Chapter 2
Engineering Approach

In 1992, the journal *Advances in Water Resources* published a series of papers on validation of hydrogeological models. In one of those papers, Konikow and Bredehoeft (1992) hold that groundwater models cannot be validated but only invalidated. It means that the real quality of a model can be judged only by comparing the prediction that the model has produced with what have occurred actually, i.e., only based on post audit, and that the accurate results in process of model calibration do not warrant that the model will predict accurately. However, if calibration goes wrong, the model cannot be trusted. Commenting their paper, de Marsily et al. (1992) write:

> We all know that the parameters of a model are uncertain, probably wrong in many cases, and easily can be invalidated. Similarly, the 'structures' of the model (2-D, multi-layered, 3-D, etc.) can be incorrectly chosen. So what? As long as they reproduce the observed behavior of the system, we can use them to make predictions. It also seems to us that the better or the longer the reproduction of the observed behavior, the more confident we can be of their validity. … Using the model in a predictive mode and comparing it with new data is not a futile exercise; it makes a lot of sense to us. It does not prove that the model will be correct for all circumstances; it only increases our confidence in its value. We do not want certainty; we will be satisfied with engineering confidence.

Writing this chapter, I had a strong urge to call it "So what?" and to use as an epigraph the last sentence of the above quotation. But I overcame the urge and named it instead after the approach engineering. It is simple and transparent conceptually. Indeed, the modeling assumptions are generally "false and known to be false" (Morton 1993; Beven 2005). However working on many similar projects in similar geological surroundings and observing the results of implementation of those projects, professionals gain personal and collective experience of what models work satisfactory, how their parameter and boundary conditions should be assigned to yield the satisfactory results, and what is the chance that a given model fails which is a factual empiric estimate of the uncertainty of the simulation results. Validated in such probabilistic way, a model can be considered as "sound, fulfilling all necessary conditions, and just good enough model" (McCombie and McKinley 1993).

Let us come back to the models based on the Dupuit-Forchheimer assumption, that when the gradient of a water table is small enough, the vertical component of

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the Darcy velocity can be neglected and the flow considered as strictly horizontal. Such sort of simplifications is pretty common in mathematical physics or in engineering. Muskat (1946) calls the Dupuit-Forchheimer assumption "not trustworthy." However he expresses his astonishment by the fact that the results of its applications are accurate compared to "those given empirically or by exact calculations." Haitjema (1995) holds that "Dupuit-Forchheimer model could have done the job, saving resources and cost." Since the Dupuit-Forchheimer assumption is false, there is no possibility to evaluate the errors of the simulation results based on it in a closed way, i.e., based on errors of the model structure and its parameters. However Beven (1981) considers it reasonable for the water table slopes which are mild, and according to Bear (1972), it generates practically acceptable errors in homogeneous shallow aquifer on a horizontal aquitard, if the squared slope of the water table is less than 0.01.

Such use of not provable and even wrong assumptions, let us call them simplifications, which lead to accepted practically results under some empirically established conditions, I call the engineering approach. My attitude with respect to this approach is rather positive. It recognizes the reality, the impossibility to evaluate the uncertainty of predictions in a provable way. I would rather trust the professionals, though I understand that their experience is subjective and that it is different from an objective proof. However, this trust, though cautious, relates to the situations where the engineering approach really exists, e.g., in the case of building small reservoirs, or drilling water supply wells for small farms or family houses. However, what to do if there is no such experience, e.g., a project is unique per se, or unique for a given surroundings? Or what does one have to do, if experienced professionals make different recommendations and estimations?

Lerner (1985), described several cases related to the ground water supply in Africa, Latin America and England in which teams of highly qualified experts made different but equally incorrect estimations and predictions, using the same data. Anderson and Woessner (1992) report several instances with not so much encouraging results of post audit in the USA. They explain the failures by errors in conceptual models in developing which the professional experience plays the major role. Andersen and Lu (2003) add several more examples of post audits, that "have not provided high confidence in the predictive accuracy" of the applied models.

In relatively good times for the Soviet hydrogeology, an extensive study of the reliability of hydrogeological estimates of ground water resources was undertaken (Yazvin 1972). The study of 89 large intakes from artesian aquifers revealed that only in 12 cases the accuracy of the predictions was satisfactory. The resources were considerably underestimated in 76 cases and overestimated in one case. The study of 25 intakes from alluvial aquifers revealed that the resources were considerably overestimated in 20 cases. In all 114 cases the estimates of ground water resources were approved by the Central Commission on Ground Water Resources of the U.S.S.R. consisting of highly experienced hydrogeologists. In most of the above example, the professional expertise was combined with model calibration, and this fact aggravates the situation even more.

It may be consoling, at least in part, that the other fields where the completeness of geological information is essential share the same plight. One of the most well documented examples demonstrating that the uncertainty of geological modeling is not just an abstract issue is the complete failure of geophysical data interpretation relating to super-deep drilling at Kola Peninsula, Russia, (Kola 1984) and in Bavaria, Germany (Kerr 1993). As drilling revealed, actual geological structures differ completely from those anticipated. The same happened to the super-deep bore in Azerbaijan (Kola 1984). These failures cannot be explained by the scarcity of data or unsatisfactory qualifications of the interpretational teams. In such expensive enterprises as super-deep drilling, the teams certainly were the best, and the data (with respect to their amount and quality) exceeded what is available in routine enterprises. The failures were caused by the use of the "sound, fulfilling all necessary conditions, and just good enough" models recognized by the professional communities, but nevertheless fallible. Bredehoeft (2005) calls this "the conceptualization model problem" and gives several examples from his and his colleagues' hydrogeological practice in the United States. Problems, including civilian and economical, related to uncertainty of predictions made by experts in seismology are discussed by Geschwind (1997) and Hanks (1997), and many others. Unfortunately, professionalism and credentials do not always warrant the confidence in models and simulation results.

The view point that the engineering confidence is good enough to trust predictions is usually grounded on two groups of arguments. First, during their studies and professional activity, practitioners accumulate knowledge and develop thorough professional experience on where and how geological and mathematical models should be applied to yield practically meaningful results. We have discussed this kind of arguments above.

The second one is that all human progress is founded on the use of invalidated or even provably incorrect models. Indeed, it is true that "astronomers, on the basis of a few days of observations, will predict asteroid and comet orbits for thousands of years with good accuracy" (McCombie and McKinley 1993). Their argument can be even strengthen by mentioning one of the greatest achievement of those models: Le Verrier's discovery "on pen's point" of Neptune based on peculiarities of Uranus' orbit. He calculated the orbit of the unknown planet, and Neptune was discovered exactly at the location he predicted.

Somehow, it is less known that Le Verrier explained in the same way the peculiarities of Mercury's orbit (Levy 1973). This hypothesis was never confirmed. Its failure gave birth to several other hypotheses that failed also. It is recognized at present that Einstein's theory of relativity explains Mercury's behavior. My point is that there has never once been a need to revise astronomic models.

Effective modern technologies based on models which are impossible to validate can be included in this argument also. However, each such technology undergoes extensive testing, and then when it is applied, e.g., in manufacturing new products, special attention is paid to controlling the quality of raw materials, to assembly, and to other pertinent procedures. Final products are also tested. For example, each airplane and ship undergoes thorough tests before their practical use.

In hydrogeology we do not have such luxuries. Each hydrogeological site is unique. We cannot control its geological structure or even know the structure in full. Its response is also unique and depends on impacts. The impacts can be intensive and diverse, and many of them do not have analogs in the past. We do not have long enough periods of observations, and no prediction for a period of more than a 100 years has actually been tested. In science, if a hypothesis is proved to be wrong, another hypothesis takes its place, then another, and another, etc. In hydrogeology it may be too late to seek another model when it becomes clear that the applied one is faulty.

Professionalism is a necessary condition for obtaining meaningful results especially for development of geological models. As Tsang (1992) points out, a sick person should go to an expert having an M.D. degree. However, faith that the professional judgment is always true is also a fallacy.

Finally, let me repeat. If a professional has experience obtained on many similar projects in similar environmental and has observed the results of implementation of those projects, it could be reasonable to trust in the professional's judgment. Often such professionals do not need any mathematical modeling, they just know what works. (In Athens, Georgia, where I am typing these lines, I have never seen geological engineering or geotechnical explorations supporting projects for developing residential middle-class neighborhoods. The builders just know what kind of foundations must be used). However, in the case of the objects which are very expensive and carrying large environmental and financial risks, it is difficult if impossible to find a professional with the pertinent experience. Even if such professional exists, it is not reasonable to rely on his or her subjective opinion. We need models (quantitative theories) to predict what can happen, and of course we need professionals for developing conceptual geological models. However, if the professional's judgment about the uncertainty related to the use of some model in some situation is supported by the pertinent statistics, it should be taken in consideration. When such statistics is not available, nothing could be said about the quality and the uncertainty of the results obtained in the framework of the engineering approach.

However the contemporary computational technique and methods permit the development of a surrogate of the engineering experience. The surrogate cannot provide the provable estimate of the uncertainty either. But it permits more informed decision-making (see Chaps. 5–10).

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