CHAIRMAN

Nous allons maintenant commencer la discussion sur les mémoires qui ont été présentées tout à l' heure. Nous nous trouvons ici dans un comité très restraint. Il s' agit donc d' une discussion qui pourra être approfondie si nécessaire. Le nombre de demandes écrites est très limité. Mais cela ne fait rien parce que j' ai découvert tout à l' heure quelques personnes qui essayent d' obtenir des informations sur un plan privé, alors nous allons les inviter à poser ces mêmes questions publiquement.

La première communication qui doit être discutée est celle de M. Carati qui a été exposée tout à l' heure par M. Fumagalli. J' ai moi-même une question à poser: M. Fumagalli a parlé de l' augmentation du module d' élasticité du béton dans le temps et a même indiqué si j' ai bien compris qu' après quelques années cette valeur avait doublé. Je voudrais lui demander s' il a pu établir pour ce barrage-là ou pour d' autres barrages une corrélation entre cette augmentation du module d' élasticité mis en évidence sur l' ouvrage et celle constatée sur des éprouvettes en laboratoire. Si quèlqu' un a encore des questions à poser à M. Fumagalli c' est le moment de le faire.

Mr. I.W. HORNBY

This is a question for Dr. Carati; Prof. Fumagalli might have some comment to make. The question concerns the calibration of the strain gauges for use in a dam. The calibration of cubes described would, as I understand it, give a value for the modulus and Poissons ratio of the concrete in some appropriate moisture condition. The strain readings which are taken in the dam however include both elastic strains and non stress raising strains - creep, shrinkage, thermal. How are the strains, from which the stresses are to be derived, obtained from the total strain measured by the recorder?

This difficulty has also been mentioned in Prof. Fanelli's and Mr. Goffi's papers.

Prof. E. FUMAGALLI

Le rapport de M. Carati avait le seul bût d' informer sur une méthode de emploi rationnel des instruments de mesure. En tout cas pour ce qui concerne les effets qui se superposent et dérangent les mesures on peut observer que pour le "creep" dans le dépouillement des résultats, on tient compte normalement chez nous, en entroduisant une valeur de module d' élasticité réduite par rapport à ce que l' on obtient en laboratoire. Par exemple, contre un valeur moyennement de 300.000 - 350.000 Kg/cm^2 qu' on obtient sur éprouvette, on adopte une valeur de l' ordre de 250.000 Kg/cm^2. Les effets thermiques peuvent être dépouillés aussi par les mesures de variation de température. Quelquefois on place aussi un instrument neutre très proche aux instruments de mesure.

En tout cas je ne veux pas sous-valuer ici les difficultés qu' on rencontre dans la détermination des contraintes statiques dans le corps d'un barrage. J'aime seulement souligner que l' allure des contraintes mesurées dans le barrage de Frera peut être considérée satisfaisante, tenu compte des fréquents insuccès que on a rencontré en plusieurs cas. A ce sujet le Prof. Oberti aura sans doute l' occasion de nous fournir par ses expériences des informations plus exactes et détaillées.

Prof. G. OBERTI

I completely agree with Prof. Fumagalli. I can add only that the question of putting directly in place the strain meter in the block, and not the isolated instrument, is also due to the fact that the concrete of a dam is made with a coarse aggregate of very great size, about half a foot; and when you put directly in place an instrument which has practically the same length you have to take into account the anisotropy of this material, this concrete. On the contrary, when we put in place a block which had been directly calibrated in the laboratory taking into account also the effect of anisotropy, we found that the strain measurements were sometimes very different from those recorded from the instrument alone. This is my opinion on this very important question.

Furthermore, due really to the possibilities we may have on the spot, due to the fact that this block has been powered in the same quality as the real concrete, and moreover that the shrinkage effects are practically finished, in any case it is necessary to grout all along the surface of the block in such a manner as to have security of continuity, because otherwise the measure changes. But the question is quite a difficult one.

CHAIRMAN

Y a-t-il d'autres questions au sujet de la communication de M. Carati? Apparemment pas! Et au sujet de la communication de l' ENEL sur les mesures dans d'autres barrages en voûte? Non plus! Dans ce cas nous pouvons passer à la communication de M. McNeice. D' après les statistiques c' est lui qui a parlé le plus longtemps tout à l' heure; il a donc droit à la question écrite!

VICE-CHAIRMAN

I have a question from Dr. Kettner: how is the gravity load (self-load) taken into account in your programme STRIDE? When block joints are grouted after the dam has been constructed to the crest, stresses due to gravity (dead) load are the same as for gravity dams: In this feature incorporated in your programme?

CHAIRMAN

J' ai une autre question à vous poser, si vous le permettez. Il s' agit de la méthode de calcul que vous employez, ou plutôt des conditions aux limites, c' est-à-dire des problèmes soulevés par les fondations. Vous nous avez parlé tout à l' heure du barrage, mais je n' ai pas noté que vous ayez mentionné le problème posé par les fondations et en particulier par l' anisotropie du rocher. Comment introduisez-vous ces facteurs dans votre programme? Merci.

Prof. G.M. MCNEICE

With respect to the first question the answer is that we have not taken into account the grouting pressures. What we have assumed and I agree that this is highly approximate, is that once the block is put in and construction has gone around that block, that it is completely monolithic with the other blocks. Now this is not correct of course. The reason why we have not taken that into account was that we were only trying to see if we could use the system for this type of investigation from the viewpoint of geometrical convenience, because otherwise there is a lot of data to produce. We were well aware of the problem and we had thought one attack would be to introduce an expansion, an internal force system, such as an increase in temperature or certain variations to simulate the grout pressure. I think the question is addressed to low dams in which the grouting may not start until the dam is well up. In cases like that the results of our work would be absolutely meaningless.

How we take deadload into account generally is fairly straight forward, we simply create energy equivalent loads within a block and add this to the system. So the loads themselves are approximately correct, but the effect of grouting is not taken into account.

With respect to the second question, as to the boundary conditions used, I spent many hours. Although I still have hair left I tore much of my hair out trying to come to grips with what type of foundation model I should use for the very first dam that we did. We did not have this system then. It was just a dam that happened to come to the university and they wanted analysis done. I had to do it in the quickest possible way. At that time, having looked into the trial load analysis, the type of boundary conditions used was that due to Vogt. When I looked at that particular model and realized that if I were to use finite elements intelligently on the North American continent, I would have to convince the people using trial load that at least I could use the same type of boundary condition as they could. Then we have some base of comparison between the two methods. Incidently, I have not seen any evidence to show that using large elements for a boundary model comes anywhere close to the Vogt type of response.

I cannot answer the question either right now, because we have not investigated it fully, but I felt that at least going the Vogt direction I would end up with a very simplified boundary model. In fact when you do it you find that the stiffness coefficients which add into the structural stiffness matrix do not introduce any additional equations. They do not even introduce any additional non zero terms.They add right into the existing stiffness matrix.

I will go through this briefly, I have some slides to explain it. Let me just very briefly look at the Vogt boundary so that I can show how we made almost an equivalent model of this into finite elements. Basically if you take a section of the half space and consider that there are these loads, as Vogt approximated the behaviour of this little rectangle in here, you basically have translations and rotations concentrated at a single point which he approximates by having average response over some little area.

This is of course a well known thing but by those of you who work on arch dams, the b/a ratio is calculated in an approximate way, but this is the little area here over which we are going to be dealing with the loads applied at this particular point. So eventually we will have a series all the way around these little strips and all the elements that come in contact with this surface will have a number of these little strips inside. So we have a Vogt boundary in each little strip and a number of little strips for the face of every three dimensional element.

The way we calculate the b/a ratio is simply to take the intersection of the boundary, the intersection of the dam on the rock, take the boundary area and flatten it out like this, then approximate that area by a series of rectangles and add up these in order to get the proper b/a. It is a little more refined than what is usually done in practice, but it is done automatically in the computer.

The model with the Vogt boundary is a flexibility matrix because it relates displacements to the forces at this point. So the bending moments, and transla tional forces are related through a flexibility matrix to the corresponding rotations and displacements. At this point you have a flexibility matrix and you can almost think of this as being a cantilever beam. This area here is a little strip that I'll be describing. Since Vogt uses the infinite space equations, then his problem is already well posed, the boundary conditions are already applied and therefore it is not a numerically sensitive problem. That is, it is tied down and so you can invert this matrix and produce a stiffness matrix. This would be the Vogt stiffness matrix then in the little strip area.

This is the area we have to concern ourselves with. The elements can be curved like this and we use local curvilinear coordinates. You relate the displacements here to the displacements at the end points and in the center, at the outside of the strip, by a matrix, let's call it C1.

Everytime we relate displacements one to another we have a matrix which relates them, so that C1 relates one set of displacements, R relates these displacements to another set, C2 relates these back to the concerns and so you go through the process of introducing these into the original stiffness matrix. This simply describes the displacements at the ends to the Vogt displacements.

Using equality of energy one simply sets up an equivalent set of forces which simulate the Vogt energy or the total work done. Although it looks a little complicated, it is really very simple. The original Vogt matrix is only a diagonal matrix with a few terms in it. These transformations simply expand this little k up to a large matrix, and of course you add one strip on to the next and accumulate these and you end up with the same size of stiffness matrix as the face of the three dimensional element has. That is, if it is an 8 noded face on the side, it would be 24 x24. That immediately adds in to the structural element that comes on to the boundary.

In our work we have always used it and until we can research it fully to see how it responds to other types of derivations and how it compares with a very complex arrangement of three dimensional elements, I really cannot say whether it is better or not. I know it is better from the point of view of computation and it is equivalent to what is used in trial load, that much I can say.

CHAIRMAN

Merci, M. McNeice pour cette explication que j' ai compris, au moins à peu près. Peut-être quelqu' un n' a pas la même opinion que vous à ce sujet...

Prof. O.C. ZIENKIEWICZ

I wanted to comment on the question of including foundation deformability in the analysis. The standard Vogt foundation coefficient method introduced by the U.S. Bureau of Reclamation in the early ' 30s has persisted till today and it is essentially a Winkler - type approximation where the influence of one element on another is not taken into account. It is very efficient computation however, and despite being such a crude approximation it is found to give remarkably good results in much of dam analysis. Why is this?

At a Sympsium on large dams held at the Institution of Civil Engineers some years ago, it was found that the influence of foundation deformability was slight on the general dam displacements and factors of of two or more on the moduli of foundations made pratically no difference in the results of the stress and displacement analyses of the structure itself. Perhaps we are lucky here.

However, Dr. Lombardi indicated that it would be useful to extend the finite element analyses into the foundations to deal with discontinuities and peculiarities of it.

We find it today, to be reasonably economical to represent the foundation by a few crude elements of high order extending some distance into the foundation. The procedure however, is not too economic and much work will have to be done on new elements to model the foundation deformations well. Here, perhaps we should think of introducing infinite elements, or alternatively, coupling the solution with integral procedure which deal well with semi - infinite regions.

I would like to congratulate Prof. Mc Neice on the excellent work done on generating the shapes of dams. The matter of shape description and its incorporation in the analysis and optimisation of dams is of paramount importance and much work on graphics has to be done.

Prof. G. M. MCNEICE

With respect to using elements in the boundary I agree that it is almost mandatory to use elements if you have anisotropy. If you have slip planes for example that you must take into account, there is no other way of doing it. The point is that many dams are designed using the Vogt foundation and this is why I have introduced it here. I agree that with Vogt there is not a direct mathematical link by coefficient from one element to another, but the stiffness coefficient on one element is determined and therefore influenced by the physical presence of the infinite media next door. These values are connected in that sense only.

I do agree that it is not connected sufficiently. Our experience has been, and we have used as many as 62 elements in one dam, which give a lot of boundary points, that we found no major discontinuity between the behaviour as we came down the valley. So you can, with respect to anisotropy, use the Vogt effectively in the sense that you can put in the modulus of elasticity of different strips, not only at different elements but throughout different elements. So you can get an approximation in that way. But coming to the real point of the problem, you mentioned a very interesting thing. You mentioned the 1 over R functions; we have already developed an infinite element. We have taken a general series solution to the basic differential equation and you do not have to have a flat or half space. It is of semi-infinite extent and you can have any topography you like. We create the stiffness coefficients for this half space by taking the finite element of the dam, placing it on the surface and stating that the traction or the charge between the two bodies must be equal-the distribution on that body and the distribution on that body-must be equal within a least square sense. What this does, is to allow yo to couple the stiffness matrix of the dam and the element if you have one with the solution in terms of undetermined coefficients. We have all of the mathematic complete, we have done two dimensional testing and we have some fairly interesting results I think. The 3D is currently being computed and unfortunately my Phd student who is doing the work was such a brilliant man that a company has hired him for an extremely large sum and he has perhaps another three or four months before he finishes his degree. So I hope that within 4-5 months we'll have something published. We are going to write a short precis on the mathematics.

CHAIRMAN

On voit donc qu'il y a un problème de rigidité. Nous avons deux matrices de rigidité, celle du barrage et celle de la fondation qu'il faut mettre en corrélation. Je voudrais poser une question à M. Fumagalli puisqu'il est là, en première file, c'est une victime toute désigneé. Est-ce que vous avez déjà eu l'occasion de déterminer sur modèle des matrices de déformation du terrain en tenant compte de l'hétérogénéité du terrain, de l'anisotropie du terrain etc. Il s'agit de matrices dans le sens de celles que vous nous avez montrées hier, au sujet des coefficients de déformabilité du barrage déterminés en vue d'un calcul sismique? Peut - être d'autres personnes peuvent également nous faire part de leurs études.

Prof. E. FUMAGALLI:

Pour répondre à la question de M. le Président, je retiens qu'on pourra bien exécuter des examens de comparaison entre résultats de modèles et de calculs pour ce qui concerne la déformabilité des fondations d'un barrage en utilisant les modèles de type traditionnel. Dans ces modèles, en effet, le rocher est généralement reproduit par corps en roche élastique, homogène et isotrope, c'est-à-dire en conditions tout à fait comparables aux hypothèses de calcul.

Plus difficile, à ce moment, il me semblerait de pouvoir utiliser les résultats des modèles géomécaniques: ces modèles sont des moyens de controle à la stabilité d'ensemble. Dans ces modèles en effet la rupture de l'équilibre est déterminée fondamentalement par les systèmes de discontinuités, quelquefois assez complexes, rupture encore difficile à vérifier par calculs.

Tout celà pour ce qui concerne une vérification de type statique; pour une vérification de type sismique, la question doit être posée à mon collègue l'Ing. Castoldi qui malheureusement n'est pas là.

CHAIRMAN

Y a-t-il encore quelque chose à ajouter sur la communication de M. McNeice? Si ce n'est pas le cas, nous pouvons examiner celle de M. Fanelli. Pour ma part j'ai quelques questions à poser. M. Fanelli a étudié une galérie tangentielle au barrage, c'est-à-dire une galérie qui se trouve à peu près dans le plan moyen du barrage et il a trouvé que l'influence sur l'état général des contraintes était très faible. Je voudrais lui demander s'il a eu l'occasion d'étudier aussi des galéries placées perpendiculairement au parement, parce qu'on peut supposer que dans ce cas l'influence serait bien plus forte. D'autre part je voudrais lui demander s'il a fait une comparaison entre ses études et les formules qui sont données par exemple par le Bureau of Reclamation au sujet des contraintes secondaires.

Un autre point qui pourrait intéresser les présents c'est de savoir de quelle façon - plus en détail, puisque vous nous avez dit l'essentiel - vous analysez séparément les effets thermiques et les effets statiques du barrage.

Est-ce qu'il y a d'autres questions à poser à M. Fanelli? De cette façon il pourrait répondre à toutes à la fois.

Prof. O. C. ZIENKIEWICZ

I am not sure of the arguments made by Dr. Fanelli for reducing the thermal stresses. These do not appear to be consistent with the deformation and temperature drop. Could he perhaps explain.

Prof. M. FANELLI

I'll try to answer the different questions in the order they were put. First of all, we analyzed the case of a tangential tunnel in the middle surface of an arch dam because that is the normal position for most of the inspection tunnels - and I think this is logical, because we try to cut the flow of the stresses in the neighbourhood of the neutral axis for bending components. Of course it would have been very interesting also to study the case of a tunnel perpendicular to the facings. We did not do that for lack of time and also for the same reason we did not compare our results with the formulas of the Bureau of Reclamation, but I intend to do so and to publish the results if they prove to be interesting.

I'll answer the question put by Prof. Zienkiewicz before answering the second question put by Prof. Lombardi. What I was trying to say before is this: when we compare displacements measured on actual dams and displacements computed on a mathematical model we must put together two different components: displacements caused by live loads and displacements caused by thermal effects. We do this on our mathematical model - at least that is what we are doing - in the frame of a simple elastic model and we assume, to start with, a certain thermal expansion coefficient and a certain modulus of elasticity. We compute displacements and provided we put in the right value of our constants they come off pretty well, (they can be pretty well comparable with what we observe in reality).

But when we come to compute stresses in the structure, then usually we wind up with thermal components which seem exaggeratedly large. This is a very common experience among arch dam designers. When you are designing an arch dam and compute the thermal effects, if you use the same modulus of elasticity as for other effects, you obtain very very large thermal stresses, so large that they would lead to a very critical appraisal of the safety of the structure. And so an empyrical viewpoin has been developed -but with some justification of course - saying that for computation of stresses due to thermal effects we are allowed to use a modulus of elasticity which is far smaller than the one we use for computing displacements due to live load (for instance) and this is explicitly born out by many regulations. I was citing the Italian dam regulations.

This is of course an inconsistency. One could reason in a very different way, one could say, well the thermal stresses develop very slowly in time - at least for the majority of the mass of concrete - and so there must be an important creep effect These stresses are stresses which are in equilibrium, they do not have to face any live load and so they are compatible with the situation in which a certain state of deformation appears externally and the stresses tend to dissipate. But in my opinion this is not very consistent with what we observe on actual structures, because in an actual dam we have a live load due to water, which in many cases has a cyclic behaviour. The reservoir is going up and down with fairly large variations and if we look at a typical curve of a reservoir impoundment we observe that there is a fairly cyclical component.

The same kind of cyclic behaviour is valid for thermal effects. So why does the one dissipate, and the other not do so? This is very hard to conceive, unless there is some factor which makes thermal stresses basically different from equilibrium stresses. This is what I was trying to say before, I don't know if I have made myself clear now.

Prof. O.C. ZIENKIEWICZ

Why are computed thermal stresses judged "too high"?

Prof. M. FANELLI

Well, simply because they would produce in some regions of the dam very large tensile stresses and normally this would appear to lead to craking. We know that if the stresses were of such magnitude and of a tensile nature, the concrete would not withstand them. And so there is a feeling, but no more than a feeling, I admit it, that those stresses are too large to be actual, to be real. There must be something which we do not take account of. Of course one can imagine many mechanisms that can be taken into account for this, but it is very difficult to choose between them and to have a consistent model of the material.

If there is some time left I could present some results for the system of displacement control that we have been developing for some years now for the ENEL dams. Basically the situation is the following: we have to follow during their life a very big number of dams. They are controlled by very different means. Basically we follow of course the water level variations, which are recorded, the temperatures of the air, of the water and at a certain number of points inside the concrete(this is a common practice for other countries), and we control the displacements of a certain number of points of the dam. All these quantities are plotted and are given over to the control authorities. Of course this is not an end in itself because all these things must be interpreted and they must be given a meaning in order to assess the safety of the structure. What we have been trying to do for some years now is to find a rational basis to give such a critical appraisal of the measurements we make; and among the different ways one could choose to do so we chanced upon this scheme. We construct a mathematical model of the dam, which is capable of producing answers if we subject it either to water level variations or to thermal variations. We construct by means of this mathematical model the so - called influence functions, unit influence functions. What do we mean by that? We mean that we produce for every point that we must control displacement-wise functions relating these displacements to water level variations and functions relating their thermal displacements to unit temperature variations in each thermometer. This is done by a mathematical trick, but I cannot extend upon that, the only thing I can say is that it works. I have published some details on other occasions and I can give you the bibliography.

By these unit functions we are able to reconstruct very rapidly or to forecast, if you want, the displacements of every point due to whatever level variation in the reservoir and whatever thermal situation as indicated by the control thermometer. Now we end up with theoretical displacements, because they are drawn from a mathematical model, given certain assumptions. These theoretical displacements must and can now be compared to the displacements we actually observe on the structure under the same conditions of water level and temperature. The feeling that we have is that as long as this comparison gives us a difference which is contained within a rather narrow confidence band, we must say that the actual structure corresponds to our mathematical model. So we prove the model by ascertaining that this agreement happens over a certain number of years. If this is the case, then we reason in the following way: let us observe the dam in time, let us forecast its displacements as time progresses, by means of our unfluence functions and compare them all along with the observed displacements. If the agreement we had for the past continues in the future, then we are allowed to say that this model is consistent with reality. As soon as this agreement is impaired and the difference between the theoretical and actual displacements goes out of the confidence band, we are allowed to say that something has happened - we do not know what, but our model no longer corresponds to the reality. Maybe the material properties have changed, maybe a crack has occurred, maybe the foundations have changed - we do not know, but something has occurred. And so we may sound an alarm, so-to-speak, go and see what has happened. This is very briefly the principle of our control method and if you allow me some more minutes I can show a few slides to illustrate this principle.

This is just to show you very rapidly what is the basis of the mathematical trick for decomposing temperature variations into unit temperature variations pertaining to each thermometer. Supposing (Fig. 1) each white circle represents a thermometer, this surface is the image of the unit temperature distribution - or shape function - that we relate to unit temperature variations for that thermometer and zero temperature variation in all other thermometers. Fig. 2 is a study we made of the error we commit in making the assumption illustrated in the previous slide and this error has been found to be minimum for certain optimal distribution of thermometers along the thickness, which depends of course on the thickness itself. Here the optimal configurations of a thermometer are illustrated and the errors we commit, which amount to no more than about 10%.

Fig. 3 is another comparison for another dam, very much like the one I showed you before: see the water level, hydrostatic component of theoretical displacement, thermal component, comparison between observed and synthetized displacements and difference. I am saving for last an illustration of how you can automatize the whole thing. Fig. 4 is the crown section of an arch dam in which there is a pendulum. The main displacement observations are made by means of this pendulum. In this case we installed on the dam a small analog computer into which we fed the coefficients of our unit influence functions. Into this analog computer we feed the signals from the electric thermometers and from a pressure transducer which signals the water level, so that the analog computer gives as an output the synthetized theoretical displacements relative to these two causes. This can be recorded on a strip of paper. Fig. 5 is the thermometer distribution for that dam. Fig. 6 is a developed view. Fig. 7 a comparison between observed and theoretical displacements over some years. Here we had a derangement of the thermometer output and after that was corrected we had a perfect agreement again.

Fig. 8 is the analog computer we installed, with 24 potentiometers to adjust the thermal coefficient and 10 diods to adjust the reservoir level function. Fig. 9 is the kind of trace we obtain as output of the analog computer and this can be directly compared with the trace of the same kind which comes out from the automatic pendulum recorder. Thank you.

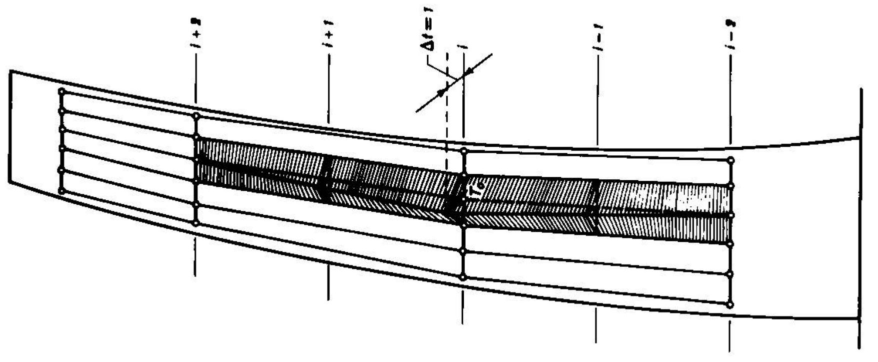

Fig. 1

Distribution de la température dans la section verticale par variations unitaire de la température indiquée par un thermomètre T_o.

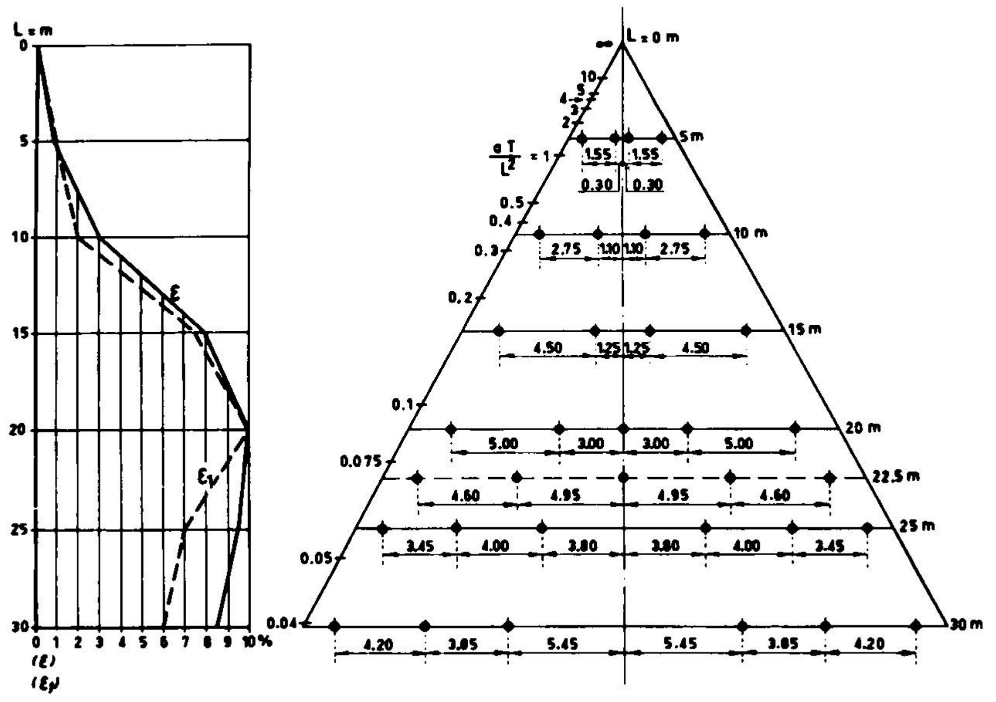

Fig. 2

Distribution optimale des thermomètres dans un mur de section horizontale variable.

(ε) Erreur % sur τ.

($ε_1$) Erreur % sur Δ τ.

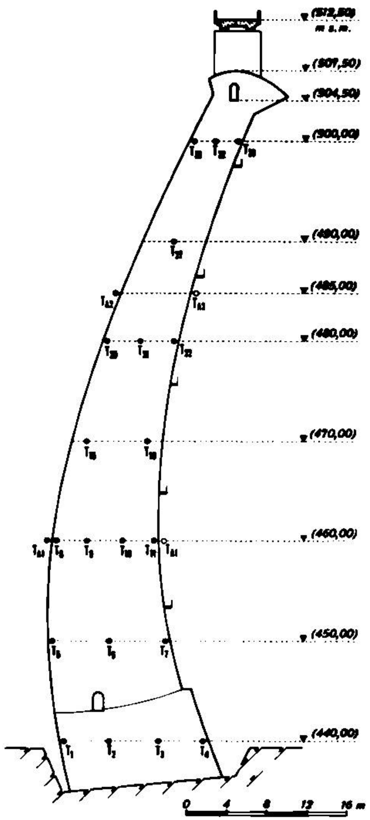

Fig. 5
Barrage de Talvacchia.
Emplacement des thermomètres dans le béton de la section de clef.

- ∘ thermomètre dans l'air,
- • thermomètre dans l'eau,
- • thermomètre dans le béton.

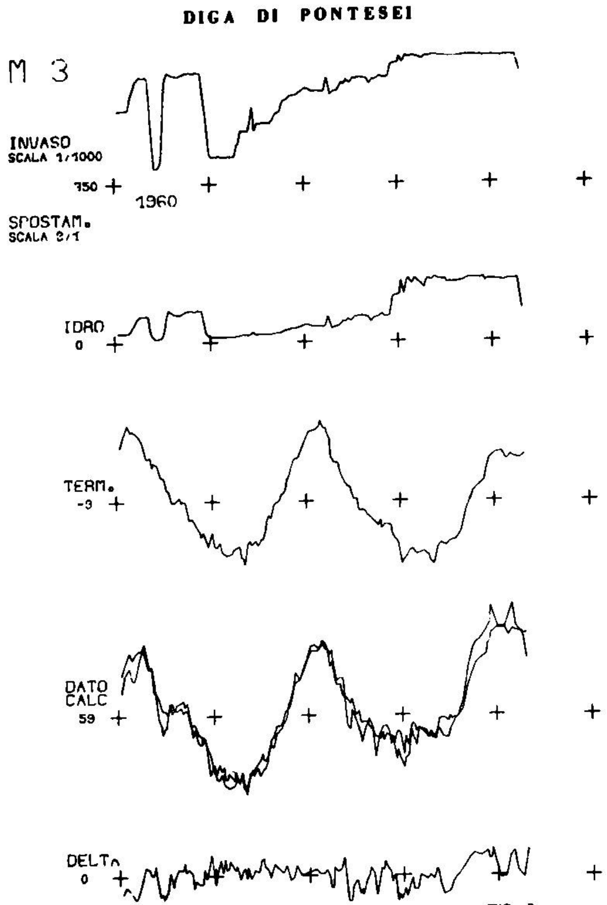

FIG. 3

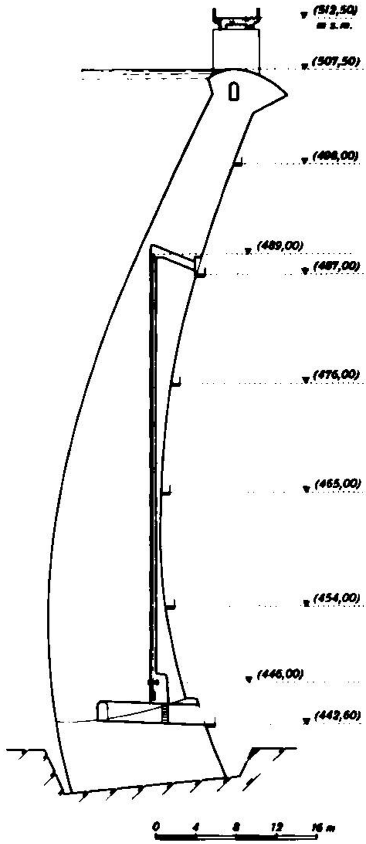

Fig. 4
Barrage de Talvacchia. Installation du pendule.

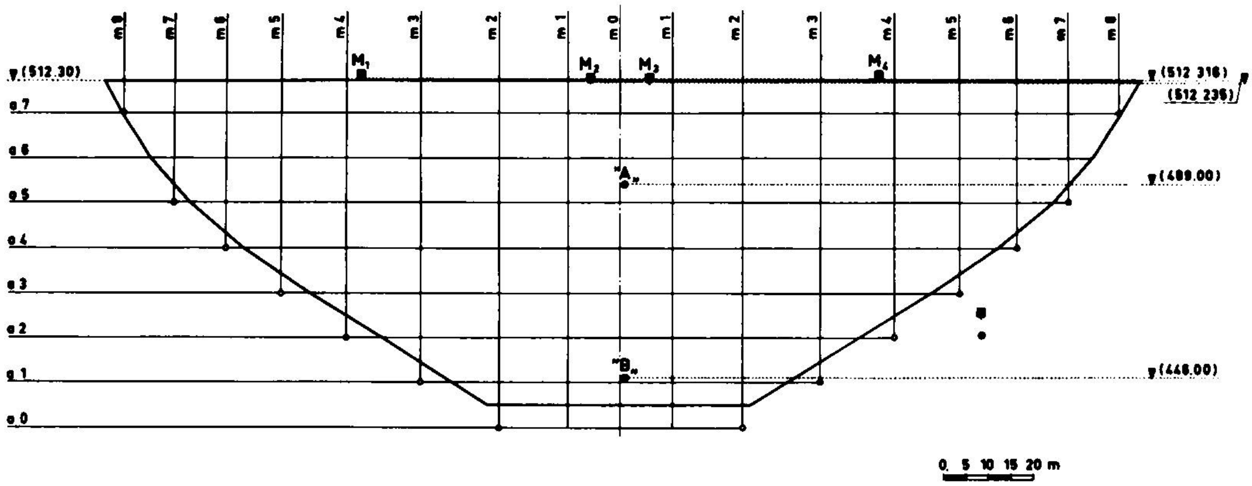

Fig. 6

Barrage de Talvacchia. Répartition arcs-consoles pour la préparation du programme automatique de calcul des déplacements horizontaux et position des alignements et du pendule.

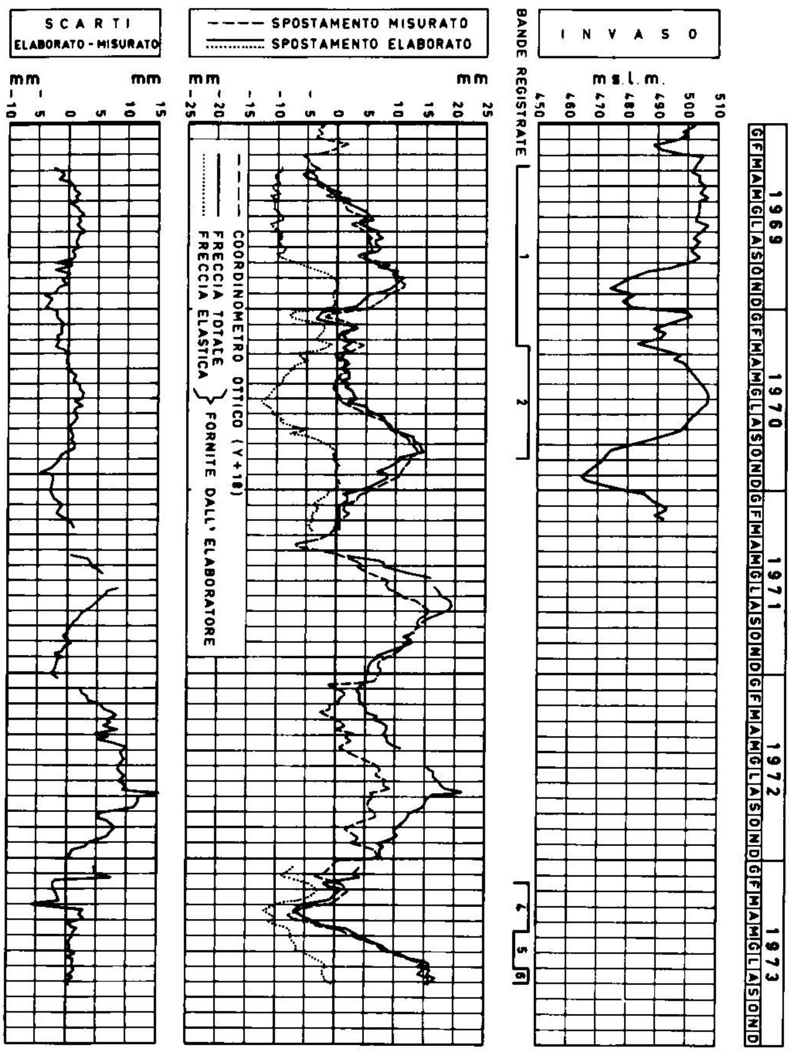

FIG. 7

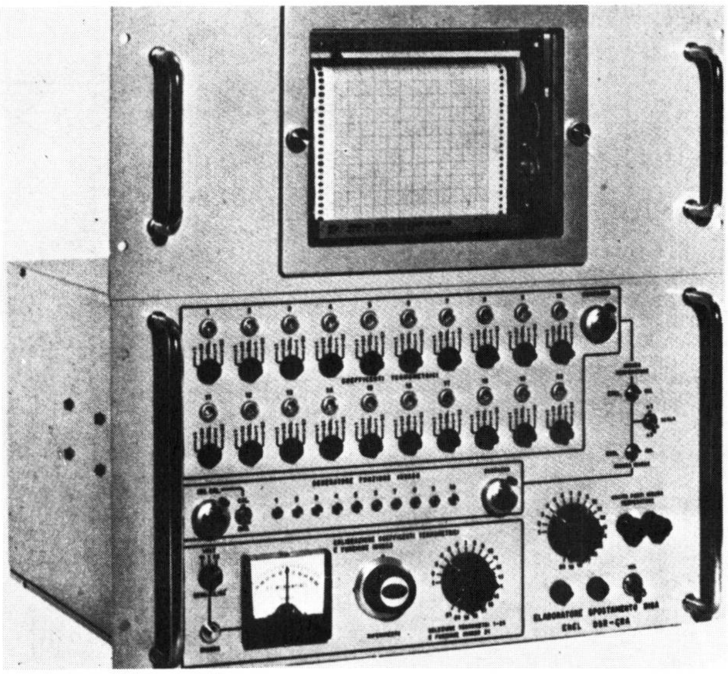

FIG. 8

Barrage de Talvacchia. Elaborateur analogique spécialisé pour l'enregistrement continu du déplacement d'un point du barrage calculé avec la théorie a priori.

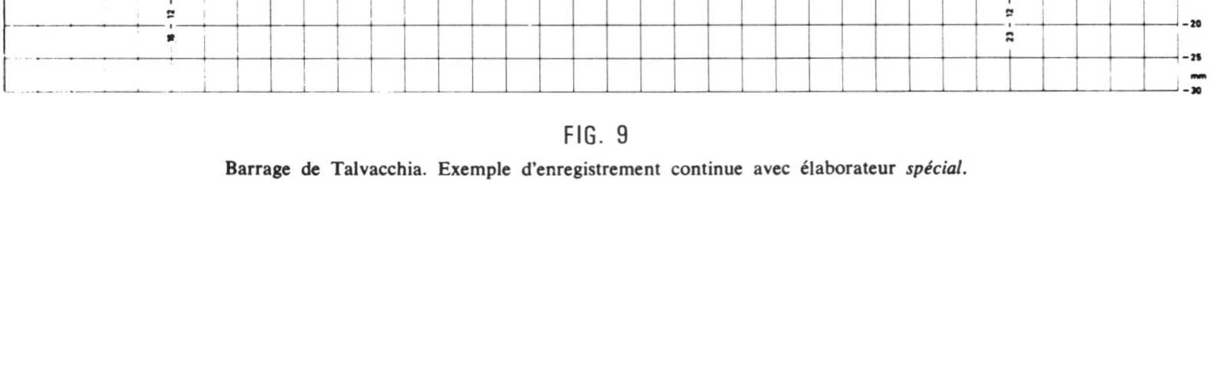

FIG. 9

Barrage de Talvacchia. Exemple d'enregistrement continue avec élaborateur *spécial*.

CHAIRMAN

Merci M. Fanelli. Nous pouvons passer à la communication de M. Kettner, est-ce qu' il y a des questions? Ce n' est pas le cas! M. Kettner a sans doute été trop clair et explicite - c' est une erreur, il faut toujours laisser un peu de suspense! Alors passons à la communication de M. le Prof. Berio, quelqu' un a des questions à poser? Pour ma part j' ai une question un peu naïve, mettons que je sois laïque dans le domaine. Vous calculez une probabilité de rupture pour l' ouvrage avec un développement mathématique que nous avons lu dans votre communication, mais avez-vous fait quelques reflexions au sujet de la valeur numérique de la probabilité de rupture que l' on peut admettre et comment peut-on en déduire un coefficient de sécurité dans le sens habituel?

Prof. A. BERIO

Celle que vous avez posée c' est la question la plus difficile dans un calcul probabiliste. Nous n' avons pas songé à cette question, c' est une question très difficile à traiter parce qu' elle entraîne des considérations techniques et des considérations économiques, et même des considérations sociales, parce qu' on doit calculer la possibilité de perdre des vies humaines. C' est une question que nous ne sommes pas en conditions de résoudre.

CHAIRMAN

S' il n' y a pas d' autres questions, j' en aurais une question à poser à M. Goffi, sur la dernière communication; c' est la question que j' avais formulée au début de la séance. Avez-vous pu prouver une corrélation simple entre la troisième contrainte, c' est-à-dire la contrainte normale au parement aumont et la pression de l' eau?

Prof. L. GOFFI

Dans la figure n. 6 nous voyons que la contrainte sigma Y est très faible; évidemment il n'y a pas de contrainte sur le parement aval, puisque il n'y a pas de pression hydrostatique. Sur le parement amont on a la pression hydrostatique et l'on devrait avoir la valeur de sigma Y qui devrait être correspondant à la pression hydrostatique maximale qui peut arriver dans notre cas seulement à 50 m environ, car nous avons examiné des lectures qui se référaient à un niveau d'eau qui n'était pas le maximum. Malheureusement les instruments sont allés hors service avant que le niveau de l'eau eusse atteint le maximum. Alors nous voyons que, avec 50 m environ d'eau, il faudrait avoir quelque chose comme 5 kg/cm^2 et ceci est vrai jusqu'à un certain point car nous avons le sigma Y qui marche très vite le premier jour après le bétonage: évidemment c'est un effet qui n'est pas dû à la charge. Après nous avons le diagramme qui se mantient à une valeur avec des oscillations peu significative. Ensuite nous avons un accroissement d'environ 15-20 kg/cm^2 et peut-être ceci est un peu fort par rapport à la pression hydrostatique. Il faut tenir compte que dans ce barrage, environ 10 m au dessous des emplacements des extensiomètres, nous avons le joint circonférentiel qui, peut-être, trouble les données expérimentales ; alors il faut prendre ces données avec un certain bénéfice d'inventaire.

CHAIRMAN

Y a-t-il d'autres questions pour M. Goffi? Non! Dans ce cas je vous remercie. Nous avons ainsi terminé la discussion des mémoires qui ont été présentées. Je crois que nous pouvons conclure cette discussion. Je voudrais remercier tout d'abord les auteurs des mémoires pour le grand travail qu'ils ont fait et pour la contribution très intéressante qu'ils ont apportée, ainsi que les personnes qui ont partecipé à cette discussion, qui a été fort animée.

Nous pouvons constater que malgré les études faites jusqu'à présent dans le domaine des barrages depuis des dizaines d' années, le problème reste encore ouvert et il y aura matière certainement pour de nombreux autres symposiums - je vois M. Zienkiewicz qui pense déjà au symposium de Swansea de Septembre 1975 et sourit en voyant tous les problèmes qu'il pourra résoudre à ce moment là -. Les problèmes les plus importants qui restent ouverts ce sont peut-être les suivants:

- un premier problème est celui des modèles rhéologiques à admettre pour le béton, car pour ma part je partage les hésitations de M. Fanelli. En effet nous constatons souvent que les tractions, que les températures devraient induire dans le béton, ne se remarquent pas. Elles ne se traduisent pas par des fissures comme elles devraient le faire "logiquement".
- un autre problème dont nous avons parlé aujourd'hui est celui des fondations. Comment tenir compte de toutes les particularités souvent très peu connues du terrain? De l'anisotropie? Des discontinuités? Et cela sous deux points de vue: celui des déformations, mais aussi celui de la résistence et de la sécurité de la fondation.
- le troisième problème est celui des sollicitations locales dans le corps du barrage. Car si on peut avoir assez facilement une bonne idée du comportement général du barrage, il reste encore divers problèmes, tel que celui des galeries dans des endroits peu favorables, celui des tampons etc. où des études de contraintes à trois dimensions devront être certainement encore entreprises ces prochaines années.

Je vous remercie donc tous de votre participation. Si je suis autorisé à clore cette session de discussion je ne suis pas autorisé à le faire pour le symposium lui-même. M. Oberti ayant ouvert ces journées très intéréssantes, Vous addressera quelques paroles finales.

Prof. G. OBERTI

Only a few words because it is rather late, but I am glade of the success of this seminar and I thank you wholeheartedly for having contributed to this success and particularly the Chairmen, Co-Chaimen and the whole staff of ISMES. Personally I am satisfied and I hope that you are satisfied too.

Only a few words about the work developed at ISMES. I think that in particular structural models even today constitute a powerful means of research that needs to be refined through modern advances in the methods of reproduction, testing and measurements, as you perhaps have seen during the visit to the ISMES laboratories.

Models represent a reliable and above all a safe method of investigation, suitable for you in the elastic range and beyond to failure, as much as for historical and ancient monuments - I have tested the Duomo di Milano - and for modern works or structures, with special difficult design and with difficult boundary conditions. They are available for tests in areas where analytical methods, even the very advanced ones, are maybe usefully helped by experimental studies.

I thank you and I close this Seminar on concrete structures subjected to triaxial stresses.