THE

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INTRODUCTION

We've been confronted with denying "equal time" to some of our critics. This condition will now be corrected by presenting an extensive and detailed critical appraisal of some past SCATTER articles, by Glenn Kinney and John Bell of the New England Wakefield Group. The authors wrote us recently recommending that we correct the "technical errors" and "misinformation" appearing in SCATTER. We asked for specific examples and the cases they selected are reprinted as the body of this issue.

Kinney and Bell also recommended a change in our editorial policy. They contended the editor should question every technical contribution and treat it as "suspect" until its technical validity is personally verified. This policy would extend to demanding proof by the author of any technical data, performance figures, etc., which the editor could not personally determine.

That any reader should assume the SCATTER aspires to this level of technical "purity" is very flattering...but hardly true. The editor can not utilize this approach for two reasons:

1. First, we do not presume to have the personal knowledge or resources to critically judge the detailed validity of every technical contribution. And, we can only assume that anyone who would thus qualify himself, does not share our opinion that the varied aspects of FAIing still dwell at about 90% "art" and (unfortunately) only 10% "science."

2. Secondly, SCATTER was never intended as an infalliable textbook on the exact science of FAIing. Rather, it attempts to present all data, ideas, and opinions readers will contribute. Each such contribution is, and will be published as received (within our space limits)...for the enjoyment, information, or disregard of each reader...according to the dictates of his personal tastes, knowledge, experience, and opinions.

This does not imply that the SCATTER staff agrees with, or indorses, every contribution we publish. But we do maintain that every contributor has a right to presentation; in his own words.

Since we are all still on the fringes of scientific advances in modeling technology, we think it worthwhile to "air" these ideas, facts, assumptions, and , indeed, out-right guesses. Likewise, any reader compelled to question or take exception to any of this material should do so. Only through such constant seeking, often followed by substantiate or disproof from other knowledge or experiments, can we attain the building blocks of real scientific advance.

Limiting our published items to only "known" facts would stiffle this

"wheat and chaff" technique...which we believe is a practical road to true scientific advances. And we believe most other newsletter editors will agree that any publication attempting such "elite" standards would wind up with a very slim volume indeed...and, perhaps, no "qualified" contributors at all....?

A CRITIQUE' OF SCATTER by Glenn Kinny and John Bell (New England Wakefield Group)

In the August, 1961 issue, on page 5, the second paragraph discusses a Wakefield design. It is said there that the model's tendency to spin under power in the wind was cured by "moving the rudder from the conventional position to the lower position shown in the drawing." It may have been true that the model spun when the wind was blowing, although the term "spun" is probably being misused (he probably meant that it spiralled to the ground under power), but how can it be claimed that the wind was responsible? It is possible that the forward rudder was too small, which would mean that the model was unstable. Windy weather, because it is turbulent, may have kept the airplane in a constant state of recovery. If such an airplane is seriously upset it is conceivable that it would dutch roll into a spiral flight path, which the author may be calling a spin. The same rudder area, if moved aft, might have been enough to cure the instability, but he did not say that the area had remained unchanged. Instead of telling the story in such a way that the reader could appreciate what may have taken place, this writer has created the impression that there is something in the rudder positions alone that cures a model's windy weather behavior. He also left out any reference to the presence or absence of other possible changes, such as different rubber, different prop setup, warps in the flying surfaces, changes in wing-stab relationships, etc. He also says, later on, that the propeller blades were allowed to hang freely in order to "seek their own minimum drag position." By what stroke of good fortune does he expect that the blades' stable position is also the position of least drag? With so many errors of omission and commission, it is very difficult to make sense of what he says.

In that same issue, Bill Hartill (and we love him like a brother, remember) writes, "It was reasoned that sweep-back would increase longitudinal and lateral stability." Bill knows full well that longitudinal stability depends upon many factors with or without sweepback. To single out sweepback to the exclusion of the other important factors is less than reasonably accurate. He says in the next sentence that the tips of the wing were cranked to "delay tip stall without resorting to inefficient washout". There are two difficulties with this statement. The first is that there is no noticeable reason for thinking that the tip design on his wing will indeed delay tip stall for the particular angle of sweepback, the particular profile and tip shape, and the particular angles of attack and flight speeds of the airplane. Too many important factors have been eliminated to allow the reader to place any credence in the statement. The second trouble is that, since washout is usually equated with the lift-to-drag ratio, and since starched Vulture probably flies with an angle of attack of approximately eight to ten degrees, as do most Nordics, the efficiency of the wing would be increased with the washout because most profiles of the sort used on SV reach their minimum L/D at much lower angles of attack, more nearly equal to the angle of zero lift than to the angle in flight. Unless Bill has done some very fancy figuring, it is hard to make sense of what he says. If what he has said is true, and if he has evidence to indicate that it is true, then a treatment of these subjects would be far more interesting that the article which was published.

On page 2 of the September issue, there is an article on thermals by R. G. Smith. His article is so full of stuff that he is passing on to the

reader from somewhere else that it is difficult to know how to begin reviewing it. For example, he says that we may think of a thermal as a rising, whirling bubble of air. This old idea is to be found in a variety of places, even in books about full-scale soaring, but there isn't a shred of evidence for it worth paying attention to, yet. What could anyone have observed which would lead him to such conclusions? In what sense does Smith think that he is writing about thermals for us readers when all he does is to repeat stuff from sources he doesn't even bother to cite? We don't want to pick on Smith, but this sort of irresponsible authorship is the most frequently employed means of spreading nonsense and disbelief in political circles - it doesn't belong in SCATTER, or in any other similar journal. Our complaints apply to much of what else he has written.

The first sentence in the section on topography uses the word "strongly" to modify how a thermal's air is heated. Why? The next sentence, to the effect that dark surfaces are poor thermal sources, is false. The next three sentences ignore several important factors, such as, the effects upon thermal formation of humidity, the effects of convection in heating the surface air, and the simple fact that thermals form regularly over such terrain as deserts, whose topography is anything but what Mr. Smith would have us believe is necessary for thermal formation. The relationship between the surface characteristics and the formation of thermals is far more complicated than this writer would have us believe.

This first paragraph on page 3 is opinion; we shall give it no further attention. His "short meteorological discussion" and the diagrams that accompany it are very confusing. He does not understand the "Coriolis force", and we suspect that again he is passing something to us from some unspecified source. He says that since the air in a thermal is hotter and lighter than its surrounding air, it is therefore at a lower pressure; air can be, and usually is at the same pressure, except for minute local differences, as the air immediately close by, and if the air he is discussing is expanding due to its being heated more rapidly than its neighboring air, then it is at a slightly higher pressure.

We shall state without proof that when air flows toward a point, which can be called a sink, and when the air moves over the surface of the earth with a component of motion parallel to lines of longitude, then there are components of motion also to the East for air moving North, and to the West for air moving South. In this case, the resultant of forces totalled over the whole motion is such as to induce rotation in a counterclockwise direction in the Northern hemisphere, and oppositely in the Southern hemisphere. These same components produce clockwise rotation when air moves away from a point. These totals will induce the described rotation only when they are of large enough magnitude to override components due to local conditions. It is not at all clear that the forces inducing rotation are large enough to produce a noticable rotation in thermals of the sort under discussion, and it is likely that they are not large enough since the time rate of change of latitude is very small indeed. It is not clear, however, that these are the forces that produce "dust devils". If Smith would stop to think for a moment he is most surely going to recognize that a thermal is certainly not a dust devil without any dust.

Because he did not think things out well, we have the rest of his discussion to deal with. If it is supposed that the air moving toward and under a rising thermal represents a small low pressure air movement, then it may rotate counterclockwise. What is the speed of rotation? Does Smith know? Is it large enough to make sense out of his left-hand rule? Only if the air does rotate fast enough so that the rotation component is greater than the component

toward the sink point can his left-hand rule be true. We are advised, however, to tow our gliders to our left when we stand with our backs to the wind, which implies that the speed of rotation is large indeed. If the air is moving more toward the sink point than it is rotating, and if a thermal forms between two flyers standing a hundred feet apart or so, then both flyers will tow away from the thermal, won't they? Is that good advice?

Mr. Smith's statement that a rising thermal bubble is more likely to rotate counterclockwise than clockwise is not consistent with his previous argument. Since a rising bubble of air would be expanding as it rose, the air in it would act like the air in a high pressure area, according to his argument about the forces, and it would rotate clockwise. Once he has assumed that the thermal is formed when the air rushes toward the spot it is in the act of vacating, and once he has convinced himself that this air is rotating counterclockwise, he is perfectly free to state that one should stand with his back to the wind so that the thermal lies to one's left. Since his advice on how to tow one's glider is based on a fallacious argument, the reader is wise to ignore Smith's advice.

The crowning touch in Mr. Smith's article comes when he tells us that the "oppressed" feeling we experience when the wind calms is evidence of the nearness of a thermal in the act of forming. Since the human body is cooled by evaporation, any calming of the wind is likely to make us feel overheated momentarily. Therefore, the feeling is not due to some property of the air peculiar to thermal formation.

As we said earlier, it is difficult to decide just how to approach a review of his article. He has assumed so much for which there is no evidence and he has presented such an incoherent argument, that it is even more difficult for us to believe that you could have missed all the errors and weak points in his article. Surely you must think about these things now and then yourself? We realize that you have a lot to do, and that you do not have a lot of time for closely scrutinising written materials. But tell us, just how much effort is required to spot these gross negligences? Didn't you suspect something when he wrote that thermals are more likely to occur where a light colored surface borders on a dark colored one? The idea that one surface does the heating while the other supplies the necessary cold air is so outrageously hilarious that you couldn't have overlooked it.

But on to other things. In the October issue, Bill Hartill (we really aren't picking on him) wrote about the Super Thunderbird. More or less with his tongue in his cheek, we suspect, he put in parentheses the remark "low drag, high lift wing section" and the drawing shows the Go 342. Why did he do this? If he was joking, then we can appreciate it, but was he serious? If he was serious, we'd like to see his data, at the correct Re number, of course. A little later he says that the fillets decrease drag. How does he know this? All the research for models on this subject that we've ever seen was done by the LSARA, and it shows that sometimes the drag is increased by adding fillets. Later he says that the use of a tongue screwed to the fuselage in the manner used on Nordics does not permit the fillets. Why does he say this? All one needs to do is to enlarge the tongue's center section to include the fillets, right?

In the November issue, page 7, Dick Gildersleeve (and we love him like another brother) makes some interesting statements. He reports that Thompson said that the airfoil Dick used had the characteristic of "riding over the hill" instead of an "honest stall" when upset in windy weather. We suppose that if Thompson said so, it must be so, but what does it mean? How did anyone determine that the stall characteristics of the airfoil (and the con-

struction is ignored) are whatever they are supposed to be? Then Dick also makes the same remark that Hartill (and many others) have often said. His airplane "seeks" thermals. It does this by the clever expedient of turning into thermals and away from downdrafts. How does the glider know which is which, and how does either Dick or Bill know that this is indeed what the airplane is doing?

Consider a glider gliding as usual. It wanders around more or less aimlessly, which means that the flight path with respect to the ground is irregular. Of course, what the flight path is with respect to the air up there where the glider is cannot be told from the ground; it may be perfectly circular, or even quite straight. Suddenly the glider turns sharply and begins to climb in a thermal, which can be observed from the ground rather easily. The flyer immediately concludes that the airplane has sought out and turned into the thermal. How does he know that the glider hasn't passed up a few other thermals in the meantime? Now suppose that the glider had turned sharply, but had not climbed, but had continued to fly as usual. The flyer concludes that the glider has turned away from a downdraft. It may just as well have turned away from a thermal for all that one can tell from the ground. A few moments' thought will reveal that to collect the information needed to conclude with any confidence that the glider seeks thermals and avoids downdrafts (especially on a statistical basis) requires a procedure far more complicated than watching it from the ground. Do you think it likely that any of the people who claim this ability for their aircraft have ever followed such a procedure?

Skipping along to the January 1962 issue, Birger Borthne says, "It seems as if the L/D of the wing grows quite rapidly together with the speed." Unless he has a special wing warp, L/D, as the equations indicate, is independent of the flight speed. The answer to his question about whether his statement is true of airfoils like the one he used is, "No." Of course, it is always possible that his wing is undergoing a transition in flow conditions when it changes speed, but one could never conclude this from his previous remarks, nor from his later discussion of turbulators that he claims are effective. One is forced to conclude that no sense can be made of what he says. In the next paragraph he mentions a "floating characteristic". What is that?

In the same issue, on page 4, Andy Crisp echoes the old tale that a rough weather model must have a low aspect ratio wing, and stability. He says that the large decalage and the undercambered stab give quick stall recovery. What other factors are important here? Has he selected those that are paramount? Who can argue about the longitudinal stability of large longitudinal decalage? We all know that a DT'd glider is exceptionally stable, and it certainly has the large decalage, but the question is a matter of degree, which Crisp has ignored. Also, we know of nothing that is inherently stable about low-aspect-ratio wings, but we do know that low-aspect-ratio wings, other things being equal, often require higher flight speeds, and we know that higher flight speeds are not necessarily conducive to greater stability, but usually the opposite. There is no more premium on stability for a rough weather model than there is for the ordinary model, is there, because the ordinary model may even be flown in rough weather? If Mr. Crisp's model also has excellent stall recovery, then why does his model mush to the ground when it is stalled off the line? This behavior sounds like there is a large hysteresis loop in the lift curve, which is hardly consistent with good stall recovery. He can't cure such a trouble by tightening the turn and putting washout in the wing, can he? Perhaps you can see why his article puzzles us.

In the February issue this year, Charles Sotich says, "I don't think dihedral affects performance since all types have been used effectively on a variety of good flying models." This is a silly statement, of course, but if it is carried to its logical extreme, Mr. Sotich has evidence that no design feature affects performance.

In the March issue, Tor Borthne (page 2) describes his glider as a rough weather model. His design is an interesting contrast with the model that Andy Crisp calls "rough weather." Mr. Borthne's model, like others mentioned above, also seeks thermals, but this time it does so because the inner panel of one wing is washed out. A little earlier he says that 6 degrees of washout on the stab tips helps to pull the model easily out of a stall. The absence of washout, we take it from this, would make the stab pull the model roughly out of a stall. Of course, Borthne has no worries anyway because, as he so casually admits, his model does from 180 to 185 seconds in calm, cold air. We find it difficult to trust his judgment, not to mention his veracity and since he gives us no other basis on which to evaluate what he has written you should have no trouble deciding what our position is.

On page 5 of the April SCATTER, Hank Cole described his A/2. We know that Hank Cole is an excellent builder and flyer, although we've never had the pleasure of seeing him in action. When he writes, however, something seems to snap down deep inside him somewhere. He says that his model with one wing turbulated was stalling along in a low and violent thermal. What is there about a violent thermal that should make his model stall along? Wasn't it trimmed? How did he manage to find a violent thermal that stayed 25 feet from the ground? Was it, perhaps, a Smith special? What is the object of the description of the flight? Further on, we are to assume, the glider was turning to its left, and whenever it stalled and fell off, the left wing was seen to drop first, and the model turned into the low wing. Does he imply by all this that this maneuver isdue to the one-wing turbulator? Every properly trimmed Nordic we have seen will drop the inside wing wheever the local wind conditions nose it upward and it then "falls off". Perhaps this model needs one wing turbulated in order to trim it?

On the same page of the same issue, John Wordin begins an article on how to launch towline gliders. We have been launching gliders for each other here on the East Coast for some time now, and we would never, under any circumstances, advise someone to follow his procedure. When we read that the launcher should give the glider a violent toss upward, we howled with laughter. If any of us ever had a launcher give the glider a violent toss up, or in any other direction, he would be sorely tempted to abandon the flight, dart back down the towline, and raise knots on the launcher's head with the tow handle, or possibly with the weighted nosepiece from the shattered fuselage.

Theoretically speaking, did it occur to wordin that the tangent of the launch angle he shows is 8/20, whence the angle is 22 degrees? If one starts with an angle of 25 degrees, and then, with the upward heave, reduces it by 22 degrees, the glider ends up with an angle of attack of three degrees, which will stall it, even at 21.5 feet per second. How does he account for this matter in his "theory"? He had earlier said that one should not stall the glider on the launch, but he seems to be telling us that it does stall, at least momentarily. (If we hear that the glider has a large angle of attack during the launch, we will insist that measurements be made.) When he went on to say that the 25 to 30 degree "theoretical" launch angle was checked by taking pictures of people launching gliders, we literally held our sides and rolled on the floor. How did he check the angle of attack by taking such pictures, remembering that the launcher's forward speed, and the speed of the wind would have to be taken into account? Wouldn't he need to level the camera? Did he? How did he check the vertical speed of 8 feet per second?

how did he manage to conclude that 38 degrees is within the theoretically expected range from 25 to 30 degrees? What a riot!

On page 6 of the May issue, we refer especially to the statements about the Benedek 6557b and the E.J.95. What is the meaning of such prattle, seriously now? Why is attention called to the great undercamber of the E.J.95 when the Benedek has an undercamber 30% greater? Can you, or anyone else, give a good aerodynamic interpretation of "diving trim" and "tolerant performance" as applied to airfoil usefulness? What is the meaning of the sentence that uses but as a conjunction and reads "Rather high undercamber but camber high point is relatively far forward at 40%?" Relative to what? What has the 40% figure to do with anything?

Asmund Shard, on page 5 of the June issue uses the term "thermal sensitive". We've seen it other places, too, but since it appears in SCATTER can you tell us what it means? How would one go about deciding that his model had it, if you know what it is?

This brings us to the July issue. On page 3, John Wordin says that it is impossible to pay too much attention to the details of the wing. It is not! In the third sentence he says that he found the NACA 6406 in 1958. Was it lost? What is he trying to say? The first sentence of the third paragraph probably does not hold for the Reynolds number involved and if it does, we would like to see the data. Besides, hasn't he ignored the induced drag? The rest of the paragraph at the top of page 4 seems to claim that his design approximates an elliptical lift distribution across the span, and that he has attached flow throughout the speed range over the whole wing. This is an admirable state of affairs, we suppose, but how does he know he has reached it in this case?

In the next paragraph he says that the wing structure is a stressed skin, box beam, which the drawing denies. He says that the structure resists bending, but later he says that it is flexible in order to avoid breakage at the wing root. He says that the wing has a minimum of sheet covering, which it does not because the sheet could have been eliminated entirely. He goes on to say that the helper has to hold the spring on the timer starter which involves another person in the launching procedure, and from this he concludes that he has reduced the possibility of human error. What is a sane reader to think of all this?

Later on he makes the statement that a stab with an aspect ratio of 8 was no more efficient than one of ratio 6. Since this statement disagrees with every known fact about the relationship between the aspect ratio and L/D he really ought to give us the benefit of seeing his data. The strange logic in this last statement of his leads him to say, by an even stranger logic, that we should all patiently wait for the thermals to come to us. He uses a term we have never seen before - dihedral chord. What is it? We suppose that this last point is rather trivial; after all he had just made the statement that he was going to change the profile on the wing and stab to an NACA 6306 in order to improve the longitudinal stability of his airplane. After that remark, anything goes.

On page 5 of the same issue, Jim Moseley says some silly things about his model, but the silliest is that his wings held together in a crack-up even though they were badly shattered. How did he manage that?

The last issue we have isthe August issue in which David Bevan gives us the jolly delights of trying to figure out what he is trying to say about stab airfoils. He says that what he is going to do is small potatoes, but this doesn't make him hesitate. He drags the reader through what is to us only

incoherent babbling, and he ends by showing that the differences that he is concerned with make less than 2% difference in duration. Of course, we shouldn't overlook that fact that he has referred to "Aerodynamik Des Flug-modells" without giving specific references. This act gains him membership in an elite group headed by that master aerodynamicist Gerry Ritz. With the example he has provided we don't know just what to expect in the "more accurate study of A/2 stability, trim, and performance" he promises for the future.

As a parting shot why does Pat Curtis, page 5 of the same issue, say that his glider must glide to the left? The airplane looks symmetrical in the drawing.

COMING NEXT MONTH

Next month we return to our regular format. We'll have Bill Hartill's out-of-the-rut A/2...an equally exciting Wakefield from England... Bill Bogart will give some thoughts on a "fresh" design approach... and goodness knows what else.