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COMMISSION INTERNATIONALE
DES GRANDS BARRAGES

VINGTIÈME CONGRÈS
DES GRANDS BARRAGES
Beijing, 2000

SOME ILLUSIONS, PITFALLS AND INCONSEQUENTIAL INITIATIVES IN RISK ASSESSMENT QUANTIFICATIONS (*)

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1. INTRODUCTION

Recent years have brought laudable concern with risk assessments, because of acceleratingly increasing concentrations, demographic and of investment values per unit area. The recognized difficulty of overtly attributing values to human life has been indirectly attended via Insurance and Judicial documentation. To some extent, also, there has been a slowly advancing effort to divorce Society from the age-old fallacies of "deterministic absolute safety", of safety embodied in arbitrary numerical Factors of Safety FS, and so on: comparative situations of death and damage tolls suffered in many activities of mankind (such as various addictions, travel accidents, risk-seeking sports, etc.) have advanced the familiarities with statistics and probabilities, in lay and journalistic levels. Risk evaluations and corresponding decision management obviously rest on the foundations of Statistics, Probabilities, and Decision Theory. The purpose herein is to point to some marked singularities of Civil Engineering of Dams, whereby a great proportion of risk assessments are an illusion, leading to erroneous unfair impositions on new projects, of greater consequences to developing areas. And, without presuming to be singular, geotechnical engineering has the obligation (and right, on behalf of Society and its multidisciplinary colleagues) to emphasize its incomparable responsibility

(*) *Quelques illusions, pièges et initiatives illogiques dans l'évaluation quantitative des risques.*

regarding some decisions that once implemented are much harder to correct than in constructed superstructures .

2. BACKGROUND REALITIES TO BE EMPHASIZED

Statistical frequency distribution "laws" derive from the past, from "universes" subjected to reasonably repetitive conditions. With respect to Civil Engineering, and Dam Engineering, and Dam Foundation Geological-Geotechnical Engineering, one has to go back to an intrinsic understanding of the trajectory from past, through present, to future, in order to mitigate the grossest of mistakes, currently abounding in an "industry" of pseudo-professional analyzing of risks.

2.1. Civil engineering has always been forced to aim at "guarantees" under critical foreseeable conditions, maxima or minima[1], and not at achieving the bull's-eye in performance compared with prediction (design, i.e. intention). Compared with two other closely known professions, medicine and law, there is tacit acceptance of Medicine as merely postponing an ultimate destiny, and Law's yes-no decisions aiming at pushing the client to better than the 50-50% chance: but Civil Engineering work is expected to be deterministically infallible, presumed to be cognitively dependent on physical/mathematical LAWS.

Further, Dams represent the most daring initiative because a "river is the geo-morphological expression of the geological path of least resistance to water's attack, and a topographically inviting site is almost infallibly a position of geologic structural discontinuity"(de Mello, 1966). There Nature's work of millions of years is to be barred, to store amazing energy upstream, for controlled release to downstream. Engineers rarely feel the comparisons: once, on seeing the erosion damages on a downstream basalt, by a trial asymmetrical opening of one spillway gate, it was computed as equivalent to 8000 D9 tractors working together.

2.2. The risky uncertainties belong to two distinct sectors: Nature's vagaries, including in many a spot, the incalculable ratio of Nature/Man, of the "acts of God concept"; and the corrective progress of our endeavours, always with insufficient knowledge and theory (i.e. logical basis for predicting and extrapolating), though obliged to pose as sufficiently certain[2] for the design-construction decision-action.

[1] *On estimations of two serious risk factors we note: in flood hydrology, the great changes over the past 50 years, through statistics-probability, and abutting in non-statistical PMP, PMF with additional factors. Seismicity suffers from much greater questionings as summarized in item 3.5.*

[2] *"What men most want is not knowledge, but certainty" (Bertrand Russell) and truly, the first psychological / technical hurdle of a good professional is to confess (to his inner*

Regarding Nature's severely conditioning factors I refrain from further mention of *meteorology-hydrology* besides recalling that in the past 25 years additional points (besides flood peak) have been incorporated, such as, time-lag of alerting as to oncoming flood, the flood volume, and the rate of rise of reservoir[3]. Risk assessment questionings on many other Natural factors of lesser consequences, such as winds and wave-protections, are herein set aside despite much affecting costs.

Surface problems easily accommodate changes during the project's operational life. The crucial Natural problems arise in the geological context: foundations, maximally stressed by the water pressures, and affecting the superstructure design, and difficult/expensive to access for remedial works after impounding. Besides the intrinsic problem of behaviors dominated by non-homogeneities and discontinuities (digressing from the mental tools of engineering) the points often forgotten are (a) the terrible disproportion of investigative quantities (3 ± inch borings at scores of meters) compared with what they are meant to represent, (b) that investigations have to be geologically oriented and not geometrically (often symmetric, grids, etc..)[4].

Engineers' obligation of diagnosis and decision despite doubts. Special surprise may arise from a declaration that the causes of failures and unsatisfactory performance have been, and continue to be, principally the deterministic professional decisions. This must be emphasized, to discard pseudo-statistics and pseudo-probabilities. In "statistical universes" of great complexity it is indispensable to shun statistics at random, in favor of analyses conditioned by presumed theorized vectors, to be questioned and proven. And the overriding interference is of engineer's deterministic decisions based on presumed theories, inevitably progressing from greater to lesser ignorance. Any wrongly vectorialized statistics is much worse than even the random one, in which erratic errors, in sufficient numbers, would average out. In short, the continually changing knowledge intrinsic to Civil Engineering's materialized prototypes, exposes a grave fallacy of presumed "precedents" to be directly respected.

Since multidisciplinary action is imperative, it is natural that the superstructure's main vector (for dams), i.e. the 3 hydraulic circuits (operational, spillway, and diversion), should assume the position of conductor of the symphonic orchestra. But, if his innovative eminence in layout and hydraulics does not wisen him to corrective and creative changes in all disciplines, the

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mirror) "I don't really know", especially while having to transmit to his client the assurance of knowledge (hopefully with recourse to cooperative colleagues, more specialized).

[3] *Note that no recurrence probabilities are associatable either to Maximum Probable Floods nor credibilities to Maximum Credible Earthquakes (de Mello, 1977).*

[4] *I have often used the analogy of the teen-age house game of "naval-battle" as an analogy. If each and every boring seeks to meet and investigate the geologic discontinuities (the naval units of the enemy), any boring that does not cross them automatically serves to characterize the homogeneous continuum (the sea) (de Mello, 1980).*

conductor achieves disbanding the orchestra. And so, well-intentioned *misguided respect for precedent* achieves the greatest proportion of *invited risks unperceived, or exaggerated project costs.*

For a continually recurrent present-day problem of risk assessment, a brief note belongs to reservoir rim landsliding. The great incidences of landslides impose the concern. A special workshop must be convened since important adjustments are needed from the small flat-slope rigid-body conventional theories, to the immense dimensions destabilizable rapidly in steep slopes.[5]

However, contrary to a tone of pessimism, I emphasize the optimisms for *dam design* by (a) averting extreme value statistics of specific universes, by selected change of universe (b) very definite deterministic guarantees (Factors of Guarantee FG) achieved, problem after problem, via creative, theoretically and pragmatically demonstrable physical solutions (de Mello, 1980).

2.3. Deterministic variation with time and theory

Ever since Middlebrooks' (1953) milestone paper, it is repeatedly shown that risks to dams depend greatly on time: (1) calendar period, reflecting ignorance and varying degrees of need and daring sited on varied geography; (2) time, reflecting different critical phases, construction and operational; (3) time incorporated in life-extent, and consequent deterioration or favorable ageing. These recognized obvious trends call for some analysis of historic trends and decisive changes, on behalf of colleagues more absorbed in other collateral specializations. Since I am deeply devoted to statistics and probability, I am impelled to emphasize the conditions wherein a deterministic change of physical universe abolishes any relation of risks, say, between dams without or with a chimney filter-drain. And I feel bound to make statements on such determinisms with impact, because of the weight carried by this International Commission on Large Dams.

Firstly, it must be accepted that risk acceptance of dam failure is essentially zero[6], by recognition that great many persons (potential victims) would be involved, in no voluntarily assumed risk, and in the face of catastrophic unforesee-able condition and consequences. Meanwhile, the question of risk-aversion and risk-seeking becomes increasingly important, adapted from Kahneman and Tversky (1982) and twice published (de Mello, 1987 and 1988). It shows the unsuspectedly high proportion in which subjective factors interfere on decisions. Note the change of attitude as value changes. In comparison with a Gaussian normal-distribution curve, the asymmetric psychologically-influenced function

[5] *Note the Vaiont slide of 9/Oct/1963 still being reanalysed, (Tika and Hutchinson, 1999).*

[6] *Probability of failure with full reservoir must definitely be zero (de Mello, 1980). The exercise of multiplying zero by infinity was explained, in secondary school, as undefinable, absurd: that applies to the downstream shoulder (slope) destabilization by high reservoir.*

(more near to simulation by some Beta distribution) shows that our sense of value for guidance in our judgements, protecting us from illusions, in taking *Design Decisions*, will greatly condition the progress from prior to posterior-probability risk estimations. Surely one recognizes also that in major geotechnically-affected structures the sense of value, prestige, risk, catastrophic collapse, responsibility, etc.: (a) will yield a strongly asymmetric Probability Distribution Function PDF against risk-seeking, risk-accepting, as opposed to risk-averting; (b) produces a strongly conservative tendency, *costly for developing regions,* towards designs presumed more robust. How doubly disastrous it becomes, thus, if the *added robustness is misplaced* because of ignorance (inevitable to varying degrees in initial steps) and bad diagnosis of the real culprit. With increased cost and questionable risk (and cost-of-risk) mitigation, the result is adding insult to injury; (c) calls for open rejection of wrong non-optimized alternates, presumed backed by "historic cases".

In short, no science/technology dispenses critical assessments of the past, its trends, and deterministic "leaps" which completely adulterate conventional statistics/probability risk evaluations by semi-informed colleagues. Not too unlike deducing electorate voting probabilities by polling children under seven.

The historic facts could receive *reasonably backanalysed weighted coefficients*, if there was a concerted top group effort, and not the publish-or-perish individualistic rush to multitudes of scattered theses and papers. The root problems, with but rough data and "feel", require the most advanced wisdom to seek, reach and dictate acceptable methods of data-treatment: subsequent calculations are thoroughly achievable by juniors and programs[7].

Restricting comments to "dry-fill layered embankments" the rough major periods could be categorized as: (1) waterfall locations of better foundations, poor engineering and construction of fill superstructure, modest ratio of dared height to potentiality, homogeneous symmetrical sections, no filters/drains; (2) flattened UPSTREAM, US, slope under intuition of "water lubrication softening" of US shoulder[8]. Downstream seeps remedied by non-weakening granular patches. Advent of seepage analyses, and the idealized (unreal) Kozeny parabola supporting the partial benefits of flatter US slopes. Progressive increase of height, superstructure still conditioning. Incorporation of downstream toe filter-drainage, later extended by horizontal filter-drainage blanket to DS toe, length oriented by Kozeny parabola; (3) Improved fill compaction and US slope protection, plus

[7] *The Prediction vs. Performance era inaugurated in geotechnique (T.W.Lambe / M.I.T. circa 1967) and not yet introduced into Dam Engineering shocking by revealed great dispersions and pessimisms in predictions. But the proviso was made (de Mello, 1999b) that design and experience are not so much centrally conditioned by bull's-eye prediction of what will happen, but much more by what will not happen, since decisions change by stepped ranges.*

[8] *By many complementary errors the flatter US than DS slope persists, dismaying geotechnique: it will be discussed regarding optimized design decision of the inclined filter-drainage chimney, the dominant element, as is steel reinforcement in reinforced concrete.*

corrections of flownet for moderate anisotropies transfer conditioning factors to foundations, selected sites being progressively less privileged. Wide central cores and zoned sections induce understanding of benefits of chimney filter-drains for flownets aggravated by possible very pervious layers and hypothetical cracks. Symmetrical sections and equal US/DS slopes continue (dumped rockfills suffering settlements of angular contact crushing on submersion). Upstream-deck dams much sought, until big deformations (at about 100m height) signal limit, and change to compacted rockfills; (4) in homogeneous compacted earth dams construction pore pressures (field-measured by USBR mostly much higher than real because of soaking soil around cells during installation) generated new problems and practices. The vertical chimney filter-drain introduced, principally to control these (de Mello, 1975) but very useful to control flownets and minimize reservoir destabilization of downstream shoulder. Rapid drawdown RDD computation difficulties and "saturated" US shoulder of U.K. dams (still with short DS horizontal filter-drainage blanket), maintain erroneous impression of greater US instability (FS values) retaining flatter US slope. Narrow US soil decks (Growdon dams, 1960) proven satisfactory, emphasizing beneficial compressive flownet stresses; (5) With higher dams, and silo-effects of incompatible deformations understood, superstructures incorporate better optimization of stability-deformation-seepage-filtering-ageing, imposing incorporation of all lessons progressively and painfully learnt. Wide range of materials usable, well compacted, academic problem shifting to difficulty on realistic laboratory tests on most nobler materials. Optimized filter-drainage inclined chimney proved as most dominant contributor to optimized design-performance, for both DS (obvious) and US slope RDD-conditioned. Foundation treatments of better sealing and controlled-drainage much advanced. Shear strength equations (curved, varying with σ) adequately understood in their sheer complexities, and advanced destabilization analyses properly done (not by discarded conventional routines and misinterpreted software) demonstrate slopes optimized with stepped slopes, steeper at top (shallow sections on abutments) and flatter towards bottom of higher sections.

In short, with such a systematic and impressive *succession of deterministic changes* introduced by scientific-technical-constructive achievements of certainty, how can the statistical past have any global PDF of any meaning for risk assessments such as put together repeatedly ?

Answers are offered below, both for global policy, and in some specific examples of crucially important design features. It is indispensable that within the global project, properly "decomposed', each specialization define separately its conditioning factors (past and prospective future) that *separate neatly distinct statistical universes*. The global project risk should thus be much more logically obtained by judicious composition of the risks of the component parts/behaviors. In brief preamble I may mention some most frequent factors :

(a) the bane, to conscious engineering of complex "intestinal behaviors", arising from Man's neurological dominance of "culture" via visual cognition. E.g. "I have almost always seen dam sections with US/DS slopes as 1 : 3 / 1 : 2.2";

(b) the trend towards symmetry (historically valid in embankments during construction) was understandable, but very wrong: the asymmetric operational conditions are exponentially worse to DS. If many a roughly symmetrical section survived, it doesn't change an iota the judgement of all such sections as wrong, exaggerated unnecessarily and ineffectively on one side or the other. Incidentally, mankind's prospective development increases the difference of DS failure, catastrophic, and inconsequential failure by RDD.

(c) By many complementary successive errors, the flatter US than DS slope persists, dismaying geotechnique. Coupled with constant slope, it represents a stupid waste, especially for the shallower sections, never to reach the "saturation" unfavorable to destabilizing pore pressures.

(d) The historic error of associating sliding failures with FS = 1.00, already mentioned, extends into the realm of absurdity regarding the "damage risk potentialities" of successive FS values (e.g. FS 1.5 down to 1.1), numbers adopted "off the hat" from collateral engineering sectors. The damages, associatable with slide volumes etc... are very different in different materials (cf. de Mello, 1980 ref. Rockfill raveling). There is a blatant immediate need for some studies (even if relatively academic and easy, idealized, for a start) to orient as to probable increment of damage vs. increment of ΔFS, failing on *passing through 1.0* .

(e) Optimization of filter-drainage features is the foremost problem, both for the superstructure (inclined chimney imperative) and for the foundations. The compatibilization of the *widely different optimized positions* for the two seems to be the most fundamental factor of deterministic domination of risks, even to the desired point of reaching unquestionable FG >>1.

(f) For averting piping, via filters, one should aim at the benefits of seepage stresses imparting compressions, not tensions, since in compression the effects of ageing can only be favorable, a fundamental aim of a good design.

3. EXAMPLES OF UNQUESTIONABLE OPTIMIZATIONS, REPRESENTING INTERFERENCE, BY DETERMINISTIC COMPETENCE, ON STATISTICAL PHYSICAL UNIVERSES

3.1. INTRODUCTORY PRINCIPLES

Despite respectful recognition of the important collective efforts conducted and published, principally by the Technical Committees of ICOLD, of immense following in developing regions, we must note (in contrition) that the painstaking listings of case histories described, do not establish bases for statistical/probabilistic risk evaluations. Neither does the establishment of simple ratio coefficients of failed and deteriorated vs. total cases, or of deteriorated vs.

failed cases (e.g. Silveira 1984). Descriptively collected listings, only lead to journalistic confusions. A statement such as "*complete compilation of data* ... (using) *a classification system of 216 different causes or types* of deterioration... This high degree of differentiation permits very *thorough statistical processing*" (Bull. 59, 1987), incorporates three misleading concepts: (1) one cannot mix *causes* and *types* (effects exposed) in the same bowl, especially since diagnosis-decision-design have to attack causes; (2) optimized statistics shies from both extremes, either too many categories with too little common significant data in each, or, at the other extreme, too few groups with plethora of data but in complex mixtures; (3) there has to be (progress has always depended on, cf. Galileo) courageous recognition of *few overriding principles* (of competence). It is incomprehensibly antagonic to state "All positions shall be staffed by *competent personnel with the necessary qualifications in the relative fields of specialization*" (1987) and, on the other hand, to collect data merely under the passive conditioning "In general the Committee has had to accept the rightness of the contributions received" (pg. 15 and pg. 568, ICOLD 1974).

In short, the work of global collecting is well done, but that of discriminating between right and wrong actions with courageous-competent well-intended recognition, and, thereupon, of separating statistical PDFs has not begun[9]. Is it democratic deference ? Judge the fact and not the doer, doubtless imbued with doing his best. In item 4 specific proposals are made. However, one must begin by exposing oneself with some judgements (to be debated and judged) as is the constant obligation of the profession's designer, teacher, consultant. Yes, some initiatives are definitely less wrong than others, and the PDFs of their cognizance across geography/time are always very different, and broadly graded.

3.2. METHOD VS. END-PRODUCT SPECIFICATION, AND INDEX TESTS UNKNOWINGLY DEGENERATED INTO INSTRUMENTS OF STAGNATION

For many a reason, including that of performances and failures to be judged, one must *reject once and for all the often cited, 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.

[9] *Cognitive discrimination is imperative, and not difficult, in a concerted effort respecting cooperating specializations. Is the name "earth dams" a defined statistical universe, all-embracing, for all conditions? No. But for some, yes, quite reasonably. For failure due to flood overtopping, external vastly dominant cause, all earth dams constitute a single statistical universe: but as regards internal body failures, there are distinct universes involved, some discardable deterministically. By deterministic "physical" reasons dams with DS horizontal toe filter-drains, and those with chimney filters, for instance, constitute totally different universes of potential failure scenarios and consequent risks. In a way the difference is as marked as the name-difference of earth vs. earth-rock dams.*

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.

Moreover, one must stop, to emphasize the *very serious criticisms on logic*, persisting in dam foundations and embankment dams by the unperceived dignification that generalists give to simpler index tests in lieu of progressively researched fundamental behavior parameters. It is a destructive determinism cutting across the varying statistical universe of available progress. How can there be progress without good comparisons of *predictions vs. performance?*

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 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*, such as dry-density checks (to be necessarily accompanied by exactly corresponding grainsizes) which 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. 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 exhilarating latitude.

3.3. Optimized features of partial sealing and dominant filtered drainages, superstructure and/versus foundations

How can there still appear in updated revisions of prestigious geotechnical engineering textbooks the idealized cross-section of Fig. 1a (of frequent dam designs of the 1930s and 1940s) which although directed only at the problem of flownets (in $\Delta V = 0$ conditioning irrespective of *anisotropies* k) will mislead young engineers into presuming it can pass as a realistic homogeneous earth dam ? It is wrong, both for foundation, and for quality-controlled built superstructure: once banished, it should not continue to pollute the risk probabilities of undesirable performance/failure of dams ! How can risk probabilities of existing dams include such doubly deterministic cases, first of probable misbehavior (both from foundation and from superstructure) as almost certain, second, of the very existence without exposed misbehavior as an act of God's charity ?

Fig. 1b and 1c represent effective advances towards *optimizing the superstructure flownet.* Its priority merit is of avoiding any change to downstream destabilizable shoulder by maximum reservoir filling, whatever the flownet conditions, even with unpredictable core anisotropies and discontinuities (cracks). Moreover it achieves compressive flownet stresses in the interface between core and filter. Many publications on tests and theories have been produced regarding filters (short-term): one point unfortunately omitted, fundamental to civil geotechnical engineering, is the guarantee that time will only improve and not deteriorate conditions (since nothing stays strictly constant through time); in such a light longer-term piping behaviors belong to tensile stresses, causing expansions and softenings, while compressions only tend to improve behaviors. Finally, with appropriate choice of optimized position one can achieve an engineered equilibrium between full reservoir flownet water losses (too narrow a core, too close to US as in figures 1b and 1c) and RDD flownet destabilizing the US slope: this point is not pursued herein, and for simplification of full design procedures the nominally optimized chimney filter position is taken as inclined 1,0 V : 0,5 H to US, in Fig. 1d.

The virtues of the chimney filter-drainage inclined to US (top-down) and relatively narrow cores have been recognized. Lewis (1955) approached the optimization purely on the basis of potential destabilizations of DS and US under idealized flownets for full reservoir and RDD, assuming sliding tangent to foundation, and minimizations of materials; for the cases of pervious foundations the entering seepages were considered controlled by idealized core trench or US

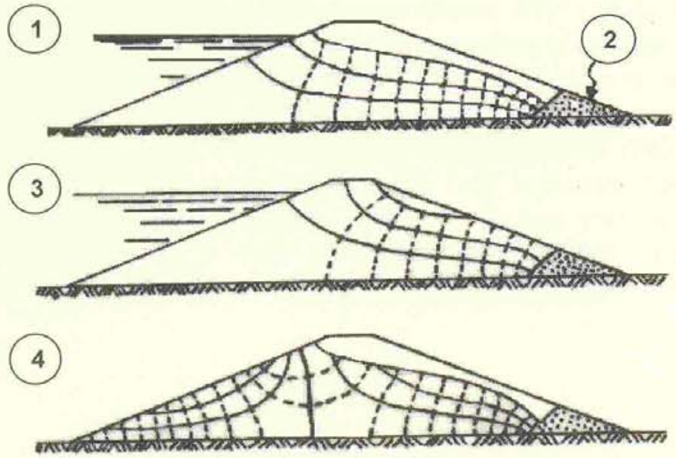

Fig. 1a
Seepage through imaginary homogeneous dam
Réseaux de percolation dans un barrage homogène imaginaire

(1) High-level state
(2) Filter toe
(3) State during continued rainstorms
(4) Drawdown state

(1) Réservoir plein
(2) Filtre de pied
(3) Situation pendant des pluies persistantes
(4) Situation lors d'une vidange

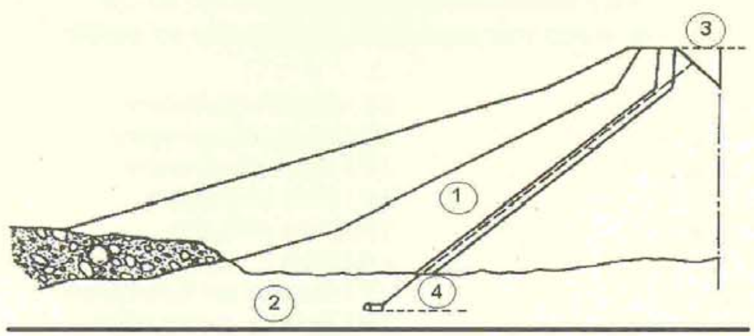

Fig. 1b
Position of the drainage gallery
Position de la galerie de drainage

(1) Core
(2) Grout buffer
(3) Elevation x (m)
(4) Elevation x – 120 (m)

(1) Noyau
(2) Injections d'étanchéité
(3) Niveau x (m)
(4) Niveau x – 120 (m)

blanket, and full relief well control of underseepage was taken at the foot of the chimney. De Mello (1973) added considerations of differential compressibilities of core and transitions/shoulders, and resulting settlements, with the need to avoid tensile cracking of the core. Both were but initial contributions. Fig. 1b and 1c exemplify the serious deterministic misguided concepts, in comparison with the final (presumed) optimization of Fig. 1d (de Mello, 1977). The erroneous concept on foundation decisions can be signaled most directly on Fig.1b. Grouting achieves moderate reduction ratios, mostly by sealing wider cracks (also of greater extent, the Rock Mechanicians' data on "persistence of cracks per family"): finer cracks gain from two effects: (a) the lower probabilities of

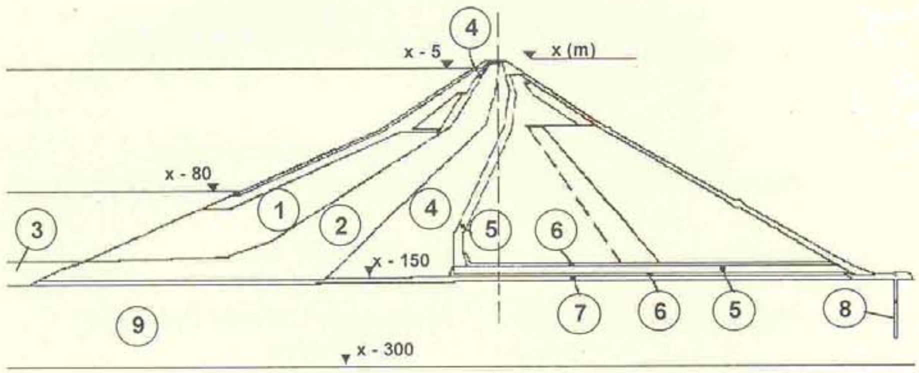

Fig. 1c
Very pervious foundation drained too far US.
Fondation très perméable drainée trop en amont

(1) Granular fill	*(1) Remblai granulaire*
(2) Impervious core	*(2) Noyau imperméable*
(3) Impervious blanket	*(3) Tapis imperméable*
(4) Transition	*(4) Zone de transition*
(5) Drainage zone	*(5) Zone drainante*
(6) Filter	*(6) Filtre*
(7) Foundation fill	*(7) Remblai sur la fondation*
(8) Relief wells	*(8) Puits de décompression*
(9) Alluvia, very pervious gravels	*(9) Alluvions, graviers très perméables*

interconnections with longer flow paths, and (b) the compression by greatest embankment pressures. Control of uplift is totally unnecessary except near the end of the DS shoulder support. In short, it is deterministically misguided to relieve foundation seepage pressures so far US, increasing risky high exit gradients.

Fig. 1d has been mentioned for optimized inclined filter-drainage chimney for the superstructure, on impervious (idealized, maximizing embankment destabilizations) foundation. The inclination must be optimized both regarding minimum width of "core" and US destabilization potential on RDD, economic considerations: but the *imperative, deterministic, sine qua non, criterion is that there should not be any change of destabilizing stresses* introduced by full reservoir on to the DS proven stable slope (at end of construction). I repeat, any/all old testament designs cease to have any statistical/probabilistic meaning (except as surviving doomed species) once this culmination of design/construction purpose is duly absorbed.

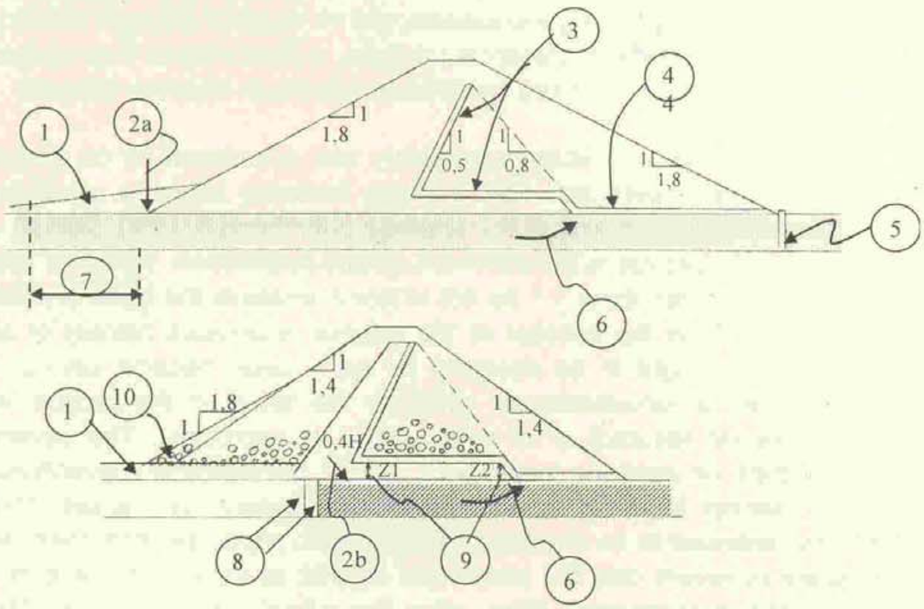

Fig. 1d

Summary updated optimized design, dam and foundations.

Schéma du projet optimisé actuel, barrage et fondations

(1) Conventional blanket	*(1) Tapis classique*
(2)(a) Gradient instant. ≈ 10 - (b)<<(a)	*(2)(a) Gradient instant. ≈ 10 - (b)<<(a)*
(3) Minor drainage; chimney filter-drain	*(3) Cheminée filtre-drain. Drainage mineur*
(4) Foundation filter-drain blanket required	*(4) Tapis filtre-drain nécessaire pour fondation*
(5) Relief wells	*(5) Puits de décompression*
(6) Reduced underseepage exit gradient, top flowline exit	*(6) Gradient de sortie des percolations à travers la fondation; réduit*
(7) Effective L of blanket resistance (Bennet 1946)	*(7) Longueur effective de résistance du tapis (Bennet 1946)*
(8) Grout buffer "width" w ≅ 10w effective permeab. (by reduction ratio ≅ 10)	*(8) Largeur injectée w ≅ 10w perméab. effective (rapport de réduction ≅ 10)*
(9) Thicknesses z_1, z_2 of semipervious internal blanket designed ref. uplift "blowout gradients" and settlements	*(9) Épaisseurs z_1, z_2 du tapis interne semi-perméable, dimensionnées réf. gradients de sous-pression et tassements*
(10) Stabilizing incremental rockfill because of (1)	*(10) Incrément d'enrochement stabilisant à cause de (1)*

There is absolutely no sense in FS calculations. The DS critical sliding surfaces have been analysed, and, above all, confirmed as behaving unquestionably, before gates are closed for impounding. The flownets to be established have some potential dispersions, but should be *checked at their worst* (and, inclusively by monitoring the slow development across the core, onto the easily overdesigned dominant chimney filter) *not to affect the critical circles*. If the DS shoulder developed some (moderate, desirable) construction pore pressures, the strengths can only improve with time. Thus, with acting stresses unchanging, and strengths only increasable, the FS concept changes to a FG (de Mello, 1980), i.e. a change of statistical universe, a *deterministic zero probability of failure*[10], which is what is desired for DS under maximum reservoir seepage.

Thus, the global optimizations of safety and economies for DS (including minimized seepage losses and filter-drainage features) begin by avoiding the absurd unnecessary shortening of the seepage path through the foundation (Fig. 1b, c): foundation seepage is the dominant, erratic contribution: control of uplift is only necessary beyond about 1:1 (to DS) where it weakens the base of potential sliding surfaces. Thus the concept of the *internal impervious blanket* of about 1.3H length (and height to be designed for each case) became *obvious and imperative*, under all circumstances, whatever the nature of the support, rocky (always somewhat fissured) or of soils (alluvia or saprolites). The advantage shows principally on pervious foundations, and in comparing a conventional US impervious blanket (e.g. Fig.1c) vs. the internal impervious blanket. The US blanket has continued to be designed by the classic paper, Bennett 1946, which unfortunately considers only the permanent flownet as established, and not the transient condition of reservoir filling, often the critical condition by far. *The US blanket is not a conceptually satisfactory design measure if constructed* (in the dry) although it is very effective, understandably, both as natural sedimented clayey blanket or as sedimented sand-silt-clay (in such favourable succession due to sedimentation velocities by Stokes' law) sealing the reservoir floor: as constructed it is under zero stresses until subjected directly to the great complete Δ stresses due to reservoir. The worst condition (dominant in unsaturated horizons underlying the blanket) arises because of considerable delay in establishing the underlying flownet while air-volumes are compressed-expelled-dissolved: thus, if the reservoir fills rapidly, there can be so high a water pressure differential between top and bottom of the US blanket as to cause punching shear sinkholes (on low bearing capacity). On the contrary, the internal impervious blanket has been subjected to full overburden pressure until before reservoir filling, and pressure changes caused by reservoir are secondary and favorable. Fig.1d indicates the comparative differences of gradients, using for the conventional US blanket the "effective constant gradient" as per Bennett, and assuming the pervious foundation stratum and internal blanket not improved by the embankment loading; it was shown (de Mello, 1977) that in reality for higher embankments (stresses above the compaction precompressions) there are

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[10] *Incidentally, sliding failures have been analysed as belonging to extreme-value statistics (Garber, Baker 1979) which I submit, with conviction, to be far-fetched.*

significant additional benefits in reducing (progressively) the exit gradients. In general the flownet pressures in the foundation will be higher than in the overlying "semi-pervious" compacted internal blanket, and the thicknesses z_1 and z_2 of the blanket must be calculated for (a) guaranteeing gravity drainage in the overlying subhorizontal drains despite the differential settlement (b) avoiding any unfavorable "blowout gradients" upwards across the blanket. In short, the internal impervious blanket only has advantages, significant, being clearly *designed to be used directly on the pervious foundation as is*, and much less vulnerable as such than the conventional US blanket. Needless to say, in any hypothetical uses of the US blanket in earth-rock dams, its connection with the core under the US shells causes a potential destabilization, that in principle would introduce the additional disadvantage of requiring US slope flattening.

3.4. Updated improvement of US slope destabilization sequential calculations. Prospect of significant change of future PDFs compared with past confused pseudo-statistical failure FDs.

Having set aside the DS shell stability, the economic considerations, of quantifiable levels for risk assessments, concentrate on the US shell, for which the more general condition is represented by the homogeneous dam section. In principle, the following postulations cut across essentially all of the past assumptions, and should indicate a noticeable excess safety, unnecessary, in existing well-designed dams. Starting with a summary listing of the position now believed less inapplicable, we shall close with the conclusion (item 4) that the only way to reach PDFs of some sense will be to retronalyse well-designed, constructed, monitored, existing dams, under comparative conditions (a) conventional, (b) as progressively changed, and (c) as herein proposed.

Slope destabilizations invite scrutiny, on principle and practice. Upstream slopes of homogeneous compacted clayey fill dams, HCCFD, invite exciting globally didactic reappraisals, both for updating theories, and by perspectives of immeasurable benefit/cost ratios, especially for modest height ($\leq$ 60m) dams or dam stretches. The historic imprint, and hysteretic behavior, impose that we compute changes ΔFS from a prior to a posterior condition, due to changes of causative factors, $\Delta\sigma$, ΔS, Δu, Δs, $\Delta\tau$, Δstrain, etc. The sequential condition of a HCCFD slope destabilizable mass is especially didactic from GEOTECHNIQUE'S FIRST PRINCIPLES. Assertions published in the past 25 years are merely listed forthwith.

1. Geotechnique implies love for accompanying changes through successive steps and stress-strain-time. No other engineering material is similarly affected by "history" in other components of the project. No soil elements in any other prototype are better know(n) (able) than in HCCFD regarding classification and characteristics, and in situ starting stresses.

2. Residual in situ stresses are know(n)(able). Measured values, especially of

suctions, must be improved, but present assessments will not change. Each layer's residual stress after the roller leaves starts with $\sigma_h > \sigma_v$, $\sigma'_h > \sigma'_v$.

3. In situ parameters can be amply, carefully investigated, determined.

4. Progressive rise of fill applies $\Delta\sigma_v$, firstly leading to isotropy, and only beyond a certain overburden height giving the $(\sigma_v - \sigma_h)$ deviator tendency to destabilize. Construction period pore pressures Δu_c change from initial suction to positive values, EVENTUALLY DESTABILIZING. Conventional UU tests for predicting Δu_c lie far from approximate realism.

5. Judicious inclusion of suctions and compacted residual stresses results in perceptibly increased construction-period slope FSs.

6. Limit-equilibrium must abandon vertical slices and the $\sigma_1 = \gamma z$ vertical $\Delta\sigma$ hypotheses, permissible in historic flat slopes, small slices. Appropriate wedge-slices kinematically admissible must be queried (Sarma, 1979).

7. Queries on slope destabilization.

7.1. It is accepted unquestionably that effective stresses determine behaviors and stability. However, I question that we have really known, measured, or credibly predicted, the concomitant pore pressures AT THE FAILURE PLANE DURING CRITICAL INSTANTS OF STRAIN, especially if fast, more damaging. Therefore, stability analyses should be incremental, final stages of $(\Delta\sigma, \Delta\tau) \rightarrow \Delta FS$ being conducted under TOTAL STRESS INCREMENTS AND ACU STRENGTH INCREMENTS (for prudence, engineering).

7.2. In back-analysed prototype failures I have further questioned the equivalence, $FS \equiv 1.00$. Failure signifies $\Delta FS = FS_i - FS_f$ positive, and PASSING THROUGH 1.0, not standing ("statics") at 1.00. Destabilization potentialities are checked via 2 or 3 STEPS OF CONVENTIONAL STATICS. Fig.2 illustrates all points.

7.3. Thus, for a starting first approximation the likely critical surface is computed (by unquestioned Limit Equilibrium updated methods, judicious general surface) for the HCCFD having reached the crest. Presumed $FS \geq 1.3$ should result. Thus, the "rigid solid body" does not become "isolated, for statics". Then, coming back with same critical surface recompute FS_1, for, say, 70% H. Proceed to recomputing FS_2, FS_3 for 85% and 100% H. However, for these ΔFSs, since the DAM CONTINUUM continues to prevail, the $\Delta\sigma$ and $\Delta\tau$ values on the surface SHOULD NO LONGER BE TAKEN BY THE VERTICAL heights of slices, but by analyses (e.g. FLAC – FEM analises) of the Influences of the added fill-weight trapezoids.

Results are quite different from conventional. Engineering prudence on the project slope is required if the successive ΔFSs increase rapidly (Fig.2).

8. Reservoir filling, with respective flownet and flownet-compatible effective

stresses in the continuum, generally introduces a favorable ΔFS to the end-of-construction FS_c. Saturation mostly requires high back-pressures (e.g. 6-10 bars, depending on porosimetries) and is too pessimistic, inapplicable to the common shallower sliding masses. In principle, again, the new FS is obtained via ΔFS.

9. Rapid drawdown pore pressure changes again call for treatment via ΔFS. Despite modest unsaturations, the continuous pores are sufficient to allow the POTENTIAL CHANGE from one flownet to another (Biarez et al, 1991); and, for conservatism, this tendency may be mentally applied as instantaneous. However the resulting TENDENCIES TO CHANGE of effective stresses (in the continuum, on the hypothetical sliding plane) are temporarily altered by the transient Δu due to ΔV, unfavourable if contractive and closer to saturated. The resulting ΔFSs must consider these compressibility Δu, and adopt the short or long-term case, whichever turns out more critical. It is unacceptable, technically/economically, to persist with simplified idealizations of Rapid Drawdown RDD u values that disregard so dominant a purposeful feature as the filter-drainage chimney affecting both flownets, full reservoir, and tendency-to-change on to lowered reservoir (de Mello, 1977).

10. Naturally several hypothetical critical surfaces should be tried. On any one of them the change of conditions, reflected in ΔFS, tends to be a more reliable index, than by allowing the computer to select separate critical surfaces (different) of secondary importance, bearing in mind the imposed hypotheses.

In summary, the recommended revisions are significant, and in several cases studied, would permit significant savings, with steepened slopes.

3.5. SEISMICITY RISK EVALUATIONS. RESERVOIR-INDUCED SEISMIC RIS SHOCKS. PRELIMINARY SUMMARY THOUGHTS

Necessity is the mother of invention... that is quite general, not excluding fallacious ones (well-intentioned). It is difficult to confess ignorance roughly proportional to the magnitude of catastrophe (and fear). However, ignorance on *extreme-value events* should be understandable, and explainable to Society, by their exceptionality, rarity, non-repetitivity. The fact is that just 50 years ago Scientific American reported with candour that the theory of plate tectonics was a hotly debated yes-no issue among geologists and seismologists. And now we feel assured that seismicity is determined by plate tectonics, but are barely beginning to "scratch the surface" of significant data, for instance using satellite pictures to measure movements, and precise accelerographs. Comparing with hydrology we should conclude that we are about 8000(?) years behind mankind's discovery that the deterministic factor was the yearly solar trajectories... a basis on which hydrologic FDs and recurrences achieved moderately successful

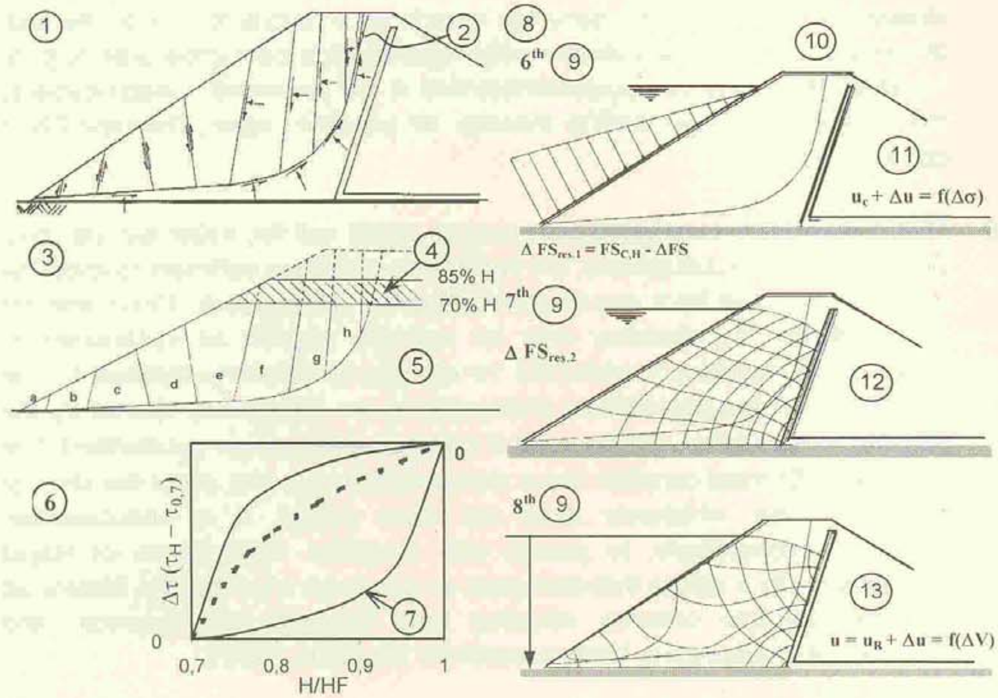

Fig. 2
Sequential ΔFS for US dam slope
ΔFS séquentiel pour un talus amont de barrage

(1) Construction period	*(1) Fin de construction*
(2) Tension crack	*(2) Fissure de traction*
(3) Construction period destabilization	*(3) Déstabilisation de période de construction*
(4) Add for 3rd step	*(4) Accroître pour 3ème étape*
(5) Σ weighted $FS_{1 \to n}$ at wedge bases	*(5) Σ $FS_{1 \to n}$ pondéré sur les bases des tranches*
(6) CPD successive ΔFS	*(6) ΔFS successifs CPD*
(7) Dangerous	*(7) Dangereux*
(8) Reservoir filling	*(8) Réservoir plein*
(9) Step	*(9) Étape*
(10) Assumed instantaneous, membrane	*(10) Supposé instantané, membrane*
(11) Δ stresses (FLAC) to critical sliding surface due to water press. triangle in elastic medium ΔFS = weighted integration of $\Delta FS_{1 \to n}$	*(11) Δ pressions (FLAC) à la surface critique de glissement résultant du triangle hydrostatique dans le milieu élastique. ΔFS = intégration pondérée de $\Delta FS_{1 \to n}$*
(12) Δ stresses (FLAC) flownet u	*(12) Δ pressions (FLAC) du réseau d'écoulement*
(13) Δ stresses (FLAC)	*(13) Δ pressions (FLAC)*

applicability for PDFs. Thus, by comparison with hydrology's adoption of spillway PMF = f(PMP) (fabricated with no "probability recurrence" associatable)[11], one can only condone the adoption of the Maximum Credible Earthquake MCE (estimated, deterministic, with no recurrence probability, and little "credibility" attachable, but intended to *guarantee against catastrophic failure*"... of what scenarios, how computed?) .

Tectono-seismicity must be recognized by dam engineering as a very complex field, barely starting to collect valid data. So, the "acts of God concept" is justifiably retainable. Also, efforts at decreasing the ignorance are highly commendable. But, in this very effort one must (a) denounce the mostly amateurish approaches published, leading to more mystifications than knowledge; (b) call for *joint effort by the specialists* of tectono-seismicity, transmission of dynamic energies, statistics, and the receiver-end effects (Alpañes 1998, Marsh 1999).

If illusions of knowledge are divulged by an august body, understandably lacking in complex modern specialties, more harm than good is done to the sorely needed progress in elucidations of so feared a phenomenon. The comfort sought through following *recommended prescriptions* (however intimately questionable) imposes a definite incremental expense, but absolutely no assurance, deterministic or probabilistic, of the safe/satisfactory performance sought. The criticisms on present promoted status are so many and so obvious that from my modest knowledge as "critic of the art and not artist" I concentrate only on 4 major points.

1) *Ultimate safety* is to be checked against the MCE, deterministic, of hypothetical Richter Magnitudes M[12] at epicenter (and hypocenter), although hitherto given systematically as definite numbers (e.g.7.3) with no error band!![13] The comfort is dispelled, however, when we note that excepting for modest numbers of instrumented observations of the past 30 ± years, the "observations" were by human and damage-scale subjective "assessment accelerometers", conditioned by Intensities at site. Therefore, the same tremendous scatter of starting from a Magnitude, and transporting along distances in bedrock according to the *several widely different attenuation curves* (de Mello, 1998) and subsequently *transferring from bedrock to surface for Intensities* (enormous variations conditioned by soil etc..) applies twice, to-and-from. How wide can the non-confidence bands be, and how can such inevitable scatter be totally disguised, hidden? (Poulos et al 1999, Rathje et al 1998).

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[11] Recent (Jul-Sept 1999) significant seismic shocks and exceptional rainstorms around the world seem to indicate (press information, credible for extreme news) that damages were worst by seismicity, secondly by landslides, and, apparently, none by dam overtopping, although very great by widescale flooding.

[12] Users are warned of the very frequent use, by prestigious authors, of Magnitude and Intensity confusedly interchanged.

[13] Refer to more specialized studies pointing to 10–20 fold scatters of instrument recordings.

2) In the same light, and with three-pronged major effect (on MCE, hopefully deterministic, and Operation Basis Earthquake, OBE, fallaciously put forth as Probabilistic, as discussed below) we have to question the observations via "effects on structures", both as input, and as output. Historic "records" require (very difficult) weighted coefficients because of varied vulnerabilities of structures. This also applies to most modern accelerographic records as per on-going debates regarding transfer of bedrock accelerations to ground effects, and ground to structures and damage. Finally, there are on-going queries/debates on damage scenarios, how far dominated by peak ground acceleration or particle velocity, or natural frequencies/resonance, tremor duration/natural period, or liquefaction, etc..

3) Over and above such general criticisms, far greater are certainly the repeated postulations on recurrence probabilities for OBE estimations. Most Civil Engineering colleagues have unreflectedly lapsed into statistical reasonings and methods derived from hydrology. Hydrology is derived from yearly cycles on which we reasonably assume essentially insignificant influence from previous years, leaving each meteorological "year" as an "independent event in the statistical universe", and a fairly rapid accumulation of representative data on high events (rains,floods). In seismicity, the lack of data has led to a first aberration, such as that of using records of minor shocks. The 10^4 proportion between M=4 and M=8 earthquakes is far beyond the ratio of measuring mm of dew, and attempting to evaluate, thereby, the floods causable by a 10cm rain, two entirely different types of phenomena.

4) Finally, above all, the energy-release aspect of each earthquake (visualised as explaining RIS events) should, if anything have a counter-effect for the subsequent similar episode. At any rate the PDF should be non-Poisson (which assumes independence): or even more, implying a contrary dependence. In earnest desire for tectono-seismological discussion, accept/adjust/reject, I offer my intuition. One realizes that any dominant fault may most frequently be accompanied by collateral secondary criss-crossing faults (fractures). So there may, indeed, be distinct conditions of energy build-ups and releases: (a) along the specific dominant fault, where the continued build-up of energy due to tendencies to move (at roughly constant yearly rates, nowadays recorded by satellites etc..., but only guessed about 40 years ago) would be broken at a major asperity, giving high M, or spared from such high single high M by frequent mini-breakages and releases. Thereby one idea (already applied by engineering intuition) has been to pump down high porepressures to facilitate mini-breaks; (b) along the secondary criss-crossing faults/ cracks, and even the dominant one, there may be sufficiently repetitive small M energy releases to permit use of conventional recurrence statistics. In short, in a layman's nutshell, the first case would be of an anti-recurrence statistics/probability, wherein one would have to separate two steps: one, the probability of build-up of greater than usual energy, because of insufficient mini-releases: and, thereupon, the longer this build-up occurs, the greater the probability of "the big one", the real damaging Strong Motion Earthquake. It would only be a case of conventional recurrence if enough data could be accumulated of PDFs of accumulation of residual pent-up energy

with time, and of periodic big M releases as related to such accumulations. Quite a distant hope!?

3.6. INSTRUMENTATION AND RISK MANAGEMENT

In the past 20 years many countries have adopted systematic policies regarding safety assessments of *existing dams*, thereby generating much expense and professional effort. Regarding design decisions for *new dams* the "conceptual classification of instrumentation problems and perspectives" (de Mello 1977) may be summarized in few compact statements for reflection while we persist in ignorance of the FDs of our actions-results: (1) instrumentation, even if "perfectly responsive and analysed", is obviously "designed", conditioned, to meet a scenario foreseen; (2) thus, it is mostly used to prove the desired, that expectations were met (for writing a technical paper); (3) serious misperformances/failures arise as "accidents", therefore extreme-value, i.e. scenarios that should be avoided by engineered *physical change of universe*; (4) regarding undesirable scenarios of lesser consequences, we presently lack any relation between measurable quantities monitored, and the calculation implements (e.g. FS values, limiting calculated deformations for crack avoidance, etc...). While this ignorance lasts, the only door open is to "design" for presumed "zero misperformance". Misperformance directly observed (visual or instrument-aided) is the appropriate indication, but one should guard against the common seductions, to (a) measure what is easiest, not what is really nevralgic (b) measure quantities that have to go through the theoretical model of analysis (itself questionable) for hopeful interpretation, which is a chimera, illogical, returning to the original ignorance (c) it is better to take the "reinforcing measures" in excess (e.g. drainage not to a given piezometric value, monitorable, but as far as practicable) (d) there is always the implicit assumption that behaviors will not be "sudden, rigid-brittle" and that we should have knowledge of the implicit rate of change of the monitored quantity as the dam tends towards graver misperformances.

In short, for dams yet to be built, in our present state of hidden ignorance, instrumentation serves principally for increasing expenses, publishing papers, and maintaining the theoretical establishment. An immediate purposeful, understanding workshop should efficiency retrieve.

It is obviously different for *investigating existing dams* as regards safety, since everything is then needy of investigation, including its *proven performance*. Two steps are at stake: to investigate its status quo; and to assess the means of extrapolating the performance into the future. The first step has been mostly subdued, since pseudo-theory is taken as prevalent fact. Once fact is respected as predominant, and a "full" investigation is conducted, including sampling, testing, reanalysing etc.., the only admonition to be added regarding *administration of ulterior safety* is (a) to use instrumentation *temporarily* monitored on really uncovered nevralgic scenarios, until they be excluded by reinforcing measures (b) to be prudent regarding (i) effective radii of dam volumes reflected into each instrument (ii) response delays/attenuations of all the steps, volume to instrument, (iii) instrument to reading, (iv) reading to accepted interpretation (v) interpretation to action (vi) action to effect, etc... *in the desired trustworthy ample desired improvement of safety probabilities.* While ignorant on FDs and PDFs, one averts merely changing of pseudo-probabilities *on safety*, but only on secondary problems *involving economic guesses.*

4. RECOMMENDED INDISPENSABLE WAY OF ACHIEVING EFFECTIVE AND HONEST PDFs FOR RISK ASSESSMENTS AND MANAGEMENT

Geotechnical engineering plays a subservient part in dams' multidisciplinary engineering, patently conditioned by superstructure hydraulics. It entails the greatest proportion of responsibility among cooperating professionals, and provides a small proportion of professional cases per lifetime, in comparison with the statistical turnover in building foundation engineering's much more vectorized requirements and occasional challenges. It is thus instructive to examine the results of the "statistical samples" from the new era introduced into geotechnique (1967-70) by the Prediction vs. Performance Challenges (de Mello 1999 a, b). Consistent lessons from such challenges in straight geotechnical problems (braced excavations, shallow and deep foundations, etc...) have been alarming/revealing that: (1) typically about 30 worldwide predictors use as many as 20 different calculation methods for the specific problem, an absurdity compared with but 1 to 2 methods used in structural or hydraulic engineering problems; (2) average results calculated are about 2–3 times more pessimistic than the measured performance; (3) the range of predictions varies between about $0.8 < FS < 10$, with 15-45% pessimistic (uneconomical), 0-20% optimistic (unsafe); (4) "experienced consensus" leads to better predictions, indicating that the wider gap resides between (a) methods of quantification expected to "give good answers" and (b) excessive individualism, deprived of group questioning, work, and respectful discernment.

Most of the above findings can only be expected to grow worse, with the present "globalized-privatized" trends in education, profession, and sociology. Any attempt to conduct similar challenges, even partial and idealized, *limited only to the dam body and foundation*, will expose much more dispersion. This, and a recognition that "layout conductors" do not encourage refinements of specialized components, probably explains why the Prediction/Performance initiative has never started even in specific partial problems of dam engineering (leave alone in global optimized choice of dam body, with broad subjective preferences/ aversions). Indeed, as hitherto practiced it would not be fruitful (not forgetting the several-year delay in obtaining performance results). But so important a need cannot be tucked under the carpet. The solution is to diagnose seriously, and to use a cheap revealing method of estimating the superposed FDs.

The postulation (Lambe 1973) that *no a-posteriori prediction could merit credibility* did a great disservice to geotechnical foundation engineering, and, as now perceivable, even more to geotechnical dam engineering. Why should we await building another dam, while dams built, monitored, performed, over the past decades are there by the scores to be *reinvestigated*[14] *and mentally redesigned ?* Which is cheaper, more effective? To recognize our deficiencies at start, and progressive advance derived from intuitions, practices, prescriptions, onerous prudence, all well-intented, incorporated for risk-aversion into Standards and Codes. Much testing, field-scale, has to be conducted, for improving knowledge of global behaviors. Testing of big proportions, "destructive" to many cubic meters (of no consequence to the prototype) can be conducted with a great exhilarating freedom of decision (deterministic) and vast range of impunity on existing works ![15]

For countless reasons we conclude that the road open for our indispensable constructiveness is to gather an *executive sense of purpose for group action.* The solution is to *retroanalyse existing dams, carefully disguised on all inconsequential details, and thus "anonymized".*

Two important separate steps are visualized: (1) For investigating the dispersions in the component geotechnical functions, to offer the section used as adjustable regarding zonings, but not changeable as to materials (and respective parameters); (2) to investigate the complex-richer creative-plus-geotechnical dispersions, to give the section *with alternate materials* to be employed at will for the same global operational purposes of the dam.[16] In both cases, for "statistical FD testing" the same case, repeatedly redisguised (about 10 times) should be presented at intervals, as apparently new challenge cases: thus the first check will be on *dispersions under unchanged conditions.* Incidentally, at each try the old-fashioned *rejected designs* should result excluded from the prior-probability PDFs for the physical universes for the subsequent tries.

Moreover, considering the wide variety, and progressive proposals of tests, parameters, and calculation-methods that still prevail across the geographic, cultural and calendar PDFs of geotechnique, one additional series of challenges

[14] *In fact even for foundation engineering the best field for investigating behaviors in quite general unsaturated soil conditions is clearly the clayey cores of earth dams, intensely controlled during construction. A funding and research partnership by the two collateral fields should result very fertile.*

[15] *Funding should be easy, a minute fraction of savings from improved over conventional solutions.*

[16] *It is imperative to evolve towards analysing alternate solutions at a given site, because that is where major decisions and economies are tied. It is pragmatically commented that different dam designers tend to fit the site to their preferred type of dam (of more confident intimacy) rather than the dam to the site. For example, deeper excavations to better rock obviously involve more excavated and reposition volumes, but also various complications to logistics: and so on. Is it not indispensable to try to assess comparative risks and costs-of-risks, when choosing between alternates? What different PDFs really count?*

must be implemented to check on the validity and effective net usefulness of the continually sprouting theories and theses. Given anonymized cases A, B, ...N, should be presented with a given set of test data as per *typical practice n^r.1*. Then, on a redisguised representation, additional (different, presumed contributory/improving) test data should be supplied as per *typical practice n^r.2*. And so on. Some practices will simply prove themselves doomed to discarding (and should not enter the PDFs of future risk assessments and management). The *progressive probabilistic satisfactions with procedures will be given by closer coincidence on averages, and narrower confidence bands.*[17]

I repeat the emphasis on the fact that against a "background of indices and crudely simplified prescriptions" no new case is ever sufficiently similar to those of the past: everything is different unless/until proved sufficiently similar. Levels of acceptability are continually becoming tighter, while challenges are forced to become greater. Both in theory and in practice, the *leaders of the profession* must be stimulated to check their proposals within meaningful statistical bands of confidence, credibility, economic benefit, and *extendable applicability*: Factors of Safety and Service-Limit regulations must be adjusted to demonstrated confidence bands.

It is hypocritical not to judge the steps of the profession's trek with candid recognition that the principal causes of accidents and unsatisfactory performances were not randomly probabilistic (as per "named types" of dams), but dominantly deterministic, by the engineer's yes-no decisions supported on insufficient knowledge, data, capacities, etc. PDFs will only result moderately credible after we have exerted ourselves to separate the physical universes of different design decisions, facing different quantifiable Nature's challenges at each site.

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[17] Incidentally, side-products of such exercises ensue as two rough correlations, the much needed realistic professional orientations (hitherto hidden as skeletons in the closet): (1) a "weighted" relation between *service-limit optimizations* and numerical FS values etc..; (2) analogous weighted coefficients for *transplanting risk assessments* across less-than-sufficiently-similar cases for improving the *data-base on "experience"*.

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SUMMARY

Risk and cost-of-risk evaluations are indispensable, progressively so in dam engineering of tremendous responsibilities to Society's continually changing needs in facing Nature's complexity. Risk evaluations require Probability Distribution Functions. These require the bases of statistical Frequency Distributions. Statistics at random is absurd, unacceptable, as can easily be diagnosed, via the "name category" adopted and the dispersions and confidence bands that result : for instance, the physical pseudo-universe of "earth dams" is merely as labelled by name, inappropriate, because what determines behaviors is how designed, how constructed, not how named. In statistical analyses of data it is fundamental to distinguish between statistics of averages, and extreme-value statistics, and the dominant interference of determinism (engineer's knowledge, wisdom, and decision) varying with time. Essentially all hitherto published FDs and PDFs are erroneous, conducted without separate specialized analyses of the phenomena and decisions that determine meaningful universes. Respect for specializations, and their FDs across geography and time, is imperative. It calls for group effort in retroanalysing existing dams, perfectly anonymized, for conscientious prediction vs. performance challenges, to identify real FDs and confidence bands of varied practices.

RÉSUMÉ

Les évaluations des risques et des coûts des risques sont indispensables dans la technique des barrages, compte tenu des responsabilités énormes face aux besoins continuellement changeants de la Société confrontée aux complexités de la Nature. Les évaluations des risques nécessitent des Fonctions de Distributions Probabilistes (FDP). Celles-ci requièrent les bases de Distributions de Fréquence (DF) statistiques. Les statistiques au hasard sont absurdes, inacceptables, comme on peut facilement le diagnostiquer à travers la "catégorie de nom" adoptée et les dispersions et bandes de confiance qui en résultent. Par exemple, le pseudo-univers physique des "barrages en terre" est simplement désigné par un nom, inapproprié, car ce qui détermine les comportements c'est : comment c'est conçu, comment c'est construit, et non comment c'est appelé. Dans les analyses statistiques des données, il est fondamental de distinguer entre les statistiques des valeurs moyennes et les statistiques des valeurs extrêmes, et l'interférence dominante de déterminisme (connaissances, sagesse et décision de l'ingénieur) qui varie dans le temps. Presque toutes les DF et FDP publiées jusqu'à présent sont fausses, établies sans des analyses spéciales des phénomènes et décisions qui déterminent des univers statistiques. Le respect des spécialisations et de leurs DF à travers la géographie et le temps est impératif. Cela exige un effort de groupe dans les analyses en retour de barrages existants, rendus parfaitement anonymes, pour une prévision consciencieuse des comportements, afin d'identifier les DF et bandes de confiance réelles des diverses pratiques professionnelles.