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Closing Discussion

Indexes
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(JOURNAL Vol. 16)

to provide a comradeship in finding the best ways to do concrete work of all kinds and in spreading that knowledge

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Every year the Institute receives many requests for the Title Page and Index of the annual Proceedings volume long after inquirers have received them; also many inquiries about binding annual sets of JOURNALS and buying bound volumes from the Institute. Within the yellow covers of this annual JOURNAL *Supplement* are Title Page, Table of Contents, Concluding Discussion and Indexes for the volume otherwise completed with the June, 1945 JOURNAL. On the back cover of this *Supplement* is an announcement about the availability of bound volumes from the Institute, about "gathering" the contents of a year's JOURNALS, plus this *Supplement* for binding a *Proceedings* volume. Please see announcement on the back cover.

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7400 SECOND BOULEVARD, DETROIT 2, MICHIGAN (Supplement) November 1945

Discussion of a paper by W. D. Bigler:

Preparation of Technical Papers*

By WILLIAM C. SPIKER†

The "Preparation of Technical Papers" by Mr. W. D. Bigler is, in the writer's opinion, a very important contribution to the advancement of engineering. The recommendations made are clear, positive and constructive. Until very lately the subject has been slighted.

For the purpose of helping to promote interest in the subject the writer makes the following suggestions:

The author of the paper, by adhering closely to the general construction of the sentences criticized has, while making the sentences technically correct, failed sufficiently to accentuate the economic waste resulting from involved, irrelavent, trivial and false statements.

Let us look at the problem from an engineering point of view. The value of a sentence is directly proportional to the value of the fact stated or the idea formulated; but, its value is reduced in direct proportion to the difficulty involved in understanding it.

The writer has borrowed two of the authors examples of poor writing to illustrate the point he wishes to emphasize:

First Example—"For each of the four types (of haunch shapes in beams) the shape of a member is characterized by two ratios, a representing the length and b the depth of the haunch. The value of $b = \frac{\text{min. I}}{\text{max. I}}$ is assumed to equal the ration of $\left(\frac{\text{min. d}}{\text{max. d}}\right)^3$, the beam width being considered constant and reinforcement disregarded."

These sentences suggest a good example of "double-talk." For, even if ratios could characterize beam shapes, if the ratios which a and b represent had been clearly stated and if the depth of a haunch could be an abstract proper fraction there would have been no good excuse for writing the sentences. It is the use of the "two ratios" that is important not their existence. Therefore, the value of these two sentences, by both tests, is zero or less.

*ACI Journal, Sept. 1944; *Proceedings* V. 41, p. 1.
†Consulting Engineer, Atlanta, Ga.

Second Example—"When blocks are placed and calculations for permissible loads are made as recommended, safe unit loads for spaced columns result (so far as strength or resistance to lateral buckling in a direction perpendicular to the mutually contacting surfaces of the blocks and individual members is concerned) and it is unnecessary to check the unstaid portions (from center of bolt in end blocks to center of spacer block) of the individual members".

The author of this sentence had something of value to tell but he failed so completely in telling it that the value of the sentence is zero. The writer is of the opinion that something like the following was intended:

If blocks are spaced as indicated in the schedule and bolted as shown in the sketch, the only calculation needed is to insure safety against bending edgewise of the timbers.

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Discussion of a paper by Paul Rogers:

Two-Way Reinforced Concrete Slabs*

By J. B. MACPHAIL† and AUTHOR

The author has performed a useful service in making the work of Dr. Marcus available in English, but his presentation of it has not the clarity which the subject deserves. Some of the definitions appear, on comparison with the numerical examples, to need amendment; and the notation is unnecessarily complicated.

The writer therefore proposes to suggest some changes which may help others in getting a better understanding of a useful paper. If any seem trivial, it should be remembered that many readers, particularly the younger ones, begin study of a paper with the presumption that the author is right, and they may go through some anxious moments before they feel sufficiently sure of their ground to correct him.

In the definitions, M_A and M_B seem to mean maximum positive moments in the A and B directions per unit width of uncut slab, using the customary convention that positive moments in a slab make it concave on top. Likewise m_A and m_B seem to be maximum positive moments on strips of unit width cut at the middle of the slab and of lengths A and B, as shown in Fig. 1, when subjected to loads w_A and w_B respectively and the end restraints appropriate to the case considered. The suffix or subscript *max* can then be omitted, because the paper considers no positive moments other than maximum ones.

The coefficients ϕ and γ appear to apply only to maximum positive moments; and not to negative moments, nor to positive moments other than maximum ones.

Briefly, the moments considered are particular ones, under restrictions of place, magnitude and sign, but the definitions do not so describe them.

*ACI Journal, Sept. 1944; *Proceedings* V. 41, p. 21.
†The Shawinigan Engineering Co., Ltd., Montreal, Que.

In the tables 1a, 1b and 1c, the use of italic alphabetical subscripts with n and z instead of numerical ones would bring the notation into conformity with the alphabetical designations of the cases and with the subscripts in the test, and would save the reader from searching the text in vain for a definition of the numerical subscripts.

To drop the subscripts for cases entirely would be still better, both in tables and test. There are plenty of precedents to support such a proposal.

Torsional moments seem not to reduce negative moments, so moments at edges which are fixed appear to be calculated on the basis of strips of unit width as $-w_A A^2/8$ or $-w_A A^2/12$, according as the opposite edge is free or fixed: with corresponding expressions for the other direction.

There are some other minor discrepancies but they will not cause difficulty.

The writer unfortunately has not convenient access to a copy of Dr. Marcus' book, so he will be glad to have either confirmation or correction of this statement of his understanding of the matter.

AUTHOR'S CLOSURE

Mr. Macphail points out that some of the notations of the paper are not clear and are misleading.

M_A or M_B could be substituted instead of M_A *max* and M_B *max*, although it is the author's intention to emphasize that these Moments are the maximum. The purpose of the paper is to provide for a working formula for practical designing and not a mathematical analysis. Thus the solutions are made only for maximum moments. The moment-diagram follows the usual parabolic curve and it is an easy matter to establish moments at different points.

The negative moments act at the supporting beams, where the torsional moments have zero value. (See diagrams) Obviously these negative moments cannot be reduced, and their value is obtained by standard procedure.

The author wishes to mention that he prepared the paper with the approval and suggestions of Dr. Henri Marcus himself, whose presence in this country is a great benefit to engineering science.

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Discussion of a paper by Leonard Bean and J. J. Tregoning:

Reactivity of Aggregate Constituents in Alkaline Solutions*

By BRYANT MATHER†

The purpose of the following discussion is to direct attention to certain relationships between the materials opal, chalcedony, and chert; and to suggest how these relationships may elucidate the reported behavior of these materials in alkali-aggregate reactivity.

Throughout the paper under discussion, opal, chalcedony, and chert were reported to have behaved in a manner differing considerably from that of all the other materials tested. From their discussion it appears that the authors regard the reactivity of chalcedony and chert as surprising. They suggest that the reactivity of the Potomac River chert appeared to be due to the presence of chalcedony, since the reactivity of the chert samples was in the same order as the amount of chalcedony present.

Contributors to the discussion of alkali-aggregate reactivity agree generally that opal is the most reactive material yet encountered. Blanks (1)‡ has commented that very small amounts of opal appear to be sufficient to impart activity to an aggregate in the presence of a high-alkali cement. In view of the well-known bad record of opal, there are set forth below certain data regarding that material and the interrelationships of the materials opal, chalcedony, and chert.

Opal has been generally considered to be an amorphous material, even though, at room temperature, its powder has given the x-ray picture of a high-temperature cristobalite (2). In any case, such evidences of submicroscopic crystallinity as have been found suggest affinities with a crystalline form of silica other than quartz. The water content of opal has been reported (3,4) to vary from 2 to 13 per cent, and, as the water

*ACI Journal, Sept. 1944, *Proceedings* V. 41, pp. 37.
†Engineer, War Dept., Corps of Engineers, Central Concrete Laboratory, Mount Vernon, N.Y.
‡See references at the end of this discussion.

content varies, the density varies from 1.9 to 2.3 (5,6), and the index of refraction from 1.406 to 1.460 (4,5).

Chalcedony has been defined as a fibrous, microcrystalline form of silica which is formed through the crystallization of colloidal silica (7). It is supposed to be specifically distinct from quartz since it does not invert at 575 C., yet x-rays reveal the same crystal structure as quartz, while both the index of refraction and the density are lower than quartz (4). The density of chalcedony varies from 2.55 to 2.63 and the principal indices of refraction are $n_o = 1.537$ (1.533 to 1.539) and $n_e = 1.530$ (4). The fibrous structure, density, and optical properties of chalcedony have been investigated by Donnay and by Correns and Nagelschmidt. Donnay reports (5) that the fibers of chalcedony are composed of low-temperature quartz and a continuous interstitial material which is probably opal. Correns and Nagelschmidt (8) established by x-ray study that the *c* axes of the constituent quartz crystals, which in large part make up the fibers of chalcedony, are always oriented normal to the elongation of the fiber, and they state that the fibers clearly ("offenbar") contain opal. Donnay (5) shows that the percentage of the interstitial opal, among the quartz crystals which make up the bulk of the fibers of chalcedony, may be calculated from the departure of the measured values for density and indices of refraction from those of quartz. Donnay calculates that the material studied by Correns and Nagelschmidt contains from 8 to 15 per cent opal. In this connection it should be mentioned that the individual particles composing the chalcedony fibers are of sub-microscopic size; whether they are small in comparison to the wave length of light, as hypothecated by Correns and Nagelschmidt, is however open to question. Donnay concludes (9) that if opal is bad, then chalcedony is bad too, on account of its opal content.

The data cited serve to elucidate not only the behavior of opal and chalcedony in the tests reported by the authors but also shed some light on the discussion offered by Rhoades (10) in which he suggested that: "Typical chalcedony perhaps is no more to be suspected of potential reactivity than quartz. However, authoritative conjecture surrounds the possibility that some chalcedony verges toward an opaline character in becoming hydrous, and the question naturally arises as to the possibility of such types also assuming other similarities to opaline silica, perhaps even becoming potentially reactive in alkaline environments. Confusion surrounds this issue, partly because the chalcedonic material is frequently so fine-grained as to preclude satisfactory microscopic study; it is possible that opaline material minutely disseminated in chalcedonic aggregates may escape observation and yet impart opaline properties which appear to pertain to the enclosing chalcedony. This point impinges directly on the problem of the potential reactivity of cherts. . . "

Of all the terms borrowed from petrography for use in discussions of aggregates, chert is perhaps the most difficult to define adequately. Wuerpel and Rexford (11) give an authoritative collection of definitions and descriptions of chert. The material studied by them was cryptocrystalline and composed principally of quartz. They report that opaline silica did not occur in any of their samples in sufficient quantity to be identified. Rogers and Kerr (12), however, regard chalcedony as the principal constituent of cherts. Twenhofel (13) says that while the silica in Mesozoic and older cherts is crystallized and consists of a mosaic of quartz and chalcedony, some of the younger cherts contain opal. The California cherts that have been found to be reactive, and that are known to contain opal, are younger than the Mesozoic. Trask and Patnode (14) studied 25 samples from the Miocene "Monterey" chert formation in California and found them to be separable into two groups: banded or layered chert, and opaline concretions.

The material known as chert, therefore, includes some samples that are predominantly quartz, some that are predominantly chalcedony and thus contain up to 15 per cent of submicroscopic interstitial opal, and some that are predominantly opal.

The writer agrees with the suggestion of the authors that the reactivity of the Potomac River chert is a function of its chalcedony content. He would however go further and suggest that the potential reactivity of the mineral, chalcedony, or of any of the forms of the rock, chert, is a function of the amount of opal contained.

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Note: J. D. H. Donnay, "Form Birefringence of Nemalite," University of Toronto, Geological Series, No. 49, pp. 5-15, 1945 (Contributions to Canadian Mineralogy), (Universite Laval, Quebec, Géologie et Minéralogie, Contribution No. 59). This paper illustrates the application of the concept of form birefringence, refers (p. 11) to "chalcedony, a mixed substance composed of quartz and interstitial opal." It is written in English.

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Discussion of a report by ACI Committee 212:

Admixtures for Concrete*

By JACOB FELD, EMIL SCHMID, R. R. KAUFMAN,
DONALD MacPHERSON and F. B. HORNIBROOK, Chairman,
R. F. BLANKS and E. W. THORSON for COMMITTEE 212

By JACOB FELD†

The excellent report by Committee 212, covering as it does all possible types of admixtures brings to mind an experience in concrete production and use which may add a thought to this report.

During the construction of a large Naval Training Station, where some of the roads were of concrete and where concrete was produced in paver-mixtures, an unexpected accelerator was found to be the highly chlorinated water. The water supply system to the Station included a network of second-hand steel pipes reclaimed from previous construction projects. The only economic water source for the concrete operation was to tap these mains and to a large extent the paving operation was governed by partial completion of the water supply. Until such time as a complete section of the water distribution system was completed, the water taken from a large interior lake was found perfectly satisfactory even though it had a high salt content. However, when the pipe lines were disinfected by injection of chlorine at the pumping station until the system retained a proper residual after twenty-four hours rest, the concrete showed a rapid setting with air temperatures ranging between 50 and 60 F., that it was almost impossible to finish the surface properly. The water at that time contained about 5 parts per million of chlorine. With a chlorine content of 1 part per million, the accelerating affect was practically eliminated.

The thought arises whether chlorine content in potable water has ever been considered in tests for the setting of cement and whether chlorine solutions have the same effect on concrete as calcium chloride solutions

*ACI JOURNAL, Nov. 1944; *Proceedings* V. 41, p. 73.
†Consulting Engineer, New York, N. Y.

If there is no difference in action, weak chlorine solutions may be a simple and possible economical accelerator.

By EMIL SCHMID*

Progress in any field of science can be assured only if developments are investigated, facts reviewed, and the possibilities and limitations of new materials and methods established. The American Concrete Institute is to be complimented on being one of the first engineering organizations to acknowledge formally the existance of so-called "admixtures". Committee 212 has rendered a service to the construction industry by bringing the admixture question into the open and writing an informative report covering the entire field. This report forms an excellent basis for discussion and further investigation.

The claims made for admixtures, often supported by results of extensive tests, usually cover a wider field than is given under "General, A to K". Some of the effects produced by admixtures cannot be obtained by other means, regardless of economic considerations, without undesirable influences on other properties of concrete. An admixture may improve certain qualities of concrete sufficiently to make such concrete acceptable, in spite of the use of unsuitable or not well-graded aggregates or poor workmanship. This statement should not imply that admixtures can be "cure-alls", but only that in some cases their use may mean the difference between acceptance or rejection of a concrete or structure.

In addition to the modifications mentioned in the report, the following improvements, which certain admixtures can produce, might be included: 1) Increase in ultimate strength, as opposed to acceleration of the rate of strength development. 2) Increase in surface hardness, either through increased density or reduced formation of scum, or a combination of both; 3) Increase in bond to previously-placed concrete; 4) Decrease in formation of scum and laitance which may or may not be brought about by reduction in bleeding; 5) Decrease in drying shrinkage and plastic flow; 6) Improvement in the uniformity of concrete and reduction of segregation of concrete in its plastic state.

The use of a fatty, cohesive, plastic concrete does not necessarily guarantee the uniform quality of a structure. In spite of close control of the weight and quality of the ingredients and the mixing time of concrete, unavoidable variations may occur. Such changes influence the action of some types of admixtures greatly, but have little effect on the action of others.

It is advantageous to use products which decrease rather than increase the susceptibility of concrete to changes in gradation, water-cement ratio,

*Chemical engineer and general manager, Sika Chemical Corp., Passaic, N. J.

mixing time, or manner of placing, regardless of whether or not they produce a fattier and more cohesive mix.

It is quite correct that the exact degree of improvement with any admixture cannot be predicted generally, and the results depend on many factors which vary from job to job. Tests can be made, or are available, which will indicate the minimum degree of improvement, if any, under anticipated conditions. These facts, however, apply to any one of the ingredients of concrete; be it aggregates or cement, and variations are not confined to admixtures alone.

It is also correct that necessary minimum results can often be obtained more economically by the use of means other than admixtures, such as by changing proportions or methods of placing. Other desirable, although not necessarily essential, benefits should be considered. In many cases, they can be obtained at a very slight increase in first cost and may be instrumental in reducing costly repair work later on. The cost of an admixture should be compared not only with the cost of the concrete itself, but with the value of the completed structure, the accessibility for repairs, the possibility and cost of shutdowns, etc.

It is stated that commercial admixtures may contain materials coming under two or more classifications. Generally speaking, the action of each constituent is independent of the action of the others. Increased ultimate strength of concrete will usually be obtained only when using a combination of a workability agent and an accelerator if the workability agent used alone will increase the strength. It will facilitate classification and evaluation if the constituents of such combinations are classified and evaluated separately, and afterwards the combined effects of the commercial product be studied. Long-terms tests are necessary to evaluate properly admixtures containing accelerators, but comparative tests of accelerating and retarding agents should only be made when the concretes in which they are used have attained a similar degree of hydration. Concrete containing an accelerator may attain 75 percent of its one-year strength (assumed end of hydration) after seven days; plain concrete, about 50 percent; and concrete containing a retarder, only 40 percent Tests at such early ages for resistance to sulphates or freezing and thawing will not be representative of the ultimate resistance of the concrete containing the retarder and will not reflect accurately what will happen in the field.

When considering strength, distinction should be made between compressive and flexural strength, as they may be influenced differently by various type of admixtures. The compressive strength usually responds more to a change in water-cement ratio than the flexural strength does, whereas; the latter may be more favorably influenced by gas-forming products.

Bleeding seems to be a very controversial subject. To a certain extent and usually within practical limits, bleeding can be controlled by changes in the proportion of coarse and fine aggregates. Entirely non-bleeding concrete is ordinarily as undesirable as one which bleeds profusely. A certain quantity of free water is necessary to finish floors, for instance. In other cases, bleeding may be of benefit by eliminating part of the surplus water needed for proper placing. Absorbent linings or other means to withdraw water after placing will not produce satisfactory results unless the concrete bleeds.

As mentioned in the report, bleeding is detrimental if it is the cause of segregation. Formation of excessive scum or laitance can be prevented by the use of certain compounds which prevent clinging of the cement and clay particles in suspension to escaping air without reducing the bleeding of clear water.

The statement that some retarders increase and others decrease strength can be explained by considering their action on the density of concrete. Retarders, which lower the surface tension of water and therefore entrain air, reduce the unit weight of concrete, particularly for concretes of unchanged water-cement ratios. Automatically, this results in a lowered strength. Other retarders, by reducing inclusion of air and possibly increasing bleeding of clear water for an unchanged water content, increase density and therefore strength.

The above also serves as an explanation to the remarks in regard to the action of the sulfonated type of admixture compared to the carbohydrate type mentioned under water-reducing agents in reference to workability and other influences. The carbohydrate type, being a non-foaming agent, will increase fluidity but not fattiness of a harsh mix and is therefore, recommended for use only in concrete containing a sufficient quantity of fines (cement or inerts) to insure proper gradation.

Air entraining agents or products decreasing the surface tension of water increase the resistance to freezing and thawing in lean mixes. The carbohydrate type will increase the resistance to freezing and thawing for lean mixes only to the extent of the reduction in water made possible by its use. Its beneficial influence on the impermeability and resistance to freezing and thawing is much more pronounced with rich concrete.

It is very difficult to classify all products in a few groups and, regardless of the way this task is undertaken, overlapping and repetition will occur. The report tries to solve this problem by classifying effects, and then mentions the class of products which have been proposed or are used to produce these effects. This classification might be supplemented by one which describes the effects which a certain class of products produces.

Such classification could be broadly subdivided into two main classes: 1) insoluble materials; 2) soluble compounds

Insoluble materials would comprise products which have no physical or chemical effect on cement during mixing, placing, or early hydration of concrete, although they may later react with certain constituents of cement, may oxidize or carbonate in the presence of moisture and air. These products are used either as pore-filling compounds to adjust the gradation of the aggregates or proposed for special purposes. Insoluble materials might be subdivided into: 1) inert compounds; 2) cementitious compounds; 3) expanding compounds.

Soluble products could be classified as: 1) products acting chemically; 2) products acting physically; 3) combinations of physical and chemical action.

Products acting chemically might be divided into: 1) accelerating compounds; 2) gas-forming compounds; 3) saponifying compounds; 4) other compounds.

An accelerator, such as calcium chloride, acts by combining chemically with the compounds contained in cement, and the main effect produced is acceleration of hardening.

Gas forming materials would be materials, which, due to a chemical action between the alkali contained in cement and themselves, form gases, which are liberated during and after mixing of concrete.

Saponification is also due to the action of alkali with the admixture. Soap is formed which entrains air during mixing only.

Chemical action is dependent on the composition of cement and the temperature and is, therefore, not uniform with all types of cement and under all conditions.

Products acting physically might be divided into products which: 1) decrease surface tension of the mixing water; 2) increase surface tension of the mixing water, or have no effect on the surface tension

Organic, soluble compounds ordinarily used to produce these effects seem to increase dispersion of cement and do not combine chemically with cement, although, by their influence on the surface and colloidal properties of cement, they may change the rate or manner of its hydration. They are added in very small percentages.

Products of a polar nature, such as sulfonated or carbo-acid compounds, influence setting time, hardening rate, density, and workability of concrete, as explained previously.

In addition to the suggestion that the present classification be supplemented by the above, it might be advisable to include in the report a section dealing with the general desirable requirements of an admixture such as; 1) It should be easy to handle and store. It should not deteriorate, harden, or become lumpy when exposed; 2) It should be distributed throughout the concrete within the regular mixing time. 3) It should be manufactured under close control to insure uniform composition so that results are more or less predictable and only vary with job conditions. 4) An excess of the ordinarily-recommended quantity added to the concrete should not produce any marked disadvantage, and at least not harm the ultimate quality of the concrete; 5) The admixture should have a distinct color, so that, when it is added to cement, aggregates, or gaging water, unskilled labor will not add twice the quantity or none at all. 6) Results should be consistent. Variation in the degree of improvement might occur due to job conditions, but in no case and with no type of cement, should the quality of the concrete be lowered.

As a further step, it might be considered that the Committee review admixtures, if requested by manufacturers, similar to reviews now made possible by the American Society for Testing Materials for additions to cement. A summary of such reviews might be published and a file maintained by the Committee for each admixture, with the cooperation of the manufacturers.

By R. R. KAUFMAN*

In its first report the Committee has made an excellent start in the matter of publishing thoroughgoing, disinterested and constructive information, on a subject that has been left relatively untouched. Consumers will benefit directly by any program designed to established the facts in a field of growing importance, and producers of Admixtures will be helped by the decision of the Institute to assign to the subject a Committee which can become a qualified and impartial authority, lack of which in the past has resulted in reliance by some inquirers on sources of information not always qualified and impartial.

Inasmuch as this report will reach many only partly acquainted with the subject, the writer believes that some of the statements can be clarified or re-stated for the benefit of the layman, who may not understand that "variability" and "lack of desirable properties" are attributes of plain concrete mixes, otherwise there would be no need for admixtures.

The first clarification might well be of the definition of the materials included: "According to this definition, materials are included which are added to the mixing water, which are added to the batch before or during mixing or which, if their presence in concrete significantly affects the properties of the concrete, are interground with the clinker (excepting gypsum used in the normal manufacture of cement)". A superficial reading of this has left several readers with the understanding that only those admixtures which are interground in portland cement during its manufacture significantly affect the properties of the concrete. This is

*Chief engineer, the Master Builders Co., Cleveland, Ohio.

obviously not the substance of the statement, but the possibility of misinterpretation suggests the advisability of re-stating it more clearly

The fact is that, today, most of the admixtures which are interground with the cement clinker are those which affect the properties of concrete insignificantly. Examples of this type are cements interground with small amounts of fly ash, volcanic ash, oil shale or diatomaceous earth, or those which employ small amounts of grinding aids to increase the output of the grinding mills but which are of such character or amount that they do not materially affect the air entrainment characteristics of the cement. On the other hand, materials which do significantly affect the properties of the concrete, with the exception of air entraining agents, are rarely interground in the cement. As an example, calcium chloride, which has a most significant effect on concrete, is rarely interground.

The statement on page 74, "Accordingly, specific effects which result from the use of an admixture can seldom be predicted accurately. Usually, tests of the particular lot of materials representative of those for a given job, sometimes tested under similated job conditions, offer the only means for obtaining reliable quantitative information on the properties of concrete containing admixtures", is one which may lead the reader to believe that the use of admixtures increases rather than reduces the variability of concrete. Once the characteristics of a mix are known, the results from the addition thereto of the admixtures can in most cases be predicted with greater certainty than can the results produced by a normal mix consisting of the usual variable factors; [(1)] portland cement, sand, aggregate, mix design and labor. For example, knowing the gradation of the aggregate and mix design, the effect of the use of small amounts of finely divided materials such as fly ash, diatomaceous earth, talc or oil shale can be quite definitely predicted. The effects of triple-pressed stearic acid paste can also be predicted about as accurately as the effect of any particular brand or lot of portland cement.

Under Group 2- Air Entraining Agents, p. 78, the statement is made, "However, by using a coarser aggregate-gradation (less sand) and by keeping the slump the same or slightly less than that required when not using an air-entraining agent, the desired strength can be maintained with little or no increase in cement content and usually with an improvement in workability". Most authorities on air entrained concrete report that mixes containing 5½ to 6½ sacks of cement per yard with the sand aggregate ratio reduced (from a proper sand aggregate ratio in the untreated mix, rather than an over sanded mix) and containing 3 to 4 percent of air will result in a concrete which will have 10 to 15 percent less compressive strength than an untreated mix of equal cement content.

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(1)Discussion by S. P. Wing, "A Study of the Cause of Non-uniformity in the Compressive Strength of Concrete Pavement Cores", ACI JOURNAL, June 1942, *Proceedings* V. 38, p. 148-1.

Concerning the consistency and advantages of cement interground today with air entraining foaming admixtures, there is a considerable difference of opinion. The success of the attempt to produce an all-purpose cement with air entraining properties is a favorite topic for argument with concrete technicians and engineers familiar with the subject. Authorities have voiced the belief that the most satisfactory method of getting controlled air entrainment in concrete mixes is to add water-soluble air entraining agents at the mixer in the proportions required by that particular mix.

It has been shown by H. F. Gonnerman in ACI JOURNAL *Supplement*, Nov. 1944, that it is possible to reduce sand sufficiently in air entrained laboratory mixes to offset the retrogression in strength normally secured. This series of tests uses sand aggregate ratios as low as 22 percent (in combination with 1-in. top size gravel) which, while giving data of interest, cannot often be employed on a job.

In classifying the admixtures the title "Gas-forming agents" was selected by Committee 212 to cover materials which prevented settlement shrinkage in plastic concrete and caused expansion. In the text, specific reference is made to the grouting of machinery. The authors correctly cover the use of aluminum powder under the category of gas-forming agents but do not point out the difficulties which may be experienced and limitations of such a material and method when employed to grout heavy industrial equipment having vibration and impact. Aluminum powder when used as a gas-forming material in grouting mixtures depends upon temperature for proper reaction as is pointed out in the article. It also depends upon the rate of hydration (hydroxal ions available) of the cement much in the same manner that water-insoluble resins, fats, and oils used as foaming agents depend upon the alkali and alkaline-earth constituents and rate of hydration of the cement for proper reaction.

Therefore, the amount of the addition of aluminum powder used with various brands of cement must be determined at the temperature, water cement ratio and under the conditions it is to be used prior to its use.

The Committee should be familiar with and in a position to provide a classification for an admixture which is not gas-forming and is of the expanding type. Such an admixture has been used extensively, especially in the industrial field for grouting bases of continuous strip rolling mills, super-hyper compressors and other equipment which transmits considerable vibration and impact to the grout and therefore requires a non-shrink, high strength, ductile grout to endure. Such a type of admixture, graded metallic aggregate with agents to promote oxidation and other constituents, has been used successfully for the past 19 years.

Under Group 5—Pozzolanic materials, the statement is made very properly that, "In all cases, however, prolonged wet curing is necessary for development of potential strength." The writer believes that a warning should be added, covering the subject of pozzolanic materials where the amount used is substantial, that prolonged wet curing is essential to assure prevention of excessive cracking due to drying shrinkage which may and frequently does take place prior to the silicious pozzolanic material entering into combination with the lime of the cement.

Reference is made to the effect of a water-reducing agent (8-b) of the sulfonated type which causes mild air entrainment and effects "an improved resistance to freezing and thawing". Having seen some of the data on this point that are available to the Committee, the writer believes the quoted phrase is highly conservative. Some of the data in question appear on pages 402-5 of the Feb. 1945, issue of the ACI JOURNAL and show that in controlled freezing and thawing tests with 13 typical brands of cement the admixture produced concrete with a durability factor of 80 after 176 average cycles, as compared with a durability factor of 20 after an average of 50 cycles for the corresponding untreated mixes.

These same data and additional findings in tests by the same authority would seem to show that use of this particular admixture involves little or no risk should circumstances make it inconvenient or impractical to make tests "with the job cement and preferably job materials before their use is attempted". Field experience with this particular admixture in some 6,000,000 cu. yd. of concrete on private and public works in the last ten years indicates plans and specifications can call for its use with complete confidence that no variables other than those controlled by the routine design and physical tests will arise in the course of the work.

In the above, the writer has referred to a specific admixture rather widely known in order to illustrate the conservative approach the Committee has properly made to a subject new on the Institute's agenda, and for which as well as for a completeness and accuracy unusual in an initial report, it deserves the highest compliment.

BY DONALD R. MacPHERSON*

The report "Admixtures for Concrete" is indeed a worthwhile contribution to the literature; in fact it is one of the very few informative papers on the subject. Since any classification of admixtures is difficult because of the large number of different types many of which produce more than one effect in concrete, the Committee should be commended

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for its efforts in classifying admixtures in a manner which should do much to clarify this subject in the minds of all interested in the field of concrete.

It is well known that for many years past those who have opposed the use of admixtures have outnumbered those who have advocated their use. This writer concurs in the following statement that "There never have been any logical reasons for this peculiar adverse attitude."* However, this attitude has discouraged consideration of additions whose use might have brought more general improvements in concrete at an earlier date. In recent years there has been a gradual trend toward a more "open-minded" attitude relative to their use because now it has been proved, even to some of the "old-school" engineers and cement producers, that admixtures can produce outstanding effects in concrete which, heretofore, could not be obtained. Therefore, if a thorough knowledge of all aspects of concrete and its ingredients is desirable, the worth or merits of various admixtures should be a part of that knowledge. To this end the Committee's report should aid greatly.

Certain statements made in the report may need some clarification and in some instances are somewhat misleading.

The first paragraph of the report sets forth a definition of admixtures and states that "According to this definition, materials are included that are added to the mixing water, which are added to the batch before or during mixing or which, if their presence in concrete significantly affects the properties of concrete, are interground with the clinker (excepting gypsum used in the normal manufacture of cement)." That part of the definition " . . . . if their presence in concrete significantly affects the properties of concrete . . . . ", does not allow the inclusion of certain interground materials which in themselves may not significantly affect the properties of concrete but under some circumstances when used with other separate additives the combination does markedly affect the properties. The writer knows of instances where such combinations resulted in concrete possessing characteristics not found in concrete containing either additive alone. Therefore, as a matter of precaution, the Committee should mention or discuss the possible effects of admixtures added at the mixer with cements interground with other additives.

It is stated under (e) on page 74 that "Accordingly, specific effects which will result from the use of an admixture can seldom be predicted accurately." With respect to this statement, it is difficult for this writer to believe that any specific effects with admixtures or specific results without admixtures can be predicted accurately.

*P. H. Bates, "Portland Cement Theories (Proven and Otherwise) and Specifications", Fifteenth Edgar Marburg Lecture, Proc. 43rd Annual Meeting A.S.T.M. Vol. 40, p. 482, 1940.

The first paragraph on page 78 is somewhat misleading in view of the published field data on strength reduction in concrete because of air entrainment. This writer does not believe that in the field, strengths can be maintained with little or no increase in cement content when using a straight air-entraining agent. Published laboratory data seem to indicate that when the amount of entrained air is within the limits of 2 to 4 percent by volume that fairly large reductions in sand-total aggregate ratio can compensate for the loss of strength due to air entrainment. On the other hand, there are very few data available which show that this can be accomplished in field operations. In other words, in the field where less rigid control can be exercised and the sand-total aggregate ratio cannot be reduced greatly, loss in strength generally results where a straight air-entraining agent is used.

It would seem that the available air-entraining agents should be divided into three distinct classifications in accordance with the function of each, namely: (1) Air-entraining agents which also contain water-reducing agents and are added separately at the mixer; (2) Air-entraining agents which, besides entraining air, also act as accelerators or catalysts in concrete and are either interground with the cement or added separately at the mixer; (3) Air-entraining agents that function only to entrain air either interground with the cement or added at mixer.

Each of these three classes of materials affects the strengths of the resultant concrete but in varying degrees. The function of the first type of air-entraining agent is to reduce the water-cement ratio sufficiently at a given cement factor to more than compensate for the strength reduction due to air entrainment of about 2 to 4 percent, with the result that increases in strength can be obtained. The function of the second type of air-entraining agent is to maintain strengths by virtue of their catalytic or accelerating effects when air entrainment is about 2 to 4 percent. The effect of the third group of air-entraining agents is well known, and since these agents only entrain air, decreases in strength can be expected with increases in the amount of air entrained.

This sub-classification of air-entraining agents would give the user a better knowledge of the types of agents available so that he alone can choose the type he wishes. Intergrinding versus separate addition of an air-entraining agent is a highly controversial subject; however, it is believed that the latter method gives the best opportunity for controlled air entrainment and that eventually this will be the recommended procedure.

Referring to the first paragraph on page 79, this writer believes that the alkaline-earth constituents should also be considered as entering into the saponification reactions of the water-insoluble resins, fats and oils. By definition, alkali constituents in this case refer only to the monovalent elements, sodium and potassium, whereas the alkaline-earth constituents would include also the divalent element, calcium.

The statements in the second paragraph on page 83 need some modification also. Commercial admixtures of the water-repellent type contain water-repelling agents in amounts ranging from 5 to 100 percent. The diluents used in the products of low water-repelling agent content, are water and various fillers as stated in the report. The 100 percent products are in dry form and contain no diluents other than very small amounts of necessary organic emulsifiers. Besides the types of undiluted chemical water-repelling agents mentioned in the report there are innumerable others, especially organic salts of fatty acids.

CLOSURE BY MEMBERS OF THE COMMITTEE

By F. B. HORNIBROOK*

The comments by the several contributors are welcome. Several of these comments concern definitions, the method of classification and the emphases employed in several instances. Naturally these factors will be affected to a large extent by the objectives assumed and by the type of reader addressed. The keynote of the committee's approach is expressed in the opening sentence of the synopsis, — "the aim of providing a perspective of the field of admixtures for the use of the engineer confronted with a need of modifying concrete to meet special requirements of a given job—." In other words the report is addressed principally to the field engineer. The definition of admixture is based on the assumption that he is concerned only with those admixed substances that create a significant effect in the concrete, i.e. that are sold as ingredients for concrete. Mr. MacPherson points out one weakness of this definition, namely, that an admixed material such as a grinding aid which in itself may not affect the concrete, may, in the presence of other additives, exert a very definite effect. Strictly speaking, an admixed material is any added substance other than the normal ingredients of concrete. That concept is employed in the following all-inclusive definition of admixtures suggested by a source outside the committee: "Admixtures shall consist of any substance or combination of substances other than portland cement, aggregate or water added to the concrete mixtures either in the field or by intergrinding with the cement at the mill (excepting gypsum used in the normal manufacture of cement)." There are good reasons for adopting such a definition rather than the functional definition used in the committee report.

The committee report, addressed as it was principally to the field engineer, also presumes that he is thoroughly familiar with the variables

*Chairman Com. 212; National Bureau of Standards, Washington, D. C.

arising in connection with cement, aggregate, mix-design and labor. The testing of cement and aggregates are standard practice. The development of mix-design by means of trial batches is nearly always recommended. The importance of competent inspection of both materials and workmanship is thoroughly recognized. The committee report calls attention to the fact that the use of admixtures may also present variables, which certainly should be treated no less exactingly than other variables arising in concreting practice.

Of the several suggestions offered for revised classifications or for a different approach to classification some are very good but others seem more appropriate for presentation to cement chemists or to admixture specialists rather than to field engineers.

Mr. Kaufman points out one class of materials not specifically covered in the report that represents a sizeable commercial use, namely graded metallic aggregates mixed with oxidation-promoting substances. The problems relating to the suitability of this type of expanding admixture for given installations, and its control in use, probably merits a separate section.

Relative to the comments on the section of the report dealing with air-entraining agents, the report states that data are meager on the effect of air-entrainment on strength when the mix is redesigned to make sure of a coarser aggregate gradation. It is unfortunate therefore that in stating that the report is misleading on this point Mr. MacPherson did not supply specific data for the benefit of us all. As to why the sand-total aggregate ratio cannot be adequately reduced in the field—once the benefit of such a reduction is acknowledged in making use of air-entrainment—is not made clear. However, the increasing use of air-entrainment and the increasing practice of redesigning the mix to make the most of the air-entrainment should soon provide a backlog of information on the points raised.

By R. F. BLANKS*

Definitions are very often the subject for much debate and they usually compromise the several individual views of committee members. It is not surprising, therefore, that a reader might be found who will take exception to the committee's definition. It was the committee's intent to regard as an admixture anything added to concrete other than cement, aggregate and water. Any of these three ingredients is subject to adulteration prior to its use. In the case of portland cement, prior additions such as grinding aids and air-entraining agents, are permitted by existing specifications without infringing on the title of portland cement. However, the committee felt that it was justified in considering

*Member Com. 212; Bureau of Reclamation, Denver.

any of these additions to portland cement as an admixture to concrete if it significantly affected its properties. Air-entraining agents are obviously in this category; a grinding aid whose influence on concrete could not be observed would not be regarded as an admixture. Mr. Kaufman admits that his difficulty with the definition arises from a superficial reading. Definitions require careful analysis and if this is not accorded them it would be better for a hasty reader to skip them. Mr. Kaufman could have helped the committee with a suggestion for restating the definition.

The writer must also take issue with Mr. Kaufman when he states that, "most of the admixtures which are interground with cement clinkers are those which affect the properties of concrete insignificantly." This is certainly not true of Darex and Vinsol Resin, the only two air-entraining additions permitted by existing cement specifications. The only other addition permitted in portland cement to date is the grinding aid TDA and the substances mentioned by Mr. Kaufman would necessarily have to be added in very small amounts to avoid detection and consequent rejection of the cement.

Several comments on the committee's report center around the statements concerning the reduction in strength accompanying the use of air-entraining agents. There is no disagreement over the fact that the introduction of air voids in the concrete, and the consequent reduction in its unit weight, reduces its strength. This will be observed more with some air-entraining agents than with others. The difference in opinion is concerned with how much of this lost strength can be regained by taking advantage of the increased workability afforded by the entrained air, thereby reducing the water content of the mix. The committee cautiously stated that the "desired strength can be maintained," implying that if equal strength was not obtained it would nevertheless be satisfactory. Some data, gathered from the Bureau of Reclamation tests on field concrete, can be offered the reader:

Five cements, without air-entraining agents were used on one job and later air-entraining cements, manufactured from the same clinkers and ground to the same finenesses, were used. In one case air-entraining agent was also added to one of the untreated cements at the mixer. The sand-total aggregate ratio was reduced to compensate for the volume of entrained air and the water in the mix was adjusted to give the same slump found for the untreated concrete. For several of the cements the strengths of treated and untreated concretes were equal and the average 28-day strength of all the treated concretes was less than 5 percent below the strength of untreated concrete.

By E. W. THORSON*

Mr. Schmid's proposal of a supplemental classification dealing with those characteristics of an admixture which involve its "foolproofness" in handling in the field, is very worthwhile. Most of the arguments against admixtures stem from inadequacies in one or more of these points.

Issue is taken with the statement in Mr. MacPherson's 5th paragraph, that ". . . it is difficult . . . to believe that . . . specific results without admixtures can be predicted accurately." By adequate attention to details in preparation of specifications and in inspection, with regard to the basic essentials necessary in the production of quality concrete (viz, water cement ratio, aggregate gradation and quality, cement factor, mixing time, placing, and curing) specific results *can* be predicted with a degree of accuracy commensurate with present design practices.

*Member Com. 212; Comdr., CEC, USNR, Bureau of Yards & Docks, Wash. D. C.

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Discussion of a paper by Lewis H. Tuthill:

Concrete Operations in the Concrete Ship Program*

By L. COFF and AUTHOR

ERRATA

Attention is directed to three errors which should be corrected in the paper. (1) In Figure 2 the shell thickness of the Savannah and Houston hull should be 4¼ inches instead of 4½ inches. (2) On page 142, last paragraph, first line, the number of lighters should be 27 instead of 25. (3) In Figure 20 box panels are shown at the left of the figure, not (right) as the title states.

By L. COFF†

Mr. Tuthill's paper, "Concrete Operations in the Concrete Ship Program," supplies official data concerning concrete hulls of recent construction. Details are given regarding the time of preparation and construction and the methods employed for the building of 77 ships and 27 lighters. Cost data would have been very welcome, also information regarding navigability of these ships.

When comparing the hulls described by Mr. Tuthill with the work of the Emergency Fleet Corporation, and concrete shipbuilding generally during and shortly after the World War I it occurs that the hulls dealt with in the paper do not differ much in design and methods of erection from the concrete ships built over twenty years ago. It would be difficult to find an analogy in other engineering fields for such a lack of progress. Were these boats so perfect as to defy the engineering skill of the next generation?

The report of the ACI Special Committee for Reinforced Concrete Ships and Barges, *Proceedings*, V. 17, p. 285, 1921, contains a very lucid summary of concrete shipbuilding activities and results of the preceding

*A.C.I. Journal, Jan. 1945; *Proceedings* V. 41, p. 137.
†Consulting Engineer, New York City.

period. The following disadvantages of concrete ships, built and in service, were mentioned.

1. The proportion of weight of hull to total displacement was unfavorable as compared with steel ships. The committee mentioned that a .55 to .60 efficiency factor—proportion of cargo to total displacement—was expected, whereas for the actual boats the hull with equipment weighed 50 percent of the total displacement. It is mentioned in the report that for a steel ship of similar characteristics, the efficiency would be .65 to .70.

The data given by Mr. Tuthill show no progress in this respect while, in the meantime, the efficiency factor of steel ships has decidedly improved through welding.

2. The committee states that the time of construction was generally longer than that of steel ships.

There is no need emphasizing that this drawback is more marked now than it was then. Steel shipbuilding beat all records as to speed in the recent emergency by adopting revolutionary methods. These were evolved in order to allow welding in the most favorable and comfortable position. The same considerations could possibly have been applied to concrete. While horizontal concreting is easy, and favorable to mass production, vertical concreting of narrow webs, between forms crammed with steel rods, has proved in the last emergency to be difficult, slow and inefficient.

3. The committee dealt with the difficult navigability of concrete ships, emphasizing their lack of resistance to impact. The statement is made, that it was difficult to secure the best class of sailors for concrete ships on this score. From the drawings produced in Mr. Tuthill's article, it cannot be inferred that these shortcomings have been eliminated or even improved upon.

4. As to the cost of concrete ships, the committee found that they were excessive. A price of $200. per ton dead weight was the basis of the estimates of the types dealt with, while the final figure after construction was close to $280. This meant no savings as against the cost of steel ships.

It would be interesting to compare the cost of the concrete boats dealt with by Mr. Tuthill with the cost of steel ships built recently.

When considering the above, the question arises in everybody's mind who, like the writer, has spent years of his professional life in designing, constructing and assisting in the operation of concrete ships, why the mistakes of the first emergency had to be religiously repeated and ships built which will be the first to be laid up, after the emergency.

Would not concrete seem an ideal mass production material, which could have relieved the shipping situation substantially, if other methods than the one proved ineffective had been used?

The reply to this question would express the feeling of frustration of a number of concrete boat engineers of 20 years ago, who thanked God when the concrete boat came up again, for getting a chance to devote to the war effort, a seemingly useless experience acquired through efforts full of agonies. The worst of them was to have to fight the mental attitude of the classification companies.

In the former emergency their surveyors, distinguished and experienced naval architects, bought their first textbooks on reinforced concrete the very day they started putting down the law and telling the concrete engineer what to do and what to leave alone. Reinforced concrete was then less than twenty years old as an industrial proposition. Supervisors of concrete boat construction are probably now more familiar with the material, than they were then.

However, what cannot be changed, as results prove, is the intrinsic contradiction betweeen a pioneering job of considerable magnitude and the functions that classification companies have efficiently discharged through several centuries, i.e., to protect insurance companies by rationing changes in shipbuilding, on strength of experience and experience only.

No concrete boats had been built since 1922. What more natural than that classification companies should insist on starting where we left off then?

However, concrete ships are implements of war. Classification companies are neither called on to insure submarines nor bombers, nor even transport planes. If concrete ships were desirable for alleviating the shortage, why ignore past experience and not give the concrete industry a free hand as to design and erection, on a competitive basis as wide as compatible with safety?

It is the contention of the writer and of many others in the industry that, given sufficient freedom, concrete engineers and contractors could have produced a serviceable, quickly built and reasonably priced concrete ship. Some people in the profession even believe that, for certain specialities, the concrete boat could have become a permanent institution. Adapting factory floor design to shipshape, proved a mistake, so why repeat it?

A start was made in August, 1941, when a competition for tanker design with bids was called by the Maritime Commission. It had an enormous response but nothing came of it.

On strength of the experience in the last emergency, the desire to obtain insurance for crew, ship and cargo must have been the deciding factor against departing from obsolete methods in concrete boat construction.

The concrete industry did not fail; it simply was not given the chance to do an appropriate job.

AUTHOR'S CLOSURE

Mr. Coff has raised some pertinent questions. On many of these, complete information was not available when the paper was written early in 1944. Others were not within the scope of the title.

It is true and admittedly regrettable that the four designs for the 77 ships built in five yards did not differ very much from the designs for the concrete ships built at the time of World War I. As Mr. Coff has correctly surmised, the classifying agency had much to do with this due to its refusal at the start of the program to permit any departure from previous standards and concepts of design because there was no time to prove the many new factors involved. Simplified designs of slightly less favorable deadweight ratio were considered initially but were abandoned in favor of proven conventional rib and shell design of better efficiency, despite the handicap placed on both time and cost of construction.

The suggestion was frequently heard during the program that since concrete ships are, as Mr. Coff points out, emergency implements of war, the restraints and delays resulting from classification might well have been dispensed with in favor of simplified design and test requirements, faster construction and delivery, and lower costs. As explained in the paper, this was prevented by the Maritime Act of 1920 which requires classification for all ships built by the Maritime Commission. Consequently the concrete ship program was automatically burdened with classification and the many resulting needless and impractical requirements that well could have been ignored in producing safe, practical hulls of high utility in the emergency.

It should not be overlooked, however, as perhaps Mr. Coff has, that a great forward step was taken toward the end of the program in the design and construction procedure for the 27 lighters built at National City and described in the paper, pages 142 to 147 and in Fig. 2, 5, 12 and 13. One of these was built and launched in 6½ days, a record equaled in the construction of few steel ships 265 feet in length. The precast construction of bulkhead walls in a horizontal position did much on these hulls to answer Mr. Coff's objection to "vertical concreting of narrow webs."

While final cost figures are not yet available for the various yards, on the basis of the preliminary figures which have been prepared, it appears that the costs of the concrete ships in this program were not out of line when properly compared with the costs of the first steel ships produced in new yards by builders inexperienced in ship construction. The Houston and Savannah yards were closed before their high costs on early hulls could be reduced on later ones. But at National City the cost of the rugged oil barges that have served the Navy so well in the Pacific was reduced from about $300 to $135 per ton of cargo capacity in the course of construction of the 22 built. At San Francisco the cost of the dry cargo barge dropped from about $193 to $101 per ton in 20 hulls. The self-propelled ship at Tampa had, as was known from the start, a comparatively unfavorable deadweight ratio and unit costs were consequently high. During construction of 24, costs dropped only from about $2,125,000 to $1,630,000.

Reports to date on performance of the concrete ships and barges have been consistently favorable. They have handled well, even in the most turbulent seas. They have been sturdy and resistant to fire and damage from close explosions. The self-propelled ships are remarkably free of vibration. All the hulls are dry and there is very little condensation. A cargo of Canadian wheat loaded in bulk against the hull concrete arrived in San Francisco without a kernel being sprouted or any grains being swelled or stuck together. Seaworthiness was demonstrated by the splendid performance of the S.S. Aspdin as it headed into the hurricane off Cape Hatteras, Sept. 14, 1944. According to the captain's report the ship "Behaved admirably throughout the hurricane. She pitched somewhat, but there was no rolling, panting, weaving, or pounding. As a result of her fine behavior, there were no injuries to any of the officers or crew—not even a scratch. The wind was estimated at 120 miles an hour, and the waves from 50 to 100 feet in height." In other severe storms the various barges and lighters showed equally admirable seaworthiness. On all hulls crews quarters were comfortable and well furnished. After becoming acquainted with the special characteristics (and the idea) of a concrete ship, sailors have generally preferred to stay with the ships built in this program.

In collision, the thin slabs of the rib-beam-and-slab-designed hulls are somewhat vulnerable but, in the several occasions where bumps of various force have occurred, the structural frame has not been damaged and repair was a relatively simple matter of recasting the panel in the manner described in the paper. Following the tests* at the National Bureau of Standards which demonstrated 1½ to 3 times greater resistance to impact with slabs with "supplementary reinforcement in the

*"Impact Resistance of Reinforced Concrete Slabs" by R. W. Kluge, ACI Journal, April. 1943, page 397.

form of overlapping helices of large diameter wire, commonly known as spirals," the builder of the Tampa ships proposed the spirals be used and the Commission approved after investigation showed they did not interfer with the placing of adequately vibrated concrete. The spirals had been installed in the shell of the first hull when the classification society objected to their use and required their removal on the technicality that the spirals were slightly large and therefore encroached a short distance into the required coverage of the reinforcement. The interwoven design of zig-zag stirrups (Fig. 5 of the paper) used in the shell of the lighters probably accomplished much the same purpose. Combined with the thicker wall, these features of the advanced and simplified lighter design did much to eliminate the weakness in impact attributed to the concrete hull.

In addition to the improvement in design for concrete ships that was developed for the lighters toward the end of this program, there were other improvements in comparison with the earlier program. Among the items which contributed to superiority of the hull structures built in the recent program are vibration as an aid in placing concrete, improved procedures for the preparation and bonding of construction joints and for the repair of imperfections, better cements, effective concrete control procedures, considerable welding of reinforcing steel, and probably better curing and painting.

Many who were connected with the recent program will appreciate Mr. Coff's comments on the "classification companies." In fairness, however, it should be recorded that the surveyors in this program were construction men generally familiar with proper conduct of concrete operations. Within the restrictions of their instructions, as individuals they were cooperative and helpful and contributed appreciably to the inspection and control of the work.

In some informal discussion there has been an indication of misunderstanding concerning the parenthetical statement about cold joints on page 152. The point was made that if this statement is true the treatment of construction joints described on page 166 was unnecessary. This inference is incorrect because it ignores the unescapable reality of the adherent contamination which invariably accumulates on the surface of construction joints, especially when several days or weeks elapse before new concrete is placed. Obviously this adherence must be removed, preferably with sandblast and as late as possible to avoid further contamination, before the new concrete is placed, if a well-bonded joint is to be obtained. The statement would have been more specific had it included the words "if it is clean and free of contamination" so the statement would have read "The main thing in preventing cold joints, regardless of age or hardness of the previous lift if it is clean and free of contamination is to see that the bottom few inches of the next lift is vibrated adequately." However, within the time during a concreting operation that a new surface may have to wait for the addition of concrete, no contamination is likely to occur that will detract from the joint bond if the first of the new concrete is well vibrated. If sufficient laitance develops to interfer with such a bond, other action is badly needed. There was practically no laitance from the ship concrete due to the high cement content and medium low slump.

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Discussion of a paper by Paul William Abeles:

Fully and Partly Prestressed Reinforced Concrete*

By K. P. BILLNER, L. COFF, K. HAJNAL-KONYI
and AUTHOR

ERRATA

p. 181, Synopsis line 5: change "formula" to "formulas."
p. 186, line 13: change "wire bonded" to "wire, bonded".
p. 192, last but one line: change "Δ_{s1}" to "Δp_{s1}".
p. 193, l. 3, formula (1a): change "p_s" to "Δp_s".
p. " 2 l. above formula (3b): change "$\frac{D}{d} = 1.1$" to "$\frac{\mathrm{D}}{d} = 1.16$".
p. " 2 l. above formula (4a): change "p_i" to "P'_i".
p. " bottom l., formula (4a): change "$P_i.e'_{so}$" to "$p'_i.e'_{so}$".
p. 194, top l., formula (4b): change "$P_i + P_i$" to "$P_i + P'_i$".
after formula (4b) insert: "Formulas (3) and (4) are safe approximations. The exact values are obtained by formulas (3′) and (4′), if P_i, P'_i, p_i and p'_i in formulas (3) and (4) are replaced by P_t, P'_t, p_t and p'_t respectively, as e.g.

$$\Delta p_s = \frac{n.P_t}{A_o}\left(1 + \frac{e^2_{so}}{g^2}\right) \quad \ldots\ldots (3') \text{ ''}$$

l. 3 below formula (4b): change considreed" to "considered".
l. 4 from bottom: change "$I_t = I_o + A_o.\ (e_{1o} - e_{1t})^2$" to
"$I_t = I_o + \mathrm{A}_o.\ (e_{1o} - e_{1t})^2 + n.A_s.e^2_{st}$".

p. 195, change formula (8) to "$A_s = \frac{M_u.s}{j.d.f_{su}}$" i.o" $A_s = \frac{M_u.s}{j.d.f_{su}}$".
l. 4 below formula (8): change "The" to "Mostly, the".
p. 196, formula (11) change "I_o" to "S_{2o}".
p. 201, l. 2 below title: change "2:$j.d.$ = 12 in." to "2; $j.d$ = 12 in.".
l. 7: change "e_{se}" to "e_{so}".
last l. before Table: delete "(No. 8)".
in Table l. 4 from bottom center column: change "1.07" to "1.01".
p. 208, Fig. 14: The widths of cracks are correct in inches but 10 times too great in mm. e.g. change "2.5 mm. = 0.01 in." to "Permissible width 0.25 mm. = 0.01 in."
l. 8 below Fig. 13 and 14: change "cracked (n=15)." to "cracked section (n = 15)."

*ACI JOURNAL, Jan. 1945; *Proceedings* V. 41, p. 181.

p. 213, Add the following Notations:

"e_{so} = distance of A_{sp} from the center of gravity of A_o.
e'_{so} = distance of A'_s from the center of gravity of A_o.
e_{st} = distance of A_{sp} from the center of gravity of A_t.
e'_{st} = distance of A'_s from the center of gravity of A_t."

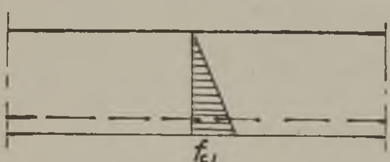
Reference 10.—: change "Forschingsarbeiten" to "Forschungsarbeiten."

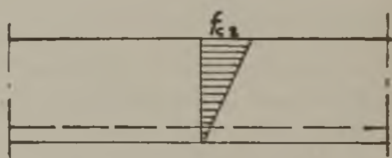
By K. P. BILLNER†

Dr. Abeles' paper is praiseworthy for the thoroughness with which he manages to condense the history of prestressing into a few pages of type and yet bring out the essential features of many efforts. It is also noteworthy that while he describes his own contribution which constitutes another forward step in the right direction, he does not emphasize its merits.

There is, however, a statement regarding prestressing in general, which frankly, this writer does not understand, and which concerns the very core of the subject. This is the reference to tests by Freyssinet and by Hoyer (p. 189), supposed to prove that a reinforced concrete specimen will support the same ultimate load whether prestressed or not.

Consider the center of the span of a beam or slab of rectangular cross section. With prestressing, the stress distribution in the concrete is reversed so that the maximum compressive fibre stress occurs at its bottom before the live load is applied. (Fig. A.) When the design load is applied, the stress distribution is reversed so that the maximum compressive stress occurs at the top. (Fig. B.) With proper design, the concrete section in the prestressed beam will, therefore, carry a much heavier load. In general, the same cross section will be good for three times the live load which can be supported by the beam not prestressed. (Fig. C).





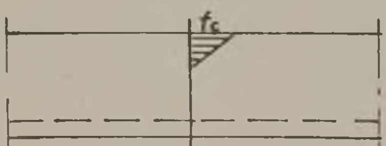


Fig. A, B, and C—Concrete stress diagrams

A (top left)—Form support removed.
B (top right)-f_{c2}-stress from live load only.
C (left)—Conventional reinforcement—f_c—stress caused by dead load plus live load

†Vacuum Concrete, Inc., Philadelphia, Pa.

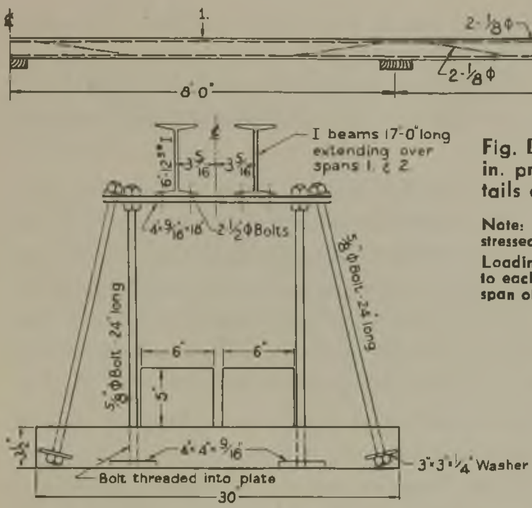


Fig. D.—One half elevation of 32-ft. 0 in. prestressed concrete beam and details of testing device.

Note: Two beams reinforced as shown: 1. Prestressed to 100,000 psi.; 2. Not prestressed.
Loading: Individual center loading was applied to each beam at point 2 and then the adjacent span of each beam was loaded at point 1.

The statement referred to holds good only if the percentage of prestressed reinforcement is very small. But then there is a waste of concrete and weight and the specimen is not a reinforced concrete beam but is rather a small suspension bridge.

Among the tests this writer has made one, intended to prove something else, may illustrate this point. Two identical concrete beams continuous over three intermediate supports were cast simultaneously. Each beam had a rectangular cross section area the height being 5 in. and the width 6 in. It was desired to ascertain the effect from prestressing as compared to a non-prestressed identical specimen. The arrangement for testing is shown in Fig. D. The reinforcement in each beam consisted of six ⅛ in. wires, two of which were bent up at the intermediate supports. The yield point of the wires was approximately 240,000 psi. The steel in one of the beams was electrically prestressed to 100,000 psi. after deducting loss from flow and shrinkage. Control cylinders cast of the same mix used for the beams, showed the concrete to have a compressive strength of about 5,000 psi.

One of the end spans of the prestressed beam was tested first. Failure occurred when the center load reached 2,700 lb. The beam then had a deflection of .37 in. in a span of 8 ft. No crack occurred at any of the supports. When the load was removed, the beam went back to nearly straight, with a permanent deflection after loading, of only two-hundredth of an inch at the center. The hair-crack which was visible under a maximum load, closed when the load was removed and could not be seen even by use of magnifying glass. The span adjacent to this end span was then loaded in a similar manner. It also took a center load of 2,700 lb. at failure. The deflection on this span was then .3 in. and a hair-crack extending from the bottom of the beam one-half way up, was visible at the center. When the load was removed, this beam became almost straight, the permanent deflection being one-hundredth of an inch.

The next test was made on one of the end spans of the beams which had not been prestressed. The deflection at a center load of 700 lb. was 4/10 in. and there was a fairly wide crack extending from the bottom of the beam to nearly the top of it under the load. Another crack occurred at the intermediate support adjacent to it, caused by the negative bending moment there. When the load was removed, the beam recovered only part of its deflection; both of the cracks mentioned were still visible.

The adjacent intermediate span of the not-prestressed beam was then loaded. Failure occurred under a center load of 800 lb. The same kind of cracks as in the end span of the beam occurred. The beam recovered only part of its deflection when the load was removed and the cracks remained open.

An analysis of the tests indicate that under the maximum load, the steel in the prestressed beam was stressed to about 240,000 psi., whereas the stress at failure in the not-prestressed beam anounted to an average of 54,000 psi. It may be noted that in each case the loading was carried on until the deflections were about 50 percent greater than the so called permissible $\frac{1}{360}$ of the length of the span. The tests were witnessed by Prof. H. C. Berry.

By L. COFF*

Dr. Abeles points to the safe use of very much higher unit steel stresses, than customary in reinforced concrete construction so far. According to this paper, a steel stress of 100,000 psi. would not lead to dangerous cracks, if combined with adequate extreme fiber restraint through partial prestressing.

Drawn wire would be the material used for reinforcing, prestressed and non-prestressed.

While a stress of 100,000 psi. seems somewhat startling, in view of the small elongation concrete can withstand without cracking, our attitude of limiting steel stresses on this score should be revised in the light of actual experience with monolithic concrete construction.

Our wisdom is based, in the first instance, on theoretical considerations, combined with tests on single span beams, freely supported under

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ideal laboratory conditions. These conditions do not exist in actual practice. Even a beam bearing on brick walls will behave differently as to extreme fiber restraint. In monolithic reinforced concrete construction, there should be very little analogy with laboratory tests on single spans simply supported.

Better bond contributes to more effective fiber restraint, and should be examined in combination with other restraining factors.

It may be argued that frame action is taken care of by our computations, the maximum steel stresses being arrived at after considering the degree of restraint. How reliable are these computations? We figure deformations of the uncracked structure and stresses for the cracked section. When the section has cracked, our original assumptions are no longer true.

Besides, the elastic theory we are using has its limitations, one of them being moment redistribution. The tests of W. H. Glanville and F. G. Thomas,* generally recognized as an outstanding research performance, bear out that the elastic theory is correct up to the point, when in the critical section, either the concrete reaches its ultimate strength or the steel is stressed to the yield point. Thereafter, the structure does not collapse, as we would expect, but other sections help to maintain the moment of resistance of the critical section constant, up to a load often several times the one assumed in our computations to result in failure. In tests on elastic frames and continuous beams, ultimate loads of three to six times the computed ones, were reached.

We are living in a theoretical world, far away from what actually happens in a structure. Of this we are acutely aware when dealing with new methods or materials.

The writer was in this position in the early thirties, when, together with G. E. Clarkson, a Canadian metallurgical engineer, he played an active role in creating the industry of cold developing reinforcing steel in the British Empire. About 40,000 tons of reinforcing steel, at computed stresses of between 24,000 and 30,000 psi. were supplied and incorporated into concrete, while he was in charge of the technical part of this venture. Simultaneously, he had access to the experience gained with similar types of developed steel on the continent of Europe, to the extent of several hundred thousand tons.

More cracking should have been expected, than when using stresses of 16,000 to 18,000 psi. However, this was not the case. After two or three years of sales, the writer who believed in the theory, higher stresses, more cracking on the job, was advised by building owners, architects

*Section V of Studies in Reinforced Concrete, Technical Paper No. 22. Issued by H. M. Dept. for Scientific and Industrial Research, London, 1939.

and consulting engineers that there was less visible cracking in their buildings, than when using ordinary, plain reinforcing bars.

Similar advice came from Europe, especially from Switzerland and Czechoslovakia. In the United States the Wickwire Spencer Steel Co., had very favorable job results. In Europe, better bond may have had something to do with it. The writer is inclined to believe, however, that the main factors resulting in less visible cracking were, in the first instance, the absence of a definite yield point in cold developed steel, and the beneficial effect on shrinkage cracks, of low percentages of reinforcement. With regard to the yielding of steel, it seems there are in every reinforced concrete structure overstressed sections, in which the steel reaches the yield point. With cold developed steel, including wire, this need not necessarily lead to the opening of cracks as it does when the yield point is pronounced.

The most conclusive evidence that high steel stresses, computed by our usual methods, do not necessarily mean more visible cracking, has been gathered through an experience on such a gigantic scale, that it is worth recording.

Earl's Court Exhibition Buildings in London, built in 1935-37 by the Hegeman-Harris Co., of New York, under the personal direction of John W. Harris, is one of the largest reinforced concrete structures ever built, containing close to 100,000 cu. yd. of reinforced concrete, and more than 16,000 tons of reinforcing steel. It covers about 25 acres of construction area, in multiple story, wide span buildings, constructed to house trade exhibitions. The general contractors appointed their own architect and consulting engineers, amongst them the writer, who was responsible for the suspended car park and chair store.

Several years after completion of the buildings, the writer was called in by the building authorities to advise on the opening of a trade show, the Ideal Home Exhibition, a yearly London institution, with a normal attendance of 100,000 visitors a day. Unknown to the authorities, some exhibitors had erected a considerable number of two-story brick homes, with elevated rockery gardens around them, in the chair store, covering about 56,000 sq. ft.

The concrete floors in bays of 20 ft. x 30 ft., so loaded, had been designed in the writer's office for a live load not to exceed 200 lb. per sq. ft. The story under the floor so excessively loaded, was a suspended car park which had to be kept free of shoring. Several of the columns supporting this floor and the chair store were bearing on a steel girder bridge crossing a suburban railroad line.

The first investigation revealed that the steel bridge showed no increased deflection, notwithstanding the increase of load. Most likely the rows of columns, with the very stiff floors on top and bottom, were supporting themselves like a Vierendeel System across the bridge, thereby relieving the column load. The two concrete floors nowhere showed greater deflections than 1/2000 of the span, nor could cracks of any consequence be discovered. A take-off and weight calculations of the brick homes and their surrounding gardens were made, and the structure checked as to stresses under the loads as described. It was found that the steel was stressed in places at more than 45,000 psi., which stress would have been increased to between 55,000 and 60,000 psi. by the crowds expected. The reinforcing steel was mainly Isteg steel tested at 54,000 psi. for a .2 per cent permanent set.

So many cases of overload capacity are known in actual practice, that the writer was not particularly worried about the safety of the structure, excepting for the steel bridge, which had to be dealt with separately. Eventually it was decided to allow the exhibition to open according to schedule, i.e., two weeks after this discussion took place, subject to testing the floors under an additional load of 100 lb. per sq. ft. in the most unfavorable positions. This test involved the handling of nearly 1000 tons of material and employing a considerable crew, measuring deflections, and looking for cracks, in revolving shifts. It was found that the deflection did not exceed 1/1500 and the cracks were negligible.

In the light of the above experience, it may well be that stresses of 100,000 psi., as mentioned by Dr. Abeles, should not be objectionable, especially if supplemented by additional extreme fiber restraint through partial prestressing.

Naturally, when comparing total and partial prestressing, the first sounds very attractive in view of the elimination of many uncertain assumptions in computing conventional reinforced concrete construction. There again, partial prestressing offers great advantages as to being able to comply easily with variation and alternation of moments.

For the time being, it remains to be demonstrated whether prestressing, either total or partial, will become a commercial proposition under American conditions, excepting for the use of cables in structures of considerable magnitude. For the handling of a multitude of individual wires, Schorer and Billner have shown a way. Other methods may follow.

The most frequent complaints against cracking in conventional reinforced concrete structures refer to shrinkage and diagonal tension cracks. In this respect, longitudinal ties with destroyed bond, according to Dill's method, would improve the shrinkage situation, while prestressed stirrups should contribute materially in reducing diagonal tension cracks. Prestressed stirrups, bonded around the tension bars, and with coated legs, threaded at the ends for pulling up, are simple to apply. So are shrinkage bars with destroyed bond. Tests designed by the writer are in preparation at Toronto University to ascertain the effectiveness of such arrangements in continuous structures. The tests of Evans, mentioned by Dr. Abeles, with conventional bars, but prestressed stirrups, form a valuable precedent.

As already mentioned, prestressing, whether fully or partly, points to the use of drawn wire. In this connection the creep of wire under sustained load should not be overlooked. Freyssinet, as well as Hoyer, dealt with it. Within the proportional limit, wire, cold drawn to a specified tensile strength, is a perfectly stable material, as shown by creep tests over a period of nine years by the John A. Roebling Sons Co. The original proportional limit can be raised considerably through cold working. Hoyer mentions 85 percent of the ultimate strength as the proportional limit of the wire he uses. His tests, supporting this percentage, cover too short a test period to be conslusive. In the absence of standard specifications for prestressing wire, the characteristics of this material should be studied carefully in every case.

By K. HAJNAL-KONYI*

Although the tests on partly prestressed beams described in Dr. Abeles' paper were preliminary only and further tests on this type of construction are necessary, they revealed some important features of partly prestressed beams. The main objection to such beams, raised in a discussion referred to by Dr. Abeles under (9), was that under the design load "strong cracking" would exist. According to the test, in a beam with only 40 per cent of the reinforcement prestressed: 1) the cracks were very fine when they first occurred and the rate of increase was slow; 2) the "permissible" width was reached at a load of about 75 percent of the ultimate, i.e. far in excess of the permissible load; 3) even after increasing the load up to 80 percent of the ultimate, cracks of .02 in. width (i.e. twice the permissible limit) completely disappeared when the load was removed. If it can be proved that these findings may be generalized, partly prestressed structures should have a wide field of application.

The favorable results of these tests gave me the idea to combine precast prestressed and in-situ reinforced concrete, with or without additional reinforcement in the in-situ portion. I submitted this suggestion to the Institution of Civil Engineers in London in March 1944 for publication.†

*Consulting Engineer, London, England.
†Correspondence on "The Use of Pre-cast Pre-stressed Concrete Beams in Bridge Deck Construction" by Alan Andrew Paul. *Journal of The Institution of Civil Engineers*, London, Oct. 1944: Supplement.

The reasons for combining precast prestressed and in situ concrete may be summarized as follows:

Precast prestressed units are more expensive per unit volume of concrete than cast-in-situ concrete. If fully prestressed, they require careful handling unless special reinforcement is provided for this purpose and the very high compressive strength of the concrete cannot be fully utilized in the structure, since the maximum stress under working loads is smaller than the stress at the release of the stretching force at a comparatively early age of the concrete. When such precast units are assembled for a floor or bridge, they act as single units, do not form a monolithic structure without an additional screed on top of sufficient strength or transversal prestressing on the site, and cannot be used as components of a continuous beam or frame. All these disadvantages can be avoided if precast prestressed units are combined with in-situ concrete in such a way that they are united in a monolithic structure. This can be achieved by securing complete bond between the precast and in-situ components, e.g. by steel left protruding from the precast units or by shaping the precast units in a suitable manner, etc.

A few examples of possible applications may explain some of the main features of such combined structures. Fig. E shows the proposed cross section of a road bridge. In flat country it is often essential to keep the overall depth of a bridge as small as possible in order to reduce the length of the approach ramps and the area covered by these ramps. The use of prestressed concrete in itself allows a considerable reduction as against steel or cast-in-situ reinforced concrete. The arrangement shown in Fig. E makes a further reduction of the depth possible. The precast prestressed units must be strong enough to carry their own weight plus the weight of the in-situ concrete forming part of the structure plus any incidental loads during erection, but any additional load such as the road surfacing and the live load is carried by the combined structure. Thus the precast prestressed units can be lighter and need not be prestressed to the same extent as if they had to carry the full load. They serve as permanent shuttering and no scaffolding or temporary shuttering for the in-situ part of the structure is required. The in-situ concrete on top of the precast units increases the effective depth.

Fig. F and G show two alternative stress distributions under full design loads. The notations in these figures are as follows:—

f_{wd} = compressive concrete stress at the top of the precast units due to the dead weight of the units and the additional dead weight of the in-situ concrete.

f_{wu} = compressive concrete stress at the top of the precast units due to any additional dead load and live load.

f_w = maximum compressive concrete stress in the in situ part of the combined structure, due to any additional dead and live load.

f_{wd} is calculated on the section of the precast beams,

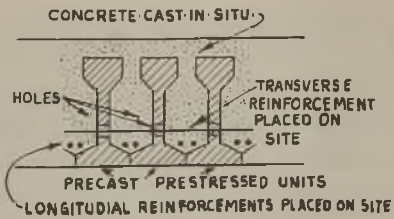


FIG. E

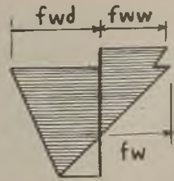


FIG. F

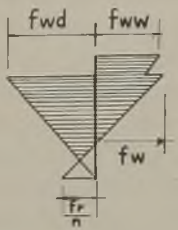


FIG. G.

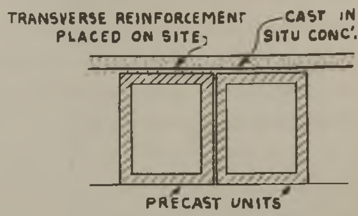


FIG. H

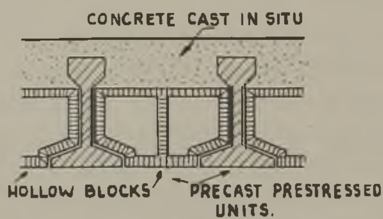


FIG. I

f_{ww} and f_w are calculated on the combined section.
f_t = tensile stress in the reinforcement, corresponding to f_{ww}
n = modular ratio for precast concrete.

The diagrams are based on the assumption of a smaller modular ratio for the precast concrete than for the in situ concrete.

Fig. F corresponds to a fully, Fig. G to a partly prestressed structure. The latter can be so designed that cracklessness under dead load is guaranteed. Apart from exceptional cases, this seems to be the most advantageous application of the suggested type of combined structure. Any fine cracks that might occur under heavy traffic will disappear immediately when the load is removed so that the bridge will remain free of cracks during the best part of its life. This is a great improvement on ordinary reinforced concrete bridges, the cracklessness of which under dead load can by no means be relied upon.

It should be noted that in such a structure, even if fully prestressed, the extreme fibre stress in the precast part under design load may substantially exceed the maximum concrete stress at the release of the stretching force. At the same time, the maximum permissible stress of the in-situ concrete can be utilized, which, of course, is smaller than the permissible stress in the precast part.

In a structure with a cross section according to Fig. E and a stress distribution according to Fig. G, the reinforcement placed in the in-situ concrete remains unstressed under the weight of the structure. However, the whole of the main reinforcement may be accommodated in the precast part, if desired.

Fig. E shows a cross section without cavities. It is obvious that cavities can be arranged. The total dead weight of a combined structure need not be greater than that of a fully prestressed precast structure, but the overall depth which is governed by the maximum fibre stress of the concrete—apart from deflection—can be smaller. Fig. H shows an application with hollow precast units without additional main reinforcement on the site. A part of the reinforcement of these beams may not be prestressed. Fig. I shows that the precast units may be spaced and the gaps filled with hollow blocks. A further development on this line is the construction of a floor of the shape of an ordinary reinforced concrete floor in which the webs of the T- and L-beams are formed by precast prestressed units and the flanges by a slab cast in-situ.

The use of precast prestressed beams as webs of T-beams with in-situ flange is mentioned in an article by Soutter* in which the results of a test on such a beam are described. This test has proved that full co-operation between the precast and in-situ components can be achieved and it is possible to utilize fully the prestressed reinforcement which was broken at failure of the test beam.

The sectional area of the steel was 3.1 sq. cm., its ultimate strength 19.5 t per sq. cm. and the distance from the centre of gravity of the reinforcement to the middle of the flange of 10 cm. thickness was 39 cm. The maximum bending moment resulting from these figures, as calculated by the author, is 3.1x19.5x39 = 23.6 tm, the actual test result was (with two point loads at the third points of the span) 28.1 tm. This is an excess of 19 per cent. Even if we assume that the resulting compressive force was nearer to the top of the beam than assumed in the above calculation, this could only account for about half of the excess.

In my opinion, this excess has nothing to do with prestressing but is due to the same effect as observed in tests with square twisted steel and discussed in my paper referred to by Dr. Abeles' reference 11.

*P. Soutter: Die Verbundwirkung zwischen vorgespanntem und nicht vorgespanntem Beton und ihre Anwendung auf den Plattenbalken mit vorgespanntem Steg. Schweizerische Bauzeitung, Bd. 124 No. 8, 26 Aug. 1944. S. 103-108. No. 9. Sept. 2, 1944, S. 126.

AUTHOR'S CLOSURE

Errors in the paper are corrected at the beginning of the discussion. One point, however, is of great importance and is further discussed before considering the contributions to the discussion.

As pointed out, equations (3) and (4) are safe approximations, the exact values being obtained, if P_i, P'_i, p_i and p'_i are replaced by P_t, P'_t, p_t and p'_t respectively. The value p_t is given by formula (2a), similarly $p'_t = p'_i - \Delta p'_e - \Delta p_{s1}$. Since the exact value of Δp_{sl} is not known, depending on the quality of the concrete and the interval between its hardening and the transfer of the prestress, the replacement of p_t and p'_t by p_i and p'_i in the formulas (3) and (4) means a simplification, the difference from the exact value being relatively small for a high prestress p_i and small percentage p, which is the case when high strength steel is used. On the other hand, when p_w is obtained from formula (6), or P_w,P'_w, P_t and P'_t from equations (10) and (11), which may be the usual case, it is just as simple to use the exact formulas, when obtaining p_t and p'_t from the following equations (2a′), representing a combination of equations (1) and (2a):

$$\left.\begin{aligned} p_t &= p_w + \Delta p_{s2} + \Delta p_p \ldots\ldots\ldots\ldots\ldots\ldots\ldots \\ p'_t &= p'_w + \Delta p_{s2} + \Delta p'_p \ldots\ldots\ldots\ldots\ldots\ldots\ldots \end{aligned}\right\} (2a')$$

In this case, however, safe values for Δp_{s2} and Δp_p have to be assumed. The values $\Delta p'_p$ and $\Delta p'_e$ may differ from Δp_p and Δp_e, owing to the different compressive stress.

If, however, the percentage p is high (i.e. if mild steel is used), then it is imperative to use the exact formulas (3′) and (4′), e.g. (3a′) corresponding to (3a):

$$\Delta p_e = n.\frac{d}{D}.\left[1 + 3.\left(\frac{2d}{D} - 1\right)^2\right].p.p_t = c.p.p_t \ldots (3a')$$

In this formula, let in the usual manner p_t be replaced by $p_i - \Delta p_e$, the more complicated, but better approximation (3a″) is obtained.

$$\frac{\Delta p_e}{p_i} = \frac{c.p}{1 + c.p} \ldots\ldots\ldots (3a'') \text{ instead of } \frac{\Delta p_e}{p_i} = c.p \ldots\ldots (3a)$$

The difference between the original approximation (3a) and equation (3a″) related to the latter is:

$$\frac{c.p - \dfrac{c.p}{1 \times c.p}}{\dfrac{c.p}{1 + c.p}} = c.p.$$

For $n = 8$ and $\frac{D}{d} = 1.16$ according to equation (3b), c is 17.5; thus the relative difference of the loss for $p = 0.1$ percent is 1.75 percent (0.0172 instead of 0.0175), gradually increasing to 17.5 percent for $p = 1$ percent (0.149 instead of 0.175). It should be borne in mind that the value c increases for smaller ratios $\frac{D}{d}$ (e.g. it is 21.8 for $\frac{D}{d} = 1.1$); consequently the approximate formulas should be used only for percentages up to 0.3 percent to 0.4 percent. Such small percentages, however, will be the normal case, when high strength wire is used. Equation (3a′) is better used instead of (3a) or (3a″), where the loss Δp_{s1}, according to formula (2a), represents a considerable part of p_i; hereby the correctness is improved, even if Δp_{s1} itself is not correct.

The difference between various prestresses and stretching forces at different stages and between the loss, owing to elastic deformation, according to the exact formula (3′) is seen from Table A, relating to 7 different systems of rectangular beams, (each 3 in. wide and 10 in. deep, the reinforcement having a cover of 0.5 in.) i.e. (3d), (3a), (3c), (3b), (6a) pre-stretched and post-stretched, and (6b) according to Table 1. Each system is designed for a permissible bending moment of 50,000 lb. in. (equivalent to a beam having an unstretched mild steel reinforcement of two square bars 7/16 in., $p = 1.26$ percent, designed for $f_s = 18{,}000$, $f_c = 1000$ psi. and $n = 15$). Systems (3d) and (3a) are designed for mild steel reinforcement, the percentage being according to the recommendation for system (3d)*: $\frac{100 f_c}{f_s} = 2.78$ percent i.e. 2.2 times that required for the traditional design. However, even with such high a percentage, the initial prestress of system (3d) becomes so high that mild steel cannot be used. The reinforcement of all other systems is of high tensile wire, calculated according to equation (8) for $s = 2.5$, $j = 0.83$ and $f_{su} = 250{,}000$ psi. : $A_s = 0.65$ in.^{2} (5 wires gage 8), $p = 0.23$ percent. The first 4 systems are fully prestressed, the 3 examples system (6) are designed for 0.4 P_w, P_w representing the stretching force required in systems (3c) and (3b). In all cases $n = 8$. For the systems with pre-stretched reinforcement it has been assumed that $\Delta p_s + \Delta p_p = 25{,}000$ psi., of which $\Delta p_{s1} = 10{,}000$ psi., whereas for the systems with post-stretched reinforcement $\Delta p_{s2} + \Delta p_p = 15{,}000$ psi. Generally, these are safe assumptions. It may be repeated that exact values for these losses cannot be obtained. If, however, higher values of the losses are assumed than those in fact occurring, an additional safety of the effectiveness of the prestress is obtained. From Table A it is seen that on the assumption of such rather high losses, in systems (3d) and (3a) very high concrete stresses occur at transfer of the stretching

*See U. S. Patent No. 2,035, 977.

TABLE A

System (Table 1)		3d	3a†	3c	3b†	6a		6b
						A	B+	
		Fully prestressed				partly prestressed		
d	in.	9.18		9.44				
p	%	2.83		0.23				
A_s	in.^{2}	0.78		0.065				
A_{sp}	in.^{2}	0.78		0.065		0.026		0.065
A_{su}	in.^{2}	—		--		0.039		—
A_o	in.^{2}	29.22		30 *				
e_{1o}, e_{2o}	in.	$e_{1o}=5.11$, $e_{2o}=4.89$		$e_{1o} = e_{2o} = 5$ *				
e_{so}	in.	4.29		4.44 *				
I_o	in.^{4}	236		250 *				
S_{1o}, S_{2o}	in.^{3}	$S_{1o}=46.2$, $S_{2o}=48.3$		$S_{1o} = S_{2o} = 50$ *				
A_t	in.^{2}	35.46		30.46				
e_{1t} e_{2t}	in.	$e_{1t}=4.36$, $e_{2t}=5.64$		$e_{1t}=4.93$, $e_{2t} = 5.07$				
I_t	in.^{4}	330.7		258.5				
S_{1t}, S_{2t}	in.^{3}	$S_{1t}=75.8$, $S_{2t}=58.6$		$S_{1t} = 52.4$, $S_{2t} = 51.0$				
p_w(equ.6)	psi.	6,660		120,000				48,000
p_t	psi.	21,660		135,000				63,000
P_w	lb.	5,200		7,800		3,120		
P_t	lb.	16,900		8,780		3,510		4,100

†post-stretched. *approximately (small A_s neglected).

TABLE A—continued from opposite page

TABLE A

System (Table 1)		3d	3a†	3c	3b†	6a A	6a B+	6b
		Fully prestressed				partly prestressed		
f_{1w}	psi.	0				—572		
f_{2w}	psi.	+567		+547		+807		
f_{1t}	psi.	+2,148		+1,073		+429		+500
f_{2t}	psi.	—926		—487		—195		—229
$\Delta p_e (equ. 3')$	psi.	15,160	—	7,880	—	3,150	—	3,680
p_i	psi.	46,820	21,660	152,880	135,000	148,150	135,000	76,680
P_i	lb.	36,520	16,900	9,930	8,780	3,850	3,510	4,980
P_i	%	368	170	100	88.5	39	35.3	50
$\frac{P_i}{P_w}$	—	7.03	3.25	1.27	1.13	1.24	1.13	1.60

force, by far exceeding the permissible stresses. In view of these higher concrete stresses, the loss Δp_p ought to be increased for these systems, which in turn would effect a further increase of the initial prestress required and thus of the concrete stresses. From this comparison it is seen that the systems, having a mild steel reinforcement, are not suitable for prestressed beams (Too great difference between P_i and P_w and too high concrete stresses at the transfer).

The difference between fully and partly prestressed reinforced concrete is seen from a comparison of systems (3 b,c) and (6). In the latter cases the concrete stresses at the transfer are much less than those under maximum live load, whereas with all fully prestressed systems, mostly the greatest concrete stresses occur at the transfer of the prestress, when the concrete is relatively young. In pre-stretched systems a reduction of the bond stress at the transfer is of great importance. This is obtained in system (6b), the bond stress being reduced to $\frac{63,000}{135,000} =$

0.47 of that occurring in systems (3c) and (6a), corresponding to the respective p_t values.

Mr. Billner's introductory acknowledgement is most gratifying. His contribution is valuable in giving an opportunity to clarify some misunderstanding. His Fig. D is an interesting example. Mr. Billner compares the cracking of a prestressed beam with that of a beam, having an unstretched high strength reinforcement. It can be assumed that the expression "failure", occurring where the first test is described, should read "cracking". This can be recognized from the description that the beam became nearly straight on removal of the load. In a prestressed construction, first cracking undoubtedly occurs at a much higher load than in a traditional structure. Total absence of cracks, however, is required only for particular constructions such as tanks for liquids, barges etc. In normal constructions the maximum width of cracks should be limited to 0.01 in., as pointed out in the "Introduction". Reference may be made to Fig. 14 in which first cracking occurred in the fully prestressed beam (c) at a load also about 3 times that in beam (a), of conventional type, whereas the load causing cracks of a permissible width of 0.01 in. is more than twice as high. It is an outstanding property of prestressed concrete, independent of the degree of prestress, as compared with traditional construction, that cracks nearly totally disappear on removal of the load. In view of this property, the width of 0.01 in.—which relates only to permanent open cracks (24)*—does not, in fact, count in prestressed concrete, provided the excessive load occurs only temporarily and does not remain for a considerable time.

On the other hand, Mr. Billner's objection relates to the ultimate load. His Fig. A to C refer to compressive stresses. Failure, however, in a construction with a small reinforcement primarily is due to yielding or failure of the steel, as has been pointed out in chapter 4. Shortly before failure, the counter-action, due to prestressing, is discontinued in the surrounding of the section where the cracks open, as the counterpressure at the normal tensile zone cannot be transmitted. Consequently, approximately the same stress f_{su} occurs in the steel, whether the construction is prestressed or not, provided in the latter case there is a sufficient shear and bond resistance. There might be only a slight difference in f_{su} in favor of a higher prestress, owing to the fact that in this case the bond in the immediate neighborhood of the cracks is destroyed to a lesser degree.

All structures have to be designed on the basis of a guaranteed safety against failure, consequently formula (8) has been put up, giving the minimum reinforcement required to ensure a factor of safety s, inde-

*See additional references, end of discussion.

pendent of the degree of prestressing. It must be borne in mind that, in a prestressed construction, first cracking can be deliberately delayed by an increase of the stretching force until such a stage is reached that cracking and failure occur at the same time as with a brittle material. This extreme case proves, how important it is to ensure a definite factor of safety against failure, independent of that against cracking.

On the other hand, it is evident that the behavior of a prestressed construction, whatever the degree of prestress, totally differs from traditional reinforced concrete, and Mr. Billner's statement that the former is capable of carrying about 3 times the load of the latter before "cracking", is fully justified, provided the stretching force corresponds to this factor. Moreover, it is undoubtedly an outstanding advantage that a structure, even if only slightly prestressed, can be re-used after being loaded up to 80 - 90 percent of the ultimate load, whereas any conventional reinforced concrete structure would have permanent deformations of such an extent that its re-use would be impossible.

Mr. Coff refers in his discussion to the strength of monolithic reinforced concrete frames, greatly increased by the plasticity of reinforced concrete. However, this is quite a different problem which will have to be investigated separately, as has already been done for steel structures([25]). In the present paper, fully and partly prestressed concrete beams have been compared with conventional beams on the basis of tests on simply supported beams. The writer's suggestion that a partly prestressed concrete structure, reinforced with high strength wire, should be designed for a permissible stress of 100,000 psi. is based on these test results and has nothing to do with the re-distribution of stresses.

The reduction of visible cracking, at a stage at which the steel stresses are still below the yield point, cannot result from the absence of a definite yield point, as Mr. Coff believes. It is obviously the increased bond that accounts for a reduced cracking, since in this case the connection between the reinforcement and concrete is destroyed only in the immediate neighborhood of the cracks, as pointed out before.

Dr. Hajnal-Konyi's contribution is very helpful in demonstrating the possibilities offered by partly prestressed structures. His remark regarding the cost per volume of precast concrete cast in place may require some further explanation. The costs of concrete products, manufactured in a factory, together with the costs of transport from the factory to the site and of placing them into position (C_F) are compared with the costs of concrete put immediately in place (C_P), assuming no separate shuttering and support to be necessary. Let $C_F = a.C_P$, the value of "a" will vary greatly for different countries. Where the costs of either transport or labor—or of both are very high, a may exceed 3, otherwise it may be considered to vary between 1.5 and 3, and thus an economy is effected if a part of the fabricated concrete is replaced by concrete cast in place, even if the total concrete quantity is slightly increased which, however, need not occur.

In the following, an example of a road bridge over a railway (span 34 ft.) is shown. The construction consists of I-shaped fully prestressed beams (A_c = 170 sq. in.) and of a deck slab for load distribution (see Fig. J), similar to the design reported in the paper (22). Two alternative designs (Fig. K and L) have been made in accordance with Dr. Hajnal-Konyi's proposition. The precast beams of reversed T-shape (A_c = 81 sq. in.) are fully prestressed to carry their dead weight and that of the concrete cast in place, the weight of the surfacing and of the live load being carried by the combined construction. In design Fig. K, an additional mild steel reinforcement is placed in the concrete, thus such a structure as a whole represents system (5) Table 1, the precast units themselves representing system (6b). On the other hand, in design Fig. L, according to system (6a) Table 1, the total high strength reinforcement is placed in the precast units, half of it being stretched. The unstretched wires are preferably arranged in pairs twisted, to increase the bond.

For a fully prestressed beam, generally, the following relation (14) between the depth D, the span L (both in ft.), the uniformly distributed load w (lb. per sq. ft.) and the stress f_{wt} is obtained from equation (13), r representing a rectification factor with respect to the Sectional Modulus of a rectangle of the width B (in ft.):

$$f_{wt} = \frac{\dfrac{B.w.L^2}{8} \times 12}{\dfrac{r.B.D^2}{6} \times 12^3} = \frac{w.L^2}{192\, r.D^2}\,\text{psi} \quad \ldots\ldots (13)$$

$$\frac{D}{L} = \frac{1}{8}\sqrt{\frac{w}{3\ r.f_{wt}}} \quad \ldots\ldots (14)$$

The rectification factor r will vary greatly, say, from 0.5 for a reversed T-shape to 0.9 for an I-shape, approximating a rectangle. The value f_{wt} should not be too high, say 1100 to 1200 psi., since with full prestressing f_{1w} should be positive. According to Fig. 3 and 4 the following relation exists: $f_{1w} + f_{wt} < f_{1t}$, hence $f_{wt} < f_{1t}$, but should not be higher than, say, 1300 psi.

For f_{wt} = 1100 psi., the formula (14a) is obtained, resulting in (14b) and (14c) for r = 0.85, corresponding to an I-shape Fig. J, and r = 0.60, corresponding to reversed T-shapes Fig. K and L respectively.

$$D = \frac{L}{20}\sqrt{\frac{w}{528\ r}} \ \ldots\ldots (14a), \quad D = \frac{L}{20}\sqrt{\frac{w}{450}} \ \ldots (14b),$$

$$D = \frac{L}{20}\sqrt{\frac{w}{315}} \ldots\ldots (14c)$$

If the beams are provided at a distance B_o, the loading w has to be replaced by $\frac{B_o}{B}w$, B being the width of the rectangle for which r is obtained.

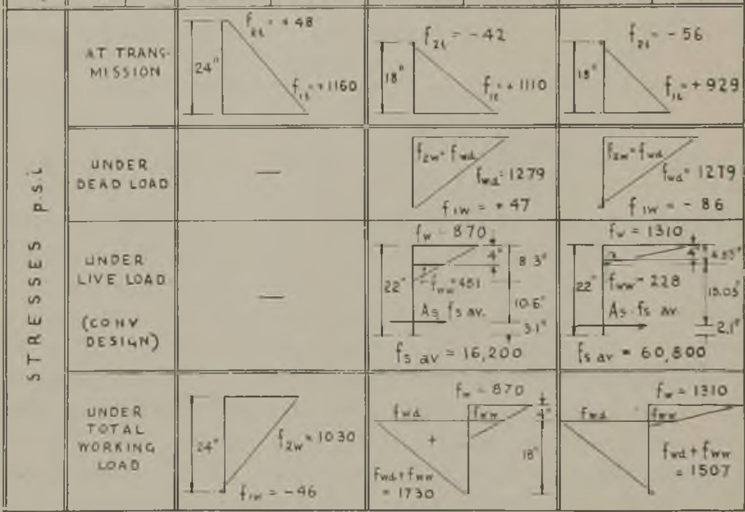
If w_o is the load per lin. ft. of the beam, w has to be replaced by $\frac{w_o}{B}$.

In the example, according to the British Regulations for Highway Bridges, the live load is replaced by a uniformly distributed load of 220 lb. per sq. ft. and by a knife edge load of 2,700 lb. per lin. ft. at the center. The dead weight of the construction Fig. J is about 250 lb. per sq. ft., thus the total load $w = 220 + \frac{2 \times 2.700}{34} + 250 = 630$ lb. per sq. ft., consequently $D = \frac{34 \times 12}{20}\sqrt{\frac{630}{450}} = 24$ in. On the other hand, for the beams of reversed T-shape, Fig. K and L, fully prestressed for the dead weight of 22 in. concrete, $w = 264$ lb. per sq. ft. and the depth required is: $D = \frac{34 \times 12}{20}\sqrt{\frac{264}{315}} = 18$ in.

In Table B, the individual stretching forces and prestresses are shown in addition to the stress distribution at the transmission and at loading. Different prestresses p_w and p'_w have been chosen for the 3 designs, consequently the stresses at transmission also differ. In design Fig. J, the same losses of the prestress have been taken into account as in the example Table A ($\Delta p_{s1} = 10{,}000$ and $\Delta p_{s2} + \Delta p_p = 15{,}000$ psi.). The latter value has been reduced to 10,000 psi. for the designs Fig. K and L, since at the stage when full prestressing has to be ensured (i.e. when the upper concrete is cast in place), only a part of Δp_p has taken place. However, it would not make a great difference, if the same amount i.e. 15,000 psi. were taken into account. For designs Fig. K and L, two loadings are distinguished: the dead weight (fully prestressed beams, $n = 8$) and the live load including the weight of the surface (combined construction, calculated in conventional manner, neglecting the concrete tensile zone, $n = 15$). When discussing the resulting stresses attained by addition of the two different stress distributions, according to the proposition of Dr. Hajnal-Konyi, it has to be borne in mind that full prestressing with respect to the dead weight remains effective even if some hair cracks might occur whenever the live load is applied. The resultant stresses at the top level of the precast beams exceed the conventional permissible concrete stresses. However, these stresses can be

TABLE B

FIGURE			J		K		L	
P_w	P'_w	psi.	115,000		140,000	100,000	110,000	70,000
P_t	P'_t		130,000		150,000	110,000	120,000	80,000
ΔP_c	$\Delta' P_c$		8,360	1,100	8,200	300	6,900	100
P_i	P'_i		148,360	141,100	168,200	120,300	136,900	90,100
P_w	P'_w	lb.	72,200	18,000	39,700	6,300	34,600	4,400
P_t	P'_t		81,700	20,900	42,500	6,900	37,000	5,030
P_i	P'_i		93,000	22,100	47,600	7,580	43,000	5,660



considered as totally harmless, since they will increase much less than the stresses at the outside fiber of the combined structure, from where failure would commence.*

From the stress distribution in Table B it is seen that design Fig. K can be considered as a fully permissible solution, whereas with respect to design Fig. L an experiment might first be required to prove that in this case the same safety against cracking and failure is obtained as in design Fig. K, in spite of the high stresses in the stretched wire. In design Fig. K, the steel stress due to live load is in accordance with the permissible stress for mild steel, thus the additional stress in the stretched wire is less than the total losses of the initial prestress. In design Fig. L, however, the initial prestress of 136,900 is reduced to 110,000 psi.,

*If the different values of n for the precast and in-situ concrete are taken into account, the theoretical stresses at the top of the precast beams are even further increased, but this does not affect the actual safety, which depends on the in-situ concrete.

but the additional stress amounts to $\frac{15.9}{15.05} \times 60{,}800 = 64{,}200$ psi., thus totalling 174,200 psi., whereas in the unstretched wires the stress is only $\frac{14.2}{15.05} \times 60{,}800 = 57{,}400$ psi., further reduced by about $8 \times 1000 = 8000$ psi. pre-compression. In spite of the great discrepancy of these two stresses, about the same safety against failure is obtained as with the fully prestressed design Fig. J. This has been demonstrated by comparative similar tests discussed in this paper. A re-distribution of stresses takes place when the bond in the neighborhood of cracks is destroyed, thus discontinuing in that surrounding the effective prestress in the steel and the pre-compression of the concrete tensile zone. Since the shear stresses in the combined structures Fig. K and L are low, there is no reason why such an experiment should give different results from those discussed in this paper. If, however, a separate shear reinforcement is provided in the webs of the precast beams, the dead weight can be considerably reduced, as e.g. in the design Fig. M, an alternative to Fig. L. By increasing the depth of the concrete slab from 4 in. to 5 in., the stress at the top fibre is reduced by 100 psi. In this solution the weight to be carried by the precast beams is only 53 percent of that in Fig. L. The reinforcement A_s could be reduced by about 20 percent, but if the same A_s is provided as in Fig. L and the effective prestress is reduced to about 2/3 (p_w = 70,000 and p'_w = 45,000 psi.), a stress distribution is obtained, as indicated in Fig. M. Also the steel stresses are reduced in this case, as compared with Fig. L. The connection of the slab, cast in place, with the precast beams is effected by links, serving as shear reinforcement and protruding from the beams into the slab. Table C contains in detail the calculation of the design Fig. K, the cross section being shown in Fig. N.

In order to compare the designs Fig. J with Fig. K—M, the cost of the concrete per square foot is calculated on the assumption that in the equation $C_F = a.C_P$, a equals 3. Thus for the design Fig. J the concrete costs are: $(3 \times \frac{170}{144} + \frac{4}{12}).C_P = 3.87\ C_P$, whereas for the designs Fig. K and L the comparative costs are $(3 \times \frac{81}{144} + \frac{264\text{-}81}{144})C_P = 2.96\ C_P$. This is further reduced in the design Fig. M to $(3 \times \frac{81}{144} + \frac{5}{12}) . C_P$*

*The cost of shuttering for the slab between the top flanges of the precast beams (e.g. by a precast thin slab) has not been taken into account, since it is only of slight influence. Moreover, it would be possible to use I-shaped precast beams in such a case, thus dispensing with any shuttering for the slab.

TABLE C

Net area A_o*)	$A_o = 18 \times 2 + (5 + 10) \times 2.5 + 2 \times 2 \times 2.5 \times \frac{1.5}{2} = 81$ in.²
	$\Delta_o = \frac{1}{81} \times 5 \times 2.5 \times 7.75 = 1.19$ in.
	$e_{1o} = 7.81$ in., $e_{2o} = 10.19$ in., $e_{so} = 6.56$ in., $e'_{so} = 8.94$ in.
	$I_o = \frac{2 \times 18^3}{12} + 36 \times 1.19^2 + \frac{15 \times 2.5^3}{12} + 25 \times 6.56^2 + 12.5 \times 8.94^2 + 3.75 \times (4.81^2 + 7.19^2) = 3402$ in.⁴
	$S_{1o} = 435$ in.³, $S_{2o} = 333$ in.³
Total area A_t	$A_t = 81 + 7 \times (0.283 + 0.063) = 83.42$ in.²
	$\Delta_t = \frac{1}{83.42} \times [5 \times 2.5 \times 7.75 + 7 \times (0.283 - 0.063) \times 7.75] = 1.30$ in.
	$e_{1t} = 7.70$ in., $e_{2t} = 10.30$ in.
	$I_t = \frac{2 \times 18^3}{12} + 36 \times 1.3^2 + \frac{15 \times 2.5^3}{12} + 25 \times 6.45^2 + 12.5 \times 9.05^2 + 3.75 \times (4.7^2 + 7.3^2) + 7 \times 0.283 \times 6.45^2 + 7 \times 0.063 \times 9.05^2 = 3559$ in.⁴
	$S_{1t} = 462$ in.³, $S_{2t} = 345$ in.³
Stresses under dead weight	$P_w = 0.283 \times 140{,}000 = 39{,}700$ lb., $P'_w = 0.063 \times 100{,}000 = 6{,}300$ lb.
	$M_d = \frac{264 \times 34^2}{8} \times 12 = 458{,}000$ lb. in.
	$f_{1w} = -\frac{458{,}000}{462} + \frac{39.700 + 6{,}300}{81} + \frac{39.700 \times 6.56 - 6.300 \times 8.94}{435} = +47$ psi.
	$f_{2w} = f_{wd} = +\frac{458{,}000}{345} + \frac{46{,}000}{81} - \frac{260{,}400 - 56{,}300}{333} = +1279$ psi.
Stresses at transmission of the prestress	$P_t = 0.283 \times 150{,}000 = 42{,}500$ lb., $P'_t = 0.063 \times 110{,}000 = 6{,}900$ lb.
	$f_{1t} = +\frac{42{,}500 + 6{,}900}{81} + \frac{42{,}500 \times 6.56 - 6{,}900 \times 8.94}{435} = 610 + 500 = +1110$ psi.
	$f_{2t} = +\frac{49{,}400}{81} - \frac{278{,}800 - 61{,}700}{333} = +610 - 652 = -42$ psi.
Initial Prestress	$\Delta p_e = 8 \times (610 + 500 \times \frac{6.56}{7.81}) = 8{,}240$ psi.; $\Delta' p_e = 8 \times (610 - 652 \times \frac{8.94}{10.19}) = 304$ psi. — according to *equ.*(4′)
	$P_i = 0.283 \times 168{,}240 = 47{,}600$ lb., $P'_i = 0.063 \times 120{,}300 = 7{,}580$ lb.

*The exact value of e_o and S_o differ only in the decimals, if the relatively small values $A_s = 0.283$ in²: and $A'_s = 0.063$ sq. in. are deducted.

TABLE C—continued from opposite page

Stresses under live load and under the weight of the surface	$M_l = \frac{220 \times 34^2}{8} \times 12 + 2700 \times \frac{34}{4} \times 12 + \frac{24 \times 34^2}{8} \times 12 =$ 699,000 lb. in.
	$d = 22 - \frac{1}{2.68} \times (2.4 \times 3.31 + 0.28 \times 1.25) = 18.9$ in.
	$p = \frac{2.4 + 0.28}{12 \times 18.9} = 0.0118$
	$k.d = 0.44 \times 18.9 = 8.3$ in.
	$f_s = \frac{699{,}000}{2.68 \times (18.9 - \frac{8.3}{3})} = 16{,}200$ psi. $f_w = \frac{2 \times 2.68 \times 16{,}200}{12 \times 8.3} = 870$ psi., $f_{ww} = \frac{4.3}{8.3} \times 870 = 451$ psi.

The different costs of the reinforcement have not been taken into account. In the Fig. J to M, only the longitudinal reinforcement is shown. About the same transverse reinforcement is required in all the designs to ensure either a load distribution (Fig. J) or a connection between precast beams and concrete in place (Fig. K—M). The amount of the longitudinal reinforcement is increased only in design Fig. K. The cost of prestressing is reduced in the alternative designs Fig. K—M, since instead of 25 wires only 11 or 12 wires respectively have to be stretched. The amount of links is reduced in the beams Fig. K and L as compared with Fig. J, but increased in the design Fig. M, in which, however, a reduction of the longitudinal reinforcement is possible.

Table D shows a comparison of constructional depth, of cost of concrete per square foot expressed in relation to the price C_P per cu. ft., and of the total initial stretching forces required. It is seen that all the values relating to Fig. J are reduced in the designs Fig. K—M.

TABLE D—COMPARISON OF DESIGN FIG. J WITH FIG. K-M

Figure		J	K	L	M
Constructional Depth	in.	28	22		23
	%	100	79		82
Cost of Concrete	C_P	3.87	2.96		2.10
	%	100	76		54
$P_i + P'_i$	lb.	115.100	55.180	48.660	33.900
	%	100	48	42	29

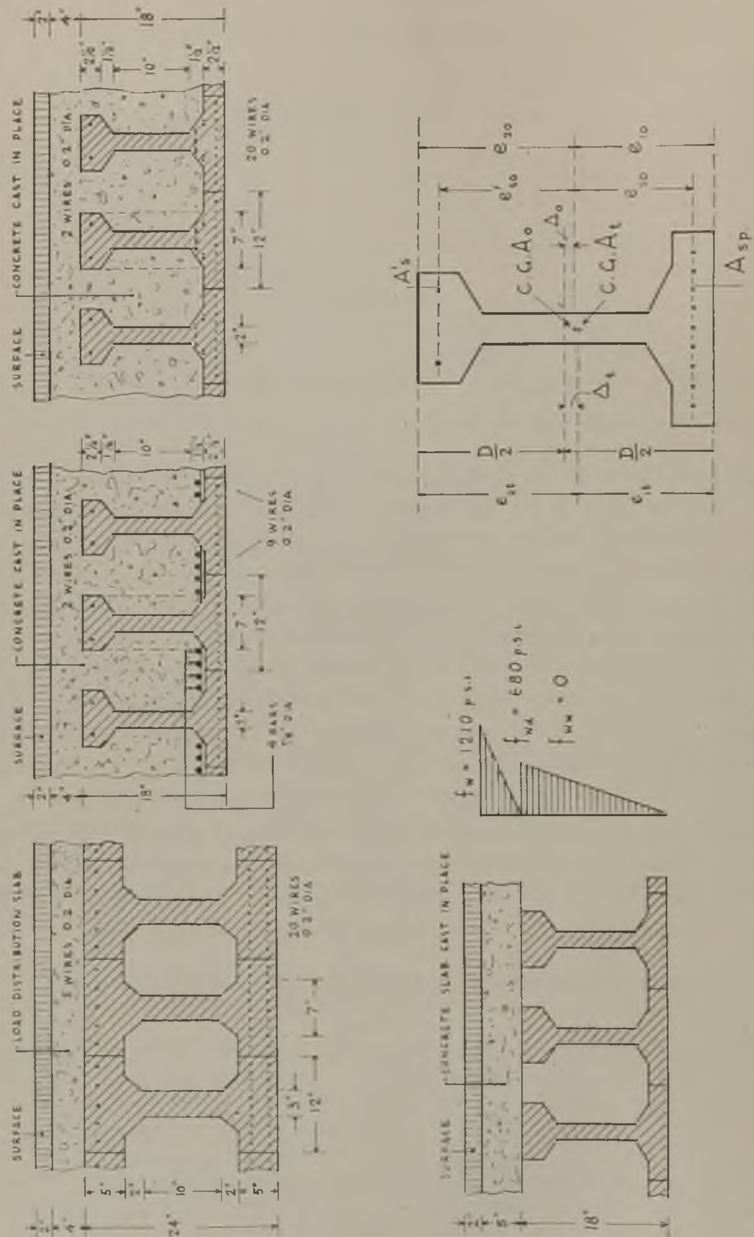


Fig. J, K, L (left to right at top)
Fig. M (precast beams as in Fig. L) and N (left to right at bottom).

Finally it may be added that, in certain circumstances, in a post-stretched construction p_t may differ from p_i, if the reinforcement is prevented from extending freely, owing to friction, resulting in a loss Δp_f (see equation 2b') [26]:

$$p_t = p_i - \Delta p_f \qquad \text{(2b')}$$

Such an occurrence, however, should be avoided by employing a more suitable arrangement of stretching. To bring the bibliography up to date, two publications relating to prestressing may be added [27], [28], the first relating to beams and the latter presenting an extensive survey on prestressed pipes, dealing with the various systems and methods.

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Coff: (21) Section V of Studies in Reinforced Concrete Technical Paper No. 22, H. M. Dep. for Scientific & Industrial Research, London, 1939.

Hajnal Konyi: (22) Correspondence on "The Use of Precast Prestressed Concrete Beams in Bridge Deck Construction" by *Alan Andrew Paul, Journal of the Institution of Civil Engineers*", London October 1944, Supplement.

(23) R. Souter: "Die Verbundwirkung zwischen vorgespannten und nicht vorgespannten *Beton und ihre* Anwendung auf den Plattenbalken mit vorgespannten Steg", Schweizerische Bauzeitung Bd. 124, No. 8, 26. August 1944, S. 103-108; No. 9, 2. Sept. 1944, S. 126.

CLOSURE:—

(24) E. J. M. Honigmann, "Witterungseinflüsse und ihre Raffung in Kurzversuch an Schleuderbe tonmaststücken, *Beton und Eisen*, 1935, Nos. 19 & 20.

(25) e.g. "Calculation of Statically Indeterminate Systems based on the Theory of Plasticity", by *Dr. F. Bleich.*" Intern. Association for Bridge and Str. Eng.", 2nd Congress, Berlin 1936.

(26) "Prestressed Beams under direct Bending and Sustained Loading" by Dr. K. L. Rao, *The Structural Engineer*, Oct. 1944.

(27) "Prestressed Reinforced Concrete", by Kurt Billig, 1st Edition, New York 1943; Second Edition London 1944.

(28) "Prestressed Concrete in Structures of Annular Cross Section", by Dr. K. W. Mautner, *The Structural Engineer*, Mar. 1945.

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7400 SECOND BOULEVARD, DETROIT 2, MICHIGAN (Supplement) November 1945

Discussion of a paper by Long, Kurtz, and Sandenaw:

An Instrument and a Technic for Field Determination of the Modulus of Elasticity, and Flexural Strength of Concrete (Pavements)*

By PAUL L MORTON and ALEXANDER DODGE

By PAUL L. MORTON†

The authors are to be complimented upon their very ingenious method of measuring the elastic modulus of concrete *in situ;* unquestionably it is capable of yielding a great deal of hitherto unobtainable information. Through their kindness in supplying the details of their method even before its publication, the Engineering Department of the University of California was enabled to do experimental work along the same lines during the past year. Although we have not been able to realize the full possibilities of the method, we feel that further work on it is well justified.

To avoid the use of the ballistic galvanometer, which Messrs. Long, Kurtz, and Sandenaw found inconvenient, we built a timing unit in which the galvanometer was replaced by a capacitor which was charged during the interval to be measured. The charge on the capacitor was measured by means of a vacuum-tube voltmeter circuit, and the final time-interval readings were taken directly from a microammeter. By this means readings could be taken as rapidly as the data could be recorded. Fig. A of this discussion shows a front view and Fig. B a rear view of the interval timer, removed from its carrying case. It was operated from a 110-volt a-c power line, but could be built for battery operation if desired. (A similar timer is now available commercially, having 8 time ranges from 0.001 to 3 seconds, full scale.)

When the timer was used in the field it became apparent that the readings were more erratic than the timer, and the difficulty was traced to the type of pickup units used. The functions of the pickup units are to start

*ACI Journal, Jan. 1945; *Proceedings* v. 41, p. 217.
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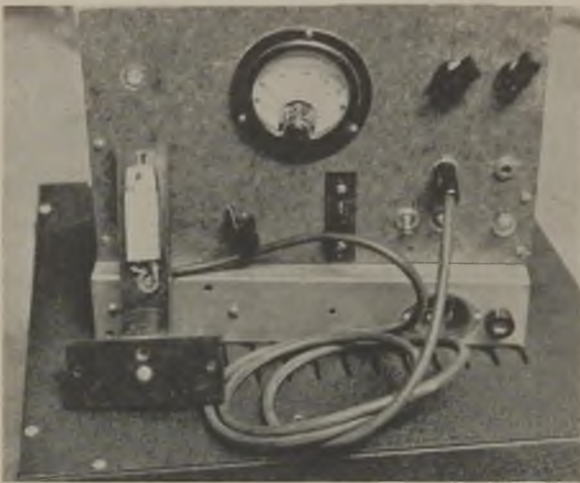

Fig. A—Front view time-interval meter, showing phonograph crystal pick-up.

Fig. B.—Rear view time-interval meter.

the timing operation as the shock wave in the concrete passes the first or "start" pickup, and to stop the timing when the wave passes the "stop" pickup which might be from one to thirty feet distant. Following the authors, we first used vibration pickups consisting of a square flat slab of Rochelle salt supported at three corners, with the fourth corner free to flap or vibrate. This crystal was contained in a small casting which rested upon the concrete. The arrival of a shock wave moved the casting and started the vibration of the crystal. In a piezoelectric crystal of this type a voltage appears across the crystal whenever distortion occurs, and it was this voltage which was used to start and stop the timing operation.

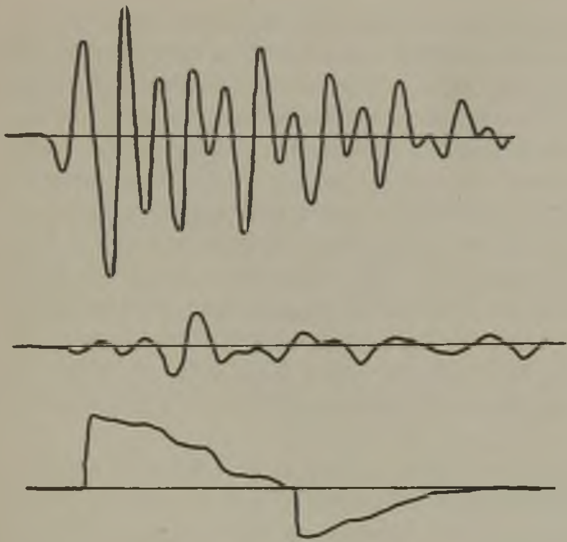

Fig. C, D, E (top to bottom)—C, Response of vibration pickup (shock wave from hammer blow—crystal 3 ft. from origin); D, Response to attenuated wave (same wave as Fig. C—identical crystal 12 ft. from origin); E, Response of phonograph crystal (shock wave from hammer blow as in C and D.)

In the type of crystal pickup just described it is the inertia of the crystal which is responsible for its distortion, since the free corner of the crystal tends to remain stationary when the crystal housing is moved by the displacement wave in the concrete. The voltage output of the pickup is therefore roughly proportional to the *acceleration* of the crystal housing, and does not reproduce the wave-form of the shock wave in the concrete. After the shock wave-front has passed, the crystal continues to flap at its own natural frequency, and the voltage output therefore resembles a damped sinusoid, modified by further motion of the concrete surface. Fig. C is a tracing of an oscillogram of the output voltage caused by a shock wave from a hammer blow on the concrete.

In Fig. C it will be noted that the voltage does not rise instantly when the shock wave arrives at the pickup, but only gradually. The instant when the timing unit starts will depend upon the sensitivity of the timing unit. Best results will be obtained if the timing unit is made extremely sensitive, so that it trips very early on the first half-cycle of the voltage. (Our unit, when set for maximum sensitivity, could be tripped by flipping the finger-nail upon the concrete near the pickup.) Note that if the timer fails to trip on the first cycle it may trip on a subsequent larger cycle. The delay between arrival of the shock wave and the tripping of the timer would not cause error if, but only if, both the start-pickup and the stop-pickup voltages operate the timer at the same point on the voltage wave.

Since the concrete is not a perfectly elastic medium a shock wave set up in it is gradually attenuated as it travels. Fig. D is a tracing of an oscillogram of the voltage output of a second identical crystal pickup, subjected to the same shock wave which caused the voltage of Fig. C, but located 9 feet farther from the origin of the wave. The wave is greatly attenuated and somewhat changed in form; considerably more amplification would be necessary to cause the second pickup to stop the timing at the same point on the wave at which it started. Since the attenuation rate is unknown it would be difficult to preset the amplifiers correctly. Furthermore, it is impossible to set up wave trains of identical form repeatedly, because of the crumbling of the concrete where it is struck. In Figs. C and D the frequency of the oscillations is about 1500 cycles per second, and the shock wave travels about ten feet in the time of one cycle. Thus a slight difference in the points on the voltage cycle at which the timer starts and stops represents a considerable difference in the apparent velocity.

Obviously it would be an advantage to have the pickup unit supply a voltage proportional to the actual displacement of the concrete surface rather than to its acceleration, and to obtain a wave-front as sharp as possible. For this reason a phonograph pickup crystal was substituted for the vibration pickup; one of the phonograph units is plugged into the timer panel in Fig. A. The phonograph needle rested on the concrete surface, and this positive connection caused the distortion of the crystal to follow the motion of the surface and provide a proportional output voltage. The resulting output wave-form is shown in Fig. E, where the blow on the concrete was against a vertical surface. (The second step shows the arrival of a shock wave reflected from the far side of the slab.) For this type of voltage the timer will trip on the wave front or not at all, and even with a greatly attenuated wave at the second pickup the error is much reduced.

With the new pickups the kind of blow which initiates the shock wave becomes important, since the pickup will respond only to motion at right angles to the needle. Thus if longitudinal waves and transverse waves travel at different velocities the corresponding differences in transit time could be recorded. Experimental and theoretical study of the types of waves established by hammer blows or other kinds of excitation, and of the significance of the velocities of the different waves, should be very fruitful. Messrs. Long, Kurtz, and Sandenaw are to be congratulated upon opening up so promising a field for research.

By ALEXANDER DODGE*

The authors may be commended on the presentation of a matter which is always a subject of vital interest to the engineering profession, but the writer regrets that the exhibited material constitutes apparently

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only a minor portion of the extensive test data available. One notices, for example, that the flexural strength data in Table 1, if plotted on Fig. 7, will fall in the area corresponding to elastic modulus of 7,000,000 psi or greater, and one wonders if these data do not represent the highest of the fourteen class-intervals of dynamic E.

A conclusion may be drawn from the paper that the exhibited data represent approximately 5 per cent of the whole testing work performed. In such a case it would be of the utmost interest to know what the average flexural and compressive strengths are and the age of all 1400 concrete beam specimens, separately for 4x4x16 in. and 8x9x40 in. beams respectively.

The authors state that the curve in Fig. 7 represents the relationship between dynamic E and the flexural strengths, with "a probable error of approximately 70 psi." For the Army Airfield D, Runway (b), it is found, in Table 1, flexural strength of 700 psi for the dynamic E value = 6.77, and for the same value of E, the corresponding value of the flexural strength in Fig. 7 is 870 psi, or the difference of 170 psi, which is approximately 25 per cent greater than the actual strength.

The writer is of the opinion that it is premature to adopt dynamic E values for all design conditions. It has been discussed by many of the foremost engineers and proven by field experiments of others that the long time loadings, such as: seasonal and daily variation of the temperature, shrinkage and variation in moisture content in concrete as well as in underlying subgrade, may be, and very often is, the primary cause of failure of an unreinforced concrete; yet, just because of uncertainty and difficulty of analysis (and sometimes because of the resulting high stresses) these factors still are not given due consideration.

The writer's own studies indicate that, for the typical airfield construction, the stresses from these causes vary from 200 to 500 psi, and it is yet to be proven how much the high values of the flexural strengths (third point loading), shown in Table 1, might be changed by a long-time testing, say, of several hours, days and weeks duration.

These comments do not constitute a criticism of the fine work performed by the authors, but are offered, rather, as a warning to the profession at arriving at too hasty conclusions on the value of dynamic E values in design of concrete pavements.

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Discussion of a paper by T. C. Powers:

A Working Hypothesis for Further Studies of Frost Resistance of Concrete*

By RUTH D. TERZAGHI, DOUGLAS McHENRY and H. W. BREWER, A. R. COLLINS and AUTHOR

By RUTH D. TERZAGHI*

SCOPE OF DISCUSSION

In his stimulating article on the frost resistance of concrete, Mr. Powers has described a possible mechanism of the disruption of concrete by freezing. In this discussion an attempt will be made to find out whether this mechanism might lead to the disruption of concrete in the field. According to Mr. Powers' hypothesis, disruption may be due to the development of a high hydrostatic pressure in the pore water of concrete adjacent to the zone of freezing. As Mr. Powers has pointed out, this process can be effective only if the concrete is saturated to a certain minimum depth which he terms the critical depth of saturation. Hence we may test the applicability of this hypothesis to field conditions by estimating this depth for concrete of given properties, exposed to natural winter weather, and comparing our estimate with the depth to which such concrete is likely to be completely saturated under normal conditions. Such an estimate is presented in the section on "Critical Depth of Saturation." The results of the estimate suggest that disruption of lean concrete under natural climatic conditions is due to some process other than that postulated by Mr. Powers.

Another topic with which Mr. Powers' article deals is the classification of the pores which contain freezable water. To the two categories of such pores mentioned by Mr. Powers, the present writer would add a third, consisting of the capillary spaces between aggregate and paste. Evidence for the existence of such spaces is presented under the heading "Locus of Freezable Water."

*ACI Journal, Feb. 1945; *Proceedings* V 41, p. 245.
†Geologist, Winchester, Mass. (now cooperating with her husband, Karl Terzaghi on a textbook on engineering geology).

A part of Mr. Powers' article is devoted to a previously stated hypothesis that ice lenses, analogous to those which develop in some soils and sediments, may form in concrete. His conclusions regarding this hypothesis are based in part on the opinion that such ice lenses do not form in relatively impermeable soils. This opinion is not supported by observations of geologists, which are discussed under the heading "Conditions for the Formation of Ice Lenses in Soils and Sediments."

CRITICAL DEPTH OF SATURATION

The purpose of this section is to evaluate the extent to which Mr. Powers' hypothesis may be applicable to the disruption of concrete by frost under natural climatic conditions. The basis for the evaluation is provided by an estimate of the critical depth of saturation corresponding to the most unfavorable winter conditions likely to be encountered.

For concrete with given properties, exposed to natural winter climate, the critical depth of saturation depends on two quantities. These are the rate of decrease of the air temperature from about 32 F to several degrees below that temperature, and the pore water pressure necessary to produce rupture. The first of these is determined by climatic conditions and is probably never in excess of 5 F per hour. The second quantity, the pore water pressure at rupture, is a function of the tensile strength of the concrete and of the shape of the voids.

Published experimental data* indicate that the shape of the voids of concrete is such that the pore water pressure is transmitted to at least 97 per cent of the cross section. Hence we must conclude that the pore water pressure at rupture is approximately equal to the tensile strength of the concrete. This conclusion is incompatible with Mr. Powers' estimate (page 253) that the pore water pressure must become equal to about five times the tensile strength of the concrete in order to damage it. However, the critical depth of saturation will be determined on the basis of both assumptions.

In the following paragraphs, equations will be derived which express the relation between the critical depth of saturation, the rate of temperature decrease of the surroundings, and the pore water pressure at rupture. Conditions at the time of rupture are illustrated by Fig. A.

The symbols which will be used are as follows:

C = constant of integration.
K = coefficient of permeability of the concrete, in cm per sec.
L = latent heat of water per gram = 80 calories.
n = volume of freezable water per unit of volume of saturated concrete.

*Terzaghi, Karl, "Die wirksame Flachenporositat des Betons." *Zeitschrift des Oesterreichischen Ingenieur- und Architekten-Vereines*, Heft [illegible]. 1934. See also a forthcoming article by the same author "Stress Conditions for the Failure of Saturated Concrete and Rock," Journal American Society for Testing Materials.

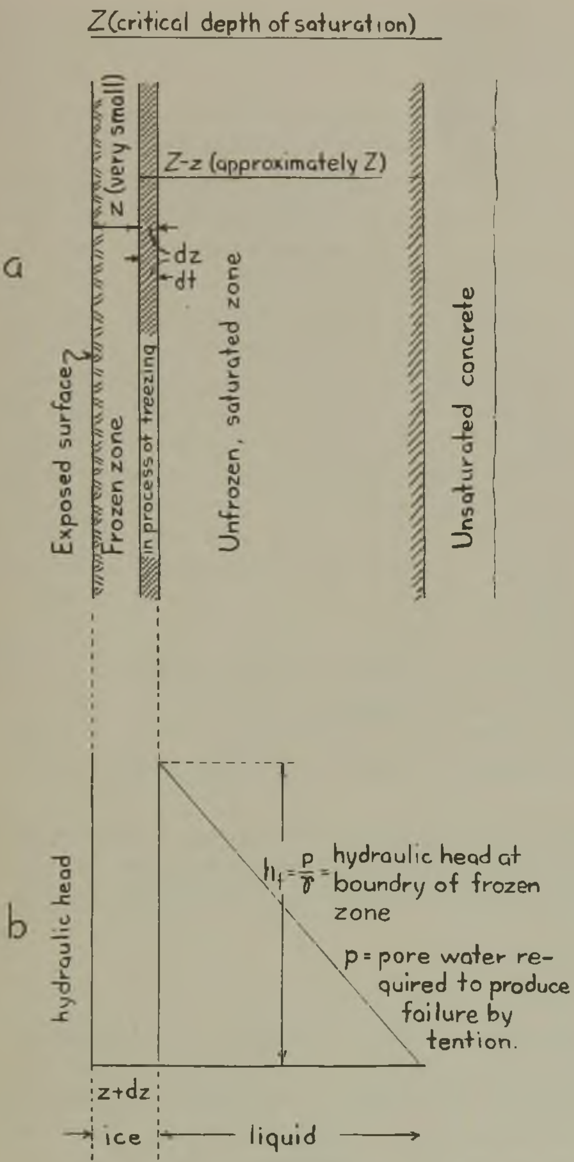

Fig. A—Diagram illustrating conditions during freezing of pore water in saturated zone. a) Cross section through concrete saturated to critical depth Z; b) Hydraulic pressure conditions in unfrozen saturated zone at moment of rupture.

p = hydrostatic pressure in pore water at failure, in gm per sq. cm.

q_c = calories withdrawn from concrete, per unit of area of cross section.

q_w = volume of water displaced, per unit of area of cross section, owing to expansion accompanying freezing.

r = maximum rate of temperature change in degrees centigrade per second under normal climatic conditions, assumed equal to -0.0008 deg. C per sec., corresponding to -3 C (-5 F) per hour.

t = time in seconds
z = depth of frost penetration
Z = critical depth of saturation
γ = unit weight of water
λ = heat conductivity of saturated concrete = 0.003 cal cm^{-1} degC^{-1} sec^{-1}*.
θ_0 = initial temperature, 32 F or slightly below, of concrete and surroundings.

We shall assume that the concrete is completely saturated to depth Z, that it loses heat through one surface only, that the mass has a uniform cross section, and that the concrete and its surroundings are initially at the same temperature θ_0 at or slightly below 32 F which is such that a decrease of temperature will cause freezing. It is assumed that the conditions of exposure are such that the temperature at the surface of the concrete is always equal to the air temperature. We shall also assume that no supercooling takes place, i.e., that the pore water freezes at that temperature at which it would freeze in the presence of an ice crystal. This assumption seems justified, because it is improbable that appreciable supercooling would take place in water-filled pores communicating with free water at the surface of the concrete, which is almost certain to freeze at or near 32 F. Of the several assumptions on which the derivation is based, this is the only one which is a possible source of an underestimate of the rate of freezing.

At time $t = 0$, the air temperature begins to fall at a constant rate r. The water in the pores of the concrete begins to freeze, and the boundary between pore ice and pore water moves into the concrete at a rate dz/dt. At this boundary, the temperature is equal to the freezing point of the pore water θ_0.

If the boundary between ice and water moves through a distance dz, the corresponding volume of water frozen per unit of cross section is $n\,dz$, and the rate at which calories must be withdrawn to freeze this quantity of water is

$$\frac{dq_c}{dt} = L\, n\, \gamma\, \frac{dz}{dt}\ cal\ cm^{-2}\ sec^{-1} \ldots\ldots\ldots\ldots\ldots\ldots (1)$$

In addition to the latent heat of water, a small quantity of heat dq_c flows out of the concrete on account of the existence of a temperature gradient between the interior of the mass and the outside temperature. A simple computation shows that this quantity is less than 1 per cent of dq_c as long as z does not exceed a few centimeters. Hence it does not require consideration.

The rate at which heat is withdrawn from the zone of freezing, per unit of area of the surface of cooling, is equal to the heat conductivity of the concrete multiplied by the temperature gradient in the concrete

*Knoblauch, Raisch, and Reiher, *Gesundheits Ingenieur*, v. 43, p. 607, 1920; cited in *International Critical Tables* (1927) v. 2, p. 314.

between the zone of freezing and the surface. The temperature in the zone of freezing is equal to the freezing temperature of the pore water, θ_0, which is identical with the temperature of the entire mass of concrete at time $t = 0$. At any time t, the temperature of the surface of the concrete is $\theta_0 + rt$, and the distance between the surface and the zone of freezing (depth of frost penetration) is z. Hence the rate at which calories are withdrawn at time t is

$$\frac{dq_c}{dt} = \frac{\lambda rt}{z} \; cal\ cm^{-2}\ sec^{-1} \quad \ldots\ldots (2)$$

In reality the temperature at depth z is slightly lower than θ_0 when rupture takes place, owing to the fact that the increase in pressure from zero to p causes a slight decrease in the freezing point. Hence our assumption that this temperature is constant and equal to θ_0 results in a slight overestimate of the temperature gradient, of the rate of heat withdrawal, and hence of the rate of frost penetration.

Setting

$$U = \frac{\lambda\, r}{L\, n\, \gamma}$$

and combining equations (1) and (2) we have

$$z\, dz = U\, t\, dt$$

$$\int z\, dz = U \int t\, dt$$

$$\frac{z^2}{2} = \frac{Ut^2}{2} + C$$

When $z = 0$, $t = 0$. Hence $C = 0$, and

$$z = t\sqrt{U} \quad \ldots\ldots (3)$$

$$\frac{dz}{dt} = \sqrt{U} \quad \ldots\ldots (4)$$

When a volume of water, per unit of cross-section, $n\ dz$, freezes, a volume of water per unit of cross-section

$$dq_w = 0.09\ n\ dz$$

must be displaced because of the expansion attending freezing. The rate at which it must be displaced is

$$\frac{dq_w}{d\,t} = 0.09\ n\ \frac{dz}{dt}\ cm\ sec^{-1} \quad \ldots\ldots (5)$$

The quantity dq_w/dt is equal to the coefficient of permeability of the concrete multiplied by the hydraulic gradient. Both theory and experience indicate that rupture (spalling) takes place near the exposed surface. Hence the hydraulic gradient at rupture is approximately $p/\gamma Z$, and the initial rate at which water is displaced from the zone of freezing to the inner edge of the saturated zone is given by the following equation:

$$\frac{dq_w}{dt} = \frac{Kp}{Z\gamma} \, cm \; sec^{-1} \quad \ldots\ldots (6)$$

Combining equations 4, 5 and 6, we have

$$Z = \frac{Kp}{\gamma \, 0.09 \, n \sqrt{U}} \, cm \quad \ldots\ldots (7)$$

U may be evaluated with sufficient accuracy for present purposes from the data given in the list of symbols. Using these values we find

$$U = \frac{3 \times 10^{-8}}{n} \, cm^2 \; sec^{-2}$$

$$\sqrt{U} = \frac{dz}{dt} = 1.73 \times 10^{-4} \sqrt{1/n} \; cm \; sec^{-1} \quad \ldots\ldots (8)$$

The critical depths of saturation for severe winter conditions, calculated by means of equation (7) for the rich and for the lean concrete described by Mr. Powers on page 255, are 0.2 in. and 7 in. respectively, if rupture takes place at a pore water pressure equal to the tensile strength of the concrete. If this pressure must reach a value equal to five times the tensile strength in order to cause appreciable damage, as assumed by Mr. Powers, the corresponding values of the depth of saturation are 1 in. and 35 in. respectively. A depth of saturation of 7 in., even in lean concrete, is probably not common, and it does not seem likely that frost damage to such concrete is limited to those situations where it is saturated to this depth. Hence we must conclude that frost damage to lean concrete exposed to natural climatic conditions is generally due to some process other than that postulated by Mr. Powers.

LOCUS OF FREEZABLE WATER

Mr. Powers has listed two classes of space of capillary dimensions in concrete, likely to contain freezable water. To these the present writer would add a third, consisting of the space between aggregate, particularly the coarse aggregate, and the paste. Evidence of the existence and relative importance of these spaces is found in the condition of some specimens of concrete recently examined by the writer, as well as in the appearance of laboratory specimens of concrete described by Merkle. This evidence is presented in the following paragraphs.

The specimens examined by the writer are drill cores taken from a large mass of concrete two years after casting. The water-cement ratio of the concrete (by weight) was 0.53. The specimens showed no evidence of internal bleeding, of segregation, or of other original defects which might have resulted in the presence of abnormal cavities in the concrete. One face of the structure is in contact with sea water and one face is exposed to the atmosphere. Sea water enters the concrete under a hydraulic gradient which varies from place to place between about one and about five. Its access to the interior of the structure has been facilitated by the presence of cracks which developed progressively and had become conspicuous two years after casting. In specimens taken from this structure, within a zone whose upper surface slopes away from the sea-face, each of the medium-sized and large pieces of aggregate, with a diameter of about one-quarter inch to three inches, bears on its surface a more or less continuous film of fresh ettringite ($3CaO.3CaSO_4.Al_2O_3.31H_2O$). In most of the specimens there is little or no ettringite on aggregate smaller than about one-quarter inch. The films are commonly a few hundredths of a millimeter (about one-thousandth of an inch) thick; exceptionally they are as thick as one-quarter of a millimeter. The thickness of the film on a given piece of aggregate is uniform. In a few specimens, the surface of coarse aggregate bears a deposit of ettringite which has been in part replaced by calcium carbonate and other substances. In these specimens, the medium-sized and finest aggregate is commonly covered with a deposit of unaltered ettringite.

The present writer's interpretation of these features is based on the following considerations. Although the constituents of the ettringite may have been derived in their entirety from the paste, the actual distribution of readily visible ettringite crystals can be explained only on the assumption that percolating sea water was necessary for their formation. Likewise, the local replacement of ettringite on coarse aggregate by calcium carbonate can be attributed only to the percolation of water (doubtless sea water) containing calcium bicarbonate or carbonic acid in solution, following the deposition of ettringite. We must, therefore, conclude that water has seeped through the concrete along the contact between aggregate and paste. Since the results of this percolation are most conspicuous at the contact between coarse aggregate and paste, the seepage velocity was doubtless most rapid along these contacts.

Evidence of a similar nature was described by G. Merkle*, who found conspicuous deposits of calcium carbonate at the contact between paste and coarse aggregate in laboratory specimens of concrete through which water with a high calcium bicarbonate content had percolated.

Whether a capillary space between aggregate and paste is universally present in concrete, or only under certain conditions, is of course an open question. Wherever such spaces are present, they would doubtless be among the first to be drained under drying conditions and the first to be

*Durchlässigkeit von Beton, Berlin 1927.

refilled when the concrete comes in contact with water. Under certain circumstances, water contained in them might play an important part in the disruption of concrete by freezing.

CONDITIONS FOR THE FORMATION OF ICE LENSES IN SOILS AND UNCONSOLIDATED SEDIMENTS

On the basis of evidence from several sources, Mr. Powers concludes that the formation of ice lenses in concrete, analogous to those formed in soils and sediments, is unlikely. One line of evidence is stated as follows:

"Inferred evidence against applying the Taber hypothesis of ice lense formation comprises those factors and observations indicating that concrete must freeze as a 'closed system,' whereas segregation of ice i.e., formation of ice lenses, can occur only in 'open systems'." (Closed systems are those whose total water content does not change, owing either to the low permeability of the material comprising it or to the inaccessibility of water.) The implication that ice-lense formation is limited to relatively permeable materials is not in agreement with observations by geologists regarding the conditions of ice lense formation in soils and sediments. It would therefore represent a service to all concerned if Mr. Powers would present in detail the observations on which his statement is based.

Observations made by Taber* in the laboratory may be summarized as follows:

Very slight segregation took place in ground quartz with an average particle size between 6 and 10 microns. Precipitated barium sulphate, very uniform in size, having an average particle diameter of about 2 microns and a maximum diameter of about 3 microns, gave well-defined segregation under favorable conditions of cooling, but no segregation under unfavorable conditions. In materials having a particle size of about 1 micron or less, segregation took place readily. Ice layers formed without difficulty in all of the clays investigated by Taber.

Similar observations regarding the influence of grain size were made by Beskow† both in the field and in the laboratory.

In these observations, there is nothing to indicate that low permeability is an obstacle to ice-lense formation. On the contrary, ice lense formation takes place most readily in the relatively impermeable soils. Hence the question as to whether or not ice lenses can or do form in concrete must be answered on the basis of other evidence.

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*Taber, Stephen, "Frost Heaving," The Journal of Geology, vol. 37, 1929, pp. 427-517.

†Beskow, Gunnar, Soil Freezing and Frost Heaving, Swedish Geol. Surv., Series C, No. 375, 26 Yearbook No. 3, Stockholm 1935.

By DOUGLAS McHENRY and H. W. BREWER*

Mr. Powers has attached a very formidable problem with commendable courage and with a degree of scientific detachment which permits no delusions regarding the complexities of the problem or the possibility of resolving its many dilemmas by the application of a single hypothesis. The writers find no fault with the hydraulic pressure hypothesis—nor indeed with any major phase of the paper—provided it is viewed as intended by the author as "a working hypothesis which, together with other hypotheses, may eventually lead to the desired solution." The comments in this discussion, based largely on experimental work in the laboratory of the Bureau of Reclamation, are not intended to simplify the problem, but rather to emphasize its complexities and the need for further study and still other hypotheses. For example, in accounting for the variable effect of increased hydration on durability, the author lists three pertinent effects:

1. It reduces permeability.
2. It reduces the amount of freezable water.
3. It possibly increases the initial degree of saturation.

According to the hypothesis a reduction in permeability and an increase in degree of saturation are detrimental to durability, while a reduction in the amount of freezable water is beneficial. To the above three pertinent effects at least three others might well be added:

4. It reduces the amount of cement available for autogenous healing.
5. It reduces the ability to creep.
6. It increases the tensile strength.

Thus, at least six factors enter into this phase of the problem, and of these 2 and 6 are favorable while 1, 3, 4, and 5 are unfavorable to durability. Even for a particular concrete it appears that the relationship between durability and degree of hydration is not a simple one. The Bureau of Reclamation has conducted tests in which the mixing and initial curing temperatures were 40, 60, 80 and 100 F. These temperatures were maintained for 24 hours, after which the specimens were fog cured at 70 F for 180 days. The number of cycles of freezing and thawing required to produce an expansion of 0.6 percent was 310, 450, 300, 245, respectively. Apparently for this concrete there is a certain optimum curing temperature, and presumably an optimum degree of hydration, to produce maximum durability under the conditions of test employed.

The author did not discuss the healing effect of continued hydration during the freezing and thawing period, but the writers believe that this may often be a significant factor with respect to durability—especially under laboratory conditions in which the tests are started at an early age.

*Bureau of Reclamation, Denver, Colo.

Reagel*has reported tests which show that thawing at 40 F causes more rapid breakdown than thawing at 90 F, and his results have been confirmed by Hornibrook.† It is inferred from the author's discussion of

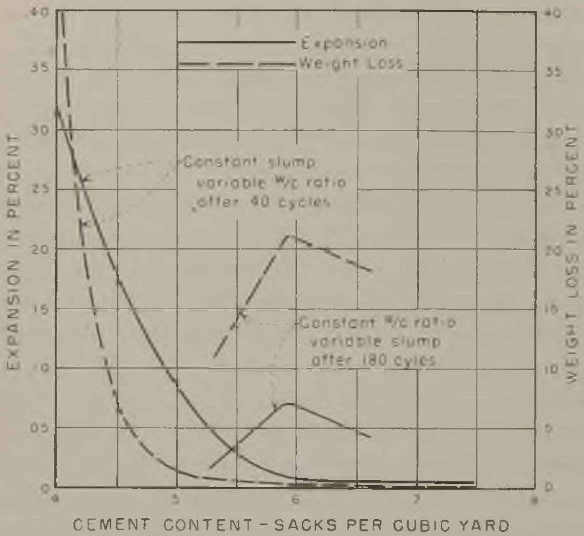

Fig. B—Durability of 3 x 6 in. concrete cylinders.

"Factors governing the amount of water absorbed during thawing" that from consideration of hydraulic pressure alone the reverse should be true because a more rapid melting of the surface ice permits greater absorption during the thawing period. Hornibrook has attributed the greater resistance with a higher thawing temperature to repair by autogenous healing, and it may be that this effect overshadows the effect of the greater absorption discussed by the author.

The results of the freezing and thawing tests on the concrete shown by Fig. 2 are contrary to all test results obtained in this laboratory. Fig. B of this discussion shows expansion and weight loss curves for two typical test series. In both instances the expansion and weight change curves are similar. The writers know of no test results by others which confirm the author's Fig. 2, and it seems that his results must be attributed to some unusual combination of circumstances.

The author mentions fissures under the aggregate particles as one type of cavity which may affect resistance to weathering, but it is not clear whether he attributes to them an increase or a reduction in durability. The writers believe that these fissures contribute to low durability, especially in concrete which bleeds freely. In permeability tests in which

*F. V. Reagel, "Freezing and Thawing Tests of Concrete." Proc. Highway Research Board, v. 20, p. 587 (1940).
†F. B. Hornibrook, Discussion of foregoing reference.

Fig. C—Ice segregation resulting from slow freezing starting when the concrete was 4 hours old — magnified 4½ x

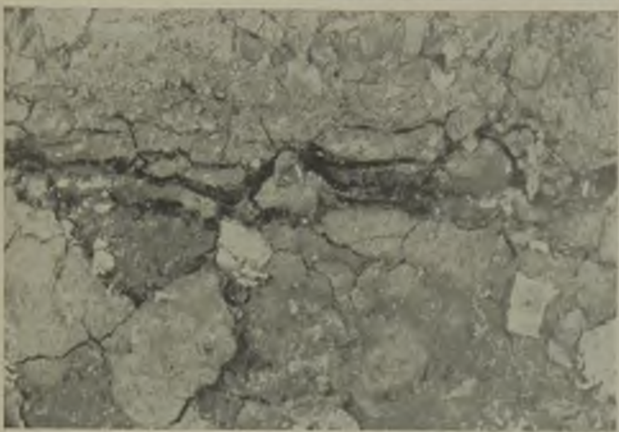

Fig. D—Ice lens formed in soil of permeability corresponding to that of moderately tight concrete—magnified 3 x

dye was used, the greatest concentration of dye was always found in the fissures between the aggregate particles and the paste. Observation of frozen concrete and of disintegrated concrete with the binocular microscope indicates that ice forms readily in such fissures, and that it exerts a disruptive effect by loosening the aggregate particle from its socket. This condition was especially pronounced in cylinders which were frozen slowly from the top downward with the bottom immersed in water. It is considered not unlikely that the concentration of ice in these regions of low tensile strength is closely related to the segregation which the author has discussed under the heading "The Taber-Collins Hypothesis." The writers have not observed any other type of segregation in mature concrete, although distinct lenses have been produced in concrete which was frozen slowly starting when the concrete was only four hours old (Fig. C).

The author remarked on the absence of permeability data on soils which showed ice segregation, but concluded that for concrete "the degree of permeability alone precludes the formation of lenses under the usual rates of cooling." The writers used Taber's technique in testing a cylinder of saturated clay having a permeability coefficient of 1370×10^{-8} feet per day, corresponding to concrete of moderate permeability, and found that a single freezing produced a lens about 1/16 in. thick extending over the entire cross-section of the cylinder (Fig. D). The rate of freezing was slower than the usual rate for freezing and thawing tests, but was not too slow to be representative of some field conditions. The test suggests that the tensile strength, rather than the impermeability, accounts for the usual absence of visible lenses in concrete.

Some test data which have been reported in the literature suggest that the author's figure of 2500 psi as the internal force required to produce disruption of concrete may be too high by a considerable amount (at least, for saturated specimens). Perhaps the best of such data are given by Leliavsky*, who determined the pore pressure required to fracture concrete tubes subjected to axial and circumferential pressures. He concluded that the pore pressure acted over about 92 percent of the cross-sectional area, and that "The fraction of the area of cross-section over which the internal pressure acts can therefore have nothing to do with ordinary porosity." Terzaghi† had previously been led to a similar conclusion by measuring the effect of pore pressure on strains in concrete cylinders. However, the writers do not consider that these tests were conclusive, and believe that more experimental work is needed in this field.

It is believed that much more laboratory work—not haphazard testing, but rather fundamental research—is needed before the mechanism of frost action can be satisfactorily explained. However, the research should not be restricted to the laboratory, for, as demonstrated particularly by recent work of Tremper‡ and Jackson**, valuable clues can be obtained from field inspections. The so-called D-cracks which are often found in concrete structures subjected to freezing and thawing have not been satisfactorily explained. The D-lines which are close to and roughly parallel to edges have been interpreted by some observers as a result of ice segregation.

By A. R. COLLINS§

Mr. Powers' excellent paper has been read with very great interest. He writes with the advantage of having a backing of unpublished data to

*Serge Leliavsky, "Uplift in Dams," *Nature* (London) No. 3770, p. 137 (1942)

†Karl Terzaghi, "Simple Tests to Determine Hydrostatic Uplift," *Eng. News-Record*, v. 116, p. 872 (1936).

‡Bailey Tremper, "The Effect of Alkalies in Portland Cement on the Durability of Concrete," A.C.I. JOURNAL, Nov. 1944, p. 89.

**F. H. Jackson, "Disintegration of Bridge Concrete in the West," Public Roads, v. 24, No. 4, p. 98 (1945).

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support his hypothesis and detailed discussion is, therefore, rather difficult.

Mr. Powers' hypothesis appears to provide a good explanation of most of the data obtained in laboratory tests and perhaps also in some field conditions, but it is felt that there are some circumstances, particularly in regard to concrete pavements, in which ice segregation provides a better explanation of the observed phenomena. This may mean that damage is caused by different mechanisms according to the conditions of freezing and the type of concrete concerned. It appears that Mr. Powers recognizes this possibility when, on page 267, in doubting the validity to the application of the ice segregation theory to concrete, he expressly confines his remarks to freezing in laboratory conditions.

The evidence that suggested ice segregation as the cause of damage in field conditions was obtained in England in early 1942. There was, at that time, an unusually long, severe and continuous frost with little or no intermediate periods of thawing. Throughout the freezing period pavement concrete of poor quality on a certain site appeared to be undamaged, though it was not subjected to traffic. When the final thaw occurred, however, a spectacular failure took place, the concrete being broken up in horizontal layers to its full depth of 6 in. This failure seems to be more easily explained by the formation of ice lenses than by hydraulic pressure.

In laboratory tests in which freezing was applied to one face of the concrete and a water reservoir maintained at the opposite face (conditions which more closely represent practice than the usual freezing test), the total moisture content of the specimen at the end of one period of freezing and before thawing was found to have increased. This suggests a process of ice crystal growth rather than one in which the water is forced away from the cold face. Similar observations have been made on stabilized soil frozen in the same way. There is also direct evidence of ice crystal growth in concrete in the appearance of sizable ice crystals beneath, and supporting, a scale of concrete forced off during freezing*, the volume of ice being much greater than that of any water that could have been driven from the scale by freezing. This phenomenon appears to be similar to the formation of the so-called "fibre ice" under the particles of soil lifted in the same way and reported by Rucki†.

As Mr. Powers suggests, further tests are clearly necessary to establish the behavior of water in concrete during freezing, but in making these tests we must be sure that a wide range of both laboratory and field conditions are covered. It might then be found that the mechanism of

*A. R. Collins, "The Destruction of Concrete by Frost," *Jour. Inst. C. E.*, 1944, 23(1), 29-41.
†R. Ruckli, "Die Gefährlichkeit des Strassenuntergrundes," *Strasse und Verkehr*, 1943, 29, (19), 311.

failure may vary according to the properties of the concrete and the freezing conditions.

AUTHOR'S CLOSURE

The written discussions submitted by Mrs. Terzaghi, Messrs. McHenry and Brewer and Mr. Collins, together with many oral discussions with Messrs. Pickett and Steinour of this laboratory have been very helpful in further developing, clarifying and modifying the author's hypothesis. This will be apparent in the following discussion of some of the written discussions of the paper.

From the information given by Mrs. Terzaghi and by Messrs. McHenry and Brewer concerning the fraction of cross sectional area contacted by the mobile water in saturated concrete, it appears that the author's computation of the hydraulic pressure required to produce stress equal to the strength of concrete gave a result that is too high. If the area of contact is 100 percent, the tensile stresses developed under the conditions assumed would be five times as high as those estimated by the author, as Mrs. Terzaghi has pointed out. This would seem to increase the force of the author's argument that disruptive hydraulic pressures are likely to develop through the displacement of unfrozen water.

Mrs. Terzaghi's computation indicated that among grades of concrete originally used, the critical depths of saturation range from 0.2 in. to 7 in. for cooling at a "natural" rate. This too might be taken as added support of the hypothesis but for the fact that the basis of the computations is incorrect. For her computations, Mrs. Terzaghi adopted the same simplifying assumptions used by the author for the purpose of illustrating the principles on which the hypothesis is based. Unfortunately, these assumptions are not permissible for a computation such as Mrs. Terzaghi undertook. The author assumed that all the freezable water would freeze at the same time. This would probably be not very far from the truth for small specimens rapidly cooled to a very low temperature.* Mrs. Terzaghi not only adopted this assumption but also the further assumption that all the freezable water would freeze "at or slightly below 32 F." This latter assumption is definitely not acceptable for such a computation, for when concrete is cooled comparatively slowly, there must be a considerable time-lag between the first and final freezing in a given region, if supercooling does not occur.

An example of melting-data for water in a saturated cement paste is:

*Nevertheless, had the significance of progressive freezing been realized at the time (see below), this assumption would not have been made, even though the author's aim was restricted to illustrating the general principle involved.

Temp. deg. C	Amount of Ice, g/g of cement	Amount of Ice as per cent of Amount at − 15 C
0	0	0
−0.25	.045	21
−0.75	.075	36
−1.00	.093	44
−1.50	.109	52
−2.00	.122	59
−2.50	.131	62
−3.0	.137	65
−4.0	.147	70
−6.0	.168	80
−8.0	.181	86
−15.0	.210	100

From this it must be concluded that the amount of water freezing at any one time and place is much less than the total freezable water as defined in the paper.

Further consideration of this phase of the hypothesis gives rise to the following speculation: Since the water may freeze progressively as the temperature falls, the ice content will have a gradient

$$\frac{di}{dz} = \frac{di}{d\theta} \cdot \frac{d\theta}{dz}$$

where $\frac{di}{dz}$ = the ice-content gradient

$\frac{di}{d\theta}$ = the change in ice content per degree drop in temperature

$\frac{d\theta}{dz}$ = the temperature gradient.

This would indicate that the ice that forms at low temperature would form in spaces already partially or wholly surrounded with ice formed earlier when the temperature was higher. The passages would either be much restricted or wholly blocked by the earlier-formed ice and thus the pressures generated by the formation of the ice at low temperature might be much higher than would be estimated from the normal permeability of the concrete. Also it might be of more nearly static nature than was first supposed.

For the sake of simplicity the author assumed that the saturated region of the concrete was fully saturated and that when this region froze, the displaced water flowed across a sharp line of demarcation into a region containing no freezable water. This was pointed out as an oversimplification (see pages 254 and 255) justifiable only for the purpose of providing a simple illustration of the principle on which the hypothesis is based. The oversimplification results in an underestimate of probable hydraulic pressure when applied to the problem attacked by Mrs. Terzaghi. To illustrate this point, we may imagine a case wherein, at saturation, the amount of water freezable at -15 C is n grams per cu. cm of concrete. We will assume that the specimen is saturated to the depth z, that the freezable water content of the rest of the specimen is $0.9n$, and that the mobile water content at saturation is equal to the freezable water content. When the saturated region is frozen to a depth of 1 cm, about $0.1n$ cu. cm of water will be forced into the region already containing $0.9n$ cu. cm of freezable water per cu. cm of concrete. Therefore, for each cu. cm of saturated concrete that is frozen, 1 cu. cm of the partially saturated region would become saturated. Thus, the depth of the saturated zone, under the conditions assumed would increase at the same rate as the rate of freezing.

This too is an oversimplification acceptable only for the purpose of illustration. Experiment shows that, except for very porous concrete, water does not penetrate dry concrete in such a way as to give a sharp demarcation between the saturated and unsaturated zones. Instead, the process resembles diffusion in that a moisture gradient develops that becomes flatter the farther the water moves into the dry interior. This fact and the probable effect of progressive freezing on permeability as discussed above would have to be taken into account to reach even an approximate solution of the problem attempted by Mrs. Terzaghi.

Mrs. Terzaghi feels that lean concrete is not likely to become saturated to a depth of 7 in. and because of this opinion she feels forced to the conclusion that frost damage to lean concrete is generally due to some process other than that postulated by the author. Such a statement indicates that the discussion of the possible role of dynamic pressure in the destruction of laboratory specimens tended to obscure the importance of static pressures in general. The hypothesis is based on the premise that the destruction of concrete is caused by hydraulic pressure generated by the expansion accompanying the freezing of water (see p. 246). This pressure may be either static or dynamic. As pointed out on page 257, failure because of static pressure will occur when escape from a saturated region is impossible, even though the depth of saturation is less than the critical depth.

To illustrate further: It might be surmised from a part of the hypothesis that a 6-in. retaining wall made of concrete having a critical depth of saturation of 7 in. would never be damaged by freezing and thawing. Such a conclusion should not be reached on the basis of the whole hypothesis, however. Failure might occur through unsound aggregate particles or it might occur as follows: Suppose the wall to have become saturated from ground water and rain and that it has become frozen by a gradual drop in temperature without appreciable damage to the wall. If the exposed side then thaws to a depth of, say, 1 in. with perhaps rain or melting snow to keep the surface saturated, a subsequent drop in temperature could cause damage by trapping unfrozen water between the ice formed near the exterior and the ice still in the interior. Under such a condition the failure would be caused by a static pressure.* To be considered also is the probable effect of progressive freezing on permeability, duscussed above. Probably the critical depth of saturation in lean concrete is much less than was indicated by the result of Mrs. Terzaghi's computation.

Regarding the probability of ice-segregation in concrete: As Mr. Collins observed, most of the discussion in the paper pertained to the probability of lens-formation under laboratory-test conditions. The author agrees that there must be some condition of cooling that could produce ice-segregation if the condition is maintained long enough, and if the concrete next to the lens is kept virtually saturated while the lens grows. The question is whether such a condition is ever found in the field, or at least whether it is found frequently enough to be considered the usual mode of concrete destruction by frost action. The author still sees little possibility that segregation can occur, even in the field, because of

(1) the relatively low freezable water content,
(2) the smallness of the portion of the total freezable water that is freezable near the normal freezing point,
(3) the "self-desiccation" caused by cement hydration,
(4) the relatively low permeability, and
(5) the rigidity of the concrete.

I must disagree with Mrs. Terzaghi's statement that there is nothing in Taber's observations to indicate that low permeability is an obstacle to ice-lens formation. In his paper "Freezing and Thawing of Soils as Factors in the Destruction of Road Pavements" *Public Roads* V. 11, 113 (1930)), Taber states: "In laboratory tests, soils that are very impermeable because of high colloid content behave essentially as closed systems, the amount of heaving being the same whether the containers

*This explanation was given by Kreüger (ibid.) for certain types of masonry failures in buildings.

are sealed at the bottom or whether they are perforated and stand in water. The surface uplift in both cases is equal to the change in volume of the water frozen. . . . . The only soils thus far tested that behave on freezing like closed systems, because of impermeability, are certain muck and gumbo soils, and soils containing bentonite." In the *Jl. of Geology*, V. 38, page 306, Taber says, "As the resistance to movement of ground water increases, an open system tends to grade into a closed system." In the laboratory Taber succeeded in producing ice segregation in mixtures containing bentonite, but in order to do this he found it necessary to "mix the bentonite thoroughly with water before placing it in cartons." The point is that when a system contains a large amount of readily freezable water, as did Taber's laboratory preparations containing bentonite, and probably the saturated clay shown in McHenry and Brewer's Fig. D, perhaps 30 to 40 per cent of the over-all volume, lenses could form under ordinary cooling conditions by extracting water from the material immediately adjacent to the points of lens formation. Also, in such specimens, most of the freezable water is freezable near the normal freezing point. Permeability becomes a deciding factor when the material close to the zone of freezing does not contain enough readily freezable water to produce a visible lens.

The part that permeability plays in the phenomenon of ice segregation can readily be illustrated by means of Fig. 1 of the paper. For the present purpose imagine the concrete represented in Fig. 1 to be saturated throughout and in communication on the right-hand side with a supply of water. Heat is being extracted through the layer of ice at the left. Under certain conditions of cooling the water in the concrete will not freeze; instead, the thickness of the ice coating will increase by extracting water from the concrete.

The conditions under which this phenomenon will occur are those in which heat is removed at a rate just sufficient to freeze the water as it is extracted from the concrete. If heat is removed at a greater rate, the temperature will fall and the zone of freezing will move into the concrete. Since the rate of flow of water from the concrete depends on permeability the permissible rate of heat removal for ice lens formation obviously depends on the permeability of the concrete.

It is true that segregation occurs only in granular materials of relatively high specific surface, as Mr. Terzaghi pointed out. But this fact is not incompatible with the conclusion reached above.

This discussion is intended to show the conditions that must be maintained while a given ice-lens grows. It does not deal with the conditions that must be met for an ice-lens to start within a rigid material like concrete. As Messrs. McHenry and Brewer point out, the tensile strength of the concrete must be overcome before a lens can start, unless it starts from water in a macroscopic cavity and even then it cannot grow beyond the cavity without rupturing the concrete.

Mrs. Terzaghi and Messrs. McHenry and Brewer express similar views concerning spaces between the aggregate and the paste. These spaces can undoubtedly be sources of disruptive hydraulic pressure, or perhaps of the growth of ice lenses, but only after the concrete has been subjected to prolonged external hydraulic pressure of considerable intensity. The condition of specimens after permeability tests, as mentioned by the discussers, shows that such spaces may become filled, at least partly. Also, this is indicated by pressure cells embedded near the upstream face and near the base of a high dam. But experience shows that in concrete of good quality, under conditions where external hydrostatic pressure is low or absent, the fissures under the aggregate become empty and the original air-filled cavities remain empty. Moreover, the capillary system of the paste itself becomes partly empty. When the primary cause of water-movement into such concrete is capillary absorption, water would move into such spaces last, if at all. Hence, fissures and other cavities in laboratory specimens, pavement slabs and other members not subjected to prolonged high pressure, should, according to the hypothesis, be beneficial to frost resistance. The known beneficial effect of entrained air is strong support to this conclusion. Also, we have data indicating that the under-aggregate fissures increase the frost resistance of concrete specimens soaked in the usual way before freezing. When powdered minerals are added to lean concrete or mortar, both the air-voids and the fissures are reduced. Such effects are accompanied by *decreased* frost resistance even though strength is usually increased.

Messrs. McHenry and Brewer note that no satisfactory explanation of D-cracking other than ice segregation has been offered. Such cracks, running parallel to each other and to the edge of a slab, can be explained by the hydraulic-pressure hypothesis.

In wet weather, water accumulates in transverse cracks and joints and in the shoulder next to the edge of the pavement. Usually water is retained in these regions long after the surface of the slab becomes dry. Consequently, the margins of the slab tend to become and remain saturated. When freezing of the saturated margins occurs, pressure is developed in them tending to expand and rupture these saturated margins. However, expansion in a direction parallel to the edge is restrained by the unsaturated portion of the slab and therefore cracks due to freezing never run perpendicular to an edge as shrinkage cracks do. The D-crack first formed readily carries water and thus aids in increasing the width of the saturated margin and thereby promotes the formation of additional parallel cracks when freezing again occurs.

A proper rate of cooling could produce surface scaling by means of ice-segregation, but it appears more probable that scaling occurs when saturated surface layers are literally pushed up by the hydraulic pressure.

The writer believes that leaching of lime and alkalies from the margins of a concrete slab is a factor hastening the process and possibly is essential to it. Leaching coarsens the texture of the paste and thereby increases its capacity for freezable water. Such an increase in freezable water increases the amount of water that must be forced into the unleached and only partially saturated region in a given length of time and would thus result in increased hydraulic pressure.

Regarding Fig. 2 on page 258: This laboratory has data, like those mentioned by Messrs. McHenry and Brewer, that apparently contradict the results given in Fig. 2. Yet we can find no basis for discarding the results shown in Fig. 2; they, as well as different results, must eventually be explained. Possibly the peculiar combination of size of specimen, amount of water around the specimens, rate of freezing and manner of thawing, and the treatment of the specimens before the cycles began would have to be duplicated exactly to get the same results. Such apparent discrepancies between the results from different laboratories, and indeed from one laboratory, show the need for establishing a test procedure that is based on a better understanding of the mechanism of frost action than we now have.

In the original paper the writer neglected to mention recent papers by H. L. Kennedy.* Some of the basic ideas on the mechanism of frost resistance given in my paper are similar to those published by Mr. Kennedy.

*H. L. Kennedy, ACI Journal, June 1944; V. 40, p. 515.
H. L. Kennedy, Proc. A.S.T.M. V. 44, 821 (1944).

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Discussion of Report of ACI Committee 208, Bond Stress:

Proposed Test Procedure to Determine Relative Bond Value of Reinforcing Bars*

By DUFF A. ABRAMS and H. J. GILKEY

By DUFF A. ABRAMS†

The proposed test procedure appears to have been promulgated contrary to the experience and better judgment of Chairman Gilkey, as indicated by this quotation from the published statement:

For the pullout specimen the steel is surrounded, during test, by concrete in compression whereas in a beam the surrounding concrete is in tension. While, on the basis of limited evidence secured from tests on plain bars the chairman questions the pertinency of this difference, it has significance in the minds of many, and for such persons evidence from pullout specimens cannot be convincing.

The writer's experience agrees with Chairman Gilkey's and suggests the following 8 specific objections to the proposed tests:

1) The test procedure is very complicated and would be so expensive as to create a monopoly for the laboratory with the apparatus, skill and experience necessary to make the tests.

2) The bond value will be determined by the properties of a handful of concrete in a location where it is almost impossible to secure representative placing.

3) The bond resistance of the main bar is complicated by the high bearing stress in the concrete over the supports.

4) It is impossible properly to evaluate the effects of the 3-in. overhang at the ends of the beam.

5) The use of these types of beams to simulate typical conditions of the bond of reinforcing bars is a misdirected effort. Such beams would not be permittted by any code.

6) The notches in the tension side of the proposed beams have no counterpart in reinforced concrete beams in service; they will greatly

*ACI Journal, Feb. 1945; *Proceedings* v. 41, p. 273.
†Consulting Engineer, New York City.

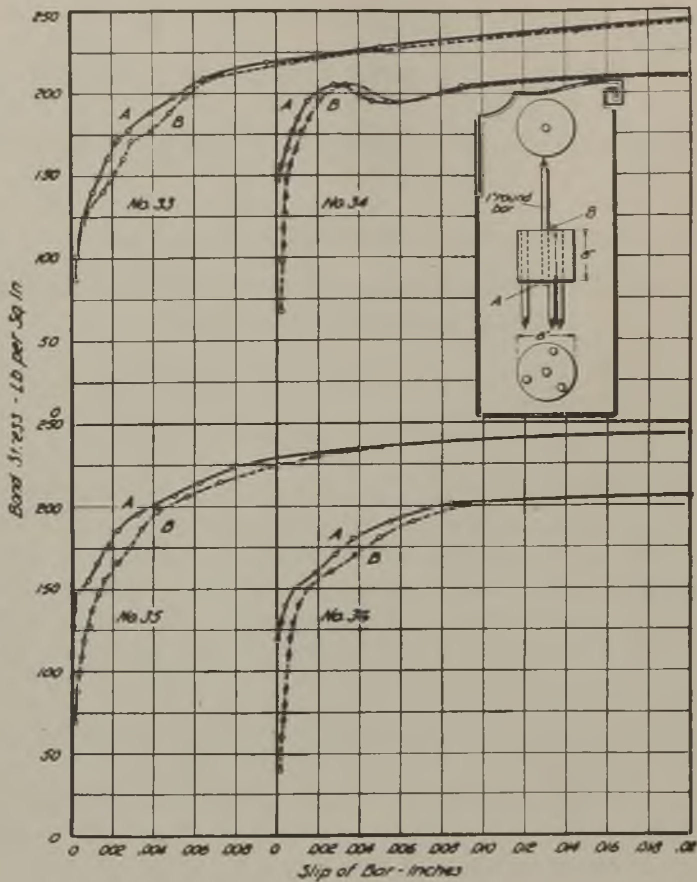

Fig. A—Load-slip curves for specimens of the form shown
(from Fig. 41, "Tests of Bond Between Concrete and Steel")

modify the normal behavior of the beams by causing vertical tension cracks to form at a comparatively low stress in the main reinforcing bar;

7) This will instantly transmit the concrete stress to the steel (as a suddenly-applied load) and cause bond failure before the required slip and stress measurements can be made.

8) The committee seems to be in much doubt as to what is needed, as shown by the 6 types of beams, which are certain to give contradictory "relative bond values" for a given bar.

The committee's statement concerning the concrete in the test beams being in tension, is, of course, correct; but this is not necessarily important and should not be decisive. However, if anyone thinks that bond tests

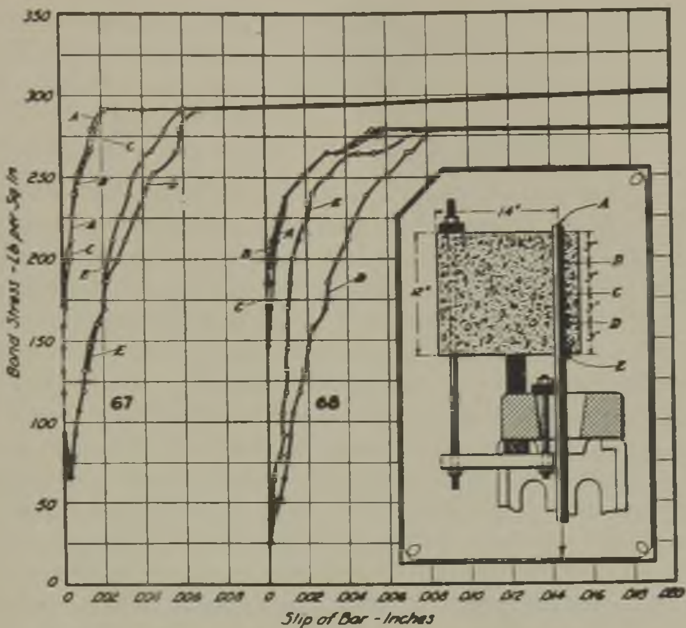

Fig. B— Load-slip curves for specimens of the form shown
(from Fig. 42, "Tests of Bond Between Concrete and Steel")

made with the concrete in tension give a better "relative bond value," a simple pull-out test can easily be designed for this purpose. This is not a speculative, untried test, nor is the idea particularly new. On the contrary, a number of different specimens of this kind were designed and used by the writer 35 years ago. Two types are shown (with representative load-slip curves) in A and B reproducing Fig. 41 and 42, from "Tests of Bond between Concrete and Steel," Bulletin 71, University of Illinois Engineering Experiment Station, 1913.

The actual stress condition near the ends of the proposed beams is much like that of the pull-out specimen in Fig. 42. In studying the load-slip curves in Fig. 41 and 42, it should be noted that this concrete had about ½ the strength of similar mixes made of modern cements, and that the plain round bars had a more rough and irregular surface than those produced in modern bar mills.

The purpose of the committee was "to determine relative bond value," but they adopted a needlessly round-about, complex and expensive test method. It is the writer's belief also that:

a) The anchorage of bars in the capitals of columns and in walls and girders gives conditions that are more nearly reproduced in the pull-out test than in the beams adopted by the committee.

b) A properly-designed pull-out test will give "relative bond values" at a cost 3 to 5 per cent of the cost of the beam tests.

c) Attempts to use the beam tests will prove disappointing.

CLOSURE By H. J. GILKEY, Chairman Com. 208

There is, perhaps, no person living more competent than Mr. Abrams to have supplied a truly searching analysis and constructive criticism by virtue of which this proposed test procedure might well have been beneficially altered; moreover the Abrams' discussion probably constitutes a fairly accurate portrayal of what the Committee should expect to be passing through the minds of many qualified critics upon a hasty first reading of the proposed test procedure. It falls short, however, of what the Committee believes the concrete public is entitled to receive from any qualified critic after he has prepared himself by studying the text and the drawings in conjunction with a careful reading of the Foreword for clarifying background and it is with distinct regret that the Committee finds itself confronted with a series of opinions, most of which are, in the light of available evidence, susceptible neither of proof nor of positive refutation. It is, with corresponding reluctance that the Committee feels forced to discuss reiteratively, questions, explanations for most of which have already been covered in the Foreword, the text and the drawings.

Mr. Abrams' opening implication of disunity within the committee is self-answered in the quotation. The purpose for which this proposed test procedure was evolved necessitates not only that we get the facts but also that those facts be convincing and accepted as facts by those whose acceptance spells use. If the technical expert for a bar manufacturer doesn't have confidence that a pullout test supplies a correct index to beam-bar bond resistance, data from pullout tests won't weigh heavily with him regardless of how thoroughly sold Mr. Abrams, the Chairman of this Committee, or any of the individual members thereof, might be on the adequacy of the pullout test as a criterion for excellence of bond performance.

Even if the item of whether the tensile bar is surrounded by compressive or tensile concrete is irrelevant (as both Mr. Abrams and the Chairman believe it to be) there are other differences between the situation existing in beams and that in pullout specimens that are not yet so easily waved aside on the basis of cleancut indisputable evidence. Settlement and water gain beneath the bar of a horizontally cast specimen, and whether the loaded end of a deformed bar projects upward or downward in a pullout specimen as cast are but two of several important differences of which, until recently, little cognizance has been taken but which certainly cannot be thoughtlessly ignored. Abrams quotes 2 (a) (p. 275) but disregards the introductory paragraph, (b), (c) and the closing paragraphs. There was within the committee no difference of opinion with regard to pullout vs. beam-type as the primary specimen to be recommended.

Going now to the numbered items of the discussion:

1) Discussed under 3, p. 276: As pointed out on p. 273 and 274 this test procedure is not intended to be a simple acceptance test affair for use in any laboratory, possibly against a purchase specification. It is for securing basic data expected to influence manufacturing methods and processes. Compared with the importance of the information sought, the possible reduction in cost of one testing technique over another is a negligible item. Again, read No. 3, p. 276.

2) Correct in part. As in any other type of bond test, the case rests largely on the results secured from "a handful of concrete" adjacent to the bar. Good placement is one of the prerequisites of good concrete. Partially through the important contributions of Mr. Abrams himself there is no longer any justification for the harsh unbalanced mixtures of the 1910-1920 decade either on the job or in the laboratory. The committee fails to share Mr. Abrams' concern over the ability of a qualified laboratory staff to secure adequate placement for *every* "handful of concrete" in *any* portion of the beam.

3, 4) The concern over the compressive stress in the concrete around the bar appears to be a bit at variance with the introductory paragraph. Mr. Abrams may have failed to note the use of a rubber sleeve to provide clearance throughout the overhanging three inches (Drawing 4).

5) The extent to which this is or is not "a misdirected effort" is, up to the present time, a matter of personal vs. committee opinion. These are not intended to be typical beams; they are beam-type specimens designed to be stressed disproportionately high in bond in order that *bond* resistance can be measured. The Committee does not recall having seen pullout specimens "permitted by any code."

6) The extent to which notches in the concrete on the tensile face beyond the steel might induce significant localized concentration of stress is speculative. Certainly much research work has been conducted on beams cut away (usually as cast) to expose the tensile steel, accepting the established design point of view that the outside concrete functions only as "fire proofing" or "protection" for the steel. In his Bulletin 71 (University of Illinois) p. 125 Abrams employed partially cut-a-way beams which embodied discontinuities not basically different from those to which he here takes exception. He states: "Openings about 1 in. in diameter were cut or formed in the concrete under the reinforcing bar at

points where measurements of slip were desired." We know that concrete develops tensile cracks at stresses as low as 4000 psi in the steel and, as a "stress raiser," the crack itself would appear to be the pertinent item. Such cracks are present in the tensile region of any concrete beam after appreciable stress has once been applied. The Committee is inclined to brand Mr. Abrams' contention as unsupported assertion.

7) Discussion of No. 6 largely applicable. The Committee again fails to see how the short blocks of concrete (between cracks) below the bar can function either as significant loading or cushioning devices. The use of the term, "(as a suddenly-applied load)," appears to be wholly out of place and not in accord with the Structural or Mechanics-of-Materials concept and definition of "Suddenly Applied Load." The Committee reiterates that, for lack of evidence, any consideration of notch effect is necessarily speculative but, on the basis of its consideration of the notches, the Committee, unlike Mr. Abrams, does not view them with alarm.

8) To state that "The Committee seems to be much in doubt as to what is needed, as shown by the 6 types of beams, which are certain to give contradictory relative bond values, for a given bar," indicates either a lack of understanding or a disregard of the purposes of these proposed tests and the reasons back of them as outlined at some length in the Foreword. As previously discussed under No. 1 the Committee is not seeking or expecting to secure open-and-shut, yes-no answers that brand one bar as indisputably the best with the others arranged 2, 3, 4 in order of excellence. While dependable, clear-cut indications would be welcome, the Committee's sights are set only for securing basic information on the bond behavior of a few carefully selected types of deformed bars, realizing that even the best of the bars tested are likely to show some pro and con aspects. Decisions as to which bar or bars may finally be selected, and which eliminated, will involve a study of the strong points and the weak points of each of the several bars for which the data secured are, all things considered, most favorable. After the laboratory findings are available the manufacturers may be expected to weigh relative test excellence against such aspects as practicality and cost of manufacture. The number of beams deemed essential hinged upon the following considerations:

(a) *Embedded length.* If the Committee had selected a pullout test technique it is probable that it would have specified more than one length of embedment. As mentioned in the last paragraph of p. 275, the test selected does closely resemble a pullout test in certain essential respects. After careful consideration the Committee selected three lengths of embedment of the bonded portion as a desirable minimum and as the workable maximum number of cases, the essential differences between types A, B and C being the embedded lengths of 8, 12 and 16 in. respectively as shown on Drawing 1.

(b) *Effect of settlement beneath the bar.* Probably no single aspect of bond is of greater practical importance than the effects of water gain and the settlement of the concrete from beneath any bar held in position (as bars usually are) during the plastic phase of concrete placement. This vital aspect, pointed out by Menzel only recently, failed to receive the attention it merits, at the hands of any of the early investigators (including Abrams). Surely its importance, however, cannot be minimized by Abrams or any other qualified concrete practitioner or critic. It follows, as pointed out in 7 at the foot of page 276 that no comparative investigation of bond performance of deformed bars could be considered complete if it failed to secure evidence on relative effectiveness of lugs with regard to settlement and water-gain effects. Beams AA, BB and CC of Drawing 1 (cast inverted) are introduced to cover that aspect.

(c) *Duplicate specimens.* Three duplicates to be cast for each case.

In accordance with the foregoing explanations the project calls for 18 beam-type specimens for each type of bar investigated: 3 lengths of embedment in the pullout region, 2 positions of casting—erect and inverted, and 3 duplicate specimens. Considering the basic nature of the investigation the Committee can only brand the charge of excessive numbers of specimens as being superficial. The Sub-Committee considered a much larger test program and finally proposed this as its concept of the minimum number of tests needed to give adequate information.

The Committee, and especially the Chairman, welcomes Mr. Abrams' reiteration of his 1913 observations with regard to tensile vs. compressive concrete surrounding the bar. Even here, however, Withey in Bul. 321, University of Wisconsin, "Tests on Bond Between Concrete and Steel in Reinforced Concrete Beams" (1909), makes contrary claims on the basis of tests similar to those of Abrams. From the tests by himself and colleagues, the Chairman is convinced that the Abrams finding is correct but the fact remains that even in this minor regard there is a conflict of recorded evidence, from two recognized authorities, which in itself justifies doubt in the minds of some. As pointed out earlier, this aspect was not the only, or major one, in selecting the beam-type as preferable to pullout specimens.

In his closing sentence Mr. Abrams states: "Attempts to use the beam tests will prove disappointing." Possibly so; certainly no member of the Committee expects the project to be carried out without the discovery of some "bugs." Whether disappointing or not the proposed test procedure represents the result of a lot of thorough, careful study on the part of a sub-group (with the approval of the main group) the individual members of whom have, on the basis of past performance, demonstrated considerable competence in the field of concrete design, research, and analysis. The Chairman knows of no project in the entire history of concrete research which was planned with a like degree of meticulous attention not only to the broad over-all objective but also to the minute details of execution.—On behalf and with the approval of the Committee, H. J. Gilkey, Chairman.

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Discussion of a paper by Watstein and Seese:

Effect of Type of Bar on Width of Cracks in Reinforced Concrete Subjected to Tension*

By J. MERCADANTE, PAUL W. ABELES and AUTHOR

By J. MERCADANTE†

The text and data in the paper admirably describe the phenomenon of cracking in concrete under tension and clearly demonstrate the advantage of high bond strength in minimizing width of cracks.

An indication of the relative efficiencies of the various types of bars considered in the paper may be obtained by the accompanying Fig. A in which the unit stresses observed in specimens to produce a given width of crack are plotted against the bearing areas of the lugs per linear inch of bar. The unit stresses are taken from the third column of Table 2 of the paper and the bearing areas from the fifth column of Table 1.

If the lines are drawn radially from the point representing the H-bar, (7/8-in. plain round) to the points representing the remaining bars, the inclinations of these lines from the vertical are a measure of the efficiencies of the patterns of the bars. This test indicates that the D-bar has the highest efficiency of the bars tested, which may be attributed to its unique twisted shape.

The possible advantages of high bond strength are now receiving intense consideration, as evidenced by new types of bar, questionnaires and investigating committees.‡

Unfortunately, at the time the tests described in the paper were made, the improved D-bar, which had been developed, was not available to the authors. The improved D-bar has transverse lugs of closer spacing and greater bearing area. Its properties, together with those of the D- and A-bars considered in the paper, are tabulated:

*ACI JOURNAL, Feb. 1945; *Proceedings* V. 41, p. 273.
†President American Insteg. Steel Corp., New York.
‡"Proposed Test Procedure to Determine Relative Bond Value of Reinforcing Bars"—Report of ACI Committee 208, Bond stress.

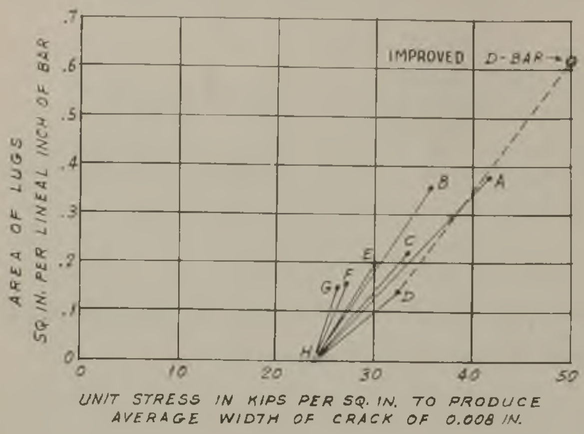

Fig. A

	⅞-in. round A-bar	No. 8 D-bar	Improved No. 8 D-bar
Cross-sectional area, sq. in.	.601	.667	.667
Bearing area of lugs, sq. in. per lineal inch of bar	.38	.14	.62
Height of lug, in.	.056	.030	.069

The improved No. 8 D-bar has not yet been tested. However, an idea of its probable performance may be obtained from the accompanying figure by projecting the point representing the D-bar of the paper along a line parallel to the average efficiency of the bars tested. This procedure indicates a possible increase in permissable unit stress over that permitted for the A-bar in the ratio of 50,000 to 42,000 without increasing the average width of crack.

It is expected that tests to be conducted in the future under the guidance of ACI committees, will also include improved D-bars.

By PAUL W. ABELES*

The authors have clearly demonstrated the gradual increase of bond of various reinforcements of old and improved pattern, dependent on the bearing area of lugs, in comparison with plain bars. From Fig. 5 and 6 it is seen that the total width of cracks per foot length, independent of width and spacing, approaches or even equals the total extension of the steel, e.g. for a stress of 40,000 psi: $\frac{40,000}{30 \times 10^6} \times 12 = 0.016$ in. This steel extension can be plotted into Fig. 6 by a straight line (one point of which

*Consulting Engineer, London, England.

is the experimental value of F) nearly parallel to, and close above, the graph. This means that the bond is almost totally destroyed at that stage (in F even totally).

However, at an earlier stage, the total width of cracks per unit of length is considerably reduced when deformed bars are used, since the bond is destroyed only in the immediate neighborhood of the cracks. This can be seen from the tests conducted by Dr. K. Hajnal-Konyi*). It might be interesting to mention that he was able to carry out pulling tests in which the maximum stress of the reinforcing bar in a cracked section was in excess of the ultimate strength of the steel, thus clearly demonstrating this phenomenon, which had already come to light at bending tests on beams, slightly reinforced with high strength steel. As the writer pointed out†, this phenomenon can be explained by a comparison with test results on steel bars, having notches or holes. It has been proved that in this case a stress concentration is not dangerous in a ductile material, if the reduction of the cross section extends to a very short length, reversal straining being avoided. This means that fine cracks do not cause a reduction of the concrete tensile resistance, as long as the bond is destroyed only in the immediate neighborhood of the cracks.

The writer would like to stress the authors' statement in the introduction that "cracking of reinforced concrete" . . . "is recognized as an unavoidable evil," and that wide cracks can be avoided "with more economical use of steel, by securing better bond between concrete and steel." There are three ways of increasing the bond efficiency of high strength steel:

(1) by the reduction of the diameter of the individual bar,

(2) by the provision of a special pattern at the surface (e.g. deformed bar), and

(3) by an improvement of the property of the concrete (e.g. by centrifugal molding).

The use of high strength wire is made possible by prestressing, as discussed by the writer‡. A special feature of prestressed concrete is its great resilience; that is to say, cracks occurring at a loading up to about ¾ of the ultimate load, close almost totally when the load is removed, even after repeated application of the load. A similar behavior is, within the writer's knowledge, only attained with tubular spun concrete products, having a closed transverse reinforcement which is prestressed by the centrifugal molding process.

*See Paper and Discussion on "Tests on Square Twisted Steel Bars and their Application as Reinforcement of Concrete," *The Structural Engineer*, Sept. 1943, Feb. and March 1944.

†*The Structural Engineer*, Feb. 1944, based on a lecture in Vienna in 1936.

‡"Fully and Partly Prestressed Reinforced Concrete," ACI JOURNAL, Jan. 1945.

In contrast, in conventional structures after cracking, there still remain cracks of measurable width, when the load is removed. It is interesting to note from the authors' Fig. 7 and 8 that after repeated loading the width of permanent cracks does not greatly differ whether plain or deformed bars are used, although originally at a small number of loading cycles as well as with respect to the greatest cracks, even after a great number of cycles, there is an appreciable reduction of the width in favor of the deformed bar. It can therefore be concluded that a resilience, as described above, can only be attained, if at least a part of the reinforcement is prestressed.

AUTHORS' CLOSURE

Mr. J. Mercadante's method of rating the bonding efficiencies of the deformed bars, as illustrated in his Fig. A, appears to be based on the tacit assumption that the stress in a reinforcing bar, for a given width of crack, varies linearly with the bearing area of lugs per unit of length of the bar regardless of the pattern of lugs and the shape of the bar. It would be interesting to see some experimental data which support this theory.

Mr. Mercadante's prediction of the performance of the improved bar D is apparently based on extrapolation. Extrapolation is at best somewhat speculative, and in this case leads to a conclusion which is largely of academic value.

Mr. Paul W. Abeles correctly pointed out that the total width of cracks per foot of length of the specimens approaches the extension of the reinforcing bar of the same length. However, Mr. Ables in his calculations used a value of E_s of 30 x 10^6 psi and arrived at the erroneous conclusion that in some cases the width of cracks per unit of length was equal to the total extension of steel of that length. When the true values of the moduli of elasticity (Table 1) were used in the calculations, it was found that in all cases the total width of cracks was substantially smaller than the corresponding extension of the steel bars.

Mr. Abeles' interesting discussion of prestressed concrete brought up the question of the effect of quality of concrete on control of cracking of reinforced concrete. In an earlier study of cracking of reinforced concrete subjected to tension.* concrete of two different strengths was used, but no significant difference between the width of cracks for a given stress and a given type of bar was observed in specimens of "weak" and "strong" concrete.

*Width and Spacing of Tensile Cracks in Axially Reinforced Concrete Cylinders, *Journal of Research* of National Bureau of Standards, V. 31, July 1943.

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Discussion of a paper by Clarence Rawhouser:

Cracking and Temperature Control of Mass Concrete*

By. DUFF A. ABRAMS and AUTHOR

By DUFF A. ABRAMS†

The US Bureau of Reclamation has considered the control of cracking so important that it has dictated nearly all other features of the design and construction of large concrete dams. This problem has been before that organization for many years; scores of papers and reports have been issued on its various phases since 1930.

In accordance with the best estimate of the writer, the USBR has, during the past 12 years used its embedded-pipe system of cooling on eight large dams with a total volume of about 27,000,000 cu. yd. of concrete, and has expended on various phases of temperature control about the following:

Studies of special cements, thermal properties and cooling of concrete	$ 500,000
Extra cost of special cements, 27,000,000 bbl. at 10¢	2,700,000
Cooling 27,000,000 cu. yd. at 23¢	6,200,000
Instruments embedded in concrete	250,000
Reading instruments, calculations and reports	200,000
Unclassified	150,000
Total	$10,000,000

The foregoing are the direct costs of crack and temperature control, by the use of special cements and cooling of concrete. If we include the cost of vertical contraction joints and grouting, the total would be about $20,000,000.

SUMMARY AND CONCLUSIONS

The paper is the end product of 15 years of continuous attention to "cracking and temperature control of mass concrete," by the US Bureau of Reclamation. About 39 of the 42 pages were devoted to "temperature

*ACI Journal, February 1945; *Proceedings* V. 41, p. 305.
†Consulting Engineer, New York City.

control," but we search these pages in vain for any evidence that temperatures in large concrete dams *either have been or can be controlled.*

In the writer's opinion, the method of control recommended by the author contains a number of fallacies that make it wholly ineffective. The principal fallacy is the assumption that portland cement ceases to hydrate when artificial cooling is discontinued. The operation of these fallacies will be demonstrated by specific reference to the results obtained in cooling of Boulder Dam, where the low-heat-cement-embedded-pipe system, recommended by the paper was used.

Data and arguments presented below will show:

1) That the author withheld the only type of information of importance, namely, existing temperatures in large concrete dams,

2) That every important assumption on which the USBR based its theory and practice of cooling Boulder Dam was erroneous,

3) That the isotherms published since completion of Boulder Dam, as representing interior temperatures, are misleading,

4) That the low-heat-cement-embedded-pipe system of temperature control was ineffective at Boulder Dam, and accomplished neither what it was designed for nor what has since been claimed,

5) That hydration of cement in Boulder Dam did not stop when the refrigerator was turned off. Cooling of the concrete was only an unimportant interlude in the hydration of this cement. About 50 per cent of the total heat of hydration is still in the dam,

6) That Boulder Dam was not cooled to its "final stable temperature." The writer estimates the temperature in the lower part of the dam to be about 64 F. higher than that assumed in design,

7) At points indicated in official Boulder Dam charts by the 56 F. isotherm, the temperature is now of the order of 120 F.,

8) Present temperature is probably higher than the average maximum reached by the concrete. All benefits of cooling Boulder Dam have been lost by the end of 12 years,

9) Due to unconsidered temperature rise, the computed concrete stress in Boulder Dam is 2800 psi. above that considered in design,

10) The writer's estimate of temperature rise in Boulder Dam is confirmed by observations of men who were on the job during construction or shortly after completion,

11) That "low-heat" portland cement is a misnomer,

12) That a large, rapidly-built dam, using 1 bbl. per cu. yd. of low-heat cement, cannot be adequately cooled by a system applied for "a month or two during the period of construction,"

13) That grouting of Boulder Dam was a mistake,

14) Experience at Boulder Dam contradicts the principal recommendations of the paper.

The paper does not offer a solution to the problems of cooling a large, rapidly-built concrete dam. These problems cannot be solved by ignoring them. Over 400 thermometers were embedded in Boulder Dam; the author is requested to give the temperatures observed in the section in Fig. A, at 2, 3, 6, and 12 years after concreting began.

TEMPERATURE CONTROL IN CONCRETE DAMS

After setting forth the merits of special portland cements and the mechanics of the embedded-pipe system of cooling and grouting of concrete dams, the author sums up as follows (p. 322):

The concrete may be *cooled to the final stable temperature in a month or two during the period of construction* after which, with the concrete at minimum volume and the contraction joints opened to the fullest extent, *the joints may be grouted*, thus obtaining the monolithic structure assumed in design.

Certain words are italicized to direct attention to them.

The writer shows that, as a consequence of the properties of portland cements, and by specific reference to Boulder Dam:

a) That under these conditions the concrete cannot be "cooled to the final stable temperature in a month or two during the period of construction,"

b) That the embedded-pipe system of cooling used at Boulder Dam did not accomplish what was claimed for it, although, in some instances, cooling continued for 6 months after concreting,

c) That, although grouting was delayed for more than a year after the concrete was placed in the lower part of the dam, the grouting of Boulder Dam was a mistake.

OFFICIAL INFORMATION ON BOULDER DAM COOLING MISLEADING

The foregoing quotation from the paper is a statement in somewhat different words of what has been claimed for the low-heat cement and the same system of cooling as used on Boulder Dam. A number of reports by USBR writers which give the engineering profession "official" information concerning temperatures in Boulder Dam will be cited. These reports were published 1 to 6 years after the dam was completed.

1936. A year after the completion of the dam, J. L. Savage, Chief Designing Engineer, published a book, "Special Cements for Mass Concrete." In discussing the cooling of Boulder Dam, Mr. Savage gave a chart, bearing the title, "Section of Boulder Dam Showing Isotherms and Location of Embedded Instruments." This chart (Fig. 54) is reproduced here as Fig. A.

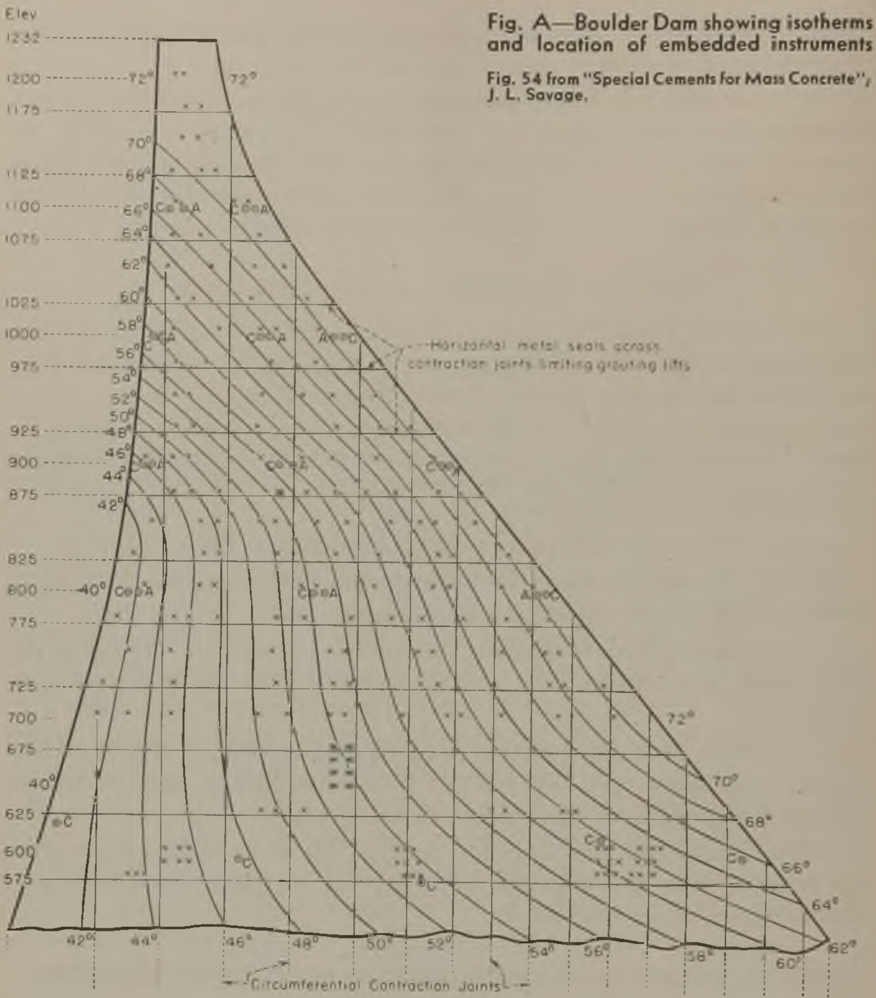


Fig. A—Boulder Dam showing isotherms and location of embedded instruments

Fig. 54 from "Special Cements for Mass Concrete", J. L. Savage.

x Location of thermometers with respect to faces of dam

⊚ Location of sets of 10 strainmeters, A at abutments, C in center of dam

Isotherms indicate temperatures to which concrete was cooled.

1938. In his paper "Temperature Control of Mass Concrete in Large Dams," in "Dams and Control Works," a book published by USBR 3 years after the completion of Boulder Dam, Mr. Rawhouser gave a chart which is essentially the same as that by Mr. Savage.

1941. A book "Boulder Dam" (Boulder Dam Project Final Reports, Bulletin IV-2) p. 190, published 6 years after the completion of the dam used the same chart as Savage and Rawhouser.

In addition to the isothermal charts mentioned above, many USBR reports published since the completion of Boulder Dam, contain direct statements of the results of the cooling operations. The following are typical:

1936. J. L. Savage in "Special Cements for Mass Concrete", (Cooling of Boulder Dam) p. 146:

The great proportions and rapid rate of construction made it imperative that some means be taken to remove from the concrete the heat developed during cement hydration, and that existing initially in the concrete above the *final temperatures* desired in the dam.

1940. "Thermal Properties of Concrete", (Boulder Dam Project Final Reports, Bulletin VII-1) p. 9:

In the construction of an arched gravity dam it is important that *cooling and complete contraction of the concrete*, followed by grouting of the contraction joints between the columnar blocks, be accomplished during the construction period.

1941. "Boulder Dam", (Boulder Dam Project Final Reports, Bulletin IV-2) p. 33:

Therefore, the problem became one of devising a construction program which would result in the attainment of *final stable temperatures*, and consequently final stable volume, during the regular period of construction. A program of artificial cooling of mass concrete together with the use of low-heat cement was adopted as the most feasible method of solving the problem.

From the above charts and frequently-reiterated statements of the benefits of low-heat cement, cooling, and that "final stable temperatures" had been reached, the reader might readily gain the impression that such material represented actual temperatures as determined by the 416 thermometers that were embedded in the dam. But that would be a grave mistake; the charts gave nothing of the kind. The "isotherms" are purely theoretical curves—made up in Denver several years before concreting on the dam was begun. Any of the above 4 reports might have given actual concrete temperatures that would have been of utmost interest and value to engineers, but all gave, instead, the original theoretical isotherms, or merely repeated the aims of the designers of the dam.

It will be shown that the cooling system used at Boulder Dam did not produce "final stable temperatures", and that the temperatures after completion of the dam were vastly different from those indicated by "official" statements.

HYDRATION OF LOW-HEAT CEMENTS

As a background for our consideration of temperature control, it is necessary to present a brief discussion of the hydration of portland cements. The discussion will be confined largely to low-heat cements;

this type was recommended by the author for use in conjunction with artificial cooling.

The only value portland cement has as a structural material is its property of "hydration", that is, the formation by chemical reactions with water of new and stable compounds. There are a number of different manifestations of cement hydration, all of which have significance in mass concrete:

a) Strength of concrete,
b) Temperature rise in concrete,
c) Heat of hydration of cement,
d) Quantity of water fixed by hydration.

It has been known for nearly 100 years that the strength of hydraulic cement increases with age. The writer was probably the first to demonstrate that, so long as concrete remains in a damp condition and at a favorable temperature, the compressive strength continues to increase essentially as a direct function of the logarithm of the age. Many series of tests are now available that show this increase to continue indefinitely. In one series tests have been made up to 30 years. There is no reason to suppose this increase will not continue indefinitely according to the logarithmic law.

Tests of temperature rise have shown results exactly parallel with those of compressive strength. If curves in Fig. 1 of the paper were plotted to a logarithmic scale of ages, they would give essentially straight lines at ages of 3 to 28 days. Similar tests of low-heat cements reported in other USBR publications show this logarithmic relationship to hold up to one year. Some of these tests are referred to below.

Similar results have been found for heat in hydration; in fact temperature rise of concrete, in the absence of loss or gain of heat from outside sources, furnishes a direct means of computing heat of hydration.

A paper on "Water Retained by Hardened Cement Pastes", by Raymond Wilson and F. A. Martin (ACI JOURNAL Jan.-Feb., 1935 Proceedings V. 31, p. 272) showed that at 3 days to 1 year the water retained by hardened pastes, increased as a logarithmic function of age.

Of the above measures of the hydration of portland cements the compressive strength has the longest history—30 years. Since all of the factors listed above are different manifestations of the same reactions, it is a safe assumption that all of them will show a continuous increase without known time limit, and that this increase will closely follow the logarithmic law.

Fig. B gives 4 curves, all based on USBR tests:

Curve A. Heat of hydration of Boulder Dam low-heat cement; tests reported in "Special Cements for Mass Concrete", by J. L. Savage, 1936, p. 152.

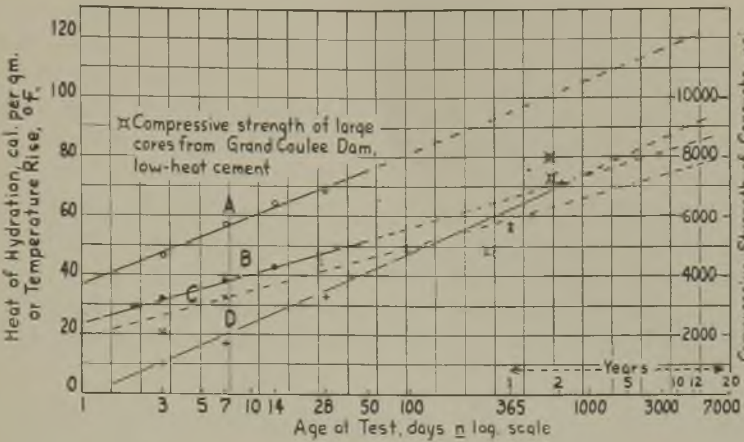


Fig. B—Effect of age on the properties of low-heat portland cements.

Curve A. Heat of hydration, Boulder Dam cement.
Curve B. Temperature rise, low-heat cement in paper.
Curve C. Compressive strength of concrete, 10 standard cements.
Curve D. Compressive strength of concrete, 13 low-heat cements.

Tests by US Bureau of Reclamation. See text for complete references.

Curve B. Temperature rise of low-heat cement, from Fig. 1 of the paper.

Curve C. Compressive strength of concrete; average of 10 *standard* portland cements; from USBR "Concrete Manual", 1942, p. 451; cured in fog room at 70F.

Curve D. Compressive strength of concrete; average of 13 *low-heat* cements; parallel to tests in Curve C. The concrete strengths at ages of 3 days to 1 year age given below—also parallel strengths of *modified* cement.

All of the curves in Fig. B are represented by expressions of the form:

$$v = a + b \log n$$

where v is the vertical ordinate, a and b are constants; n is age at test in days. In the figure a is the 1-day ordinate, and b is the slope of the curve (to log. scale). b is equivalent to the ordinate at 10 days minus that at 1 day. This makes the derivation of the constants quite simple; for example Curve B, temperature rise of low-heat cements, has the expression: $t = 24 + 16 \log n$. The indicated value of 1 day is a little higher than that determined by test, but the expression gives almost exact values for later ages.

When a variable increases as a logarithmic function of age, it means that the rate of increase becomes smaller and smaller. For concrete strength, the increase between 3 and 12 years would be the same as that between 3 and 12 months, or 3 and 12 days. Similar rates apply to heat of hydration and temperature rise.

The near-parallelism of the curves for heat of hydration, temperature rise, and concrete strength is accidental and results from the particular vertical units and scales used, but it emphasizes the similarity of three different measures of hydration. Concrete strength may be used as a measure of the heat of hydration. Curves A and D show that for this cement and mix, after the first day, each increment of 1000 psi. was the result of the evolution of 10 cal. per gm. of heat. Further study would, no doubt, establish a general low of this type for other cements and mixes.

The important feature of these curves for purposes of this discussion is that all of these properties of the low-heat cements continue to increase as a logarithmic function of age, within the limits of the tests. We are justified in the belief that these properties will continue to increase with age, as indicated by the curves projected beyond the test limits.

Data in USBR "Concrete Manual", 1942, p. 451, may be summarized, using strengths scaled from curves:

Type of Portland Cement	Number of Samples	Compressive Strength of Concrete					Ratio 1 yr. to 28 d.
		3 d.	7 d.	28 d.	90 d.	1 yr.	
Standard.	10	2100	3300	4500	5000	5700	1.27
Modified.	10	1700	2500	4100	5100	5500	1.34
Low-Heat.	13	1000	1700	3300	4900	5800	1.76

Water ratio 0.54 to 0.56, by weight; cured at 70F. in fog room.

The tests show:

a) The low-heat cements had low early strength, but at the end of 1 year gave concrete strength higher than either the standard or modified,

b) From 28 days to 1 year, the rate of increase in strength was much higher for the low-heat than for the other types of portland cement.

There is no reason to believe the low-heat cement would not continue to gain in strength at the higher rate up to 12 years, as shown by Curve D. This, of course, is only another way of saying that heat of hydration will also develop at a higher rate after the first year.

The first concrete placed in Boulder Dam is 12 years old; since there is no known method of stopping the hydration we are justified in concluding that the total heat of hydration of this cement up to date (Curve A) is about 120 cal. per gm., and that the low-heat cements in Curve D, would at 12 years, give a compressive strength of about 9000 psi. The points plotted for compressive strength of cores cut from Grand Coulee Dam (low-heat) at ages of 9 to 21 months, give a measure of the concrete strengths to be expected at later ages.

When measured in terms of 5 to 12 years, the low-heat portland cements probably have a higher heat of hydration than the standard portlands in use at the time Boulder Dam was under consideration. These low-heat cements should have been designated "low-early strength."

The most casual and elementary consideration of hydration of portland cement demonstrates that the hydration of the cement in Boulder Dam did not cease when the refrigerator was turned off after cooling the concrete "a month or two".

The most certain method of restricting temperature rise in a large concrete dam is to reduce the heat-producing element to a minimum. It is working at cross-purposes to put in heat-producing material that is obviously not needed, then go to great expense in removing a portion of the unwanted heat. When we buy portland cement, we are in reality paying for Btu., and no more should be bought than can be used to advantage.

The above considerations raise anew the whole question of special cements for mass concrete. The USBR solved this problem in the construction of Elephant Butte Dam, where a sand-cement was used, consisting of equal parts of portland cement and pulverized sandstone, interground to exceed the fineness of cement. This dam is 306 ft. high; when built it was one of the highest masonry dams in the world. The method saved about ½ the normal cost of cement, and required no expenditures for cooling or grouting. This dam is 30 years old; it is probably as free from cracks today as any large concrete dam in existence.

ESTIMATED TEMPERATURES IN BOULDER DAM

Concreting on Boulder Dam was begun June 1933; by the end of 1934 the bulk of mass concrete was in place. The first concrete is now 12 years old. Scraps of information scattered through many USBR reports published during the past 11 years enable us to piece together a balance for the heat in this dam:

$$H = A + B + D,$$

where H is total heat of hydration of the cement,

A is heat lost during construction,

B is heat of hydration removed by artificial cooling,

C is heat used in depressing temperature of concrete below that at which it was placed,

D is heat still in the dam.

Numerical values of the symbols will be developed. It will become apparent below why C did not appear in this balance.

H. Total heat of hydration.

Fig. B shows that at 12 years Boulder Dam cement would be expected to have a heat of hydration of 120 cal. per gm. This permits an estimate of the total heat generated by hydration. 1 lb. is 454 gm.; 1 cal. is equivalent to 0.004 Btu. 1 lb. of the cement develops 454 x 120 x 0.004 = 218 Btu. This heat would raise the temperature of 26 gal. of water 1 F.

Each cubic yard of concrete contained 384 lb. of cement, and weighed 4220 lb. Cooling was applied to 3,290,000 cu. yd. The total heat generated by the cement would then be:

H = 3,290,000 x 384 x 218 = 275,000,000,000 Btu.

A. Heat lost in construction.

B. W. Steele in an article, "Cooling Boulder Dam Concrete", *Eng. News-Rec.* Oct. 11, 1934, stated that the heat lost in the interval between the placing of 5-ft. lifts, which averaged 4.7 days, was roughly 1/3 of the total heat generated. The significance of his statement is not altogether clear, but he obviously referred to the amount of heat generated at 4.7 days, since there was no appreciable loss after the blocks were covered by the next lift. Fig. B shows heat of hydration of Boulder Dam cement at 4.7 days to be 51 cal. per gm. The loss would then be 17 cal., or 14 per cent of the 12-year value of 120 cal. per gm., so that total heat lost was

A = 39,000,000,000 Btu.

B. Heat of hydration removed by cooling.

USBR Bulletin IV-2, "Boulder Dam Project Final Reports, Design and Construction of Boulder Dam", 1941, p. 199, stated: "The total amount of heat extracted from the concrete was about 159 billion Btu. . . . . "

But we must go to still other USBR papers before we obtain information necessary for setting up a balance sheet for the heat of hydration in Boulder Dam. The book, "Special Cements for Mass Concrete", by J. L. Savage, 1936, p. 2, gave curves, "Average Placing, Maximum and Final Temperatures in Boulder Dam". In using these curves, it was necessary to make allowances for 2 factors: a) Temperatures for the first month or two were omitted; b) Final temperatures for the last few months were probably not representative of the large bulk of the concrete placed, on account of reduced volume placed as the top of dam was approached.

The curve "Observed avg. max. temp. of concrete placed at time plotted", shows an average of about 108F. Similarly, from the curve, "Observed initial temp of concrete", we find about 72F; and from the curve, "avg. final temp of concrete" we find 55F. The same curves were given to a reduced scale in a paper, "Cracking of Mass Concrete", by Blanks, Meissner and Rawhouser, ACI JOURNAL, Mar.-Apr., 1938, *Proceedings*, V. 34, p. 484.

The drop in temperature, as a result of cooling was then, 108 − 55 = 53F. We know the quantity of concrete cooled and its unit weight. Specific heat, 0.215. With this information we can compute the amount of heat used in dropping the temperature 53F:

$$3{,}290{,}000 \times 4220 \times 0.215 \times 53 = 159{,}000{,}000{,}000 \text{ Btu.}$$

This agrees with USBR value for the total heat removed by cooling.

It is important to note that the above value includes both *B* and *C*. We must now segregate these quantities. The heat used in removing heat of hydration was proportional to the drop from maximum to placing temperature, or 36/53 of heat removed by cooling; hence:

$$B = \frac{36 \times 159{,}000{,}000{,}000}{53} = 108{,}000{,}000{,}000 \text{ Btu.}$$

C. Heat used in depressing temperature of concrete.

In a similar manner we find:

$C = 51{,}000{,}000{,}000$ Btu.

Heat balance.

With the information at hand, we can set up a balance sheet for the heat in the Dam (units of 1,000,000,000 Btu):

H =	Total heat of hydration	275
A =	Heat lost in construction	39
B =	Heat of hydration removed in cooling	108
C =	Heat used in depressing temp. of concrete	51
	Total heat of hydration removed (A + B)	147
D =	Heat still in dam H − (A + B)	128

D. Heat still in dam.

It is important to note that we are concerned here only with the *heat of hydration removed* by cooling, hence *C* does not enter into the final computation. *C* consumed 51/159 = 32 per cent of the heat removed by cooling, but it *did not remove any heat of hydration.* To illustrate this point further, the temperature of a block of steel might be depressed 50 F. We can compute the quantity of heat extracted, but it is obvious that no heat of hydration was removed.

The above figures show that 53 per cent of the total heat of hydration was lost in construction, or was removed in cooling, and that 47 per cent, or 128,000,000,000 Btu. is still in the dam.

Temperature rise.

All USBR writers agree that no heat was lost after cooling, hence the 128,000,000,000 Btu. still in Boulder Dam can have but one effect, that is, to raise the temperature of the concrete:

$$\frac{128{,}000{,}000{,}000}{3{,}290{,}000 \times 4220 \times 0.215} = 43 \text{ F.}$$

Curve B in our Fig. B shows the low-heat cements of the paper would have a temperature rise of 85F. at 12 years. Since 47 per cent of the heat remains in the dam we would expect, on this basis, a rise of 0.47 x 85 = 40F., as the rise computed from independent data. This is essentially the same as the 43F. computed above based on the indicated heat of hydration of Boulder Dam cement.

Heat from other sources.

There are 2 other factors which should be considered, each of which would cause a heat gain, in addition to the *normal* hydration of the cement considered above: *a*) Heat absorbed from foundation rock, b) Hydration of cement at an increasing rate due to temperature rise.

Heat absorbed from foundation rock.

In the design of Boulder Dam it was assumed that the foundation rock was permanently at the mean annual temperature of 72F, and that the concrete in contact with rock could be cooled to, and would remain at, its "final stable temperature". These assumptions involved a number of fallacies, 3 of which will be mentioned:

72F was too low for the mean annual temperature,

The foundation rock is permanently at about 100F. This is about mean *sun* temperature.

It is obvious that concrete cannot remain at a temperature of 40 to 60F when it is in contact with an unlimited volume of rock at 100F.

It seems a safe assumption that the temperature of the lower part of the dam was quickly raised by contact with the warm rock. It is impracticable to be exact, but the writer estimates an increase in temperature up to this time near the bottom of the dam of 11 F. due to this cause.

More rapid hydration of cement.

It is a practically invariable principle of chemistry that the rate of reaction increases with a rise of temperature. Since the lower part of Boulder Dam would soon be heated by absorption from the rock, it is doubtful whether the rate of hydration of the cement was seriously retarded as a result of the cooling operations. On the other hand, the rate would almost certainly be increased as the concrete became heated. It appears to be a conservative estimate that the over-all effect of this increased rate of hydration over 12 years, would raise concrete temperature about 10 F, in addition to that already computed as due to the normal rate of hydration.

Present temperature of Boulder Dam.

We can now make a synopsis of estimated temperature in the lower portion of the dam:

Average temperature after cooling	56F
Rise due to normal hydration of cement	43
Rise due to warm rock	11
Rise due to hydration at a higher than normal rate	10
Estimated present temperature	120F

It is the writer's opinion that the temperature along the lower portion of the central isotherm in Boulder Dam, marked "56F", in Fig. A is now of the order of 120F, or about 64 F. higher than that assumed in the design.

TEMPERATURE STRESS IN BOULDER DAM

In their efforts to avoid cracks in Boulder Dam, USBR engineers seem to have created another problem that may be even more difficult to solve. If we assume a coefficient of thermal expansion of concrete of 0.0000055, and a modulus of elasticity of 8,000,000 psi., a temperature rise of 64F in restrained concrete represents a compressive stress of:

$$0.0000055 \times 8{,}000{,}000 \times 64 = 2800 \text{ psi.}$$

The actual stress represented by this figure is problematical, since we do not know how to evaluate the combined effects of time, temperature, multi-axial stress, etc. At best a computed stress of 2800 psi., over and above that for which the dam was designed, suggests the necessity for a candid reconsideration of the stress to which Boulder Dam is now subjected.

The assumed modulus may appear too high, but the writer considers it conservative for 12-year old concrete, in view of actual tests on 18- and 36-in. cylinders which gave 5,200,000, at 28 days, as reported by J. L. Savage, "Special Cements for Mass Concrete", 1936, and over 6,000,000 at 1 year, as reported by Blanks and McNamara, ACI Journal Jan.-Feb., 1935.

BOULDER DAM COOLING CONTRADICTS PAPER

The foregoing figures on the cooling of Boulder Dam contradict many of the most significant statements of the paper. It is plain that the cooling system used at Boulder Dam accomplished neither what it was designed for nor what has since been claimed for it. Our knowledge of the hydration of cement makes it apparent that no system of artificial cooling of concrete can adequately remove the heat of hydration from a large, rapidly-built dam in "a month or two during the period of construction", as stated in the paper. The bulk of the heat of hydration cannot be removed before it is generated. The combined effects of dropping temperature of concrete and "subcooling" are generally limited to a drop of 15 to 20 F. below placing temperature; hence only an insignificant quantity of heat can be accounted for in this way.

The fact that the writer's figure of 159,000,000,000 Btu. removed from the dam agrees with the total reported by USBR, indicates that cooling produced no appreciable effect before the concrete reached its maximum temperature. This is a reasonable conclusion, since cooling circuits included several adjacent blocks, and no block could be cooled until the last block in the group was concreted. This raises a question as to the applicability of the curves in Fig. 1 of the paper, marked "Average temperature rise, 5 foot lifts exposed to air". It is not apparent how cooling can become effective in 1 or 2 days, as indicated by these curves, nor is it clear how artificial cooling can be "started immediately after placing the concrete", as stated (p. 322) unless each block has a separate cooling circuit, which is generally impracticable and was not the practice of the USBR on such dams as Boulder, Grand Coulee, Shasta.

CORROBORATING TESTIMONY OF USBR WRITERS

The estimate of present temperature in Boulder Dam may at first appear to be "theoretical". It was of course, based essentially on 3 thoughts: a) Known quantities of heat lost or removed by cooling; b) Known properties of the concrete; c) The belief that there is no way to prevent the continued hydration of cement in a large dam.

But it is interesting to note that various USBR writers have, during the past 11 years, given testimony that fully corroborates the writer's conclusions. Four such instances will be cited:

1934. B. W. Steele, in the article referred to above, written about September 1934, when two-thirds of the concrete was in place, showed actual temperatures at 49 points in Boulder Dam. At 2 points near the bottom, in concrete placed during the previous summer, he recorded temperatures 8 and 13 F. higher than those desired. At these points the concrete was supposedly "subcooled" 5 to 10 F. below the isotherms shown in Fig. A; and cooling had been discontinued only 6 months. In other words, temperature here was 13 to 23 F. higher than intended.

1934. In a report, "Movement of Boulder Dam Due to Grouting Contraction Joints", (USBR Technical Memorandum 496) by A. W. Simonds, we find a statement on this subject, describing the conditions observed before grouting was begun in May, 1934, and when the lower concrete was less than 1 year old:

At this time it was discovered that the dam was warming up more rapidly than desired after cooling had been terminated in the completed section.

1936. In a report, "Rock Temperatures in Black Canyon", (Tech. Memo. 530) by J. G. Ross, we find:

Resistance thermometer number 77 embedded in block L-7 at elevation of 580 is approximately midway between upstream and downstream faces and less than 35 feet from the Nevada abutment. The concrete in this vicinity, after being poured, was artificially cooled to 49 degrees in December, 1933 after which cooling was discontinued. Within one year the temperature in this block gradually rose to 70 degrees and, by the end of the second year after artificial cooling (December, 1935), the temperature had risen to 80 degrees. Other thermometers in the same block verify these figures. Temperatures in nearby blocks show the same tendencies although in none of them were temperatures higher than 77 degrees recorded.

In Fig. A the location of thermometer 77 can be identified from the above description; it is in the middle cluster near the bottom of the dam, where the desired temperature was 53F.

Mr. Ross attributed this temperature rise to heat from foundation rock and to numerous hot springs that discharged large quantities of water of 100 to 114F. This explains part of the observed rise of 28 to 31 F. above the "subcooled" temperature, but it was no doubt partly due to hydration of the cement.

1944. The paper under discussion gave some evidence. It stated that at "one point" on each of the exposed faces of Boulder Dam the temperature is above that intended: Upstream face, midwinter, 1 F.; midsummer, 11 F. Downstream face, midwinter, 4 F.; midsummer, 16 F.

Summer temperatures would apply to about 2/3 of the year. While there are questions as to what was meant by "face" temperatures, and the location of "one point" on a face 900 ft. long, the figures show a rise of about 10 F. above that intended.

Summary of USBR evidence.

The foregoing examples cited from four different USBR reports over the past 11 years show:

a) That the temperature rise in Boulder Dam is real, and that the writer's estimate is not mere "theory",

b) Temperature rise was noted by men on the job when the first concrete was less than 1 year old and before grouting of vertical contraction joints was begun,

c) A few months after completion temperatures near the bottom of the dam were 15 to 31 F. above those intended,

d) Temperatures at the faces of the dam 9 years after completion were about 10 F. higher than intended,

e) That "final stable temperature" was not reached in "a month or two during the period of construction",

f) Actual temperature rise occurred at a more rapid rate than that suggested by the writer's analysis.

Practically all the important recommendations of the paper were contradicted by experience at Boulder Dam.

FURTHER COOLING OF BOULDER DAM IMPOSSIBLE

That those in charge of the design and construction of Boulder Dam overlooked the continuing hydration of the cement, is clearly shown by the fact that all connections with the cooling system of 590 miles of embedded pipes, were buried in the dam *and the pipes filled with grout,* soon after refrigeration was discontinued. This precluded any resumption of cooling at a later date.

GROUTING OF BOULDER DAM

The paper lists grouting of vertical contraction joints as: ". . . a special requirement of mass concrete in order to insure its integration and action as a continuous mass after it has been brought to the desired temperature."

USBR writers are responsible for a number of different estimates for the average widths of the contraction joints after cooling Boulder Dam. The most recent, and probably the most detailed statement is in Bulletin IV-1, "General Features", 1941, p. 157: "The average width of the joint openings was 0.126 in. for the radial joints, and 0.087 in. for the circumferential joints". The temperature rise required to close these joints would be:

Joint	*Average Size of Blocks*	*Average Opening*	*Temperature Rise to Close*
Radial	45 ft.	0.126 in.	42 F.
Circumferential	37	0.087	35 F.

The temperature rise necessary to close these joints is essentially that computed by the writer, when the cement was assumed to hydrate at its normal rate; and considerably less than the estimated temperature rise due to all causes. If these joints had been left open:

a) Most of the expansion of the concrete, due to unconsidered temperature rise, would have been taken care of,

b) The joints would have closed before the reservoir was full,

c) The excessive concrete stress computed above would have been avoided.

Grouting was not necessary to prevent leakage through the dam; this would have been prevented by the grout stops. These considerations make it appear that the grouting of Boulder Dam was a mistake, and that all benefits of cooling have been lost at the end of 12 years.

WANTED—BOULDER DAM TEMPERATURES

The only statement in the paper on temperatures in large concrete dams was that quoted above on "face" temperatures at 2 points on Boulder Dam. More than 400 thermometers were embedded in this dam, and thousands have been embedded in other concrete dams built by the USBR. These and thousands of other instruments give numerical data on "Cracking and temperature control of mass concrete", but this is one of the best guarded secrets in the world today. Not one reading of these instruments has been "officially" released during the past 11 years.

Since the present paper again brings the subject of temperature control to the fore, it seems to the writer that it is incumbent on the author to give the temperatures observed in Boulder Dam, at the section shown in Fig. A, at, say, 2, 3, 6, and 12 years after concreting began in June 1933.

AUTHOR'S CLOSURE

Temperature control of mass concrete was first considered of sufficient importance to warrant special measures when plans were formulated for the construction of Boulder Dam, which was nearly twice the height of any previously constructed concrete dam. The measures successfully used at Boulder Dam for the control of cracking have been the subject of study by designers and builders of dams over the entire world. It is only natural that as more information becomes available more exact and effective measures will be developed for coping with the troublesome problem of cracking of large concrete dams.

The paper presented a discussion of the methods of temperature control that are available and the effects of various measures that might be used. The subject matter was assembled primarily for use by ACI Committee 207, "Properties of Mass Concrete" in conjunction with other material assembled by the committee. It is appreciated that in presenting this information separately to others interested in the problems, the lack of specific data in the form of observed temperatures of actual structures weakens the presentation. Mr. Abrams calls attention to this lack of observational data. The paper as presented was not intended to be a report of conditions at Boulder Dam or any other specific structure. However, in reply to Mr. Abram's discussion, which focuses attention almost entirely on what has or has not taken place in Boulder Dam, some temperature data* are presented which, it is hoped, will correct the impression that these data are being purposely withheld, and will

*A more complete record of the temperature history of Boulder Dam is to be contained in one of the final reports on the Boulder Canyon Project, a bulletin entitled, "Cooling of Concrete Dams" now in preparation.

dispel any further desire to make wild conjecture as to the temperatures that actually exist.

No attempt will be made to answer point by point the 14 numbered conclusions and the additional lettered statements of conclusion throughout Mr. Abrams' discussion since many of these points are supported only by argument which presupposes that the present temperature in the interior of Boulder Dam is about 120 F. All that one needs to do to refute most of the argument is to look at the structure itself. The highest temperature recorded at present by any of the many thermometers embedded in the dam is 84 F. The temperature in the interior of the dam farthest removed from any surface is actually 75 F and has not increased perceptibly for more than a year.

Mr. Abrams' discussion of the hydration of low-heat cements leads him to the conclusion that the increase in adiabatic temperature rise will continue indefinitely in proportion to the logarithm of the age. Predictions to 12 years of age are made on this basis from observed data from age 1 day to 28 days. His discussion is based on the assumption that generation of heat exactly parallels development of strength and some short-time data for temperature rise are presented in support of this assumption.

Obviously, the logarithmic relationship of heat generation and age, or of strength and age, is not a rational relationship because it implies an ultimate heat generation which is unlimited in amount. It is also drastically in error in the interval between the time of casting and age one day. It is an empirical relationship that at best can be made to fit the data over a relatively short time and cannot be used for extrapolation of the curves or prediction of temperatures or strength far beyond the limits of observation. Study of a considerable amount of published and unpublished data of long-time strength development of concrete reveals that usually the data are fitted better by a curved line than by a straight line on the logarithmic time scale. This is true especially of large specimens or of cores extracted from service structures. Usually it is only with small specimens in the presence of excess moisture that a straight line will fit the strength data for more than a few years. Even with the short-time data presented in Fig. B of Mr. Abrams' discussion, the points of curves A and C can be fitted more closely by a curved line than by a straight line. Departure from the straight line is more apparent as time increases. Thus the basis of Mr. Abrams' discussion of hydration of low-heat cements is not supported by observed data. His subsequent computations of estimated temperatures and of temperature stress in Boulder Dam are, therefore, grossly in error.

Mr. Abrams states that the principal fallacy in the paper is the assumption that portland cement ceases to hydrate when artificial cooling is discontinued. It is regretted that this meaning was read into the statements of the advantages of cooling the concrete artifically. Such meaning was not intended. At the time of the construction of Boulder Dam very few data were available on the long-time heat development of low-heat cement and it was estimated that under adiabatic conditions the temperature of Boulder Dam concrete would ultimately rise about 9 degrees above the value attained at age 28 days. Statement of this expectation was made in the Bureau of Reclamation publication, "Special Cements for Mass Concrete", by J. L. Savage, referred to by Mr. Abrams. On page 154, in discussing the concrete temperature of Boulder Dam, it is stated:

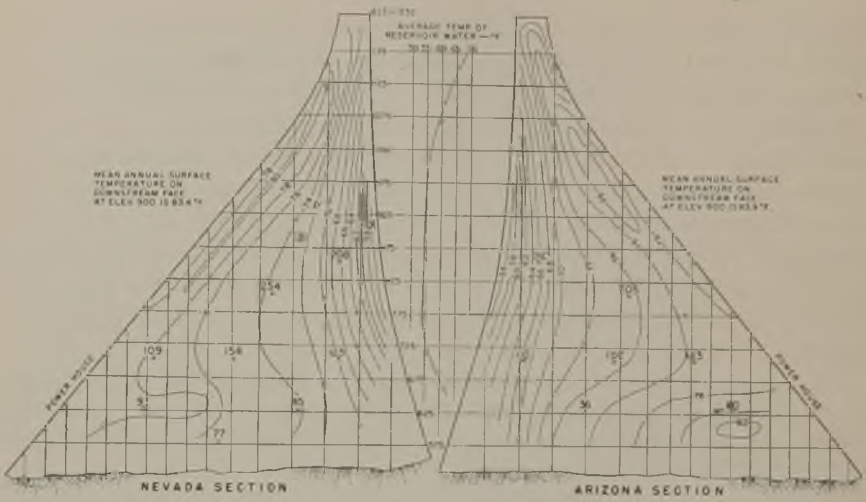
In the latter case (with the use of low-heat cement) the maximum temperature occurred at the time cooling was started, which was within one month after the concrete was placed, and the temperature was thus prevented from rising to its otherwise ultimate value, estimated to be about 9 degrees higher.

Laboratory data obtained since the early estimates were made indicate that the temperature rise that might be expected after 28 days is about 15 degrees. The data from Boulder Dam prove that the original estimate of the amount of heat that would be developed after cooling was stopped was too low, the actual average rise being about 20 degrees. These same data also prove that Mr. Abrams' estimate of 64 degrees rise is much too high.

The temperatures of the concrete of Boulder Dam as of March 1945 as determined by 396 resistance thermometers are as shown on the isothermal charts of Fig. C. The majority of resistance thermometers embedded in Boulder Dam are located in two vertical sections roughly 100 feet on either side of the central plane. The sections are shown as viewed from the central plane. In addition more detailed data are included in Table A from representative thermometers whose locations are spotted on the sections of Fig. C. The thermometers selected include No. 77 referred to in Mr. Abrams' discussion.

Fig. D shows the temperature rise since age 6 months of representative thermometers in the interior of Boulder Dam. Six months age was used as the reference point for all thermometers because in all instances cooling had been completed by that time and the concrete temperature then was at or very close to the minimum recorded. Curve A shows the average temperature rise indicated by the first 14 selected thermometers listed in Table A.

The leads from all thermometers embedded in the concrete extend to galleries within the dam. Nearly all thermometers were therefore located from 40 to 75 ft. from some gallery so that the leads might not be excessively long. Some instruments located in the region marked B on the Nevada section of Fig. C are the only thermometers in the dam which



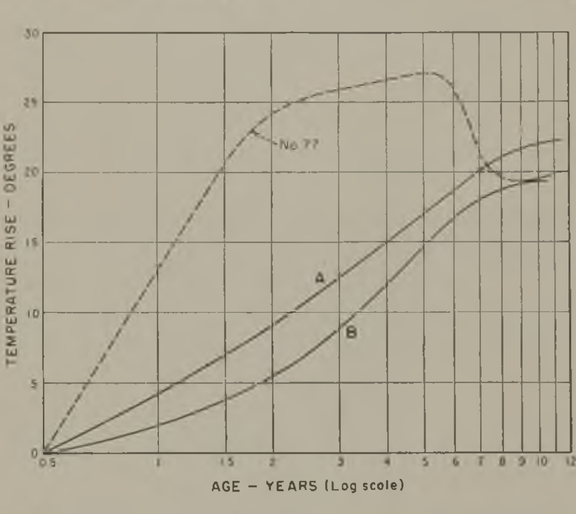


Fig. C (above)—Sections of Boulder Dam showing temperature of concrete as of March, 1945.

Fig. D (left)—Temperature rise of representative thermometers embedded in Boulder Dam.

are more than 100 ft. from a gallery or from any other exposed surface. The average temperature rise indicated by 6 of these thermometers between elevations 880 and 900 is shown by curve B of Fig. D. The temperature data of these six thermometers are also listed in Table A.

Although behavior of thermometer No. 77 cannot be considered representative of interior concrete temperatures because of its proximity to the foundation rock, it is included in the list because of the previous reference to it. This instrument is within the inner gorge of the canyon and its record indicates the influence that the rock temperatures have had on the concrete at that location. Normal portland cement was used in the construction of the lower 80 ft. of Boulder Dam which in-

TABLE A—TEMPERATURE OF CONCRETE OF BOULDER DAM

(Records from selected thermometers embedded in the concrete)
(Temperatures in degrees F.)

Therm. No.	Age—Years											
	0.5	1.5	2.5	3.5	4.5	5.5	6.5	7.5	8.5	9.5	10.5	11.5
36	48.5	54.0	59.3	63.0	65.8	68.2	70.7	72.3	73.5	74.3	75.0	75.2
80	56.8	63.8	67.7	70.4	72.5	74.3	76.2	77.5	78.5	78.8	79.0	79.1
102	49.0	56.2	59.8	64.0	66.9	69.1	71.3	72.8	73.8	73.9	74.5	
110	43.9	52.4	56.4	60.2	62.8	64.3	66.3	67.4	67.9	68.0	68.0	
143	57.8	64.7	67.3	68.8	70.2	71.6	73.0	74.0	74.9	75.4	75.7	
205	54.7	59.3	61.4	63.8	66.0	68.0	70.0	70.7	71.4	72.2	72.9	
247	44.0	51.2	57.8	61.1	63.0	64.1	64.8	65.5	65.5	65.6	65.7	
45	47.5	55.9	60.7	64.4	67.4	69.5	71.3	73.2	73.9	74.3	74.5	74.7
93	56.4	64.5	68.8	71.0	72.5	73.6	74.9	77.0	78.0	78.2	78.2	78.2
109	60.0	67.2	68.5	69.7	71.2	72.3	73.4	75.6	76.0	76.3	76.5	
123	45.3	53.2	58.5	62.1	64.8	65.8	67.0	69.5	70.1	70.0	70.0	
154	52.7	58.6	62.5	65.7	67.5	68.7	70.0	72.5	73.3	74.0	74.6	
208	44.7	52.2	58.5	61.8	63.3	64.3	65.0	66.0	66.0	65.8	65.5	
254	52.3	58.2	60.8	63.7	66.3	68.7	70.3	71.6	72.4	73.0	73.6	
204	51.5	55.6	58.9	62.5	65.3	67.9	69.4	70.8	71.2	71.7	71.8	
228	52.8	57.2	60.8	64.2	66.8	68.8	70.0	71.0	72.0	71.8	71.0	
278	52.3	55.8	59.2	62.5	65.4	67.9	70.0	71.1	72.0	72.6	72.6	
289	50.5	55.5	59.4	63.0	65.5	68.0	69.3	70.7	70.8	71.0	71.0	
327	53.7	57.0	60.2	63.6	66.4	68.4	70.0	71.0	71.2	71.5	71.5	
367	55.1	57.3	59.9	63.4	66.2	68.8	70.7	71.9	72.6	73.0	73.3	
77	55.1	75.6	80.5	81.2	81.9	82.0	79.0	75.1	75.0	74.3	74.8	75.2

cludes the location of this thermometer. However, the effect of type of cement cannot be discerned in the temperature-rise behavior of this instrument. For comparison with curves A and B, the temperature rise of No. 77 is also shown on Fig. D.

Curve A of Fig. D shows a temperature rise very close to a straight line on the semilogarithmic plot up to age 8 years, but definite deviation from the straight line after that time. Comparison with curve B indicates that some of the temperature rise in the first year after cooling may have been due to heat gained by exposure from the galleries. For the first year or two curve B is considered to be more representative of the temperature rise of interior concrete uninfluenced by surface exposure conditions. Both curves A and B show that close-to-equilibrium temperatures are established at the present time in Boulder Dam and that the temperature rise is no longer in accordance with any "logarithmic law".

Underestimates were made of the temperature of the water in the reservoir and of the average exposure temperature at the downstream face of Boulder Dam by about the same amount that the temperature rise of the concrete following cooling was underestimated. The mean annual temperature of the reservoir water at various levels is shown in Fig. C. Direct sunlight on the downstream face has the effect of maintaining the surface at a mean annual temperature of about 84 F. instead of at the mean annual air temperature which is 72.5 F. Some of the temperature rise since cooling was stopped must therefore have been due to heat gained from exposed surfaces and cannot all have been due to continued hydration of the cement.

Boulder Dam was the first dam in which artificial cooling was used in other than an experimental fashion. The primary purpose was to cool the concrete during the construction period to what was estimated to be the final stable temperature of the dam. At the end of the cooling period the joints were expected to be opened to their widest extent. The contraction joints were then grouted and it was anticipated that the temperature changes subsequent to grouting would be small and would take place slowly over a long period. The temperature rise from the time the joints were grouted to March 1945 averages about 20 degrees. It was desired to grout the joints with the concrete subcooled and it is considered that the amount of subcooling that was actually obtained is not excessive. Since then artificial cooling has been used to some extent in the construction of at least 16 other large dams. In later instances, notably in the construction of Friant Dam by the Bureau of Reclamation and of Fontana Dam by the Tennessee Valley Authority, measures were taken to control by artificial cooling the placing temperature and the temperature rise of the concrete following placement so that fewer contraction joints would be required with consequent saving in construction costs.

In Mr. Abrams' discussion of "Heat Lost in Construction", he misinterprets the statement included in B. W. Steele's article on "Cooling Boulder Dam Concrete" that the heat lost in the interval between the placing of 5-ft. lifts, which averaged 4.7 days, was roughly one-third of the total heat generated. Mr. Abrams considers that Mr. Steele obviously referred to the amount of heat generated in 4.7 days. That is not the case. The meaning was, as stated, that during the time the top of the lift was exposed, an amount of heat would be lost from the concrete that roughly approximates one-third of the total heat that would ultimately be generated. Engineers of the Bureau of Reclamation consider that the ultimate heat development is a finite amount and estimate of that amount is based upon laboratory determination and an exponential equation of the form of the writer's equation (12). This type of equation is quite different from the logarithmic equation used by Mr. Abrams. It is understandable that Mr. Abrams does not think in terms of total amount of heat to be generated since the "logarithmic law" which he uses implies that there is no limit to the total amount of heat.

Exception must be taken to one other positive statement made by Mr. Abrams. In discussing the heat absorbed from the foundation rock he states:

The foundation rock is permanently at about 100 F. This is about mean *sun* temperature. It is obvious that concrete cannot remain at a temperature of 40 to 60 F. when it is in contact with an unlimited volume of rock at 100 F.

Only in a relatively small region of the foundation contact, where hot springs were encountered at the base of the dam, were temperatures as high as 100F recorded. The hottest water encountered during the construction period had a temperature of 114F. This was from a hole drilled from one of the diversion tunnels. The hottest water that flowed from any of the foundation drains extending to the drainage galleries of the dam was 103F. In holes drilled just outside the excavation limits of the dam at various levels for the purpose of measuring rock temperatures, average temperatures of about 80F were recorded. These holes varied in depth from 5 to 75 ft. It is not clear what Mr. Abrams means by "mean sun temperature". If he means the mean temperature of the rock surface that is exposed to direct sunlight, which might be considered the effective mean exposure temperature, then all available evidence indicates that 100F is too high a figure. Average surface temperatures measured by recording thermometers at two points on the downstream face of Boulder Dam over an 8-year period are 83.4 and 83.9F. A large amount of direct solar radiation was received at these locations. The average of the mean annual temperatures observed in three holes drilled in the rock to 5 ft. depths for the purpose of measuring rock temperatures was 76.3F.

It is reasonable to believe that rock temperatures beneath the dam will eventually be lowered due to contact of the cold reservoir water on the upstream face of the dam and on the rock surfaces upstream from the dam rather than that the concrete temperatures will eventually be raised by contact with the rock to the value of the rock temperatures before the reservoir existed. Warm water flowing from foundation drains extending to galleries within the dam has decreased in temperature since the reservoir was filled. The maximum temperature of water flowing from foundation drains at present is 80F. It is therefore expected that the concrete temperatures at the base of the dam near the rock contact will gradually be lowered rather than that they will continue to rise.

The reduction in temperature of thermometer No. 77 by about seven degrees between the fifth and the eighth year, as indicated in Fig. D, is unexplained, though a similar reduction in temperature was recorded by several other thermometers in this region. A possible explanation is that supplemental foundation grouting and drilling of additional drain holes may also have altered the temperature condition of this hot-spring area.

In conclusion, the temperature conditions that exist at Boulder Dam are sufficiently close to the estimates made by the designers to justify confidence in the methods employed to control the subsequent volume change of the concrete by controlling the temperature, removing the excess heat, and grouting the contraction joints during the construction period.

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Discussion of a paper by D. R. Cervin:

A Practical Procedure for Rigid Frame Design*

By W. D. BIGLER, PHIL M. FERGUSON and AUTHOR

By W. D. BIGLER†

Mr. Cervin's procedure for designing concrete buildings by tabulating computations and dimensions on prepared forms is compact and efficient. Every structural engineering office needs such a system to reduce work and errors to a minimum.

The two-cycle approximate method of moment distribution that the author recommends is obviously a great time saver, sufficiently accurate for the usual type of rectangular building frame. This method assumes that ends of beams are fixed two spans from the joint under consideration and that the far ends of the columns are fixed. It consequently has the limitation that unbalanced beam moments transmitted to columns do not enter into computation of beams above and below. Moreover, the assumption that a column is necessarily fixed at a footing may lead to error. Especially if the column rests on a single-pile footing or on a relatively narrow continuous foundation, the lower end actually is only slightly restrained, if not hinged.

As a second limitation, the two-cycle method does not consider the effect of lateral deflection or "sidesway" resulting from an adjustment of the frame so that internal shears equal zero. It is true, however, that in wide, multistoried buildings and in small buildings not considerably unsymmetrical in shape or loading, stresses resulting from lateral deflection are negligible.

To study the accuracy of the two-cycle method under unusual, but not improbable, conditions the writer selected a portion of the frame used by Mr. Cervin as shown in Fig. A. Dimensions, K values, and fixed-end moments are the same as those in the original example. First-

*ACI Journal, Apr. 1945; *Proceedings* V. 41, p. 453.
†Long Beach, Calif.

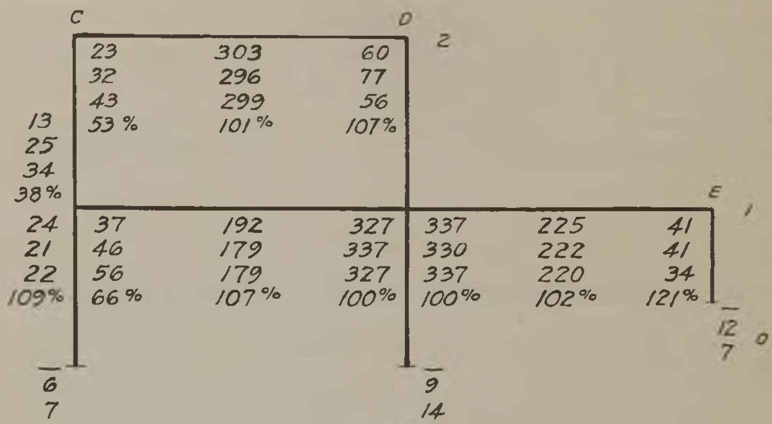

Fig. A

story columns are assumed half-fixed at footings. In this selection the K of wall column D12 is three times the K of wall column C12, but the dimensions of a square section need to be increased only 32 percent to triple the I, and such increase might result from wall crane loads. K values equally unbalanced as those in this example would exist in a building frame on the side of a hill with all columns of unequal length but with normally proportioned sections.

Fig. A shows a comparison of positive and negative beam and exterior column moments from three sets of computations. Starting with the upper, the four tabulated figures are:

1.–Maximum moment computed by the two-cycle method.

2.–Maximum moment selected from computations by the Cross method on all seven possible patterns of live loading. This value, like the figure above, is based on the assumption that there exist forces at C1 and C2 which prevent lateral deflection.

3.–Maximum moment (No. 2 above) corrected for lateral deflection. This is the true moment.

4.–The percentage of the value obtained by the two-cycle method as compared with the true value.

These figures show only a negligible discrepancy in negative interior and positive beam moments. However, in exterior negative beam and exterior column moments the approximate method shows a maximum error of -62 per cent. It is apparent, therefore, that small frames considerably unsymmetrical in shape or loading require exact analysis. As the author suggests, such procedure is laborious. The designer can keep the work at a minimum by sketching the elastic line of the continuous frame as it deflects when one beam or two adjoining beams are loaded and thus find the patterns of loading that produce maximum moments.

Often more convenient for solving small frames is the method described in the following six steps:

1.–Distribute the total fixed-end moments of one beam, considering no load, either live or dead, to be on the other beams. To lessen the labor involved, neglect all distributed moments farther away than, horizontally, two spans or one column length plus one span and, vertically, one column length from the beam under consideration. These discarded moments will be negligible. Three cycles of distribution are sufficient. Three are necessary, however, to include returns from the column ends.

2.–Correct for unbalanced shear by one of the methods described below.

3.–Compute the positive moments at midspan, which equal $FEM_T \times 1.5 - \frac{FM_L + FM_R}{2}$ for uniform loading.

4.–Repeat the procedure for all other beams, one at a time.

5.–Tabulate on a diagram of the frame, at its proper location, the various partial moments thus obtained and beside each the product of the partial moment times $\frac{FEM_D}{FEM_T}$. The second figure is the corresponding partial moment for dead load alone.

6.–At each tabulation, select the combination, one from either the left or right figure of each pair, that will add to the maximum. The sum is the true maximum moment.

The maximum stress in a column is $\frac{P}{A}$ plus the maximum compressive stress due to bending. In computing P, consider live load on all those beams framing into the column which are not represented in the tabulation by dead-load moments alone.

For correcting unbalanced shear, step No. 2 above, a combination of the Morris method* with the Cross method will keep the shear at approximately zero during distribution of *FEMs*. If more than two-place accuracy is used, however, it is more convenient to carry out the three-cycle distribution of *FEMs* first and then find the shear corrections. A third method, more easily understood and less liable to error, and usually less laborious, especially in a lower story that has columns of unequal length, is as follows:

Find $TM = -S \times F \times \frac{R}{\Sigma R} \times \frac{h}{2}$ for all columns, in which TM = trial cor-

*See discussion by Prof. C. T. Morris, Trans. A. S. C. E., vol. 96, page 68.

recting moment to be distributed from a column end. The two values for any one column are equal.

$S = \sum \frac{FMca + FMcb}{h}$, the unbalanced shear in a story.

$FMca$ and $FMcb$ = moments at upper and lower end of column after distribution of $FEMs$.

$R = \frac{I}{h^3}$ of a column.

$\frac{R}{\Sigma R}$ = proportional part of shear in a story taken by a column.

$F = \frac{1}{2N} \times \sum \frac{1}{1\text{-}nD}$ for the story under consideration. There are two values of $\frac{1}{1\text{-}nD}$ for each column. One value of F, greater than unity, applies throughout a story and each story will have a different value.

n = number of columns at a joint.
N = number of columns in the story.
D = distribution factor of column.

If all columns in the story have the same length, then:

$$TM = \frac{-Mc \times F}{2} \times \frac{R}{\Sigma R}\text{, in which}$$

Mc = algebraic sum of the moments at the upper and lower ends of the colums after distribution of $FEMs$.

Distribute TMs and add the corrections thus derived to the moments already obtained. If further corrections are necessary, repeat the procedure with S equal to the remaining unbalanced shear. No repeated correction was necessary in solving for side way the frame shown in Fig. A.

The author has not made provisions in his forms for checking frames for wind loads. One or two sheets can be added conveniently for comparing bending moments and direct stresses due to wind and vertical loads.

By PHIL M. FERGUSON*

The author's re-emphasis on the very great time saving possible by use of the two-cycle moment distribution method (originally developed by the Portland Cement Association) is a definite contribution toward

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more rational designs. This has been coupled with tabular forms which greatly systematize the auxiliary calculations.

Moments by the two-cycle method will not be "exact" when applied to irregular spans and loadings. The two-cycle method does not consider any rotation of joints at the floors above or below the one under investigation, nor does it consider the rotation of more than three joints in the calculation of any one maximum moment. To illustrate the results of this procedure, where spans are irregular as in this case, the analysis was repeated for maximum loadings, this time using the entire second floor and multiple distributions at all five joints. These maximum moments are tabulated and compared below.

Method	A	B	C	D E
Entire floor....	−8+145*−157	−156−54−175	−185+148−269	−270+164*−9
Two cycle.....	−7+135 −152	−150−53−191	−198+146−261	−264+171 −8
		Min. positive		
Entire floor....		−140		
Two cycle.....		−116		

*At mid-span. Maximum is off center, amounting to +150 for AB and + 169 for DE

None of these errors is serious, although the variation at C is noticeable and the minimum positive moment in BC by the two-cycle method is rather on the unsafe side. Lest the error in moments at C be thought to typify general errors that will *always* be on the side of safety, the following values of maximum moment at C on the third floor are tabulated:

	C	
Entire floor....................	−156	−162
Two cycle.....................	−146	−150

Here the error is on the unsafe side. With major differences in spans and in loadings adjoining C, the results of the two-cycle method must be considered good. It is to be hoped that more engineers will utilize this method to improve their design data.

Since the author proposes his method as a model that others may follow, one error of serious import should be noted. Sheet 7 shows several beams (especially beams CD at different levels) where approximately three-fourths of the positive moment steel is provided in the form of trussed bars. In my experience, no case of beams this length is recalled where it would have been possible to truss or bend up three-quarters of the positive steel without infringement on either the positive or negative moment needs. Since these are deep beams, it seems unlikely that much over half the area of the positive steel can be bent up

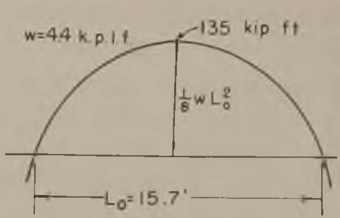

Fig. B (left) Max. Positive Moment.

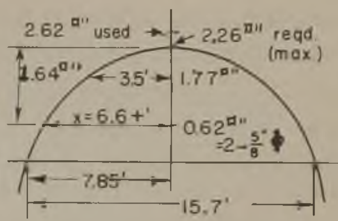

Fig. C (right) Reqd. A_s Curve

and made available for negative steel. The 1940 Joint Committee Specification will not often permit the trussing of as much as half the bottom steel (when beams are designed for uniform loads). The bend down points specified on Sheet 11 (i.e., at the point of inflection which accompanies maximum negative moment loading) are entirely unsatisfactory for usage in ordinary building construction, unless it be for shallow construction such as slabs.

The unsatisfactory condition in the extreme case of beam CD at the second floor level will be shown numerically. The maximum positive moment of 135 kip ft. (Sheet 7) exists very near the center line of the span. Since w_t is 4.4 kips p.l.f., the distance *Lo* between points of inflection (Fig. B) can be obtained from the relation $135 = \frac{1}{8} \times 4.4 \times L_o^2$, or

$L_o = \sqrt{\frac{1080}{4.4}} = 15.7$ ft. The moment curve can be used as the required

A_s curve, as shown in Fig. C. The *required* steel, 2.26 sq. in. (Sheet 7) has been used as the maximum ordinate, instead of the total area of 2.62 sq. in. which was provided, since this permits slightly shorter lengths for that part of the bottom steel that is bent up. This curve shows that some part of the 2-1 in. sq. bars must be continued out a distance $x = 7.85$

$\sqrt{\frac{1.64}{2.26}} = 6.6^+$ ft. If the 1-in. sq. bars are bent down at 45 deg. according to Sheet 11, at 6.5 ft. from the center of column D, they will be available at the level of the positive steel at 6.5 + 2.5 (assuming 30-in. offset) = 9.0 ft. from the center of column or 3.5 ft. from the middle of span. This compares with the needed 6.6 ft. and leaves a length of 6.6 − 3.5 = 3.1 ft. in the bottom with deficient steel. Stated another way, the steel needed at 3.5 ft. from the middle of the span is 2.26

$\left[1 - \left(\frac{3.5}{7.8}\right)^2\right] = 2.26 \times 0.788 = 1.77$ sq. in. Since the author provides

at this point only 0.62 sq. in. or 35 per cent of the required area, a steel stress well beyond the yield point is indicated on the straight bottom bars.

The situation could be helped, but not completely remedied, by bending these particular bars down at *about* 3.5 ft. from the center of the column. The typical bend point called for on Sheet 11 should be moved closer to the column, and the steel on Sheet 7 should be selected with the extent of the positive moment requirements more in mind.

Two other comments will be made, neither so serious as the above. The author's method of calculating V_{con} on Sheet 9 by using *double* the difference between the *maximum* end moments, is new to me. It seems to work well for the frame shown. However, it should be noted that such a shear does not reduce to zero in the case of beams of uniform length and with equal live load, even though the maximum negative moments at the two ends of such a beam would be equal. Possibly some arbitrary *minimum* V_{con} should be taken into account, say 10 per cent of the simple span shear as a minimum for the case when the maximum end moments are nearly the same.

The value of V_{con} calculated by the author's method will add to the simple beam shear only on that half of the beam adjacent to the *larger* negative moment. On the half of the beam adjacent to the smaller negative moment this particular value of V_{con} will subtract from the simple beam shear, but this reduced shear would not be the maximum. A different loading of adjacent spans, the same as that for maximum negative moment at this end, is required for the maximum shear, and this loading gives a different value of V_{con}. This value of V_{con} will be smaller than the author's value and may even be of opposite sign to the simple beam shear at this end, thus sometimes giving a design shear adjacent to the smaller negative moment that is less than the simple beam shear. Since V_{con} is not large, except when the difference in the maximum negative moments at the ends of the beam is large, the differences discussed here will not usually be important, and some designers will prefer to ignore them. However, in beam DE on the second floor, more complete calculations show that the maximum total shear at the E end is really $-51 + 10 = -41$ kips, instead of the 63 kips shown on Sheet 9.

The author selects his beam sizes by determining the balanced rectangular beam size necessary to care for the maximum negative moment. Since some bottom steel always runs into the support, it would seem economical to reduce beam depths (and thereby structure height) by utilizing this straight bottom steel as compressive reinforcement at the face of columns.

In summary, the two-cycle moment distribution method is a distinct advance in design procedure and is entirely practical. However, there is an element of danger always present when the detailer and designer of continuous structures are too far apart. A good designer must know his details, and a good detailer can scarcely proceed by rule of thumb alone. Poor detailing can be and often is as great a source of error as approximate moment assumptions.

AUTHOR'S CLOSURE

The writer acknowledges the two complete discussions of his paper "A Practical Procedure for Rigid Frame Design," submitted by Mr. W. D. Bigler and Prof. Phil M. Ferguson. Both papers are concrete in their discussion and have some genuinely helpful criticisms or suggestions to offer. Of considerable interest to the writer is the frank and candid opinion of both men in accepting and advocating the use of two-cycle moment distribution for continuous frames. That exceptions to its use can and will occur has been carefully explained by both commentaries.

Professor Ferguson has properly detected what he calls a serious error regarding the amount of positive steel and the bend down point of the negative steel. His own suggestion that the steel should be selected with the extent of the positive moment requirements more in mind, coupled with his very clear computations of how much positive steel should exist at various parts of the beam, provides a clue for systematically checking this feature as a part of the beam design on Sheet 7.

Between the lines "Straight, bottom" and "Steel provided" should appear this equation:

$$L_o = \sqrt{\frac{8\,(+M_{max})}{w_t}}$$

where L_o is defined in Symbols and Notation as the length of the beam having positive moment.

Applied to the Beam 3CD in question, this length is 15.7 feet. Half of this distance, measured from the centerline of the beam is therefore the point of inflection for positive steel. By bending up one bar at this point, it will develop useful negative steel at a distance of 2.0 feet from the centerline of the column. Since the parabolic shape of the negative moment is practically triangular, the first turn-down for negative moment can be made at one quarter of the negative inflection point, or 1.3 feet, somewhat less than the point already selected. It is then necessary to determine if sufficient steel exists after the first trussed bar to satisfy the positive moment requirements. For the beam in question Prof. Ferguson has already computed that 1.77 sq. in. are necessary, and the steel provided by one 1-in. square and two-⅝-in. round is 1.62 sq. in., less than ten per cent deficient. If it now be tacitly assumed that Prof. Ferguson's first suggestion regarding care in selection of positive steel be adopted, no such variation in positive and negative steel would exist as demonstrated in this example. Thus, the ten per cent deficiency estimated above would conceivably be a lesser value, and any happier choosing of positive and negative steel areas would minimize or completely remove this deficiency. It hardly seems necessary to perform any calculations in routine problems other than substituting in the formula already proposed; doubtful cases should be checked analytically. Care should be taken, however, to have the bend down point of the steel nearest the column centerline not closer than one fourth of negative inflection point. It is reasonably apparent that turning up all the trussed bars slightly before reaching the positive inflection point is not serious since at least one third of positive steel area should be carried through to the supports. Only a small change is necessary on the beam schedule and on the typical beam drawing to accommodate this addition.

Regarding the calculation of V_{con}, Professor Ferguson's suggestion that some arbitrary *minimum* V_{con} of about 10 per cent of V for the simple span be used, is based on sound reasoning and is worthy of adoption. However the computation of the lesser shear of the two ends of a beam seems hardly worthwhile. It is doubtful if the average beam would save over a single stirrup by such a procedure; and the inconvenience of dissymmetrical detailing coupled with the additional computations involved, would hardly balance the steel saved.

Prof. Ferguson's last suggestion proposed that the compressive reinforcement at the face of the column be considered in computing beam depths as a means of economizing in concrete and overall building height. Again the suggestion is in order, but care must be taken to devise a tabular method of solution which permits rapid design of many beams. Any method used depends upon the individual tastes of the designer; the writer believes that the method recommended in "Reinforced Concrete Design Handbook"* lends itself well to mass use on Sheet 7.

Mr. Bigler has as the kernel of his discussion, the effect of lateral deflection or "sidesway" resulting from an adjustment of the frame so that internal shears equal zero. The comparative results of a highly dissymmetrical frame analyzed by two-cycle moment distribution, Cross moment distribution without correction for sidesway, and Cross distribution corrected for sidesway (See Fig. 1), is truly revealing in that some types of frames or loadings warrant an exact analysis.

However, the results of Mr. Bigler's computations do clearly reveal that the only major errors are in the outside negative moments of wall beams and in wall columns. Since the magnitude of end moments in wall columns is usually small, the degree of error here is of little consequence

*Reported by ACI Committee 317.

because the available steel from other moment considerations is usually far more than required. The same favorable considerations are not applicable to wall columns. It can be pointed out, however, that the major influence on column design is axial load and not bending moment; therefore, moment deficiencies of 50 per cent obtained by two cycle distribution will result in an overall column design deficient only about 10 per cent.

This is not to be construed as a brief defending slipshod or faulty methods of design in order to save engineering time. It must be remembered that the purpose of this paper was to layout a practical procedure for rigid frame design. Most designers are acquainted with some method of sidesway correction, but it is very doubtful if many are prepared to spend the time required for such work. Mr. Bigler shows considerable versatility in handling lateral deflection, but even he admits a large amount of work to achieve his results.

Until a method of correcting for sidesway is developed which has the comparable speed, simplicity, and thoroughness of two-cycle moment distribution, it does not appear feasible to this writer to make the desired correction for lateral deflection. The challenge is left to Mr. Bigler and other capable theoreticians to solve this problem so that all practicing engineers can obtain fully accurate results consistent with a reasonable time expenditure.

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Discussion of a paper by Jacob J. Creskoff:

Estimating 28-Day Strength of Concrete from Earlier Strengths—Including the Probable Error of the Estimate*

By S. P. WING and AUTHOR

By S. P. WING†

The author's method of estimating 28-day concrete strength from tests at earlier ages appears more complicated than it really is. His explanation of the method of least squares for fitting a straight line to data, together with his algebraic expression of the results, tends to obscure the essential simplicity of the treatment. In brief the method consists of four steps: (1) making a pilot test series with varying water-cement ratios; (2) plotting the strengths of 28-day cylinders against those of companion cylinders broken at an earlier age; (3) fitting the data with a straight line passed through the point defined by the averages of the two series; and (4) using for subsequent tests the numerical value of the slope of the line as the coefficient by which an observed departure of an early age test from its mean is to be multiplied to obtain the predicted departure of the 28-day strength from its mean. Experience shows that these coefficients (slopes) vary from about 0.5 to 2.0, the exact value being dependent on the particular job.

Graph A, Fig. A, shows a re-plot of data from the author's Fig. 3 with a straight line fitted through the point defined by the 7- and 28-day average strengths. Its slope is 1.13. Graph B shows a straight line similarly fitted to field control data from another job with tests extending over a 2-year period‡; its slope is 0.65. On the job represented by Graph A, 7-day concrete testing 100 lb. above its average would be expected to yield 28-day concrete testing $1.13 \times 100 = 113$ psi. above the 28-day average. For the job of Graph B, for the same 7-day strength change, the predicted deviation at 28 days would be only 65 psi. These simple prediction coefficients are hidden within the author's values for

*ACI Journal, Apr. 1945; *Proceedings* V. 41, p. 493.
†Civil Engineer, Bureau of Reclamation, Denver, Colo.
‡Each plotted point from averages of the 28- and 7-day cylinders of two batches made 3 days apart. W/C = 0.58.

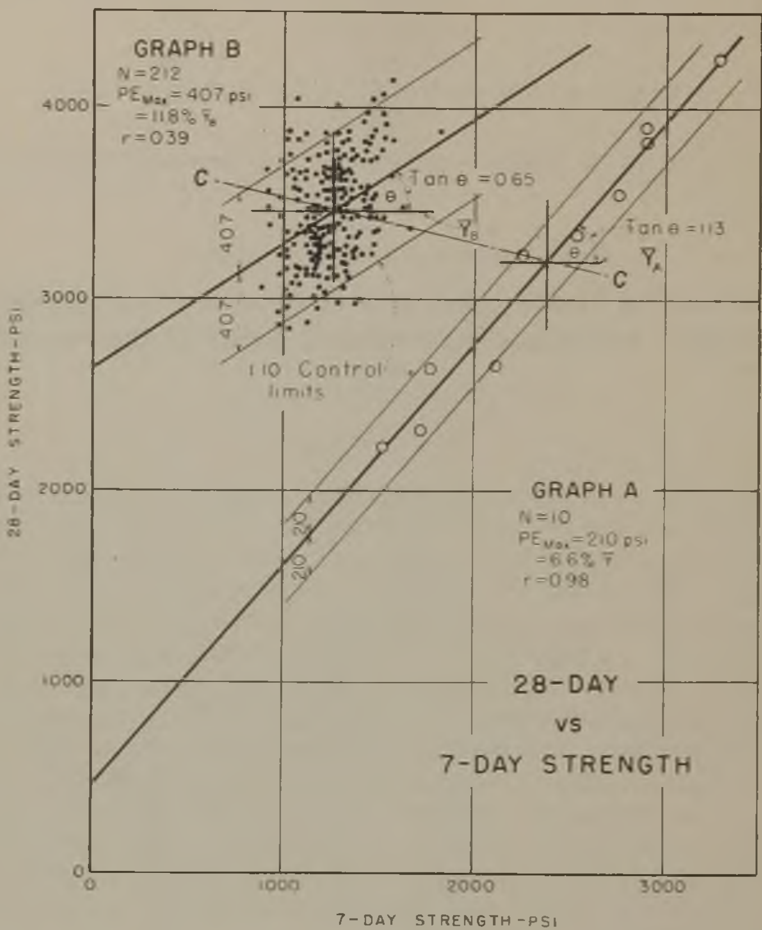

Fig. A

K and for the coefficient of *C*. The product of these two, taking their values from the author's Table 7, is $512 \times 0.00221 = 1.13$, the slope of the line of Graph A.

Although the paper goes into a number of alternatives for refining this method, such as using a weighted mean of 3- and 7-day strengths as the independent variable, or using a curve to fit the data, it is believed that a study of the results from a large number of tests from any job will result in a plot similar to that of Graph B. With limited data of the type the author presented in Fig. 1-A in which only a single variable such as water-cement ratio controls the variation, a low prediction error of ±6.6 percent (1:10 chance) makes the method look encouraging. But for Graph B, where tests cover a considerable time, the prediction error of

±11.8 per cent is but little less than the error which would be obtained by assuming the average 28-day strength as the predicted value regardless of the indication of a 7-day result.

That the large error in prediction for Graph B is not the fault of the mathematics of the author's method is seen by the fact that whereas his theory calls for 10 per cent of all predictions to be outside the error limits, in Graph B with 212 tests, 9 per cent or 19 tests fall outside, a satisfactory verification of the statistical theory.

The method's weakness lies rather in the fact that it is based on only one variable whereas many variables control the relation between late and early strengths. These variables are not adequately represented in a short series of tests. They include inadvertent changes in fineness of cement, mixing temperatures, and chemical differences in the clinker, each of which can be controlled only within certain practical limits.

For example, the effect of a variation in the chemical composition of the cement of Graph B, Fig. A, in the direction of that used for the job of Graph A, may be seen by glancing at the dashed line C-C connecting the two averages. Under such a condition, an increase in 7-day strength would be reflected by a decrease in 28-day strength. Since the slope of a line expressing 28-7 day relationships can only indicate average results, if chemical variations are present it must be flatter than would be the case for variations produced by changed water-cement ratios alone. In the writer's opinion, no method for forecasting strengths can be expected to be of much practical use unless it includes in its basic data, several independent variables.

For the above reasons, it is thought the author's method of prediction will find its greatest use in laboratory work where unwished for variables may be controlled, or where their effect can be studied by an extension of the author's method involving multiple correlation. Used in this way it is undoubtedly an extremely effective tool. However the author's discussion of "probable error," together with his Table 11, needs some extension before his implication can be accepted that 10 or 12 companion tests are adequate to determine the probable error within a margin of 10 per cent. It is believed that this latter figure, which is determined from a limited number of tests, refers to the fact that on the *average* the computed standard errors are too low and need to be increased by the amounts tabulated in Table 11 if bias is to be avoided.*

Of greater importance than the bias errors of the author's Table 11, is the reproducibility of the "maximum probable error" if computed from

*A more extensive table of correction factors is given in A.S.T.M. Manual on Presentation of Data, Supplement B, p. 50, Table I. The values of C_2 are coefficients by which standard errors computed from a limited test series must be divided to give the unbiased result. Values obtained by this method agree approximately with the author's value.

different test series from the same job. Approximately its per cent error is given by $100 \frac{1}{(2N)^{1/2}}$ where N is the number of tests. For 10 tests its standard error is 22 per cent, or 36 per cent for a 1:10 chance. One would hardly be warranted in changing a mix because a predicted 28-day strength fell below a limit which in itself had 1 chance in 10 of being 36 per cent too high. Only a considerably larger number of tests than 10 will give values of sufficient precision to be used for concrete control.

Finally the writer wishes to raise the question, "Why on a job do we need to make, or to predict the strength of, so many 28-day tests?" Are we not, having made preliminary trial mixes and having set up a control for our materials, as sure of what our strength will be as we are of the lusciousness of mother's pie? Surely we don't need to bite into it to know it is going to be good, provided we have not fallen down in supplying first-class materials. Would not a part of the effort currently spent on compression testing yield more profitable results if it were applied to a more regular and systemmatic control of the uniformity of the manufacturing processes all the way from the cement mill to the finished structure. Lack of control discovered along the assembly line leads to action before damage is done; lack of control discovered 28 days after concrete has grown into a structure only leads to a postmortem and the burial of the corpse. Do not averages of 25 to 40 tests at varying ages up to a year provide all the precision needed for an autopsy, an autopsy principally of benefit to the next job? The writer hopes the author in his closure will comment at least briefly, on these points.

AUTHOR'S CLOSURE

The writer directs attention to a statement on page 494 of the paper: "Parallel specimens must be used in this method. These are defined as concrete specimens of approximately identical mix, type of cement, aggregates, specimen dimensions, and curing."

Mr. Wing does not state, and the writer does not know, whether the specimens represented in Graph B are parallel specimens. From the results, the presumption is that they are not parallel specimens.

Mr. Wing takes the position that variables such as changes in fineness of cement, mixing temperatures and chemical differences in the clinker cannot be represented adequately in a short series of tests. The writer's experience in estimating 28-day strengths over a period of four years, involving hundreds of tests on 3,000-lb standard concretes, and more than 1500 tests of 5,000-lb lightweight concretes, indicates apparently that the above variables can be represented by the condition of parallel specimens.

Mr. Wing believes that the method will find its greatest use in laboratory work, and that, used in this way, it is an extremely effective tool. This conclusion is surprising, inasmuch as all the data presented in the paper, and all the estimates made by the author, have been obtained from job specimens.

Mr. Wing questions the acceptance of ten parallel tests as adequate to determine the probable error with a margin of 10 per cent. He believes that the latter figure refers to the fact that, on the average, the computed standard errors need to be increased by the amounts given in Table 11 if bias is to be avoided. The writer is willing to accept Mr. Wing's opinion, inasmuch as both statements amount to the same thing in practice.

The writer agrees with Mr. Wing that too many 28-day tests are made on a job, and that the 7-day tests should, in general, be sufficient. Unfortunately, the job specifications usually require 28-day tests, and there is very little likelihood that engineers, architects, or specification writers could be persuaded to eliminate the requirement for 28-day tests, unless they were convinced that a reasonably correct picture of 28-day strengths could be obtained by other means. To provide such a method was the principal reason for writing the paper. In addition, a semi-automatic means of evaluating concrete control is thrown in as a by-product.

In closing, the author agrees with Mr. Wing regarding the essential simplicity of the method presented, inasmuch as it can be used with facility by anyone able to read to three places on a slide rule. However, the method is valueless unless the requirement for parallel specimens is strictly observed.

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7400 SECOND BOULEVARD, DETROIT 2, MICHIGAN (Supplement) November, 1945

Discussion of a paper by Bertin, Di Stasio, and Van Buren:

Slabs Supported on Four Sides*

ERRATA

Part 1, Par. 3
The fifth line should read:
"in *two-way* construction in either direction are obtained."

Fig. 2
Curve marked "1-C (read down)" should also be marked below the curve:
"C (read up)"

Fig. 7
The second line of note near bottom of diagram should read: "span *b* both ends free" instead of "span *a* both ends free."

Table 9
Sub-heading ending "-End Shear", should read:
"- Load for End Shear."

Table 5
Max. Pos Moment under L^2, last line "18," should read: "18^2".

By WILLIAM C. SPIKER†

Well planned, clearly stated and referring to a very important subject, this paper should receive a great deal of attention.

The writer has checked the moments and loads per foot on beams and slabs reported in Tables 9 and 5. His figures agree almost exactly in nearly all cases with those in the tables assuming that the beams do not deflect.

The basis of the writers method is as follows:

The distribution of any load on any point on any slab is inversely in proportion to the total deflection of that point under any load calculated along the various routes which pick up some of the load as it travels to the columns. The distribution of the total uniform load on any panel is directly proportional to the distribution of the load on the one square foot of the panel at the point of maximum deflection in the panel, including the deflection of the beams if there are any.

*ACI JOURNAL, June 1945; *Proceedings* V. 41, p. 537.
†Consulting Engineer, Atlanta, Ga.

Practically the only problem is to calculate the maximum deflection. If the beams do not deflect and θ at the slab supports is the same for both ends of both slab strips which contain the point of maximum deflection the distribution is inversely as the fourth power of the two spans at right angles to each other. However, the writer recommends the deflection formulas contained in "Statically Indeterminate Stresses" by Parcell and Maney and that a complete tabulation of coefficients and distances "x"—distances from supports to points of inflection—be made if one has a great deal of calculating of two way slabs to do.

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PROCEEDINGS OF THE AMERICAN CONCRETE INSTITUTE

VOLUME 41—1945

From JOURNAL OF THE AMERICAN CONCRETE INSTITUTE, Vol. 16, Sept. 1944, to June 1945
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This is an Index of:

Original contributions to the JOURNAL OF THE AMERICAN CONCRETE INSTITUTE—papers, reports, discussions by subject, title and author.

For the convenience of the reader who is referring to JOURNAL issues rather than to Bound Volumes (in which discussion is assembled following the paper discussed) there is a reference to JOURNAL issue in which discussion appeared. This reference to discussion is by supplementary page numbers. If the last page of a paper is 28, the first page of the discussion, published later, is 28 - 1.

Important subjects are classified and indexed under approximately 30 main headings each one appearing in its proper alphabetical order in bold face capital letters—as for instance, ARCHITECTURAL DESIGN and other subjects classified under this head, are indented. These samples do not apply fully to any single year's index.

In general, key words to important subjects appear in alphabetical order in addition to the general classification—as for instance "Admixtures" and "Aggregates" each referring to MATERIALS AND TESTS under which all allied references appear, indented. Authors' names and original titles of papers appear in bold face type in proper alphabetical sequence with the subjects, with references to their contributions.

Specific data on Beams are so indexed by reference to ENGINEERING DESIGN or TESTS OF MEMBERS AND COMPLETED STRUCTURES thus avoiding an oversight by the searcher of important allied data.

The complete title of each paper is indexed. This title is repeated in full following the name of the author in its proper alphabetical order. Numerous cross references to parts of the subject matter covered, do not always indicate the full scope. This may be found by looking up the item referred to under the name of the author or the title of the paper.

The readiest use may be made of this index by gaining some familiarity with the main classifications as follows:

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BRIDGES
BUILDING CODES
BUILDINGS
BUILDING UNITS
CENTRAL MIXING
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A New ACI Proceedings Volu
November Supp

Title Page, Contents, Closing Discussion and Indexes

—How to Assemble

The regular November Supplement is sent to all ACI Members and all non-member subscribers with the November JOURNAL each year. It is the answer to the scores of requests that come to the Institute every summer, autumn and winter for Title page, Contents and Index to the JOURNAL volume which is otherwise completed with the issue for the previous June.

When the JOURNALS for September and November of *last* year and the issues for January, February, April and June of this year are pulled apart (after removing the wire stitches) and the covers, "masthead" (two pages just inside the front cover of each JOURNAL issue) and the *News Letter* sections are all discarded, the remaining parts may be assembled with the parts of the November Supplement (after discarding its cover) to complete a new Proceedings volume

Start the assembly with Title page and "Contents" which together make up the first section of the Supplement and follow with the first section (the first subject unit, whether paper or report) from the first issue of the volume of the previous September and so on, following the order of listed items as shown in the Table of Contents, and being careful to insert sections of discussion immediately following the papers or reports discussed. Don't be confused by blank pages; count them as though they bore page numbers consecutive with the last numbered page of a subject unit. Blank pages are necessary for several reasons: to make possible an orderly assembly of JOURNAL subject units with subsequently published units of discussion and to provide separate prints of reports and papers from the same press run which provides the JOURNAL issues. Discussion units bear supplemental series of page numbers identifying them with the last page of the original matter discussed.

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(Continued Inside Back Cover)