a dry sand into a 2-in.-diameter by 4-in.-high mold from a constant height. The maximum density test consists of compacting four 100-gm layers with 25 blows each of a 4-lb hammer falling 12 in. in the same mold. This method has the advantages of being accurate, easily reproduced, not difficult to perform and, for the limited amount of soil which normally is available for the test, gives the widest spread of maximum and minimum densities.

b. Placement methods for triaxial specimens. An investigation also was made of the methods used to prepare sand specimens for triaxial testing. The purpose of this investigation

was to determine the best method to be used in preparation of dense, medium, loose, and very loose specimens so as to obtain the most uniform density possible in each case. Triaxial specimens were prepared by several different methods. The specimens were then saturated, frozen, and cut into four horizontal segments. The density of each quarter was determined and the methods which gave the least variation in density ($\pm$ 1 lb) throughout the specimen were selected for future use.

c. Membrane tests. An investigation was made of the effect of the rubber membrane used on the triaxial sand specimens. This was investigated by first measuring the stress deformation characteristics of the membrane itself; the membrane had been marked with a system of grid lines to determine the deformation between given points during loading. The membrane was then used on a dry sand specimen which was deformed at a constant lateral pressure. The deformation of the specimen and membrane was obtained at various load increments by measurements taken between grid lines on a photographic negative of the specimen. Distortion of the specimen dimensions on the plane surface of the photographic negative had been computed prior to testing and was checked by actual measurements during the test. All measured values were corrected for this distortion. Computation of membrane restraint, based on the deformation, indicated that the actual average increase in lateral pressure caused by the membrane was in the order of 0.1 to 0.2 psi at 10 per cent axial strain. Tests were performed with 1, 2, 3, and 4 membranes on a specimen of medium density, and at lateral pressures equal to 25 in. of vacuum. The deviator stress had a tendency to increase with increasing number of membranes but it was not consistent. This was explained by the fact that the variation of deviator stress due to the increase in the number of membranes was less than the accuracy of the apparatus used for these tests. Other series of tests were performed, including three tests at each of the four densities which had previously been selected; two confining pressures of 25 in. of mercury and one of 10 in. of mercury, all with one membrane. The vacuum was removed at a given amount of axial strain (5, 10, or 15 per cent) and the sample, supported only by the membrane, was then tested to 20 per cent strain. The load carried by the specimen was due to the confining pressure caused by the membrane and was in the order of 0.6 to 0.8 psi at 10 per cent axial strain, which was what could be expected from the computed membrane restraint, since the principal stress ratio $(\bar{\sigma}_1/\bar{\sigma}_3)$ for the sand being tested was in the order of 3.5 to 4.5. From these

results it was concluded that the effect of the thin membranes being used was negligible in drained triaxial shear tests.

d. Distribution of volume change in a triaxial specimen. From the measurements taken in the membrane test, it was possible to compute the distribution of volume change throughout the specimen by assuming that material between any two horizontal planes prior to shear remained between those planes during shear. It was found that the volume change was very nonuniform. The center of the specimen tended to expand and the ends tended to contract. These data were checked by the methods used in the investigation of specimen preparation; that is, after straining a saturated specimen it was drained and frozen in the deformed condition and the density of each quarter was determined. This furnished a qualitative check of the computed densities and indicated that the variation in volume change was at least as nonuniform as had been determined from the external dimensions of the specimen as previously discussed. This was a qualitative check only, since there is a possible variation in starting densities of ± one pound per cubic foot, as was shown previously. It is to be remembered that the information was obtained from photographic negatives, taken for the purpose of computing membrane restraint only; therefore, only a limited number of photographs on each test were available and none at strains less than 2-1/2 per cent except in the case of the last test, which was the only test performed for the purpose of determining density variations. It was at this point in the investigation that a memorandum to Professors Casagrande and Taylor was prepared giving the general details of the data and asking for their comments; copies of the memorandum and the individual replies are attached to these minutes.

Effect of results obtained

11. Mr. Maxwell presented the conclusions of personnel connected with this work concerning the effect of these results on any attempt at a rational approach to the problem of liquefaction. These conclusions were as follows:

a. The net volume change, or net pore pressure change, measured in a triaxial test on sand may be neither indicative of the changes which occur in the failure zone of the specimen nor

indicative of the changes which occur in a mass of sand in nature.

b. The concept of critical void ratio as presently defined may not be applicable to the conditions existing in the failure zone, since the material in that zone apparently always tends to increase in volume as failure conditions approach.

Miscellaneous data

12. Correlation between grain-size distribution and density values of sands. Mr. Garber then reviewed the miscellaneous data which had been obtained in this study. He pointed out that an attempt had been made to correlate the characteristics of the grain-size distribution curve with given density values for sand, particularly the maximum, minimum, and natural densities. Personnel connected with the study were not completely in agreement in the conclusions which they had drawn from the results obtained. It is generally believed that the maximum and minimum density values possibly could be correlated with the grain-size distribution characteristics and there is some indication that the natural density also may be correlated with these characteristics. The characteristics used to correlate the density values were very similar to those used by J. McNeil Turnbull, of Australia, in an attempt to correlate Proctor compaction with grain-size distribution; in particular, the area between the grain-size curve and a given ordinate was used as an index. A shape factor was also devised which magnified the fine portion of the grain-size curve, since it was believed that the fines had a considerable effect on density.

13. Effect of small moisture contents on maximum and minimum densities. Mr. Garber also pointed out that it had been determined from the study of maximum and minimum densities in a near dry condition (that

is, at moisture contents of 1 per cent or less) that there was a considerable variation in the densities. In general, the densities had a tendency to increase with decreasing moisture content, below a moisture content of 1 per cent. However, there were indications that this relationship was reversed below a moisture content of 0.2 to 0.4 per cent. These variations of densities could be interpreted several different ways, one of which indicated that there may be an optimum density in this narrow range.

14. Static electricity. It had been found that there was a very pronounced charge of static electricity on the dry sand being used in the laboratory. Review of the available literature on this subject indicates that the charge of static electricity is almost always present, that it is both positive and negative in a given sand, and that the magnitude of the charge can be varied considerably by most of the laboratory tests performed on dry sand. It was pointed out that while these charges may have some effect on the determination of minimum density they probably do not have any influence on the results of the other tests.

15. Saturation. It was found also that the effect of the degree of saturation was very pronounced in the closed system type of constant volume triaxial test, even above a degree of saturation of 98 per cent. The initial degree of saturation was determined from the pressure and volume change measurements taken during the application of the initial pore pressure and chamber pressure. The degree of saturation at all times during the test was determined by the same method. It was found that, in the case of constant volume tests on dense specimens which tended to expand, the volume change occurring in the air voids was in the order of one-half that which occurred in a completely drained test. The pore

pressure in the tests in which the degree of saturation was determined depended entirely upon the degree of saturation.

16. Triaxial specimen caps. In the tests on membrane restraint it was found that the use of tilting caps gave wide variations in strength and caused severe distortion of the specimens. The use of nontilting caps gave very uniform deformation and only by the use of nontilting caps was it possible to measure the volume changes throughout the specimen.

PART II: DISCUSSION OF DATA

Empirical Approach

Adequacy of the methods being used

17. Mr. Johnson asked for the comments of the consultants with regard to the adequacy of the empirical approach now being used.

18. Professor Taylor generally recommended that the procedures which have been under discussion and outlined so far be continued. He felt that the cone penetration test offered promise of an excellent means of exploring sand deposits and that the correlation of this with density and relative density should be continued. However, it may be necessary to determine how much faith can be placed in the assumption that the undisturbed sample boring and cone sounding encounter the same strata at equal elevations. That is, it probably will be necessary to determine the effect of the 5- to 15-ft spacing now used between these two types of borings on the change in strata. He suggested that closely-spaced, alternate undisturbed sample borings and cone soundings, say three of one type and two of the other, be made on a line before any attempt is made to correlate the results of the two types of tests.

19. Professor Casagrande stated that a year ago he felt the empirical approach was the best to follow, but at the same time he did not believe that everyone felt it would necessarily succeed. In view of the data which have been accumulated since the last meeting, he feels there is a very good possibility that such an approach will succeed. He thought the empirical approach used, that is, the investigation of old survey data,

aerial photographs, geological surveys, etc., and the comparison of stable and unstable areas, was a very good approach to the problem; that probably, after examination of more data, it would be possible to locate more exactly the areas where trouble could be expected. Professor Casagrande felt he would recommend a more detailed investigation than had Professor Taylor in regard to the number of borings necessary to determine lateral variations in strata. He believed a few general exploratory borings should be made first, to determine an area which would be suitable for the desired research. He would then select an area of the order of probably 100 by 100 feet and cover the area by penetration soundings spaced about 10 feet apart. Only by such concentrated investigation could enough information on the distribution of the density of the sand be obtained. After the soundings are completed, two or three points in the area should be selected to make undisturbed sample borings, preferably at locations which are surrounded by penetration tests which have shown a fairly consistent profile. This is necessary to make more effective a correlation between the undisturbed sample borings and the penetration borings.

20. Mr. Foster advanced the concept that it will not be possible to predict liquefaction or susceptibility to liquefaction from the cone penetration. He pointed out that it had never been possible to predict the performance of the soil from the cone penetration index in the natural state in the trafficability studies on a wide range of soils, including sand. After some discussion of his question, Professor Casagrande summarized his views as follows: In interpreting the cone results, the main point of interest is their correlation with density, and this density would have to be correlated with susceptibility to liquefaction at a later

date. In comparison with the work done by Mr. Foster the potamology studies are dealing, in general, with one material, and therefore at a given depth a change in penetration resistance in this material can only mean a change in strength. Therefore, if strength is dependent upon density, penetration resistance is, in effect, measuring the density of the material. If it can then be determined from the cone penetration resistance what materials are dense and what are loose, a major step will have been taken toward determining whether the area is susceptible to liquefaction or not.

Additional investigations

21. Professor Casagrande stated that the answer to the problem of determining variations in density might lie in the manner of deposition of the sands. He could visualize very loose sand being deposited on the lee side of a bar, where rollers have an upward velocity component. There was no question in his mind about the fact that large quantities of sand could be deposited in a much looser state than sand deposited in quiet water or in minimum density tests in the laboratory. He said that this sort of action probably occurs in point bar deposits. This type of deposition is probably very complicated and would require large-scale testing. Professor Casagrande stated that this was the type of problem wherein the combined capabilities of the Soils and Hydraulics Divisions here at the Station could be used to the fullest advantage. He suggested that the two divisions cooperate in the construction of a flume or model wherein this type of deposition could be reproduced and establish ideas and principles governing the deposition of sand. At the present time he could

visualize two subdivisions to this research. One would be the investigation of the deposition of the material at the steep face of the sand bar, both in the field and in a hydraulic model or ordinary large flume. The other question is, how the distribution of vertical velocity components across the entire river at the point bar affects the sedimentation of the material. This type of test would require the use of a model in which a bend could be formed and materials fed into the model to build up on the inside of the curve. The currents should be measured and also the density of the material being deposited. He felt that this latter type of test could include tests which would in some manner create deposits of material formed with the continuously-upward currents of water. The slope of this deposit could be carefully undermined by a sideways current until finally a slide would occur which, if the material were loose enough, should continue as a flow slide. He felt that this model test would be not only instructive from the standpoint of educational results but would be a very useful tool for studying the process of liquefaction.

22. Mr. Garber pointed out the similarity between Professor Casagrande's views and those which have been published by Bagnold. Bagnold explains the deposition and the densities which occur in desert dunes on the basis of the wind current and velocity. Mr. Turnbull pointed out that he thought the work done by Bagnold was very important to our studies, and that he could verify some of Bagnold's findings by his own experiences in sand dunes of Nebraska. All parties agreed that the investigation of the depositional features of density distribution on a sand bar would be a necessary phase of future work.

23. Mr. Johnson then advanced the proposal that direct measurements

be made of the pore pressures developed at various points of the mass of sand in the field by means of pressure-type piezometers after application of shock or vibration of some type. He questioned the consultants as to their opinions on this approach and its possible merit. Professor Casagrande stated that he felt sure if the procedure involved the use of discharge of explosives that positive pore pressures would be developed in any mass of sand and that the piezometers would probably measure pore pressures of a considerable magnitude, but interpretation of these pore pressures would be impossible. He stated that a few years ago he believed shock and vibrations with little strain would be enough to cause liquefaction. Since that time, however, he had changed his mind and now believed that strain is a very important factor in liquefaction. He did not believe that pore pressures developed by vibration would be any better indication of susceptibility to liquefaction than those caused by explosives.

Rational Approach

Laboratory phase

24. Mr. Johnson asked if there were any comments or questions anyone would care to make on the procedures used in the laboratory program and if there were any suggestions on additional work.

25. Mr. Turnbull asked if work had been done on any sand other than the one originally discussed. Receiving a negative answer, he stated that he thought it would be well to run a similar series of tests on sharp angular sand to see if there would be any difference in the results.

26. Professor Casagrande stated that he always examined the data very closely in cases of this type for possible discrepancies or errors, but apparently there were none in this case. He knew that the conditions within a triaxial specimen were nonuniform but had never guessed the non-uniformity to be as severe as the data showed. He has suspected for some time that the density varies diametrically but had never considered it to vary axially within the specimen. He thought it would be very beneficial if additional tests were performed to determine the diametric variation of density in a triaxial specimen.

27. Professor Taylor said that he was impressed with the procedure used and apparent accuracy of the data. He agreed with Professor Casagrande that it would be worth while to determine the diametric variation in density. He also favored performing the same type of tests on a constant volume type of test specimen. He thought that both axial and diametric variation in density should be determined in this case.

28. Mr. Johnson asked for opinions on how a rational approach to the problem could be made, in view of the nonuniformity of the test specimens during shear.

29. Mr. Turnbull stated that he was in favor of direct shear tests, since this type of test was simple, probably did not have the big variation in dry density and, in his experience, knowledge of direct shear test results could have prevented at least two major slides in the granular shell material of a dam. He believed the use of a longer shear box and a thinner sample would overcome the usual difficulties encountered in the direct shear type of test.

30. Professor Taylor thought that nonuniform conditions exist in

nature along the failure plane and are similar to those which occur in a constant volume triaxial test specimen. He therefore favored the constant volume test as previously stated. He believed it would be possible to vary the length-diameter ratio of the test specimen until volume change conditions in the triaxial specimen were obtained which would approximate the field conditions on the failure plane. He did not consider this to be a rational approach, however, but a very good empirical tool. He said that he would like also to see the same type of test (constant volume) performed on undisturbed samples, wherein the diametric and axial variations in density would be measured at initial stress ratios corresponding as closely as possible to natural conditions.

31. Professor Casagrande remarked that he was not sure laboratory tests would ever be of any use in connection with analysis of flow slide problems. It was his opinion, however, that it is necessary to find out what happens inside a sample in the constant volume and constant lateral pressure types of tests. If, after determining this, the results of the constant volume type test are no different than the results obtained in the constant lateral pressure test, it then is of no especial help in solving the problem. Professor Casagrande believed that the volume changes and strains in nature probably are uniform throughout the entire mass of sand rather than nonuniform on the failure plane. He believed that the first 1 to 2 per cent axial strain in the triaxial test might be representative of what occurs in nature and that the trend observed during small strains may be a measure of susceptibility to liquefaction in a qualitative sense. He stated that only an empirical approach was applicable, that we should try to correlate the average volume change

occurring in the first 1 to 2 per cent strain with some empirical value. He agreed with Mr. Turnbull that the direct shear test might have possibilities, that the change in density throughout a direct shear specimen at a small strain should be investigated and that it should be done in a shear box in which the length and thickness of the sample tested were greater than in the usual direct shear box.

32. All parties were in agreement that the diametric variation of density in the constant lateral pressure type of test should be determined as well as the axial and diametric variation in the constant volume type of test, and the normal variation in density in the direct shear test. It was agreed also that the direct shear test did have possibilities, since any laboratory test was reduced to the status of an empirical tool.

Significance of
laboratory test results

33. Mr. Johnson asked the consultants to discuss the rational approach, particularly the significance of the laboratory test results obtained and their implications on the procedures to be used in the future.

34. Professor Casagrande stated that his greatest objection to laboratory tests was and still is, more than ever, that, at large strains, the deformation of the specimen is not homogeneous and is not representative of what happens to an element inside a large mass subject to liquefaction. That is an inherent fault of the triaxial test which has not so far been and may never be overcome. There is perhaps a possibility that the best information exists within the first 2 per cent of strain and is representative of what happens at all strains in the field. He therefore

suggested that we pay special attention in any future research, both by triaxial tests as well as by direct shear tests, to the volume changes which take place at a very moderate strain.

35. Professor Taylor was in agreement with the views expressed by Professor Casagrande with respect to the empirical nature of any laboratory test. He felt, however, that laboratory tests could be used as a measure of susceptibility to liquefaction, since he believed the conditions in nature were very similar to those which occur in the laboratory specimens. He believed that what we wanted eventually was the simplest and most workable test giving a satisfactory measure of danger of liquefaction, and there certainly has to be exploratory work to determine what type of test should be used. If the direct shear test is finally found to be the most effective, this must be found by comparative tests. He also said that he believed the best indication to date of the measure of the degree of liquefaction is that particular strain where the stresses on the failure plane change from a decrease to an increase, as illustrated by the τ versus σ plots in the Reid Bedford triaxial report. He pointed out that at one extreme -- the case of a very loose specimen -- the specimen liquefies and the stress actually goes to zero. At the other extreme -- the dense specimen -- the stresses continuously increase. The stresses do not actually reverse in the latter case but the plot of τ versus σ does show a deflection, and he believes that strain at this point of deflection may be a measure of the susceptibility to liquefaction.

Miscellaneous data

36. In the review of the miscellaneous data, Professor Casagrande

stated that he favored a triaxial cap with a small amount of allowable tilt. In general, his findings on the effect of tilt on test results were very similar to ours. He thought that, for our purpose, a completely nontilting cap would be satisfactory.

Priority

37. All parties were in agreement that the field investigation was of primary importance and should receive first priority. The laboratory phase was important and should be continued, but in view of the recent findings it was of secondary importance with respect to the field program.

Waterways Experiment Station

CONFERENCE ON

SOIL ASPECTS OF POTAMOLOGY PROGRAM

17-18 June 1950

AGENDA

I: INTRODUCTION
- A. Review of Approach to Study of Bank Stability
  - 1. Empirical approach
  - 2. Rational approach
- B. Statement of Certain Aims of Meeting

II: RATIONAL APPROACH
- A. Review of Field Methods
- B. Review of Laboratory Approach
  - 1. General discussion
  - 2. Investigation of laboratory testing technique
    - a. Relative density tests
    - b. Placement methods
    - c. Membrane tests
    - d. Distribution of volume change throughout sample under load
- C. Effect of Results Obtained on Use of Laboratory Tests to Investigate Flow Slides
- D. Discussion by Consultants of Item B above
- E. Discussion by Consultants of Item C above
- F. Review of Direct, Torsion and Triaxial Shear Tests Performed since Last Meeting (only if previous discussions and conclusions indicate to be desirable)
- G. Miscellaneous Data (if time permits)
  - 1. Correlation between M.A. and density values of sands
  - 2. Effect of saturation
  - 3. Static electricity
  - 4. Effect of small amounts of moisture on density
  - 5. Triaxial specimen caps

III: EMPIRICAL APPROACH

A. Review of Methods

B. Development of Geology of Area
   1. Airphoto interpretation
   2. Geological reconnaissance

C. Review of Recent Cone and Density Data
   1. Reid-Bedford
   2. Bauxippi-Wyanoke
   3. Discussion by Consultants

D. Proposed Methods of Evaluating Cone and Density Data
   1. Comparison of results from stable and unstable areas
   2. Correlation of cone penetration resistance with depth (i.e., effective overburden stresses), measure of sand properties and suggestions by Consultants

E. Other Investigational Methods
   1. Geophysical

IV: GENERAL SUMMARY AND STATEMENT OF VIEWS OF EACH CONSULTANT ON POTAMOLOGY PROGRAM, TO INCLUDE:

A. Comparison of Empirical and Rational Approaches

B. Utility of Rational Approach and Applicability of Current Laboratory Approach

C. Research Required, if Desirable, on Laboratory Testing

COPY

12 May 1950

MEMORANDUM FOR PROFESSORS A. CASAGRANDE AND D. W. TAYLOR:
(Copy to each)

Gentlemen:

Recent developments from the laboratory phase of the Potamology Investigation have raised certain questions upon which we desire your opinion before continuing our laboratory program. The purpose of this memorandum is to describe briefly the results we have obtained to date. We would like for you to review these results and advise us of your opinion of them and of the questions raised. It is preferred that your opinions be given by letter; however, if you consider a conference to be necessary please so state in your reply, together with the earliest time that you could attend. Arrangements will be made to reimburse you for your time spent in reviewing and commenting on the inclosed data.

We had planned to determine the critical void ratio of 5 or 6 typical Mississippi River sands and perhaps one or two sands from other locations. The tests were to be made with a 2.8-in. triaxial machine on which the applied loads are measured with a proving frame inside the chamber. Prior to starting tests for critical void ratio determination it was considered necessary to do a considerable amount of preliminary work in order to develop the best possible technique so as to avoid experimental and equipment errors present in most previous work. Major items of the preliminary work were the development of a sample preparation technique resulting in very uniform samples, and the evaluation of membrane restraint. The preliminary work has been completed but work has not started on the main program, as results from preliminary tests indicate that our proposed program should be carefully re-examined at this time.

The results of tests to develop specimen preparation techniques will not be described nor will the detailed results of the tests to evaluate membrane restraint; however, methods used in the latter will be described as it was in connection with these tests that we obtained the results that are causing us considerable concern.

A triaxial specimen does not deform uniformly under load and it thus appeared that membrane restraint would be considerably different at the center of the specimen than at the ends. Therefore, an attempt was made to evaluate the membrane restraint at various points on the specimen. To accomplish this, a grid of lines was drawn on the membrane and the vertical and lateral deformation of the grids measured at axial strains of 2-1/2, 5, 7-1/2, 10, and sometimes 15%. The tests were made on dry sand and

lateral pressures were provided by applying a partial vacuum to the interior of the 2.8-in. diameter specimen. Thus, the specimen was accessible for direct external measurements. Full-scale photographs were also taken of each specimen at various percentages of axial strain.

Typical Test Results

The particular sand being tested was obtained at Reid Bedford Bend. It is a clean medium sand having a 10% size of 0.23 mm and a uniformity coefficient of about 1.4. It has a laboratory maximum density of approximately 106.7 lb/cu ft, and a minimum density of 89.3 lb/cu ft. The results of a complete series of direct shear tests and a limited number of torsion shear and triaxial tests indicate that the critical density is about 96 to 98 lb/cu ft. A flow failure was obtained in a constant volume type of triaxial test at a density of 93.5 lb/cu ft.

The measurements used to determine axial and diametric strain in the membrane tests were obtained from negatives of photographs similar to those attached to this letter. These were checked by sufficient external measurements to assure the validity of this process. The measurements were corrected for distortion (which necessarily occurs when photographing a cylinder). The attached photographs represent a complete series on one test at 0, 2.5, 5.0, 7.5, 10.0, 12.5, and 15% total axial strain.

When the volume between any two horizontal grids at a given value of total axial strain was compared with the original volume, it was found that the ends of the specimen decreased in volume and the central portion increased in volume. Typical volume changes for the top, center, and bottom of a dense, medium, and a loose specimen are shown in figures 1, 2, and 3. Incremental volume changes versus height of specimen at 10% total axial strain are shown on figure 4 for the same three specimens. These curves would indicate that, at least after 2.5% total axial strain, the volume in the zone of maximum bulging is increasing and the volume at the ends is decreasing. If the soil between any two horizontal grids prior to testing is assumed to remain between those grids during deformation, then the density at the ends increases and the density at the zone of failure decreases, even for the loose specimen.

This later assumption is so important to the analysis of the results that it has been checked in a qualitative sense by freezing the sample, cutting it into increments, and determining the density of each increment. This is a qualitative check only, since the previous tests on placement density indicated that the initial density of any quarter of the sample might vary by ± 1 lb/cu ft from the average density for the procedures used to obtain dense, medium, and loose specimens, and by as much as ± 2.5 lb/cu ft for the very loose specimens. The densities measured after freezing are compared with densities computed from the photographs for three specimens (medium, medium and very loose) on figure 5. From these results it appears

that the error involved in assuming that the soil between any two horizontal planes prior to shear remains between those two planes during shear may be very small.

One item that is most readily apparent in the results is that the point of maximum diametric strain (maximum bulge) occurred slightly below the center of the sample (see figures 4 and 5). This was true in all tests. Actually, the point of maximum bulge occurred at a height above the base approximately equal to one-half the diameter of the base times the tangent of $(45° + \phi/2)$. This would indicate that failure was occurring as near to the base as possible. If the volume in the failure zone tends to increase and the volume outside of the failure zone tends to decrease, then the net volume change in the laboratory test as generally measured must be a function of the dimensions of the mass of sand being deformed; in particular the height of the mass. Can it be concluded from this that the net volume change occurring during deformation of a large mass of sand in nature might not in any respect compare with that obtained in a small laboratory sample?

It appears (from plots such as figures 1, 2, and 3) that the soil in the failure zone continuously expands during shear, regardless of the initial density, which is contrary to what has been believed previously. This may be modified in two respects. First, the density in the failure area (computed from measurements on the photographic negatives) has never been observed to decrease below a value which roughly corresponds to the minimum laboratory density of the soil. Presumably then, if a sample were tested at the very minimum density, the volume change in the failure zone would be equal to zero at all times during deformation. Second, the preliminary statement may have to be modified on the basis of the relationship of maximum diametric strain to total axial strain. Figure 6 represents maximum diametric strain versus total axial strain for a dense, medium, loose, and a very loose specimen. The initial curved portions of these curves could be due, to some extent, to an undetected error in axial strain. However, it is more likely that the curvature is due to consolidation, since, if the straight portion is projected back the intercept of the projection and the axial strain axis occurs at increasing values of axial strain with decreasing density. As shown by comparing these curves with the curve representing zero volume change, some volume decrease does occur in the failure zone prior to 2-1/2% axial strain in the case of the loose and very loose specimens, even though a continuous expansion at a constant rate takes place thereafter.

Even though the second consideration cited above indicates that a certain amount of volume decrease may occur in the failure zone at the lower densities, for strains of less than about 2-1/2% the condition of zero volume change and true peak deviator stress did not occur simultaneously in the failure zone. True peak deviator stresses, based on the maximum measured diameter for the failure zone, are shown in figures 7, 8, 9 and 10 for the same tests shown in figure 6. The true average deviator stresses (arithmetic average of measured areas for entire sample) and the deviator

stresses computed on the assumption that $A_x = A_o/(1 - e_x)$ are also shown for comparison. In all of the tests performed to date it has never been observed that true peak deviator stresses in the failure zone occurred below a value of 3% total axial strain or even below 4% in the case of the lower starting densities. On the other hand, the point of zero volume change in the failure zone has never occurred at axial strains greater than 2%. Therefore, an exceptionally low density must be necessary if the point of zero volume change and the true peak deviator stress are to occur simultaneously in the failure zone. Furthermore, since the volume decrease in the failure zone is very slight and if it is due to consolidation under an increased stress ratio, it is very difficult to visualize how any such volume decrease could occur in nature where the sand has already been subjected to a stress ratio greater than one.

At present, our thoughts regarding these tests can be summarized as follows:

(a) The net volume change, or net pore pressure change, measured in a triaxial test on sand may be neither indicative of the changes which occur in the failure zone of the specimen nor indicative of the changes which occur in a mass of sand in nature.

(b) The concept of critical void ratio as presently defined may not be applicable to the conditions existing in the failure zone, since the material in that zone apparently always increases in volume as failure conditions approach.

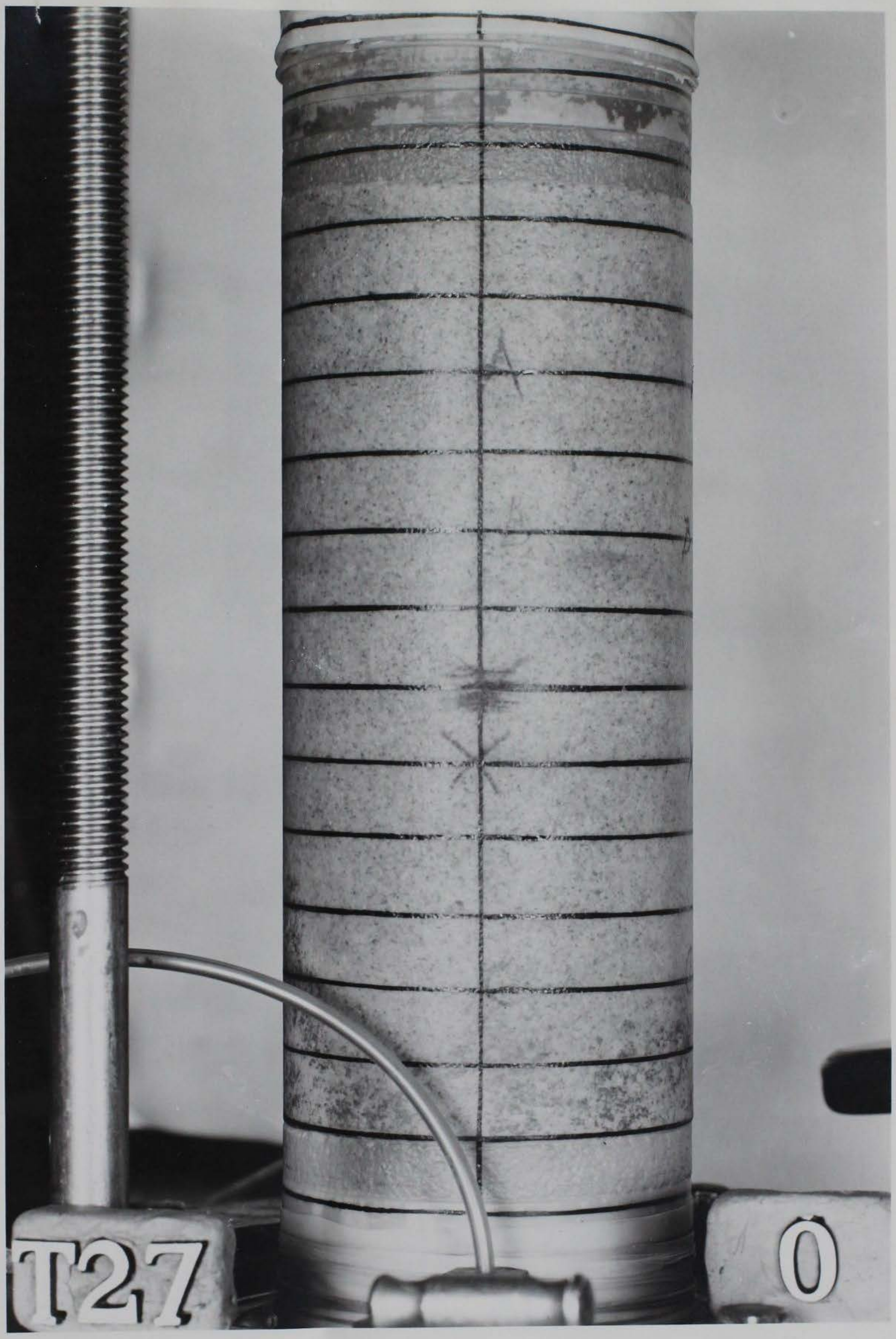
As stated previously, we are highly desirous of having your opinion of the above thoughts, including any additional concepts or proposals that you might feel are justified, at your very earliest convenience.

FOR THE DIRECTOR:

Sincerely yours,

Incls
Figures
Photographs

W. J. TURNBULL
Engineer
Chief, Soils Division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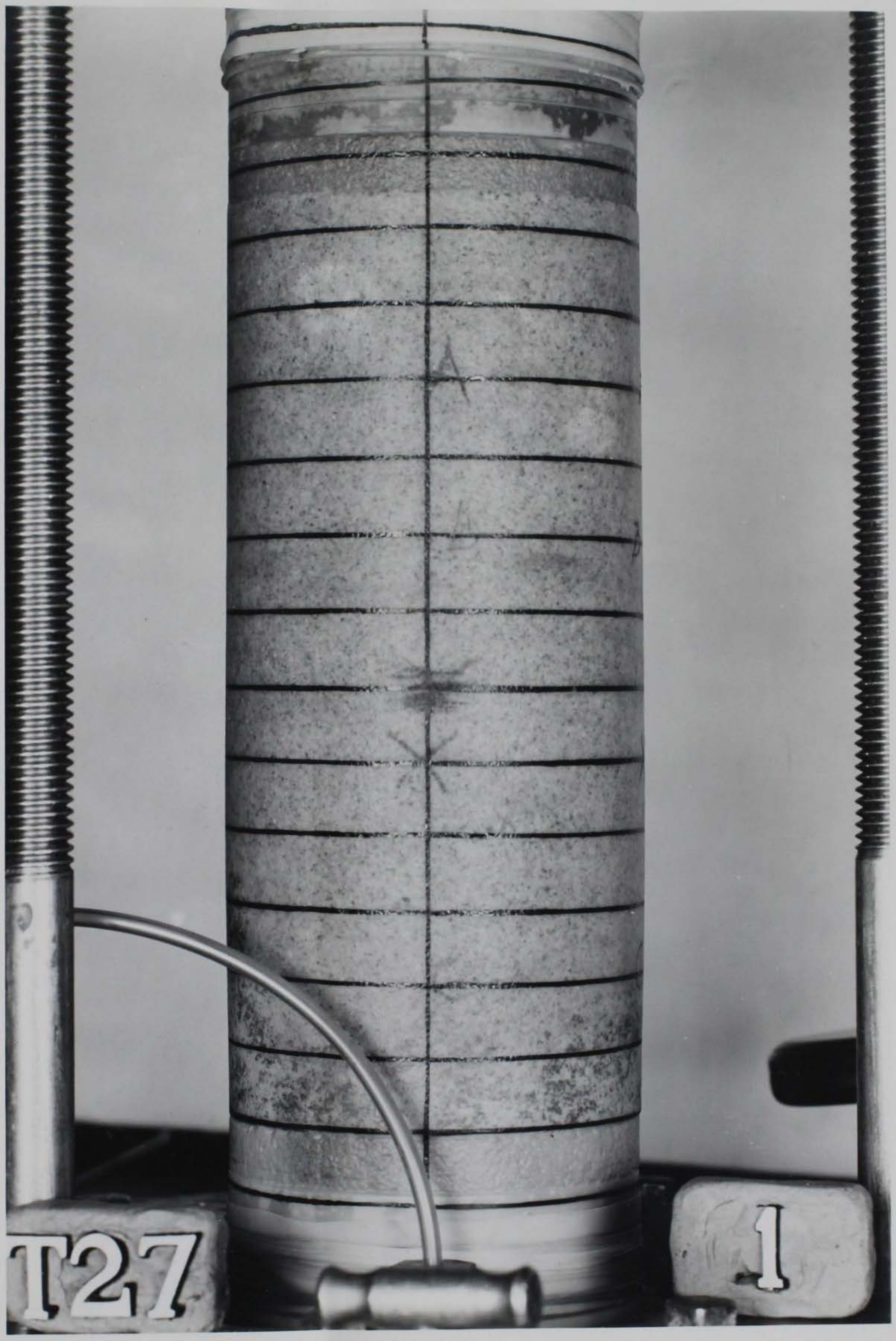
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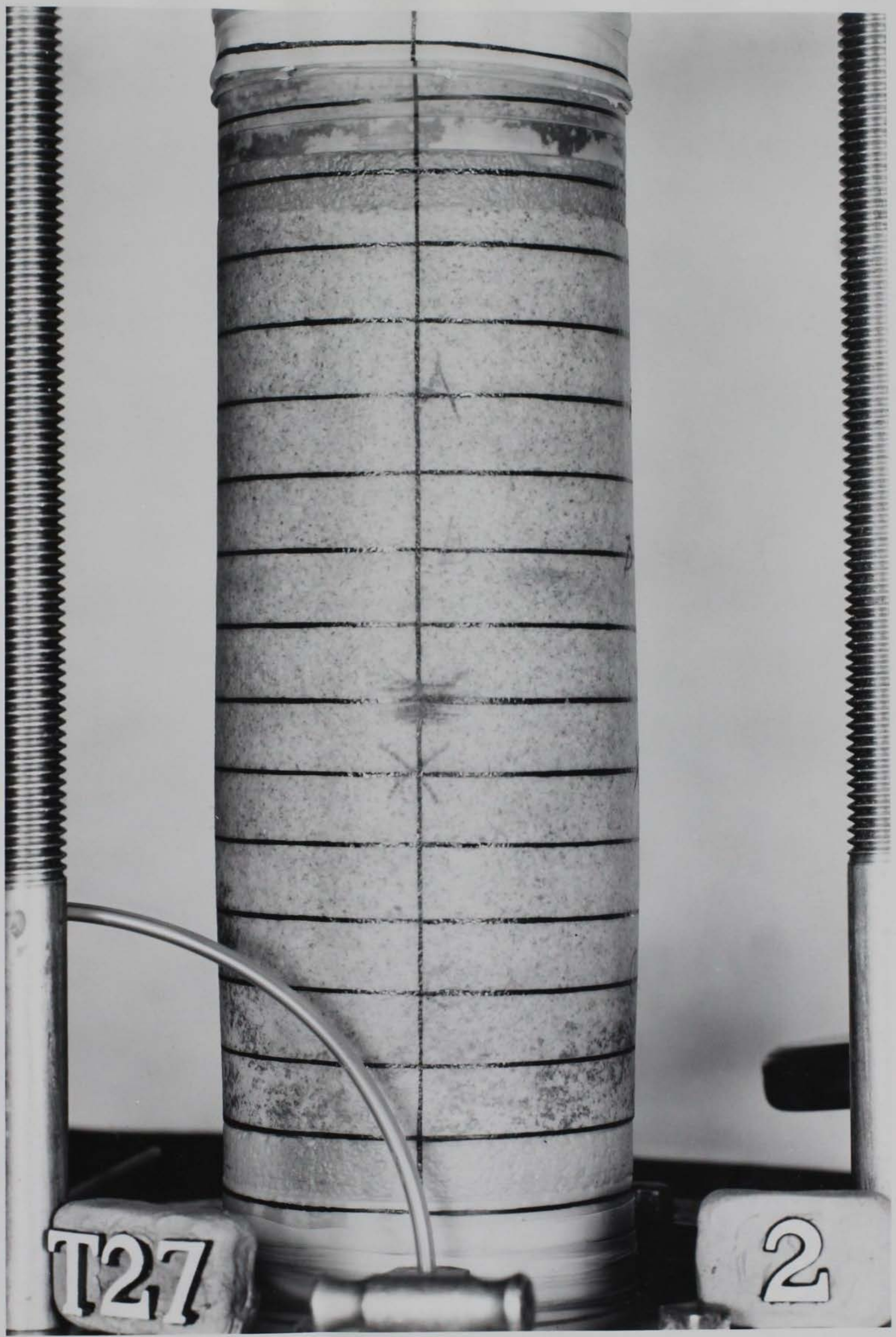
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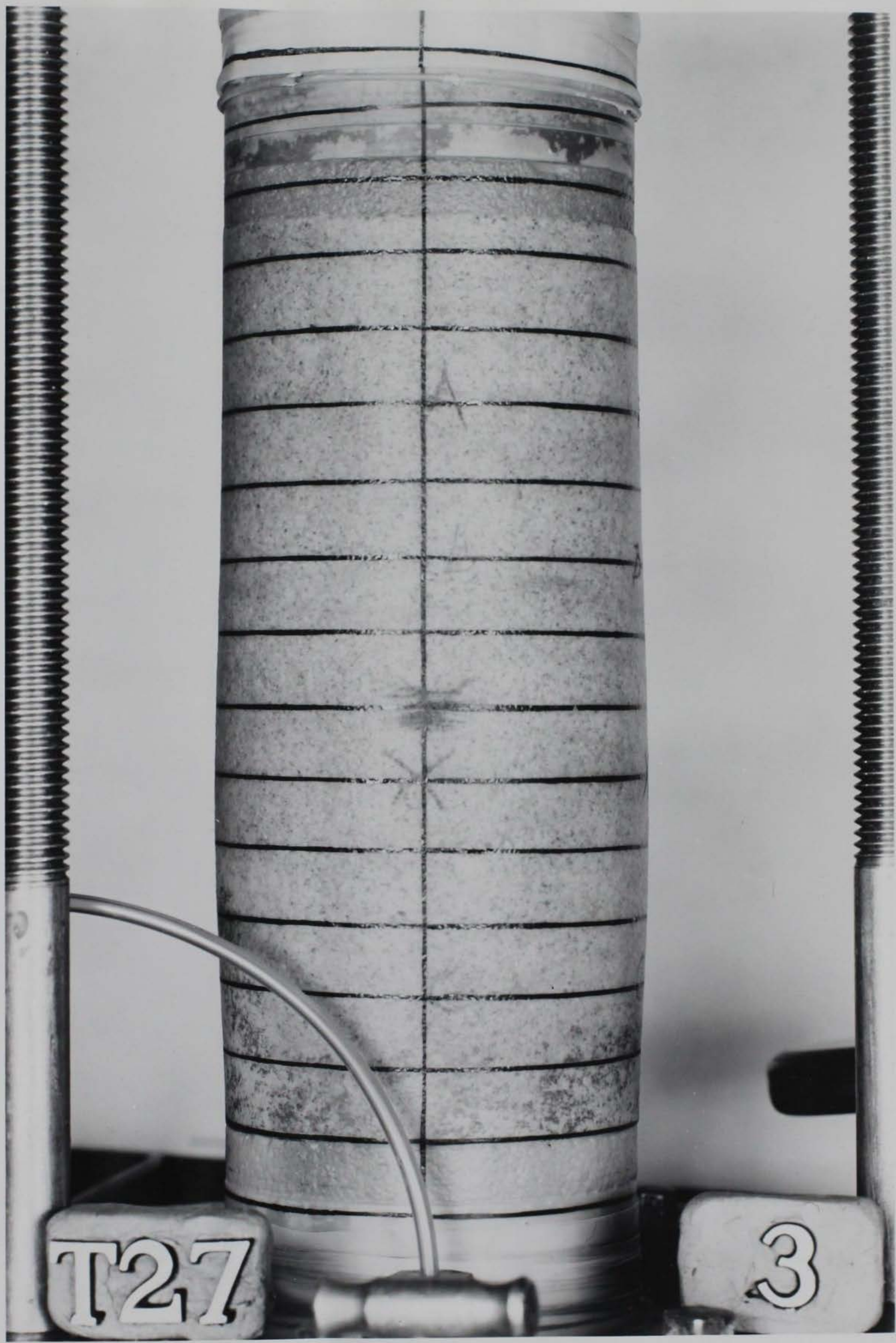
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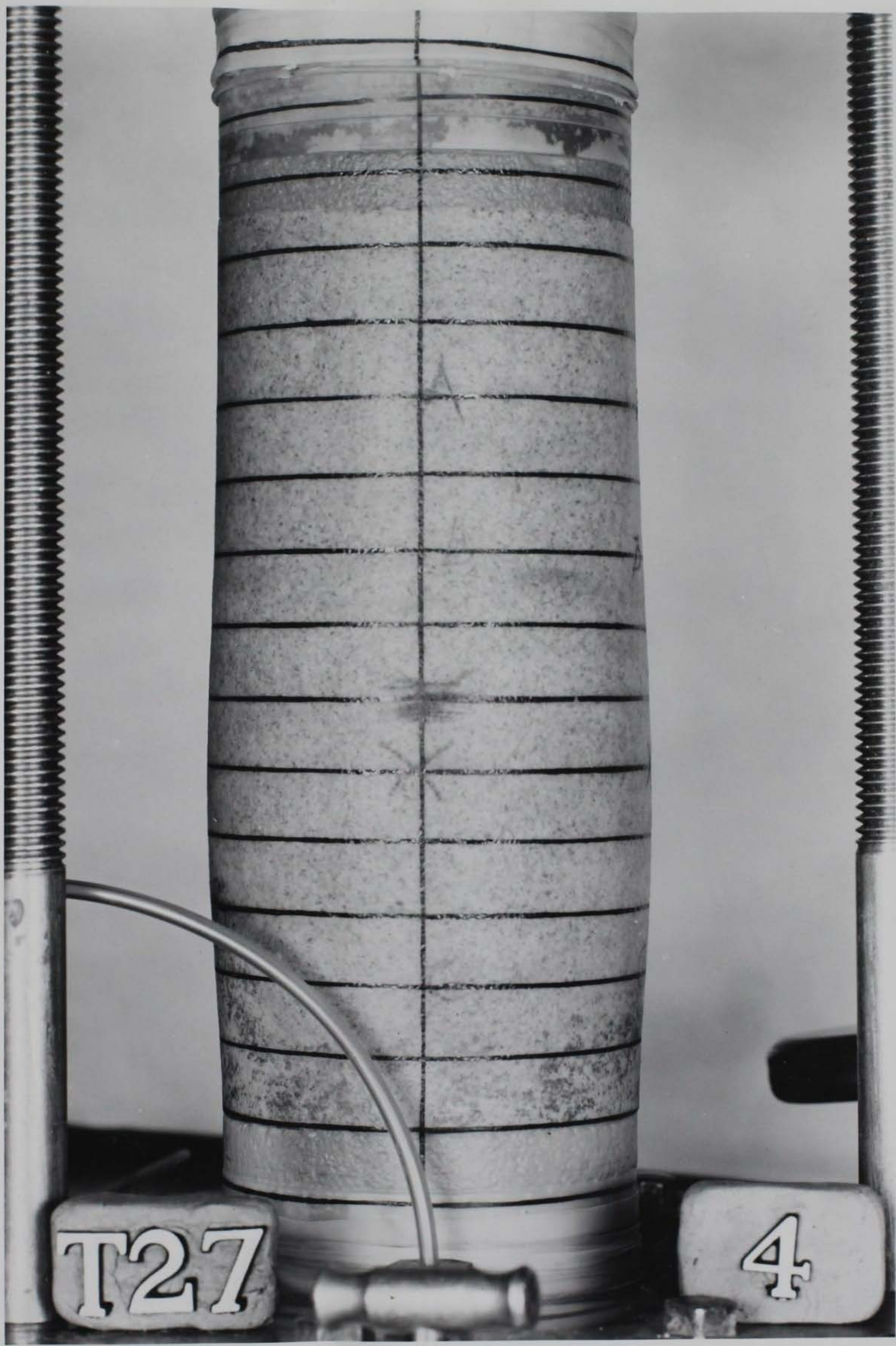
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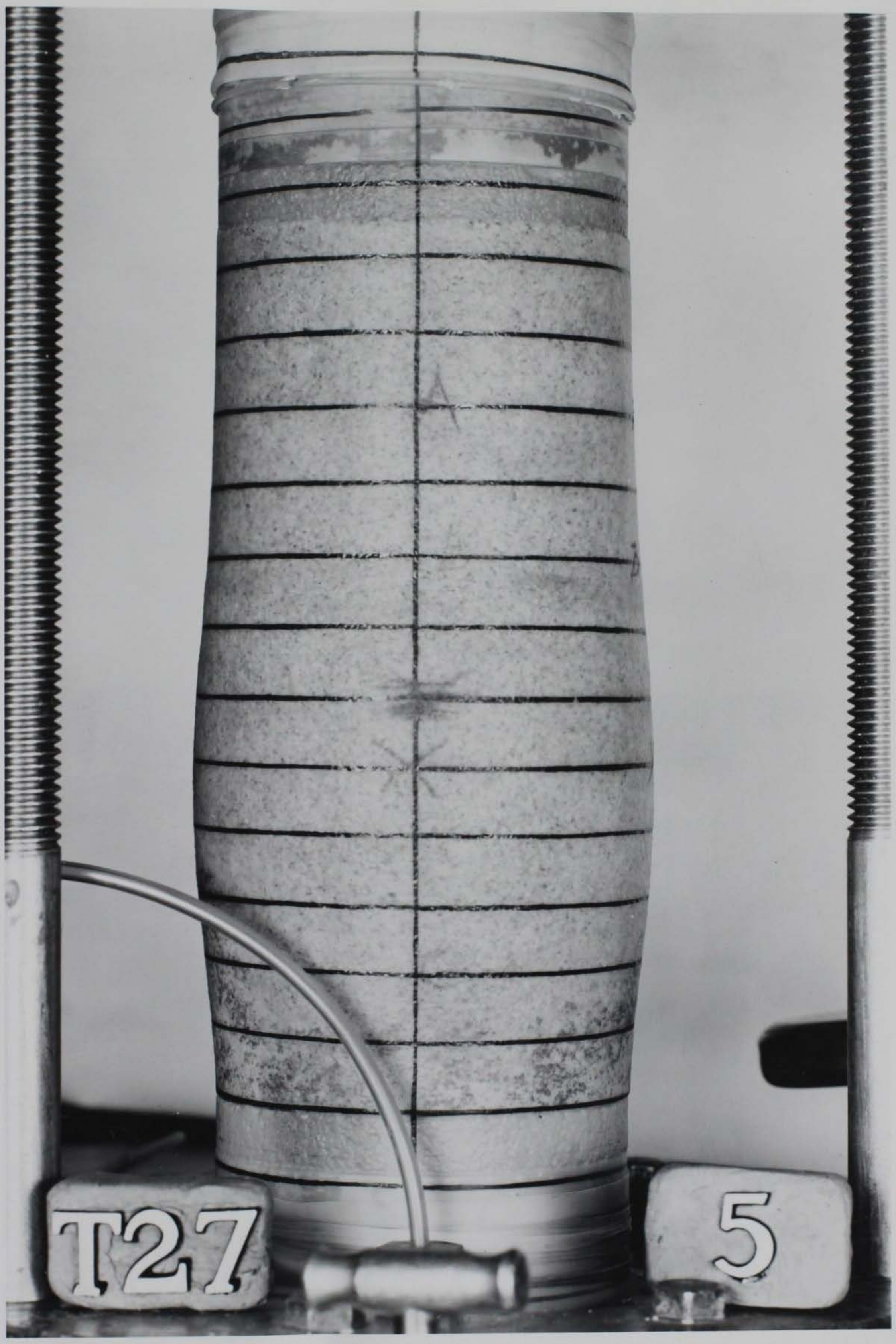
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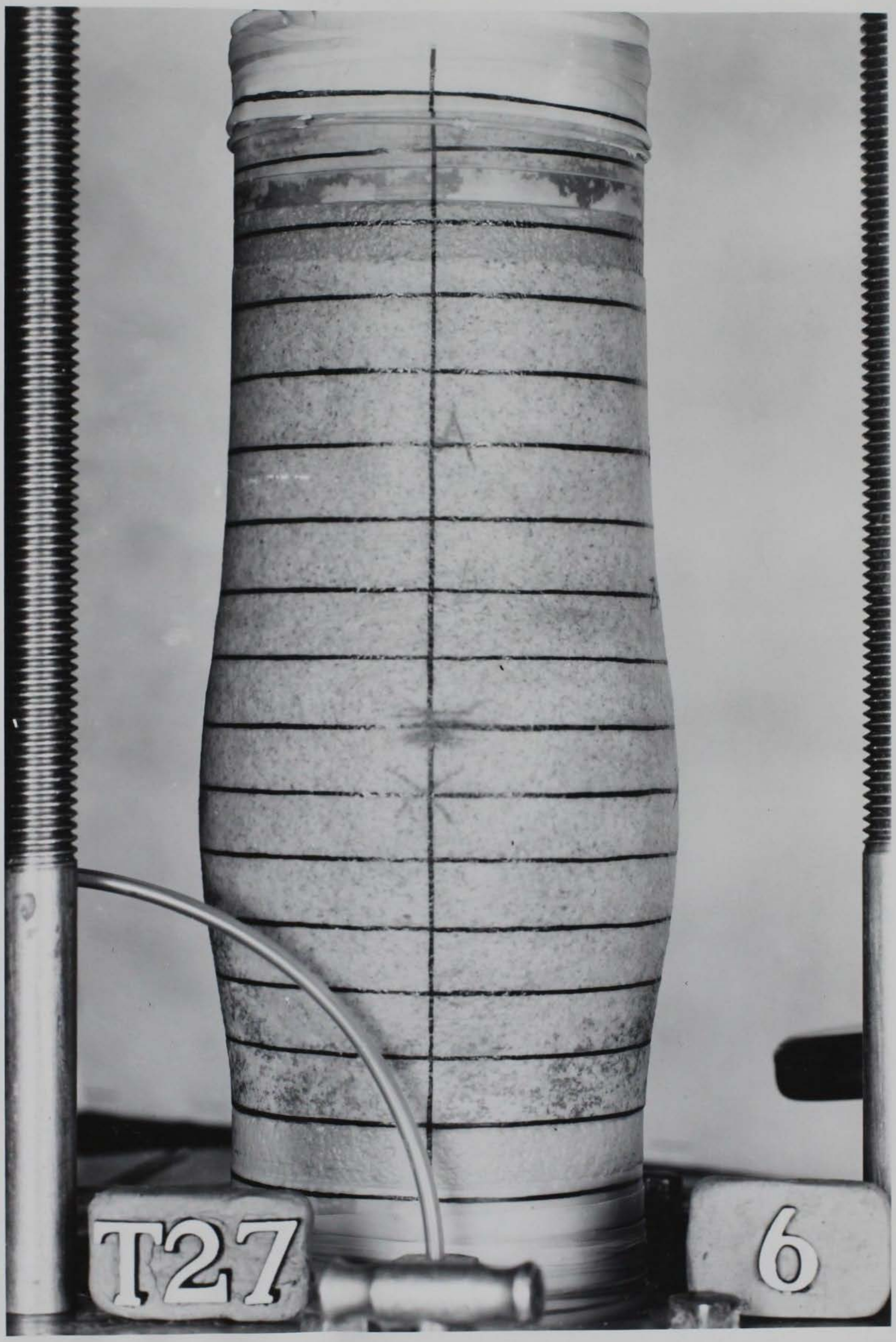
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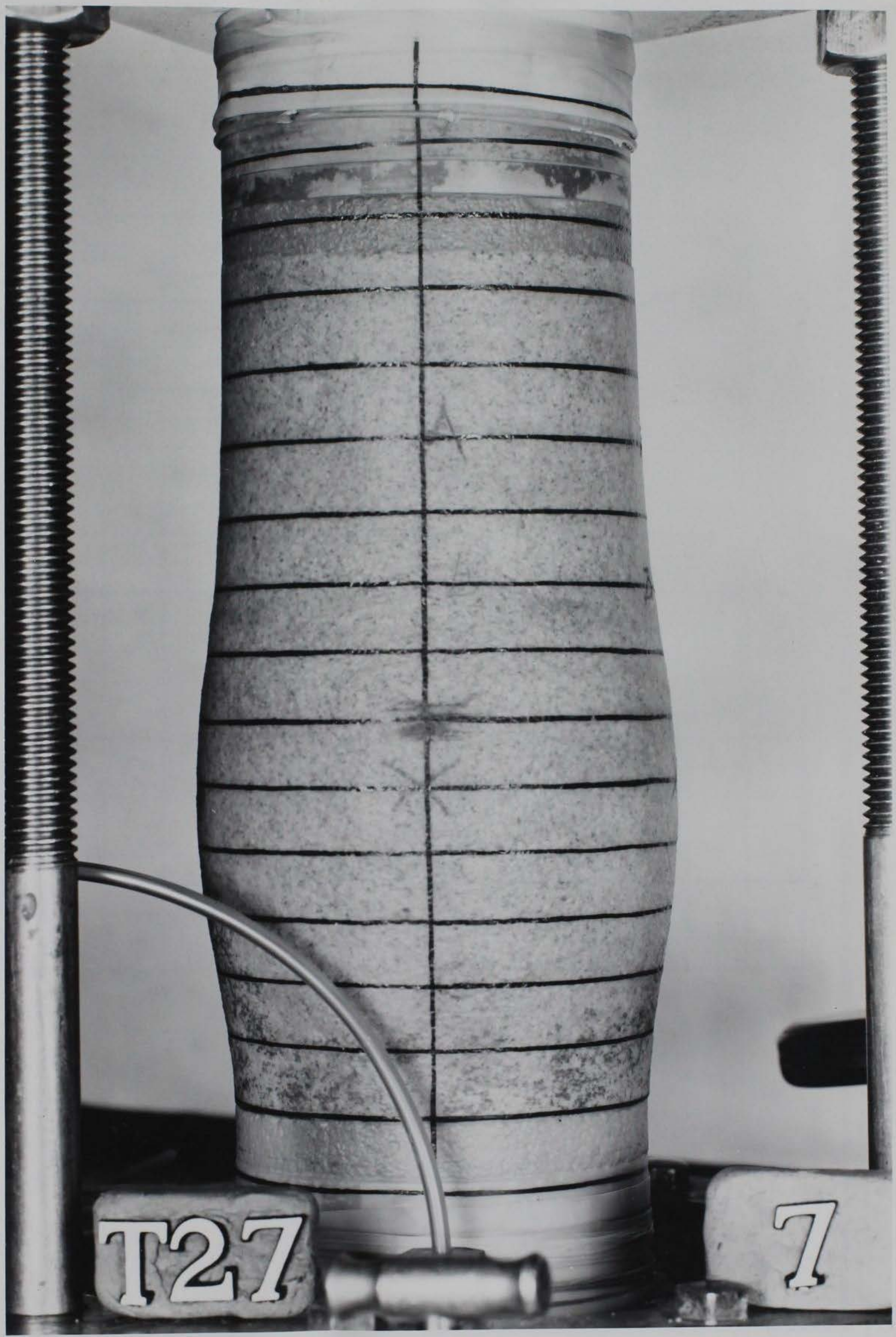
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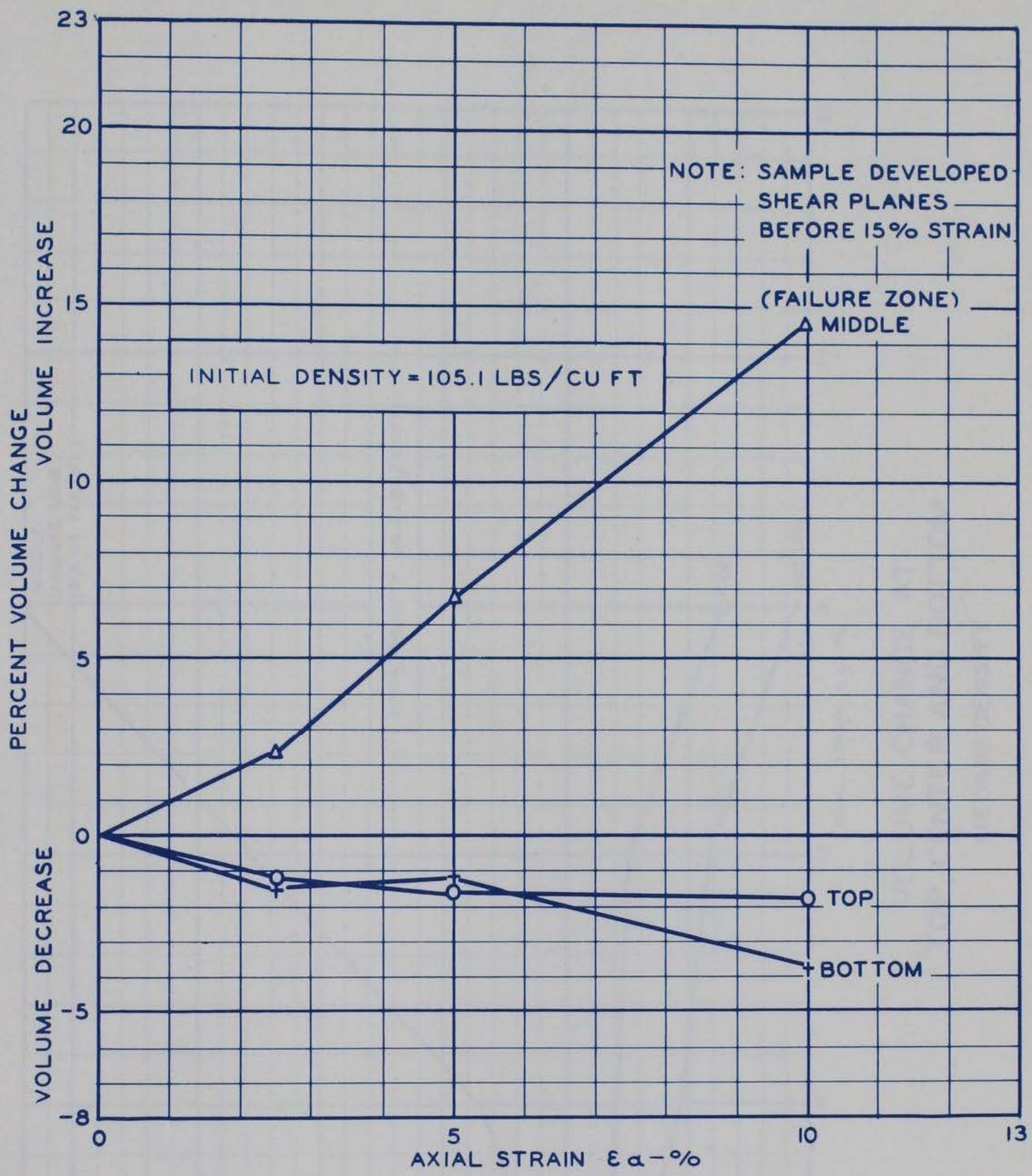
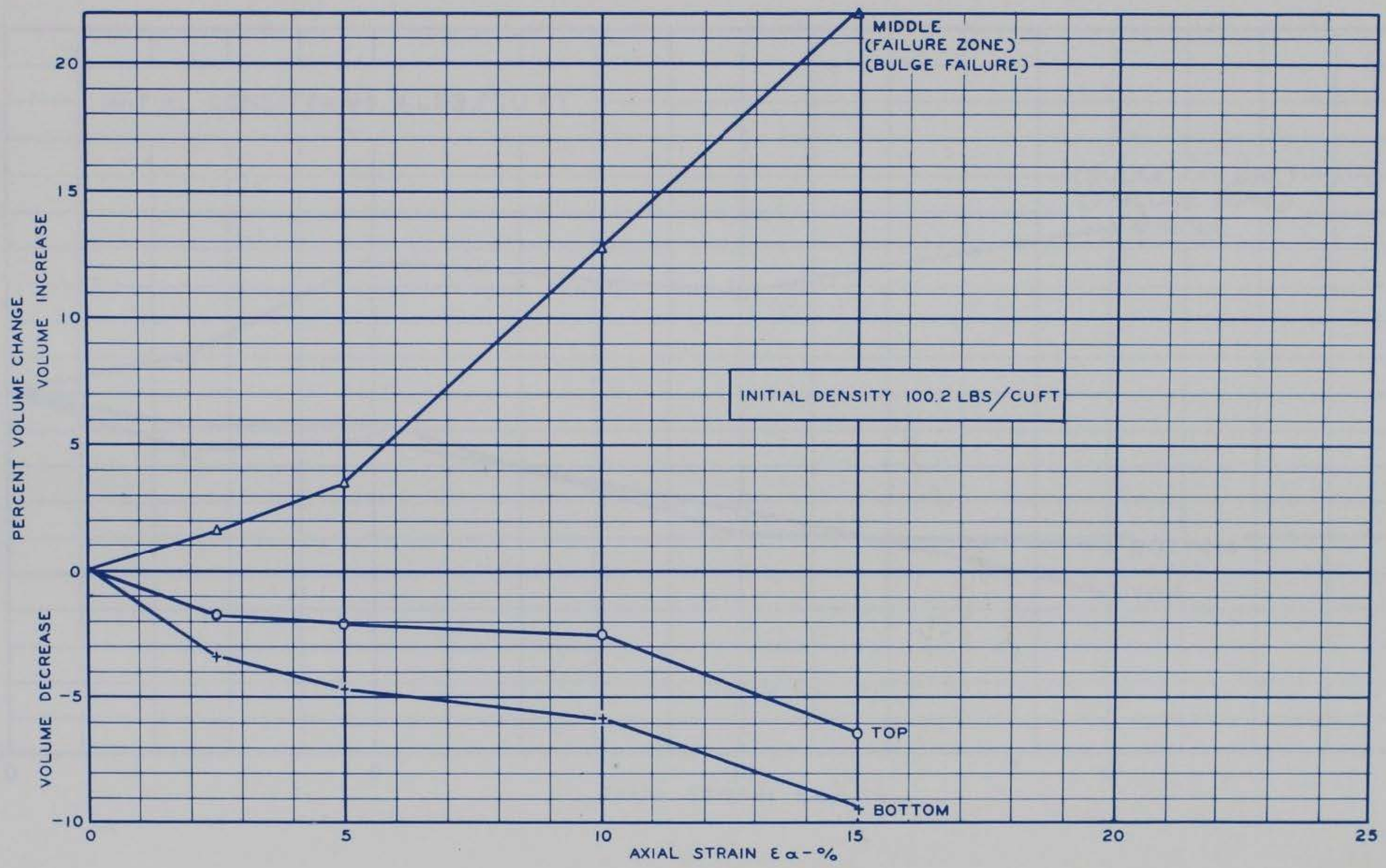


VOLUME CHANGE AT
TOP, CENTER AND BOTTOM
HIGH DENSITY



VOLUME CHANGE AT
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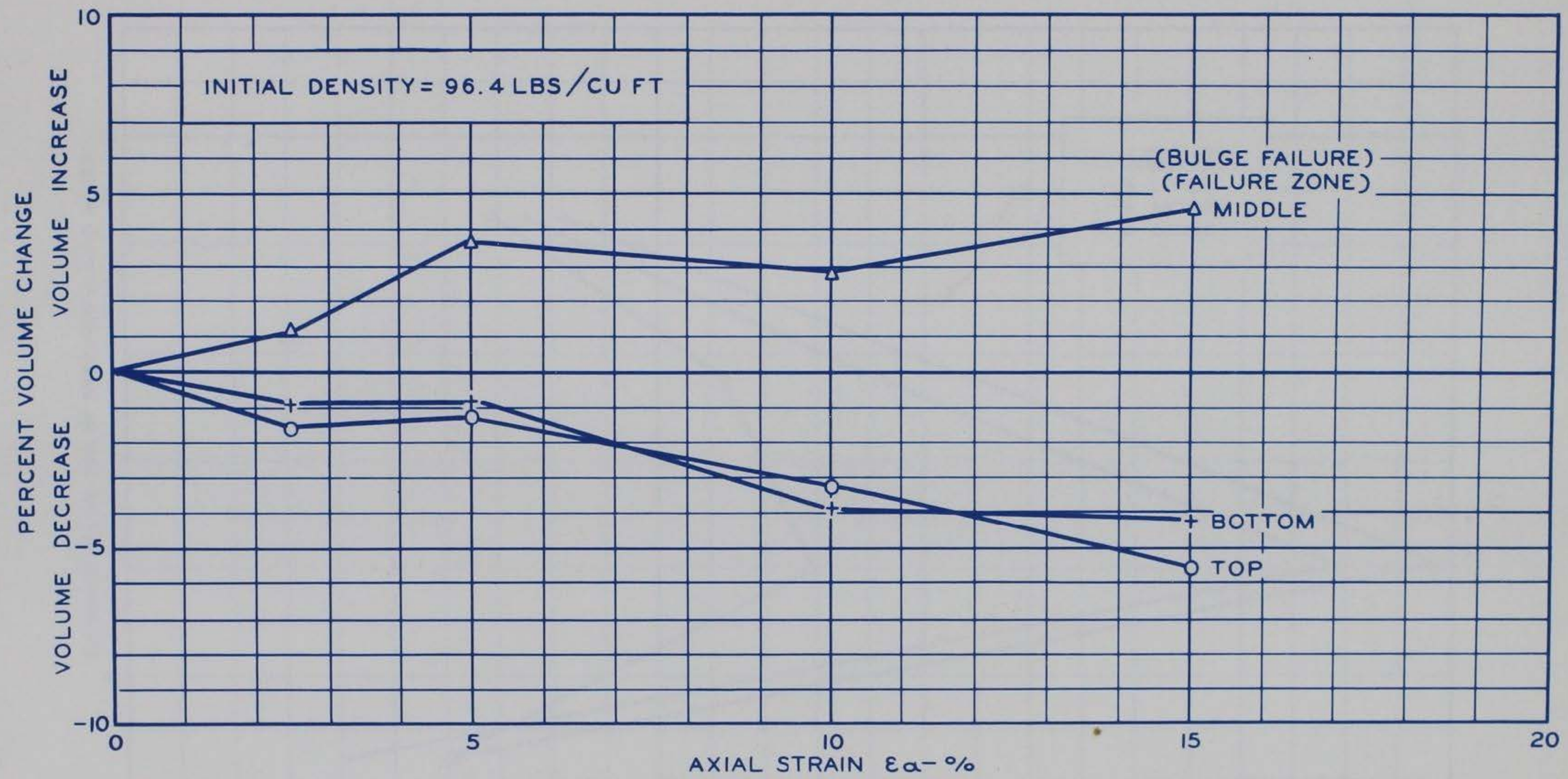
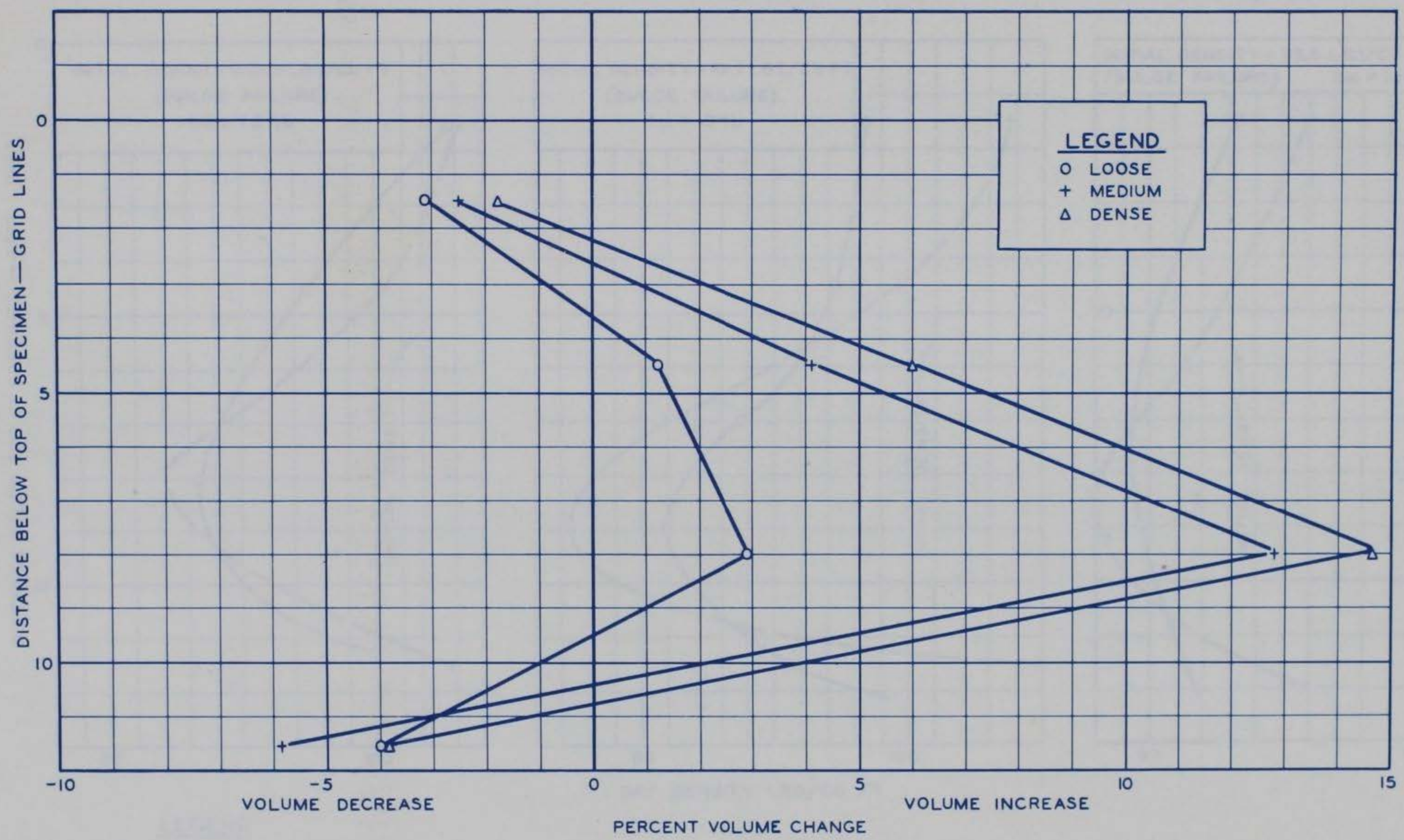
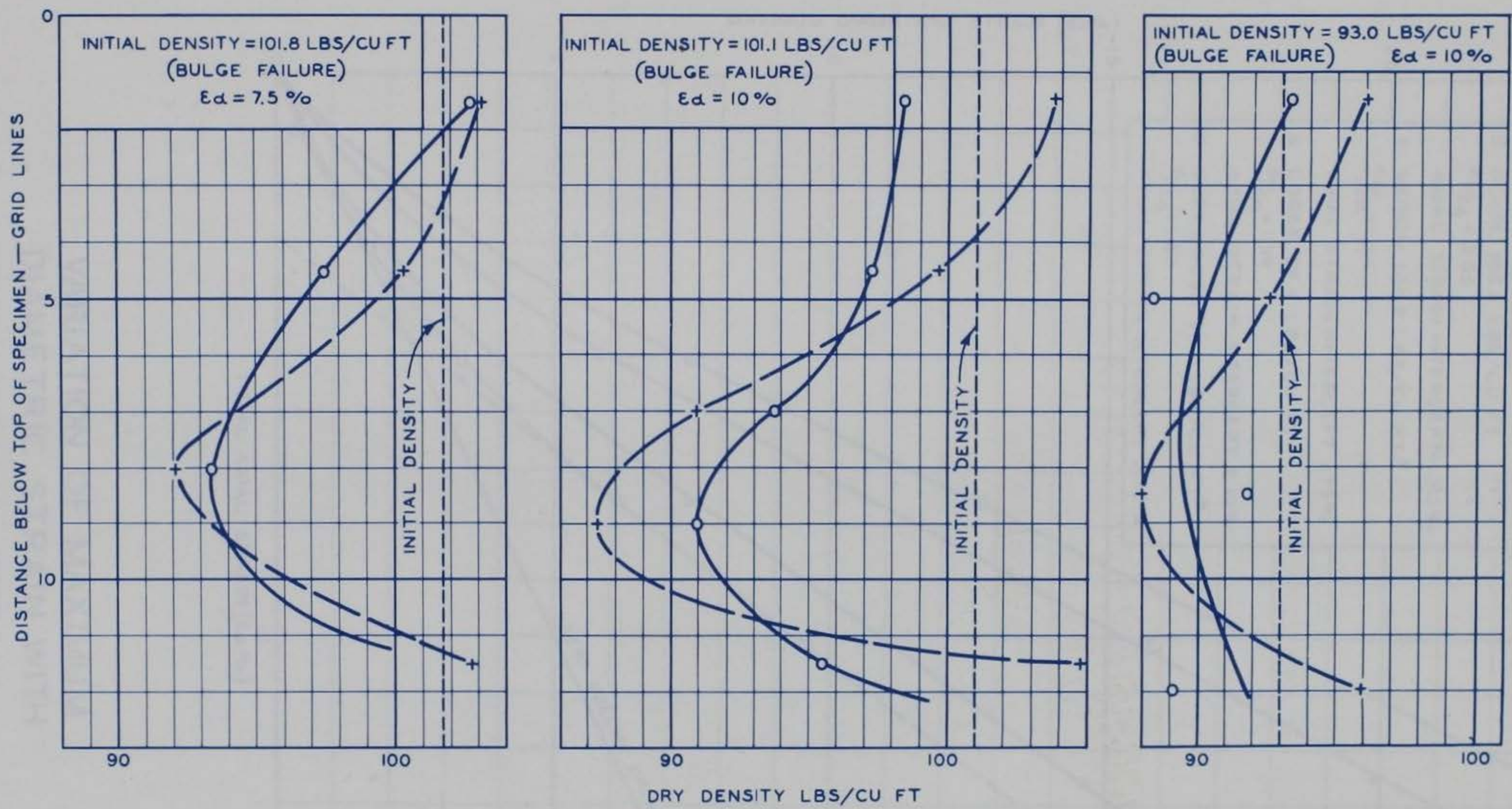
MEDIUM DENSITY



VOLUME CHANGE AT
TOP, CENTER AND BOTTOM
LOW DENSITY



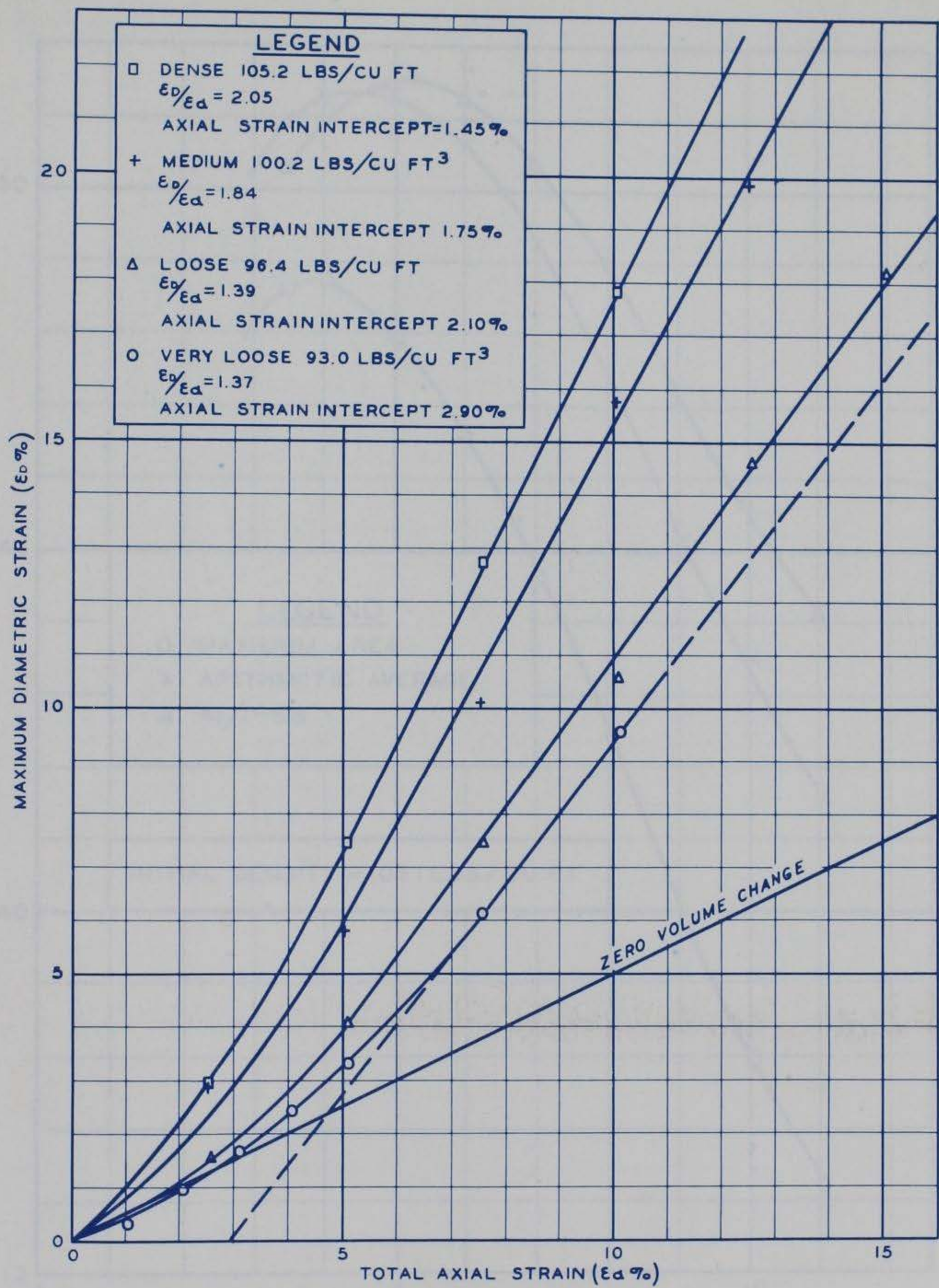
VOLUME CHANGE AT
TEN PERCENT AXIAL STRAIN



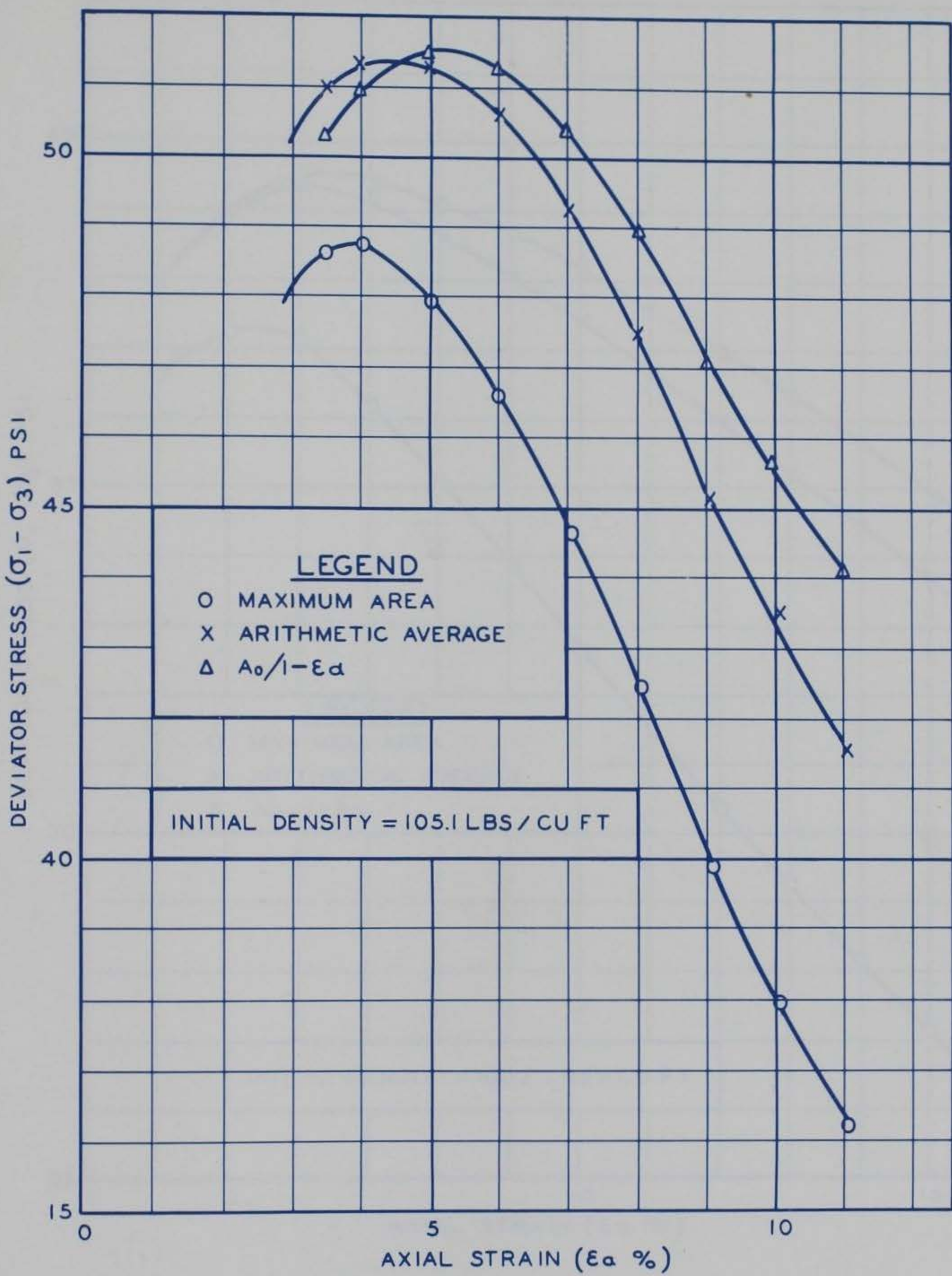
LEGEND

O FROZEN SEGMENTS
+ COMPUTED FROM PHOTOGRAPHS

COMPARISON OF DENSITIES
COMPUTED AND MEASURED

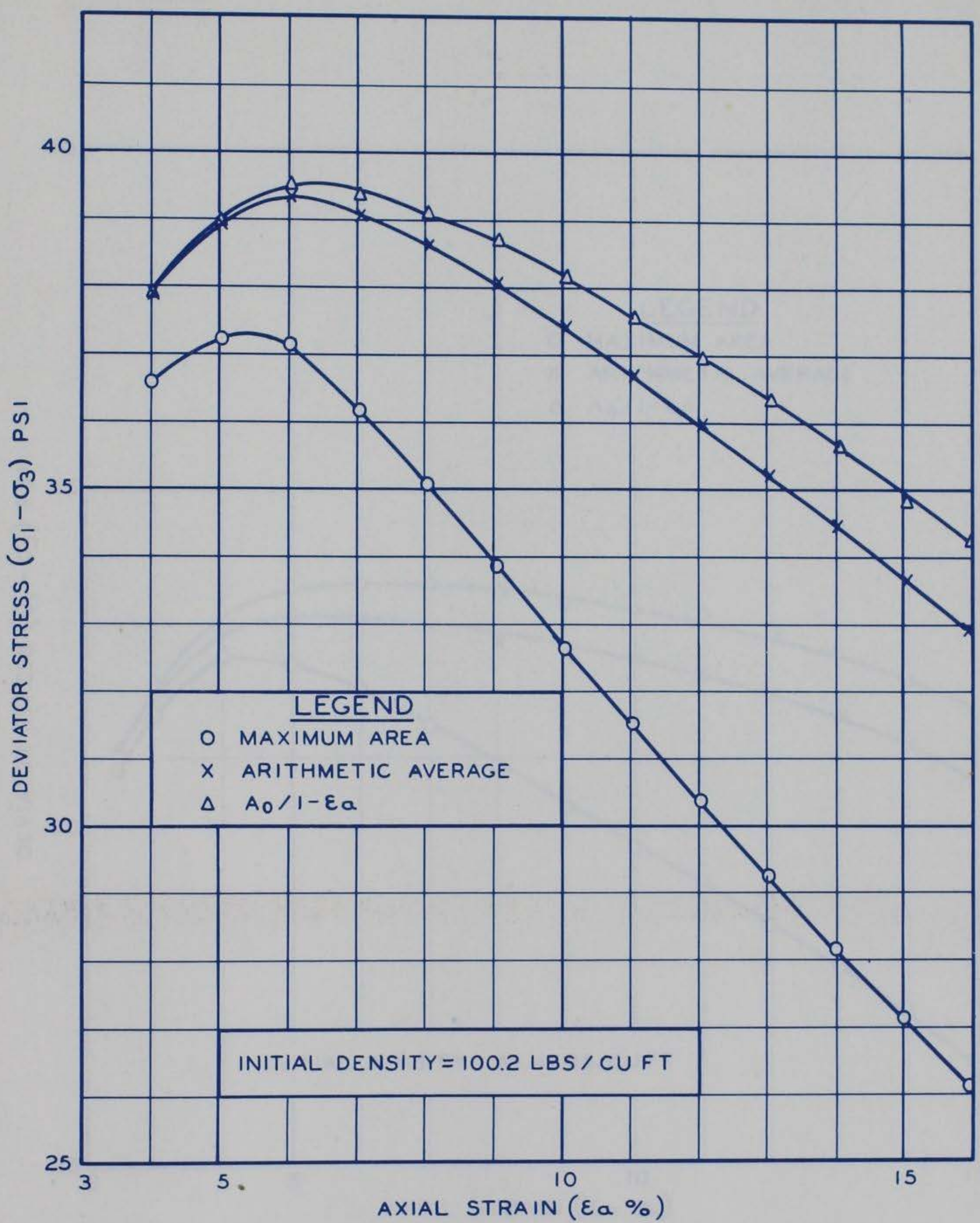


VARIATION OF MAXIMUM DIAMETRIC STRAIN WITH TOTAL AXIAL STRAIN



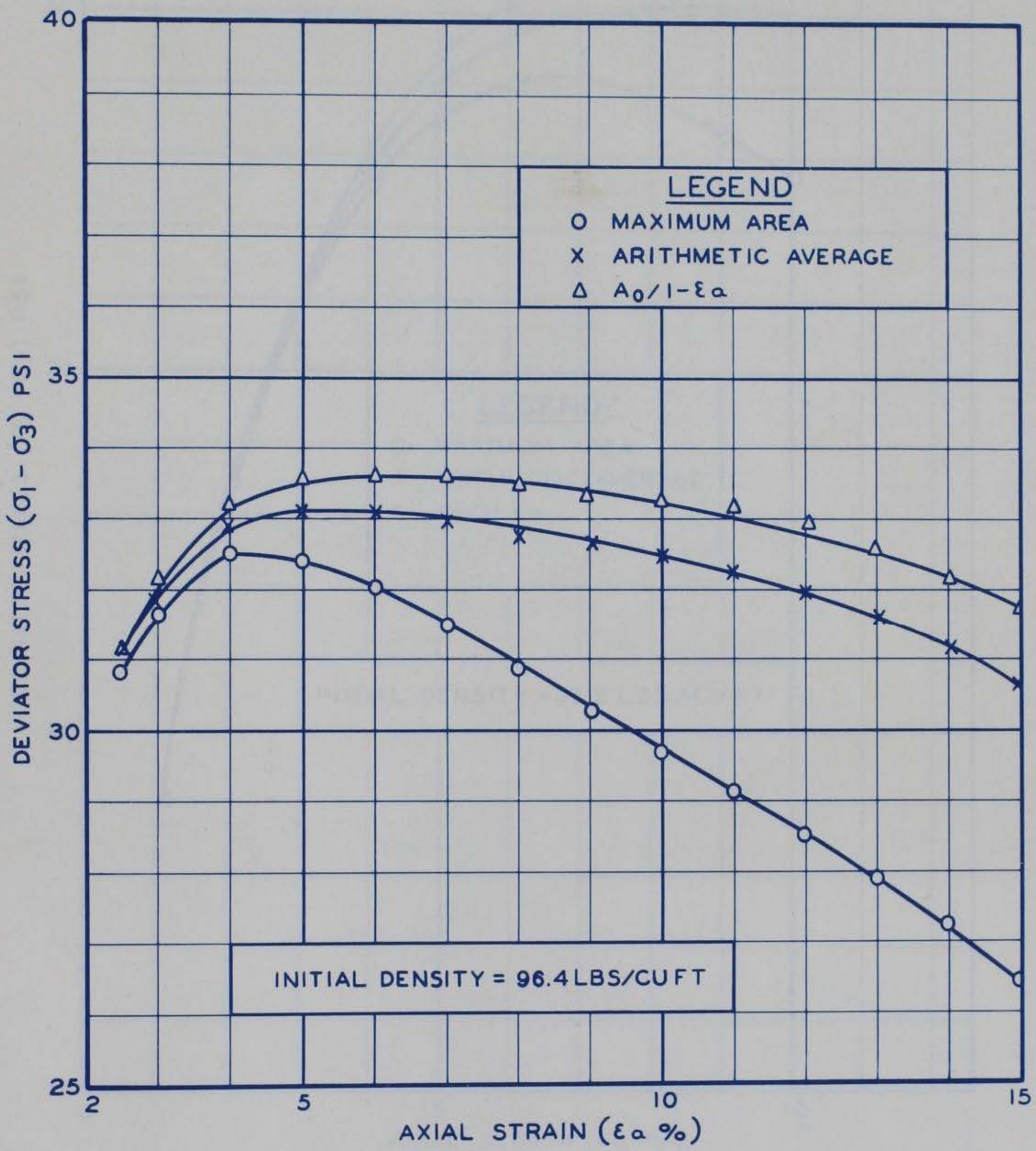
COMPARISON OF STRESSES COMPUTED ON DIFFERENT DIAMETERS

HIGH DENSITY



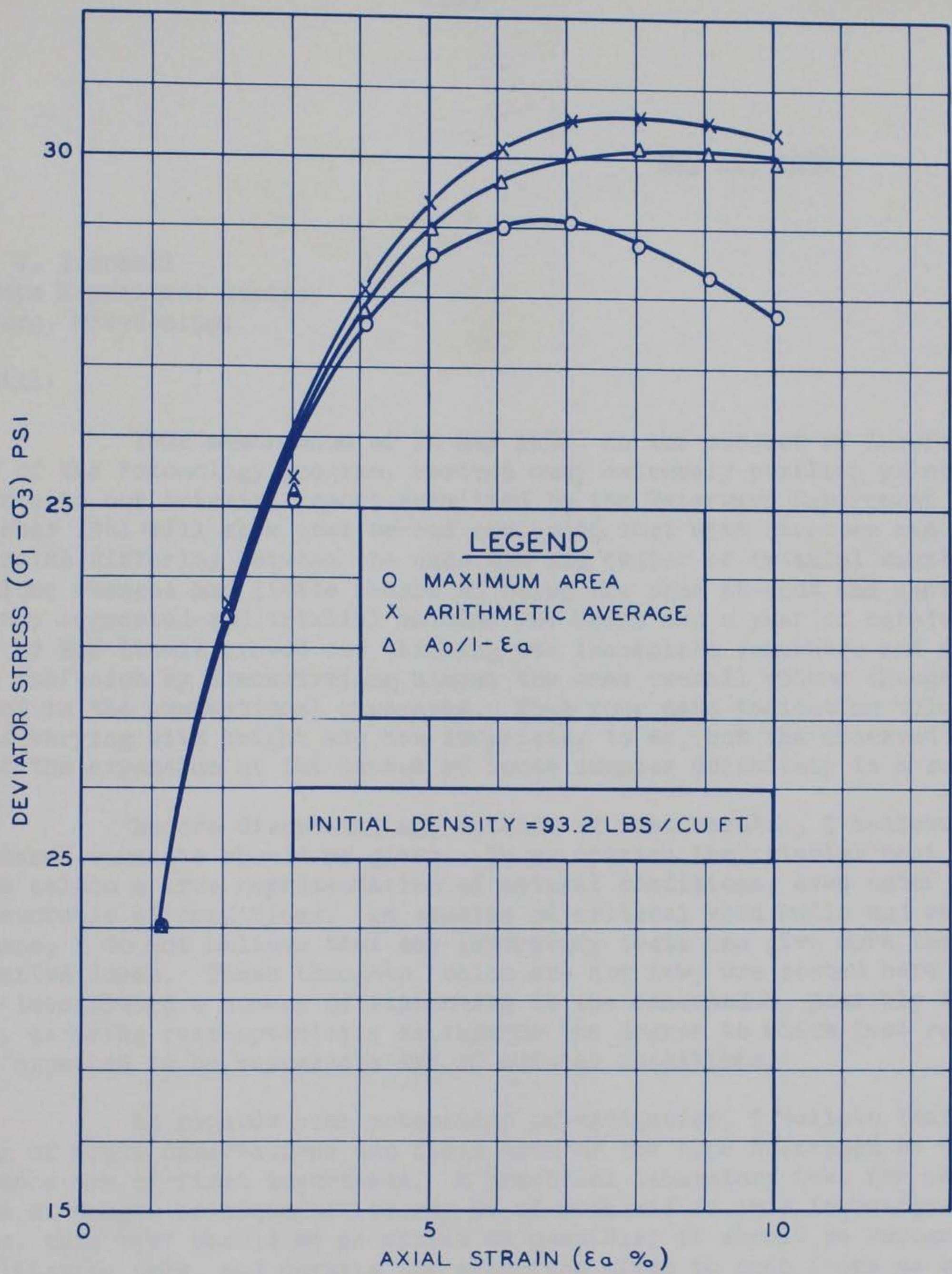
COMPARISON OF STRESSES COMPUTED ON DIFFERENT DIAMETERS

MEDIUM DENSITY



COMPARISON OF STRESSES COMPUTED ON DIFFERENT DIAMETERS

LOW DENSITY



COMPARISON OF STRESSES COMPUTED ON DIFFERENT DIAMETERS

VERY LOW DENSITY

COPY

May 22, 1950

Mr. W. J. Turnbull
Waterways Experiment Station
Vicksburg, Mississippi

Dear Bill:

Your memorandum of 12 May 1950, on the subject of laboratory phases of the Potomology program, revives many extremely puzzling points. Reference to our triaxial report submitted to the Waterways Experiment Station in October 1941 will show that we had concluded that with stresses and shearing strains differing between the ends and the center of triaxial samples, the volume changes had little chance of being the same at ends and center. Our fancy segmented-end triaxial machine was built and a year of careful research by Ken Linell proved our thinking was incomplete somewhere and added to the confusion by demonstrating almost the same overall volume changes as obtained in the conventional apparatus. Thus your data indicating volume changes varying with height are not surprising to me, but the observed magnitude of the expansion at the center of loose samples definitely is a surprise.

Before discussing any details of test results, I believe a few general comments should be given. In my opinion the triaxial test in many ways is seldom a true representation of natural conditions, even under the most favorable of conditions. In studies of critical void ratio and related phenomena, I do not believe that any laboratory tests can give more than rough qualitative ideas. These thoughts, which are not new, are stated here because I have interpreted a number of statements in the memorandum, possibly incorrectly, as being over-optimistic as regards the degree to which test results can be expected to be representative of natural conditions.

As regards your potomology investigation, I believe that the obtaining of field observations and field data of the type discussed at the last conference are of first importance. A practical laboratory test for use as a measure of danger of liquefaction may be of much aid in this investigation. However, this test should be as simple as possible; it should be recognized as qualitative only, and perhaps the attention given to such items as membrane restraint should be kept to a minimum. As you know, I am an enthusiastic proponent of long-range fundamental research, but such work often discloses new questions faster than older ones can be answered, and this can give the impression of negative progress. I have the impression from your memorandum that at this time a review is needed of the relationships of the various phases of the program; that is, field investigations, the laboratory tests for guidance in the immediate studies and the fundamental research.

From a telephone conversation with Professor Arthur Casagrande, I find that he agrees as to the desirability of a conference.

Passing now to discussion of details, I would mention again that your indications of volume always increasing in the failure zone are hard to explain. However, might not a test at an initial unit weight of about 94 lb. per cu. ft. (Fig. 3 is for 96.4) show compression in the shear zone? Perhaps you have demonstrated one item tending to make critical density determinations conservative, but there are so many other unknown items, both conservative and the opposite, that I would say this information makes very little change in my impression of the value or dependability of Casagrande's critical void ratio as a qualitative measure of danger of liquefaction.

My choice of critical void ratio test is still the constant volume test. Since the potomology conference I have come to think of your flow failure concept as defining complete liquefaction and the constant-volume critical void ratio, as defined in the M. I. T. triaxial reports submitted to the W. E. S., as a rough attempt at locating the zero per cent liquefaction point. From your new data, I would expect that you could now also demonstrate that the center of constant volume specimens expands somewhat, with outer portions compressing, but quite possibly the same thing occurs in nature. In nature the compression might extend out from a failure zone to a distance greater than exists in a triaxial sample, giving (as you state on page 3 of the memorandum) a definite difference between laboratory and natural conditions, but again I would say (and this is my answer to your question one-third way down on page 3) that this is just one of many such differences. I would sometime like to see some fundamental research carried out on these points, using constant volume specimens of different heights and obtaining densities as you have done in the constant σ_3 tests (Note: I have assumed the tests are at constant σ_3, although no note is given on the figures and no σ_3 magnitudes have been given).

A point to which I attach much significance is that demonstrated by constant volume tests with numerous small deviator reversals, as given in Fig. 2 of our January 1941 report. These little reversals work a constant volume test to very small intergranular pressures. They surely would make a constant σ_3 test decrease in volume and it's my guess, moreover, that the central zone of a loose sample would decrease in volume under such conditions. Partly because of these concepts I question your suggestion (beginning with "Presumably" middle of page 3) of the volume remaining constant in the failure zone of a sample at the very minimum density. Also I am not persuaded (two-thirds point of page 3) that the early curves of Fig. 6 can be attributed at all to incorrect axial strain data; perhaps we should not yet reject the old idea that interlocking starts to work and introduces expansion after a certain amount of axial strain has occurred.

Your reason for expecting maximum deviator stress at the point of zero volume change is not clear to me. It is my feeling that the maximum

deviator occurs when the curve of ΔV versus axial strain has its maximum slope (covered in my book on page 346).

Comments on your final statements (a) and (b) have been given above. I hope these comments, which have been prepared hurriedly at odd moments over the weekend, are not too disconnected and that they are somewhat in line with what was desired.

You probably already have heard from Dr. Casagrande that, in case you decide to arrange a conference, after June 13 would be a good time for both of us, but dates before then probably could not be arranged.

Yours sincerely,

Donald W. Taylor

DWT:sr

COPY

HARVARD UNIVERSITY

Division of Engineering Sciences

Pierce Hall
Cambridge 38, Massachusetts

May 19, 1950

Mr. W. J. Turnbull
Chief, Soils Division
Waterways Experiment Station
Vicksburg, Mississippi

Dear Bill:

Your Memorandum of May 12 has not helped a bit to cure the skeptical attitude I have had for some time regarding the possibility of deriving from laboratory tests a reasonably reliable criterion regarding the susceptibility of a large mass of saturated sand to liquefaction. Since, on the other hand, Don Taylor does not share my pessimistic viewpoint, it may well be preferable to meet in a conference.

After I returned from a brief visit to Washington, Don called me up this morning to tell me about his telephone conversation with Stan Johnson, and that he told Johnson that he could not attend a meeting in Vicksburg before June 15. The same limitation applies to my time. We agreed that we could attend a meeting on June 16 and 17; and if necessary extend it also to Sunday the eighteenth and Monday the nineteenth. My present thoughts on this entire project are perhaps best presented by summarizing what I tell my students in greater detail in my lectures.

In my considered opinion it is not possible to determine positively by any laboratory tests which have so far been used or suggested, whether a given sand deposit is susceptible to liquefaction. I believe that the original laboratory method for determining the critical porosity of sand, which I developed, has since been proven to give results which are on the unsafe side. The best we can hope for is that a long-range program of laboratory investigations combined with field investigations will eventually lead to a laboratory test which will identify reasonably well critical conditions. However, at the present time we are obliged to rely on empirical criteria derived directly from the study of flow slides.

On the basis of a review of the case histories of the Ft. Peck dam slide, of the flow slides in the province of Zeeland, Holland, and of the recent bank failures along the Mississippi River, I consider the following conclusions as strongly indicated by the available evidence:

(1) The porosity of river-deposited fine sands can vary widely within the same stratum, ranging from loosely deposited sand which is definitely susceptible to liquefaction, to medium dense sand which will not liquefy under any type of disturbance.

(2) There is no sharp demarcation between the ranges of porosities for which a given sand is susceptible or non-susceptible to liquefaction. There seems to be a range of porosities within which the degree of sensitivity of a sand to liquefaction gradually increases from non-sensitive to extremely sensitive. The sensitivity can probably be best expressed by the magnitude of strain required to cause the transfer to the pore water of a major portion of the stress which is carried by the grain skeleton. Saturated, fine uniform sand in an exceptionally loose condition, particularly in a bulked condition, and also deposits of rock flour, may liquefy as a result of a single shock or or vibration, i.e., due to only a very small strain in the sand. However, deposits of loose river sand which are susceptible to liquefaction appear to require substantial strains so as to effect sufficient loss of shear strength to create a flow slide.

(3) The speed with which a mass of sand liquefies depends on the manner in which the strains are developed within the mass. If the strains affect simultaneously a large volume of sand, the flow slide will move very rapidly, and will usually be completed in a matter of minutes. If critical strains are developed progressively such that at a given moment only a small volume of sand is being liquefied, then a flow slide will progress slowly and, depending on the total mass of sand involved, may continue for a period up to many hours.

In the attached Table I, I have attempted to classify the sensitivity of soils into three groups and have tabulated pertinent information to illustrate the three types. Supplementary discussion of the examples of flow failures cited, which is intended to bring out the characteristic differences, is presented below.

During the construction of the Ft. Peck Dam a weakness in the Bearpaw Shale resulted in the development of strains in the overlying foundation sand and in a portion of the upstream section of the dam. For days before the failure, maintenance crews observed movements in railroad tracks within the area of the upstream slope which failed. These strains probably caused a drop in the shearing resistance within that portion of the dam and the underlying sands, which in turn caused an increase in the shearing stresses in the foundation. These two effects finally culminated in a combination of shear failure in one or more of the Bentonite layers of the shale, and liquefaction of a large mass of overlying sand, both in the foundation and in the saturated upstream portion of the dam. I consider it most likely that large strains have preceded the liquefaction of the sand and that if the underlying rock had been strong enough to carry the developed shear stresses with only insignficant strains, liquefaction of the sand would not have resulted. On the other hand, if all sand which was affected by the flow slide had been in a sufficiently dense state to assure safety against liquefaction, I believe that a shear failure in the shale would not have resulted in a catastrophic failure of the dam.

The flow slides along the shores in the province of Zeeland in Holland are described in a publication by Koppejan, Wamelan and Weinberg in Vol. V, pp. 89-96 of the Proceedings of the Second International Conference on Soil Mechanics and Foundation Engineering, Delft, 1948. The sand strata which are affected by these slides are old river deposits. The slides appear to start either by steepening of the shore line due to wave erosion, or due to seepage pressures which develop during falling tide. Strains are created in the mass immediately adjacent to the slopes, which in a very loose sand may lead to the collapse of the grain structure. As this portion of the sand liquefies and flows out, an adjacent volume of sand loses its lateral restraint and in turn is subjected to strains which lead to its liquefaction. In plan such flow slides are fan-shaped, the width increasing toward the land slide. After the failure the average slope of the ground surface in the area where the mass has flowed out is only a few degrees, proving beyond any doubt that the material which flowed out must have been in a liquefied state. The largest of these slides has displaced about 4,000,000 cu yds over an area of about 50 acres. These flow slides progress at a rate of about 160 ft per hour, thus relatively slowly.

The Mississippi River bank failure at Free Nigger Point may have started as an ordinary shear slide. At any rate, it is probable that the steepening of the banks has developed large strains. If the river then scoured into a pocket of sand susceptible to liquefaction, those strains could start a flow slide which would continue until such a pocket were removed by progressive liquefaction. In plan the bank failures along the Mississippi are similar in appearance to the failures in Holland. Quoting from your and Senour's paper, Vol. VIII, pp. 117-121, Proceedings of the Second International Conference on Soil Mechanics and Foundation Engineering, 1948: "Both areas resemble a cup or pocket ... having narrow openings or gullies through the outboard rim, which again flares or broadens after passing through the gorge."

Perhaps I can state briefly the conclusion which I have drawn from the available evidence, as follows: Deposits of loose, uniform, fine river sands are capable of liquefaction, but only if subjected to large strains, such as would result from steepening of slopes or from large strains in underlying weak strata. The implications of this conclusion are chiefly of importance in connection with the design of earth dams or foundations consisting of river sands. Today I feel much happier about the safety of the foundations for the Missouri river dams than I did a few years ago. This change has brought me much closer to Dad Middlebrooks' viewpoint, although it will be up to Dad to decide whether the canyon which has separated us on this question has been narrowed to the point where we can actually shake hands.

I realize that what I presented above is not helping you very much in connection with your problem of identifying conditions which may lead to river bank failures; yet I believe that the chances are better for arriving at useful information by further studies of field evidence than by laboratory tests.

Sincerely yours,

A. Casagrande

Copy to Taylor

TABLE I

Flow Slides in Soils

Sensitivity of Soil to Liquefaction	Soils Which May Be Affected by Indicated Types of Flow Slides	Character of Strain Necessary to Start Flow	Character and Speed of Flow Failure	Examples of Flow Failures
High sensitivity "Type A"	Sand in bulked condition; rock flour	Small strains, such as earthquake shocks, explosions or vibrations, simultaneously affecting a large mass	Rapid flow (a few minutes)	Flow failure of railroad embankment in Holland (1918); silt flows in Laurentian Mts.
Low sensitivity "Type B"	River sands; rock flour	Large strains*) created simultaneously in a large volume; e.g. shear failure in clay or shale transmitted into underlying sand	Rapid flow (a few minutes)	Ft. Peck dam (river sands in foundation and hydraulic fill sand in dam)
Low sensitivity "Type C"	River sands; rock flour; varved silts and clays; clays having very great sensitivity to remolding	Large strains created progressively	Progressive liquefaction; up to several hours' duration depending on mass involved	Mississippi river bank slides; flow slides in Holland; flow slides in "bull's liver" and in varved clays in excavations

*) Large strains may be invited by intrusive pore pressures created for example in a varved clay. However, these intrusive pressures are only indirectly responsible for subsequent liquefaction in silt layers, or layers of super-sensitive clays.