Catalyst

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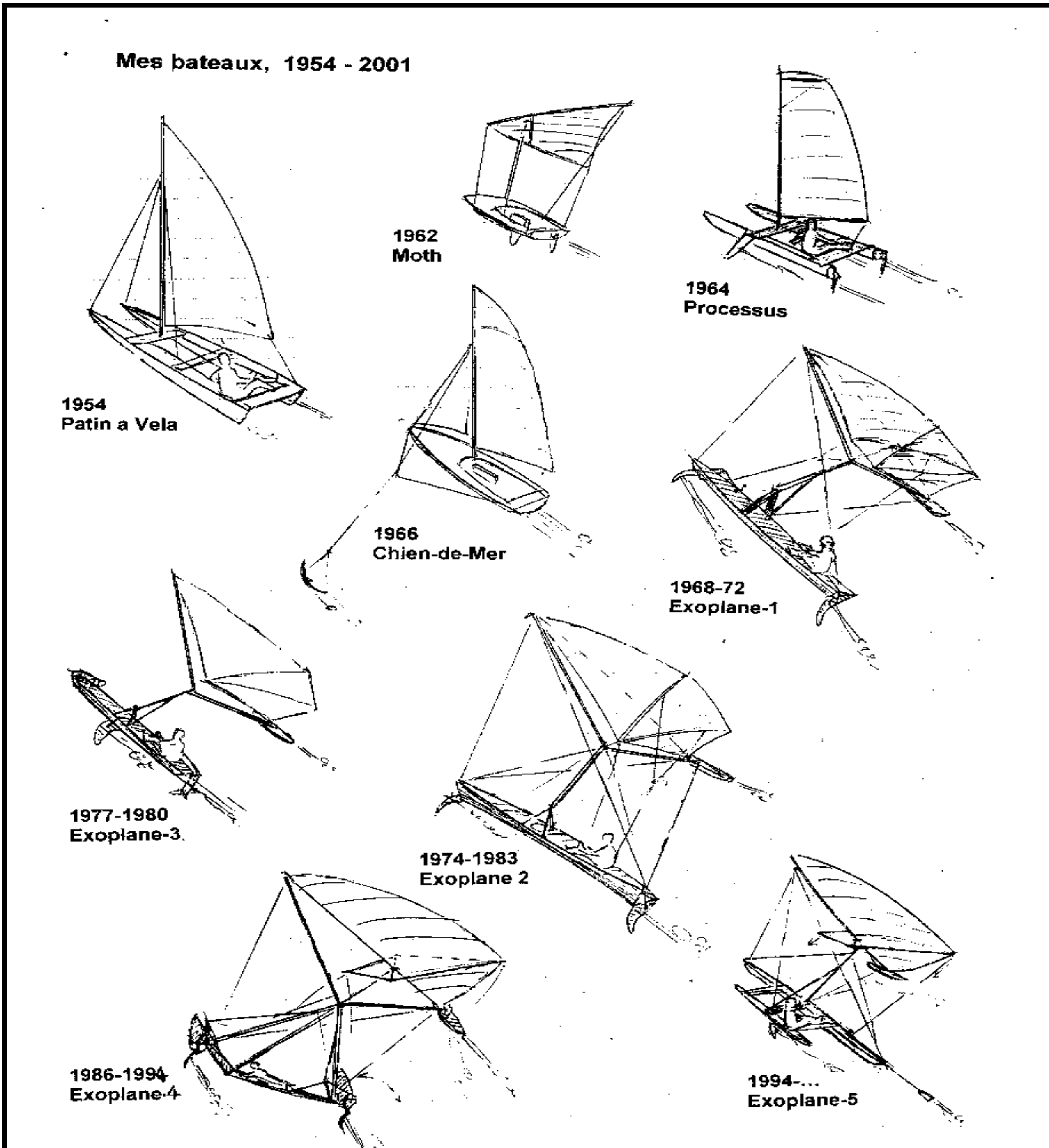
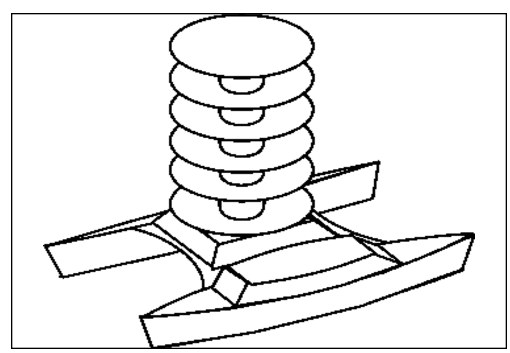
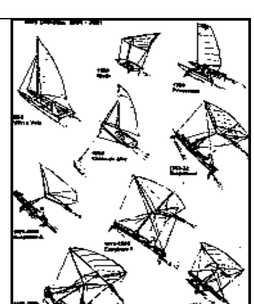


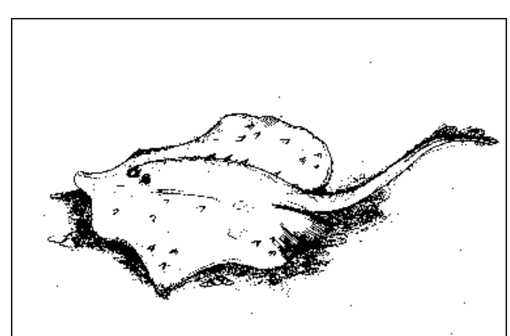


On the Cover –

Didier Costes' boats

(See page 19)





Catalyst

Journal of the
Amateur Yacht Research Society

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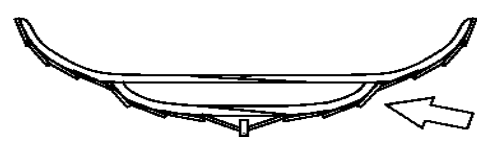
Meginhufers and other antiquities

I spent most of July in Norway, chasing the midnight sun and in passing spending a fair amount of time in Norway's maritime museums looking at the development history of the smaller Viking boats.

Now as most AYRS members will know, the Vikings rowed and sailed their boats and themselves over all of Northern Europe, and as far away as Newfoundland to the west and Russia and Constantinople to the east. Viking boats were lapstrake built, held together with wooden pegs or rivets. Originally just a skin with ribs, and thwarts at "gunwale" level, by the 9th century AD they had gained a "second layer" of ribs and upper planking, and the original thwarts served as beams under the decks. Which brings us to the *meginhufer*.

I'm told this term literally means "the strong plank", and is applied to what was once the top strake of the "lower boat". Frequently it was set at a sharp angle to the strake below it, and also to the one above, so they formed a notch in the hull skin. Thirty years ago, it was looked on as a curiosity, evidence of the conservatism of boatbuilders who put that plank like that because their fathers and grandfathers had put that plank like that, right back to the time when it was the top strake of an open boat. But more recently archaeologists have built replicas of these old boats and are finding that they perform better than they ought under sail; and their suspicions are turning to look at the *meginhufer*.



When moving fast through the water, that notch in the bilge appears to make a major difference to the flow of water around the hull. Certainly there is evidence that it sheds a substantial vortex – photographs of the wake show this quite clearly. What is less clear is whether it makes any contribution to the lateral resistance of the boat.

Now I know about the Megin jolle of Denmark – a 15ft (4.5m) sailing dinghy with a similar notch in each bilge – but I am wondering whether anybody out there has ever taken a measured look at this phenomenon, and come up with any conclusions. Certainly a notch instead of a projecting keel would be very useful to those people who sail in shallow waters, and though Phil Bolger's "box-keel" boats are mainly powerboats, there may be some experience there. American Seabright skiffs may also be possible sources of data.

It seems to me that this could be an interesting problem for someone to look at. How about it?

Simon Fishwick
AYRS Editor

Winds of Change 2001

A Rally for Innovative Craft

Bob & Gen Quinton

Friday the start day of the rally and people arriving with their trailers full of many and various parts that slowly began to form into strange boats that by now AYRS members are starting to take almost as commonplace. Of course this does not apply to the members of the host sailing club, the Royal Harwich Yacht Club. Eyes widened as the packages were put together. Chris Evans had stolen a march on the majority already having had the opportunity to sail his Foiler 21 "*Boomerang*" during the preceding week. Philip Middleton had also been on the scene for a couple of days being helped by Chris to assemble "*Triton's Chariot*" maybe better known as the "*Big Yellow One*".

The River Orwell is rated as one of three of the most beautiful rivers in the UK, and we can only concur with this opinion, though we might be biased. The week before the meeting was in fact absolutely ideal weather-wise for feeling the boat out and tuning the odd bits of string. Chris and Philip, having worked up some fairly good speeds with the Bruce foils deployed, felt things out a bit too literally by hitting a mud bank with one of the Bruce foils; but this did not of course faze the intrepid Chris as he had Plan B lined up. In his massive camper, or rather on the roof of same, there were two T-foils ready to be tried out.

Next to arrive was another Foiler 21 owned by Patrick Mayne. Again he was quickly into assembly mode. No sooner had he got ready than Slade Penyore pulled up with aluminium rails and sponsons tethered atop his car and deflated hulls in the boot, showing the useful transportability of his Catapult. Close behind him came new faces from Newcastle with a Ketterman trifoiler - Gordon Stanger-Leathes with his daughter Lucy. No sooner had he come though but Gordon was away again telling us he was driving way back up the A12/A14 that evening to fetch his elder daughter Ali from Peterborough. During the course of the weekend both these girls spent several hours sailing with their dad.

BOOMERANG (*Foiler 21) and* TRITON'S CHARIOT

The Duncan's Lateen-rigged Wayfarer

Things were hotting up, but the owners were extremely civilised in working with and round each other so that everyone had sufficient space to assemble. More new faces rather arriving from Totnes, Devon, were David Duncan plus two of his team; his brother Alan and his wife arriving a bit later. This group have developed a self-tacking lateen rig to be used on a 28ft trimaran, (already built) but on this occasion they had brought a Wayfarer hull to act as the test bed for their innovative rig. They had also brought along two substantial concept models, one of a one manned crew catamaran and one of a folding trimaran designed for accommodation and performance. We (Bob & Gen Quinton) of course had our two Broadboards - junior 8ft and senior 10ft versions. Also on Friday night an old friend of ours, Steve, brought his boat around from Felixstowe Ferry to act as general work boat for the weekend of the rally - a staunch friend always ready and willing to lend a hand.

Sheila and Simon Fishwick arrived having been suddenly given the job of running the timing course, due to Bob Downhill's, temporary we sincerely hope, bad health. Bob in spite of his health had arranged to meet up with them at a halfway point to hand over the much-needed timing equipment. It goes without saying that Simon and Sheila had also had to run about to get this all together and safely down to us. This was one of the little hiccoughs that had to be contended with; but with all of their help nevertheless overcome. A local yachtsman, Michael Collis, also an AYRS member had volunteered his boat, "*Gentle Jane*" a Red Fox 200E design, to be one end stake boat for the timing crew. Our club, the Royal Harwich was, as ever, playing the excellent host offering us the use of three support boats plus their harbour launch, Lion, as the other stake boat and shelter for the timing team. Unfortunately, they could not provide crews for these club boats as almost the entire committees and boats crews were proudly showing the flag down at Cowes Week. However among the Winds of Change participants we found enough skilled boat handlers to keep us abreast of the situation, particularly as two of the very willing and able members of the Devon contingency ran the Dell Quay Dory all Saturday long. Simon had to take on yet one other duty which was, apart from the morning of Saturday, to handle the stake boats - we don't think this extra small load on his shoulders made much difference to him.

On Saturday as well as the excitement on the slip with launch and recovery it seemed to be pretty obligatory for Bob to get wet at least up to his waist. But in the beautiful weather of the day this really mattered very little. The loss of one shoe in river mud caused some grief until the tide ran out and it could be retrieved from a two foot deep footprint. The Ketterman Trifoiler not having been sailed by the owner for more than a year seemed to labour about aimlessly as she left the slip, very worrying to an organizer with many moored boats about. But Gordon soon regained his control and got the measure of this very fast beast and sailed safely off into the open waters. Should it have looked more seriously dangerous any of the boats could have been towed out. All the boats this year were very well handled in that at least none collided with anything more solid than the lee shore bank and the mud there! Rescuing these boats was Steve's domain, and he had them back in the deep water as soon as possible.

Coupled with organizing the support boats having put the harbour launch on station for the course, I managed the time myself to meet with Anne Toms who had come down specifically to study and try the Boatek foilsail and to have some experience at sailing with the one deployed on the Broadboard. Since this "event" was jammed in with general hubbub, the run down I gave her for operating the sail was very brief and as she sailed away I realized about 30 things I hadn't told her. Fortunately I had told this fine lady to head up wind and tide so in any event she could come back easily. Since she was tacking I managed to chase down the middle in a rowing boat, shouting instructions as she came by on the next leg of her tack. This may not have been necessary because after the first few minutes Anne had more or less grasped the general functions. After rowing up tide and up wind for half an hour I had to call out to her that I must return to the slip at which time she followed me back with no further instruction and her little adventure was over.

At midday, two participants took time off from the speed course for a challenge race - a high noon duel in the sun! The two F21s had already arranged to race from the club start line to the last navigation buoy before the Orwell Bridge and back to the start line. Chris was the undisputed winner but since he had the foresight to make both first and second place prizes equal perhaps it didn't matter, though honour was at stake here too! Patrick surely put his best effort in.

In the background various models were being laid out in the clubhouse ready for the talk at the night while other radio controlled models were being demonstrated at the head of the slip.

At the end of the day, with boats approaching the bottom of the slipway, the tension and excitement shore-side mounted again, but apart from the loss of one shoe all boats came ashore and found their safe harbour for the night without mishap. This was the end of a lovely day, no strong winds, yet maybe the epitome of the other face of sailing where though the adrenalin is less, the appreciation is more for the smooth power and sensation of peace when sailing. Everyone seemed happy and appetites heightened ready for the excellent meal provided by our in-house caterer John Ashby. Since we could all be seated at one large table it was merely a moment's decision to agree to empty several bottles of excellent house red and white wine.

Speakers afterwards talked about their ideas: David Duncan with his lateen rig; John Thurston with his triscaph multifoil sail; Slade with the Hapa paravane foil; and Bob who touched briefly on Kim Fisher's project, introduced as a concept last year, but now three floating free-rotating wheels linked to produce a man-carrying waterborne version of a Sand yacht.

It was a very civilized way to spend an entire Saturday among friends, both old and new.

Sunday weather suited those other people, who like strong winds and pouring rain. First blow was that our intrepid Devon lads and lasses had to go home early because of a family crisis. Finding crew for the support boats on such a day, when if you had any heart you wouldn't turn a dog out, was a difficult problem but resolved. A new member to AYRS, Ivor Morris from Cheshire was invited to come out on the stake boat with Sheila. After a wet, windy and bumpy stint of nearly five hours, he said he had thoroughly enjoyed every minute of this dramatic day. He was like an excited kid though all of 80 years of age!

Sunday being the type of day it was, support boats were kept extremely busy all day pulling boats from lee shore mud flats. Only two boats managed to take several runs at the speed course. The Ketterman Trifoiler helmed by Gordon and crewed by his elder daughter, Ali, achieved the fastest speed, over 21 knots. All boats by the end of the day were recovered and only small(ish) damage sustained, such as broken rudders etc, though some entertainment was provided by the the harbour launch, when its engine overheated, while in shallow water on a falling tide!

Hope everyone enjoyed the event as much as we did. Next stop - Weymouth!

Bob & Gen Quinton

Regretably, due to the post-event dicovery of errors in the timing arrangements, all the speeds recorded at this event have to be considered suspect. Sorry!

Simon & Sheila Fishwick

David A. Keiper Hydrofoil Kit Files

At the time of his death on 27 June 1998, David A. Keiper was finalizing the design and taking orders for a bolt-on hydrofoil kit. Designed to fit Beach Cats and other small catamarans, the planned foil kits would also be adaptable to similarly sized motorboats.

Dave Keiper is best known for designing the ocean-going hydrofoil yacht WILLIWAW and sailing it solo throughout the Pacific.

A set of Dave's recent files related to these kits (approx. 270 loose sheets) is still available from IHS. This set includes draft instructions for attaching the kit, cutting patterns for making up the foils from the raw extrusions (previously intended to be kept confidential as proprietary information), extensive email correspondence related to testing and fine-tuning the design, calculations for selecting the foil profile, and many notes on capabilities and prices of potential extrusion suppliers. Dave only completed a couple of kits, and the remaining raw extrusions were sold to various people worldwide. Thus it will not be possible to buy a set of the original extrusions unless one of those purchasers decides to put his set up for sale. Nevertheless, this set of files will be of interest to anyone who with the desire and knowledge to design a hydrofoil kit. Keep in mind that these files are unfinished and unedited. You must have the requisite technical and mechanical ability if you are to put these files to successful use.

Price for one copy of the files including Priority Mail postage is US$30.00 to addresses in the USA, or US$35.00 to Australia, Canada, Germany, Great Britain, New Zealand, and many other countries. To verify that your country accepts US Global Priority Mail, check the US Postal Service webpage on this subject. If your country does not accept Global Priority Mail, then the package will have to go regular airmail, which will add an extra $US10.00 to $US15.00 to the cost.

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Bendywood™

Last January, a man walked up to the AYRS stand at the London Boat Show and gave us a 2 ft length of wood, about 2 inches by ¼ inch in section. Quite normal - until he bent it to a small radius in his bare hands! This was Bendywood™ - a natural wood that has been compressed using a patented technique which allows the wood to be easily bent in a way that has never been possible before.

The patented manufacturing process for producing Bendywood™ was developed in Denmark in 1988. It is made from temperate hardwoods (oak, elm, walnut, maple, beech etc) using a mechanical process that requires very high quality, straight hardwood that is partially seasoned. Standard sized planks (120mm x 80mm x 3000mm) are exposed to steam in an autoclave which softens the cell walls (lignin) enough to allow the piece to be compressed along its length by up to 600mm. This compression process permanently shortens the piece and in effect concertinas the cell walls at microscopic level, allowing them to fold and unfold rather like an accordion or a bendy straw.

Bendywood™ can be bent, even when it's cold (unlike steam bending which requires pieces to be bent quickly). You can bend it in 3 dimensions and twist it. You can bend very large sections (e.g. window frames). You can bend it to a much tighter radius than is possible with steam bending. Unlike laminating, no glue is used making this an environmentally superior process for making bent components. You can emboss it easily (with less tendency to split). These combined properties mean that (a) shapes in wood can be made that have never been possible before, and (b) existing items can be made more easily or with better performance properties (potentially reducing manufacturing time and material wastage).

At present, Bendywood is available as a raw material (beech 120mm x 80 mm x 2800mm sealed in plastic), and as dowels (in beech 10, 20, *25,* 30, *35,* 40, *45,* 50mm diameter).

Mallinson, 7 The Coachworks
80 Parsons Green Lane, London SW6 4HU
website: http://www.mallinson.co.uk

Your Letters

Flyby and the Course Theorem

Mr. Gilfillan's concepts in "The *Flyby* Sailboat" in AYRS 126 *Low Flying Boats* article are interesting. He uses the "drag-angle law" from Marchaj's *Aerodynamics and Hydro-dynamics* as a speed determining law.

The "drag angle " or "course theorem" as it is often referred to in AYRS publications is not a speed determiner for any sailboat. It is a result for every sailboat at equilibrium speed.

Equilibrium speed is determined in theory and in fact by the balance of driving forces in the direction of motion (with respect to the true wind course sailed) with the resistance forces that are opposite to the direction of motion.

The drag to lift ratio of the sails (which produce the driving forces in conjunction with the apparent wind) does appear in the drag angle law. However, it is just one of three ratios that produce the driving force(s) in the direction of motion for most sailboats and most true wind courses.

The three ratios that decide the driving forces are described in Chapter 3 of my book *Primitive Benchmark: A Short Treatise on a General Theory of Sailing with the Limits for Sailboat Speed* (ISBN 0-9671566-0-2, 1999)

The book contains new knowledge about how sailboats make their speed.The new knowledge is in a single gem of a primitive mathematical expression (p.34). The expression contains the three governing ratios for driving forces and displays how the ratios fit together to represent the driving forces in the direction of motion for any sailboat.

The balance between the two sets of forces (driving and resistance) is what decides equilibrium speed, if one is greater than the other, the boat accelerates, if the signs reverse, the boat decelerates. The character of these forces and their interplay is what maintains the equilibrium speed. The character of the topline driving forces is described in detail in *Primitive Benchmark*, which is available through amazon.com and some copies through AYRS channels.

Jerry Selness
30 May 2001

The Bauer Vehicle -- Michael Collis' objections

In his letter to the editor (April/May 2001, #4), Mr. Michael Collis, Chartered Marine Engineer, rejects the Bauer vehicle's ability to go downwind faster than the wind. He claims it does so only by using the flywheel energy stored in its spinning propeller. He also rejects my explanation of how the Bauer vehicle works, as shown in my drawing of an abstract mechanical analog vehicle. (See the left side of my drawing, "Propeller" Mode, overleaf.)

Specifically, Mr. Collis claims that the ruler in my drawing must move in the "OPPOSITE" direction (rearward; down the page) in order for the vehicle to move in the direction as shown (forward; up the page). He therefore considers my explanatory model to be "unhelpful" with respect to understanding the Bauer vehicle. Mr. Collis says that he built a model of the vehicle shown in my drawing and that it confirmed his reasoning, not mine. He states, "A simple experiment with some Meccano confirms this." (Meccano is a toy set of beams, gears, wheels, etc.)

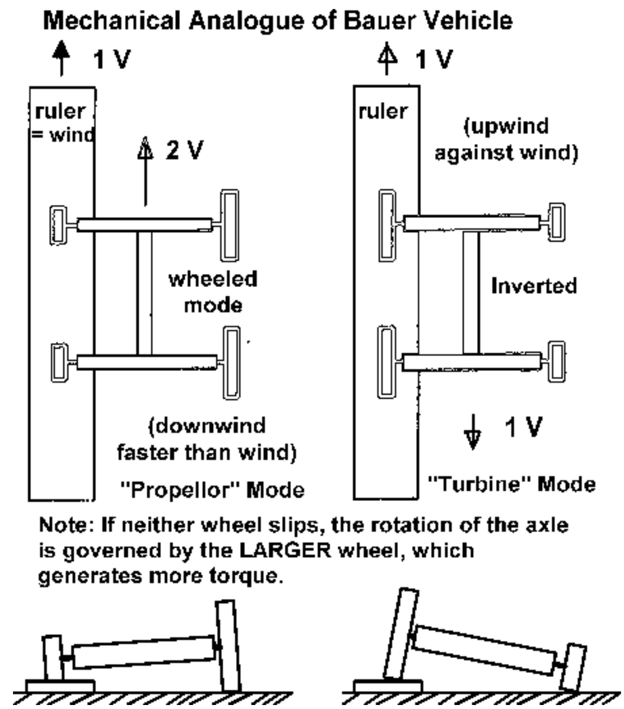
[Referring to the drawing reproduced overleaf] The analog model may be understood by noting that, for each revolution, the small wheels can roll only half as far as the big wheels, so the ruler must make up the difference, the other half, by always moving in the same direction. (The ruler is assumed to slide without friction, as it would on rollers.)

Let us call the circumference of the small wheels 1 C, and then move the ruler rearward 1C. That will rotate the small wheels forward one revolution, relative to the ruler. But the small wheels will remain in place relative to the table. However, the big wheels must rotate one revolution forward also, which would carry them forward a distance of 2 C relative to the table. Oooops! Not possible. The vehicle is now miraculously in two places at the same time. We therefore know that there was something different about Mr Collis' model.

Mr. Collis also suggests that the Bauer vehicle did not sail downwind faster than the wind. He asserts that the energy stored in the spinning propeller, acting as a flywheel, is what enabled the Bauer vehicle to exceed the speed of the wind; and further, that once the flywheel energy were expended, the Bauer vehicle would have to slow down.

As I mentioned in my paper, the model Bauer vehicle was able to maintain its position on a moving belt in a windless room. If it were doing so using flywheel energy from the propeller, it would not be able to sustain its position. Instead, it would start to move along in the direction of the belt as the propeller slowed down while losing its stored flywheel energy. As far as I know, that did not happen. The observers were responsible engineers. It is extremely unlikely that they could have missed such an obvious behavior by the Bauer model. Further, a number of theorists have published mathematical models explaining the physics of the Bauer vehicle. is it plausible that all of the experimenters and theorists were unable to recognize a flywheel when they see one? The flywheel theory is discredited by the experimental facts already in evidence and also by the existing theoretical papers.

Mr. Collis refers to my abstract analog model of the Bauer vehicle, as shown in the drawing, as "the Theo Schmidt device". However, I stated, "Our model is inspired by two devices built by Theo Schmidt." Credit should be given where credit is due. Mr. Collis may compare my abstract models to Schmidt's real devices by referring to AYRS 100, 1985, the reference that I cited in my paper.



Mr. Collis calculates that, theoretically, in a wind of 25 knots, "Lifting Aerohydrofoil" craft could achieve a Vmg downwind of 1.86 times wind speed and an upwind Vmg of 1.8 times wind speed. However, Mr. Collis neglected to make any point about those numbers. So I will.

Mr. Collis' numbers illustrate that the theoretical speeds of even the fastest direct-sail-power (DSP) boats are considerably lower than the theoretical speeds of Power Alternating Sailing (PAS) craft, which seem to have no limit in principle. DSP boats are inherently handicapped with respect to speed. As their speed increases, their relative wind approaches the front (bow), thus limiting their forward thrust. And worse, a DSP boat must also remain in contact with the water in order to provide a reaction surface, thus creating considerable hydrodynamic drag. This is demonstrated by the difference in speeds between DSP boats and DSP ice boats.

PAS craft, however do not necessarily have these limitations. They seem to be capable of much higher average speeds than DSP boats, perhaps more than twice as fast (average Vmg for all angles to the wind). If I am correct, then from a theoretical perspective, in terms of speed, DSP boats are now obsolete, even though they are not yet close to their full potential. Consider also that PAS craft, while stationary, would be well suited to make use of wave energy, which could provide an enormous increase in PAS power. (Waves are a concentrated form of wind energy.) In theory, PAS is limited only by material strengths, power transmission techniques, and design ingenuity. These continually advance. What PAS progress now requires most is for designers to question

With reference to the left side of the drawing, "Propeller" Mode, here are the analogous parts.

Thermal Helicopter	Analog Vehicle	Bauer Vehicle
thermal	ruler	wind
cord	table surface	ground
spool	large wheels	wheel
rotor	small wheels	propeller

With reference to the right side of the drawing, "Turbine" Mode, here are the analogous parts.

Gravity Helicopter	Analog Vehicle	Upwind Windmill Vehicle
cord	ruler	wind
ambient air	table surface	ground
spool	large wheels	windmill
rotor	small wheels	wheel

their core assumptions about sailing, and to let themselves dream. If I am right, then sailing has fundamentally changed.

The following examples are intended to add another perspective from which to view the Bauer vehicles and upwind windmill vehicles. Consider that, theoretically, it would be possible to fly a helicopter straight up using a quickly rising column of hot air (called a thermal) for power, while ascending faster than the thermal itself. (Note that when hawks and gliders rise using thermals, they descend relative to the air in the thermal. So they ascend more slowly than the thermal itself.) This "thermal helicopter" would use the Bauer technique. The key would be to tether the helicopter to the ground using a cord, and wind the cord around a spool in the helicopter. The spool would be appropriately geared to the helicopter rotor so as to minimize the downward pull of the cord. The rising of the helicopter would spin the spool and rotor. The cord for the thermal helicopter would serve the same function as the ground does for the Bauer vehicle. Both provide a reaction surface.

Like the Bauer vehicle, a thermal helicopter would need help to get it started (upward). An autogiro mode (or a balloon) could function like the Bauer vehicle's downwind windmill mode. As the helicopter approached the speed of a quickly rising thermal, it would be able, if correctly designed, to ascend on its own as a helicopter, accelerate upward, and exceed the vertical speed of the thermal – at least until the cord ran out. (Note that this is *not* a gyrocopter kite.)

The same helicopter could be made to ascend in still air by attaching a heavy weight to the end of the cord. Letting the weight fall would provide the power for this "gravity helicopter". It would be necessary to support the helicopter over the edge of a tall building, and then drop the weight, thus spinning the spool. Once the rotor were up to speed, the helicopter would ascend until the cord ran out. A gravity helicopter is analogous to a windmill vehicle moving directly upwind against the wind. Please keep in mind that the helicopter does not support the heavy weight – only a small portion of the weight. A heavy weight would be used just to ensure that it would fall rapidly – so that the cord could spin the spool rapidly.

A more practical power source would be a conventional motor mounted at ground level. Mechanical power could be transmitted to the helicopter using a long loop of cord as a belt-drive from the motor to the helicopter. The helicopter would be almost silent, and it could remain aloft indefinitely at a moderate altitude. It might serve as an observation platform equipped with cameras. A toy helicopter of this type should be relatively easy to construct, and it would be pleasingly counter-intuitive. Like a gravity helicopter, this "tethered belt-drive helicopter" would be analogous to an upwind windmill vehicle.

Peter A. Sharp
Oakland, California, USA

Lug Rig

I am interested in finding more information about the traditionally rigged lugsail. I have recently converted my 20 foot boat to a dipping lug configuration. I am having some trouble in lumpy seas and with the sheeting angle and the positioning of the halyard point on the yard. I designed the rig from first principles and used old photographs of similar boats to decide the design, however I am now looking for more information to help me fine tune the set-up.

If anyone could be of any help I would be extremely appreciative. I would also welcome any advice as to where I could gain any more information.

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Wind Profiles and Yacht Sails

Mike Brettle

It is common knowledge that wind speed increases with height near the ground. In principle wind speed must be zero at the surface itself and at some height above the surface it will be free of surface friction. However the exact rate of change in between can vary tremendously a shown here.

Wind Speed Profiles near the Surface

The simplest situation to consider is when the atmosphere has so-called neutral stability. This means that there are no effects of temperature structure to complicate the wind profiles. The atmosphere has no tendency to generate convection or to produce a stagnant stable layer at the surface. This neutral condition may result from a particular temperature profile known as the 'dry adiabatic lapse rate' or DALR. This is about 10 degrees Celsius per kilometre. It simply means that if air is raised or lowered over some change in height by turbulence or other disturbance then the change in temperature that results from the change in pressure is exactly the same as the change of temperature over the same change of height in the atmosphere. Fig.1 shows this more clearly, along with unstable and stable atmospheres.

This drawing goes some way to explain the paradox that temperature falls with height yet we all know 'warm air rises'. In fact a constant temperature with height is a very stable atmosphere which restricts transport of heat upwards by convection or turbulence. Since the atmosphere is, in general, heated from below (most solar radiation passes through the atmosphere to be absorbed at the surface) these conditions do not last very long outside polar winters. In strong winds the atmosphere will behave as though stable irrespective of the temperature profile because mechanical turbulence will in this case overwhelm any effects of the temperature profile. This means that for the design of boats for speed in stronger conditions a neutral profile will be the most appropriate.

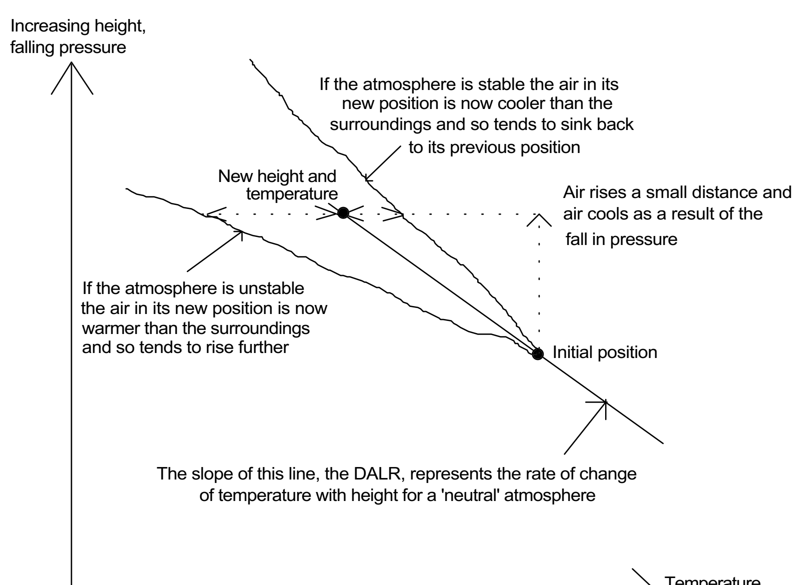


Fig 1: Neutral, stable and unstable temperature profiles.

Wind speed profiles in a neutral atmosphere

Experimental evidence, and some very obscure mathematics, has shown that in neutral conditions the change of speed with height follows a logarithmic form : -

$$u = (u_*/k)\ln(z/z_0)$$

where u is wind speed, u_* is a parameter which relates to the turbulence properties of a given situation, k is Von Karmans' constant, usually taken as 0.4, z is height and z_0 is a measure of the roughness of a surface, known as the 'roughness length'. Over water a value of about 10^{-4} metres is typical. Table 1 gives a typical speed profile over water for a wind speed of 20 knots at 10 metres.

Height (m)	Speed (kts)
10	20
5	19
2	17
1	16

Table 1 A typical speed profile over water for a wind speed of 20 knots at 10 metres in a neutral atmosphere.

Whether this change is significant or not is a matter for sail makers and sail trimmers rather than for meteorologists like the author.

Over land roughness lengths are much larger, for example grass is more like 10^{-2} metres. It may not feel like it but to a meteorologist the Solent in a force 5 is smoother than a cricket pitch!

There are a couple of points to consider. Firstly the gust speeds in a given situation tend to vary less with height than the mean speed. Secondly the wind takes a finite time to respond to a change in surface roughness. Meteorologists refer to an internal boundary layer forming at a change in roughness. This has a slope of the order of 1:100. Thus for our example previously the pure 10 metre profile described would strictly only apply after a fetch of 1000 metres or so.

The effects of stable or unstable temperature profiles

At lighter wind speeds the effects of temperature gradient can alter the wind speed profile. Unstable conditions, typically convective conditions with a surface relatively warmer than the overlying atmosphere, will probably result in less variation with height over a yacht mast. However the speed must still approach zero at the surface so less variation at one level will imply more at another. Stable temperature profiles will in principle increase variation with height but again a lot depends on how the profile varies with height. A shallow surface layer, cooled by the water below, will produce more extreme profiles within it, and less variation above. However a deep or elevated stable layer can produce a layer of uniform stagnant air at the surface. Statistics of wind speed from tall masts in uniform terrain sometimes show a clear daily variation with wind speeds near the surface being higher at midday, when conditions are more likely to be unstable. Higher up, around 300 metres or so the speed tends to actually fall at midday as the effects of the convection from the surface are felt and the speeds increase at night as the higher levels become 'decoupled' from the effects of surface friction.

Wind Direction Profiles near the Surface

Measurements of mean wind direction changes with height near the surface are in very short supply. Possibly changes in direction are considered less important than speed variations, or perhaps they are simply harder to measure. It is important to remember that wind near the surface is usually turbulent and subject to fluctuations in speed and direction such that at any instant the actual profile of speed and direction can be very different to that averaged over a longer period. The wind direction of interest here is the direction averaged over several minutes or so and ignores the effects of shorter period gusts.

Theoretical discussion of direction changes at the surface is also rare. Some authors, in discussing boundary layer winds, seem to take for granted that there are no significant direction changes near the surface. Others take for granted a rather extreme model, based on the so-called 'Ekman spiral'. The Ekman spiral is basically a model of the response of ocean currents at different levels to the driving force of surface wind. However it has been adapted to surface wind direction, in the influence of surface friction, with no particular observations as justification. For meteorologists trying to produce a model of wind direction at the surface there is a mathematical problem in that there is no obvious boundary condition to wind direction to constrain possible values at the surface. Wind speed can always be assumed to be zero at the surface itself. Wind direction, however, can tend to different values at the surface according to different models.

The change in wind direction between the free atmosphere (meaning above the boundary layer) and the surface wind has been thoroughly studied. Usually the wind at 10 metres is taken as representing the surface wind and levels of 900 metres or 900 hPa as the best approximation to the wind above the boundary layer. These studies have led to various rules of thumb for estimating surface winds from winds in the free atmosphere ('geostrophic winds') in various circumstances. Those published by the Meteorological Office, (1993), are typical and give values of up to 25° for the amount the 10 metre wind is backed relative to the geostrophic wind over the sea. The largest values are for the most stable temperature gradients and strongest wind speeds. However these changes occur anywhere between 10 and 900 metres and may not occur in a uniform manner. They could easily be concentrated near the surface or through an elevated temperature inversion.

There is a widespread belief in the yachting community that wind direction does change significantly with height at the surface of the earth, even within ten or twenty metres of the surface. For example the Royal Yachting Association advises that changes in wind direction of twenty degrees over twenty metres can occur (Houghton, 1998) and other guides go into more detail (Houghton, 1992, Smith, 1988). Unfortunately, in the absence of published measurements collected for the purpose and subjected to peer review, this information is based on reports of yachtsmen collected after yacht races in sailing club bars or on individual sailing experience. In principle yachtsmen ought to be very sensitive to wind direction changes at the surface for the following reason. If there is a real change in wind direction over the height of the sail then the sail trim will not be symmetrical, that is more twist will be required if the yacht is sailing with the wind on one side than if it is sailing with the wind on the other side.

In view of the confusion amongst meteorologists and the circumstantial evidence of the yachtsmen I decided to collect some data myself. The rest of this article summarises the various theoretical considerations, previously published work and the results from my own studies.

Possible causes of wind direction changes at the surface.

1. Effects of surface friction

Fig. 2(a) shows the balance of forces on a particle of air clear of the surface. This is the simplest model of airflow as it assumes there are no influences on the air other than the pressure gradient and the Coriolis effect due to the rotation of the earth. The wind is assumed to be in equilibrium following a straight path without acceleration or deceleration. Despite these assumptions this model called the geostrophic wind, is useful and often makes a good estimate of wind above the effects of friction. Friction at the surface will reduce wind speed and produce a gradient in wind speed, which has a form that varies according to the roughness of the surface, the wind speed and various thermal and turbulent characteristics of the flow. This reduction in speed as the surface is approached will result in a change in direction. Fig 2(b) shows how adding friction to the model changes not only wind speed but direction as well. This is because the Coriolis effect depends on wind speed but the pressure gradient does not. Therefore the reduction in speed as the surface is approached results in the pressure gradient pulling the wind direction round to lie at an angle to the isobars. The size of the direction change will depend on the roughness of the surface and the temperature gradient. In principle an unstable atmosphere will result in a smaller change than a stable one. This change will not necessarily occur uniformly over the boundary layer and may be concentrated over quite shallow layers if there are pronounced changes such as temperature inversions. Note that in the Northern Hemisphere the change due to this effect will always be in a direction which results in the surface wind being backed (moved anti-clockwise) relative to that above.

A particular form of wind speed and direction changes due to friction is described by a spiral pattern known as the Ekman spiral. In theory the form of the spiral pattern is not necessarily uniform but the total change in direction between along isobar flow in the free atmosphere and the surface is, according to some authors, 45° (Bennett, 1997).

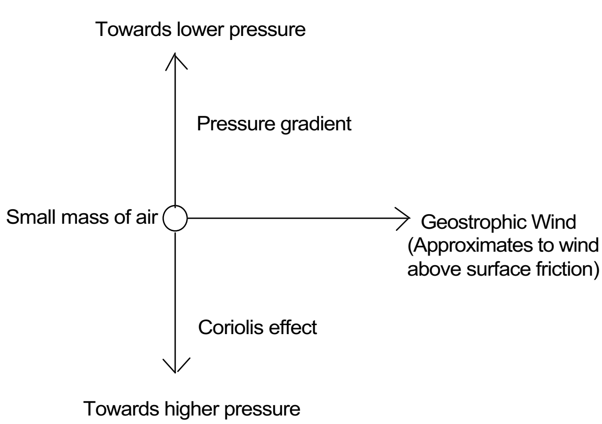


Fig. 2 (a) Forces on a particle of air in the free atmosphere.

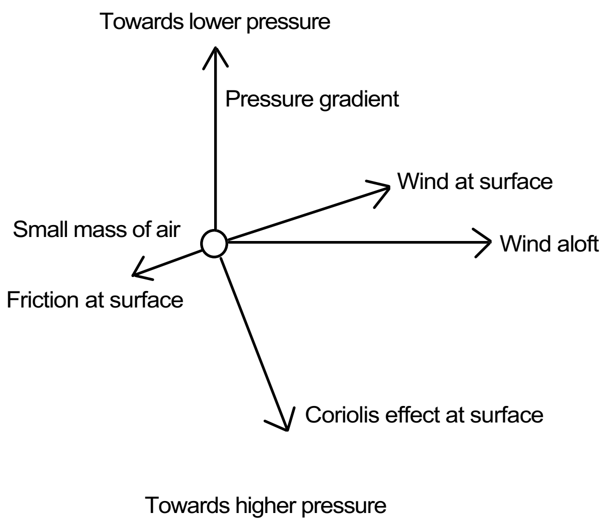


Fig. 2 (b) Forces on a particle of air in the influence of friction.

2. Effects of terrain

Even relatively small height variations can influence wind direction near the surface. Studies at Cardington in Bedfordshire, Grant (1994), have shown how profiles of wind direction in a stable boundary layer can be affected by topography in a valley about 8 kilometres wide but only about 60 metres deep. Typically the change in direction between 20 metres and 300 metres varied between about 20° and 50° according to the direction of the upper wind relative to the valley. It may be possible to make some qualitative judgement of likely effects of a given terrain in some circumstances but in stable boundary layers anything other than the flattest terrain can cause variations in direction through the boundary layer as large as the other effects discussed in this article.

Obviously in the open sea with a fetch of more than a few kilometres terrain has no influence. However the surface of the sea is often covered with regular patterns of swell waves and these have been suggested as a cause of wind direction changes at the surface.

3. Tidal streams or currents

A current that is not parallel to the wind combined with a gradient in speed at the surface can produce a variation in the wind direction with height. Fig 3 shows how this occurs. In principle, given a long enough distance and time for the wind and current to interact, equilibrium will be reached such that, in the frame of reference of the water, the direction does not vary with height (however in this case direction variations would occur if measured from a fixed reference frame such as a structure anchored to the sea bed). In practice currents vary in time and over distance. The wind itself is also constantly changing so an equilibrium state between current and wind is probably the exception rather than the rule. It is possible to estimate the possible magnitude of a gradient in direction, as measured by a yacht under sail, that results from wind crossing a current. In the example in fig. 3 the wind speed at the top of a sail at 15 metres above the surface is 10 knots and it is 6 knots at the base of the sail. If the tide is 2 knots the change in direction over the sail will be 14 degrees if the yacht is sailing with the wind on the port tack and 10 degrees on starboard tack.

4. Inertial Oscillations

Inertial oscillations in the boundary layer can result in significant direction changes over a depth of a hundred metres or so. Inertial oscillations result when the formation of a stable boundary layer results in a de-coupling of winds above the layer from those at the surface itself. The winds above, in simple terms behave as if 'the brakes have suddenly been released' and increase in speed, sometimes even forming a low-level jet. They also change in direction to flow more closely along the isobars, or even slightly against the pressure gradient for a while. Inertial oscillations may also be caused in other circumstances, such as an abrupt change in surface roughness. They have been used as an explanation for dramatic changes in direction within a couple of hundred metres of the surface. For example weather balloon ascents and aircraft measurements near Hannover in Northern Germany, Jacobi et al, (1994), which showed changes in direction of about 180° in the lowest 100 metres. Curiously these variations with height lasted only three hours or so and disappeared without any major changes to the overal meteorological situation.

These observations are difficult to explain except in terms of inertial oscillations. It is possible that inertial oscillations might sometimes have an effect much lower down than generally thought. Indeed in the cases described from Hannover it would be surprising if there had not been a significant direction change over the lowest few tens of metres.

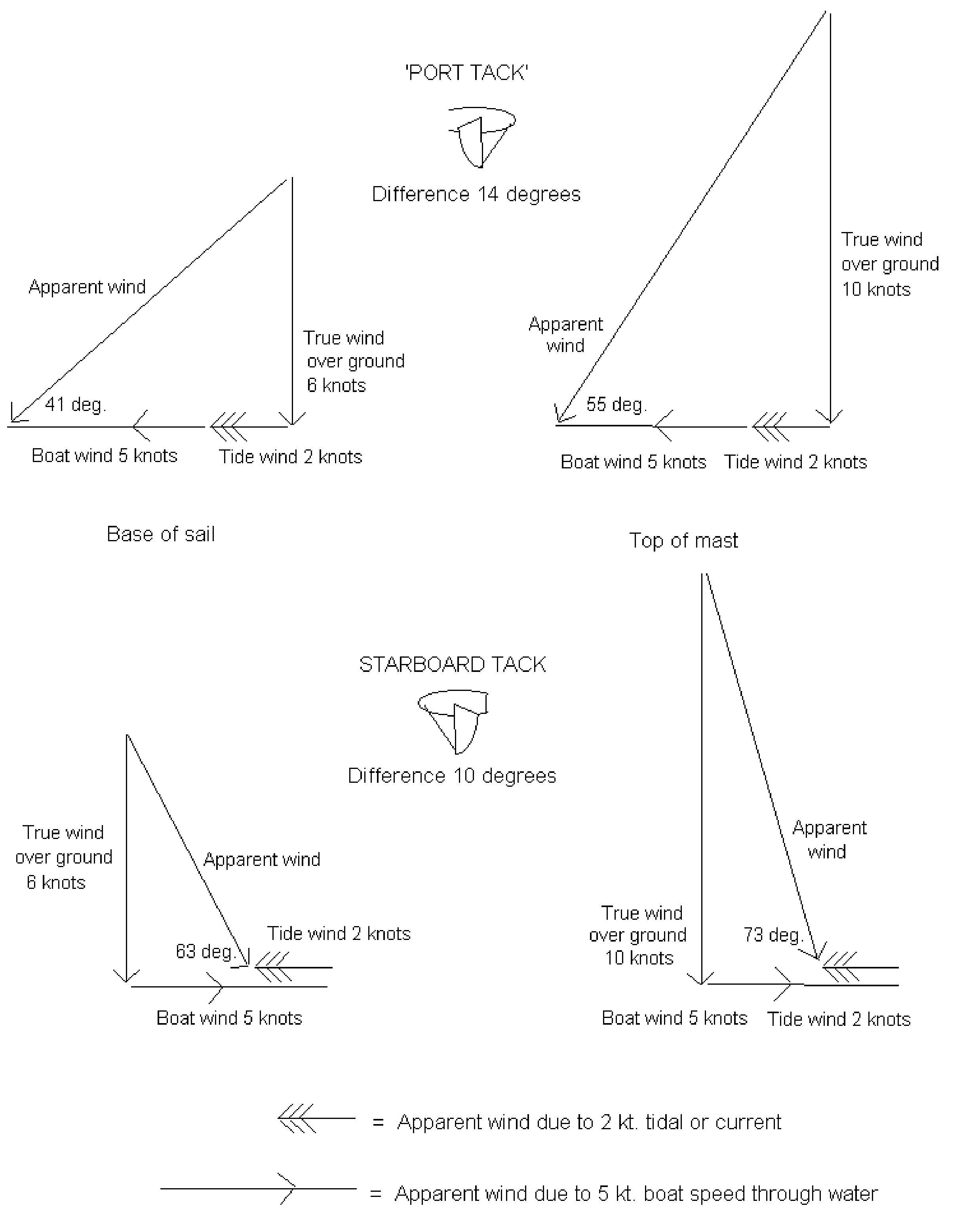


Fig 3: Why different amounts of sail twist may be required on different tacks in currents or tidal streams.

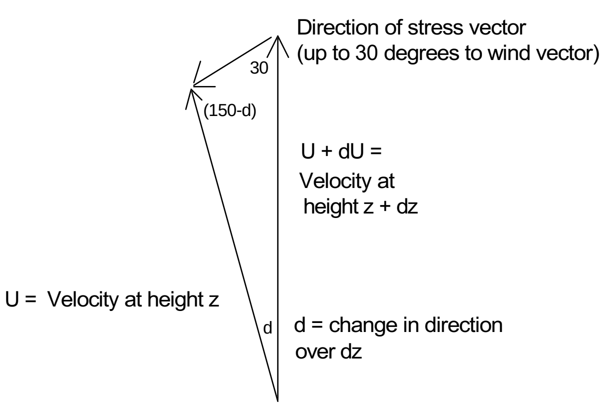


Fig 4:The relationship between stress and wind velocity vectors.

Existing data on wind direction changes near the surface

There is very little data published on wind direction changes at the surface. Most of it is based on indirect measurements, not of wind itself but of wind stress. Stress, in the meteorological sense, is similar to drag and is basically the amount of momentum being transferred to a layer of air by faster moving layers above and lost to slower layers below as a result of turbulent eddies. Wind stress can be measured using special wind sensors. These instruments can measure wind speed and direction (including the wind speed in the vertical direction) at a high sampling rate, typically taking measurements ten times a second or even faster. This allows a direct assessment of stress in the atmosphere because horizontal speed and direction variations can be correlated with the vertical wind speed variations. The greater the correlation between speed increases and the downward component in the vertical wind speed then the greater the size of the stress at that point. This technique is called eddy correlation. Similarly a correlation between changes in direction and the vertical wind implies that the wind direction and stress direction are not aligned (incidentally it is often forgotten that stress is a vector quantity. Even in textbooks it is sometimes treated as a scalar quantity, as will be shown later this is effectively assuming that wind direction does not change with height). In layman's terms if the stress and wind vectors are not aligned near the surface it means that drag on the wind is not just pulling the air back and restricting its speed but is also pulling it to one side. Measurements of stress taken over sea have been published, Geernaert, (1988, 1993), and show angles between the stress and wind vectors of up to 30 degrees. These can be used to make rough estimates of associated direction changes. Note that since the stress is due to the transfer of momentum between layers by eddies we can directly relate the direction of the stress vector in a thin layer to the vector change in direction over the layer. We can do this without any knowledge of the size of the stress vector. However to estimate the direction changes we do have to make an assumption about the rate of change of wind speed. Assuming the simplest speed profile, a logarithmic neutral one, over water, in round numbers, the mean wind speed at 10 metres should change by about 1% over 1 metre. Using values of around 30 degrees for the angle between the stress and wind vectors (see Figure 4) we have

$$U/\mathrm{Sin}30 = 1.01U/\mathrm{Sin}(150-d)$$

If dz =1 the trigonometry gives a rate of change of direction with height of about 0.3 degrees per metre. This is small but over a tall mast may be noticeable. There is no evidence available on whether the angle between stress and velocity will itself vary with height.

The most important result from these studies is not the direction changes themselves, although this is important, but that the sign of the changes varies. That is sometimes the wind direction is changing in a clockwise manner as the surface is approached and sometimes it changes in an anti-clockwise manner. A wind changing in a clockwise manner, in the Northern Hemisphere, is not compatible with an Ekman spiral or other explanation based on the effects of surface friction. It has been suggested that swell from different directions could be having an effect. Possibly tidal streams could also account for these results but I was unable to obtain relevant data to see if this is possible.

I was also able to find some data on wind direction variation with height, which has yet to be published. This was obtained from the meteorological mast at BNFL Sellafield. Very kindly they allowed me to look at some of the data from this site, which is probably a unique facility in the UK. Wind direction is sampled every 2 seconds to an accuracy of about 1-degree and a resolution of 0.1 degrees at various heights up to 48 metres. The mast is about 1-km inland and the fetch between the shore and the mast has few significant obstacles. I judged that wind directions from about 270° to 300°, measured at 10 metres and above, would be fairly representative of conditions at sea. Wind data is archived in the form of 10-minute averages and I was able to obtain data at 1200UTC for every day in April 1997. I chose data around midday to simplify any comparison with other data I might want to make and April so the sea would be suitably cold in the hope that this would imply a stable boundary layer both shallow and more likely to show direction changes. Out of the 30 days, 11 gave wind from suitable directions. In fact there was no significant variation of wind direction over the height of the mast on any of these days except one. This was on the 9th April 1997. I was lucky to obtain this because if I had known more about the synoptic situation during the month before selecting days I would not have asked for data from this day. This is because the wind speed and slightly unstable conditions would, according to the yachtsman's rules, have made direction changes small or non-existent. The results are shown in Fig 5. These data are for ten minutes from 1200UTC. I was able to obtain additional data for the next 50 minutes and the profile remained relatively constant over the whole period of 60 minutes. The wind speed was about 6.5 m/s at 48 metres and the speed and temperature profiles were all consistent with a slightly unstable atmosphere.

I have no explanation for these results. The instruments were all in good order with in-date calibrations and good exposure to winds from this direction. In any event there was no significant variation with height on other days with the same instruments and with similar wind directions and speeds. All that can be deduced with certainty from this data is that direction shear does occur but in conditions where it is least expected.

Data Collected at Grafham Water on 12th May 1999

The Sellafield data was very useful but it did not go below 10 metres. According to some authors the rate of change of direction with height increases as the surface is approached. Therefore I had to collect wind data to fill this gap. To do this I set up a mast on the eastern shore of Grafham Water, a large reservoir in Cambridgeshire. The mast site was actually in shallow water at the edge with an onshore wind of about Beaufort force 4 and a clear fetch over water of at least 1 km. Wind direction was recorded at 7.0 metres and at 1.35 metres. I also recorded measurements of air temperature (in a radiation screen at 2.0 metres),

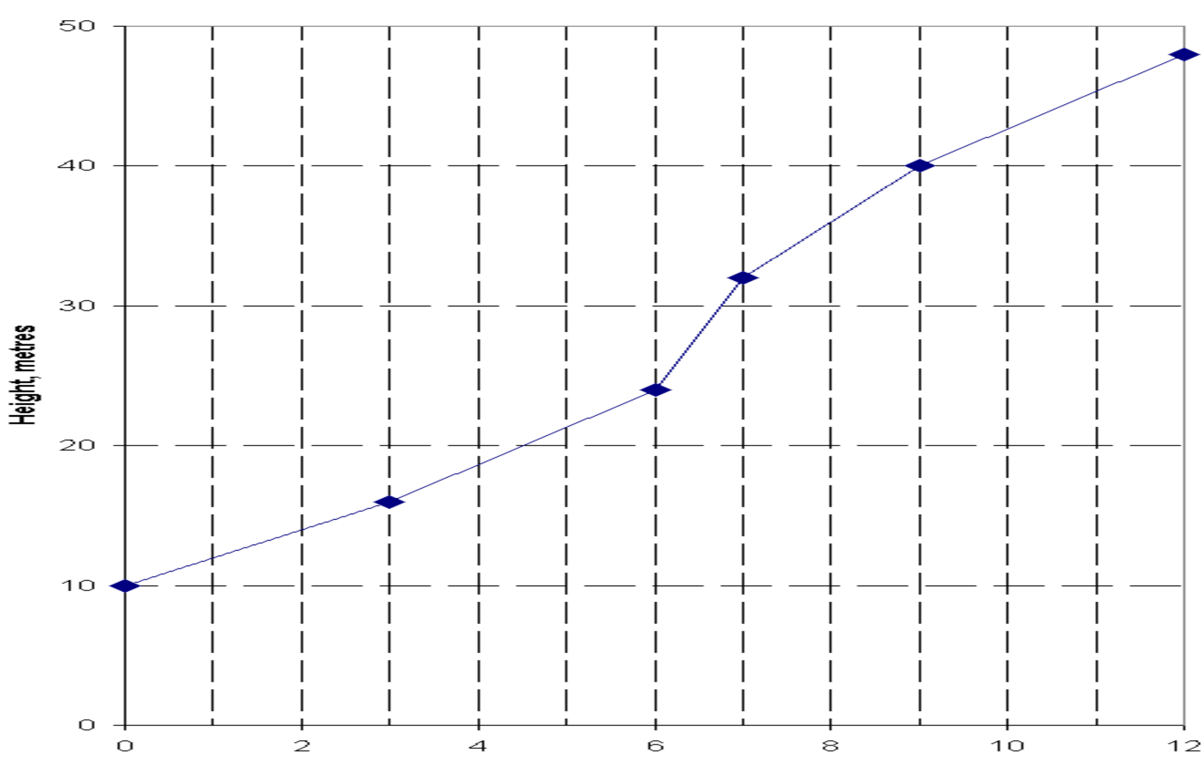


Fig. 5 Changes in wind direction with height measured at Sellafield at 1200UTC on the 9th April 1997. Directions are 10-minute averages.

water temperature (measured about 2 cm. below the surface) and wind speed measured at about 2.5 metres. I took three runs of data each lasting about 15 minutes and comprising 179 data points collected at 5-second intervals. The 12th May was not an ideal day. However with restrictions of my 'day job' and the need to plan the trial well in advance, on a weekday, it was the only possible day for data collection. A south-westerly airstream containing several troughs lay over Southern England with widespread showers (although none occurred while data was being collected). At least it was sufficiently early in the year for water temperatures to be well below their annual maximum.

I took particular care in reducing and assessing possible systematic errors that could affect differences between the wind directions at the two levels. The instruments were both calibrated with the same protractor spirit level both before and after the trial using the same data logging equipment as used in the trial. The total error budget, for differences between directions at the two levels came to 3 degrees. This is a conservative estimate and includes possible errors in measuring the relative offsets between the sensor mountings, possible calibration errors and the maximum possible distortion of the vanes themselves. The response times for the windvanes, in the wind speeds experienced in this trial, were of the order of 0.1 seconds.

The results are summarised in table 2. The frequency distributions of direction changes looked reasonably smooth and symmetrical so I was able to calculate the standard deviations given. The direction changes recorded are small but larger than can be accounted for as experimental error. They were also consistent between the three runs. The direction change detected, veering clockwise with height, is as predicted by simple models of frictional effects. However in view of the results reported by Geernhaert I would not conclude that these models are supported by this limited data.

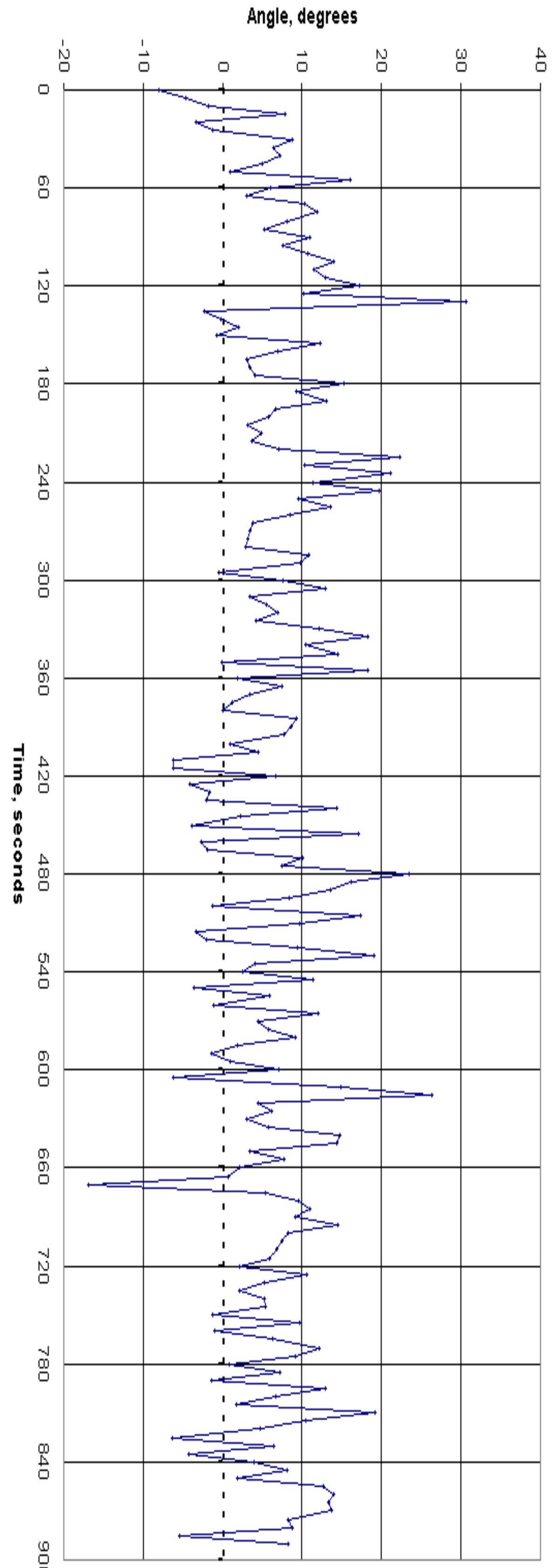
Discussion and Conclusions

There is evidence that significant variations in mean wind direction do occur within a few tens of metres of the surface. However the nature of this evidence does not unambiguously support any of the various explanations offered in the past. Neither does it mean that yacht club bar talk was correct all along. The simplistic explanations given by sailing coaches are not borne out by the evidence and the most significant changes have been recorded in conditions very different to those in which yachtsmen are advised to expect them. There is one other point that makes me doubt that yachtsmen can detect and use the direction changes as measured. The changes measured were based on profiles measured over several minutes. Over shorter time scales, several seconds or so, the direction profile changes dramatically. The standard deviations of the direction changes in Table 2 gives an idea of what happens. Figure 6 shows all the data from Run 1. The graph looks a mess and this is the most important point. The change in direction with height over the lowest few metres can even reverse in sign, from veering with height to backing with height, over periods of 5 seconds or so. Since sail trim is not realistically altered this quickly it seems unlikely that the mean twist in the wind is something that yachtsmen can judge and adapt to. The lesson to be drawn from this graph, and the other results discussed, is that the angle of attack of a sail to the wind will not only change faster than any yachtsman can react but will also vary significantly over the height of the sail in a way that the yacht's rigging could never possibly compensate for. Yacht sails operate under totally different conditions to the aircraft wings they are often compared with and need to cope with a variety of angles of attack simultaneously to be efficient.

Probably meteorologists should keep an open mind until more data is available and at least be grateful to yachtsmen for bringing their attention to it. The instrumentation required is not great and this work could form the basis of useful projects for amateur scientists, (perhaps including AYRS members).

Run	Mean wind speed, m/s	Mean air temperature, °C	Mean water temperature, °C	Mean change in direction between 1.35 and 7.0 metres, degrees	Standard deviation of change, degrees
1	6.0	14.3	14.7	6.6	7.0
2	6.5	15.1	15.5	6.0	7.2
3	6.8	15.7	15.6	6.1	6.5

Table 2 Measurements taken at Grafham Water on 12th May 1999.



Acknowledgements

I am grateful to BNFL for supplying meteorological data and for permission to quote it.

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Fig. 6 Changes in wind direction with height (between 1.35 and 7.0 metres) measured at Grafham Water, 12th May 1999. Direction changes are spot measurements taken at 5-second intervals .

Remarks on Sea-wings (Hydrofoil Sailboats)

Didier COSTES, 4 June 2001

This brief survey of the options for hydrofoil sailboats, and description of the work that I have done and am continuing to do is made in the hope it can help others carry the work forward.

Hydrofoil options

As regards hull or float, hydrofoil sailboats can be:

- Traditional (monohull, catamaran or trimaran with fixed geometry, bilaterally symmetric, with a bow and a stern): most fast boats from Monitor of 1954 (34 knots, unofficially, after a tow start) to Longshot (42 knots in 1992), including Icarus, Mayfly, etc ·
- Still with bow and stern, but assymmetrical, optimised for performance on one tack only, for example with two staggered hulls (e.g. the impressive Clifton Flasher in 1972, Crossbow), ·
- Proas, with fore and aft symmetry, taking the wind always same side, reversing to change tack (shunting), like my Exoplanes from 1968
- Assymmetrical, tetrahedral or pyramidical, with several hulls which may be steerable: e.g. B Smith's proposals (1964), to Yellow Pages (45,5 kts).
- Monohulls directed by the feet of the pilot (sailboards, 44 kts).

As regards sails: ·

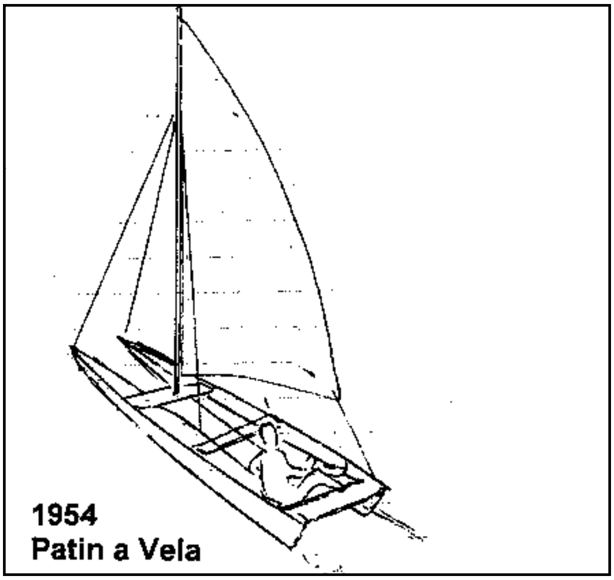
- Upright mast(s) on the hull, with one or more sails that do not produce significant upward lift (Longshot), sometimes they even drive the boat under when it heels;
- Sail canted to provide lift, frequently moved to leeward so that the resulting force does not heel the boat any more, as for the kite-sails (Bemard SMITH, my Exoplanes, Yellow Pages),
- Sail canted to provide lift, constantly kept upright by the pilot's weight (sail boards),
- Free flying kites (Jacob's Ladder, Stewkie...),

The hydrofoils, ensuring the lift of the emerged hull and the resistance to leeway, are:

- Simple blades cutting the surface at an angle (often equipped with "fences" against the ventilation of the suction face, and a vertical lower part);
- Ladder grids (Monitor),
- Curved blades (Exoplane), improve vertical stability,
- Immersed lifting sections with incidence adjusted by feelers on the surface, lateral resistance being provided by vertical blades (Longshot, Rave).

Seadogs (hapas), drawn upwards by an oblique cable, are not lifting. They can be associated with an ordinary boat to which they give stability in heeling, or with a sailplane such as a kite, a parapente (paraglider), or a balloon. My choice, to lift about as much weight with the foils as with the sail, allowed me to achieve 20-25 knots probably as long ago as 1968–1972, and certainly by 1976, when the speed record was also of this magnitude. It is noteworthy that, then, boats as well as boards could go around 25 kts, whereas now boatspeeds have a wide spread, and boards, in very strong winds, reach 40-45 knots although no more. For boards, lift from the sails makes it possible to limit the effects of cavitation underwater but the air-side streamlining is adversely affected by the unstreamlined sailor. The quality of the athlete on the board appears however to be all-important.

For boats, foils and sails are now of great quality and for records one chooses water that remains fairly flat in spite of the force of the wind. Speeds tend to reach values of twice the real wind, which justifies reducing the area of the foils relative to the sail. Their correct orientation can be obtained by an suitable mechanism, whereas the orientation of the lifting sail remains difficult if one wants, as is normal, to ensure the capacity to sail on both port and starboard tacks. The Longshot choice of the non-lifting sails is not illogical, but it does not reduce cavitation on the foils. The "ultimate" solution could be a low-drag kite, resembling a sailplane, carrying a pilot, and coupled to a Seadog, or a boat functioning like a Seadog, with low-area foils and a streamlined cockpit. I continue to study the solution of an appropriately shaped airship operating as a sail.



My lifting aerofoil boats

Having practised in the Fifties with centre-boarders and, in Africa, a catamaran of the "patin a vela" type, I found it somewhat irrational that the sail drives the boat into heeling, and that the sailor has to devote most of his efforts to limit it. I began a long series of trials, building my boats myself, thanks initially to my parents who were happy that I use their living-room, then with friends who allowed me to work in their garden in the Paris area, or with others where I had lodgings. My wife was understanding. My boats were transported in disassembled pieces on the roof of the car. They are outlined on the figures included.

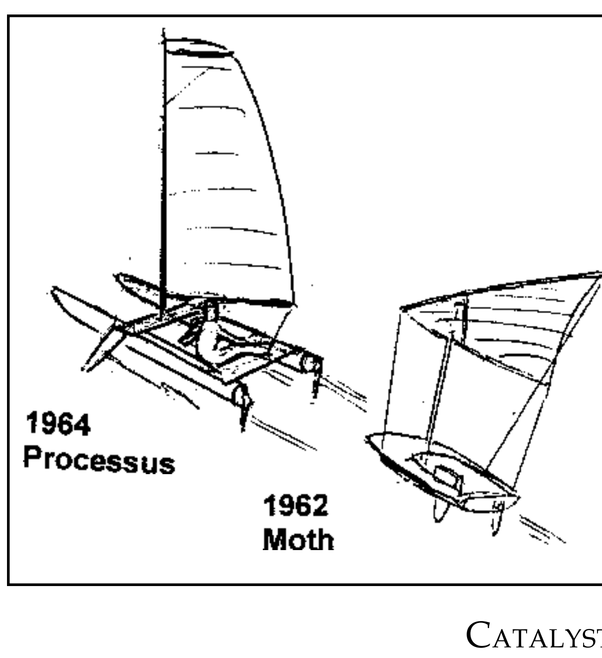
At the beginning of the Sixties, I equipped a Moth with a directional triangular aerofoil of 6 m^2 articulated at the top of a mast with the idea it should fly like a kite. The force produced, having a lifting component, would as the boat heeled, be to leeward of the hull and the centreboard. I found I could not control this sail, and I finally used it as an ordinary lateen, swivelling around the mast and not of very great interest.

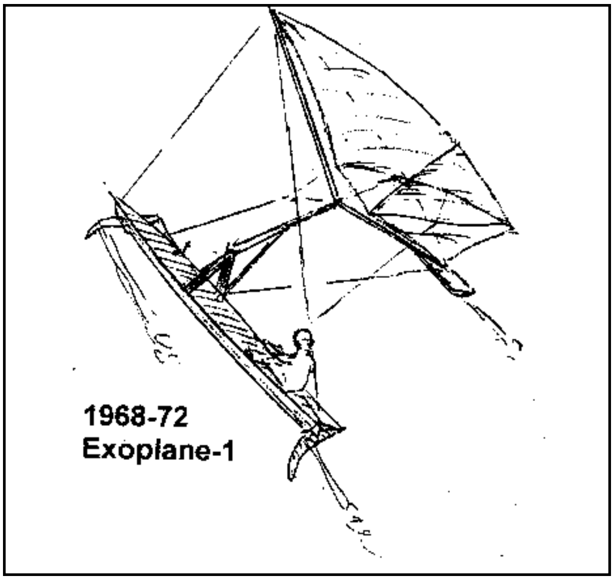
To obtain more platform stability and to control a lifting sail, I built "Processus", a catamaran with inflated floats, which was to be equipped with a lifting, non-heeling, sail and also a lifting inclined 'board, envisaged as being under the leeward float. Sailing was not possible, due to instability in yaw, except when the 'board, always lifting, was placed on the windward side of the boat, with the effect the heeling pushed the leeward float underwater. The boat remained rather slow.

I then read Bernard Smith's book " The Forty-Knot Sailboat", which reported experiments on a tetrahedral system having three thick lifting foils that also functioned as floats, and a thick canted wing-sail to leeward, which took the wind on one side or the other as required. The experiments had been conducted on models but no full-size tests. I drew the following conclusions from the reports:

- So that the machine is transportable and the aerofoil can be de-powered in the wind, one needs a fabric sail.
- Hydrofoil-floats give too much resistance at low speed. It is necessary to use a shaped hull, allowing reasonable speed, provided with thin steerable sea-wings (foils).
- According to my tests on the Seadog, stability in semi-emergence requires curving the sea-wings, so that only the quasi-vertical lower part remains immersed at high speeds.

I built Exoplane-1, a proa with a fore-aft symmetrical polystyrene foam hull and composite glass fibre skin (sheathing), reinforced by a wooden batten and carrying a fixed side arm to leeward. On this arm rested a mast with a 10 m^2 sail extended towards the end of the arm. This sail was formed of two symmetrical panels articulated along their centreline axis of tension, able to be closed (like a book) to leeward, or opened by pulling on two sheets. A strut and stays prevented closing to windward. At the ends of hull, were two foils controllable in yaw, pitching (angle of attack) and rolling ("hooking") in water. At the end of the arm was a float in the shape of wing set at a lifting incidence. The whole of the boat weighed 70 kg





1968-72
Exoplane-1

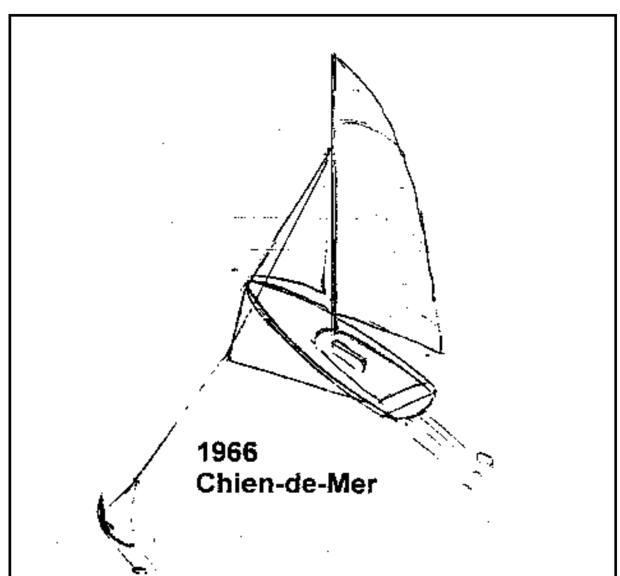
approximately. With each change of direction of travel, the pilot adjusted the foils (reducing the "hooking" at the bow), sheeting the forward sail and, sitting on the end which was to be the stern, steered onto the new course by the after foil and after sheet.

The tests showed that the leeward float hydroplaned clear of the water and without taking any weight. Initially this float was free to revolve about a vertical axis. I then used a non-revolving board, controlled by ropes so it was weight-carrying in both directions. The foil attachments, moulded out of glass & resin, were modified several times. In 1967 or 68, during sea trials, the boat would go really well a little off the wind, which allowed "flights" on outward and return courses without leeway, probably faster than 20 knots. In "flight" on the foils, the hull turned a little towards the wind, allowing the fixed arm to swing. When it luffed too much, the boat could scarcely be controlled without slowing down, putting the hull in the water, and steering with the front foil. To return downwind at the end of a run, it was necessary to let the hull drift transversely (make leeway). I noted then that the boat was weighed down by water soaking into the polystyrene hull.

I described these provisions in my patent in 1968 and believe I communicated it to Bernard Smith with my observations. Curiously, his patent in the same areas goes back only to 1972, but my precedence was not quoted by the US Patent Office.

I had invented (my patent of 1966), the "Seadog", a hydrofoil drawn by a slanting cable attached to a sailing boat or an air machine (I quoted the kite, sailplane, airship), which gave it support for sailing on the wind. Stability of the Seadog in semi-immersion, and in direction, were achieved by the curved form of the wing and its attachment to the cable by a three-legged bridle. An axial empennage under water gave stability in pitch. The tests on a 505 dinghy gave an excellent operation at low speed, but at approximately 10 knots the laid cables began to vibrate, giving great resistance and limiting the speed. It would have been necessary to keep control of the wing by attaching to parts extending above the surface, which would need more strength and rigidity, at a time when carbon fibre was unknown. I thought also of streamlining the cables using directional sheaths, but I did not carry out such tests. The developments on the Seadog are indicated in a document available from me or the AYRS office.

In 1972, I discovered the existence of the Weymouth Speed Week. I then partially dried out the foam of my Exoplane-1, by boring channels, ventilating and heating. With Weymouth, timing was provided normally only for runs in the direction chosen by the majority of entrants, and for which my boat was tuned. My tests across the wind were promising, but I broke a foil on a rock in front of the sailing base. I repaired it, after a fashion. At last, I could take advantage of a special timed run across the wind. The boat leapt forward and in mid-course I probably went to more than 25 knots, but the repaired foil broke... It was necessary to change the boat, it really was too powerful and the hull was still too heavy.

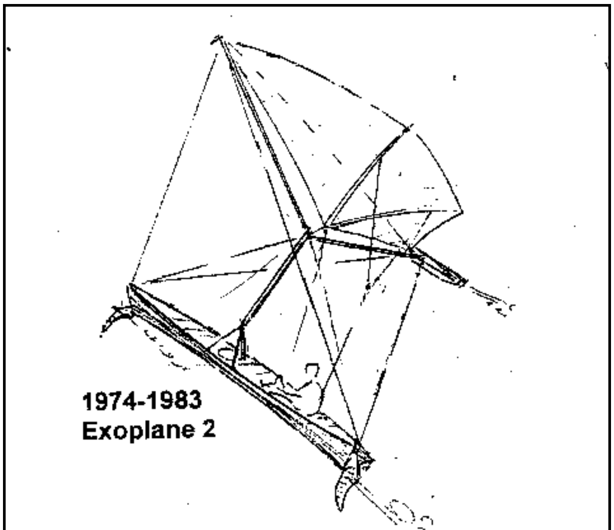


1966
Chien-de-Mer

Exoplane-2 was larger, with a sail of 13 m^2. The hull, out of riveted aluminium sheet, was partially furnished with polyurethane foam filling. The two foils were made from 7 mm aluminium sheet cold formed into an S curve. The side arm, carrying the mast, the leeward float and the sail, could be adjusted around a vertical axis, which made it possible to move the sail forward to reduce the tendency to luff. The float, assembled in an articulated parallelogram, took the aquaplaning angle set by the rotation of the arm. Instead of the "book" sail, a pentagonal sail was set with the leading edge forming an angle, supported by two convergent slats with curves limited by struts and stays. The forward part of the sail allow me to bring the traction force forward and limit the sheeting force. To change tack, one pulled in the sheet on the corner at the front of the sail "by eye", while the loose end of the sheet, which formed a loop passing around the mast, was picked up again by the pilot replacing himself on the after seat. One then readjusted the "hook" of the foils.

The shape given to the pentagonal sail by the wind gave a traction force too far aft, which created a tendency to luff, which in turn caused significant rotation of the side arm forwards, and finally loss of angle of attack of the sail.

Although the tests were satisfactory, the boat remained too uncontrollable and the riveting let in water. After sustaining some damage at Weymouth in 1975, in 1976 I achieved several rapid outward journey and return runs , and the Crossbow team, who had been following me in a motor boat, indicated to me that I exceeded 25 knots, in an ideal wind of 17-18 knots. This would have given me the category record if I had been timed, but my orientation relative to the wind did not always allow it. In addition, it was a problem to hold position, control the run up to the start, and shape a course through other waiting competitors (only one run allowed at a time), with a boat that had little manoeuvrability. Some "excursions" at high speed, when I tried to remain within the limits of the course, finally dissuaded me. The day when I obtained good control, there was little wind, resulting in a 13 knots speed... I came away with a "Design Award" and several pleasant remarks!



1974-1983
Exoplane 2

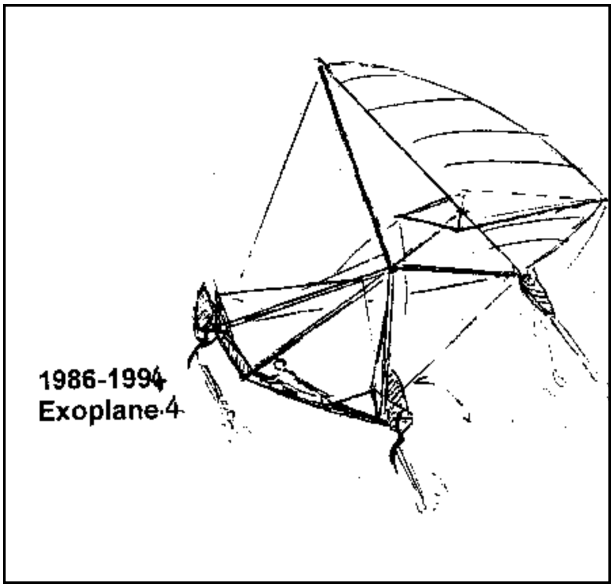
In 1977, I helped Hervé Le Goff to build a very similar boat, Exoplane-3, which also came to Weymouth. To avoid the luffing, we used a central lifting foil, with two trim-flaps both to hydroplane and to steer using the after one. The central wing had probably too much camber for the speed anticipated, which slowed it down, and it set attoo low an angle of incidence. It did not take off well. The boat, solid but heavy, with a relatively small sail, could sail in heavy weather but was not fast.

Exoplane-2 evolved with a larger sail, set in 1980 on a wing mast carefully profiled to move the traction vector forward, in spite of the removal of the front sail triangle. I tried to add a platform to windward, made of laddering, so that the leeward wing could raise its float, allowing a higher speed at the cost of an even less-controllable boat. The tests at the time of the 1983 Brest Speed Weeks were not conclusive. In 1984, at the Port-Camargue, in a strong wind, waves and with much water in the hull, I was unable to tack to return to the beach, and was saved by fishermen. The boat, left at sea on the anchor, was recovered by the maritime police who damaged most of it. I kept the principal parts.

The principle of the sea-wing plus a lifting air-wing had proven reliable, but it was necessary that the boat be easier to operate and able to run in either direction. Perhaps it would have been especially necessary, that I live at the seaside so as to be able more easily to carry out all the modifications and the tests, without limiting me to participations in speed weeks.

In 1981-82, I thought of a "Tic-Tac", a symmetrical monohull with T-shaped aerofoil carrying floats at each end.

I then decided that, since I could not rely on official timing, I had to assemble my own system. In 1983, I acquired a BBC 5 MHz computer, read-write memory of 32 KB, and cassette storage, and I wrote race management software in machine language, reassigning keys to signal the start and

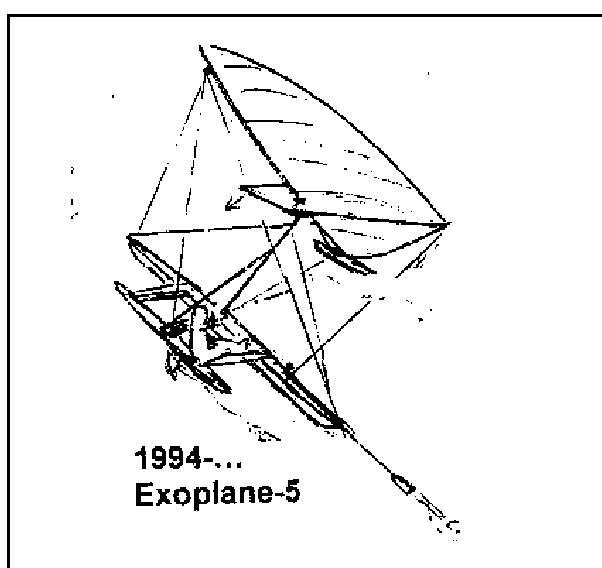


finish, withprinted output of tables of the best results for each boat. Nicolas Hurel and I assembled a complete kit, with video cameras, and in 1984 and 85 were able to arrange a speed week at Grau-du-Roi. Nicolas continued on several occasions to time races in France and elsewhere, under the name of Southwind, with the same program and a more advanced hardware. But I had not made any runs with my boat.

Then I built Exoplane-4, a tetrahedral star of four tubes connected by stays, three floats on the water, a battened canted sail of 21 m^2, weighing a total of 130 kg. The float under the sail could hydroplane in each direction, the other two floats, linked in orientation, had at their sterns 'boards canted to support lift-off. Unstable for lack of curve, they were replaced by double curved wings ("gull wings") arranged to have a suitable curve in the water. Bridging between these two floats formed an overhanging angle where the pilot could place himself as windward ballast. This evolved through three versions: aluminium laddering, then wooden square section beams, then better shaped beams out of composite, to avoid the shock loads of the waves. The boat was tested in 1989 at the Wirral site close to Liverpool, by there was almost no wind. I carried out other tests in the Mediterranean and in Brittany, and at the appropriate time at Brest Week. There were occassions when it took off on the wings and wnet fast, but the shocks from the waves caused this structure, with its multiple joints, to deform awkwardly. I continued my tests around Paris, but one day the sail and the hardware were removed by persons unknown... The remaining bits went to join Exoplane-2 on my countryside property and could be used again. By then I was busy enough with my participation in the Zeppy-2 airship project.

There was no point in continuing with important, heavy boats (I received a ruptured muscle whilst moving the boat on the beach), that were difficult to park, and took too much time for a man on his own. So, in 1995-96 I built Exoplane-5, a lightweight proa (the principal 5 m hull weighed 18 kg, hull beam and bridging were 15 kg) with a sailboard sail of 7.5 m^2. The tests at Weymouth in 1996 were not good, for lack of power. I carried out other tests with Calvi in Corsica with little wind and I wanted to continue on a lake in the Alps, but the useful elements and the sail were again stolen... In 1998, I found an enthusiast willing to use the proa with a kite, but a "discussion" on the concept held up that enterprise. Exoplane-5 is still available.

In 1997, I prepared a Seadog for use with a parapente for Pierre Falk and we spent a week on the shores of the Lac de Nantua in the Alps waiting for the right wind so we could navigate by sail after a takeoff behind a motor boat... In 1998 on the Lake of Serre-Ponçon, François Fourment and I equipped a delta wing with three profiled floats to test, after a hydroplane takeoff under tow 100 m behind a motor boat, if a Seadog could be substituted for the boat. The floating delta wing behaved well, and planed suitably, but the wind remained light. After the substitution and many incidents, the Delta came down too quickly. All that work could be repeated.



I look back now at my tests, with seven boats in 40 years, at the tests of models, the tests of alternatives with each one, at multiple repairs, never being able to stay long enough at the seaside to modify and wait for the good wind conditions... I did not get the best from the concept of the combined lift of the foils and the sail. I wish still to get to the bottom of it, without now hoping to beat a record already of the order of 45 knots, which would require a disproportionate effort in study and perfection of realization, but to pass on my extraordinary experiences, and if possible to get to 25 or 30 knots with a practical, self-contained and cheap boat.

At the speeds of 20-25 knots obtained in the Seventies, a significant share of the force from the water must be devoted to lift, obtained by setting the sea-wing and sail at a significant angle. Sailing on rough water implies a need for relatively long foils, to raise the boat high enough, but then the appreciable surface area required with available materials adds to the friction drag. If one therefore sails only where there is water without waves (on the canal at Saintes Maries for example) where you can get to high speed with little extra drag, one can adapt the area and the setting of the foils to decrease the friction drag. Lift from the sail becomes less necessary also, and great overall width is needed only to give stability in roll. These were the options chosen for Longshot.

Exoplane-5

Photo by permission of D Costes

I intend to re-equip Exoplane-5, keeping the concept of mixed lift. I have begun tests of models drawn on water by a wire simulating the action from the sail. For these modifications, being kept very busy by my activity on the airships, I would prefer to associate interested people with me.

I am open to all discussions. I hope that this synthesis will be able to help other builders.

Didier COSTES

5 June 2001

This article was originally written in French and freely translated by AYRS. Errors may be blamed on the translation.

AYRS Editor

Designing Racing Dinghies – Part 2

I sat some well-known designers round a table, and talked to others by email. The results will surface in various forms over the next year or so, but I think AYRS Members are sufficiently aware of what's being discussed to find a transcript of interest. This is the second and last part. Hope you enjoy it!

Jim Champ

Participants:

Julian Bethwaite. 18s, B14, 49er, 29er. Has a background in Industrial design, and worked for Ian Bruce (Laser designer) in Canada. Julian is now full time with Starboard Products, the Bethwaite family business which is the Australian arm of the International consortia behind the 29er and 49er.

Paul Bieker. - Worlds winning International 14 designer. Shocked the Australians by designing faster boats than they do! Professional Naval Architect

Andy Paterson – Cherubs (Patersons 1-7) and Moths (Axemen 1-7). Degree in physics and chemistry.

Simon Roberts – Cherubs (Dog, Platypus and Slug). Works for the UK Motor Industry Research Association as a noise and vibration engineer specialising in low frequency structural dynamics.

Dave Roe – Cherubs (Italian Bistro, Pasta Frenzy) and 14 (Indian Takeaway). Trained at Southampton, but dropped out of the Naval Architecture course into straight Engineering. Now works on non-destructive testing in the Building industry

JC - (to PB) So, what sort of things have you been involved in designing, I know you do 14s but ... ...

PB - Well I do keelboats; I do ferry boats; I do fishing boats; all kinds of... On the bigger commercial boats, I mostly do structural engineering and stability, but I've done a couple of water ballasted keelboats, and right now I'm working on a 41ft water ballasted keelboat. Last summer I did a pretty neat little trimaran. So a wide range of boats.

DR - Pretty much the sort of thing you do, if you're a working naval architect

PB - Sure, what you've got to do - 14s aren't going to pay your way!

DR - I sort of did a naval architecture degree.

PB - Oh yeah?

DR - "Sort of" to the extent I did b*****r all work and they didn't give me one [laughter]

PB - Life's cruel

DR - So I'm familiar with the formal training of a Naval Architect

JC - So you haven't got into anything else besides the one 14 which wasn't exactly a howling success

DR - Mainly because I have a job. I'm not going to do it as a job so I'm not looking out for commissions to design things, so I will only be designing things that I want for myself, and that's always been Cherubs. And to be honest I'm not comfortable and I'm not practised at designing other things, and I like to start with a good database. I don't want to design something I haven't got experience of, like Yachts for example. OK I could go away and study loads of designs and development, but I'm not going to do that for fun. And I've already got a job

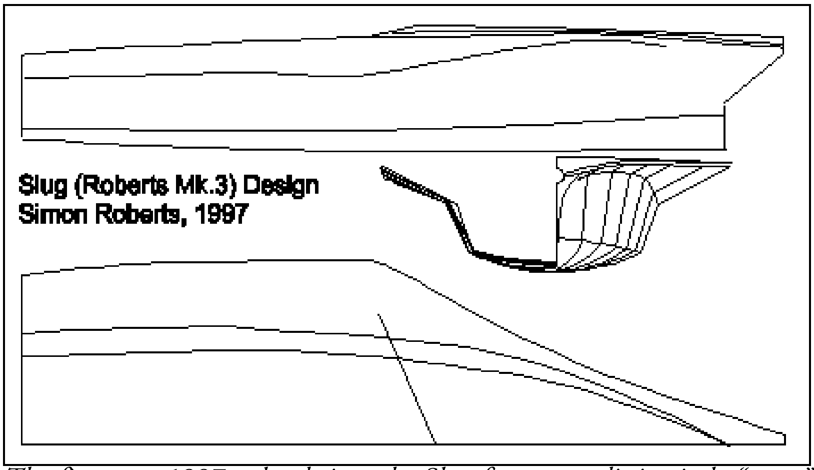
JB -Eighteens, 14s, 16s, B14, 49er, 29er, rigs, Asymmetric spinnaker system, gearing of sail plans.

JC - Something I've found is that the Bethwaites have a huge database of boats they've tow tested, and now they say "let's design a 15ft boat", and they get out the numbers and say "yes its got to be that, that and that", and that's it.

DR - I'm not always sure how they draw the conclusions they do from their detailed observations... But then scaling is just so misunderstood. And I think that's such a...

PB - Yes it's got to be dimensionless and you can only compare boats of different sizes using dimensionless coefficients.

DR - Yes, and we did this with the 14 design and we looked at it and non dimensional figures like the displacement/length ratio and power to weight ratio. It was very similar, so we thought,



The first true 1997 rules design, the Slug features a distinctively "wavy" flare line, and waterplane rather further aft than its rule-constrained predecessors.

"Wow, we can design one like a Cherub and it will work" - and we were wrong! Basically I believe it comes down to the scaling of wetted surface and actual skin drag. And that's pretty poorly understood in general.

PB - And that's what makes boat design so interesting. It's just too complicated.

DR - That's quite an interesting point because in the Cherub we're quite lucky in that the wetted surface is necessarily quite low compared to the amount of sail area and the leverage we have, so we can almost forget about it in design, and we can design no-compromise planing hulls, and we can make it flat, and hard chine, and boxy and we can get away with it in light weather, and similarly.

JC - - [interrupts] ... and what boat have ***you*** *been sailing the last 10 years Mr Roe*

DR - In terms of relative to other Cherubs to be fair it's never going to be a rocket ship when it's not planing. If you go to a much longer boat - you know like a Flying Dutchman you know it's going to stick, but I think a 14... I actually pictured a 14 being roughly in that awkward place where you can't quite jump one way or the other, you have to take account of both things - I mean certainly that was the mistake in the 14 we drew - ignoring the wetted surface.

PB - Was it pretty wide at the chines or something?

DR - Well it was just one set of chines and a planing hull Cherub thing - it would have made a great Cherub

JC - Dunno, I think it was a bit full in the bows

DR - No, no I tell you, the Bistro would have made a good 14 and the Takeaway a good Cherub, but, we know this now. It's hugely misunderstood. Someone in the Moth fleet might come along and say "No, no you want to make a Cherub like this", and have no conception of the kind of displacement we're trying to achieve and so on. We get a lot of people from other fleets coming along and telling us how we should do things, but nobody's got an overview of which bits are important at what size and I think that's key.

PB - But a lot of that comes out – not all of it – but a lot of things come when you look at displacement at waterline. For me the big thing is iterating, iterating, iterating. You can iterate a lot of times and still be getting better each time, as you worry through the issues.

DR - You try and define a balance of compromise between conflicting requirements. You've got an idea at the start of how it's going to pan out and you'll draw it and you say "No I was wrong". So you do it again, and you change it, and you change it again, so that the design process comes a learning process.

JC - It was funny how when I was drawing mine [the PlusPlus singlehander] what I ended up with was a lot different from the shape I thought I was aiming at when I started - I mean I had a target prismatic, I knew the boat would stick being 14ft long if it got the chance, (and I got it wrong), but it ended up quite a lot more V'd than I thought it would, especially around the mid sections, just because that's what seemed to get you not only the prismatic but also the wetted surface area. It's ended up quite a lot more sort of V'd in the middle than I thought it would. Not enough rocker maybe

DR - Yeah, well the last Cherub I did had a prismatic of like 0.67

PB - Whoo-hoo!

DR - Getting quite high really

SR - The Slugs higher than that, its 0.68

PB - That's getting a shoe box, you know just slightly tapered at the ends!

DR - Yeah, shoe box with a point on the front *[laughter]*. But that's what happened when we brought the beam restrictions in, reduce the rocker, then the prismatic goes up.

JC - Yeah that's the thing as that middle comes down a bit, the prismatic going to go up,

DR - Yes, it tends to go towards the waterplane

JC - And when the waterplane goes aft. . .

PB - I'm interested in what's happening in the Twelves [12 foot Skiffs] lately. Do they have a web site?

SR - We got a set of lines for Woof when Tim [Dean - a Cherub Sailor who's a professional working on America's Cup, Volvo Ocean race etc boat rigs] was down there about 18 months or so ago. It was just before they got stuffed out of sight, so must have been two years ago,

JC - And they got stuffed again this time. That's the funny thing – the Kiwis win everything and they build Australian Boats.

PB - The reason I was interested was that in 14s we've been talking about getting rid of the rise of floor rule, and the Twelves don't have a rise of floor rule. Last time I was checking out Twelves they were pretty conventional.

SR - Well I was quite surprised when I saw this drawing of Woof, I thought they were a lot wider than that on the waterline and I thought "They're as narrow as we are, basically they're just 3ft at the waterline".

DR - Yes but there's two feet of topsides as well so you've got - it doesn't matter which way you roll it

SR - Reasonably tight curvature on the turn of the bilge

DR - I remember reading an article a few years ago now where they said you had to have enough area on the topsides forward so if the boat got pressed on its side by a gust it would sail out of it rather than just being nailed and then pitchpoling.

JC - That's what you get if you put 35 foot masts on a twelve-foot boat!

DR - That's another thing - We have the potential to reduce rig weights. 35 foot of aluminium on a twelve foot boat must have been a total nightmare, but halve the weight of it with a bit of carbon and it all becomes more practical.

PB - The boat gets a lot easier that's for sure

SR - Yes, when Alan and I sailed that Twelve (in Sydney back in 1992), we were not the heaviest of people, but it was, like, the number 2 rig and it got to, like, 20 degrees of heel with us both out on the wire, and it just fell over! It was just the weight of the rig. It was an oldish twelve but it was unbelievable.

DR - Like Xerox [Grand Prix Era Murray 18 ft skiff that Dave sailed occasionally over here in the early days of the current 18 revival in the U.K.]

PB - For us it was lucky that carbon masts were just established when we went to the Big Rigs on the 14s. Right before we went carbon masts were just getting squared away and when we did it everyone was changing over from their Aluminum ones and they thought "Gosh the boat isn't any harder to sail" *[laughter]* If you ever went on an Aussie boat that had that full size aluminum rig – the boat would just tip over in the dock.

DR - That's one of the good things about the Cherub rig I have at the moment. You can lean over a spectacular distance and the boat won't fall over because the rig is so light. I mean the mast is 3kg, so in that respect the boat is so easy to sail

SR - Yes the carbon rigs have made things so much easier

DR - But having said that the moment of inertia is so much lower you have to respond so much quicker - if you're fully extended and the wind goes away you have to respond pretty quick

JC - Yeah I think we ended up too stiff on the tin ones to keep the power on.

SR - Mmm, but also you needed them to be very stiff low down and then you couldn't get the taper to be stiff at the bottom and floppy at the top

DR - Couldn't weld enough taper in it

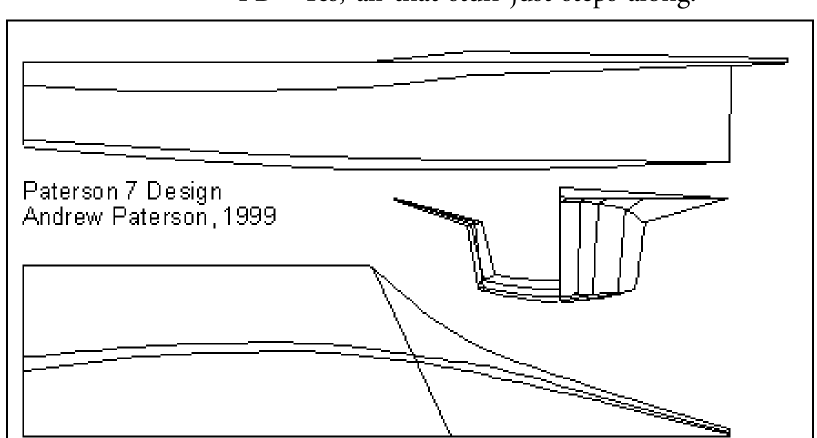
SR - Yes, but now with the laminates . . .

DR - You can just build what you want . . .

PB - And you can start your taper from way low

DR - Which then kicks into your hull design. You go "OK, well I can just push the hull a little bit further because I've got a bit in hand with the rig", and the all up weight of a Cherub has dropped maybe 20kg in the last fifteen years because of lighter foils, spars, everything else – and now you go "well I'll have less rocker cause I can plane a little bit earlier" . . .

PB - Yes, all that stuff just steps along!



Like the Slug, the Paterson 7 has the waterplane further aft than its predecessor. The shape of the boat is characteristic Patterson, with very vertical topisdes, and the flare is particularly flat.

And later by Email…

JC - - Do you build models at all?

PB - -Not sailing models

SR - No

JB - Only for the purpose of industrial design, not for hull shape.

JC - - I note that a lot of top dinghy designers are also top sailors in their classes. (All you guys, Farr, Murray, Bowler, Proctor in his day etc.). Is that because you have a better feel for what's fast do you think or just because if you don't win no one else will build your designs!

PB - - In 14's you are balancing so many conflicting characteristics - speed vs. control, light air vs. heavy air performance, upwind vs. downwind speed. All of the balances are basically statistical, requiring the designer to decide on the relative importance of conflicting characteristics over a typical range of conditions. To do this well requires a good understanding of the boat - so sailing them oneself helps a lot.

SR - Don't know. Helps to know the boat well though. Also more committed maybe

JB - Unless you immerse yourself in a class you cannot hope to have a full understanding of the idiosyncrasies of that particular class and therefore cannot hope to be able to fully exploit all their benefits.

The cream will always float to the surface, if you do a better job, and stick to it, it does not matter where you end up, with time you will be either copied or recognised. It helps if you can sail fast.

JC- We talked about target numbers a lot, especially prismatics. Where do you get target numbers from - similar boats and take an opinion on which way to go or what?

PB - -It depends on the displacement to length ratio of the boat and how hard it is driven in non-planing conditions. The heavier the boat the more important prismatic coefficient is, and the harder the boat is driven, the higher the optimum prismatic. But everything in moderation - you can't afford to be a victim in light air.

SR - Yes, but ranges are well known and published. We are not quite along standard lines, but you get a feel for it from the literature and then modify according to similar boats/classes.

JB - I have never used prismatics in my life and have no idea of how you would use them to achieve anything worth putting on the market!

JC - - We discussed how you take a starting transom immersion value, but what do you use as a point to take a representative prismatic from?

PB - - In a fourteen I use a baseline prismatic with about 35mm static transom immersion. This is approximately the depth where water will leave the transom cleanly in moderate air.

SR - Upwind trim. Downwind looks after itself mainly

JC (to JB) - Your whole family is noted as being heavily involved in dinghy development, your father having taken a much more numeric approach with consistent data gathering than anyone else in the world that I know of. Yet from his book it reads almost as if Mark's Dribbly was a clean sheet of paper approach resulting in a breakthrough boat, and since then, to an extent, it's been more an evolutionary development in hulls. Any thoughts on that?

JB - Mark is very deliberate and a no-nonsense sort of person. Someone saying that this works or that does will not wash with Mark and as such he brutally "cuts through the crap". You need a Mark in every class, but they also cause plenty of upheaval!

In some ways, since then we have done 4-5 upheavals along the way to where we are at, and with evolution, principally brought on by the advancement of materials, there will be more such upheavals.

To some extent, we are probably getting, on one hand, very good at optimising the direction in which we are presently going, but on the other, at risk of becoming staid ourselves. We go to great lengths to investigate even the most absurd ideas that we come across to try and ensure we do not become stuck in our ways. HSP project(s) are very much a case in point.

If you wanted me to pick our "clean sheets of paper" most obvious would be the Prime two handers and the asymmetric spinnaker. Also those boats, especially Mk III, were the start of the humpless hulls and automatic rigs. B14, followed by the 49er and then again the 29er, has lifted what we could possibly hope to achieve in terms of given performance. The biggest difference in the 18teens was between the Mk IIC, and the Mk III. To some extent we were fooling around with the 18teens previous to [doing] the Mk III, by then [the time we did it] we knew what we were doing!

Jim Champ
<jimc@hjones.compulink.co.uk>

ROTORS REVISITED

Joseph Norwood

In my recent book, Twenty-First Century Sailing Multihulls (AYRS #120), I discussed three advanced sailing rigs: wingsails, turbines, and rotors. The first of these is being developed by John Walker. The second scheme, turbines, which are theoretically capable of sailing directly to windward and downwind faster than the wind are impractical as nearly as I can tell. The third scheme, rotors, has a lot to recommend it (I think) and has not been taken up in a serious way by anyone since I wrote AYRS #120. Thus I had best try to nudge it along a bit more.

I have no serious doubts that the lift and drag coefficients that I gave, as obtained from the work of Alexander Thom, are by and large correct. The important question of the power required to turn a full-scale rotor is much less certain, and it is that question that I will address here.

In AYRS #120, I took Thom's windtunnel data on power input and scaled it using the Reynolds number to obtain the results that I reported. In January of 1998, I got a letter from Richard Varvill, an aeronautical engineer in Bristol. Mr. Varvill had several criticisms and suggested another approach to the problem, essentially by calculating the skin friction drag force

$$\mathbf{F} = \int_0^A \tfrac{1}{2}\rho_a U^2 C_f \, dA \quad \text{....... (1)}$$

where $\rho_a = 2.38 \times 10^{-3}$ slug/ft^3 is the mass density of air, U is the azimuthal component of the net air speed seen by the rotor, C_f is the skin friction coefficient, and the integral is over dA, the increment of area. With the torque N defined by

$$\mathbf{N} = \int \mathbf{F} r\,dr \quad \text{............. (2)}$$

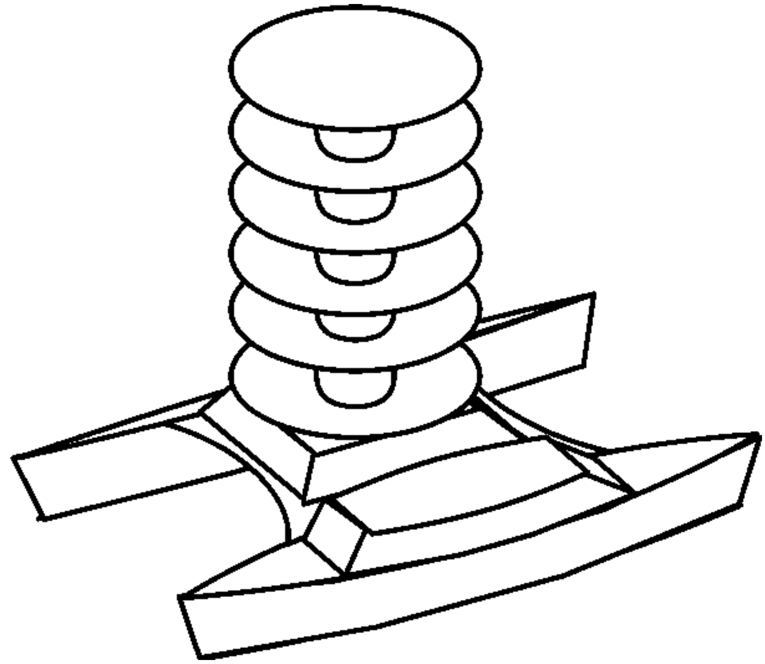
and the power by

$$\mathbf{P} = \omega \mathbf{N} \quad \text{................ (3)}$$

where ω is the angular frequency of rotation (radians per sec), we can now calculate the power input to skin-friction drag, which can then be compared with Thom' s scaled measurements.

Let's do the calculation in two steps, first considering the unfenced cylindrical rotor and then adding in the effect of the fence discs.

The configuration of the canonical Thom rotor features a rotor radius *a*, fences separated by a distance 1.5*a*, and a fence radius of κa, where $\kappa = 3$. That being the case, we can consider the power input to one "cell" of the rotor (a cell is defined to include a length 1.5*a* of the rotor plus two fence surfaces), and then get the total power by multiplying by the number of cells.



Considering first one cell of the unfenced rotor and referring to Eq. (1)

$$\mathbf{U = 2V\sin\theta} + v \quad\text{.................... (4)}$$

where V is the apparent wind incident on the rotor and v is the peripheral speed of the rotor so that

$$v = \omega a \quad\text{........................... (5)}$$

The area increment is

$$\delta A = \tfrac{3}{2} a^2 \delta\theta \quad\text{....................... (6)}$$

Using Eqs. (1) and (4)—(6), we find

$$F = \tfrac{3}{4}\rho_a a^2 C_f \int_0^{2\pi} (2V\sin\theta + \omega a)^2 \, d\theta$$

$$= \tfrac{3}{2}\pi\rho_a a^2 C_f (2V^2 + \omega^2 a^2) \quad\text{............ (7)}$$

and the power input to the rotor is

$$P_R = Fa\omega = \tfrac{3}{2}\pi\rho_a a^3 \omega C_f (2V^2 + \omega^2 a^2) \quad\text{................................. (8)}$$

Using the definition introduced in AYRS #120

$$\alpha = v/V = \omega a/V \quad\text{or}\quad \omega = \alpha V/a \quad\text{.......... (9)}$$

we can write PR in the alternate form

$$P_R = \tfrac{3}{2}\pi\rho_a C_f a^2 \alpha V^3 (\alpha^2 + 2) \quad\text{........ (10)}$$

Now let's calculate the power input to the two fence surfaces associated with each cell. In this case, the friction force element is

$$dF = \left(\int_0^{2\pi} \rho_a U^2 C_f r . d\theta \right) dr \quad\text{.......... (11)}$$

with

$$\mathbf{U = 2V\sin\theta + \omega r} \quad\text{................. (12)}$$

We calculate the torque N as

$$N = \rho_a C_f \int_a^{\kappa a} \int_0^{2\pi} [2V\sin\theta + \omega r]^2 r^2 . d\theta . dr$$

$$= 2\pi\rho_a C_f a^3 \left[\tfrac{2}{3} V^2 (\kappa^3 - 1) + \tfrac{1}{5}\omega^2 a^2 (\kappa^5 - 1) \right] \quad\text{.............................. (13)}$$

Thus the power input to the two fence surfaces is

$$P_F = 2\pi\rho_a C_f a^3 \omega [\tfrac{2}{3} V^2 (\kappa^3 - 1) + \tfrac{1}{5}\omega^2 a^2 (\kappa^5 - 1)] \quad\text{.............................. (14)}$$

or, in the alternate form

$$P_F = 2\pi\rho_a C_f a^2 \alpha V^3 \left[\tfrac{2}{3}(\kappa^3 - 1) + \tfrac{1}{5}\alpha^2 (\kappa^5 - 1) \right] \quad\text{.............................. (15)}$$

Adding the contributions from Eqs. (8) and (10) to those of Eqs. (14) and (15), we find the total power required to drive one cell of the rotor at angular frequency ω is:

$$P_T = \pi\rho_a C_f a^3 \omega \Big\{ V^2 [3 + \tfrac{4}{3}(\kappa^3 - 1)] + \omega^2 a^2 \left[\tfrac{3}{2} + \tfrac{2}{5}(\kappa^5 - 1) \right] \Big\} \quad\text{............. (16)}$$

or, in terms of a = v/V

$$P_T = \pi\rho_a C_f a^2 \alpha V^3 \Big\{ 3 + \tfrac{4}{3}(\kappa^3 - 1) + \alpha^2 \left[\tfrac{3}{2} + \tfrac{2}{5}(\kappa^5 - 1) \right] \Big\} \quad\text{.............. (17)}$$

Let's calculate two practical cases and see how the results compare with those previously obtained. For the model rotor I built: a = 0.083 ft, κ = 3, ω = 377 radians per sec, V = 0, and the number of cells was 8. For this V = 0 case, Eq. (16) becomes

$$P_T = \pi\rho_a C_f a^5 \omega^3 \left[\tfrac{3}{2} + \tfrac{2}{5}(\kappa^5 - 1) \right] \quad\text{...... (18)}$$

At this point, we need to address the value of the friction coefficient C_f. This is given (see my High Speed Sailing -Design Factors, p.12) by

$$\mathbf{C_f = 1.369/\sqrt{R_e}} \quad\text{for}\quad \mathbf{R_e < 2 \times 10^5} \quad\text{........ (19)}$$

or $\mathbf{C_f = 0.472/(\log_{10} R_e)^{2.58}}$ for $\mathbf{R_e > 1.5 \times 10^6}$. (20)

where R_e is the Reynolds number defined by

$$R_e = \frac{\rho \alpha u l}{\mu} \quad\text{..................... (21)}$$

u being a typical velocity, which we will take to be

$$\mathbf{u = \omega a = \alpha V} \quad\text{.................... (22)}$$

and l is a typical length, which we will take to be $l = a$. The quantity μ is the viscosity of air (3.8×10^{-7} slug per ft sec at 20 deg C). Thus Re = 1.63×10^4 and we use the Blasius formula appropriate to laminar flow, Eq. (19), to calculate C_f=0.011. Equation (18) then gives for the power input to my model rotor

$$\mathbf{P_T = 13.7 \text{ ft-lb/sec} = 2.49 \times 10^{-2} \text{ hp}}$$

which is well within the capability of the 1/3hp electric motor used to turn it and is less than the 0.3 hp calculated from the scaled Thom data.

Unfortunately, this tells us nothing.

For Thom's example (see AYRS #120), a = 0.5ft, κ = 3, ω = 1200, V = 100ft/s, and α = 6. There are 8 cells. In this case, Re = 1.88×10^6, the flow is turbulent (supersonic at the edge of the fences), and Eq. (20) gives C_f = 0.0041. We then find for the power required

$$\mathbf{P_T = 1.31 \times 10^6 \text{ ft-lb/sec} = 2392\text{hp}}$$

much greater than I got from the scaled Thom data (118hp), but about half of his erroneous estimate.

So who knows where the truth lies? Strictly speaking, we should have included the drag contribution of the outside surfaces of the end-fences (two more fence surfaces), but owing to the uncertainties, this refinement hardly seem justified.

Two conclusions are clear. First, full-scale power input requirements need to be ascertained by experiment. I will give some thought as to how this might be done within the scope of amateur effort. Please dive right in and don't wait on me, however.

The second conclusion is evident from an examination of the term in brackets in Eq. (17)

$$\theta = \frac{P_T}{\pi \rho_a a^2 V^3 C_f}$$

$$= \alpha\{3 + \tfrac{4}{3}(\kappa^3 - 1) + \alpha^2[\tfrac{3}{2} + \tfrac{2}{5}(\kappa^5 - 1)]\} \quad . (23)$$

This term, θ, is presented in Table 1 as a function of α and κ. There we see that the power required to turn the rotor increases sharply as κ increases, therefore it may be worthwhile to consider using fence plates whose radius is less than the canonical Thom value of κ = 3.

θ		κ			
		1.5	2	2.5	3
	2	45.4	136	366	862
	2.5	80.0	248	684	1630
	3	130	412	1150	2770
	3.5	199	639	1800	4350
α	4	289	939	2660	6440
	4.5	405	1320	3760	9130
	5	548	1800	5130	12500
	5.5	722	2380	6810	16600
	6	931	3080	8810	21500

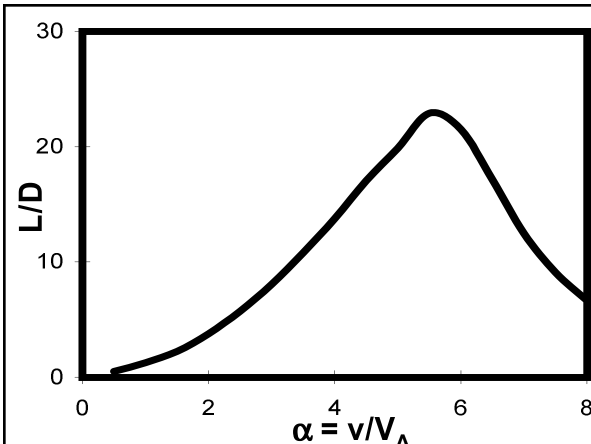
Table 1. θ as a function of α and κ.

Looking at Fig. 7-14 of AYRS #120-II (see right), we see that for α = 2.5, the practical lift-to-drag ratio of everything above the water reaches a value of 5.7, which, considering the limitations on the value of hydrodynamic drag angle, is about the maximum value that is effective in lowering the apparent wind angle, beta (see the discussion in Ch. 1 of AYRS #120).

Using Eq. (7-8) from AYRS #120, we see that for α = 2.5, the value of κ required to keep the stagnation point fenced is κ ≤ 2. Therefore, I would suggest that until we have a better handle on the power requirements that we limit κ to 2 and keep α ≤ 2.5. This will give us a value of θ of about 250 and, hopefully, limit the power requirements to practical values.

Thanks for putting up with all the equations. I hope that this discussion will provoke some experimental work to tell us once and for all whether or not high L/D rotors are a practical proposition.

Joe Norwood



α	0.50	1.00	1.50	2.00	2.50	3.00
C_L	0.75	1.70	2.80	4.30	6.00	7.80
C_{DD}	1.17	1.05	0.95	0.84	0.75	0.67
C_D	1.47	1.35	1.25	1.14	1.05	0.97
L/D	0.51	1.26	2.24	3.77	5.71	8.04
α	3.50	4.00	4.50	5.00	5.50	6.00
C_L	9.70	11.7	13.8	15.9	18.1	20.0
C_{DD}	0.60	0.55	0.51	0.50	0.49	0.63
C_D	0.90	0.85	0.81	0.80	0.79	0.93
L/D	10.8	13.8	17.0	19.9	22.9	21.5
α	6.50	7.00	7.50	8.00		
C_L	21.8	23.6	25.0	26.3		
C_{DD}	0.97	1.59	2.45	3.64		
C_D	1.27	1.89	2.75	3.94		
L/D	17.2	12.5	9.09	6.68		

AYRS #120 Fig 7-14: Lift and drag coefficients and their ratio for a Thom rotor mounted on a realistic multihull

The AYRS John Hogg Memorial Prize

REMINDER!

The Amateur Yacht Research Society announces the establishment of a Prize to be awarded in memory of John Hogg, the distinguished yachting researcher and amateur, who died on July 24th 2000. The prize, of a value of £1000, will be awarded for the most meritorious contribution to innovation in yacht science made by an amateur researcher. The prize has been donated by his family to celebrate John's life and work.

Applications for the prize, which is open to anyone of any country, whether or not they are members of the Society, should be submitted to the Secretary of the Amateur Yacht Research Society, BCM AYRS, London WC1N 3XX, to arrive by 15th October 2001. **Early/provisional application is encouraged.** Applications should be supported by evidence of the merit of the work done, peer review if any, details of publication (which may be in a recognised journal, or the Internet), and all other information that may be of use to the Prize Committee. If the work or any part thereof has been supported by grants or other funds, full details should be given. Receipt of such funds will not in itself be a bar to acceptance, but since in part the purpose of the prize is to encourage work by amateurs, it is a consideration. Research carried out as part of normal employment will not normally be eligible. All information received as part of a application will be treated in confidence.

GET YOUR APPLICATION IN NOW!!

Award of the Prize will be adjudged by a Committee chaired by Mr. George Chapman, who is himself distinguished by his contributions to sailing hydrofoils and marine instrumentation, and a long-time friend of John Hogg. The award will be announced at the London Boat Show, January 2002.

The Amateur Yacht Research Society acknowledges with gratitude the generosity of the Hogg family that has made the establishment of the AYRS John Hogg Memorial prize possible. John Hogg's writings in AYRS publications rank with those of, for example, Edmond Bruce and Harry Morss, and are a lasting memorial.

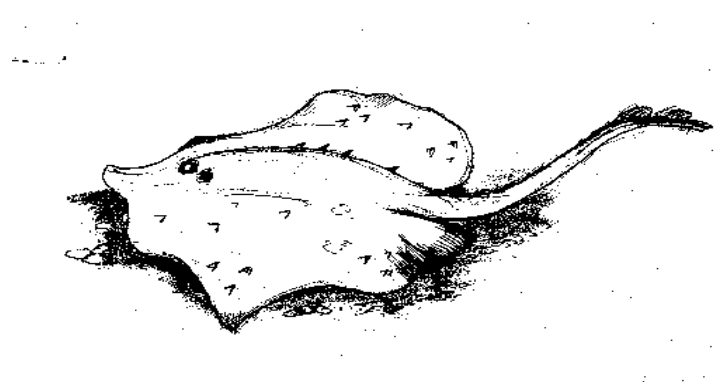
He was a good friend to the Society who will be sorely missed.

A Laminar Flow Propulsion System

Frank Bailey

I think it is fair to say there is no such thing as rotary motion in the animal kingdom. Evolution did not come up with a stator inside of which was a rotor. The legendary Hoop Snake did not form itself into a wheel, chase, and devour people. Animals, or more rightly the Kingdom Animalia, move around in their element by mostly using, in the widest sense, a reciprocating motion. Feet and legs move back and forth, birds flap wings, and fish move tails from side to side. Snakes, however, are a bit unusual. They move using friction forces I believe. With the invention of the wheel, mankind was off on a race not yet ended. One application of the wheel is a boat's propeller, which sometimes is still called a wheel.

There is another type of propelling motion not really similar to any of the above which we have probably not thought of lately. It is an undulating sine wave. Where is this system found? It is found in the cartilaginous fish called rays and skates, or to be biological correct, the Batoidea.* If you have ever waded in the shallow waters of the Gulf of Mexico and reached down for a shell and all of a sudden found your foot on top of something moving in the sand, you might have encountered a small ray and then found yourself moving in another direction swiftly using the ray shuffle. If you visited an aquarium such as The Florida Aquarium in Tampa Florida and observed some small rays you will be I hope amazed at their method of locomotion. From rest they can be in instant forward or reverse motion using the port and starboard undulations of their pectoral fins plus no doubt they can steer a course without a rudder by having one fin oscillate at a different rate than the other.

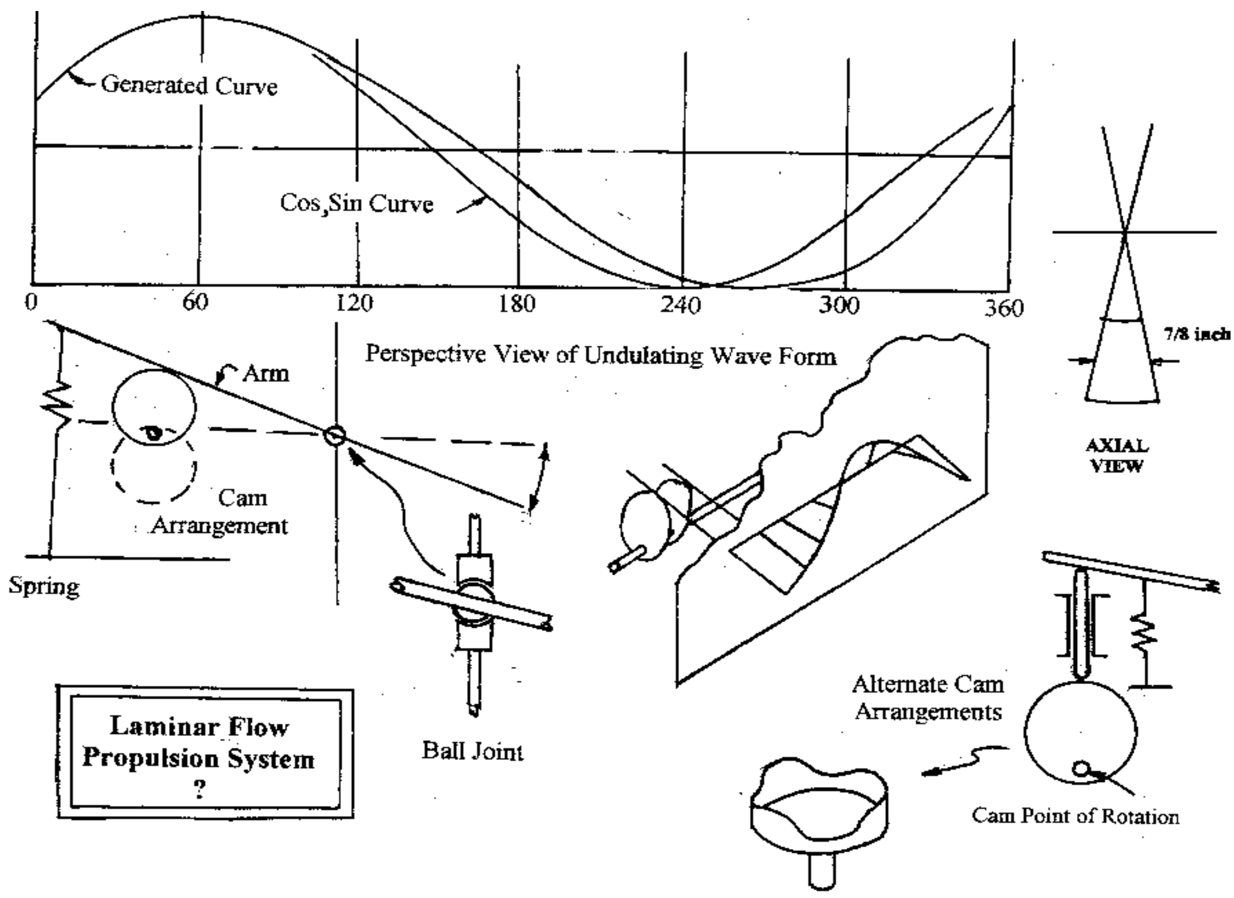


At first thought it is not immediately apparent how forward or reverse motion is achieved but it appears the undulating sine wave works exactly the same as a propeller, that is by giving an amount of velocity to a mass of water. Thus the old "axial momentum theory" of the familiar propeller can be used for the basic theory of what I would like to call "raymotion". However, and in addition, where propeller theory uses such variables as propeller diameter, revolutions per second, pitch, blade area, and a few more such items you can find in any propeller design handbook, in this instance we have to consider, but are not limited to, oscillations per second and wave amplitude and perhaps other non-dimensional variables. The rays use multiple wave lengths, one behind the other, as did the early propeller designs using a true helix instead of just a portion of a helix for a blade. I am not at all certain if the rays actually have chosen an exact sine wave for their motion but philosophically it would seem so and that over millions of years of evolution, it would seem the best shape of wave would emerge as being the most efficient and energy conserving and it is sort of comforting to surmise that an exact sine wave would finally be chosen but you may draw your own conclusions or non-conclusions from all of this.

* For you biologists, to be specific, we must go through the following routine before we get to the rays and skates: Kingdom Animalia, Phylum Chordata, SubPhylum Vertabrata, Class Chondrichthyes (cartilaginous fish) Subclass Elasmobranchi, finally Superorder Batoidea (rays and skates, about 480 species).

The above led to an actual experiment in which a mechanical device was constructed to simulate the driving motion of the ray. Thirteen circular cams were arranged on a ¼ inch rotating shaft, spaced about 5/8 inch apart, representing every 30 degrees from 0 to 360 degrees. Referring to the graph of the actual generated curve you will see it follows only closely the sine/cosine curve. If you are interested in the geometry of the thing, I have shown how you get from the cam diameter to the wave height. A cam could be designed which would give you a sine curve but the circular cams were chosen for their ease of manufacture and the time available for the experiment (Are we not always in a hurry?) There may be other geometrical arrangements and mechanical linkages for obtaining the wave form desired. Why not try and figure one out?

Thirteen brass rods 12 inches in length and .081 inches in diameter were employed as the oscillating arms. One end of the rods rested, with the help of small rubber bands, on the cams, which had side edges to keep them from wandering off the cam. On the other end of the rods was cast a thin silicone rubber flap 7 ½ inches long and 3 inches wide, the rods being embedded in the rubber. As the rubber makes the undulating motion, it will stretch and give in some unknown way so that is why the inner end of the rods must be constrained longitudinally along the rotating axis. The end view of the flap in motion would be a trapezoid, sort of, since the flap is attached to the rods which radiate from a row of holes drilled in a straight line as compared to the propeller whose axial view is of course a circle. Thus the average wave amplitude from minus to plus was a mere 7/8 inch. The flap was about 1/8 inch in thickness. The entire device was mounted on a suitable floating platform (actually pieces of Styrofoam) so that the rubber membrane was vertical in the water and about 1 inch below the surface. Note that two horizontal undulating wave forms are utilized by the rays. A small 12 volt electric motor was use to drive the system. The largest unknown was what cycles per second should be used. With gears taken from the scrap box, the eventual cycles per second at 6 volts were 4. At 12 volts, the speed of rotation was too high for visual measurement.



The only measurements actually taken were static thrust tests at two different voltages. This was compared with a two inch diameter three bladed propeller. The thrust was measured with a spring of known spring constant, which was 2.44 oz. per inch of extension.

Although the wave form mechanism was a quick and dirty job built only to get a few measurements, on the whole it behaved quite remarkably well. Acceleration of the moving platform was almost instantaneous and equal or more than equal visually to the propeller. The measurement of the results was quite crude not knowing the RPM of the motor at various voltages. Future analysis of the mechanism would of course require an accurate method of measuring the oscillations per second which means knowing the motor characteristics in relation to its voltage input or having on hand an accurate RPM counter. In any event the machine worked remarkably well. When I first got this concept, I was very uncertain whether any thrust would be developed at all.

If there is to be any future research, there are several items to be examined. You would want to run a series of experiments as you would with a propeller but with the proper change of parameters, for instance oscillations per second versus rev. per sec. for the propeller. You would want to find out how thrust changes with wave amplitude. Is it linear? At any particular wave amplitude, is thrust proportional to total area of the submerged membrane? What about end conditions? I suspect that laminar flow could be achieved with useful thrust if the proper combination of oscillations per second and membrane area were used. Low oscillations per second and large area might achieve this. The 12 volt thrust caused quite a bit of agitation in the water and probably was not laminar but the 6 volt trial could have been. Over all, the analysis of the system could take on the form of the different propeller analysis systems now in vogue, that is with dimensionless ratios, etc. Further, steering without a rudder could be achieved with a port and starboard membrane oscillating at different rates. I suspect the end view angle of orientation of the membrane with the surface of the water has no effect. Further, would there be any advantage to using this system if it achieved laminar flow comparing this with the average commotion caused by the wake of your ordinary propeller? The reader might think of other items to consider.

A few thrust results are as follows.

2 inch dia. Prop	at 6 volts .	4.1 oz.	or 1.3 oz. per square inch of end view cross section
Wave	at 6 volts	4.1 oz.	or 1.6 oz. per square inch of end view cross section
2 inch dia. Prop	at 12 volts	9.2 oz.	or 3.1 oz. per square inch of end view cross section
Wave	at 12 volts	10.4 oz.	or 4.0 oz. per square inch of end view cross section

From the above measurements, it appears the raymotion (or skatemotion) described is at least comparable to the propeller it was tested against.

I have tried to keep this article short to leave room in this publication for the experiments of you readers. In closing, it appears the rays and skates have achieved a remarkable means of propulsion, which may or may not be useful to us.

Frank Bailey, Toad Hill Boat Shop

Catalyst Calendar

This is a free listing of events organised by AYRS and others. Please email details of events for possible inclusion to: Catalyst@fishwick.demon.co.uk, or send by post to Catalyst, BCM AYRS, London WC1N 3XX, UK

September

4-9th Amsterdam Seaport Boatshow
In-water show at the Seaport Marina, IJmuiden, Netherlands, Website: www.hiswa.nl

14th -16th The KSS Tour of the US. – Tacoma Wa.
Learn how to build a hull in a week-end! By Derek Kelsall – COST – prepaid $400 three weeks before the start date. 50% to reserve your place. $450 after that date, payable before the workshop begins. Couple - $600. General details from – Derek Kelsall, R.D.2, Waihi, New Zealand. Tel 0064 7863 3332. fax 0064 7863 7915. email: Derek@kelsall.com

Tacoma contact – Tom Hales, 608 Hi-Ab- La Place, N.E., Tacoma, Wa. Tel 98422-1702. e-mail. betty@pacifier.com

14th-23rd Southampton Boat Show
Myflower Park, Southampton, UK. Website: www.bigblue.org.uk / html/boatshows.cfm

29th-5th Oct Weymouth Speed Week
Portland Sailing Academy, (old RNAS helicopter base) Portland Harbour, Dorset UK. Contact: Bob Downhill, 40 Collingwood Close, Eastbourne, UK; tel: +44 (1323) 644 879 email: robert@speedweek.demon.co.uk; http://www.speedsailing.com

October

3rd "Speedsailing"
AYRS meeting 19.00 for 20.00hrs at the Royal Dorset Yacht Club, Weymouth, UK. Contact: AYRS Secretary, BCM AYRS London WC1N 3XX; tel: +44 (1727) 862 268; email: ayrs@fishwick.demon.co.uk

5th - 7th The KSS Tour of the US. - Fort Lauderdale. Fl.
Contact: Bill Burpee, tel: 954-557-6690; email: waburpee@earthlink.net

19th - 21st The KSS Tour of the US. - Alvin, Tx
Contact: Robin Shaw. 10675, County Road, 583, Alvin, Tx. 77511. Tel 281-331-4535; email: robin@robinshaw.com

25th - 28th The KSS Tour of the US. - Alamo, Ca.
4 days. – to include sail making. $500 pre-registered. $550 otherwise. 3 days – as above. Contact: Ray Walker. 147 Cross Road, Alamo. Tel 925-362-8245 email: rayiw@Yahoo.com

November

6th AYRS London meeting
Subject to be announced. 19.30 for 20.00hrs at the London Corinthian Sailing Club, Upper Mall, London W6. Contact: AYRS Secretary, BCM AYRS, London WC1N 3XX, UK; tel: +44 (1727) 862 268; email: ayrs@fishwick.demon.co.uk

December

4th Proas: a panel discussion
(Speakers to be announced.) AYRS London meeting 19.30 for 20.00hrs at the London Corinthian Sailing Club, Upper Mall, London W6. Contact: AYRS Secretary, BCM AYRS, London WC1N 3XX; tel: +44 (1727) 862 268; email: ayrs@fishwick.demon.co.uk

January 2002

3rd - 13th London International Boat Show
Earls Court Exhibition Hall. Those who can, from 16th December onwards, give a day or two to help build/staff the AYRS stand (**reward - free entry**!) should contact Sheila Fishwick tel: +44 (1727) 862 268; email: ayrs@fishwick.demon.co.uk

12th AYRS Annual General Meeting
19.30 for 20.00hrs at the London Corinthian Sailing Club, Upper Mall, London W6. Contact: AYRS Secretary, BCM AYRS, London WC1N 3XX; tel: +44 (1727) 862 268; email: ayrs@fishwick.demon.co.uk

February

5th AYRS London meeting
Subject to be announced. 19.30 for 20.00hrs at the London Corinthian Sailing Club, Upper Mall, London W6. Contact: AYRS Secretary, BCM AYRS, London WC1N 3XX, UK; tel: +44 (1727) 862 268; email: ayrs@fishwick.demon.co.uk

March

5th AYRS London meeting
Subject to be announced. 19.30 for 20.00hrs at the London Corinthian Sailing Club, Upper Mall, London W6. Contact: AYRS Secretary, BCM AYRS, London WC1N 3XX, UK; tel: +44 (1727) 862 268; email: ayrs@fishwick.demon.co.uk

Catalyst — *a person or thing acting as a stimulus in bringing about or hastening a result*

On the Horizon . . .

From Hulls to Boards to Foils — Rich Boehmer
Electric Propulsion Design — Theo Schmidt
Proa Foil Sections — Tom Speer
The Maximum Speed of Yachts — Bob Dill
Alerion Electric Auxiliary Conversion — Charles Houghton
More sources and resources: reviews, publications and Internet sites

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