Reopening questions in embankment dams, design-performance

Reapertura de preguntas sobre presas térreas, diseño-comportamiento

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1. Introduction.

It is important to profit of symbolic occasions to recollect, recognize, distill, but principally, to discard. It is not without a touch of nostalgia that I recall my first impetuous contact, turned affectionate, with Mexico, upon receiving from Arthur Casagrande, in late June 1959, the surprising belated summons to participate in the First PANAMCSMFE, Sept. 1959. The Paranoa Dam, Brasilia, was being forced to rise 35 meters in 42 days in order to avoid overtopping by forthcoming rains, because of grossly insufficient diversion gallery : and the porous red silty clay of compaction parameters (1.38 t/m^3, 40%) had to go up at a series of 6m 1 : 1 slopes interrupted by 2m wide ditch-berms against tropical rainfall erosion. I was receiving nightly phone calls on the rough monitoring of pore-pressure and slope movements, for signs of feared construction period destabilizations. And the challenge's success, attributable to God's grace, resulted in a marked lesson (as yet far from absorbed by worthy geotechnicians from other ecologies, imbued with the authoritative lessons from Selset Dam, U.K.) .

And with equivalent nostalgia, and respects, I must profit of the opportunity to emphasize the advanced lessons given to embankment dam engineering by Raul Marsal, collaborators, and Mexico, in the multiply difficult conditions locally faced.

Much has been, and is being, achieved. But the inertia of unquestioning impressions and first-order LAYOUT decisions spreads to exponentially growing masses of cornered "specialist engineers". And, with these all the more subdued by PRESCRIPTIONS DISGUISED INTO THEORIES AND CONSECRATED PRACTICES the more respectfully UNDER-STANDING they are judged, the future portends a growing stifling of creative economies. Discarding requires care and courage. Regarding risk-acceptance I have exposed the recognized heavy natural bias towards conserving, ... preferring to shun risk of loss, rather than to seek gain. Civil-geotechnical case-by-case singular prototype engineering becomes subdued by the very luster of multiples that seduces into prescriptions ; fortunately, the simultaneous exponential advances of supporting industrial sectors (equipments, treatments, etc...) compensate the Civil stand-still in enhancing feasibility of the global project. But, at what costs, damaging to the world's QUALITY OF LIFE ? What are the collateral priority contributions to be made by dam engineering to the end-purpose of optimized Water Resources as principal natural gift to Ecology ? Indeed, the Dam is but the means for the Reservoir as the undisguisable aim. Dam engineering has to begin within an enthusiastic multi-disciplinary approach of Civil engineering. But a modern, optimized, symphonic layout civil engineering cannot stifle each increasingly specialized, well-orchestrated contributor of profitable advances from his area. Because of time and space limitations, I must restrict myself to the dam, but duly emphasizing the close interference of the three hydraulic circuits (diversion, operation, spillway) on optimizations of the embankment cross-section.

It is appalling that civil-geotechnical engineering vanguard publications have been 99.9% deprived of any reference to costs, and cost comparisons. It used to be said that "an engineer is a professional who can do for one dollar what others could do for two". It is really frightening and ill-boding for the profession to have become so insensitive to two of its fundamental three-legged supports - technical (means), social (ends), and "economic" (end/means). The environmentalism wave serves as the reminder of the tsunami comeback of the second. Will we be on a single leg as the second wave strikes ?

2. Purpose and scope.

2.1 Dispersions, singularities, underlying conservatisms, subjective preferences.

The lauded search for reports on PERFORMANCE vs. DESIGN has fallen into diminishing returns since the first marked successes 30 years ago. One has to guard against THEORIES OF SINGLE AND SINGULAR CASES. Moreover, one knows about the concentrations at the two extremes of the statistical distribution: a) the dominant bias to described success; b) the less frequent bias to the flashy descriptions by "acts of God syndrome", whether tamed, or led to failure. The real desire and need must be recognized of statistically dissecting PERFORMANCES OF GROUPS OF CASES that respected analogous design concepts.

The already frequent cases of astounding dispersions in Prediction vs. Performance challenges stand as evidence of the great dominant conservatism, uneconomical ; if the cases of markedly unsatisfactory performance are so few, and/but the predictive design ability is so terribly disperse, the automatic conclusion is that professional design/construction practice has progressively incorporated more and greater defensive measures indicated and vindicated because of the individual case-histories wherein the defficiency proved noticeable, gross, failure-invoking. And comprehensibly / regrettably such cases tend to be privy, shielded from concentrated independent analysis. It seems, therefore, that there should be considerable margin for optimizations, and for abstraction from the too-oft-declared and practiced tenet that PERSONAL PREFERENCES ARE A DOMINANT FACTOR IN CHOICE OF TYPE AND DESIGN OF DAM. In short, therefore, my proposal is to list the principal points of design divergences and my postulation of PREFERENCE IN PRINCIPLE for each point, consciously preferring to exclude any case reference, for reasons of respect for the many singularities that prevail, case by case, enhancing in us the perennial lesson of delighting in differences, around the accepted basic principle, with equivalent safety, and without incurring in unperceived incremental expenses.

The spatter of authoritative opinions accepting subjective personal preferences as professionally valid is, to my mind, an unfortunate stance to pass on to successors in the profession. Personal preferences have to be enhanced, indeed, as validly superposed on a general preference in principle.

2.2 Method vs. end-product specification.

For many a reason, including the important case of performances to be judged, and even cases of failures, it is fundamental to REJECT ONCE AND FOR ALL THE OFTEN CITED, AND EVEN LAUDED, METHOD SPECIFICATION. It is illogical. A design desires and imposes a prospective performance parameter. The only principle acceptable is the end-product specification. If the designer has good experience tying methods to probably achievable end-products, the end-product specification can be complemented with recommendations (and never more than an offered recommendation) on the method.

One important by-product from the insistence on end-product performance specifications will be the pressing developments of desirable complements to, and substitutions of, index tests, by DEVISED APPROPRIATE-SCALE PARAMETER TESTS, of which these structures of exponential responsibility, and yet considerable latitude of economies, are so grossly deprived.

2.3 Performance parameter checking vs. overall structure monitoring.

Let us forego the criticisms on subconscious biases in any a-posteriori global complex-behavior monitoring and interpretations. There are other VERY SERIOUS CRITICISMS ON LOGIC, as prevalent in the present chasm between index tests and global performances.

The missing link depends on the assumptions (among others) that : (1) behaviours depend fully and adequately on INTEGRATED (AVERAGE) EFFECTS ; (2) the reasonings that prevail for these integrations are fully and adequately COVERED BY OUR THEORIES AND COMPUTATIONS. Thus unconsciously our dam performance observations are moulded to reinforce the status-quo. And, if any behaviour is localized, dictated by extreme-value statistics, and/or is minutely-progressive though chronic, we (a) will not detect (b) will not heed to the alerting signs that may be offered by singular cases that slightly exceed the limits of impunity. I therefore feel the calling of this occasion towards exposing misconceptions (some) that have been transmitted and routinely used, either geomechanically inconsequential by over-conservatism, or pardoned from risk and damage by God's infinite grace.

Take for instance the QUALITY CONTROL INDEX on clayey soils by referencing to the relative dry-density via spot-tests and the PERCENT COMPACTION and the Compaction Water Contents as percentages of the Standard Proctor Optima (N.B. multiple appeals have been voiced and published, to abandon the use of water content differences around the optimum, because, e.g., a (2%±) difference is insignificant for a clay of optimum around 40%, but unbearably high for a coarse sand-silt-clay of optimum around 10%). LOGICAL CRITICISMS ABOUND, though the practice has become ingrained. And meanwhile almost 20 years have gone by since the development of VIBRATORY ROLLER COMPACTION-METERS emitting, during the passes, micro-seismic waves for recording their velocities between front and rear axles: since dominant behaviors of embankments are associated with "nominal-elasticity" MODULI , and the quality-checking is done "on line", and good "cousin-parameter" correlations can be expected and developed between such quality-control micro-moduli and the embankment deformation moduli, why is it that the profession has not moved forward to this obvious, logical improvement ?

The case is far more dubious, even RIDICULOUS, regarding rockfills, specified, constructed and inspected only by TOTALLY ILLOGICAL INFERENCES, since dry-density checks (necessarily to be accompanied by exactly corresponding grainsize curves) reflect absolutely nothing associated with point-contact-crushing and consequent moduli changes. And what to say about the a-posteriori expensively derived VERTICAL DEFORMATION MODULI OF THE DAM, mainly to be used for wishful thinking pseudo-correlations for the really needed INCLINED DEFORMATION MODULI for the water-load deformations of the impervious deck (very preferably concrete-face) ?

Publishing proponents of such pseudo-correlations obviously don't raise ifs and buts to their own thesis, and become prophets (to their own avail, and) to the stagnation of the profession in medieval revelation. Regarding the compacted rockfill COMPACTION PRECOMPRESSION AND SHEAR STRENGTH IN SITU, and the hypothetical SLOPE DESTABILIZATIONS : how many millions of cubic meters (of differentiated petrographies) are used across the world, without ALMOST ANY REASONABLE-SCALE TESTING HAVING BEEN PROPOSED OR RUN ? What would be the benefit-cost ratio of a prediction-performance challenge, if we honestly exposed our hypocrital "already-known" posture ? Perhaps 1000 to 1, or more ?

It is not individual success cases, but narrowing statistical estimations of the LOGIC OF BOUNDARIES OF SUCCESS-UNSUCCESS, that build firm steps for our progress... for which there is yet an exhilerating latitude.

2.4 Exclusion of seismic considerations.

With the exception of the Alibey Dam, Istanbul, 1967, near the Bosphorus Fault[1] I concede herein that my professional experience has not involved seismic effects of significance on dams. The subject, of special relevance to Mexico, invokes contributions from eminent panelist colleagues. I limit my comments to the following brief points :

(1) Microseismic reservoir-generated activity has regrettably consumed very significant budgets to no avail as regards embankment dams. The accepted reasoning of hypo- and epi-central foci (assumed localized) as very close to the structures, indeed generates greater accelerations, of noticed consequences to tall rigid structures and powerhouse equipment. But it is believed, and positively submitted, that concerns to embankment dams should be discarded. In passing we may note that for buildings on saturated soils in Sweden the vibrations due to heavy rail traffic have been investigated, recording accelerations up to 1%g over many years : more to the point, but still employing mere partially relevant analogy, I note that with regard to compacted rockfills I have published the indication perceived, that the impacting vibrations imparted to each successive lift by the current vibratory rollers are not far from comparable to those generated by significant earthquakes, the principal effects not covered being the stress levels of the elements within the body, and the integrating effect through the prototype. Incidentally, as is well known, the needed estimations of catastrophic and strong historic earthquakes have relied very much on "felt" INTENSITIES. Humans can feel accelerations $\geq$ 0,001g (varying in the range of 100:1 depending on position, sitting, standing or lying) and buildings of average-good quality suffer from accelerations $\geq$ 1g, the first fissures on cladding occurring with 0.1 to 1.0 g.

Since contact crushing changes a finite compression modulus to an infinite decompression one, two inferences have seemed logical : (1) prototype deformations from seismic events should be much lower than computed from monitored deformation moduli (and progressively decreasing with time and repetition-hardening) ; (2) good prospects of developments open up via closer analysis of the effects of vibratory rollers consciously changeable in weights and impact to suit rocks and lifts.

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[1] *which, incidentally, was instrumental in my fostering at M.I.T. the very rewarding push of Prof. C.A. Cornell, and others, into the assessments of probabilities and risks on important geotechnical works.*

(2) Broad dispersions and consequent confidence bands of probabilities of greater seismic events.

Notwithstanding the professional competences engaged, and respectable successes achieved, any independent viewer should question the logic behind the pseudo-deterministic calculations and presentations that are almost the rule. The appalling disproportion in the scales of dispersions of GEOTECTONIC CAUSATIVE FACTORS TRANSMITTED, and of the effects of real consequence to geotechnique and to sensitive structures of our modern Society, has been kept from exposure. The really formidable tasks involved are well cited by authorities, with respect.

Summary mention of only four points will suffice for my present purpose, re-enunciated forthwith, of choosing a diametrically opposite observation-research-design approach. It seems most surprising that hypo- and epi-centers are quoted as geographical "points", when undefin(ed)(able) presumed plate-contacts and breaks along very wide surfaces are at play. What revisions will be produced if each plotted point is represented as the wide ellipse of "fact" as observed, hypothesized, and interpreted ? Next, the computed (estimated) MAGNITUDES are given as definite numbers (to one decimal place) when probably established from a wide scatter of results (I have always "received" the number at face value, unfortunately never accompanied its calculations). Thirdly, there are many widely different published curves of ATTENUATED SEISMIC TRANSMISSIONS WITH DISTANCE, "in rock" : another great dispersion (cf. Fig 1 as a mere example). Fourthly, there are the great dispersions in DAMPING OR MULTIPLYING factors, from bedrock to upper layers, wherein the final complex effects suffered are really related to INTENSITIES. The fact that these data were inevitably rudimentary and subjective (historically), and so also their charting and class-subdivisions, led to two natural subconscious judgments of modernity :on the one hand, that advances of different and progressively varying procedures of scientific research theses and data processing on specific points of the complex global problem, can establish YES-NO CRITERIA; on the other hand, that it was a forelost competition between determining the statistical/probabilistic universes via great many data but subjectively observed/recorded, or via very few seismographic points of precision (from great distances and triangulating directions) analytically treated into idealized modelling. However I permit myself to use the case to emphasize a concept of serious importance to many a civil-geotechnical problem of special complexity, in the face of lures and successes of progressing analytics. The profession is at the service of Society for attenuating EFFECTS, complex effects, whether or not analytically dissected. The complex LUMPED-PARAMETER approach and the PROGRESSIVE ANALYTIC approach have to complement each other ; often, especially at the beginnings, the referencing to "reasonably analogous lumped-parameter indices" can be more honest and effective, whilst the analytical tests advance. Point seismographic data of great precision doubtless improve estimations of MAGNITUDES : but how do the precise seismographic point data transmit to diverse surface effects (cf.Fig. 1b), and very diverse human structures near surface ? By a vicious circle Magnitudes have to reverse their transmitted EFFECTS OF INTEREST AND CONSEQUENCE through the first three mentioned dispersions.

I question : does the referencing of local seismic effects to Magnitudes thus rely on an exponential falsity of statistical/probabilistic logic that in a ROUND-TRIP OF VERY WIDE DISPERSIONS one hopefully closes the circle, rather than opening it even more widely ? The crucial need is for near-source seismographic data from shallow earthquakes of larger magnitude : caveat regarding "source".

(3) Engineered solution to seismically-generated liquefactions ?

Once again, I report to a hypothesis hinted twenty years ago, that would exemplify an engineering principle too often suppressed by well-intended scientific research : ENGINEERING VS. SCIENCE. In lieu of more and more meticulous research on when a specific nevralgic behaviour occurs, the search for answers and procedures for when its occurrence can be averted. Since many are the conditions for non-occurrence of a particular singular complex behavior, it generally proves much easier and simpler to bypass or divert a problem.

Besides structural deformations and damages caused by seismic accelerations and resonances, the more nevralgic geotechnical phenomenon is that of liquefactions of loose saturated sands : research on these special conditions has been intensive. Everything dependent on contractive behaviors, and rates of developments and dissipations of dynamic excess pore pressures. What economic procedures could produce (for instance) within the saturated sands some air-injected unsaturated pockets for more-than-sufficient internal drainage, such air-injected pores to be periodically reestablished ? Or coarse compaction-drainage piling?etc.?

2.5 Basic principles for effective progress.

Designs and commissionings of new dams/reservoirs must go on unhindered, at faster rates than can be idealized for effectively anticipating them with improvements of the gaps and dispersions in our confidences of knowledge. Therefore, fundamental principles must be respected as check-lists for decisions as economic/safe as possible. Summarizing very briefly my proposals of 25 - 20 years ago, I emphasize the reminders :

(a) Always question and reason, inasfar as possible, whether the failure phenomena to be avoided are dictated by STATISTICS OF AVERAGES, OR OF EXTREMES. Piping, point-generated and inevitably progressive once started, comprises extreme-value behavior. In a dam body, compression settlements result from integrations, therefore responding to averages, whilst failure surfaces (disregarding classroom idealizations) follow irregular positions of minimal tensile and shear strengths. Having diagnosed, avert extreme-value problems by interposing JUDICIOUS PHYSICAL CHANGE OF UNIVERSE. In cases of averages, work with (and within) the appropriate determining limiting CONFIDENCE BAND, and progressively test/check narrowing the confidence band for progress. Revise the conventional FS values, mostly untenable, correspondingly, for logic. Benefit/cost ratios, including costs of risks are always at stake.

(b) In realism, humility, and professional respect, recognise and press for the professional need for decent-size field tests on near-prototypes built expressly for confirming analytic theories : programme them as the devil's advocate. No profession can progress in the blind, much less in the delusions of wishful thinking and/or in comforts of numbers of concurring opinions.

(c) In the face of each decision, reflect on what would happen if premises reach the limit of proven impunity. What would be the consequences of eventual mis-performance, and failure ? Right decisions derive somewhat from facts/knowledge/wisdom synthesized from the past, but have nothing to do with or for the past : they are accountable to the future (e.g. spilling a maximum probable flood to downstream onto a river that may have greatly densified downstream occupation). Dam decisions are like unto marriages, that are quoted to have to be rebuilt, in principle, every day. In case of any unfavourable development, how rapid its consequences, what corrective measure(s) are possible, with what speed of mobilization and of effectiveness ?

3. Optimizations on diversion hydraulic circuit and risks. Hypothesis on spillway hydraulic circuit.

Geotechnicians cannot persist in a path suicidal to embankment dams by failing to recognize that a large proportion of optimizations still open depends on the costly problems of length-delay-risk of diversions. Layout engineers (mostly Hydraulics-based), acting on VISUAL TRANSMISSION OF "CONVENTIONAL MODELS", repeatedly conclude in favour of RCC-gravity or CFRD dams because of these dominantly important factors.

In how many of the cases published by geotechnicians do the following points make even a timid stage appearance ?

(1) Deep incorporation of cofferdams into the maximum section ?

(2) Comparative foundation treatments at river bottom, which is whereon hangs most of the success/failure of dams ??

(3) Emphasis on varied cross-sections along the axis ?

(4) Above all, representation of a couple of typical sections on the abutments, which is where foundations often impose the embankment alternate ?

(5) etc ...?

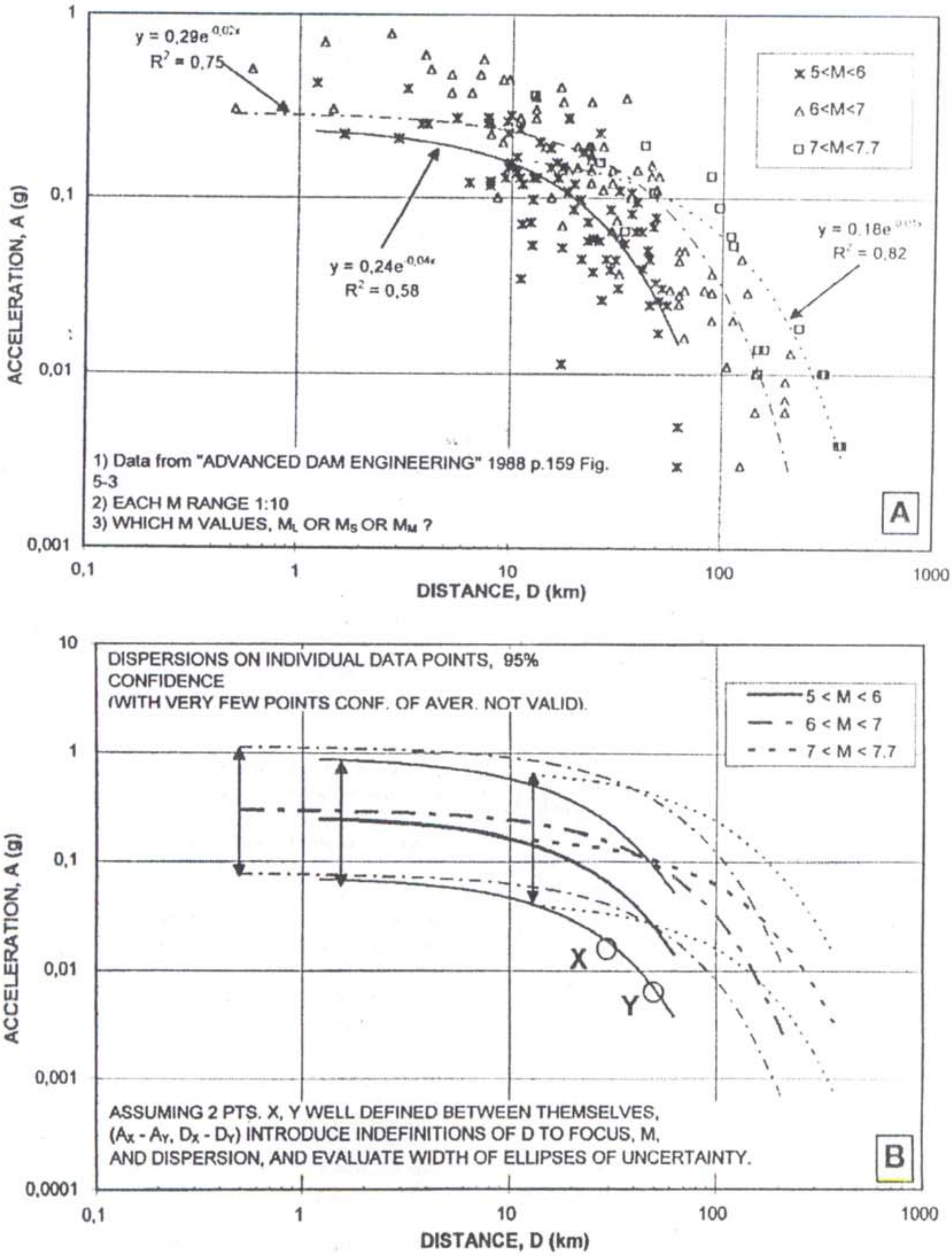


Fig. 1 - Sample of inexorable dispersions in seismic data and analyses.

The professional fact is that new solutions (already ipso facto seductive) are generally PUT FORTH BY THE MOST DARING GLADIATORS, while the old solutions tend to be stifled in the (1) presumed limits of PRACTICE, older, less-advanced, less-courageous (2) diminishing returns of progressively staler VALIDITY-PROVING ANALYSES of third-order importance, in lieu of PROFESSIONALLY CREATIVE "unproven but undeniable" engineered physical options.

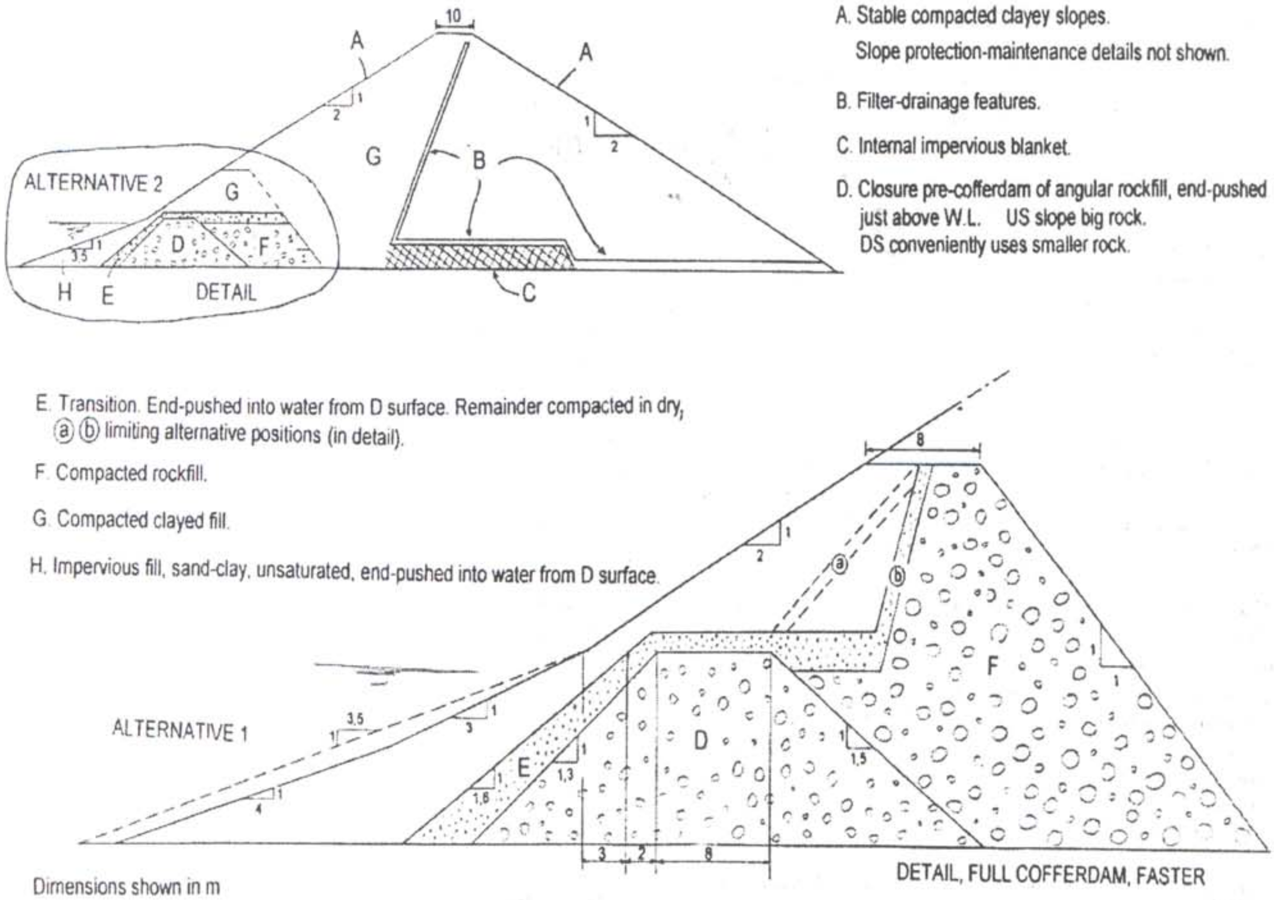
Brazil's hydrologic-hydraulic and geologic river-channel vs. abutment realities invite my presenting a mere example to elucidate principles. Hydrology (1) is so abundant, that cofferdams frequently reach 40 - 50% of the dam height, (2) divides neatly into a dry period of about 6 months, favourable for few-day closure by "precofferdam", followed by rapid cofferdam raising as part of dam for first wet-season risk (roughly 1 in 10 - 20 year recurrence) (3) often requires further rising with a partial upstream dam section to meet 1 in (200 - 500) year risks for subsequent wet seasons until spillway sill and chute become operational.

Geology provides bedrock riverbeds, and abutments of deep weathering (residuals and saprolites) unsaturated down to about half of the thickness. The bedrock level rises somewhat in the abutments, often steeply enough to favour use of diversion tunnels. Materials for compacted soils and angular compacted rockfills are almost unfailing. Weathered rockfill spread and compacted into crushed continuous grain-size has been the mainstay transition material. Alluvial sands and gravels, essentially inexistent, are not being missed in updated practice (cf. item 6(2)).

3.1 Optimized cofferdam, in river, on rock.

The subject of optimized idealized dam bodies (and competent analyses and experience to justify them) is treated in separate, as it constitutes the principal aim of dam design/construction. Assuming for the while that good compacted sections may tend towards steepest average slopes of 1on 2 (IV:2H) DS and US for earth, and 1:1,3 US with 1:1.2 DS for ECRD earth-rock, the purpose herein is to present key points on incorporation of relatively high US cofferdams into the main permanent structure.

The following Figures 2 and 3, with expanded details, appear to offer the best concise manner of presentation for discussion, item by item (as is summarized in the legends). The following comments appear important as explanatory.



A. Stable compacted clayey slopes.
Slope protection-maintenance details not shown.

B. Filter-drainage features.

C. Internal impervious blanket.

D. Closure pre-cofferdam of angular rockfill, end-pushed just above W.L. US slope big rock. DS conveniently uses smaller rock.

E. Transition. End-pushed into water from D surface. Remainder compacted in dry, ⓐ ⓑ limiting alternative positions (in detail).

F. Compacted rockfill.

G. Compacted clayed fill.

H. Impervious fill, sand-clay, unsaturated, end-pushed into water from D surface.

Dimensions shown in m

FIG. 2 - "HOMOGENEOUS" EARTH DAM.

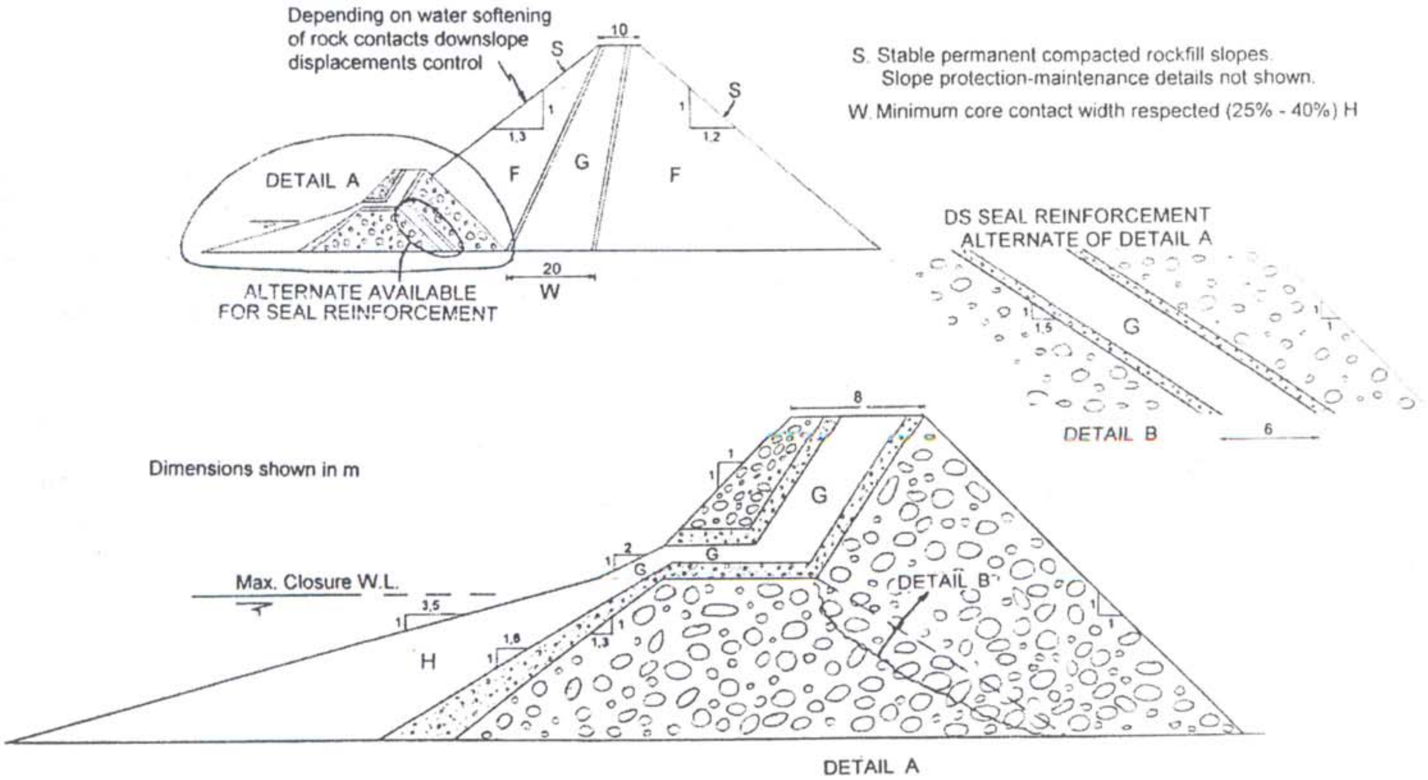


FIG. 3 - COMPACTED EARTH-ROCK DAM.

One important concept that my experience forces me to emphasize concerns the incorporation of the upstream cofferdam, WITH SEAL ASSUMED REASONABLY PROVEN, as important on two counts: a) providing a large model-to-prototype check on rock foundation GROSS PERMEABILITY AND PREFERENTIAL FLOW PROBLEMS, on presumed similar geology [2] without grouting and core-contact treatments; b) since the greatest unknowns and problems of seepages are through the foundations, and increased heads should produce proportionally increased effects in them (if well-behaved), it is of uncontestable practical interest to profit of this first seal as a complement to the seals to be carefully built in the permanent dam structure.

The first reality is that end-pushed angular rock into water is inevitable in low-flow river closure, but the great preference given to spreading/interlocking/compacting in dry lifts automatically leads to 1) maintaining the rockfill jetties as low as possible above water, gradually rising with channel striction 2) a pre-cofferdam closure body with crest of crushed well-compacted rock fines.The US and DS slopes naturally result somewhat different, and at final closure the advancing end-pushing is a little to upstream.

[2] *Regarding geology it is emphasized that the background knowledge is indispensable and dutifully respected, as is the "past" that generated the "present". But the engineer should not harbour illusions beyond the gross orders of magnitude inferrable: the decisions of significance need a much tighter scale of data. To use an analogy of impact, no person imagines deciding on a consort based on photographs and DNAs of the grandparents of the prospective choices.*

The second reality is that by end-pushing to US a minimum top width of finer crushed rock-transition material for the US-face of the pre-cofferdam, the protective width automatically increases with depth down to the bottom because of the flatter (e.g. 1:1.6) critical "natural" slope assumed by finer granular material (N.B. careful tests and checks were made, for confirming this logical expectation, in cases of very deep cofferdams pushed into water). Finally, the earth materials (preferably of wide grainsize, and unsaturated) end-pushed also increase in thickness top-down by the same reason, and when rapid pumping-out of the cofferdammed volume begins, the compressive seepage stresses across the soil-and-transition thickness automatically force sealing action. Inasfar as necessary the amount of soil end-pushed, and the pumping rate, are progressively increased until clogging-sealing becomes effective to satisfaction, and PROVEN BY THE REDUCED LEAKAGE.

For rapid subsequent raising of the pre-cofferdam to the wet-flow cofferdam height full advantage must be taken of the much better parameters of well-compacted materials in the dry (N.B. a certain thickness of submerged rockfill downstream is easily accepted for logistics, merely taking into account the greater compressibility settlement). Zonings are minimized for facility and speed.

Figs. 2 and 3 suggest the possibilities (quite successfully used in cases) for a homogeneous earth dam, and for an earth-core rockfill section. In optimizing the connection between early US-seal, and later preferred core-seal (for US protection from erosions by rockfill) the conventional dimensions of proven practice are a) impervious blanket thickness of about 0.1 H b) impervious core width of about (0.25-0.35) H c)US special transition unnecessary, well enough disintegrated partly-weathered rock) from inside out d) DS filter-transitions of wide grainsize well-graded, comfortable width for quality construction by equipment.

The sections are schematic, but using slopes fully proven by experience and analyses.

3.2 Raising compacted fills on steeper temporary slopes.

Mention will not be made of various slopes within the dam body (principally in rockfills in steep V-shaped valleys) used much steeper than conventional, for facilities of accesses and logistics : for multiple reasons compacted rockfills have been treated with great confidence regarding deformations and destabilizations, the only significant problem having been (not infrequently, under inadvertent practices) that of unfavourable very coarse segregations at edges and interfaces.

Very briefly the intent here is to emphasize that there have been successful cases of very fast raising of partial sections of clay-fill dams for rapidity of closure. Good compaction on the dry side is the key, having permitted the rise of a temporary DS slope of a partial dam section in a slope of IV : 1,4 H in 42 days to a height of 35m. Check analyses were made by UU triaxial tests (as logically indicated), compaction preconsolidation pressures duly considered. Suctions (regrettably not measured) hitherto very insufficiently researched and monitored, should justify the fact in due time, while the practical fact stands as undisputable, and great many office theoreticians deny the fact's very possibility because hypotheses and theory would dictate otherwise.

At any rate, regarding temporary vs. long-term behaviors and risks, it is consecrated engineering practice to accept much smaller factors of safety "during construction" . The question then bifurcates into : (a) much smaller costs of risks during construction ; (b) the feared (unknown) change of behavior parameters in long-term time, GENERALLY SUBCONSCIOUSLY inferred as inevitably DETERIORATING. This inference is natural for materials "fabricated", brick, wood (dead), steel, concrete, etc.., and has been automatically attributed also to compacted clayey soils. I find it necessary to emphasize that in many tropical red soils, apparently the effect of time is to GENERATE STRENGTH IMPROVEMENTS (by micro-cementations of ferrous oxides etc..) and NOT DETERIORATIONS (modern environmental pollutants excluded).

The question therefore arises : if an END-OF-CONSTRUCTION CONDITION has proved perfectly stable (including stopped deformations down-slope) and time can only be expected (and test-proven) to improve strength, barring new/additional destabilizing stresses (e.g. seismic), why should the steeper "temporary slope" be complemented with backfill to produce a flatter slope visually more acceptable, merely as preestablished by conventional design analyses ? Does not the so-called Observational Method stand firm by its logic ? As will be emphasized in item 5.6, is it not imperative (on the logic of the importance of historic stress-strain-time paths in geotechnique) to abolish compartmentalized Stability Analyses, and to substitute them by SUCCESSIVE DESTABILIZATION ANALYSES for each new condition, as imparting an incremental triggering effect on a known prior condition ?

3.3 Steepness of transverse slopes for successive phases.

One of the critical concerns that dominated many features of earth-dam design and construction (specifications and procedures) in the period 1955-1980[3] must be recalled as the question of TRANSVERSE CRACKING, and postulated failures because of erosions along these "open cracks" . One first important point, too often omitted in publications and pronouncements, is the qualification of "OPEN" vs. closed cracking, automatically distinguishing between TENSILE CRACKING vs. SHEAR-PLANE DISPLACEMENT ("plastification") well recognized, from several viewpoints, as contributing to "tightening, sealing" except in strong very rigid-brittle material.

Undeniably the nominal modulus of elasticity in tension is much greater (about 4 times , cf. Fig.4a) than in compression and thus in the upper 10 - 20 meters of high dams subject to noticeable differential settlements along the crest, the tendency to opening of transverse tension cracks (V-shaped top-down) is definite.

An important warning must be made at this point, repeating what was published 25 years ago as a result of a rough statistical analysis of our data at the time (mostly residual soils). It concerns the intuitions on "flexibility" and "plastic deformability" repeatedly mentioned in consulting reports, and publications, as a prospective defense against tensile cracking. Automatically a higher (unquantified) capacity to deform without cracking has been associated to higher values of the soil's Plasticity Index I_p.Actually the I_p is no more than a measure of the range of water contents over which a clayey material exhibits, under atmospheric pressure and suctions (unresearched, unknown, but estimatable), the empirically defined "plastic behavior". Incidentally, should it not be definitely more logical to relate to the Plastic Limit w_p (although I_p and w_p are roughly related) ? At any rate, the principal point is that it is meaningless to discuss the "potential range of plasticity ability" of a type of soil, when what is at stake is its PLASTICITY AS COMPACTED, and, in our experience, it is difficult-to-impossible to compact clayey soils beyond ranges of (0.95 to 1.05) w_{opt} and (0.90 to 1.10) w_{opt}. At compactable water contents high suctions (indispensable for trafficability, and to avoid overcompaction slickensides, etc.) render the MORE CLAYEY MATERIALS MUCH MORE BRITTLE. Fig.4b shows that even when referencing to the modest indirect index discussed, the plasticity behavior occurs within an intermediate range, and does not increase with increased "clayeyness"of the soil (unless used in well confined volumes).

Forty years ago, for reaching an emergency engineering decision on the allowable settlements of the porous red clays of the Tres Marias Dam left abutment, it occurred to me to appeal to a rough analogy (suggested by the Skempton-McDonald paper on differential settlements affecting buildings), of vertical slices of the compacted clay, with brick walls : and so, based on distortions (approx. 1 : 200) causing fissures in high buildings, it was proposed to smooth-out compressible foundation thickness to avoid specific differential settlements of the crest greater than 1 : 100. It was agreed-upon by A. Casagrande, and has been used (much too indiscriminately[4]) in very many cases. HAVE ANY CRITERIA BEEN DEVELOPED AND DIVULGED, MORE COMPLETE AND

[3] *Wherever periods (approximate) are cited, they are estimated with regard to the vanguard of best practice : at any time, and in any given case, the dispersion of knowledge and decisions covers a much wider range, since often it takes more than 10 - 15 years for an advancement to be divulged (even within academia), and more than 25 - 30 years for a practice to be relinquished.*

[4] *For buildings going up (N.B. totally different in buildings settling because of urban tunneling etc..) the ILLOGICAL CONDITION was emphasized of the association of fissures on the N^{th} floor because of settlements of foundations from the beginning, that is, most of it occurring before the floor came into existence. Similar reasoning applies to a large part of self-weight settlements of dams. However, CONSERVATIVE PRESCRIPTIONS are necessary because of tensile cracking being of weakest-link extreme value statistics.*

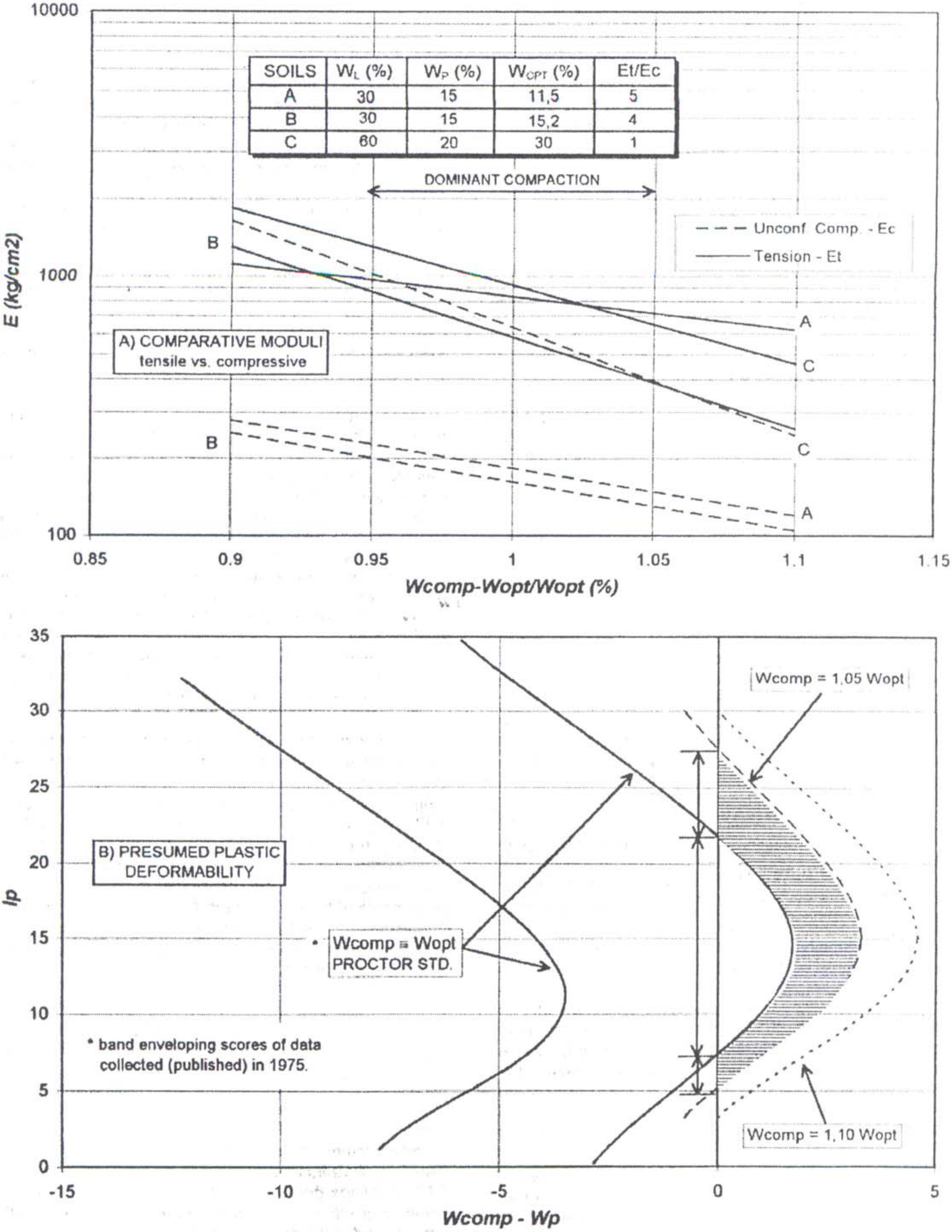


Fig. 4 - Index data ref. cracking of compacted clay fills.

RATIONAL ?[5] In the frequent finite element analyses conducted since then, have there been adjustments to the greatest interfering factors, seemingly (a) compaction built-in lateral stresses (highly favourable) (b) more rigid behavior in tension than in compression (unfavourable) ?

Among the many problems, real and/or fictitious, affecting FEASIBILITIES OF EARTH DAMS, only two need be mentioned herein, that will bring sour memories to job engineers. And it may be mentioned that in costing embankment dams, a crude rule-of-thumb is that : after calculating the cost of the entire section, the final cost of the dam is ROUGHLY DOUBLED BECAUSE OF THESE DETAILS, that do not appear in teachings or publications.

Core-rock contact smoothing. Intuitive criteria generated from very questionable logic have caused immeasurable expense in smoothing rock-contact treatments, mostly unnecessary. Obviously, negative slopes have to be avoided. However, regarding stress concentrations because of sharp steps and angles, what credence can be given to estimates (and, sometimes, finite element analyses) leading to tensile cracking at great depths ? Compaction precompressions start off with pressures higher than 6 - 8 kg/cm^2 : and, in core-foundation contacts of dams higher than 35 - 45m, final overburden pressures will be higher. Would it not be sufficiently convincing to plot the prospective Mohr circle of stresses of the soil elements, and, comparing with the strength envelope, conclude that PLASTIFICATION would inexorably be in shear, and never in tension ? Does not shear-plane "remolding" (e.g. fault gouge) reduce permeability ?

Steepness of transverse slopes (e.g. for second phase closure section). I well recall the days when such slopes were considered absolutely inadmissible steeper than IV : 3H (even when local natural soil abutments were steeper), and I nurtured misgivings that possibly the tighter requirement was because it would be of a contact between two fills, still partially moving, at different rates. In our dams, of settlements almost totally during construction, the whole matter was progressively discarded by several unquestioned facts, and good performances.

It is in steepest V-shaped valleys (ipso facto rocky) that dams have been compacted earth-rock with narrow cores : steepness often (1.5 to 2)V : 1H ; obviously no change of average slope was remotely considered (though smoothing of irregularities could not be dispensed, by convention) . A second interesting observation derived from such narrow earth-rock (cores approx. 40 - 50%H) dams. The first-phase dam transverse US - DS face awaiting the closure section tends to stay steep, roughly 1V : 1,4H , because the core's face stability is dictated by the rockfill : it is against such a slope that the core-core contacts have been successfully built.

The third condition of countless good performance data concerns core contact against concrete gravity walls of on-river hydraulic structures. Structural engineers had "optimized" and grown accustomed to slopes of about (8 to 10)V : 1 H (any breaks in the slope being anathema) ; and geotechnicians' concerns were powerless against concrete costs. This case has been analysed twenty years ago, closing off the era of the previously-imposed wrap-around section, in favour of end-butting. It was argued as logical that the worst possible face slope should be that roughly corresponding to the interface shear strength. Slopes neatly steeper would not hinder core settlement, with increasing favourable wedge action. At slopes around 45° (presumed interface shear strength) some volumes would settle normally while possibly a higher zone might be held up by the shear hang-up (silo effect) causing the opening of subhorizontal tensile cracks (or predisposition to hydraulic cracking by reservoir seepage pressure). Obviously at the other extreme, of very flat slopes (less than 1 V : 2,5 H) there is no problem of interface shear and the differential settlement distortions will be inconsequent.

3.4 Different elevations of diversion tunnels.

As often occurs, it was by a forced condition in a case that the innovation occurred in our practice. The bottom tunnel(s) dimensioned for dry-period diversion require the expensive gate structure, greater length, tighter scheduling, etc. Instead of complementing for wet-period flows at the same elevation and deeper into the abutment, with aggravations on all counts, the practice has asserted itself of locating such tunnels sufficiently higher (and further into the abutment). With due scheduling the final closure is achieved merely by a "permanent cofferdam" and/or concrete plug within the upper tunnel during the dry season before reservoir filling. Overall economies are really very significant.

3.5 Shallow and low-flow cases without diversion gallery.

Once again, the solution arose in a specific case of need to complete the gross dam section during one dry season, low diversion flow and corresponding very slow rise of reservoir. In truth, considering the sufficient satisfactory experiences of temporary cofferdams up to 50m high "dumped" into 35m of flowing water, there should be no technical problem at all in pushing across the valley with a temporary cofferdam, and "reinforcing it" for permanence. But psychological factors overruled, principally because weathered rock abutments were rather irregular, indicating presumed need of careful contact treatment.

The solution was to build the formal dam rising in alternating steps of adequate transverse inclinations and "canals" of retarded compacted fill serving as successive diversions. While the contact treatment advanced on one bank, with the diversion depression nearby, the fill rose on the other bank against the already treated surface (up to 4-5m higher). And so the canal diversion shifted successively from one bank to the other, at each forthcoming step being easily blocked and redirected by temporary end-pushed earth cofferdams. Depending on flow velocities (about 1-1,5m/sec being well accepted by the compacted clay fill) some soil-cement surfacing (one layer, 6% cement) was used against erosion risk.

3.6 Hypothesis of spillway on embankment.

The spillway structure is a gravity body of much more stringent requirements than a routine concrete gravity dam, because of the disproportionate water loading caused by the gates (very big in our hydrology), and the responsibility on differential movements capable of causing jamming. The complement of the spillway chute is a structure of enormous responsibility with regard to any possible start of damage because of the energies liberated and the rapidly progressive destruction around started damage.

By conventional obligation the global unit has sought support on good rock. However, as mentioned, our geology has frequently presented abutments much more weathered, and cases have already been faced of support on partly-weathered horizons,

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[5] *Twenty years ago it was shown that the volume of data on "foundation" settlements acceptable on transverse cracking increases considerably upon recognizing that the lower part of a dam is the "foundation" for its upper part. Cases such as Balderhead, Chicoasen, etc., suggest that the use of wetter compactions may boomerang unfavourably, invoking rates of settlement vs. rigidifying (thixotropic/consolidation) as a crucial parameter. Parametric variations in finite element analyses could rapidly assist in setting criteria, to be adjusted by ulterior specific testing/observations.*

reached by deep cuts (often requiring stabilizing reinforcements etc..).

The logical question arises of the comparative acceptance of supporting the global unit (to varying degrees, depending on layout optimizations) on the well-compacted embankment rather than on weathered rock horizons. Psychological deterrents excluded, two logical reasonings arise:

a) excavation and foundation conditions on natural horizons are always subject to much more "indeterminate quality assurance" (extracted from point investigations within natural heterogeneities) than a constructed embankment; and behaviors of these stress-relieved horizons tend to deteriorate with time (quantifications lacking research);

b) the qualities of constructed embankments have been systematically and greatly improved with time, while dam locations have had to accept progressively less favourable geotechnical siting.

Geotechnicians have major tasks ahead for documenting themselves with the convincing experimental and theoretical bases for countering the understandable weight of CONVENTIONAL EXPERIENCE that influences decisions by well-intentioned colleagues from hydraulics and structures.

3.7 Summary admonition.

Embankment dam costs might represent 25-40% of the global layout, and variations between good designs on the formal dam section would seldom represent more than ± 10% of the dam cost. It is not on the third-order particulars of the constitution of the dam, and much less of the analyses thereof, that hangs the survival of Dam Geotecnical Engineering. It is much more in the cooperation in layout optimizations, in which savings of 10-20% of the global project can be wrought. Embankment dams mostly lose out because of hydraulic circuits. Optimizations are unquestionably possible, long overdue, imperative. They start with the cofferdams, tightened and incorporated into the dam slopes. Successful use of reinforcings for acceptable overtopping on compacted earth and rock surfaces and downstream slopes need not be mentioned, since it constitutes an obvious complement available.

4. Key questions and proposals on foundations.

4.1 Introduction.

It would be quite absurd to attempt summarizing the problems and solutions regarding foundations, since each case is to some extent singular. The intent herein is to broach some generalizable considerations derived from our experience, dominantly on fractured sound rock riverbeds, and deeply weathered residual-saprolitic abutments, unsaturated and "porous" at the top, with low water-tables above bedrock and rising less steeply than the topography..

4.2 Gravity dam foundations.

There have been many pronouncements and some earnest discussion on establishing Rock Mechanics parameters for sliding shear. Justified worries center on the "cohesion" parameter, of great dispersion in the small scale of tests. With recommendations of FS_c of the order of 3 to 5 (on what confidence band ?) along planes assumed "fully cracked" , the frequent result is of reliance on the friction $\Delta s/\Delta\sigma$ which shows negligible dispersion, supporting acceptability of a very low $FS\varphi$. Great prudence is imperative because of the generally rigid-brittle failure with but millimeters of displacement to reach failure peak. "Cohesion" derives both from rock bridges, and merely from ASPERITIES reflecting on the linearized Mohr-Coulomb interpretation. Multistage tests have been useful to avoid dispersions between specimens, but require interpreted adjustment because of the progressive damage to cohesion with strain : as can be easily shown in comparative plots of test results, much depends, therefore, on the sequence of pressure stages.

It appears that the well-intended proposals on how to account for shear dilatancies etc.. as functions of undulations and asperities have led to confused conservatism, resulting in near-zero cohesion, expensive. Illogical (conservative) points arise in : (a) proposal of lifting-off sheared half, exposing the two sheared surfaces, for measurements (already polished) ; (b) making measurements on existing rock surfaces (already exposure-affected) ; (c) measuring statistical asperities without reference to the simultaneity of strains, and millimetric displacements.

In contraposition to the extreme-value point dispersions there is the fact that the gravity-dam's rigid body forces the averaged behavior. Therefore, so long as one is conscious about averaging, it is of interest to run many tests. And, in averaging, one must be conscious of the difference between a real low-stress cohesion, and a COHESION INTERCEPT FOR THE RANGE OF STRESSES involved in the job.

A point of importance and possible interest I have observed is that crack-asperities could not fail to reflect a significant precompression effort (and corresponding shear strength increase). Gravity dams have normal stress reductions (vertical precompressions), concomitant with the water-load horizontal shear, over the upstream part of the potential sliding plane. Should not the conventional Rock Mechanics tests be revised and/or complemented ?[6]

In many Design Criteria for dam sliding stability there is the requirement of calculating for DRAINS FULLY INOPERATIVE (attached to a guilty conscience concession of $FS \geq 1{,}00$).

The criterion is ILLOGICAL AND ABSURD (why doesn't one require a concrete beam to give FS = 1,00 with zero tensile steel ?, or an earth dam without a filter ?), and it is unpardonable to suggest compensating for one important lapse of logic (however sired) by another one.

4.3 Grouting and drainage.

My earnest attentions have been drawn to this problem since 1955, and I have voiced my convictions so often, that I could only wish to be blatantly declared wrong, and even to be partly wrong, so ashamed I am of repeating myself, lacking challenge to renewed thinking.

Water-loss tests (Lugeon, and imperative improvements thereof) are crack-opening, whereas under dams tendencies are mostly crack-compressive : pessimistic ; but forget it. The principal point is that by more conscious tests, and, "pinching-in on cracks" , test results (to be adopted at pressures as low as possible, to estimate the shift from the tensile test condition quadrant to more appropriate near-compressive, pump-out test quadrant and modest gradients) in most rocks the dispersions range across 4 to 5 log cycles. How to integrate hypothetical flownets and effects ? Nobody has yet dared to attempt : this is one main reason for my proposal of using DIRECT FLOW OBSERVATIONS (interpretable as a huge "well") from the cofferdammed area (cf. item 3.1) , for assessing benefit/cost ratios of cutting seepage losses by grouting (almost never the case in high-flow modest-head dams, but depends on comparison with alternate provision of lost water) . From field experience it is proven that once "systematic pilot" grouting starts, it

[6] *See item 4.3(e) regarding frequently erroneous forced reduction of φ because of the misnomer "clay infillings".*

tends to grow to irrational scales because of historic, never re-thought, acceptance criteria applied to "point" data progressively obtained. The terse assertions made are :

(a) If grouting is needed, to protect cracks from deterioration leading to unsafety, or to control economically excessive losses (to be increased, irrevocably, by PRESSURE-RELIEF "drainages") it has to be the first measure, because it is a "naturally selective" treatment, sealing best and farthest the widest cracks[7] .

(b) Grouting creates a "grout-buffer"of widely varied width, modestly homogenized by sealing-rejection of wider cracks, the MISNOMER "GROUT CURTAIN" being a mental stumbling-block. Employ if flows, and preferential exit gradients, risk being high.

(c) Some reduction of water losses (REDUCTION RATIO, nominal) is proven and undisputable, but control of seepage (UPLIFT AND EXIT) pressures by grouting is ineffective.

(d) Pressure-relief (drainage) holes are specific for tailored effect, and call for LOGICAL DESIGN (not geometric array), by (1) water-loss tests (2) testing of efficiency of the battery of holes upon alternating closing-off of some, and checking pressures/flows in others.

It may be emphatically postulated that very often indeed relief well curtains are both/either UNNECESSARY and/or BADLY DESIGNED . Average uplift destabilization on subhorizontal drainage blanket encounters precompressed strength benefits.

Exit gradient protection, narrowly near exit, is proven by good filters in extreme conditions of pumped water wells, dispensing going down to significant depths. If washing - through piping and/or clogging are really feared, it should be INDISPENSABLE TO HAVE ACCESS FOR MAINTENANCE (MEDIUM TO LONG-TERM).

(e) As fundamental logic of DESIGN PRINCIPLE I have recommended always using the check-question : "if my designed feature does not function, what then ?" . Well, what indeed will/can happen to a crack with weathered clayey coatings, in a gneiss or basalt ? NOTHING. Erodibility stops when coatings are removed : moreover, the coatings WERE PERMITTED TO DEVELOP because the rock stresses across the crack are carried by other contacts. Neither settlements nor strengths are affected. Note the important DIFFERENCE IN REALLY ERODIBLE ROCK MATRIX, e.g. sandstone etc. (with failures suffered).

(f) Important collateral effects must be noted regarding compression/expansion tendencies in the foundation, concentrating greater head losses and gradients in the more compressed zones (compressing cracks or pores). In this respect, embankment dams favourably concentrate highest gradients near the center, the seepage exit being progressively self-protective against exit piping. BLATANTLY DIFFERENT IS THE CASE OF CONCRETE GRAVITY DAMS (and some others) decreasing compressions at the heel upstream, and compressing at the toe DS. Honest side-by-side comparisons of concrete gravity sections with the contiguous embankments on the same rock should expose much greater, unnecessary conservatism of conventional requirements for the embankment.

[7] *Incidentally, historic "geometric" practices had frowned on foreseen grouting inefficiency if PRE-OPENED HOLES happen to be filled by tired grout arriving from a distant hole. This has suppressed collecting data on the very important parameter of grout-travel distance. To begin with, it is STRONGLY RECOMMENDED THAT WATER-TRAVEL DISTANCES BE SYSTEMATICALLY INVESTIGATED : and so also some grout-travel distances (different, comparative) even if requiring some complementary grout holes for remedy.*

4.4 Longitudinal galleries for grouting and/or relief holes.

By some unexplainable circumstance, in most dams in which grouting and pressure-relief are intensely discussed and applied, there is a tendency to neglect, or peremptorily discard, the use of such a gallery (mostly tunneled in rock, for higher dams). The positions seem to be taken on a basis of creed, religion, fundamentalist : possibly in part because of some historic aversion to tunnels, diversion tunneling notwithstanding. I strongly remonstrate against such disregard for design logic, which only persists because in a significant percentage of cases really there was no need of the grouting or relief treatments anyhow, but the above described crudity of data bypasses conscious decisions towards economy (cf.item 2.5 a).

If there are, indeed, strong enough well established reasons for needing them, meaning that their failure to perform to content may transpire or develop, it is inadmissible that neither the true effectiveness may be monitored, nor any complementation or maintenance may be applied reasonably. Quite a few dams, principally concrete which induce high seepage gradients, are recorded as having required regroutings and/or redrillings of relief holes.

In my experience a number of difficult cases would have been immeasurably more difficult, if galleries had been foregone or rejected. Regarding first-cost, it might even be favourable in cases to employ a gallery for the chance of monitored dispensing or postponing the grouting and relief curtain. At any rate, a principle cannot be abandoned on the sly : it can be openly rejected and revised, upon documentation and reasoning. I repeat, if a feature is fundamental, it must be prepared for the effective fast-enough action "if things do not happen as predicted". It is in the foundations that lie the greater unknowns and dispersions.

4.5 Upstream blankets and "surface compaction blanketing".

Some very important cases of the past twenty-five years have pointed to the critical problems of cracking and sinkhole formation across these blankets that suffer a very major CHANGE OF LOADING CONDITIONS. The problems derivable from differential settlements and/or shrinkage cracking (by early protracted exposure to inclement sun) are well understood, and do not lack the tests and computations for supporting decisions. It appears, however, that the more serious problem that awaits emphasis, recognition, and simplified support from numerical analyses by academia, concerns the TRANSIENT FLOWNET PRESSURE CONDITIONS IN THE UNDERLYING UNSATURATED HORIZON, IN CONDITIONS OF FAST RESERVOIR FILLING. Once described the condition becomes obvious.

We have had cases of reservoirs filled in a couple of weeks, at rates of 3 to 5m per day. The classical solutions for blankets tackle the permanent flownet established, whereby the seepage pressures under the blanket are but somewhat lower (desired loss of head) than above. With some significant degrees of unsaturation (especially in the "wider flood-stage valley"and in abutments) the time required for the advancing permanent underblanket flownet can be many times greater. Especially nearest the blanket-dam transition one can have full reservoir load on top, and zero water pressure below: thereupon, punching shear causes sinkholes.

Since a "natural horizon" when reworked and compacted frequently reaches permeabilities reduced to 1% or less, one convenient and cheap alternate to the superposition of a dimensioned blanket has proved preferable: to employ a surface compaction blanketing of the natural horizon, as late as possible before reservoir filling. There are collateral details, but they dispense expatiation herein.

4.6 Silting of valley bottom, and clogging of rock cracks.

Very many rivers (almost all of ours, for sure) are very coloured, heavily sediment-loaded; and monitored data on flows and piezometric heads indicate significantly benefited upstream sealing by silting in first few years. In principle there is a great difference between pre-placing a sand-silt-clay layer, and the naturally selective successive clogging by sedimenting from a reservoir body. In the latter case, Stokes' law of sedimentation ensures that the sizes reach bottom in proper succession, sands, then silts, then clays: and seepage stresses carry and press the materials into the cracks that have seepage; and the succession is appropriate for clogging against the filtering action of the coarser to finer arrivals at crack strictions. In the case of pre-placement, the progressive action often tends to be exactly opposite, finer particles eroded-out first (cf. Fig. 5, and item 5.7).

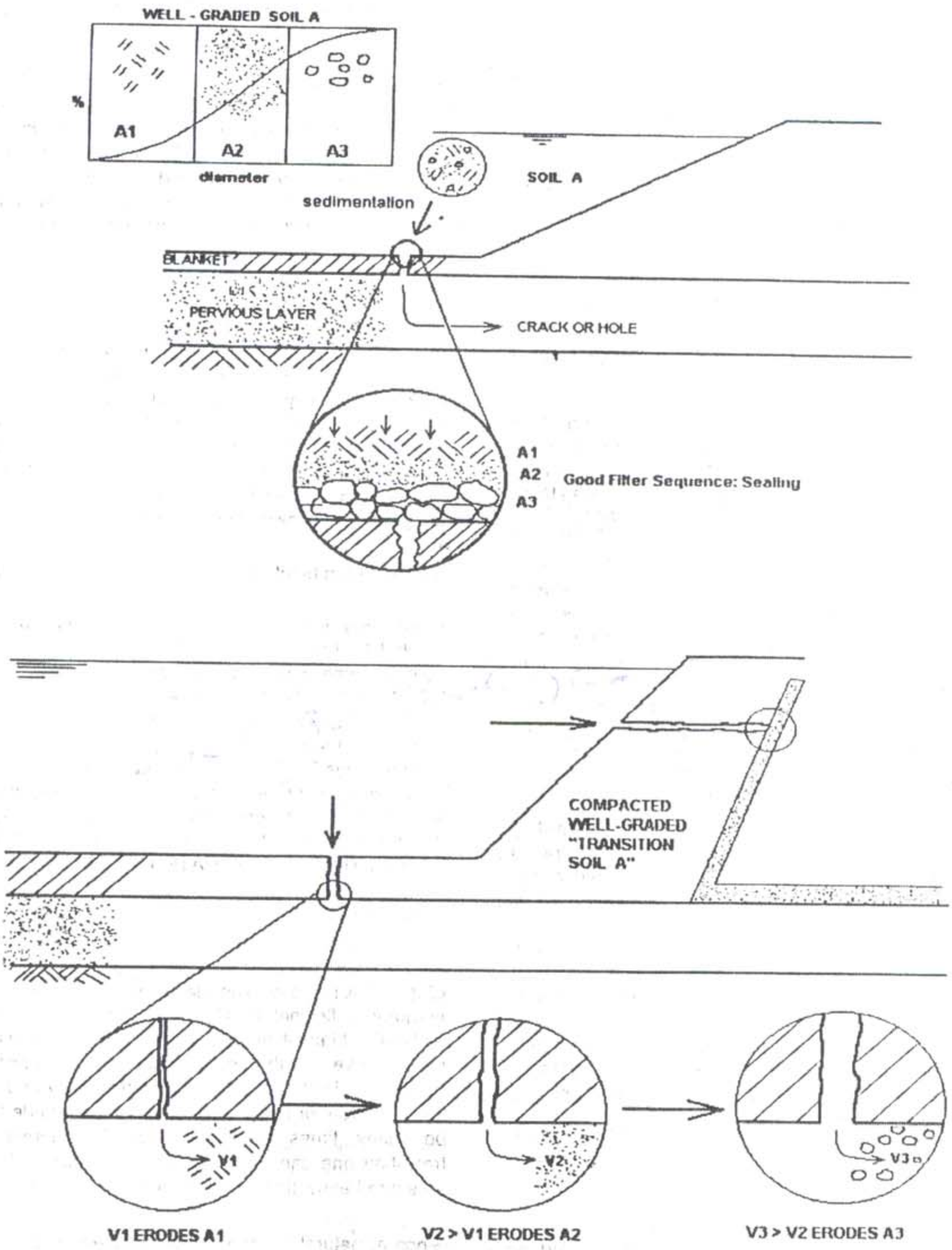


Fig. 5 - Favourable Siltation Sealing vs. Illusion of "Self-Healing" Compacted US Transition

This behavior, repeatedly observed but not yet systematically reported for statistical quantification, should be another significant consideration towards dispensing grouting.

We have had some success in dumping into reservoirs, immediately upstream of the toe, boat-loads of prepared sand-silt-clay mud. Good benefit/cost ratios can be foreseen from a systematic collection/analyses/publication of data to permit conscious quantified design decisions towards averting grouting.

5. Key questions and proposals on the dam body.

5.1 Introduction.

It is imperative to aim at FEW PRIORITY POINTS that belonged to the well-intentioned past but should be frankly debated and redefined, for conclusions to be declared aloud, for dominating project-design generalists to open the rightful latitudes for the geotechnical specialization's updated contribution.Every item has been published, scattered within frontline papers of multiple other purposes: but the selection and opinions are mine, at present. The selection of priority derives from experience of the past decade of CONSULTING BOARD decisions, and of the dearth of publications concerned with benefit/cost revised decisions, however supported by global performance components, tests, theories, analyses, and monitored prototypes.

5.2 Axis and type of dam.

There was a time, 1955-70 mostly, when a strong current, concerned with transverse cracking, favoured embankment dam arched upstream, and avoiding concave curves to upstream. Upon closer quantified examination it was definitely concluded that these intuitions were most often false, to be buried: moduli of elasticity and presumed-arch geometry, considerations of stress-strain and built-in compaction stresses, extreme-value tensile cracking reality, and principally the effective downward direction of seepage stresses towards inclined chimney filters, etc., added up towards recognition of the irrelevance.

The case of upstream bituminous-asphaltic deck compacted rockfill dams (one-time quite comprehensible for desired flexibility etc.) continues adamantly resisting merited summary rejection (very singular cases of reservoir operation excluded ?) in comparison with the concrete-face. Every project requires a concrete plant anyhow, and one questions why burden with an additional one, asphaltic, so specialized. The US rockfill slope has to be flatter (with all the respective unfavourable consequences, cf. item 3.1) because of creep.

But, most importantly, it must be emphasized that under present technology the deck is continuous and "uniform", therefore subject to eventual extreme-value cracking at any erratic location. Thus, if repairs are required, the investigation of positions is not simple, and even requires the very expensive reservoir emptying, if possible. In comparison, concrete decks have the joints as "fuses". This is another example which becomes flagrant principally when one takes conscience of the immeasurable difference between the statistics, of extremes vs. averages.

5.3 Blatant questions of geometry of the section.

Needless to say, it is never geometry, but separate component questions of stability, deformations, and seepages and effects, that must dictate. But the strong influence of visual transmission of culture cannot be overlooked, and NEEDS IMPERATIVE REJECTION BY LOGIC.

The tendency to symmetry (understandable because of gravity on free-standing bodies) must be fought persistently. Nothing is more asymmetrical than a dam, with reservoir and potential energy on one side, and responsibilities immeasurably greater to downstream. The much worse ABERRATION OF ASYMMETRY that is persistently encountered, is of the US slopes flatter than the DS one. Regarding potential destabilizations the principal arguments are of the immeasurably greater CONSEQUENCE (COST) OF RISK of a DS failure with full reservoir (cf. item 5.6).

How did the false notions arise ? It seems easily interpretable, as forthwith over-simply summarized. In rockfills (historically dumped and impacted by high-pressure water monitors) the strength reduction by wetting and high-stress contact softening increased the already high deformabilities to worrisome levels : incidentally, the index of unconfined compression strength ratio, immersed vs. dry, should be UNFAILINGLY OBTAINED, AND DATA PUBLISHED/ANALYSED for statistically documented progress. In the cases of homogeneous embankments, the historic roots lie in early less-advised visions of flownets, "softening by water", and rapid drawdown RDD analyses (cf. items 5.5, 5.6) before optimizations of inclined chimney filter positions as discussed twenty years ago.

Another presumed controversy should be dispelled forthwith IN PRINCIPLE, different singular conditions respected. For narrow-core high well-compacted earth-rock dams, we may dub it the choice between vertical-central and upstream-inclined cores. Infiernillo, of merited success and fame, belongs to the vertical-central group, with exaggeratedly wide "transitions"[8]. But tendencies to hang-up in the upper 20m (±) are undisputably recognized in analogous cases.

Hang-up effects are to be definitely avoided, especially in reservoirs that cannot be rapidly drawn down, and because they are nevralgically dependent on extreme value statistics (cf. item 5.7).Therefore, avoid differentiated compressibility materials on subvertical interfaces of high shear resistance. The case against upstream-inclined cores derived principally from RDD destabilization hypotheses that have been superseded; and this problem is demonstrably much less conditioning in optimizations. Therefore, in summary, up-dated practice should strongly favour upstream inclined cores.

5.4 Some updating on intervening material properties.

The profession has been suffering from the near exclusivity of laboratory vs. field tests, and, even in the laboratory tests, from the inertia of some routines in lieu of some challenging hypotheses regarding better representation of suspected field realities.

For instance, on rockfills I have postulated the absolute need for field slope destabilization check tests, because laboratory shear tests cannot reproduce field crushed-contact stresses, and densities can never reach the precisions to reflect degrees of such crushing, of great importance both to precompression moduli and to overcompressed shear strengths.

Regarding compacted clayey materials, since 20 years ago I have emphasized the difference (right ?, or wrong ?) between laboratory compaction within a rigid mould, and field conditions approaching

[8] *In truth the transitioning should be on all three important counts: but when best for filter-drainage, and strength, is fully recognized as most unfavourable on compressibility and consequent core hang-up (silo effect).Many a similar case chanced not transposing the limits of impunity by second-order factors of a posteriori logic. They do not, however, swerve from the principle sought.*

bearing capacity. It has nothing to do with exclusions of coarser sizes, because even block-sample sizes and its components, are insignificant compared with prototype shear surface dimensions and ability to choose its "conditioning matrix".

Based on block-sample conventional tests, average first-approximation correlations (N.B. averages applicable) were given for the two fundamental oedometric compressibility parameters, precompression pressure σ'_p and compression index Cc, as follows: PC% $\approx$ 94.38 + 6.43 log σ'_p (PC= percent compaction), Cc $\approx$ 0.23 (2.55 - $\gamma_{d\ max}$). Have colleagues attempted to check, reject, revise? How would triaxial tests compare ? How do dam settlements compare, interpreted analogously with precompression included ?

Regarding the all-important slope-conditioning shear strengths, even from block samples, have any minimum series of comparative tests been run, for instance: a) conventional, consolidated directly to the σ'_3 (below σ'_p); b) first reconsolidated to σ'_p (presumed in situ stresses) for some time, to remedy sampling-trimming adulterations in the rigid specimen, and then returning to the desired σ'_3, etc..?

Thirdly, regarding the decreasing permeabilities with compressions (settlements) in higher clayey dams, an estimate was proposed of roughly (1.0 to 1.5) log cycle decrease of permeability for one log cycle of increased overburden pressure. Thus, a "homogeneously compacted" earth dam ipso facto becomes consistently unhomogeneous with depth: this affects flownets significantly. Have there been complementary data analyses along these lines ?

From any/all such special test series and comparisons, the first aim is to extract the approximate ADJUSTMENT FACTORS APPLICABLE TO CONVENTIONAL TESTS AND ROUTINES, for assessing degree of relevance. But routine professional practice doesn't have the time/ budget (and courage to confront authority ?), and academia would appear insensitive (and partly unaware ?) to such field realities that could signify so great an integrated economic benefit to the world.

Having introduced, in 1951, the concern on "seepage saturating" test specimens for dams, I have been progressively more perturbed at the routines automatically adopted worldwide of back-pressure fully saturated tests and theories, completely inapplicable to modest-height dams. In Figs. 6 (a), (b) and (c) I present as mere examples the typical sets of data on the Skempton-Bishop parameters A,B,B for pore-pressure development. The very high back pressures required for real saturation (B=1.00) have long since been recognized. The test-routine for checking progressive changes of B with increasing back-pressure was started about 20 years ago, and will be available for closer analyses, with anticipated relation to porosimetries. The effects on RDD destabilizations are discussed in item 5.6 : for that purpose, in view of Bishop's stated preference of the parameter B, in terms of the assumed $\Delta\sigma_1$ caused by drawdown, in Fig. 5(c) the data from the same tests are so represented.

5.5 Flownets revised seeking better representation of reality.

Once a theory appears good (and more profoundly difficult to query) the efforts have the bias of trying to confirm. Meanwhile, prototype instrumentation and monitoring is always too limited for proof, much more so for disproof, for clayey dams, because of multiple parameters (especially time-lags, dam-body and instrument) and point observations, to be integrated. So, hypotheses, theses, theories advance by hunches.

The most documented variation has been obvious, anisotropy: possibly much underestimated (conventional 9:1) in lower dams, and overestimated in the higher ones, in the lower highly compressed part. In lower dams, "homogeneous" with practically no recompression below the compaction precompression residual stress, anisotropy must be high. Twenty-three years ago, no finite element refinements, I tried crudely assessing the consequence of a loose layer across the entire US body up to the vertical filter: overall influence seemed to be small, but nearby noticeable influences suggest pursuing with some systematic thesis (B.Sc.?) for helping interpretations of prototype piezometer monitoring. Purposeful checking in prototypes should follow for minimal confirmations.

On the other hand, in the bottom zone of higher dams one should expect a reduction of the anisotropy because of the tendency of homogenization by higher compressions, the looser layers tending to compress more than the denser ones.

To my knowledge, the only other theoretical intuition was the one presented in my Rankine Lecture twenty years ago, incorporating the inexorable fact that permeability decreases with increased compression. It was shown that the full reservoir flownet rises considerably. Data from some higher earth-core dams have been reported since, confirming the tendency for much (most) of the loss of head to occur within a narrow band just upstream of the filter.

There are a number of REPRESENTATIVE FLOWNETS openly requesting some M.Sc. theses. Firstly the RDD flownet revised for the varying permeability with depth: presumably the effects of deep "full drawdown" conditions should be greatly attenuated. Important for economies in many a design analysis. Secondly, the IDEALIZED ANALYSES OF HYPOTHETICAL CRACKS : how do the fearful references to cracks persist over forty-five years, without query and revision by refinements of numerical and well-planned test analyses? For sake of simplicity the flownets of Fig. 7 used for the stability analyses have not incorporated this refinement. (cf. item 5.7).

5.6 Dominant destabilizing analyses.

The subject has been tackled often enough, and merits being restricted to principal items of controversy that are seen to persist, doubtless because of the great difficulties at checking in clear-cut prototype cases. Some instances of great practical significance could be:

(1) Regarding compacted angular rockfill slope destabilizations. Firstly there is the important complementary strength due to compaction precompression pressure, which seems well enough accepted: it is clearly revealed by adequate settlement Δh vs. log $\Delta\sigma$ data plots of high dams, interpreted analogous to oedometer tests, but with the care to derive $\Delta\sigma$ at least from elastic stress transmission INFLUENCE FACTORS OF THE SUPERPOSED TRAPEZOIDAL LOADINGS, $I\gamma Z$, or from finite element analyses.

Moreover, I postulate that in the event of started sliding displacement, in such granular MATERIALS OF INSTANTLY-ADJUSTED σ', as weight over slide surface changes (reduces in upper part and sometimes increases over lower part) the realistic shear strength in the upper part should reflect some of the DECOMPRESSION HYSTERESIS (therefore, "overconsolidated envelope") compensating for "strain-softening".Otherwise it seems difficult to understand the often-observed stopping of slide descent to a long-standing stable position slightly lower. Conventional tests (even so rarely run) that never decrease σ' during shear are only one hypothesis, and must be comparatively complemented.

Incidentally, in "progressive failure analyses", Bishop's proposed Brittleness Index I_B related to peak and residual data were submitted, 20 years ago, as meriting revision, to my proposed RATE OF CHANGE OF STRESS-STRAIN SLOPE WITH STRAIN: profitable? or discardable ?

(2) Debate is invited on the postulation that it is QUITE ILLOGICAL to achieve adequate back-calculations of rapid (and even violent catastrophic!) mass slides by recourse to RESIDUAL φ' values. It is intrinsic to the concept of φ'_{res} that it lowers very gradually asymptotically. The thesis arises from needing to compensate one gross error by another.

(3) Potential rapid sliding analyses, by EFFECTIVE STRESSES vs. by TOTAL STRESS PRINCIPLES, DULY REVISED.

One cannot expatiate on such a memorable and fundamental milestone as the Boulder 1960 victory of the "effective stress school", that gathered as many deep wounds as ostentatious laurels. The fact that ALL GEOTECHNICAL BEHAVIOURS DEPEND ON EFFECTIVE STRESS really should not have needed pseudo-proof by estimated back-figured failure case histories: it was really a question of unchallengeable basic definition and decision.

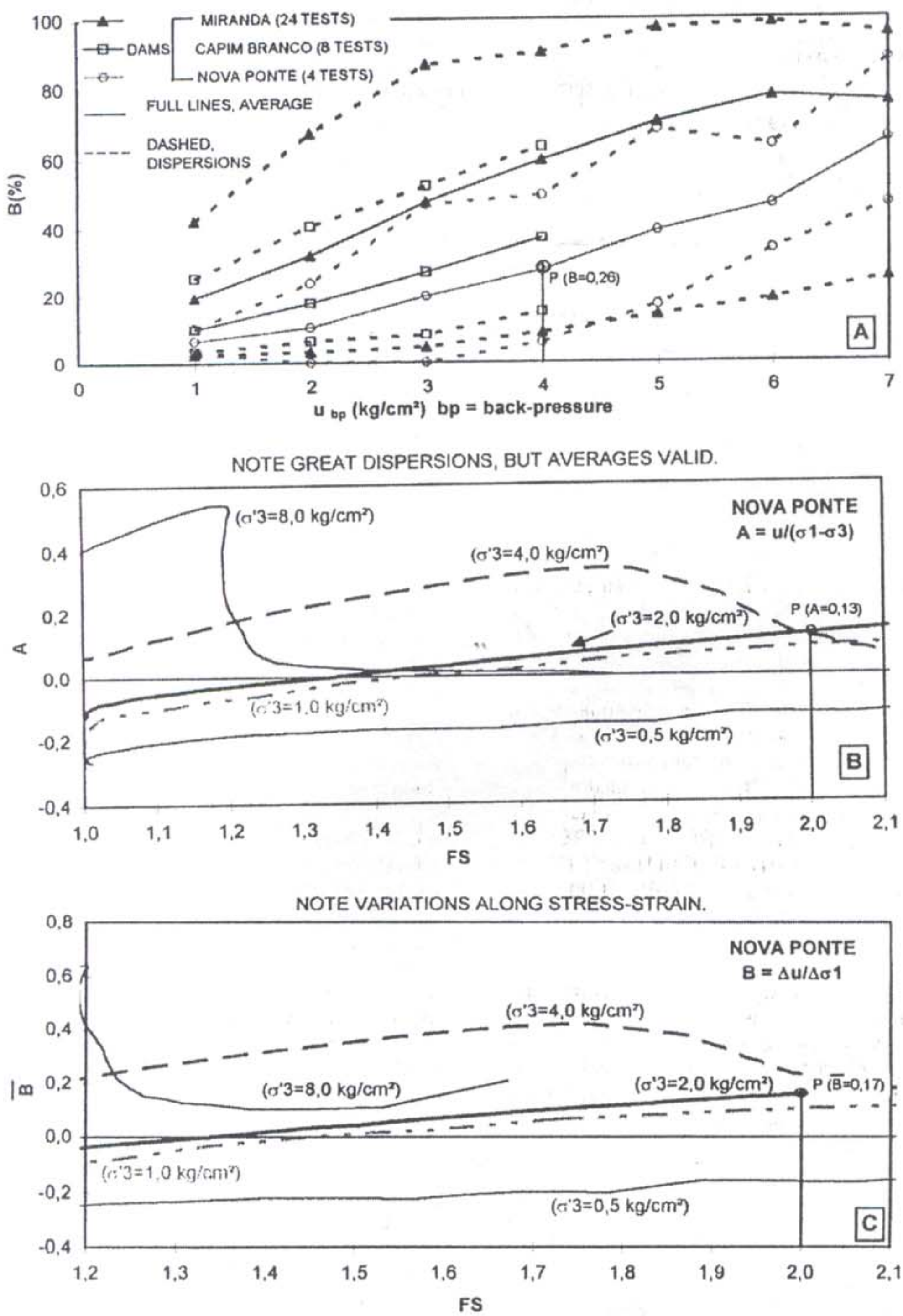


FIG. 6 - Principal Pore Pressure Parameters for analyses, examples from three dams.

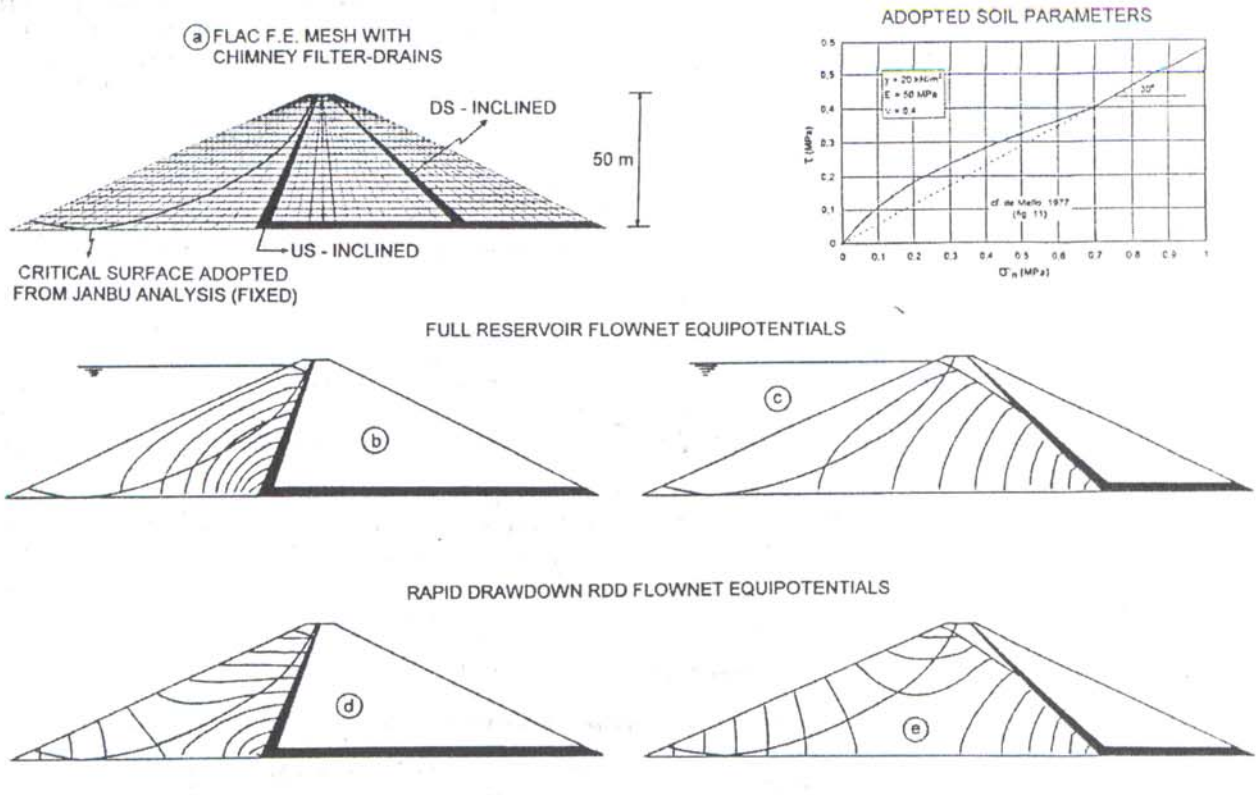


FIG. 7 - BASES OF US - SLOPE DESTABILIZATION ANALYSES.

The battle and victory were needed as adolescent self-assertion, supported on "definite knowledge", dispensing the humility of letting the test-specimen incorporate the vagaries of conditioning behavior: forefront academic publication can and must continue to heed the call to ultimate knowledge, but how can professional designs assume such postures? Three unfortunate serious consequences to geotechnical design were: 1) the elimination of the indispensable distinction between a pre-failure "preparation state (anisotropically consolidated) of the soil element" and the "triggering-to-failure", and, thereby, the stress-strain-time concept of prior-to-posterior states; 2) the assumption that you CAN KNOW THE PORE PRESSURES AT THE FAILURE PLANE AT THE INSTANT OF FAILURE[9] ; (3) the hypothesis that at failure the sliding mass IS AT FS≡ 1,00 , and NOT PASSING THROUGH FS= 1,00 IN GOING FROM FS inic TO FS final.

However, three to two decades ago I was content with comparing two successive stability analyses, that is, obtaining the desired destabilization conclusion VIA LUMPED COMPLEX RESULT (a step forward, no doubt, from the still dominant practice of calculating each single FS result). On further reasoning, and counting on the intervening facilities of finite element computations at determining "all" component parameters, I now propose that the ΔFS on the erstwhile estimated critical plane(s) is BETTER COMPUTED VIA CHANGES OF THE CAUSATIVE FACTORS $\Delta\sigma$, Δu, Δs, $\Delta\tau$ (and even possibly Δ strain). In principle, this procedure permits much better assessment of relative importance of CONSEQUENTIAL PARAMETERS, INSTRUMENTATION/MONITORING etc.

In order to exemplify the extent of the importance of my proposed method of sequential analyses of "preparation condition" and "triggered destabilization" I submit a hypothetical case of a 50m high dam. Note, as a start, that I postulate that there is an ILLOGICAL GAP between the TESTING PRINCIPLES OF STRESS-STRAIN-TIME PATH, and the independent SPOT ANALYSES OF SLIDING STATICS. The principles of the statics, different failure surfaces, upper and lower bounds, kinematic viability, etc.., have been exhaustively analysed and established by a great number of most authoritative colleagues. Nothing regarding those results is believed to merit even distant concern at this moment, in comparison with the BASIC GEOTECHNICAL POSTULATES OF THE IMPORTANCE OF PRIOR TO POSTERIOR CONDITIONS, change of conditions.

This stated hypothesis of mine is earnestly submitted to discussion, under desired accept/reject risk : unfortunately it will never be easy or cheap to test it in prototype, and therefore it will never move forward unless, as a start, it proves capable of validation by accepted numerical analyses. For the effective stress strength envelope, with the benefits of preconsolidation pressure, I repeat the use of the unquestionable Harvard Soil Mechanics Series published data, as in my Rankine Lecture 22 years ago. And for the pore pressure parameters and values I employ those compatibly extractable from Fig. 6 : the values used are as stated in Figs. 7-10, being of reasonable orders of magnitude, but only of secondary interest because they are used for direct comparisons of alternates.

Fig. 7(a) presents the mesh used in program FLAC, the position of the assumed sliding circle (selected as critical for one condition, but

[9] *It is, in my observation, experience and opinion (herewith submitted to debate), the failure to recognize the significantly higher transient excess pore-pressures AT THE SURFACE/INSTANT OF FAILURE, that has led to such frequent back-analyses of failures to resort to strangely reduced φ' values. On overcompaction slickensides I have observed THIN FILMS OF FREE WATER, and well conceive of a skidding possibility as by skies on snow. At any rate, the important recognition is that "deformations are the reality, stresses are conjectures", and it is not the rate of loading but the RATE OF SHEAR AND VOLUME STRAIN AT FAILURE SURFACE that dictates the conditioning excess pore pressures; and geotechnique hasn't yet advanced into evaluations of such rates.*

then maintained fixed for convenience of comparison) and two hypothetical positions of chimney filters. Figs. 7(b) (c) (e) present four flownets, full reservoir and total RDD, two cases each for the two extreme filter positions, inclined upstream and inclined downstream.

In Figs. 8,10 I present a summary of the procedures used for DESTABILIZATION ANALYSES, and especially emphasize the important difference of concept of the two approaches. In the conventional analyses (e.g.Janbu, et al.) the stress changes that take place above the pre-failure surface are taken as APPLIED DIRECTLY AND ONLY, THROUGH THAT "ISOLATED RIGID BODY", ONTO THE SLIDING PLANE. In the analyses (comfortably prefailure) that adopt changes of stresses in the ELASTIC CONTINUUM [10] as they would develop on the IMAGINARY SEPARATING SURFACE, the stresses, both of overburden and flownets, are TRANSMITTED THROUGH THE ENTIRE BODY. It seems firmly postulatable that this second approach is more valid, presently the ONLY ONE VALID.

Fig. 9 presents the succession of FS values by the two comparative concepts of ΔFS calculations,Fig. 9(a) vs. 9(b), and including the recognized important phases, for construction period nearing end-of-construction, succeeded by full-reservoir, and RDD. Regarding the construction period I have to note that my design recommendation has been to check the rate of change of FS with the final steps of rise of the fill, because of the tendency for the B parameter, rate of change of u with $\Delta\sigma_3$, to steepen with increased tendency to compress : conditions of eventual rapid drop of FS should call for greater attention to risk. The three points A, B, C, corresponding to 70%, 85%, and 100% of fill height are calculated with due incorporation of the estimated negative pore pressures (suctions) below 4kg/cm^2 (related to the compaction precompression) : however, since historically almost no dam has measured, or incorporates, suction values, the comparative points A', B', C', have been calculated asuming u = 0 up to the start of the rise of positive u values with $\sigma_3 = \sigma_1$. The difference is seen not to be important, but the feared end-of-construction destabilization is seen to be clearly remote (barring conditions of excessive pore pressures on slickensided planes).

Two basic principles and one very basic acceptance criterion are strongly emphasized. The FIRST BASIC principle is that, as a start, one can/must locate by the best possible conventional-proven static stability analysis the close-enough-to critical failure surface as the rising dam starts nearing completion. The Janbu static stability analysis has been used herein, and with the simple idealized near circular surface (never real, but herein adopted in order not to divert attention from the main goal). As usual, in practice several critical surfaces will be checked. Thereupon, the SECOND BASIC principle is that, as stated above, changes of conditions are much better calculated, with lesser global errors and dispersions of hypotheses, parameters, etc..., NOT BY SUBTRACTING FS_2 from FS_1 via two independent stability analyses, but by accepting that ALL INCREMENTAL CONDITIONS OF STRESSES, STRAINS, AND POTENTIAL DESTABILIZATIONS should be calculated via accepted $\Delta\sigma'$, Δs and $\Delta\tau$ calculations on the critical surface. Different constitutive models may be chosen at will to represent the dam-body continuum: in our present case we have adopted the elastic medium hypothesis, assuming that a satisfactory dam structure should generally have a good enough FS to permit that hypothesis. The BASIC ACCEPTANCE CRITERION OF DESIGN should be that the calculated changes of conditions should either result favourable, or inconsequential, or, at worst, only modestly to moderately unfavourable: an accelerating condition of increasing destabilization implies a DEFECTIVE DESIGN CHOICE, and should preferably be altered (if possible).Especially as regards destabilizations of the downstream slope and mass of the dam, the catastrophic consequences of risk make it imperative that there should be NO INCREMENTAL DESTABILIZATION ON IMPOUNDING. Thereby there is, to downstream, no "probability"of failure, risk becomes indeed ZERO, one deals with what I have repeatedly attempted to familiarize as concept and term, as FACTOR OF GUARANTEE, in lieu of factor of safety.

As stated above, the first point, A, for construction period stability (at 70%H) was established (for the critical surface of conventional stability analysis with full height derived by the Janbu analysis). Thereupon, subsequent conditions were established independently by the two procedures. Since nothing is ever fully equivalent, it is not surprising that the results by the reasonings/methods are different, both accompanying very reasonable trends. One first CHALLENGING QUESTION I SUBMIT TO DEBATE IS ON SUPPORT, OR NOT, OF MY PRESENT PREFERENCE, for ΔFS based on $\Delta\sigma$, Δu, Δs, $\Delta\tau$.

The second point to note is the very favourable effect of the US-inclined filter compared to the DS - inclined one. Most especially the immeasurable safety effect to DS - mass failure, via Factor of Guarantee, because full reservoir flownet does not introduce any flownet pore-pressure to the DS critical plane[11].

Thirdly, special mention is made to the method I proposed twenty years ago (Rankine Lecture) for the RDD destabilization analysis, as "corrective"[12] of the proposal made by Bishop (most dearly respected) and to the two method/results FS_B and FS_{VM}. The previous method in essence 1) assumes full saturation B=1.00, etc., unrealistic and most unfavourable to economy on well-compacted lower dams; 2) does not incorporate any influence of optimized positioning of filter-drainage (notably chimney-filter); 3) assumes that the hydrodynamic pressure changes towards RDD flownet do not develop, by a dichotomic distinction (already in Terzaghi and Terzaghi-Peck) between "draining materials of low compressibility"and "low permeability fills characteristically compressible", a yes-no confused dichotomy quite inapplicable in reality, theoretical and professional. My proposal was that 1) for ALL MATERIALS, INDISCRIMINATINGLY, the quantifiable unsaturations and compressibilities can yield B,A,B parameters (cf.Fig.6); (2) despite the unsaturation (roughly 15%-5%) the LAPLACE EQUATIONS are forced by the "instantaneously changed boundary conditions of pressures" to lead to the prevailing of a TENDENCY TO DEVELOP THE INSTANTANEOUS CHANGE from one flownet to the prospective next, particularly in the pores of sufficiently continuous saturation (not occluded); (3) under the prospective changes of total stresses and hydrodynamic pressures, as a start the tendencies to volume changes generate the excess pore-pressures Δu (via B,A,B parameters) so that the prospective effective stresses of the RDD flownet are not (in the general case) "instantaneously made effective"; (4) thus the final RDD stability is given by the WORST OF THE TWO CONDITIONS, either the instantaneous $U_h + \Delta u$ pore pressures, or the long-term RDD U_h if the Δu chances to turn NEGATIVE implying the rheological

[10] *N.B. eventually adaptable to elastic of varying elasticities, elasto-plastic, etc.., at will.*

[11] *If the DS mass is purposely compacted somewhat wetter, as I recommend, to generate higher B and construction - period pore pressures, the inevitable increase of strength with time by consolidation incorporates a significant increase of Factor of Guarantee, under unchanged destabilizing acting stresses.*

[12] *Note that Prof. A.W.Bishop had been most noble to have declared (in ICOLD, Edinburgh, 1964, V p.318) "About ten years ago I was rash enough to suggest a method of predicting theoretically the magnitude of pore pressures on rapid drawdown".*

General Assumptions

1) Shear Strength (fig.7)

Note: In professional work recommend anisotropic-consolidated-undrained-envelope for FS < 1,3 cf. item 5.6 (3) + footnote.

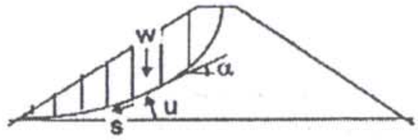
2) Pore Pressures

2.1) Construction (A, B, C; fig.9a, b) data from Osório Dam (de Mello, 1973).

2.2) Full reservoir (D - fig.9a, b). Flownets - fig.7.

A) Static limit equilibrium analysis (SLEA)

A.1) FS - Janbu's slices method (Janbu, 1953)



A.2) Rapid drawdown pore pressures - u_{RDD} (E, fig.9a)

A.1.1) $u_{RDD} = u_{FN} + \Delta u$ (see E, fig.9a).

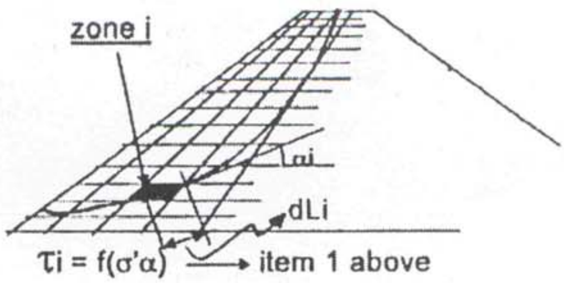
u_{FN} - flownets (fig.7) $\Delta u = 0$ (adopted, as per general practice)

A.1.2) cf. Bishop - see E_B (fig.9a).

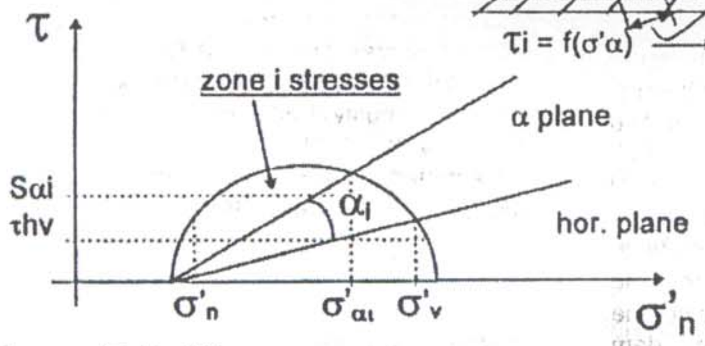
$$u_{RDD} = \underbrace{u_{FR}}_{\text{fig.7}} + \underbrace{\bar{B}}_{\text{fig.6}} \times \underbrace{\Delta\sigma 1_{(FR \rightarrow RDD)}}_{\Delta s_v = \text{total vertical stresses}}$$

B) Finite differences analysis (F.D.A.) of elastic medium

B.1) Software - FLAC



B.2) $$FS = \frac{\sum \tau i \times dLi}{\sum S\alpha i \times dLi}$$



B.3) u_{RDD} (E, fig.9b)

$u_{RDD} = u_{FN} + \Delta u$

u_{FN} = rapid drawdown flownets (fig.7)

$\Delta u = B.\Delta\sigma_3 + A.B.\Delta(\sigma 1 - \sigma 3) \longrightarrow$ A, B from fig. 6 and stresses from FLAC.

Fig. 8 - Details of calculation steps, example for 1 point.

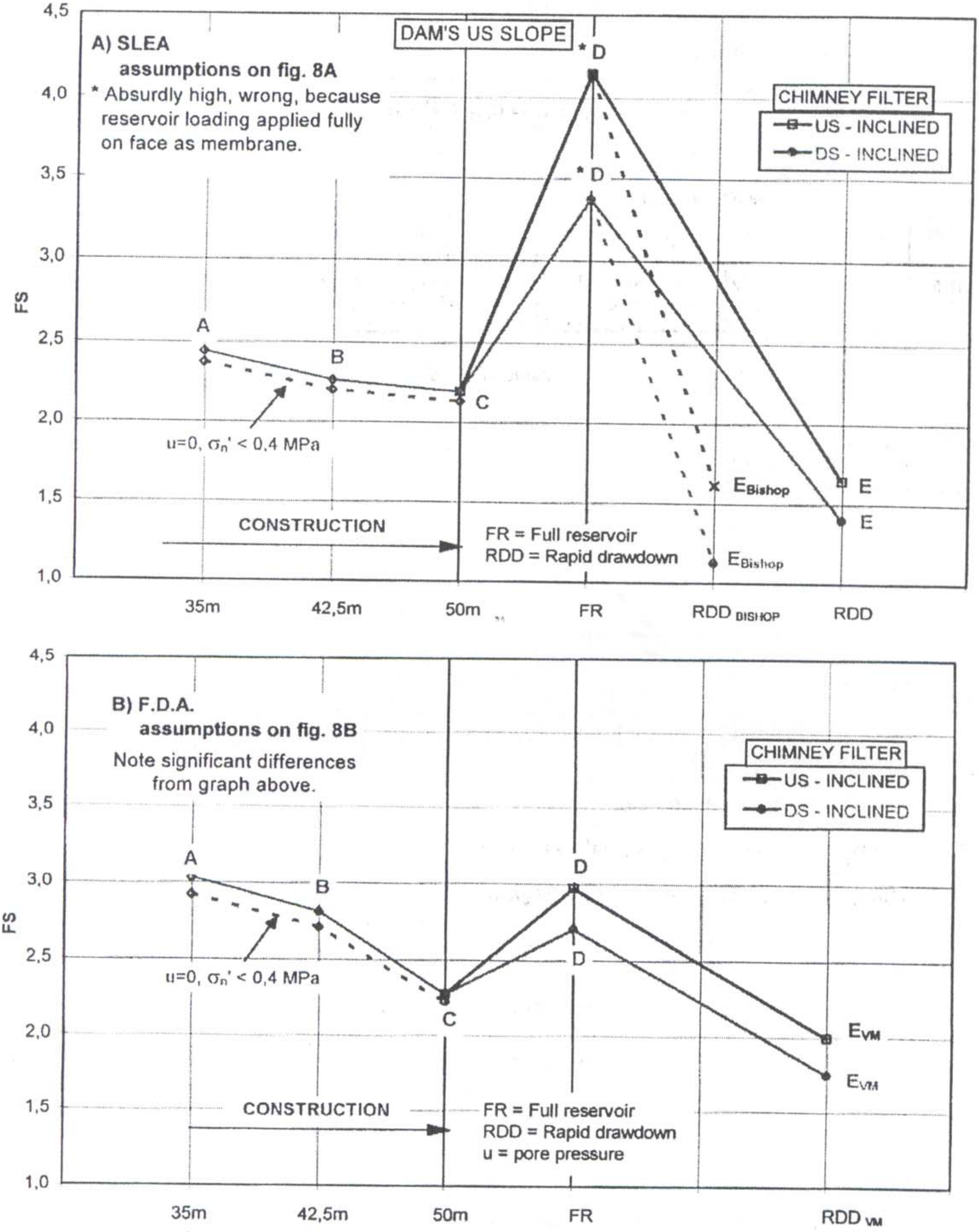
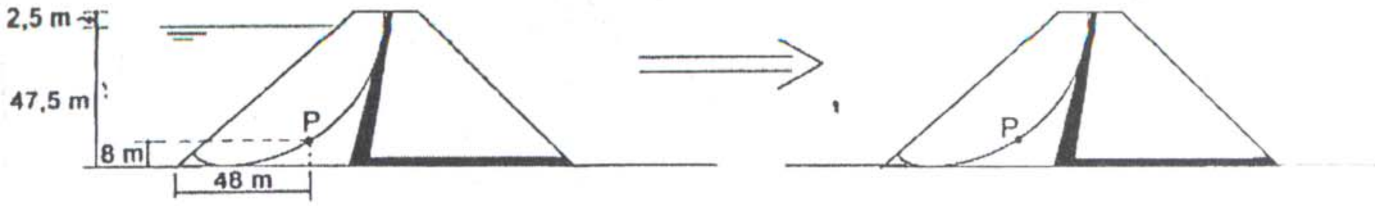


Fig. 9 - Successive destabilizations, FS values. In F.D.A. analyses (PROPOSED) calculations via Δstress values from prior condition.

US - Slope destabilization: pore presssures (u_{RDD}) from upstream inclined chimney filter-drain.

a) Adapted from Bishop.

$$\boxed{u_{RDD(B)} \approx u_{FR} + \bar{B}\,\Delta\sigma_1}$$



Values in KPa

$u_{FR} = 402$
$\sigma_V = 714$, $\sigma_h = 608$, $\tau_{hv} = 46.5$ → $\sigma_1 = 731$, $\sigma_3 = 590$, $\sigma_1 - \sigma_3 = 141$

$u_{RDD} = 150$
$\sigma_V = 476$, $\sigma_h = 373$, $\tau_{hv} = 127$ → $\sigma_1 = 561$, $\sigma_3 = 288$, $\sigma_1 - \sigma_3 = 373$

$$u_{RDD(B)} = 402 + (561 - 731) = 232$$

b) Proposal

$$\boxed{u_{RDD(VM)} \approx u_{RDD} + \Delta u}$$

$\Delta u = B.\Delta\sigma_3 + A.B.\Delta(\sigma_1 - \sigma_3)$

from fig.6a $\rightarrow u_{FR} = 402 \cong 4{,}0\ kg/cm^2 \rightarrow B = 0{,}26$

from fig.6b $\rightarrow \sigma_3' = 590 - 402 = 188 \cong 2{,}0\ kg/cm^2$
FS = 2,0 (iterative calculation) $\Rightarrow A = 0{,}13$

$\Delta u = 0{,}26 \times (288 - 590) + 0{,}13 \times 0{,}26 \times (273 - 141) = -74$ (-ve for lower part, not general)

$\therefore u_{RDD(VM)} = 150 - 74 = 76$

FIG. 10 - Summary indication of calculations used, approximate, for any point P.

tendency of the soil to swell, therefore soften[13].

The data on initial established, and ultimate to-be-established, stresses are all extracted from the FLAC analyses (assumed elastic behavior), being coupled with the Δu excess-pore-pressures, as already explained, in the "instantaneous condition" via the B,A,B coefficients most applicable for the respective ranges of stress changes. For the desired elucidation of this more complex iterative computational procedure HEREIN PROPOSED AS BASIC FOR IMPROVED REALISM, the procedures that pertain to each computational step are given in Fig.10.

The point is that a great majority of dams (the critical US failable surfaces being much shallower than the dam height) the final effect is of DEMONSTRATING THAT THE RDD US-STABILITY IS MUCH HIGHER THAN HITHERTO DERIVED by the well-intended erstwhile authoritative suggestions, especially if the chimney filter is optimizedly designed (US-inclined) and accounted-for. Don't prospective great cumulative economies warrant a SPECIAL RESEARCH-TEST ON A NEAR PROTOTYPE CASE ? Do we feign to test only if/when/what is easy, and not what, however difficult, has high prospective benefit/cost ratios to humanity through our profession ?

Finally, a furtive reintroduction of compaction residual stresses. In Fig. 11 I venture to submit for mere comparison one case of the construction-period analysis carried out with the inclusion of some nominal values of compaction residual stresses and suctions. It had been an earnest postulation in my efforts 25 years ago (summarily put forth in the Rankine Lecture): but neither were the computer programs amenable, nor were there any supporting thoughts or tests. The analysis required special incorporations in the FLAC program (as noted in Fig. 11). An effect is inevitable. The subject merits much more detailed treatment, elsewhere. The benefit to stability (and economy) should invite interest.

5.7 Hydraulic fracturing; piping and filters; self-healing of cracks; dispersivity.

In some respects these topics must be grouped together. They have been associated with uncontested failures, some serious; being connected with localized preferential behaviors deep within the impervious-core, and being ipso-facto DESTRUCTIVE OF ALL CAUSE-EFFECT EVIDENCE; and also, being mostly interpretable as connected with EXTREME VALUE STATISTICS; the fact is that they lent themselves to a period of persistent authoritative postulations of fearsome and (I think) illogical intuitions.Systematic mental, theoretical, and experimental research is sorely needed for straightening out misconceptions: I shall attempt to provoke some mental challenging.

(1) Hydraulic cracking. Very briefly, the theory in published cases has been that if a plane (dominantly horizontal) reaches ZERO EFFECTIVE STRESS in conventional geostatic finite element analyses coupled with full-reservoir flownet, the dam's anticipated performance is unacceptable because of likelihood of hydraulic cracking, reasonably considered very dangerous. I myself followed such predicates 25 years ago (ICOLD, Madrid 1973). Soon after, I began systematically to submit earnest remonstrations that the THESIS WAS FALLACIOUS, excessively simplified. Regarding the DAM'S BODY and STRESSES three important questionings were : (1) assuming the vertical stresses on horizontal planes as resembling σ_1, the lower, critical, stresses for comparison should be the σ_2 or σ_3 (the σ_2 on transverse vertical US - DS planes, VERY SELDOM ANALYSED, being the main concern) ; (2) there has not been any incorporation, justly considered indispensable, of the compaction residual stresses, which should bear relation to the compaction preconsolidations, high enough to be dominant down to significant depths ; (3) there seems to be ample evidence from narrow-core dams that unless there is crack propagation, it doesn't take more than a really narrow strip to dissipate flownet stresses adequately.

Very importantly, therefore, one should mentalize the process of CRACK PROPAGATION, and thereby courageously submit to discussion the postulate : zero effective stress is a NECESSARY BUT NOT SUFFICIENT CONDITION, the flow-affected crack only progressing if INFLOW >> OUTFLOW, the inflow rate at US is greater than can flow out by seepage through the semi-impervious medium. Fig.12 submits sketches illustrating the concept. Out of any crack, with higher hydrodynamic pore pressure from US than the surrounding porous medium, there is a certain rate of outflow (by well-behaved flownet) : if the INFLOW IS RAPIDLY INCREASED, faster than the flownet outflow can accomodate the necessary complementary flow release, then the crack increases BY JACKING ACTION[14].

Thus, the most important conclusion is that the RATE OF RISE OF RESERVOIR LEVEL IN THE UPPER DAM ZONE should be the determining factor : and it REQUIRES OPEN-MINDED REFLECTION AND RESEARCH to dispell myths. An important favourable corollary should derive, that cracks developed in the downstream zone of the core SHOULD NOT PROGRESS BY CRACK PROPAGATION (the crack there acting as drain), although the danger of crack widening by erosion persists and INCREASES BY SHORTENING OF PATH FOR HEAD DISSIPATION.

Effective formulations, presently quite possible, have been stifled by mystifications.

(2) Piping and filters.There are many instances of a good pragmatic solution obstructing most unfavourably the formulation of the desired LOGICAL APPROACH CONDUCIVE TO SYSTEMATIC PROGRESS and incorporation of apparent unconventional soils. The Terzaghi-Bertram filter criteria resulted in SO EFFECTIVE AN INDIRECT ANSWER TO PIPING, by stereometric hindrance of base-particle movement by FILTER GRAINSIZES, that fifty years have gone by without reformulation. Wash-throughs and cloggings have occurred often enough, but not heeded as intrinsic to an insufficiently logical approach.

My own professional problem 46 years ago, of mixing fine yellow sands from aeolic sandtones with crushed black fines from crushed basalts, to attempt composing the CONTINUOUS NON-UNIFORM FILTER GRADATION, led to the pragmatic method of ASSESSING GAP-GRADING, by arbitrary subdivisions of any grainsize curve into two hypothetical parts : then, to check if the coarse fraction fitted the nominal filter criteria of the finer part. An obviously LAME ON-THE-SPOT ENGINEERING SOLUTION, which, in mid- 1975 lectured at Berkeley and published in Durban, was passed-along to the wider public by Sherard.

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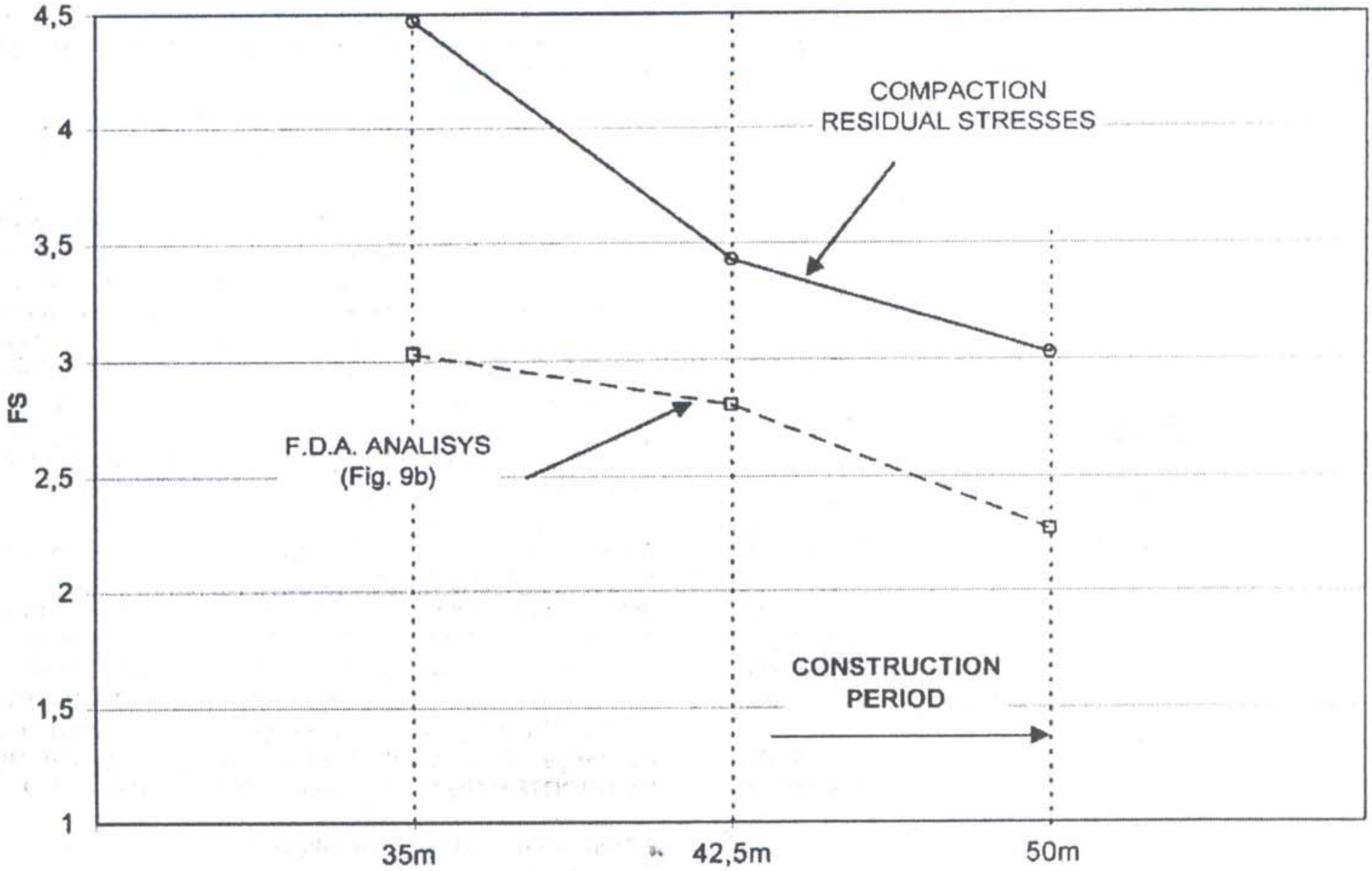
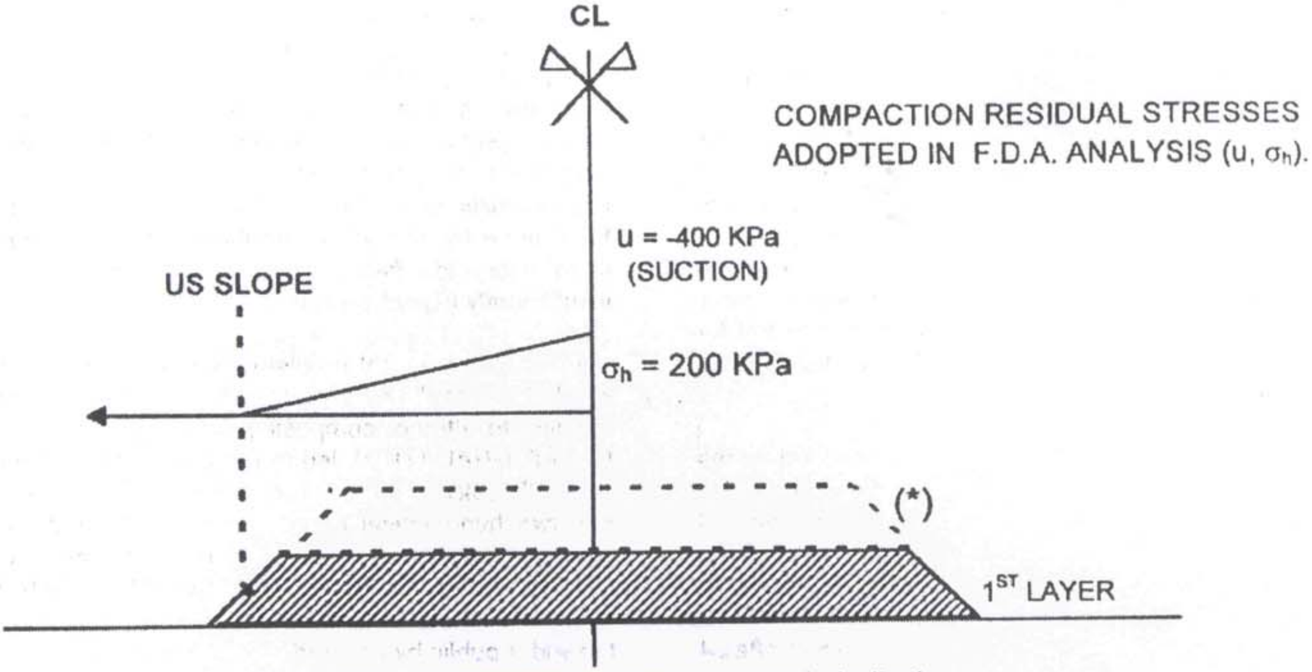
[13] *If such conditions occur, under more frequent operational partial drawdowns, their cumulative occurrence too repetitiously may indeed be responsible for drawdown failure after many a prior ineffective episode.*

[14] *Only reaching equilibrium if by increase of the area of equipotential boundary for the diffusing flownet (against decreasing surrounding flownet pressures DS) the outflow capacity becomes sufficient. In reverse phenomenon, postulated forty years ago, shrinkage cracking corresponds to "opening additional free drainage surfaces" for facilitating suction-dictated internal flow, by reducing distances for "consolidation drainage" when the surface evaporation rate is forced to be much higher than the supply of the soil water to the evaporation surface.*





(*) Additional successive layers started similarly.

NEXT STEP PLANNED: Differentiated E's within body; check on stresses rotations; etc.

FIG. 11 - First-step try at incorporation of compaction residual stresses (in progress, step by step).

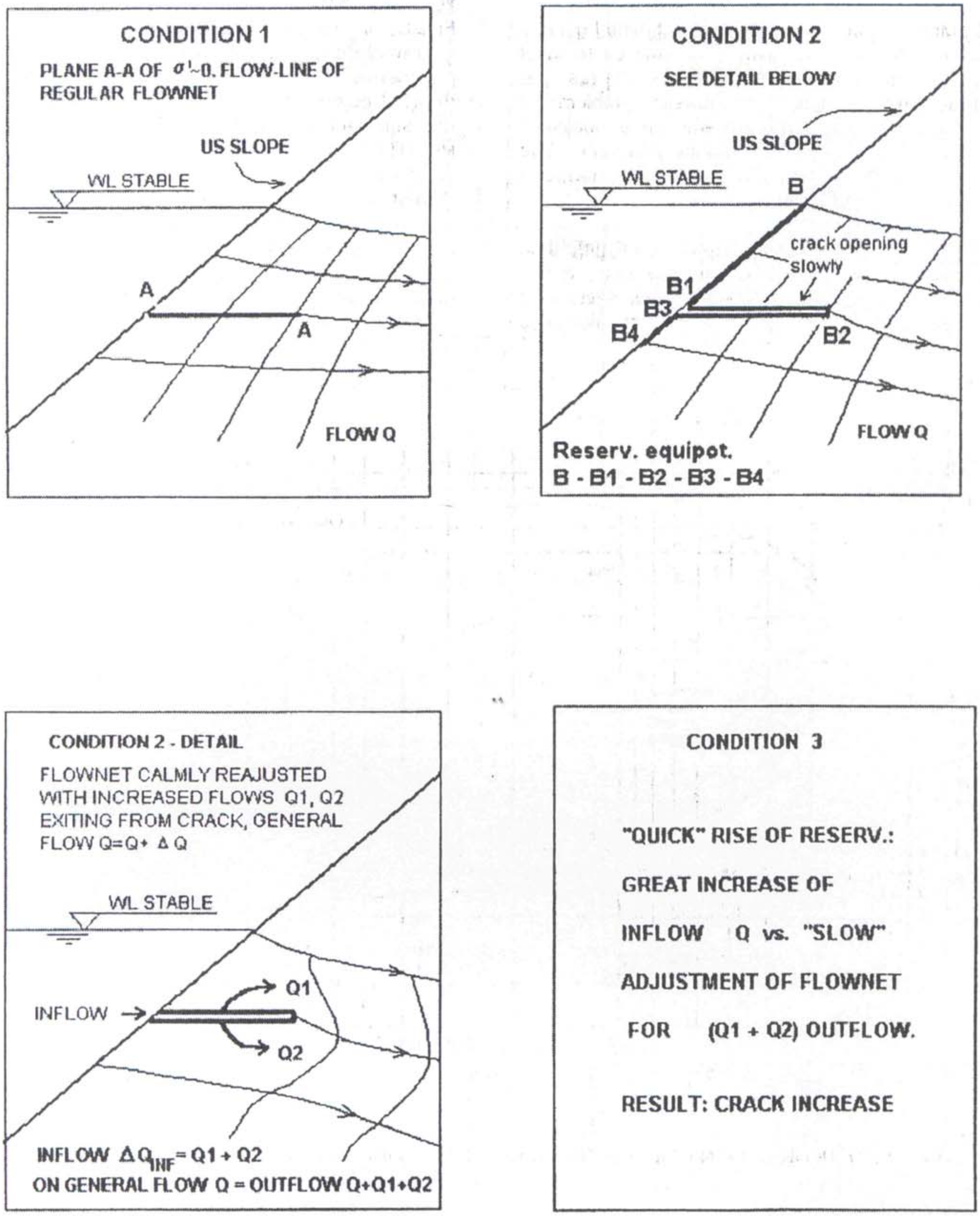


Fig. 12 - Proposed principle of crack formation

But the really basic emphasis, incomprehensibly standing by the wayside for over 2 decades, is that THE FILTERING ACTION HAS TO BE DERIVED FROM THE POROSIMETRY. It is really shocking to recognize how very little the porosimetry testing has come into geotechnical practice : correction of this serious insufficiency is long overdue, since in logical interpretation, all important soil behaviors depend on porosimetries, only indirectly broached in conventional geotechnique.

Ten years ago (ICSMFE, Rio de Janeiro 1989) I submitted queries that to me seemed important, on the almost generalized trend of publications validating the single-parameter criterion (as per Bertram-Terzaghi) for the complex probabilistic problem of grainsizes, grain arrangements, and extreme-value localized variabilities of filtering, clogging, and washing-through. The supporting test programs seem to have been conducted and interpreted in rather limited perspectives.

I reproduce in Figs. 13, 14, 15, the three interpretative figures then submitted: it is my earnest wish that either I be proved wrong, or this important problem be submitted to the highest-level systematic research. Fig.13 shows an uncomfortably wide dispersion, alongside with the appearance of a trend of significantly decreasing ratio CF = D15F / d85B of the yes-no non-failure/failure diameters, as one moves from finer to coarser "base" (B) materials. Should professionals feel safe in using higher (e.g. CF = 8 to 10) ratios in finer materials than the Bertram/Terzaghi 4 - 5 ?

Moreover, Fig. 4, using extreme-value Gumbel probability pper, would suggest that the Bertram-Terzaghi criteria incorporate up to 20 % probabilities of failure in sands-gravels and 0.5% in silts-clays. Finally, in Fig.15, the broadest available spectrum of (then) published results clearly suffers from greater variations of influential parameters, including very distinctly the direction of flows, vertical (upward or downward) and horizontal. Are these demonstrations not sufficient to entice special more conclusive research, OR REFUTATION ? It seems that possibly in prototypes the comfortable result has been very frequently lent by the tendency to auto-stabilizations of the local extreme-value provocation, rather than progressive degenerations of uniform materials (a hypothesis advanced in my Rankine Lecture).

(3) Self-healing of cracks by upstream transitions

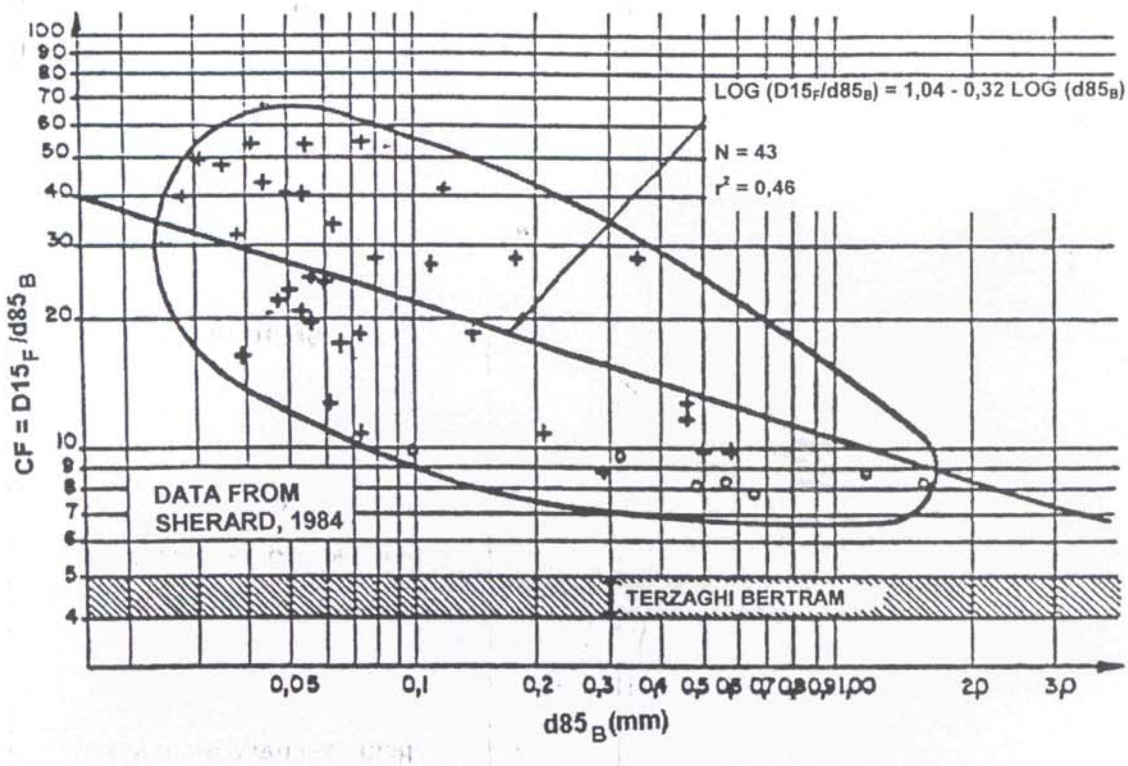


FIG. 13 - FAILURE / NON-FAILURE TESTS RESULTS CHECKING CRITERION FOR FILTERING.

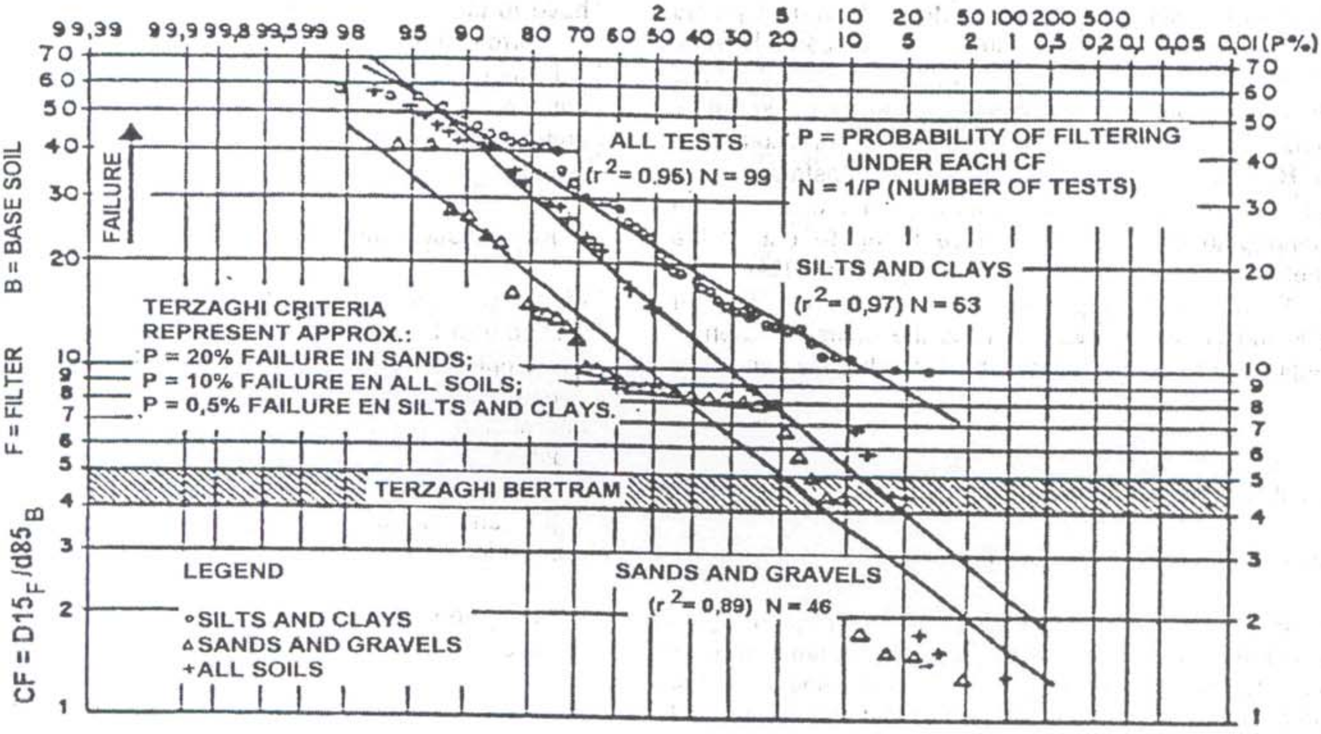


FIG. 14 - FILTER CRITERIA CF TESTS ANALYSED IN GUMBEL EXTREME-VALUE PROBABILITIES.

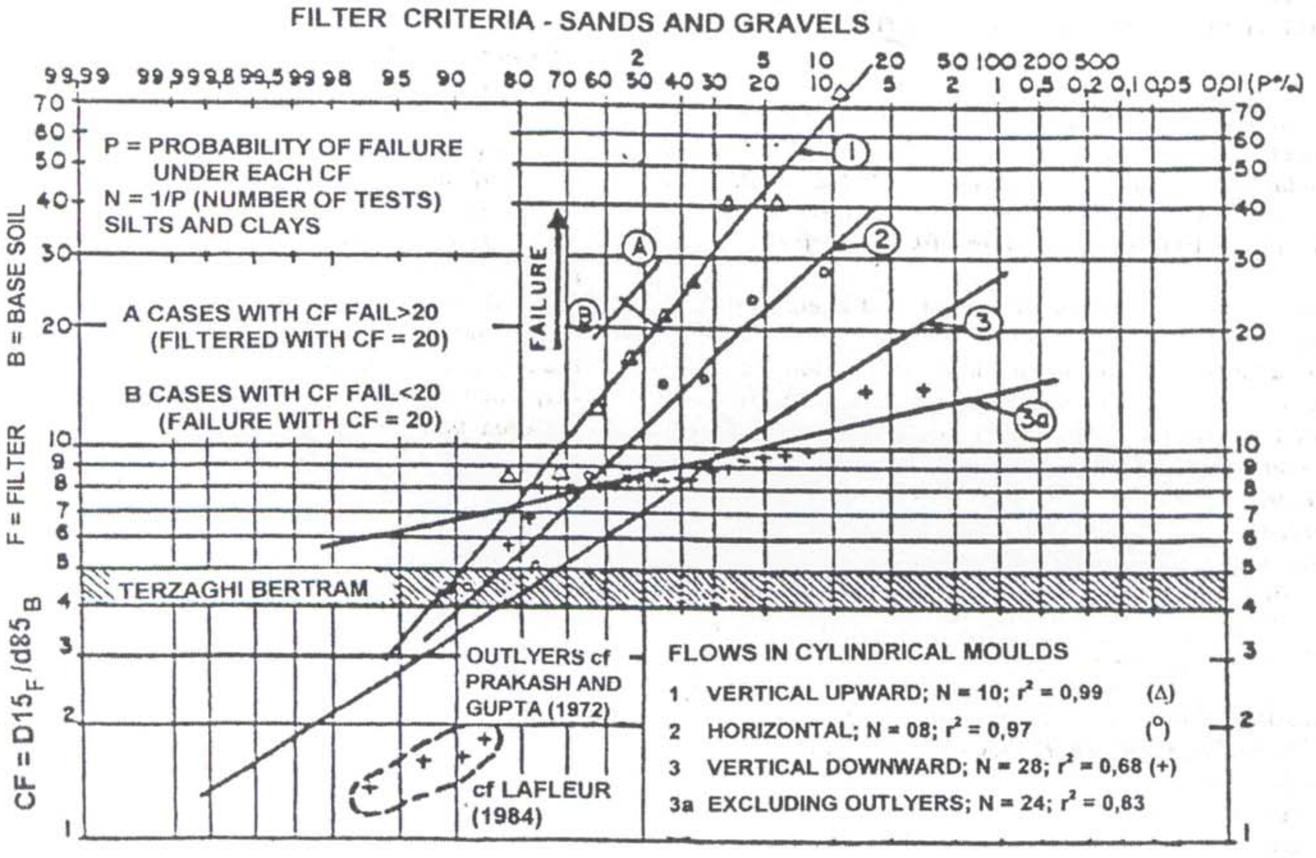


FIG. 15 - "ALL" TESTS IN GUMBEL PROBABILITIES, SEPARATING SOIL-GROUPS, FLOW DIRECTIONS.

To my knowledge, this all-too-frequent consulting recommendation has never profited from experimental evidence. Thereupon I submit the following mental test (with illustrative sketch of Fig.5), with the suspicion that the POSTULATION IS FALLACIOUS : I am provoking on purpose, because the matter is of consequence.

Progressive sealing by siltation is an unquestionable boon.It profits from the fact that by Stokes' law the sedimentation across a body of water deposits first the coarser grains, and progressively the finer ones, all induced to clog and compress (and seal) by seepage stresses against the "filters first deposited". The condition of a PREPLACED BROAD-GRAINSIZE TRANSITION upstream of a potential crack development is totally different. The differentiated grainsizes intended to clog the crack have to erode out of the preplaced zone: however, such erosion BEGINS BY EXTRACTING THE FINER SIZES, and only progressively attacks the coarser sizes, as crack and erosions advance. Thus the coarser materials that should begin the obstructive action of the crack, only come into play belated.

Must I be refuted and comforted, or should the hypothesized engineering solution really be rethought and tested ?

(4) Dispersivity ("dispersion"≈ "deflocculation").

Starting from case-histories from Australia 40 years ago (arid-region compactions and hydrologies, small homogeneous dams without chimney-filters, declared medium-term chemical influences of reservoir water with/without solutes on colloid-clay-pore fluid equilibria, etc.) the fears of so-called dispersivity as a mystical culprit of failures have stumbled along, accumulating YES-NO CREEDS, because to the Geotechnical-civil engineer the universe of extreme-value statistics coupled with colloid-chemistry is uncomfortably elusive. Causative identifications, nomenclature, crude index tests, and very rough accept-reject criteria, have all contributed to such confusion that as a MOST ILLOGICAL STAND; for instance, one should note that EVEN TESTING WITH THE RESERVOIR WATER AND SOLUTES AS COMPARED WITH COMPACTED FILL PORE-SOLUTES, HAS BEEN FORGOTTEN by the wayside.

What immeasurable damage is caused to the profession by premature, unreflected standardizations of crude index tests! Is not the most fundamental principle of geotechnique the attempt to simulate in the laboratory specimens the presumed effects (safely extrapolated) on SOIL ELEMENTS IN THE PROTOTYPE?

Since the aura of CREED continues dominant, and effective mostly on the less-prepared colleagues of developing areas, I shall summarize but a few impact statements. In my over-extended effort, I can only clamour for conscientious reappraisal and revision, while offering to debate and prevail or fall: (1) well conducted grainsize distributions conceptually employ the best available dispersant, establishing a lower bound for UNITARY PARTICLE SIZES (to be physically filtered), totally disregarding the nucleations and crumby nature of very many soils; (2) Fully-intercepting chimney filters (holding back the particles at exit, and by the train of continuity thereby holding back the entire path) cannot help being an adequate solution. And since this feature is HIGHLY DESIRED ON ALL COUNTS IN DAM DESIGN, one cannot imagine why the dispersivity erosion should persist as an INDEPENDENT SERIOUS CONCERN; (3) All five (or more ?) index tests (crumb test, SAR sodium absorption ratio, total CEC cation exchange capacity, ESP exchangeable sodium percentage, double hydrometer test, pinhole test) have unpardonably illogical details (distilled idealized water, compaction at "near the plastic limit" and not at realistic compaction conditions, inordinately high seepage gradients eroding the hole's surface roughened and unconfined, no consideration of the soil particles' dominant cations, etc...) : so much so, that in my experience world authorities themselves demonstrated having lost all understanding of the phenomena involved (REAL, in some cases, and always subject to analysis and understanding).

In short, the complex problem is offered to confusion by existing postulates and standardized tests. Engineered engineering tests have to be run simulating prospective conditions and incorporating a) borrow and compacted-fill fabric b) adsorbed cations of the colloid-clay c) pore water and solutes d) reservoir water and solutes, as CAPABLE OF AFFECTING THE LATTER. Corrective chemical admixing (e.g. with lime, etc.) for borrow soil follows basic reasonings.

6. Key question and proposals on surface details.

My present effort has already resulted over-extended, and cannot presume to be all-embracing. It seems enough at present to begin by emphasizing that these surface details are important for construction costs, and for maintenance facility and endless operational costs. They have been treated left-handedly, by indications borrowed from collateral professional specializations : but even in the geotechnical participations there are questionings of logic, and unnecessary insufficiencies of convincing research publications for specific design concerns.

With regard to soil-cement slope protection, so very useful in deeply weathered tropical terrains lacking rock :

(1) One qualm (however unrealistically bloated) was (is) the hypothesis of the soil-cement being sufficiently less pervious as to create uplift pressures under its "slab" in RDD (including in canals) : prototype empirical assurance of good performance is not sufficient for theoretical minds, especially of hydraulics colleagues. Some series of exactly comparable tests of permeabilities of compacted soil vs. soil-cement, should be sufficient to quell the qualm.

(2) On the other hand, in lieu of so many empirical data on mixes and widths, it should be of interest to develop index testing of R_c (unconfined compression strengths, functions of soils and mixes) necessary to resist potential wave or channel-velocity erosions, probably relatable to jets and flows of different energies.

With regard to rip-rap and transitions.The fundamental question is obviously "transition with respect to what ?" and the answer is, with regard to erodibility, transmission of water turbulences through pores, and generating downslope destabilizations by seepage and pore pressure. Once answered, the first comment is that a clayey material, under a minimum confining pressure, generally does not swell, and lose cohesion and erosion-resistance ; it is far more resistant than a filter-sand. There is no seepage exiting from the clay such as to call for "filter action" : anyhow, many tests from decades ago proved that piping conditions are far less critical in clays than in uniform sands. "Perfectly homogeneous" contact pressure by sand cushion on the clayey surface is not necessary, because by pressure distributions by a widely-graded transition material, the uniformity is guaranteed at dx depth below the contact. Thereupon, considering destabilizations, how on earth can one even conceive of successive transition of uniform sands and aggregates (gravels much worse) ? The slope stability of a succession of materials of $\varphi \approx 55^{\circ}$ (rock), 45° (aggregates), 30° (sand) is as low as determined by the weakest 30° (reduced by a little transient pore pressure). Very much worse is the case, and pseudo-design choice, if we consider construction difficulties, unable to compact unconfined on slopes.

In short, the multiple-layer is illogical, vulnerable, wrong ab initio : the transition of run-of-quarry widely-graded fragments-up-to-rock, spread and compacted in single continuous lifts "pushed from inside-out" , is to be immeasurably preferred, by PRINCIPLES OF GEOTECHNICAL BEHAVIOR.

7. Concluding remarks.

The strangeness of the approach taken in this report calls for explanation. My earnest hope is to arouse geotechnicians to the perennial richness of queries and quandaries within so very many presumed theories and truths-established, attributed to "performance conditions" taken as conventional for comfort, within our own yard, ipso facto more so by our professional neighbours. If we open flanks so widely, we indeed have to go to the moon, or outer galaxies, to seek our usefulness. And collateral professionals will occupy the spaces vacated. And humanity will head irrevocably down the road towards increasing poverty, material and of spirit. The careful balance of extremes is needed between a holistic conviction of our mission, and a humble intensity of purpose to do our imperative share, as unfailingly as possible , always much more of courageous/timid DECISION than of CERTAINTY.

8. Acknowledgments.

Deepest thanks are owed to all great mentors, who queried or not, brought me to my present complex of decisions, with all their sense of purpose and of insufficiency. Affectionate thanks are registered to my young collaborators, Eng[s]. A.C. Sobral, E. Sasaki, R. Quintanilha, L. Sakamoto, and also W. Bilfinger of VECTTOR PROJETOS, and supporting staff L.B.de Mello Alessio and E.D. Silva, without whose dedicated help my efforts are vain. May they have learnt some points of use, and, most of all, the lessons of the inexorable nurture of the biological function of intellectual life, of ingesting, digesting, absorbing-energizing-creating, and, no less important, cyclic discarding.