hydropower caverns, and we gradually go on up through storage caverns, water tunnels, to some limit for permanently supported openings. If you go beyond this limit with an unsupported span, you are going to reduce the standup time to something below permanent. What we consider permanent is a matter for discussion but, say over 100 years.

To conclude, in Fig. D are the vital statistics of all those excavations which plotted below the boundary and are permanent. For a permanently unsupported cavern, we recommend that you require these characteristics. I will try to translate the symbols for you. The top line shows these absolute requirements - you require three joint sets or less, otherwise you've got problems with a lot of overbreak. You require joints which are not slickensided. They must have a slight roughness, preferably a lot of roughness. You must have no alterations to speak of, very minimal water seepage, and stresses must be favorable. If you have a low RQD, less than 40, you should have a smaller number of joint sets, preferably less than 1 joint set. If you have as many as 3 joint sets which is the limiting case for 3 dimensional blocks, you should have rougher joints and higher RQD, in other words a wider joint spacing. If joint roughness is at the lowest level, you should have 2 joint sets or less. If the stress reduction factor is unfavorable, you should have rougher joints. If the span is greater than 10 meters, you should have less than 3 joint sets. If the span is greater than 20 meters, you should have less than 2 joint sets and favorable stresses. All these, I think, are common to our experience but it is useful to collect them in a table and, when you're considering a large unsupported excavation, to check through these conditional factors, particularly if the excavation falls near the no-support boundary.

Bischoff

I'd like to respond to Mr. Pacher's suggestion that decisions to use rock reinforcement as opposed to steel sets or an internal support system shouldn't be based on economics alone. I quite agree with that, and I believe that I mentioned some things in the paper which point out the obvious advantages of rock reinforcement over steel sets or internal supports. However, the purpose of this paper was to provide a means of calculating a rock reinforcement system equivalent to a given internal support system. The other thing mentioned was that I hadn't taken into account the increased rock mass cohesion due to the installation of the rock reinforcement. This is quite true. I just used the friction angle of the rock joint for the rock mass. My reason was that I had some difficulty in assessing what that increase would be. The only way to do this type of thing is to actually map the area very carefully in the field and provide some sort of an assessment during the design stage. The procedure discussed in our paper is intended as a preliminary assessment for a design or economic comparison.

Einstein

I only want to make a brief comment that may help to put the paper in context. It is quite clear that swelling is only one of many time-dependent phenomena that you may encounter in tunneling. Even if you look at swelling alone, there are several aspects of this phenomenum that might be considered. The paper concentrated on the "stress release - water content increase" problem, particularly because anhydrite and many of the shales and marls are mainly subject to that kind of swelling. As has been pointed out by Mr. Pacher, we were able to develop a test-analysis procedure and also recommend a few design features and construction procedures. I should, however, point out that if you have creep this has to be considered in addition; also squeezing, which I would define in a simplified manner as the reduction of strength with time, which then naturally leads to volume increase, has to be considered in addition to the swelling as we considered it. Also, we used an oedometer type test in the laboratory to get our input to the analysis. The stress conditions in the oedometer corresponded well to the cases that we looked at but that may not be the case under other circumstances, so you have to take this into consideration and possibly use other tests or make appropriate corrections.

Haimson

I would like to respond directly to Mr. Pacher's questions about the method. The first one was regarding the breakdown and shut-in pressures and their relation to the stresses. The procedure involves a borehole with the area to be hydraulically fractured sealed off with packers and a pump and pressure measuring equipment at the surface. The breakdown pressure is the pressure needed to induce the fracture and it is related by elastic theory to the two horizontal principal stresses when we have a vertical hole. The shut-in pressure is the pressure necessary to keep the fracture open and is measured just when the pumps are shut off after the fracture has been extended. It is considered to be equal to the stress acting perpendicular to the fracture.

Determination of the stresses requires knowledge of the properties of the rock. One property which is a problem to determine is the tensile strength. We have found it best to run simulated tests of this hydrofracturing method in rock samples in the laboratory by actually simulating the same processes as in the field. We feel we can thus better obtain the value of the tensile strength and then use it in such a way as to make a better determination of the principal stresses from the breakdown pressure.

The second question was about the effect of the direction of the borehole, the inclination of the borehole, on the determination. This is a very important point. The present method is only useful if the axis of the borehole is parallel within something like 10° to the direction of one of the principal stresses. The references to other measurements that I have checked in most cases show one of the principal stresses to be vertical, or very close to vertical. Not in all cases, I admit, but in most cases this is true. These are measurements using other methods. What we have found in the laboratory by testing simulated inclined holes, is that with holes inclined at 30° or more to the direction of vertical principal stress, we can get almost any type of fracture - not just vertical but sometimes inclined and sometimes

perfectly horizontal. This does not happen when the direction of the vertical principal stress is parallel to the axis of the borehole. In one borehole in the field, a 300 meter borehole, for example, you may run something like 6 tests to get a good value of the stresses. Now, if the results of the impressions are such that the fractures are always within a very small range of directions, let's say within plus or minus 20° or so, as happened in the cases cited in the paper, we feel very strongly that this is a vertical fracture that represents vertical and horizontal principal stresses.

In the case of Helms Creek Power House in California we were required to run tests in an inclined hole in addition to the tests in the vertical hole. The tests in an inclined hole at 30° to the vertical confirmed, as is stated in the paper, that one of the principal stresses was parallel to the vertical borehole axis. I can also report that the Los Alamos Scientific Laboratories have conducted probably the most elaborate type of hydraulic fracturing in conjunction with a geothermal energy project. They were able, at a depth of, I think, 6,000 ft. below surface, to determine that, although the direction of the well at that depth was something like 4° off the vertical, the direction of the hydraulic fracture was perfectly vertical.

Lindner

First off, I'd like to thank Mr. Pacher for his comments; we appreciate them. I would like to add a few points. Exploration is generally performed in stages. However, in each stage, even in the final stage, there is uncertainty. There is uncertainty in the knowledge of the geology below the surface. There is uncertainty when we predict the cost and time of the project, in the manpower and equipment and how they will perform in that environment. With a computer model, with the tunnel cost model that we have developed, we can evaluate these uncertainties to come up with a time-cost spread for the project. The time-cost spread quantitatively illustrates the uncertainty. In other words, the project time has a range, and the project cost has a range. You can see that for a specific case study on Fig. 4 of our paper. Exploration has the purpose of reducing the uncertainty in your time-cost projection.

However, in using exploration tools and performing exploration, there is additional uncertainty. Even the knowledge we get directly out of a borehole is open to question. This will again generate a degree of uncertainty about our knowledge of what is down there. The method proposed in our paper uses common probabilistic techniques, I emphasize the word 'common', to analyze these uncertainties, using both statistical input and subjective input from the geologist on the job, because he probably has a better feeling of what is down there from his total experience with the site than the bare data retrieved from a few boreholes can tell us. The computer method gives you a quantitative assessment of what is down there. It can show you, and this perhaps is the major point of the paper, how to compare the cost of exploration with the reduction in the time-cost uncertainty for the project.

Panek

The design of a tunnel lining is greatly dependent on the assumptions that one is forced to use, both with respect to the characteristics of some proposed liner, and with respect to the characteristics of the rock that one envisions to have supported. Much of the confusion with liner design is brought on by designers having a preconceived idea of what kind of a liner they are going to use. The load that is brought on the liner in a large measure is caused by the liner itself, not just the rock. So we made an attempt to find out, free of any a priori choice of liner design, what one might be able to do in the way of innovative tunnel support by simply asking the question, what kind of distribution of pressure against the wall is required to prevent the most simple kind of failure? This situation we consider to be well represented by the triaxial test to determine Mohr's envelope.

SESSION 3 - GENERAL DISCUSSION

Question by Gerdeen (for Barton, Lien and Lunde)

The authors are to be congratulated on a very interesting and potentially useful paper.

Our work has been mainly with roof bolting and so my question deals with this method of support. Would you explain the difference between categories 10 and 11 in Table 8 where the quality indices for block size are the same, but where different types of bolting are recommended? It seems to me, you favor tensioned mechanical bolts over untensioned grouted bolts for larger spans. The question is why? Are there not cases where tensioning may cause additional damage? Does your analysis apply as well to flat horizontal roofs or only to arches?

Reply by Barton

The two support categories referred to in Mr. Gerdeen's comments were categories 10 and 11, and you can see from Fig. 3 that between these two categories there is no change in the rock quality. The rock quality ranges from 40-100 in each case. That's pretty good rock. Some people would say it's the best rock you ever get, but that's not our Scandinavian experience. So we've just got a difference in span and/or in the use of the excavation. You can say the EST (excavation support ratio) which describes the type of excavation makes the effective span for category 10 less than that for category 11.

I am in full agreement with your point on the application of tension bolts as opposed to grouted bolts. If this paper were expressing purely the opinion of we three authors in NGI, we would have, I think, in most cases have recommended just grouted bolts. We have analysed approximately 200 case records, or 200 usable case records, from which it appears that the general practice of some years ago, 5 years ago let's say for an average construction completion, was the use of tensioned bolts in these

larger excavations. I think that when the rock quality is good, this is a very questionable practice, but when the rock quality is very poor, and really requires an active support pressure, then one may obviously favor tensioned bolts. However, I have some reservations on this and I'd like to refer to the Roman arch in the following figure. It is not the usual type of rock mechanics we deal with but it is quite interesting.

This is part of a famous Roman aqueduct in Spain. There are something like four levels going right across the valley, if I remember correctly. Now, let's suppose this were a continuous rock mass. If we consider the different effects of grouted untensioned bolts, and tensioned bolts in this arch, it's very easy to see that the tensioned bolt is going to destroy the arching effect. When you apply a tensile force in good rock, I believe that even though you are applying a support pressure, of course, you are also reducing the arching effect, or the ability of the rock to support itself. In very poor rock I presume you've got no option but I would welcome further discussion on this point.

Your last question, I think, was whether our method is applicable to flat roofs as well as curved ones. Well, we have very few case records from mining projects. I wish we had more, and I would appeal to those of you in a position to provide them to send us details of these flat-roof cases since nearly all our records are of classical arch roofs-typical civil engineering water tunnels, hydropower caverns, etc.; nearly all our roofs are curved in some manner or other. Consequently, I'm not in a good position to answer that question. I expect that we would have to modify the system somewhat for flat roofs.

Discussion by Bieniawski

I would like to comment on the necessity of monitoring during tunnel construction while using rock mass classification systems. To start with, I think we should note that there are a number of rock mass classifications in use today and it may be interesting to compare some of these with that of Barton. In South Africa we use a method known as geomechanics classification. I have taken all the available case studies that we have, together with some of Barton's case studies, and compared our ratings with those of his. The conclusion from this is that there obviously is some sort of relationship between the two. Having obtained a description of rock mass quality the next thing, of course, is the specification of support as well as excavation sequence. Now, how do we do it? Using, for instance, Barton's classification method to determine primary support in South Africa, I would find that (by dividing his q value by a factor of say 2-1/2 to 5) most of my tunnels would be left unsupported; and having tried this, I find that it would not be acceptable. The question is: What do we do in such a case if we want to apply a classification, method, or check one classification method against another? The answer brings us to the first importance of in situ measurements -we must monitor the actual behaviour of the tunnels.

Another rock mass classification,developed by Lauffer, is based on stand up time. Now I've extended this by adding our case studies in South Africa to Lauffer's original case studies in Austria. Then I have compared this with the limits that would be found using Barton's classification. His method would indicate that, even in poor rock, one could have free spans up to 30 meters, which certainly in South Africa we would not find acceptable.

The answer to the questions raised by these comparisons is to monitor the behavior in an actual tunnel where the rock conditions are classified and the tunnel advance and support are recorded.

Very simple monitoring measures, such as convergence stations, can offer an excellent opportunity for cross checking classifications. Now I believe very firmly that, while any rock mass classification system can be very useful, it can become irresponsible if it is not backed by a system of monitoring. So we would say use classification together with monitoring.

A simple monitoring test such as I have suggested, and those shown by Mr. Pacher, can very easily be used to establish whether a classification method can be trusted—whether the behavior of the tunnel conforms to the prediction given by the classification method. So to conclude I would like to ask Dr. Barton the following: What are his views on the use of monitoring in combination with rock classification? Is such a procedure practiced in Norway, and if it is, what methods of monitoring are used? In view of what Mr. Pacher has said concerning the length of extensometers, what length of extensometers should be used in tunnels and caverns? Finally, is it necessary to use what Mr. Pacher referred to as the 'closed ring', and is there any experience on this subject in Norway?

Reply by Barton

First, just a comment on the comparison between our two rock quality determinations. I am glad to see that you have now extended the range for unsupported spans to 20 meters instead of the previous 8 meters. We are certainly coming closer to agreement. I think that I interpreted the characteristics of your very best rock as well as I could and converted the results to my rock quality, Q, arriving at a figure somewhere between 50 and 100. I wonder if you have any different ideas on that. We have a type of rock in Norway and Sweden which obviously may be out of South African experience, and perhaps the same is true in Canada.

We are very much in favor of deformation measurements using extensometers. I think people from Sweden are in a better position to answer questions on monitoring tunnels than I am. I don't think the amount of money needed for monitoring programs is usually available in Norway, although we do have some big monitoring schemes going ahead on some of our very large hydroelectric power stations and pump storage stations. I am not very knowledgeable about the details of these monitoring programs. It may be that Janelid could provide some information on this.

Comment by Janelid

For our projects in Norway we haven't done much monitoring. We have simple convergence measurements in tunnels, just to check that the tunnel is stable. However, we are contemplating more elaborate monitoring in some very large excavation. As to the length of the extensometers, we would usually extend out to 1-1/2 diameters of the opening, something of that order. In addition to extensometers we are also installing stress measurement devices and measuring pressures and temperature.

Since I have the floor, I would like to make a comment on design. I feel a very great agreement among us that in tunneling and other underground excavations you have to use a design-as-you-go approach, an observational method. Now this is often very difficult because of the requirements of the authorities who receive the plans and authorize the execution, of the work. I think it would be helpful if this method could be made the official position of the people involved, and if we could remove the expectation of a clear-cut safety factor given in advance. Everyone will have to accept that safety factors are derived during the execution of the work, and that at the end of the work, when all movements have finally stopped, something can then be said about the safety of the final structure. We need to have it understood that we cannot provide a complete design and say this is exactly what should be done, it will cost this much, and we have a safety factor that high. I feel that this should made clear to the authorities to reduce their demands for complete design plans. If, during excavation, measurement and analysis of the physical conditions encountered call for changes to optimize the plan, say, a flat roof for a more curved one, or a very high one, the decision should be made then depending on cost of support versus excavation to obtain both technical and economic optimization. If you are going to explore all possibilities in advance of excavation, then you will have to spend more on exploration than you spend for the excavation and support work.

Comment by Bergman

I have been exposed to the use of your method for design of underground openings and, based on my experience, I would like to comment on certain points in the paper. First, however, I would like to state my great interest in the approach taken by the authors to the problem of support estimation. As I see it, you have taken into consideration the main influencing parameters.

I would suggest that the method be utilized as a rough guideline, or perhaps as a "philosophy" especially when working with it in the preconstruction phase of a project. I think the authors make this point, frequently in the paper, but I would like a more powerful warning against the misuse of the method, that is, its use without restriction. I would make the following points:

1. There are obviously difficulties in determining accurately some of the six parameters especially joint set number, alteration factor, and roughness factor, from drill hole data. You certainly will have subjective judgments, which means that different people could arrive at different Q-values for the same situation, perhaps by as much as 100 percent, depending on the experience of the people using the method.

2. As mentioned by the authors, the method of blasting and the resulting damage to the rock has an important effect on the support requirements and this cannot be overstressed in my opinion.

3. The fact that an opening is stable after completion does not prove that the right level of support, or the optimal margin of safety, has been chosen. Many, or perhaps most, underground openings are overdesigned in this respect. Many of the case examples used in the paper were completed more than five years ago. Since then methods of support have improved. For example, shotcreting technique has advanced and reduced the use of systematic bolting (not spotbolting). Consequently, if some of these excavations were constructed today, they most probably would be constructed with less support.

4. The ESR (Excavation Support Ratio) determination should be discussed. I would propose, for example, that the ESR for storage caverns, where people do not have access after completion, could be increased. For such caverns, by experience, I know that the temporary support level normally will be quite close to the permanent support level. This leads to the proposal that, instead of varying ESR, you could let ESR = 1 for all types of underground excavations and instead let the relation between temporary support and permanent support vary with the purpose of the excavation. The required level of temporary support should not differ according to the use to be made of the excavation.

Question by Cikanek (for Barton, Lien and Lunde)

Dr. Barton makes reference several times in his paper to an internal publication by NGI, and it would appear that this publication has quite a valuable summary of information in it. Such information I would like to receive, and I am sure many people here would too. My question is, is it possible to receive a copy of this publication?

Reply by Barton

First, to answer some of the points brought up by Bergman in his very excellent comments, I agree and would like to emphasize the fact that our classification is a rough guideline which should not be used as a cookbook. Engineering and geological common sense is required. The question often arises as to how much of the information for these six classification parameters could be obtained from core analysis alone, if surface mapping was difficult due to cover, for example. I think that you must use your geologist as much as possible to obtain a really good picture of the tectonic history of the area and what different rock types are present in order to get a better background for interpreting what you see in the core actually obtained.

I will quickly go through the six parameters. RQD you can obviously get from the core. Of course, it is important to have a high skill in the core drilling operation. The number of joint sets would be rather difficult to determine accurately with one core hole. If you can orient all the core pieces relative to each other, you could perhaps count two intersection joint sets but you are probably going to miss one or two sets. You may need at least a couple of boreholes drilled at different orientations for the number of joint sets. Joint roughness is difficult because you only see a few inches of roughness. This is the small scale roughness which is sheared off under normal engineering stress levels. You will not see the waviness so you would get a rather conservative picture, which is on the safe side. Here again you must use the available geology. Are these foliation joints? Are they sheeting joints? Are they bedding joints? What have we seen before for this type of joint? Should I take a little higher roughness than appears in the core to account for waviness? Joint alteration is also difficult because the skill used in the drilling determines the amount of alteration and filling materials recovered. I really don't know what one can do about this; whether there's any possibility of analyzing the flushing fluid. You should try to use the best possible equipment - double barrel or triple barrel coring. But there are certainly going to be problems in this interpretation. When in doubt, go to the conservative side.

The water measurement can be made quite easily from the borehole, I would say. You can, of course, measure the outflow of water, or you can conduct pump-down tests. The final parameter relates to stress, which has several aspects. We would hope the geologist could help us with this. If we got very poor core recovery there may be many faults and we may have to consider loosening loads. Now the tectonic history of the area, and perhaps, information from other excavations in the region can help determine the existence of high rock stresses. We should make loading tests along the core to estimate the strength. Compression tests can be used but point load tests are preferable because you can go straight along the core classifying strength as you analyze it. Squeezing and swelling problems may show up if core recovery is down to a small percentage. This is an indication of big problems anyway.

Dr. Bergman is correct that most of our case records were from excavations completed around 5 years ago (some of them considerably longer than that), so we are using quite a conservative system. It is interesting that Bergman would like to make our classification less conservative, while Bieniawski would like to make it more conservative. Norway seems to be somewhere in the middle now, which is a good position. Whenever I discuss this method of classification, I ask people to send me information on projects with which they're familiar, that might extend our case records. We will undertake, to re-evaluate the whole system when we get enough records, say another 100 records. If anyone else wants to do that themselves they are most welcome, but it will be helpful if the data are sent to NGI for processing. In perhaps one or two year's time the whole system will have to be re-evaluated. We are learning more all the time. Blasting techniques are getting better, as are other excavation methods, as well as support methods.

As to Bergman's final comment in which he suggests I use ESR = 1 throughout all excavation cases, I can only remark that reanalyzing all those case records is a lot of work and I think I will give him the job.

Mr. Cikanek asked if the original NGI report was available. Well, it is a little embarassing, but I think I must mention costs now since we are getting into a little bit of trouble with our accounting department. I have sent out 60 of these reports. Each one has to be reproduced from the original and in Norway copying is expensive, and there are some color photographs. If people don't mind being charged about 12 or 13 dollars apiece plus postage, they are welcome to have a copy of the report.

Question by Cundall (for Barton, Lien and Lunde)

My question is not so much about your splendid classification system but its implications for numerical methods. Your method in principal enables the support requirements for any opening in any rock to be estimated. What scope is there for calculations? What in your judgment is the optimum direction, if any, for numerical development to proceed? What sort of calculations would you like to see done to supplement your classification, or should we just give up playing with computers?

Reply by Barton

I may have a slightly jaundiced eye toward finite element methods; I must confess that. I don't know if it is justified. It is probably our fault; those of us who are trying to convert geology into numbers and data that the numerical method people can use are falling behind. We're not able

to give them enough data. Personally, I think even if we were able to give them enough data, they would still have problems. However, any criticism you make of the finite element method becomes out of date after a few years. The techniques are advancing very fast. All these clever people doing finite element analysis and interactive graphics should obviously continue. One problem that arises is that these methods are so sophisticated that the people working with them, in my opinion, are not going to have too much time to investigate rock mass properties. Consequently, those of us investigating the rock mass properties should do our very utmost to provide data that we think can be used in the numerical studies. We need numerical studies. We need a lot of interface between the two areas.

I am very interested indeed in the computer graphics method because I think we can learn a lot in a very simple manner. I would encourage as many people as possible to follow this line. However, I do not mean to suggest that those people doing finite element should abandon their effort. I think they are going to come up with good results in the future, though it may take a number of years.

Comment by Lane

I would like to offer a few thoughts about the paper of Dr. Barton and his colleagues. I congratulate them on a monumental study of some 200 case histories. I think their classification system is a real help in trying to refine the RQD which, of course, is very simple, its principal advantage. I become a little concerned when you start applying the system to tunnel design. I don't think you intended this. It is really a summary of case histories to use for preliminary estimating. Nevertheless, people will tend to use things for more than they are intended and as Dr. Pacher noted there can be a lack of rationality there. I appreciate that when you match up or collate case histories, you use what you might call adjustment factors to try to achieve a fit. In American slang we call them "buggaration factors" or fudge factors. For example, the effect of water pressure (really a seepage force), the effect of geostress, the effect of opening size, the possibilities of overstressing around the opening causing a plastic zone - these things seem to be considerably underemphasized and yet they can be very important. Your classification system may provide reasonable factors for early estimating and I encourage you to continue your efforts, but I want to inject a note of caution. Don't take these as the gospel and apply them to final design.

Let me give you an example of what can happen. In the field of soil mechanics and seepage through the foundation under a dam, some years ago an Englishman, Bligh, in India came up with a whole collection of fudge factors. They were called percolation coefficients. They were refined by a man named Lane, no relation of mine, in the United States. This was a forward step, but unfortunately, this approach was used as a manual for design for 10-20 years, until Dachler in Europe came up with the idea of the flow net, and Casagrande taught it to us here in the United States, and we got a rational method. But I think that for the problem of seepage, frankly, the use of these fudge factors set us back 20 years, and I don't want this to happen in the rational design of tunnel support which is so urgently needed.

Reply by Barton

I appreciate those comments of Mr. Lane, and I must say I agree with them basically. We are using at least six fudge factors, but I think they are pretty good fudge factors. If you have a better method to use, use it. I would really like to hear about it.

Question by Weakly (for Barton, Lien and Londe)

I believe that the observations reported in this paper refer to finished excavations already completed when the analysis was made. I would like to ask for a rough appraisal, an order of magnitude estimate, of the effect on stability and the reinforcement required because of damage from blasting, late installation of temporary support for a break, and similar factors. I think such an addition to Barton's determination of support would be extremely worthwhile.

Reply by Barton

The point you raise is very important. We have noticed a very big effect of blasting practice in the case of large caverns excavated on the west coast of Norway where the rock is extremely competent, with more or less one set of foliation joints. In a few places where the blasting was very crude, you can count three joint sets. In some of the caverns where the blasting was done with smooth wall technique it is almost impossible to see any joint sets whatever. I think that one should, where possible, classify the rock mass from exposures which are going to be excavated in the same manner as the planned excavation. If you can't do that, you are going to have to be very careful how many joint sets you choose, and similarly with the other parameters.

So blasting is going to have a very large effect. If smooth wall blasting is used, you should not see very much block fallout, you are going to count a smaller number of joint sets, and you will disturb everything less than if the blasting is very poorly carried out. And that is exactly what you should expect, because the rock that is damaged by bad blasting is going to be the big problem.

As to late placement of support, I don't think I am in a position to say much about that. Of course, our whole method is built around shotcrete and bolting, where one would hope that the contractor is experienced in using these methods and would not be placing the support late, because the whole essence of the method is that it can be placed rather rapidly after excavation.

Comment by Franklin

I have several diverse comments. The first is related to the comments by Dr. Lane and Dr. Cundall about design of tunnels. Can we design tunnels or can't we? I think for years we have kidded ourselves that we can design tunnels by some sort of precise method and we have regarded our own

judgement, our rules of thumb, as something immoral. I think that the tendency towards using more detailed rock descriptions and rock classifications to supplement our judgement is a move in the right direction. However, I personally feel that we are getting a little too complicated in these rock classifications. I would much prefer to see proper weight given to individual observations, with predetermined classification parameters, particularly important ones like block size and the strength of the rock, used for supplementing our visual observations.

Yesterday I think there was an inference that computer modeling is a very expensive sort of thing. Today Dr. Barton inferred that monitoring could be an expensive thing. I would like to suggest that both of these can be done quite economically. To moniter convergence, for example, instruments for a profile can be installed for the order of $200. Applying a computer analysis, using a developed program, can provide a lot of valuable data for a similar sort of price.

Finally, Dr. Bieniawski pointed out that classification approaches to design needed monitoring. I think that both analytical and classification approaches to design need monitoring, and a very important addition to that is that the support system should be adaptable. We ought to be prepared to act on our monitoring results and not have the construction contract written in such a rigid way that we can't adjust the plan if monitoring tells us we need to adjust it.

Question by Bello (for Bischoff and Smart)

A radial distribution of pressure on circular arches that would induce mainly a thrust in them is an exceptional case in tunnels; also rock arches are not able to take tension by flexion as steel arches are. Therefore, the applicability of your development seems to be very limited. What would be your recommendation to adjust the working hypothesis, in order to extend the applicability of the concept of generating a natural load-carrying arch in the rock mass around openings as the main support element?

Reply by Bischoff

First of all, I don't really think that the assumption that the arch is a purely thrust carrying structure is a bad assumption, because of the fact that the arch is quite a thick structure. We must remember that when the excavation is first made the rock is in a state of compression and the only tension that might develop would be that which is at the inner surface of the structure. Because of the fact that the structure is as thick as it is, I really don't feel, for the purposes of this particular procedure, that I would go beyond this simple assumption as to the structural characteristics of the arch.

Comment by Deere

I wish to comment on the paper by Bischoff and Smart, at least obliquely. I certainly agree with their method and we have been using similar considerations for some time. They compare rock bolt support and steel arch support, apparently for the purpose of recommending the replacement of steel support by rock bolts, or rock bolts and shotcrete. I certainly agree with their intent. Steel supports are used to a very great extent in the United States in tunneling and in underground caverns. Contractors use them for safety in the workings and for providing stability of the sidewalls and the face. They would usually prefer not to use steel supports because the rate of tunneling is slowed down from an average of about 12 or 15 meters per day to an average of perhaps 8 or 9 meters per day. However, when the rock quality reduces and pieces begin to fall from the roof while rock bolt holes are being drilled, the tunneling foreman starts reaching for the steel.

The point I wish to make here is that steel supports are really not a very good type of support from the point of view of keeping the rock from moving and loosening. They may be a good support system from the point of view of the contractor who can grab a piece, throw it up, and his men then are not afraid to work under the area. He can continue advancing the tunnel. However, the rock often suffers because of this. There are at least three cases of outright failure of steel supports in the United States, of very extensive proportions, in the last three or four years. In each case loosening loads developed progressively with time and failure took place one week or more after the steel had been installed and the tunneling face was 20 to 40 meters beyond the point of failure. Evidence of support deformation was seen in all three cases and remedial measures were being taken by the contractor by adding additional sets of steel, often called jump sets, to reduce the spacing by half. The sets were in the process of being installed when outright failure occurred. In all cases failure would not have occurred, in my opinion, if shotcrete had been used with the steel supports, or rock bolts with the steel supports, or very possibly with rock bolts and shotcrete alone.

In one of the cases, for instance, the tunnel encountered a foliation shear zone and shotcrete and rock bolts were used. The tunnel continued following the shear zone for several days and the contractor changed to steel supports. After a few days the steel started to deform and the contractor then added 1 or 2 centimeters of shotcrete. This was not sufficient and movement continued with the shotcrete being cracked. Pieces began falling onto the workmen who promptly decided they didn't like shotcrete. Meanwhile deformation continued. The steel supports were pushed down from 5 centimeters to 15 centimeters. At the top the steel joints began to open up. They were approaching the final failure condition in which joint bolts would break and the steel would twist and turn inside out. Hurriedly, steel columns were placed in the center of the tunnel and stability was temporarily achieved. Later 8 meter grouted rock bolts were placed, together with 20 centimeters of shotcrete, all between the steel sets. The columns were removed and it was evident that stability had been achieved.

Now my key point is that the failure in the steel supports went right down the tunnel progressively until the area supported by the rock

bolts was encountered. There stability was still excellent, even though the foliation shear zone with all of its characteristics continued right on through this area supported with the shotcrete and the bolts. I am very strongly in favor of shotcrete and rock bolts.

In the United States there has been a definite trend in the past 3-4 years to the use of polyester grouted steel rebars. However, these must be installed at the face immediately, before deformation has taken place. I think we will see a continued trend in the United States, with contractors slowly getting away from leaning on steel sets as the only thing they have available to give them safety.

Question by Costa Nunes (for Einstein and Bischoff)

I wish to ask some information of Professor Einstein on his very important paper. We are using prestressed bolts to stabilize rock and soils when encountering swelling pressures. I wish to know what sort of pressures are measured when deformation occurs, because from our experience the swelling pressures depend very much on the deformation. We find the best results are obtained by very quickly placing the anchors to avoid the beginning of the deformation. A second question concerns the counterstress slots that were suggested by Professor Einstein. I believe these counterstress slots must be reinforced and possibly anchored.

Reply by Einstein

Concerning the installation of bolts, anchors, or rebars, we have shown in the paper that they can be used but they have to be installed to sufficient depth to get past the main swelling zone. Otherwise they do not serve any purpose. In some cases, for instance, it would have been cheaper to use an invert arch rather than these longer bolts, but that depends on the local economic situation. Concerning the need for installation immediately after making the opening - if you let swelling go on for awhile, then the swell pressure that develops later on will be less. Thus, if you install the bolts or invert arch right away, you definitely reduce the swell deformation, but you increase the stresses either on the invert arch or your bolts. You have to figure out what you want to accomplish. If deformations are detrimental, then you have to install the support right away. If you can afford to let it go awhile and then install the support, that's fine, you reduce the stresses. The use of slots is an idea I haven't tried in practice. I cannot tell if they are superior with reinforcement or not.

Comment by Grob

I should like to make a comment on Professor Einstein's paper on swelling. It has to do with our "child of sorrow", I should translate it, the Belchen tunnel. It also relates to the question of allowing swelling to develop in what I think is a rather vain hope of then reducing the final pressures. In this case we had upheaving of the order of something over 1 meter. Finally, the invert arch had to be placed and the lining had to be completed. We could not allow any more deformation, except for the compressibility of the lining which of course gives in a bit. Since completion, that is now over 5 years ago, the tunnel has been under observation. Deformation measurements have been made and stress measurements in the concrete have been made with overcoring. About three years ago in some places we found stresses of 130 bars, and recently we have repeated the measurements and we have found 200 bars, which indicates that the building up of pressure is proceding nearly linearly with time. We will see in 4 or 5 years more what happens to that tunnel as it comes more and more under pressure.

Question by Bieniawski (for Haimson)

This relates to Dr. Haimson's paper concerning the importance of in-situ measurements in the design of underground excavations. I would like to make the point that while very few would dispute the importance of in-situ stress measurements in the design of underground power stations, the paper, in my opinion, gives the unfortunate impression that the importance of rock mass properties is placed at a lower level. For instance, the paper just mentions rock material properties on two occasions. I am very worried about this because if you conduct finite element analysis, or any stability analysis, and have the in-situ stress field but not the rock mass properties, the output from the analysis method can depict a completely wrong picture. My main question is this: not withstanding the importance of in-situ stress measurements, of what importance are the rock mass properties? I also have a little question, in fine print: what sort of budget have you got?

Reply by Haimson

I accept your criticism. The point was not to belittle the importance of rock properties, just to highlight the importance of stress measurement prior to excavation. So the paper in this sense may be misleading. It is not an attempt to cover all the different stages of pre-excavation design. It was an attempt to highlight this one particular part of the entire investigation, which is stress measurements prior to excavation of the pilot tunnel or of the entire underground chamber in general. As far as the budget: I am not in a position to disclose the budget for the entire site investigation simply because I don't know it. All I know is the amount spent on the stress measurements, if you're interested I can give you that information.

As far as the properties that should be measured, this varies from one project to the other. You can run all kinds of tests in a pilot tunnel if you are going to have one, or you can use laboratory tests. In the latter case, there is always the question mark as to their usefulness in the field situation. It certainly was not my intention to imply that no field measurements of rock mass properties should be conducted. In the particular case history that I mentioned, the one in California, no pilot tunnel was planned because of other technical problems. The rock there is incredibly good. It is granite with very little jointing, very little disturbance at all, geologically speaking. I was not directly involved, but other people instigated a series of laboratory

tests and geological investigations to provide the data needed for a finite element analysis.

Question by Gyimesi (for Haimson)

The design of underground powerhouses is closely related to the design of pressure tunnels. It would be desirable to extend the presentation to include the most recent design methods with special attention to calculating the minimum rock cover. I have two questions: (1) How does the presence of deep overburden (200-300 ft.) influence the calculation of minimum rock cover over a pressure tunnel? (2) Could you refer to reference material covering these design problems?

Reply by Haimson

These questions may not relate directly to my paper, but I understand that pressure tunnels are not unlike some of the tunnels in underground powerhouses. With regard to the calculation of minimum rock cover, I may not understand your question precisely, but determination of the stress conditions would not be different from the way we described in the paper. Afterwards, depending upon the pressure that you are designing for in the tunnel, you can decide the depth and the direction that would be optimum for your tunnel, considering the stress conditions and other relevant factors.

With regard to the second question, I am not sure what references you are interested in, but a lot of papers related to pressure tunnels have been published, particularly in Norway. I believe there was a conference that covered the subject recently in Norway, and another in 1971 in Arizona.

Question by Ladanyi (for Pacher)

In your pressure determination plot you have shown only one characteristic line for the rock mass. In my opinion, there should be at least two such lines - one for the short term case corresponding to the state of rock at the moment of tunnel driving, and another one for the long term case, which eventually determines the ultimate rock pressure on the lining. If your line corresponds to the second case, how do you determine the strength and other properties of the rock mass? Do you ever use such lines or such a method for determining the rock pressure?

Reply by Pacher

It depends whether you have a task where you have to consider short-time or long-time influences. The latter is what Eggers and Daemon and Ladanyi have done. They have worked out this problem in detail. The idea of our method is to handle the pressure in the short-time case - while excavating, to reduce the original pressure of the rock in increments so as not to let its strength be destroyed.

Discussion by Rissler (resubmitted at later date)

I would like to make a short contribution which refers to the application of large prestressed anchors in underground work. It is concerned with some experience Prof. Wittke from the Technical University of Aschen, Germany, and myself gained during investigations for the underground powerhouse of the pumped storage scheme WEHR in the southern Black Forest, Germany.

This cavern is about 220 m long, 20 m wide and 35 m high and its situated in gneiss with an overburden of about 350 m (Fig. 1). Geological investigations showed that the area of the cavern was intersected more or less randomly by distributed joints of 5 families, some large masterjoints and one fault (Fig. 2). One of the masterjoints strikes nearly parallel to the axis of the cavern and intersects the upstream wall over a length of about 50 m and over the whole height of the powerhouse, forming with this wall a 35 m high rock wedge the apex being located near the bottom of the cavern. In some parts this masterjoint, called "Paralleljoint", was some 50 cm open and in other parts it was filled with silty coatings.

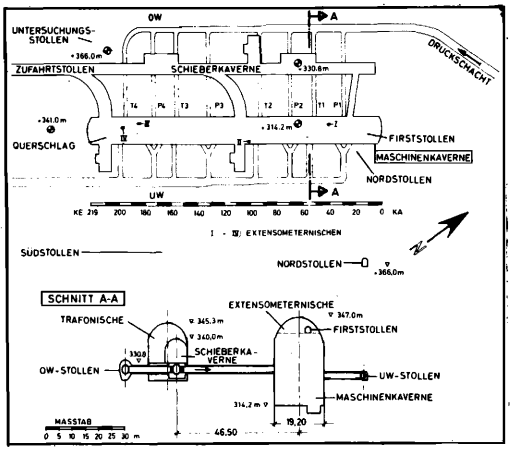

Figure 1 Underground powerhouse

I only want to draw your attention to this problem. After we had performed a lot of laboratory and in-situ tests we made three-dimensional FE-calculations for the special cross-section shown in Fig. 3 and also for other cross-sections using a linear elastic, linear plastic stress-strain behaviour for the rock masses and also taking into account the weakening influence of the homorandomly distributed joints. The "paralleljoint" was considered to have no cohesion and a low angle of friction.

The computations, performed for several steps of excavation showed very interesting results. After the excavation of the upper parts we found a symmetrical state of stress, only small vertical

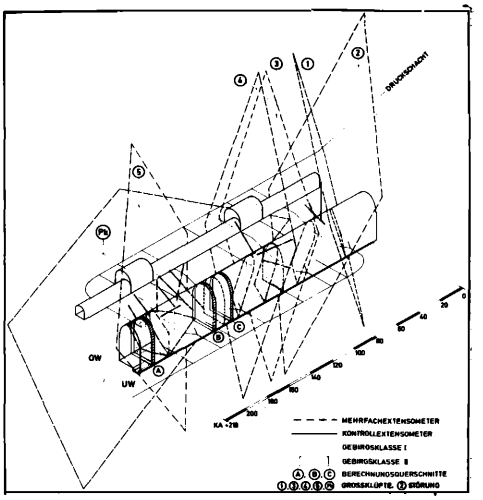

Figure 2 Fault and master joints.

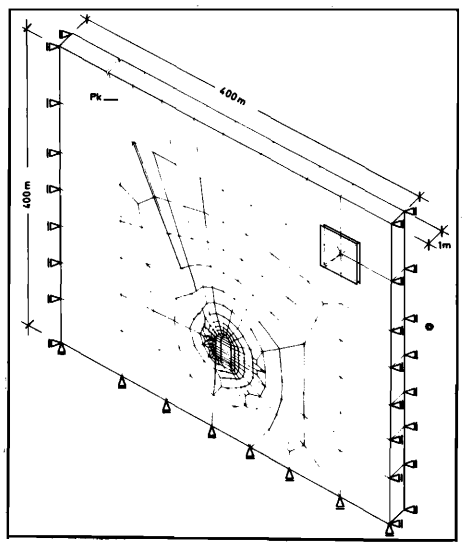

Figure 3 Finite element mesh B (see fig. 2).

displacements of the roof and stability of the cavern. But at a certain step of excavation, when the foot of the rock wedge nearly had been reached, the wedge moved along the "paralleljoint" into the cavern. Furthermore the results showed shear failures along the joints within the wedge and a shifting of the vertical stresses from the wedge to the downstream parts of the rock mass. The most important result however was, that the displacements of the wedge, increased during an iteration process. So it must have been expected that without any support there would be instability at this cross-section.

Further calculations showed however that stability of the cavern should be achieved by an arrangement of prestressed anchors (Fig. 4) and that the displacements of the wedge could not be prevented but limited to about 10 mm.

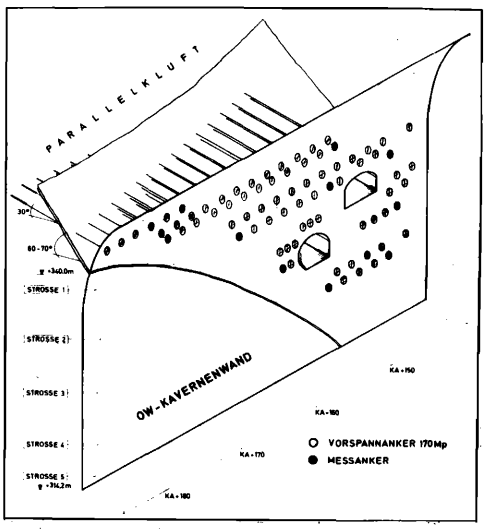

Figure 4 Prestressed anchors.

Now I would like to tell you what happened at the site. Before the excavation extensometers had been installed. They showed that the measured displacements even in this very complex zone correspond well to the predicted ones (Fig. 5 and 6).

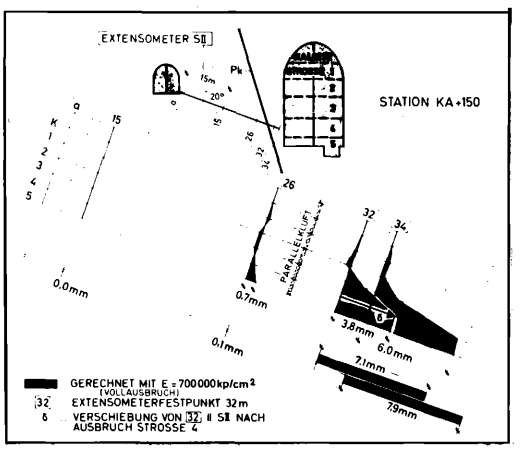

Figure 5 Results of extensometers S II.

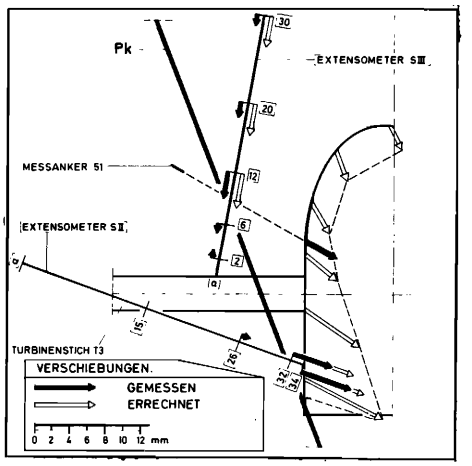

Figure 6 Measured and calculated displacements.

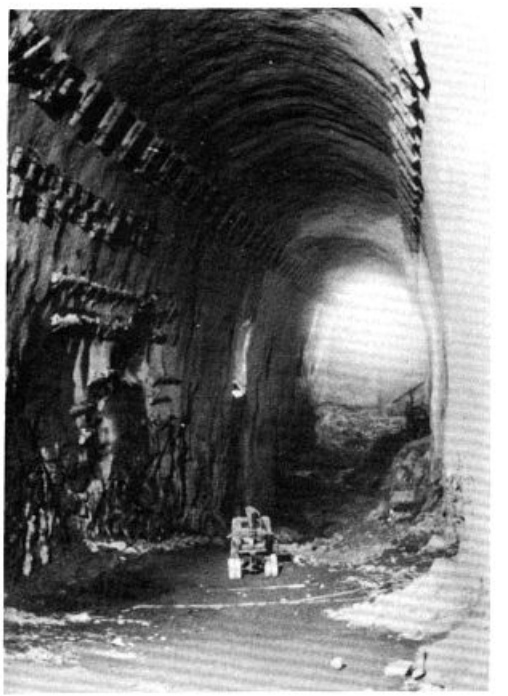

Figure 8 Underground powerhouse in Spring 1973.

It is most interesting, however, that the measurements enabled us to find out when the shifting of stresses occurred. It happened during the excavation of some areas near the upstream wall characterized by a remarkable increase of displacements in several extensometers (Fig. 7).

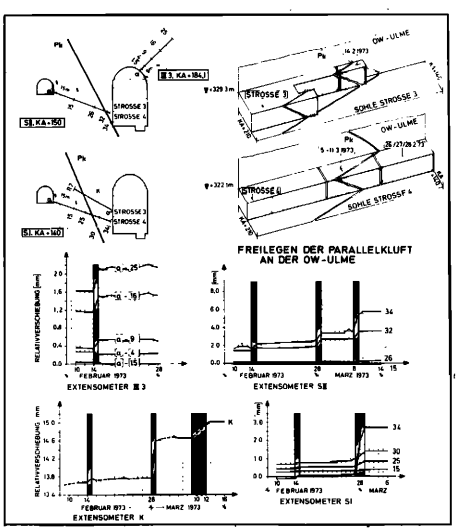

Figure 7 Progress of stress transformation.