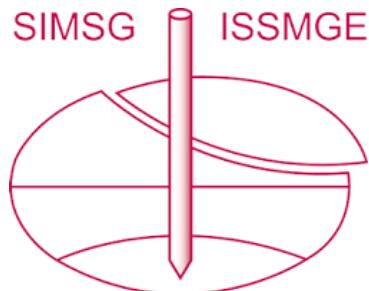


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Roads, Runways and Rail-tracks

Routes, Pistes d'Envol et Voies Ferrées

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M. R. Peltier	<i>France</i>



M. R. Peltier

General Reporter, Division 4 / Rapporteur Général, Division 4

The Chairman

I declare the seventh session of the fourth conference open, and I will ask the General Reporter to introduce his report.

General Reporter

1. Monsieur le président, mesdames, messieurs, la quatrième section du congrès de mécanique des sols est consacrée à la

géotechnique routière, c'est-à-dire à l'étude du comportement des sols sur les routes, les pistes d'envol et les voies ferrées. La géotechnique routière est d'ailleurs la plus jeune des branches de la mécanique du sol et, ce n'est qu'au dernier congrès, à Zurich, qu'une section spéciale lui a été consacrée. Mais son développement a été très rapide et 20 communications ont été présentées, ici, sur ce sujet.

Je pense que le rôle du rapporteur général est d'essayer de vous présenter une synthèse, aussi courte, mais aussi claire que possible, de l'état actuel de la géotechnique routière, afin d'ordonner au mieux la discussion qui va suivre, et de la rendre plus facilement compréhensible, donc plus fructueuse. Cette synthèse ne sera pas seulement basée sur les communications présentées à ce congrès, mais aussi sur les études publiées depuis le congrès de Zurich ou entreprises dans les laboratoires et dont j'ai eu connaissance.

En fait, on peut distinguer dans la géotechnique routière trois grands problèmes qui sont:

- le compactage des sols,
- le calcul des épaisseurs à donner aux chaussées et,
- la stabilisation des sols.

2. Le compactage a fait quelques progrès depuis le dernier congrès de Zurich par suite, d'une part, de l'apparition sur le marché de nouveaux engins de compactage et, d'autrepart, grâce à des études expérimentales. Pour ces dernières on doit noter le développement des fosses de compactage, telles que celles que nous avons pu voir avant hier au road research laboratory à Hardmondsworth; ces fosses permettent d'effectuer des expériences grande nature sur les sols; d'où, des résultats beaucoup plus valables que ceux obtenus dans les moules métalliques classiques de faibles dimensions.

Mais ce sont sans doute les nouveaux appareils de compactage apparus ces dernières années, tels que les cylindres vibrants légers ou lourds, qui ont fait faire les plus grands progrès dans l'art du compactage. Ces cylindres vibrants permettent non seulement d'améliorer la compacité finale, mais aussi et surtout de réduire la durée du compactage d'où, possibilité d'économies. La vibration ne peut toutefois être utilisée que pour certaines catégories de sols, notamment, les sols graveleux et les sols à cohésion faible ou nulle.

Il y a lieu d'indiquer aussi, au sujet du compactage, les récents perfectionnements des appareils de mesure *in situ* de la compacité obtenue. Les nucléodensitomètres, notamment (c'est-à-dire les appareils de mesure de la densité à l'aide d'isotopes radioactifs), ont donné des résultats intéressants et prometteurs. Je ne partage pas à leur sujet le pessimisme manifesté l'autre jour par le rapporteur de la deuxième question.

3. Le calcul de l'épaisseur à donner aux chaussées a tout d'abord progressé, grâce au perfectionnement des méthodes classiques basées sur le critère de portance du sol (CBR notamment). Mais une forte tendance s'est dessinée dans plusieurs pays pour substituer à ce critère de portance, le critère de déformabilité, car c'est généralement lui qui conduit aux résultats les plus défavorables, notamment, si la chaussée comporte des couches bitumineuses épaisses. On peut citer à ce sujet la méthode du N. N. IVANOV (4/6) en U.R.S.S. Ces méthodes sont généralement basées sur une solution mathématique du problème des sols élastiques à deux ou trois couches, solution dans laquelle on fait intervenir un certain nombre de paramètres que l'on détermine ensuite expérimentalement pour faire coïncider la théorie avec la réalité. Je vous signale qu'une méthode de ce genre est en cours d'élaboration en France en partant de la solution mathématique du problème des trois couches élastiques, solution qui a été obtenue à l'aide de machines à calculer électroniques. Un orateur doit d'ailleurs nous faire un exposé à ce sujet au cours de la discussion.

D'une façon générale, cette tendance à substituer le critère de déformation au critère de portance est très importante et elle se développera rapidement au cours des prochaines années. Elle est susceptible de bouleverser complètement nos méthodes actuelles.

A ce problème des épaisseurs des chaussées il y a lieu de rattacher les études concernant la teneur en eau d'équilibre des sols. Celle-ci est commandée d'une part par la succion et les conditions hydrologiques du sol et, d'autrepart par des conditions extérieures telles que les variations climatiques. A ce sujet, les rapports présentés sont assez contradictoires, en apparence tout au moins quant à la possibilité d'évaluer avec précision cette teneur en eau limite. J'espère que la discussion qui va s'ouvrir apportera quelques lumières sur ce point, de nombreux orateurs s'étant fait inscrire pour discuter ce problème.

Il y a lieu aussi de rattacher à ce problème, celui de l'influence de la répétition des charges sur la portance des chaussées. Ce phénomène est à la base des calculs de la capacité des pistes d'envol; aussi, a-t-il donné lieu à deux communications importantes, celle de N. W. MCLEOD (4/12) et celle de G. MORALDI (4/14).

Enfin, bien que ceux-ci n'aient fait l'objet d'aucune communication à ce congrès, il faut citer les importants essais pratiques du Washo, aux U.S.A., pour le calcul des épaisseurs à donner aux chaussées. Leurs résultats sont particulièrement intéressants pour la vérification ou la mise au point des formules d'épaisseur.

4. Le troisième problème important est celui de la stabilisation des sols.

La stabilisation mécanique a fait d'importants progrès dans certains pays, grâce notamment à l'essai d'équivalent de sable élaboré par Hveem en Californie et qui permet un choix et un contrôle rapide et précis des sols à utiliser.

La stabilisation au ciment, d'autrepart, a fait l'objet de perfectionnements importants qui en ont fait une technique sûre et efficace. Certains pays ont même rédigé des spécifications techniques pour les sols ciments: c'est le cas de la Grande Bretagne, notamment.

D'autres se sont orientés vers une technique voisine, celle de l'amélioration des sols au ciment; elle consiste à incorporer de faibles quantités de ciment à des sols qui s'écartent mais peu des normes de la stabilisation mécanique, de façon à les faire rentrer dans ces normes, c'est-à-dire, à les rendre stabilisables mécaniquement.

Mais les plus grandes nouveautés en matière de stabilisation semblent devoir provenir de l'incorporation en très faibles quantités aux sols, de produits chimiques très fortement actifs analogues aux 'dopes' employés actuellement pour améliorer les bitumes. Ces produits font l'objet de nombreuses recherches dans plusieurs pays, mais peu de résultats ont encore été publiés. Seule une communication, celle de T. W. LAMBE (4/10) U.S.A., leur est consacrée. Ces produits paraissent être efficaces, notamment, pour la lutte contre la gélivité des sols.

5. Je termine mon exposé très sommaire des récents progrès de la géotechnique routière. Pour ordonner la discussion qui va suivre, j'ai proposé à M. le président que les communications soient divisées en cinq parties, les trois premières correspondant aux trois problèmes principaux de la géotechnique routière que je viens d'indiquer: compactage des sols, calcul les épaisseurs à donner aux chaussées et stabilisation du sol. La quatrième partie sera consacrée aux mouvements de l'eau sous les chaussées. Quant à la cinquième partie, elle comprendra toutes les questions qui n'ont pu être rattachées directement aux trois précédentes.

W. A. LEWIS (U.K.)

I should like to say something on the subject of the comparative efficiency of pneumatic-tyred rollers and vibrating compactors in the compaction of soil.

Since 1945 we at the British Road Research Laboratory have been carrying out very comprehensive studies of the performance of most types of compaction plant including almost all sizes of pneumatic-tyred roller, vibrating roller and vibrating plate compactors available in the British Isles. The pneumatic-tyred rollers tested ranged in size from 12 ton to 45 ton with tyre inflation pressures ranging from 36 to 140 lb./sq. in. Four sizes of vibrating roller have been investigated from 4½ cwt. to 3½ ton in dead weight and four sizes of vibrating plate compactor from 4 cwt. to 2 ton dead weight have also been studied.

All the investigations with these compaction machines have been carried out under carefully controlled conditions on the same range of soils from heavy clay to sand making possible, therefore, a direct comparison of the performance of the various machines. Many of you will have already seen or will see during the technical visits the building and techniques we have developed at the laboratory for testing compaction plant. It is not possible, however, for me to give you in the very short time available more than a very brief outline of the main results obtained with the various pneumatic-tyred rollers and vibrating compactors.

Very broadly, the results show that the state of compaction produced in soil at any given moisture content is a function of the magnitude and duration of an application of the stresses provided by the compaction plant. With most of the vibrating compactors the duration of the stresses and their magnitude are insufficient to produce satisfactory states of compaction with the very cohesive soils. The one exception to these results was obtained with a 3½ ton vibrating roller. With this machine the magnitude of the stresses produced was such that very high states of compaction were obtained even with the heaviest clay

soils. All the types of vibrating compactors were capable of giving excellent results in the compaction of granular soils where the vibrating effect helps in re-arranging the particles of soil into a better state of packing. With the very light vibrating compactors only a relatively thin layer of soil 2 to 3 in. thick is, however, highly compacted.

Pneumatic-tyred rollers, in general, are capable of producing extremely high states of compaction in most soils provided the tyre inflation pressures and wheel loads are of the correct magnitude. The tyre inflation pressure largely governs the contact pressures which determine the maximum stresses developed in the soil and hence the state of compaction produced. On the other hand, the magnitude of the wheel load for a given tyre pressure affects the area of the tyre in contact with the soil and this determines the depth of penetration of the stresses. Clearly when soils are in a very wet condition, as is often the case in the British Isles, or because of their grading they have a low stability, only comparatively low tyre inflation pressures of the order of 30 to 40 lb./sq. in. are necessary or in fact desirable, whereas with, say, cohesive soils in the dry conditions often encountered abroad, tyre pressures of about 140 lb./sq. in. will be required to achieve satisfactory states of compaction. In order to compact as thick a layer as possible the maximum wheel load which can be used with the particular tyre pressure as recommended by the tyre or roller manufacturer should, in general, be employed.

Because of the higher towing or roller speeds or greater rolling widths, pneumatic-tyred rollers are capable in general of much greater outputs than those which can be achieved with vibrating compactors. It is often claimed that the depth of compaction which can be achieved with vibrating equipment compensates for the lower rate of coverage, but our results suggest that the figures often quoted for the depths which can be compacted to a high state of compaction are very optimistic. Even with the heaviest vibrating roller we have tested—the 3½ ton machine—or even with the 2 ton vibrating plate compactor we would hesitate to recommend the use, in general, of loose layers much thicker than about 12 in. if satisfactory states of compaction are to be obtained in the lower portions of the layer.

To sum up, therefore, we feel that for mass earthworks where very large outputs are required, pneumatic-tyred rollers are the most suitable plant to employ. In certain special cases, however, where non-cohesive soils are to be compacted, vibrating equipment despite its possible lower output might be preferable, especially for the compaction of the upper 2 or 3 ft. of the formation. Vibrating compactors are also in general, as stated by the General Reporter, the most suitable type of equipment for the compaction of granular base material.

Finally I should like to make two comments on Paper 4/9, a method of estimating settlement by roller compaction by G. KUNO and T. MOGAMI. The statement is made that 'Compaction control by measuring settlement is more practical than with measurement of density, because the determination of field density cannot be made without considerable error.' I am afraid I cannot agree with this view. Control of compaction by density measurements is now a universally accepted procedure and provided a sufficient number of density measurements are made a very high degree of accuracy can be obtained.

I should also like to ask the authors if they would explain how they obtained the contact pressures given in Table 4 for the various items of plant. For example, a contact pressure of 50 kg/cm², or over 700 lb./sq. in., seems improbably high for a pneumatic-tyred roller weighing only 6 ton.

P. P. BROWN (U.S.A.)

With regard to the comparable efficiency of pneumatic and vibrating compactors—a subject proposed for discussion by the

General Reporter—I should like to mention some items of interest disclosed in research by the Bureau of Yards and Docks, United States Navy Department. This research has been accomplished on both field and laboratory scale, the basic work being primarily under the direction of F. J. Converse of the California Institute of Technology.

To compare the two methods one must consider the applications. The Navy's studies have shown that in natural sand deposits it is possible to obtain 95 per cent of maximum density (modified AASHO) to a depth of 2 ft. and greater than 93 per cent of maximum density to a depth of 5 ft. This was accomplished with a 3 by 5 ft. sled-type vibrator weighing 6·5 ton and operating at resonant frequency. Undoubtedly the depth affected is a function of the size of the unit.

This great depth of the compacting effect is probably the most significant feature of vibratory compacting equipment. It can be particularly beneficial in waterfront construction where in many cases the lower portion of the fills beneath pavements is placed by hydraulic dredge. For this application, vibratory methods provide a very distinct advantage over conventional compactors.

A second and very promising application for vibratory compaction which has been studied briefly is that of compacting granular subgrade soils or base course layers through an existing pavement. The same vibrator noted above was used on a test pavement consisting of 3 in. asphaltic concrete surfacing and 6 in. soil cement overlying a breach sand subgrade. The density of the underlying sand was increased by as much as 15 per cent, resulting in subgrade densities up to 107 per cent of maximum density (modified AASHO). These results have considerable significance to us in the rehabilitation of old airfield pavements for modern aircraft wheel loadings.

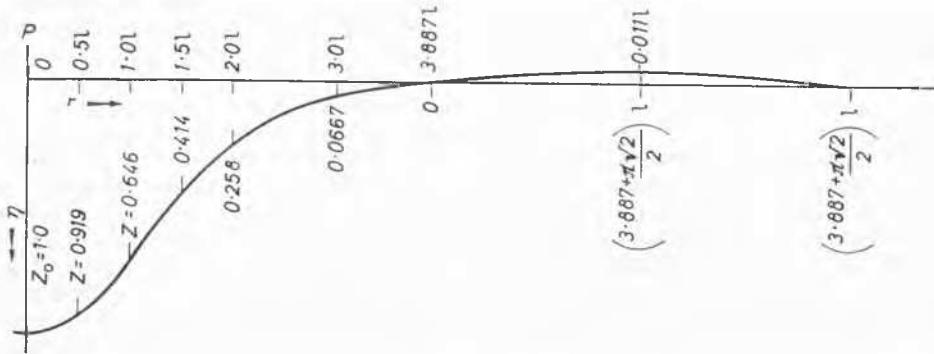
To be feasible and efficient, vibratory compaction must be used under circumstances which bring out its peculiar advantages. In compaction of the thin layers normally specified for conventional equipment there appears to be little advantage for vibratory compactors. Actually there appears to be some question of its effectiveness on surface layers, and it may be most efficient to combine the methods—obtaining deep compaction with vibratory equipment and surface compaction with pneumatic equipment. These comments refer primarily to granular soils.

This research has extended on a laboratory scale to cohesive soils, but as is generally acknowledged the effectiveness is not as marked as with the granular soils. The advantages of vibratory compaction with cohesive soils seem to lie in producing high contact pressures and rapid repetitive loading as distinguished from the rapid rearrangement of particles as is the case with sands.

I. HABER-SCHAIM (Israel)

The problem of the distribution of stresses and strains in an infinite half-space due to a single force can be solved in full agreement with experimental results only by the wave theory of Herz. When the force acts, the surface of the half-space is deformed into a system of concentric waves the amplitude of which decreases very rapidly. Fig. 1 indicates the wave form.

Only the deflections of the first trough and the first crest can be measured: the others are too small. The first trough may conveniently be called the deflection dish. The ratio of the deflection body to the stress body is the modulus of subgrade reaction K . The parameter l can be calculated directly from the deflection dish without the necessity of determining any material constants. It is equally simple to calculate l from the stresses in the line of the acting force, but it is more complicated to measure stresses than deflections. If one integrates the stresses one finds that the integral over the first trough is much



$$r = nl \quad \eta_r = \frac{z_r}{z_0}$$

$$(1) \quad 2\pi \int_{r=0, l}^{r=(3.887+\pi l^2/2)l} \sigma_0 \eta_r r dr = P = (9.2 - 1.23)l^2 \sigma_0 \sim 8.0l^2 \sigma_0$$

$$\sigma_r = K z_n, \quad \sigma_0 = K z_n \quad \sigma_r = \sigma_0 \eta_r$$

$$(2) \quad 2\pi \int_0^{n' z(r)} \frac{z(r)}{z_0} r dr = I, \quad I = 9.2l^2$$

$$K = \frac{\sigma_0}{z_0} \quad \sigma_0 = \frac{P \times 9.2}{8I}$$

Fig. 1 General Herz wave curve
Courbe généralisée des ondes Hertzziennes

greater than the acting force. This is confirmed by all experiments.

One can find the parameter 1 in the following simple manner from only two deflection measurements. Measure the deflections z_o at the centre and z_r at any distance r from the centre. Dividing z_r by z_o one obtains the ordinate η_r of a point on the general Herz wave curve. To this corresponds the abscissa $r=nl$, where n can be read off on the l line. Then $l=r/n$. For this one can calculate all other important values.

If the force is transferred through any plate, we can find the volume of the deflection dish by integration. Dividing this volume by z_o we obtain I , and hence l , k and σ_0 .

Without going into details, I may say that the general Herz wave curve is the basis for the investigation of all problems of soil mechanics connected with foundations and the construction of runways.

The parameter 1 is a characteristic of any subsoil alone or of any superstructure plus subsoil, elastic or otherwise. The detailed theory and results of field measurements will be published shortly.

M. JEUFFROY (France)

Monsieur le président, messieurs, nous avons lu avec intérêt, dans le deuxième volume des Proceedings, une communication du R. L. SCHIFFMAN (4/16) sur l'étude de ce qu'il est convenu d'appeler maintenant les systèmes tricouches, c'est-à-dire les schémas représentant les chaussées par une couche de revêtement, une couche de fondation et le sol naturel, l'ensemble étant supposé élastique. Cette étude est le prolongement des calculs présentés au congrès de Rotterdam par le M. Fox.

Nous avons pensé qu'il serait peut-être intéressant de donner connaissance d'une étude parallèle que nous avons menée avec M. Bacheler et qui est relative à un schéma analogue mais dans lequel la couche supérieure, c'est-à-dire le revêtement pour les chaussées souples ou la dalle de béton pour les chaussées rigides, est traitée comme une plaque, c'est-à-dire dans l'hypothèse de Navier et non plus comme un solide élastique avec les fonctions de tension.

Le schéma que nous proposons est le suivant (Fig. 2): le revêtement est caractérisé par une épaisseur h et un module de déformation E , la fondation par une épaisseur h_1 et un module de déformation E_1 , le substratum élastique par un module de déformation E_2 .

Les calculs que nous avons faits concernent les contraintes de traction par flexion dans le revêtement, les pressions verticales sur le sol de fondation et les flèches à la surface du revêtement, pour une charge supposée appliquée avec une pression uniforme sur un cercle de rayon a .

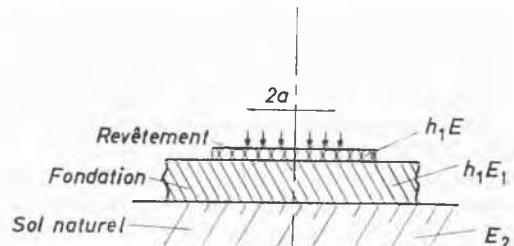


Fig. 2

La Fig. 3 est un exemple des diagrammes que nous avons tracés. Dans ce diagramme les courbes en traits pleins sont les lignes de niveau des contraintes de flexion dans le revêtement et les courbes en pointillés sont les lignes de niveau des pressions sur le sol naturel, sous la charge, en fonction des deux paramètres principaux qui sont: en abscisse, le rapport de l'épaisseur du revêtement au rayon de charge multiplié par une combinaison des modules d'élasticité du revêtement et de la fondation et, en ordonnée, l'épaisseur de la fondation rapportée au rayon de charge. Chaque diagramme est établi pour une valeur du module d'élasticité de la fondation rapporté à celui du substratum. Autrement dit, pour des matériaux déterminés et pour un rayon de charge donné, l'abscisse représente l'épaisseur du revêtement et l'ordonnée l'épaisseur de la fondation.

Les calculs que nous avons faits n'avaient d'intérêt que dans

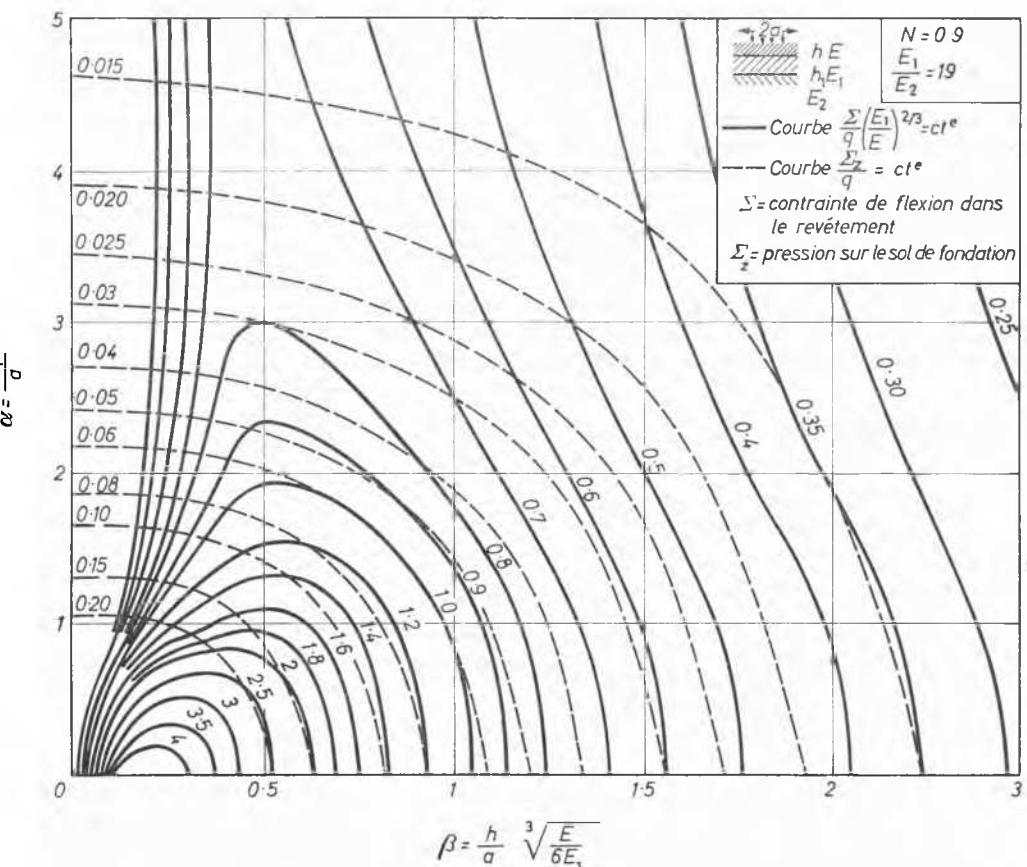


Fig. 3 Contraintes dans le revêtement et sur le sol de fondation en fonction des caractéristiques de la chaussée et de la charge

Stresses in the pavement and in the foundation soil as a function of the road characteristics and of the load

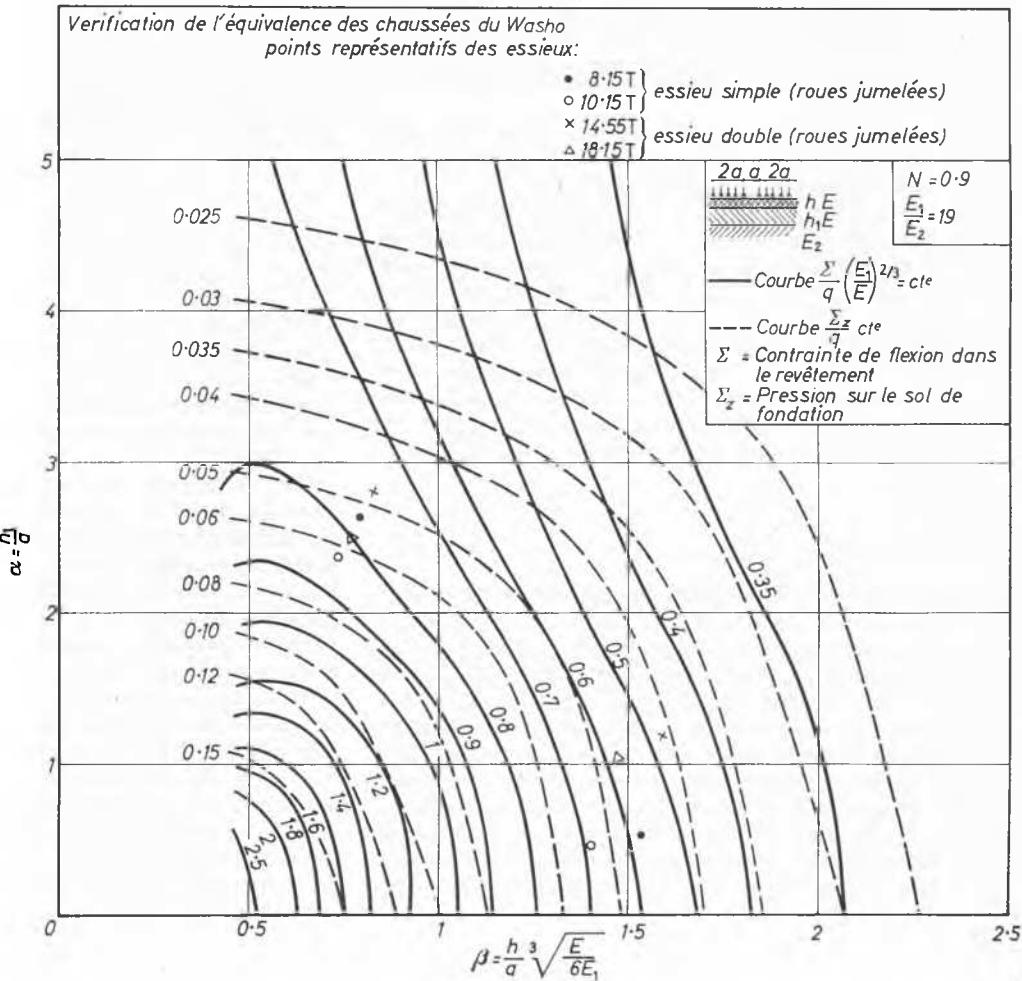


Fig. 4 Courbes donnant la contrainte dans le revêtement et sur le sol de la fondation, en fonction des caractéristiques de la chaussée et de la charge dans le cas d'un jumelage ($D=3a$)

Curves showing the stresses in the pavement and the foundation soil as a function of the road characteristics and the load for a two wheel load

la mesure où l'expérience pouvait les confirmer. Il nous a paru intéressant pour cette confirmation d'utiliser les travaux du Washo qui, vous le savez, a fait exécuter des essais de trafic accéléré sur une dizaine de chaussées différent tant par l'épaisseur de leurs revêtements que par celle de leurs fondations. Quatre types de camions différents ont circulé sur ces chaussées et de nombreuses mesures de déflexion ont été effectuées. Les conclusions des Ingénieurs du Washo leur ont permis de déterminer une équivalence de résistance entre des chaussées d'épaisseurs totales différentes, suivant l'épaisseur du tapis de béton bitumineux.

Nous avons reporté sur la Fig. 4 les points représentatifs des quatre essieux du Washo. On voit qu'avec une approximation assez correcte, revêtement et fondation sont soumis à

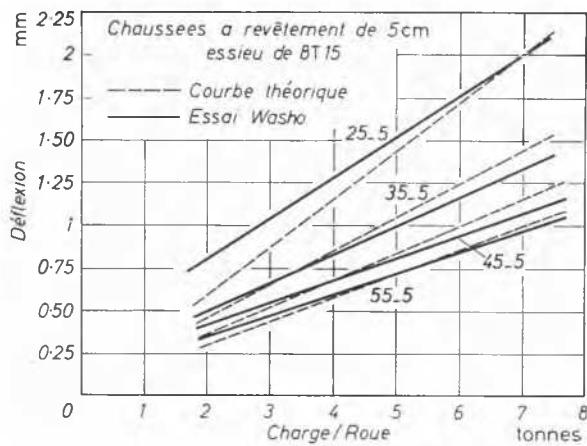


Fig. 5 Comparaison entre les déflexions mesurées et calculées pour diverses chaussées du Washo en fonction de la charge essieu
Comparison between calculated and measured strains for different roads of the Washo, as a function of the axle load

des contraintes sensiblement égales pour des chaussées d'épaisseurs très différentes, ce qui donne une justification des équivalences annoncées par les Ingénieurs du Washo.

Nous avons également procédé à une confrontation avec la résultats théoriques des déflexions mesurées sur des chaussées d'épaisseurs différentes avec des charges variables et régulièrement croissantes. Nous avons porté sur la Fig. 5, en fonction de la charge sur essieux, en traits pleins les droites de régression des déflexions mesurées sur les chaussées du Washo et en pointillé, les déflexions calculées. Nous avons également dans ce cas, une concordance relativement satisfaisante, obtenue en attribuant des valeurs aux seuls trois paramètres représentant les modules de déformation du sous-sol, de la fondation et du revêtement.

J. A. J. SALAS (Spain)

With regard to the difference between the amount of moisture under pavements in the British Isles and Iraq: the difference between the development of moisture content in humid climates and arid climates was described and emphasized in the Spanish report to the Tenth Road Conference held in Istanbul in 1955, when moisture patterns of Spanish soils were presented which were typical of moisture occurrence in regions where rainfall is much less than free surface evaporation. As was then stated, a moisture pattern is not indicative of the distribution of humidity in the soil because different factors in the soil, particularly its texture, have to be taken into consideration. The most perfect method would, without doubt, be to determine the capillary potential at each point, but this is difficult as the

irregularity of moisture distribution in arid climates requires the investigation of hundreds of spots. In consequence, in order to find a simple method, the ratios of moisture content *versus* liquid limit were tested, and of moisture content *versus* percentage of fines passing sieve No. 200, and a desiccation index defined as:

$$\frac{\log \text{moisture content} - \log \text{hygroscopic moisture at pF 6}}{\log \text{plastic limit} - \log \text{hygroscopic moisture at pF 6}}$$

was tried. After all these attempts the most representative value was found to be the ratio of the moisture content *versus* plastic limit.

By the study of the development of this ratio it was found that in the soil, under arid climate conditions, 'clouds' of high moisture content are formed, principally due to infiltration during periods of rain. These 'clouds' develop slowly and change their shape and depth, apparently in an erratic fashion, frequently taking the form of perched water tables and never attaining equilibrium.

This slowness in development is due to the moisture transfer being effected under lentocapillary conditions.

In an arid climate the annual evaporation from humid soil surfaces exceeds by far the annual rainfall. The result is that the rain water evaporates rapidly until it reaches the so-called 'break point', where the evaporation rate decreases enormously. A soil in an arid climate has to be within the 'break point' or below the 'break point' and therefore the lentocapillary conditions prevail.

When in a humid climate equilibrium conditions of capillary potential can be attained which simulate the hydrostatic equilibrium. The problems of the development of humidity in an arid climate closely resemble those of meteorology affected by small gradients of temperature and pressure, involving all its difficulties and uncertainties.

Furthermore, in Spain we have followed up the studies of the Australian engineers H. T. Loxton, M. D. McNicholl and H. C. Williams, presented at the Third Conference on Soil Mechanics, and a drainage index was used as follows:

$$D.I. = \frac{NMC - GMC}{BMC - GMC}$$

NMC = Natural moisture content.

GMC = Moisture content in the case of good drainage, according to H. T. Loxton, M. D. McNicholl and H. C. Williams.

BMC = Moisture content for bad drainage.

Although these studies have not been sufficiently applied for a proper conclusion to be reached, it is felt that this index may be useful.

With regard to the question proposed by the General Reporter on the design of flexible pavements, either based on the resistance or based on the deformation of the soil, my view is that the question should be treated in two different ways. First, would the road engineer necessarily choose his road calculations based on either the resistance or on the deformation? Secondly, could the road engineer disregard, at least in a great number of cases, one of these two factors? My reply to the first question is no. Both the road engineer and the foundation engineer have to consider the two aspects. It is a false position if *a priori* a choice is made between the two ways of dealing with the question. However, a reply to the second question presents more difficulty, as the road problem is generally standardized—similar loads applied under rather similar conditions. From this point of view I think that the present practice is greatly inclined to be content with the criterion of deformation. In effect, the two methods which,

according to the General Reporter, are today mainly employed are based on the criterion of deformation, as, in my opinion, the California Bearing Ratio is a test of deformability.

For the purpose of appraising it, it is useful to extend the test until much deeper penetrations are obtained than those required for normal tests. Then it is clearly shown how, in most soils, failure at the 0·1 in. penetration is not probable. Furthermore, we have found in cohesive soils a rather surprisingly good correlation between the CBR and the modulus of elasticity of the soil, determined by means of the unconfined compression test in the zone where the diagram stress/strain is still a straight line.

Evidently in the CBR test one part of shear failure produces a certain confusion in the phenomenon. This is mainly due to the fact that the surface of application of the load is absolutely rigid. This results in a concentration of stresses at the edges and a local shear failure. It is this shear failure which gives to the sample its 'punched' aspect and, consequently, the impression of a process where shearing stress prevails.

We may then conclude that the present practice of pavement design is based on the deformation of the soils. However, the question of the General Reporter does not refer to the present practice but to the path to be followed in the future.

As a matter of fact we have seen many examples of failure by shear stress on Spanish roads. In addition we have sometimes observed such failures by shear stresses apparently produced by only one pass of a heavy vehicle over the pavement, the general conditions of which had been satisfactory before. This leads to a suspicion that although in a flexible pavement the phenomenon of deformation acts by the fatigue of the surfacing material, the phenomenon of shear failure may cause the destruction of the pavement by only one moving load passing over it. Therefore, the total quantity of traffic should preferably be considered under the deformation criterion. The total weight and pressure of the tyres of the heaviest vehicle that can be allowed on a road should be estimated from considerations of shear failure of the foundation of the pavement. Consequently, the road engineer in practice should dispose of a means for appraising, more or less approximately, whether loading to a point of failure is imminent, apart from tests of deformability which permit him to remain aware of the critical deformations which will produce undue fatigue in the pavement.

A. KÉZDI (Hungary)

I should like to make two remarks on the CBR test based on the work of my assistant S. Lazányi.

First, the CBR test depends on many factors: the original water content, the method of compaction and the wetting or drying affecting the results and causing regular errors. By fixing the conditions of the test these errors can be eliminated. The irregular errors are caused mainly by the soaking, the water content becoming wholly irregular and impossible to verify as is shown in Fig. 6. This illustration gives the distribution of water content in the compacted specimen after soaking. Water content in the middle zone is appreciably lower than near the mantle surface; the main irregularities appear on the top surface in a layer 3 to 5 cm thick. Tests on specimens moulded with standard and uniform method but without soaking show that the scattering in the value of the CBR is low and completely tolerable, the test results are reproducible. It is proposed, therefore, to omit soaking and to examine the variation of CBR as a function of water content and dry density.

Figs. 7 and 8 show the influence of these factors. Above a certain water content there exists a critical density above which the CBR decreases. The peak values correspond to a definite degree of saturation $S=0\cdot75-0\cdot8$. Above this value the soil is practically saturated; there are only unconnected air bubbles

in the voids, therefore neutral stresses develop and shear strength decreases radically. As shown in Fig. 8, this phenomenon occurs even in fine sands.

(A. Kézdi continued his remarks in French.)

Vous me permettez de continuer mon exposé en français par considération pour notre Rapporteur général.

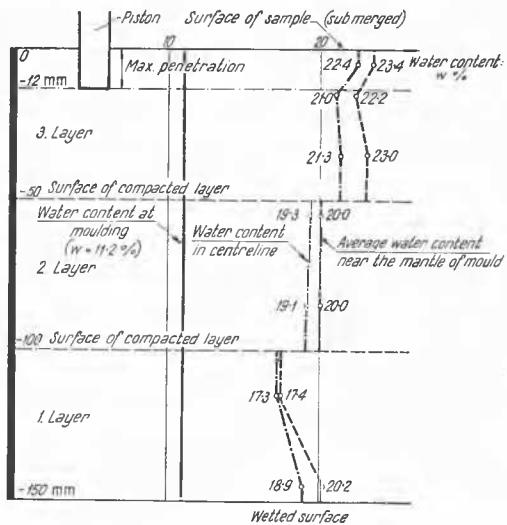


Fig. 6 Distribution of water content in the compacted specimen of the CBR test after soaking
Répartition de la teneur en eau dans un échantillon damé de l'essai CBR après immersion

Ayant conscience de l'imperfection des théories pour le dimensionnement des revêtements, je voudrais présenter quelques idées applicables en premier lieu à l'étude des routes du solciment.

Le sous-sol et le revêtement constituent un système à deux

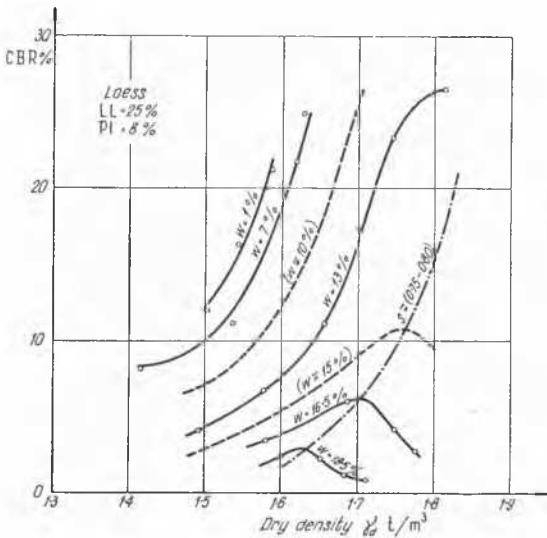


Fig. 7 CBR values at given water content versus dry density
Rapport entre les valeurs CBR pour des teneurs en eau déterminées et la densité à l'état sec

couches sur la surface de contact desquelles il ne peut se produire aucun déplacement relatif horizontal. Il existe donc une parfaite continuité aussi dans les tensions. Les tensions engendrées peuvent être déterminées en partant de la théorie de Burmister et faire l'intégration des rapports pour le cas d'une charge uniformément distribuée sur un disque circulaire. La détermination des tensions peut se faire par la graphique de

la Fig. 9. Les tensions dépendent, hors de l'épaisseur du revêtement et du diamètre de la plaque circulaire du rapport des modules d'élasticité du revêtement et du sous-sol. L'épaisseur du revêtement est déterminée par la technologie de la construction. La charge de la roue, à considérer comme agissant sur le revêtement, et la surface — de la forme approximativement circulaire — transmettant la charge sont aussi connues. En exécutant un essai à la traction et à la compression uniaxiale sur le sol, au dosage choisi, on peut déterminer

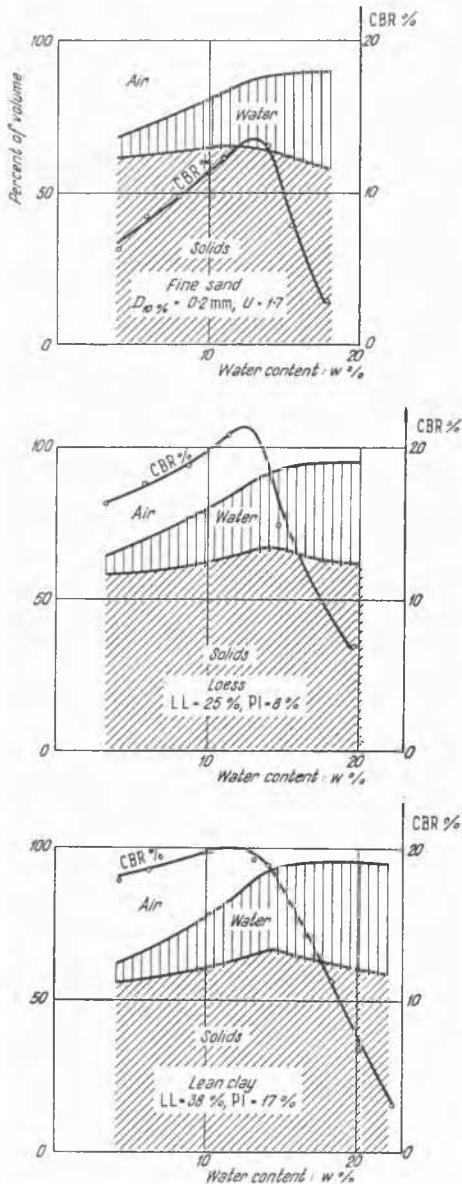


Fig. 8 CBR and γ_d versus water content at constant compactive effort

Rapport entre les valeurs CBR et γ_d et la teneur en eau pour force de damage constante

la ligne de Coulomb du matériau de revêtement, c'est-à-dire, son angle de frottement interne et sa cohésion. La résistance du revêtement sera satisfaisante, le cercel caractérisant l'état de tension ne touchera pas encore la ligne de Coulomb si l'inégalité suivante subsiste:

$$\sigma_u > \sigma_z - \sigma_{r1} \tan^2 \left(45^\circ + \frac{\phi}{2} \right)$$

les valeurs Φ et c déterminées de la façon précédente, se rapportent à l'état de rupture et, ainsi, l'application de la théorie de l'élasticité serait injustifiable. En effet, il peut être utile de faire usage des résistances intérieures appartenant à la

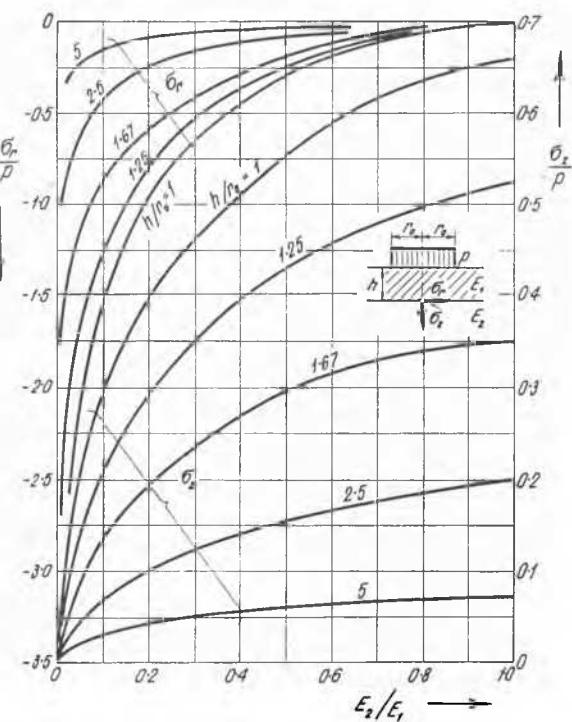


Fig. 9 Tensions sur la surface de contact de revêtement et du sous-sol
Stresses at the boundary between pavement and soil

limite de proportionnalité. Ces valeurs sont obtenues si, sur les courbes appartenant à la traction et à la compression uniaxiale, on détermine le point jusqu'où les tensions et les

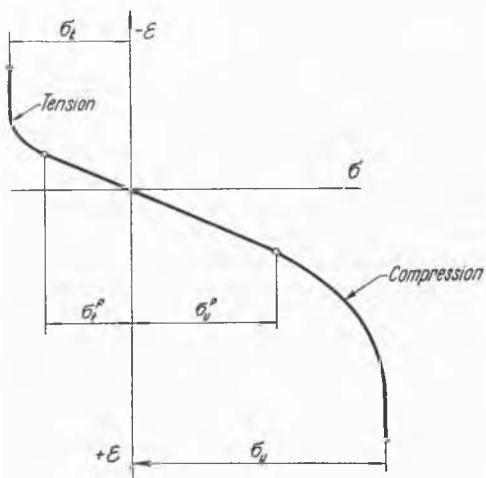


Fig. 10 Courbes de déformation
Strain curves

déformations sont proportionnelles (Fig. 10). Avec ces valeurs, on construit les cercles de Mohr, la ligne tangente commune donne ce qu'on peut appeler la ligne de Coulomb 'proportionnelle', l'angle de friction proportionnel du sol ainsi que la

cohésion (Fig. 11). Avec ces valeurs, on obtient la résistance à la compression requise. C'est-à-dire:

$$\sigma_u^P > \sigma_z - \sigma_{r1} \tan^2 \left(45^\circ + \frac{\Phi_p}{2} \right)$$

A l'aide de cette résistance, on peut déterminer le dosage nécessaire du ciment.

Quelques mots encore sur la détermination des modules d'élasticité. On fait des essais de mises en charges répétées en

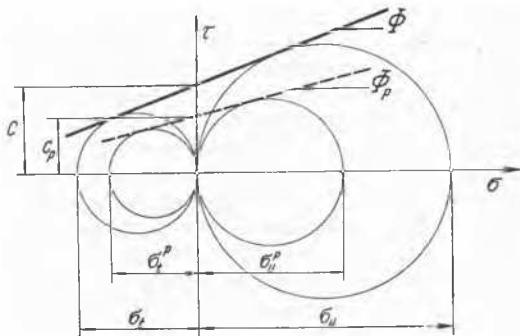


Fig. 11 Détermination des lignes de Coulomb; de rupture et de proportionnalité
Determination of the Coulomb lines; failure and proportionality

laboratoire à l'aide d'un œdometre, avec échantillon du sous-sol et du sol-ciment. Le rapport de la charge appliquée et de la déformation élastique fournit le module cité. Étant donné que ce n'est que le rapport des modules d'élasticité du revêtement et du sous-sol qui entre dans le formules, les erreurs inhérent à la détermination ne sont pas tellement significatives.

H. NOVAIS-FERREIRA (Portugal (Angola))

Monsieur le président, messieurs, dans des études des sols pour fondation des routes, on considère souvent chaque couche géologique comme uniforme. Toute fois, ces couches ne sont pas uniformes et spécialement dans le cas de sols résiduels. C'est ainsi que le sol de l'Angola, désigné localement par 'muceque' présente des caractéristiques qui le situe entre A-2 et A-6. Malgré cela, ses propriétés ne varient pas beaucoup en ce sens que la dispersion autour de la valeur moyenne est petite.

Le degré de compactage et la teneur en eau que l'on obtient dans la construction d'une route n'est pas uniforme non plus mais présente certaines fluctuations autour d'une moyenne. Une centaine d'essais sur plusieurs types de sol: argiles, siltes, sables et limons, ont démontré que les fluctuations des poids spécifiques ont une distribution du type de Gauss. Pour les CBR ce sont les logarithmes qui ont une distribution gaussienne. Ces lois ont été vérifiées au moyen d'essais statistiques de χ^2 et on a trouvé un degré de certitude acceptable.

La conjugaison de ces deux lois confirme également que le logarithme du CBR est proportionnel au poids spécifique: $\gamma = a + b \log CBR$. Pour l'application de cette loi il faut aussi compter sur la dispersion.

D'un autre côté la définition du CBR du projet doit être faite en vue de la probabilité de la rupture. L'étude de l'aspect économique peut être faite; elle m'a conduit aux conclusions suivantes: si le coût des réparations est d'environ une fois et demi le coût d'exécution, les CBR, d'exécution doivent présenter une fréquence de 5 pour cent inférieure à celui du projet. Si les réparations sont encore plus chères cette valeur descend à 2 pour cent. En ce qui concerne les routes aux réparations à bon marché, la valeur de la fréquence peut monter à 10 pour cent.

J. G. ZEITLEN (Israel)

It was very interesting to note C. VAN DER VEEN's report of loading tests on the airfield at Beek (4/20), and particularly to note his comparisons of CBR data before and after construction. Unquestionably much value is gained by comparisons of methods, as well as post-construction observations and tests such as he reports.

However, I would like to comment on the relatively low

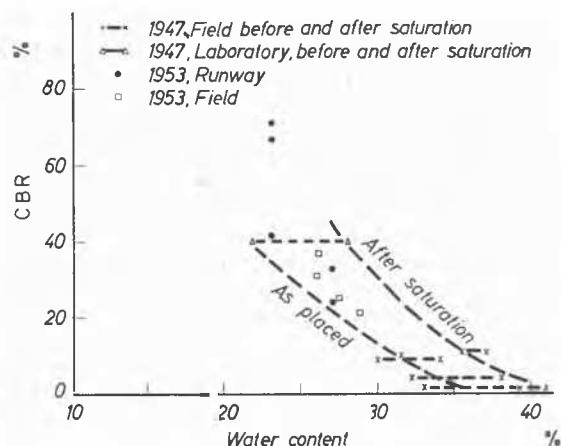


Fig. 12

values of CBR 7 which are reported for the original field conditions as compared to the average value of 44 which was found after construction and compared well with the plate-bearing tests. The high water content and average low density of the 1947 tests may be considered largely responsible for the low bearing values, as may be seen by plotting CBR results versus water content (Fig. 12) and CBR versus dry

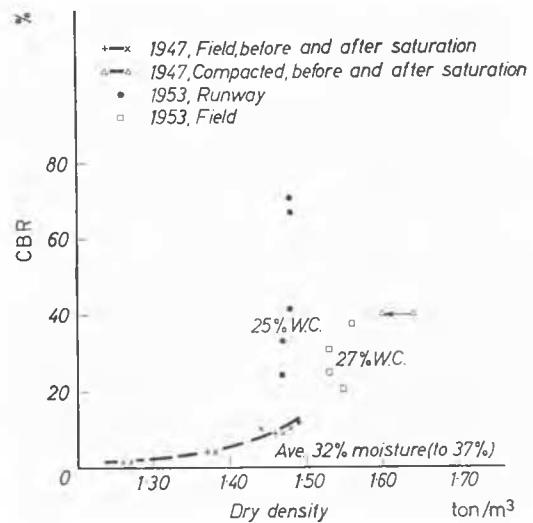


Fig. 13

density values (Fig. 13). These plots have been obtained by computation, using the bulk density and moisture content figures furnished in C. van der Veen's report.

There appears little question that the original CBR values were well below those finally obtained after construction. However, it is not obvious that the low values are the result only of soaking, since the original field moisture was about 32 per cent, and low CBR's would also have been obtained on unsoaked samples. No tests were made on unsoaked samples.

My basic objection to the conclusions of the report rests on the point that results of tests on undisturbed samples are not ordinarily directly applicable to predict the CBR to be expected in a subgrade. If the Corps of Engineers pavement design method is to be used, it should not be criticized unless it is used in its entirety. Specifically, in addition to undisturbed material being tested in the laboratory or field, subgrade soil should be tested under remoulded and compacted conditions at various water contents. For any pavement, compaction of the sub-grade is necessary to prevent detrimental settlement under wheel loads, as well as to improve bearing capacity. At Beek there must have been subgrade compaction—if not intentionally, at least under base course compaction, as well as under later air traffic. Preliminary studies should have been accomplished on remoulded soil, compacted at various initial densities and moistures. Such a study, as described in detail in Corps of Engineers publications, would have provided a full picture of CBR strength for different conditions, and would enable the selection of the proper CBR value for design, based on anticipated field conditions. My own experience with studies of this nature on such soils is that laboratory results can check well with the CBR's encountered in later evaluation studies.

It is thus felt that the case presented, in lacking a fundamental study of subsoil CBR behaviour, is inconclusive in establishing that CBR samples tested soaked give an under-estimate of the bearing capacity of flexible runways. It is recognized as obviously best to test for strength under conditions of moisture and density corresponding to those in the field after construction. Although the soaked CBR does not necessarily reproduce such conditions, it will in many cases give close results.

A point to be stressed is that recognition should be given to the increase of strength because of compaction effort. Any compaction of soil, whether intentional (for improving the subgrade) or occurring during re-grading operations, usually gives a 'pre-stress' strength to the soil. Such strength is retained on later wetting, even if moistures and densities comparable to uncompacted soil are reached at a later time.

Further collection and study of field data, such as is now being accomplished by the U.S. Corps of Engineers and other organizations, will throw more light on the actual final moisture and density conditions to be expected under pavements in different climates. The techniques and approaches under development at the Road Research Laboratory look particularly promising for providing better scientific methods for predicting final moisture conditions.

P. L. CAPPER (U.K.)

The design of a road or runway pavement, in common with other types of structure, depends upon four items. First, the applied loads, secondly, the internal stresses, thirdly the deformations, and fourthly, the mode of failure when overloaded. As regards the subgrade, the link between the first two and the last two items depends upon the relevant soil properties. In the CBR method of pavement design, the CBR itself and its relationship to the pavement thickness are both empirical, although in Paper 4/18 by W. J. TURNBULL and R. G. AHLVIN an attempt is made to find a rational relationship between the loading, the CBR and the pavement thickness.

A more logical approach to the problem would be the use of the elastic modulus as the governing soil property. The pavement thickness would then be chosen so as to limit the deformation of the subgrade to a value which the pavement could sustain without damage.

In Paper 4/15 by Ú. NASCIMENTO and A. SIMÕES the authors have correlated the CBR with what they term the modulus of strength, that is, the modulus of subgrade reaction multiplied by the diameter of the plate. Ignoring the edge effects of the

loading plate this modulus is, for an elastic material, proportional to Young's modulus.

In Paper 4/12 N. W. MCLEOD has studied the deformation characteristics of the soil by means of the plate-bearing test, and analyses the total deflection of the plate into elastic deformation and settlement. The ratio of settlement to total deflection shows surprising consistency, and one would have expected greater differences owing to variation in the compaction at different sites. N. W. McLeod has also taken into account a very important factor, that of repetitive loading.

Some experimental work has been done at University College, London, to determine the relationship between the CBR and Young's modulus E , as measured in the undrained triaxial test. The materials used were a silty clay and mixtures of this clay with varying proportions of sand. The soil was compacted at various moisture contents, using British Standard compaction. When there is a non-linear stress/strain relationship it is necessary to define the modulus arbitrarily. In these experiments it has been taken as the mean ratio of stress to strain over a range of stress from 0 to half the ultimate shear stress. Over the limited range of materials tested the value of E appears to be directly proportional to the CBR for values of the moisture content between the Proctor optimum and the PL. Before any definite conclusions can be drawn, further experiments are required covering a wider range of soils, and consideration should be given to the effects of repetitive loading.

J. REICHERT (Belgium)

Monsieur le président, mesdames, messieurs, plusieurs irrégularités rencontrées dans des résultats de 'Plate Bearing Tests' exécutés tant par le 'Centre de Recherches, Routières de Belgique que par des expérimentateurs d'autres pays nous ont conduit à exécuter des 'plate bearing tests' en répétant les chargements un très grand nombre de fois sur des sols et des chaussées. Les durées des cycles de chargement ont été de 2 secondes, $\frac{1}{2}$ ou $\frac{1}{3}$ de seconde, suivant le cas. Certains des essais ont été exécutés sur un grand massif artificiel et ont comporté de 200 à 300,000 répétitions alors que dans certains cas nous avons atteint 1,500,000 répétitions. On ne perdra pas de vue que, dans un trafic réel il s'écoule, entre deux chargements successifs un laps de temps assez important, plusieurs secondes ou plusieurs minutes.

Dans sa communication 4/14, G. MORALDI a montré qu'il existait une corrélation entre la non-linéarité des relations déformation z/\log du nombre n de répétitions, un écrément anormalement élevé de la pente γ et l'apparition de phénomènes d'instabilité, rupture de la dalle de béton pour des essais de coin ou refoulement extérieur pour les chaussées flexibles.

Nous avons pu vérifier cette conclusion lors d'essais sur chaussées flexibles. Des précautions doivent être prises pour les effets thermiques.

A partir d'autres résultats, nous avons pu étendre cette conclusion aux cas de:

(1) Chargements jumelés répétés, matérialisant le passage d'essieu de camion sur des dalles de béton. Les dalles ont été poussées jusqu'à rupture. Ces essais ont été exécutés à la route expérimentale d'Hekelgem, sur l'autostrade Bruxelles—Ostende (route expérimentale avec dalles d'épaisseurs différentes et quatre types de fondation).

(2) Lors d'essais sur du sol (silt) et des fondations diverses.

Mais, alors que G. Moraldi a établi que ce phénomène d'instabilité se produisait pour une valeur très élevée de la pression, nous constatons, au contraire, trois faits: (1) même pour de très faibles valeurs de la pression, les relations ($z/\log 10n$) ne semblent plus linéaires pour un nombre suffisant de répétitions; (2) après l'apparition du 'croc' dans les relations

($z/\log 10n$) les déformations continuent à augmenter; (3) si on applique des pressions croissantes répétées il se produit des 'crocs' pour chacune des valeurs des pressions.

En conséquence, si ces faits se confirment, la méthode de G. Moraldi serait inexacte en ce qu'elle ne considère: (1) qu'une seule valeur de pression critique; (2) qu'un nombre trop restreint de répétitions.

En conclusion de tout ce qui précède, l'extrapolation faite par plusieurs auteurs des résultats obtenus après quelques répétitions (5, 20 ou 100 par exemple) à un plus grand nombre de répétitions est inexacte en ce qu'elle suppose la linéarité des relations ($z/\log 10n$) qui n'est pas vérifiée en réalité.

Ceci a une répercussion immédiate sur les relations d'équivalence de chargement. A partir des résultats expérimentaux, nous avons pu: (1) établir de telles relations corrigées d'équivalence; (2) vérifier que l'effet de l'application de charges, dont la valeur est inférieure ou, tout au plus, égale à 50 à 60 pour cent de la valeur de la charge maximum exercée est totalement insignifiant. Ceci corrobore les résultats de l'action d'un trafic sur une chaussée, pour laquelle les petites voitures sont sans importance par rapport à l'action des essieux des camions.

Il serait utile d'avoir des explications détaillées sur les relations d'équivalence et les valeurs absolues maximum des déformations données dans la communication 4/6.

D'autrepart, notons que contrairement aux résultats obtenus par N. W. McLEOD, communication 4/12, Fig. 2, nous n'obtenons pas non plus de relations linéaires, si on trace un diagramme logarithme déformation/logarithme du nombre de répétitions, quand le nombre de répétitions est supérieur à 100 qui est la valeur indiquée par McLeod.

D'après les résultats d'essais, nous constatons que les valeurs des déformations sont d'autant plus faibles que la fréquence de chargement est élevée.

Ceci n'a-t-il pas pour conséquence de condamner les modes de chargement qui correspondent pas aux vitesses réalisées sur la chaussée? C'est, par exemple, le cas des essais de chargement à déformations stabilisées: exemple ($\Delta z/\Delta t \leq 0.02$ mm/minute). Ce dernier type d'essai a seulement l'avantage d'être du côté de la sécurité.

Nous concluons également de nos essais qu'il est tout-à-fait normal que les valeurs des déformations sont d'autant plus faibles que les fréquences de chargement sont élevées.

Ce sont les quelques éléments que je voulais soumettre à votre discussion.

G. MORALDI (Italy)

Monsieur le président, messieurs, dans mon papier (4/14) présenté à cette division et concernant les essais de chargement avec plaques pour déterminer la portance des pistes, j'ai cru pouvoir affirmer que l'accroissement brusque de la valeur des déformations plastiques γ pouvait mettre en évidence les phénomènes d'instabilité qui se produisent à la suite d'une augmentation de la charge appliquée soit sur un sol, soit sur n'importe quel type de revêtement, même si on limitait le nombre de répétitions à 4.

Les résultats des expériences de J. Reichert du Centre de Recherches, Routières de Belgique, semblent non seulement rendre inexacte cette affirmation, mais aussi porter à la conclusion que pour n'importe quelle valeur de la pression p la linéarité de la fonction $Z_n = Z_1 + a \log n$ ne soit plus respectée.

Ayant pris connaissance des résultats obtenus par J. Reichert, mon opinion est que, au moins pour ce qui concerne les valeurs réduites de p on ne se trouverait pas encore de phénomènes progressifs d'instabilité qui entraînerait rapidement la rupture, mais peut-être plutôt de phénomènes passagers comme le prouverait le fait qu'après avoir subi une inflexion la fonction susdite reprend sa linéarité; c'est comme si on avait

a faire à un certain moment à un réarrangement sur d'autres positions d'équilibre.

Mais, à part ces considérations, je voudrais attirer tout particulièrement l'attention de ceux qui s'occupent de l'exécution d'essais de charge avec plaque sur l'aspect scientifique et l'aspect pratique du problème.

Pour ce qui concerne l'aspect scientifique, puisqu'il n'y a pas de doute que les sols et les revêtements accusent sous les charges, soit des déformations élastiques, soit des déformations plastiques, je trouve qu'il est de la plus haute importance d'adopter une méthode d'essais qui mette en évidence les deux types de déformation plutôt qu'un seul, ou la déformation totale; ceci, non seulement pour une meilleure compréhension des phénomènes en jeu, mais aussi pour pouvoir préparer le terrain à ceux qui, demain arriveront à nous donner une méthode analytique ou empirique de calcul acceptable méthode qui pourra être basée soit sur les déformations élastiques, soit sur les déformations plastiques, soit sur l'ensemble des deux déformations.

Le second aspect de la question est tout-à-fait pratique. N'oublions pas que la plupart de nous sommes des ingénieurs et que le problème urgent que les administrations nous posent et pour la solution desquels nous ne pouvons pas attendre plus longtemps le résultat d'expériences futures de longue durée, est de déterminer la portance des revêtements des pistes et des chaussées existantes, pour pouvoir faire face à l'évolution rapide des moyens de transport aérien et à l'augmentation du trafic sur les routes.

Or, si j'examine cet aspect du problème, je m'aperçois qu'au moment actuel, les conclusions auxquelles les expérimentateurs sont arrivés sont les plus différentes, non seulement pour ce qui concerne la définition d'une charge limité ou d'une charge de sécurité qu'un revêtement peut supporter, mais encore plus pour ce qui concerne la technique des essais et le nombre des répétitions à adopter. Je me borne à rappeler par exemple, en me référant seulement aux pistes d'aviation les 10 répétitions adoptées par le Highway Research Board et par N. W. McLEOD (4/12), les plusieurs milliers de répétitions adoptées ailleurs, et enfin l'absence de répétition adopté par une administration américaine.

La demande que je formule à vous tous qui êtes intéressés à ce genre d'essai est la suivante: ne serait-il pas souhaitable de profiter de l'occasion unique que ce congrès nous offre en nous ayant réunis ici pour chercher de discuter entre nous les critères qui ont porté à des méthodes si différentes et voir s'il n'est pas possible d'arriver à ces conclusions communes acceptables?

Je réalise parfaitement qu'une discussion de ce genre ne pourrait se faire ici publiquement par manque de temps, et pour ne pas mettre à trop dure épreuve la patience des autres auditeurs; je me permets dans ce conditions de proposer la tenue d'une réunion amicale, pas le moins du monde officielle j'insiste particulièrement sur ce point, strictement limitée à tous ceux qui ont contribué à étudier les problèmes de l'évaluation de la portance au moins de 'plate bearing tests'.

L. ERLENBACH (Germany)

In Germany we have in the last few years built some test roads with different types of bases and sub-bases particularly to test the qualities of flexible and rigid bases for heavy traffic. There are two long trunk roads in South Germany which have different coats and bases and which are used as test roads. One was built in 1952, the Lahr test road of 4 km length, and the other one was built near Stuttgart, the Grunbach test road of 5 km length. Details of these roads are published in two small booklets by the German Highway Research Board.

The Lahr road will be loaded by additional controlled traffic equalling the Washo road test. On one side of the road lorries

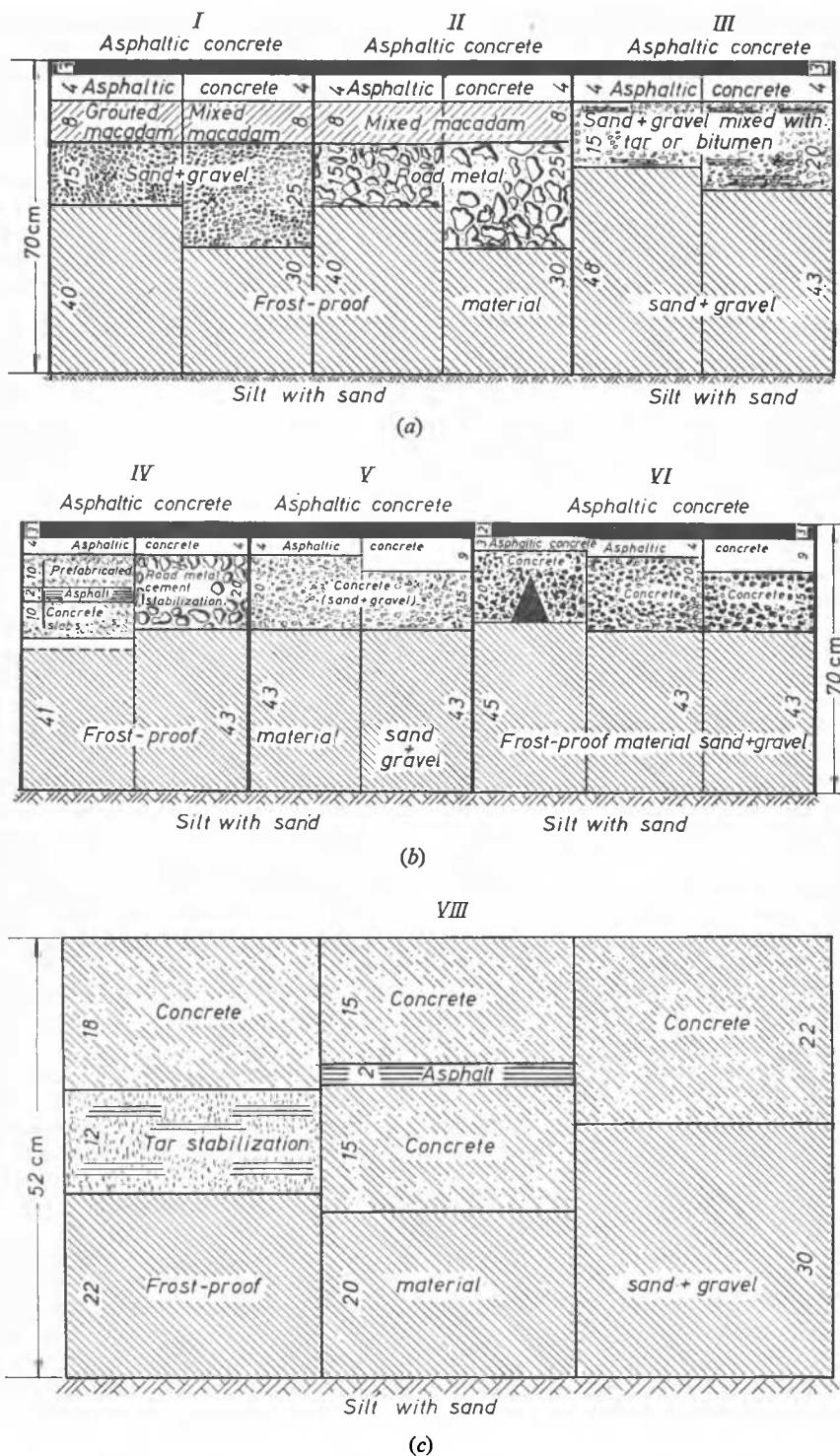


Fig. 14

with trailers of 32 ton (10 ton to each axle) will run and on the other side lorries with 24 ton trailers (8 ton to each axle) will run over this road for six months. During this period exact measurements will be made on the lorries, the trailer and on the road.

I should like to report on some details of the second test road near Grunbach which lays in the valley of the River Reurs. The subsoil consists of sandy silt and is a frost-heaving soil with ground water 1 m under the surface. Frost penetration in the last few years was nearly 70 cm. Therefore on the whole

length a frost-protecting layer of sand and gravel, 30–42 cm thick, was placed and compacted. Fig. 14a, b and c shows cross-sections of the road bed. Sections I to VI have asphaltic concrete as a top coat and various bases of gravel, road metal, asphaltic base and concrete. Section VIII has a concrete top and various bases. All the layers are well compacted and were tested by the plate-bearing test and other methods. At different depths there are earth pressure cells and apparatus for measuring the compressibility and temperature under the road. Moreover, weighing apparatus is built into the road to weigh each

vehicle running over it. The road has been under traffic for six months.

I think it would be extremely valuable if all the results of test roads throughout the world were collected.

D. J. MACLEAN (U.K.)

On the subject of soil compaction, my colleague, W. A. Lewis, has already discussed the relative merits of vibratory and pneumatic-tyred roller compaction on the basis of the results of our researches at the Road Research Laboratory. I should, however, like to refer to the problem of compacting graded crushed rock, which is the subject of Paper 4/19 by W. J. TURNBULL and C. R. FOSTER.

In Great Britain we lay crushed rock bases either by spreading a layer of 2 in. single-sized stone and blinding with dry fines or by laying a graded stone at a moisture content of about 5 per cent. In both cases we prefer heavy vibratory compaction. With the stone laid in a dry condition we have found that with heavy vibrating plates it is possible to obtain a dry density which is not subsequently increased by heavy road traffic. With the stone laid in a wet condition there appears for vibratory compaction to be an optimum moisture content for obtaining a maximum dry density around 5 per cent. In this way we avoid the complete saturation of the base which is apparently required to obtain a high state of compaction with pneumatic-tyred rollers.

On the subject of pavement design I would agree with the General Reporter that the present trend is to consider the modulus of deformation of soils as being more important than the strength, but at the same time I would suggest that even in the CBR test something more akin to a modulus of deformation than to strength is being measured. I have heard the suggestion made in the U.S.A. that the CBR test would be improved by basing the measurement on the load required to effect a penetration of the plunger of 0·04 in. and this would make the CBR value more nearly a function of the modulus of deformation.

In spite of much research on the subject the CBR method still maintains its predominance over other methods of flexible pavement design, and I would suggest that before a change is made we must have available a technique for testing the soil which represents an important technical advance over the CBR test. Important requirements of any new technique are that the pore water pressure in the soil should be readily adjustable, that the permanent deformation of the soil under repeated applications of stress and its modulus of deformation should be capable of measurement, and that the test technique should be applicable to base and sub-base materials as well as to soils. I have in mind a test which could be applied with equal confidence to materials differing in their properties as much as cement-stabilized soil and bitumen-stabilized soil. It would, in my opinion, represent a very big advance if a single test procedure could be used to measure the relevant properties of all types of material, including soils, used in road foundations. It seems that, to meet the requirements I have indicated, the only suitable form of test would be a triaxial compression test in which the vertical stress can be applied repeatedly. A test of this type is already being studied for soils at the University of California, and I would suggest that the study of the test should be widened to include the full range of road foundation materials.

The CBR test is nevertheless proving to be a convenient tool not only for pavement design purposes but also for designing cement-stabilized soil mixes. Our experience suggests that because the stiffness of such materials is evaluated in the CBR test under conditions of restraint, a better indication is obtained of the relative stability of such materials in the road structure

than with the unconfined compressive strength test that has been widely used for soil cement hitherto.

On the subject of soil stabilization, I thought the evidence provided by W. AICHHORN and W. STEINBRENNER (4/1) of the considerable resistance of stabilized soils to freezing of importance. Their results confirm British experience, although the test conditions, both in the laboratory and in the field, were more severe than we have studied. If the high resistance of stabilized soils to frost can be accepted as a general conclusion this means that such materials can be designed with confidence purely on the basis of the strength required to resist the repeated application of known stresses.

The General Reporter has referred to Paper 4/4, by my colleagues F. J. GRIMER and N. F. ROSS, which is concerned with the problem of stabilizing heavy clays. I should like to say that the sole object of this research was to determine the reduction in size of the clay aggregations required to obtain a satisfactory clay-cement mix. The data obtained are to be used in connection with a study of the performance on clay soils of rotary tiller machines of the type used in soil stabilization. Although high cement contents were used for experimental purposes in the laboratory work, the results indicate that a useful stabilizing effect can be obtained with heavy clays, given a suitable machine, with economic proportions of cement.

On the question of the possibility of using trace chemicals to improve soil properties, I would refer to the paper by CLARE and myself to the Zurich conference in which we showed that certain active chemicals markedly affected the suction properties of soils, and that to evaluate the strength of treated soils in the road structure it is necessary to permit the material in the laboratory test to reach an ultimate moisture content. I would suggest that a full evaluation of this type of process, so far as road foundations are concerned, requires measurement of the strength of the treated soil to be made under equilibrium pore water conditions corresponding to a range of water table levels.

The use of trace chemicals to improve the properties of stabilized soil mixes in which Portland cement or bitumen are used is undoubtedly a subject of considerable importance in road engineering and one in which we at the Road Research Laboratory have taken some interest. Our view is that a secondary additive can affect the behaviour of either the soil or the primary stabilizer or both together. For example, when sodium chloride is found to increase the strength of a soil-cement mixture this may result from a change in the structure of the soil or to an acceleration of the reactions whereby the cementing matrix is produced. There is no doubt that the mechanism whereby these trace chemicals operate could be highly complex and essentially requires a fundamental approach. It is encouraging to see from the present proceedings that this is being undertaken.

D. CRONEY (U.K.)

At the opening session of this conference the General Reporter to Division 1, I. TH. ROSENQUIST, was strongly supported by our President when he stressed the fundamental importance of moisture retention and moisture migration in soil. In no branch of soil mechanics can that be more true than in the soil mechanics of roads and runways. Their shallow foundations are vulnerable to the effects of climatic changes which may be all the more effective because of the comparatively light vertical pressures exerted by the structures themselves.

The mechanism by which water is retained in the soil structure and by which it is enabled to move from one point to another is of the greatest importance and it is gratifying to see the ever-increasing interest which it is arousing both in the fields of soil mechanics and soil science. Our discussions at

this conference have shown however that we are still a long way from understanding this problem fully, and we at the Road Research Laboratory in England have for a long time been following the thermodynamic approach to soil moisture. This approach enables the ultimate moisture distribution beneath pavements to be calculated irrespective of the mechanism by which the moisture is retained and irrespective of whether the migration of moisture takes place in the liquid phase as a result of hydrostatic pressure gradients, in the vapour phase as a result of vapour pressure gradients or as a result of local freezing in the soil.

The absence of methods of estimating the ultimate moisture condition in the soil beneath road and airfield pavements is surely one of the main reasons why the development of fundamental methods of pavement design has been slow, and why the soil mechanics of shallow foundations is fundamentally so far behind that branch which treats of deeper building foundations. It is of little advantage to appreciate that soil tests are necessary in the design of pavements unless the conditions under which the test should be carried out can be postulated.

Papers on the Road Research Laboratory approach to soil moisture migration have been contributed to the two previous conferences and in the present proceedings Paper 4/2 (by W. P. M. BLACK and D. CRONEY) describes field experiments conducted in Great Britain over the past seven years, in which the methods have been subjected to practical field verification. In these experiments a careful search has been made for any evidence of thermal moisture migration either in the vapour phase or by the process of thermo-osmosis. No evidence of any appreciable migration of this type was found. However, the small temperature gradients and the comparatively low air voids in the soil would not have been conducive to such movements.

There is no doubt that steadily applied temperature gradients do cause a migration of moisture towards low temperature areas. We have the work of Smith in America, Maclean and Gwatkin in Great Britain, Marshall and Gurr in Australia as well as the interesting experiments described in the present papers by P. HABIB and F. SOEIRO (1a/10) to support this view. Furthermore, in the field of electrical engineering the migration of moisture from the soil surrounding warm heavily loaded cables reduces the thermal conductivity of the soil and leads to the condition referred to as 'run-away' where temperatures build up dangerously. However I have not myself seen any conclusive evidence that the daily and annual sinusoidal fluctuations of soil temperature provided by nature do in fact lead to a thermal migration of water likely to affect the stability of pavements. Undoubtedly a soil covered when dry will tend to become wetter under certain conditions but in the cases which I have investigated this upward migration has been consistent with anticipated changes of pore pressure.

In an attempt to obtain more evidence in connection with thermal moisture flow, the British Air Ministry in co-operation with the Road Research Laboratory has carried out very similar tests to those reported by L. W. HATHERLY and M. Wood in Paper 4/5 in this Division. Over a period of several years moisture contents were measured at various depths and at monthly intervals beneath impervious surfacings and in the adjacent exposed verges. These experiments were conducted at various overseas air stations representing a wide range of climatic conditions. In some cases the seasonal variation of temperature was as much as 17°C. and the daily variation 11°C. Although the analysis is not yet completed, no evidence of any appreciable moisture accumulation under the pavements due to seasonal temperature gradients has been found even under conditions which might be expected to favour vapour migration.

In analysing the results of experiments of this type, where the

moisture content borings necessarily extend over a considerable area, care must be taken to distinguish between variations of moisture due to moisture migration and those associated with changes of soil type. A valid appraisal of L. W. Hatherly and M. Wood's paper is difficult to make because of the absence over the greater part of the experiments of plasticity data. To investigate this matter scientifically I feel that we must exclude the problems arising from variations of soil type by measuring changes of pore water pressure rather than moisture content. The difficulty is that there are at present no reliable *in situ* methods of measuring high negative pore water pressure. With present knowledge however it should not be difficult to develop suitable gauges and this matter is at present under consideration at the Road Research Laboratory.

I personally would be very glad to hear the views of the delegates on the importance which they feel should be attached to vapour or thermo-osmotic flow. This would be of the greatest help in formulating any future research programme.

On the General Reporter's point that there appears to be some difference of conclusion between papers of L. W. Hatherly and M. Wood and that of W. P. M. Black and myself, I would like to emphasize that the conclusions in our paper are based on British conditions, where water tables are generally close to the surface. J. A. J. Salas has discussed this point. However, in view of the relatively high water contents measured by L. W. Hatherly and M. Wood, I am surprised that they found such large moisture variations under the pavement. In my view these large variations must have been due primarily to changes of soil type over the test area.

K. RUSSAM (U.K.)

The effect of temperature gradients on the moisture distribution in the subgrade and the various layers which compose a road or airfield pavement is of particular interest to workers who are concerned with methods of pavement design to be used in territories where extreme climatic conditions are met. The effect of temperature gradients has received considerable attention from theoretical workers, and many interesting laboratory experiments such as that reported in Paper 1a/10 by P. HABIB and F. SOEIRO have been carried out. However, there have been few direct field experiments to determine the consequences resulting from thermal gradients. The evidence presented in Paper 4/5 by L. W. HATHERLY and M. Wood is therefore welcome, and although not conclusive does suggest that moisture migration due to temperature gradients does take place under road pavements and can result in a build up of moisture.

As previously mentioned by D. Croney, an analysis of records of soil moisture contents beneath paved and unpaved areas in overseas airfields is being carried out at the Road Research Laboratory. The analysis suggests that on sand subgrades any moisture migration due to temperature gradients is not of sufficient magnitude to effect materially the strength of the soil, and does not present a pavement design problem. Beneath sealed road or airfield pavements heavy clay subgrades generally remain at or near saturation throughout the year and migration in the vapour phase cannot then take place except possibly through continuous fissures. Since the effect of temperature on the suction properties of soil is also relatively small, migration of water in the liquid phase through clays solely because of thermal gradients is not likely to occur in appreciable amounts. It would appear likely, therefore, that thermal effects will be most important in the case of the intermediate class of soils such as sandy clays and silts which, because of their structure, contain air voids enabling vapour movements to occur and whose strength can rapidly decrease when the soil becomes wet.

The conclusion of L. W. Hatherly and M. Wood that pavement design in Iraq should be based on the characteristics of the soil in a saturated condition (I presume this means the soaked condition) seems however to be rather a wide extrapolation. It would be interesting to know if the soil is in point of fact saturated at the highest moisture contents measured and how this condition compares with that of the soaked sample.

A. R. JUMIKIS (U.S.A.)

It is encouraging to observe in Paper 4/5 (by L. W. HATHERLY and M. WOOD) that engineers now pay increasing attention to the soil moisture migration problem and the consequent change in physical properties of soil. It is also of interest to have data on record of soil temperature and moisture conditions in Iraq.

KERSTEN (1944, 1945) has also presented soil moisture data underneath rigid and flexible airfield pavements built in arid, semi-arid and humid regions. No matter by which soil moisture transport mechanism the moisture accumulates under the pavement—by film, capillarity or vapour, whether the upward moisture transport is started under freezing or non-

freezing conditions, or whether under the influence of a thermal potential water is set free underneath a pavement by thawing—excess moisture in soil changes its original shear strength, causing pavements under load to deform or even break (WINTERKORN, 1945; Report, 1950; TURNER and JUMIKIS, 1956; JUMIKIS, 1955).

BREAZEALE and his co-authors (1951) also report some soil moisture data underneath and adjacent to an asphalt runway at an air base near Tucson, Arizona, which confirm quantitatively the phenomenon of accumulation of soil moisture underneath a pavement.

The migration of soil moisture from warmer regions to cooler ones is a universal phenomenon which occurs in humid as well as arid regions. Temperature differences caused by shading the ground surface with a cover, for example, an impervious pavement or structure, in warm seasons could cause the soil moisture to migrate towards the chilled or cold front, whichever the case is (JUMIKIS, 1955, 1956), Figs. 15 and 16.

It is impossible from Paper 4/5 to evaluate the authors' opinion as to the means by which the soil moisture accumulated underneath the impervious, flexible pavement, whether by

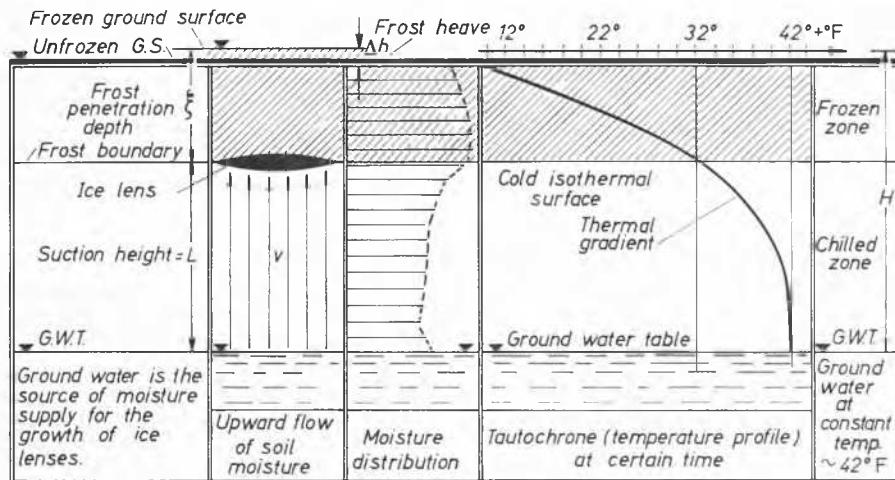


Fig. 15 Sketch illustrating the concept of uni-dimensional upward flow of soil moisture towards the frost boundary (or ice lens) upon freezing—open system
Schéma illustrant l'idée d'un mouvement uni-dimensionnel vers le haut de l'eau contenue dans le sol, vers la limite de glace (ou lentille de glace)—système sans couverture

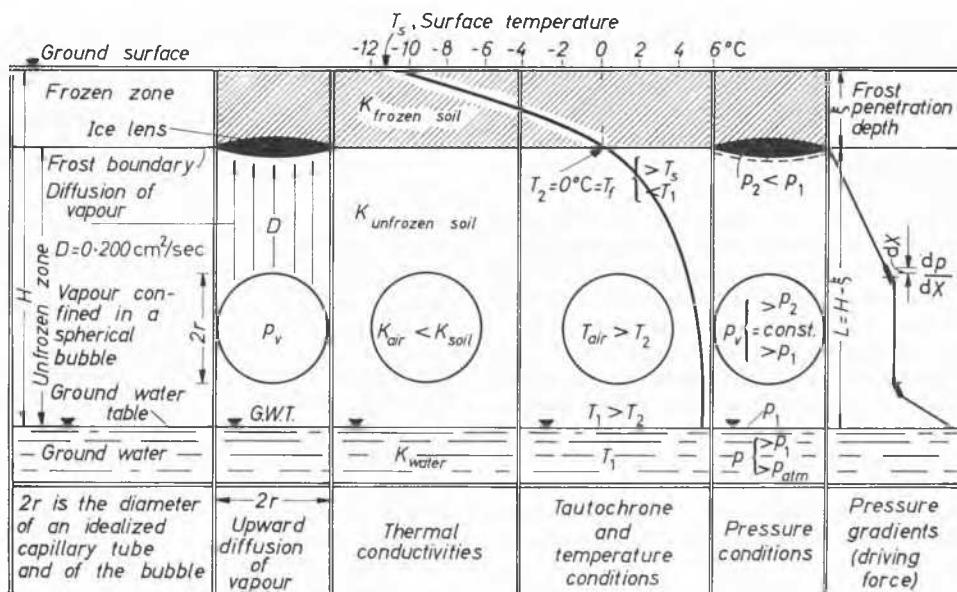


Fig. 16

vapour diffusion or by other moisture transport mechanisms, since they have not presented the basic information upon which such an evaluation could be made. The authors do not report any layers or stratification of soil types, nor difference in soil texture which might cause non-uniform soil moisture distribution, such as, local zones of saturation or a perched ground water table. In the absence of data no reasonably approximate calculations can be made relative to rates and amounts of heat and moisture transferred, or the degree of saturation of the soil. It is an established fact that the size of the voids determines the moisture transfer mechanism. This idea is illustrated in Fig. 17. It has also been shown that soil moisture transfer in the vapour phase is more likely to take place in coarse voids than in fine ones (BESKOW, 1955; JUMIKIS, 1957; CARAZZA, 1953). The southern end of the Mesopotamian Valley surely contains much silt and plenty of clay. It is also hard to imagine

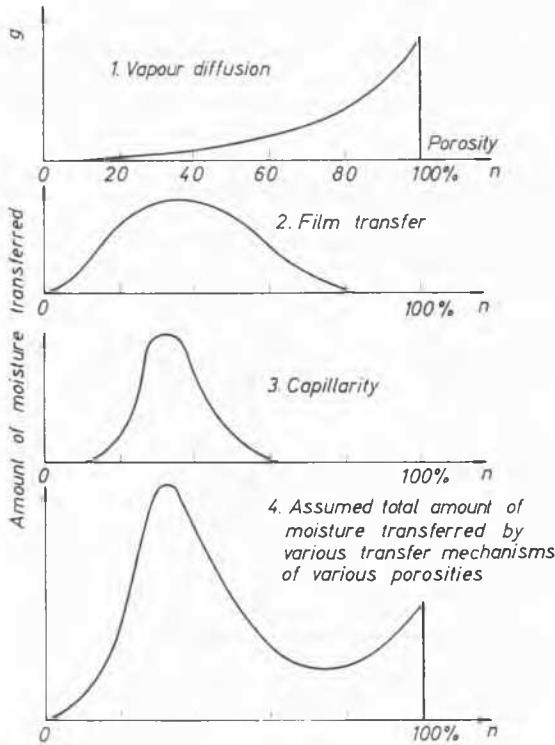


Fig. 17 Various moisture transfer mechanisms and their effects on the amounts of moisture
Différents mécanismes de transport de l'humidité et leur effet sur les quantités d'eau transportées

without adequate explanation why ground water should not be present in that valley.

It would be desirable for the authors to describe whether their pavement is in cut, fill or on level ground, and to say clearly its size and whether it is a runway or a highway pavement, and if it is periodically flooded. It is likewise not only desirable but fundamental to show the soil profile and properties and the position of the water table in order to support the authors' conclusion that the pavement design should be based upon saturated soil conditions. It is also felt that the auger holes in the Baghdad area as reported are far too few to make a generalized conclusion of Southern Iraq.

The authors do not indicate whether the soil moisture content increases progressively under the pavement over a period of time or whether it approaches asymptotically some constant, final value which may be less than saturation. My observation is that the reported soil moisture contents are somewhat above the PL, and far below the LL in the first 3 to 4 m below the

ground surface. The PI range between the 3·5 to 5 m depths is somewhat narrower than above these depths.

The method of measuring soil temperatures by sticking a mercury thermometer into disturbed soil brought up by a metal auger from a depth as great as 5 m cannot be said to conform to current scientific practice, and can be considered only as a rough routine measure. In this respect the paper probably shows the need for more reliable methods of measuring soil and ground water temperatures.

The wide variation in soil property such as the LL at comparable elevations for various months, as reflected particularly in Table 1 of the paper, can mean one of the following things: (1) the soil samples were taken too far apart; (2) they were taken from different depths; (3) the testing was inaccurate; (4) frequent manipulation has broken the soil particles down into finer sizes. ATTERBERG (1911) and CASAGRANDE (1932) have shown that temperatures between 20 and 40° C. do not appreciably affect the LL test results.

It seems that the temperature of 20° C. in January 1954 at the 3 m depth (Fig. 1, p. 115, Vol. 2) should read 30° C. to be consistent with the temperatures at the adjacent points of co-ordinates: likewise, the correct moisture content in January 1955, at the 2·5 m depth (Table 1, p. 117, Vol. 2), should be $W=28$ per cent as in Fig. 2, and not 22 per cent. Minor discrepancies between tabular data and graphs can also be noticed, particularly at depths between 3·5 m and 5 m, but these do not affect the subject matter. I consider that more data are necessary for a correlation to be established between thermal properties, moisture relationship and some of the simpler soil index properties.

References

- ATTERBERG, Alb. (1911). Die Plastizität der Tone, *Internationale Mitteilungen für Bodenkunde*, Bd. I, Heft 1, p. 33.
 BESKOW, G. (1955). *Tjälbildningen och Tjälliftningen*, p. 154. Stockholm: Statens Väginstitut.
 BREAZEALE, E. L., McGEORGE, W. T. and BREAZEALE, J. F. (1951). Movement of water vapor in soils. *Soil Science*, 71, No. 3, 181.
 CARAZZA, L. (1953). *The Influence of Temperature Gradient on Soil Moisture Flow (Master of Science Thesis)*, Utah State Agricultural College, Logan, Utah, U.S.A.
 CASAGRANDE, A. (1932). Research on the Atterberg limits of soils, *Public Roads*, Washington, D.C., 13, No. 8.
 JUMIKIS, A. R. (1955). *The Frost Penetration Problem in Highway Engineering*, pp. 5-7, 108-117. New Brunswick, New Jersey; Rutgers University Press.
 — (1956). The soil freezing experiment, *Highway Research Board Bulletin No. 135*, p. 150. *Factors affecting ground freezing*, Washington, D.C.
 — (1957). Soil moisture transfer in the vapor phase upon freezing. *36th Annual Meeting of the Highway Research Board*, Washington, D.C.
 KERSTEN, M. S. (1944). Survey of subgrade conditions. *Proc. Highway Research Board*, 24, 497, Washington, D.C.
 — (1945). Subgrade moisture conditions beneath airport pavements. *Proc. Highway Research Board*, 25, 450, Washington, D.C.
 Research Report No. 10-D (1950). *Load carrying capacity of roads as affected by frost action*. Highway Research Board, Washington, D.C.
 TURNER, K. A., Jr. and JUMIKIS, A. R. (1956). Subsurface temperatures and moisture contents in six New Jersey soils, 1954-1955, *Highway Research Board Bulletin No. 135*, p. 77. *Factors influencing ground freezing*, Washington, D.C.
 WINTERKORN, H. F. (1945). Theoretical aspects of water accumulation in cohesive subgrade soils. *Proc. of the 25th Annual Meeting of the Highway Research Board*, 25, 422, Washington, D.C.

N. V. ORNATSKIJ (U.S.S.R.)

Monsieur le président, mesdames, messieurs, parmi les différents problèmes qui intéressent cette section de notre conférence, nous considérerons particulièrement comme importante pour l'U.R.S.S. — à part des questions relatives au calcul pour les revêtements souples qui à fait l'objet d'un rapport publié dans le compte rendu — les questions suivantes:

(1) L'étude détaillée des régimes naturels hygrométriques et techniques existant dans les infra-structures des routes. L'étude de nos collègues anglais (W. P. M. BLACK et D. CRONEY, 4/2) a mis en évidence l'importance de la méthode pour mesurer la pression interstitielle et du calcul de la teneur en eau des sols, basés sur les résultats de ces mesures.

Cette méthode fort intéressante est cependant une méthode indirecte, puisqu'elle est basée sur un rapport empirique entre la pression interstitielle et la teneur en eau. De plus, il n'est pas toujours possible de l'employer partout.

C'est pourquoi, la mesure directe de la teneur en eau des sols au moyen de cellules placées dans l'infrastructure de la route constitue un problème à la fois urgent et nécessaire.

En U.R.S.S., on effectue ces mesures à l'aide de plusieurs matériaux hydrophiles, mais seulement pour des sols non gelés. A présent, cette méthode ne peut pas s'employer pour mesurer la teneur en eau des sols gelés.

Entretemps, cette question est particulièrement importante en U.R.S.S. où le gel pénètre à grande profondeur dans la plus grande partie du pays.

J'espère que l'étude de ce problème sera poussée plus avant, parce qu'elle est d'importance pour plusieurs pays à climat froid.

Des études dans le même sens, effectuées par nos collègues anglais L. W. HATHERLY et M. WOOD (4/5) pour les routes en Iraq ont donné des résultats qui sont en accord avec les résultats expérimentaux obtenus en U.R.S.S. par certains instituts de recherche pour les routes.

Ces résultats prouvent que le régime hydrothermique de l'infrastructure est important également pour les régions à climat chaud et sec (Bezrock, 1953; Birula, 1952). Les travaux sont en voie de continuation.

(2) La méthode d'évaluation du danger de soulèvement dû au gel, présentée par le K. F. KEIL (4/7) (Allemagne) ainsi que les recommandations nouvelles des W. AICHHORN et W. STEINBRENNER (4/1) (Autriche) et T. W. LAMBE (4/10) (Etats-Unis) sont extrêmement intéressantes pour l'Union soviétique, parce que dans notre pays, il y a plusieurs régions où le gel pénètre jusqu'à un mètre et demi et même deux mètres et demi.

Plusieurs spécialistes de l'U.R.S.S. travaillent à l'étude du problème de distinguer la région la plus active de la zone gelée dans laquelle la plus grande partie du soulèvement doit se produire.

Lorsque le gel pénètre aussi profondément, il n'est pas possible de remplacer le sol qui gèle sur toute sa profondeur, et on essaie de trouver une solution plus économique mais suffisamment effective pour résoudre ce problème.

(3) Les nouvelles propositions de nos collègues suisses (E. EGOLF, F. GERMANN et W. SCHAAD, 4/3) sont particulièrement intéressants pour l'U.R.S.S. Ces propositions concernent un revêtement combiné 'Flax concrete' qui permet de réduire le danger de fissuration du revêtement lorsque le gonflement dû au gel n'est pas trop grand.

Il sera extrêmement intéressant d'essayer cette construction dans les différentes conditions climatiques et différentes conditions de circulation sur le territoire de l'U.R.S.S.

Je vous remercie cordialement.

G. D. AITCHISON (Australia)

I should like to support the remarks made by D. Croney concerning the need for precise information of a fundamental nature to find moisture changes which can occur in subgrades. Paper 4/2 by W. P. M. BLACK and D. CRONEY provides an excellent example of the type of study that is required. From theoretical considerations by these authors and their colleagues it has been deduced that, within any environment comparable to that of Southern England, the energy of retention of water at any point in the subgrade is merely that which is in equili-

brium with the gravitational potential or, in other words, the soil moisture tension in the subgrade is directly related to the position of the ground water table. This conclusion implies that there is no accession of water to the subgrade due to precipitation, and no loss of water from the subgrade due to evaporation or evapo-transpiration, and also that there is no transfer of water due to thermal gradients.

If these exceptions are made the final equilibrium moisture condition can be determined by simple statics as the authors have suggested. The forces of retention of the soil water can therefore be predicted and measured experimentally and the assumption can be verified. This the authors have done and as a result it must be concluded that for such environments the only significant factor defining the water content of any particular soil is the suction in equilibrium with the ground water table.

Two points are worthy of note in connection with this paper. These are, first, that the soil moisture tension predicted and measured falls well within the working range of the tensiometer, which instrument has been used by the authors. Secondly, it follows from the predictions and can be seen from the experimental data that the moisture condition under the pavement is significantly drier than the wettest natural condition in an uncovered site and, at the same time, significantly wetter than the driest natural conditions.

Turning now to the paper by L. W. HATHERLY and M. WOOD, Paper 4/5, the data given here are qualitative in the sense that they can provide no complete support for any postulated mechanism of water movement. It is noted that there is an absence of a ground water table at any modest depth and so the mechanism mentioned previously cannot be operative. It is suggested by the authors that there is also no possibility of water gain or loss in the subgrade soil due to precipitation or evaporation. Consequently it is logical to invoke the remaining mechanism, that of a dynamic equilibrium in the soil water associated with the phenomena of thermo-osmosis as determined by the seasonal temperature cycle.

The experimental evidence of the authors supports this opinion as far as is possible with such indirect observations. Since the mechanism proposed is in fact a complex continuous cyclic process leading possibly to a simple dynamic equilibrium, any theoretical treatment must involve both potentials and rates of flow, and experimental support must be sought for both quantities. Measurements of this type have not yet been made.

In the meantime the data of L. W. Hatherly and M. Wood support the view that the thermo-osmotic phenomenon is a significant factor in determining subgrade soil moisture in Southern Iraq. The design moisture conditions noted in the subgrade beneath the impervious pavement is in fact that of saturation, that is, effectively equivalent to the wettest condition obtainable during the seasonal cycle.

As a contrast to these data I would like to present some comments on moisture conditions in the subgrades of pavements in warm semi-arid areas as found in some parts of Southern Australia. As a working hypothesis in studying water movements under such conditions we may consider the case of a soil without a shallow water table. If in addition to the thermo-osmotic phenomena just discussed we admit the possibility of water gain in the subgrade due to direct infiltration through the pavement or due to lateral flow from the verges, and if we also recognize the probability of some water loss due to evaporation through the pavement (or due to evapo-transpiration associated with distant vegetation) we arrive at a modified form of dynamic equilibrium affecting moisture retention at any point. This modified dynamic equilibrium value differs significantly from the thermo-osmotic equilibrium only if the net evaporative loss from the subgrade is a dominant factor. As the basis for developing a working hypothesis it is

therefore assumed that the net evaporative loss from a point beneath a pavement may be of comparable order of magnitude to the evaporative loss from a point similarly located beneath a bare soil surface. In this connection it is important to note that a pavement is considered to consist of a graded gravel base course with an extremely light bituminous surfacing—as is typical in many parts of Australia. If this assumption is made, it can be shown that there may be a tendency for a net water loss from the subgrade until dynamic equilibrium is reached in a comparatively dry state. Values of soil moisture tension

water table investigations have shown that the soil under the centre of a pavement achieves an approximate equilibrium condition at a soil moisture tension of the order of pF 4, as shown in Fig. 18. At the edges of the shoulder the soil follows a full cycle of wetting and drying, and this cyclic movement of soil water penetrates some distance under the pavement, but is not evident at the centre.

In other experiments in areas with a shallow water table a similar pattern of moisture distribution appears to be evident with a stable, relatively dry condition beneath the centre of the pavement, while at the edges of the pavement there is a marked seasonal effect which diminishes with distance towards the centre. The moisture tension in the relatively dry zone under the centre of the pavement is obviously dependent upon the water table location. The term 'relatively dry' is used to suggest that the tension is significantly higher than that indicated by the approach of W. P. M. Black and D. Croney, and therefore also significantly higher than in the saturated state suggested by L. W. Hatherly and M. Wood.

We thus have at least three different approaches towards defining the factors determining the design conditions for soil moisture tension and soil water content in a pavement subgrade. Since both the strength and the deformation characteristics of the subgrade can be related to soil moisture tension there is obviously a need for a great deal of additional evidence before any design, except of a conservative nature, can be accepted.

F. A. SHARMAN (U.K.)

I wish to refer to three papers presented to this Division, all of which throw light on the effect of soil moisture and climate on the design of pavements. W. P. M. BLACK and D. CRONEY in Paper 4/2 give criteria for estimating in advance of construction the equilibrium moisture content to be expected beneath a concrete slab in a temperate climate with an ascertainable water table. They deduce that the CBR developed under flexible pavements can be reliably forecast from laboratory tests, adding the *caveat* that small leaks in the pavement can have a profound effect on the moisture beneath.

Some types of flexible pavement are so different from concrete, in respect of permeability, probability of leaks, and of pore size at the boundary between the pavement and the sub-grade, that it seems a very big jump from the experiment described to the conditions under, for instance, a crushed rock base surfaced with a bitumen premix. Perhaps some stepping-stones will appear with the full account of this work.

L. W. HATHERLY and M. Wood in Paper 4/5 suggest that when the seasonal temperature range is great, condensation in the soil voids under an 'impermeable' surface may raise the moisture content to saturation, and that pavements should be designed under these conditions on a soaked CBR basis. It is worth noting that in their case the pavement was a flexible one with asphalt surfacing. C. VAN DER VEEN in Paper 4/20 reports a case in which the soaked laboratory CBR had given much lower values than were found by *in situ* CBR tests, either under the pavement or in the exposed soil. In commenting on this the General Reporter states, if I have understood him correctly, the difference is remarkable in that the compaction and moisture contents in the two sets of tests were similar. According to Tables 1 and 2 of the paper it would appear that in fact the 1947 soaked specimens were much wetter than the soil was when it was tested during dry and hot weather in 1953. If a worst seasonal moisture content of around 35 per cent is ever experienced, as Fig. 2 suggests it may be, the pavement may not be so greatly over-designed after all.

In the design of some flexible base roads for West African conditions using water-sensitive laterites as sub-base material,

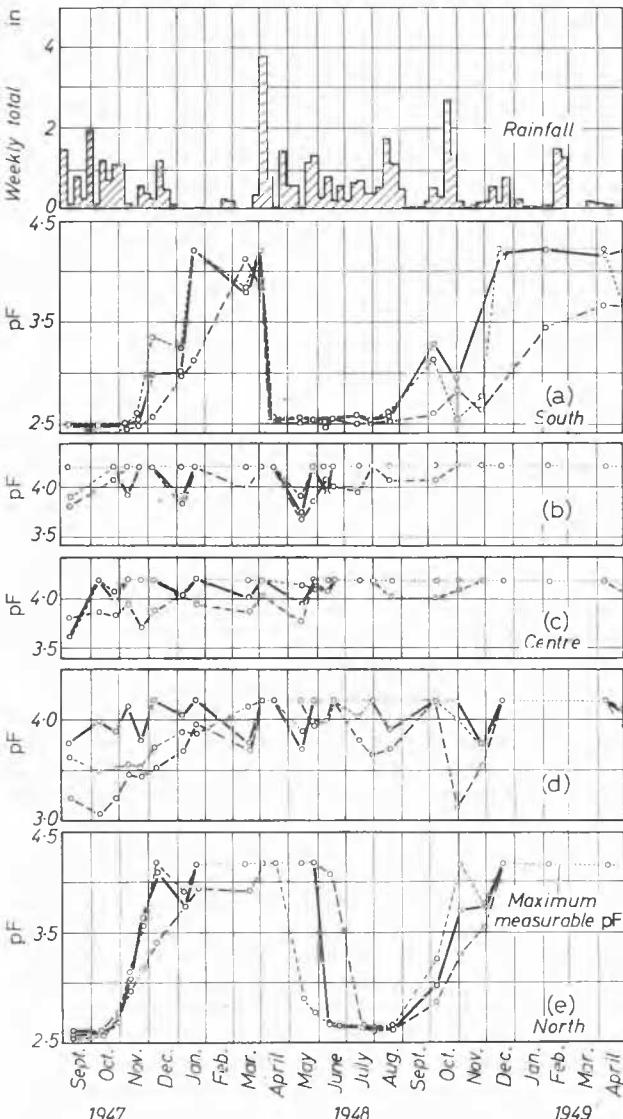


Fig. 18 Changes of soil moisture tension beneath a sealed pavement on a cross road, Adelaide

Changements dans les tensions dans l'eau du sol en dessous d'un pavé étanche sous un carrefour, Adelaide

associated with such a mechanism may be as high as those corresponding to pF 5 approximately. Thus the experimental confirmation of such a hypothesis requires measurements outside the working range of the tensiometer as used by W. P. M. Black and D. Croney. The only instrument readily available for the *in situ* measurement of such moisture stresses is the gypsum block which, despite its inaccuracies, can provide valuable data with intelligent handling.

Some limited experimental investigations have been made in Australia with these techniques. At a site without a ground

it has been found expedient to specify a minimum CBR value for the sub-base after a length of soaking time which has been varied according to the drainage conditions, the nature of the surfacing and the grading of the base. It seemed easier to relate a soaking period to real conditions than to guess a worst moisture content and to see whether this intersected a CBR moisture curve at an acceptable level. It cannot be claimed that the methods adopted so far are very precise or scientific. However, neither the tidy and temperate solution of W. P. M. Black and D. Croney, nor the vaporous cycles of L. W. Hatherly and M. Wood quite fit the case. Although I have no doubt that at some seasons of the year an over-design as striking as the one reported from Beek could be deduced by CBR *in situ* tests, I can say with painful certainty that in one or two places, after exceptional rains, there has been no over-estimate of the softening which can occur.

L. W. HATHERLY (U.K.)

In view of the fact that our General Reporter has suggested that we discuss the relative importance of capillary suction and thermo-osmosis in foundation soils, I should like to state that in my opinion thermo-osmosis may only be a significant factor influencing the moisture distribution in subgrade soils in certain extreme conditions.

M. Wood and I have carried out a certain amount of further investigation and it appears that several conditions must be fulfilled before thermo-osmosis may become a significant factor. The first condition is that the particle size distribution of the subgrade soil should approximate to a soil which could be considered as being susceptible to frost heave. The second is that the soil must contain ionizable salts; this point has been demonstrated on a laboratory scale by P. HABIB and F. SOEIRO in Paper 1a/10. Thirdly, there must be a water table within a reasonable depth below subgrade level.

In the conditions in which we were working in Iraq all these conditions were fulfilled, and in addition the annual variation in temperature was probably as large as anywhere in the world. We believe that we were dealing with a particular set of circumstances which possibly may be reproduced in other parts of the world, but that as a general rule thermo-osmosis may be neglected in-so-far as it affects the moisture content of subgrade soils.

N. R. SRINIVASAN (India)

I should like to say a few words on T. W. Lambe's paper (4/10) dealing with the stabilization of the soil taking into account its mineral composition.

I shall first attempt to make a slight distinction between the geological and the mineralogical approach to soil mechanics. The geological processes, on which many speakers in this conference have laid emphasis, are continuous, starting from the distant past, and may take varied forms depending upon the environments. This makes them quite complicated. What we are concerned with in soil mechanics are the materials and their properties as they exist. This could probably be better studied by finding the mineral components and their structure which influence their behaviour. Already it is well known how the inner structure of the clay minerals can directly influence the physicochemical properties and through them the engineering properties of the soil. Hence it appears that studies of the type outlined in this paper are important.

I should now like to comment upon two points in this paper. One concerns the aging of kaolinite in water during which it is said that aluminium ions are removed from the structure. This aluminium is then supposed to take part in base exchange reactions. It has previously been suggested by Mukerjee and others, in their studies into the electrochemical properties of

clays, that some such leaching of the structural aluminium is possible; but I do not think that this has been finally established and there are difficulties, more so in the case of kaolins. Among the clay minerals, kaolin has electrically the stablest structure. It has nearly the same chemical composition since isomorphous replacement does not take place in its structure even though it is found under different environments. In such a structure as that of kaolin it is difficult to conceive of water breaking the ionic bonds and releasing the aluminium. If any such removal is possible, it should leave the remnant kaolin structure in an electrically unbalanced state, thus increasing its base exchange capacity many times, probably approaching that of montmorillonite. A surer method of determining whether or not such removal actually takes place is perhaps to determine the base exchange capacity of the fresh and the aged kaolins. So far as is known natural kaolins from different areas and environments, unless subjected to special processes such as mechanical grinding or perhaps the polyphosphate treatment, etc., always have a base exchange capacity between 3 and 15 me/100 g.

The other point I should like to stress is the perhaps well known one that when trace additions of chemicals are recommended, a method of dispersing the trace chemicals evenly in the soil should be evolved. Even with cement stabilization of clayey soils where a larger percentage of stabilizer is used, this difficulty is present. There is no need to point out how much more difficult it is when dealing with fractions of a per cent of the stabilizer in the field.

M. P. P. DOS SANTOS (Mozambique)

Monsieur le président, mesdames, messieurs, je voudrais retenir votre attention pendant quelques instants pour vous présenter les résultats d'une expérience vieille de 10 années environ en matière de stabilisation des sols au Mozambique, Afrique Orientale Portugaise.

La stabilisation mécanique, par simple compactage d'un sol ou d'un mélange des sols a connu un très grand essor malgré des facteurs défavorables en ce qui concerne les conditions climatiques et physiographique du territoire. En se dirigeant très nettement vers le critère du contrôle de la plasticité au lieu d'avoir le préoccupation de la granulométrie, il y a été montré, par une expérience de quelques années qu'il était possible d'obtenir d'excellentes couches de fondations et de roulement, particulièrement avec une protection bitumineuse mince, même quand la distribution granulométrique était assez différente de celle traduite par la courbe de Fuller pourvu, évidemment, qu'on s'assura de l'existence de conditions satisfaisantes en matière de drainage et de contrôle de la nappe phréatique.

A l'heure actuelle, une enquête de vaste envergure sur les conditions optima d'application des latérites et des sols latéritiques a été entreprise en coordination par les trois laboratoires portugais de Lorenço Marques, Luanda et Lisbonne. Quelques routes expérimentales ont été construites au Mozambique et mis en observation.

En ce qui concerne la stabilisation chimique, c'est, sans doute, le procédé sol-ciment qui connaît la plus grande faveur, d'ailleurs bien justifiée si on pense à la tenue des routes qui ont déjà souffert l'épreuve cruciale du temps. Pour certains types de sol, le pourcentage de ciment nécessaire a été aussi bas que 4 pour cent ou à peu près 11 kg/m²; pour les autres, sable des dunes notamment, ce pourcentage a dû être porté à 12 ou 22 kg/m².

Toutes ces routes ont été construites avec des moyens assez puissants, ce qui est regardé comme une condition essentielle si l'on veut réussir une stabilisation du ciment.

Des routes expérimentales ont été construites également en vue d'analyser le comportement des sols-ciments sous protec-

tion bitumineuse mais avec application de gravillons enrobés dans un laitier de ciment dans les dernières phases du compactage. Les résultats provisoires semblent indiquer les possibilités de cette méthode pour un trafic léger.

La détermination de l'épaisseur des chaussées est toujours conduite d'après la méthode CBR avec 4 jours de saturation et qui a donné jusqu'à présent entière satisfaction.

Pour terminer, je voudrais dire encore qu'on attache, au Mozambique, une extrême importance à l'observation permanente des travaux effectués afin de juger de leur comportement sous le trafic. Toute détérioration est signalée au laboratoire central ou à un des laboratoires mobiles opérant en place pour une complète détermination des causes des ruptures constatées.

Jusqu'à présent, tous les cas examinés ont pu être classés comme suit:

- (a) épaisseur de la chaussée insuffisante d'après la méthode CBR,
- (b) insuffisance du compactage du sol porteur ou de la couche de fondation,
- (c) mélange des sols mal exécuté,
- (d) insuffisance du drainage ou du contrôle de la nappe phréatique,
- (e) emploi de revêtements bitumineux trop perméables.

La surface totale des chaussées sur lesquelles on a constaté des ruptures n'excède pas 1 pour cent de la surface totale revêtue.

W. AICHHORN (Austria)

Stabilization with bituminous material confers two very important properties on the soil. They are: (1) the cementing



Fig. 19 Before stabilization the road showed heavy frost damage after every winter

Avant la stabilisation, la route était sévèrement endommagée chaque hiver

effect of the binder increases the cohesion and, thereby, the bearing capacity of the soil. The percentage of binder used

has to be carefully controlled as there is an optimum binder content above which the cementing effect is reduced and the binder acts as a lubricant. If an excess amount of binder is used the strength can be reduced below that of the unstabilized soil. (2) The water-repellent binder, which will coat particularly the fine particles, causes a decrease of the permeability and, thereby, also diminishes the danger of frost action on the soil. In this field considerable success has been achieved in several cases (Figs. 19 to 21).

When constructing a forest road in the forest district of



Fig. 20 Applying hydrated lime by hand
Application de chaux éteinte

Heilbronn, Germany, the very plastic local loam, with a PI higher than 18, was first stabilized by the addition of 3.5 per cent hydrated lime and admixing of sand, spread in an 18 cm thick layer upon the ground, thereby the PI was considerably decreased. This done, the road could be used by trucks. After that, the soil was left in this state for three months and then pulverized by means of a Seaman-Pulvi mixer and finally stabilized by adding 6 per cent tar emulsion. No damage



Fig. 21 The road after stabilization
La route après stabilisation

could be observed after the winter. At the same time the stabilized road described in Paper 4/1 showed not the slightest frost heave after the winter.

There are sometimes difficulties when the bituminous binder is mixed in place with the soil. To overcome such difficulties and in order to achieve a good coating of the individual particles it is necessary to admix about $\frac{1}{2}$ per cent sodium carbonate or, in some cases, small quantities of hydrated lime

to the mineral aggregate. This is best done by spreading the sodium carbonate on the pulverized layer to be stabilized and mixing it by means of a motor grader or a ground mill before applying the binder. More than 100,000 m² of road have been successfully treated in a similar way in Austria.

P. J. ALLEY (New Zealand)

I should like to refer to Paper 4/10 by T. W. LAMBE and to suggest that it would be better, in studies of soil stabilization with cement, to regard the additive as a waterproofing agent only and not to regard it as contributing to the strength of the resulting mixture. It requires only about 3 per cent of cement to prevent slaking in a loess material in New Zealand, and a soil cement mixture with 7 per cent by volume of cement has been used extensively for house building. The strength of the soil cement mixture is derived from the soil as it dries out. It is true that increases in the amount of cement will give increased compressive strengths, but the result in the end would be a concrete which is what should be avoided. A mixture of a sand and cement cannot be termed a soil cement, but it is a sand concrete. The aim should be to keep the moisture content below the PL.

There has been some house building in soil cement in New Zealand. The soil used in the Christchurch area is a modified loess, with 15 to 20 per cent of the particles finer than 5 microns and 70 to 80 per cent finer than 50 microns. Outside walls are made 8 in. thick and internal walls 6 in. thick, and the floors are solid. In Wellington the Ministry of Works has just let a contract for the building of ten soil cement houses by mass production methods. Here the soil is derived from the breaking down of greywacke and is finer than loess.

A. P. J. VERHEYDEN (Belgium)

Monsieur le président, messieurs, en ce qui concerne le premier sujet désigné par notre Rapporteur général, M. R. Peltier, et dans le cadre de l'intervention de G. Moraldi, je me permets d'attirer l'attention de nos collègues d'Afrique, sur la possibilité d'exploiter au maximum les circonstances favorables qu'offre le sol naturel dans les zones immenses de territoires comme le Congo Belge, l'Angola, les Rhodésies et l'Afrique du sud.

Sans vouloir généraliser il est permis de dire que très souvent le sol naturel, le sol en place ou un sol d'apport éventuel peut constituer la fondation même de la route et porter directement le revêtement hydrocarboné.

Ceci est spécialement le cas dans les régions sableuses qui constituent un très grand pourcentage de la surface des territoires en question et où le trafic est souvent lourd mais, généralement, peu dense.

A titre d'exemple, je signale qu'un tronçon expérimental, long d'un kilomètre a été réalisé sur la route Léopoldville-Matadi, à la sortie de Léopoldville. Le trafic journalier est de l'ordre de 1,200 véhicules dont 60 pour cent de voitures légères (maximum 700 kg par roue) et 40 pour cent de véhicules lourds (une à quatre tonnes par roue).

Le sol en place est constitué de sable fin, peu limoneux. Sa granulométrie est en moyenne la suivante:

tous les grains sont inférieurs à 1 mm

5 pour cent des grains supérieurs à 0·42 mm (tamis ASTM 40)

75 pour cent de grains entre 0·42 mm et 74 microns (talis ASTM N° 200)

20 pour cent des grains inférieurs à 74 microns

de 5 à 10 pour cent des grains inférieurs à 20 microns

environ 2 pour cent des grains inférieurs à 2 microns

Ces sables ne sont pas plastiques. Leur poids volumétrique naturel est d'environ 1·5 t/m³. Ils donnent à l'essai Proctor

(U.S. Army Engineers modified) un poids volumétrique sec d'environ 1·95 t/m³. La teneur en eau optimum est de 8 à 10 pour cent. A un degré de compaction de 95 pour cent le CBR est environ 50 pour cent après 4 jours d'immersion. Pour des degrés de composition de l'ordre de 100 pour cent, le CBR est environ 75 pour cent. Après 4 jours d'immersion le degré de saturation ne dépasse pas 85 pour cent.

Dans les 30 cm supérieurs, des degrés de compaction de 95 à 100 pour cent ont été obtenus sur chantier en compactant, à un teneur en eau de 7 à 11 pour cent, avec un rouleau pneumatique de 50 tonnes sur quatre roues.

En cas de remblais les couches successives ont une épaisseur de l'ordre de 25 cm après compactage. Les sols des zones d'emprunt ont, généralement, et cela durant toute l'année, une teneur en eau qui se situe aux environs de la teneur en eau optimum de compactage.

Le sol ainsi compacté a reçu une simple imprégnation et nous y avons posé un enrobé dense de 4 cm d'épaisseur.

Depuis trois ans le tronçon expérimental en question porte, d'une façon très satisfaisante et sans le moindre entretien, le trafic signalé, soit 1,200 véhicules par jour.

Nous avons constaté par la suite, sur d'autres tronçons d'essai qu'il est possible de compacter des sols similaires d'une façon suffisante en canalisant judicieusement le trafic des engins de chantier: les scrapers et les bennes.

En ce moment, nos recherches portent sur l'utilisation de plaques vibrantes, type Vibro-Verken, pour le finissage du compactage de la couche supérieure qui recevra directement le revêtement.

Il est important de signaler que nous n'épargnons aucun effort pour assurer l'évacuation immédiate des eaux de pluie condition essentielle pour la tenue de ces routes. Les eaux de la nappe phréatique ne présentent aucun danger; elle se trouve, en effet, généralement à plusieurs mètres de profondeur et le sous-sol est sableux. Pour des routes de ce genre d'avoir une assise de portance homogène. La mince couche de revêtement doit, en effet, pouvoir jouer entièrement et efficacement son rôle protecteur et les déformations de la carpette doivent être limitées le plus possible.

L'homogénéité des zones d'emprunt, prospectées soigneusement au préalable est contrôlée au moyen d'essais d'équivalent de sable tandis que l'homogénéité de portance du sol est contrôlée au moyen d'appareils de pénétration légers d'une puissance maximum de 2·5 t et des sondes batteuses légères.

Ces deux appareils se complètent très bien et donnent entière satisfaction.

Le premier appareil est décrit par G. PLANTEMA à la page 237 du volume I des proceedings et la sonde batteuse est l'appareil g décrit par messieurs E. SCHULTZE et H. KNAUSENBERGER à la page 249 du même volume.

K. ARULUNANDAN (U.K.)

I have a few comments to make, particularly with reference to Paper 4/20 by C. VAN DER VEEN. It is interesting to note the comparison made between the CBR method of design and the method by loading test, but, unfortunately, test results before and after construction offer no comparison of the strength whatever of the subgrade to enable the author to reach his conclusion 'that the CBR values obtained on soaked samples give an under-estimate of the bearing capacity of flexible runways'.

Initially, with regard to Fig. 1 of the paper, the fact that the material could exist at a moisture content of 40 per cent was established, which could be quite reasonably assumed for this material as the worst likely moisture condition which could be expected under the pavement; but the average CBR values assessed as 28·5 per cent before construction were in a moisture content range of 26 to 29 per cent, and the average CBR value

of 44 per cent assessed after construction was in the moisture content range of 23 to 27 per cent. The CBR value of 7 per cent assessed after four days' soaking in the laboratory was in the moisture content range of 34 to 41 per cent, which range is the most likely worst condition which could exist under the pavement, and could be used as the best assessment for design purposes.

With regard to the loading test carried out after construction, the condition of the subgrade did not represent the worst conditions, and hence the comparison is not valid. It would have been interesting to note the behaviour of the runway if the higher CBR value of 44 per cent had been used for the design.

I had the opportunity while investigating the strength of a runway in West Africa to check the existing strength by *in situ* CBR determinations at probably the worst moisture conditions, and then to carry out in the laboratory CBR determinations under the same conditions: there was good agreement in the results. Loading tests were also carried out to assess the load classification number accurately in accordance with the practice in this country, but there was no agreement between the assessed load classification number value and the corresponding CBR value as proposed by certain authorities for the thickness of construction which existed, suggesting that an over-design could possibly result by using the CBR method in certain types of material.

As a matter of interest, I should like at this point to suggest to those who find D. Croney's work and the terminology rather difficult to grasp that it is almost the same as the concept put forward by A. W. Bishop of Imperial College, that is, an empirical formula for the determination of equilibrium moisture content based on determination of the percentage passing through a No. 200 sieve and the PL. While investigating in this country during the winter season several runways which were about 7 to 10 years old, for strengthening purposes, I had the opportunity to determine the moisture content which existed under the runways and to assess the percentage passing through the No. 200 sieve and the PL for cohesive soils which had a PL range of 15 to 30, and the percentage passing through No. 200 sieve between 60 per cent and 98 per cent. The results were plotted on a triangular diagram and the value of E.M.C. worked out at $(PL + Pass\ 200)/5$ to the nearest whole number. This formula was used to assess the equilibrium moisture content for the subgrade under the runways in West Africa and checked by field determinations: over a period it was found to agree very closely. I had the opportunity to check this with some results obtained by the Air Ministry, and a close agreement was reported for the particular types of soil mentioned previously.

In Paper 4/13, S. R. MEHRA states that 'a CBR value of 36·5 is a reasonably good value for low cost roads not exposed to heavy traffic'. That is questionable. I can only say that after constructing a few hundreds of miles of first-class low cost roads in West Africa, and observing their behaviour for as short a period of a year or two, in some cases it was found that roads constructed with a CBR of 35 per cent failed badly; bases with CBR of 60 per cent showed less signs of failure, but bases with CBR of 80 to 110 per cent seemed to show up reasonably well; slight signs of waviness tended to show up which presumably will increase with time.

It is rather interesting to note the results and tests carried out by U. NASCIMENTO and A. SIMÕES (4/15) on the relationship

between CBR and the modulus of strength. I would like to add that recently some authorities in this country, finding the inconsistent and unreliable results of *in situ* CBR obtained on stony base materials, are endeavouring to establish a loading test using a plunger of greater diameter than the CBR plunger to give results comparable to that expected with a CBR test. This has possibly been the trend which I gather from various papers published for this conference. I feel a loading test to assess some value, possibly called 'stiffness', could be developed which would eventually take into account such problems as vibration. The nearest approach to this so far is the Dutch Shell vibrating machine.

C. VAN DER VEEN (Netherlands)

I should like to state very shortly, in regard to the airfield at Beek, that the CBR upon which the design was based was determined after four days' soaking without compaction. The CBR of compacted samples was also determined but not accepted as a base for the design because it appeared impossible to compact the soil in the field due to the very high water content. Actual compaction of the soil beneath the constructed runways cannot have been very much as appears from the measured density. I believe therefore that the tests have fairly well established the predominant influence of the water content on the obtained CBR values for the type of soil involved.

As to the comment of F. A. Sharman, who questioned whether the runways at Beek were really over-designed, I can say that since the tests reported by me were carried out, heavier aircraft were allowed on the airfield, in accordance with the tests, and up till now the runways have behaved perfectly well.

I am afraid that I am not able to comment on K. Arulundan's remarks due to the lack of time.

General Reporter

Monsieur le président, mesdames, messieurs, comme mes prédécesseurs, les Rapporteurs généraux des autres sections, je serai très bref car il ne paraît pas possible de tirer, en quelques minutes, des conclusions claires et courtes de la discussion longue et variée que nous venons d'avoir.

Je dois toutefois souligner l'intérêt des exposés présentés. A mon avis, cette discussion orale a mieux fait apparaître les progrès accomplis en géotechnique routière que les communications écritées, car les nombreux orateurs qui se sont succédés à la tribune ont traité de sujets très variés s'étendant presque sur la totalité du domaine de cette 4ème section.

Je me bornerai alors à une appréciation générale.

Nous disons souvent en France, que la route est une création continue, c'est-à-dire qu'il ne s'agit pas seulement de la construire, mais qu'il faut aussi l'entretenir, la réparer, l'améliorer progressivement pour l'adapter aux progrès de la circulation.

Je pense qu'il en est de même pour la géotechnique routière où aucune formule, aucune méthode ne doit être considérée comme définitivement établie mais doit sans cesse être améliorée et réadaptée.

Il s'ensuit que cette science doit être en évolution continue et même en évolution rapide, vu sa jeunesse. Je pense que les discussions d'aujourd'hui nous auront convaincus de la réalité de cette évolution et même du dynamisme de cette évolution. Aussi, je suis certain qu'au prochain congrès nous aurons à enregistrer de nouveaux et très grands progrès en cette matière.