An explanatory mail to Dr.-Ing. Hollenbach at HSVA concerning the first Post ANONYMA Trial Evaluations PATE_01.1 to .3 and PATE_02.1 to .2 with PATE_00.2

The following e-mail is the translation of an extended explanation of my independent evaluations of traditional powering trials with two sister-ships in the East China Sea. The provision of the basic mean values, being objects of a joint HSVA / SSPA project, and the permission to publish the results granted by Dr. Hollenbach at HSVA are gratefully acknowledged.

As usual a translation is instrumental in clarifying arguments, though in this case only marginal changes and few additions have been necessary. The 'final' versions of the PATEs under discussion together with my complete related correspondence with Dr. Hollenbach, of 'cause' in German, are to be found on my website www.m-schmiechen.de under 'News on ship powering trials'.

From: Michael Schmiechen
Sent: Wednesday, June 4, 2014 3:12 PM
To: Uwe Hollenbach
Cc: Klaus Wagner ; Friedrich Mewis ; Stefan Krüger ;
Bettar Moctar ; Som D. Sharma
Subject: Our correspondence on PATE_01 and _02 cont'd

Dear Dr. Hollenbach,

during further, more 'physical' home work I had plenty of time to ponder the comparisons of our evaluations of the powering trials with two sister ships in the East China Sea.

In advance!

My correspondence with Dr. Klaus Wagner at Rostock is much more extended and detailed than ours. It is as intense as my style of working, at least so far. Between my drafts and results and his responses delays of two months never occur!

Since our first meeting on occasion of my 2nd INTERACTION Berlin '91 he is not only one of the few colleagues always interested in the development of my ideas, but he has often taken active part in that development.

And since my retirement from VWS, the Berlin Model Basin, Dr. Wagner has played the role of my lector, always creative and prompt. And for this service I am most thankful. During all my professional life I have always had my drafts scrutinised by lectors before 'delivery', so this mail.

Statistics over all double runs

But now to the subject itself. Two worlds can in fact not differ more than ours! Without referring to details I noticed in your remarks, that you always consider individual double runs. But I will not, I cannot follow you onto this 'level'. According to my long, pertinent, painful experience the analysis of single runs is not meaningful due to the omnipresent random disturbances due to causes of 'any' type.

Therefore I always jointly consider all double runs available, or selected for ‘good’, qualified reasons. And I analyse the residua with utmost care concerning deviations from normal distributions. This way I check the adequacy of my conventions adopted and at the same time the applicability of the elementary theory of samples.

Friedrich Mewis occasionally mentioned that I am evaluating trials like a physicist. And of course he was right! I am in fact doing it as a ‘mechanist’ according to the current state of the art and not according to the traditional practice of naval architects. I have repeatedly stated that there are too many naval architects in ship model basins.

They ‘believe’ to know, what the output ‘should’ be, and there are too few theoreticians, who ‘know’ how to ‘arrive’ professionally at the output. The ritual repetition of the misunderstood rules of the elementary theory of samples is not sufficient for the difficult problems at hand.

Analysis of ‘raw’ data

My procedure is already necessary in view of the fact, that I myself could not scrutinise and analyse the basic data, as has been possible in case of the ANONYMA trials. ‘Mean’ values of unknown origin I am always using only with extreme care.

As I have experienced during the evaluation of the METEOR model test results, and just now during the continued analysis of my quasi-steady ‘model’ test of 1986, in cases of doubt not more or less obscure mean values are relevant, but stationary values, extrema! Even at ballast conditions the smallest accelerations upset the energy, alias power balances.

Balances of partial energies

Here comes the repetition of another fundamental statement: I am not considering momentum, alias ‘force’ balances, but following Lagrange I consider balances of partial energies, alias power balances. As a consequence a number of problems encountered in the traditional approach do ‘principally’ not exist in this approach! In particular the propulsive efficiency is not at all necessary for the analysis of traditional trials data.

This is in contrast to the ‘ITTC 2012 Guideline’, not yet approved by the Full Conference, but already ‘universally’ accepted. In this Guideline the propulsive efficiency ‘figures’ as a fundamental ‘input’, surprisingly not even occurring in the list of symbols and ‘forgetting’ about its ‘origin’, evidently playing the role of a joker pulled out of the sleeve. As I have explained earlier in my view the name ‘direct power method’ for this procedure is the most blatant desinformation possible.

Supplied power first

Due to the usually relatively small variation of the propeller loading during trials the analysis of the data can be separated into two partial problems. The stable solution of each of them is simply obtained as solution of a system of linear equations, provided one uses numerical methods adequate for solving more or less ill-conditioned systems of equations.

As appropriate I have first analysed for the power supplied and thus jointly identified the current and 'calibrated' the propeller, full scale (!) under trials conditions (!), i. e. at the extremely small nominal submergence at the ballast condition and in the prevailing sea state.

Checking my results PATE_01_1 to _3, based on three different sub-jsets of double runs, I notice, that the propeller power characteristics and currents I have identified are 'practically' independent of the number of double runs accounted for. Using a traditional method, known to be error prone, you have identified considerably different values of the current, and thus the propeller characteristic you identified differs also considerably from mine.

In case of PATE_02 at more favourable environmental conditions the current values we have identified are nearly identical and thus the propeller characteristics. And the latter are in very close agreement with the characteristic I have identified before for the sister ship (PATEs_01).

Current: 'fundamental' solution

Your remark that my method to identify the current is more elegant than that of Peter Schenzle, HSVA is still using, is a typical 'understatement' of naval architects, who do not 'want' to understand the problem and its solution. You may want it or not, my axiomatic interpretation of the concept is in fact the only meaningful. It 'works' without any expensive and delicate devices and without any extra calibration at any wind and waves condition.

Even Dr. Klaus Wagner and Dr. Giulio Gennaro at Genova in the depths of their hearts felt that my solution was provisional, some day to be replaced by logs to be developed using 'advanced' techniques available. But any these of logs suffers from the same fundamental deficiency as any of the 'simple' thrust meters invented by dilettantes and developed in wasteful 'research' projects. Even if they would 'function' some day, neither the thrust meters nor the logs could be calibrated! But what sort of 'measuring' systems are they, if they cannot be calibrated? Would you consider buying any of them?

Power required

After having jointly identified the current and the propeller power characteristic in behind condition I have analysed the power required, in order to reduce the data to the nominal (!) no wind and no wave condition defined.

That my very crude model of the power required used in the case under consideration and others has repeatedly been felt inadequate by Dr. Wagner and Dr. Gennaro. But both admitted that the [only crudely 'estimated'] few data often available do not permit more than 'to nail the egg onto the rail', as Columbus did before.

Further detailed comparison of the data acquired during the trials with the two two sister ships may provide deeper insights and further 'results'. Thus in case of PATE_02 I have used a parameter of the required power identified before in PATE_01; see below.

Analyses of significance

To answer your detailed questions I will have to study the confidence ranges, which I have always determined and reported. I admit that my loose, qualita-

tive, marine engineers remarks concerning the quality of results and their agreement based on those ranges are certainly too vague to meet the ‘standards’ and claims of naval architects.

In case of the ANONYMA trials I had the confidence ranges of the average values available, based on the raw data scrutinised before. I am looking forward to your analyses, that must be basic constituents of your joint research project with SSPA.

With my thanks for the permit to publish my analyses and their results I ask you, kindly to excuse this repeated attempt to explain aspects I consider essential and, at the same time, that you also publish all details of your evaluations. Only this will permit all interested colleagues, among them Stefan Krüger and Bettar el Moctat, to arrive at their own judgement.

Surprising coincidence

Again and again I have explicitly stated, that the values of the concepts constituted and interpreted by my conventions need in principle not to coincide with the values of the corresponding traditionally interpreted concepts. For linking up with prior experience the coincidence is of course ‘useful’, but maybe misleading.

The surprising, nearly perfect coincidence of our final results, despite my restraint on the essentials, avoiding naval architectural folklore and ‘thousands’ of little corrections, will cause and require even hard-boiled naval architects to think twice.

How you arrived from your defective intermediate values [in case of PATE_01] at you final results and came up with the idea that I have tuned my results with your results, you will certainly explain to me and our colleagues occasionally.

Who is afraid of the wicked guy?

Your opinion expressed earlier, that clients of HSVA may be shied away by mentioning my name, frightened by my naked pragmatism, is hard to believe, maybe even for yourself. Frightened for well understood reasons are my colleagues at some model basins.

For clients everything is ‘the same’. As long as they accept the same ‘people’ to provide the predictions and their confirmations ‘as well’, they want to be cheated or want to cheat IMO in proving to conform to the required EEDI.

Acceptable standards

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Since my Schiffstechnik and STG papers of 1980 it is known that acceptable and lasting conventions are nothing else but axiomatic systems. And that their construction should not be left to naval architects, but experts in formal languages.

Consequently I have asked such experts [also in connection with other, fundamental standards], but so far without success. Because: ‘Everybody is concerned with himself and his own problems, only I am concerned with myself and my own problems.’

But everybody interested will admit, that my procedure is very transparent and, as the examples show, is objective, i. e. independent of the ‘observer’, of the person in charge of the evaluation. It depends on very few, ‘self’-evident conventions, and, as it must (!) be, does not depend on any further prior knowledge, any prior data selected *ad hoc* (!) and data derived from model tests suffering from the lack of similarity of flow conditions, in particular without values of the propulsive efficiency.

My procedure, as far as I have developed it so far, thus meets the prerequisites and requirements of a reasonable, acceptable standard, as I last noted in my HANSA paper of 2013. And for that reason I repeat my publicly stated and now even more solidly founded conviction, that ITTC, ISO and IMO in the ‘wake‘ of MARIN, the emperor in his new clothes, the ‘unbelievable’ STAimo method, obstruct the urgently necessary rationalisation for at least the next decade.

Further developments

Personally I shall most likely not witness the end of this obstruction. But I am confident that young colleagues will pick up my efficient rational xxx and develop them further. Michiel Verhulst and Patrick Hooijmans at Wageningen (!) are doing that already for a long time, with explicit acknowledgement of my pioneering work.

They do that in view of extremely efficient trials and monitoring requiring no thrust measurements, that is much simpler than my very ambitious METEOR project in 1988. "But [as] Jesus said unto them: A prophet is not without honour save in his own country, and in his own house" (Matthew 13, 57), 'of course' no such research and development efforts take place in Germany [except for my own, results to be presented at the forthcoming 27th ITTC at Copenhagen].

Quasi-steady trials and monitoring

How the propulsive efficiency can be reliably identified based on quasi-steady trials without thrust measurements (!) I have just demonstrated using the data of my 'model' test of 1986. So there is no need to pull the joker out of the sleeve.

The problem in this case is the reliable identification of the current. The solution is possible as before, if only the steady states during the quasi-steady test are determined and analysed.

Critical discussion

I did not lecture over forty years professional problem solving in 'treating' hydro-mechanical systems to let the dilettante ITTC 2012 Guideline 'pass' without comment.

Under Lerbs, Otto Grim, Odo Krappinger, as well as Fritz Horn, Hans Amtsberg and Siegfried Schuster such a sloppy report would never have left a model basin.

And Hans Edstrand, former director of SSPA, would have fired each of the members of the ITTC Specialists Committee on Powering of Ships in Service (SC PSS) individually. His credo was that Specialists had nothing to do at the

Conference of Tank Superintendents (!), who still knew the problems under discussion and to be solved by them.

I have proposed the same 'procedure' to the Chairman of the Executive Committee after the members of the PSS Specialists Committee on occasion of one of their expensive meetings came up with the finding for him (!), that my procedure for the evaluation of traditional trials requires thrust measurements.

Despite the detailed documentation of the exact opposite, repeated since 1998 to meet any taste, non of the members, including yourself, prevented the blatant des-information of the Chairman.

Credibility ahoy!

Subsequently I have observed with interest, that the untenable ITTC 2012 Guideline, prematurely forwarded to IMO contrary to the Rules of ITTC, for a while vanished from the website of ITTC, to reappear only shortly later, and that in the meantime ITTC suddenly has a new Chairman! I wonder how he will sort out the complete mess into which the SC PSS and his predecessor have produced.

That the MARIN inspired ITTC 2012 Guideline will not only be adopted by IMO, but part of the revised standard ISO 15016 will in the meantime have been approved by all national groups, including the German consisting of you alone (?). My request to provide the example included in the standard for independent scrutiny as in 1998, could not been granted due to the alleged lack of such an example.

Rules of the game

Subsequent to my detailed draft of a new edition of the fundamental standard DIN 1313 'Grössen' ('Magnitudes', alias 'Quantities') and its emotional, unqualified xxx by the authors of its current version, some of them logicians at my age, I now not only understand, how standards are 'made', but why it is done that way.

The rules of DIN and of ISO, to establish a consensus of interested groups, tend to perpetuate the current state of practice and thus to delay or even to inhibit progress. Individual experts are explicitly excluded and my correspondence with DIN is strictly confidential!

'Accordingly' my website has been regularly checked for 'illegal' publications. I even had to delete not only links, but the corresponding files from my website. But my draft a well as related discussions of the interesting, fundamental project and the documentation of the whole 'history', whatever DIN could not 'prohibit', is to be found on my website.

With my best wishes for Pentecost, 'Pfingsten, das liebliche Fest', as Goethe started his 'pretty' obscene 'Reinicke Fuchs',
yours Michael Schmiechen.

