Future Ship Powering Trials and Monitoring Now!

Principles of rational conventions further clarified,
consistently applied in a particularly delicate case
and lessons (to be) learned

A Letter to my colleagues and my students
and to whom it may or must concern,
ship owners, ship buyers and ship builders,
member organisations of the STA Group
and governing bodies and pertinent committees
of ITTC, ISO and IMO in particular

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NOTES

The pdf-file of this draft is to be found on my website the beginning of the sub-section 'News on ship powering trials'. For convenient off-line reading the pdf file may be printed as DIN A5 brochure. Use the landscape format to keep the margins all right, amply provided for your notes, but do not turn the first output of the printer, even if requested!

The original doc-file, including hyperlinks to all the material referred to, has also been converted into an html-file, preserving the live links, but parts of the layout have been lost, the line numbers in particular. Further links are to be found in the annotated documentations of all my papers and related written discussions on Propulsion in general, on Ship powering trials and on Ducted propulsors in particular.

Substantial, critical contributions to the discussion will be welcome and may be published together with the final version of the paper at my discretion. In any case suggestions and arguments put forward will be considered, duly referred to and acknowledged in the final version of this paper.

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ABSTRACT

Naval architects are predicting the powering performance of ships at design and at ballast conditions traditionally based on results of model tests and/or, more recently, on results of numerical calculations.

But using any of the traditional trials codes as standardised, *e. g.*, in ISO 15016: 2002-06, and more recently in the not yet approved ITTC 2012 Guidelines, based on the 'industry standard' marketed by MARIN, they cannot prove that their predictions are correct, *i. e.* trustworthy demonstrating full scale performances and improvements, they are 'promising', within the narrow confidence limits required for many purposes today.

The reason for this state of affairs is that 'theoretical' naval architects have been and still are so fascinated and absorbed by the possibilities provided by CFD, computational fluid dynamics, that they missed to take notice of the threatening problems around and ahead of them. 'Consequently' they neglected to develop an appropriate theory of ship propulsion to overcome the 'dreadful' problems and to improve the efficiency of research, teaching and testing.

They are mistaking CFD as well as SID, systems identification, for ship theory not realising that both of them are 'only' two, though completely different ways to determine values of the concepts they are using, without wondering where these concepts came from. Their concepts have not been handed down from heaven, but have been inherited from their grand-grandfathers.

Thus, *e. g.*, *all* traditional trial codes mentioned are still based on the naïve model of hull-propeller interaction based on the Newtonian balance of forces and still inconsistently interpreted by Froude's conventions, if possible at all, definitely not on full scale and not at ballast conditions, and/or relying on values of parameters often to be sucked from their thumbs.

How the traditional conceptual framework can be interpreted consistently, how the powering performance can be monitored in every detail, even on full scale under severe service conditions, based on a theory conceived in 1980, I have demonstrated in the METEOR project, the tests in the Greenland Sea performed in November 1988, twenty five years ago now.

Following the principles stated in 1980 the search for simple, acceptable conventions replacing Froude's conventions, *i. e.* hull towing and propeller open water model (!) tests, in case of monitoring the powering performance on full scale and model scale has of course reached its final goal only as a result of further intense thinking. Instrumental has been the experience gained in repeated analyses of a 'model' test, performed in 1986, prior to the

METEOR tests, undertaken to demonstrate the feasibility of quasi-steady testing promoted.

Much later, in 1998 I have proposed a rational solution of the much simpler problem, the evaluation of traditional powering trials. And I have shown that it is not only feasible, but permits reliable evaluations of trials, even if all traditional methods are doomed to failure. This has again been shown in the recent evaluation of trials with a bulk carrier in ballast condition at two different trim settings 'including' propeller ventilation, further extended insights to be discussed and illustrated by results.

The approach promoted avoids the unacceptable deficiencies of the traditional trials codes by adopting the Lagrangean approach, phrased 'only' in terms of shaft powers supplied and required, thus accounting for the fact that usually only power measurements are 'available' and/or meaningful for assessing the powering performance.

In the Lagrangean approach the concept of thrust, including the energetically neutral component balancing its own suction at the hull, does not 'occur' at all; it is not even mentioned. As in case of the design of energy wake adapted ducted propulsors thrust is not a useful measure of propulsive performance.

And most important, contrary to all traditional codes, no model test results and no other prior data whatsoever are required, as it must be for the rational resolution of the 'conflicts' at hand. The method is solely based on extremely simple conventions and their few parameters to be identified professionally from the data observed.

The naked marine engineering pragmatism followed and the simplicity reached serve the dual purpose to permit the stable, 'objective', *i. e.* observer independent identification of the parameters introduced and to be as 'self-evident' as possible and thus acceptable not only for theoreticians of naval architecture, but for practicians in model basins and ship yards, and, last but not least, for ship builders and owners as well.

Although my research has been primarily concerned with the rational solution of 'technical' problems its results will have a disruptive impact on the rational resolution of contractual conflicts. In view of the objective, observer independent evaluation of trials developed ship owners and buyers need no longer to accept and sooner or later will no longer accept the same people providing the predictions of the powering performance and accessing the delivery trials 'as well'.

As has been shown the powering performance at trials conditions reduced to the nominal no wind and waves condition can be established right after the trials transparently and objectively, independent of the observer and of any prior data, solely based on the observed data. Accordingly ship buyers are thus well advised to contract for meeting the predicted performance at the trials conditions instead of at the design conditions. The details of the predictions and the consequences of differences between the measured values are (then) no longer subject of the assessments of the trials, but solely of discussions between the contracting parties.

The aim of the paper is to demonstrate the power of the axiomatic approach, permitting to solve fundamental problems of ship theory impossible to be solved by the traditional approaches. The exposition will refer to clear 'visions', 'Anschauungen' in Goethe's spirit, to simple principles and common sense, so that even those trained in the traditional way can understand the approach and take advantage of it in solving their own problems.

The paper will stress, that the departure from the inherited traditional approach will result in dramatic gains in efficiency and quality of research and teaching, that the costs for testing on model scale and on full scale can be drastically reduced, the reliability of the results increased at the same time, that these considerable returns are to be obtained for the small effort of using only some common sense, and that the 'disruptive innovations' (MIT Technology Review) outlined are in the interest of the industry we all serve.

CONTENTS

"First things first, and do them now!"
The instant decision maker's basic rules.

1 INTRODUCTION

1.1 PROBLEM

Naval architects are predicting (not only) the powering performance of ships at design and ballast conditions traditionally based on results of model tests and/or, more recently, on results of numerical computations, the latter even referred to as 'numerical sea trials' (Hochkirch, 2013). The large variety of 'software trends' in the maritime industry has been discussed in about sixty papers presented at the recent COMPIT 2013 (Bertram, 2013).

But using traditional trial codes, as *e. g.*, standardised in ISO 15016: 2002-06, or in the not yet approved ITTC 2012 Guidelines based on the 'industrial SAT standard' aggressively marketed by MARIN to shipping companies, classification societies, even a research institute, and IMO (van den Boom, 2013), they cannot 'prove' that their predictions are correct within the narrow confidence limits required for many purposes today, *e. g.*, trustworthy demonstrating the performances and improvements they are promising.

Naval architects at research institutions have left these fundamental problems to the practicians in model basins and ship yards, not realising how difficult the problems are, that all procedures based on the traditional conceptual frame work are inherently inconsistent and error prone and that their results are thus no longer acceptable.

1.2 MODEL

The purpose of trials is to resolve the 'conflicts' between parties interested in the results, *e. g.*, ship buyers and ship builders. In order to serve this purpose, the results have to be objective, *i. e.* observer independent, reliable and acceptable for both parties. And this can be achieved rationally only by conventions so simple, self-evident and transparent, that the parties readily agree upon them and, last but not least, on their consequences!

If set-up professionally conventions 'happen' to be 'nothing else but' shared coherent, formal languages. In terms of logics these are 'nothing else but' axiomatic models, a frightening name for very practical, very powerful tools. Their basic sentences are the axioms, 'another name for prejudices' as Mark Twain appropriately noted.

And Bertrand Russell stated in 1912 that all our knowledge, *e. g.*, in classical mechanics, is based on 'instinctive beliefs', prejudices, working hypotheses (to be) agreed upon. This fact is not usually explicitly taught to engineers and thus often 'comes' as a surprise.

In terms of the 'theory of theories' axiomatic models are constitutive models, 'constituting' the micro(!)-universe of discourse, *e. g.*, ship propulsion and ship powering trials and monitoring in particular, to be discussed in this paper. Accordingly I have also used the term 'constitutive equations' instead of 'axioms' or 'conventions', if appropriate in a given context. This pragmatic point of view and its implications, underlying the following exposition, may be most acceptable for practicians in ship yards and model basin.

1.3 Goal

The goal of the present paper is to explain and illustrate the state reached and to *stress the lessons learned and further clarified in many detailed analyses of data and in exposés and discussions on the underlying principles*.

Of particular interest are the insights extended during the recent evaluation of trials with a bulk carrier in ballast at two different trim settings, with the propeller even ventilating up wind and waves at the smaller trim by the stern, *i. e.* at very small nominal submergence.

The goal is not to repeat statements on my early interests in assessing the performance of propulsors (1961, 1966, 1968) or to repeat general surveys of the theory, of its development and of its reception presented earlier, *e. g.*, on the occasions of MAHY 2008 at Visakhapatnam, of SMP '09 at Trondheim and other symposia.

1.4 Plan

In order to reach the goal of the paper the plan is to discuss all the pertinent problems in the informal fashion of a letter addressed to my colleagues and students, asking them to follow the exposition of the simple ideas underlying my work, being referred to not only by selected bibliographical references, but also by hyperlinks permitting to access the sources by mouse clicks.

With utmost care I have phrased the arguments, trying to avoid all professional ballast and to arrange them in methodical order (Janich, 1997). As carts cannot be put before the horses, problems cannot be solved by starting from the wrong end, or by confusing all the inter-related issues and all the difficult sub-problems, each to be solved professionally.

Following this introduction the material will be arranged in the chapters shown in the 'live' table of Contents, serving as 'Subject index'. The exposition of the fundamentals of conventional approaches, traditional and rational, will be followed by the discussion of the rationalised Newtonian approach, the naïve conception of propulsors as thrusters, and its applications in detailed monitoring of the powering performance on full scale and on model scale, respectively.

Subsequently I shall discuss the alternative Lagrangean approach in terms of powers, based on the conception of propulsors as pumps, and its applications in traditional 'speed' trials, mentioning the application of this conception in propulsor design only by the way.

In various earlier expositions of the theory of propulsion I have followed the reverse order, starting with the theory of traditional trials, to demonstrate unmistakably and without doubt, that for the reliable evaluation of trials not even the most elementary ship theory is necessary! In any case I shall try to state the lessons (to be) learned as concisely as possible.

2 CONVENTIONAL APPROACHES

2.1 BASIC PRINCIPLES AND RULES

At the end of the chapter titled 'The basis of all dialectics', the third of the introductory chapters of his 'Art of being always right', a collection of thirty eight rhetorical stratagems, Arthur Schopenhauer (1896) explicitly states *the most fundamental rule of all cooperative (!) problem and conflict solving*:

> "… in every disputation or argument on any subject we must agree about something; and by this, as a principle, we must be willing to judge the matter in question. We cannot argue with those who deny principles: *Contra negantem principia non est disputandum*."

What 'we must agree about' are conventions, essentially *languages, maybe informal, called traditional, or maybe formalised, called rational conventions.* I note explicitly, that 'conventional' and 'traditional', though usually used as synonyms, are two completely different concepts. All our theories are based on conventions.

Traditional conventions are not necessarily explicit and thus often not coherent, but inherited, 'instinctive beliefs', as Russell called them, phrased in the versatile 'natural' languages and professional jargons, often 'grown' over centuries. By contrast, rational conventions are explicit and simple in order to be transparent and thus readily acceptable, phrased in terms of consistent formal languages, permitting to follow and check the derivation of the consequences to be accepted.

Rational models are reference 'frames', generalised 'coordinate' frames, and their (phenomenological) parameters are the 'coordinates' of the systems investigated in the context of the model adopted. The naïve idea of 'true' values of concepts, *e. g.*, of resistance, independent of a reference frame to be agreed upon, is obsolete, as I have stated and explained over and over again from the beginning of my 'formal' work on the rational theory in 1980.

The outstanding advantage of rational models is that they do not require any prior values of the parameters whatsoever, but they rely solely on the values of the few relevant parameters to be identified from the measurements taken, 'hopefully' professionally. Frequently the latter is not the case due to the widely met lack of craftsmanship and ignorance of the most fundamental 'facts'; *e. g.*, of the theory of systems identification.

At my age I am of course not so naïve to believe that everybody is aiming at the rational solution of problems and the rational resolution of conflicts. But I shall not discuss well understood 'reasons' for 'not willing to agree about' conventions and their consequences, not to take part in the joint, rational solution of problems. It may suffice to note, that all persons 'concerned' are not only colleagues, but are also competitors in markets.

A recent example of this fact is the promotion of the 'industrial SAT standard' by MARIN and its 'cooperation' with the re-established 'ITTC Specialist Committee on Trials and Monitoring', now 'on Performance of Ships in Service'. The ITTC Guidelines, based on that procedure are stated, to be 'Approved by 27th ITTC 2012', although the Conference, that may eventually approve, or probably not, will take place only in 2014.

2.2 Intellectual discipline

According to the fact, that *problems can never be solved by the methods, which have caused them*, I have not phrased the solutions of basic problems of ship theory in terms of the traditional jargon of naval architects, but in terms of the rational jargon of generally accepted principles and of common sense, which every body, even high school students, can easily understand and accept – maybe except naval architects trained the traditional way.

Their handicap is that the neuronal networks under their skulls have been 'indoctrinated' according to a conceptual framework, which has been adequate for traditional hull-propeller configurations, but even for those inadequately interpreted operationally by Froude's conventions. And from my own experience I know, that it requires extreme intellectual discipline to change 'hard wired' connections of neurons and overcome the doctrines still taught world-wide.

Rigorous discipline is widely considered to impede creativity, although 'exactly' the opposite is true. But who dares to ask for discipline today, when even professors *expressis verbis* declare, that they do not intend to read what I have written, forget about understanding and admitting, that I have solved problems, which they still ignore and which cannot be solved by the traditional methods they are still teaching to future problem solvers.

'Nobody' can seriously believe that this self imposed ignorance increases 'his' own credibility. With this lack of curiosity, of imagination and of judgement it becomes difficult to admit, having for decades repeated what ignorants have told (you). As long as colleagues still walk around in the conceptual costumes of our grand-grand-fathers and indoctrinate our grandchildren accordingly, I shall continue to work for our grand-grand-children.

If I personally do not understand an idea, that somebody is proposing, developing and promoting for decades, I am not smiling pitifully at the old man, but try very hard indeed, until I understand, *what* he is saying and *why* he is saying so. I never believe, what other people tell me about a paper on a subject, but *I care and dare to think myself.*

'*Sapere aude*' has been the motto of rationalism not only since Immanuel Kant, but since the Greek philosophers two and a half thousand years ago. The rule, to '*conveniently discuss problems only with people, who also do not know anything about the subject*', though widely followed, belongs to the particularly stupid rules of 'research'.

2.3 THEORY OF THEORIES

From the theory of knowledge I knew that the axiomatic approach was the only way to go. But even knowing examples from the history of science I did not imagine how powerful and fertile this method was, even in ship theory. It kept me busy for more than thirty years, in fact more than fifty now since my first model tests 1961 on ducted propulsors, to develop at least some branches of the theory to maturity. But to my surprise none of my colleagues joined me in my effort during the past decades.

When colleagues ask me to provide my theories without deducing them from the underlying 'philosophy', without the meta-physics necessary to understand, what has to be done, they ignore the facts, that 'nobody' gets along without 'philosophy' and that their own ill-defined philosophy, inherited from their grand-grand-fathers, 'happens' to be obsolete, no longer serving today's purposes.

Everybody knows that there is nothing more practical than a good theory, but hardly anybody knows, that there is nothing more powerful than a sound philosophy. As my results show, only little knowledge of the rational theory of theories is sufficient to solve fundamental problems professionally.

Contrary to the opinion of Uwe Hollenbach, explicitly expressed in a letter to Klaus Wagner, I do not believe that the exposition of the fundamentals and the appropriate reference to my pioneering work, the words (!) of which Hollenbach has used in the presentation of his paper of 2008, would have shied the clients of HSVA away. Sooner or later the same clients will no longer accept to be treated like stupid, prompt payers.

They will ask for trustworthy results obtained cheaply by model propulsion tests of only two minutes duration and for the corresponding trustworthy confirmation full scale by rationally evaluated traditional trials or, much cheaper, by quasi-steady tests full scale as well, of twenty minutes duration, without any body noticing trials and monitoring tests taking place!

2.4 Coherent interpretations

The most important rules are to draw up conventions as axiomatic models, as formal languages proper and, *only in a second step, to interpret the concepts introduced in the context of the formal languages*. Any incoherent measurements of magnitudes introduced cause new, unnecessary conflicts resulting in further irresponsible waste of intellectual and financial resources.

Although even naval architects ritually repeat that in an 'orderly' exposition the concepts have to be 'defined' before being discussed, the second rule is the most difficult for them to understand and to accept. Whenever in presenting a model at the Institut für Schiffbau in Hamburg I introduced a concept I immediately have been interrupted by the question: 'And how are you measuring it?'

Hull towing and propeller open water tests have already been mentioned to 'produce' incoherent results. Attempts to measure the hull speed through the water by 'some' method is another example of this unprofessional approach, to be discussed in detail further down. The same applies to 'smartly' invented 'thrust meters'.

I have explained the reason for my approach meeting the simple facts of the theory of knowledge in my letter to the convener and in my ISO '98 Proposal. Both documents have been filed by JISC/JMSA as 'Prof. Schmiechen's comments to ISO/TC8/SC9/WG2/N20, Informative' under ISO/TC8/SC9/WG2/N28, dated 1998-06-23.

The reason for my comments and proposals being qualified as 'informative' only is, that as a private person, not 'authorised' by the German group, I was formally not 'permitted' to approach the Convener. And for the same reason I have already been excluded formally from future, long overdue revisions of ISO 15016, finally being felt necessary, and related discussions of the German group! How long are we going to follow, to afford this and other incredibly inefficient 'bureaucratic' procedures?

2.5 Lessons (to be) learned

The important insights to be noted at this stage are

- that the most fundamental task is to set up rational conventions adequate for the purposes at hand and so simple and self-evident, that they and their consequences are acceptable for the all parties interested in the results,
- that the interpretation of the concepts and parameters introduced has to be completely separated from the construction of the axiomatic models, of the formal languages proper, and

- that the concepts and parameters introduced are to be identified only in the contexts of elementary mechanics and of the models or languages adopted.

3 BALANCE OF FORCES RATIONALISED

3.1 STATE OF THE THEORY

3.1.1 BASIC CONCEPTS INTRODUCED

The traditional, naïve concept of a propeller is that of a thruster overcoming the resistance of the hull to be propelled. And thus this traditional point of view in terms of the balance of forces may be called the Newtonian approach.

More appropriately it should be called the Eulerian approach, based on the balance of momentum, of convective momentum flows, diffusive momentum flows, *alias* surface forces, and momentum storage, *alias* inertial forces. In water momentum production, *alias* body forces, cannot 'normally' be realised, but they play a considerable role as convenient substitute models in theoretical and computational hydromechanics.

Accordingly the basic concepts underlying the powering analysis are the hull resistance R_H at a given hull speed V_H through the water, the shaft thrust T_S and shaft power P_S of the propeller in the wake w behind the hull. Shaft thrust and power and the hull speed over ground V_G are considered to be 'directly' measurable.

The difference between the hull speed over ground and through the water is the unknown current velocity V_C. The reliable determination of its values will be subject of the following chapter. *But at this stage it is already mentioned that if this problem has not been solved professionally, any further evaluation of the powering performance is not trustworthy!*

In order to determine the value of the hull resistance with the propeller in operation, but without the suction caused by propeller operating, and the value of the wake conventions have to be introduced. According to Froude's conventions values of the hull resistance are 'in principle' to be directly determined by hull towing tests and values of the wake are 'in principle' to be determined using the results of propeller open water tests.

The reason for the state of affairs is, that in Froude's days hull towing tests and propeller open water tests have been, and in all model basins still are, the only means to arrive at values for the hull resistance and the propeller advance speed, and thus of the corresponding thrust deduction and wake fractions.

3.1.2 Traditional conventions obsolete

This traditional procedure is still widely used in predicting the powering performance and evaluating ship powering trials despite its serious deficiencies. The disturbing fact of the tests mentioned is, that they are carried out at flow conditions 'totally' different from those at the propulsion tests.

And worst of all, hull towing and propeller open water tests cannot be performed under full scale service conditions, but only on model scale! But all these serious deficiencies have caused sleepless nights for only very few naval architects, most prominent among them Fritz Horn at Berlin.

Replacing Froude's conventions by extreme engine manoeuvres, *e. g.*, crash stops, as proposed by Martin Abkowitz and others, is going further along the naïve mechanical engineering approach. This suggestion is not only impractical, but also unacceptable for routine trials and monitoring and, most important, in view of the flow conditions, definitely totally different from the flow conditions at the service conditions to be investigated.

Horn came up with a procedure 'to determine the wake from propulsion tests', which has been tested in the Netherlands and in Japan and the results have been subject of discussions at the 4th ITTC held at VWS, the Berlin Model Basin, in 1937. But at that time inadequate conceptual, experimental and computational tools caused insurmountable problems (Horn, 1937).

That development had been completely disrupted by the war. Post war attempts at Wageningen, replacing propeller open water tests by tests behind grids as in cavitation tanks, have not been developed for routine application, being much too involved, not even trying to reach Horn's goal.

When I stumbled over the problem I proposed a solution, not only for the interpretation of wake, along a completely different approach and with power tools our forebears could not even dream of. Rational meta-physics was far beyond the horizon of naval architects and digital computers did not even figure in science fiction novels at their time.

When I was looking for a theory to solve the problems at hand I purposely did not 'ask' naval architects stuck, in the morass of their daily problems, struggling for sheer survival, but I 'asked' logicians and philosophers, knowing how to set up theories professionally.

The result has been my Schiffstechnik paper 'Eine axiomatische Theorie der Wechselwirkungen zwischen Schiffsrumpf und -propeller. Fritz Horn zum 100. Geburtstag gewidmet', published in 1980. It has been conceived, when I could no longer believe and accept the 'incredible' stories naval architects told me, and it was written at a weekend.

In the same year a closely related paper has been presented at the annual meeting of STG at Berlin. Its title 'Nachstrom und Sog aus Propulsionsversuchen allein. Eine rationale Theorie der Wechselwirkungen zwischen

Schiffsrumpf und -propeller' refers explicitly to Horn's earlier work; bibliographic details to be found in the References.

3.1.3 Horn's Copernican turn

The problem is to replace hull towing and propeller open water tests by conventions permitting to determine values for resistance and wake from propulsion tests alone, full scale and model scale in the same way. And the following solution promoted is based on the rational theory of hull-propeller interaction.

As axiomatic theory I have 'simply' adopted Rankine's elementary theory of ideal propellers, though not in open water, but in uniform energy and displacement wakes. This procedure, known as model based axiomatic approach, has the advantage that for the ideal case of an ideal propeller in uniform wakes the theory is 'correct' by definition, as it must be.

At this stage the concept of equivalent propellers comes in. Horn did not look forward towards the stern of the ship, but backwards, wondering what happened to the jet directly and far behind the ship, respectively. And he even 'designed' equivalent propellers far behind the ship in the energy wake, but 'outside' the displacement wake.

In analogy to 'Kant's Copernican turn' Horn's change of view may rightly be called his Copernican turn. As this analogy applies to many aspects of the present exposition a short explanation is quoted here for ready reference (Mertz Hsieh, 1995):

> "In the Prolegomena, Kant introduces a whole new method of doing philosophy, particularly metaphysics, which radically influenced all subsequent philosophy. Kant's paradigm shift is the 'Copernican Turn', which abandons study of (unknowable) reality-in-itself in favour of inquiry into the world-of-appearances and the innate structures of the mind that determine the nature of experience. According to Kant, only through an account of the a priori principles of the mind can knowledge be validated and objective, and thus lead to metaphysics as science, i. e. as an accepted body of knowledge."

Using Horn's idea, but without designing equivalent propellers in detail as Horn did, just observing the conditions of identical mass and energy flows, the theory of interaction permits to derive a thrust deduction theorem. Accordingly the thrust deduction fraction t is a function of the ideal, external or jet efficiency η_{TJ} of the propeller and the displacement influence ratio χ in the propeller advance speed through the water.

3.1.4 Rational conventions adopted

The thrust deduction theorem is much too intricate to replace hull towing tests, for the identification of the hull resistance. For that reason I have introduced the extremely simple, but very precise approximation

$$t = t_{TJ} \cdot \eta_{TJ} \ .$$

of that function as convention for the thrust deduction fraction in terms of the jet efficiency η_{TJ} of the propeller and the nominal thrust deduction fraction

$$t_{TJ} = \text{const} \ .$$

In various model tests the values of the resistance identified accordingly have been in close agreement with the values of the towing resistance, while the corresponding approximation

$$t_{TJ} = 0.58 \cdot \chi$$

has been found to be too crude to identify the displacement influence ratio.

A similar wake convention

$$w = w_{TJ} \cdot \eta_{TJ}$$

in terms of the jet efficiency η_{TJ} of the propeller has been introduced with the nominal wake fraction

$$w_{TJ} = \text{const} \ .$$

Further the convention of maximum hydraulic efficiency of the propeller

$$\eta_{JP} = \max$$

in the range of interest has been introduced and has explicitly to be observed as explained further down.

At the early stages of the development the axiomatic model and its usage have 'of course' not been perfect due to lack of experience. But the quasi-steady 'model' test performed prior to the METEOR tests, provided data permitting to continue the development. This concerns in particular the condition of maximum hydraulic efficiency. explicitly to be observed.

3.1.5 LESSONS (TO BE) LEARNED

The important insights at this stage to be noted are,

- that extremely simple thrust deduction and wake conventions are sufficient to replace hull towing and open water propeller tests model and full scale, and
- that simple rational conventions replacing Froude's conventions, are 'useful' not only on model scale but full scale as well, thus permitting *e. g.*, to determine scale effects in thrust deduction and wake experimentally, impossible using the traditional approach.

3.2 METEOR PROJECT

3.2.1 TESTS IN THE GREENLAND SEA

In the METEOR project, the quasi-steady tests taking place in the Greenland Sea in November 1988, the theory has been proved to permit the monitoring of the powering performance on full scale under service conditions and on model scale and thus to identify scale effects in wake and thrust deduction for the first and still the only time ever.

The results of the METEOR tests, derived from quasi-steady tests of only 20 minutes duration in severe sea states, have been subject of my International Workshop 2nd INTERACTION Berlin '91. All parts of the Proceedings are documented on my website under Papers on propulsion.

Even more than twenty years later the whole project and its implications are still far beyond the horizon of naval architects 'spoiled' by traditional training and are thus still subject of unqualified discussions and judgements.

3.2.2 THRUST (TO BE) MEASURED

I agree that you have to measure the thrust, if you want to analyse and/or monitor the powering performance in every detail according to the balance of forces. But 'nobody' can seriously expect to obtain anything for nothing! I have shown how full scale thrust measurements can be performed reliably.

METEOR tested under service conditions in the Greenland Sea in November 1988, picture taken during daylight lasting less than two hours.

If planned in time it is not very costly to install a short hollow shaft section professionally calibrated at least for thrust and torque in a laboratory. In case of METEOR a complete section of the shaft has been replaced by a hollow section, instrumented and calibrated as 6-component balance.

But please forget all the simple, 'smart' inventions, which definitely do not work. The last 'successful' one I have been referred to in the know-it-all attitude, that 'thrust measurements are no problem any longer', during a discussion at an STG meeting happened to have been scrapped long before it was still 'proudly' being 'sold' to me!

The fact that the thrust is hardly ever measured by appropriate balances confirms my earlier observation, that naval architects are so absorbed by their computational methods, that they 'simply' do not care for the proof of the pudding, for full scale measurements proving their predictions and promises.

And not only this! The conceptual framework developed has of course implications for the design of propulsors and other efficient applications of CFD methods not yet exploited!

3.2.3 QUASI-STEADY TESTING

And not to be forgotten, I have shown how quasi-steady tests have to be conducted professionally in noisy environments. In order to avoid systematic errors due to feedback of noise I have superimposed a saw tooth test signal, independent of the omnipresent noise, on the signal of the shaft frequency ordered. In that case the test signal did not even need to be recorded, but correlation of the data with time has been serving the purpose.

The amplitude of the shaft frequency variation has been only ten per cent of the ordered mean value. For fear of hysteresis the frequency of the test signal has been chosen at the lowest possible limit. A higher value would have been acceptable and would have increased the reliability of the results.

3.2.4 PROPELLER (TO BE) CALIBRATED

The data reduction has conveniently to be based on the propeller calibration to be discussed in the following chapter. Though the METEOR propeller has been calibrated in quiet waters, not yet according to the technique developed much later and to be discussed, the evaluation of the tests has been based on a 'calibration' obtained as average over the various service conditions met according to the oceanographic research programme carried out, the main purpose of the voyage.

3.2.5 LESSONS (TO BE) LEARNED

The important results to be noted at this stage are,

- that quasi-steady, arbitrary changes of the shaft frequency provide for the necessary variability of the data,
- that systematic errors due to the feed back of noise have to be avoided by introducing and correlating all data with 'reference' changes of the shaft frequency independent of the omnipresent noise, and
- that prior to the monitoring of all interactions the propeller has to be calibrated in traditional trials, to be discussed in the next chapter.

3.3 Model scale testing

3.3.1 Quasi-steady tests

On model scale thrust measurements are readily available. And I have shown that the complete analysis of the powering performance is possible based on quasi-steady propulsion tests of only two minutes duration. To do such tests you would not even need a towing carriage. Over the years I have developed the technique to maturity as documented in the evaluation of the 'model' test mentioned before.

As can be seen, for the hull investigated the results compare well with those of the traditional procedure, based on hull towing and propeller open water tests, of course except for the rotative efficiency. This concept, accounting for the incoherent interpretation of the wake by open water tests, and accordingly 'universally' called the 'rubbish bin' of the traditional approach, is 'by definition' not necessary in the rational approach.

Contrary to most papers today my papers, often just Mathcad documents, provide all the details, often including sensible confidence checks, so that anybody can follow the reasoning and check the procedures using my data and/or his own. Thus Klaus Wagner has carefully scrutinised among others the evaluation of the 'model' test and pointed out a problem in identifying the wake fraction.

After considerable effort I detected the reason for the problem. The condition of maximum hydraulic efficiency, which I had purposely introduced to stabilise the procedure, happened (!) to have been 'observed' accidentally, although I should have observed it explicitly. Now, being aware of such accidental 'good luck', I have avoided a similar 'mistake' in the evaluation of the current prevailing during the ANONYMA trials.

3.3.2 Plausibility checks

Naively I have been asked, whether my methods can be 'programmed'. Evidently my 'poor' colleagues have been looking for a black box to throw their data in and get the results out, thus saving them the trouble to look at the data. In fact the ISO code and others are used in that incredibly careless way.

But trials can never be evaluated by a black box. According to my experience the problems to be solved are always quite different and much too delicate for such crude approach. Of course my methods have been 'programmed'. And the Mathcad environment I am using is perfectly suitable for the purposes at hand. It readily permits to plot and document any intermediate results of plausibility checks necessary at any stage. Any other advanced computational environment may serve the purpose.

Without a digital computer my methods cannot even be applied! Solving 'only' six equations for four unknown parameters is a formidable problem not to be solved by do-it-yourself algorithms, as a student's exercise in Japan has shown. And I am still meeting students uncritically programming Gauss' procedure, which as an integrating method is correct only 'in principle', but obsolete for the solution of real, often nearly singular problems requiring differentiating methods.

In any up-to-date programming environment singular value decomposition and the left-inverse of non-quadratic matrices, which I have developed already fifty years ago to cope with the problems I was facing, are standardly available today. I am using the left, the 'generic' inverse as a matter of convenience and transparency.

3.3.3 NOT INVENTED HERE!

Quasi-steady testing has also been developed by Jan Holtrop at Wageningen, but to my knowledge 'hanging on' to Froude's conventions. And I have heard a rumour that colleagues at another model basin want to use my method, but they have not yet talked to me. The rule, to *'conveniently discuss your problem only with people, who also do not know anything about the subject'*, though widely followed, is the most stupid I know.

This rule is closely related to the widely followed doctrine 'Not invented here!' The negative consequences of such narrow minded decisions are well known from the introduction of the metric system and of differing railway gauges. As the name says, 'conventions' are not one-man-shows, neither mine nor any others, but joint efforts to solve common problems. And accordingly I repeat my invitation to join forces and I repeat my advice, not to try and invent your own method along obsolete conceptual approaches, as has been done at HSVA, VWS, SSPA, MARIN to mention only these.

As Novalis noticed already in 1800 new ideas, even if they 'happen' to be new, may be an unnecessary luxury. In a study sponsored by DIN it is even claimed, that standards are more important for progress in science and technology than 'inventions'. But this is definitely not true in general according to my repeated experience. The rules tend to perpetuate current practices and to protect mushrooming 'research' and related profitable 'businesses'.

Proposals for procedures and even standards are often not even meeting the standards of decent students' exercises, and (Clifford A. Truesdell, 1984):

> "… research papers are often not more than chants of beliefs common to the hogan, the members of which rock back and forth in applause of each repetition of the tribal lore."

This is a fact in physics and other sciences, as well as in standards, not only in ISO 15016: 2002-06, but also in such fundamental standards as

DIN 1313: 1998-12: 'Grössen' and the related standard ISO 31: 1992: 'Quantities', now ISO 80000-1: 2009: 'General'.

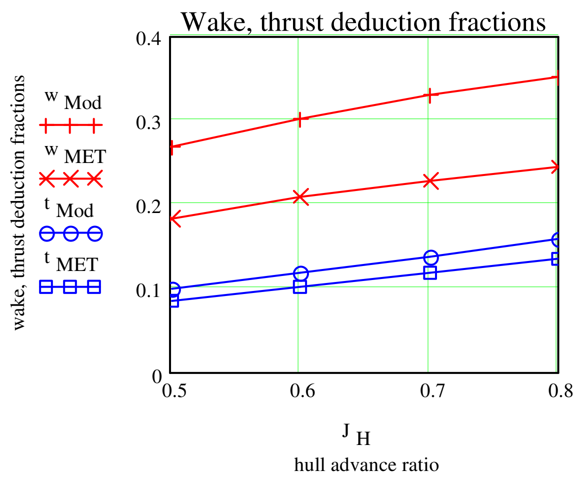
A particularly illustrative example of Truesdell's verdict is the story of the SAT Group managed by Henk van den Boom of MARIN and the inclusion of the 'industrial SAT standard' in the ITTC 2012 Guidelines and their adoption envisaged by IMO, details to be discussed under the heading 'The emperor's new clothes'.

In my detailed drafts ISO '98 Proposal and DIN '11 Vorschlag, the latter for a revised edition of DIN 1313: 1998-12, I have shown how standards meeting lasting scientific 'standards' must be designed, based on sound meta-theory.

3.3.4 Scale effects

With the simple thrust deduction and wake conventions stated the METEOR data have been re-evaluated and scale effects in wake and thrust deduction (!) fractions have been determined experimentally and reliably for the first time ever (2002).

The figure shows that the traditional 'axiom' of 'vanishing' scale effects in thrust deduction fraction underlying traditional evaluations is not warranted



Scale effects in wake and thrust deduction fractions derived from quasi-steady tests performed and analysed on model and full scale in the same way.

in a consistent theory and its interpretation.

Further I note, that according to the METEOR results model tests should only be performed at speeds corresponding to the service speeds in order to avoid 'unnecessary' problems due to excessive scale effects at the smaller speeds.

3.3.5 Lessons (to be) learned

In summary the important results to be noted at this stage are,

- that the simple conventions replacing hull towing and propeller open water tests, respectively, permit extremely efficient propulsion tests on model scale,
- that quasi-steady full scale and model tests performed in the same way permit to identify scale effects in thrust deduction and wake fractions, and
- that this theoretically solidly founded technique should be tested routinely in model basins and further developed to be prepared for the needs and demands of researchers and clients.

4 Balance of powers promoted

4.1 State of the theory

4.1.1 Thrust (to be) abandoned

In the traditional, naïve approach to powering performance evaluation in terms of forces, propellers are conceived as thrusters producing thrust to overcome the resistance of the hull to be propelled. As has been mentioned the fundamental disadvantage of this approach is that thrust is not a meaningful measure of powering performance.

The thrust includes a component balancing its own (!) suction at the hull. This component, due to a hydrodynamical short circuit, depends on the displacement wake and the corresponding elevated pressure (!), at which the propeller operates, and is thus, ignoring secondary effects, energetically neutral, whatever its value happens to be.

Further, the 'real' shortcoming of this approach is, that full scale the thrust cannot be measured routinely for the simple reason, that all the 'thrust meters' invented work only 'in principle', but none works reliably in reality. Today the problems are no longer due to lack of sensors, but still due to lack of care for the essentials.

To measure thrust reliably requires the identification of the complete calibration matrix of the thrust meter under combined full service thrust and torque loads, accounting for the deformation of the shaft, as has been demonstrated in case of the hollow shaft section of METEOR, calibrated even as a six-component balance.

4.1.2 Lagrangean approach adopted

In view of the latter deficiency stated the only rational way to proceed is to abandon the naïve approach in terms of balances of forces, even if interpreted by rational conventions, and to resort to the Lagrangean approach in terms of the balance of powers supplied, required and stored, relying on rational conventions. More adequately this approach is in terms of energy balances, convective and diffusive energy flows and energy storage.

In order to be specific the following exposition will be limited to the essentials of traditional *steady* powering trials. But I repeat my earlier statement, that waiting for steady states may have been necessary, when today's data acquisition and processing systems were not available, but is now 'wasting', not recording all the really interesting information available at no expense during changes of course and of speed.

Quasi-steady testing, including energy storage as in case of METEOR, permits to reduce the testing time drastically, and at the same to increase the reliability of the results. I am still working on this problem. With the filtering technique I have developed the identification of the horizontal acceleration, in the order of only few thousands of the acceleration of free fall, and of the aggregate horizontal inertia of the system is not a problem.

But to repeat, quasi-steady testing requires test signals independent of the omnipresent noise to be introduced and referred to in order to avoid systematic errors due to feed back of noise. If somebody tells you that he has 'taken *some* measurements' you can be sure, that he is not a professional.

All the traditional procedures are definitely no longer acceptable, particularly not in case of trials at ballast conditions, and not in related cases of propeller ventilation, which I have studied. All the traditional codes mentioned are not even mentioning, forget about adequately addressing any of the problems to be solved, as I have in great detail explained and discussed in very many papers and presentations published, at least on my website.

The most fundamental, the essential deficiency of all traditional approaches is, that they require 'unknown' values of parameters, conventionally derived from incoherent sources, if any, *i. e.* to be sucked from (your) thumbs. In case of ballast conditions hardly any values are available. The problems cannot be solved by increasing the number of parameters, but to reduce their number, so that they can be identified! The Lagrangean approach is a 'global' power approach.

Introducing more parameters than can be identified is to introduce 'singularity'. Contrary to a single solution, as the name suggests, such problems have many solutions. And many 'people' are of course interested not to change this 'favourable' situation, permitting to select solutions as 'required'.

4.1.3 Propeller convention

As ‘local’ model of the powering performance of the propeller in the behind condition I have used from the beginning of the development the 'pump' function

$$P_S = p_0 \cdot N^3 + p_1 \cdot N^2 \cdot V_H$$

relating shaft power P_S , frequency of shaft revolutions N_S and hull speed through the water V_H .

Contrary to a statement by Toki I have explained the reasons for adopting my two parameter powering model (theoretically: dimensional analysis of pump operation, and pragmatically: very few data known only with limited confidence), not only in private mails, but also in papers many times, among them some especially and explicitly devoted to the 'logics' of my approach.

It is important to note that the powering function adopted for the full scale propeller in the behind condition, maybe only slightly submerged, has nothing, to stress: definitely nothing whatsoever (!), to do with the open water performance of the 'corresponding' deeply submerged model propeller, upon which the ISO and ITTC procedures are based.

In normalised form the function of the power supplied by the propeller, the power ratio is a linear function depending on the hull advance ratio through the water in the limited range of operation. Suggestions to 'improve' the convention by a term quadratic in the hull speed through the water and to identify its parameter are 'purely academic'. Due to the limited confidence range of the power values measured the problem becomes singular, the whole procedure becomes unstable, as I have repeatedly checked.

The simple powering function has the considerable advantage, permitting simply (and) cleanly to separate the identification of the propeller and current parameters from the identification of the environmental parameters. According to my experience claims, that two runs up and down wind and waves may 'in cases' be sufficient reliably to evaluate trials, can definitely not be substantiated; see below.

After the calibration of the propeller at the given trials condition the problem of determining variations in the frequency of shaft revolutions due to load variations does 'not exist', if necessary the solution is obtained by iterative solution of a cubic equation.

4.1.4 Current convention

Only the shaft frequency (of revolutions) and the shaft torque Q_S , and thus the power

$$P_S = 2\,\pi \cdot N_S \cdot Q_S$$

can be measured directly. Further the hull speed over ground V_G can now reliably be measured by means GPS-Systems.

The hull speed over ground and through the water are related by the current velocity V_C prevailing at the time and location of the trials

$$V_G = V_C + V_H$$

*Thus the parameters of the propeller powering function in the behind condition can*not *be identified unless the current velocity is determined reliably as well.*

Even with very crude local current conventions the procedure to identify the parameters of the propeller and the current convention has been very stable and a very reliable 'diagnostic' tool. Whenever unrealistic parameters resulted the basic data exhibited some 'unusual' features, maybe just the misprint of a single digit as in the ISO example.

In case of ANONYMA the data at the smaller trim by the stern turned out to be 'unusual' due to ventilation of the propeller during the runs up wind and waves. Accordingly the few data did no longer permit to identify the parameters of even the simplest current convention. Thus the trials at the larger trim have been analysed, no problems arising, and the current had to be extrapolated to the (earlier) time and location of the trials at the smaller trim.

The lesson I have learned during that exercise is that the current convention can be and has to be a two parameter function as well in order to avoid singularity and instability of the procedure and provide reliable extrapolation where necessary. In many cases the current may be conceived as a mean constant current superimposed by a harmonic tidal current. And the simplest convention adequate in this case is the two parameter model

$$V_C = v_0 + v_1 \cdot \sin[\omega_T \cdot (t - t_T)]$$

with the 'universal' circular tidal frequency ω_T and the time of high tide t_T at the day and the location of the trials, known from the tidal tables. Various attempts failed to identify the tidal phase reliably based solely on the data observed.

If trials take place in waters without pronounced tides, other, appropriate conventions will of course have to be adopted and to be agreed upon.

4.1.5 Lessons (to be) learned

The important insights at this stage to be noted are,

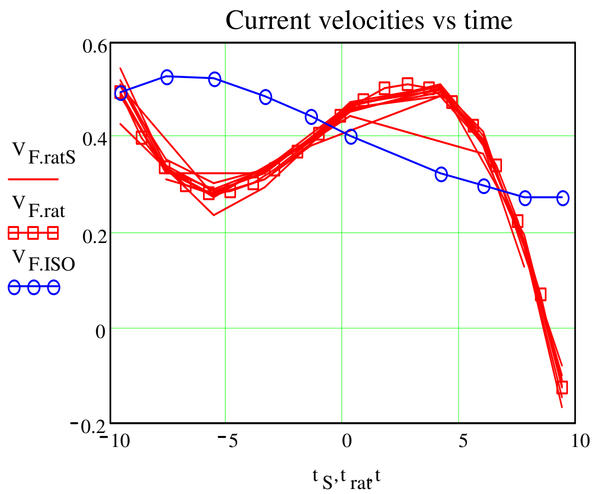
- that an adequate propeller convention is a function of two parameters only, and
- that an adequate current convention is a function of only two parameters as well, and
- that both sets of parameters can be identified as the solution of only one set of linear equations.

4.2 ISO 15016: *ET CETERA*

4.2.1 ISO EXAMPLE ANALYSED

Much later than the complete monitoring problem, since 1997 I have studied the simpler problem of evaluating traditional powering trials. When I saw the Japanese draft proposal for ISO 15016 on traditional powering trials a half sentence in my METEOR report 'told' me, how to analyse such tests in a rational fashion. My letter to the convener and in my ISO '98 Proposal have already been mentioned.

And as I have shown already in 1998, when I analysed the example appended to the draft of ISO 15016 with my extremely simple and transparent method, the ISO procedure is not even acceptable in case of fully loaded ships. The reason is that it is error prone, as has been confirmed a number of times since at different institutions in Germany.

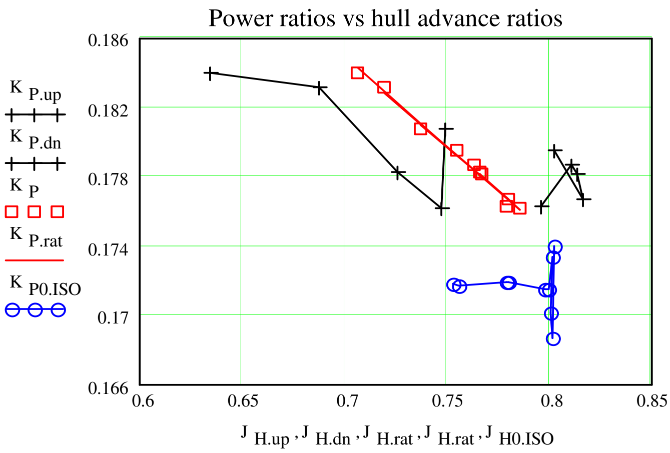


Current velocities in the example appended to ISO 15016: 2002-06, as derived by the ISO procedure, blue circles: as identified by the rational procedure, red squares:

I have brought the wrong results produced by the ISO method to the attention of all national groups well before the Japanese draft proposal became ISO 15016: 2002-06 despite its evident serious deficiencies. To my surprise 'nobody' felt disturbed and the example has not yet been corrected, more than ten years later, although my counter-example evidently 'falsifies' the procedure!

By any 'standard', not only mine, this is very surprising and in my personal view a most irresponsible attitude! '*Kill the bearer of the 'disturbing' message*' has been known to be the most stupid 'strategy' since antiquity.

As I have repeatedly stated and confirmed in my recent exercises not only the propeller powering characteristic in the behind condition has to be identified reliably, but the current velocity at the same time, in a coherent fashion! And the following figures show that even in the example attached to the standard the ISO method fails 'exactly' in this most fundamental task to be solved.



Normalised powering performance of the propeller in the example appended to ISO 160126: 2002-06, blue circles: as derived by the ISO procedure, red squares: as identified by the rational procedure.

If you cannot identify the current velocity reliably, you can forget all the rest, you have to take the necessary steps for *a full stop of any further evaluations*, as has always been the practice of knowledgeable hydrodynamicists and as I myself have experienced again only recently in the evaluation of the ANONYMA trial at the smaller trim The comparison of the normalised powering performances identified in the ISO example provides a particularly drastic example.

Incidentally, my method has been tested at Kyushu University and found 'not to work'. It took me two years to find out the reason. The student 'in charge' did not know how to solve nearly singular sets of linear equations! But his 'finding' is still spread in Japan. For the full documentation please inspect my website.

Since the early applications I have developed this technique to maturity. The whole development is documented on my website in the sub-section Papers on ship powering trials. My meanwhile 'historical' Lavrentiev Lecture of 2001 is currently referred to most frequently. On my website a html version of that paper, including hyperlinks to all the material referred to, is also to be found.

The only colleagues opposed to the ISO proposal have been the Korean colleagues, but for the 'wrong' reason. They wanted to introduce an even more fancy theory of the added resistance due to waves than the Japanese theory, incorporated in the standard.

Not only in view of the crude observations of the sea state should both theories definitely not figure in rational, acceptable standards for the assessment of trials. This 'conflict' is referred to in the report on the recent COMPIT, in fact the only reference I have found so far, but without any indication of how to solve it (Bertram, 2013/25).

4.2.2 DATA ARE 'CONFIDENTIAL'

Many times I have tried very hard to obtain trials data, to test and further develop my method and to demonstrate its power. In most cases my request has been turned down, the data claimed to be confidential. Only after repeated explanations some colleagues understood, that their data remain confidential, that I was not interested in the names of their ships, but in analysing the powering performances of the latter.

The results of my analyses have of course always been of greatest interest to the owners of the data. In view of the costs of trials I have often been wondering, how carelessly the data have been treated, crudely using the traditional codes.

But I shall not repeat my earlier invitation to send me complete sets of trials data as test cases for my methods. At my age I cannot spare any more time and analyse them myself. I think this work is now a matter of students' exercises, but I am prepared to assist, where necessary and if requested.

But please note that simulated data are not acceptable, as I have explained over and over again. I remember wasting my time to find out, that the EVEREST test case produced by Kinya Tamura has been simulated based on an inadequate theory. This 'exercise' is also documented at length on my website. Of course I myself am using simulated data, as everybody else does, to debug my computer codes, to verify their formal correctness.

And please note, that I am talking not about 'numbers' to be compared, but about methods to be compared. The conventions promoted will by definition result in more or less different values compared to those arrived at by traditional procedures, provided the latter can be applied and/or produce any useful results at all.

4.2.3 LETTER TO A STUDENT

In this connection I have also written a summary of problems to be solved in setting up an acceptable procedure for testing non-traditional hull-propeller configurations, in my letter to a student, a student that could be my grand-daughter.

To my surprise her teacher told me, that he did not even understand what it is all about. This confirms my observation that naval architects world-wide still have to go a long way to overcome their professional superstition. But as I wrote in that letter, the coming generation is already much more open-minded and aware that there are 'countries beyond the ocean' mentioned in my words of thanks for being awarded the Silberne Gedenkmünze of STG.

The traditional conceptual framework, originally developed for traditional hull-propeller configurations, can be 'applied' as long as the hull and propeller can be separated at least conceptually. This is no longer possible with hull integrated propulsors, due to fact that the concept of thrust can no longer be interpreted in a meaningful way. In these cases only the Lagrangean approach in terms of powers is adequate.

4.2.4 Ducted propulsor design

Typical examples of hull integrated propulsors are ducted propellers. And it is worth mentioning in this context, that thus the Lagrangean approach is underlying my procedure for the design of ducted propulsors, where the concept of thrust is no longer useful and not required!

Designing energy wake adapted propulsors as pumps offers the dramatic advantage, that all interactions are treated implicitly. No prior information on thrust deduction etc is necessary, information not available anyway, as *e. g.*, in case of trials at ballast conditions etc. Details are to be found on my website in the section on ducted propulsors. The development started with a 'speculative reconstruction' (1983) based on the results of my model tests with a propeller in systematically varied Kort nozzles behind sea-going ships in 1961.

The explanation of the results in terms of elementary hydrodynamics, in fact just referring to Bernoulli's equation, became the germs of my rational theory of propulsion. But as my later results they were, and still are, not in accordance with the professional superstition of 'experts', thus my report has immediately been hidden in the basement.

As most ducted propulsors are still designed for operation in open water, naval architects not yet facing the problems of interaction 'ahead' of them, I had prepared a paper on ducted propulsors in open water for SMP '11 at Hamburg.

Following my explanatory response to the 'peer' review the paper has been rejected and neither been printed, nor presented, but 'only' been published on my website together with all subsequent discussions. Having my work patiently seen 'judged' by 'peers' for more than fifty years, I feel doubts expressed in my modesty unjustified, unless scientific discussions are reduced to talk shows, to ritual 'repetitions of the tribal lore'.

4.2.5 Lessons (to be) learned

The fundamental lessons to be learned at this stage are,

- that the traditional methods, including that of ISO 15016: 2002-06, are error prone, mostly inadequate, even in cases of ships with traditional hull-propeller configurations at fully loaded conditions,
- that you have to order 'full stop' of any further evaluation, if you cannot identify the current velocity reliably in the coherent fashion described, and
- that any other 'invention' to measure the hull speed through the water is causing unnecessary new conflicts and irresponsible waste of resources.

4.3 ANONYMA trials

4.3.1 Problems (to be) solved

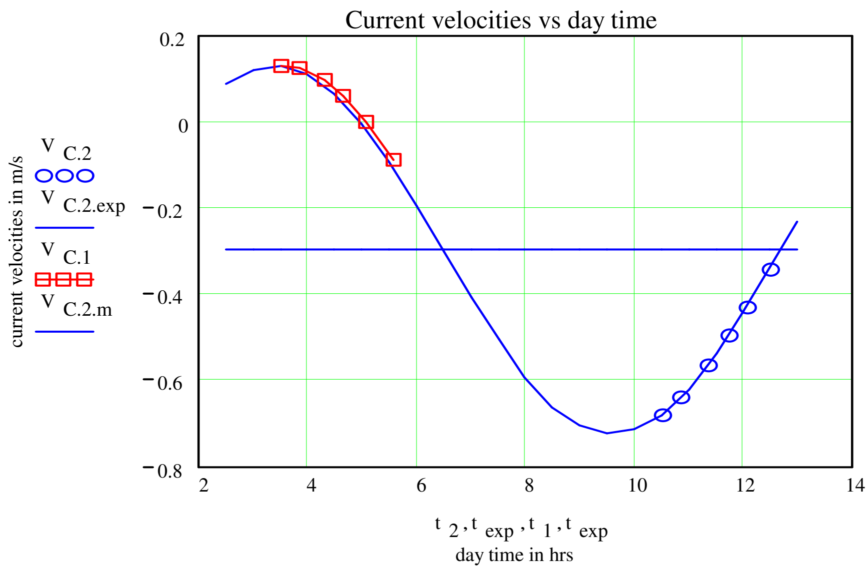
The power of my approach has recently been demonstrated in a very demanding project, the reliable comparison of two trials with a bulk carrier in ballast at different trim settings, confirming my earlier statement, that the ISO and ITTC 'codes' are completely inadequate for such delicate problems.

Similarly the method suggested in the forthcoming paper of Naoji Toki is hopelessly old-fashioned and obsolete already before its publication, despite my timely, repeated, detailed explanations and suggestions concerning the basic problems to be solved. None of these problems has been mentioned, forgetting about adequately addressed and solved.

In case of ANONYMA the first evaluation, that of the trials at the larger trim and thus the larger nominal propeller submergence, posed no problems using the routines developed as outlined before. The two current parameters, the mean current and the tidal amplitude identified, permitted to extrapolate the current velocity reliably over seven hours to the time of the earlier trials with the smaller trim setting.

The next evaluation, that of the trials at the smaller trim and nominal propeller submergence, had to be tailored to account for the ventilation of the propeller in the up wind conditions. In view of the omnipresent noise it is evident, that though only two parameters of the propeller function and two parameters of the current function are to be identified, this fact does not imply that two runs up and down wind are sufficient, reliably to evaluate trials, as has already been mentioned before.

The analysis of the ANONYMA data has confirmed the implication of statistics, that there is no way to distinguish current conventions resulting in residua within the confidence range of the mean values of the shaft powers derived from four hundred values measured during ten minutes quasi-instantaneously; see the next but one section.



Current identified from data measured at the second, the larger nominal propeller submergence, blue circles, and extrapolated to the first, the earlier trial at the smaller nominal propeller submergence, red squares.

The figure on the next page shows that the first order convention and the current convention adopted happened (!) to result in nearly identical local current values and thus the same values of the power residua.

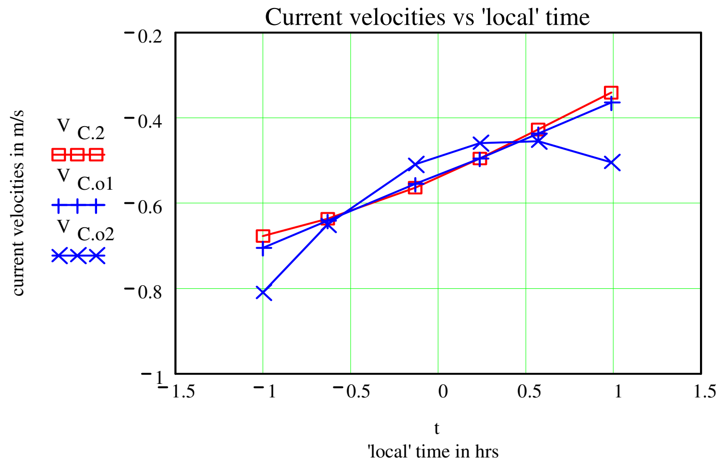
Even 'some more' up and down wind runs are not sufficient to provide for statistical confidence in the results and for a decision on the most appropriate convention.

In the case of ANONYMA the additional convention was to assume that the trials took place in a tidal current of the type described and that its phase, the time of high tide was known.

4.3.2 Required power convention

Subsequently in a second step the parameters of simple models for the partial shaft powers required have to be identified, conveniently again as solutions of a system of linear equations.

Being traditionally trained myself I have of course at first been thinking of the partial powers required due to the motions through water, wind and waves. But during my numerical exercises I realised that these connotations, belonging to the 'folklore' of naval architecture, as *e. g.*, in the 'industrial STA standard', are not only misleading, but even unnecessary.



Currents arrived at by adopting the current convention (red squares), the first order convention (blue circles), and the second order convention (blue crosses).

In case of the ANONYMA the two parameter 'required power convention'

$$P = q_0 \cdot V_H^{\,3} + q_1 \cdot |V_{W.rel.x}| \cdot V_{W.rel.x} \cdot V_H \, ,$$

which I had used many times before, turned out to be 'perfectly' adequate to model the data in the confidence range.

The 'environmental parameters' of the partial powers have been identified unambiguously, 'objectively'. Evidently these power parameters have nothing, to stress: definitely nothing whatsoever, to do with the 'resistance coefficients' traditionally considered in this context, even in the SAT-JIP procedure in the most incredible way as will be explained.

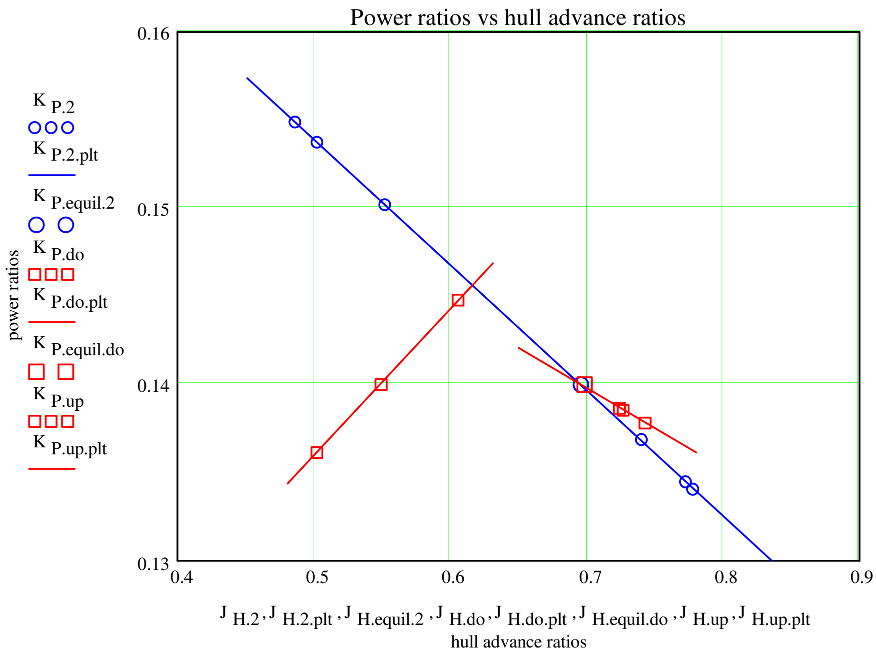
While the hull speed through the water has to be determined as described before, the relative wind speed in forward direction can be derived from the measured relative wind speed and direction. It is worth noting that in the context of the Lagrangean approach the wind speed is a nominal speed. Any attempt to calibrate the wind meter subject to the influence of the ship structure and the boundary layer of the airflow above the water surface has to rely on additional conventions (van den Boom, 2013.2/3-4), and is thus as 'unprofessional' as are incoherent measurements of the hull speed through the water.

I had used the convention stated already earlier to account for the fact, that usually the relative wind and wave speeds are closely correlated. Thus the problem is inherently singular, the two effects cannot be separated without some additional convention and parameters to be assumed, *i. e.* sucked from your thumbs, and thus any result to be obtained 'as required'!

The required power convention permits further to define the nominal no wind and waves condition

$$P_{S.NoW} = (q_0 + q_1)\cdot V_H^{\;3} \equiv C_{PV}\cdot V_H^{\;3},$$

i. e. the final convention for the assessment of trials. So far I did not care to produce plausible, more or less theoretical explanations for the conventions of the power required and of the nominal no wind and waves condition.



All normalised results, performance at the second, the larger nominal propeller submergence (blue circles), and performance at the first, the smaller nominal propeller submergence, red squares, exhibiting propeller ventilation at the up-wind runs. Powering performances versus hull speed reduced to the nominal no wind and wave conditions, large blue circle and large red square, differing only in the order of the confidence radii of the mean power values, provided the propeller is not ventilating.

In case of the ANONYMA trials only the wave height has been reported, its value estimated to be constant over the whole time of both trials. Thus there was no chance objectively to identify the influence of the sea state, additional conventions being the only ways to obtain the confidence required.

4.3.3 Contractual conflicts

So far I have not been concerned with the completely different problem of 'extrapolating' from the reliably established nominal no wind and waves condition derived at the trials condition to the nominal no wind and waves condition at any other loading conditions.

But in view of the basic model of rational conflict resolution and the state of development of the rational procedure and that of the 'numerical sea trials' the following approach, fundamentally different from the traditional approach followed in the ITTC 2012 Guidelines (2013.2/9), appears not only feasible, but also desirable form the ship buyers and owners point of view.

Starting point is the 'principle' that it is 'rather absurd' to contract results of delivery trials at conditions, at which the trials will definitely not be performed and thus the results in question cannot be determined as 'directly' and objectively as possible.. And if somebody tells you, he will solve your problem, but needs to invent something or needs to do 'some research' before hand, implying that he does not know how to solve your problem, refrain from contracting that particular item!

'Consequently' it is suggested to contract for trials at conditions that can be established and for which the performance can be identified objectively and right after the trials, independent of the observer and of any prior values of parameters, as has been shown.

While the assessment of the trials at the given conditions is straight forward, the prediction of the performance at the trials, *e. g.*, at ballast condition, can no longer rely on traditional model tests, but has substantially to be based on the 'numerical sea trials' being developed not only at FutureShip (Hochkirch, 2013).

According to the rational procedure suggested, to assess the performance at the nominal no wind and waves condition at a given trials condition, is no longer a problem. The prediction of the performance at the ballast condition and at any other contract condition is not a matter of the trials, but solely of the performance predictions, naval architects may like it or not.

Although my research has been primarily concerned with the rational solution of 'technical' problems on many levels, its results will thus have a disruptive impact on the rational resolution of contractual conflicts. 'Consequently', in a similar case shipbuilders have stopped to support my research! But any attempts to prevent research and its results from being spread are felt to be sailing in the wrong direction.

In view of the objective, observer independent evaluation of trials developed ship owners and buyers need no longer to accept and sooner or later will no longer accept 'the same people' providing the predictions of the powering performance and accessing the delivery trials 'as well'.

4.3.4 THE EMPEROR'S NEW CLOTHES

While this draft was undergoing its final revisions a pertinent note and paper have been published by the 'manager of the SAT Group and Member 27th ITTC PSS', referring to the 'cooperation' of the MARIN promoted SAT Group and the (re-)established 'ITTC Specialist Committee on Performance of Ships in Service (PSS), the former Specialist Committee on Trials and Monitoring, notably with HSVA (van den Boom, 2013.1; 2013.2).

The result of this 'cooperation' is the ITTC 2012 Recommended Guideline 'Speed and Power Trials, Part 2: Analysis of Speed/Power Trial Data', based on the so called 'industry standard' developed in the 'Ship Trials Analysis' Joint Industry Project (STA-JIP) by MARIN. And surprisingly the ITTC Guideline is not only claimed to have been 'approved by the 27th ITTC 2012', but even to have been adopted by IMO. It remains unexplained how all this could possibly 'happen', as the Conference, which might eventually approve, or more likely not, will take place only in 2014!

In the light of the present exposition even more surprising is the strictly traditional approach 'advocated' in the 'Guidelines', according to my experience definitely inadequate for many purposes of considerable interest, typically trials at ballast conditions. As the subtitle 'Level *playing field* established for IMO EEDI' (van den Boom, 2013.2) indicate, the authors themselves are aware of the deficiencies of their 'incredible' approach.

The 'playing field' proposed can definitely not serve as a basis of a decent, acceptable standard for the purpose claimed. As in the earlier publication of HSVA (Hollenbach, 2008) the SAT procedure developed at MARIN carefully avoids any reference to the state of research, which I twice had the opportunity to demonstrate and explain in detail to the colleagues at Wageningen.

For my 'taste' the 'transparency' and objectivity claimed for their method is 'completely' lacking, when I read the following sentences (2013.2/3):

> "To derive the speed/power performance of the vessel from the measured speed over ground, shaft torque and rpm, the Direct Power Method is to be used. In this method the measured power is directly corrected with the power increase due to added resistance in the trial conditions: …"

In particular it is stated:

> "The above approach is referred to as the Direct Power Method and is far more transparent, reliable and practical than the use of the propeller open water diagram proposed by Taniguchi & Tamura in 1966 and adopted by ISO 15016 (2002), …"

Trying to find out the meaning of the label 'Direct Power Method' I noticed that this 'happens' to be grossly misleading, des-information as this type of 'information' is called in political propaganda! The various partial towing powers required are converted to shaft powers, before accounting for the current velocity, using the propulsive efficiency of the model propeller (ITTC 2012/5, eqn.3):

> "The recommended procedure for the analysis of powering trials is the direct power method and requires displacement / power / rate of revolutions / η_D and η_S as input values."

No question, this extremely simple minded approach is very 'practical', but at the same time it is definitely inadequate and unacceptable according to my experience. Wondering, where the 'input value' of the propulsive efficiency, the concept not even occurring in the Nomenclature, might 'come from', I finally found a hint though not a direct one (ITTC 2012/6):

> "The effect of added resistance on the propeller loading and thus on the propulsion efficiency coefficient η_D is derived from the results of load variation tank tests."

This 'Guideline' is of course unacceptable in view of the fact, that the propulsive efficiency on full scale under service conditions, maybe in ballast, would be of interest, if anybody should seriously consider to accept the approach despite its deficiencies, evident in the context of the present exercise and pointed out from a more traditional point of view in a detailed discussion by Wagner.

Revealing is, that instead of the propulsive efficiency the relative rotative efficiency by use of the thrust identity shows up in the Nomenclature, indicating that the SAT procedure is, despite an explicit statement to the contrary, still relying on model propeller open water thrust measurements, as do the obsolete procedures of Taniguchi and Tamura, ITTC, ISO, Toki and others.

Further in view of my exposition I do not understand the meaning of the sentence:

> "The importance of the quality of model test results for the analysis of speed/power trials is now recognised by ITTC and the IMO."

According to my rational approach the evaluation of trials at the trials condition does not require any model data and/or any other prior data whatsoever! So I modestly dare to ask: *Who exactly has 'now recognised' exactly what?*

Further according to my experience explained in detail, the 'Guideline' to identify the current following the power corrections described as follows, is obsolete (van den Boom, 2013.2/2-3):

> "To eliminate the current from the speed over ground, the results of double runs (i.e. speed runs on reciprocal courses), can be averaged according

> to the “mean of means” method also referred to as 'Pascal’s triangle', which was already presented by Van Lammeren in 1939 and also recommended by the Principles of Naval Architecture. To account for time varying currents such as tidal currents, two or more double runs are required for the same power setting.
>
> The 'mean of means' is applied after correcting the measured speed/power points for wind, waves and other deviations from ideal conditions except the conversion from the (ballast) trial draught to the contract design draught. All corrections for non-ideal conditions are expressed in shaft power corrections (except for shallow water) and the propeller efficiency is corrected for non-ideal loads by use of the results of load-variation model tests."

Here again model test results are referred to.

The problem of 'transforming' the results from ballast to design conditions should have been of major concern in the development of the STA-JIP (van den Boom, 2013.2/2):

> "Particular attention was requested for the conversion of trial results at ballast draught compared to the (contract) design draught."

But as has been pointed out by Wagner in the detailed discussion mentioned before, the solution proposed is not at all satisfactory. If trials at different loading conditions during ship operation are performed a corresponding power parameter can of course be identified as Wagner has demonstrated in an example, jpg files of the procedure to be obtained on request.

The 'incredible' story of the SAT Group, including even a university institute, confirms my repeated statement that the fundamental, intricate problems of evaluating acceptance trials and of setting up appropriate, acceptable standards for that purpose should not be left to practicians in model basins and ship yards.

Each little boy proudly identifies himself with the little child 'dismantling' the emperor and his weavers in Hans Christian Andersen's archetypal tale of 'the emperor's new clothes'. But growing up nearly all of them forget the lesson learnt and join the crowd, instead of using a little bit of common sense to expose the crowd. For ready reference the plot of the tale is quoted here from the Wikipedia:

> "A vain Emperor who cares for nothing except wearing and displaying clothes hires two swindlers who promise him the finest, best suit of clothes from a fabric *invisible to anyone who is unfit for his position or 'hopelessly stupid'*. The Emperor's ministers cannot see the clothing themselves, but *pretend that they can for fear of appearing unfit for their positions* and the Emperor does the same. Finally the swindlers report that the suit is finished, they mime dressing him and the Emperor marches in procession before his subjects. *The townsfolk play along with the pretense not wanting to appear unfit for their positions or stupid.* Then a child in the crowd, too

young to understand the desirability of keeping up the pretense, blurts out that the Emperor is wearing nothing at all and the cry is taken up by others. *The Emperor cringes, suspecting the assertion is true, but continues the procession*." *Italics: MS.*

Analogies of the various aspects addressed are self-evident, and thus need no explicit explanation.

4.3.5 Lessons (to be) learned

The fundamental lessons to be learned at this stage are,

- that only three two parameter models are serving the purpose of objective, observer invariant evaluation of measured trial data, even in the delicate cases investigated,
- that in view of the few data available only these models provide the confidence in the results, only six parameters to be identified from the data recorded, and
- that the prediction of the performances at the trials conditions and any other conditions is thus no longer a matter of 'assessing' the trials.

5 Conclusions

5.1 Evaluation

I have tried to explain my approach and its power in terms as simple as possible. So far I have been concerned with the state of research, clearly to be distinguished from the state of the 'art', the current 'unbelievable' practice, essentially the practice of our grand-grand-fathers standardised although shown to be no longer acceptable, neither theoretically nor practically.

My conventions have reached the required simplicity. They permit to identify and treat 'unusual' effects, the presence of a misprint in the ISO example, or the presence of propeller ventilation as in my recent study. The parameters identified not only permit to reduce the data observed to the nominal no wind and wave condition, but also permit acceptable estimates of the powering performance at different environmental conditions. A blow-up of the results around the nominal no wind and waves condition can be inspected in the pertinent file on page 16.

In 'normal' cases scrutiny of the data, check for normal distribution of the data, determination of the averages and their standard deviations can be completed after each run, and after completion of typically three runs up and down wind and waves the evaluation including reduction to the nominal no wind and waves condition and eventual conversion to another wind condition is a matter of half an hour. Propeller ventilation will not escape the attention of the investigator, but is detected 'immediately', if runs at MCR are scheduled to be conducted first.

And finally I explicitly state, that I have not solved 'all' related problems, but only 'my' problems, the problems I had the opportunity to be concerned with. But I have tried to provide paradigmatic solutions, so that other problems can be solved in the same spirit. Some further developments and ramifications have already been pointed out.

5.2 Assessment

In the paper it has been shown that Froude's traditional conventions to identify values of wake and thrust deduction fractions, *i. e.* model hull towing and propeller open water tests, can be replaced by extremely simple rational conventions. Due to the fact that the resulting procedure is based on only one coherent model and one coherent set of data it is not suffering from the various serious deficiencies of the traditional procedures.

In particular the rational procedure can be applied on model and on full scale under service conditions in the same way. And after decades of development since 1988 the rational procedure is as stable as the traditional procedure based on hull towing and propeller open water tests, but which can be applied on model scale only.

And, maybe most comforting not only for naval architects, in a test case the results of both methods differed only very little, if at all. The 'smart' conclusion that we might thus as well stick to the traditional procedure misses all essential points of this exposition, reliable full scale applications in particular, and dramatically increased efficiency and consistency of model tests, of research and teaching.

The conventions for the evaluation of traditional trials developed over the years are also extremely simple and the Lagrangean, very stable procedure, avoids all the serious deficiencies of the traditional procedures. Thus it produces reliable, observer independent results independent of any prior data, even in the most delicate cases as has been shown, while similar claims in favour of 'the emperor's new clothes' relying on model test results cannot be substantiated.

5.3 Consequences

As with any change of approach, or of paradigm as it is fashionably called following Kuhn's 'paradigm of paradigms' of 1962, there remain many tasks and problems, unnoticed and thus unsolved before due to lack of adequate conceptual tools. Most important among the tasks ahead is to continue the development of the procedures following the principles outlined in this paper and linking up with the past, vast experience.

So far the problem of accounting for changes in displacement in terms of model test results is not adequately addressed. This problem must also be solved professionally, maybe following Wagner's proposal, definitely in a conventional, clear-cut way, acceptable for all parties involved.

If ship theory is to become a serious science, teachers of naval architecture should not leave the solution of fundamental problems they cannot solve to practicians in model basins and ship yards, but must provide future generations of problem solvers with power tools meeting professional standards accepted and adhered to in other fields.

We can no longer afford the 'luxury' to follow the folklore of our grandfathers and bark up the wrong trees, asking for things impossible, theoretically and/or practically, and unnecessary for the purposes at hand, if the horses are put before the cart. To repeat Einstein's dictum: Problems can never be solved by the methods, which have caused them!

5.4 Lessons (to be) learned

The final conclusions to be drawn are,

- that the departure from the inherited traditional approach will result in dramatic gains in efficiency and quality of research and teaching,
- that the costs for testing model and full scale can be drastically reduced, if performed quasi-steadily, the reliability of the results increased at the same time,
- that these considerable returns are to be obtained for only little effort using some common sense, and
- that the disruptive innovations are in the interest of the industries we serve.

6 Acknowledgements

Gratefully I repeat my words of thanks to STG, Schiffbautechnische Gesellschaft, for many occasions granted to present my ideas and results at many, various meetings of the society.

Further, I gratefully acknowledge the permission granted by FutureShip, Germanischer Lloyd Group, to publish not only results of my evaluation of the trials with ANONYMA for illustrative purposes, but also to include hyperlinks to the Mathcad files of the evaluations and of the routines developed by the way. The opinions expressed in this paper are not endorsed by FutureShip, but are solely mine.

Finally, I owe thanks to Dr.-Ing. Karsten Hochkirch for his critical comments and inquisitive questions concerning intermediate results of my analysis of the ANONYMA trials, and in particular to Dr.-Ing. habil. Klaus Wagner, promptly responding to my long daily mails concerning all aspects of the evaluations and of the ideas explained and further developed in this paper.

His expert comments and questions have necessitated continuous re-thinking and further developing of my procedures and, last but not least, careful re-phrasing of my arguments until these reached the clarity and simplicity pre-requisite for general acceptance. The remaining, unavoidable misprints, mistakes and errors are solely mine.

7 REFERENCES

7.1 MY WEBSITE AND OPUS

As has been mentioned in the introductory note this paper, including hyperlinks to all the material referred to, is to be found in the sub-section News on ship powering trials on my website www.m-schmiechen.de, a publication proper subject to the rules, laws etc stated in the Preliminaries, down to routes of access to material published and/or archived elsewhere.

Further links are to be found in the carefully annotated documentation of all my work and related discussions on Propulsion in general and on Ship powering trials in particular.

In my rational reconstruction of classical mechanics, my opus magnum, the detailed exposition of the rational theory of propulsion in Chapter 22 serves 'only' as an example of the general principles developed in great depth and applied in great detail.

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8 Name Index

9 Discussions

(if any)

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