Wave-Riding Dolphins: How Do They Do It?

At present only the dolphin knows the answer to this free-for-all in hydrodynamics.

P. F. Scholander

One of the thrills of ocean travel comes with the sight of a school of dolphins speeding along through the waves. At more or less regular intervals the animals leap elegantly through the air, often the whole school in unison. These leaps, playful as they often seem, serve in fact their respiratory needs, for while the miniature whales are shooting through the air they complete in a split-second performance their exhalation-inhalation cycle.

Commonly, some individuals will part company with the school just to come dashing to the bow of the ship. An attentive observer leaning out over the railing can watch them speeding along close to the bow, and he may even hear them whistling together, while streams of bubbles escape from the blowhole; but unless already briefed, he is likely to miss the fact that the porpoises just seem to be "standing" there motionless, as if getting a free ride. This puzzling situation was pointed out by Woodcock (*1*), who gave his keen observations in the following words.

"Dolphins in the Gulf of Panama have been seen moving through the sea at a speed of ten knots, their entire bodies showing no apparent swimming motion. This performance was confined, in my observations, to the area immediately forward of the stem (prow) of a seagoing tug and to an estimated depth of one metre or less. Elsewhere near the bow, vertical oscillations of dolphins' tails were readily timed with a stop watch.

"When this 'motionless' swimming was first noticed, the animals were in the normal swimming position. In this position it was difficult to be sure that vertical motions of their tail surfaces were not occurring, since the direction of such motions would be nearly parallel with the line of sight of the observer. However, on several occasions dolphins were seen to turn on their sides during the 'motionless' swimming in such a position that their usual swimming motions would have been normal to the line of sight. No motion was visible in these animals, which were clearly seen just below the surface of the water. One animal remained on its side in this manner for 59 seconds, which represented a distance, at 5.15 m/sec. (10 knots), of 304 meters. At this time dolphins swimming near by used 1.9 tail oscillations per second in keeping pace with the vessel.

"These 'motionless' dolphins seemed to be riding the bow wave (that is, falling down the inclined surface). However, if dolphins are equal in weight to the weight of the water they displace, wave riding is not possible."

Buoyancy

Thus, it becomes necessary to find out about the buoyancy of these animals. Near the surface this depends largely upon the amount of air they carry in the lungs. Like other whales, they dive typically on inhalation and are then lighter than water (*2*), but, as pointed out by Woodcock and McBride (*3*), they have often been observed in aquaria to let out so much air while under water that they sink. The question then is: "Provided a porpoise exhales, will he become heavy enough under water so that he can plane or coast down a 15° wave slope at a speed of 10 knots, maintaining all the while his position in the wave?" It was established at Marine Studios, Florida (*3*), that a 200-pound porpoise, when dead and deflated, sank with a force of 9.2 pounds. The forward component at a 15° angle would amount to only 2.4 pounds, but on the basis of calculations of the drag, it was proposed that this slight force might still suffice for propulsion, provided the flow were laminar; if the flow were turbulent the force would be much too small.

While this explanation seemed severely limited in its application, the problem became even more challenging when Hayes (*4*) proposed that a porpoise would be propelled along in the front slope of a wave even when it was neutrally buoyant. By two different methods of calculating the hydrodynamic forces involved, he came to the conclusion that it is the total weight that matters and not the excess weight, and that the force available for propulsion would therefore be ample to overcome the drag, even if the flow were turbulent. This surprising solution was also given in the following general terms: "A third equivalent point of view is that an immersed body with no excess weight would be acted upon by a force which would give it the same acceleration as the fluid in the same vicinity." The validity of the latter formulation can hardly be doubted (compare the situation that particles accelerated in a centrifuge remain suspended unless they are heavier or lighter than the liquid), but as far as the wave-riding problem goes, this suggests Woodcock's idea rather than Hayes', for the acceleration in the front slope of a regular nonbreaking wind wave consists merely in a slow upward movement of the water. Only in the crest is the orbital movement of the water particles directed forward, but even so, their maximum velocity is only a small fraction of the wave velocity, commonly 10 to 12 percent, and only rarely exceeding 20 percent. In the trough the water moves backward; in the slope behind the crest, downward. If, therefore, one were to tow a streamlined body of neutral buoyancy within the wave, and at the velocity of the wave, one would expect to encounter drag in all positions, but slightly less in the crest than in the trough and an intermediate amount in the slopes—that is, the drag would simply reflect the relative velocities of the object and the water.

This was indeed found to be the case when two such objects of different shape and size were towed in the waves produced by a tug moving at 8 knots. It must readily be admitted that a fair evaluation of all the variables involved would call for rigorous testing in a tow-

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ing tank, but so far we have been unable to find evidence for the effect predicted by Hayes. It cannot be doubted, on the other hand, that a body sufficiently heavier than water could gravity-plane within the rising water of the front slope (like an aerial glider in an updraft), as suggested by Woodcock, and no doubt the reverse would also be true —namely, that a body lighter than water could "float-plane" within the falling water behind the crest.

Riding the Bow Wave

Whether or not one might find experimental support for these ideas on how to ride wind waves does not matter for our problem, for dolphins do not seem to ride wind waves, but only bow waves, and these are not necessarily identical propositions. On the contrary, it occurred to me, when Woodcock announced his interpretation, that there must be some other solution available, better than gravity-planing, because (i) porpoises regularly dive on inspiration, and are then lighter than water; and (ii) even if they were to exhale under water, the gravity vector for propulsion would be disproportionally small; moreover (iii), it seemed that an alternative explanation was at hand: the porpoise might be pushed forward by the bow wave, simply by putting its tail fluke at an angle into the upwelling water while planing its body horizontally in the ship-course, forward of the bow wave—that is, it would be picking up shear-force thrust with the fluke. This possibility avoids the buoyancy problem altogether and gives a propulsion in principle closely similar to that which the animal employs in a normal downstroke of its tail peduncle, as illustrated by Parry (*5*).

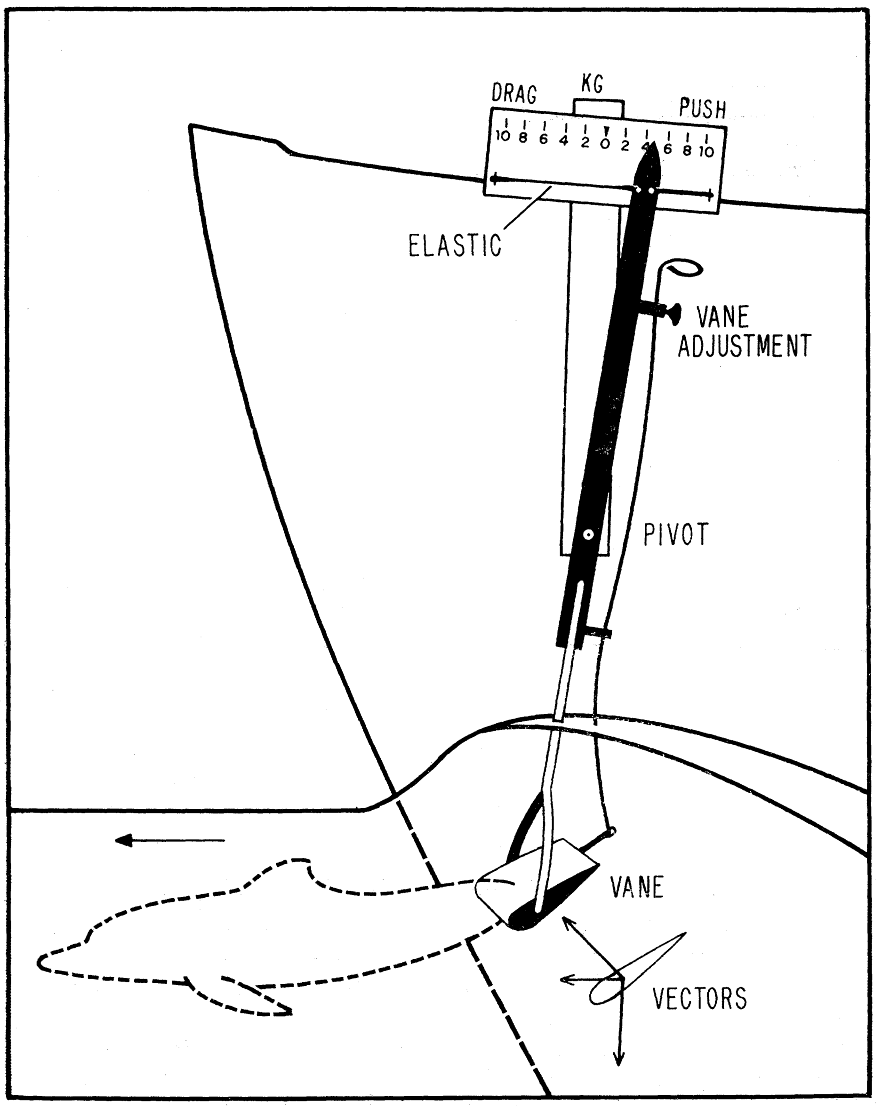

Fig. 1. Schematic presentation of the vane experiment. The relative height of the bow was about twice that shown in the drawing. The contour of an imaginary dolphin riding the bow wave is indicated.

Vane Experiment

While I was cruising in the quiet waters of a West Greenland fjord last summer on board the *M. S. Rundøy,* an opportunity was offered to test quantitatively this, by now, 10-year-old idea. The ship was equipped with a laboratory and machine shop. A streamlined vane was made, 20 by 40 centimeters in area and 4 centimeters at its thickest. It was suspended from a frame which was fastened onto the railing of the bow (Fig. 1). The vane could be clamped at any angle. Drag, or forward thrust, was measured by mounting the vane on a separate board which pivoted on the supporting frame. The board was spring-loaded and calibrated in either direction from a center position.

When the ship was going at 8 knots it was found that, as the vane was slanted downward at a 28° angle from the horizontal, it gave a considerable thrust forward, but on either side of this angle there developed a heavy drag. The forward thrust oscillated in step with the slight up-and-down pitching of the bow, from some 4 to 10 kilograms, averaging 7 kilograms. When we moved the whole gadget midships to a region of horizontal water and reset the vane, the minimum drag of our rather clumsy vane plus the submerged mounting averaged 12 kilograms. A "dragless" vane would accordingly produce a forward thrust of some 19 kilograms, and a real tail fluke of similar size but far more elegantly streamlined would hence be able to produce a thrust of some 18 to 20 kilograms at 8 knots' speed (*6*).

If, therefore, the porpoise, assisted by his pectoral fins, steers himself horizontally and "leans" his tail fluke against the upwelling water of the bow wave, he cannot help but be pushed along with the ship. As the water, in fact, is thrust not only upward and forward but also outward, he may ride keeled over on the side if he wishes. Moreover, as this mode of propulsion does not require that his lungs be empty, he need not take his ride in silence but may whistle to his fellow freeloaders as much as he deems fit. This, I believe, is the way dolphins ride the bow wave, and if it is not, they should try.

This scheme of picking up shear-force

thrust by the tail fluke suggests other possible ways of riding wind waves. If, for instance, the porpoise were cruising in the trough (or crest), he might be propelled along with the wave by sticking the tail fluke into the rising (or falling) water of the slope behind him. He could also combine shear-thrust riding with gravity- or float-planing. Since, however, these smart and playful animals evidently pay little attention to wind waves, this goes to show that none of our proposed ways of riding them can be practical. Evidently wind waves are not steep enough, and do not persist long enough, to do a porpoise much good. Only the abrupt and steep rise of a bow wave seems to be capable of giving him a worth-while push. But how does the porpoise produce the down-thrust of his tail fluke which he obviously needs in order to retain his position?

There appear to be other examples of bow-wave riding in the sea. Cousteau (7), in his book *The Silent World*, describes and shows a photograph of a tiny pilot fish which apparently rode the nose wave of a shark: "A thumbnail of a pilot fish wriggled just ahead of the shark's snout, miraculously staying in place as the beast advanced. He probably found there a compressibility wave that held him. If he tumbled out of it, he would be hopelessly left behind."

References and Notes

1. A. H. Woodcock, *Nature* 161, 602 (1948).
2. P. F. Scholander, *Hvalrådets Skrifter No. 22* (1940); L. Irving, P. F. Scholander, S. W. Grinnell, *J. Cellular Comp. Physiol.* 17, 145 (1941).
3. A. H. Woodcock and A. F. McBride, *J. Exptl. Biol.* 28, 215 (1951).
4. W. D. Hayes, *Nature* 172, 1060 (1953).
5. D. A. Parry, *J. Exptl. Biol.* 26, 24, Fig. 1 and plate 3, Fig. 9 (1949).

6. I wish to express my thanks to L. K. Coachman, O. Iversen, and H. Jensen for help in constructing and testing the bow-wave gadget. This part of the investigation was undertaken in spare time on the Arctic Institute Greenland Expedition 1958, which was aided by a contract between the Office of Naval Research, Department of the Navy, and the Arctic Institute of North America. At Scripps Institution of Oceanography I have enjoyed the benefit of discussions with W. H. Munk, and I had the able assistance of J. S. Kittredge and H. Andersen in the towing tests.

7. J. Y. Cousteau, *The Silent World* (Harper, New York, 1950).

The Forgotten Man: Sir John Lubbock

His contributions to zoology and his liberal record as a member of Parliament ought to be remembered.

R. J. Pumphrey

When Sir John Lubbock, the first Lord Avebury, died in 1913 (before the outbreak of the 1914–18 war) he was deeply mourned by thousands who knew him and revered by millions who only knew of him. By the end of that war his reputation was in complete eclipse, and it is only now and very partially beginning to emerge from an unmerited obscurity.

In a recent number of *New Biology* I found the following passage: "It is remarkable that up to 1914 there was no definite proof that bees could see colours. Everyone from Sprengel onwards had assumed it, but there were only a few experiments such as those of Lubbock (1875–6). These, though suggesting that bees possessed colour-vision, did not eliminate the possibility that they discriminated between different colours by their brightness alone. Indeed, the first full-scale experiments came from Hess (1913) who claimed that this was the case: that honey bees could not see true colours but only various shades of grey. For a time there was doubt, but in 1914 von Frisch began his classic work. . . ."

This is one example of how Lubbock's work is forgotten or, if remembered, described in such a way as to diminish its importance. It is simply not true that von Frisch proved what Lubbock had failed to prove 40 years earlier. Von Frisch does not mention Lubbock in his bibliography, and it may well be that he was only impelled to begin his color-vision work by a distrust of the work of Hess, who started out with a bee in his bonnet and was wrong about most things. Nevertheless, von Frisch's technique resembled Lubbock's very closely, and his results are open to the same sort of criticism. The final answer was given, so far as bees are concerned, not by von Frisch in 1914 but by Kühn in 1927, using pure spectral colors (a method invented by Lubbock though applied by him only to *Daphnia* and to ants). It is worth noting that Lubbock's experiments on bees were supported by extremely pertinent observations on the color sensitivity of wasps and ants, water fleas and dogs.

Lubbock answered contemporary criticism temperately and convincingly. He can hardly be blamed for not replying to the effusions of Hess, which in any case were not published till he was dying. Nothing subsequent should be allowed to obscure the fact that Lubbock was the first by 40 years to do experiments in this field, and that he got answers which, as far as they went, were absolutely right.

Yet, when I took a course in zoology at Cambridge in the twenties, although my pastors and masters spoke highly of the virtues of the experimental method (held by some of them to be a Cambridge invention), I never heard Lubbock mentioned. It is true that I never heard von Frisch mentioned either, and the extraordinary postwar development of comparative physiology in Germany passed almost unnoticed. Until I began to read for myself, I did not realize how much Lubbock had done, not only in his experiments and in pointing the way to further work, but in creating the climate of opinion in which experimental work in biology was possible.

The obscurity into which Lubbock's work relapsed after the 1914–18 war did not, however, cover only his contribution to zoology. He had been eminent, indeed preeminent, in many fields, and

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