This indicates that the amount of energy lost in deforming the concrete plastically is roughly proportional to its strength while the elastic energy stored in the concrete and the machine increases much more rapidly than the strength. At maximum load, the machine potential energy is about three times that in the concrete.

No definite data are available regarding the amount of energy required to produce failure after the maximum load is reached; but since the ultimate strains appear to be smaller for stronger concrete, the energy required for destruction probably does not increase as rapidly as the strength. This indicates again why the strong cylinders appear to destroy themselves near maximum load while the weaker ones do not.

The writer will not attempt to discuss the plasticity ratio method in detail but he thinks that it should be based on a better approximation of the actual stress-strain behavior if it is to satisfy the needs of research workers and justify the extra elaboration due to the use of both the elasticity and plasticity ratios.

Since both these ratios are empirical functions of the concrete strength, they can both be eliminated for practical design purposes and simple formulas can be written involving only the concrete strength and the steel strength in addition to the necessary dimensional quantities. It should be possible to develop the very much simpler plastic theory* so that it will be adequate for design. Research directed along that particular line is greatly needed and the writer hopes that the American Concrete Institute will sponsor such a project as part of its postwar program.

By H. J. GILKEY†

INTRODUCTORY REMARKS

In his introduction the author states, "In this paper there is described a concept of the action of concrete in compression which permits the derivation of formulas for the ultimate resistance of beams." The paper and its companion bulletin (9) are frankly efforts to explain in mathematical vernacular the oft-observed fact that reinforced concrete beams always develop a greater ultimate bending resistance than that calculated by the conventional elastic theory and assumptions of design. As such, they are sequels to, and an extension of, a theory of flexural analysis which is of European origin (Saliger and others, see author's references No. 1, 2, 3) and which has been actively promoted in this country by C. S. Whitney (references 4 and 4a). The approach is similar to Whitney's

*See reference 4a and Mr. Whitney's discussions of references 5 and 12, p. 584 - 25.
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and is virtually identical with Saliger's up to the introduction of the "plasticity ratio." As implied by the title, it is the plasticity ratio concept and use upon which the justification for the paper is apparently based.

It is with the utmost reluctance that the writer undertakes to criticize this paper adversely in several important respects. He makes the following allegations:

(a) The paper is speculative in its background assumptions and is not the rigorous analysis one has a right to expect from such a pretentious and involved treatment. Some of the implications are highly misleading.

(b) The paper is apparently incomplete; there has evidently been considerable "behind-the-scenes" manipulation, the results from which the reader is expected to accept on faith; results which cannot be verified by anything to be found within the paper.

(c) The paper purports to establish a linkage between the *compressive* stress-strain diagram for concrete and the behavior of concrete beams, thereby augmenting and perpetuating a false concept of concrete compressive stress-strain behavior which has been widely circulated and accepted through the work of Saliger.(1)

Before starting the general discussion of contentions (a), (b) and (c) it may be well to call attention to an evident discrepancy in the definitions given for "plasticity ratio." In the synopsis on p. 565 it is defined as, "the ratio of the plastic strain to the total strain at rupture of the concrete." On p. 566, "A measure of the extent of the plastic action is given by the ratio of the plastic to the elastic strain and is called the 'plasticity ratio'." These two definitions appear to be mutually incompatible. According to Fig. 3(b) the former definition seems to be the favored one.

DISCUSSION OF THE CRITICISMS OFFERED

In studying this paper one intuitively assumes that Fig. 1, 2 and 3 are based on a solid background of carefully generalized data from actual compressive tests and that such figures as 11, 12 and 13 constitute the "proof of the pudding;" that they are really final checks against independent tests of beams which demonstrate the overall adequacy of the analysis, proving, among other things, that the slight liberties taken, in squaring off the mosque-like figures of 3(a) to construct the near-gothic forms of 3(b), were negligible in their effect on the accuracy of the treatment.

Evidently, this is not the case; Fig. 1 is apparently the only figure in the paper that so much as makes a pretense to being based on compressive tests. Incidentally, even Fig. 1 is formalized and somewhat overidealized. As the writer has pointed out elsewhere, (ref. 13, p. 10), the strains at the ultimate have no such definite values as Fig. 1 appears to show. Generally speaking, the writer's observations indicate that instead of all comparable concretes reaching their ultimate loads at identical strains, the stronger concretes seem to have somewhat greater strains at the ultimate than do the weaker ones. Moreover, referring to the last sentence on p. 566, the author might be surprised to note how nearly identical the shapes of curves may often be for concretes of widely varying strengths (ref. 13, p. 27-29). Suffice it to say that Fig. 1 isn't a bad representation, in the writer's estimation, and that there are at hand much more important items than that of quibbling over any possible minor discrepancies there.

Fig. 2 is, as the author has evidently recognized, nothing more than Saliger's concept of a type of compressive stress-strain diagram which would, if true, explain in terms of the compressive stress-strain diagram, the excess strength of reinforced concrete beams. That the author accepts Fig. 2 as nothing more than a hypothetical concoction seems reasonably clear from the legend, "Possible stress-strain diagram for concrete at strains approaching rupture—Saliger." As an additional indication that the author was fully alive to the weakness of what purports to be his take-off, one may quote his own words on p. 567 where he discusses strains beyond the ultimate, "The exact character of this portion of the stress-strain curve for concrete of any strength is not well established. In fact, test data are very meager and are likely to be greatly influenced by the speed of testing."

For an investigator thus clearsightedly to post his own stop light and then ride brazenly through it with an elaborate development, the validity of which rests squarely on the validity of his premise is a thing scarcely to be condoned in a scientist, even though the writer does feel that, in fairness to the author, there are in the present instance certain extenuating factors which should not be overlooked.

The Saliger hypothesis had been advanced and published by a widely recognized authority and its validity had not been seriously challenged during the several-year interim; moreover, it has seemed to square unusually well with the available data from flexural tests. While not having been verified experimentally from the bottom up, it has appeared to have excellent verification from the top down.

Although the writer is convinced that the Saliger diagram is emphatically not representative of the behavior of any concrete under axial compression, he agrees that there is every reason to believe that it constitutes an excellent qualitative portrayal of the behavior of the concrete on the compressive side of a beam. In other words, the writer believes that Saliger and his successors have simply lost sight, for the moment, of the fundamental difference between the respective situations in a uniformly axially loaded member and a member in flexure where all fibers are not equally stressed and there are opportunities for redistribution. If, for example, the author's caption for Fig. 2 had been, "Possible (or even probable) stress-strain diagram for some of the more heavily stressed fibers of a beam at loads approaching the ultimate resistance of the member," the author's take-off, while still speculative, would have been much less vulnerable.

Much of Saliger's backlog of evidence, such as it is, seems to date back to relatively early tests when techniques for controlling rate of loading and for securing good observations on strains were in an elemental state. Even today it would require some of the best of modern testing and observational equipment and carefully planned, skillfully executed tests to secure significant evidence on the post-ultimate compressive behavior of concrete. It is not surprising, therefore, that Saliger should have worked backward from the beam-test strength results to evolve an imaginary compressive stress-strain diagram which would account for the observed excess beam strengths. He failed to take tangible cognizance, however, of the fact that the stress-strain diagrams for flexure and compression are bound to differ in certain vitally fundamental respects; he over-shot his mark badly in attempting to carry his explanation back to the compressive stress-strain behavior, as have his successors and disciples.

In making the transition from Saliger (Fig. 2) to the representations of Fig. 3 the author states on p. 567, "For the purpose of explaining the behavior of concrete in a flexural member, a typical set of stress-strain curves extended to rupture for gravel concrete is assumed as shown in Fig. 3a." Here at the most vital juncture in the whole development the technical trail has become very, very dim. This set of curves is supposed to be *typical* of what? A reader would properly expect them to be the generalized results of experiments which are themselves readily available for checking the reasonableness of the generalization. If compressive tests are not their origin (and apparently, for the extensions beyond the ultimate, they are not) then certainly their source and derivation should be carefully and fully indicated, for, after all, what is the significance of any subsequent development that might be based on such an unusual set of diagrams of mysterious and wholly unexplained origin? After having previously indicated the dearth of evidence on the compressive stress-strain behavior beyond the ultimate, on just what factual basis is the author able to show such a set as typical? How did he decide just how far in terms of strain, each strength of concrete was able to stand up under it's ultimate stress?

Apparently, the "typical" post-ultimate compressive stress-strain diagrams of Fig. 3 never saw a compressive specimen but have been guessed in purely by a trial and error technique, not by working forward from Saliger (Fig. 2), but by working backward from the check series of Fig. 11, 12, 13 and 14. In other words, Fig. 3, instead of being the take-off, really appears to be the end point of the analysis. The real sequence in the preparation of the paper appears to have been (a) the qualitative acceptance of Saliger's premise followed by (b) a working backward from the composite data of Cox, Columbia University and Slater and Lyse to evolve by trial a set of fictitious "typical" compressive stress-strain diagrams resulting in the architectural effects of Fig. 3.

If the foregoing is correct, what appear to be such excellent checks are not checks at all since the nicely plotted points on Fig. 11, 12, 13 and 14 represent nothing more than the closure of a manipulative circular sequence of operations; a return to the starting point. This is not to imply that a reasonable check with results from good independent series of beam tests is not to be expected; it is merely to point out that such a check is not a part or parcel of this analysis.

To conduct extensive analyses from other investigators' data is always a difficult and somewhat hazardous undertaking but in a project which is already such a heterogeneous mixture of the speculative, the empirical, the mathematical and the mystical, it is doubtful if the ingenious device of supplying hypothetical strengths on the basis of the recorded cement-water ratios (p. 578) has weakened the development in any essential respect (in spite of the great range of cement-water ratio vs. strength relationships that exist for different cements and mixtures.

COMPRESSIVE VS. FLEXURAL STRESS-STRAIN BEHAVIOR

Notwithstanding the keen insight displayed by Saliger and his successors with respect to what occurs within a stressed beam as the proportional limit of some fibers is exceeded and/or ultimate stresses are approached, or even passed, all of the writers seem to have displayed a common weakness in their attempted linkage of compression and flexural phenomena. They have seemingly failed to take cognizance of the fact that in the axially uniformly loaded member, "as goes the fiber, so goes the member"; when one fiber reaches its maximum resistance, all do; the failure of the member is coincident with the failure of a fiber.

In the case of the flexural loading of a member having a plastic range of stress, there are reserves that are brought into action as needed. An overloaded fiber can deform plastically still offering substantial resistance while other lightly-stressed fibers are brought into service to take on increased strains and correspondingly increased stresses. The ultimate resistance developed by the member is not developed simultaneously with the ultimate resistance of some fibers. Various readjustments, coalitions of over and under-stressed fibers which add to the sum total resistance of the member, continue to occur long after some parts of the cross-section have reached or even passed the peak of their respective resistances.

The redistribution within the beam and the probable manner in which it accounts for the reserve strength found in beams is discussed, illustrated and dwelled upon by Saliger[1], Whitney[4 and 4a] and others. Some of the elementary textbooks on strength of materials show qualitatively the similar action which takes place in plain concrete, stone or cast iron flexural members to give them moduli of rupture as much as twice the ultimate tensile strengths of the materials.

It needs to be continuously recognized and realized, of course, that there is no magic in these apparent differences between flexural and axial strengths. The redistribution that follows the non-proportional action just discussed simply produces departures from what the conventional straight line formulas of flexure assume to be happening, giving apparent rather than true values for computed ultimate stresses. That the well-known hyper-elastic compressive behavior of materials in flexure should have been carried over to the case of axial compression is obviously due solely to lack of thought rather than lack of understanding. Analogy, valuable tool that it is, can be, and often is, blindly misapplied by the best of us.

At first thought one might raise the question, "Why the urge to explain flexural behavior in terms of the compressive stress-strain diagram anyhow?" The answer appears to be two-fold and rather simple. First, the compressive stress-strain diagram as determined by the regular short-time test is the recognized medium for evaluating the structural properties of concrete; it supplies the values used to describe the elastic and ultimate strengths, the stiffness, elasticity, extensibility, resilience and toughness of the concrete. Flexural as well as compressive, bond and diagonal tension design stresses are all expressed as percentages of the compressive strength as determined by the conventional test cylinder and established test procedure. Second, to determine what the actual stress above the proportional limit may be at any specific part of the flexural cross-section is not a simple matter. It does seem strange that with a definitely computable bending moment at any section, it should not be possible to determine readily and simply just how the total compressive and tensile stresses (equal to one another as a basic fact of statics) are distributed over their respective portions of the cross-section. The problem of breaking down the known value of the bending moment at any section into the correct resultant and/or distributed forces and arm of the resisting couple is in its difficulty and uncertainty, not unlike that of attempting to reproduce the force system within the wall, or other support of a cantilever beam (or for any other indeterminate stress situation). The fact that in such situations the product of the resultant stresses and their arm is definite and known doesn't shed much light on the questions of distribution and redistribution of stress within the beam or within the wall.

With this much for general comment, it is now desirable that the compressive test phenomena be scrutinized in some detail to see why the Saliger type of short-time compressive stress-strain diagram can scarcely be accepted as a correct portrayal of what occurs.

COMPRESSIVE TESTS

On the basis of a great number and variety of concrete compressive tests in which strains were observed and recorded all the way to the ultimate load the writer feels qualified to speak with some assurance on compressive testing. Just prior to the ultimate, when the load is practically at a standstill, the strain continues to increase with the travel of the testing head. As the ultimate is attained and passed, the load drops off rapidly for a time while strains increase whether the testing head is held stationary or is allowed to continue the gradual rate of descent at which the ultimate load was reached. With the head in motion the compressometer dial continues to record rapid increase of strain under rapidly decreasing load followed by almost immediate collapse of the specimen. There is no such bearing up under a load approximating the maximum as is shown in either Fig. 2 or Fig. 3.

In Fig. 1 of Whitney's Am. Soc. C. E. paper (ref. 4a, p. 254) from the bachelor's thesis of Kiendl and Maldari[(14)]) are shown some actual data, of sorts, on stress-strain relationships beyond the ultimate. These are doubtless correct portrayals of what occurred in spite of the fact that they are, "highly unusual stress-strain curves for concrete cylinders" as is pointed out by Hadley (ref. 15, p. 292). It is remarkable that with a continuously descending testing machine head the specimens should have held together.

These observations were on 28-day standard cured 6 by 12 in. specimens tested in a Riehle 200,000 lb. (presumably lever type) testing machine with the head traveling continuously at the (idling) speed of 0.06 in. per min. (Actual speed under load, undoubtedly less than 0.06 in.

per min.). Readings were continued to the stage where damage to the specimens precluded further observations.

Granting the validity of the data, the writer fails to see that they have any significance as an index to the supporting strength of an axially loaded member. The member has passed its ultimate resistance and if the head of the testing machine had not been restrained from following down, the specimen would have collapsed the instant the ultimate load was passed.

By reversing the testing machine as soon as the ultimate load is reached the writer has found by test that a compressive specimen can often be reloaded to a new ultimate as high as 90 per cent of the original ultimate. As a practical matter, however, that 90 per cent just wouldn't be enough to support the 100 per cent load that is presumably resting on the specimen. Even a 99 per cent residual resistance isn't enough. (By way of information, it may be stated that just a few successive reloadings to successive new—but progressively lower—ultimates, results in complete disintegration but this is beside the present point.) The point being made here is that, in terms of an axial load actually resting on a member, effective resistance ceases the instant the ultimate is passed regardless of whether that ultimate be the ultimate for a cylinder, the somewhat higher ultimate for a cube or the lower ultimate for a slowly conducted test. Just how hard the failing specimen happens to be pushing upward against the descending load doesn't matter if the upward push is less than 100 per cent of the propelling downward push.

Once the ultimate is passed, the axially loaded member is on its way out (and fast, if the load is free to follow down as for a load resting or hanging on a member). Whether the specimen accepts its ultimate fate (collapse) cheerfully, offering only token resistance or whether it remonstrates and drags its figurative feet every fraction of the way is beside the point; it is doomed from the instant the downward push exceeds the magnitude of the greatest resistance the specimen can muster. Resistance can cushion the fall but it won't prevent it. Even a freely falling body will exert an upward push against another descending body above it if the acceleration of the latter exceeds the acceleration caused by gravity. The Kiendl-Maldari data which, even for the conditions under which they were secured, supply little support for the Saliger assumption, are entirely meaningless with regard to having any significance with respect to the phenomena under discussion here.

Rather than striving to endow a uniformly loaded compressive cross-section with some fantastic super characteristic whereby a 75, 80 or 95 per cent upward push halts in midair a 100 per cent downward load, might it not be more rational to start from the premise that beams are distinctive because they are beams, endowed as all plastic beams are endowed with a capacity for stress redistribution?

This is, of course, exactly where the author and his predecessors have landed as the second major step in their analyses. Why not let it go at that? Why not drop the erroneous pretense that Fig. 2 and Fig. 3 are valid representations of the stress-strain behavior of concrete in axial compression? Use the equivalent of Saliger's assumption, if desired, but with some such legend as, "Possible stress-strain diagram for some of the more heavily stressed fibers of a beam at loads approaching the ultimate resistance of the member."

CONCLUDING COMMENTS

In spite of the rather vigorous exceptions taken to certain primary aspects of this paper, it should be clearly noted that the scope of the criticism is distinctly limited.

The excess strength of beams over that given by the conventional straight-line analysis is an observed non-controversial fact, recognized many years ago even before the general adoption of straight-line in preference to parabolic or other type of design formula. Any valid analysis that places the extent of the excess strength to be expected in the easily computable category is all to the good and the objective sought by Saliger, Whitney, the author, et al is certainly to be commended rather than criticized.

Exception is not taken to the general objective; nor does the discussion concern itself with the correctness of the end-point reached by any of these workers, or the relative merits or possibilities of usefulness of the plasticity ratio approach in comparison with the preceding contributions; moreover, the writer is not taking sides on whether or not the established straight-line procedure should be discarded completely, in part, or retained. (The author (p. 565) likewise professes neutrality on this point.)

Because the observed excess strength of flexural concrete members is inherent in the compressive behavior of concrete in a beam, it has been an inevitable sequel of established methods that our conventional "balanced" reinforced beams are never balanced; that they are always disproportionately strong in compression. In the interest of economy (in normal times when steel is not a war scarcity) and of common sense, that discrepancy needs to be corrected. To remedy much of the current *unbalance* of "balanced design" one doesn't have to discard the straight-line design technique. As pointed out by Hadley (ref. 15, p. 294), simply increasing the permissible concrete compressive stress used in flexural design would go far toward rationalizing current practice without any upsetting of established processes. Whether the added precision potentially possible via the Whitney or Jensen route is enough to be a controlling factor is a question to be settled in the light of comparative studies and the collective judgments of specification-making groups.

The objections offered to this paper are not that its aims are at fault or that its objectives are unworthy; in a sense the right thing is simply believed to be being done in a wrong way. As indicated at the beginning; the premise is alleged to be unsound and the treatment is accused of being devious and mystical. Moreover, like its predecessors in the field, what is believed to be an entirely erroneous concept of standard stress-strain behavior is accepted and heavily built upon. Such faults are inexcusable in an engineering treatise, no matter how commendable the aim or how nearly correct the ultimate answer may be.

By deleting all allusions to compressive stress-strain behavior (except in beams), by indicating clearly that representations of the compressive diagrams of Fig. 3 are wholly empirical and apply only to the compressive action that *may* exist within a beam, and that the exhibits of Fig. 3 are in essence the end point of the paper rather than the beginning of an analysis, the writer believes that the author could greatly strengthen his paper and make it something of an asset rather than a distinct liability to the advancement of concrete understanding.

Perhaps some of the criticisms are unwarranted and perhaps others are unduly severe; in some respects the writer may be entirely at fault through his own ignorance and failure or inability to understand. The closure can be relied upon to set the record straight with respect to all such. The writer hopes, however, that in the preparation of his closure the author will not restrict his efforts to a mere attempt at vindication. Right or wrong, the author's path has in places been devious and difficult to follow; undoubtedly some of the difficulties encountered by the writer are representative of points that have puzzled and will continue to puzzle others; points that need to be cleared up rather than waved aside or covered up. Some of the questions raised call for answers, real answers and explanations; possibly some admissions. The writer hopes that the closure will be made to supplement both the paper and the discussion in such a manner as to clarify where haze obscures and correct where error exists, let face be lost where it may.

By K. HAJNAL-KONYI*

Mr. Jensen has made a very valuable contribution to the theory of reinforced concrete beams. It has been known for some time that the

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