*Discussion on Paper No. 6986**

Dynamic relaxation

by

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Professor Otter gave a short history of the development of DR from computer tidal calculations based on Professor Hansen's method and subsequently given in detail in Rossiter and Lennon's paper.[11] Mr A. S. Day, who had worked on the tidal problems had the idea that a similar formulation of the dynamic elastic equations would settle down to the static conditions of equilibrium and compatibility if a viscous damping term were included. At the same time he suggested that 'dynamic relaxation' would be a suitable name for the method.

85. Early in 1962, the Authors' firm were requested by the CEGB to calculate the stresses in the Oldbury prestressed concrete reactor pressure vessel, and the method suggested by Day was developed and used both for the model and the prototype vessel with gratifying results. The calculation of temperatures and thermal stresses, and of the effects of the curved prestressing cables including friction as well as the gas pressure and gravity were all carried out successfully assuming elastic conditions.

86. Further work was undertaken for the proposed designs for the Wylfa pressure vessels and for the AEA advanced gas-cooled reactor vessel, and also for the concrete dam designs for the Arch Dams Committee of the Institution, and later for CERA. The latter work comprised both shell and continuum solutions.

87. The main virtues of Hansen's method for the tidal problem were its use of interlaced nets, and separated equations for continuity and energy and the ease of introducing the various types of boundary conditions. The progress of the tidal iterations was easily visualized. All of these were also true of the DR method as used for elastic calculations.

88. It was found later that a somewhat similar form of iteration had been suggested in 1950 by Fraenkel, but only for relatively simple forms of partial differential equation using the equations direct in their second order form and with non-interlacing nets. The second part of the Paper was more of an index of the papers in which mathematical justification of the method could be found, and Professor Otter was glad to know that there were mathematicians who were taking up the problem of making the basic mathematics of the method understood by engineers.

Dr R. E. Hobbs described progress made in the analysis of arch dams by DR since the preparation of the Paper. Using tensor methods, a general program had been developed which could deal with any dam whose faces were surfaces of revolution. This program had been used to provide solutions to several profiles very quickly.

90. Current research was concerned with the development of a program to deal with a completely general profile, and with the incorporation of valley flexibility into the analyses.

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* *Proc. Instn civ. Engrs*, 1966, **35** (Dec.) 633–656.

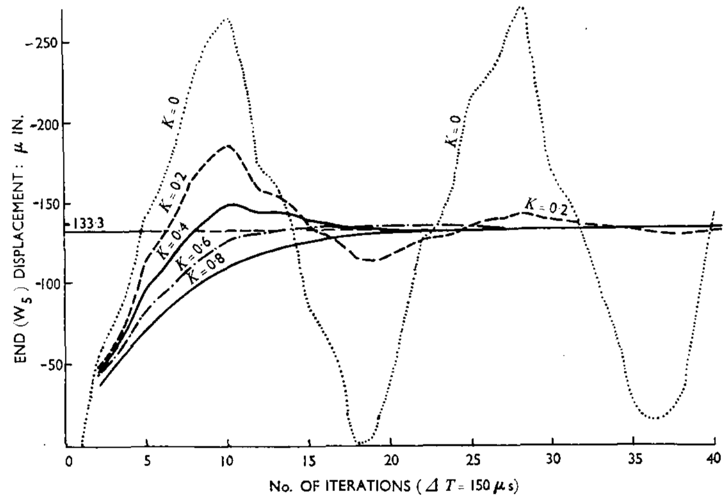
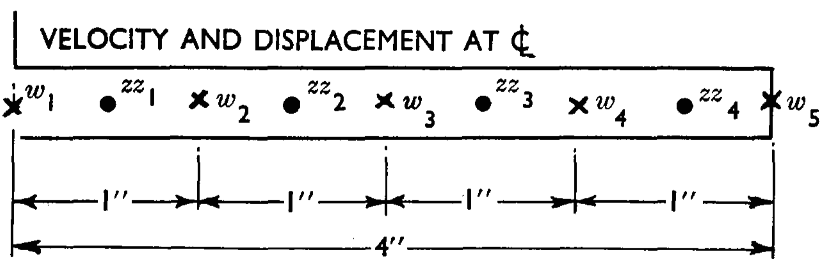


FIG. 18: DR EXAMPLE—AXIAL LOAD ON BAR; EFFECT OF DIFFERENT VALUES OF DAMPING FACTOR K

One dimensional example

Bar, 8 in. long, 100 lb/sq. in. at the ends, $E = 3 \times 10^6$ lb/sq. in. $\rho = g/12$ lbm/cu. in.



Two methods to determine approximately optimum damping:

1) Use natural period of end to end motion:

$$\text{Frequency} = \frac{\sqrt{E/\rho}}{16 \text{ in.}} = 375 \text{ c/s} = \omega_0/2\pi$$

$$\text{For } \Delta T = 150\,\mu\text{sec}, \quad K = 2\omega_0 \Delta T = 0{\cdot}71$$

(2) Study vibration for $K = 0$.

Half wavelength occurs in $9\Delta T$

$$\omega_0/2\pi = 1/18\Delta T$$

$$K = 2\omega_0 \Delta T = \frac{2\pi}{9} = 0{\cdot}69$$

Numerical stability criterion

$$\Delta T < \Delta Z/\sqrt{E/\rho} \quad \text{i.e. } \Delta T < 167\,\mu\text{sec.}$$

FIG. 19: ONE-DIMENSIONAL EXAMPLE

No. of iterations = 2										
Velocities are	0·		0·		0·		0·		−0·350	
Displacements	0·		0·		0·		0·		−63·	
Stresses are		0·		0·		0·		−188·9		−11·1
No. of iterations = 3										
Velocities are	0·		0·		0·		−0·330		0·094	
Displacements	0·		0·		0·		−59·		−46·	
Stresses are		0·		0·		−178·4		40·4		−240·4
No. of iterations = 4										
Velocities are	0·		0·		−0·312		0·178		−0·433	
Displacements	0·		0·		−56·		−27·		−124·	
Stresses are		0·		−168·5		86·2		−289·4		89·4
No. of iterations = 5										
Velocities are	0·		−0·295		0·252		−0·547		0·395	
Displacements	0·		−53·		−11·		−126·		−53·	
Stresses are		−159·1		126·9		−345·2		218·9		−418·9
No. of iterations = 6										
Velocities are	0·		0·318		−0·669		0·648		−0·871	
Displacements	0·		4·		−131·		−9·		−210·	
Stresses are		12·4		−406·1		366·1		−601·3		401·3
No. of iterations = 7										
Velocities are	0·		−0·535		0·936		−1·291		1·214	
Displacements	0·		−92·		37·		−242·		9·	
Stresses are		−276·6		388·4		−836·3		751·3		−951·3
No. of iterations = 8										
Velocities are	0·		0·832		−1·562		1·977		−2·226	
Displacements	0·		57·		−244·		114·		−392·	
Stresses are		172·4		−904·2		1074·9		−1518·3		1318·3

FIG. 20: DR EXAMPLE—AXIAL LOAD ON BAR; NUMERICAL INSTABILITY EFFECT ($\Delta T = 180\ \mu s$)

Dr A. C. Cassell discussed some basic features of DR. The effect of altering the viscous damping in the one-dimensional example could be seen in the graph of the end displacement (Fig. 18). $K_{cr.}$ was about 0·7, but reasonable efficiency of computation was achieved with a K of 0·4 showing that it was not necessary to know K with great precision. $K_{cr.}$ itself could be estimated from the natural period of the structure, or from a short run on the computer for $K=0$ (Fig. 19). Numerical instability was easily recognized, for example the effect of exceeding the stable value of $\Delta t = 167\ \mu s$ could be seen in Fig. 20 where 180 μs had been used. These characteristics of DR were a great help in eliminating programming errors.

92. Recent work included the application of DR to the full shell equations, when it had been found that inclusion of all secondary terms presented no difficulty, and its application to multiple skew slab bridges.

Professor A. J. S. Pippard (Past President) said that he was only able to judge the Paper on an empirical basis. The tide of engineering mathematics had been running so fast in recent years that in the whirl of matrices, computer technology, etc. he was left stranded and gasping for breath. However, he still felt entitled to express his admiration for the results the Authors had given and, *ipso facto*, for the method which had produced them. In doing this he was sure his university friends would not take it amiss if he added that the pleasure was all the greater since the responsibility for the approach described rested with a practising engineer. This interest of the practitioner in, and his contribution to, the study of a problem which many of his contemporaries and the vast majority of his predecessors would have regarded as of academic value only (which was tantamount, as far as they were concerned, to saying 'of no value at all', since 'academic' was a favourite term of abuse) augured well for a not too distant future when close alliance between theory and practice would be so normal that it would not call for comment in a discussion in this Institution. At present it was rather rarer than it should be.

94. It was in about 1957 that Dr Leliavsky had suggested to him that the Institution should appoint a committee for the study of uplift in dams—a pet subject of Dr Leliavsky. For a number of reasons Professor Pippard felt that such a proposal was unlikely to be received with enthusiasm at that time but it gave him the idea for the Arch Dam Committee and he was able to obtain the agreement of the Council to its formation. This Committee, with the generous support in its formative years of a number of consulting engineers, and the active co-operation of a number of university laboratories, had been engaged in studying the subject ever since, and widely different approaches to the many problems involved had been explored.

95. Professor Pippard had always been very alive to the difference between the real structure and what was described during the great gravity dam controversy at the beginning of the century, as the 'mathematician's dam'. However, he had believed that whether gravity or arch dams were in question, a solution for the theoretically perfect elastic structure was not only essential to a true understanding of the behaviour of the real one but, in the absence of a reasonably accurate theoretical analysis, it would be impossible to judge the value of any practical design method which might be proposed.

96. For this reason he had always looked on the work of the Arch Dam Committee as primarily that of establishing a yardstick by which proposals for practical design procedure could be assessed.

97. Since the Committee was formed, however, electronic computation had advanced so much and so fast that approximate solutions might quite possibly have become unnecessary. If this were true there was all the more reason that the mathematical basis of a programmed design (which could be used for practical design purposes) should be sound and well authenticated.

98. So it was with the greatest interest and with some surprise that he saw the close agreement between the Authors' results and those obtained by the trial-load method. He said 'surprise' because the assumption of inter-penetrating cantilevers and arches on which the latter method was based, while ingenious and practical, had always seemed to him to require more justification than it had ever received. The close agreement between the DR and the trial-load methods appeared to provide, at any rate to a great extent, some measure of substantiation or justification for the trial-load method.

99. Although he had not been concerned with the work of the Arch Dam Committee for some time, it seemed to him that the evidence now available indicated a satisfactory congruence of a number of methods, both analytical and experimental, for the treatment of one of the most interesting and difficult problems in modern civil engineering practice.

100. He had no doubt that the many individual workers in this field would continue to develop their ideas, but it looked as if the Committee as such should be able to conclude its main job satisfactorily and recommend an all-British design method which was readily programmed for practical application. Calculations of this type, backed by concrete model tests to destruction, which had proved so valuable both in Britain and (earlier) elsewhere, would enable designers to feel a degree of confidence in their work which had been hitherto unattainable.

101. He suggested that the Authors should apply their methods to the analysis of perhaps the most daring arch dam of all time—Malpasset. It would be of considerable interest and value to know the calculated stresses in this structure, on the assumption of truly rigid abutments.

102. In conclusion he would ask one question. Could the DR process be used for the actual limiting case when the Poisson's ratio was 0·5, as it was with india-rubber, or did it break down, like other analytical procedures, owing to the fact that $1-2\sigma$ became zero, so that the result was indeterminate? Had the Authors been able to overcome this difficulty?

Professor O. C. Zienkiewicz (University of Wales) said that the 'invention' of the calculus of finite differences and of diverse variational processes (such as that of finite elements) had solved *in principle* the engineering problem of dealing with two- and three-dimensional continua. He said 'in principle' as the reduction of unknowns from infinity to a finite number permitted the final problem to be written as a solution of a system of linear equations. Unfortunately the practical problems involved in solving the equations—the number of which was invariably large—took a considerable time to overcome.

104. The first step forward was that introduced by Southwell where a particularly humanized process of iteration, known as 'relaxation', allowed hand-computed solutions to be achieved. Even so the labour content for large problems[12] was overpowering. With the advent of computers the situation changed rapidly. The 'impracticable' became immediately possible. However, even here limitations of the present generation of computers soon became apparent and limits were soon attained.

105. At the present stage, therefore, a search for economy was being conducted. This economy related not only to computer times but also to programming time.

106. The problem, as shown in the Paper, clearly split itself into two not altogether unrelated parts:

(*a*) idealization (discretization) model,
(*b*) solution of simultaneous equations.

In the first we should search for the most versatile, simple and economical model capable of achieving a *desired accuracy* with a *minimum* number of unknowns; in the second, the optimum way of solving the resulting equation.

107. The Authors, by the simultaneous use of a *finite difference* model of low order, a choice of suitable variables and the introduction of a semi-fictitious physical phenomenon, had succeeded in a remarkable way in achieving good results to the two objectives. The second part of their Paper showed clearly that the two phases were recognized as being separable. Professor Zienkiewicz proposed, therefore, to discuss these *seriatim.*

Idealization model

108. The Authors chose here a low-order *finite difference* formulation. Some years previously, being faced with many practical engineering situations similar to those dealt with in the Paper, the speaker had decided to branch off into the direction of *finite element* formulation, and he would summarize why this choice had been made and outline some of the advantages and disadvantages.

109. The main advantages of the finite element model as compared with the finite difference one were as follows.

(*a*) Arising from a variational rather than differential approach certain general statements on bounds of solution could be made. Thus, though any solution achieved numerically was only an approximation, some statements about the exact solution could be made.
(*b*) Differential conditions on boundaries became integrated, and simple conditions on boundaries (which were of standard structural type) arose. The Authors achieved the same objective by a clever use of additional variables.
(*c*) As each element or subregion could be of an arbitrary shape it could follow the boundary as closely as possible and the speaker assigned different properties. In dams, where the transition from one material to another arose, this was of great importance.
(*d*) An irregular subdivision to economize on the number of unknowns could be used. This was difficult with regular finite difference meshes.
(*e*) By choice of suitable element properties any degree of approximation could be achieved at a penalty of complexity in formulation.

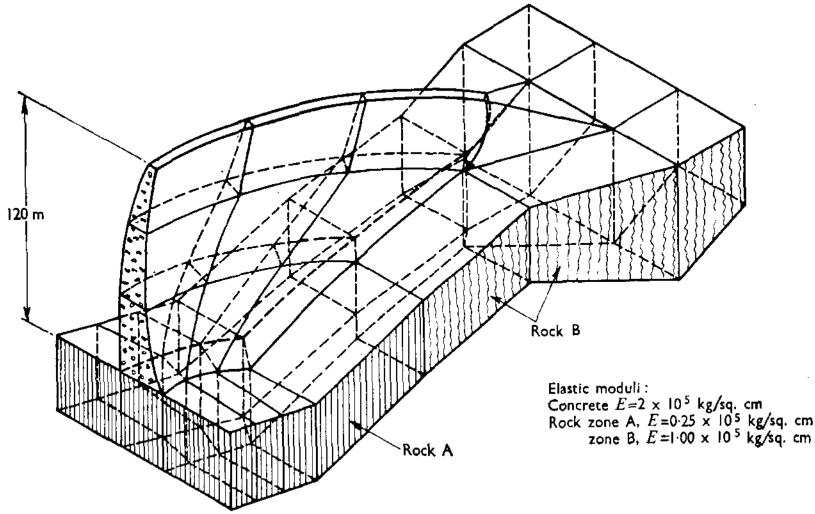


FIG. 21: A DOUBLY CURVED ARCH DAM WITH ITS FOUNDATION

(*f*) Once a program was written it was general and no change of co-ordinates was needed to deal with special situations. Thus a simple 'engineering' input was possible.

110. On the debit side there was, however, one important factor. The relatively complex expressions for element properties (such as stiffness matrices etc.) consumed some computer time (with complex elements up to one-half of the total time). The extreme simplicity of finite difference expressions for a regular mesh allowed the governing equation algorithm to be used at each step of iteration without the necessity of storing the coefficients (which in any case were repeatable).

111. Some of the finite difference simplicity, however, disappeared at boundaries and certainly if non-homogeneous problems had to be dealt with.

112. Concerning the accuracy for a given number of unknowns, the theoretical assessment had shown this to be identical for Laplace type problems if the crudest form of elements was used. With more elaborate elements an increase of accuracy would occur. For stressing problems only the simplest situations had been compared in detail.

113. The problem of the arch dam discussed in the Paper was also dealt with using a three-dimensional finite element of some complexity (60 degrees of freedom). With this element a solution very close to that given by the Authors had been achieved with as few as 600 degrees of freedom in total. Further, the program then written was applicable to all geometries. For a doubly curved arch dam, shown in Fig. 21, no difference of approach had to be used and similarly foundation conditions could be incorporated by extending elements into this. A very complex nuclear reactor vessel had been analysed recently with good results by using the standard program.

Solution method

114. The particular form of iteration used in the DR approach appeared, as stated in the Paper, to offer no advantage over SOR systems. The advantage of computer organization of storage was nevertheless a powerful one.

115. Professor Zienkiewicz had found that iterative solutions yielded most rapid solutions only when one load vector was considered. As more often than not several

loadings were specified, direct solution (in which parts were stored) was more efficient, subsequent loads requiring 10% (or less) time than the first one.

116. Clearly possibilities of combining some advantages of DR and of finite element idealization existed. It was a possibility which he hoped to look into in future.

Dr K. R. Rushton (University of Birmingham) said that the University had followed with interest the work on DR in regard to the solution of arch dam problems. They had recently carried out some basic work using the DR method and some of the pitfalls encountered by them might be of interest.

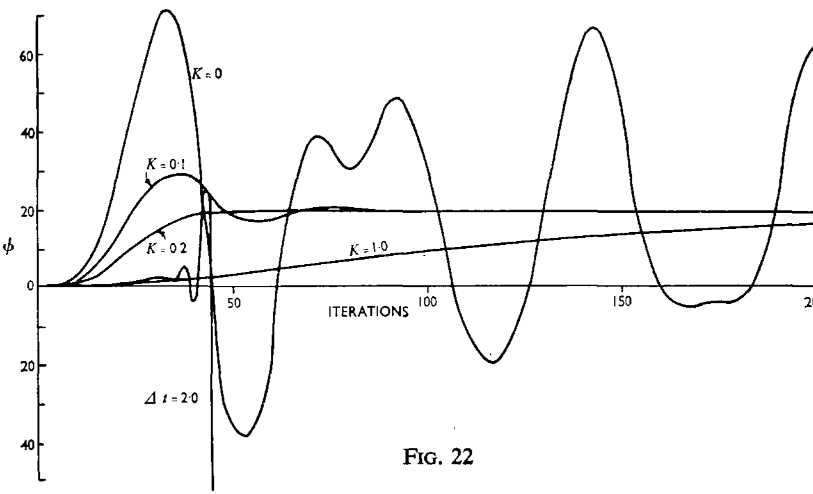


FIG. 22

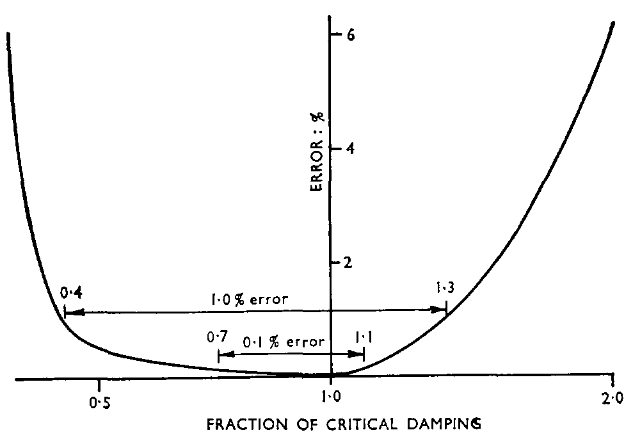


FIG. 23: ERRORS AFTER 100 ITERATIONS

118. They had worked mainly on the Laplace equation, solving problems concerned with wind tunnels. The DR solution of the Laplace equation could be visualized as an elastic membrane which had been plucked and allowed to vibrate. If one imagined that the air exerted a damping pressure on the membrane then the oscillations would be damped slightly, but if the membrane vibrated in oil then the oscillations would be damped far more. Fig. 22 illustrated the damping of oscillations with time; the air damping would be equivalent to $K=0{\cdot}1$ whilst for the oil damping $K=1{\cdot}0$. The dependence of the accuracy of the final solution on the choice of the damping factor was illustrated in Fig. 23. In this figure the maximum error at iteration 100 or beyond was plotted against the damping factor K. For the critical damping after 100 iterations virtually the correct solution to the finite difference equations had been obtained. Further, it was clear from Fig. 23 that it was preferable to underdamp, and with damping of only 70% of the critical damping, the error after 100 iterations was 0·1%. However, overdamping could lead to serious errors.

119. With DR it was sometimes difficult to know whether a correct solution had been obtained, but this could be checked by using the residuals. For example, if equations (11) to (19) were combined with the velocity terms set to zero, then by writing them in finite difference form the residual equations were obtained. If a correct solution was obtained, residuals would be of the order of 0·01% of the average value of the function. Further, by calculating the residuals, a check was made that the correct equations had been solved, since the residual equations were independent of the equations used in the DR computation; otherwise it was possible to converge to solutions of incorrect equations.

120. The final difficulty, which had sometimes caused problems in the early stages of preparing the program, was computation instability. At times it was hard to determine whether instability was likely to occur, and this applied particularly when graded meshes were used. Referring to the stability equation (27), the critical time increment depended on the mesh intervals Δx and Δy; thus the region where the mesh spacing was closest was likely to be the critical region. During certain solutions the computation had become unstable in the fine mesh region, but had remained stable elsewhere.

121. The DR method had been applied to several other problems, and despite the pitfalls described above it had proved to be a powerful method.

Mr R. G. T. Lane (Sir Alexander Gibb & Partners) said that the Paper was given at a most opportune time when their thoughts were concerned very much with this type of problem. They had excellent co-operation at Imperial College with Professor Sparks and Dr Cassell and at present had in hand many tests, including tests for the effects of earthquakes on arch dams, and his first question arose from this.

123. Although called 'dynamic relaxation', this method was used for the purpose of determining static stresses. Did it provide a method which was better than others for arriving at stresses due to dynamic causes?

124. Another aspect, touched upon by Professor Pippard, was the question of design procedure. This method gave an excellent stress analysis of a structure which existed at least on paper; but the design engineer started knowing little more than the shape of the ground and had to fit his structure into it. Would the coarse-mesh method be a reasonable tool by which to examine a whole series of shapes which would lead to another convergence—in other words, the optimum shape of dam for its particular conditions?

125. The next point was in regard to boundary conditions. Dr Cassell had already mentioned that conditions other than the rigid valley could be analysed. Would it be too difficult to go yet a stage further and to include the effects of the dam on the valley and not only the effects of the valley on the dam? Could they include in the

mathematical model part of the valley itself without becoming too laborious and unwieldy?

126. Another matter had arisen from the statement in the Paper that the stresses due to temperature were as high as those due to hydraulic loading. This might not be so, because a dam was a discontinuous structure with many joints. Could it be foreseen that at some not too distant date effects of joints in the structure which led to discontinuity of stress could be taken into account in this method of design?

127. Mr Lane would be grateful for a little more clarification on the actual time taken to carry out the work, both in the case of fine-mesh considerations and coarse-mesh considerations, and the time in preparing the program, not just the computer time.

Professor W. G. Bickley (Imperial College, Emeritus Professor of Mathematics) said that to him the charm of the Authors' method was the simplicity of approach—a brilliant but simple idea of associating a process of successive approximation with a dam clamped vibration. In this sense it was surely quite a simple idea to convey, even to engineers. It seemed perfectly clear that this method, in the hands of the people who had been using it, had led to remarkable results with, of course, the aid of the modern computer. He added the comment, however, that the ability to write a computer program did not enable a person to solve a problem he did not understand.

129. The important thing was that the Authors' approach enabled the engineer to understand what he was doing. He then had, of course, to learn the technique of making the computer do the work for him, but, provided one knew what was required, a modern computer was only too glad to enable this to be done.

130. The basis of physics was important. The Authors had written down a physical idea in mathematical language, thinking of the damped vibration, and had obtained their results. At this point some of the more pure-minded mathematicians might be sceptical, thinking that intuition was not enough, but in this case intuition worked and the results were obtained. Indeed, there was another question. Having used a method of this sort, the mathematician—and also the engineer—would want to know somehow whether his results were reliable. This could be an enormous difficulty. There were literally thousands of turgid pages of mathematics which tried to prove that a process in a numerical analysis did in fact converge and do the things it ought to do. There were other pages in which there was a stability problem, and this again was something the numerical analyst was faced with in time-dependent problems. But the physical insight of the Authors seemed to ensure that, provided damping was chosen properly, the process could not help converging. Moreover, if one used a damping which was slightly undercritical, one then had an oscillatory solution which approached the final limit from opposite sides as the damped wave in the solution progressed. This progress to the limit in terms of a wave seemed to Professor Bickley to answer this question as to whether the results could be relied upon. Following the way through in successive intervals of this fictitious time, the converging extreme gave bounds between which the true solution lay.

131. After a few of the things said about the alternative finite element methods he wondered whether in the last resort there was an underlying dichotomy between the methods. With interlacing nets and so on, the Authors had used what, mathematically speaking, could be called an explicit method since they had expressed each value at one time level in terms of the values already determined at previous time levels. However, he could not see that there was any reason why the method should not be applied to a set of equations of the type called 'implicit'. He had not thought this out in detail, and there just might be some obstacle which he had overlooked.

132. As a mathematician who had had the interests of engineers at heart for a long time, he congratulated the Authors on the ingenuity of the original idea and upon the success of the way in which it had been applied to rather large scale, rather important, and certainly not easy engineering problems.

Mr R. S. Jenkins (Ove Arup & Partners) quoted from Timoshenko's *History of strength of materials:* 'Rayleigh shows how, by making velocities vanish, the solution of static problems may be extracted from vibration analysis.' This was a pretty accurate description of DR, although before the electronic computer the analysis must have taken a different form; but the deflexion of bars and shells and so on was defined by this method. They were constantly taking up or rediscovering ideas originating in the nineteenth century which had now become practical for solution with the computer. It would be interesting if somebody were to outline the early history in this field which had now become of great importance.

134. He appreciated that in this first publication of new techniques the Authors had chosen simple examples, but he felt that the beginning of the synopsis was something of an over-statement, when they wrote of a 'full three-dimensional solution of the problem of an elastic arch dam'. A simple thick cylinder sitting in a valley of rigid rock had not much to do with arch dams as built at the present time. Some of the Authors' comments had shown that they were well aware of this.

135. Mr Jenkins believed it was now possible to exploit the speed and capacity of the second generation of computers by using defined systems of co-ordinates in general terms for the analysis of doubled curved and variable thickness Coyne type arch dams, particularly by tensor methods. It meant that instead of the one variable coefficient $(1/r)$ of the polar co-ordinates in the equations of motion, more undifferentiated stress components appeared with variable coefficients, which were functions of all three directions. There were also more terms in the stress/displacement relationships, again with the same type of variable coefficients.

136. Although this idea of interlacing nets was very elegant for polar co-ordinates, could they be used for these general co-ordinates? If not, it meant having to use a simple single grid and doing the usual centred finite differences at the node points. There was, of course, one thing it did. It was not necessary to average undifferentiated components at a node point, as with the interlacing nets.

137. That would lead one to consider whether there was any advantage in using, instead, the equations of displacements in zero, first and second differences in space and time:

$$(\lambda+\mu)u_m|_r{}^m+\mu u_r|_m{}^m+(3\lambda+2\mu)\psi|_r+F_r = \rho f_r$$

where r denoted the row index and $(3\lambda+2\mu)=E/(1-2\sigma)$ was the thermal elastic constant, not as misprinted in equation (26). Since $u_m|^m$ and ψ were scalar invariants their covariant derivatives in α^r might be replaced by the ordinary partial derivative. The final displacement print-out might be used directly for the six equations of compatibility at each node.

Mr A. K. Welch (Taylor Woodrow Construction Ltd) said that over the previous two years he had been mainly engaged in studying dynamic relaxation, developing computer programs incorporating the method and applying these programs to the analysis of a wide range of elasticity problems. The main impetus to this work had been the analysis of prestressed concrete pressure vessels. The result of this study has been a series of programs which solve various forms of the two- and three-dimensional elasticity problems, these programs being in constant use in the design office. The largest runs to date had involved just over 10 000 grid points and required about 12–15 min computation time on an IBM 7094 computer.

139. From the experience gained in running numerous jobs the method had been found, in most cases, to be easy and quick to apply. A further point, which was important, was that the method represented a physical process and the convergence could be followed and understood in engineering terms, and not just in abstract mathematical ideas. This helped in its application and understanding.

140. The Authors had quoted the solutions to the dam problems as indicating the economy of the method. Mr Welch suggested that the dam problem was not an ideal example to choose to indicate this for two reasons. First, the large differences

in block dimensions between the radial and the other two directions had the effect, by way of equation (27), of greatly reducing the time interval that could be used. A possible way of reducing this could be to introduce what he termed pseudo-densities, the values of which depended on the direction being considered. He had found this to be a way of speeding up calculations involving elongated blocks when using Cartesian co-ordinates, although if very high Poisson's ratios were present these caused cross waves which tended to nullify any gains. The ideas were still in the study stage and had not yet been applied to problems in polar or cylindrical co-ordinates.

141. The second effect which made the dam problem rather lengthy was the fact that the dam's slowest mode of vibration was a bending mode and this governed the time required to obtain an acceptable solution. The effect was more troublesome in this case, but could be overcome for other problems possessing different forms of vibrations, such as a large radius ring beam under axisymmetrical loading. In this problem the low frequency vibration could be almost removed by forcing the ring into an equilibrium position before allowing distortion of the cross section, and so, having the slowest mode missing, a higher damping could be used and a faster convergence obtained.

142. In the dam problem, it might be possible to carry out a very simple trial load calculation as a preliminary and starting from this solution the influence of the slowest mode might be greatly reduced. But as the problem was basically a shell problem then a shell-type solution was probably the best method to use.

143. Mr Welch was interested in the comparison of results obtained using a DR solution based on cylindrical co-ordinate elasticity equations and a DR solution based on the shell equations. Could the Authors compare other aspects of the analysis, such as time required for solution, between the two approaches?

144. Turning to the mathematical basis of the method, the Authors had stated in § 71 that the faster modes of vibration disappeared 'by overdamping'. Should not this read 'by underdamping' as the value of critical damping increased for the higher and faster modes of vibration?

145. In their comparison with other iterative methods, the Authors had classified dynamic iteration as an accelerated simultaneous iteration, suggesting that the method was related to Jacobi's in a similar manner as successive overrelaxation was related to Gauss–Seidel's. Mr Welch believed that this straightforward comparison was not applicable since if one tried to accelerate Jacobi when solving the one-dimensional problem, when formulated in normal structure matrix notation, the process always diverged. Also Forsythe and Wasow[13] quoted an example which indicated that there was no advantage in overrelaxing a Jacobi iteration.

146. DR (or iteration) deviated from the other three types mentioned in its use of two first-order equations successively, and this, together with the use of interlacing nets, constituted its main deviations from the other iterative schemes, as well as its main advantages.

147. Finally, Mr Welch suggested that DR's efficiency when compared with the other methods should be measured, when practical applications were envisaged, not solely on the length of time required for solution, but also on the following factors:

(*a*) ease of formulation,
(*b*) ease of programming,
(*c*) ease of application,
(*d*) ease of understanding.

He had found that DR possessed all these properties and considering its representation of an easily envisaged physical event, as emphasized earlier, he strongly recommended it as an engineering tool.

Dr K. I. Majid (University of Manchester) said that DR was essentially a numerical method for the solution of a set of mathematical equations, and engineering came

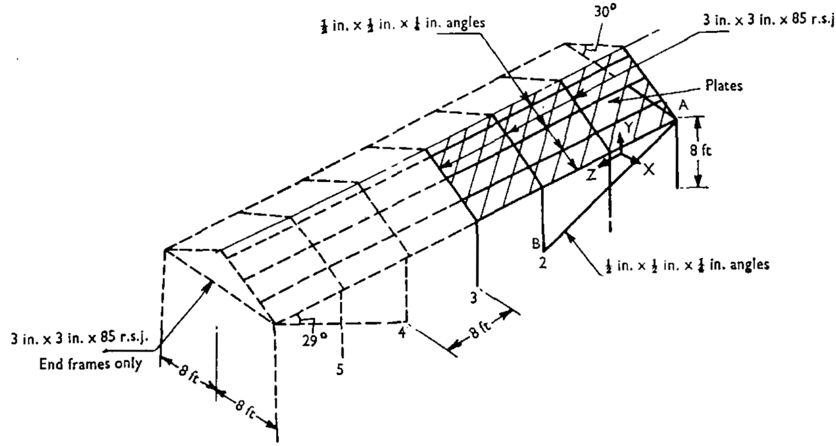


FIG. 24: SEMI FULL-SCALE SHED

into the method merely as an application. Because of this it appeared that the Paper would have been equally appreciated and discussed by numerical analysts had it appeared in a mathematical journal.

149. The Authors were to be congratulated on demonstrating that DR was an iterative procedure of comparable efficiency to those which Jacobi and Gauss–Seidel developed in the last century. But, as pointed out by the Authors, and comparing equations (52) and (54), it was clear that the present method was slower than the successive overrelaxation method. With a value of η of 0·96 (which was a very reasonable value for large matrices of type 'A' properties), the DR rate for convergence gave 0·82, while successive overrelaxation gave 0·66; in other words, successive overrelaxation was 24% faster. With a value of η of 0·9, which was also possible with large matrices, the DR, using equation (52), gave a rate of 0·643, while equation (54) gave a converging rate for successive overrelaxation of 0·393; in other words, over 60% faster than DR.

150. He had used successive overrelaxation himself for very large matrices on structural problems, using the tridiagonalization technique and magnetic tape facilities, and no organizational difficulties were faced with the problems analysed.

151. It was imperative, however, to point out that the various numerical methods available, including DR, only helped to solve a set of mathematical equations. The very difficult problem of setting these equations for a given engineering problem remained unresolved and the present Paper did not contribute towards that. For every new problem other than those given in the Paper, the engineer was still faced with the task of formulating the differential equations involved, of representing the boundary conditions and of converting these equations into difference equations. Thus the computer was employed only as a calculating machine to speed up the numerical operations at the final stage.

152. For these reasons the modern trend, in the last decade, had been to move away from the finite difference method and towards matrix methods of structural analysis, namely, those of force and displacement, together with the finite element approach. For instance, the sheeted pitched roof shed shown in Fig. 24 had six degrees of nodal freedom at each joint, the plates being subject to in-plane and out-of-plane forces, and the prismatic members were under torsion and bending in two directions, with interaction between the plates and the members.

153. It could be seen in Fig. 24 that there were purlins coming into contact with the rafters, not at joints but away from them, shear centres being away from the centres of area of the sections, and purlins being connected by cleats to the rafters. The formulation of the exact differential equations for such a structure was far more difficult than solving these equations numerically. When it came to the matrix method the whole task was reduced merely to that of data preparation concerning the frame geometry and the material properties of its components. The computer then took over and formulated the required matrices. Such a computer program had now been developed in the Department of Engineering, Faculty of Science, Manchester University, and the solution of the problem, once it was formulated, became rather a simple matter, whether one used an iterative method or a direct method. Now that a computer program existed that could analyse any structure, be it a cylindrical dam, a variable cross section bridge, or a portal shed, the formulation of differential equations for every given problem could be avoided.

Mr R. S. Taylor (Taylor Woodrow) congratulated the Authors on their fine Paper and said that they would no doubt join with him in paying tribute to Mr A. Day, who first had the flash of inspiration which led to the development of this method, which he had named 'dynamic relaxation'.

155. As engineers they were dealing with natural things and natural forces. As they became more sophisticated in their methods of analysis, developing better and sharper tools such as DR, they must keep those tools well sharpened by close observation of nature in the behaviour of actual structures. This was done through research, through the making and testing of models, and by then applying these analytical tools to the observed behaviours of structures to see how they stood up under different conditions with varying parameters. How, for example, did one decide upon the specific strain to allocate to a stress concentration? He did not know; but these were the sort of problems which had to be faced.

156. As they went on using tools of analysis, so to speak, as a zoom lens on a camera, to look closer and closer at the workings of nature, as engineers they must be prepared to look more closely at the criteria by which they had previously judged the acceptability of their structures. He was sure they would find that these too required re-appraisal in the light of their new capabilities.

Dr I. P. King (University of Wales) said that as he was no mathematician he would not contradict the statements about the rates of convergence. However, taking the Laplace equation given by the Authors, rewriting Δ, it became apparent that one needed to hold the two vectors in storage or somewhere. Overrelaxation only required one vector and two vectors presented a severe storage limitation. Looking at this in terms (as shown by Hodgkins[14]) of two vectors it was a semi-iterative process of the sort Varga[15] described in his book, where he stated that he was looking for faster initial convergence rates, so perhaps the final solution which did not give as fast a convergence rate in the limiting infinite position might be more suitable for getting to an engineering approximation to the answer more quickly.

158. To the suggestion that finite elements and finite differences were opposed, with DR on the side of finite differences, Dr King suggested that DR could be applied to finite elements by writing down the equivalent dynamic equations. One would then have a mass matrix which could be selected. This would improve the time interval size and there would be a damping matrix for which the appropriate values would have to be selected. Did the Authors know of any investigation of this side away from finite differences?

Mr C. Snell (University of Nottingham) said that the replacing of a continuous material by some form of numerical process involved a step of discretization. There were basically three ways of doing this, each of which had its propagandists. The oldest was probably that which was conventionally attached to the name of relaxation, where one set up a differential equation for the displacement components and solved it by finite difference approximation. A second method, used by the Authors, was to set up the separate stress/strain equations and the equations of motion and to solve these explicitly in numerical form with first-order finite difference approximations but without the intermediate step of eliminating the stresses between the two sets of equations. This method gave rise to nine unknowns at every point instead of three, and correspondingly more differential equations of a lower order. The third method involved the assumption that small elements had simple states of stress in them, defined by polynomial expressions truncated in an arbitrary manner, and this gave rise to the finite element approach. All three methods introduced truncation errors in that all the methods assumed the behaviour locally to be much simpler than it was in fact. It was important to know which of these processes of discretization was the best, which gave rise to the quickest and most accurate solution on a computer of modest size at the smallest cost.

160. The first of these three methods, by eliminating the stresses algebraically and solving for the displacement, assumed that for any given strain distribution there was a unique stress solution. This was not true if Poisson's ratio were equal to 0·5 and all processes which relied on setting up a differential equation which purported to describe the displacement, perhaps in terms of displacement functions, were not valid for incompressible material.

161. The Authors had referred to the difficulties experienced at Imperial College, using this first method of obtaining convergent iterations. The reason was that the process broke down. One was not entitled to assume a unique state of stress for a given state of strain, whereas the reverse relationship was always unique. The process of elimination was therefore 'one way'. The Authors had not used the elimination at all and therefore there was no analytic objection to their method when Poisson's ratio approached 0·5.

162. There was considerable evidence that this first method, quite apart from being invalid for high Poisson's ratios, could converge to the wrong answer or could appear to converge to the wrong answer, and it was recognized by Southwell that relaxation solutions were dependent on mesh size. In several cases this dependence had been shown to be quite strong.

163. On this particular problem of arch dams, both at the University of Sheffield and at Imperial College, solutions had been obtained which were clearly wrong, although they appeared to be derived from iterations which had converged and in which further iteration produced negligible changes.

164. The difference between these two methods when one thought of the approximation involved, was quite simply that in DR one used central difference approximations of first derivatives whereas in the Southwell relaxation, or Gauss–Seidel iteration one applied central difference approximations to second-order derivatives.

165. It was not at all clear why the former appeared to be relatively insensitive to the number of mesh points while the latter was known to be more sensitive to the size of mesh. If the only evidence for the negligible dependence on mesh size was the three results reported earlier, it should be noted that in going from eight to 16 mesh points one would expect the change to be much larger than in going, say, from 16 points to 32. One ought not to be very surprised at the small change in going from 16 to 24, and on its own this was not a convincing proof. One knew from relaxation type solutions that it was the ratio of the number of mesh points which had to be increased in a test for convergence to a fine mesh, rather than by a simple arithmetic progression in the number. Supposing that DR gave a satisfactory check for 32 mesh points, there was still the uneasy feeling which most people must surely have that these two finite difference methods, which for small Poisson's ratios appeared to be very similar, were giving such vastly different results, and one would like to know why. Did the Authors have any comments on this point?

166. Also he would like to ask for a little more information about the demands on computer storage. Taking $24 \times 24 \times 10$ mesh points, with variables at each point, his calculations indicated that it took at least 20 000 words to store the information, and the rest of the program must surely take a lot more. Could some indication be given of how much store was being used in the big IBM machine for these programs? What was the total number of words being taken up in relation to the number of mesh points? It appeared to him that this number would be greater than the product, $24 \times 24 \times 10 \times 9 \times 2$, which was well in excess of 20 000. Could some information be given on this point?

167. Mr Snell added in writing that the Authors had referred to experimental work produced by himself and two colleagues, and although reference had been made to the fact that the experimental conditions were not exactly similar to those involved in the theoretical analysis some of the difference between experiment and theory could clearly be attributable to the method of test, and for this reason, a fuller description of the experimental work was perhaps helpful.

168. The study of some typical arch dam shapes by a wide variety of methods, experimental as well as theoretical, had been undertaken by a number of investigators under the sponsorship and co-ordination of a body that, in due course, became the Civil Engineering Research Association. The work had been undertaken in October 1959 with five distinct dam types, one of which had subsequently been abandoned.

169. The dam described in the Paper was a fixed centre dam. The more realistic dam shapes were defined by circular arcs of varying radius and varying subtended angle measured from a fixed centre in plan, or, more commonly, of variable centre in which at any one height the dam was bounded by concentric circular arcs, but the centre of these arcs would vary with the height of the section. Initially, the photo-elastic experimental work had been centred on this latest type, and by July 1962 these had been fully analysed. The constant thickness, fixed centre dam had been analysed shortly afterwards, and the dam described by the Authors in their introduction was examined for three separate conditions of foundation support.

170. The whole of this work had been completed before any exact numerical solution had been obtained and it was now apparent that these tests had failed to reproduce the practical conditions in an important detail. The dam was supported by an amount of valley and valley floor sufficient in extent to ensure the diffusion of the local stresses produced at the boundary of the dam, and a dimension considered adequate for this purpose was chosen arbitrarily in the light of theoretical work on Dokan Dam. Loading was applied through liquid mercury and a valley was therefore constructed to retain this fluid. The dimensions of the valley were now clearly seen to have been inadequate to prevent significant deformation of the valley and, by virtue of the rotations at the abutments, additional stresses have been induced in the dams as a result of the fluid loading on the valley sides. This was recognized at a late stage and the two results shown in Figs 14 and 15 were from comparative tests in which varying amounts of valley had been loaded.

171. The difference between the calculated curves and the experimental points might well be due to the effect of an inadequate valley coupled with the different boundary conditions which in the theoretical work had assumed zero displacements at all points on the intersection of the dam and valley. The effect of high Poisson's ratio of the model material appeared to have resulted in bending moments in the arches which were somewhat higher than would have been the case for concrete with its low ratio.

172. Previous work on the effect in arch dams of different values of Poisson's ratio seemed limited to the comment by Serafim arising out of studies in Lisbon for the Salamonde Dam. He had concluded that, except in one region, there was no great dependence on the constant, and the testing policy had been not to take great pains over the ratio of the material used in the construction of the model. Testing

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at Bergamo took the opposite view, and regarded the third decimal place as significant. The exception noted by Serafim was that at the junction of the dam with the valley sides a condition of plain strain was effectively established with the tangential normal stress being equal to the product of Poisson's ratio and the direct stress normal to the valley floor. If the strain gauge results from resin model tests were interpreted on this basis, agreement was found between a reasonable range of model materials.

173. Certainly the Authors had produced results with a larger dependence on Poisson's ratio but this could easily have arisen from the idealized conditions applied at the boundary. The work described in the Paper referred to a dam rigidly constrained at all points around its periphery, and it was easy to show from theoretical grounds that a problem of imposed displacement on the boundary was very much more dependent upon the value of the elastic constants than a problem in which normal tractions were defined.

174. Concerning the numerical procedures, these had been applied to a co-ordinate system matched to a problem where boundaries were defined by simple relationships between the co-ordinates. If the method was to have a general validity, it would be necessary to show that the generally shaped surface with its complications did not destroy the well-behaved nature of the approximation. Alternatively, if the equations could be applied to a general curvilinear co-ordinate system in which the coefficients of the algebraic equations at the mesh points varied in a three-dimensional manner, it would again be necessary to establish the good behaviour of the iterative procedure. Further comment on these points by the Authors would be very valuable.

The following contributions were received in writing:

Mr A. N. Jolliffe (Research Assistant, International Courses, Delft, Netherlands) wished to ask the Authors if the reduced truncation error obtained by putting $\Delta y = \Delta x = K = C\Delta t$ had any marked effect on the final values obtained by the machine.

Mrs W. L. Wood (Department of Applied Mathematics, University of Reading) wrote that she had read the Paper with great interest and was pleased to be invited to give a mathematician's point of view.

177. It was sometimes said that one of the greatest barriers to communication between Great Britain and the United States was that we apparently spoke the same language. This could also be said about mathematicians and engineers. There were certain words which could cause confusion and there was an example of this in § 57 where the word 'convergence' was used in two different senses. It was natural for an engineer, especially in the context of DR, to use the word convergence in the 'visual' sense, i.e. the convergence of the iteration, as the damped wave died away, to the steady-state solution. This must be the meaning of the word in the first sentence. But mathematicians often used the word in quite a different sense; they spoke of the convergence of the solution of the difference equation to that of the differential equation as the mesh size tended to zero. Since the accurate solution of the problem referred to in the third sentence must be the solution of the differential equation, it must be the second meaning of the word convergence that was being used there.

178. In § 50 the equality in equation (27) was used to eliminate $\Delta t/C$. Equation (27) was obtained by Forsythe and Wasow assuming a square or box-shaped region. It was interesting to note that $\Delta t/C$ could be left in and obtained later for a region of more general shape.

179. The investigation of the convergence of the iteration was done on the 'error' function, i.e. the difference between the function actually obtained during the iteration and the steady-state solution. The u^r in §§ 49 *et seq.* must be this error function, since equations (28) and (29) had zero on their right-hand sides and not some function of x and y as was implied in equation (42). Thus one could start with equation (32)

and the understanding that u^r was this error function and must therefore vanish on the boundary:

$$\left(1+\frac{K}{2}\right)\delta u^{r+1} = \left(1-\frac{K}{2}\right)\delta u^r + C^2(\Delta t)^2 F_u^r. \quad . \quad . \quad . \quad . \quad (32)$$

F_u^r could be replaced by the five point finite difference Laplace expression $L(u^r)$, say, as in equation (34), which would give

$$\left(1+\frac{K}{2}\right)\delta u^{r+1} = \left(1-\frac{K}{2}\right)\delta u^r + C^2(\Delta t)^2 L(u^r) \quad . \quad . \quad . \quad . \quad (34)$$

and

$$u^{r+1} = u^r + \delta u^{r+1}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (35)$$

Hence by eliminating u and δu in turn

$$\left(1+\frac{K}{2}\right)u^{r+1} - 2u^r + \left(1-\frac{K}{2}\right)u^{r-1} = C^2(\Delta t)^2 L(u^r) \quad . \quad . \quad . \quad (55)$$

and

$$\left(1+\frac{K}{2}\right)\delta u^{r+1} - 2\delta u^r + \left(1-\frac{K}{2}\right)\delta u^{r-1} = C^2(\Delta t)^2 L(\delta u^r) \quad . \quad . \quad (56)$$

so that u^r and δu^r satisfied the same recurrence relationship and were multiplied in each iteration by the same factor m, say (this being also implicit in equations (46) and (47)). m was such that

$$\left(1+\frac{K}{2}\right)m^2 - \left[2-\left(\frac{C\Delta t}{h}\right)^2\mu\right]m + \left(1-\frac{K}{2}\right) = 0 \quad . \quad . \quad . \quad . \quad (57)$$

where $-\mu/h^2$ was an eigenvalue of L (h being the mesh size). This was the equivalent of Frankel's[7] equation (40). The product of the roots of the quadratic (57) was $[(1-K/2)/(1+K/2)]$. Thus if the roots were real and unequal one of them had to have a modulus less than $\sqrt{[(1-K/2)/(1+K/2)]}$ and the other greater. The best overall effect was obtained by making all the values of m have modulus equal to

$$\sqrt{\left[\frac{(1-K/2)}{(1+K/2)}\right]} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (58)$$

i.e. by making the maximum and minimum values of μ (μ_m and μ_0, say) give equal roots to (57) and all the values in between, complex roots, i.e.

$$2-\left(\frac{C\Delta t}{h}\right)^2\mu_m = -\sqrt{4-K^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (59)$$

and

$$2-\left(\frac{C\Delta t}{h}\right)^2\mu_0 = +\sqrt{4-K^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (60)$$

leading to

$$\frac{C\Delta t}{h} = \frac{2}{\sqrt{\mu_m+\mu_0}} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (61)$$

and

$$K = K_{\text{cr.}} = \frac{4\sqrt{\mu_0\mu_m}}{\mu_0+\mu_m}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (62)$$

180. Suppose that $u(ih, jh) = u_{i,j}$, say, was an eigenfunction corresponding to the eigenvalue $-\mu/h^2$. This meant that

$$u_{i+1,j} + u_{i-1,j} + u_{i,j-1} + u_{i,j+1} - 4u_{i,j} = -\mu u_{i,j}. \quad . \quad . \quad . \quad . \quad . \quad (63)$$

The physical equivalent of the following argument amounted to saying that the $u_{i,j}$ were thought of as displacements in a vibration. It was expected that, provided the boundary conditions were suitable, to any one mode of vibration there corresponded another such that the signs of the displacements at alternate mesh points were reversed, e.g. in the fundamental mode the displacements had the same sign at all mesh points and with the highest frequency of vibration alternate signs were expected at alternate points, i.e. provided the shape of the boundary was such that (63) was satisfied at all interior mesh points, then since the error function was zero on the boundary, hence $(-1)^{i+j}u_{i,j}$ was also an eigenfunction and from (63)

$$(-1)^{i+j+1}u_{i+1,j}+(-1)^{i+j-1}u_{i-1,j}+(-1)^{i+j+1}u_{i,j+1} +(-1)^{i+j-1}u_{i,j-1}-4(-1)^{i+j}u_{i,j} = (-8+\mu)(-1)^{i+j}u_{i,j} \qquad (64)$$

so that to any eigenvalue $-\mu/h^2$ there was another $(-8+\mu)/h^2$ and in particular $\mu_0+\mu_m = 8$. This argument could easily be adjusted to one or three dimensions so that in general

$$\mu_0+\mu_m = 4n \qquad (65)$$

for n dimensions. Hence from (61), for optimum rate of convergence of iteration

$$\frac{C\Delta t}{h} = \frac{1}{\sqrt{n}} \qquad (66)$$

for any region such that the above conditions were satisfied.

181. Equation (58) was not the same as (50) because (50) had been deduced from the solution of the differential equation. This latter solution would only be approximately the same as the solution of the difference equation when the mesh size h was small compared with the dimensions of the region and this meant that K *itself* was small compared with unity. This illustrated the danger of making deductions about the solution of the difference equation from that of the differential equation.

182. A rough estimate of the times required by DR and SOR could be made as follows. If η was an eigenvalue of the Jacobi matrix or difference operator as in § 69, it could easily be seen from equation (63) or its equivalent in n dimensions that

$$\eta = 1-\frac{\mu}{2n}. \qquad (67)$$

Hence the maximum eigenvalue or spectral radius of the Jacobi operator was given by

$$\eta = 1-\frac{\mu_0}{2n}. \qquad (68)$$

Also if $\mu_0 \ll \mu_m$, i.e. h was small, then

$$\sqrt{1-\eta^2} \simeq \sqrt{\frac{\mu_0}{n}}. \qquad (69)$$

Hence from equation (52)

$$m_{\mathrm{DR}} \simeq 1-\sqrt{\frac{\mu_0}{n}} \qquad (70)$$

and from equation (54)

$$m_{\mathrm{SOR}} \simeq 1-2\sqrt{\frac{\mu_0}{n}}. \qquad (71)$$

If k iterations had to be made, i.e. multiplied by m, k times to reduce the error by a factor $1/R$ say, then

$$m^k = 1/R. \qquad (72)$$

Hence

$$k \log m = \log R, \qquad (73)$$

i.e. k was proportional to $-1/\log m$.

183. Thus for μ_0 small, DR required approximately twice as many iterations as SOR. The SOR equations by the successive substitution method, would require

something like $2p$ multiplications per iteration where p was the number of unknowns. DR required substitution in explicit equations which could also be arranged to be about $3p$ multiplications. Thus the total times of iteration were approximately DR:SOR as 3:1. It should be emphasized that this was based on the assumption $\mu_0 \ll \mu_m$ but it showed that the times were probably of the same order. Also this was only for Laplace's equation.

Mr B. A. Walker (Partner, Kenchington, Little & Partners) wrote that he was very interested in the Authors' ingenious development of a relaxation technique linked to digital computer. Whilst he was familiar with Southwell's original manual technique, the Authors' method was completely new to him.

185. He had some difficulty in trying to follow the Authors' arguments in arriving at the conclusion for the rate of convergence expressed by equation (48) and it seemed to him to be in error.

$$u^r = u^{r-1} + \Delta t . \dot{u}^r \quad \ldots \quad (74)$$

$$u^{r+1} = u^r + \Delta t . \dot{u}^{r+1} \quad \ldots \quad (75)$$

$$\dot{u}^{r+1} = \dot{u}^r + \Delta t . \ddot{u}^{r+1}. \quad \ldots \quad (76)$$

Hence

$$m = \frac{u^{r+1} - u^r}{u^r - u^{r-1}} = \frac{u^{r+1} - u^{r+1} + \Delta t . \dot{u}^{r+1}}{u^{r+1} - \Delta t . \dot{u}^{r+1} - (u^r - \Delta t . \dot{u}^r)}$$

$$= \frac{\Delta t . \dot{u}^{r+1}}{u^{r+1} - u^{r+1} + \Delta t(\dot{u}^{r+1} - \Delta t . \ddot{u}^{r+1})}$$

$$= \frac{\dot{u}}{\dot{u} - \Delta t . \ddot{u}} = \frac{1}{1 - \Delta t . \ddot{u}/\dot{u}}, \quad \ldots \quad (77)$$

where $\dot{u}$ and $\ddot{u}$ were calculated at the $(r+1)$th iteration.
The correct value appeared to be:

$$m = \frac{1}{1 + (\alpha - \omega_s)\Delta t} = \frac{1}{1 + (\alpha - \sqrt{\alpha^2 - \omega_0^2})\Delta t}$$

from which equation (50) should be:

$$m_{\text{opt.}} = \frac{1}{1 + K_{\text{cr}}/2}$$

and equation (52) should be:

$$m_{\text{DR}} = \frac{1}{1 + \sqrt{1 - \eta^2}}.$$

186. If possible he would like to use the Authors' method to solve some problems in the elasto-plastic range, and there seemed to be no reason why this should not be done but he would be glad of the Authors' comments. If one considered an ideal material in which the material extended without further increase of stress after the yield point was reached, the following equation could be deduced:

$$u = u_f(1 - e^{-2\alpha t})$$

from which:

$$m = \frac{1}{1 + 2\alpha\Delta t} = \frac{1}{1 + K}.$$

Thus one might expect convergence and the choice of as large a value of K as was consistent with convergence in the elastic range would appear to be appropriate.

187. He would be grateful if the Authors would expand a little on the choice of the numerical value to be adopted for the viscous damping factor K for any particular problem. Possibly the answers to his questions were contained in some of the references at the end of the Paper, but since these were not readily available, perhaps the Authors would forgive him for asking them to repeat what they had already

given elsewhere. Having decided suitable mesh lengths for the problem in hand, a value for Δt was presumably selected to satisfy equation (27). Considering the structure to be analysed as made up of a number of rods, in each of the co-ordinate directions, different values of ω_0 (the fundamental longitudinal angular velocity) would be obtained depending on the length between boundaries and on the boundary conditions, i.e. whether free or fixed. Some compromise, therefore, had to be arrived at in deciding the value of K to be used. Could the Authors' say how this compromise was arrived at? In the example of the dam analysis given, had different values of K been used in each of the co-ordinate dimensions? Had problems of slow convergence arisen as a result and had the Authors found any difficulties arising from ignoring modes other than the fundamental?

Dr M. J. L. Hussey (Lecturer in Civil Engineering, King's College, London) wrote that the Authors had presented their method as a system for solving problems of linear elasticity in finite difference approximation. Techniques similar to those described might as readily be applied in conjunction with other methods of discretization, as well to linear problems of skeletal structures, or more significantly, to non-linear conservative problems of frames or discretized continua. Newmark[16] went further and suggested 'by suitable means of application of the loading, and with the introduction of enough damping . . . the (dynamic) procedure can be used to determine the 'static' behaviour of a structure as it progresses through various degrees of inelastic behaviour up to collapse'. It seemed, however, that great caution would be necessary in path-dependent applications.

189. Concisely, the Authors' heuristic was that the solution of

$$[\mathbf{S}]\mathbf{u} = \mathbf{b}$$

(in which $\mathbf{u}$ was a vector of displacements, $\mathbf{b}$ a loading vector derived from body forces and stress boundary conditions, and [S] a positive definite stiffness matrix obtained by some discretization procedure) coincided with that of

$$\mathbf{M}\ddot{\mathbf{u}} + [\mathbf{C}]\dot{\mathbf{u}} + [\mathbf{S}]\mathbf{u} = \mathbf{b}$$

if $\ddot{\mathbf{u}} = \dot{\mathbf{u}} = 0$. This equation, in which [M] and [C] were arbitrary square matrices, had the appearance of a problem in dynamics. Provided that [M] and [C] were positive definite, $\dot{\mathbf{u}}$ and $\ddot{\mathbf{u}}$, interpreted as successive time derivatives of $\mathbf{u}$, would tend to zero with indefinitely increasing time. A stable procedure for the numerical integration of the differential equation then generated an iterative procedure for solving the original problem.

190. The Authors used as [M] the actual mass matrix of the structure, and as [C] the mass matrix multiplied by $K/\Delta t$. The constant K was a parameter whose value was to be chosen to promote optimum convergence of the iteration. In implementation it was proposed that the elastic accelerating forces $(\mathbf{b} - [\mathbf{S}]\mathbf{u})$ should be obtained in two steps. In the first step the stresses were evaluated from the displacements and boundary conditions, and in the second the stresses were differenced to give the required forces. The use of Gilles' interlaced grids was advocated in all cases.

191. The discussion of convergence rates in terms of the solution for continuous time (§ 72) would imply that all stable integration procedures were equally efficient. That proposed by the Authors was said to use interlaced nets in time, although the Authors' equations ultimately coincided with those derived from the simple discretizations

$$\dot{\mathbf{u}}_r = (\mathbf{u}_{r+1} - \mathbf{u}_{r-1})/2\Delta t \quad \text{and} \quad \ddot{\mathbf{u}}_r = (\mathbf{u}_{r+1} + \mathbf{u}_{r-1} - 2\mathbf{u}_r)/(\Delta t)^2.$$

192. It was easily verified that the rate of convergence (in terms of iterative steps) was *not* independent of the method of integration. If, for example, the equations of motion were expressed as a system of first-order equations and integrated by the Euler (forward difference) method, then the optimum asymptotic convergence rate

(even with *both* Δt and K used as optimizing variables) was just half that of the Jacobi iteration. It followed that the dynamic heuristic was not itself the source of the satisfactory convergence obtained by the Authors and, further, that a treatment of convergence based on the solution of the differential equation was unreliable.

193. The substance of § 68 *et seq.* was also faulty in that the conclusion was inconsistent with the premises. Since damping critical for some given frequency was sub-critical for all higher frequencies, the conclusion (equation (49)) requiring critical damping at the fundamental frequency invalidated §§ 71 and 72. Indeed, if the contention of § 68 and equation (46) were true for all $r > s$, say, then further iteration would be unnecessary. Equation (46) would have the solution:

$$\mathbf{u}_r = \frac{1}{1-m}[(1-m^{r-s})\mathbf{u}_{s+1} - (m-m^{r-s})\mathbf{u}_s]$$

with

$$\lim_{r\to\infty} \mathbf{u}_r = \frac{1}{1-m}(\mathbf{u}_{s+1} - m\mathbf{u}_s).$$

This was equivalent to Aitken's extrapolation, which was well-known to be inapplicable with accelerated iterative procedures.

194. In fact the convergence obtained by the Authors arose coincidentally from their method of integration. The discretized equations of motion were

$$\frac{1}{(\Delta t)^2}(1+\tfrac{1}{2}K)[\mathbf{M}]\mathbf{u}_{r+1} + ([\mathbf{S}] - \frac{2}{(\Delta t)^2}[\mathbf{M}])\mathbf{u}_r + \frac{1}{(\Delta t)^2}(1-\tfrac{1}{2}K)[\mathbf{M}]\mathbf{u}_{r-1} = \mathbf{b}.$$

If $2/(\Delta t)^2 = \gamma/\beta$, $(1+\frac{1}{2}K)/(\Delta t)^2 = 1/\beta$, then the eigenvalues, ϕ, of the iteration operator and the natural radian frequencies, Ω, of the structure were given respectively by

$$|\phi\beta[\mathbf{S}] + (\phi^2+\gamma-1-\gamma\phi)[\mathbf{M}]| = 0$$

and

$$|[\mathbf{S}] - \Omega^2[\mathbf{M}]| = 0.$$

Thus

$$\phi^2 + (\beta\Omega^2 - \gamma)\phi + \gamma - 1 = 0.$$

Here the matter rested unless [**M**] was a scalar multiple, say n[**D**], of the diagonal (or block diagonal), [**D**], of [**S**]. This occurred fortuitously in the Authors' applications to homogeneous elastic bodies, but would not happen without deliberate choice in more general problems. However, in this event the Ω^2 were related to the eigenvalues, μ, of the Jacobi (or block Jacobi) matrix by

$$n\Omega^2 = 1-\mu.$$

This indicated, incidentally, that it might well be the greatest 'natural frequency' of the discretized structure, rather than the fundamental, which limited the rate of convergence. Upon substituting for Ω^2, and writing $\beta/n = \omega$, the solution of the quadratic equation for ϕ gave

$$\phi = \tfrac{1}{2}[\gamma - \omega + \omega\mu \pm \sqrt{(\gamma-\omega+\omega\mu)^2 - 4(\gamma-1)}]$$

and as the μ of greatest modulus might be either positive or negative, the best that could be done to reduce the modulus of ϕ was to set $\gamma = \omega$. The Authors' relation (27) happily tended to have this effect, in the problems they treated, and equations (51) and (53) were then valid.

195. But now the equation for ϕ was

$$\phi^2 - \omega\mu\phi + \omega - 1 = 0$$

and this was precisely equation (5·47) of ref. 9 cited by the Authors. It characterized the degenerate form of Chebyshev iteration which would be written

$$[\mathbf{D}]\mathbf{u}_{r+1} + \omega([\mathbf{S}] - [\mathbf{D}])\mathbf{u}_r + (\omega-1)[\mathbf{D}]\mathbf{u}_{r-1} = \omega\mathbf{b}.$$

The dynamic heuristic had become completely suppressed, and was seen to be superfluous for *linear* problems. As an example, the Authors' equation (34) was readily reduced to the above form, and the intricacies of its development could thus be by-passed completely. It was noteworthy that the above formulation was much less demanding both in storage and programming than was the Authors', although, if compelling reasons existed, the two-stage construction of $(\mathbf{b}-[\mathrm{S}]\mathbf{u})$ was still possible. The convergence of this iteration was discussed in pp. 142–148 of Varga's book.[9] The parameter was identical with that in Young's SOR,[17] and when [S] had point (or block) property 'A' the asymptotic convergence of the corresponding SOR was exactly twice as fast as for the above iteration. Contrary to the Authors' implication in § 82, this was true whether or not ω assumed its optimum value. SOR was therefore much to be preferred when readily applicable. The Authors' reference to tridiagonalization in this context was obscure, since this was not a requirement for SOR.

196. In problems of plane elasticity without rigid inclusions, formulation on a conventional rectangular net yielded matrices with point property 'A'. It thus seemed doubtful whether the refinement of shear stresses by the use of interlacing grids was sufficiently significant to justify the consequent doubling of computation.

197. Nevertheless, in many problems of axisymmetric bodies and in practically all three-dimensional work the use of SOR entailed block operations of inconveniently high order. The facility of point or small block operations then fully compensated for a doubling of iterations. Chebyshev iteration procedures required no special ordering or topological properties, and were always applicable to structural problems.

198. The Authors' intuition had thus led them, by way of a heuristic which should prove valuable in non-linear elasticity, to an established and useful iterative procedure which was not widely known to engineers.

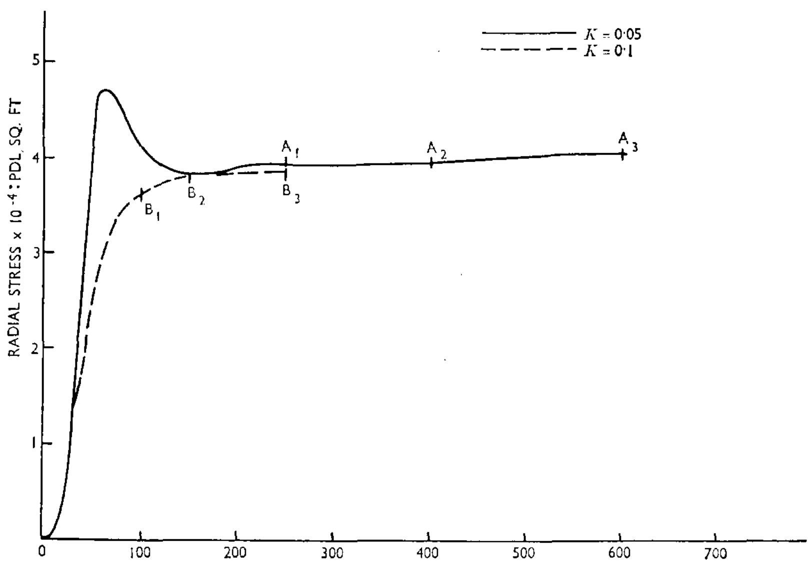

FIG. 25: RADIAL STRESS WITHIN DAM *versus* NUMBER OF ITERATIONS

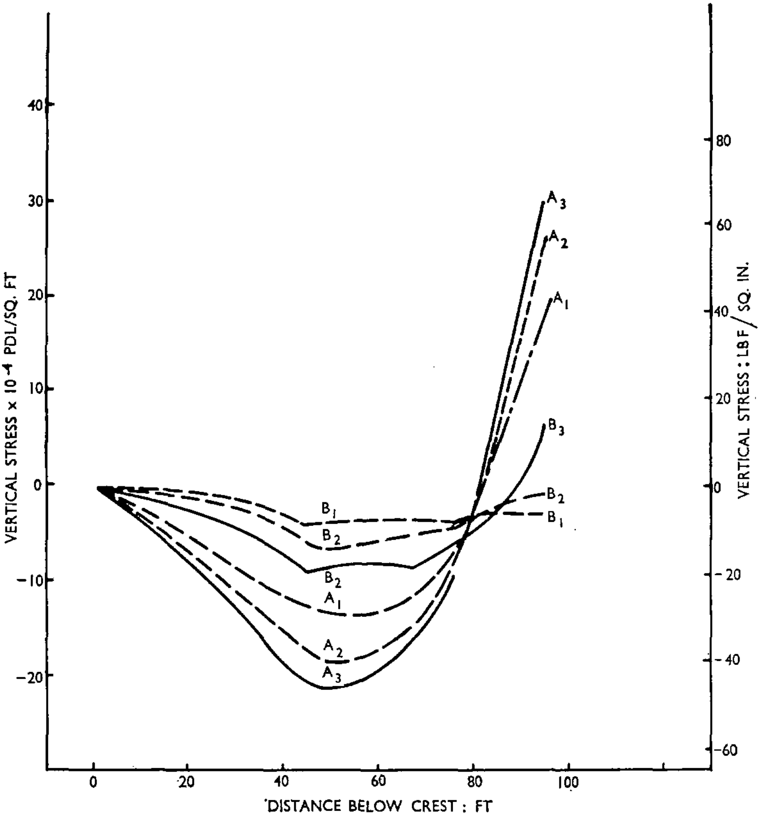

FIG. 26: VERTICAL STRESS AT WATER FACE *versus* DISTANCE BELOW CREST

Mr N. J. Cubitt (Dept of Mechanical Engineering, University of Technology, Loughborough) wrote that DR would appear to offer a very attractive method of solution, particularly for three-dimensional problems in elasticity. What did seem to be lacking was a completely foolproof criterion for terminating the iteration procedure. This could best be illustrated by reference to a real example.

200. A plain cylindrical dam was subjected to water and gravity loading and the radial stress at a point within the dam was printed out after each iteration together with the iteration number. This was plotted out in Fig. 25. It would be reasonable to assume from this that, with $K=0{\cdot}05$, the system had reached something approaching static equilibrium after about 250 iterations and that after 600 iterations there would be very little change from the state at 250. That this was far from being the case was shown by Fig. 26 where the vertical stress just inside the surface at the water face was plotted against distance below the crest. There was a significant change here in the values of stress between the three stages shown and there was no evidence at all that any steady state had been reached, the stresses being lower than would be expected.

201. The above results were consistent with the idea that examination of any one stress would indicate the degree of damping for oscillation associated with one degree

of freedom only and that its apparent steady-state condition could not be used to infer steady states for other degrees of freedom. What was also significant was that the damping constant used was almost five times that recommended by Professor Otter in his earlier paper[1] and that this was not apparent from the graph of radial stress against number of iterations. In fact one would only just infer this from the graph using a damping constant of 0·1.

202. An obvious parameter to print out would be the sum of the squares of the displacement velocities and this criterion appeared promising in the limited number of tests so far carried out, but tests over a wide range of problems were necessary before a quantitative evaluation could be given.

203. A fuller discussion of the choice of time increment, damping constant and total number of iterations and of the interaction between them would have been welcome.

Mr R. W. Postlethwaite (Section Engineer), **Mr J. H. Davidson** (Assistant Engineer) and **Mr R. J. Hayes** (Assistant Engineer) (all of Rendel, Palmer & Tritton) wrote that they had recently been using DR to analyse the shell of a 700 ft × 65 ft dia. multiflue r.c. chimney using a computer program developed from one written by Professor Otter. The chief objectives of the analysis had been to investigate the effects of ovalling due to wind loads, barrelling due to dead load and other aspects of flue duct penetrations, possible differential base deflexions, propping forces at the internal floors and the fixing condition at the base on the behaviour of the (slightly) conical shell.

205. The velocity equations were set up using the six independent bending stress resultants, three applied loads and three deflexions of full thin-shell theory. Poisson's ratio had been taken as zero. With symmetry about a diameter it was necessary to analyse only half of the shell, boundary conditions for symmetry being fed in at the vertical joints. Initially, an analysis of the lower portion of the chimney containing the duct penetrations had been attempted but it had proved extremely difficult to simulate the rest of the stack in the boundary conditions at the top cut edge reliably, so a coarse mesh (30 ft × 15 ft) analysis of the whole chimney was effected.

206. The Writers were at this time using values of stress and moment produced in the coarse mesh analysis to provide boundary loadings for a fine mesh (10 ft × 10 ft) analysis of part of the chimney. The degree of agreement between the deflexions for the two cases would be a check on the accuracy of the transitional loading and the coarse-mesh analysis.

207. Some interesting aspects of this work were as follows.

(*a*) Fictitious density values had been used to increase the critical time interval (and hence to decrease settlement time) in two locations:
  (i) where boundary loading made calculations at particular mesh blocks unstable, and
  (ii) where the rectangular shape of the mesh blocks made the critical time interval appreciably smaller in one direction.

(*b*) As pointed out by the Authors in their introductions, it was possible to visualize the reaction of the structure during a DR analysis, especially to dynamic loading. The Writers had been able to see the effects of wind gust loading on the chimney by examining the stresses and deflexions at various stages in the settlement, bearing in mind that the damping in the calculations was not necessarily the correct structural damping. Thus, as manual calculations predicted, the ovalling was quickly set up while the cantilever deflexions took considerably longer to develop.

(*c*) By performing the relaxation in several stages and writing out intermediate results on tape, it had been possible to save computer time by manipulating the damping factor, the applied loads, and the velocities to give the desired gradient on the settlement curve. For instance, by initially feeding in grossly exaggerated wind loads with no damping, the predicted cantilever deflexions were quickly reached, at which point the wind loads were reduced to their correct values and the ovalling reverted to its proper magnitude while damped final settlement of the cantilever mode took place. Also by this means, modifications to the program and data could be introduced without 'wasting' previous settlement.

(*d*) It had been found possible to examine various construction conditions starting each time from the settled stresses and deflexions of a previously considered case.

208. The Writers were also analysing a 370 ft × 25 ft dia. single flue chimney using the same computer program with a different set of data cards.

The Authors were grateful to the contributors for a lively and informative discussion.

210. Professor Pippard had commented upon the agreement between analyses using the trial load method and three-dimensional elasticity; there was good agreement for the cylindrical Type 1 dam, but there was not such good agreement between the results for the thicker, doubly curved Type 4 dam. The other query about Poisson ratio of 0·5 was answered by Mr Snell, but, of course, Professor Pippard knew this, and was probably drawing attention to the weakness of classical theory of elasticity in its inability to deal with the layman's elastic material—rubber.

211. Professor Zienkiewicz had given a masterly summary of the important differences between the finite element and finite difference methods; it should soon be possible to define the classes of problem best suited to each method, and this would be of use to engineers needing a guide to the applicability of recent research.

212. Dr Rushton's description of work at Birmingham University confirmed the Authors' experience that where the optimum damping factor was not known, it was better to underdamp. His practice with residual errors was different; an alternative approach used at Imperial College was to rely on equations (11)–(16) to enforce compatibility, and to examine the right-hand sides or equilibrium terms of the equations of motion, such as equations (17)–(19). The equilibrium errors were then given explicitly in easily understood terms allowing full play for engineering judgement. Additional checks were provided by examination of the equilibrium of arbitrarily selected parts of the structure.

213. Mr Lane had raised several matters, which served to show how much there was yet to be done. Dynamic relaxation could be turned into a step-by-step dynamic study when a realistic damping factor was used; one disadvantage was that the amount of computer time would be proportional to the real time length of the dynamic disturbance of the prototype. Another possible disadvantage lay in the approximate nature of the numerical representation of the dynamics; cumulative errors in the values of the displacements did not matter in static analysis, but might be troublesome in dynamics. It was proposed to look into this aspect at Imperial College.

214. Mr Lane's next point was fundamental in practical analysis: how crude could a calculation be and still be good enough for proper comparisons in design? There was insufficient experience for a reliable answer. The effect of dam upon valley could be analysed, but it was probably better to consider the valley problem as a separate calculation, the reason for this being that the material properties of a dam were controlled by the engineer and the approximations used in the analysis of a dam might well be of a different order of magnitude from the approximations proper to the analysis of a natural situation such as a valley. There seemed to be no essential difficulty in taking the effects of joints in dams into account. Finally, the time spent in developing a program for the Type 4 dam was two man-months, whilst adaptation of the program for other profiles might take an extra 10 man-hours; computer time for the coarse mesh (450 displacement components) Type 1 calculation was 8 minutes on an IBM 7090.

215. The Authors were grateful to Professor Bickley for his kind remarks and suggestions; in particular the oscillatory nature of a solution, when used with a slightly undercritical damping, ought to have been mentioned in the Paper. It meant that not only was the equilibrium error known at every node, but bounds to the displacement components were also known.

216. The Authors agreed with Mr Jenkins about the interest of historical study of this field, but development and practical use were in some ways as important as the original idea. Ideas themselves are often hidden by difficult notation or inaccessibility in the literature. It was agreed that the problems solved in the Paper were simple, but they were regarded as difficult only three years before, and they were live subjects for research by the Arch Dam Committee. Although the Authors had shown in their introductory remarks that the method had been applied to real dams, the Paper had been intended to show the principles of the method and had been confined accordingly, to simpler problems.

217. Three-dimensional continuous structures with generalized geometry had been solved by tensor methods and Mr Jenkins's own paper[18] was being used as a guide line for a doubly curved dam, using both continuum and shell solutions. Interlaced nets were still used even for these problems. One of the main advantages of the method appeared to be the use of separated stress/strain and equilibrium equations. The Authors thought that the use of the displacement equations would be disadvantageous.

218. The Authors agreed with Mr Welch that the dam was not an ideal example, but it was used to obtain a comparison with other work of the Arch Dams Committee. The time interval was controlled largely by the small thickness of the dam. Since in this case the thickness of the dam was constant, pseudo-densities would not be helpful, but in polar co-ordinates some advantage would be gained, possibly at the expense of more programming. Mr Welch's remarks on the control of the number of iterations by the flexural mode of vibration had been appreciated, and one of the Authors had also had trouble with a ring beam problem. Shell solutions were quite adequate for the dam considered and were much faster, but for thicker dam types this might well prove not to be adequate owing to the neglect of transverse shear. Mr Welch's comments on the basic mathematics were noted; it was not really possible for engineers to come unscathed from an attempt to present the problem mathematically, and the Authors were glad to note that some mathematicians were attempting to give the basis in terms an engineer could understand.

219. Dr Majid was correct in his comparison of convergence rates, but this was not the whole story and it was not clear that the application of SOR with magnetic tape storage was as efficient in computer time as DR. It must be emphasized that none of the large matrices solved by DR had been beyond the core storage capacity of an IBM 7090.

220. The setting up of the differential equations and boundary conditions in other co-ordinate systems had not proved to be as difficult as Dr Majid believed, particularly when tensor transformations were used. Mr Jenkins's paper[18] indicated the method which had proved satisfactory in practice. It was however agreed that once a satisfactory finite element formulation had been made and the program written (neither of which were trivial tasks), the finite element method was the more flexible method, and, for problems with arbitrary geometry, was probably the method of choice. This applied also to the problems of stress concentrations mentioned by Mr Taylor.

221. The Authors agreed with Mr Taylor about Mr Day's originality, and they regretted their omission of a reference to his paper[19] giving valuable sidelights on application of the method to framed structures.

222. Dr King had suggested the use of Fraenkel's iteration for finite element solutions but there was probably difficulty in achieving stability. Professor Zienkiewicz had suggested its use for finite elements, but so far as the Authors knew no work had been done on it so far.

223. With regard to Mr Snell's comments, the Authors had been guilty of obscuring things where they had hoped to make them simple. It was a matter of definition. Where there was a line going through a structure, with specified displacements and stresses along it, did one say that there were 12 unknown displacements or 24 unknown displacements and stresses? In earlier work on the Type 1 dam, 24 had been chosen, although now the Authors used 12. This would account for Mr Snell's puzzlement as to how they could fit so many unknowns into a computer of moderate size.

224. Mr Snell mentioned mesh size and convergence. To begin with, the Authors' first solution was not on a particularly coarse mesh. After all, $8 \times 8 \times 3$ was really quite a fine mesh for a three-dimensional problem. This was chosen mainly because, as a matter of engineering judgement, it did not seem worth while trying anything coarser; the explanation was that it had nearly converged before advancing to finer meshes.

225. Solutions both by DR and finite elements had been compared for the dam described in the Paper, using both continuum and shell solutions, and the agreement between the methods was very good. Further work was nearing completion for the comparison of more complex shapes of dam. There seemed now no difficulty in achieving a satisfactory discretization by either method. The Authors believed the considerable effects of high Poisson's ratio for dams with rigid boundaries were real, although it was probable that the effects might be smaller when valley deformation was taken into account.

226. The difficulties at the Imperial College and at Sheffield were not connected with Poisson's ratio, or indeed with DR at all. They had occurred in solutions of the displacement formulation using non-interlacing nets. The technique adopted, for the computers then available, was a Gauss–Seidel iteration between blocks of displacements and direct solution of the blocks. The matrix was (450×450) and ought to have been adequately large, and the reasons for the inaccuracies were not understood.

227. Mr Jolliffe's suggestion was interesting when the DR approach was being used for dynamic studies, but square meshes could be a geometric limitation.

228. Since the work on which the Paper was based had been carried out, Mrs Wood had shown in private correspondence that the method was sound though the Authors' mathematical treatment left something to be desired. It was to be hoped that her analysis would be published in an engineering publication, as her aim was to make the subject clear to engineers. With this in mind the Authors would not comment on Mrs Wood's contribution, but would recommend her remarks to engineers wishing to probe the method more fully.

229. The Authors' equation (48) and its consequences were accurate, and the difference was that the equation was based on the rth iteration, and not on the $(r+1)$th as in Mr Walker's analysis. For any given problem the optimum damping factor was uniquely determined. It was connected with the fundamental vibration frequency of the structure, taking the boundary restraints into account. In practice it could easily be determined from an undamped run, as described by one of the Authors, in the introduction.

230. The Authors were interested in Dr Hussey's remarks as to other possible uses of the iteration, and would support his suggestion that caution should be used in such cases. Whilst in linear cases it seemed clear that the solution given at the end of a sufficient number of iterations approximated closely to the theoretical solution, in non-linear cases this might not be so. The Authors suggested that interested readers should compare Dr Hussey's analysis with that of Mrs Wood. His remark that DR was 'an established and useful iterative procedure which was not widely known to engineers' seemed to be a considerable overstatement. The Authors had discussed the problems of dam and pressure vessel analysis with most of the engineering stress analysts in Britain, and a large number in the USA, and neither they nor the literature indicated that DR iteration was known to engineers at all before Day thought of the technique.

231. The Authors agreed with Mr Cubitt that the use of the sum of the squares of the displacement velocities would give a norm for estimating the effective end of the iteration. However the use of underdamping which led to oscillatory convergence did, as Professor Bickley remarked, give a clear indication of a satisfactory solution. $K_{cr.}$ could of course be obtained from an undamped run.

232. The account of Messrs Postlethwaite, Davidson and Hayes showed that it was possible to use DR on shell theory. Shell analysis by DR had been carried out at Imperial College. It was possible to apply DR to the full equations of a thin shell, to Flugge's equations and to the Donnell–Jenkins shallow shell equations. Boundary conditions presented no essential difficulty. A Paper describing this work was awaiting publication.

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CORRIGENDA

The Authors wished to make the following corrections to the Paper:

§ 18: for '$\rho = \frac{1}{12}$ lb/cu. in.' read '$\rho = g/12$ lb/cu. in.'.

§ 20, first sentence: for 'plane stress' read 'plane strain'.

§ 35, equation (26): for '$E(1-\sigma^2)\alpha.\Delta\theta$' read '$E\alpha\Delta\theta/(1-2\sigma)$'.

§ 78: for 'twice optimum damping factor' read 'half optimum damping factor'.