INSTITUT FOR SCHIFFBAU DER UNIVERSITAT HAMBURG

Bericht Nr. 376

I. Georg-Weinblum-Gedächtnis-Vorlesung

Sip Theory, Ship Design and Georg Weinblum

gehalten von

J. V. Wehausen, Berkeley

am 22. November 1978 in Berlin

und

am 29. März 1979 in Washington, D.C.

Hamburg, Juni 1979
Ship Theory, Ship Design and Georg Weinblum

by

J. V. Wehausen
If one had to categorize Georg Weinblum among naval architects, it would be fair, I believe, to call him a ship theorist. He himself liked the term "ship theory", his own research was primarily devoted to this, he supported it among others, and one of his chief goals as a teacher was to try to provide prospective naval architects with a good understanding of the fundamentals of their profession.

I have been rereading many of Georg Weinblum's papers during recent months and one aspect that has struck me is his constant concern for the application of ship theory to ship design. In almost every paper I have looked at, some part of it is devoted to its implications for design. Some are almost completely oriented in this direction.

This isn't surprising, I suppose. Georg Weinblum was serious about his profession, in fact, more than that, he was enthusiastic about it and he was convinced that the study of ship theory could and would result in the improvement of ships. It is just this last point that I should like to examine. Does ship theory really play an important role in ship design, and if not, could it? This is a question that any ship theorist must ask himself from time to time.

Note that I am not asking if ship design needs ship theory. It seems clear, fairly clear anyway, that ship designers could get along by the method of trial and error, of cut and try. In a field like naval architecture, with a long history behind it, one has the option of starting any more-or-less conventional design problem from a safe position. The designer may indulge his spirit of creativity by making small modifications of a known acceptable design. Whether well-founded or not, they do not usually result in disaster, and if the consequences are observed and recorded, these small changes contribute to the advancement of the art of design. I suppose that much of the progress in technologies of all sorts has been a result of such an empirical process. Of course, there are examples where a "small" change does result in a disaster, but these are also learned and avoided.

If one doesn't actually need ship theory, where then does it fit in? One obvious place is in those design situations where there is no tradition to start from, and another is where something more that a "small" modification is required to achieve a real advance in design practice. Let us look at some examples.

Our field is particularly rich these days in examples of the first kind, those situations where one cannot rely upon a well tested body of experience, codified in rules, but only upon one's intuition,
model tests and the laws of mechanics. I am thinking, of course, of the design of the various off-shore platforms, some floating, some fixed after they are towed into position. Their diversity is enormous, as one can easily see by thumbing through some issues of Ocean Industry, and they attest to the inventiveness of their designers. But this ingenuity in conception needs to be supported either by a tradition of experience or by calculations based upon the laws of mechanics. Since the former is lacking, reliance must be placed upon the latter.

To be faced with such problems is, of course, a frightening prospect. One is suddenly painfully aware of the limitations in the various theoretical developments. For example, in calculating hydrodynamic forces, may one really neglect viscosity or is it just that we don't know what else to do, are linearized approximations adequate, what sort of spectrum should one assume for the incident waves, what does a 100-year wave really signify, etc? A particular problem can no longer be set aside because it is "messy" or not a good academic research problem. It must be accepted as it has been presented by circumstances, although it is indeed prudent to confirm that the "right" problem has been presented if it has been formulated by someone else.

Still, decisions have to be made and calculations carried through, and it is just the academic research based upon clear-cut problems that must provide the background and basis for dealing with the more complex problems. Indeed, unless one can examine simple problems first, one cannot usually analyze the complicated problems presented to an engineer. A direct attack upon a too complicated problem may be in danger of not uncovering the underlying principles.

In recent years I suppose that more than half of our students have ended up doing ocean engineering rather than naval architecture in its strict sense. However, it is essentially ship theory that they apply, for if they have understood, for example, the uses of potential theory in calculating the motion of a ship in a seaway, they also know how to deal with floating bodies of different configurations. Indeed, such computations have become close to commonplace. And even more esoteric ones, such as the second-order drift forces acting on floating bodies moored in waves, occur as part of the design process. The number of such examples can be increased considerably and they cover most of ship hydrodynamics. Moreover, parallel and more pressing ones exist in ship-structure theory.

Does this mean that every naval architect or ocean engineer must be an expert in the mathematics of the equations of fluid dynamics and structures? I think not. Just as one can make effective use of the telephone without understanding, or even being very interested in how it works, one can make effective use of ship theory without digging into the details. Georg Weinblum's papers on ship motions illustrate the possibilities. In an early paper [Z. VDI 78 (1934), 1373-79] concerning the motion of a ship in a seaway he has emphasized that in using the linearized equations of motion, it is important to take into account the added-mass terms derived from the hydrodynamic force, as well as the damping terms. The actual calculation of either of these terms is, of course, difficult and methods are still being developed for
doing it, especially in three dimensions. However, once one knows that such terms must be taken into account, one may try to find their values by other means than mathematical calculation, by model tests for example. Ship theory has still played an essential role by identifying quantities easily overlooked in a more elementary analysis. Indeed they were overlooked by Krylov in his classic papers on ship motions.

In my opinion, applications of ship theory of this sort, i.e. ones that clarify the nature of the underlying physics and point out the appropriate equations to express it, are at least as important as the specific calculations they may lead to. It is a consequence of this point of view that the education of naval architects should include enough ship theory so that these fundamental aspects are always within their grasp. Even though detailed calculations may be left to specialists, the designer needs to recognize when certain physical phenomena are important, how they show up in the calculations, and to know that they can indeed be calculated by specialists. These were essentially Georg Weinblum's pedagogical principles. He introduced them at the Institut für Schiffbau in Hamburg and also at the University of California in Berkeley. Since then they have spread worldwide. The aim is not to make every naval architect a ship theorist, but rather to instruct him to recognize when and where ship theory is useful and how to make use of it when it is.

I should like to think that wave-resistance theory has played a role in practical design similar to ship-motion theory, especially since Georg Weinblum devoted so much effort to it, but I believe that I would be overstating the case if I claimed this. The insights that wave-resistance theory, in the form of Michell's integral, could have offered to ship design had already been discovered empirically through model-series testing. Furthermore, it has not proved useful as a computational procedure for predicting wave resistance; it is simply not accurate enough for ships of normal dimensions. For ship motions linearized theory seems to work well enough, for wave resistance it does not. Perhaps one must be content to call it bad luck. Nature is not always kind.

This brings us to the second category of situations where ship theory can make a contribution to ship design, those where small modifications of existing designs will not disclose a possible significant advance. Here again we have a splendid example, and one in which Georg Weinblum played a part, the bulbous bow. It doesn't seem reasonable to assume that the large bulbs used on contemporary ships could have been developed by a step-by-step process from the small bulbs used earlier. They are too far from the norm of earlier days and would have offended the aesthetic sense and probably the common sense of almost any practicing naval architect. Something more fundamental was needed that would allow one to get beyond this barrier of tradition. And indeed it was the analytical investigations of Inui, Takahei and Kumano followed by model tests, that showed the substantial improvements that could be obtained with large bulbs. However, in this case the analytical investigation had to come first and to suggest that a radical revision of the conventional ideas concerning bulbs was necessary. Of course, once Inui and his colleagues had completed these pioneering investigations, others could begin to
I have mentioned that Georg Weinblum played some part in the development of bulbs. Because of his interest in design, he had concerned himself for most of his professional life with ships of minimum wave resistance. In 1934 both he and Wigley, more or less simultaneously and independently, published papers whose purpose was to explain the working of the bulbous bow. In each paper part of the analysis was devoted to the optimum size and position of the bulb. In my opinion either one of these two could have discovered the efficacy of large bulbs at this time. Better computing machines would have helped, of course, but a more serious handicap, I believe, was the technical climate. A serious suggestion at that time that bulbs of a type now commonplace should be used would not likely have fallen upon sympathetic ears. Indeed, I imagine that 35 years later it required some courage on the part of Inui to make this suggestion.

The story of the bulbous bow cannot be terminated, of course, without mentioning that remarkable bit of serendipity, the even greater and unanticipated success of the bulbous bow for ships in ballast at low speeds. As everyone now knows, this was finally explained by the ingenious experimental investigations of S. D. Sharma, described in a paper by Eckert and Sharma, [Jbuch STG, 64(1970), 129-171, esp. pp. 140-158]. However, can one really attribute this astonishing advance in ship design to an application of ship theory? In the narrowest sense, certainly not. The theoretical computations of Inui and his colleagues certainly did not include the possibility of eliminating wave breaking at low speeds by ships in ballast, nor would they have supported the use of a large protruding bow at low speeds. On the other hand, without these calculations and the consequent introduction of large bulbs, it is unlikely that their effectiveness in such situations could have been discovered. But perhaps in this case we should be content with our good fortune and not inquire too deeply into its origins.

Are there other examples where a theoretical insight has opened up new design possibilities not likely to have been discovered by taking only small steps? I don't think that one should anticipate finding many of these, but at least one more comes to mind, supercavitating propellers. Without Marshall Tulin's initial development of a linearized theory for cavitating hydrofoils and of the consequent foil shapes of least drag/lift ratio, it is unlikely that the idea of designing supercavitating propellers would have presented itself. Propellers behaving in this fashion would certainly have occurred, but Tulin's discoveries allowed the presence of a cavity to be included in the design and the performance to be optimized under this condition.

In the above remarks, I have restricted myself to accomplishments of ship theory that have taken place in relatively recent times, but ship-theoretical calculations that have become
commonplace are still ship theory. Even the hydrostatic calculations were once revolutionary. The story of Archimedes' excitement upon discovering a hydrostatic law is too well known to need repeating. Those developments in ship theory that prove useful in design will certainly, in the future, become as much a part of a naval architect's tool kit as hydrostatic calculations or Froude's Hypothesis are today. And this is, of course, what Georg Weinblum foresaw and planned for when he proposed a modernization of the curriculum of study for naval architects that would allow them to grasp the significance of the most recent developments in ship theory. In the years to come this may prove to be his greatest contribution to Naval Architecture.