Terug naar overzicht

Colloquium *Numerieke oppervlaktewater modellering, mogelijkheden en beperkingen*

Antwerpen 23-24 oktober 2003

FORTY YEARS OF NUMERICAL MODELLING

Dr-Ing Jean A. Cunge, D.I.C.
Grenoble, France

Abstract

In the presentation three main aspects of numerical modelling technology, as it has been developed and introduced in hydraulics and hydrology will be mentioned and characterised. Namely: historical development and conditions that allowed such development; scientific and engineering aspects of applications, including market availability of the software and influence of the market on developments; reliability and limitations of the technology for engineering purposes. Last but not least some still unsolved theoretically problems and difficulties in that domain will be mentioned.

The presentation is engineering-oriented, stressing the applications point of view and concerns essentially deterministic modelling. The history sketched in the presentation goes back to XIXth century in order to show interdependence between available theory, algorithms and informatics tools, including traditional and psychological obstacles. Then the history of four generations of models used from 1960 to day as well as current situation of "mass-market" for software are shortly summarised.

Present situation of some confusion between application domains of correlative modelling (Artificial Neural Networks, Genetic Algorithms) and of deterministic models is mentioned with a frontier drawn between two categories.

A chapter is devoted to engineering applications and related use and misuse of the tools as well as to the so called "good practice" generally admitted paradigm of modelling; it is based on four steps*: set up-calibration-validation-running*. Criticism of the paradigm from the point of view of current state of knowledge and proposal for new paradigm is given.

Should one believe in models results? The limitations can stem from lack of understanding of physics, lack of formulations, blind application of unsuitable schematisations, etc. What can do an engineer, who must solve a problem even if there is no "good practice" validated tool available? Example of river and estuary hydro- and morpho-dynamics is taken to show limitations and doubts one can have about the results of modelling.

***N.B.:** Present text is accompanied by presentation of figures (transparencies) as used by the author during the Colloquium. This presentation is recorded as separated Power Point File that can be printed out in colours or under black and white form. Numbers of figures referred to in the text correspond to order numbers of the transparencies in Power Point file.*

Introduction: background, prehistory

The history of numerical modelling in hydraulics and hydrology begins very precisely in 1953, i.e. exactly 50 years ago (as compared with the date of the present paper). But there is also a "prehistory" that, in the spirit of the paper should be pointed to. One of important aspects of numerical modelling is its relationship to other domains: hydraulics and hydrology (including geology, soil science,soil chemistry), mathematics (hydrodynamics), numerical analysis and computer science (hardware and software). It is impossible to explain the developments and limitations of numerical modelling without these references. Indeed, the main thesis of the present paper is that important feature of numerical modelling is not a tool (i.e. software) but the way the tool had been developed and the way it is used.

In development of modelling tools main stages are:
- understanding essential physical phenomena; elaboration of a concept of the phenomena, culminating in mathematical formulation of physical laws (mostly conservation laws) governing the phenomena;
- elaborate ensemble of numerical algorithms that solve equations and respect physical laws
- the software, user interfaces etc., are but technology that completes a tool and evolves continuously.

The way a tool is used is generally more important that the quality of the tool itself. In order to be successful applications ask for users who understand physics (hydraulics or hydrology), essential physical laws that govern the phenomena of interest, who know the limitations of applied tools and, finally, have been trained in the application of the tool. Last but not least, it is essential for the users to understand the purpose of modelling. Indeed, since the tools are not perfectly representing reality they are not independent of the intention of the users. Their setting up and interpretation of results depend upon the users' minds and, hence, upon how knowledgeable they are but also upon their professional ethics. There are two aspects of this understanding. First is related to engineering. In engineering applications adequate solutions or adequate prediction of the future situations have to be found within the realm of limited understanding and formulation of physics (we never can simulate full complexity of the world) and within the realm of limited budget. Second is related to social requirements. 50 years ago engineers decided about the solutions to be chosen. Nowadays it had been understood that there are other stakeholders and needs in the Society (sustainable development, preservation of ecological assets, preservation of social values accepted as important within European culture, etc., etc.).

Few landmarks (their number is far from exhaustive!) that are reminded here as illustration of a "prehistory" of numerical modelling in hydraulics go back before first modern (i.e. using computers) application took place in 1953 (Stoker et al.). One should mention in this context Antoine Chèzy (1718-1798) and Pierre Louis Georges Du Buat (1734-1809) because they introduced the conceptualisation of open channel velocity and that of hydraulic radius. These two conceptualisations are illustration of understanding of essence of physical phenomena and their mathematical formulation. Barré de Saint-Venant (1797-1886) theory of one-dimensional unsteady flow and its two equations is perfect example of conceptualisation of complex physical situations through adoption of hypotheses that grasp the essence of parameters and forces that define their mechanisms. Actually these two equations published in 1871 and encompassing Chèzy concept of resistance are the very basis of numerical modelling. The first attempt was successfully carried out by Junius Massau (1852-1909) from Ecole du Génie Civil of Ghent. Using mathematical theory of partial differential equations going back to Cauchy-Kowalewska existence of solutions theorems and Riemann work carried out in 1860, as well as first developments of numerical analysis (theory of characteristics) he solved by hand, using graphical methods, unsteady flow equations. This work had no practical application future if "by hand" method was to be used. But, waiting for further numerical analysis developments and for computer tools it did open the way showing that approximate numerical solution is feasible.

One of most memorable and important for numerical modelling event was publication of the paper that was not, at that time, considered as such and has been until 1950's totally ignored by hydraulics world: Courant, Friedrichs and Lewy (1928) introduced the theory of solution of partial differential hyperbolic equations (the class to which De Saint-Venant equations belong) using finite differences. Apparently this work was useless for applications, because, as for Massau case, the tools were not there…Then arrived Manhattan Project of nuclear military applications carried out in Los Alamos. Construction of

ENIAC and requirements (social?….) related to development of nuclear weapons led to practical applications of finite differences in time-dependent fluid flows and, in parallel, to the theoretical developments of numerical analysis such as Lax Theorem defining convergence of numerical solutions to analytical ones, definition of numerical stability concepts, etc. Courant, Friedrichs and Lewy (1928) paper has been at the heart of all this. Following main developments have been developed by very small applied mathematicians circle of New York University Courant Institute. Eventually the principles have been published by R. Richtmyer (1957). The stage was set for hydraulic engineering applications but the actors were absent: it is a long way from nuclear and aerodynamics domains to engineering hydraulics…Indeed, first mature application of numerical modelling in hydraulics, using most (at that time) recent numerical methods and computer, has been carried out by New York University Courant Institute researchers Isaacson, Stoker and Troesch (1954). Nothing happened, however: the work was widely ignored outside USA and discarded in the US by Corps of Engineers as academic work far from engineering applications.

Short history 1959 - 2003

While the idea was put aside in the US it has been given full consideration in Europe and on the different basis: it was assumed that there is little fundamental research necessary for immediate applications and that applied research and development would allowed for creation of tools and engineering use of a new technology. Because of that private investment of engineering companies proved possible and successful: first industrial applications for engineering studies have been carried out in France in 1959-1960 (SOGREAH, then EDF), then these activities spread quickly in Europe through development of numerical hydraulic one-dimensional and two-dimensional modelling codes.

Numerical modelling, after some 10-15 years become by 1975 a standard engineering tool in Europe. By that date three new elements appeared. First, American engineers began to understand the importance of the approach and the applications begun to spread in the US. Second, commercial competition between European institutes created the demand and a market for simulation studies. Third, there emerged the need for collaboration between competing institutes in both, R&D and ethical domains. The latter need is extremely important for understanding of current (2003) difficulties in use of numerical simulation software, problems of "good practice" and of the trust one can have in the results supplied by various codes.

The history is important. In 1950 the conceptualisation of physical phenomena and applied mathematics-numerical methods were advanced far beyond what was then possible as hydraulic applications because of lack of tools, i;e. hardware (computers) and software. Advent of computers allowed for development of applications :1st and then 2nd generation of modelling systems, the latter applied to one- and two-dimensional unsteady flows in early 1960s. The success of the first relatively simple applications to real engineering problems created immediately demand for further, more complex applications. It was impossible to satisfy this demand without technology jump (new generation of computers) but when this jump occurred the applications have been blocked by lack of adequate theory: simple transfer of numerical methods from applied mathematics to hydraulic simulations, fruitful up to now, proved insufficient. There appeared demand for new domain: that is how Computational Hydraulics has been born. The evolution towards Computational Hydraulics went through the integration of

- encapsulated results of hydraulics (experimental!) and observations in nature,
- conceptualisation, mathematical formulation of features which up to now were dealt with only experimentally,
- applied mathematics and numerical algorithms,
- tools (hardware and software).

Nowadays the word "hydraulics" corresponds to the realm encompassing Computational Hydraulics as well as Experimental Hydraulics, be it scale modelling or real-life observations and engineering activities

The advent of Computational Hydraulics allowed for more complex simulations and for simulation of more complex phenomena. This in turn make engineering institutes and their clients to ask for more informatics, i.e. for more computer power and for new software tools easy to apply by developers of simulation codes. When those became available, demand for applications beyond the domain that was conceptualised, formulated, increased. Etc. This interaction, illustrated in Fig.2 has been going on since 1960 until today through the steps conventionally called by the involved scientific and engineering community "four generations of simulation software". These generations are defined as:

- 1st, before 1960:	Computerised formulas
- 2nd, until 1970-1975:	Specific one-off models
- 3rd, from 1970-1985:	Main-frame design systems
- 4th, since 1985:	Mass-marketable tools

Fig.3 shows the "ascending spiral" of development through these generations in parallel with availability of informatics tools.

It is very important to realise that this process neither is nor has been smooth and characterised by constant positive growth gradient. Thus there was one error in prediction of where it would go: we have believed in 1990's that the advent of 5th generation of the software, integrating IA with existing developments, was imminent. Nothing happened. Instead there appeared one new unexpected domain: Hydroinformatics. Currently, we can say that the developments have been blocked for last several years by slow-down in conceptualisation and mathematical formulation of phenomena - we shall come back to this further on. Because of advent of 4th generation, modelling software became an industrial tool, often used without deeper understanding and precautions. This technology became a part of engineer's life and practice in hydraulics as well as in hydrology and it is now (in year 2003) a general belief, comforted only too often by commercial aggressiveness of some developers, that it can solve all problems.

Data-driven correlative models and deterministic models

In hydraulics/hydrology engineering practice there are essentially two classes of models to represent reality, namely, data driven or correlative models and deterministic models. Only few words are said here about these two categories - more can be found in a recently published paper (Cunge, 2003).

Consider Fig.4 which shows schematically the object modelled (a catchment basin) with input (rainfall) and output (outflow discharge hydrograph). We may need to model such a catchment for various reasons (land use modifications, hydraulic structures to be implemented, to obtain outflow hydrographs for exceptional rainfalls, etc.). Nearly always these reasons are related to the need for prediction of consequences of engineering projects or exceptional situations.

Correlative or Data-driven models

A data driven model is a transfer or correlative function, the parameters, coefficients and components of which has been fitted (trained, calibrated) using input/output data recorded in the past. The simplest model of this class is of course a linear input/output correlation as depicted in symbolic way in Fig.5.

The symbolism of Fig.5 shows clearly the basic conception of data-driven modelling tools: they are not based on any explicit integration of physical laws. This class of models comprises, however, a large variety of sophisticated methods such as (from simple to complex) multicorrelations, ARMA methods, transfer function identification, Artificial Neural Networks (ANNs), Genetic Algorithms (GAs), Genetic Programming (GPs) and methods based on Chaos Theory.

The main advantages of data driven models are:

- rapid and nearly always automatic training and validation using past recorded data;
- once the model is defined, the calculations of input/output transformation are extremely rapid and, hence, well adapted to real-time applications.

Data driven models have essentially two limitations of predictivity:

- their validity is limited by the training sample of inputs/outputs;
- they are not predictive when the system is modified.

At this point it is useful to define what might be called the *predictivity* of a model. First of all consider Fig.5 assuming that a data driven model has been trained using observed recorded daily input (rainfall) /output (discharge) data for the last 50 years. Try now to compute with this model the discharge corresponding to an exceptional rainfall the frequency of occurrence of which is 200 years. The data point is definitely out of range of the sample and one must be a very strong believer in the model's virtue to maintain that it would predict the output correctly. Then consider Fig.6-A, i.e. a free-flowing river reach for which there is a large sample of observed inflows and outflows and for which a data driven model has been trained.

Suppose now that a dam project is proposed for this river as in Fig.6-B and that the same "trained" or "calibrated" model is used in an attempt to compute expected outflows for given inflows. This time the outflows are influenced by the dam operations rules and obviously the "trained" parameters obtained from original input/output sample are not adequate any more and the model cannot predict the outflows because it does not take into account the modification in the system itself.

Deterministic models

A deterministic modelling tool is based on a number of physical laws that have been formulated in suitable mathematical form (usually in the form of equations), the latter being solved numerically through the application of appropriate algorithms. The laws that provide the foundations of the tool define its domain of application and the user may choose one or another tool as a function of the adequacy of the laws upon which the tool is built, together with the user's requirements and needs. Such models are predictive in as much as the physical laws that they encompass will always be valid and respected by the model, whatever is the future potential situation. As an illustration, consider following example taken from *op. cit.* (Cunge, 2003):

Reservoir model of rainfall-runoff transformation over a catchment

This conception, schematised in Fig.7 is based upon the formulation of the law of conservation of volume in the form:

$$\frac{dS}{dt} = I(t) - Q(t)$$

where S(t) is the volume stored in the reservoir, I(t) is the input (rainfall) and Q(t) is the output (outflow). The purpose of using such a formulation is to be able to define, for a catchment, the outflow Q(t) when the rainfall I(t) is known. This is classical Nash reservoir model.

To make above equation operational for such a purpose one should add to it a relationship between storage S and state variables I(t) and Q(t). The simplest and most often used relationship is

$$S = kQ$$

where k is a supposedly constant coefficient of proportionality. Note that we have already introduced into the system two different categories of concepts

- First equation is a physical law stating that the volume of rainfall must be equal to the volume of outflow.
- Coefficient k is a parameter of unknown but constant value that is supposed to represent <u>*all the dynamics of transfer, including all the complex physical phenomena of propagation across the catchment*</u>.

The introduction of second corresponds to a simplification hypothesis of an infinitely more complex reality, it is not a physical law.

Eliminating S(t) between two equations leads to a simple ordinary differential equation that can be solved analytically:

$$\frac{dQ}{dt}=\frac{1}{k}(I-Q)$$

As an example, consider an inflow hydrograph I(t) divided into time intervals of Δt. Suppose that during one time interval, $t_1<t<t_2$, $\Delta t= t_1-t_2$, the rainfall I(t) can be considered constant $I(t)=I(t_1)=I_1$. Then, during this interval the solution for the outflow is

$$Q(t)=Q(t_1)\exp(\frac{t-t_1}{k})+I_1k[1-\exp(\frac{t-t_1}{k})]$$

All this is generally known and trivial *except for its implicit meaning*. Indeed, the modelling concept represented by the last equation is a deterministic one in as much as volume continuity is concerned. There is no way to say something *a priori* about the value of parameter k however. Usually the value of k results from some kind of calibration. Consider a suburban catchment area of which is mostly occupied by parks and playgrounds and for which long periods of data (i.e. of synchronous I(t) and Q(t) hydrographs) have been recorded. Then the value of k can be calibrated as, say, $k=k_1$=const by a trial and error procedure reproducing past events. Now we have a model and if there is a new input $I_A(t)$, we can compute a corresponding real-life output $Q_A(t)$ such as shown in Fig.7. Note what happens, however, if the catchment is urbanised and parks and playgrounds are transformed into parking areas, , streets and houses. Then the input (rainfall) $I_A(t)$ will result in real-life in the outflow shown in Fig.7 as $Q_B(t)$. But our model, with parameter value k_1, will still give the same outflow hydrograph $Q_A(t)$ as before. Thus we can conclude that such a model is deterministic and predictive in the sense that the water volume is conserved. Its predictive capacity is, however, very limited because the parameter k corresponds to the whole set of physical processes and to the whole set of characteristics of the catchment (e.g. soil occupation) and, hence, has no explicit physical meaning. This model is ***not*** predictive as far as engineering purposes (i.e. modifications of the catchment or consequences of exceptional input) are concerned.

Good practice and modelling paradigms

The example given above leads to revision of what is generally accepted as "good practice" modelling. Here again we draw on already cited reference (Cunge 2003) where the subject is developed in more detail.

Current modelling paradigm

In modelling practice we have a well-established 'good practice' principle taught at Universities, engineering schools and in general assumed as a dogma. It says that model application is to be carried out in four stages:

(a) *Instantiation* or set up or 'construction'. This consists in defining such features and parameters as discretisation, computational grid, limits, boundary conditions; in introduction of topography, soil occupation, structures, initially assessed values of roughness coefficients, etc.
(b) *Calibration,* that consists in executing a number of simulations of past observed events and in varying parameters of the model until an acceptable (to the modeller) coincidence between observations and computations is obtained.
(c) *Validation,* that consists in executing with a calibrated model a number of simulations of past-observed events (different from those used for calibration) and checking if the simulated results are sufficiently close to the observed.
(d) *Exploitation* runs (studies) with the model recognised as a validated tool.

These four stages historically come from hydrological correlative or black box modelling practice. They are a natural and indisputable approach when data-driven 'models' are concerned. This approach is the very essence of such models, parameters of which in most cases have no physical meaning.

When we consider, however, deterministic modelling (based on physical laws describing simulated processes and their interactions), this four-stage paradigm is not only illusory as a way of increasing accuracy but it may also lead to dubious and unreliable results. Hence it should be abandoned and a ***modified paradigm is to be applied when physically based deterministic models are concerned***. More precisely, ***the calibration stage should be eliminated*** from the paradigm while the validation stage, as compared to current practice, should be carried out in a different way and in a different spirit. We shall illustrate this position in the sequel and by examples of current practice.

If to calibrate, what is to be calibrated?

When the calibration of *deterministic* models (or deterministic components of models) is considered, one may ask oneself: ***what is to be calibrated*?** What is modified during the calibration procedure? An obvious principle (often violated in practice) is that the calibration must be limited to the model parameters that are *invariant* between the instantiation and exploitation stages, unless the purpose is to study the sensitivity of the model to modifications in its parameters. To calibrate parameters that will subsequently be modified during the exploitation runs used for simulating the impact of future projects is most often a useless, as well as costly, exercise. It is better by far to let a model be truly deterministic, i.e. a model without 'inner black boxes' describing physical processes the only parameters of which are empirical or experimental coefficients related to physical characteristics. For example, in open channel flows in rivers, such parameters are roughness Manning/Strickler/Chezy coefficients, singular head-loss coefficients, discharge coefficients of structures (weirs, gates, culverts, etc.). But certainly not topography, dyke elevations, operations rules for structures, etc. Roughness coefficients are *invariant* parameters between calibration and exploitation stages, unless the exploitation concerns projects that could modify them. Such would be the case of cleaning up and dredging a badly maintained a river stretch invaded by vegetation: this will necessitate the modification of the roughness coefficients. In this situation, roughness coefficients cannot be invariant. In order to study the impact of cleansing, one may wish to calibrate the current (before cleansing) coefficients in order to ascertain that the model reproduces the present conditions accurately. But to assess the impact of the change one has to modify the coefficients for the future situation and there is no way to calibrate these new values: they are defined through an engineering assessment. Any calibration of 'global' head loss coefficients along a stretch including features the characteristics of which vary between calibrated situation and exploitation runs (structures, sills, narrowing or widening of the river bed, etc.), may lead to a *non predictive* black box model.

Is a meaningful calibration feasible?

Another question to be asked and considered: in practice, is a ***meaningful calibration possible?*** The answer is negative, at least for most cases, because of the lack of appropriate data, or the cost of their acquisition. The scientific approach is clear: one can calibrate invariant parameters if one has the data that define these parameters in unique way. For example, if water levels and velocities are measured at two cross-section of a river and if between these cross sections there are no singular head losses then the resistance/roughness coefficients can be calibrated in clear and unique way from measured energy gradient between the sections. A counterexample was given above with the reservoir: a single constant parameter *k* was calibrated but this parameter represents in the reality a number of physical dynamic processes scale of which is the same as the computed results!

It can be shown on other examples how 'obvious' applications of a paradigm including calibration may well lead to serious errors because of the belief that calibration is meaningful or because of a wrong choice of calibrated parameter. One such problem area is that of open (sea) boundary conditions for two-dimensional tidal estuarine models. There are still modellers who impose tidal free surface elevations at open boundaries and 'calibrate' roughness coefficients within the model using stage hydrographs recorded at a few shore stations. Water elevations within modelled area, however, both in reality and in the model follow the variations of the boundary elevations, because inertia forces are predominant in such systems. Which means that even large variation in roughness coefficients of the model have little influence on elevations that are compared with observed values. Meanwhile the velocity fields may be very sensitive to boundary conditions. The main parameters that should be calibrated to make models of this kind *reliable* are the variation of in-flowing and out-flowing discharge hydrographs and their distribution at the open seaward boundary. Roughness coefficients within the domain should be assessed using engineering experience of the modeller and then they can only be modified rarely and only locally.

A proposed modified paradigm

The modified paradigm identifies the following stages in the modelling process:

(a) *Choice* of the tool based on analysis of physical phenomena to be simulated.
(b) *Instantiation* or set up or 'construction' of the model: definition of the methodology necessary to define the range of uncertainty in the results of the computations.
(c) *Validation* that consists in execution of a number of simulations of past-observed events with the model, computing or otherwise finding the range of uncertainty for the results and analysing and finding physically logical reasons for differences between the simulated and observed results. After this, analysing the impact of the differences as well as of the uncertainties upon the exploitation results. Decision on applicability of the tool, possible change of the tool (then back to (b)).
(d) *Exploitation runs (studies)*: supplying the results and impacts *and* their range of uncertainty to the end-user or client in a comprehensible form.

The modified paradigm for deterministic modelling as proposed above eliminates the calibration stage as such. The validation stage is not only maintained, but reinforced. In a way it incorporates the calibration stage. The past measured data will of course be as useful and as necessary as for calibration under the currently admitted paradigm but they will be used **for a *validation analysis of computed results***. It is claimed that a deterministic model, with values of parameters defined by inspection on the basis of engineering practice, should simulate reality correctly and its results should be close to past observed results *without* calibration in its traditional and irrational sense. 'Close' does not mean an immediately satisfactory coincidence. But making computed results nearer and nearer observed ones must not be carried out through calibration process as it is currently understood and applied. Indeed:

- If the differences between the computed and the observed lie within an acceptable interval of uncertainty, or can be explained by physical reasons, and if the consequences of differences upon exploiting the model as it is are analysed and acceptable, then there is no reason to go any farther.
- If the differences are greater than the uncertainty interval, *then they must be explained*. The reasons must be found and analysed taking into account, once more, the consequence of using the model as it is or amending it. Most often the findings lead to modifications of originally erroneous data such as topography, hydraulics characteristics or boundary conditions, and have not much to do with parameters. Sometimes there are factually important errors in values of parameters assessed during a visual inspection. But, sometimes, one may find that the modelling tool is not adequate: such as often occurs when using 1D model where only 2D can simulate the real flows.

The new paradigm insists on the fact that this modified approach *is not* a calibration that hides its name. The new 'good practice' asks for the collection and analysis of the data for the purpose of validation, and validation is not just a check that computed values are not very far from observed ones: it is a study of ***reasons*** why there is a difference between the two! And it must of course be substantiated by a report leading to an understanding how such an analysis was carried out and how the conclusions were reached.

There are two consequences of adoption of the new paradigm:

- An acceptance that the simulation results of past events do not reproduce exactly the in-site measurements. A requirement for an engineering analysis of the differences.
- An acceptance, and even requirement, that the results of models should be presented as uncertain and with a sound evaluation of the uncertainty range or interval.

Final remarks and conclusions

After 40 - 50 years, where do we stand? If a simple question is asked "Can we believe in model results?", is it possible to answer it simply, by *yes* or *no*? In 1960's the inner circle of developers and users used to ask nearly the same question: "*Why* do you believe that a model is correct?" and to give the answer: "Because its author (builder) says so…". Up to some point the situation today is similar because the only reasonable criteria are engineering judgement. The main difference is that it is not enough to "say so". It is up to the modeller who is going to use the results for engineering purpose to make sure that they are physically sound, that they are consistent with hypotheses lying at the basis of modelling system employed and that the limitations of such system are acceptable for the purpose. This implies that the user must know the hypotheses and limitations. In other words: it is impossible today to declare that a modelling software or a model built with a given software give correct results because the software has been "checked, guaranteed by the developer, model calibrated and validated". If, eventually something goes wrong, an engineer has no right to hide his responsibilities behind declarations such as "the work was carried out according to good practice rules and usinge recognised modelling software".

It is interesting at that point to stress that in principle of the last statement there is not much new. In the chapter **Short history 1959 - 2003** above I have indicated that "…commercial competition between European institutes created the demand and a market for simulation studies"…and… "there emerged the need for collaboration between competing institutes in both, R&D and ethical domains". Indeed, while severe competition aiming at dominance of the market of modelling was going on, all main European competitors (*IHE, Delft*; *Delft Hydraulics*; *SOGREAH*; *EDF-LNH*; *DHI*; *Hydr. Research Wallingford*) published on behalf of IAHR Use of Computers Section a common warning paper devoted to the difficulties, dangers and potential pitfalls of use of models in engineering practice (Abbott et al., 1986). Most of the content of this paper is still of burning actuality.

However, since 1986 things evolved and while general careful approach is required, it is possible to distinguish within the realm of modelling two categories of tools.

(i) First one is composed of tools that have been applied now for many years and have been tested by large numbers of engineering organisations. Thus their limitations and hypotheses are well known. They simulate phenomena for which mathematical formulation of physical conservation laws is well established, mathematical theory of existence of solutions is either well developed or at least reasonably advanced, numerical algorithms are known and widely accepted. Thus 1-D and 2-D hydrodynamic long wave propagation simulation models belong to this category, although for the latter, boundary conditions guaranteeing existence of solutions are still a problem that begins only to find its solution (Guinot, 2003).

(ii) Second category is composed of tools that limitations, algorithms, mathematical theory and even formulation of physical laws are hypothetical and uncertain, at least when there is a question of integrating them as tools of standard engineering practice. Consider few examples:

- 3-D hydrodynamic models. Why ? Because they depend upon theories of turbulence that are still a research subject. Because the theory of existence of solutions in function of boundary conditions is not established (hence nobody knows what conditions should be imposed). Because they are all based on the hypothesis of hydrostatic pressure distribution while pretending to simulate helical currents of strong curvature. Because the validity of their numerical algorithms is limited and far from accurate for all cases of interest (especially when schematisation of uncovering tidal flats or ADI methods are concerned).
- All models dealing with sediment movement, deposition, erosion, exchange between bed-load and suspended-load. Why? Because there is no recognised theory of mathematical formulation of these phenomena. Because corresponding physical laws are not formulated and there is no consensus on so called "sediment formulas" (counting in scores and giving the dispersion of results in hundreds per cent). Because while there is a consensus on the existence of links and interactions between hydrodynamics (resistance to flow, roughness,

bed forms) and sediment movement, such links and interactions are generally not introduced into available modelling tools.

- All models attempting to deal with morphodynamics and development of plan forms of river and estuaries. Why? Because no recognised and accepted theoretical bases exist for that problem: neither usable mathematical formulation of physical laws, nor practical algorithmic solutions. Because all objections enumerated above concerning sediment movement modelling. Because non formalised understanding of interaction between evolution of the river banks and channels.

The most of real problems are of course related to the second category. There is a specific example of difficult problem without a "cook book" solution: *morphodynamic estuarial problems*, i.e. future evolution (natural or influenced by human activities) of a mobile bed estuary . Whatever is mathematical, numerical or scale model tool applied there is no serious engineer who can vouch for truth of the results of *one single tool.* The decisions are, in the end of the day, based on engineering experience and the responsibility is that of engineer. But such decisions should be based on application of all available tools. In particular when numerical modelling is concerned, the doubt and uncertainty are in their full, since such modelling should involve sediment movement, plan forms, banks-channels interaction and all this, usually, in 3D environment! Reasonable approach to apply (together with other tools) a numerical model could be the one that for decades have been successfully applied to scale models: validation of the model through reproduction of the past morphological evolution of the estuary. This technique has been applied as calibration technique for two variables: morphological time scale and diameter and density of the material used on the model. It was and still is necessary for laboratory scale modelling because the similitude theory cannot define accurately these two variables. Note that the idea to reproduce morphological past evolution rather then free surface elevations is highly unpopular with numerical modellers and yet turbulence theories and sediment formulas they use cannot really be trusted and could well be calibrated when they do not reproduce the past.

So, finally, where do we stand in 2003 when numerical modelling is concerned? We are submitted, in this world, to the engineer's condition: we have to give answers and solutions to the Society, to satisfy social requirements and the Society does not ask us if we have or do not have adequate modelling tools. If the problems are such that we can answer the questions and predict the consequences of our proposals with first category of tools, fine. If not, however, should we reject the tools of second category? Certainly not but we cannot trust them blindly and completely. We are engineers and we have to use both scientific method and engineering judgement according to the rules of professional ethics. Since the results obtained through application of existing tools are uncertain, we have to use all available means of investigation and studies. To combine and compare numerical and scale models results with field surveys and with past experience of the expertise. This is a hard truth to accept by the generation brought up in computer-internet culture who learnt in school that numerical modelling is a "good practice" and that the models are useful. And they are useful indeed! Question is, up to what limit and, more to the point, in which way? We must not adopt religious war attitude (numerical models are good, hence scale models and engineering experience are wrong) or institutional chauvinism (software tool *I* propose *to apply* or *to buy* is the *only one* that is correct, all other products are no good). We must be ready to explain in details the hypotheses, limitations, algorithms and accept criticism of others. And, in turn, explain the difficulty to the Society, to the stakeholders who charge us with finding the solutions. And do it in intelligible way - and numerical models are very helpful to do it.

References

Abbott, M.B. (IHE, Delft), Berkhoff, J.C.W. (Delft Hydr. Lab.), Cunge, J.A. (SOGREAH), Hauguel, A. (EDF-LNH), Warren, I.R. (DHI), Weare, T.J. (Hydr. Research, Wallingford),"Computer Modelling: a

Warning Notice on behalf of IAHR Computer Section ", *Dock and Harbour Authority*, March 1986., pp. 251-256.

Courant, R., Friedrichs, K. O. and Lewy, H., "On the partial difference equations of mathematical physics", (in German), *Math. Ann.* ***100****, p. 32*, 1928.

Cunge, J. A., "Of data and models", *Journal of Hydroinformatics, 05.2/2003*, IWA Publishing 2003, p.75 to 98.

De Saint-Venant, B., "Théorie du mouvement non permanent des eaux…", *Acad. Sci., Comptes Rendus, vol. 73, p.148-154, 237-240, Paris*, 1971.

Du Buat, P. L. G., *"*Principes d'hydraulique, vérifiés par un grand nombre d'expériences faites par ordre du gouvernement*"*, 2nd edition, Paris 1786.

Guinot, V., "Riemann solvers and boundary conditions for two-dimensional shallow water simulations", *Intern. J. Numer. Methods Fluids* 2003;41:1191-1219 (DOI:10.1002/fld.488)

Isaacson, E., Stoker, J.J. and Troesch, B. A., "Numerical solution of flood prediction and river regulation problems (Ohio-Mississippi floods)", *report II, New York university, Inst. Math. Sci. Rept. IMM-NYU-205, p. 1-46*, 1954.

Massau, J., "Mémoire sur l'intégration graphique des équations aux dérivées partielles", *Annales des Ingénieurs sortis des Ecoles de Gand*, *Vol.12*, 1900.

Richtmyer, R. D., "Difference Methods for initial - value problems", *Interscience publishers Inc., New York - London*, 1957

oOo