Dr. Wagner's contribution

Response by Michael Schmiechen

Note: The following response refers to the contribution by Dr. Klaus Wagner of Rostock. In view of this detailed response the translation of the German original is felt to be unnecessary.

Since the 2nd INTERACTION Berlin '91, when we first met, Dr. Klaus Wagner has actively taken interest and part in the development of the various applications of my rational theory of hull propeller interaction, purposely meeting current standards of theory formation in order to arrive at lasting solutions.

That work started with my inaugural paper 'Eine axiomatische Theorie der Wechselwirkungen zwischen Schiffsrumpf und -propeller. Fritz Horn zum 100. Geburtstag gewidmet' (Schiffstechnik 27 (1980) 2, 67-99), which Dr. Wagner saw earlier and put it into his private archive. The underlying ideas on hull-propeller interactions have been the result of my first experimental task at the Berlin Model Basin.

My report on 'Modellversuche mit Kort-Düsen für Seeschiffe' (Abschluss-Bericht zum ERP-Vorhaben S 1100. 1961) has *not* been included in the list of official VWS Reports, but was banished into the basement. My observations did *not* conform to my superiors' prejudices, still widely entertained, but later resulted in the fundamental thrust deduction theorem and the corresponding convention.

Over the past decades my correspondence with Dr. Wagner has at times been extremely intense, his critical questions often taking me weeks to come up with answers satisfying both of us, either to reconstruct my own earlier results or to depart from remaining cherished prejudices, as in the present case; see below.

Having been concerned with powering trials to 'prove' the designs of his team, he has been one of the few colleagues intimately familiar with the inherent deficiencies of the traditional procedures and thus immediately envisaged the potential of quasi-steady trials to solve those problems efficiently, requiring no extra instrumentation and no extra time and cost intensive manoeuvres.

The hurdles concerning the use of my results he mentions, the use of a formal language adequate for the problems at hand and of the corresponding intuitive (!), powerful programming environment Mathcad, originally MathSoft version 8.3, now PTC version 15.0, may be stumbling blocks for traditionally trained, not to say indoctrinated colleagues, but not for young professionals, familiar with different programming systems.

The fundamental need for developing an appropriate language in recon-

structing current traditional, but deficient conventional practice has been drastically addressed by Paul Feyerabend in his paper on 'How to be a good empiricist' (1999/101 f):

"Presence of synonymy, intuitive appeal, agreement with customary modes of speech, far from being *the* philosophical virtue, indicates that not much progress has been made and that the business of investigating what is commonly accepted *has not even started*."

As Dr. Wagner did, anybody else seriously interested in the solutions I have successfully developed to maturity and repeatedly applied in delicate cases, will have to try the solutions himself in his own way. The large number of explanatory notes for any taste, I have published since 1980, may be helpful, but the proof of the pudding is in eating it yourself.

Since my *opus magnum* is now freely available on my website, the 'readable' Chapter 23 'Propulsion mechanics' Dr. Wagner is referring to, is now readily accessible and does not need to be published separately. Further, the Proceedings of the 2nd INTERACTION 1991, with the complete METEOR Report and discussions, and the three volumes of the METEOR Festschrift of 2013 2014, 2015, with detailed examples and many explanatory documents, are also to be found on my website.

In view of his various numerical exercises, listed by Dr. Wagner and provided on request, a warning concerning simulated data is in place here. Such data are not useful to prove the adequacy of my rational conventions (!) and of the identification of the corresponding parameters, in noisy environments in particular (!).

At the end of his contribution Dr. Wagner explicitly states, that he is not yet satisfied with my solution on the 'third floor of my building' he has detailed in his appendices A01, A02 and A03. He still calls my identification of the thrust deduction fraction (file mod_7_hpi_rev5.pdf) still a 'weak point'.

But as I have explained in my final solution, most of the problems he mentions do not exist 'any longer'. The remaining problem, to identify the values of the hull-propeller interactions without reference to the results of thrust measurements does not exist on model scale, but only on ship scale, where reliable values of the thrust cannot be obtained in practice.

The solution of this 'last' problem of my rational approach turned out to be much simpler than expected and is demonstrated in every detail in the pertinent worksheet mod_7_hpi_rev5.pdf. But, as Dr. Wagner noted, it suffers from an unacceptable, implicit (!) convention concerning the relationship between nominal thrust deduction and wake fractions.

And in further discussions I noted further deficiencies due to incoherent approximations of the thrust deduction theorem. Consequently I have started

work to remove the resulting weakness.

Further, after the 'final' validation of my procedure on model scale by comparing the results of the rational procedure with those of the traditional procedure (file mod_8_trad_rev5.pdf), I shall try to find out, whether the published data of the quasi-steady METEOR trials of 1988 are sufficient for definite full scale validation.