FORUM

MATHEMATICS AND THE COLLISION REGULATIONS

IN the January number of the *Journal* (15, 104) Captain Wylie discussed the apparent conflict which existed between certain mariners and certain mathematicians about the efficacy of the Collision Regulations at sea. His contribution, which followed the publication in the October 1961 number (14, 379) of Mr. Calvert's 'Comparison of Two Systems for Avoiding Collision', was discussed in the same number by a selection of seamen and mathematicians. Followers of this controversy will be interested to read the remarks of Captain Weekes, printed on p. 271 on the subject of how imperfectly many ships' officers understand relative movement. Commander Clissold's suggestion, printed in the same discussion, that Calvert's and Hollingdale's rules should be tested on marine radar simulators is also to the point.

GENERAL REFERENCES

Sadler, D. H. (1957). The mathematics of collision avoidance at sea. This *Journal*, **10**, 306.

Calvert, E. S. (1960). Manœuvres to ensure the avoidance of collision. This *Journal*, **13**, 127.

Garcia-Frias, J. (1960). Anti-collision radar sectors. This *Journal*, **13**, 316.

Morrel, J. S. (1961). The physics of collision at sea. This *Journal*, **14**, 163.

Hollingdale, S. H. (1961). The mathematics of collision avoidance in two dimensions. This *Journal*, **14**, 243.

Wylie, F. J., Sadler, D. H., Gauw, J. A., Clissold, P., Wynne-Edwards, C. J., Bini, M., Freiesleben, H. C. (1962). Mathematics and the Collision Regulations (Forum). This *Journal*, **15**, 104.

Philosophy, Rules and the Collision Problem

from Dr. J. S. Morrel

CAPTAIN Wylie begins his paper by evoking a philosophic vision: a planet full of colliding ships, all staffed by mathematicians. He concludes by rejecting, on behalf of mariners, the help of the mathematicians. The maritime record also evokes a vision: the colliding ships are somewhat fewer, but the main difference is that every one is staffed by persons other than mathematicians. The latter vision rather implies a need for help from somewhere.

Since a resort to mathematics no more makes a mathematician than a resort to philosophy makes a philosopher, I shall assume that the Wylie controversy is not

https://doi.org/10.1017/S0373463300042739 Published online by Cambridge University Press

with 'mathematics,' as in his title; nor with 'the mathematicians,' as in his conclusion and elsewhere; but with Mr. Calvert, an engineer—and perhaps a little with Captain Garcia-Frias, a fellow mariner.

I concur in the Wylie rejection of the Calvert doctrines, but I must dissent from the Wylie reasons for rejecting them. My own reasons are that I believe them to be logically faulty, mathematically inadequate, and operationally unsuitable. The controversy is over *rules*: Wylie wants a rule of responsibility, Calvert wants a rule of thumb. In my view the issue is secondary, because it does not touch the crucial problem. It is also nugatory in part, because it rests largely on differences neither mathematical nor philosophic, but only semantic.

As to mathematics itself, there simply is not any issue. For mathematics and rules and responsibility all have one thing in common: their benefits are not intrinsic, but arise solely from the propriety—and the astuteness—with which they are used.

(1) Rules are for manœuvre selection. Good selection is essential, and well-conceived rules for it are necessary. But they are not enough. Reliable and timely prediction of risk is equally necessary. Prediction and selection are closely interrelated; but prediction is the first problem, and it is much the more difficult. It cannot be either solved *or* by-passed by *any* rule. What is required is physical data, relevant in kind, quantitative in nature, adequate in amount and in quality. At present, such data all too often are not to be had.

If this diagnosis is valid, then despite their mutual tone of total disagreement, Calvert and Wylie are actually victims of the same error. Both assume that the data accessible at present are adequate; i.e., that what can be seen by eye or by radar, is inherently effective to distinguish a dangerous encounter from a non-dangerous one, conclusively enough to justify the risking of a ship upon the distinction.

Neither party examines the data, as such. But respecting the effectiveness, Calvert is explicit: he states as a fact, too evident to need a proving, that mariners have 'good information'. (The more usual scientific view is that no inferential fact is this evident.) Wylie is not explicit; but he commends the present Rules for only prescribing 'when there is risk of collision' as the condition for invoking them. Manifestly the approval is unwarranted except upon the proviso that the condition is discernible.

A good diagnosis implies its remedy. If the one I have given is good, the primary task is neither to attack nor to defend the Rules, but to find and bring to use the means to provide the needed data. The problem is not philosophic but mundanely technological. In such problems, mathematics rather often *is* of help.

I think that heretofore these means have been out of reach, and that lacking them the present Rules are very nearly as good as could have been devised. But I believe that current technology can repair the lack, both effectively and feasibly. This will not cast out the Rules. On the contrary; it will implement them, so that responsibility will gain the wherewithal with which to work, and rules of thumb will recede into a merited oblivion.

(2) The Rules will not be cast out. But they can be made more definitive, and ought to be. There are objections, usually made to hang upon such words as: rigid, mandatory, unequivocal. I think these words are mostly bugaboo—indeed, when put in proper context they go far to *destroy* the objections. The issue does not lie in method but in consequences. Its resolution will never be found in epithets, but in numbers.

In the Wylie version the issue is 'specifying unequivocally' versus 'allocating responsibility'. In themselves these phrases set no issue. Unequivocal is no more to be deplored than equivocal is to be admired; in fact it is the latter that usually carries the odium. And responsibility is no benison; it can become a crushing and futile impost.

The next Wylie topic is 'Rules which...compel.' The word is fearsome. But the reality is not, for the feat is impossible. No rule can 'compel'; the very Ten Commandments have failed at it, signally, for millennia on end. And if a less fearsome but more meaningful word is used—say 'prescribe'—then the stricture falls on all rules; for all do this, or else they simply are not 'rules'. The differences are not in kind but only in degree. Indeed the Wylie context itself confesses just this, for it actually deals not with compulsion but with 'assurance'. The two are not opposites; the import here must be that the latter is the prerequisite for the former. In fine: the degrees of specifying and of assurance must be commensurate. *Agreed*. But still no issue. For the test is universal; the prerequisite for specifying is not different from the prerequisite for responsibility.

The true test of the Calvert system, then, is 'What does it assure?' Calvert answers 'All that is needed'; but his proof is defective. Wylie does not demonstrate the defects; he only dismisses all such systems as 'panacea' (a thing eminently preferable when possible) and hurries on to an encomium for what the present Rules do *not* say. The encomium likewise might be dismissed as mere admiration of the equivocal, but to do so would spring the same semantic trap whose other bait is 'unequivocal'. What is to the point is that all this only half-sets an issue, and resolves it none.

The true test of the present Rules, with the responsibility they impose, is also 'What do they assure?' Calvert again is forthright: 'Not enough, by far,' with supporting statistics. The nearest thing to a Wylie answer is in his conclusion: 'Vast numbers...follow (the Rules) through their lives without mishap.' Not relevant. The matter in question is the less vast but still substantial numbers who have mishaps—including some who *lose* their lives by them. The issue may now be considered two-halves set, but it is still not resolved.

(3) The truth is that the Rules *are* equivocal. As things now stand they have to be. But this is no blessing; it is a necessary evil. At present the mariner perforce must often cope with uncertainty. In great uncertainty, a rule saying 'Do thus and not otherwise' is unfitting and potentially iniquitous; the best available rule is in essence only an injunction to caution and alertness. At the opposite extreme, if all uncertainty could be dispelled for all the parties—whether two or many—a one best action could be singled out for each. The best rule then would say, 'This is what is right; *do it*.'

To be sure, this sort of rule would deprive the mariner of responsibility—the responsibility to guess, with the safety of his ship as the forfeit. Slightly to alter a Wylie phrase, what mariner 'of commonsense and, one might say *prudence*,' would not rejoice at such a deprivation?

Assurance *is* the true measure for specifying. The ideal is not attainable; there *are* no panaceas, not even in the Wylie 'two-ship situations'. Not in Responsibility, not in Rules, not even in better data. In the face of imperfect assurance, responsibility must remain, and it must not be relaxed. But it can be eased, and at the same stroke enhanced in effectiveness, by diminishing the handicap of uncertainty against which now it must labour.

https://doi.org/10.1017/S0373463300042739 Published online by Cambridge University Press

A commensurate refining of the Rules will help, not hurt. (How would the Rules have to read today if there were no binoculars or foghorns?) But the refining must be commensurate, and the relation is not reversible; neither rules nor responsibility can diminish physical uncertainty. The only thing that can is factual physical information. What is wrong with the Calvert system is not in the least that it 'specifies,' but that it disregards the prerequisite for specifying. What is right with the Rules is not that they equivocate, but that they have bowed to the realities born of the previous inaccessibility of the same prerequisite.

(4) The logical fault in the Calvert diagnosis is that with its facts it mixes an unproved and challengeable *premise*, but yet draws an uncompromising conclusion. Briefly (13, 128 and 129):

> Mariners see other ships, ergo they have good information; yet their ships collide, ergo they use it badly. Conclusion: 'What is needed is, *of course*, a simple rule of thumb.'

(The emphasis is mine.) The facts here are two: mariners see, ships collide. The premise: seeing can give 'good information'. Only one thing is left unclear: whether the alleged bad use is bad choice of manœuvre as such or bad interpretations of the Rules. But both are varieties of incompetence in judging; wherefore the proposed cure is to stop mariners from using their judgment.

With the same two facts, a different premise might have been mixed; that mariners *can* judge competently. The syllogism then would read:

> Mariners can see and are competent, yet their ships collide; ergo, seeing does not yield good information. Conclusion: obvious.

The second premise is not inherently less likely than the first.

Whatever its facts, a syllogism is no sounder than its premise. Calvert does not take up the burden of proof for the first. I shall likewise refuse it for the second—I have no proof, only a conviction. I believe that the competence of mariners to judge is good, among the highest any class of mere men may boast. I have no misgivings of the consequences of their *informed* exercise of it. But this is no more than a mere unscientific confession of faith, proving nothing. I can only plead that the Calvert facts are no less consistent with my confession than with the Calvert dictum.

Fortunately there is another way to examine the adequacy of the seeable, and in the best scientific tradition. Determine what exists to be seen, what must be known for safety, and compare the two directly. I have tried to do this. The evidence is not only mathematical but also physical, psychological and last but not least, experiential. It seems to me to show strongly that what exists and what must be known simply do not meet. I have previously outlined some of it in this *Journal* (11, 18), (11, 318), (14, 163).

(5) The Calvert remedy is nothing if not explicit: *better rules*. I have been equally explicit: *better data*. Wylie is not explicit, but could be construed as advocating: *better mariners*. For he strongly asserts the Rules are not to blame; but the blame must lie somewhere, and the mariners themselves are the next most obvious suspects. However, I shall not speculate here on just where a Reformation-of-Mariners would rank among panaceas, since my own diagnosis calls not for reforming but for informing. Moreover there are alternate possible interpretations of the Wylie text. These include one which makes it read as an

endorsement of the better-data remedy (and entails no slightest semantic stretching).

The first item is the vigorous Wylie objection to a manœuvre criterion based on bearing only. *Agreed.* I myself have previously insisted upon range, course, speed and advance notice of intent, along with bearing (*loc. cit.*). The second item is in the concluding 'vast numbers' declaration I have already quoted. The key word here is 'mishap'. I shall not presume to say why the author chose this term but to me it is beautifully precise and completely apropos. My dictionary gives several definitions, all really saying the same thing. The very first is in just two short words: *ill luck*. The antidote to luck is knowledge, distilled by judgment.

Unhappily, this interpretation forestalls me from using the Wylie vast numbers as support of my earlier confession of faith in mariners; for if the less vast are victims of ill luck, at least some of the more vast may be only the beneficiaries of good. But neither does the interpretation compromise my faith, since I have pinned it on judgment not on divination.

(6) The mathematical inadequacy of the Calvert argument stems from the same root as does the faulty diagnosis: the premise that what is seeable is adequate to distinguish a collision course. Consistent with this, the Calvert analysis is restricted to mathematically perfect collision courses. There are only *non-quantitative* allusions to other cases; e.g., the section on 'reverse manœuvres'. This is a crippling over-simplification, for it blurs out the very important effects of initial miss, especially undetected or misdetected miss, upon manœuvre effectiveness and hence upon manœuvre selection. There is a whole train of damaging consequences. For instance, the author gives a test for his Rule (**13**, 130): 'If one single case could be found in which these manœuvres failed, the whole argument would collapse.' The test confirms the rule only for the decidedly special case treated; in reality it amounts to only a little more than saying that when a situation is already as bad as it can get, a change cannot worsen it further. True, but there is a corollary: when the situation is thought to be so bad but really is not, a change that would help if it were, *can* worsen it. And a second corollary: when the situation is thought not to be so bad but really is, the change will not be made—with possibly a catastrophic sequel. Or to complete the author's own Euclidean analogy: the isosceles-triangle theorems are not extensible to triangles which merely may be isosceles, but also may not.

Examples of failure of the Calvert rule may be found in plenty, simply by postulating only quite reasonable errors of observation. In fact all of the usual types may occur: failure to manœuvre, or to manœuvre in time, or not to manœuvre; wrong manœuvre, mutually incompatible manœuvres. The neglect of this postulate is regrettable, especially as the Calvert 'key' itself—relative motion—suffices to reveal the failures. In fact it *has* been so used, frequently, in previous publications, including several in this *Journal*. The author's impression that it stood in need of rediscovery is astonishing.

In fact the same 'key' might have corrected a fallacy at the very beginning of the Calvert paper (**13**, 127): that airmen seldom see threatening craft, because their view-field is only 20 per cent of the sphere. Page 134 gives the chance of not seeing as over 4-in-5, apparently in echo of the 20 per cent. But the key, used alone, will show that the true chance of cockpit-blind encounter is far, far less than this; with a bit of dynamics and statistics added it shows that such blindness probably occurs no oftener than once in many hundreds of cases, despite the 20 per cent.

(7) A further and surprisingly unmathematical over-simplification in the Calvert analysis is its sweeping paucity of quantitative treatment—of virtually everything except size of turn (13, 133). A notable example is the section on 'reverse manœuvres'. There is good quantitative evidence tending to show that if bearing drift can be 'found' at all, there *is* no dilemma—the parties are quite safe as they go. It is only when the drift cannot be 'found' that the dilemma arises.

Another example is the statement (13, 136) that 'the master of each craft can—see at a glance what both craft are doing.' If the author's own Stockholm–Andrea Doria trace, on the same page, is replotted as Stockholm's radar would see it, it becomes evident that the 'glance' must be an intent and a long-protracted one, at the least.

Still another example is the proposal (13, 133) to combine turn and speed increase. The *amount* of increase is left unstated, but some of the stipulated turns are large. If also easy, they must consume much time and space. If hard, there will be *no* accompanying speed increase, for few ships have power enough just to *maintain* speed in a hard turn. Moreover, whether in a turn or not, the reserve over normal cruise speed is usually small, and to get even a substantial part of this is a slow business.

Somewhat different but no less embarrassing difficulties arise in the case of aircraft. E.g., at operationally acceptable turning accelerations and only moderate airspeeds, the Calvert instant-turn convention is wholly inadmissible unless intolerably long times and ranges are postulated. And at lesser ranges the substitution of physically realistic turns sometimes works drastic deterioration upon manœuvre effectiveness. A marrow-chilling instance is the Calvert-recommended 'jump technique'.

(8) In the absence of quantitative treatment, it can only be presumed that the Calvert manœuvres are intended to be early enough or violent enough or protracted enough—or all three—to swallow up the effects of initial miss and of errors in estimating it. This is a most snareful notion. Big manœuvres entail more big manœuvres to get back on course (or speed or altitude). Sometimes it is also necessary to get back on *track*, which entails still more big manœuvres. Whatever the success of initial prediction, the assurance that so complex a path is clear and will remain so until one is safely past is far more difficult to contrive. The bigger the manœuvres the bigger the difficulty.

The bigger the manœuvre the bigger the errors it can absorb. But big manœuvres take time and space, and must be begun at correspondingly long ranges. Unhappily the prediction errors that must be absorbed also grow bigger at longer ranges—much bigger. Thus the real gain in safety may be small—it can even be a loss. The balance between long-range uncertainty and short-range impotence is a vital aspect of manœuvre analysis. When it is omitted the results are of little value, and are quite likely to mislead.

There is yet another snare in bigness. It is related to the two-ship-panacea approach condemned by Wylie, but in fact its victims seem to have been mostly mariners. These are often heard to advocate early and decisive action. The doctrine is precarious; for in multi-ship environments the required *frequency* of manœuvre increases sharply as manœuvre is made either earlier or larger. Too big can mean also too often, and together these can mean chaos. Thus *economy of action*, in both time and space and hence in frequency, becomes a desideratum. The balance between economy and safety is as vital as that between uncertainty and impotence; in fact the former is near kin to the latter, but in the large rather than the particular.

(9) If as I have maintained, the rules issue is secondary, by the same reckoning the display issue is only tertiary. The function of a display is to order and shape its input data so as to facilitate a ready and correct perception of their import. This is important, and careful attention to display design is not misplaced. But displays can *create* nothing. Whatever information is not in the input inherently, will not be seen in the display, however cleverly the latter may have been contrived.

The Calvert display is no exception. Possibly it is preferable to other types. Possibly not; for one potentially grave defect is apparent at once. Its alleged merit is that it compensates out one's own manœuvre displacements; but the radar itself cannot do this, so the required data must be provided and fed in from other sources. How? And how accurately? *Any error in this compensation will appear in the display as a manœuvre by the other ship.* The more accurate the radar the more distinct this effect will be. The consequence may well be to obscure and mislead rather than to reveal. The risk is aggravated—at both ends—by the fact that real manœuvres are never instantaneous, but often consume minutes to reach full development. In any event, the input data with which to feed the display remain the paramount need.

(10) Some confusion as to the role of mathematics is betrayed in both the Calvert and the Wylie papers. In fact, it plays several and quite diverse roles: as an instrument for discovery, or for appraisal, or for exposition, or for decision and action.

On the one hand it is surprising to hear that mathematics is not needed or helpful, and especially so to hear it from a mariner. Hardly a facet of the art of navigation has not felt the touch of mathematics. Not a few of the facets have been born of it, nurtured by it, even shaped directly to the needs of navigation by it. Moreover much of this is the work of persons who never had set foot on any bridge. The problem of the Longitude was not formulated, nor solved, nor implemented by mariners. No mariner invented the glass, or perfected it—or the barometer. Virtually the whole of celestial navigation was hammered out by astronomers, using the most refined and subtle arts of mathematics at every step. Mercator was no seaman but a mathematician. Even the collision criterion of steady bearing is probably due to a mathematician. It is said first to have been announced about 600 B.C., in a discourse on similar figures by Thales, a giant among geometers—and a landlubber.

On the other hand, it is equally surprising to hear—from anyone—that vector handling of simple relative motions transcends the wit of mariners, when in fact it is routinely mastered by schoolboys every day. This is not to say that mariners are so well-versed on it as they should be; I agree with Calvert that they are not. I think the accident investigations reveal this plainly, not only as to mariners, but as to Boards also. The situation here needs improvement; but the role of mathematics in it is not discovery and not action. It is exposition.

It is not discovery. As to risk, prediction, manœuvre, manœuvre consequences, the field is well ploughed. Nothing both new and useful is likely to be turned up by further ploughing. In particular the Calvert manœuvre system is not new, no more so than is the Calvert 'key', except in minor detail, and possibly in the emphasis it puts on the dubious resort to speed changes. Aside from this, a scheme somewhat akin is even epitomized in an old seamen's shibboleth (most likely *not* born of mathematics): 'Beam-to-beam won't hit.'

As Calvert says, vector manipulations are also not a fit precursor to decision or action. It is no more in order for a mariner in the midst of encounter to work them out than for a mathematician to reconstruct all the proofs of Euclid each time he wishes to use one of the theorems. Fortunately both exercises are equally as unnecessary as they are inappropriate. First, neither the situations of ships nor their actions are so various as to require it. Second, such manipulations as are required are readily done automatically, by a simple adjunct to the display device; or if desired, by a manual device not more elaborate or difficult to learn than the simplest sextant, less difficult to interpret than a cocked hat.

In the discovery role, there is but one field in need of further mathematical ploughing. It is the striking of optimum balances; between uncertainty and impotence, and between economy and safety, as mentioned above. In contrast to others, this field is not easy to plough, especially the second part. Nevertheless I think the yield will be well worth the effort, not only in safety itself but also in expedition without sacrifice of safety.

(11) Perhaps I should add a second field for ploughing—the *economic* balance for collision avoidance. How much safety can maritime enterprise afford to buy? How much can it afford *not* to buy? I do not know. But I have heard it responsibly estimated that the money cost of Stockholm–Andrea Doria alone, direct and indirect, exceeded fifty millions of American dollars, net and irretrievable. Even today a tenth of this sum would buy a very great deal of excellent technology, plus quite a bit of its working product.

Mathematicians and Navigators

from E. S. Calvert

As Mr. Sadler points out, there can be no conflict between mathematicians and navigators, provided the mathematicians have adequately fulfilled their job. If, however, the navigators fail to follow the mathematics, or even to check the results by taking a few examples, then there will be plenty of misunderstandings; to bring 'philosophy' into the discussion is to return to the attitudes of the Middle Ages, and to obscure technical issues which are perfectly clear and simple.

Two of the contributors, Commander Clissold and Rear Admiral Gauw, do, however, make practical suggestions which are useful and interesting, and which require comment. Commander Clissold suggests that the proposed system of collision avoidance should be tested on a simulator. This system, like the existing one, applies, strictly speaking, to an encounter between two craft, and its correctness was demonstrated on a two-place radar simulator at the Royal Radar Establishment in 1959. The results were communicated to a meeting of the Technical Committee in January, 1961, and no one then or since has seriously questioned these results. It would therefore seem that what Commander Clissold has in mind, is an experiment which compares the two systems in various multiple situations, both qualitatively and quantitatively. After all, what