Berlin, 18.09.2002

Sub.: 23rd Oral Discussions
here: Various Contributions
Ref.: General Information

Dear Admiral Grazioli,
dear Dr. De Bernardis,

having returned safely back home after an enlightening journey following Goethe's footprints through your wonderful country, though only a very small part, and a stimulating conference my wife and I would like to thank you for all the arrangements during the conference and the social program, which we thoroughly enjoyed.

Attached please find my contributions to the various Sessions according to your specifications on page 3 of the General Information. Conveniently I send this letter as an attachment to an e-mail, so that a separate diskette will not be necessary.

With best regards sincerely yours,

*Michael Schmiechen.*

Attachments: Oral discussions
The SC Report deals with traditional methods for evaluating traditional trials in great depth, essentially without drawing any conclusions. But the Report hardly mentions other methods, referring only to the Proceedings of the 2nd INTERACTION Berlin '91 devoted to the METEOR project, far beyond the scope of the SC work. Therefore I would like to draw the attention of the Conference to rational procedures for evaluating trials.

At first I mention the development of a rational method for evaluating traditional trials, which has been triggered by the Japanese proposal for an ISO standard five years ago and has since reached a state of maturity. During the same time ISO Committee Draft 15016 became a standard proper, although since the early stages of discussion the procedure proposed has been shown to lead to inconsistent results.

Secondly I like to draw the attention of the Conference to the rational method for evaluating 'rational' quasisteady trials already applied in the METEOR project. This method has finally reached a state of maturity only during the last half year, triggered by a seminar I gave at the Gdansk Ship Model Basin in January 2002 taking into account all the lessons learned during the past fifteen years.

For those interested in the development and I brought a short leaflet with references to all the related material on my website http://www.t-online.de/home/m.schm. For those who do not like to surf the Internet and to download files I brought a longer version with all the details of the evaluations printed on paper.

The essential point of the rational procedures is to get away from the ever more detailed models generating more problems than solving them and to move towards highly aggregate models with only a few parameters to be identified from the few data available. This permits to evaluate trials without reference to model test results and other prior information, as it should be. Unless we start evaluating trials as objectively as possible we cannot reasonably talk about scaling.

The problem is not so much to analyze random errors, but the dominant problem is still to avoid systematic errors. As examples I only mention the determination of the current, already treated in a Written Discussion presented at the 22nd ITTC, and the resistance in wind and waves. The current can be identified reliably together with the powering characteristic in the behind condition. After this the power can be correlated with the wind and wave parameters, resulting in wind and waves resistance coefficients 'automatically' taking into account systematic errors in wind and wave estimates.

This contribution does evidently not only relate to the Report of the SC on Speed and Powering Trials but to Reports of the Committees on Propulsion, on Resistance, on Procedures for Models Tests and on Test Procedures for Waterjets.
In listening to the very interesting presentation four questions or rather remarks came to my mind:

In some methods for evaluating the powering performance at the narrow service conditions (Abkowitz, Kracht) a wide range of test conditions, including the condition of zero thrust, is required to provide the data necessary. Am I right to assume that this is not the case in the method presented?

In the 'hybrid' model the inertial term is missing. So the question arises: Is the inertia being treated statistically, assumed to vanish in the average? Some forty years ago in a Japanese study it has been shown, that even very small accelerations, less than a thousand of a g, may easily upset the momentum balance. And I have observed that taking averages or, even worse, relying on ill-defined averages provided by somebody else may be 'exactly' the wrong thing to do. Traditional methods usually rely on steady conditions and thus these have to be 'established' or constructed!

Concerning the thrust deduction 'axiom' adopted, also used by Kracht, I have very strong theoretical reservations, which I have discussed in detail in a paper to be found on my website. The thrust deduction fraction does depend on propeller loading and on scale. The differences may appear very small in the first place, but due to the differentiating nature of our analysis the final results differ quite considerably.

Finally, I am of course very happy that, after all, quasisteady testing has been successfully implemented and shown to provide 'useful' results, exactly as I have promised for nearly two decades now.
Again I want to take the role of the *advocatus diaboli* and propose an alternative approach. While the SC is completely relying on the momentum flow approach I want to mention that the energy flow approach, advocated by former Committees, has dramatic advantages. 'Of course' the two approaches have to complement each other adequately in any particular case.

The traditional, naive view of a thruster overcoming the resistance of the vehicle to be propelled may be quite adequate for conventional hull-propeller configurations. But in case of hull integrated propulsors the approach of pump designers is much more adequate, not only for the evaluation of the powering performance, but even for the design, as has been shown in a project on ducted propellers carried out at VWS, the Berlin Model Basin, and discussed in a number of papers to be found on my website.

The starting point is the condition self-propulsion, of overall zero momentum flow, essentially the effective resistance, and the corresponding net power to be fed into the flow. As in pump design everything else is being dealt with in terms of energy flows and the thrust and all interactions are being treated implicitly observing the optimum condition from the beginning! As in pump design the thrust comes in only at the end, as a nasty by-product. All pumps develop thrust and need thrust bearings. Although pump designers do not want to produce thrust, they cannot avoid it and have to know it in order to design the bearing.

Even traditional open screw propellers may be looked at in the same way. Introducing the concept of equivalent propellers feeding the same net power into the same flow one obtains a relationship between the 'displacement' wake and thrust deduction fractions, implying that both 'together' are energetically neutral. Thus wake and thrust deduction as such are not useful performance criteria as has been pointed out on various occasions.
The SC Report deals with the well known scale effects in model screw propeller performance essentially without drawing consequences. The usual 'way out' is to perform open water tests, even with wake adapted propellers, at 'sufficiently' high Reynolds numbers. But in model propulsion tests the propellers are usually run at much lower Reynolds numbers, though in the behind condition. And the powering performance analysis is based on these two sets of incoherent data!

This is the simple reason that since many years I am promoting the powering performance analysis solely based on propulsion tests, on model and full scale (!) propeller performance in the behind conditions. Accordingly, in order to perform the powering performance analysis in the traditional conceptual framework, axioms, constitutive equations, have to be introduced for wake and thrust deduction, permitting the robust determination of their values. As has been mentioned in the Session on Speed and Powering Trials this goal has finally been reached in the rational evaluation of quasisteady tests, model and full scale.

In this connection I would further like to point out, that four of the traditional methods for evaluating traditional trials discussed by the SC on Speed and Powering Trials are based on model propeller open water tests. In view of the scale effects in propeller performance and wake (!) this is neither desirable nor necessary, as has been shown. I wonder what the opinion of the Propulsion Committee is on these matters.
Discussor: Michael Schmiechen

To Technical Committee: Stansberg : TC on Waves

Please let me mention a basic problem of ‘craftsmanship’. Spectra and power spectra are usually determined from finite sets of sampled data. Consequently unacceptable systematic truncation errors, classical uncertainty, may be encountered, if FFT algorithm are applied without scrutiny. These errors, particularly in spectral peaks and tails, can be avoided, if auto-regressive models are being employed.

The errors mentioned may be acceptable in ‘quick and dirty’ work, but in scientific investigations it is certainly of importance to base conclusions on correctly determined spectra. The algorithm FTP (Fourier Transform Proper) has been published in Schiffstechnik 1999 and is to be found on my website. For routine applications, if any, the algorithm may need to be optimized for speed.

This is a Written Discussion which has not been submitted before and has not been presented at the Conference. But the algorithm may be of general interest.
Concerning squat I would like to draw the attention of the Conference to a very large project contracted by the German Wasserstrassen-Direktion Nord at Kiel in the early nineties with the goal to obtain a sound basis for decisions on designing and utilizing waterways. In view of the more than ill-defined traditional 'formulae' full scale squat measurements have been carried out with five Container ships on the River Elbe in five measuring sections, each two kilometers long.

VWS, the Berlin Model Basin, in fact I personally was in charge of the formidable task of the overall evaluation of the vast amount of data and finally was permitted to publish some of the results in a Squat-Workshop held at the Fachhochschule Oldenburg/Elsfleth in November 2000. The paper and its presentation, both in German, are to be found on my website. The essential result is that the full scale results confirm Tuck's theoretical results. After careful elimination of systematic effects the evaluation included not only the establishment of confidence ranges, but checks of statistical distributions.