From METEOR 1988 to ANONYMA 2013 *and further!*

Future Ship Powering Trials and Monitoring Now!

Volume 1

Principles of rational conventions further clarified,
consistently applied in a particularly delicate case
and lessons (to be) learned,
various subsequent presentations
and written discussions added

Michael Schmiechen

Published on occasion of the
108th Annual Meeting of the
Schiffbautechnische Gesellschaft
at Berlin, November 20 to 22, 2013

VWS Mitteilungen Heft 62, post mortem
Berlin 2013

in memoriam
Versuchsanstalt für Wasserbau und Schiffbau, Berlin

"It's the best possible time to be alive, when almost everything you thought you knew is wrong."

From the Tom Stoppard play 'Arcadia'. Quotation following Marine Technology, October 2013, p.38.

PROBLEM

The evaluation of ship powering trials is still treated as hydro-mechanical problem, although it is of 'conventional' nature – *not* to be mistaken for 'traditional' –, part of a whole range of intricately intertwined legal and contractual conventions.

Theoreticians have 'simply' left the very difficult problems of trials and monitoring of the powering performance to 'practicians' at ship yards and model basins. And, hard to believe, ship owners still accept, that the same 'people' providing the predictions are not only carrying out and analysing the trials 'as well', but are even setting up the standards to be met!

IMPORTANCE

The structure, the implications and the relations of the conventions involved are usually *not* stated explicitly and are thus only vaguely known. In particular, the underlying 'instinctive' beliefs and convictions are *not* generally shared, although the same 'principles', as they are fashionably called, – 'principles' being another name for 'prejudices' as Mark Twain aptly noted –, are essential pre-requisites of conventions.

Presently it is widely realised, that very many methods have been developed to predict the powering performance of ships based on results of physical and/or numerical model tests, erroneously mistaken for propulsion theory, but that hardly any methods have been developed for the *convincing, trustworthy proof* of their results full scale, meeting today's requirements, their own in particular.

METHOD

Conventions are agreements, are languages and their implications (to be) agreed upon. While traditional conventions are usually *not* explicit, incoherent languages, rational conventions are explicit *formal languages constructed ad hoc for the purposes at hand*. In terms of logics these are axiomatic systems, a frightening name for extremely useful tools.

Continued on the back end-paper

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to ANONYMA 2013
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Preface

> "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*."
>
> *Paul Feyerabend: How to be a good empiricist (1999/101 f).*

The basis of the following collection is a 'letter' to my colleagues and my students, as well as to whom it may or must concern, governing bodies and pertinent committees of the ITTC, ISO and IMO in particular.

The letter has been conceived after my recent evaluations of powering trials with a bulk carrier in ballast at two different trim settings and is published here with subsequent related presentations and written discussions to commemorate

- the 25th anniversary of my propulsion tests with the research vessel METEOR in the Greenland Sea in November 1988,
- the 15th anniversary of the submission of a proposed rational standard for the assessment of ship powering performance to the Japan Marine Standards Association in April 1998,

and, last, but not least, to contribute

- to the current, long overdue revision of the standard ISO 15016: 2002-06, being error prone, inherently wrong as demonstrated already in 1998, long before it became a standard, and thus being no longer acceptable, being inadequate for most of today's purposes.

The material published here has been selected from work that originated in 2013 and is completely documented in the Section 'News on ship powering trials' on my website www.m-schmiechen.de.

Figures in the basic paper and in the Mathcad documents are printed here in black and white, on the website they are available in colour!

Contents

Links to the pdf-version of this volume as well as all the material in this volume are to be found in the section 'From METEOR 1988 to ANONYMA 2013 and further' under 'News on ship powering trials' on my website www.m-schmiechen.de. Further the html-version of the basic paper provides live links to all material referred to in the paper.

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

by Michael Schmiechen, Berlin

apl. Professor for Hydro-Mechanical Systems, released from the duty to lecture at ISM, Institut für Schiffs- und Meeres-Technik of Technical University Berlin, and retired Deputy Director, Head of R&D at VWS, the Berlin Model Basin

NOTES

The pdf-file of this paper 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 have been invited and been welcome, published here together with this final version of the paper at my discretion. In any case suggestions and arguments put forward have been considered, duly referred to and acknowledged.

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 socalled 'industry 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 at 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. This insight is not a platitude,

but a commonplace, expanded e. g. in Ronald Dworkin's *opus* 'Justice for Hedgehogs' (2011).

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 discussed earlier, *e. g.*, during the MARIC Lectures 2004 at Shanghai, in the MAHY paper presented 2008 at Visakhapatnam, in the SMP '09 paper presented 2009 at Trondheim and at 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 ap-

proach, 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 grand-children 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

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

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.

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 'pro-

grammed'. 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."

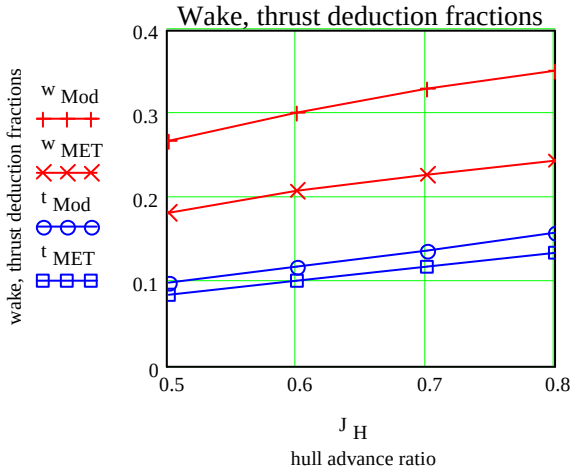
This is a fact in physics and other sciences, as well as in standards, not only in ISO 15016: 2002-06, but also in such fundamental standards as DIN 1313: 1998-12: 'Grössen' and the related standard ISO 31: 1992: 'Quantities', now ISO 80000-1: 2009: 'General'.

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



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.

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 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

$$P_{S\,sup} = p_0 \cdot N^3 + p_1 \cdot N^2 \cdot V_H$$

relating shaft power supplied $P_{S\,sup}$, 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,

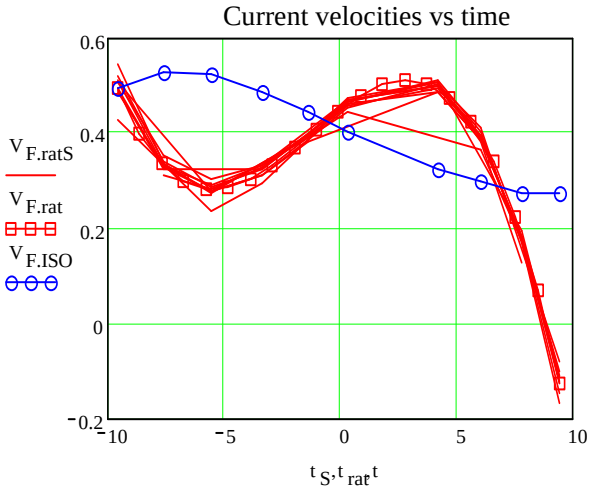
- that an adequate propeller convention is a function of two parameters only, and
- that an adequate current convention is a function of only two parameters as well, and
- that both sets of parameters can be identified as the solution of only one set of linear equations.

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.

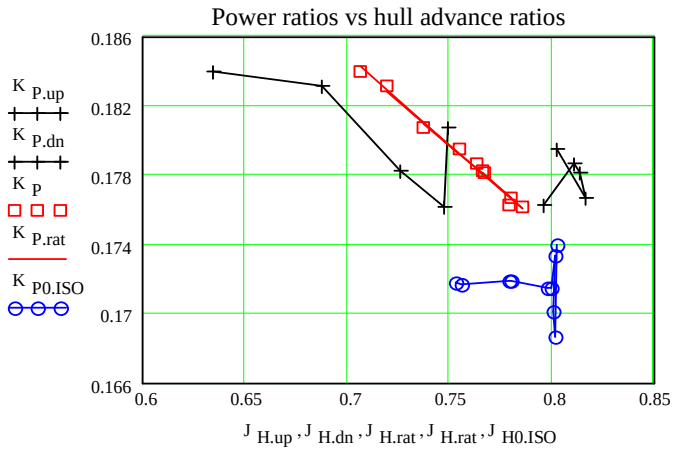


Current velocities in the example appended to ISO 15016: 2002-06, as derived by the ISO procedure, blue circles, and as identified by the rational procedure, red squares:

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, as derived by the ISO procedure, blue circles, and as identified by the rational

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.

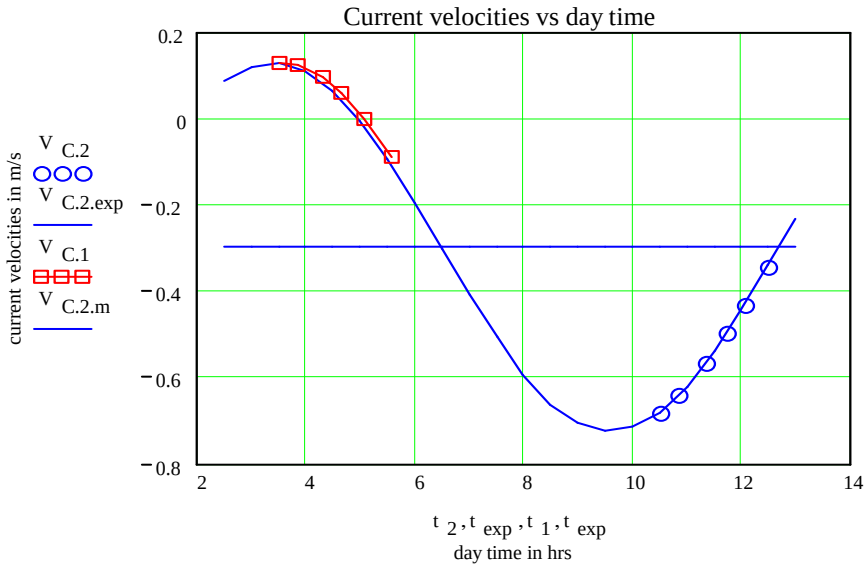
Similarly the method suggested in the forthcoming paper of Naoji Toki is hopelessly old-fashioned and obsolete already before its publication, despite my timely, repeated, detailed explanations and suggestions concerning the basic problems to be solved. None of these problems has been mentioned, forgetting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

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

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

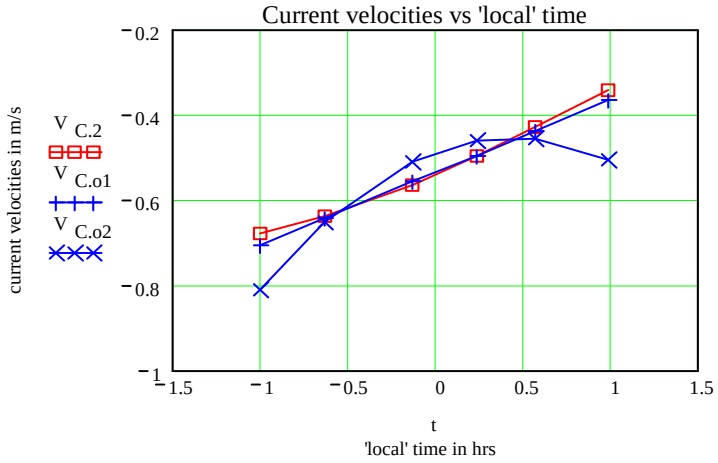
Subsequently in a second step the parameters of simple models for the partial shaft powers required have to be identified, conveniently again as solutions of a system of linear equations.

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.

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.



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

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.

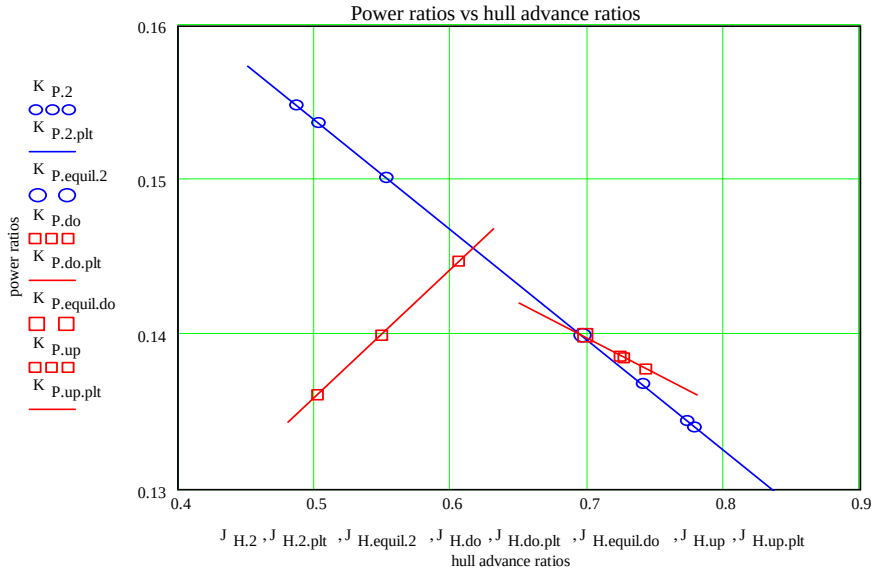
I had used the convention stated already earlier to account for the fact, that usually the relative wind and wave speeds are closely correlated. Thus the problem is inherently singular, the two effects cannot be separated without some additional convention and parameters to be assumed, *i. e.* sucked 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.

In case of the ANONYMA trials only the wave height has been reported,



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.

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 rethinking 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

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Cover letter inviting discussions

----- Original Message -----

From: "Michael Schmiechen" <m.schm@t-online.de>

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Sent: Tuesday, May 14, 2013 2:00 PM

Subject: Future Ship Powering Trials and Monitoring Now!

Dear colleagues and students,

with the letter attached I want to draw your attention to a paper, which I have drafted for publication and presentation in commemoration of my METEOR tests in the Greenland Sea in November 1988 and of my proposal of 1998 for the rational assessment of ship powering trials and, last but not least, in view of the long overdue revision of the not only error prone standard ISO 15016: 2002-06.

You find the paper on my website www.m-schmiechen.de under 'News on ship powering trials', as documentary pdf-file and as htm-file, the latter including live hyperlinks to all the material referred to, in particular to my recent evaluation of the trials with the bulk carrier ANONYMA in ballast condition at two different trim settings.

With kind regards yours,
Michael Schmiechen.

PS: Please feel free to forward this mail to colleagues, who also may be or should be 'concerned', to members of the governing bodies of ITTC, ISO and IMO in particular.

MS 20.08.2014 09:48 h

Michael Schmiechen

To my Colleagues and Students and to Whom it may Concern

Berlin, May 12, 2013

Future Ship Powering Trials and Monitoring Now! Paper drafted for publication and presentation

Dear Colleagues and Students,

in the Section 'News on ship powering trials' on my website www.m-schmiechen.de you find the draft of a paper on 'future ship powering trials and monitoring now' proposed for publication and presentation on the occasions of

- the 25th anniversary of my propulsion tests with the research vessel METEOR in the Greenland Sea between Spits Bergen and Greenland during her voyage from Hamburg to Bergen from October 27 to November 22 1988,
- the 15th anniversary of a proposed rational alternative standard for the assessment of the powering performance of ships based on traditional trials submitted to the Japan Marine Standards Association on April 15, 1998 triggered by the Committee Draft of ISO 15016,

and, last but not least, in view of

- the long overdue revision of the not only error prone standard ISO 15016: 2002-06.

The paper, in the informal fashion of a letter to you, is based on numerous recent exposés and on various letters to many colleagues; who have been asking for explanations, advice and assistance related to my rational theories of classical mechanics and ship theory in particular and to my profound experience in ship powering trials and monitoring.

After decades of self imposed ignorance of the state of research they begin to realise that not my purposely provocative style, intended to pinpoint deplorable states of 'the art' in theory and practice, needs to be discussed, but that my powerful solutions for their own fundamental problems require in-depth discussions and understanding, based on clear 'visions', 'Anschauungen' in Goethe's spirit, on simple principles, on only little common sense and, last but not least, on appropriate craftsmanship on all levels.

The purpose of my 'letter' is not to provide another survey of my work on propulsion, but to put together the large variety of arguments, recently for-

warded in exposés and mails addressed to colleagues and students, into one coherent 'story' and *explicitly and concisely state the principles underlying and the rules to be observed in the professional conduct and assessments of powering trials and monitoring*.

As problems cannot be solved by the methods, which have caused them, this exposition of the principles cannot be phrased in terms of the traditional jargon of naval architects, but in the languages of various pertinent, appropriate theories, from the theory of rational conflict resolution, to the theory of theories and 'down' to the theory of systems identification in noisy environments, and of solving nearly singular sets of linear equations.

My draft already refers to the ITTC 2012 Guideline, based on the aggressively marketed 'industry STA standard' and claimed to be approved, but it does not yet refer to pertinent contributions, that may have been presented at SMP '13, the 3rd International Symposium on Marine Propulsors, held at Launceston on Tasmania, while this draft underwent its final corrections.

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'.

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

With kind regards yours,
Michael Schmiechen.

MS 20.08.2014 09:48 h

Future Ship Powering Trials and Monitoring Now!

Related discussion and analyses

Discussion with Dott. Ing. Giulio Gennaro

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giulio.gennaro@sinm.it , http://www.sinm.it

[The following collection of mails is essentially as exchanged, misprints have been eliminated as well as some passages, considered to be irrelevant and/or confusing.]

Date: Thu, 23 May 2013 17:47:41
Subject: Meta-convention: limited micro-universes, cont'd

Dear Prof. Schmiechen

I perfectly agree with you when you say that if we are considering the 'limited micro universe' of current standard sea trials practice, well, to ask for more than you have engineered and documented is to ask for the moon.

I am now focused on what could be achieved with better thought and carried out sea trials, still within a practical approach.

E.g. I find it unlikely that thrust can be readily measured on-board, therefore I discount it, while I know that sea state can be readily and reliable measured, so it could be easily incorporated.

The fact that your peers try to discount your approach is clear, the reason behind it is clear: money, their money in particular!

Kind regards,
Giulio Gennaro.

Date: Thu, 23 May 2013 13:09:35 +0200
Subject: Meta-convention: limited micro-universes

Dear young colleague,

many thanks for your further affirmative statements. 'But' your mail inspires me to my last response, to a statement of my 'final', call it my meta-convention, before we shall set out for our trip to Weimar tomorrow morning.

MS 20.08.2014 09:48 h

In case of traditional 'steady' trials my well-defined micro(!)-universe of discourse has from the beginning been purposely limited to trials as usually performed, not including any decent thrust measurements, not including any decent logs, not including any decent observation of the sea state. Anything else is unrealistically asking for 'things', which may be or even are definitely unnecessary, as I now know for sure!

Whenever I am 'presenting' a paper on a well-defined micro(!)-universe of discourse, my 'peers' do not discuss, what I have achieved in my well-defined limits, but they talk about what else 'needs' to be done, what they should have done, but did neither notice as a problem nor solve within their hopelessly inadequate conceptual framework!

And discussions at the IfS Hamburg always (!) ended with statements on what I could not do. Usually I came back the next year having solved the next problem. Particularly interesting examples have been my design and tests of an energy wake adapted ducted propulsors without ever mentioning the concept of thrust, also historical now and also not yet acknowledged!

Continue to dare and care to think yourself! Sapere aude!

Yours, Michael Schmiechen.

PS. Concerning your last question, I have been referring to a misleading header of a plot, and maybe others. No problem, just corrections necessary sometime!

Date: Wed, 22 May 2013 23:58:07
Subject: More pragmatism!, cont'd

Dear Prof. Schmiechen

I agree that, in lack of better knowledge, one can use a limited number of measured parameters and simple equations and be able do get meaningful results.

But, at the same time, whenever possible, a combination of more parameters and less complicate equations can be used. The goal might not only be to reconstruct the ideal no-wind-no-sea condition, but to get a more comprehensive evaluation on the actual effect of the environmental conditions on the propulsion.

By conducting 'long' sea trials, at different speed and along different headings, it would be possible to test different axiomatic equations for wind and sea and select the ones that give the lowest residual, and them compare them, for instance, to safekeeping results or with wind tunnel results. The goal in this case, is not just to obtain a rational and meaningful interpretation of the sea trials, but to check the modelling of different phenomena.

Of course, the lesser measurements are taken the more simple must by the form of the axiomatic equations. In this I perfectly agree with you.

I do not get why I would not like the nominal no-wind-no-sea condition, I have lost you there.

Kind regards,
Giulio Gennaro.

Date: Wed, 22 May 2013 21:48:31
Subject: More pragmatism!, cont'd

Dear young colleague,

if you continue the line of this morning's thought and apply it to environmental conventions, you will notice that any (!) function of the hull speed through the water and of the relative wind speed in forward direction resulting in residua in the confidence range 'does the job'!

And my very crude function 'happens' to have done the job many times, maybe not perfectly due to the crude wave observations as explained, but acceptably! Thus there is no need for fancy seakeeping theories and measurements of wave spectra. And thus another piece of professional superstition goes down the sewer!

I am assuming that trials are usually carried out against the waves. In my Mathcad file I have seen a mistake in the header of one of the plots, referring to the 'no relative wind condition'. This is wrong and this mistake may be met in other headers, to be corrected occasionally.

It is the 'nominal no wind and waves condition' I am referring to and which you do not like. The point you have raised has also been raised by Dr. Wagner, but again I wonder what you are talking about? In face of the crude data

available the simplest possible rule does the job of an acceptable convention, or just a reasonable job, if you like!

More sometime later next week as we shall be visiting Weimar for some days.

Yours, Michael Schmiechen.

Date: Wed, 22 May 2013 10:49:02
Subject: More pragmatism!

Dear young colleague,

of course there is no need to distinguish between equivalent current 'laws' unless you have other purposes, as I had in case of ANONYMA, the need to extrapolate explicitly stated!

Another piece of professional superstition down the sewer!

More maybe later during the day.
Yours, Michael Schmiechen.

Date: Tuesday, May 21, 2013 9:17 PM
Subject: Common ground reached!

Dear young colleague,

many thanks for your detailed response.

Now we have established common ground, not the stupid 'level playing field' Henk van den Boom is talking about in the subtitle of his paper. Only two remarks for now.

I have used the term 'flow meter' generically, for any means other than mine to measure the current, including those you mentioned.

As I have explained in detail in my draft the residua are not sufficient to decide on the current model. In case of the ANONYMA the linear model and the tidal models happened (!) to result in nearly exactly the same current and residua within the confidence range.

For that reason I have explicitly stated, that additional information is necessary for the decision on the model, if different models result in residua in the confidence range; see page 31, line 5 thru 13, and the figure on page 32).

With kind regards yours,
Michael Schmiechen

Date: Tuesday, May 21, 2013 3:55 PM
Subject: Further comments, cont'd

Dear Prof. Schmiechen

thanks for your kind reply.

MS: With my axiomatic models of the propeller and the current I am not only precisely defining, 'constituting' the meaning of the concepts of the propeller in the behind condition and of the current, respectively, but providing methods to identify their values. Naval architects may like or not! And nobody can be surprised, that my procedure always works, on model scale and on full scale, even under severe service conditions, when all your fancy flow meters, if any, are doomed to fail!

GG: I perfectly agree that an axiomatic model of the current, by forcing the value of the current, can be of extreme help in making sense in data that, without the use of axiomatic model, would be of little use if any I never doubt it and I used a similar model in the past to criticise the extremely biased and unprofessional evaluation of sea trials / service records made by several shipping company and consultancy firms.

MS: As soon as you have understood this, you will know that it is hopelessly unprofessional to continue talking about flow meters that cannot be calibrated accordingly! As I have explained this is as unprofessional as inventing thrust meters that cannot be calibrated.

GG: I have perfectly understood the above. Please note that I am not at all talking about flow meters, and I have no idea where you got your impression that I was talking about flow meters. As a matter of fact I despise any kind of flow meters and I deem that any engine monitoring based on measuring FOC by means of flow meters is doomed to fail.

MS: Similarly with my extremely simple thrust deduction and wake conventions I am not only precisely defining, 'constituting' the meaning of the con-

cepts of resistance and wake, but also providing methods to identify their values. Naval architects may like or not! That is exactly what Froude did, but I have understood what he did and I have rationalised his procedure already in 1980 and demonstrated in 1988 how it works full scale.

GG: Understood.

MS: The tidal model does not work in the ISO example, as a typhoon 'happened' to disrupt the trials! So the whole example is basically inappropriate, unless analysed in great detail as I did in case of the ANONYMA with the propeller ventilating up wind. But my first crude evaluation in 1998 already demonstrated that the whole ISO method is inherently 'wrong'. But nobody reacted at that time!

GG: I perfectly agree about the faults intrinsically built inside ISO method. The fact that a single axiomatic model for the current cannot be always right is not a criticism to your method, but a fact. In principle one should use the equations that results in the smaller residuals.

MS: This is what I call irresponsible! In fact all the colleagues involved have seriously damaged their own reputation. And exactly that happens again with all those following 'the emperor in his new clothes'.

GG: I perfectly agree with you.

Kind regards,
Giulio Gennaro.

Date: Tue, 21 May 2013 11:27:04
Subject: Further comments, cont'd

Dear young colleague,

your first two mails have been fresh and to the point, but the later ones are a mess, and now I notice that you did not understand what I am saying. Before repeating my whole paper I suggest you read it from the beginning, sentence by sentence! And try not only to understand, what I am saying, but also to draw the conclusions!

With my axiomatic models of the propeller and the current I am not only precisely defining, 'constituting' the meaning of the concepts of the propeller in the behind condition and of the current, respectively, but

providing methods to identify their values. Naval architects may like this or not! And nobody can be surprised, that my procedure always works, on model scale and on full scale, even under severe service conditions, when all your fancy flow meters, if any, are doomed to fail!

As soon as you have understood this, you will know, that it is hopelessly unprofessional to continue talking about flow meters that cannot be calibrated accordingly! As I have explained this is as unprofessional as inventing thrust meters that cannot be calibrated.

Similarly with my extremely simple thrust deduction and wake conventions I am not only precisely defining, 'constituting' the meaning of the concepts of resistance and wake, but providing methods to identify their values. Naval architects may like this or not! That's exactly what Froude did, but I have understood what he did and I have rationalised his procedure already in 1980 and demonstrated in 1988 how it works full scale.

The tidal model does not work in the ISO example, as a typhoon 'happened' to disrupt the trials! So the whole example is basically inappropriate, unless analysed in great detail as I did in case of the ANONYMA with the propeller ventilating up wind. But my first crude evaluation in 1998 already demonstrated that the whole ISO method is inherently wrong. But nobody reacted at that time!

This is what I call irresponsible! In fact all the colleagues involved have seriously damaged their own reputation. And exactly that happens again with all those following 'the emperor in his new clothes'. I am looking forward to the 27th ITTC. Even if I should not be invited as a senior delegate, I shall be at Copenhagen, only a very short trip from Berlin.

So much for this morning, as always (still) in a hurry
yours, Michael Schmiechen.

PS. For ready reference I again quote the plot of Anderson's archetypal tale: "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 pretence 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 pretence, 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.

Date: Tue, 21 May 2013 08:05:15
Subject: Further comments

Dear Prof. Schmiechen,

thanks for your kind email.

I will wait four your comments to my letter, as to amend it in a suitable manner.

As far as the ISO example I just wanted to know if my final results were comparable to yours.

I have noticed and in the case of the ISO example the cubic polynomial for the currents give small residuals, while the tidal model results in high residuals.

As far as measuring the hull speed through water: I agree that it cannot be feasible with flow-meters / speed-logs and the like. But it is feasible with other instruments, in particular with radars. In principle the accuracy on the measurement of current is +/- 0.2 m/s and +/- 5°.

A similar situation is present for the measurement of the sea state.

I agree that in case of a correlation between wind and waves the correction for wind and sea becomes coupled, but this is not always the case, e.g. in case of swell being present.

I addition I understand the influence of the wind is more or less symmetric, while this is not the case for waves and sea, as the behaviour of the ship is the same for ahead or following wind but it is different in case of ahead or following seas.

In order to explore wind and waves behaviour and to decouple one from another it would be interesting to analyse runs made not in just two opposite direction, but encompassing more direction (e.g. 45° intervals). This is a

problem with current practice, that requires a more ore less 'steady' speed to be reached, it would much less of an issue in case of instantaneous readings.

Kind regards,
Giulio Gennaro.

Date: Mon, 20 May 2013 20:48:29
Subject: Many thanks for your 'final' approval

Dear Dott. Ing. Giulio Gennaro,

many thanks for your numerical exercises and your 'final' approval.

In the first case I do not know what to comment on and I certainly will not try to do this tonight. I know that Excel is a popular environment, but for my purposes it is quite inconvenient. What are your findings beyond those you already mentioned?

Concerning your 'better' and more elaborate reply I shall make a number of suggestions tomorrow. For my taste it needs a better structure and to refer to my statements.

If I state explicitly that it does not make sense, that it is unprofessional, to measure the hull speed through the water by some flow meter, you should not tell me that it is possible. Forget it! Dr. Wagner also tried again and again to tell me the same story. Most of your comments in your mail are of the same type.

Dr. Wagner was also quite unhappy with my joint treatment of wind and waves. But I have shown in an appendix and explained at length that the relative speeds of wind and significant wave have been strictly correlated in case of ANONYMA, and they usually are so in other cases, that their effects thus cannot be separated, the problem being singular! So what are you talking about? If you have more information please use it!

For this evening I stop here and maybe I simply send you an update of your remarks and together my reply, if necessary. Before your mail arrived I had just had re-read my draft sentence by sentence again and I feel that all your questions have already been answered. Admittedly my style is peculiar and the sentences are so densely packed, that nearly each of them is a paragraph of its own.

MS 20.08.2014 09:48 h

With many thanks so far
yours, Michael Schmiechen.

Genoa, 20 May 2013

Dear. Prof. Schmiechen,

It was a pleasure to receive your draft of a paper about the rational evaluation of sea trials and model test. In the following I express some comments on the matter raised in your draft paper.

First of all it should be clear to most of people working in the shipping business that the subject of model testing and sea trials is indeed still much too obscure and there is an urgent need to shed some light.

This matter is made even more urgent due to the introduction of regulations by IMO (e.g. EEDI, EEOI, SEEMP) which should require scrutiny and monitoring of the propulsion system which, by itself, represents the largest energy consumer for the vast majority of the vessels.

Of course without proper and sharp tools any attempt to monitor the propulsion system, and in particular the performance of the hull and of the propeller, is doomed to fail. The good (or bad, depending on the point of view) is that probably few will notice the failure. Ships sail on paper, sadly, and as long as the paperwork is done, most will be content.

One of the first point you raise is the necessity to discriminate between what can and is measured and what cannot or is not measured, and in particular you point out that without a reliable measurement of the current any attempt to judge the propulsion performance is futile. I completely endorse your view.

Nowadays it is possible to measure current in a reliable way, albeit, to my knowledge, this is very seldom done. In this respect it should be recalled that most operators prefer to discard completely the Speed Log, due to its intrinsic unreliability, and decide to use the Speed Over Ground, uncorrected. Another big concern is the objective and reliable measurement of the sea state, as per the current the possibility is present, but seldom taken.

Another important point is that the evaluation of sea trials must not refer to model test in any way, sea trials are needed to evaluate the performance of the vessel and to validate, so to say, the model tests. To mix sea trials and model tests has the only result of impairing the possibility of an unbiased comparison of the two.

In my opinion the use of axiomatic equations, as per your proposal, goes in the right direction of completely separating the model test with the sea trials and of establishing a fair and unbiased ground for comparison. ….

In particular it is clear that the propeller can (and in principle must!) be used as a measurement device: …

Moving the focus from sea trials to model tests further comments arise. I concur that the entire notion of open water testing is devoid of any meaning. The propeller must be investigate 'in behind' conditions, conditions for which it is (should!) be designed and in
which it will be operated. Therefore testing in open water is testing in an extreme off design conditions.

A final remark about confidential and sensitive data. A lot of people use the above just to frustrate other's effort. Data, even the ones that are truly sensitive, are such as long as they are complete, take the name tag away, make the vessel unrecognisable, take away the superfluous data, and 99% of the confidentiality / sensitivity goes away. The problem is that too many people are jealous of their data, and they envy the curiosity of the colleagues. So they obtain their small revenges by refusing to share what they have.

Business is business, even when scientific in nature. If one does not have access to the data one is harmless, or far less dangerous. The above can be easily seen when attending conferences: most of the people are advertising their own success, very few people actual share knowledge and data. It is sad and bitter, but it is the nature of the man.

In conclusion I congratulate for the interesting and 'hot' draft paper. Probably too hot to be handled for many reviewers.

I share your views and concerns.

Kind regards,
Giulio Gennaro.

Date: Monday, May 20, 2013 7:21 PM
Subject: Many thanks for your approval, cont'd

Dear Prof. Schmiechen,

please find herewith attached a better and more elaborated reply to your draft paper. Please, let me have your comments. Feel free to publish it on your website if you like.

MS 20.08.2014 09:48 h

[I have copied the formal letter and deleted only few 'irrelevant' paragraphs.]

In the mean time I have taken the liberty to work on the ISO example, here with attached, for your review.

in the same file I also copied the data for the sea trials of two sister vessel, for which I kindly ask your comments.

A further comment. I do not know what 'form' you use for the equations needed to model the influence of the wind and of the waves as the incidence angle changes.

What I mean is that we can write:

Pwind = q1 |vrel| Vrel Vg

Psea = q2 H^2 Vh

but the coefficient q1 and q2 are not constant for all incidence angle, and on that I think that some additional axiom is in order.

In addition I find that

P = (q0 + q1) VH^3

is too restrictive.

In principle, by analysing model tests, sea trials and the like I have found that either of the two following equations are more apt to describe the power vs speed curve:

P = A Vh^B
or
P = A Exp (Vh B)

Kind regards,
Giulio Gennaro.

Date: Mon, 20 May 2013 11:26:40
Subject: Many thanks for your approval, cont'd

Dear Prof. Schmiechen

I kindly ask you to refrain from publishing my last email.

I will write a more comprehensive comment, if you like not just a quick e-mail full of misspelling.

I am currently gong over you analysis of ISO example (albeit I lack some input data and part of your output is missing).

I am also applying your method to a couple of cases of my own.

The first comment is that the output is consistent, which is a major success.

I also note that, in principle, sometime the tidal model gives very good results, some time not so good, in that case, in principle, the polynomial model of the current allows to 'solve' the current.

I will send my 'good' comment in the next few days, together with some more detailed questions and considerations.

Kind regards,
Giulio Gennaro.

Date: Sun, 19 May 2013 13:35:36
Subject: Many thanks for your approval!

Dear Giulio Gennaro,

many thanks for your prompt response, the first substantial discussion of my draft, and in fact a professional approval of all my statements. If you do not mind I will put it on my website immediately and later publish it together with the final version of my paper.

After having mailed my letter to many colleagues I have sent addtional explanations where necessary and addressed further individuals. A mailing to all members of the three ITTC Committees concerned with the subject is ready to be sent.

MS 20.08.2014 09:48 h

Apart of my intense correspondence we enjoy a wonderful Whitsun day here at Berlin.

With my best regards yours,
Michael Schmiechen.

Date: Sat, 18 May 2013 23:38:05
Subject: Future Ship Powering Trials and Monitoring Now!

Dear Prof. Schmiechen

I have read your paper with great interest.
I will re-read it with greater focus and paying attention also the various attachments.

I concur with most you have stated.

Let me state here below some comment of mine, in a disordered and incongruent fashion, I beg your pardon for that.

I find absurd that one states that the quality of model testing has an influence in the assessment of sea trials. The two matters are and must be totally separated. To mix the two matters implies to rendering the entire comparison meaningless. The fact that IMO and ITTC apparently endorse such statement goes a long way in assessing the usefulness of the two institutions.

I also agree about the fact the open water tests are totally useless, as one is interested in the 'in behind' efficiency, for which the propulsor is designed, not in the 'open water' efficiency, which is a completely off design condition.

Another matter of interest is the wake. The relative rotative efficiency should, at least in principle, represent how well the propulsor behaves 'in behind' in respect to the 'open water' condition. Of course, due to the different 3D wake (I underline 3D, i.e. wake distribution on the propulsor, not just average wake) between model and full scale, it goes without saying that the relative rotative efficiency should change going from model to full scale. On the contrary is kept constant!

Moreover the BIG problem, at least from the propeller designer point of view (which, I beg your pardon, is my main concern), is that model test fa-

cilities are totally uninterested in providing an estimation of the full scale 3D wake. So we end up testing (and more often than not designing) propellers in model scale 3D wake instead of the 3full scale wake in which the will operate.

I have never, I repeat, never experienced a model basing advising about 3D full scale wake, they are perfectly happy measuring the 3D model scale wake, once done that they consider their job done.

Even thought the difference might be small, the conceptual problem is huge!

I agree about the need for an accurate, reliable and repetitive measure of the current, VC, without it any consideration about powering of the vessel is rubbish! As a matter of fact nowadays the speed of the surface current can be measured directly with good confidence an reliability, albeit very few, if any, do it during sea trials.

I also agree about the wicked meaning given to 'steady' conditions, often average conditions being considered representative of steady states, which are not steady.

Today is it also possible to measure with good reliability the sea spectrum (not just the sea state by means of an equivalent BN), however, just as in the case of current, the largest majority of the
concerned parties neglect this matter.

I also concur about the false confidentiality of the data. I find totally unscientific that data, once deprived of the 'name tag', are not freely distributed, blaming it on false confidentiality issues.

E.g. we are working about the extrapolation of 3D wake from model to full scale, but most of the people in possess of model and full scale wake refuse submitting them to us, they pretend not to understand is that nobody is interested in the wake, but in its scaling, and the we do not need to know the details of the vessels.

I completely agree on the sheer folly of setting contractual obligations for conditions that will not be tested at full scale, thereby leaving a lot of room to manipulate the results to one's advantage.

Kind regards,
Giulio Gennaro.

MS 20.08.2014 09:48 h

ANONYMA trials evaluated

Detailed analyses and routines

MS 20.08.2014 09:48 h

Prof. Dr.-Ing. M.Schmiechen

MS 0306011630
0310091100
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To whom it may concern

Powering performance of a bulk carrier during speed trials in ballast condition at two trim settings reduced to the nominal no wind and waves condition

Title of the file and title of a plot corrected on 1306171630

As first evaluated data at the second, at the larger trim, i. e. at the larger nominal propeller submergence

Units, constants, routines

Reference:C:\ANONYMA_5\routines .mcd

Trials identification

TID = "ANONYMA"

Trials condition $trim := 2$

The data of the second, the later trials at the larger trim have been evaluated first, after the preliminary evaluation of the data of the first trials resulted in an unrealistic propeller power characteristic, indicating that something was 'wrong' with the data. Reasons to be revealed subsequently, when the data of the first, the earlier trials at the smaller trim are being evaluated next.

Constants

Trim at trials $\Delta T_{nom} := 3.64 \cdot m$ $\Delta T_{nom} := \frac{\Delta T_{nom}}{m}$

Draught aft $T_{aft} := 7.15 \cdot m$ $T_{aft} := \frac{T_{aft}}{m}$

Propeller tip below undisturbed surface, estimated $\Delta T_{Tip} := 1.35\ m$

Input of mean data

means := READPRN("Means_2.prn")

rstdevs := READPRN("rSdvM_2.prn")

nr := rows(means) run := 0 .. nr – 1 nr = 6.000

nc := cols(means) mag := 0 .. nc – 1 nc = 17.000

Assign data	reported		
Time	$t := means^{<0>} \cdot hr$	$t := \frac{t}{hr}$	
Shaft frequency	$N_S := means^{<2>} \cdot Hz$	$N_S := \frac{N_S}{Hz}$	$N_{S.rsdm} := rstdevs^{<2>}$
Shaft power	$P_S := means^{<1>} \cdot W$	$P_S := \frac{P_S}{MW}$	$P_{S.rsdm} := rstdevs^{<1>}$
Speed over ground	$V_G := means^{<3>} \cdot \frac{m}{s}$	$V_G := \frac{V_G \cdot s}{m}$	$V_{G.rsdm} := rstdevs^{<3>}$
Wind speed	$V_W := means^{<7>} \frac{m}{s}$	$V_W := \frac{V_W \cdot s}{m}$	$V_{W.rsdm} := rstdevs^{<7>}$
Wind direction	$\psi_W := means^{<6>} \cdot \frac{deg}{rad}$		$\psi_{W.rsdm} := rstdevs^{<6>}$
Trim	$\Delta T := means^{<5>} \ m$	$\Delta T := \frac{\Delta T}{m}$	$\Delta T_{rsdm} := rstdevs^{<5>}$
Ship speed in water	$V_{H.rep} := means^{<15>} \frac{m}{s}$	$V_{H.rep} := \frac{V_{H.rep} \cdot s}{m}$	$V_{H.rep.rsdm} := rstdevs^{<15>}$

Data in SI-Units non-dimensionalized in view of further use in some mathematical subroutines, which by definition cannot handle arguments with (different) physical dimensions!

Mean values, intermediate results

For ready reference the matrices of the mean values of the measured magnitudes, alias 'quantities', are printed here. Further down intermediate results are printed as well to permit checks óf plausibility.

$$t = \begin{bmatrix} -1.004 \\ -0.638 \\ -0.142 \\ 0.227 \\ 0.571 \\ 0.986 \end{bmatrix} \quad N_S = \begin{bmatrix} 1.748 \\ 1.748 \\ 1.900 \\ 1.587 \\ 1.587 \\ 1.898 \end{bmatrix} \quad P_S = \begin{bmatrix} 4.824 \\ 5.547 \\ 6.924 \\ 4.143 \\ 3.621 \\ 6.281 \end{bmatrix} \quad V_G = \begin{bmatrix} 7.203 \\ 5.725 \\ 6.637 \\ 4.970 \\ 6.675 \\ 7.796 \end{bmatrix}$$

$$V_W = \begin{bmatrix} 7.742 \\ 21.690 \\ 20.870 \\ 20.550 \\ 7.871 \\ 6.565 \end{bmatrix} \quad \Psi_W = \begin{bmatrix} 3.759 \\ 0.617 \\ 0.250 \\ 0.244 \\ 3.808 \\ 3.852 \end{bmatrix} \quad \Delta T = \begin{bmatrix} 4.020 \\ 3.850 \\ 3.845 \\ 3.754 \\ 3.791 \\ 3.839 \end{bmatrix} \quad V_{H.rep} = \begin{bmatrix} 7.203 \\ 5.725 \\ 6.637 \\ 4.970 \\ 6.675 \\ 7.796 \end{bmatrix}$$

$\Psi_{W_1} := 0.256$ The value reported does not fit 'into the pattern'

Relative (!) standard deviations of mean (!) values

For ready reference the matrices of the relative (!) standard deviations of mean values of the measured magnitudes are also printed here, conveniently in %. Multiplied by the factor 2 these values are estimates of the relative 95% confidence radii of the mean values.

$$\frac{N_{S.rsdm}}{\%} = \begin{bmatrix} 0.019 \\ 0.016 \\ 0.016 \\ 0.051 \\ 0.019 \\ 0.016 \end{bmatrix} \quad \frac{P_{S.rsdm}}{\%} = \begin{bmatrix} 0.099 \\ 0.077 \\ 0.071 \\ 0.102 \\ 0.110 \\ 0.080 \end{bmatrix} \quad \frac{V_{G.rsdm}}{\%} = \begin{bmatrix} 0.030 \\ 0.058 \\ 0.061 \\ 0.160 \\ 0.034 \\ 0.032 \end{bmatrix}$$

$$\frac{V_{W.rsdm}}{\%} = \begin{bmatrix} 0.604 \\ 0.249 \\ 0.233 \\ 0.366 \\ 0.565 \\ 0.687 \end{bmatrix} \quad \frac{\Psi_{W.rsdm}}{\%} = \begin{bmatrix} 0.145 \\ 5.662 \\ 7.374 \\ 11.270 \\ 0.136 \\ 0.181 \end{bmatrix} \quad \frac{\Delta T_{rsdm}}{\%} = \begin{bmatrix} 0.381 \\ 0.732 \\ 0.695 \\ 1.888 \\ 0.413 \\ 0.318 \end{bmatrix} \quad \frac{V_{H.rep.rsdm}}{\%} = \begin{bmatrix} 0.030 \\ 0.058 \\ 0.061 \\ 0.160 \\ 0.034 \\ 0.032 \end{bmatrix}$$

At the up-wind conditions, runs 2, 3, 4 (indices 1, 2, 3), the wind direction is varying considerably. The variations in the trim are also noteworthy.

Normalise data

for preliminary check of consistency only!

$n_i := last(t)$

$i := 0 .. n_i$

$J_{G_i} := J\left(D, V_{G_i}, N_{S_i}\right)$ $K_{P_i} := KP\left(\rho, D, P_{S_i}, N_{S_i}\right)$

$$J_G = \begin{bmatrix} 0.710 \\ 0.565 \\ 0.602 \\ 0.540 \\ 0.725 \\ 0.708 \end{bmatrix} \qquad K_P = \begin{bmatrix} 0.134 \\ 0.154 \\ 0.150 \\ 0.154 \\ 0.135 \\ 0.137 \end{bmatrix}$$

Sort data in down and up-wind

$S := Sort\_runs\left(J_G, K_P, \psi_H\right)$

$J_{G.do} := S^{<0>}$ $J_{G.do} = \begin{bmatrix} 0.710 \\ 0.725 \\ 0.708 \end{bmatrix}$ $K_{P.do.or} := S^{<1>}$ $K_{P.do.or} = \begin{bmatrix} 0.134 \\ 0.135 \\ 0.137 \end{bmatrix}$

$J_{G.up} := S^{<2>}$ $J_{G.up} = \begin{bmatrix} 0.565 \\ 0.602 \\ 0.540 \end{bmatrix}$ $K_{P.up.or} := S^{<3>}$ $K_{P.up.or} = \begin{bmatrix} 0.154 \\ 0.150 \\ 0.154 \end{bmatrix}$

Analyse power supplied

Confidence range of mean powers

$i := 0 .. last(P_S)$

$P_{S.sdv_i} := P_{S.rsdm_i} \cdot P_{S_i}$

$P_{S.Conf_i} := 2 \cdot mean(P_{S.sdv})$

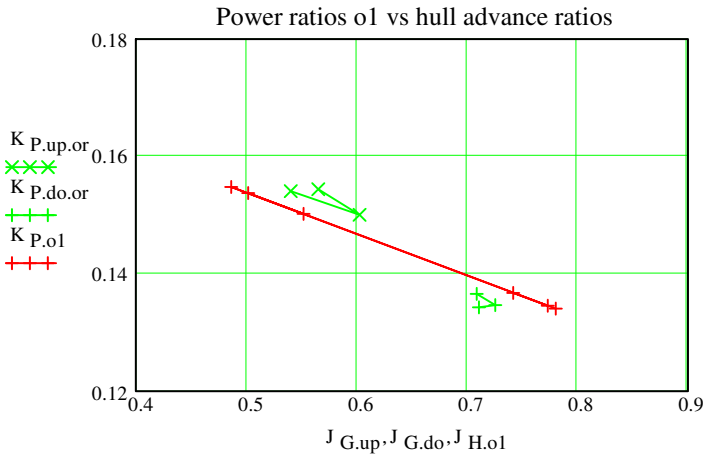
Identify current

Linear current convention

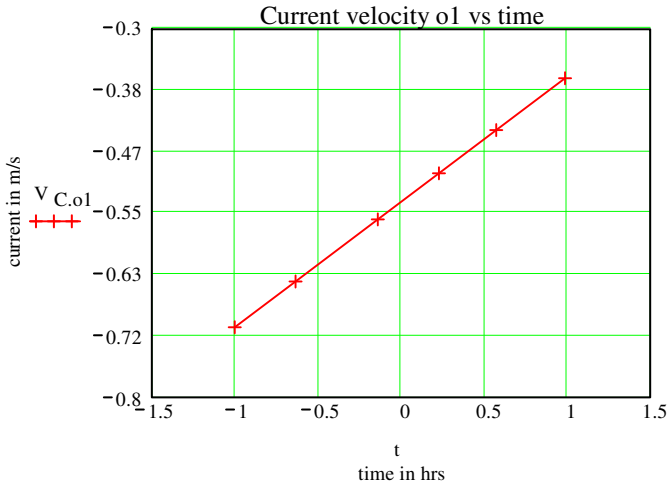
$o := 1$

$Res_{sup.o1} := Polyn\_current(o, \rho, D, t, \psi_H, V_G, N_S, P_S)$

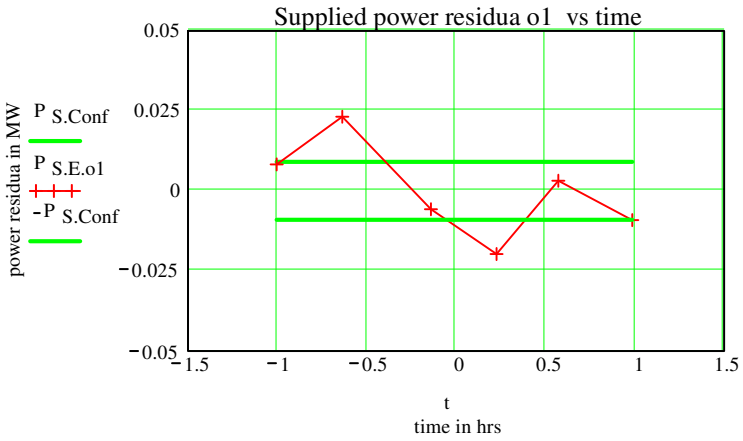
$[P_{S.E.o1} \ v_{o1} \ V_{C.o1} \ p_{o1} \ V_{H.o1} \ P_{S.o1} \ p_{nor.o1} \ J_{H.o1} \ K_{P.o1}] := Res_{sup.o1}$



Current velocity



Power residua

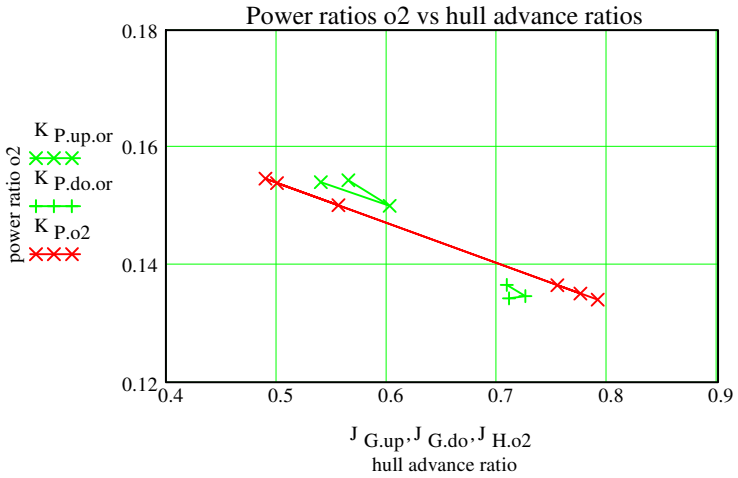


Quadratic current convention

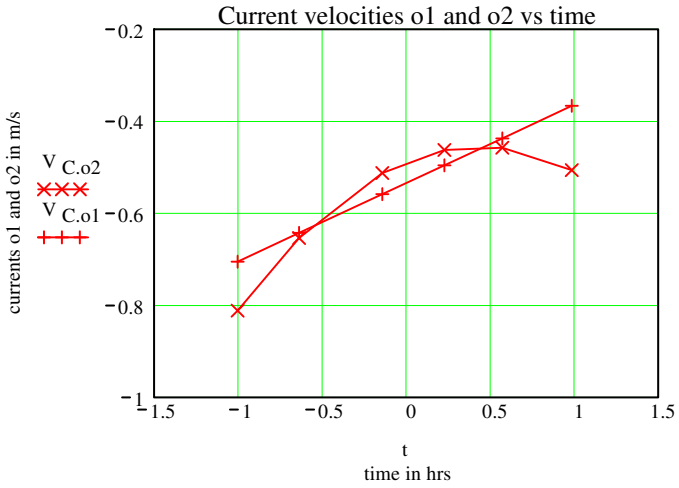
o := 2

$Res_{sup.o2} := Polyn\_current(o, \rho, D, t, \psi_H, V_G, N_S, P_S)$

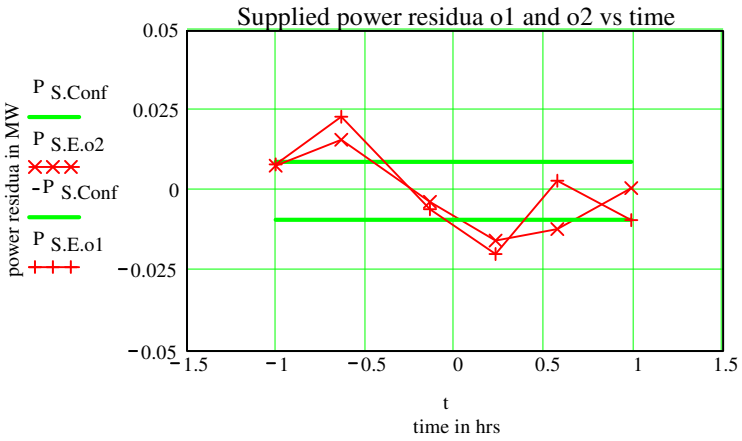
$[P_{S.E.o2}\ \ v_{o2}\ \ V_{C.o2}\ \ p_{o2}\ \ V_{H.2}\ \ P_{S.o2}\ \ p_{nor.n2}\ \ J_{H.o2}\ \ K_{P.o2}] := Res_{sup.o2}$



Current velocity



Compare power residua



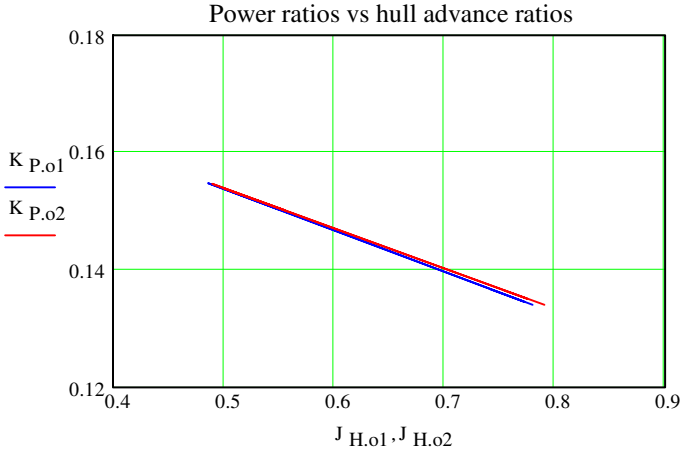
According to this detailed analysis **the linear law for the current may be considered as optimal, as most acceptable in the range of observations, as the quadratic law does not improve the quality of the approximation.**
This criterion has been used earlier for optimal estimates of spectra as described e. g. in the paper:
Schmiechen, M.: Estimation of Spectra of Truncated Transient Functions. Schiffstechnik/Ship Technology Research 46 (1999) No. 2, pp. 111/127.

And as shown in the following it happens accidentally (!) that the linear law results in nearly exactly the same current as a simple tidal law, a constant current super-imposed by a harmonic tidal current, the latter permitting extraplolation to the earlier trial at smaller trim.

An interesting observation

concerning the propeller characteristic

According to the above evaluations the propeller characteristic does not change significantly with changing order of approximation, but the small differences matter.

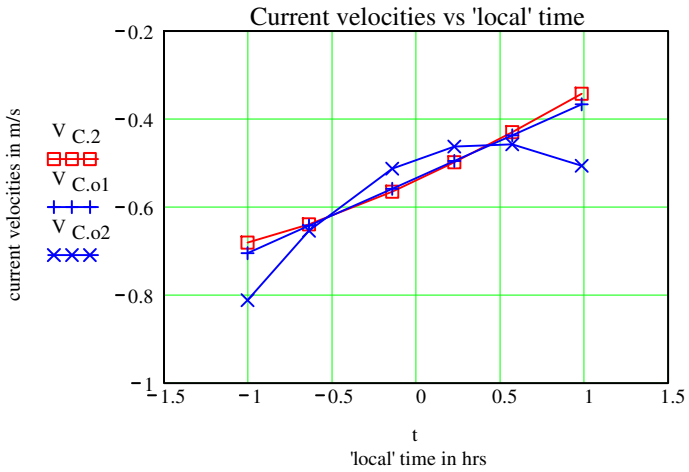


Identification of current at the larger trim

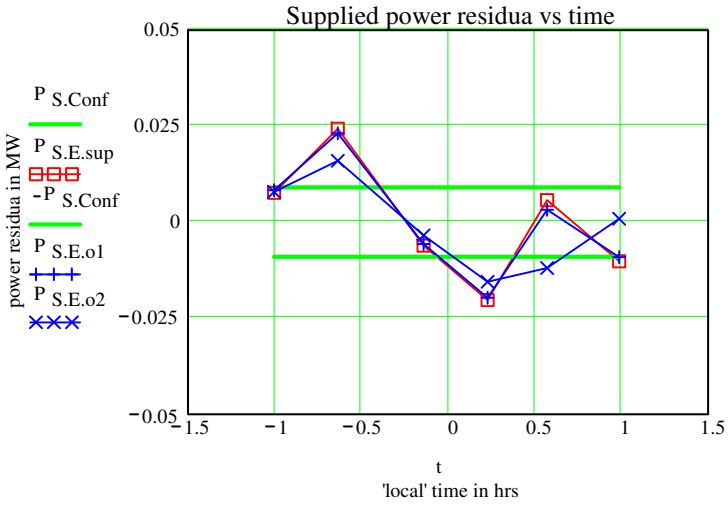
$Res_{sup} := Tidal\_current(\omega_T, t_T - t_{2.m}, \rho, D, t, \psi_H, V_G, N_S, P_S)$

$[P_{S.E.sup} \; v_2 \; V_{C.2} \; p_2 \; V_{H.2} \; P_{S.sup.2} \; p_{n.2} \; J_{H.2} \; K_{P.2}] := Res_{sup}$

Accounting for the 'uinversal' tidal period and the tidal phase, known from the table of tides, the constant current velocity and the tidal current amplitude are identified.



$$V_{C.2} = \begin{bmatrix} -0.681 \\ -0.640 \\ -0.565 \\ -0.499 \\ -0.430 \\ -0.343 \end{bmatrix}$$



$V_{C.2.mean} := v_{2_0}$ $\quad V_{C.2.mean} = -0.298$ The **mean** northerly **current** is **0.58 kn**

$V_{C.2.ampl} := v_{2_1}$ $\quad V_{C.2.ampl} = 0.427$ The **tidal current amplitude** is **0.83 kn**

Results stored

$WRITEPRN("Res\_sup\_2.prn") := Res_{sup}$

Extrapolate to current at the smaller trim

As has been mentioned earlier the identification of the current at the first trials with the smaller trim is not possible. Thus its values are determined by extrapolation based on the current and tide identified from data recorded at the second trials.

Due to the very high length of the tidal wave crudely estimated from a source readily at hand* there is no need to account for tidal phases due to the different locations of the runs in the two sets of trials, but only for a mean phase shift between the two sets of runs.

* Albert Defant: Ebbe und Flut des Meeres, der Atmosphäre und der Erdfeste. Berlin: Springer, 1953; p. 86.

The location of the first set of runs was north of second set, the rotating tide in the North Atlantic is also moving north at the location of the trials. Thus the tide at the first trials was later than that at the first trials.

$t_2 := t + t_{2.m}$ 'Global' or day time at the second trial

$\Delta t := \frac{\Delta s_{12}}{c_T}$ $\Delta t = 0.125$ Evidently the global phase correction is quite small.

$k := 0 .. 21$

$V_{C.2.m_k} := v_{2_0}$

$t_{exp_k} := -9.0 + 0.5 \cdot k$

$V_{C.2.exp_k} := VC\left(v_2, t_{exp_k} + t_{2.m}, \omega_T, t_T\right)$

Time at first trials

$means_1 := READPRN("Means\_1.prn")$

$\Delta t_1 := means_1^{<0>}$

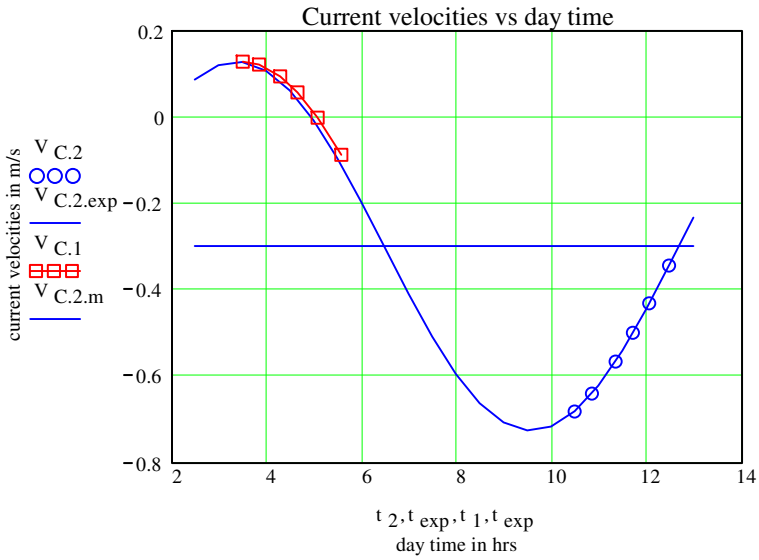
$t_1 := t_{1.m} + \Delta t_1$

$V_{C.1_i} := VC\left(v_2, t_{1_i} - \Delta t, \omega_T, t_T\right)$

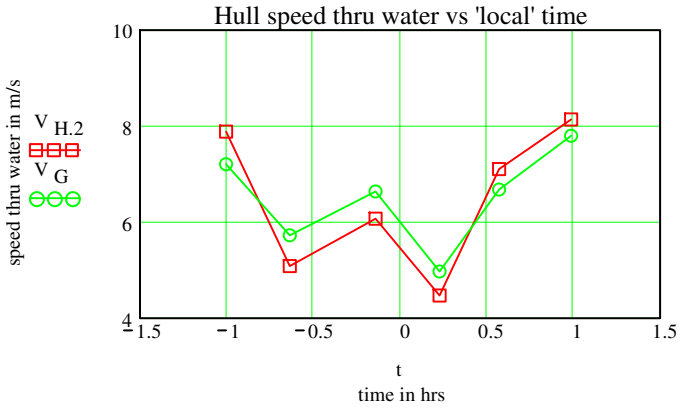
$WRITEPRN("V.C.1.prn") := V_{C.1}$ Store for the analysis of the data at the smaller trim.

$t_{exp} := t_{exp} + t_{2.m}$ 'Local' time at second trim

Plot current velocities at both locations



Ship speed thru water



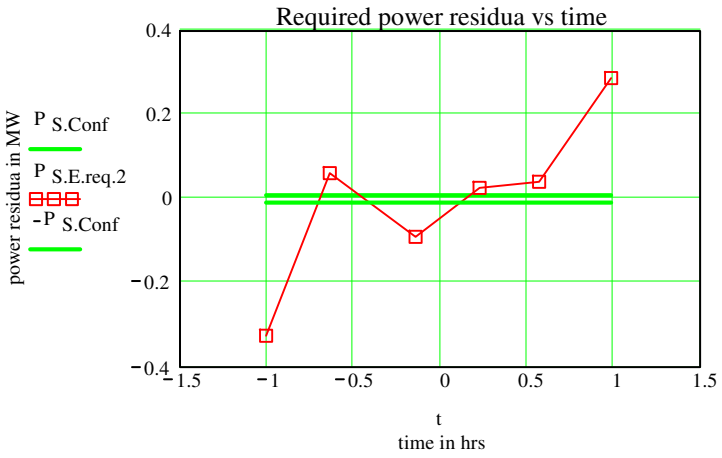
Analyse power required

Identify power (!) 'coefficients' of environment convention

$$Res_{req.2} := Required\left(V_{H.2}, \psi_H, V_{C.2}, P_S, V_W, \psi_W\right)$$

$$\left[P_{S.E.req.2} \quad q_2 \quad P_{S.req.2} \quad P_{S.req.2.0} \quad P_{S.req.2.1}\right] := Res_{req.2}$$

Required power residua



As usual the required power residua are much larger than the supplied power residua due to the uncertainties of the wind measurements and the crude wave observations.

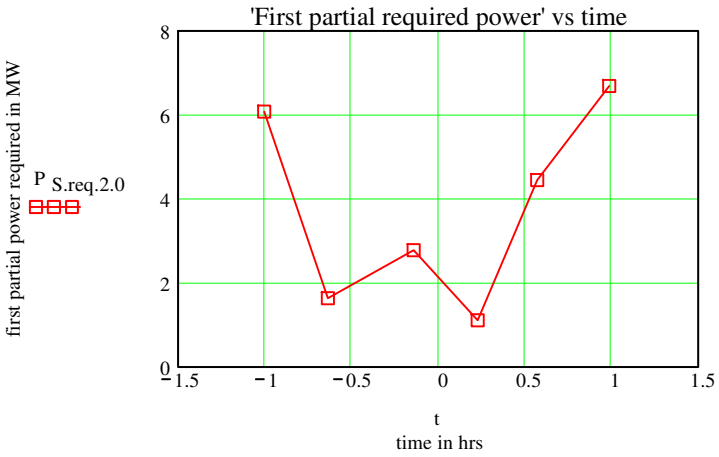
The residua can be considered as a measure of changes of the inviroment

Power required

$$P_{S.req.2} = \begin{bmatrix} 5.150 \\ 5.486 \\ 7.014 \\ 4.116 \\ 3.581 \\ 5.993 \end{bmatrix}$$

First partial power required

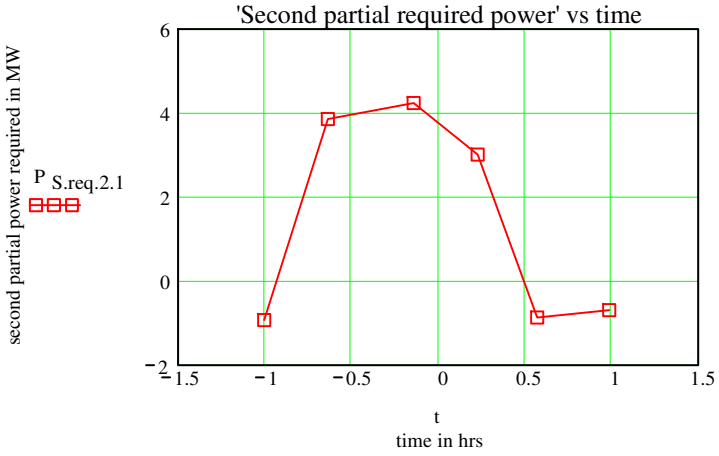
This concept has formerly, *misleadingly* been called 'water' power.



$$P_{S.req.2.0} = \begin{bmatrix} 6.076 \\ 1.631 \\ 2.776 \\ 1.108 \\ 4.448 \\ 6.685 \end{bmatrix}$$

Second partial power required

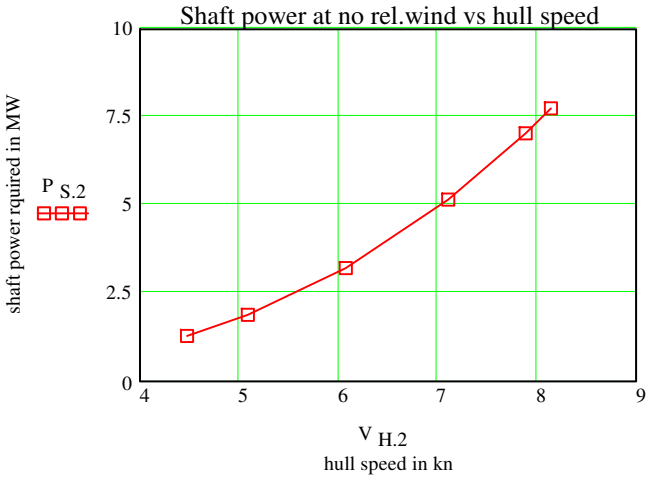
This concept has formerly, *misleadingly* been called 'wind and wave' power. both concepts include additional powes due the seastate.



$$P_{S.req.2.0} = \begin{bmatrix} 6.076 \\ 1.631 \\ 2.776 \\ 1.108 \\ 4.448 \\ 6.685 \end{bmatrix}$$

Power vs hull speed
at the nominal no wind and waves condition

$C_{PV.2} := q_{2_0} + q_{2_1}$ $\quad C_{PV.2} = 0.01437$ $\quad V_{H.2} := \text{sort}(V_{H.2})$ $\quad P_{S.2} := C_{PV.2} \cdot V_{H.2}^{3}$



$$P_{S.2} = \begin{bmatrix} 1.285 \\ 1.890 \\ 3.217 \\ 5.155 \\ 7.042 \\ 7.748 \end{bmatrix}$$

Powering performance

at the nominal no wind and waves condition

Power coefficient normalised

$$C_{PV.2.n} := \frac{C_{PV.2} \cdot 10^6}{\rho \cdot D^2}$$

Identify equilibrium

$J := 1 \qquad K := 1$

Given

$K = p_{n.2_0} + p_{n.2_1} \cdot J$

$K = C_{PV.2.n} \cdot J^3$

Solve

$$\begin{bmatrix} J_{H.equil.2} \\ K_{P.equil.2} \end{bmatrix} := \text{Find}(J, K)$$

$J_{H.equil.2} = 0.695$

$K_{P.equil.2} = 0.140$

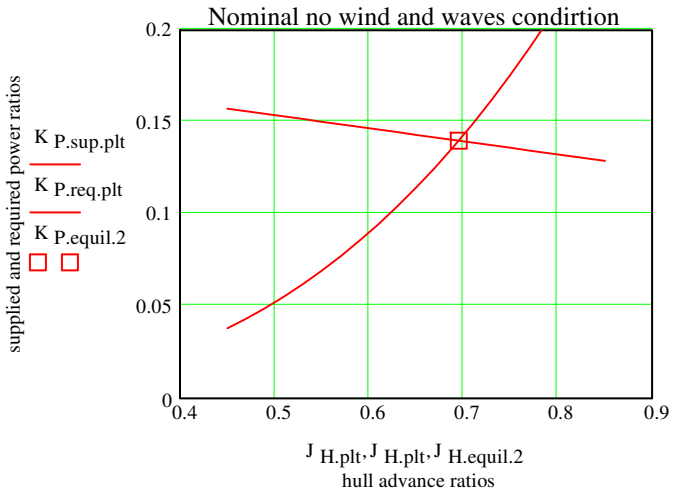
Results plotted

$k := 0 .. 20$

$J_{H.plt_k} := 0.45 + 0.02 \cdot k$

$K_{P.sup.plt_k} := p_{n.2_0} + p_{n.2_1} \cdot J_{H.plt_k}$

$K_{P.req.plt_k} := C_{PV.2.n} \cdot \left(J_{H.plt_k}\right)^3$



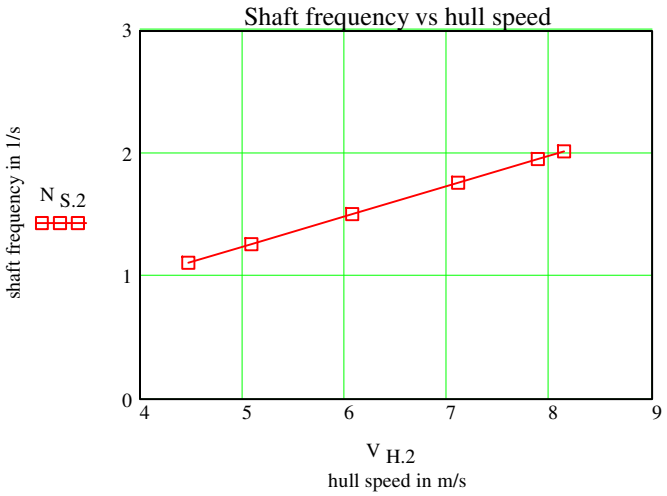
Due to the model adopted in this case the propeller is permanently operating at the same normalised condition.

Check of consistency

Frequency of shaft rev's vs hull speed at the nominal no wind and waves condition

$N_{S.2_i} := 1$ initial values

$N_{S.2} := \text{Identify\_freq}(p_2, V_{H.2}, P_{S.2}, N_{S.2})$



$$N_{S.2} = \begin{bmatrix} 1.109 \\ 1.262 \\ 1.506 \\ 1.763 \\ 1.956 \\ 2.019 \end{bmatrix}$$

Linear approximation

$A_{N.2_{i,0}} := 1$ $A_{N.2_{i,1}} := V_{H.2_i}$ $X_{N.2} := \text{geninv}(A_{N.2}) \cdot N_{S.2}$ $X_{N.2} = \begin{bmatrix} -3.1677 \cdot 10^{-5} \\ 0.2481 \end{bmatrix}$

$N_{S.E.2} := N_{S.2} - A_{N.2} \cdot X_{N.2}$ $N_{S.E.2.Conf} := 2 \cdot \text{stdev}(N_{S.E.2})$ $N_{S.E.2.Conf} = 7.225 \cdot 10^{-5}$

Per definition this result is in accordance with the nominal no wind and waves condition derived: the frequency of shaft rotation is directly proportional to the hull advance speed.

$C_{NV.2} := \dfrac{1}{D \cdot J_{H.equil.2}}$ $C_{NV.2} = 0.2481$ $N_{S.2} := C_{NV.2} \cdot V_{H.2}$ $N_{S.2} = \begin{bmatrix} 1.109 \\ 1.262 \\ 1.506 \\ 1.763 \\ 1.956 \\ 2.019 \end{bmatrix}$

Required power results

$Res_{req} := \left[P_{S.E.req.2} \;\; q_2 \;\; V_{H.2} \;\; P_{S.req.2.0} \;\; P_{S.req.2.1} \;\; P_{S.2} \;\; N_{S.2} \right]$

Store results

$\text{WRITEPRN}(\text{"Res\_req\_2.prn"}) := Res_{req}$

Appendix

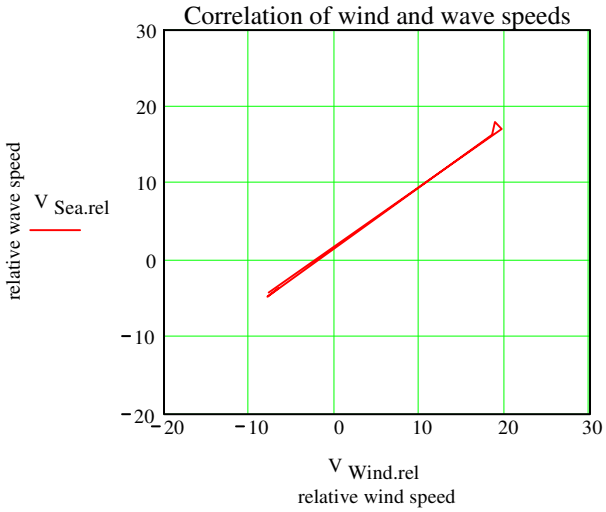
Check correlation of relative speeds of wind and hypothetical waves

$$V_{Wind.rel_i} := -V_{W_i} \cdot \cos\left(\psi_{W_i} - \psi_{H_i}\right) \cdot dir\left(\psi_{H_i}\right)$$

$$V_{Sea.rel_i} := -\left(V_S \cdot dir\left(\psi_{H_i}\right) - V_{G_i}\right)$$

$$V_{Wind.rel} = \begin{bmatrix} -7.717 \\ 19.606 \\ 18.815 \\ 18.470 \\ -7.867 \\ -6.565 \end{bmatrix}$$

$$V_{Sea.rel} = \begin{bmatrix} -4.195 \\ 17.123 \\ 18.035 \\ 16.368 \\ -4.723 \\ -3.602 \end{bmatrix}$$



END
As first evaluated data at the second, at the larger trim, i. e. at the larger propeller submergence

Prof. Dr.-Ing. M.Schmiechen

MS 0306011630
0310091100
1107121300

To whom it may concern

Powering performance
of a bulk carrier
during speed trials
in ballast condition
at two trim settings
reduced to the nominal no wind and waves condition

1205041600
1207201400

1301081830
1305081300

Title of the file corrected on
1306171650

As next evaluated data at the first, at the smaller trim, i. e. at the smaller nominal propeller submergence

Units, constants, routines

Reference:C:\ANONYMA_5\routines .mcd

Trials identification

TID = "ANONYMA"

Constants

Trials condition: trim := 1

Trim at trials: $\Delta T := 1.44 \cdot m$ $\quad \Delta T := \frac{\Delta T}{m}$

Draught aft: $T_{aft} := 6.07 \cdot m$ $\quad T_{aft} := \frac{T_{aft}}{m}$

Propeller tip below undisturbed surface, estimated: $\Delta T_{Tip} := 0.27\ m$

Input of mean data

means := READPRN("Means_1.prn")

rstdevs := READPRN("rSdvM_1.prn")

nr := rows(means) $\quad$ run := 0 .. nr − 1 $\quad$ nr = 6.000

nc := cols(means) $\quad$ mag := 0 .. nc − 1 $\quad$ nc = 17.000

Assign data reported

Time	$t := means^{<0>} \cdot hr$	$t := \frac{t}{hr}$	
Shaft frequency	$N_S := means^{<2>} \cdot Hz$	$N_S := \frac{N_S}{Hz}$	$N_{S.rsdm} := rstdevs^{<2>}$
Shaft power	$P_S := means^{<1>} \cdot W$	$P_S := \frac{P_S}{MW}$	$P_{S.rsdm} := rstdevs^{<1>}$
Speed over ground	$V_G := means^{<3>} \cdot \frac{m}{s}$	$V_G := \frac{V_G \cdot s}{m}$	$V_{G.rsdm} := rstdevs^{<3>}$
Wind speed	$V_W := means^{<7>} \frac{m}{s}$	$V_W := \frac{V_W \cdot s}{m}$	$V_{W.rsdm} := rstdevs^{<7>}$
Wind direction	$\psi_W := means^{<6>} \cdot \frac{deg}{rad}$		$\psi_{W.rsdm} := rstdevs^{<6>}$
Trim	$\Delta T := means^{<5>} \ m$	$\Delta T := \frac{\Delta T}{m}$	$\Delta T_{rsdm} := rstdevs^{<5>}$
Ship speed in water	$V_{H.rep} := means^{<15>} \frac{m}{s}$	$V_{H.rep} := \frac{V_{H.rep} \cdot s}{m}$	$V_{H.rep.rsdm} := rstdevs^{<15>}$

Data in SI-Units non-dimensionalized in view of further use in some mathematical subroutines, which by definition cannot handle arguments with (different) physical dimensions!

Mean values, intermediate results

For ready reference the matrices of the mean values of the measured magnitudes, alias 'quantities', are printed here. Further down intermediate results are printed as well to permit checks óf plausibility.

$$t = \begin{bmatrix} -0.989 \\ -0.647 \\ -0.200 \\ 0.161 \\ 0.587 \\ 1.088 \end{bmatrix} \quad N_S = \begin{bmatrix} 1.588 \\ 1.580 \\ 1.746 \\ 1.892 \\ 1.893 \\ 1.747 \end{bmatrix} \quad P_S = \begin{bmatrix} 3.700 \\ 3.602 \\ 5.027 \\ 6.590 \\ 6.343 \\ 4.945 \end{bmatrix} \quad V_G = \begin{bmatrix} 6.819 \\ 4.475 \\ 5.455 \\ 6.584 \\ 7.946 \\ 7.439 \end{bmatrix}$$

$$V_W = \begin{bmatrix} 7.120 \\ 11.710 \\ 12.190 \\ 12.630 \\ 6.721 \\ 6.685 \end{bmatrix} \quad \Psi_W = \begin{bmatrix} 5.095 \\ 0.406 \\ 0.369 \\ 0.306 \\ 5.489 \\ 5.442 \end{bmatrix} \quad \Delta T = \begin{bmatrix} 1.276 \\ 1.222 \\ 1.225 \\ 1.211 \\ 1.266 \\ 1.278 \end{bmatrix} \quad V_{H.rep} = \begin{bmatrix} 6.819 \\ 4.475 \\ 5.455 \\ 6.584 \\ 7.945 \\ 7.439 \end{bmatrix}$$

Relative (!) standard deviations of mean (!) values

For ready reference the matrices of the relative (!) standard deviations of mean values of the measured magnitudes are also printed here, conveniently in %. Multiplied by the factor 2 these values are estimates of the 95% confidence radii of the mean values.

$$\frac{N_{S.rsdm}}{\%} = \begin{bmatrix} 0.031 \\ 0.093 \\ 0.054 \\ 0.021 \\ 0.019 \\ 0.026 \end{bmatrix} \quad \frac{P_{S.rsdm}}{\%} = \begin{bmatrix} 0.139 \\ 0.297 \\ 0.210 \\ 0.083 \\ 0.077 \\ 0.115 \end{bmatrix} \quad \frac{V_{G.rsdm}}{\%} = \begin{bmatrix} 0.039 \\ 0.114 \\ 0.077 \\ 0.058 \\ 0.027 \\ 0.036 \end{bmatrix}$$

$$\frac{V_{W.rsdm}}{\%} = \begin{bmatrix} 0.619 \\ 0.356 \\ 0.252 \\ 0.352 \\ 0.556 \\ 0.578 \end{bmatrix} \quad \frac{\Psi_{W.rsdm}}{\%} = \begin{bmatrix} 0.098 \\ 0.834 \\ 0.810 \\ 0.715 \\ 0.167 \\ 0.129 \end{bmatrix} \quad \frac{\Delta T_{rsdm}}{\%} = \begin{bmatrix} 1.425 \\ 4.980 \\ 3.363 \\ 2.613 \\ 1.291 \\ 1.288 \end{bmatrix} \quad \frac{V_{H.rep.rsdm}}{\%} = \begin{bmatrix} 0.039 \\ 0.114 \\ 0.077 \\ 0.058 \\ 0.027 \\ 0.036 \end{bmatrix}$$

At the up-wind conditions, runs 2, 3, 4 (indices 1, 2, 3), the wind direction is varying considerably. The variations in the trim are also noteworthy.

Normalise data

for preliminary check of consistency only!

$n_i := last(t)$

$i := 0 .. n_i$

$J_{G_i} := J\left(D, V_{G_i}, N_{S_i}\right)$ $K_{P_i} := KP\left(\rho, D, P_{S_i}, N_{S_i}\right)$

$$J_G = \begin{bmatrix} 0.740 \\ 0.488 \\ 0.539 \\ 0.600 \\ 0.724 \\ 0.734 \end{bmatrix} \qquad K_P = \begin{bmatrix} 0.137 \\ 0.136 \\ 0.140 \\ 0.145 \\ 0.139 \\ 0.138 \end{bmatrix}$$

Sort data in down and up-wind

$S := Sort\_runs\left(J_G, K_P, \psi_H\right)$

$J_{G.do} := S^{<0>}$ $J_{G.do} = \begin{bmatrix} 0.740 \\ 0.724 \\ 0.734 \end{bmatrix}$ $K_{P.do.or} := S^{<1>}$ $K_{P.do.or} = \begin{bmatrix} 0.137 \\ 0.139 \\ 0.138 \end{bmatrix}$

$J_{G.up} := S^{<2>}$ $J_{G.up} = \begin{bmatrix} 0.488 \\ 0.539 \\ 0.600 \end{bmatrix}$ $K_{P.up.or} := S^{<3>}$ $K_{P.up.or} = \begin{bmatrix} 0.136 \\ 0.140 \\ 0.145 \end{bmatrix}$

All results at trim 2

trim := 2

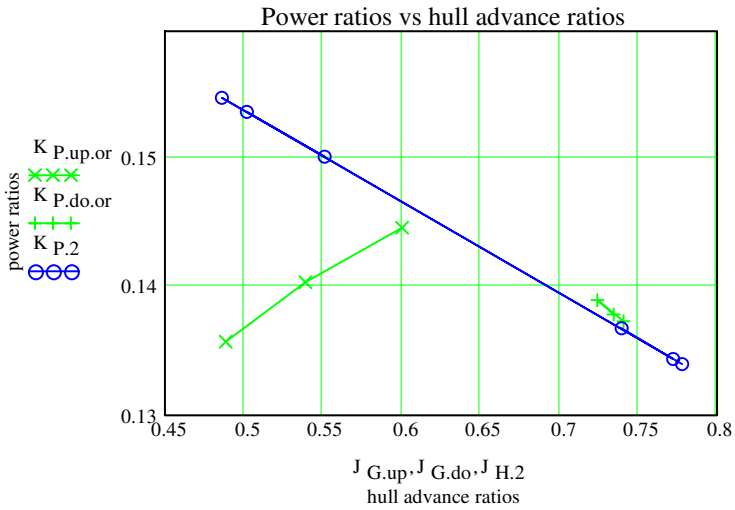
Res sup_2 := READPRN("Res_sup_2.prn")

Res req_2 := READPRN("Res_req_2.prn")

$\left[P_{S.E.sup.2} \; v_{C.2} \; V_{C.2} \; p_{C.2} \; V_{H.2} \; P_{S.2} \; p_{n.2} \; J_{H.2} \; K_{P.2} \right] := Res_{sup\_2}$

$\left[P_{S.E.req.2} \; q_2 \; V_{H.2} \; P_{S.req.2.0} \; P_{S.req.2.1} \; P_{S.2} \; N_{S.2} \right] := Res_{req\_2}$

Scrutinise data



Evidently **the propeller is ventilated at the up-wind condition. Thus the global evaluation is non-sensensical, particularly with 'corrected' values!**
The ventilation is presumably due to the very small submergence of the propeller in combination with the pitching in the sea state reported..

Evaluation

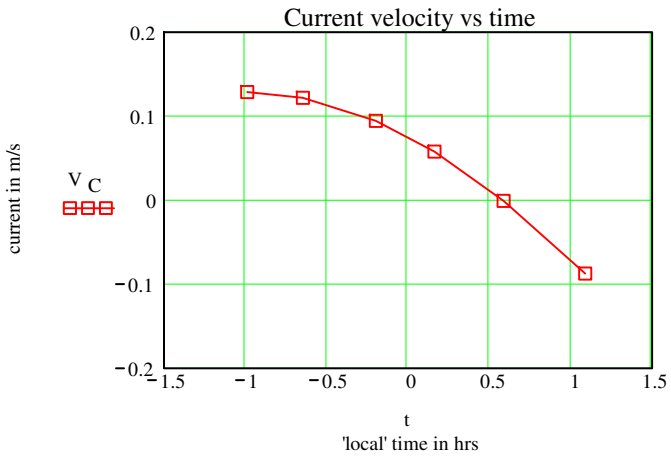
differing from my standard routine
concerning the power supplied
due to propeller ventilation up-wind

trim := 1

Current velocity

as extrapolated from trials at the larger trim!

$V_C := READPRN("V.C.1.prn")$

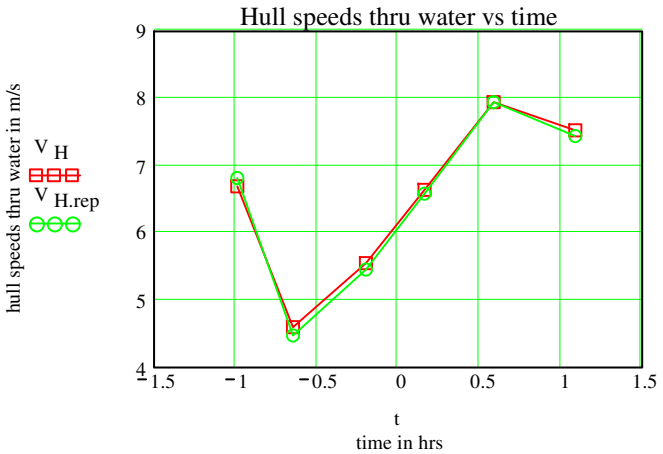


$$V_C = \begin{bmatrix} 0.129 \\ 0.122 \\ 0.094 \\ 0.058 \\ -9.648 \cdot 10^{-4} \\ -0.087 \end{bmatrix}$$

Hull speed thru water

$V_{H_i} := V_{G_i} - dir(\psi_{H_i}) \cdot V_{C_i}$

As in case of the reported KP = 2 π KQ values one correction has been made in the original evaluation according to ISO 15016: 2002-06 reported.



$$V_H = \begin{bmatrix} 6.691 \\ 4.597 \\ 5.549 \\ 6.642 \\ 7.947 \\ 7.526 \end{bmatrix}$$

Sort data for runs up and down wind

$S_{i,0} := V_{H_i}$ $S_{i,1} := P_{S_i}$ $S_{i,2} := N_{S_i}$ $S_{i,3} := P_{S.rsdm_i}$

$S_{i,4} := t_i$ $S_{i,5} := V_{C_i}$ $S_{i,6} := V_{W_i}$ $S_{i,7} := \psi_{W_i}$

$S := csort(S, 0)$

$V_{H.1_i} := S_{i,0}$ $P_{S.1_i} := S_{i,1}$ $N_{S.1_i} := S_{i,2}$ $sd_{m.r_i} := S_{i,3}$

$t_{srt_i} := S_{i,4}$ $V_{C_i} := S_{i,5}$ $V_{W_i} := S_{i,6}$ $\psi_{W_i} := S_{i,7}$

$n_r := \frac{n}{2} - 1$

$j := 0 .. n_r$

$V_{H.up_j} := V_{H.1_j}$ $P_{S.up.or_j} := P_{S.1_j}$ $N_{S.up_j} := N_{S.1_j}$ $sd_{m.r.up_j} := sd_{m.r_j}$

$t_{up_j} := t_{srt_j}$ $V_{C.up_i} := V_{C_j}$ $V_{W.up_j} := V_{W_j}$ $\psi_{W.up_j} := \psi_{W_j}$

$\psi_{H.up_j} := \psi_{H.up}$

$V_{H.do_j} := V_{H.1_{3+j}}$ $P_{S.do.or_j} := P_{S.1_{3+j}}$ $N_{S.do_j} := N_{S.1_{3+j}}$ $sd_{m.r.do_j} := sd_{m.r_{3+j}}$

$t_{do_j} := t_{srt_{3+j}}$ $V_{C.do_i} := V_{C_{3+j}}$ $V_{W.do_j} := V_{W_{3+j}}$ $\psi_{W.do_j} := \psi_{W_{3+j}}$

$\psi_{H.do_j} := \psi_{H.do}$

Analyse powers supplied

$\left[P_{S.E.sup.up} \;\; p_{up} \;\; P_{S.up} \;\; p_{n.up} \;\; J_{H.up} \;\; K_{P.up}\right] := No\_current\left(\rho, D, V_{H.up}, N_{S.up}, P_{S.up.or}\right)$

$\left[P_{S.E.sup.do} \;\; p_{do} \;\; P_{S.do} \;\; p_{n.do} \;\; J_{H.do} \;\; K_{P.do}\right] := No\_current\left(\rho, D, V_{H.do}, N_{S.do}, P_{S.do.or}\right)$

Confidence ranges of mean powers

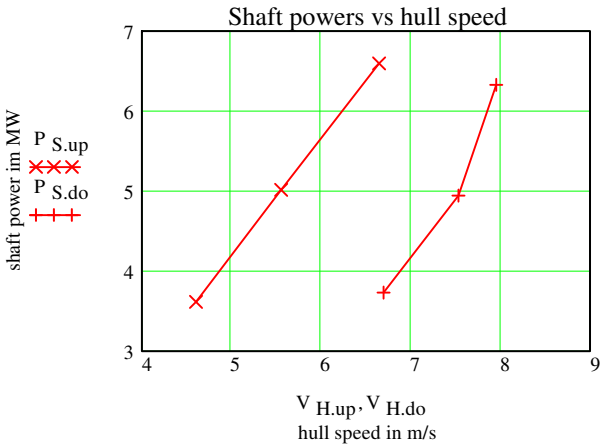
$j := 0 .. n_r$

$P_{S.sdv.up_j} := sd_{m.r.up_j} \cdot P_{S.up_j}$

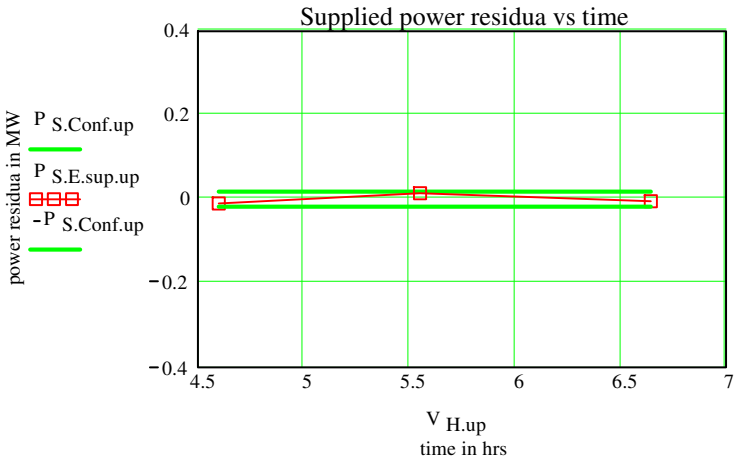
$P_{S.Conf.up_j} := 2 \cdot mean\left(P_{S.sdv.up}\right)$

$P_{S.sdv.do_j} := sd_{m.r.do_j} \cdot P_{S.do_j}$

$P_{S.Conf.do_j} := 2 \cdot mean\left(P_{S.sdv.do}\right)$

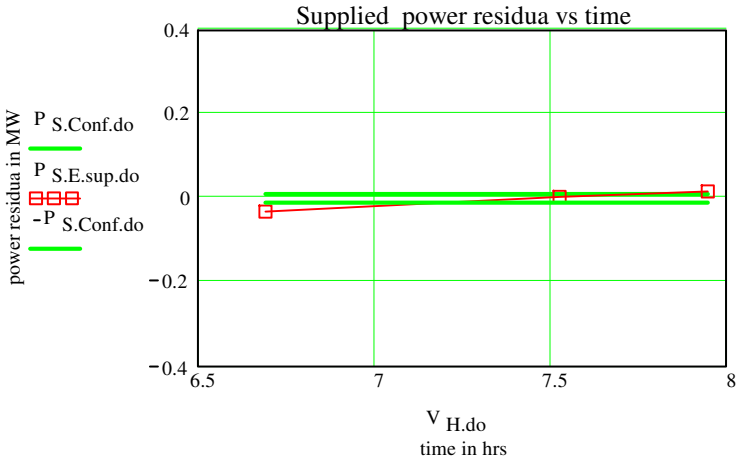


Supplied power residua up wind



$$P_{S.E.sup.up} = \begin{bmatrix} -0.011 \\ 0.014 \\ -0.005 \end{bmatrix}$$

Supplied power residua down wind



$$P_{S.E.sup.do} = \begin{bmatrix} -0.032 \\ 0.003 \\ 0.016 \end{bmatrix}$$

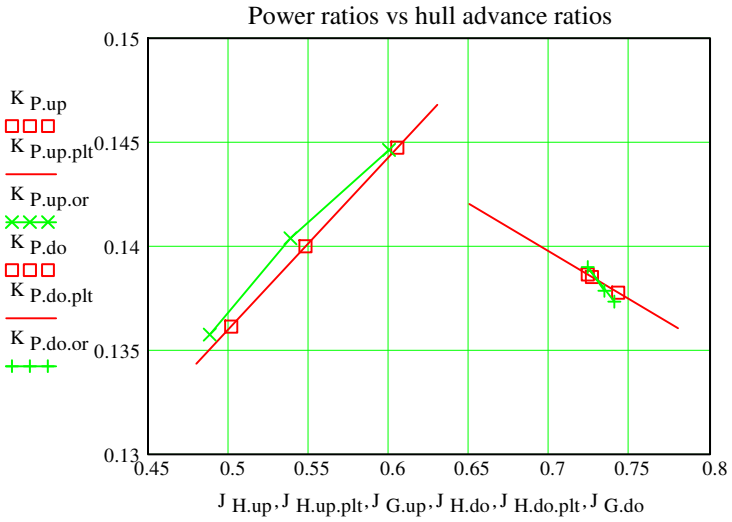
Plot normalised results

$k := 0 .. 1$

$J_{H.up.plt} := \begin{bmatrix} 0.48 \\ 0.63 \end{bmatrix}$ $\quad K_{P.up.plt_k} := p_{n.up_0} + p_{n.up_1} \cdot J_{H.up.plt_k}$

$J_{H.do.plt} := \begin{bmatrix} 0.65 \\ 0.78 \end{bmatrix}$ $\quad K_{P.do.plt_k} := p_{n.do_0} + p_{n.do_1} \cdot J_{H.do.plt_k}$

$J_{H.2.plt} := \begin{bmatrix} 0.45 \\ 0.85 \end{bmatrix}$ $\quad K_{P.2.plt_k} := p_{n.2_0} + p_{n.2_1} \cdot J_{H.2.plt_k}$



$J_{H.do} = \begin{bmatrix} 0.726 \\ 0.743 \\ 0.724 \end{bmatrix}$

$K_{P.do} = \begin{bmatrix} 0.139 \\ 0.138 \\ 0.139 \end{bmatrix}$

Analyse powers required

Due to the ventilation of the propeller at the up-wind runs of the trial with the first, the smaller trim the routines had to be further adapted.

Partial powers required identified

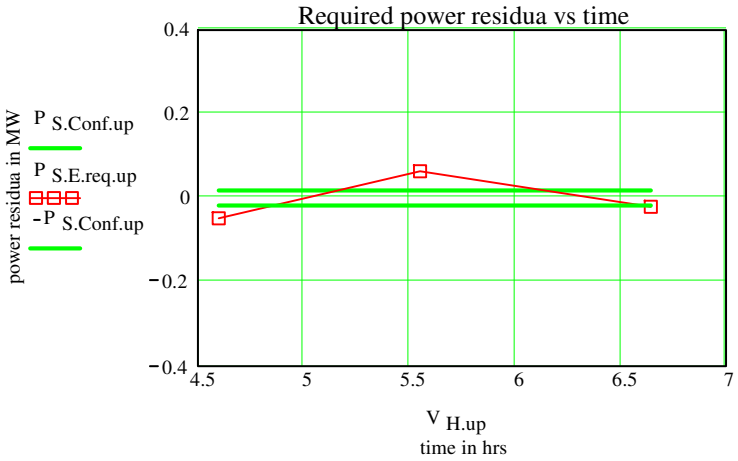
$Res_{req.up} := Required\left(V_{H.up}, \psi_{H.up}, V_{C.up}, P_{S.up.or}, V_{W.up}, \psi_{W.up}\right)$

$\left[P_{S.E.req.up} \quad \eta_{1.up} \quad P_{S.req.up} \quad P_{S.req.up.0} \quad P_{S.req.up.1}\right] := Res_{req.up}$

$Res_{req.do} := Required\left(V_{H.do}, \psi_{H.do}, V_{C.do}, P_{S.do.or}, V_{W.do}, \psi_{W.do}\right)$

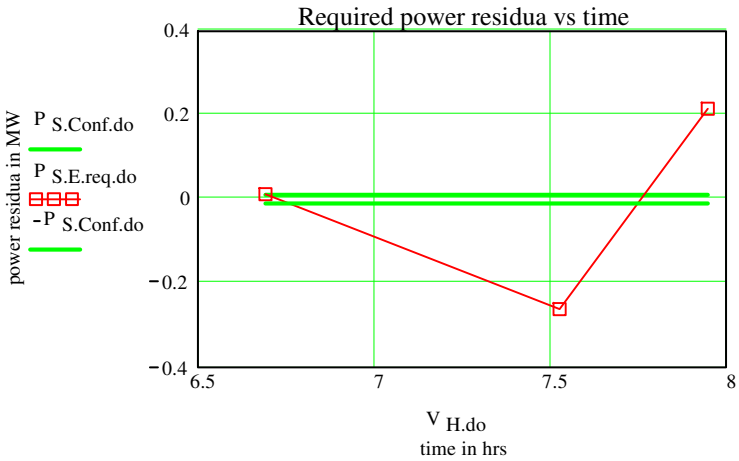
$\left[P_{S.E.req.do} \quad \eta_{1.do} \quad P_{S.req.do} \quad P_{S.req.part.0} \quad P_{S.req.part.1}\right] := Res_{req.do}$

Required power residua up wind



$$q_{1.up} = \begin{bmatrix} 0.0064 \\ 0.0052 \end{bmatrix}$$

Required power residua down wind



$$q_{1.do} = \begin{bmatrix} 0.0122 \\ -9.2718 \cdot 10^{-4} \end{bmatrix}$$

As usual the required power residua are much larger than the supplied power residua due to the uncertainties of the wind measurements and the crude wave observations.

But in case of the down wind condition the few values available evediently do not permit to identify the value of the second parameter reliably. To solve this problem the convention is adopted, that its value is the same as in case of the lager trim.

$$q_{1.do_1} := q_{2_1}$$

Power required, propeller not ventilating, at the nominal no wind and waves condition

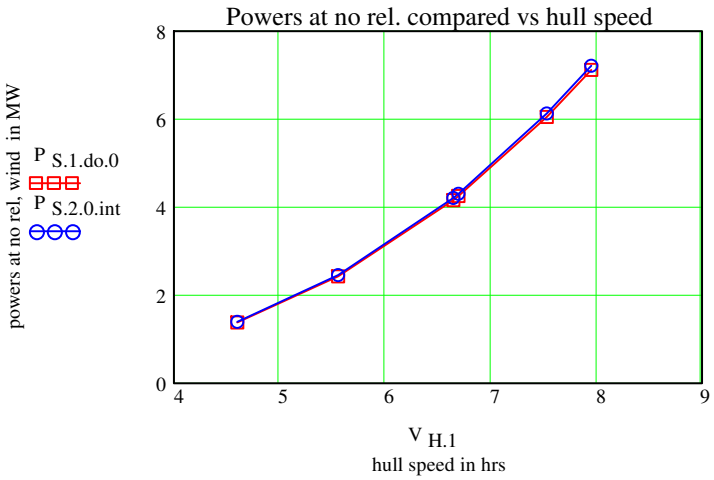
$C_{PV.1} := q_{1.do_0} + q_{1.do_1}$ $C_{PV.1} = 0.01419$

$P_{S.1.do.0_i} := C_{PV.1} \cdot \left(V_{H.1_i}\right)^3$

Power required, at the larger trim interpolated, at the nominal no wind and waves

$C_{PV.2} := q_{2_0} + q_{2_1}$ $C_{PV.2} = 0.01437$

$P_{S.2.0.int_i} := C_{PV.2} \cdot \left(V_{H.1_i}\right)^3$



$P_{S.1.do.0} = \begin{bmatrix} 1.378 \\ 2.424 \\ 4.156 \\ 4.248 \\ 6.048 \\ 7.119 \end{bmatrix}$

$P_{S.2.0.int} = \begin{bmatrix} 1.396 \\ 2.456 \\ 4.211 \\ 4.304 \\ 6.127 \\ 7.213 \end{bmatrix}$

Thus the power ratio at the two different trim settings

$\frac{C_{PV.2}}{C_{PV.1}} = 1.0131$

According to this analysis **the power required at the no-wind condition at the second, the larger trim is 1.3 % larger than at the first, the smaller trim** in the down-wind, the non-ventilated propeller condition, **'in accordance' with the crew's best trim practice, provided the propeller is not ventilating.**

In view of the average confidence radii of the mean values of the powers observed, roughly 0.02 MW, the small difference in the no wind conditions for both trials of about 0.06 MW is considered as negligible without further analysis of the progression of errors.

All results plotted

Trim 2: over-all

Power at no wind and waves faired

$$C_{PV.2.n} := \frac{C_{PV.2} \cdot 10^6}{\rho \cdot D^2}$$

Identify equilibrium

$J := 1 \qquad K := 1$

Given

$$K = p_{n.2_0} + p_{n.2_1} \cdot J$$

$$K = C_{PV.2.n} \cdot J^3$$

Solve

$$\begin{bmatrix} J_{H.equil.2} \\ K_{P.equil.2} \end{bmatrix} := \text{Find}(J, K)$$

$J_{H.equil.2} = 0.695$

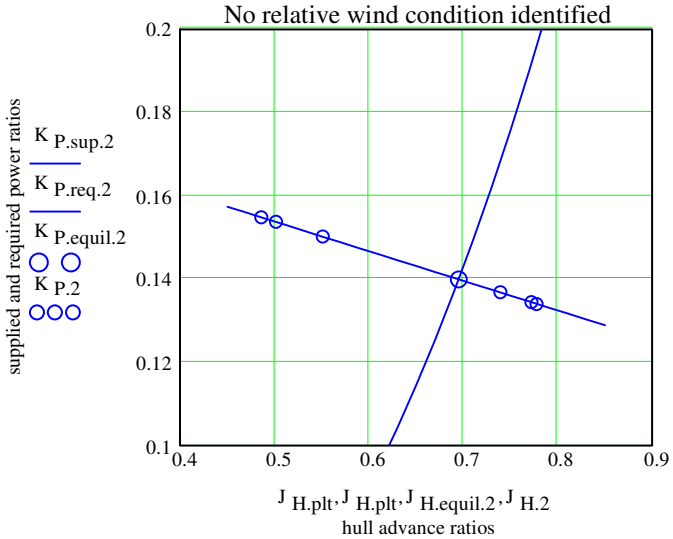
$K_{P.equil.2} = 0.140$

Results plotted

$k := 0 .. 20 \qquad J_{H.plt_k} := 0.45 + 0.02 \cdot k$

$$K_{P.sup.2_k} := p_{n.2_0} + p_{n.2_1} \cdot J_{H.plt_k}$$

$$K_{P.req.2_k} := C_{PV.2.n} \left(J_{H.plt_k}\right)^3$$



Trim 1: down-wind, non-ventilated

Power at no wind faired

$$C_{PV.1.n} := \frac{C_{PV.1} \cdot 10^6}{\rho \cdot D^2}$$

Identify equilibrium

$J := 1 \qquad K := 1$

Given

$$K = p_{n.do_0} + p_{n.do_1} \cdot J$$

$$K = C_{PV.1.n} \cdot J^3$$

Solve

$$\begin{bmatrix} J_{H.equil.do} \\ K_{P.equil.do} \end{bmatrix} := \text{Find}(J, K)$$

$J_{H.equil.do} = 0.698$

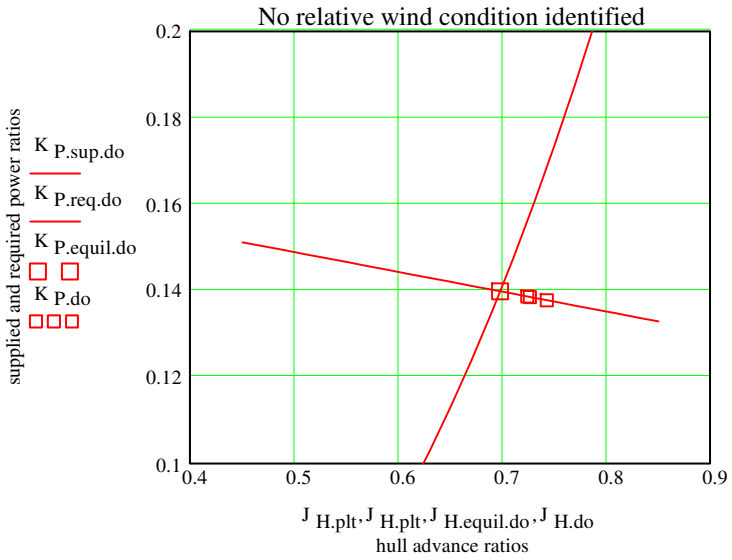
$K_{P.equil.do} = 0.140$

Results plotted

$k := 0 .. 20 \qquad J_{H.plt_k} := 0.45 + 0.02 \cdot k$

$K_{P.sup.do_k} := p_{n.do_0} + p_{n.do_1} \cdot J_{H.plt_k}$

$K_{P.req.do_k} := C_{PV.1.n} \cdot \left(J_{H.plt_k}\right)^3$



Trim 1: up-wind: propeller ventilated

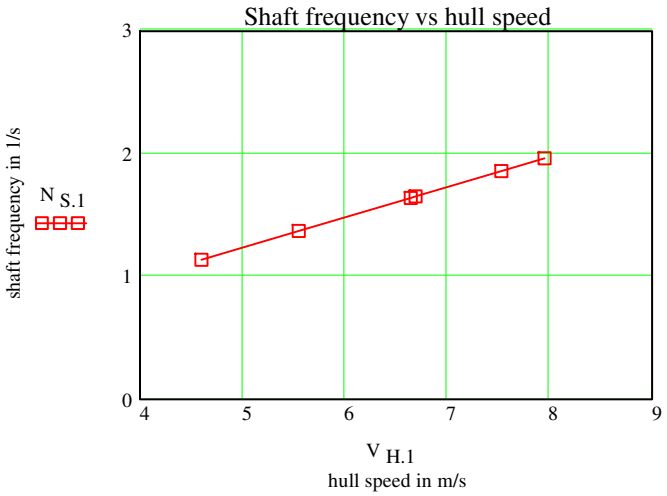
A separate no wind and waves equilibrium does not exist The propeller has only one characteristic, though with a discontinuity in slope.

Check consistency

Frequency of shaft rev's vs speed, propeller not ventilating, at the nominal no wind and waves condition

$N_{S.1_i} := 1$ initial values

$N_{S.1} := \text{Identify\_freq}(p_{do}, V_{H.1}, P_{S.1.do.0}, N_{S.1})$



$$N_{S.1} = \begin{bmatrix} 1.136 \\ 1.371 \\ 1.641 \\ 1.653 \\ 1.859 \\ 1.963 \end{bmatrix}$$

Linear approximation

$A_{N.1_{i,0}} := 1$ $A_{N.1_{i,1}} := V_{H.1_i}$ $X_{N.1} := \text{geninv}(A_{N.1}) \cdot N_{S.1}$ $X_{N.1} = \begin{bmatrix} -1.4166 \cdot 10^{-4} \\ 0.2471 \end{bmatrix}$

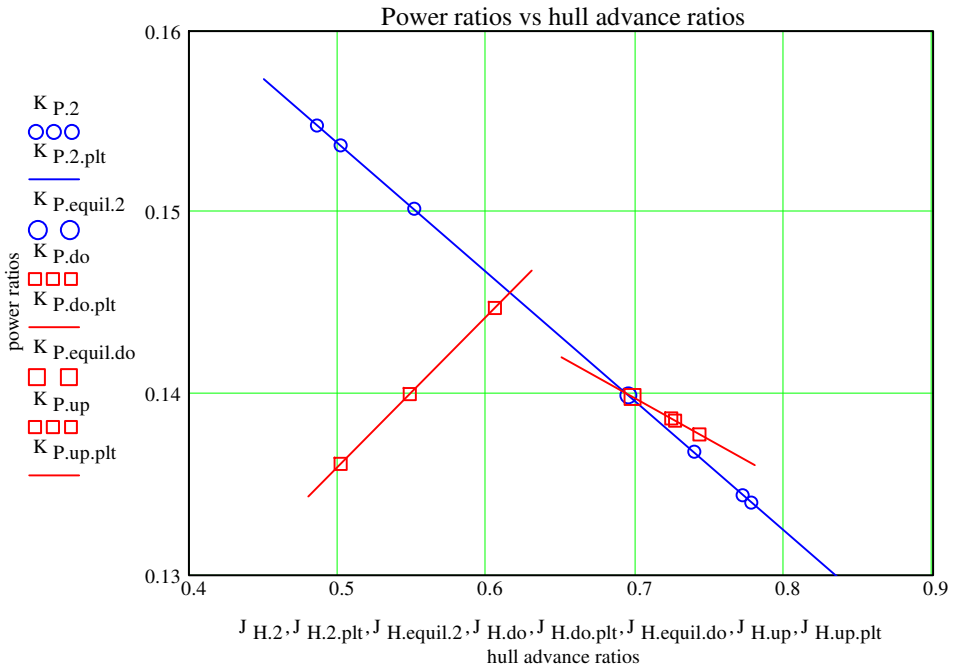
$N_{S.E.1} := N_{S.1} - A_{N.1} \cdot X_{N.1}$ $N_{S.E.1.Conf} := 2 \cdot \text{stdev}(N_{S.E.1})$ $N_{S.E.1.Conf} = 2.662 \cdot 10^{-5}$

Per definition this result is in accordance with the no wind and waves condition derived: the frequency of shaft rotation is directly proportional to the hull advance speed.

$C_{NV.1} := \dfrac{1}{D \cdot J_{H.equil.do}}$ $C_{NV.1} = 0.2471$ $N_{S.1} := C_{NV.1} \cdot V_{H.1}$ $N_{S.1} = \begin{bmatrix} 1.136 \\ 1.371 \\ 1.641 \\ 1.653 \\ 1.859 \\ 1.963 \end{bmatrix}$

The value of the constant is very nearly the same as that at the larger propeller submergence provided the propeller is not ventilating.

All normalised results



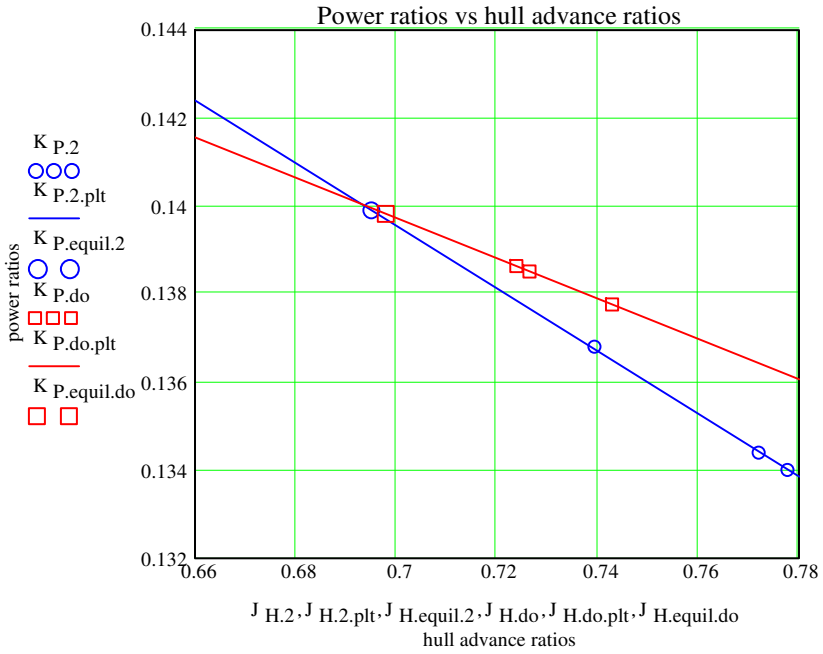
According to these results the nominal no wind and waves powering performance at the smaller trim differs from that at the larger trim even in the non-ventilating condition. One of the reasons may be the surface effect due the very small nominal submergence of the propeller.

Further it is noted that due to a considerable swell the ship has been pitching. This together with the very small nominal submergence of the propeller may have favoured intermittent ventilation at the up-wind condition.

Blow up around the no wind and waves conditions

$k := 0 .. 1 \qquad J_{H.2.plt_k} := 0.66 + 0.12 \cdot k$

$K_{P.2.plt_k} := p_{n.2_0} + p_{n.2_1} \cdot J_{H.2.plt_k}$



Note: The values of the power ratios at the down wind conditions for both trim settings are 'of course' the faired values, being based on the current velocity identified, as are the hull advance ratios!

Conclusions

Important observations

The most important lesson of this very elaborate exercise is that the results of trials, as any tests with any hydromechanical system, depend crucially on the precise determination of the current speed. If this is not possible any further evalution has to be terminated! Full stop!

'Accordingly' the final results of this final evaluation of the two trials at different trim settings differ from the results of earlier evaluations. **The changes are due to replacing the former much too crude current convention by a very robust, more reasonable and more acceptable convention permitting reliable extrapolation of the current identified from data observed at the larger trim to the trials at the smaller trim performed earlier at the same day.**

This extrapolation became necessary due to the propeller ventilation during the up-wind runs at the smaller trim, resulting in sets of data not permitting the evaluation successfully applied at the larger trim.

According to this analysis **the power required at the no wind and waves condition at the second, the larger trim is 1.5 % larger than at the first, the smaller trim** in the down-wind, the non-ventilated propeller condition, **'in accordance' with the crew's best trim practice provided the propeller is not ventilating. But even in view of the very small confidence level of the powers observed this small difference may be considered as negligible.**

In the absence of detailed observations of the sea state there is no possibility to identify the influence of the sea state on the required power. The procedure followed is the only reasonable and perfectly sufficient for the comparison of the no wind and waves performance at the two trim settings.

This result suggests that the reliable estimation of propulsive performance at the ballast condition depends crucially on the correct estimation of the propeller power characteristic and of the current at the conditions in question. **The problem is that for those conditions reliable data are not readily available, resulting in breakdown of all traditional codes including the ISO code and the more recent ITTC 2012 code.**

In the light of this very detailed analysis the evaluation according to ISO 15016: 2002-06 is considered as doubtful in many respects. The main reservation is that the standard, since its adoption known to be error prone even at fully loaded conditions, provides no adequate procedures at all, neither for ballast conditions nor for extremely small submergences of propellers in seaways. The same applies to evaluations according to the STA and ITTC procedures.

Further explanations

The rationale of the present exercise is explained in detail in a paper drafted for publication and presentation on occasion of the 25th anniversary of the METEOR tests in the Greenland Sea in November 1988.

The draft with hyperlinks, including hyperlinks to the present evaluations, is to be found under 'News on ship speed trials' on my website www.m-schmiechen.de and is open for discussion and contributions.

END
As next evaluated data at the first, at the smaller trim, i. e. at the smaller nominal propeller submergence

Prof. Dr.-Ing. M.Schmiechen

MS 0306011630
0310091100
1107121300

To whom it may concern

Powering performance
of a bulk carrier
during speed trials
in ballast condition
at two trim settings
reduced to the
no wind condition

1205041600
1207201300

1301051100
1305061500

Units, constants routines

Units

second	$s := sec$	
minute	$min := 60 \cdot s$	
hour	$hr := 3600 \cdot s$	
frequency	$Hz := \frac{1}{s}$	$Rpm := \frac{1}{min}$
distance	$nm := 1852 \cdot m$	
speed	$kn := \frac{nm}{hr}$	$kn = 0.514 \frac{m}{s}$
mass	kg	$t := 10000 \cdot kg$
force	$N := newton$	$kN := 10^3 \cdot N$
		$MN := 10^3 \cdot kN$
power	$W := watt$	$kW := 10^3 \cdot W$
		$MW := 10^3 \cdot kW$

General constants

field strength	$g := 9.81 \cdot \frac{m}{s^2}$	$g := 9.81$
density of seawater	$\rho := 1.025 \cdot 10^3 \cdot kg \cdot m^{-3}$	$\rho := \frac{\rho}{kg \cdot m^{-3}}$
tidal frequency	$\omega_T := \frac{2 \cdot \pi}{12.417 \cdot hr}$	$\omega_T := \omega_T \cdot hr$

Constants related to trials

identification

TID := "ANONYMA"

diameter of propeller

$D := 5.80 \cdot m$ $\quad D := \frac{D}{m}$

date

Date := "2012-02-05"

distance between trial 1 and 2, positive north

$\Delta s_{12} := 50 \cdot nm$ $\quad \Delta s_{12} := \frac{\Delta s_{12}}{nm}$

mean daytime of trial 1

$t_{1.m} := 4.474 \cdot hr$ $\quad t_{1.m} := \frac{t_{1.m}}{hr}$

mean daytime of trial 1

$t_{2.m} := 11.474 \cdot hr$ $\quad t_{2.m} := \frac{t_{2.m}}{hr}$

Courses

course down-wind, 'reference' course, towards south

$\psi_{H.do} := 220 \cdot \frac{deg}{rad}$ $\quad \psi_{H.do} = 3.840$

course up-wind

$\psi_{H.up} := 40 \cdot \frac{deg}{rad}$ $\quad \psi_{H.up} = 0.698$

number of runs up and down wind

$n := 6$

courses at trials

$$\psi_H := \begin{bmatrix} 3.840 \\ 0.698 \\ 0.698 \\ 0.698 \\ 3.840 \\ 3.840 \end{bmatrix}$$

Tide

rotating tide speed towards north at the location, estimated

$c_T := 400 \cdot kn$ $\quad c_T := \frac{c_T}{kn}$

day time of high tide

$t_T := 12.667 \cdot hr$ $\quad t_T := \frac{t_T}{hr}$

Sea state

significant wave direction

$\psi_S := \psi_H$

significant wave height

$H_S := 3 \cdot m$ $\quad H_S := \frac{H_S}{m}$

$i := 0 .. n - 1$ $\quad H_{S_i} := H_S$

$$H_S = \begin{bmatrix} 3.000 \\ 3.000 \\ 3.000 \\ 3.000 \\ 3.000 \\ 3.000 \end{bmatrix}$$

Sea state: additionally assumed (!) for various studies

significant wave period $T_S := 7.3 \cdot sec$ $T_S := \frac{T_S}{sec}$

significant wave speed $V_S := \frac{g \cdot T_S}{2 \cdot \pi}$ $V_S = 11.398$

Check disributions

Values of random variables need to be tested for normal distribution before using mean values and and standard deviations

$$\begin{aligned}
&\text{norm_distr}(sampl) := \\
&\quad r \leftarrow \text{rows}(sampl) \\
&\quad c \leftarrow \text{cols}(sampl) \\
&\quad \text{for } i \in 0..r-1 \\
&\quad\quad fract \leftarrow \frac{2 \cdot (i+1)}{r+1} - 1 \\
&\quad\quad dst \leftarrow fract \\
&\quad\quad distr_i \leftarrow \sqrt{2} \cdot \text{root}(\text{erf}(dst) - fract, dst) \\
&\quad\quad \text{for } j \in 0..1 \\
&\quad\quad\quad A_{i,j} \leftarrow (distr_i)^j \\
&\quad \text{for } j \in 0..c-1 \\
&\quad\quad sampl_{sort}^{\langle j \rangle} \leftarrow \text{sort}(sampl^{\langle j \rangle}) \\
&\quad par \leftarrow \text{geninv}(A) \cdot sampl_{sort} \\
&\quad sampl_{sort.fit} \leftarrow A \cdot par \\
&\quad \text{for } j \in 0..c-1 \\
&\quad\quad par_{2,j} \leftarrow \frac{par_{1,j}}{\sqrt{r}} \\
&\quad \begin{bmatrix} distr \\ sampl_{sort} \\ sampl_{sort.fit} \\ par \end{bmatrix}
\end{aligned}$$

Normalise data

$$J(D,V,N) := \frac{V}{D\cdot N} \qquad KP(\rho,D,P,N) := \frac{P\cdot\frac{MW}{W}}{\rho\cdot D^5\cdot N^3}$$

$$Fn(V) := \frac{V}{\sqrt{g\cdot L}} \qquad CP(\rho,D,P,V) := \frac{P\cdot\frac{MW}{W}}{\rho\cdot D^2\cdot V^3}$$

Sort runs

For srutiny runs have to be sorted into down-wind and up-wind runs in that order..
The criterion adopted suits the data at hand.

$Sort\_runs(J_H, K_P, \psi_H) :=$

- $j_0 \leftarrow 0$
- $j_1 \leftarrow 0$
- for $i \in 0..last(J_H)$
  - if $\psi_{H_i} > \frac{\pi}{2}$
    - $S_{j_0,0} \leftarrow J_{H_i}$
    - $S_{j_0,1} \leftarrow K_{P_i}$
    - $j_0 \leftarrow j_0 + 1$
  - otherwise
    - $S_{j_1,2} \leftarrow J_{H_i}$
    - $S_{j_1,3} \leftarrow K_{P_i}$
    - $j_1 \leftarrow j_1 + 1$
- S

Supplied shaft power function

$$PS_{sup}(p,N,V) := p_0\cdot N^3 + p_1\cdot N^2\cdot V$$

Current velocity function

$$VC(v,t,\omega_T,t_T) := v_0 + v_1\cdot\sin\left[\omega_T\cdot(t - t_T)\right]$$

Required shaft power function

$$PS_{req}(q,V_H,V_{W.rel}) := q_0\cdot V_H^3 + q_1\cdot V_H\cdot V_{W.rel}\cdot|V_{W.rel}|$$

Directions of runs

$$dir(\psi_H) := if\left(\psi_H > \frac{\pi}{2}, 1, -1\right)$$

Analyse power supplied

including identification of polynomial current

$$\text{Polyn\_current}\left(o, \rho, D, t, \psi_H, V_G, N_S, P_S\right) :=$$

for $i \in 0..\text{last}(t)$

$A\,sup_{i,0} \leftarrow \left(N_{S_i}\right)^3$

$A\,sup_{i,1} \leftarrow \left(N_{S_i}\right)^2 \cdot V_{G_i}$

$A\,sup_{i,2} \leftarrow -\left(N_{S_i}\right)^2 \cdot \text{dir}\left(\psi_{H_i}\right)$

continue if $o < 1$

for $j \in 1..o$

$A\,sup_{i,2+j} \leftarrow A\,sup_{i,1+j} \cdot t_i$

$X_{sup} \leftarrow \text{geninv}\left(A_{sup}\right) \cdot P_S$

$E_{sup} \leftarrow P_S - A_{sup} \cdot X_{sup}$

for $k \in 0..1$

$p_k \leftarrow X\,sup_k$

$p_{n_k} \leftarrow \dfrac{p_k}{\rho \cdot D^{5-k}} \cdot \dfrac{MW}{W}$

for $j \in 0..o$

$v_j \leftarrow \dfrac{X\,sup_{2+j}}{X\,sup_1}$

for $i \in 0..\text{last}(t)$

$V_{C_i} \leftarrow \sum_{j=0}^{o} v_j \cdot \left(t_i\right)^j$

$V_{H_i} \leftarrow V_{G_i} - V_{C_i} \cdot \text{dir}\left(\psi_{H_i}\right)$

$P_{S_i} \leftarrow PS\,sup\left(p, N_{S_i}, V_{H_i}\right)$

$J_{H_i} \leftarrow J\left(D, V_{H_i}, N_{S_i}\right)$

$K_{P_i} \leftarrow KP\left(\rho, D, P_{S_i}, N_{S_i}\right)$

$\left[E_{sup}\ v\ V_C\ p\ V_H\ P_S\ p_n\ J_H\ K_P\right]$

Analyse power supplied

including identification of tidal current

$\text{Tidal\_current}\left(\omega_T, t_T, \rho, D, t, \psi_H, V_G, N_S, P_S\right) :=$

for $i \in 0..\text{last}(t)$

$A_{sup_{i,0}} \leftarrow \left(N_{S_i}\right)^3$

$A_{sup_{i,1}} \leftarrow \left(N_{S_i}\right)^2 \cdot V_{G_i}$

$A_{sup_{i,2}} \leftarrow -\left(N_{S_i}\right)^2 \cdot \text{dir}\left(\psi_{H_i}\right)$

$A_{sup_{i,3}} \leftarrow A_{sup_{i,2}} \cdot \sin\left[\omega_T \cdot \left(t_i - t_T\right)\right]$

$X_{sup} \leftarrow \text{geninv}\left(A_{sup}\right) \cdot P_S$

$P_{S.E.sup} \leftarrow P_S - A_{sup} \cdot X_{sup}$

for $k \in 0..1$

$v_k \leftarrow \dfrac{X_{sup_{2+k}}}{X_{sup_1}}$

$p_k \leftarrow X_{sup_k}$

$p_{n_k} \leftarrow \dfrac{p_k}{\rho \cdot D^{5-k}} \cdot \dfrac{MW}{W}$

for $i \in 0..\text{last}(t)$

$V_{C_i} \leftarrow VC\left(v, t_i, \omega_T, t_T\right)$

$V_{H_i} \leftarrow V_{G_i} - V_{C_i} \cdot \text{dir}\left(\psi_{H_i}\right)$

$P_{S_i} \leftarrow PS_{sup}\left(p, N_{S_i}, V_{H_i}\right)$

$J_{H_i} \leftarrow J\left(D, V_{H_i}, N_{S_i}\right)$

$K_{P_i} \leftarrow KP\left(\rho, D, P_{S_i}, N_{S_i}\right)$

$\left[P_{S.E.sup} \;\; v \;\; V_C \;\; p \;\; V_H \;\; P_S \;\; p_n \;\; J_H \;\; K_P\right]$

Analyse power supplied

excluding identification of current

$$\text{No_current}(\rho, D, V_H, N_S, P_S) := \left|\begin{array}{l}
\text{for } i \in 0..\text{last}(N_S) \\
\quad \left|\begin{array}{l} A_{\text{sup}_{i,0}} \leftarrow (N_{S_i})^3 \\ A_{\text{sup}_{i,1}} \leftarrow (N_{S_i})^2 \cdot V_{H_i} \end{array}\right. \\
X_{\text{sup}} \leftarrow \text{geninv}(A_{\text{sup}}) \cdot P_S \\
P_{S.E.sup} \leftarrow P_S - A_{\text{sup}} \cdot X_{\text{sup}} \\
\text{for } k \in 0..1 \\
\quad \left|\begin{array}{l} p_k \leftarrow X_{\text{sup}_k} \\ p_{n_k} \leftarrow \dfrac{p_k}{\rho \cdot D^{5-k}} \cdot \dfrac{MW}{W} \end{array}\right. \\
\text{for } i \in 0..\text{last}(V_H) \\
\quad \left|\begin{array}{l} P_{S.sup_i} \leftarrow PS_{\text{sup}}(p, N_{S_i}, V_{H_i}) \\ J_{H_i} \leftarrow J(D, V_{H_i}, N_{S_i}) \\ K_{P_i} \leftarrow KP(\rho, D, P_{S.sup_i}, N_{S_i}) \end{array}\right. \\
\left[P_{S.E.sup} \;\; p \;\; P_{S.sup} \;\; p_n \;\; J_H \;\; K_P\right]
\end{array}\right.$$

Analyse power required
no wave data available

$$\text{Required}\left(V_H, \psi_H, N_S, P_S, V_W, \psi_W\right) := \left|\begin{array}{l} \text{for } i \in 0..\text{last}\left(V_H\right) \\ \quad \left|\begin{array}{l} A_{req_{i,0}} \leftarrow \left(V_{H_i}\right)^3 \\ V_{W.x_i} \leftarrow -V_{W_i} \cdot \cos\left(\psi_{W_i} - \psi_{H_i}\right) \cdot \text{dir}\left(\psi_{H_i}\right) \\ A_{req_{i,1}} \leftarrow V_{W.x_i} \cdot \left| V_{W.x_i} \right| \cdot V_{H_i} \end{array}\right. \\ X_{req} \leftarrow \text{geninv}\left(A_{req}\right) \cdot P_S \\ P_{S.req} \leftarrow A_{req}\ X_{req} \\ P_{S.E.req} \leftarrow P_S - P_{S.req} \\ \text{for } i \in 0..\text{last}\left(V_H\right) \\ \quad \left|\begin{array}{l} P_{S.req.0_i} \leftarrow A_{req_{i,0}} \cdot X_{req_0} \\ P_{S.req.1_i} \leftarrow A_{req_{i,1}} \cdot X_{req_1} \end{array}\right. \\ q \leftarrow X_{req} \\ \left[P_{S.E.req}\ \ q\ \ P_{S.req}\ \ P_{S.req.0}\ \ P_{S.req.1}\right] \end{array}\right.$$

Frequency of revolutions

$$\text{Identify\_freq}(p, V, P, N) := \left|\begin{array}{l} m_i \leftarrow \text{last}(V) \\ \text{for } i \in 0..m_i \\ \quad \left|\begin{array}{l} a \leftarrow P_i \\ b \leftarrow V_i \\ c \leftarrow N_i \\ N_i \leftarrow \text{root}\left(a - p_0 \cdot c^3 - p_1 \cdot c^2 \cdot b, c\right) \end{array}\right. \\ N \end{array}\right.$$

END
Units, constants. routines

On Trials and Monitoring

Abstract of a paper proposed for presentation
at the STG Annual Meeting, Berlin 2013

NOTE

This paper, including hyper-links to all the material referred to, is to be found on my website www.m-schmiechen.de at the beginning of the subsection News on ship speed trials. Further links are to be found in the annotated documentation of all my papers and related written discussions on Propulsion in general and on Ship speed trials in particular.

ABSTRACT

Naval architects are successfully predicting the powering performance of ships, traditionally based on results of model tests and/or, more recently, on results of numerical calculations. But using traditional trials codes, as standardised, *e. g.*, in ISO 15016: 2002-06, they cannot prove that their predictions are correct within the narrow confidence limits required for many purposes today, *e. g.*, trustworthy demonstrating the improvements they 'promise'.

The reason for this state of affairs is that naval architects have been and still are so fascinated and absorbed by the possibilities provided by Coloured Fluid Dynamics, that they missed to take appropriate notice the threatening problems ahead of them. They are mistaking CFD for ship theory, not realising that it is only one way to determine values of the concepts they are using, without wondering where the latter came from.

'Consequently' they missed to develop an adequate theory of ship propulsion to overcome the 'dreadful' problems and improve the efficiency of research, teaching and testing. Their concepts did not fall from heaven, but have been inherited from their grand-grand-fathers. Thus, *e. g.*, all traditional trial codes are still based on the naïve Newtonian model of hull-propeller interaction still inconsistently interpreted by Froude's conventions as far as possible and/or relying on parameters to be sucked from 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, has been demonstrated in the METEOR project, the tests in the Norwegian Sea performed now twenty five years ago.

Following the principles stated in 1980 the search for simple, acceptable conventions replacing Froude's conventions, hull towing and propeller open water tests, in case of monitoring the powering performance on full and model scale has of course reached its final goal only later, based on the ex-

perience gained using preliminary versions evaluating a 'model' test performed to simulate the METEOR tests.

Ten years later, in 1998, a solution of the much simpler problem, the evaluation of traditional speed trials, has been proposed and shown to be not only feasible, but to permit the reliable evaluation of trials, even if all traditional methods are doomed to fail. This has again been shown in the recent evaluation of trials with a bulk carrier in ballast at two different trim settings, results and insights extended to be discussed explicitly.

The approach promoted avoids the unacceptable deficiencies of the traditional trials codes by adopting a Lagrangean procedure, phrased 'only' in terms of shaft powers supplied and required. It is accounting for the fact that usually only power measurements are 'available' or even meaningful. Thus the concept of thrust, including energetically irrelevant components, does not 'occur' at all, it is not even mentioned, as in case of the design of a energy wake adapted ducted propulsor.

And most important, contrary to all traditional codes, no 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 simplicity of the conventions is not a purpose in itself and is not a matter of elegance. Following Ludwig Boltzmann Albert Einstein noted, that 'elegance should be left to the dress makers and shoe makers'. The naked pragmatism followed and the simplicity and reached here serves the dual purpose to permit the stable, 'objective' identification of the parameters introduced and to be as 'self-evident' as possible and thus acceptable not only for naval architects, but for ship builders and owners as well.

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 approach. The exposition will refer 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 model and full scale will be drastically reduced, and the reliability of the results increased at the same time, that these considerable returns are to be obtained for only little effort using common sense, and that the 'disruptive innovations' outlined are definitely in the interest of the industry we serve.

MS 20.08.2014 09:59 h

On the ANONYMA trials

Paper presented with
related discussions
and analyses

Abstract on ANONYMA trials

proposed for presentation at the
Meeting of the Hydrodynamics Committee
of the Schiffbautechnische Gesellschaft
at Eckernförde on September 19, 2013

Analyses of traditional powering trials with the bulk carrier ANONYMA in ballast at two trim settings

by Michael Schmiechen

Berlin, 01.08.2013

The incentive of my recent, varied work on the analysis of ship powering trials has been the request to analyse the trials with a bulk carrier in ballast at two different trim settings using my rational method. As in earlier projects the purpose of the exercise has been, trustworthy to establish full scale differences predicted, in this case numerically.

The analyses turned out to be particularly delicate, thus forcing me, thoroughly to re-think my rational conventions and throw further 'ballast', remaining professional superstition, over board. The insights gained have been continuously discussed with Dr. Klaus Wagner and, following his encouragement have in detail been described in a paper. My thanks are also due to Dr. Karsten Hochkirch of FutureShip, Germanischer Lloyd Group, for critical impulses and especially for granting the permit to publish the details of the analyses.

In view of the many other recent incentives my paper, in the style of a rather formal 'letter' to my colleagues and students, became longer than originally intended. The purpose was to provide a reminder of well known deficiencies of the traditional methods and to explain, how these can be avoided on principle. The letter is also addressed to all those, who should be interested in the results of my work, ship builders and ship owners, members of towing tanks as well as members of the STA-Group and of the governing bodies of ITTC, ISO and IMO.

At the recent situation many colleagues note at the latest, that many methods for the powering prediction have been developed, erroneously mistaken for ship-theory, but except for mine none for the proof of the pudding, the trustworthy full scale evidence of the results, meeting today's, *i. e.* their own requirements. 'Theoreticians' have left the very difficult trials problem 'simply' to the practicians at ship yards and model basins. And ship owners still accept that the same people providing the predictions are performing

and evaluating the trials 'as well'.

The 'letter' mentioned has the same structure as the theory with all its branches and their developments and successful applications over the past 25 years, the analyses of the ANONYMA trials marking the (current) end. But many of my expositions purposely start with the theory of trials, clearly to demonstrate and beyond doubt, that the evaluation of powering trials does not require any theory of propulsion, but only some elementary mechanics, some common sense and, last but not least, the often missing extreme care in analysing the trials data obtained at great expense.

This short talk has to be restricted to the theory and the examples stated in the title of traditional trials, having been performed as usual, *i. e.* without measurements of the propeller thrust, of the ship speed through the water and of the sea state. Following the short, necessary explanation of the rational conventional method the conventions for the power delivered, current velocity and power required are explicitly stated and the results for both trials are discussed.

The three conventions or 'laws' adopted have only two parameters each, the values of which usually can be identified *solely* from the data at hand, as it must be for the objective, observer independent evaluation, not only in case of trials in ballast. Due to the propeller ventilation with the smaller trim at runs up wind only few additional 'assumptions', *i. e.* acceptable conventions, became necessary.

The complete analyses, the 'letter' mentioned and all related discussions etc are to be found under 'News on ship powering trials' on my website www.m-schmiechen.de. At the same place the complete draft (presently available in German only) of the talk proposed has already been published with the invitation to contribute to the discussions.

MS 20.08.2014 09:59 h

On the ANONYMA trials

The lecture with notes and handouts

From METEOR 1988 to ANONYMA 2013

Analyses of traditional powering trials
with the bulk carrier ANONYMA
in ballast at two trim settings

Michael Schmiechen, Berlin

apl. Prof. for hydro-mechanical Systems at ISM/TUB,
retired Deputy Direktor and Head of RaD at VWS

The complete analyses, the 'letter' and all related discussions etc are to be found under 'News on ship powering trials' on the website www.m-schmiechen.de

Status: 2013-08-06/10-06

Schmiechen **Future trials and monitoring now / 1**

The primary reason for my recent, intense activities related to the analysis of ship powering trials has been the request to re-analyse the data of trials with a bulk carrier in ballast at two different trim settings using my rational methods.

As in former projects the purpose of the exercise was trustworthy to confirm numerically predicted differences full scale. I have neither been involved in the predictions, nor in the trials, nor in the assessment of my results.

The analyses turned out to be extremely delicate, forcing me thoroughly to re-think my rational conventions and throw further ballast, *i. e.* professional superstition over board.

The insights gained during that work have been discussed continuously with Dr. Klaus Wagner and following his suggestion have been described in a paper. My thanks are also due to Dr. Karsten Hochkirch of FutureShip, Germanischer Lloyd Group, for critical impulses, particularly for granting the permit to publish all details of the analyses.

From METEOR 1988 to ANONYMA 2013

Further recent motivations

The 25th anniversary of my propulsion tests with METEOR in the Greenland Sea, November 1988,

the 15th anniversary of a proposed rational standard for the assessment of trials, April 1998,

the overdue revision ISO 15016: 2002-06,

the 'incredible' STA-method promoted by MARIN,

its premature integration into the 'ITTC 2012 Guidelines',

their *approval contra legem* by the Executive Committee,

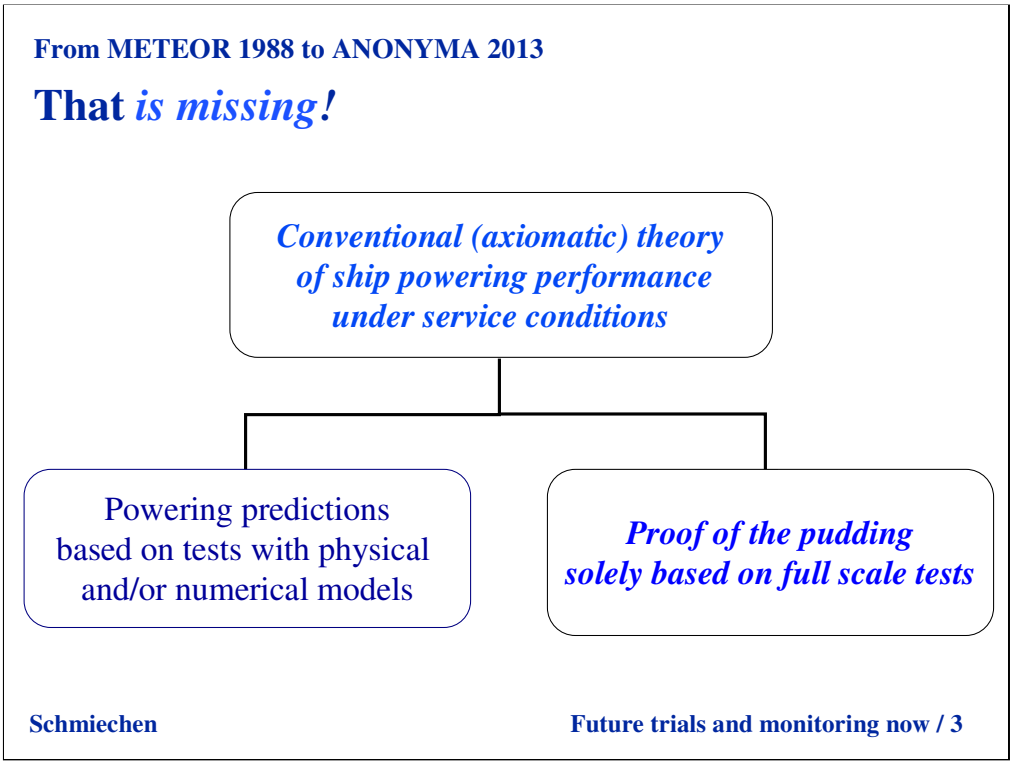
and their submission to MEPC of IMO.

Schmiechen **Future trials and monitoring now / 2**

In view of the many other recent grounds my paper, in the style of a formal 'letter' to my colleagues and students, has become much more elaborate than expected. The intention was to recall the well known deficiencies of the traditional methods and explain, how they can be and have been overcome based on few fundamental results of the theory of knowledge..

The letter is also addressed to all, who 'should' be interested in my work, ship-builders and ship-owners, staffs of model basins, and members of the STA-Group and of the governing bodies of ITTC, ISO and IMO.

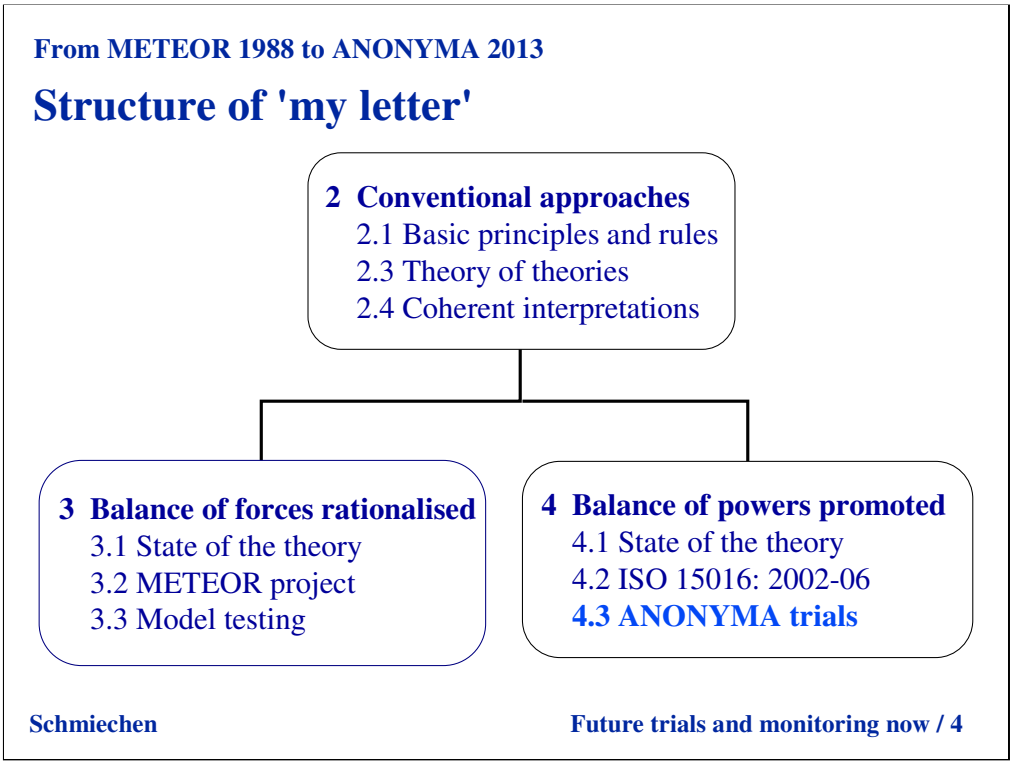
It is not my fault, that the many developments of the rational methods have been consistently ignored for decades at universities, model basins and the ITTC for the sole reason, that they cannot be phrased in the jargon of our grand-grandfathers.



At the present situation many colleagues notice at the latest, that very many methods have been developed to predict the powering performance of ships, erroneously mistaken for propulsion theory, but that except for mine no methods have been developed for the convincing, trustworthy proof of the results full scale, meeting today's, *i. e.* their own requirements.

Theoreticians have 'simply' left the very difficult problems of trials and monitoring the powering performance to practicians at ship yards and model basins. And ship-owners still accept, that the same 'people' providing the predictions are carrying out and analysing the trials 'as well'.

I just mention by the way, that the rational theory is a powerful tool not only for the development of theories of trials and monitoring the powering performance, but also for the computer aided design of ships and their propulsors, a potential not yet exploited.



The 'letter' mentioned has the same structure as the theory with all its branches and their development and successful tests over the past 25 years. The analyses of the trails with ANONYMA marks the end of that development so far.

But many of my expositions start with the theory of traditional trials, so in my opus magnum, clearly and unmistakably demonstrating that the evaluation of trials does not require any theory of propulsion, but only some elementary mechanics, some common sense and, last but not least, extreme care, often to be missed, in evaluating the valuable data acquired at considerable costs.

This short lecture has to be limited to two examples of traditional trials as usually performed, i. e. without the measurement of thrust, without the measurement of hull speed through the water and without the measurement of sea states. A more extended presentation is to be found on my website.

From METEOR 1988 to ANONYMA 2013

2.1 Rational conventions

Conventions are agreements, are languages and their implications (to be) agreed upon.

- **Traditional conventions** are usually not explicit, often incoherent languages.
- **Rational conventions** are formal languages constructed *ad hoc* for the purposes at hand. In terms of logic they are axiomatic systems, a frightening name for most useful tools.

Schmiechen **Future trials and monitoring now / 5**

But for understanding the following some meta-theory is necessary here as well. The misconception, that one can get along without such theory, without 'philosophy' is entertained only by colleagues, who most urgently need these theories to solve their own very difficult problems professionally.

Whatever we as humans undertake jointly, *e. g.* the theory of classical mechanics in general or the theory of ship propulsion in particular, is based on conventions. This fact and its consequences are hardly known to physicists and engineers, although only that knowledge permits efficiently to solve problems, *i. e.* free of traditional ballast.

The grammar and the usage of formal languages are usually known only rudimentary. As a consequence much research is quite inefficient, if not irresponsible waste of intellectual and financial resources.

From METEOR 1988 to ANONYMA 2013

Lessons (to be) learned

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***.

The interpretation of the concepts and parameters introduced to be completely separated from the construction of the axiomatic models, of the formal languages proper.

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

Schmiechen **Future trials and monitoring now / 6**

Earlier, giving talks at the Institut für Schiffbau in Hamburg, whenever I introduced a concept, I have been interrupted instantly by the question: 'and how are you measuring it?' That this conception is hopelessly naive and antiquated, is hard for naval architects to understand.

The concepts and their interpretation, inherited from our grand-fathers and still in use, did not fall from heaven and happen to be not applicable under service conditions. Their meaning and values are obtained only in the context of conventions, *i. e.* 'reference systems'.

'Independent' interpretations require additional, totally unnecessary conventions 'without end', resulting in an infinite regress. Example are meters of any type that cannot be calibrated.

And conventions are appropriately designed for the purposes at hand, so that the values of the concepts introduced can be identified under any condition. An example is my thrust deduction convention permitting to identify the resistance of ships not only on model scale in a towing tank, but full scale under service conditions as well.

From METEOR 1988 to ANONYMA 2013

Delivered power 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

$$P_S = p_0 N_S^3 + p_1 N_S^2 V_H$$

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

Schmiechen Future trials and monitoring now / 7

In the light of this short, but necessary introduction the details of the analyses of the trials with ANONYMA are as follows.

By their nature propulsors are pumps. And thus to treat them accordingly offers dramatic advantages, not only in evaluating trials. I only mention the design of hull integrated propulsors, *e. g.* ducted propellers. In that case all (!) the interactions are treated implicitly, no prior values have to be sucked from thumbs.

If as usual only power measurements can be performed, then only the power ratio as function of the hull advance ratio can be identified. But if reliable thrust measurements are possible, as in the cases of models and of the METEOR, all interactions between hull and propeller may be identified. The corresponding ideas and suggestions by Fritz Horn and the related model tests at various basins have already been discussed at the 4th ITTC 1937 in Berlin.

From METEOR 1988 to ANONYMA 2013

Speed through the water

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

$$V_H = V_G - V_C \cdot$$

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

Schmiechen **Future trials and monitoring now / 8**

If responsible hydrodynamicists cannot reliably identify the flow velocity, then they instantly and unconditionally disrupt any further evaluation.

This has not been done by naval architects fifteen years ago. Although in 1998 I had demonstrated serious deficiencies concerning this fundamental aspect in the draft of ISO 15016, the latter has been accepted as standard in 2002 by all national groups informed.

And in the STA method of MARIN the current is still identified in that inadequate way.

From METEOR 1988 to ANONYMA 2013

Current convention

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

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

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.

Schmiechen Future trials and monitoring now / 9

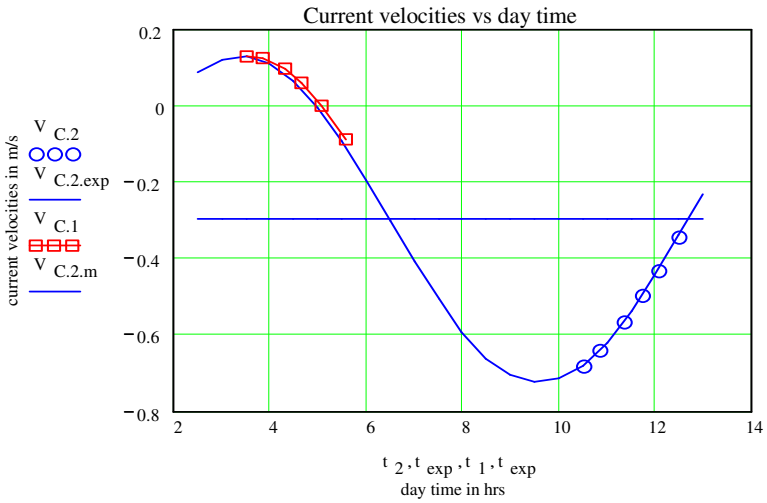
The propeller and the current conventions have two parameters each. Due to the linearity of the propeller convention, adopted in view of the limited range of hull advance ratios, these four parameters can be jointly identified as solutions of one set of linear equations.

Prerequisite is are adequate routines based on singular value decomposition. Do-it-yourself routines are not sufficient in case of nearly singular problems.

The example of ANONYMA demonstrates, that any trial is a special case, not adequately to be treated according to some recipe. Thus some conventions have to be agreed upon *ad hoc*. If *e. g.* the assumption of a tidal current is not appropriate, an adequate convention has 'simply' to be adopted.

From METEOR 1988 to ANONYMA 2013

Current identified, extrapolated



Schmiechen **Future trials and monitoring now / 10**

My propeller and current conventions have often proved to be extremely sensitive probes. Whenever the application produced unrealistic results, these could be traced to some problems in the input data. In case of the ISO example I have thus detected a misprint in the data. In case of the ANONYMA the situation was more intricate.

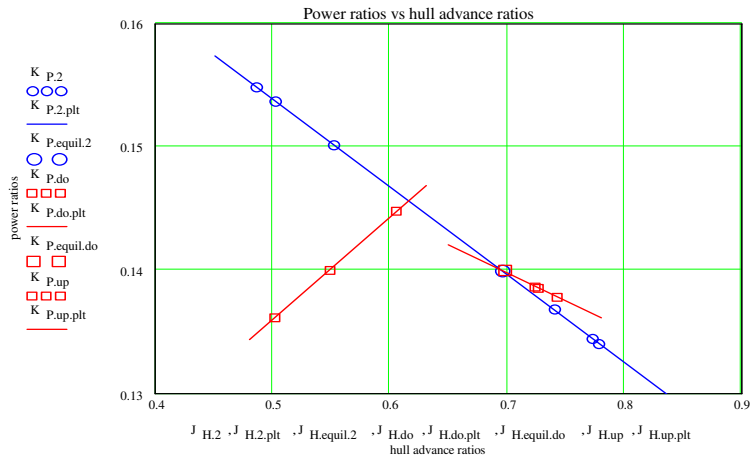
The evaluation of the first trial, that with the smaller trim and thus smaller nominal propeller submergence, 'did not work'. But the reason for some unlikely data remained of course obscure.

The evaluation of the second trial, that with the larger trim posed no problems at all. Subsequently the 'only' problem was reliably to extrapolate the current for the location and the time of the first trial. This problem could be solved as described referring to the tables of tides.

According to a crude estimate the current was 'just' negligibly small. Evidently this is true only in the average, while during the trial the current changed by more than half a knot!

From METEOR 1988 to ANONYMA 2013

Powers identified, normalised



Schmiechen **Future trials and monitoring now / 11**

The result of both trials clearly show the reason for the failure of my simple, over-all analysis of the first trial with the smaller nominal propeller submergence. The propeller ventilated during the runs up-wind! And as a consequence the extrapolation of the current became necessary.

Results of 'standardised' evaluations, *e. g.* according to ISO 15016 or the STA procedure of MARIN, *contra legem* integrated into the 'ITTC 2012 Guidelines', are of course completely non-sensical.

In principle all references to the performance of deeply submerged model propellers, as in most traditional methods, or to the propulsive efficiency observed in model tests, as in the STA procedure, are unacceptable, as they require any number of additional conventions and parameters, which the observer has to or may suck from his thumb 'as required' for his (!) purposes.

The way the STA procedure is sold as 'industry standard' is for my taste a particularly drastic example of Andersen's archetypal tale of 'the emperor's new clothes'.

From METEOR 1988 to ANONYMA 2013

Required power convention

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

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

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 unambiguously, 'objectively' identified have nothing, to stress: definitely nothing whatsoever, to do with the 'resistance coefficients' traditionally considered in this context.

Schmiechen **Future trials and monitoring now / 12**

That environmental influences can be identified only after the reliable identification of the speed through the water is self-evident practice for all experts. Only in the procedure marketed by MARIN the opposite is advocated, maybe due to the fact that the current cannot be identified trustworthy.

Using my simple convention it is sufficient to solve another system of linear equations. In view of the few data available down wind, the environmental parameters for the first trial, that with the smaller trim, could not be identified reliably. Thus the values identified for at the second trial have been used as well.

Addition 21.09.2013

Dott. Gennaro as well as Dr. Wagner have already pointed out, that the convention used is not generally acceptable. I shall try any other proposal, provided the data available are sufficient for that purpose!

From METEOR 1988 to ANONYMA 2013

Nominal no wind and waves condition

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

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

.

Schmiechen **Future trials and monitoring now / 13**

For the whole day of the trials only the constant wave height of 3 m has been 'observed'. Thus the comparison of the powers may be acceptable.

If more detailed observations of the sea state have been available I have always accounted for them as far as possible.

Addition 21.09.2013

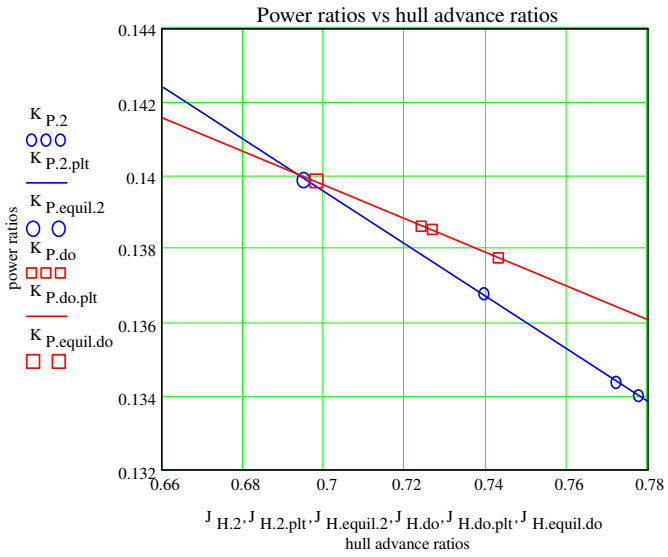
The correct title should of course have been 'nominal no wind condition', as all measurements at both took place at the wave height reported.

Addition 06.10.2013

Decisions for one of 'equivalent' conventions, all resulting in residua within the confidence interval of the data, are possible only by additional conventions, as has been shown in detail in the evaluation of the trials at the larger nominal propeller submergence.

From METEOR 1988 to ANONYMA 2013

Nominal states compared



Schmiechen **Future trials and monitoring now / 14**

Though the difference of the powers at both trim settings at the nominal states is significant, it is very small compared to the confidence intervals, that it can safely be considered as negligible.

But as the plot shows the influence of the nominal submergence identified is considerable.

From METEOR 1988 to ANONYMA 2013

Warning!

Reading of my papers endangers Your principles!

"You cannot have a theory without principles.
'Principles' is another name for 'prejudices'."
Mark Twain: 'The Disappearance of Literature'
Speech, 20 November 1900.

Schmiechen **Future trials and monitoring now / 15**

And here at the end I am back at the start!

The present situation concerning the methods of powering assessment is *e. g.* comparable to the recent situation in some Arab states. If majorities, hopefully not only illiterates forced to the urns, vote for the traditional 'prejudices', conventions inherited and accepted so far, then rational conventions, more adequate for today's purposes, will be accepted only by the next generations.

Of course many people are not interested to have their intact worlds and their profitable businesses disturbed. But if the STA method, meeting none of the requirements stated, will be adopted by the 27th ITTC 2014, it will not only impede or even prevent progress for the next decades, but seriously damage the reputation of the ITTC..

From METEOR 1988 to ANONYMA 2013

Justice for Hedgehogs

In his fundamental book 'Justice for Hedgehogs' Ronald Dworkin has tried to outline how the conflicts mentioned may be solved rationally, if including scientific conflicts I just try to find out.

He refers to the 'insight' of Archilochos (680 – 654 BC):

"The fox knows many things,

but the hedgehog knows one big thing!"

The hedgehog knows, ***that*** **all 'things' and** ***how and why*** **they are related to and depend on each other, mutually supporting each other.**

Schmiechen **Future trials and monitoring now / 16**

From a poem published in DIE ZEIT (68 (2013) 38, 52) I quote the following lines, although the last line is definitely not correct:

"We are responsible for	"Wir sind für die Zustände
the states of the whole,	des Ganzen zuständig,
not for the details."	nicht für die Details."

And in due modesty I close with a remark by Jean-Jacques Rousseau:

"I would not be so arrogant to teach people,
if I did not see, how others are misleading them."

And as many of us have been brought up with conceptions inherited from our great-grandfathers and students, who could be my grandchildren, are still indoctrinated that way, I am already working for the generation of my great-grandchildren, that is for the generations of the children and grandchildren of my students.

From METEOR 1988 to ANONYMA 2013

DNV GL Merger, effective Sept. 12, 2013

"**Standards are improving, but there is a lack of international governance. The industry needs strong, independent players that promote greater openness, consistency and effectiveness in the profession and push the development of new adequate measures and standards.** For our part, DNV GL, must take an active stance and show that we have qualified opinions on technical, operational, environmental and risk management issues. **We aim to deliver technical solutions that are practical and in the best interests of our customers and other stakeholders**."

Henrik O. Madsen, CEO of the DNV GL Group.

Schmiechen **Future trials and monitoring now / 17**

These remarks of the Chairman of the DNV GL Group explicitly highlight the fact, that the problems I have addressed do not belong into some esoteric realm, but are pressing, being of urgent practical importance.

From METEOR 1988 to ANONYMA 2013

Future trials and monitoring now!

Schmiechen **Future trials and monitoring now / 18**

From METEOR 1988 to ANONYMA 2013

Analyses of traditional powering trials
with the bulk carrier ANONYMA
in ballast at two trim settings

Michael Schmiechen, Berlin

apl. Prof. for hydro-mechanical Systems at ISM/TUB,
retired Deputy Direktor and Head of RaD at VWS

The complete analyses, the 'letter' and all related discussions etc are to be found under 'News on ship powering trials' on the website www.m-schmiechen.de

Status: 2013-08-06/10-06

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Status: 2013-08-06/10-06

Further recent motivations

The 25th anniversary of my propulsion tests with METEOR in the Greenland Sea, November 1988,

the 15th anniversary of a proposed rational standard for the assessment of trials, April 1998,

the overdue revision ISO 15016: 2002-06,

the 'incredible' STA-method promoted by MARIN,

its premature integration into the 'ITTC 2012 Guidelines',

their *approval contra legem* by the Executive Committee,

and their submission to MEPC of IMO.

That *is missing!*

Conventional (axiomatic) theory of ship powering performance under service conditions

Powering predictions based on tests with physical and/or numerical models

Proof of the pudding solely based on full scale tests

Structure of 'my letter'

2.1 Rational conventions

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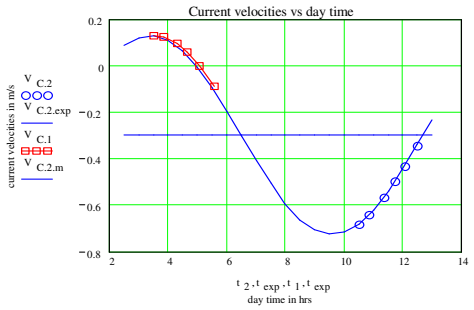

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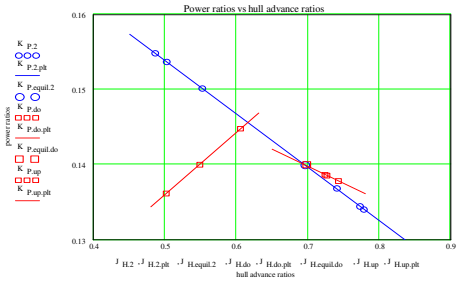

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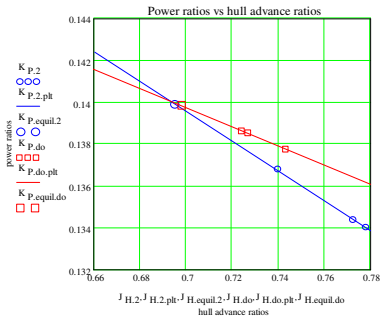

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Future trials and monitoring now!

Diskussion vor dem Vortrag

Diskussionsbeitrag zu dem Vortrag
von Prof. Michael Schmiechen

„Analyse von Probefahrten mit der ANONYMA in Ballast bei zwei Trimmlagen“

anlässlich der Tagung des Fachausschusses „Hydrodynamik“
der STG in Eckernförde am 19. September 2013

von Dr. Klaus Wagner Rostock, August 2013

Zunächst möchte ich dem Vortragenden meine Hochachtung dafür ausdrücken, mit welcher Leidenschaft und Hartnäckigkeit er seine rationale, d.h. Im Wortsinne „vernünftige“ Theorie der Schiffspropulsion gegen alle Widerstände der Traditionalisten vertritt. Sicher mutet seine Vorgehensweise manchen revolutionär, vielleicht sogar erschreckend an, aber ohne Revolutionen kommt der Fortschritt nur in Trippelschritten voran, wenn überhaupt.

Die Analyse und die Interpretation der Ergebnisse von Meilenfahrten war und ist eine diffizile Angelegenheit, da es hier um die Erfüllung oder Nichterfüllung wichtiger Vertragsbedingungen zwischen Lieferanten und Auftraggeber geht und weil die Fahrterprobungen fast nie unter vertragsgemäßen Bedingungen stattfinden.

Umso wichtiger ist es, dass bei der Auswertung möglichst transparente, rationale und von allen Beteiligten akzeptierte Konventionen zur Anwendung kommen. Hierzu hat der Vortragende einen Beitrag geliefert, der endlich die (!) Anerkennung bekommen sollte, die er verdient.

Prof. Schmiechen geht von 2 wichtigen Prämissen aus:

1. kein Rückgriff auf Modellversuche und
2. Verzicht auf den in der Praxis nur schwer messbaren Schub.

Der Vortrag zeigt, dass bisherige 'Normen' für die Bestimmung der Geschwindigkeits-Leistungs-Relationen (z. B. ISO 15016, ITTC 2012 Guidelines) zu schweren Analyse- und Umrechnungsfehlern führen können. So sind z.B. keine Vorschriften zur ausreichenden Propellertauchung, d.h. zu einem Mindestwert Propellerdurchmesser zu achterlichem Tiefgang enthalten, was sich bei dem ANONYMA-Beispiel verheerend ausgewirkt hat.

Nun einige kritische Bemerkungen zur Analyse- und Umrechnungsmethode des Vortragenden:

1.: Die aus der Pumpentheorie entlehnte lokale Propellerkonvention (propeller convention) ist einfach, klar und zusammen mit der Stömungskonvention (current convention) auch zur rechnerischen Bestimmung der so wichtigen Geschwindigkeit durchs Wasser V_H geeignet. In dimensionsloser Darstellung führt sie zu einer linearen Kennlinie des Propellers hinter dem Schiff (K_Q bzw. $K_P = f(V_H)$). Diese lineare Kennlinie wird aus einem in der Regel überbestimmten Gleichungssystem ermittelt, sie ist nur dann vertrauenswürdig, wenn der Schiffsfortschrittsgradbereich ΔJ_H bei den Messungen genügend weit aufgespreizt wird. Voraussetzung dafür ist, dass sich die Propellerbelastungen bei den Hin- und Rückkursen möglichst stark unterscheiden. Das ist bei idealen Bedingungen nicht der Fall (kubische Leistungskurve und konstante Propulsionswechselwirkungen ergäben für alle Messungen nur einen einzigen Punkt $K_P^* = f(J_H^*)$, durch den natürlich keine Gerade gelegt werden kann). So war z.B. das schwere Wetter bei der METEOR-Erprobung eigentlich ein Glücksumstand. Was wäre ein Ausweg, wenn die Bedingungen „zu gut“ sind? Quasistationäre Messfahrten mit Beschleunigen und Verzögern des Schiffes durch Drehzahländerungen.

2.: Sollte man nicht das Augenmerk darauf richten, die Geschwindigkeit durchs Wasser V_H mit ausreichender Genauigkeit direkt (!) zu messen, wie das für die Windgeschwindigkeit V w.rel.x schon der Fall ist? Dann brauchte man keine Strömungskonvention (current convention) mehr. Die Entwicklung mobiler Lasertechnik lässt hoffen, wie in der Vorstudie zum Projekt KONKAV gezeigt wird. Bei einem Schiff mit L/B = 6 beträgt die Übergeschwindigkeit eineinhalb Schiffsbreiten neben dem Schiff nur noch 1% der Schiffsgeschwindigkeit. Es müsste dies allerdings eine routinemäßig anwendbare Messeinrichtung sein, die die jetzt üblichen zu ungenauen Fahrtmessanlagen ersetzen würde.

3.: Die 2-parametrische Konvention zur Ermittlung der erforderlichen Leistung (Required Power Convention) – im Vortrag Umweltkonvention genannt - sollte nur als Notlösung benutzt werden, wenn keine Seegangsdaten erfasst wurden. Anderenfalls sollte immer eine Trennung des Wind- und Seegangseinflusses durch Einfügen eines dritten Parameters erfolgen, wie dies Prof. Schmiechen früher im ISO-Beispiel selbst praktiziert hat.

Warum? Während der Windeinfluss proportional dem Produkt aus absoluter (relativer) und vorzeichenbehafteter (relativer) Windgeschwindigkeit ist, also bremsend oder schiebend wirken kann, erfordert der Seegangseinfluss immer eine zusätzliche Leistung. Er ist neben dem Quadrat der („charakteristischen“) Wellenhöhe h_{wave} noch dem Quadrat der Wellenbegegnungs- (oder -verfolgungs-) Geschwindigkeit $V_{waverel}$ proportional, also immer (!) bremsend. Die Auswirkung zeigt ein von mir berechnetes Beispiel, in dem als Vertragsbedingungen u.a. die Fahrt bei glatter See und

Wind 2 Beaufort gegenan vereinbart war. 2- und 3- parametrischer Ansatz führten bei „no wind, no waves“- Bedingungen natürlich zur gleichen erforderlichen Leistung, aber für die o.g. Vertragsbedingungen ergab der 2-parametrische Ansatz (erwartungsgemäß) eine um 7,8% höhere erforderliche Leistung und eine um 1,8% höhere zugehörige Drehzahl. Das Beispiel kann bei mir als Papierkopie abgefordert werden.

Für die Trial-Guidelines wäre also unbedingt zu fordern, dass während der Messungen auch die Seegangsdaten festzuhalten wären. Neben der Schätzung durch die Nautiker gibt es heute auch schon satellitengestützte Ergebnisse für fast alle Weltmeere für die gewünschten Zeitpunkte.

4. Als offener Punkt bleibt die Umrechnung der Leistungs-Drehzahl-Geschwindigkeits-Relation auf andere Beladungszustände. Ausweg: Messungen auf einem zweiten

Tiefgang und Einführung eines 4. Parameters und der Proportionalität zur Verdrängung$^{2/3}$ (die gute alte Admiralitätskonstante) in die „Required Power Convention“ oder man schließt den Vertrag gleich über Bedingungen ab, die man dann bei der Schiffsübergabe auch realisieren kann, so wie das Prof. Schmiechen auch schon in Betracht gezogen hat.

Dr. Klaus Wagner
Maxim-Gorki-Strasse 5
D-18106 Rostock
e-Mail: ikwag@web.de

MS 20.08.2014 10:02 h

Erwiderung auf den Diskussionsbeitrag
von Dr. Klaus Wagner

Schon seit unserem ersten Treffen gelegentlich meines internationalen workshops 2nd INTERACTION Berlin '91 hat Dr. Wagner mein Engagement für die rationale Analyse der Propulsion von Schiffen ausdrücklich gewürdigt. Und sofort danach hat er mich mit Testdaten von Versuchen in der SVA Potsdam 'versorgt', deren Auswertungen sich am Ende der Proceedings befinden.

Und seither hat er durch seine kritischen Anmerkungen und konstruktiven Beiträge die Entwicklung meiner (erkenntnis-)theoretisch begründeten, 'erschreckend' einfachen (!) Lösungen von mit konventionellen Vorstellungen unlösbaren Problemen begleitet und gefördert. Dafür bedanke ich mich sehr herzlich. Trotz dieser engen Kooperation ist es mir aber immer noch nicht gelungen, ihn als Schiffbauer von einigen fundamentalen Dingen ganz zu überzeugen.

Ad 1. Doch der Reihe nach! Tatsächlich war das schwere Wetter im Falle der METEOR kein Glücksfall in dem von ihm erläuterten Sinn. Denn die Versuche wurden gar nicht wie traditionelle Probefahrten ausgeführt, sondern quasistationär, und zwar bei laufendem Forschungsbetrieb, ohne dass es überhaupt jemand merkte.

Versuche von jeweils zwanzig Minuten Dauer genügten für die vollständige Analyse der Propulsion. Entsprechende Modellversuche von nur zwei Minuten Dauer lieferten danach die Daten für die Bestimmung der Maßstabseffekte in Nachstrom und Sog, weltweit erstmalig und bisher einmalig.

Das schwere Wetter war nur insofern ein Glücksfall, als 'damit' demonstriert wurde, dass die ganze Versuchstechnik bei 'jedem' Wetter funktioniert. Die übliche, naive 'Einschränkung', dass dazu 'aber' Schubmessungen notwendig seien, erinnert an den Bären, der bittet: 'Wasch mir den Pelz aber mach mich nicht nass!' Dazu sogleich noch eine 'notwendige' Bemerkung.

Es ist völlig sinnlos, immer wieder Schub-Messgeräte zu erfinden und zu entwickeln, die sich 'einfach' auf die Wellen 'schnallen' lassen, die sich aber gar nicht kalibrieren lassen. Solche Vorhaben, die offenbar im Stile von *peer reviews* befürwortet und bewilligt werden, stellen eine unverantwortliche Verschwendung von Ressourcen dar. Um das festzustellen, braucht jeder 'andere' nur sehr wenig gesunden Menschen-Verstand, wie ich behaupte.

Ad 2. Doch jetzt zu einer grundsätzlichen Sache, zur Messung der Fahrt des Schiffes durch 'das' Wasser. Wie um Himmels Willen soll denn je irgendein teures Log unter allen möglichen Betriebs-Bedingungen funktionieren? Und wie soll das denn kalibriert werden? Das ist doch genau so unsinnig, wie alle teuren Schub-Messgeräte, die sich gar nicht kalibrieren lassen. Werfen Sie Ihr Log und Ihren professionellen Aberglauben endlich auf den

Schrott wie das Harburger Schub-Messgerät!

Sie enden einfach bei Konventionen 'ohne Ende', in einem völlig undurchschaubaren 'Draht-Verhau'! Und genau der war ja bisher nicht ohne Grund auch erwünscht, ist aber nicht mehr akzeptabel. Jetzt geht um Transparenz, um so wenige Konventionen wie möglich und nur mit Parametern, die sich aus den wenigen teuren Daten identifizieren lassen und nicht aus obskuren Quellen stammen, z. B aus dem Daumen gesaugt werden müssen!

Sehen Sie sich die Folien 5 und 6 meines Vortrages an und lesen die Bemerkungen dazu, auch die zu den Wind-Messgeräten! Für stationäre Zustände genügen die eingeführten Propeller- und Strömungs-Konventionen um die Fahrt durch das Wasser und die Strömung *gemeinsam* 'implizit' zu definieren (Hilbert, 1900), *gemeinsam zu konstituieren.* Das ist Stand der Erkenntnistheorie.

Ob Sie es wollen oder nicht, der Begriff der Fahrt durch 'das' Wasser ist nicht vom Himmel gefallen, sondern wird sind völlig frei zu sagen, was wir unter allen Bedingungen (!) darunter verstehen wollen und wie wir die Werte aus den messbaren Grössen bestimmen können. Alles andere ist schiffbauliche Folklore, überliefert von unseren Urgroßvätern.

Ad 3 und 4. In den beiden letzten Punkten stimme ich mit Dr. Wagner völlig überein. Über die Probleme, die noch zu lösen sind, habe ich oft geschrieben. Viele davon hätten schon lange gelöst sein können, wenn meine Ansätze und bisherigen Ergebnisse nicht konsequent ignoriert worden wären.

Ich habe die Daten meines vor den Versuchen mit der METEOR durchgeführten 'Modell'-Versuchs (alle Details auf meiner website!) jetzt benutzt, um zu zeigen, wie eine quasi-stationäre Probefahrt ausgewertet werden kann. Mit dieser Methode können die Kosten für Probefahrten in Zukunft drastisch gesenkt werden! Die Entwicklung und Erprobung des Verfahrens *full scale* ist m. E. ein lohnendes Thema für eine Magister-Arbeit oder sogar eine Dissertation.

Michael Schmiechen
m.schm@t-online.de
www.m-schmiechen.de

Berlin, 08./26.08.2013

MS 20.08.2014 10:02 h

Diskussionen nach dem Vortrag

From: "Michael Schmiechen" <m.schm@t-online.de>
To: "Karsten Hochkirch" <Karsten.Hochkirch@gl-group.com>
Cc: "Heinrich Söding" <h.soeding@gmx.de>;
"Moustafa Abdel-Maksoud" <m.abdel-maksoud@tu-harburg.de>;
"Gerhard Strasser" <prof.dr.g.strasser@sva.at>;
"Klaus Wagner" <IKWAG@web.de>
Sent: Saturday, September 21, 2013 5:08 PM

Subject: Diskussion fortgesetzt

Lieber Herr Hochkirch,

nachdem mein Vortrag und seine Diskussion, wie ich es schon lange gewohnt bin, abgebrochen wurden, hatte ich inzwischen Gelegenheit, weiter darüber zu denken.

Ich habe darauf heute zwei Nachträge in die Notizen zu meinen Folien eingefügt. Die vollständige aktuelle Fassung befindet sich wie immer auf meiner website. Dort finden sich auch die drei 'trefflichen' Zeilen aus dem Gedicht in der ZEIT.

Zu der Folie 13: Nachtrag 21.09.2013 Korrekt muss es natürlich 'nominal no wind condition' heissen. Denn die Messungen fanden ja bei beiden Probefahrten bei der angegebenen Wellenhöhe statt.

Zu der Folie 12: Nachtrag 21.09.2013 Sowohl Herr Dr. Gennaro als auch Herr Dr. Wagner haben bereits ausdrücklich festgestellt, dass die benutze Konvention nicht 'allgemein' akzeptabel ist. Ich werde gerne jeden 'besseren' Vorschlag prüfen.

Vermutlich bezog sich auch der Hinweis von Herrn Abdel-Maksoud auf dieses Problem. Wie ich erwähnte, habe ich, wenn immer mehr Seegangs-Daten vorlagen, schon viel 'bessere' Konventionen benutzt.

'Besser' heisst hier unseres Vertrauens würdiger. In meinem 'Brief' habe ich erörtert, dass die Entscheidung für eine der Konventionen, die Daten im Rahmen der Vertrauens-Grenzen beschreiben, nur auf Grund von zusätzlichen Konventionen getroffen werden kann.

Auf diese Weise bin ich bei der ANONYMA zu meiner 'tidal convention'

für die Strömung gekommen! Alle Details finden sich in der veröffentlichten Analyse.

Herr Söding hat natürlich Recht, dass wir von den traditionellen, erprobten Verfahren soviel übernehmen können, wie akzeptabel ist. 'Ausgerechnet' die Bestimmung der Fahrt durchs Wasser gehört aber 'leider' nicht dazu! Betreffend dieses fundamentale Problem versagt sowohl das ISO Verfahren, als auch das STA Verfahren.

Betreffend Methoden zur Umrechnung auf Zustände, die von denen der Probefahrt abweichen, neige ich zu aller grösster Vorsicht. Wie ich in der Diskussion feststellte, handelt es sich dabei um weitere Systeme von Konventionen.

Das trifft zu selbst bei Vorliegen der Ergebnisse von Messungen bei anderen Umwelt- und Beladungs-Zuständen, wie sie Herr Dr. Wagner untersucht hat. Fehlen solche Ergebnisse, dann sind Rückgriffe auf apriori Daten ganz unvermeidlich, aber selbstverständlich wieder Gegenstände akzeptabler (!) Vereinbarungen.

Völlig überrascht haben mich Feststellungen von Mitarbeitern der HSVA und der SVAP, dass beide Versuchsanstalten keine Experten für Probefahrten mehr haben.

Gelegentlich einer Diskussion zu dem Thema im Advisory Council der ITTC hat Herr Strasser die folgende Bemerkung gemacht: "Im Mittelalter war die Erde eine Scheibe. Wer sagte, dass die Erde eine Kugel wäre, wurde verbrannt. Vielleicht muß Prof. Schmiechen aufpassen, dass er nicht verbrannt wird!" Das war auch der Tenor meiner viel bescheideneren Bemerkungen zur Situation in den arabischen Ländern und zur Reformation vor fünfhundert Jahren hier zu Lande.

.
Mit freundlichen Grüssen
Ihr Michael Schmiechen.

PS. Ihre wiederholten Bemerkungen über die Diskrepanzen von full scale Prognosen basierend auf Ergebnissen mit numerischen und physikalischen Modellen für kleine Geschwindigkeiten bestätigen mir, was ich schon in meinem METEOR Bericht von 1990 ausdrücklich festgestellt habe und seither ständig wiederhole.

Die englische Fassung meines Berichtes ist wesentlicher Bestandteil der Proceedings meines Workshops '2nd INTERACTION Berlin '91' über die

Anwendung der rationalen Theorie im Falle der METEOR; sie ist auch auf meiner website veröffentlicht.

----- Original Message -----
From: "Heinrich Soeding" <h.soeding@gmx.de>
To: "Michael Schmiechen" <m.schm@t-online.de>
Sent: Monday, September 23, 2013 1:33 PM
Subject: Re: Diskussion fortgesetzt

Lieber Herr Schmiechen!

Schön, dass wir hier noch etwas diskutieren können!

On 21.09.2013 17:08, Michael Schmiechen wrote:
Lieber Herr Hochkirch,

nachdem mein Vortrag und seine Diskussion, wie ich es schon lange gewohnt bin, abgebrochen wurden, hatte ich inzwischen Gelegenheit, weiter darüber zu denken.

Ja, das tut mir auch leid, aber bedenken Sie: Dass Sie überhaupt noch zu Wort gekommen sind trotz der lange dauernden Uboot-Besichtigung, liegt an der Disziplin Ihrer Vorredner. Ohne solche Zeit-Disziplin geht es eben nicht.

Ich denke auch, Sie hätten gut daran getan, die Vortragszeit anders zu nutzen. Ihre Kritik an dem Vorgehen von Marin und ISO-Normierern ist für die meisten Zuhörer, mich eingeschlossen, unverständlich, weil wir diese Papiere ebenso wie Ihre früheren Papiere nicht so weit präsent haben, dass wir das verstehen könnten. Ich hätte es gut gefunden, wenn Sie ausgeführt hätten:

Was war das Ziel? Welche Messungen wurden durchgeführt, welche Messgrössen wurden festgestellt, welche nicht? Wie haben andere die Auswertung durchgeführt, und was haben Sie anders gemacht? Was war das Ergebnis der anderen Auswertung, was war Ihr Ergebnis?

Ihr Bild Leistungsbeiwert über J (ergänzt 2013-11-13: mit der Schiffsgeschwindigkeit durch das Wasser statt der mittleren Propeller-Anströmung) fand ich sehr gut, aber ich (und vermutlich die meisten anderen Zuhörer) haben es nicht im Detail verstanden. Meine Frage dazu haben Sie, denke ich, falsch beantwortet: Nach rechts nimmt wohl die Geschwindigkeit durch

das Wasser ab, nicht zu. Und wie das Bild mit der eigentlichen Fragestellung (die war doch wohl wie von mir formuliert: Antriebsleistung abhängig von der Geschwindigkeit durch das Wasser) zusammenhängt, ist sicher fast niemandem während des Vortrags klar geworden. Ich denke jetzt: Es dient vor allem zur Aussonderung der Fälle mit zu schwach getauchtem Propeller.

Mit meiner Einleitung habe ich versucht, die Zuhörer auf die Fragestellung einzustimmen, weil ich in Ihrem Papier dazu gar nichts gefunden habe. Probefahrten haben die Erprobung von sehr Vielem und die Messung von Vielem zum Ziel, nicht nur die Messung und Zuordnung der Antriebsleistung. Ohne eine Klarstellung, was das Ziel ist, verstehen Ihre Zuhörer mit Glück erst nach und nach implizit, was Sie wollen. Die Ablehnung, die Sie oft erfahren und die mir leid tut, beruht nicht in erster Linie auf der Dummheit der Zuhörer, sondern auf der Unverständlichkeit Ihrer Ausführungen für alle ausser denen, die sich viel mit dem Gebiet befasst haben.

Die letzteren äussern sich dann meist sehr positiv, wie z.B. in der Diskussion Herr Hochkirch und schriftlich Herr Wagner. Also freuen Sie sich über die Zustimmung der Kenner, und bemühen Sie sich bei späteren Präsentationen, den anderen Ihre Position verständlich zu machen. Eventuell wäre es dafür gut, dass Sie einen, der nicht zu den Kennern gehört, bitten, den Text zu überarbeiten, bis Sie und der Helfer beide einverstanden sind. In diesem Sinne hatte ich vor langer Zeit einmal Herrn Kracht gebeten, Ihre Position für die 'Schiffstechnik' aufzuschreiben, was er dann auch getan hat. Ich glaube, der Aufsatz war gut verständlich, aber Sie waren nicht ganz zufrieden damit.

Mit besten Grüssen Ihr Heinrich Söding

From: "Michael Schmiechen" <m.schm@t-online.de>
To: "Heinrich Soeding" <h.soeding@gmx.de>
Sent: Monday, September 23, 2013 2:29 PM
Subject: Diskussion fortgesetzt: Vielen Dank!

Lieber Herr Söding,

vielen Dank für Ihre freundlichen Ermahnungen, die nur bestätigen, was ich gesagt habe. Und genau das wollte ich aus guten Gründen sagen! Ich halte nichts von der rituellen Wiederholung der vertrauten Phrasen, sondern will zum Nach-Denken und Nach-Lesen, und womöglich zur Mitarbeit anregen.

Was hätte ich denn in der kurzen Zeit noch alles erzählen sollen? Die

Entwicklung von fünfundzwanzig Jahren rekapitulieren, die bisher von meinen Kollegen konsequent ignoriert wurde? (Das ist wie gesagt nicht mein Fehler!) Und die umfangreichen Details der veröffentlichten, delikaten Analysen der ANONYMA Probefahrten erläutern?

Die Zeilen aus dem Gedicht lauten übrigens:
"Wir sind für die Zustände
des Ganzen zuständig,
nicht für die Details."
Mein 'Glück' ist, dass ich auch mit den 'handwerklichen' Details vertraut bin.

Vielleicht machen Sie sich doch die 'Mühe', einmal den 'Brief' an meine Kollegen zu lesen. Herr Kracht hat überhaupt nicht verstanden, was ich gemacht habe. Als Schiffbauer kann er das so wenig wie andere traditionell ausgebildete Schiffbauer.

Inzwischen gibt es aber ausser den von Ihnen genannten noch einige andere Kollegen, die meine Ansätze bestätigen und/oder meinen Rat suchen, wie Herr Hochkirch. Lesen Sie z. B. meine Diskussion mit Herrn Gennaro aus Genua. Und was sagen Sie zu der Bemerkung von Herrn Strasser, Chairman des Advisory Council der ITTC?

Mit freundlichen Grüssen
Ihr Michael Schmiechen.

----- Original Message -----
From: "Heinrich Soeding" <h.soeding@gmx.de>
To: "Michael Schmiechen" <m.schm@t-online.de>
Sent: Wednesday, September 25, 2013 3:50 PM
Subject: Re: Diskussion fortgesetzt: Vielen Dank!

Lieber Herr Schmiechen:

Sie schrieben: Vielleicht machen Sie sich doch die 'Mühe', einmal den 'Brief' an meine Kollegen zu lesen.

Wie Sie richtig schreiben, muss ich als `traditionell ausgebildeter Schiffbauer' dazu vieles andere auch lesen. Und da ich in meinem Leben wohl nie eine Meilenfahrt auszuwerten habe, lasse ich das bleiben. Ich hoffe, Sie haben Verständnis dafür.

Mit besten Grüssen Ihr Heinrich Söding

----- Original Message -----
From: "Michael Schmiechen" <m.schm@t-online.de>
To: "Heinrich Soeding" <h.soeding@gmx.de>
Sent: Wednesday, September 25, 2013 5:06 PM
Subject: Re: Diskussion fortgesetzt: Vielen Dank!

Lieber Herr Söding,

natürlich habe ich Verständnis dafür.

Mich wundert nur, dass viele Kollegen mir ausdrücklich bestätigen, von Probefahrten (ship powering trials), alias Meilenfahrten, keine Ahnung zu haben, dass sie aber trotzdem darüber urteilen und im Zweifelsfall auch noch für die inakzeptable, frech so genannte 'Industrie-Norm', die STA-Methode stimmen und sogar Mitglieder der STA-Group sind, wie z. B. die (?) TUHH, also vermutlich das FDS. [Ergänzt.2013-11-13: Nein! Wie ich auf Nachfrage erfuhr, ist es das Institut von Prof. Krüger.]

Ein besonders trauriges Beispiel bietet das 'Spezialists Committee on Performance of Ships in Service' (SC SPP), dessen Mitglieder offenbar überhaupt keine Ahnung von den Problemen und dem Stand der Forschung haben und auch nicht einmal lesen können, wie dessen Chairman mir auf Nachfrage mitteilte! Es ist deshalb kein Wunder, dass die Mitglieder von Henk van den Boom (MARIN) 'überfahren' werden konnten.

Das Gleiche trifft für das Executive Committee (EC) zu, das sich seiner Schuld bewusst ist, aber (noch) nicht weiss, wie es aus der selbstgestellten Falle wieder herauskommt. Auch Herrn Friesch habe ich empfohlen, mit der HSVA so schnell wie möglich wieder aus der STA-Group auszusteigen. Aber, selbst Mitglied des EC (!), vertraut er immer noch auf die Kompetenz des SC SPP!

Solange ich atmen kann, werde ich meine Stimme gegen soviel Unverstand und die Unverschämtheit von MARIN erheben! Meine detaillierte Kritik an dem STA-Verfahren findet sich in meinem 'Brief' im Abschnitt 4.3.4 unter dem unmissverständlichen Titel 'The Emperor's New Clothes'. Lesen Sie dazu auch das von mir zitierte Plot des Märchens aus der Wikipedia, die präzise Beschreibung dessen, was zu besichtigen ist.

MS 20.08.2014 10:02 h

Der Brief bildet übrigens das Hauptstück meiner 'Festschrift', die zur STG Tagung erscheinen wird. Konstruktive Beiträge dazu sind herzlich eingeladen und werden, wie z. B. meine Diskussion mit Dr. Gennaro, auch mit abgedruckt.

Ich hoffe, auch Sie haben Verständnis für meine Position.

Mit freundlichen Grüssen
Ihr Michael Schmiechen.

MS 20.08.2014 10:02 h

Quasi-steady trials and monitoring

On an ongoing project, a 'model' test and a discussion

Quasi-steady monitoring trials and monitoring

On an R&D project concerning quasi-steady trials and monitoring and the first exercise. Presented at the Meeting of the STG Committee on 'Ship Hydrodynamics' at Eckernförde on September 19, 2013.

Kurzbericht über das aktuelle FuE-Vorhaben
Quasi-steady ship powering trials and monitoring
von Michael Schmiechen

Berlin, 01.09.2013

Die Durchführung traditioneller Probefahrten ist praktisch standardisiert, aber sehr aufwändig und muss deshalb dringend rationalisiert werden, nicht zuletzt auch deswegen, weil dieses Verfahren für die Überwachung der Propulsion unter Betriebs-Bedingungen überhaupt nicht geeignet ist. Seit meinen quasi-stationären Propulsions-Versuchen mit der METEOR 1988 im Nordmeer habe ich deshalb immer wieder die grossen Vorteile quasi-stationärer Probefahrten und Betriebs-Überwachungen 'beschworen'.

Aber erst jetzt im Rahmen des aktuellen Jubiläums 'From METEOR 1988 to ANONYMA 2013' bin ich selbst dazu gekommen, am Beispiel quasi-stationärer, schon für verschiedene Entwicklungen nützlicher Modell-Daten von 1986 zu demonstrieren, dass und wie das Verfahren 'funktioniert' und was dabei zu beachten ist.

Um weiteren (!) groben (!) Missverständnissen vorzubeugen, stelle ich ausdrücklich fest, dass auch dieses Verfahren, wie das rationale Verfahren zum Auswerten traditioneller Probefahrten, mit elementarer Mechanik und ohne Schub-Messungen auskommt! Das Verfahren wird jetzt für die Anwendung auf Schiffen bei Probefahrten und zur Überwachung weiterentwickelt werden.

Dabei wird auch das bereits zur Reife entwickelte rationale Verfahren zum Auswerten traditioneller Probefahrten für die Analyse der 'passierten' stationären Zustände zur Anwendung kommen müssen. Denn während die Durchführung traditioneller Probefahrten praktisch standardisiert ist, gibt es für die Auswertung der Daten immer noch kein anderes, allgemein akzeptiertes Verfahren, das den Anforderungen und den Erwartungen, z. B. des Verbandes Deutscher Reeder, genügt.

Meine erste Vorstudie, in Form eines Mathcad Programms, und schon Fragen und Antworten dazu sind unter 'News on ship powering trials' auf meiner website www.m-schmiechen.de veröffentlicht. Interessenten sind herzlich zur weiteren Diskussion der bisherigen Ergebnisse und, mehr noch, zur Mitarbeit an dem wegen der bei der IMO und der ITTC anstehenden Entscheidungen hoch aktuellen Projekt eingeladen. Meines Erachtens lassen sich dafür auch öffentliche Mittel einwerben, nur nicht mehr von mir.

MS 20.08.2014 10:02 h

An up-dated version of the proposal

Conducting traditional trials is practically standardised and the existing 'codes' will be further, hopefully finally harmonised in the revised edition of ISO 15016. But this method is very clumsy and inefficient and thus not at all suitable for monitoring of the powering performance under service conditions.

Therefore I have again and again pointed out the considerable advantages of quasi-steady trials and monitoring since my successful quasi-steady propulsion tests with the research vessel METEOR 1988 in the Greenland Sea. Only now, preparing for the current anniversary 'From METEOR 1988 to ANONYMA 2013 and further' I myself found the time to start developing a procedure for full scale applications.

Attached is the first exercise demonstrating the procedure and its inherent problems based on quasi-steady model data acquired in 1986 to prove the feasibility of the METEOR tests, and since having been useful for many more studies. As stated on various occasions I never rely on simulated data for the purposes at hand.

In order to guard against further (!) crude (!) misconceptions and grossly misleading rumours spread I explicitly state, that this procedure, as that developed for traditional trials, requires no theory of hull-propeller interaction and no thrust measurements.

In developing the procedure for full scale applications the procedure developed for traditional trials will have to be applied for the evaluation of the steady states 'passed'. While the conduct of trials is practically standardised there is no other acceptable procedure meeting the requirements identified.

Everybody interested in the problem and its solution is invited to contribute to the discussion and, last but not least, to join forces. In view of the current work on the revision of ISO 15016 no time is to be lost!

Prof. Dr.-Ing.
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201308261500
201308312230

To whom it may concern

'Model' test of quasi-steady ship powering trials and monitoring

Ref: The basic 'model' test directly accessible via the following link:
http://www.m-schmiechen.homepage.t-online.de/HomepageClassic01/**mod_evaf.pdf**

Preface

The following 'model' test of quasi-steady ship powering trials and monitoring is intended to demonstrate that quasi-steady trials full scale without thrust measurements of only one hour duration under service conditions, without anybody noticing that such tests are being performed, permit to monitor the powering performance in great detail.

This paradigmatic test is based on the data of the 'model' test of only two minutes duration with models VWS 2491/1340 performed on 09.09.1986 to demonstrate the feasibility of the more ambitious quasi-steady tests including thrust measurements performed with the research vessel METEOR in the Greenland Sea in November 1988. The same data have since been extensively used further to develop the technique, details to be found in the file directly accessible via the link quoted in the Reference.

'Unneccesary' to mention that in routine applications the programming will be quite different, typically in terms of subroutines, which have been used only occasionally in this document. But in view of the sensitivty of the problem at hand colleagues are warned: there will be 'no plug and play' program. In any case careful scrutiny of data and intermediate results is absolutely mandatory.

And to repeat: The method proposed offers dramatic technological and commercial advantages. No hull towing tests and propeller open water are necessary and the extremely short propulsion tests provide a wealth of consistent data and results.

Exposition improved by plots of data	MS 201308281200
Text and layout marginally changed	MS 201308311630

Preliminaries

Mathcad permits to handle physical quantities, but all data are being used without their SI units in view of further use in mathematical subroutines, which by definition cannot handle arguments with units.

Constants

Gravity field $g := 9.81 \cdot m \cdot sec^{-2}$ $g := g \cdot m^{-1} \cdot sec^{2}$

Units

Force $N := newton$ $kp := g \cdot N$

Torque $Nm := newton \cdot m$

Power $W := watt$

Model data VWS 2491/1340

Test identification $TID := "VWS\ 2491\ /1340"$

Date of test $Date := 860909$

Test No. $Test := 8$

Basic data

Ship model VWS Mod. 2491.0 Barge Carrier, which has not been built, body plan and contours of stem and stern to be found in the first appendix.

Length $L := 6.5 \cdot m$ $L := L \cdot m^{-1}$

Breadth $B := 1.00 \cdot m$ $B := B \cdot m^{-1}$

Draught $Tg := 0.255 \cdot m$ $Tg := Tg \cdot m^{-1}$

Displacement $V := 1.431 \cdot m^{3}$ $V := V \cdot m^{-3}$

Block coefficient $\phi := \frac{V}{L \cdot B \cdot Tg}$ $\phi = 0.8633$

Density of tank water $\rho := 1.00 \cdot 10^{3} \cdot kg \cdot m^{-3}$ $\rho := \rho \cdot kg^{-1} \cdot m^{3}$

Mass, model $M := \rho \cdot V$ $M = 1431.0000$

Model scale $\lambda := 37.23$

Added inertia $m_x := 0.024$

Surface $S := 8.967 \cdot m^{2}$ $S := S \cdot m^{-2}$

Propeller model VWS Prop. 1340

CP propeller, right handed

Diameter of propeller $D := 0.195 \cdot m$ $D := D \cdot m^{-1}$

Disc area $A_D := \frac{\pi}{4} \cdot D^2$ $A_D = 0.0299$

Pitch ratio, design $P_{D.des} := 0.825$

Pich ratio, actual $P_{D.act} := 0.813$

Number of blades $Z := 4$

Rate of revolutions at open water test $n_{open} := 12 \cdot Hz$

Model test conditions

Carriage velocity $F_n := 0.168$

$v_{carr} := F_n \cdot \sqrt{g \cdot L}$ $v_{carr} = 1.3415$

Frictional deduction $C_F := 0.183$

$F_F := C_F \cdot \rho \cdot D^2 \cdot v_{carr}^2$ $F_F = 12.5234$

Tank dimensions $h := 4.2$

$l := 240$

Data input

Digitized .jpg files

Fig's 6, 7, 8, 9 in VWS Report No. 1100/87 to found in the first appendix.

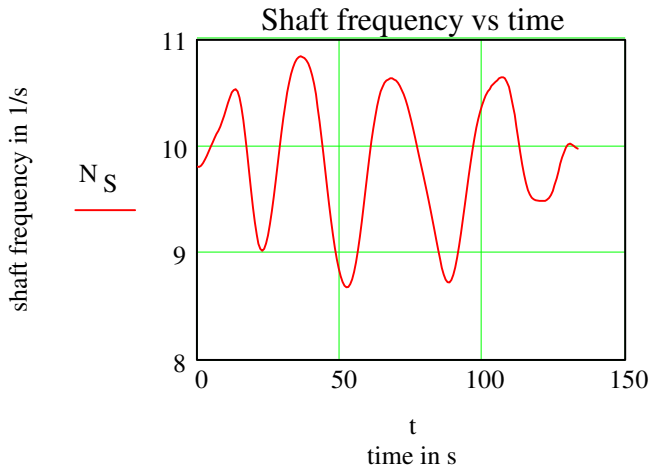
In the fundamental 'model' test mod_eval.mcd the raw data have been scutinzed, faired and recorded for ready reference..

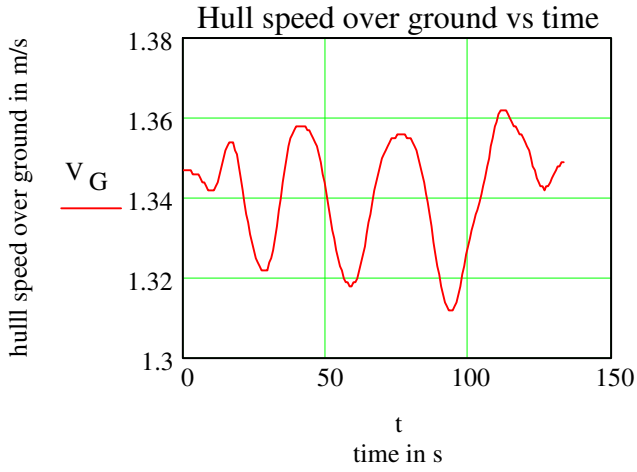
$Dat_{fair} := READPRN("dat\_fair.dat")$

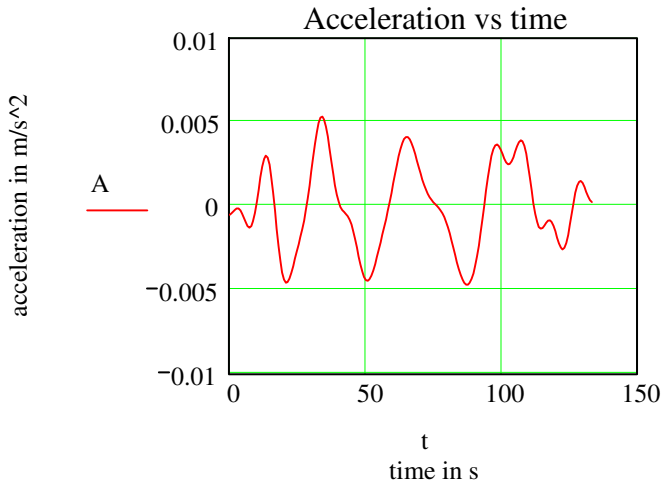
$t := Dat_{fair}^{<0>}$ $ni := last(t)$ $i := 0 .. ni$

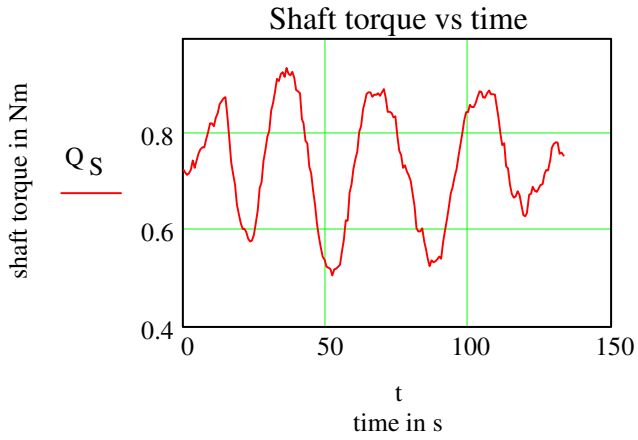
$N_S := Dat_{fair}^{<1>}$ $V_G := Dat_{fair}^{<2>}$ $A := Dat_{fair}^{<3>}$ $Q_S := Dat_{fair}^{<4>}$

$Q_P := Q_S$









Parameters identified

Hull speed

$V_{C_i} := 0.0$

Mean current in the tank
assumed for lack of more precise information.

$V_H := V_G - V_C$

$V_{H.mean} := mean(V_H)$ $V_{H.mean} = 1.3417$

$\Delta V_{H_i} := V_{H_i} - V_{H.mean}$

Hull advance ratio

$$J_{H_i} := \frac{V_{H_i}}{D \cdot N_{S_i}}$$

$J_{H.mean} := mean(J_H)$ $J_{H.mean} = 0.6984$

$\Delta J_{H_i} := J_{H_i} - J_{H.mean}$

Shaft power

$P_{P_i} := 2 \cdot \pi \cdot N_{S_i} \cdot Q_{P_i}$

$P_{P_i} := P_{P_i}$

$P_{P.mean} := mean(P_P)$ $\Delta P_{P_i} := P_{P_i} - P_{P.mean}$

$P_{P.mean} = 46.4870$

Set up of equations

$A_{P_{i,0}} := -V_{H_i}$

$A_{P_{i,1}} := -V_{H_i} \cdot \Delta V_{H_i}$

$A_{P_{i,2}} := P_{P_i}$

$A_{P_{i,3}} := P_{P_i} \cdot \Delta J_{H_i}$

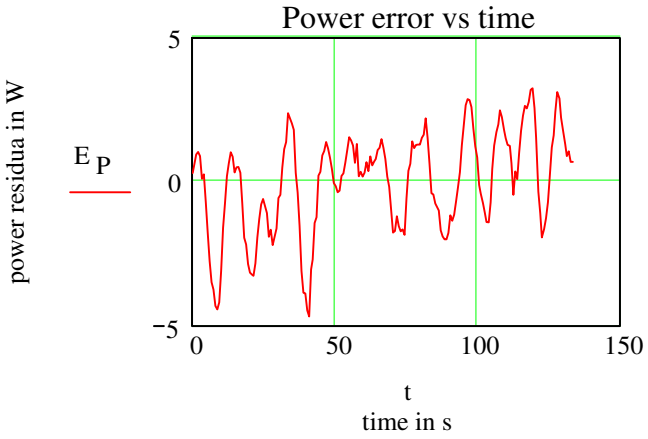
$B_{P_i} := \left[(1 + m_x) \cdot M \cdot A_i - F_F \right] \cdot V_{H_i}$

Solution of equations

$$X_P := \text{geninv}(A_P) \cdot B_P$$

$$X_P = \begin{bmatrix} 29.2225 \\ 59.2086 \\ 0.4821 \\ -0.0603 \end{bmatrix}$$

$$E_P := B_P - A_P \cdot X_P$$



At this stage it is noted that the residua exhibit a roughly linear trend with time.

This trend may be assumed to be due to a change in the inclination of the free surface.

Trend of residua identified

$$t_m := \text{mean}(t)$$

$$\Delta t := t - t_m$$

$$A_{E_{i,0}} := 1$$

$$A_{E_{i,1}} := \Delta t_i$$

$$A_{E_{i,2}} := (\Delta t_i)^2$$

$$X_E := \text{geninv}(A_E) \cdot E_P$$

$$X_E = \begin{bmatrix} -0.004483 \\ 0.019872 \\ 0.000003 \end{bmatrix}$$

The analysis shows that the trend is in fact linear.

$$P_{E.trend} := A_E \cdot X_E$$

Total change of inclination identified

$$\Delta t := t_{ni} - t_0$$

$$\Delta P_E := P_{E.trend_{ni}} - P_{E.trend_0} \qquad \Delta P_E = 2.6470$$

$$\alpha := \frac{\Delta P_E}{M \cdot g \cdot V_{H.mean}} \qquad \alpha = 0.000141$$

At the same time it is noticed, that the basic value is strictly accidental!

Solution iterated to account for correlation of power residua with time

$$P_P := P_P + A_E \cdot X_E$$

$$A_{P_{i,2}} := P_{P_i}$$

$$A_{P_{i,3}} := P_{P_i} \cdot \Delta J_{H_i}$$

$$X_P := \text{geninv}(A_P) \cdot B_P$$

$$X_P = \begin{bmatrix} 32.2455 \\ 66.4285 \\ 0.5734 \\ 0.3859 \end{bmatrix}$$

$$E_P := B_P - A_P \cdot X_P$$

$$P_{P.mean} := \text{mean}(P_P)$$

$E_{P.slope} := slope(t, E_P)$ $E_{P.slope} = 0.008$ There is still something left!

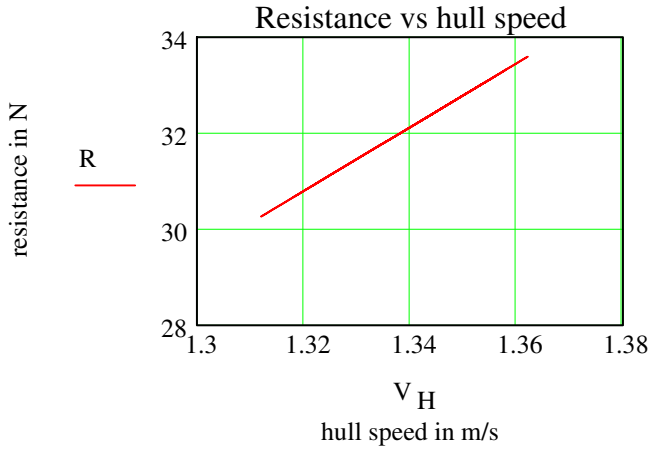
$E_{P.dev} := stdev(E_P)$ $E_{P.dev} = 1.5969$

$P_{P.mean} = 46.487$ $\frac{E_{P.dev}}{P_{P.mean}} = 0.0344$

In the following the results of the present analysis are compared with those obtained in the earlier analysis including the thrust measurements, the 'model' test documented on my webiste under 'News on ship powering trials' od directly via the link in the Reference.

Resistance identified

$R_i := X_{P_0} + X_{P_1} \cdot \Delta V_{H_i}$



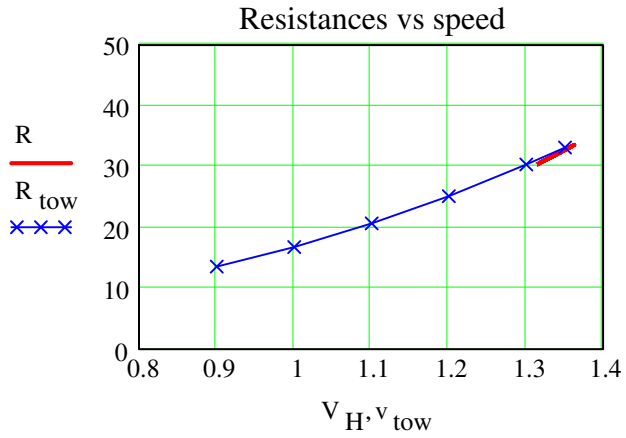
Resistance compared with towing resistance

$$Data_{tow} := \begin{bmatrix} 0.90 & 13.6 \\ 1.00 & 16.8 \\ 1.10 & 20.7 \\ 1.20 & 25.2 \\ 1.30 & 30.4 \\ 1.35 & 33.2 \end{bmatrix}$$

Values v in m/s, of R in N read from Fig. 3.4 in VWS Bericht Nr. 1126/88.

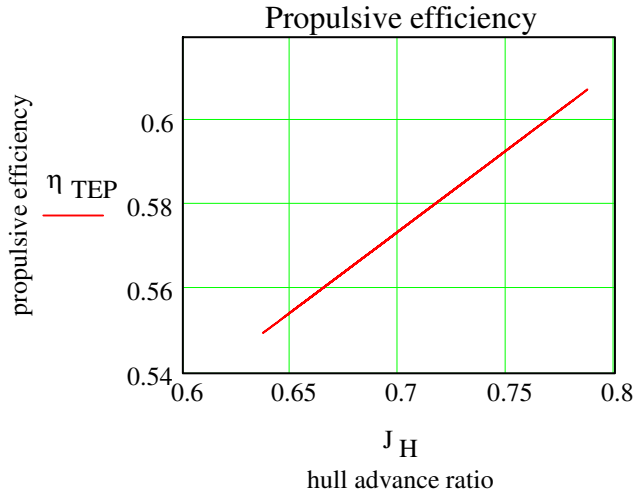
They conicide with those in VWS Report No. 1100/87.

$v_{tow} := Data_{tow}^{<0>}$ $R_{tow} := Data_{tow}^{<1>}$



Propulsive efficiency identified

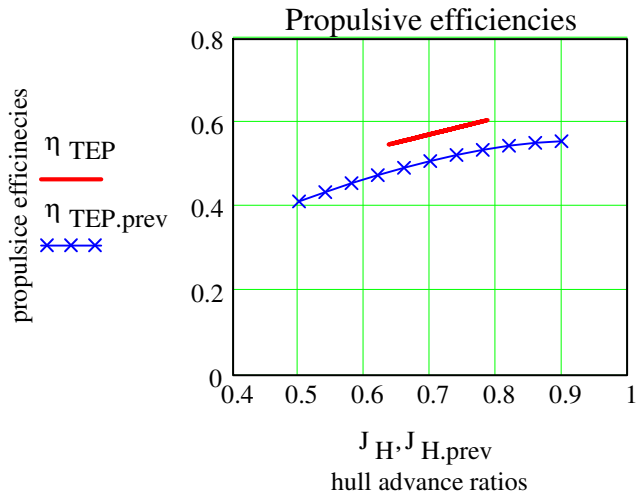
$\eta_{TEP_i} := X_{P_2} + X_{P_3} \cdot \Delta J_{H_i}$



Propulsive efficiency compared with previous values

$$J_{H.prev} := \begin{bmatrix} 0.5000 \\ 0.5400 \\ 0.5800 \\ 0.6200 \\ 0.6600 \\ 0.7000 \\ 0.7400 \\ 0.7800 \\ 0.8200 \\ 0.8600 \\ 0.9000 \end{bmatrix} \qquad \eta_{TEP.prev} := \begin{bmatrix} 0.4141 \\ 0.4363 \\ 0.4572 \\ 0.4765 \\ 0.4942 \\ 0.5103 \\ 0.5245 \\ 0.5366 \\ 0.5464 \\ 0.5536 \\ 0.5577 \end{bmatrix}$$

In the range of interest the previous values
are the same for rational and traditional evaluations.



While after accounting for the trend in the residua the model resistance is nearly exactly the same as the towing resistance reported, the resulting propulsive efficiency is 'still' about 14 % larger than previously obtained, implying that the actual power is less by that percentage.

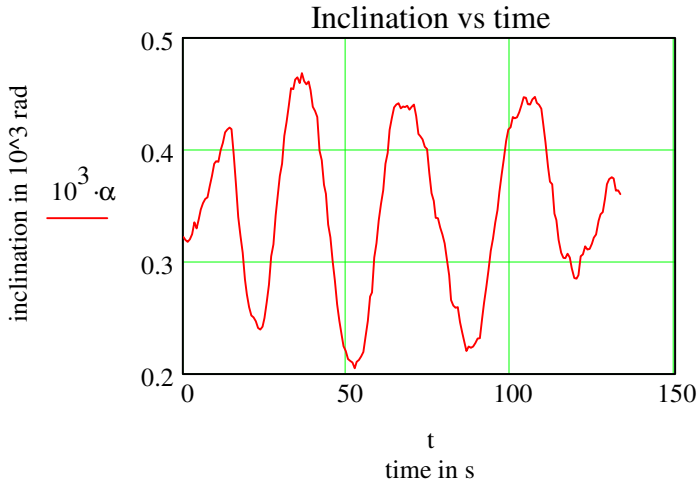
Inclination of model identified

$c := 0.14$

For this exercise based on the propulsive efficiency determined traditionally! In future to be identified from repeated trials!

See Conclusions!

$$\alpha_i := \frac{c \cdot P_{P_i}}{M \cdot g \cdot V_{H_i}}$$



The inclinition thus identified is strongly correlated with the acceleration.

$\Delta P_{O_i} := M \cdot g \cdot V_{H_i} \cdot \alpha_i$

$P_{P_i} := P_{P_i} + \Delta P_{O_i}$

$A_{P_{i,2}} := P_{P_i}$

$A_{P_{i,3}} := P_{P_i} \cdot \Delta J_{H_i}$

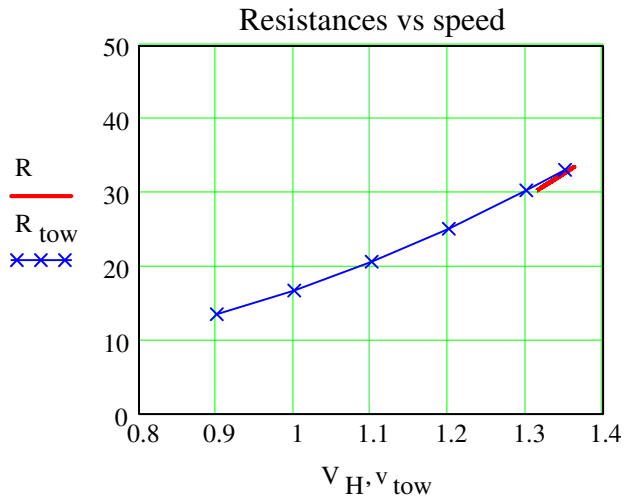
$X_P := \text{geninv}(A_P) \cdot B_P$

$$X_P = \begin{bmatrix} 32.2455 \\ 66.4285 \\ 0.5030 \\ 0.3385 \end{bmatrix}$$

$E_P := B_P - A_P \cdot X_P$

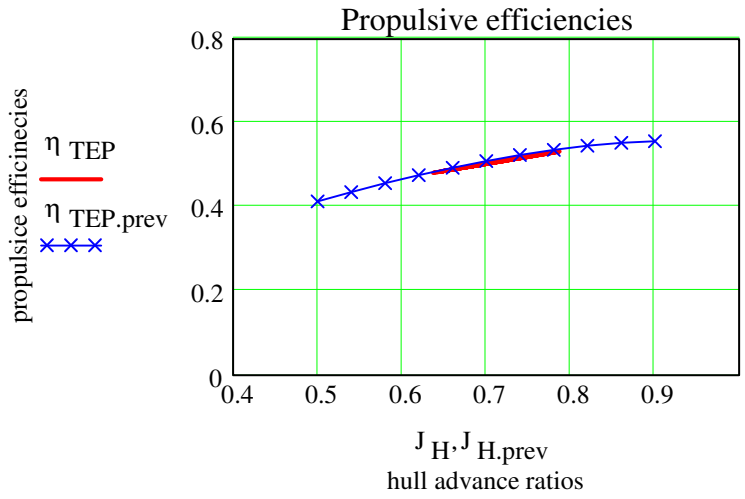
Resistance identified

$R_i := X_{P_0} + X_{P_1} \cdot \Delta V_{H_i}$



Propulsive efficiency identified

$\eta_{TEP_i} := X_{P_2} + X_{P_3} \cdot \Delta J_{H_i}$



Conclusions

From the preceding **basic exercise**, the evaluation of data acquired at a quasi-steady 'model' test of only two minutes duration, ignoring the thrust data (!), it is concluded that quasi-steady trials of an hour full scale will be possible for detailed monitoring of the powering performasnce of ships.

Evidently extremely small changes of the surface inclination will not effect the resistance, but the propulsive efficiency. Quite 'naturally' the values of the latter will increase if the model is moving 'down-hill'.

Thus for **trustworthy trials and monitoring** level surface has to be established at least computationally and in view of the omnipresent noise may thus require a number of repeated quasi-steady tests or, much simpler if possible, an extended test covering more than four cycles and maybe of shorter periods.

Assuming full scale tests over one hour covering 12 to 16 periods will permit to analyse 'all possible' sections, always over full periods, and thus establish confidence in the results. The model data at hand of only four periods permitted only for a rudimentary test of this proposed procedure.

Towing tanks can easily test this procedure, as they did in 1936/37 with Horn's proposal, and can ask for such tests at the next trials they are involved in. Of course in evaluating full scale data others of my procedures developed have to be applied. The pertinent development may be subject of a master's or even a doctoral thesis.

END
'Model' test of quasi-steady
ship powering trials and monitoring

Quasi-staeady trials: Fragen zum Verfahren

Es existiert hierzu eine umfangreiche Korrespondenz mit Dr. Klaus Wagner, von der hier nur das Beispiel von meiner website wiedergegeben wird

From: "Michael Schmiechen" <m.schm@t-online.de>
To: "Klaus Wagner" <ikwag@web.de>
Sent: Friday, August 30, 2013 7:17 PM
Subject: Re: Quasi-steady trials: basic exercise questioned

Lieber Herr Doktor,

diesmal mache ich es mir einfach und füge meine Antworten in Ihre mail ein. Haben Sie die aktuelle Fassung der Auswertung auf meiner website gesehen, insbesondere die Conclusions?

Mit freundlichen Grüsse
Ihr Michael Schmiechen.

PS. Da diese Erläuterungen von allgemeinem Interesse sind, stelle ich sie zu der Analyse auf meine website.

----- Original Message -----
From: "Klaus Wagner" <ikwag@web.de>
To: "Michael Schmiechen" <m.schm@t-online.de>
Sent: Wednesday, August 28, 2013 4:15 PM
Subject: Quasi-steady trials: basic exercise questioned

Lieber Herr Professor,

mit mod_trial_01.mod und mod_trial_16.mod habe ich wohl die kompletten Ergebnisse angesehen und habe 'natürlich' wieder Fragen und Bemerkungen.

1. Werden Sie auch die quasistationären full scale Messungen der METEOR nach dem neuen Schema auswerten und mit den Ergebnissen aus den 'stationären' (traditionellen) Meilenfahrten vergleichen?

> Was ich von den METEOR Daten noch habe, muss ich erst prüfen. Die
> Roh-Daten waren auf Bändern, die vermutlich keiner mehr lesen kann,
> selbst wenn sie noch auffindbar und noch lesbar wären. Ich habe aber da-
> für gesorgt, dass alle Kisten mit meinen Aufzeichnungen zu dem Projekt
> schon im Archiv der TUB sind.

MS 20.08.2014 10:05 h

> Ich habe nur gewisse Gradienten leicht zugänglich zur Verfügung. 'Mal'
> sehen, was sich evtl. daraus machen lässt. Traditionelle Probefahrten habe
> ich nicht gemacht. Wer die gemacht hat, weiss ich gar nicht. Später wur-
> den von der HSVA noch einmal Modell-Versuche gemacht. Da das ur
> sprüngliche Modell bereits verheizt war, musste noch ein neues gebaut
> werden!

2. Wurde etaTP aus R* VH / PP berechnet?

> Nein! Ich füge dazu meine gestrige Mitteilung hier ein, damit Sie nicht
> suchen müssen. [Zunächst eine Vorbemerkung:] Natürlich habe ich den
> Gütegrad der Propulsion früher immer korrekt mit etaTEP bezeichnet
> nämlich als das Verhältnis der effektiven (!) Schub-Leistung zur Wel
> len-Leistung. [Ich ändere die Symbole in der Auswertung!] (Hier ist
> das 'bezeichnet' korrekt verwendet, nicht so schlampig wie meistens,
> selbst bei Goethe.)

> Mit 'dem' Widerstand hat das 'nichts' zu tun, auch nicht nur mit dem
> Reibungs-Abzug, denn es gibt ja, insbesondere bei quasi-stationären
> Versuchen, auch noch den Trägheits-'Widerstand' und, wie das Folgende
> zeigt, auch noch die Gewichts-Komponente infolge 'der' Neigung der
> Wasser-Oberfläche.

> Deshalb habe ich ja den Wert des Gütegrades der Propulsion auch unab-
> hängig von dem Wert des Widerstandes identifiziert. Und deshalb haben
> m. E. die Engländer Recht, wenn sie von 'thrust deduction fraction'
> sprechen und nicht irreführend von 'Sog-Zahl', wie wir Deutschen.

3. S.1 vorletzte Zeile: für den Propellerentwurf (Festlegung der Propellergeometrie), wie er heute üblicherweise ausgeführt wird, wird R, t und w = f (VH) benötigt. Der von Ihnen ermittelte Propulsionswirkungsgrad ist ein Entwurfs- bzw. in Ihrem Fall Analyseergebnis.

> Nachstrom- und Sog-Zahlen lassen sich natürlich nicht bestimmen,
> wenn man den Schub nicht gemessen hat. Wird der aber gemessen,
> dann ist so zu verfahren, wie ich das in mod_evaf.mcd 'vorgemacht'
> habe!

4. S.2: Warum mx = 0.024 abweichend von den Werten in mod_trial_01.mod?

> Weil es 'völlig egal' ist, welchen Wert Sie annehmen. Es geht nur um pro

> mille! Selbst die früher erwähnte, evtl. fehlerhafte Ballastung des
> Modells spielt keine nennenswerte Rolle, nicht nur bei dieser Übung.

5. S.3: NS, VG, A (acceleration?), QS := Dat fair wurde aus mod_trial_01.mod entnommen?

> Ja! Inzwischen finden sich plots der Daten in dem file.

6. S.4: Warum VC = 0 abweichend vom (allerdings unwahrscheinlichen) Ergebnis nach mod_trial_01.mod?

> Ja! Das habe ich am Ende so gemacht, weil ich mit der 'Konvention' bei
> den Schleppversuchen konform sein wollte. Sonst hätte der Vergleich mit
> deren Ergebnissen überhaupt keinen Sinn. Dazu diverse Bemerkungen
> in vorherigen mails.

7. S.5 u. 6: Woher soll das Gefälle in der Tankwasseroberfläche (0.008 Grad) kommen, wenn doch VC = 0 ist?

> Wie gross soll denn die Strömung zu der extrem geringen Neigung sein?
> Die zufällige Neigung spielt nur für die Gewichts-Komponente (!) eine
> Rolle!

> Ich weiss ein Lied davon zu singen, seit ich mein Low Cost Inertial
> System > (LOCIS) für die Messung der sechs Lage- und sechs
> Geschwindigkeits- Komponenten, unter Verwendung von nur sechs (!)
> Feld-Sensoren, alias > Beschleunigungs-Gebern, entwickelt habe.

> Die geringsten Abweichungen von der Horizontalen (und der idealen
> Ausrichtung der Sensoren) produzieren dabei extreme Fehler, die sehr
> sorgfältig 'kompensiert' werden müssen. Und weil das möglich ist,
> bin ich davon überzeugt, dass es auch bei den quasi-stationären
> Probefahrten möglich sein wird.

8. S.6: Was ist mit der 1. und 2. Ergebnisspalte für XP?

> Die zweite Spalte auf Seite 8 ist vermutlich das Relikt irgendeines
> früheren Vergleichs. Die isoliert stehende Matrix kann und muss
> umgehend gelöscht werden! [Ist bereits geschehen!]

9. S.7 und 8: Die linearen Ansätze für R (VH) und etaTP (JH) sind lokal annehmbar, für größere Bereiche müsste zumindest für R ein quadratischer Ansatz gewählt werden. Wird die Lösung dann instabil?

MS 20.08.2014 10:05 h

> Ich finde mich schon sehr gut, die Werte und die Steigungen identifiziert
> zu haben. Und ich bezweifle, dass bei den geringen Variationen der
> Geschwindigkeit und des Fortschrittsgrades noch mehr [Information] drin'
> ist.

> Ich habe prinzipiell etwas gegen grosse Bereiche, insbesondere gegen
> Modell-Versuche bei kleinen Geschwindigkeiten, wegen der dann
> 'übertriebenen' Massstabsefekte. Siehe meine Bemerkungen dazu in dem
> METEOR-Bericht.

10. S.10: Woher c := 0.14? Wie soll ich mir die fast lineare Korrelation von Beschleunigung und Gefälle erklären?

> Ad 1: Wie ausdrücklich festgestellt, habe ich den Wert hier gewählt,
> damit 'es' passt. Aber schon das ist verblüffend! Denn, wie gesagt,
> hat diese zweite Korrektur 'natürlich' keinen Einfluss auf den Widerstand!

> Ad 2: Das habe ich mich auch gefragt! Aber was passiert denn mit 'der'
> Wasser-Oberfläche 'am Ort' eines schwimmenden Körpers, wenn Sie den
> Körper beschleunigen? Die ganze Geschichte fing ja damit an, dass die
> Residuen eine unerklärliche, linear von der Zeit abhängige Tendenz
> aufwiesen. Wurde die berücksichtigt, dann stimmte 'schon' der
> Widerstand!

11. S.11: Ich gratuliere zum Ergebnis, aber es könnte der Zweifel aufkommen, ob nicht die Prozedur zu sehr dem gewollten Ergebnis 'angepasst' wurde.

> Ja! Zugegeben, der Eindruck kann leicht entstehen! Aber hier wird schon
> so viel 'gezeigt', dass ich eine 'Veröffentlichung' für gerechtfertigt hielt.
> Vielleicht fängt ja doch jemand an, das Verfahren zu entwickeln und sich
> einen Doktor-Hut zu verdienen!

12. S.12: Schade, dass es keinen Versuch 'bergauf' gibt!

Soweit für heute.

Mit freundlichen Grüßen
Ihr Klaus Wagner.

Remarks concerning related work on monitoring

Added on 2014-01-15

Monitoring of the powering performance of ships is not a new subject, but has been of considerable interest and concern to ship owners and operators since the advent of engine powered ships. And the first towing tank was established to promote design for economical coal consumption of Her Majesty's Ships.

Today there are many procedures and/or systems marketed to improve and/or to monitor the performance, among others by Propulsion Dynamics, FutureShips, JOTUN.

And international conferences are held worldwide, among others the Ship Efficiency Conference at Hamburg in September 2013, and presently the 8th Vessel Efficiency & Fuel Management Summit in London at the end of January 2014, dedicated to 'Proficiency in ECDIS, Clarity in SEEMP and Responsibility in Power Management'.

Thus it does not come as a surprise that an ISO standard providing a market standard is being under development. In his paper at Hamburg on 'Hull Performance Solutions' Tom H. Evensen stated:

- The draft standard is now ISO 19030-1 to 3 and the working group is ISO / TC8 / SC2 / WG7.
- Jotun has been appointed project leader of the draft standard.
- The working group now consists of 12 experts from China, Korea, Japan, US, UK and a BIMCO representative.
- It is expected that another 8 to 10 experts will be directly involved.

As far as I am aware most the activities mentioned are based on the traditional concepts of propulsion inadequate for the purposes at hand. In relation to all these activities my project concerning quasi-steady trials and monitoring is modestly limited to the most fundamental problem, the transparent, trustworthy, efficient, reliable monitoring of the powering performance under operational conditions.

It is thus 'basically' concerned with the 'efficiency' of all the activities mentioned, in particular standardisation activities as in case of ISO 15016.

Added on 2014-08-20

A mistake in the published program has been repaired. The original version is kept here as a document, while the correct version has been published on my website and in Volume 2 of the 'Festschrift'.

MS 20.08.2014 10:05 h

'ITTC 2012 Guidelines' abandoned

Related correspondence and alerting colleagues

'ITTC 2012 Guidelines' abandoned

----- Original Message -----
(wo notwendig korrigiert)
From: "Michael Schmiechen" <m.schm@t-online.de>
To: "Klaus Wagner" <IKWAG@web.de>
Cc: "Gerhard Strasser" <prof.dr.g.strasser@sva.at>
Sent: Monday, October 21, 2013 7:46 PM
Subject: Erster, sehr wichtiger Erfolg!

Lieber Herr Doktor,

als ich gestern von akribischer Recherche schrieb, da dachte ich vor allem daran, zu meiner eigenen Sicherheit einer 'alten' Frage nachzugehen, da ahnte ich noch nichts von meinem ersten, sehr wichtigen Erfolg!

Denn eben habe ich festgestellt, dass auf der website der ITTC das Dokument

7.5-04 -01-01.2: Analysis of Speed/Power Trial Data
Pages 1 to 25, Effective Date 2012, Revision 00 (!).
(Updated / Edited by the Specialists Committee on Powering
of Ships in Service of the 27th ITTC. Not approved!)

ohne Kommentar und ohne ein von mir erbetenes statement, s. u., still und heimlich gegen das folgende alte Dokument ausgetauscht wurde:

7.5-04 -01-01.2: Analysis of Speed/Power Trial Data
Page 1 to 11, Effective Date 2005, Revision 00
Updated / Edited by the Specialists Committee on Powering
Performance of the 24th ITTC. Approved by the 24th ITTC 2005.

Mein Verdacht, klar formuliert in einer mail an Herrn Minchev, ausgelöst durch die 'unerklärlichen' Revisions-Angaben, war also 'richtig'! Tatsächlich sind die vorliegenden Angaben über die Revisionen in allen Quellen liederlich, schlicht falsch und irreführend!

Ich habe dazu auch die Minutes des MEPC-Treffens vom Mai ("ITTC Recommended Procedure 7.5-04-01-01.1 Speed and Power Trials Part 1; 2012 revision 1") und den Final Report des MEPC ("ITTC Recommended Procedure 7.5-04-01-01.2 Speed and Power Trials, part 2; 2012 revision 1") noch einmal geprüft.
Damit haben sich also auf jeden Fall der Chairman des Executive Commit-

tee der ITTC, Dr. Stig Sand, und der Chairman des Specialists Committee on Powering of Ships in Service, Dr. Anton Minchev, zufällig beide Angehörige von Force-Technology-DMI in Lyngby, Sang- und Klang-los von MARINs STA-JIP-Verfahren verabschiedet!

Ich bin gespannt was der Chairman des Advisory Councils, Prof. Gerhard Strasser, dazu sagt. Oder ob der gar wusste, dass dem MEPC jetzt eine alte, ihrerseits unbefriedigende Version untergeschoben worden ist?

Diese ganze Geschichte ist natürlich höchst brisant und nicht nur für mein hansa-online paper von grösster Bedeutung, für das der Redaktions-Schluss am Ende des Monats bevorsteht.

Was wird jetzt Herr Friesch sagen, Mitglied des Executive Committee der ITTC, dem ich den möglichst schnellen Ausstieg aus der STA-Group dringend empfohlen hatte? Er wollte sich aber lieber auf die Specialists verlassen, die ihn jetzt 'verlassen' haben und das Specialists Committee jetzt sicher verlassen müssen, unter ihnen auch Herr Dr. Hollenbach.

Meiner Frau habe ich schon angekündigt, dass ich darauf heute abend einen Schluck Wein mehr trinken werde!

Schon jetzt in der heitersten Laune
Ihr Michael Schmiechen.

PS. Die Historie muss ich nicht erzählen, die folgenden mails sprechen für sich selbst.

----- Original Message -----
From: "Michael Schmiechen" <m.schm@t-online.de>
To: "Stig Sand" <ss@force.dk>;
"Anton Minchev" <ami@force.dk>
Cc: "Gerhard Strasser" <prof.dr.g.strasser@sva.at>
Sent: Monday, October 07, 2013 8:25 PM
Subject: Fw: Trials without end, cont'd

Dear Dr. Sand,
dear Dr. Minchev,

attached please find my complete lecture on the ANONYMA trials, which I delivered at the recent meeting of the Ship Hydromechanics Committee of STG.

At that meeting colleagues told me, that they do not know anything about trials and that their renowned institutions no longer 'pay' expert employees concerning this subject. But all of them held firm opinions concerning trials and the role of 'specialists' in the ITTC!

Pondering these facts it occurred to me, that such colleagues should be asked honestly and responsibly to abstain from voting on fundamental subjects at the Full Conference, which according to my understanding has never been voting of 'illiterates' [, to say it politely].

In that connection please note my repeated reference to the 'Justice for Hedgehogs' by Dworkin, to the problem of arriving at fair resolutions of conflicts. The book does in fact not provide ready solutions for the situation at hand, but according to my understanding supports my above point of view.

Further I attach the abstract and the draft paper 'On Trials' to be published in the November issues of HANSA and hansa-online, respectively. While the deadline for the abstract has been the end of September, the deadline for the paper is the end of October, thus still permitting the incorporation of corrections and additions, if any.

Sincerely yours,
Michael Schmiechen.

PS. Please note, that the documentation 'From METEOR 1988 to ANONYMA 2013' in the Section 'News on ship powering trials' on my website is continuously updated, further analyses and discussions being added, though some of the latter are of 'cause' in German.

----- Original Message -----
From: "Michael Schmiechen" <m.schm@t-online.de>
To: "Anton Minchev" <ami@force.dk>
Cc: "Stig Sand" <ss@force.dk>
Sent: Tuesday, September 24, 2013 11:41 AM
Subject: Fw: Trials without end, cont'd

Dear Dr. Minchev,
dear Dr. Sand,

while I am working on my short note to be published in HANSA, I dare to

mention, that according to the rules of the game 'no answer is an answer as well'.

In the meantime I have not only published an updated version of my presentation at Eckernförde, but subsequent discussions as well, 'of cause' ('aus gutem Grund') in German.

Sincerely yours,
Michael Schmiechen.

----- Original Message -----
From: "Michael Schmiechen" <m.schm@t-online.de>
To: "Anton Minchev" <ami@force.dk>
Cc: "Stig Sand" <ss@force.dk>
Sent: Wednesday, September 18, 2013 9:01 PM
Subject: Fw: Trials without end

Dear Dr. Minchev,
dear Dr. Sand,

I refer to my earlier mail attached and to the following document:

According to the Final Report of the IMO MEPC 65-22, Annex 18, page 2, Amendments to Guidelines on Survey and Certification of the Energy Efficiency Design Index (EEDI) (Resolution MEPC.214 (63)), Paragraph 4.3.8 has been amended as follows:

4.3.8 The submitter should develop power curves based on the measured ship speed and the measured output of the main engine at sea trial. For the development of the power curves, the submitter should calibrate the measured ship speed, if necessary, by taking into account the effects of wind, tide, waves, shallow water and displacement in accordance with ITTC Recommended Procedure 7.5-04-01-01.2 Speed and Power Trials, part 2; 2012 revision 1, or ISO 15016:2002. Upon agreement with the ship owner, the submitter should submit a report on the speed trials including details of the power curve development to the verifier for verification."

Accordingly the 'ITTC 2012 Guideline' has not been accepted as the only (!) method for the purpose at hand, contrary to the firm assertion by Henk van den Boom and co-authors in their note in HANSA (150 (2013) 4, 58). This is another reason for my serious difficulties to believe any statement of my esteemed colleagues! Urgently required is a revision of the international

standard ISO 15016 meeting the requirements of all (!) parties concerned, not only those of MARIN.

Further I would like to know in detail (!) what exactly (!) the clause "2012 revision 1" implies. Is that already the response of the SC PSS to my detailed discussion of the 'ITTC 2012 Guidelines' in section 4.3.4 'The Emperors New Clothes' in my draft paper? You remember the one to be published, after due revision, on occasion of the STG Annual Meeting at Berlin in November under title:

'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'

With many thanks for your kind assistance and my and best regards
yours, Michael Schmiechen.

PS. Very early tomorrow morning I shall travel to Eckernförde to attend the meeting of the STG Ship Hydrodynamics Committee. The final version of the talk I shall deliver is to be found on my website.

----- Original Message -----
From: "Michael Schmiechen" <m.schm@t-online.de>
To: "Stig Sand" <ss@force.dk>;
"Anton Minchev" <ami@force.dk>
Sent: Monday, September 02, 2013 9:31 PM
Subject: Trials without end

Dear Dr. Sand,
dear Dr. Minchev,

the Editor in Chief of HANSA has invited me to respond to the short note by Henk van den Boom and colleagues of MARIN in his journal (HANSA 150 (2013) 4, 58) and to the detailed exposition of their proposal and the state of affairs according to their view (www.hansa-online.de STA-JIP.pdf).

In the process of collecting and screening the material for my paper I am wondering whether you have any contributions and pieces of advice, 'what to say and what better not to say', and/or useful input I should refer to, any pertinent excerpts from minutes of the EC and SC Meetings?
In the meantime I have heard so much about the meeting of the MEPC at

London, among others from VDR (Verband Deutscher Reeder), expressis verbis noting the unsatisfactory state of affairs, that I am wondering whether there are minutes available, which I may be permitted to inspect or which I am even expected to have inspected before writing my paper?

On my website you will have noted my presentation at the forthcoming meeting of the Ship Hydrodynamics Committee of STG, now also in English, and the accompanying abstract. Further I have added my basic study concerning the
feasibility of quasi-steady powering trials and monitoring together with the first very critical questions by Dr. Wagner and my detailed answers.

With kind regards yours,
Michael Schmiechen.

----- Original Message -----
From: "Michael Schmiechen" <m.schm@t-online.de>
To: "Stig Sand" <ss@force.dk>
Cc: "Gerhard Strasser" <prof.dr.g.strasser@sva.at>;
"Anton Minchev" <ami@force.dk>
Sent: Thursday, June 13, 2013 9:24 PM
Subject: Wer A sagt, muss nicht B sagen!

Dear Stig Sand,

as it happens, this morning I stumbled over the literary version of my suggestion, forwarded yesterday, by Bertold Brecht, the German dramatist (1896-1956): "Wer A sagt, muss [!] nicht B sagen. Er kann auch erkennen [und zugeben], dass A falsch war:"

Please do not mistake my remarks and my style to aim at offending anybody, but at frankly pinpointing deplorable states of affairs and attempting to assist rationally to resolve the conflicts at hand.

As I have stated in the draft of my paper, to be published under the unmistakable title 'Future Ship Powering Trials and Monitoring Now', conventions are, as their name says, not one-man-shows, but joint agreements among people knowing, what they are talking about.

The conventions, we have to look for, are not majority votes of practicians in model basins and ship yards, left alone with one of the most difficult problems of ship theory, since decades totally ignored by theoreticians at

the universities.

Some things are rotten [not only in the state of Denmark, but (*addition referring to an earlier mail*)] in these 'institutions' as well, as I have explicitly pointed out on various occasions, with the result, that my papers 'tend' not to be published! Perfectly convincing 'arguments' in favour of my argument!

With kind regards yours,
Michael Schmiechen.

MS 20.08.2014 10:05 h

Alerting my German colleagues

----- Original Message -----
From: "Michael Schmiechen" <m.schm@t-online.de>
To: "Wolfgang Menzel" <wolfgang.menzel@gl-group.com>; "Wolfgang Hintzsche" <hintzsche@reederverband.de>; "Volker Bertram" <volker.bertram@gl-group.com>; "Stefan Krüger" <krueger@tuhh.de>; "Stefan Harries" <harries@friendship-systems.com>; "Som D. Sharma" <s.d.sharma@t-online.de>; "Robert Bronsart" <info-mst@uni-rostock.de>; "Rainer Grabert" <grabert@sva-potsdam.de>; "Peter Schenzle" <peter@schenzle-hamburg.de>; "Moustafa Abdel-Maksoud" <m.abdel-maksoud@tu-harburg.de>; "Kay Meyerhoff" <kaymeyerhoff@t-online.de>; "Karsten Hochkirch" <Karsten.Hochkirch@gl-group.com>; "Jürgen Friesch" <friesch@hsva.de>; "Jan Wienke" <jan.wienke@gl-group.com>; "Iwer Asmussen" <asmussen@stg-online.de>; "Horst Nowacki" <horst.nowacki@naoe.tu-berlin.de>; "Heinrich Söding" <h.soeding@gmx.de>; "Günter Ackermann" <ackermann@tu-harburg.de>; "Gerhard Strasser" <gerhard.strasser@sva.at>; "Gerhard Jensen" <info@schottel.de>; "Gerd Holbach" <gerd.holbach@naoe.tu-berlin.de>; "Friedrich Mewis" <otto.fried@web.de>; "Frank Dau" <nsmt@din.de>; "Ernst August Weitendorf" <e.a.weitendorf@onlinehome.de>; "Dirk Jürgens" <dirk.juergens@voith.com>; "Cornel Thill" <thill@dst-org.de>; "Bettar Ould el Moctar" <ould.el-moctar@uni-due.de>; "Andrés Cura Hochbaum" <cura@tu-berlin.de>; "Andreas Kraus" <andreas.kraus@hs-bremen.de>; "Andreas Junglewitz" <andreas.junglewitz@gl-group.com>; "Erich Wolf" <erich-wolf@versanet.de>

Sent: Monday, October 28, 2013 3:24 PM
Subject: Probefahrten: Wieder auf Kurs!

Liebe Kollegen,

im Anhang finden Sie zu Ihrer Information die Kopie meiner mehr als brisanten 'Entdeckung'! Die Konsequenzen daraus muss jeder für sich selber ziehen, sie ziehen aber auch bereits weite Kreise. Informationen über die Hintergründe finden sich auch in dem zweiten Anhang, der einigen von Ihnen schon bekannt ist, und in den zitierten Quellen auf meiner website www.m-schmiechen.de.

Meine sorgfältig begründeten Mahnungen, den guten Ruf der Versuchsanstalten und der ITTC nicht leichtgläubig zu beschädigen, haben offenbar ihre Wirkung nicht verfehlt. Tatsächlich hat es mich ein halbes Jahr intensi-

ver Arbeit und sehr 'vielseitiger' Korrespondenz gekostet, den Chairman des Executive Committee der ITTC davon zu überzeugen, die ITTC nicht von MARIN als Trojanisches Pferd missbrauchen zu lassen.

Nachdem das unter neuem Namen wieder gegründete Specialists Committee on Powering Performance das von MARIN entwickelte STA-Verfahren zur Analyse von Probefahrts-Daten kritiklos in die sogenannte 'ITTC 2012 Guideline' übernommen hatte, wurde die vom Executive Committee der ITTC vermutlich unbesehen und ohne approval durch die erst 2014 stattfindende 27th ITTC an das Marine Environment Protection Committee (MEPC) der International Maritime Organsation (IMO) weitergeleitet.

Dass das Executive Committee jetzt stillschweigend auf ein seinerseits sehr unbefriedigendes Dokument zurückgreift, das vom Specialists Committee on Powering Performance der 24th ITTC aktualisiert und von der Full Conference der 24th ITTC 2005 'gutgeheissen' wurde, ist natürlich nur eine Not-Lösung. Aber mit dem Ausscheiden der ITTC aus der Prozession im Gefolge des Kaisers in seinen neuen Kleidern ist der Weg jetzt endlich frei für eine vernünftige Lösung.

Denn schon seit 1998 war bekannt, dass das traditionelle Verfahren nicht nur Fehler-anfällig ist, sondern den Anforderungen an die objektive, also weitestgehend vom Bearbeiter unabhängige Analyse von Probefahrts-Daten überhaupt nicht genügt. Trotzdem wurde das Problem von den Versuchsanstalten offenbar für 'endgültig' gelöst gehalten, denn zu meiner sehr grossen und wiederholt öffentlich geäusserten Verblüffung wurde das genannte Specialists Committee deshalb nach den Regeln der ITTC aufgelöst.

Wie jetzt 'langsam' viele Kollegen bemerken, ist das Problem aber noch weit davon entfernt, befriedigend gelöst zu sein. Denn weder die aktuelle Fassung der relevanten ISO Norm (15016: 2002-06) noch das von MARIN als 'industry standard' vermarktete STA-Verfahren genügt den heutigen Anforderungen an eine allgemein akzeptable Norm.

Ich werbe deshalb im November-Heft der HANSA und in hansa-online ausdrücklich für eine Neu-Ausgabe von ISO 15016, die in Kooperation mit dem neu zu besetzenden Specialists Committee entstehen muss. Denn nicht nur die Zeit, als Spurweiten von Eisenbahnen noch verschieden 'festverlegt' wurden, ist längst vorbei, sondern auch wir können uns 'ähnliche' und andere Kindereien schon lange nicht mehr leisten.

Anders als bei ISO, DIN und andern Normungs-Organisationen müssen dazu auch Experten, wohlgemerkt keine 'Specialists', zugezogen werden,

insbesondere einer für Normung. Denn wenn schon das Auswerten von Probefahrten kein hydrodynamisches Problem ist, dann ist es dessen Normung erst recht nicht.

Nach Auskunft der DIN Normen-Stelle Schiffs- und Meerestechnik (NSMT) vertritt die deutschen Interessen bei der inzwischen endlich in die Wege geleiteten Neu-Ausgabe der Norm ISO 15016 zur Zeit noch alleine Herr Dr. Hollenbach. Wie der mir mitteilte, vertritt Hans Huisman von ER Schiffahrt die Interessen der Reeder sowohl gegenüber der ITTC als auch gegenüber der ISO. Und meine bescheidene Frage war: Wie geht denn das? Seit wann 'dient' die ITTC Interessen-Vertretern so wie die genannten Organisationen?

Und zum Schluss ein Wort zum Zweck dieser mail. Nach der Entscheidung des Technisch-wissenschaftlichen Beirats (TWB) wird es im Programm der kommenden Hauptversammlung der STG keine Gelegenheit zur Diskussion dieses höchst aktuellen Themas geben, 'eventuell im nächsten Jahr', also nach der 27th ITTC. Auch nicht nach dem Vortrag von Herrn Dr. Hollenbach, da dessen Text nach seiner eigenen Auskunft, und mit Duldung des TWB, bis dahin nicht vorliegen wird, also auch schon dessen Diskussion von vornherein ausgeschlossen ist!

Auch die für das Projekt von HSVA und SSPA unbedingt notwendige unabhängige Analyse der vorliegenden Probefahrts-Daten ist nicht möglich, 'weil die Daten vertraulich sind'. Diese häufig gehörte, völlig 'perverse' Ausrede schliesst nicht nur den Erfolg des genannten Projektes aus, den proof of the puddding, sondern auch den Erfolg anderer Projekte..

Substantielle Diskussionen wie früher üblich, nicht nur talk shows von fünf Minuten wie jetzt oft, führe ich deshalb schon lange per e-mail und dokumentiere sie auf meiner website. Jeder von Ihnen ist herzlich eingeladen daran teilzunehmen, das um so mehr als ich jeden Adressaten gerne persönlich 'angesprochen' hätte. Und bitte nicht vergessen: Keine Antwort ist auch eine, wie das aktuelle Beispiel zeigt.

Natürlich muss niemand meine Prinzipien, meine 'Vor-Urteile' teilen, ich selber würde aber sehr gerne 'Vor-Urteile' teilen, die (noch) nützlicher sind als meine.

In diesem Sinne mit freundlichen Grüssen
Ihr Michael Schmiechen.

PS 1. Die oben genannte 'vielseitige' Korrespondenz wird natürlich in mei-

nem Nachlass permanent archiviert, einige Seiten erscheinen aber eventuell schon auf meiner website in einer kleinen 'Festschrift' zum Jubiläum meiner Versuche mit der METEOR und der seither erfolgten Entwicklungen. Dafür muss ich mir aber natürlich erst noch Erlaubnisse erbitten.

PS 2. Auf die Reihenfolge der Einträge in der Anschriften-Liste habe ich zwar Einfluss, leider aber (noch) nicht auf das, was Outlook Express dann damit macht.

MS 20.08.2014 10:05 h

ISO 15016: current revision

Vote for an acceptable standard meeting all requirements

Urgently required!
Future Ship Powering Trials and Monitoring Now!

Vote for a revised, generally acceptable, lasting edition of ISO 15016, concerning not only trials, but also monitoring of the powering performance, meeting theoretical, contractual and legal standards and requirements

by Michael Schmiechen, Berlin

Evaluation of ship powering trials is traditionally treated as hydromechanical problem, though it is of conventional nature, part of a whole range of legal and contractual conventions, fundamental pre-requisites of which are shared convictions, fashionably called 'principles', being prejudices as Mark Twain aptly noted. As their name says, conventions are *not* 'one man or one institute shows' as currently being performed by MARIN.

That the present version ISO 15016: 2002-06 of the pertinent international standard, based on the conceptions of our great-grandfathers, is error prone has already been demonstrated 1998, long before it nevertheless has been standardised. "Reliable ship-speed assessment more relevant than ever" was thus the perfectly correct title of a note in this journal (HANSA 150 (2013) 4, 58).

But the note itself is quite 'incredible', hardly any of the claims in the detailed exposition of the STA method developed at MARIN (www.hansa-online.de/STA-JIP.pdf) being substantiated, but reminding of the time when railway gauges were purposely selected differently in different countries for 'protective' reasons. A detailed review of the STA method promoted by MARIN even at ITTC and IMO, has been published in section 4.3.4 'The Emperor's New Clothes' in my paper on 'Future Ship Powering Trials and Monitoring Now!'

At the end of Andersen's archetypal tale (Wikipedia): "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." To continue the procession will further delay progress for decades as did ISO 15016: 2002-06.

In view of the deficiencies of the ISO and STA methods, both 'adopted' by the IMO MEPC in its Final Report 65-22, the only reasonable decision is *promptly* to agree on a revised, generally acceptable, lasting edition of ISO 15016, concerning not only trials, but monitoring of ship powering perform-

MS 20.08.2014 10:05 h

ance as well, meeting theoretical, contractual and legal standards and requirements.

The purpose of the standard, to permit the *objective* resolution of 'conflicts', are to be met by simple conventions with few parameters jointly identified from the data acquired, without any reference to results of model tests or other prior data.

A detailed note on trials and monitoring is provided online:

http://www.hansa-online.de/fileadmin/pdf/fachartikel/Schmiechen.pdf

Urgently required!
Future Ship Powering Trials and Monitoring Now!

Vote for a revised, generally acceptable, lasting edition of ISO 15016, concerning not only trials, but also monitoring of the powering performance, meeting theoretical, contractual and legal standards and requirements

by Michael Schmiechen, Berlin

ABSTRACT

An executive summary of this short paper has been published under the same title in HANSA (150 (2013) 11, 55) and as 'Note on trials' (trl_note.pdf, trl_HANSA.pdf) in the Section 'News on ship powering trials' on my website.

In addition it is worth noting, that sometime during end of September, the deadline for the abstract, and end of October, the deadline for this paper, the Executive Committee of the ITTC decided to abandon the 'ITTC 2012 Guidelines', to back out of the procession following the emperor in his new dresses.

[Added 2013-11-13: And further, that work on the revision of ISO 15016 is under way. An account of the status has been presented by Tsuyoshi Ishiguro right after this paper had to be sent to the editor. The presentation is to be found on my website under 'ISO15016: Status of revision 2013-11.pdf'. And I have immediately alerted the author of the situation, to be found under 'ISO 15015: On the current state of revision.]

THE PROBLEMS

The evaluation of ship powering trials is still treated as hydro-mechanical problem, although it is of 'conventional' nature – *not* to be mistaken for 'traditional' –, part of a whole range of intricately intertwined legal and contractual conventions.

At the focus of this short paper is the fact, that the structure, the implications and the relations of the conventions involved are usually *not* stated explicitly and are thus only vaguely known.

In particular, the underlying principles are *not* generally shared, although the same beliefs, convictions or 'principles', as they are fashionably called, – 'principles' being another name for 'prejudices' as Mark Twain aptly noted –, are essential pre-requisites of conventions. As their name says, conventions

MS 20.08.2014 10:05 h

are *not* 'one man or one institute shows' as currently being performed by MARIN.

The present situation reminds of the time when railway gauges were purposely selected differently in different countries for 'protective' reasons, but which turned out to cause unnecessary costs and to delay progress for many decades, if not centuries as in case of the SI Units. In view of the urgent demands for a generally acceptable, lasting standard meeting theoretical, contractual and legal standards and requirements this 'strategy' is self-defeating.

RELIABLE PROOFS

Presently many colleagues realise, that very many methods have been developed to predict the powering performance of ships based on results of physical and/or numerical model tests, erroneously mistaken for propulsion theory, but that hardly any methods have been developed for the convincing, trustworthy proof of the pudding, of the results full scale, meeting today's requirements, their own in particular.

Theoreticians have 'simply' left the very difficult problems of trials and monitoring of the powering performance to 'practicians' (*for my taste this original, old fashioned term is more suitable than the recent 'practitioners'*) at ship yards and model basins. And, hard to believe, ship owners still accept, that the same 'people' providing the predictions are not only carrying out and analysing the trials 'as well', but are even setting up the standards to be met!

Further many colleagues at universities, model basins and the ITTC realise that they have consistently ignored developments of rational methods of performance analysis for decades for the sole reason, that these methods have *not* been phrased in the jargon of our great-grandfathers, *not* noticing that these methods cannot be phrased in that jargon, as the deficiencies of current methods show.

THE MODEL

Ship powering trials are based on two or even three very different, clearly to be distinguished and cleanly to be separated systems of conventions. Firstly those concerning the conduct of trials and of measurements, secondly those concerning the evaluation of the performance at the trials conditions, often ballast conditions, and thirdly, if requested (!), those concerning predictions (!) of the performances at conditions differing from the trials conditions, typically at the conditions contracted.

Conventions are agreements, are languages and their implications (to be) agreed upon. Traditional conventions are usually *not* explicit, often incoher-

ent languages, while rational conventions are explicit *formal languages constructed ad hoc for the purposes at hand*. In terms of logics these are axiomatic systems, a terribly frightening name for extremely useful tools.

The most important parts of their grammar to be agreed upon are *not* the rather simple rules explicitly and implicitly to define concepts and the more or less simple rules to derive the consequences, but the principles underlying the introduction of basic concepts and conventions, the principles of objectivity in particular!

The concepts and their interpretations still taught worldwide did *not* fall from heaven, but have been inherited from our great-grandfathers and 'happen' *not* to be adequate for present day purposes, *not* to be applicable at full scale service conditions. *Concepts are defined and obtain their values only in the contexts of conventions, i. e. of suitable reference systems constructed ad hoc for the purposes at hand.*

THE GOAL

Of particular interest are still traditional trials as usually performed, *i. e.* without measurement of thrust, of hull speed through the water and of sea states. The fundamental task in the fair resolution of conflicts is to set up rational conventions so simple and 'self-evident', that they and their consequences are intelligible and thus acceptable for the all parties interested in the results.

As has been demonstrated unmistakably in a number of published cases, the evaluation *at the trials condition* does *not* require any theory of propulsion, but only some elementary mechanics, some common sense and, last but not least, an often to be missed extreme care in evaluating the valuable data acquired at considerable costs.

The most fundamental principle to be agreed upon is that the evaluation should *not* require any prior data, in particular no results of model tests, *as it must be for the objective assessment* of the powering performance *at the trials conditions*. 'Objective' implies *independent of the 'observer'*, of the person in charge of the evaluation and its prejudices and preoccupations.

This short paper just permits to mention the fundamental deficiencies of the standard ISO 15016: 2002-06 and of the STA methods, the latter developed and 'marketed' by MARIN, detailed explanations and references to be found in the few links quoted. As a consequence I am promoting the long overdue generally acceptable, lasting revision of ISO 15016, concerning not only trials, but also monitoring of powering performance as well, meeting theoretical, contractual and legal standards and requirements of all groups concerned.

ISO 15016: 2002-06

That the current version ISO 15016: 2002-06 of the pertinent international standard on the assessment of the powering performance of ships, based on the conceptions of our great-grandfathers, is outdated and error prone, has already been demonstrated and brought to the attention of all national groups long before it nevertheless has been adopted as standard.

The proposed draft alternative (trl_prp.pdf), filed as 'Informative' by the Japan Marine Standards Organisation under ISO/TC8/SC9/WG2/N28 dated 1998-06-23, has in fact already been rather detailed and theoretically solidly founded and has since been tested successfully, *i. e.* shown to serve the purpose! The early work is fully documented under 'Papers on Ship powering trials' on my website (pap_trl.htm).

THE STA- METHOD

"Reliable ship-speed assessment more relevant than ever" has thus been a 'late', perfectly correct title of a short note by Henk van den Boom of MARIN and co-authors in this journal (HANSA 150 (2013) 4, 58). But that note itself is more than surprising and 'incredible'.

The author, Head of MARIN Trials and Monitoring, Manager of the Ship Trials Analysis (STA) Group and Member 27th ITTC Specialists Committee (SC) on the Performance of Ships in Service (PSS), the re-established Specialists Committee on Powering Performance, is explicitly referring to the 'cooperation' of the MARIN promoted SAT-Group with the ITTC SC on PSS, and notably with HSVA and 'TUHH', in fact the Institute of Ship Design and Ship Safety of that Technical University.

In the note it is claimed, that the STA-Group has established an 'industry standard' and that the 'ITTC 21012 Guidelines' (ITTC 7.5-04-01-01.1 and 2), the second part based on that 'standard', has *not* only been approved by 'the ITTC', but forwarded to the IMO and that "finally the IMO Marine Environment Protection Committee (MEPC) has accepted these Guidelines as the only method to be used for speed-power analysis of vessels above 100 m length worldwide".

ITTC AND IMO

According to the rules of ITTC the 'Guidelines' may be approved or, more likely, *not* approved by the Full Conference at the 27th ITTC to be held at Copenhagen only in September 2014. How then could it possibly be "presented as a transparent, unambiguous and practical analysis method" to the IMO MEPC and accepted by the latter to be used 'worldwide'?

Further, according to the Final Report of the IMO MEPC 65-22, Annex 18, page 2, Amendments to Guidelines on Survey and Certification of the

Energy Efficiency Design Index (EEDI) (Resolution MEPC.214 (63)), Paragraph 4.3.8 is amended as follows:

> 4.3.8 The submitter should develop power curves based on the measured ship speed and the measured output of the main engine at sea trial. For the development of the power curves, the submitter should calibrate the measured ship speed, if necessary, by taking into account the effects of wind, tide, waves, shallow water and displacement in accordance with ITTC Recommended Procedure 7.5-04-01-01.2 Speed and Power Trials, part 2; 2012 revision 1, or ISO 15016:2002. …"

Thus the assertion by van den Boom and co-authors happens *not* to be true in two important respects: The 'ITTC 2012 Guidelines' have *not* yet been approved by 'the ITTC' and they have *not* been accepted by the IMO MEPC as the *only* method for the purpose at hand.

STANDARDS

In the note of van den Boom it is further stated, that to fulfil the new IMO rules to reduce CO_2 each new vessel *has to undergo unified strictly prescribed speed trials*. What trials else? But who is *prescribing how speed trials* are to be conducted and who is *prescribing how speed trials* are to be evaluated? Definitely *not* a single institute claiming to have produced an 'industry standard', a 'standard' that itself does *not* meet elementary standards, principles and requirements to be met by decent, lasting standards.

Even if the community has only an embryonic understanding of the purposes of standards, namely the fair resolution of conflicts among all parties involved, it cannot possibly accept the STA method, as none of the claims in the detailed exposition (www.hansa-online.de/STA-JIP.pdf) of the authors' proposal is substantiated. The goal of ITTC and its reputation have always been to meet the urgent requirements of researchers and clients based on the current state of research. The 'incredible' STA procedure confirms my repeated statement that the fundamental, intricate problems of evaluating powering trials and of setting up appropriate, acceptable standards for that purpose should *not* be left to naval architects and to practicians in model basins and ship yards.

THE EMPEROR'S NEW CLOTHES

Most 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. A detailed analysis of the STA procedure has been published as section 4.3.4

'The Emperor's New Clothes' in my draft paper on 'Future Ship Powering Trials and Monitoring Now!' (METEOR_25_pap.pdf).

At the end of Andersen's archetypal tale (Wikipedia) "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.*

In my view it is *not* a viable alternative to 'continue the procession' and prevent innovation for further decades, as did the standard ISO 15016: 2002-06 before. To 'continue the procession' will seriously damage the reputation of the ITTC. The goal of ITTC, founded as the International Conference of Towing Tank Superintendents, the latter originally personally at the forefront of research, has never been to perpetuate the procedures originated more then hundred years ago and to protect related profitable businesses.

ITTC BACKED OUT!

Having brought the draft of this short paper with all these facts timely to the attention the Executive Committee of ITTC, finally before sending this paper to the editor in vain I have asked for a statement concerning the state of affairs and for advice 'what to say and what better *not* to say'. According to the rule of the game: 'No answer is an answer as well', I was thus 'forced' to come up with my own statement.

In order to protect myself from mistaking any vague clues and to be explicit and correct I carefully followed the inexplicable revision numbers in the Minutes of the MEPC Meeting, London, May 13-17, 2013: "Adopted amendments to resolution MEPC.214(63) 2012 Guidelines on survey and certification of the energy efficiency design index (EEDI), to add references to measuring sea conditions in accordance with ITTC Recommended Procedure 7.5-04-01-01.1 Speed and Power Trials Part 1; 2012 revision 1 or ISO 15016:2002.", and in the Final Report of the MEPC quoted before: "ITTC Recommended Procedure 7.5-04-01-01.2 Speed and Power Trials, part 2; 2012 revision 1".

And to my surprise I 'discovered' that in the meantime on the website of the 27th ITTC the reference to the document

> 7.5-04 -01-01.2: Analysis of Speed/Power Trial Data
> Pages 1 to 25, Effective Date 2012, Revision 00 (!).
> (Updated / Edited by the Specialists Committee on Powering
> of Ships in Service of the 27th ITTC. *Not* approved!)

had been replaced, so far without notice, at least to my knowledge, by the reference to the older document, unsatisfactory itself,

7.5-04 -01-01.2: Analysis of Speed/Power Trial Data
Page 1 to 11, Effective Date 2005, Revision 00
Updated / Edited by the Specialists Committee on Powering
Performance of the 24th ITTC. Approved by the 24th ITTC 2005.

My conclusion is that the Executive Committee 'finally' abandoned the STA procedure and backed the ITTC out of the procession following the emperor in his new clothes, thus making the way free for a rational, generally acceptable procedure.

RATIONAL APPROACH

A fundamental deficiency of all traditional methods, ISO 15016: 2002-06, ITTC 7.5-04-01-01.2 (2005) and STA-JIP, is that they all do *not* permit trustworthy to identify the current velocity and subsequently the hull speed through the water, particularly at ballast conditions. Any reference to the performance of the corresponding deeply submerged open water model propeller, as in most traditional methods, *e. g.*, ISO 15016:2002-06 and ITTC 7.5-04-01-01.2 (2005), and/or to the propulsive efficiency in model propulsion tests, as in the STA-JIP method, are unacceptable.

But this fundamental problem can be solved satisfactorily by extremely simple propeller (in behind condition!) and current conventions, the four parameters of which jointly to be identified from the data acquired by solving only one system of linear equations. Further conventions necessary to account for the wind and waves have to be introduced in the same fashion, as simple as possible, with only few parameters that can be identified reliably from the data acquired.

That the environmental influences can be identified only after the reliable identification of the hull speed through the water is self-evident practice of all experts. Only in the procedure marketed by MARIN the opposite is advocated, maybe due to the fact that it does not permit reliably to identify the current.

Decisions for one of 'equivalent' conventions, all resulting in residua within the confidence interval of the data available, are possible only by additional conventions, as has been shown in detail in the delicate evaluations of the ANONYMA trials at two different trim settings, *i. e.* at two different nominal propeller submergences.

MONITORING OF PERFORMANCE

In conclusion it is noted that traditional trials are quite ineffective! Waiting for steady conditions to be established and ignoring the wealth of information available during the long intermediate quasi-steady states is an incredible waste in view of the present state of measuring and computing

techniques. And even more important, traditional trials are *not* useful for monitoring the powering performance under service conditions!

For the latter purpose quasi-steady trials have been advocated since the successful tests with the METEOR in the Greenland Sea in November 1988, subject of the international workshop '2nd INTERACTION Berlin `91' (int_rep.pdf). The corresponding method requiring *no* thrust measurements is under development only now, a basic exercise identifying some problems to be solved has already been published (mod_trial.pdf). In this connection the method of Torben Munk, marketed by his company Propulsion Dynamics (www.propulsiondynamics.com) founded in 2002, is of interest.

If trials are (to be) performed at off contract conditions and the data at hand do *not* permit to extrapolate to the contract conditions, *i. e.* do *not* permit to identify the parameters of interest, predictions based on prior data and/or parameters have to be agreed and relied upon. The 'disadvantage' of the pertinent conventions is that the resulting predictions cannot be proved during the 'acceptance' trials, but they may be proved during the subsequent journeys using a conventional monitoring method to be developed and to be agreed upon, *i. e.* to be standardised. At this stage the question arises: Why not contract monitoring under service conditions following the 'acceptance' trials?

THE CONSEQUENCE

In view of the deficiencies of the two methods mentioned by the IMO MEPC in its Final Report 65-22 it is concluded, that the only reasonable consequence of the enduring unacceptable situation is *promptly* to develop and to agree on a revised, generally acceptable, lasting edition of ISO 15016 concerning not only trials, but monitoring of ship powering performance as well, meeting theoretical, contractual and legal standards and requirements.

In order to arrive at this standard all groups concerned have to be included, *not* only naval architects at model basins and ship yards, but ship owners as well as hydrodynamicists looking for trustworthy proofs of their numerical predictions and, last but not least, experts in standardisation.

Standards organisations, as *e. g.*, DIN and ISO, claim to pioneer innovations, but their rules to admit on their working groups only representatives of 'pressure groups', often retired colleagues, tend to perpetuate the current, deplorable state of practice trapped in the past, hardly accounting for the current state of research, as I have experienced even in case of the fundamental standard on 'Quantities' (din_raw_draft.pdf).

REFERENCES

All recent work of the author related to the subject of ship powering trials and monitoring is being published continuously on his website www.m-schmiechen.de, the most recent work and pertinent discussions under the title 'From METEOR1988 to ANONYMA 2013' in the Section 'News on ship powering trials' (news_trl.htm).

All files (xxxx.yyy) annotated on my website may also be directly accessed via links http://www.m-schmiechen.homepage.t-online.de/HomepageClassic01/xxxx.yyy, the present paper also on http://www.hansa-online.de .

MS 20.08.2014 10:05 h

ISO 15016: On the current revision

----- Original Message -----
From: "Michael Schmiechen" <m.schm@t-online.de>
To: "SC 6 / TC 8 / ISO" <customerservice@iso.org>
Cc: "Kuniharu Nakatake" <nakatake@aqua.plala.or.jp>; "Frank Dau" <nsmt@din.de>
Sent: Saturday, November 16, 2013 6:49 PM
Subject: Fw: Revision of ISO 15016

Prof. Michael Schmiechen
retired Deputy Director of
VWS, Berlin Model Basin

ISO / TC 8 / SC 6

Dear colleagues,

please find forwarded a note originally addressed to Tsuyoshi Ishiguro of the Japan Marine United Corporation triggered by his recent presentation on the 'Current status on revision work of ISO15016 for EEDI verification - Conduct and analysis procedure of speed trial – ' at the 7th Asian Shipbuilding Experts' Forum, November 7th to 8th, 2013, in Kobe.

With many thanks for your kind attention and for forwarding my remarks to the colleagues in charge of the project yours,

Michael Schmiechen.

PS. Since one week I try in vain to find out the e-mail address of Tsuyoshi Ishiguro! So he himself has not yet received my mail directly, maybe via other channels.

----- Original Message -----
From: "Michael Schmiechen" <m.schm@t-online.de>
To: "Tsuyoshi Ishiguro" <tsuyoshi_ishiguro@ihimu.ihi.co.jp>
Cc: "Andreas I. Chrysostomou" <info@imo.org>; "Frank Dau" <nsmt@din.de>; "Gerhard Strasser" <prof.dr.g.strasser@sva.at>; "Giulio Gennaro" <giulio.gennaro@sinm.it>; "Kinya Tamura" <tamurak@jf6.so-net.ne.jp>; "Klaus Wagner" <IKWAG@web.de>; "Kuniharu Nakatake" <nakatake@aqua.plala.or.jp>; "Mitsuhiro Abe" <mitsuhiro.abe@pep.ne.jp>; "Naoji Toki" <toki.naoji.mz@ehime-u.ac.jp>; "Stig Sand" <ss@force.dk>
Sent: Thursday, November 14, 2013 10:24 PM

Subject: Revision of ISO 15016

Dear Mr. Ishiguro,

your 'very' recent presentation on the 'Current status on revision work of ISO15016 for EEDI verification - Conduct and analysis procedure of speed trial – ' at the 7th Asian Shipbuilding Experts' Forum November 7th to 8th, 2013, in Kobe has been forwarded to me by a younger colleague.

According to my first impression your account of the current status 'nicely' links up with my current work and publications related to ship powering trials.

Need for revision since 1998

For ready reference I attach the two most recent ones before I knew the current status of the work, in the subtitle explicitly voting 'for a revised, generally acceptable, lasting edition of ISO 15016, concerning not only trials, but also monitoring of the powering performance, meeting theoretical, contractual and legal standards and requirements'.

As I have stated repeatedly, the situation reminds me of the time when railway gauges were selected differently for protective reasons. Not only MARIN is following that stone-age doctrine, but HSVA and SSPA are still working along that line on a joint project to be presented next week at the Annual Meeting of STG here at Berlin.

As a matter of fact I have promoted the revision of ISO 15016 since 1998, long before it became a standard, as documented by the Japan Marine Standards Organisation (JMSA) under ISO/TC8/SC9/WG2/N28 dated 1998-06-23. All the correspondence with Prof. Ikehata, the convener at that time, is 'of course' documented on my website.

Current work on rational procedure

Please find all my current work and related discussions documented also on my website www.m-schmiechen.de, in the Section 'News on ship powering trials' in reverse order under the heading 'From METEOR 1988 to ANONYMA 2013 and further'.

Following my analyses of the ANONYMA trials, documented in every detail (!), I have published the draft of a review of the development of the rational theory of trials with the unmistakable title 'Future Ship Powering Tri-

als Now'. The only substantial written discussion yet is that by Dott. Giulio Gennaro of Genova.

The section 4.3.4 'The Emperor's new Clothes' of that draft, added after the draft was 'finished', provides a rigorous criticism of the STA method aggressively marketed (!) and promoted 'otherwise' by MARIN and followed by a procession of 'specialists', certainly not 'experts', surprisingly without causing any serious professional discussions.

'ITTC 2012 Guidelines' withdrawn

But finally I have convinced the Chairman of the Executive Committee of ITTC to back out of that procession and to abandon the 'ITTC 2012 Guidelines', 'produced' by the ITTC SC PSS and prematurely forwarded to the IMO MEPC by the Executive Committee These Guidelines and consequently the basis of the joint effort of ISO and ITTC are obsolete, not only theoretically, but 'legally' as well!

I shall not repeat here, what I have written in many papers. Please note my latest, rather concise presentation of the essentials, the English translation of which I also append for ready reference as well. Here I just state that the times of Kinya Tamura and Frits Mennen are gone. Their concepts concerning fundamental aspects and problems are not adequate for our problems and purposes at hand.

In particular this concerns the reliable identification of the current. Any responsible expert immediately stops any further analysis, if that problem is not (to be) solved satisfactorily. What I saw on your ppt-presentation is unacceptable. The community 'simply' cannot afford to repeat the ISO 15015: 2002-06 mistakes and wait further decades for the urgently necessary progress.

Time table unrealistic

The time table drafted, evidently under the pressure of the MEPC after I had alerted its Chairman, is definitely too 'narrow'. Please keep in mind, that the 27th ITTC, only that may approve any Guidelines or more likely not (!), will take place at Copenhagen not before September 2014.

And further note, that changes in thinking inherited from our great-grandfathers and still indoctrinated at 'schools' worldwide take much longer. It is twenty-five years since my tests with METEOR, since my rational interpretation of the naive concepts of hull-propeller interaction

on full scale, but naval architect still do not take advantage, at least on model scale.

What we urgently need is 'Future now!', conventions that meet all the requirements of all the groups concerned. Always remember: The most practical tool is a theory based on appropriate principles. 'Consistently' ignoring the state of research, not only in naval architecture, is a self-defeating strategy.

What needs to be done

With only little common sense all experts, not to be confused with 'specialists', know what needs to be done, what can be harmonised and what cannot be 'harmonised'. Evidently there are three or rather four systems of conventions clearly and cleanly to be distinguished, 'clare et distincte' as the Romans concisely said.

1. 'Pre-Processing': Conduct of trials, acquisition of data. Concerning this matter all existing conventions can easily be harmonised, differing only very little. But as many trials are performed at ballast conditions these conventions have to be augmented as the ANONYMA trials have drastically shown!

2. Objective, observer independent evaluation at the trials condition. This is the crucial problem, concerning which all traditional conventions in use are unacceptable. They all rely on unreliable prior data, selected 'as required' for the purpose at hand! This has been my central concern and my solution already described in 1998 has been successfully applied many times since.

And my repeated question is, how long will ship buyers accept the same people to provide the predictions, to conduct the trials and to analyse the data 'as well', and even setting the standards [addition 2014-01-19: to be met]! If you think about this situation, you will find it as ridiculous as any layman.

3. 'Post-Processing': Prediction (!!!) of powering at conditions differing from the trials conditions. In this case one 'has to use' prior data, if variations of trials conditions do not permit reliably to identify the relevant parameters. Concerning this point the 'competing' conventions may be harmonised as well, - if one does not prefer to follow my proposal and rely on objective monitoring under service conditions after the 'acceptance' trials.

MS 20.08.2014 10:05 h

4. Finally, monitoring of powering under service conditions. This very important point has already been mentioned under 3. Any standard not taking care of this fundamental problem is incomplete in my view! I have already published a preliminary exercise, demonstrating what needs to be accounted for.

[Addition 2014-01-19: Evidently this problem is subject of the separate standard ISO 19030 under development. Note my **Remark concerning related work on monitoring a**dded on 2014-01-15 on page 94.]

Specific contributions envisaged

Personally I am ready to contribute to further developments of ISO 15016. The first thing I shall do after November 22, right after the Annual Meeting of STG (Schiffbautechnische Gesellschaft) here at Berlin, will be independently to analyse the set of trials data provided as example with the update of ISO 15016.

I did this already fifteen (!) years ago with the earlier example, demonstrating that the ISO procedure was unacceptable, being inherently wrong, not adhering to first, simple, 'self-evident' principles of common sense.

Looking forward to your mail with trials data 'only', no 'prior' data of model tests or any other (!), I remain with my best regards to your colleagues on the Working Group yours,

Michael Schmiechen.

PS. Please make sure that all colleagues working on the revision of ISO 15016 not only receive, but read [2013-11-15 added: 'and understand'] (!) my remarks and maybe some of my papers and detailed analyses.

Addendum 2014-01-20

----- Original Message -----
From: "Michael Schmiechen" <m.schm@t-online.de>
To: "Tsuyoshi Ishiguro" <ishiguro-tsuyoshi@jmuc.co.jp>
Cc: "Kosei Hasegawa" <hasegawa@jstra.jp>;
"Kuniharu Nakatake" <nakatake@ja3.so-net.ne.jp>
Sent: Monday, January 20, 2014 1:56 PM
Subject: Contributing to work on ISO DIS 15016 and ISO CD 19030

Dear Ishiguro San,

since two months now I am waiting for any response on my request for the data of the example in the DIS 15016 for independent analysis. According to the rules of the game no answer is a well understood answer as well.

Thus, knowing the rules of ISO, I am currently applying to be authorised member of the DIN NSMT Working Groups contributing to the revision of the standard ISO 15016: 2003-06 and to the standard to-be ISO 19030.

This will give me the chance to perform the exercise outlined and necessary for the benefit of the standard ISO 15016 and contribute to the work on the evolving standard ISO 19030 on monitoring of the powering performance along the lines of my preliminary exercise documented in the 'Festschrift'.

In the meantime I have updated my 'Festschrift', (and I will continue to do so as appropriate,) distributed on the occasion of the Annnual Meeting of the Schiffbautechnische Gesellschaft, the current version always to to be found on my website www.m-schmiechen.de under 'News on ship powering trials'.

With season's greetings and kind regards
yours, Michael Schmiechen.

Michael Schmiechen, apl. Prof.
for Hydromechanical Systems,
retired Deputy Director of VWS,
the Berlin Model Basin.

MS 20.08.2014 10:05 h

Continued from front end-paper

SCOPE

Evidently there are three or rather four systems of conventions concerning the following 'operations' to be clearly and cleanly to be distinguished.

Conduct of trials and acquisition of data. Concerning this matter all existing conventions, differing only very little, may easily be harmonised. But as many trials are performed at ballast conditions these conventions have to be augmented as the ANONYMA trials have drastically shown!

Objective, observer independent evaluation at the trials condition. This is the crucial problem, concerning which all traditional conventions in use are unacceptable. They all rely on unreliable prior data, selected 'as required' for the purposes at hand! This is the problem I have been concerned with.

Prediction (!) of powering at conditions differing from the trials conditions. If this is requested, one 'has to' use prior data, if variations of trials conditions do not permit reliably to identify the relevant parameters. Concerning this point the 'competing' conventions may also be harmonised as well, - if one does not prefer to follow my proposal and rely on objective monitoring under service conditions after the 'acceptance' trials.

The monitoring of powering at service conditions. Any standard not taking care of this is incomplete! I have already published a preliminary exercise, demonstrating the problems encountered.

READERS

The following is basically a 'letter' addressed to my colleagues and my students, as well as to whom it may or must concern, governing bodies and pertinent committees of the ITTC, ISO and IMO in particular.

AUTHOR

In 1997 apl. Professor Michael Schmiechen retired as Deputy Director, Head of Research and Development, from the Versuchsanstalt für Wasserbau und Schiffbau (VWS), the Berlin Model Basin, and was released from the duty to lecture on Hydro-mechanical Systems at the Institut für Schiffs- und Meeres-Technik (ISM), Technische Universität Berlin (TUB). But since then he has continued lecturing at ISM until 2011 and to promote his ideas around the world.

Warning!

Reading these papers endangers Your principles!

"You cannot have a theory without principles.
'Principles' is another name for 'prejudices'."
Mark Twain: 'The Disappearance of Literature'
Speech, 20 November 1900.