

Nick Barton's interview

Skopje, September 14, 2026



Nick Barton



Ivan Vrkljan¹

Introduction

I met with Dr Barton in Sarajevo on May 28, where he delivered two lectures to the members of the Association of Geotechnical Engineers in Bosnia and Herzegovina. It was then that we agreed to conduct this interview during the ISRM Regional Symposium EUROCK 2026 in Skopje. For this symposium, Dr Barton prepared a keynote lecture titled: NMT-NATM hybrid method for sustainability and risk reduction. Through his participation in conferences organized by the geotechnical societies of the former Yugoslav countries, Dr Barton has strongly supported their efforts to actively contribute to the development of geotechnics both regionally and internationally.

He has held workshops and lectures on multiple occasions in Croatia, Bosnia and Herzegovina, Slovenia, and North Macedonia, forging enduring friendships that continue to this day. Given the immense contribution that Dr Barton has made, and continues to make, to rock mechanics and geotechnical engineering, every interview with him presents a valuable opportunity to better understand his entire career, as well as current trends in these disciplines.

¹ Ivan Vrkljan is a Professor Emeritus at the University of Rijeka, Croatia. He served as the ISRM Vice-President for the period 2011–2015 and is an ISRM Fellow.

Brief information about Nicholas R. BARTON

Dr Barton is perhaps best known for his development of the Q-system in early years (1974) when he worked in NGI. He had moved to Norway for one year after his PhD from Imperial College in 1971. This first year in Norway has stretched to 55 years (as of 2026) but minus four years in TerraTek in the USA during the first half of the 1980's where he became engaged in nuclear waste and defence project research. In the same early period in Norway, he developed the rock joint shear strength parameters JRC and JCS in 1973, which together with Bandis's PhD developments are now an integral part of the non-linear Barton-Bandis shear strength criterion for rock joint modelling.

This was fully described in 1982 and became part of Cundall's UDEC as UDEC-BB in 1985. As well as scale effects it incorporates hydraulic and mean aperture tracking, so with obvious 2D limitations can be used for M-H coupled analyses. During early years Barton also developed a simple shear strength criterion for intact rock in 1976 extending to the ultimate critical state and showing stronger and more correct non-linearity than presently cited models. In 1992 Barton gathered a group of several Norwegian authors from several companies including his NGI Q case-record colleague Grimstad and detailed single-shell NMT as an economic alternative to NATM.

In the 1990's Barton gradually became involved in TBM projects and published his first book: TBM Tunnelling in Jointed and Faulted Rock in 2000. Here he developed the prognosis for AR – the actual advance rate – often much different to PR due to delaying fault zones and water causing deceleration. He was the first to quantify TBM deceleration in relation to declining Q-values. In 2000 he left NGI and established his one-man consultancy Nick Barton & Associates. Some years later he published a major text book on Rock Quality, Seismic Velocity, Attenuation and Anisotropy in 2006 following an adjunct professor position in São Paulo's USP.

This explored links between geophysics and rock properties at all scales – from microcracks to 750km depth subduction zones. His work as a private consultant as NB&A led to an acceleration in foreign projects now amounting to several hundred in a present total of 43 countries. He has made good use of his practical experiences in more than 360 publications and some 50 international keynote lectures given in many countries. During the last two decades his cooperation with Neil Bar resulted in the publication of Q-slope in 2015 and its 10 year retrospective in 2024.

His cooperation with Baotang Shen, formerly CSIRO, Brisbane and the developer of FRACOD has resulted in Barton's simple formula for cliff and mountain wall height limits which involves tensile strength and Poisson's ratio, as also applicable in deep tunnels. The much used '0.4 x UCS' failure initiation tangential stress is more correctly written as tensile strength divided by Poisson's ratio. Weakest-link extension fracturing comes first. His regular attendance at ISRM congresses and regional symposiums, his participation in publishing ISRM Suggested Methods, and his involvement in other ISRM activities were honoured with the ISRM Fellow award.

The Müller Award, as the highest ISRM recognition, was conferred upon him in 2011 in recognition of distinguished contributions to the profession of rock mechanics and rock engineering. He also received 12 other international awards. In most recent years Dr Barton has been honoured as a 'distinguished professor' at Ningbo and Shaoxing Universities in China, and is taking part in ground-breaking research on shear strength scale effects in these Key Laboratories. He is currently an adjunct professor at the University of Skopje, having previously held the same position at Luleå University of Technology in Sweden and the University of São Paulo (USP) in Brazil.

Concerning publications Barton has more than 42,000 citations in Google Scholar with seven of his papers with more than 1,000 citations and more than 6,000 for the first Q-publication from 1974. Most of his publications can be downloaded from his web-site

Interview

Q1: You hold a B.Sc. degree from King's College, University of London. After that, without completing an M.Sc., you started working on your PhD at the Royal School of Mines on the topic of rock slope stability and the shear strength of fractures. Why did you skip the master's degree program, and was it common at the time for a bachelor's graduate to directly pursue a PhD? Who was your supervisor/mentor, and what was the research environment like where you completed your doctoral studies?

I joined a research project that had just started in the Royal School of Mines in 1966, and we could obtain a PhD after 3 or 4 years work with no MSc requirement. But I do recall a written exam after one year. Three of us (with Peter Cundall and John Sharp) joined the open pit stability project following civil (Barton and Sharp) and electrical engineering (Cundall) BSc. Our mentor who introduced all of us to the importance of rock joints was RTZ's David Pentz, the most enthusiastic mentor one could ever hope for. He was already responsible for the 100 tons direct shear machine in the RSM basement for testing large-diameter core from the Rio Tinto mines in Spain.

It was here at the Atalaya pit that we spent many summer research weeks (1967-69) and learned to order 'cervéz y tapas por favor' as our first Spanish words. We tackled shear strength and stability (Barton), permeability (Sharp) and dynamics (Cundall), with the brilliant Cundall dreaming up μ DEC before he was finished in 1970. The research environment was superb and the arrival of Evert Hoek as our professor one year into our research ensured an international environment, which included visits of prominent French (Louis, Joanna), German (Kutter) and USA (Goodman) long-term and short term visitors to the growing rock slope stability project. The Hoek-Franklin triaxial cell and the Hoek portable shear box took form while we were there. And the talented academic John Bray also had an influence on our work.

Q2: What circumstances led you from London to the NGI in Norway, where you spent at least the first half of your professional career?

In a highly illustrated autobiography I am working on, several pages are devoted to this, as a series of chance circumstances and people of influence are actually responsible for this move. The circumstances start when I was about 6 years old with an antique dealer father, who together with me slowly towed a trap (without the horse) behind our car to a country estate. My father had purchased this antique trap but immediately accepted a higher offer (this was Step #1 of my destiny to get into rock mechanics!) Our subsequent friendship with 'the Richards' of Abergwynant even included weekly baths there since no electricity when we later moved to our 300 years old stone cottage in Wales.

I was also fortunate that they had a private hydroelectric project that was shown to me several times by the famous train model-maker Jim Richards, one of the charming brothers. A subsequent important 'academic' step (not only downwards) was not being successful in entering Oxford University (for physics?) since forgetting in the college interview to mention running fast: forgetful here because I was unable to give a good reason for having both a B_b and an A clarinet. Finally, our friends the Richard's later need of income meant renting out holiday flats on their estate.

The key person of influence for me above all others was a subsequently holidaying professor of soil mechanics who we met during our invite to a musical evening at our good friends. After some Mozart with my Mum on the piano: 'why don't you apply to King's College, London but read my book on civil engineering first'? The kind Prof. Kevin Nash behind this invite later suggested the Rock Slope project at Imperial in 1966 (instead of a career in soil mechanics at Binnies or Motts ?) and later even introduced me to his ISSMFE president Dr

Laurits Bjerrum, the founding director of NGI. 'We have a lot of rock in Norway – why don't you come for a year when you've finished your PhD'?

The one year in 1971-2 stretched to 2000 while at NGI, and I am still in Norway in 2026. Happily for me but not for Bjerrum I did not become an NGI FEM modeller with my supposedly 'impressive' Imperial College credentials. I chose to continue with physical models: now a large vertical Hele-Shaw parallel-plate model to study air leakage from compressed air surge chambers which were being used in some of Norway's hydropower projects at the time. JRC and JCS and Q that soon followed in 1973 and 1974 were 'easy' to develop by comparison to understanding compressed air leakage through water-filled rock joints. But this was a helpful 'qualification' for meeting Eda Quadros my hydraulically focussed wife of 23 years, twenty years later. Destiny at work there too?

Q3: In the interview you gave me in Zagreb back in 2011, you spoke about the current challenges in geotechnical engineering and your expectations for the future. Over the last 15 years, has anything emerged that you would particularly highlight?

During the last 15 years, in fact we could say 20 years, there has been too much of the 'easy' solution of isotropic continuum modelling. Most rock masses are bedded and/or jointed and perhaps faulted, and are often anisotropic ('Anisotropy is everywhere: to see, to measure, and to model' – was a joint article with my wife Eda Quadros in 2015, who I was sad to lose in 2023). We should have more people – engineers and researchers and university staff – active in using and teaching discontinuum modelling. International court cases have proved that continuum modelling with 'today's FEM methods' exaggerate so-called plastic zones. They also give users the impression that circular or spoon-shaped failure is relevant for slopes in jointed rock.

In fact unless the rock is very weak, perhaps less than 5 MPa, or is actually rockfill, the cohesive strength of rock is too high for shear through the intact bridges of the potential failure surfaces. Yet we see circular failures predicted even when the rock has UCS as high as 80MPa. So there must be something seriously wrong with the levels of understanding and methods used. One of the things that is likely to be incorrect goes to the heart of our subject. 'All' are assuming that linear Mohr-Coulomb or non-linear Hoek-Brown, with the traditional summation of the cohesive and frictional components (we presumably inherited this from soil mechanics?) is the correct description of the shear strength of rock masses.

In fact, as we are able to monitor the progression of potential failure, it suggests that the 'summation' is not correct. Research 25 years ago and since then has shown that cohesion weakening and friction hardening (CWFH) produces much more realistic results. A more recent suggestion by the interviewee: the CcSs criterion, has four potential progressive failure components that fully acknowledges that rock slopes of some size may fail progressively and not instantaneously as with the above 'summation' assumption.

Q4: You have worked on many projects all over the world; could you single out the one from which you learned the most?

The most dramatic project and one with important lessons was in Colombia where earlier work for the contractor at the adversely oriented caverns led to my later engagement when something went very seriously wrong with a large supplementary diversion tunnel. This was constructed for better control of the rate of a future rising reservoir, to guarantee time for dam completion. The regular single-shell Q-system support for the 14m span diversion tunnel had no invert treatment. The designer had also inexplicably drawn a sharp bend after the intake. I later learned about the 36km/hr or 10m/sec flow velocity that was expected and indeed exceeded in practice. The results were catastrophic.

First an erosion cone to the surface of 105m diameter just after the sharp bend, then further drama downstream with gradual hill-side degradation above the original but now drowned intakes, and final emergency flow *through the powerhouse and transformer hall*. A 5 years delay and a record several billion dollars claim were the result. The planned flow velocity was far beyond the 1.5 to 2.5 m/sec flows of regular hydropower projects which prevents bed-transport of fallen blocks or shotcrete rebound debris. We thus avoid 'rocks in the turbines' (a Canadian consultant's worry) – a rather important consideration in a country relying on hydropower.

Q5: Artificial intelligence has suddenly entered our lives. How will AI impact the development of rock mechanics and geotechnical engineering, and how will it affect educational processes at universities? Do we need to transform curricula so that students do not lose their core engineering instinct and understanding of physical laws? What is your message to young engineers who will inevitably turn to artificial intelligence?

I have some adverse and positive experiences of AI both from two years ago with ChatGTP and in the last months in 2026. They are perhaps worth relating. At first the experiences were all negative. I tested ChatGTP with a close to home: 'Please explain the BB criterion and the Q-system of Barton'. According to the ChatGTP of 2024, more than a year after its release, 'the BB criterion is used for characterizing rock masses and the Q-system is used for estimating the shear strength of rock joints'. I complained and 'was told' that 'we do not have access to articles in Research Gate'. Surprising. But it seems that this has changed and today the replies are not diametrically incorrect.

In fact ChatGPT today gives a very good answer explaining the differences. Most recently in early 2026 I was contacted by 'Times CEO' magazine who enquired if I would be interested in having an AI interview and also have AI give my answers! (I was asked to send them my website). 'Curiosity killed the cat' as they say so I payed 290USD and soon after received the interview and 'my answers'. Some of 'my answers' were impressively detailed, much beyond my website – about the reasons for BB non-linearity compared to Mohr-Coulomb convention, with dilation differences at low and high stress due to joint roughness damage. But I had to correct serious mistakes concerning TBM tunnelling prognosis.

So a message to young engineers is to make sure they are reading widely as AI may not be understanding enough of our complex medium – and its reaction to excavation – tunnels or slopes – and besides this there are a lot of mistakes being made in our 'geo-field' modelling so no wonder AI sometimes gets it wrong too. Furthermore we do not publish even a small fraction of what we experience on projects as engineers or geologists so how could AI possibly get really, really good in rock engineering – ever?

Concerning university or educational challenges of AI we perhaps need to go back to oral exams as then the teachers and professors at least know what students have understood and remembered, whether or not with private use of AI in their studies.

Q6: In 2025, NGI published the brochure Using the Q-System, Rock Mass Classification and Support Design. Why was a revision of the previous 2013 brochure published at this time?

Just so that it is clear, the two chief developers of the Q-system (Barton and Grimstad) left NGI a decade or two ago and had no influence on what a younger team at NGI were doing with their assumed stewardship of the Q-system back in 2014. I recall we were disappointed with their presumed Road Authority suggested truncation of the RRS rib reinforced shotcrete arches to focus on 10 to 12m spans. This is now improved. But one should not use passive lattice girders – they have no valid place in Q/NMT. There is a surprising recurring error in the

NGI 'handbook'. Q was not developed from 1971-1974 but during my six busiest months of 1973 following a tough question from our State Power Board.

There are some serious misconceptions in 'the handbook'. For instance, advice that the Q-system may have limitations in the case of 'rock masses with anisotropic properties (jointing, rock stress situation)'. This is a major misunderstanding as that quoted sentence describes most rock masses. One could ask why does J_n have values less than 9, and why is SRF often needed? Users must realize that Q is often an anisotropic property, because rock masses very often have that characteristic. (See Barton and Quadros, 2015). Surprisingly the original 1974 article continues to get almost daily citations and 50 to 70 reads per week in Research Gate.

Grimstad and I have a detailed 'Illustrated Guide' from 2014, and '50 years' retrospectives of Q and related methods (Q-slope and Qtbm) in 2024, also as an Indian keynote.

Q7: After the International Society for Rock Mechanics was founded in Salzburg in 1962, Prof. Leopold Müller and Dr Franz Pacher gave an interview to Radio Salzburg. The reporter asked: "Do we know the strength of rock?" Prof. Müller replied: "For rock (specimens) tested in the laboratory, yes. For a rock mass, no. This is what we need to determine. This is why we need an International Society for Rock Mechanics." With this, Müller defined the central question of rock mechanics: What is the strength of a rock mass? Today, 65 years later, what would you tell Müller—how close have we actually come to solving this problem?

An interesting question and partly answered under 'current challenges' a few questions back. Here you give me a serious challenge because of two factors. Regrettably we have lost Evert Hoek who many of us had the pleasure to know well, not least as one of his PhD students of long ago and from a subsequent consulting engagement with him in Taiwan. And very occasional keynote lecture crossing-of-paths and dinners (Croatia and Peru). As we should all follow 'de mortuis nihil nisi bene, de vivis nihil nisi verum' to a respectful degree, much care is needed here as Evert obviously contributed a lot during decades.

The second factor I need to explain to Müller is that too many people have been performing continuum modelling in the past two decades, in part due to the convenience of Rocscience software and the presumed relative ease of selecting GSI values and their application in Hoek-Brown equations for (originally) the H-B 1980 shear strength ' c ' and ' ϕ ' of intact rock that was later modified for rock masses in about 2002. I suspect that Müller would be surprised to hear of the continuum modelling and might immediately recall his extensive reporting of the very anisotropic Vaiont disaster that we all studied long ago, and many other of his projects (in jointed rock).

He would also likely recall his 1966 opinion: 'cohesion is broken at small strain, while friction is mobilized at larger strain and remains to the end of the shear deformation' (here translated). He might also ask why we are still adding ' c ' and ' ϕ ' components. I would mention that some have indeed used CWFH with success, and I might pluck up courage and tell him of my plans for CcSs (crack, crunch, scrape, swoosh) a sound-bite for the actual progressive failure process of a large rock slide that can be monitored. (So the addition of ' c ' and ' ϕ ' components cannot obviously apply on a large scale). I suspect he would be positive to this though would point out the remaining difficulty: what percentage of each component?

It is inevitable that he would be surprised to the core – but at first totally disbelieve – that a method so widely marketed by Rocscience and used by thousands has the key input parameter repeated 128 times. If introduced to the GSI diagram he might also wonder why clay-filled rough joints in weathered rock cannot be selected, nor clean slickensided joints without clay and weathering. I suspect we would also discuss why an equation for the assumed shear strength of rock masses is just modified from a popular intact rock criterion (with actually

insufficient curvature at very high stress) rather than from the often non-linear shear strength of joints and a simplified representation of faults (perhaps using JRC, JCS and J_r/J_a as in the case of CcSs).

Inevitably I would mention the 'circular slope failure syndrome' – that so many are publishing because they believe in numerical models with circular slope failure modes despite joint sets and maybe faults and much too high UCS. I suspect Müller would agree that the cohesive strength of inevitable rock bridges cannot possibly be overcome unless almost with the strength of saprolite or very weak rock. So we would conclude that a lot of work is still needed, amongst other things in helping to demonstrate that designers do in fact need higher budgets to enable them to utilize more comprehensive models like UDEC-BB and 3DEC and FRACOD and other more recent dynamic code developments like the Russian Prorock.

Q8: This year, one of your major projects will be completed. In the middle of this year, you will publish a book co-authored with Stavros Bandis titled Engineering in Jointed and Faulted Rock, which spans around 800 pages. Was the idea for this book born during the lifetime of Stavros Bandis (who passed away in 2016)? Could you tell us a bit about the content of this book?

I will not dispute the 'major project' aspect of this project but some more chapters remain to be written and a finish in 2026 is too ambitious. Yes indeed about 16 of the planned 24 chapters were completed while Stavros was alive. Our mode of operation was to spend at least a week together in an undisturbed environment – two locations in Greece – one in a remote 'village' in the mountains of southern Greece or by the sea in Halkidiki in the north, also in my flat near Oslo, and in São Paulo at my wife Eda's house. Each time I was the one writing the most as Stavros was often over-worked with consulting projects for GeoEngineering where he was a superb modeller (for me also on occasion).

But he was very active in the planning of our book, in lengthy discussions and wrote much of some of the early chapters. Unlike many other textbooks on rock mechanics we have several chapters on rock joint properties and several on rock mass properties, tackling strength, deformability, permeability and characterization in each case. Then chapters on seismic properties, pre-injection, tunnels, caverns, TBM and rock slopes and some nuclear waste research. We once had 32 chapters but after his unexpected death in 2016 I reduced this to about 24 chapters. It remains my 'major project'.

Q9: Together with Dr Neil Bar, you first introduced the Q-Slope method at the ISRM regional symposium EUROCK 2015 in Salzburg. Subsequent revisions followed in 2017 (Rock Mechanics and Rock Engineering), 2018 (Periodica Polytechnica Civil Engineering), and 2024, (Rock Mechanics and Rock Engineering) when you discussed 10 years of applying this method, updated some of the charts, and addressed the time-dependent behavior of rock slopes. In your opinion, how has the professional community accepted this method? How do you see this method evolving in the future? Dr Neil Bar is also in Skopje, so this will be a good opportunity for direct communication regarding this issue.

First a brief history: I developed the method while working in São Paulo under a very big umbrella in Eda Quadros's back yard. I was on a part sabbatical at USP after leaving NGI. I tested it in the Dominican Republic in 2007 where there was no budget for rock slope reinforcement, and then at a motorway project in Panama also in 2007. In 2012 I gave a two-days course in Brisbane and one of the attendees was by very good fortune Neil Bar who clearly felt affinity for my 7th lecture about a so far unpublished Q-slope.

Over the following two years he sent me interesting photographs of some of the open pits he was involved in, and he began to collect case records which confirmed the Q-slope

versus unreinforced slope angle that I had been using since Panama in 2007. We decided to publish in 2015 (in Salzburg) and for the only time I was first author. Mostly our publications were Bar and Barton. Neil Bar was also a key co-author of the international rock mass characterization questionnaire and publications of Erharder et al. 2023, 2025. As well as seeing the statistics collected, Neil has stated elsewhere that Q-slope is now used in more than 40 countries.

There are certainly frequent publications of applying the method, particularly for road cuttings from countries bordering the Himalayas. Since there are far, far more unreinforced rock slopes than reinforced it appears that Q-slope fills an important gap. It remains to be seen how much it will penetrate the strangely GSI/RMR dominated mining (open pit) community. At present slopes for civil engineering purposes (road and rail, access roads) is the chief and growing area of Q-slope application, but open-pit benches are clearly a potential inroad to more use in surface mining.

Q10: At the INDOROCK-2025 conference held in New Delhi, you delivered a keynote presentation titled: 'Strengths and weaknesses with NMT and NATM and due care with numerical modelling'. This is just one of your many papers analysing NMT and NATM. Will these methods continue to develop independently in the future, or will a hybrid model emerge that incorporates the strengths of each method?

As always, those who have the budget for the much more expensive NATM with its much larger volume of concrete (since double-shell) will continue to construct such transport tunnels while those with more limited budgets will choose the more economic and faster single-shell NMT. A logical compromise which one could call a hybrid method can be NATM at each end, through the weathered portal areas and NMT where rock mass quality is higher through central kilometers. It is believed that improvements could be made in the NATM stretches by replacing lattice girders with something safer as there have been dramatic collapses (folding) of the latter.

Inevitably the usually poorer quality extremities of tunnels suffer greater overbreak, and in such areas the value of lattice girders, and their degree of actual support beyond psychological can be questioned. These aspects will be addressed in my keynote lecture in Eurock in Skopje in 2026.

Q11: Utilizing deep geothermal energy from the Earth's crust, particularly through Enhanced Geothermal Systems (EGS), presents a massive engineering challenge. What challenges does this technology pose to rock mechanics, considering that the rock mass is subjected to significantly different conditions (temperature, stress, fracturing) compared to operations in near-surface areas?

This is a very interesting question. There is an almost totally neglected area of rock mechanics that I have called **over-closure, or thermal over-closure**, that can have and has had, a strong influence on the success of geothermal and EGS projects. It may also influence nuclear waste disposal. Consider a very big volume of rock deeper and warmer than it may ever be in the future, that is about to be subject to tectonic fracturing – in fact rock joint generation due to stress difference and Poisson's ratio and perhaps sufficiently limited tensile strength. The joint set that is formed in that geological epoch may be relatively planar if stress differences were high, in which case thermal over-closure TOC effects may be minor or non-existent.

Let us return to the same volume of rock in a later geological epoch when principal stresses have rotated and are perhaps *more equal in magnitude*. If a new set of fractures are formed they will be rougher. (We can reverse the time-scale and have rougher joints first that are continuous, that are intersected later by a more planar set of joints that have offsets when

crossing the former). If we now 'fast forward' perhaps multiple-millions of years to one of today's EGS or regular geothermal projects, and visualize the injection of cold water with or without enhanced fracturing pressure, there will in both cases be a contraction of the rock mass surrounding the injection well due to the local cooling.

This may or may not compromise the production of warmer water at the production well(s) that have perhaps been drilled in the direction of the principal horizontal stress, or have taken account of the local joint set orientation, or something of both. We will have been able to drill into the now nearer-to-the-surface volume of rock and take joint samples and measure principal stresses. Now comes the interesting part. The rock mass where the project will take place and our core samples are each now much cooler than when the fracturing took place eons ago. The joint samples are tested at perhaps 20°C and when sheared or normal stiffness tested are likely to follow Barton-Bandis predictions of non-linearity and scale effects.

The fact is that the joints do not fit together so tightly as when formed at much higher temperature. The same might apply to the in situ rock about to be converted into a geothermal project, unless the rougher joints are still tightly closed as not yet disturbed. In fact today's cooler JRC after core sampling is lower than the original JRC_{thermal initiation}. Originally the opposed walls of the new joints fitted together very tightly. Thermal contraction coefficients of the constituent minerals or quartz grains mean poorer fit today when cooler.

Borehole heater tests in Stripa granite (Sweden's SKB) and large-scale heater tests in Yucca Mountain (DoE non-lithophysal jointed rock) and a carefully instrumented heated mine-by-tunnel in Climax granite, and an HTM in situ block test in quartz monzonite in the CSM experimental mine in Idaho Springs (TerraTek) – these widely different experiences, have demonstrated one or several of the following: changed shear strength, changed joint normal stiffness, changed seismic velocity, changed thermal expansion coefficients, changed deformation moduli and changed aperture and permeability, each as a result of higher temperature testing.

Joints, especially rough ones, apparently fit together more tightly when heated due to their structural-geologic history. There is a risk that the rougher joints will remain tightly closed when cooled by cold water, and that the injected fluids will be captured by smoother joints that open immediately. In the case of Fenton Hill in California and Cornwall's HDR experiment, injected water travelled kilometres in totally *unexpected* directions. This was because TOC was not understood and is so far ignored in our profession.

Q12: Twenty-one days ago (26th August 2026 is a date that will forever be remembered) an extreme natural phenomenon occurred on the border between Nepal and China (Tibet) with catastrophic and terrible human consequences. According to satellite imagery, the front section of a Himalayan glacier broke off triggering a major rock and debris avalanche. But some have suggested the avalanche triggered the ice fall. This dynamic event caused perhaps several 100's of millions of tons of rock and ice and weathered materials to loosen and plunge down a long valley and finally across the river valley far below, probably temporarily damming it. Soon after a 10's of meters high surge wave 'tsunami' of mud and water was released and started to wipe out everything in its path in a truly cataclysmic event for thousands living or working or vacationing downstream (including hundreds of pilgrims from many countries). For tens of kilometers downstream, people lost everything including their lives. Many thousands are missing including many hundreds of workers in multiple Nepalese hydroelectric projects that were under construction but are now buried in mud. Given climate change, similar phenomena must be expected in the future which presents a terrible dilemma for several Himalayan-bordering countries. What have you learned about this avalanche triggering and the possible mechanisms involved in what at first appeared to be a seismic event, like an earthquake of magnitude 5?

The loss of life of perhaps as many as 7,000 poor souls is hard to accept. It is a tragedy for so many more as well. Before-and-after satellite photographs show an area from the front of the local glacier that is missing with a sharp dark edge suggesting a certain vertical thickness. Below this appears a very steep rock slope and then some 3 to 4 km (?) of probably deeply weathered valley-sides, with permafrost in the upper kilometer at least. The avalanche materials all ended up in and across the river valley far below, including some huge blocks of ice but also blocks of 'finely' broken rock suggesting at least three joint sets. Scale is hard to judge but the millions of remaining blocks at the toe may be a meter and up to several meters in size.

However, since writing this first reaction to the tragedy I have learned that the rock-slide toe (in the foreground of Figure 3, top-right image) is most likely to be a second event that can explain the M4.2 seismic recording that apparently occurred about 3 hours after the main valley-side impact that is assumed to have caused the larger but low-frequency M5.2 event. The sliding surface seen near the toe of the Figure 3 (top-right image) and a more recently available Figure 7 has tell-tale twin colours and different degrees of smoothness. While the lighter-coloured scoured rock base might have $J_a = 1.5$ to 2 or $JRC_n = 2$ to 4, the adjacent grey colour is smoother and perhaps is the smoothed top of permafrost ice (or clay?).

The much larger volume of avalanche debris that temporarily blocked the (now twin) river valley arrived presumably at high velocity about 2km further upstream. This can be seen in the satellite photograph, showing the elongated avalanche path (Figure 4).

The large amount of dark mud and clay seen in the shocking videos of *initially* at least 20 to 40m high *river-tsunami events* along the river channel downstream was presumably derived from a huge volume of weathering products developed during thousands of years since ice retreated from these high valley locations. The geology was reportedly well-fractured leucogranites perhaps with high permeability so they could be deeply weathered. With a melting glacier far above, full saturation is likely. If the 'smoothed ice' referred above can be permafrost: then the main avalanche could have had 'a perfectly lubricated basal plane' namely a velocity-accelerating water/ice layer when heated by friction.

The force with which perhaps more than 100,000,000 m³ of rock and weathering products would hit the opposite valley wall can hardly be imagined. It seems that this impact may have been the major seismic event of M5.2. Short-term blockage of the valley and perhaps dynamic release of huge volumes of entrained water due to the valley-side impact (like dynamically compressing a wet sponge) would have been followed by break-through and transport of *mostly* fines (grey silt and clay and a lot of entrained black and brown mud).

This destructive dense-fluid phase could be larger or smaller in future events which is a sobering and terrible dilemma for those considering repair of Nepal's damaged hydroelectric projects where nearly 900 were still trapped in tunnels while writing this answer to Q12. The section of glacier that fell was reportedly approximately 650m wide. Perhaps the glacier front cracked at the same time as rock avalanche initiation at its top end. Progressive down-dip mobilization of strength (and weakness) components would have to be involved.

Jeffrey Kargel, a senior scientist at the Planetary Science Institute has reported in the internet that the tsunami-like wall of mud descended the first 22 kilometers at an average speed of about 190 kilometers per hour. This wave of finer debris presumably represented a potentially even more destructive higher-density force than glacial lake outburst floods (GLOF): the sudden and catastrophic release of vast volumes of water from melting glacier-fed lakes. However these also pick up large amounts of weathered materials as they flow at high speeds.

Such climate-change-related dangers regrettably remain within the region's extreme and beautiful topography. Regarding the heat and ice melting (and lubrication potential) of rock avalanches a pers. comm. from a former NGI colleague, Prof. Marte Gutierrez is relevant. He took his Colorado students to the Philippines to learn about some of the effects of a mountain-released rock avalanche that travelled many kilometers further than expected and jumped a hill leaving palm trees undamaged on the down-stream side. The avalanche stopped

eventually after burying a village. Rescue workers could not begin their work at first because the avalanche masses were too hot.

This led me to suggest that steam production could even be involved allowing even greater speeds and therefore travel much greater distances than expected. Unfortunately, we have water and ice and steam to add to the potential lubrication of smeared clays: all adding enormously to the potential speed and level of destruction, and potential for blocked river valleys, and the terrible destruction that can follow with sudden dense fluid release.

Q13: I know that you are preparing your autobiography. I had the privilege and pleasure of reading the first chapter. I am certain that your autobiography will reveal a great deal about your life that our geotechnical community will find deeply interesting. When can we expect its publication?

It is true that I have sometimes been busy writing a highly illustrated autobiography covering very many subjects and including simple descriptions of some of the most interesting projects that I have been involved in during more than 50 years. Of course childhood and family and school and university and running and conferences and awards are all touched upon and generally highly illustrated. I suspect it is very doubtful that any publisher will publish such a diverse title as: 'Running, Tunnelling, Rock Mechanics and Family – an Illustrated Journey. Let us see.

Dear Nick, thank you so much for your time. Given your reputation and immense contribution to rock engineering, I hope that the rock mechanics community will find within this interview answers to some of the burning questions that many in our field may have.

Appendix: A suggestion to show Leopold Müller one day, and a recent geotechnical tragedy even greater than Vajont.

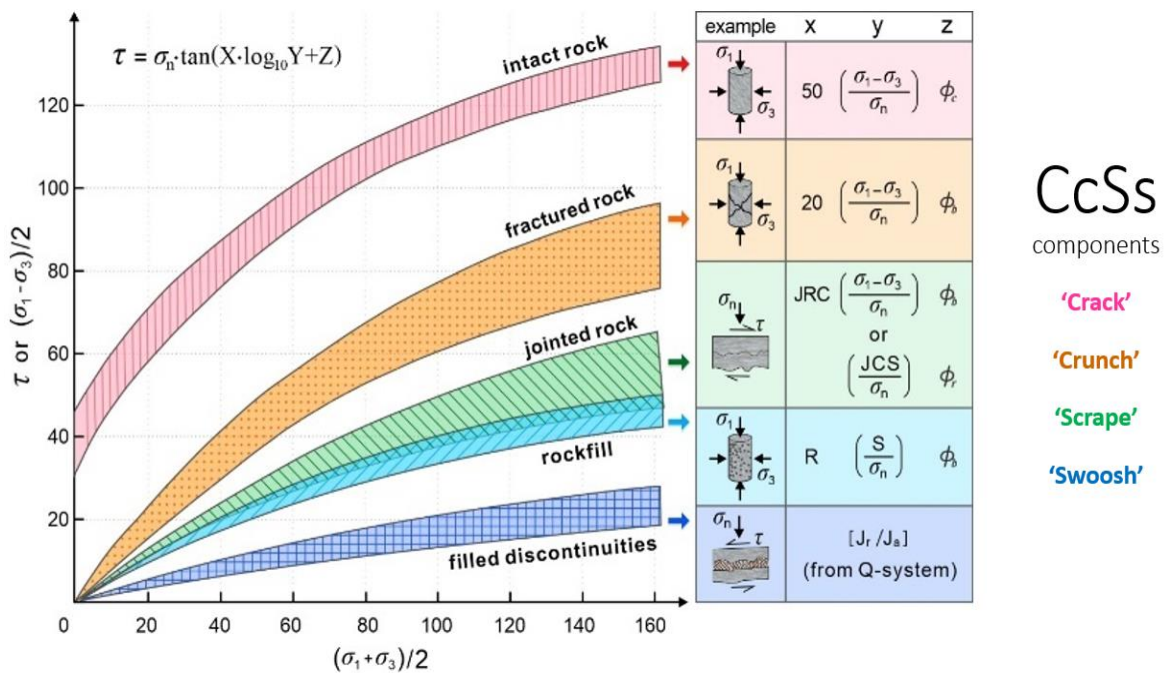


Figure 1. A progressive failure suggestion based mostly on the strength or weakness of discontinuities. See Shen, Shi & Barton, 2019 for an improved intact rock component. One could assume that all these components would be mobilized at different locations down the paths of the Nepal avalanche(s).

References

Barton, N. and E. Quadros, 2015. Anisotropy is everywhere, to see, to measure and to model. Rock Mechanics and Rock Engineering, July 2015, Volume 48, 1323-1339.

Shen, B., Shi, Barton, N. 2019. Graphic Examples of a Logical Nonlinear Strength Criterion for Intact Rock. Technical Note. Rock Mechanics and Rock Engineering
<https://doi.org/10.1007/s00603-019-01902-9>.

Acknowledgements for Nepal photographs (Figures 2, 3, 4 and 5). There were many sources during the first week after the disaster. A Chinese tour guide and his party looking across from Tibet provided many fine but distant images. These also ended in NRK, BBC and CNN news reports and videos, from which they were photographed.

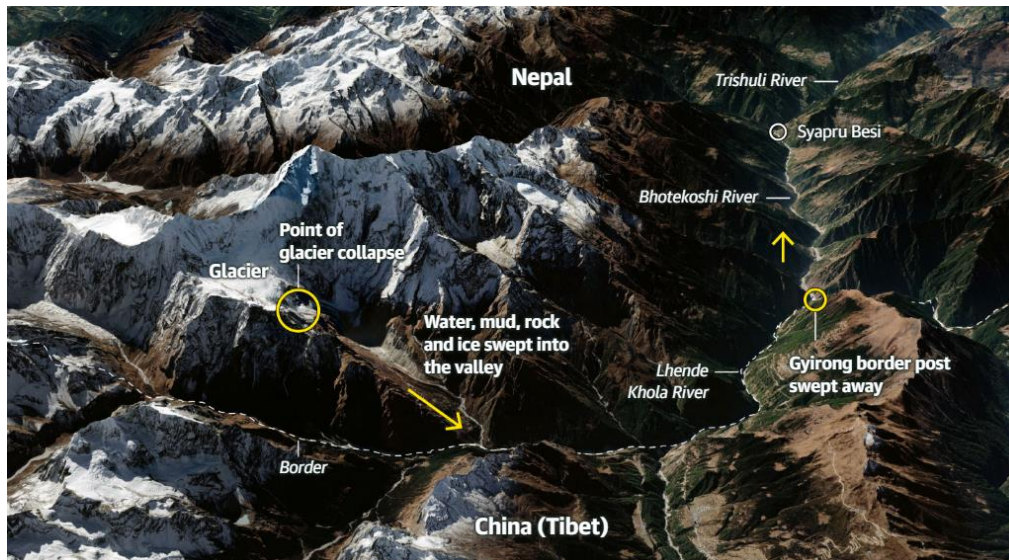


Figure 2. Overview of a tragic and much too geotechnical event that initiated in Nepal.



Figure 3. A photographic record of actually three avalanche events. The glacier front breakage and/or upper rock release, heard as a very loud 'crack' sound by Chinese tourists many kilometers away in Tibet was reportedly followed by the roaring and extended sound of the main avalanche travelling several kilometers down a steep valley. The head wall (cirque) of the Tsangbu Ri peak is 6,780 m high.

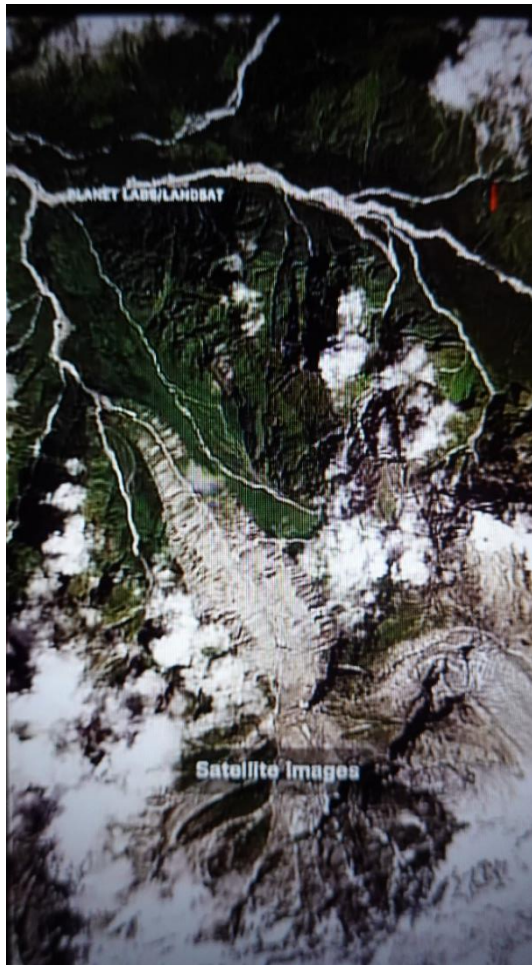


Figure 4. Landsat photographs before and after the major avalanche. The major ice and rock collapse at the top end of the avalanche path has clearly mobilized a very large volume of both weathered materials and jointed rock between the two lateral streams of melt water. Maybe 3 to 4 km length of travel?



Figure 5. Why do we see extensive dark dust clouds in the foreground beneath the white clouds several kilometers from the wide toe of the avalanche? It is perhaps evidence of the dramatic impact into the opposite valley wall, due to an extreme ice/water/steam(?) lubricated (and reduced σ_n ?) condition and therefore a potentially accelerated event. From Chinese tour guide / tourists video.



The rock face below the glacier on Langtang that started the cascade of events. Photo: GUOXIONG ZHENG

Figure 6. The upper part of the long avalanche path. G. Zheng.



Figure 7. The lower remains of the main avalanche seen in the distance. Chinese TV.