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SHIPBORNE AUTOMATIC TEST EQUIPMENT IN THE ROYAL NAVY

R. L. Short, R.N.S.S.
Admiralty Surface Weapons Establishment

Introduction

This paper will discuss briefly some of the factors which must be taken into consideration before the adoption of any Automatic Test Equipment (A.T.E.) for shipborne fitting. Although no attempt is made to deal with the problem of testing R.N. equipment ashore, it will, hopefully, become clear that the policies of testing ashore and at sea do interact strongly; three levels of maintenance effort are applied to a ship, *viz.* self-maintenance, assisted maintenance and dockyard maintenance, and these should be complementary. Techniques employed throughout the development and production of operational equipments strongly influence the ship's maintenance tasks.

The need for A.T.E. in H.M. Ships is, as in many situations, to increase efficiency, consistency and reliability in the performance of a maintenance task, but this paper will deal only with those considerations of A.T.E. which are peculiar to H.M. Ships at sea; with the degree of automation currently adopted; with the factors which have limited progress in shipborne A.T.E. and with some of the possible approaches which might be envisaged for the future.

The Navy's Need

The Environment

No-one who has crossed the sea to Ireland for a Christmas holiday will need an elaboration of the effects of the environment on human efficiency, and even the less fortunate must have some experience of ship motion nausea.

A comprehensive list of adverse factors to which the maintainer at sea may be exposed is given below.

All equipment maintenance	Above decks maintenance
Ship motion	Wind
Vibration slamming	Green seas
Noise	Salt spray
Underwater shock	Hail
Artificial light	Rain
Static inclination	Blown snow
Temperature extremes	Ice accretion
Humidity extremes	Funnel gas
Ladders and hatches	
Difficult access	

A.T.E. could relieve the shipborne maintainer of all those tasks, which though routine, become extremely arduous in these environments; it could carry out tests on equipment which is difficult of access, and this type of equipment is commonplace in a ship.

The Complexity

Most papers on most electronic subjects make some reference to the explosion in complexity of equipment. The maintainer is at the end of the line, his task has been increasing at an ever increasing rate and he is stuck with the errors and omissions of all the rest of us. An example of the increase in complexity is that the automatic monitoring system for the *Sea Dart* radar about to be fitted in ships has a higher component population than the entire *Seaslug* radar already fitted; the proportion of monitoring to total components is the same for the two equipments.

In addition to the increasing component population, micro-electronics and the new assembly techniques associated with these circuits are posing new maintenance problems and making still higher demands on the particular skills and specialized tools of the maintainer.

However new techniques allow a re-assessment of old philosophies, digital techniques with integrated circuits widen the scope of maintenance policies in a way which would have been quite impossible in the days of the thermionic valve.

The Maintenance Effort

The rate of increase in the quantity of the maintenance shows no sign of decreasing; moreover the quality of the task is also increasing. The Navy relies heavily on the skill of its senior ratings to maintain its equipment; these men are drawn from the top bracket of recruits and they receive considerable in-house training, often as much as five of the first seven years in service.

In common with most employers, the R.N. are finding it difficult to recruit men with the necessary technical capability.

The R.N. has its own additional recruitment problems and it is evident that the only practical solution is a substantial reduction of the maintenance task. A.T.E. can be a major instrument in the achievement of this reduction; automatic diagnosis to one or several replaceable units would reduce the quality of the task; if those replaceable units were reliable enough for sufficient spares to be carried, the skill required at sea to maintain some equipments could be negligible.

The point made in the introduction (Section 1) concerning the need for rationalization of the ship and shore maintenance policy now becomes apparent: if the full benefits of A.T.E. are to be reaped then the effects of A.T.E. must be reflected in the earliest operational equipment engineering decisions, in the module sizes, the interconnection techniques, in development and production testing, in the spares policy, in the dockyard repair policy, and finally to revert to the theme of this section, in the personnel recruitment and training policy.

The Documentation

As a result of the increase in complexity, because R.N. ships have hitherto relied heavily upon their skilled maintainers, and because, due to the organizational problems, equipment development, technical training, test equipment and documentation have typically been disjointed efforts, the quantity, complexity and costs of documentation are also increasing.

Documentation is not a once and for all capital cost; in fact the costs of modifying and maintaining the documentation for R.N. ships is a continuing burden.

Our present methods of dealing with equipment documentation are inefficient and outdated and new concepts already formulated will undoubtedly improve the situation, but these are only palliatives, the vast amounts of paper associated with the maintenance task will only be significantly reduced by a reduction in the task itself. One result of Computer Assisted Design may well be an increase in test instructions, automatically printed, but these should go straight into an A.T.E. programme, not to a man.

The Cost

The complexity, quality and quantity of, aids and support for the maintenance task have already been mentioned.

By its very nature, maintenance engineering is a process highly expensive in the costly commodity of human labour.

It has been estimated that one extra skilled rating in a frigate for maintenance duties may cause an increase in ship production cost of about £5K, an increase in running costs of about £150 p.a. in fuel, and personal and overhead costs approaching £4K p.a.

One system maintainer means two men on the ship, allowing for only two watches, and is worth at least £200K for a 25 year ship's life even excluding the costs of their training at shore establishments and all the aids and support they need.

At present time shipborne maintainers are being pared down to cut costs and at the same time the lack of availability of ship's systems is being deplored.

£200K can buy a lot of A.T.E., but in terms of capital cost, if it only replaces two men on the ship we are simply preserving the status quo, well worth doing indeed but not a dramatic justification for an A.T.E. policy.

The real benefits of A.T.E. are only seen when the availability achieved by £200K worth of A.T.E. is compared with the availability achieved by two men in the ship at a cost of £200K PLUS the cost of: all the built-in manual test facilities; all the portable test equipment; all of his training at the shore establishments; the loss of one highly skilled man to the nation.

The Ship Survival

Each ship may have a number of roles; a number of functions may contribute to each role; a number of systems may be integrated to carry out one function; system effectiveness is that quality of a man/machine complex which ex-

presses its capability to successfully carry out a given function and it may be judged by two factors: the system's performance and the system's availability.

These two factors, defined with sufficient care, will represent the value of the system to the ship; a synthesis of all the systems effectiveness will represent the value of the ship to the nation.

The specification and achievement of system performance is not within the province of this paper, suffice to say that it has never lacked attention.

The specification and achievement of availability, comprising reliability and maintainability, is very germane to the subject with which this paper is concerned and also, as the preceding paragraphs demonstrate, of direct relevance to the value of the ship to the nation.

The author may be accused of labouring the point and giving undue prominence to availability, but it must be of some significance that, although Reliability has become fashionable of late, few people can talk with confidence of maintainability and availability.

Over 60 separate functions can be identified in one frigate, each makes some contribution to the ship capability in at least one of its roles; in wartime the availability of one weapon system which is vital to the role of self-defence could alone determine whether or not the ship will survive.

Why no Standardized A.T.E. Policy for R.N. Ships ?

The Time Factor

Many factors have combined, for many years, to produce in the Royal Navy the reactionary approach to change which has been the despair of so many so often—it is not sheer cussedness.

Research, development and production of a modern ship system may take 10 years and the system may be in service for a further 20 years; in the 20th year the survival of the ship may still depend on the availability of that system.

A maintenance policy with specified aims requires a broad based plan and a high level of sustained effort; the policy is naturally inflexible.

If you have maintained, for many years, shore establishments at which able men are trained to a very high level of skill indeed, and if you know that skill is still in short supply, you aim at improving the level of training until an acceptable alternative is established.

If these able men, trained to cope with any type of system failure, have for many years been sent to sea in ships with the vital necessity of being entirely self-sufficient for many weeks on end, you have to be very certain that this capability

is not likely to be in danger before you make a change.

In an organization with that sort of unavoidable inertia and that sort of time factor, change must be evolutionary and not revolutionary.

The Flexibility Factor

In any new ship, say a frigate with 60 functions, one system may be very old and outdated though still adequate to carry out a function, and one may be brand new and up to date; the systems meeting the requirements of the other 58 functions cover the whole range between those two extremes.

Throughout its life, say 25 years, the old outdated system will at some time be replaced by a brand new one, the original brand new one will become old and out of date and the systems for the other 58 functions will be scrapped, replaced or modified again and again.

At present modern systems at sea are rarely fully developed, the rapid development of new techniques ensures that fully developed systems are obsolete before they get to sea; modifications to ships' equipment are frequent and are usually carried out with on-board facilities; action damage necessitates emergency repairs; a " feel " for the system is usually essential and a two-year turnover of ships' maintenance staff has always permitted the build-up of intimate knowledge and experience.

Maintenance staff trained in all the basic principles of engineering, aided by all the adequate documentation and drawings and provided with a common range of basic test and measurement equipment have for many years coped with this situation; any alternative policy must have this order of flexibility unless the situation is changed by different development, modification and repair policies.

The Parameter Spectrum

The variety in types and design standards of equipment carried in one ship has already been mentioned in the previous section and naturally results in an extensive parameter range.

Since the requirement for the measurement of one of these parameters can rarely be forecast in a time continuum, it is always possible that it may arise at a critical moment and when the ship can only rely on self help.

Not only are most parameters commonly available in every ship but they usually cover the range from the " micro " or " milli-micro " region right up to the " kilo " or " mega " region.

Any A.T.E., no matter how sophisticated, demands design compatibility from the operational equipment on which it operates, and when the design techniques in the operational equipments embrace all those in a time period of 20 - 30 years,

the versatility requirement becomes extremely rigorous.

Even the very sophisticated TEAMS out of the U.S. Northrop Nortonics division, requires sensors planted throughout the equipment it monitors and standard test injection and measurement points throughout the equipment; the TEAMS philosophy has undoubted merits, but the costs of ensuring standard operational system compatibility even on a small scale could be high and the benefits of TEAMS are only great when the scale is large.

If we assume that retrospective modification of most existing R.N. shipborne equipments is not cost/effective, the result of a TEAMS type standardization policy on all new equipments is unlikely to have a large scale effect in the next decade, during which time the TEAMS policy during its shakedown period may well cost more and be less effective than the existing one.

A second point, arising from the variety of operational equipment, affects the requirements a standard A.T.E. might be required to meet.

Most requirements of an A.T.E. can probably be found under one of the following headings:—

(a) Overall performance checks
(b) Alarm annunciation
(c) Fault diagnosis
(d) Geographical fault location
(e) Detailed accurate performance measurement
(f) Setting-up and tuning
(g) Warning of incipient failures
(h) Information print-out
(i) Self test facilities.

The list may still be incomplete but it is adequate to make the point that the emphasis on any requirement will vary considerably from one type of equipment to another.

(a) and (b) may be of lesser importance to some systems, *e.g.* some communication systems but may be of paramount importance to other systems, *e.g.* rarely used weapon systems, data handling computers or surveillance systems with automatic detection.

(c) and (d) may be desirable for large equipments of which only one or two may be carried in the ship, but may be of little value for small equipments where replication provides adequate availability.

The cost and complexity of the A.T.E. will vary considerably with the emphasis placed upon each requirement, and with a standard centralized system it will be difficult to optimize on the various programme and parameter requirements.

A third point arising from the variety of one ship's operational equipment concerns the use of operational units to generate test signals and carry out self-test procedures.

Any new development of a large modern system introduces a completely new range of many parameters; it is this feature which has contributed as much as any to the breakdown of the C.R.E.T.E. policy, and which has led to the trend towards integrated built-in test equipment; to give but a few examples: a sample of Tx r.f. power provides a very cheap, convenient Rx test signal; an operational oscillator signal will test amplifiers, filters *etc.*; a built-in noise source with integrated control is the cheapest monitor of Rx noise figure; built-in monitors of power, be they crystals, thermistors or calorimeters are also highly cost/effective; testing and diagnostic routines for operational computers must be written in to the operational programmes; examples are obviously endless.

The point about these integrated self-test features is that in a single operational system with its own autonomous, integrated testing and monitoring systems, every advantage can be taken of the operational facilities and total costs can be significantly reduced; with a standard, centralized system some of these opportunities may be lost, the standardization requirement limits the development potential or the new development requirements result in continuous modification of the centralized A.T.E. and the Fleet will be left with a permanently immature design.

The Cost Factor

The rising cost of maintenance in H.M. Ships and the need to reduce this cost has already been noted; however, the R.N. policy of recruiting able men, training them to a very high level of skill broad-based in the principles of engineering, and providing them at sea with a Common Range of Electrical Test Equipment (the well-known C.R.E.T.E. range) did for many years meet the demands of the maintenance task at a cost and with an effectiveness which alternative policies may find difficult to match.

It is this need to prove a high probability of gain in policy cost/effectiveness, combined with the high inertia to change of the policy which accounts for the relatively reactionary approach to A.T.E. in R.N. ships.

The advantages of A.T.E. have long been appreciated; the gain in whole life cost of equipment may, in specific cases, be demonstrated; but the overall cost/effectiveness of an all-embracing maintenance policy involves far more than an independent consideration of the policy's separate parts.

A policy of standardised A.T.E. in R.N. ships would involve, amongst other things, a reappraisal of: the recruitment policy; the shore establishments; the training policy; the documentation

policy; the availability of the systems, the functions and the ship; the policy for new equipment development.

Because of the time factor, the flexibility factor and the parameter spectrum, any cost/effectiveness study of the R.N. maintenance policy would be required to forecast over a time period of at least an average ship's life, 25-30 years.

For these reasons, and others, the author's opinion, already stated, is that change must be evolutionary and not revolutionary; costing of that sort of complexity on that sort of scale is unlikely to produce a clear-cut yes/no answer.

The Ship Self-sufficiency

When, if ever, all the other objections to a standardised A.T.E. policy in R.N. ships have been overcome, this final factor alone could still be an insuperable obstacle.

When, if ever, a ship can be sent to sea on an operational mission with all its equipments designed to need no tuning, no setting-up or knob-twiddling of any kind; when spares of all units adequate to meet all emergencies can be carried by all ships; when systems adapt automatically to action damage; when no repair or preventive maintenance of any kind is required; then, and not before then, the skilled maintainer at sea will be out of a job. A ship or shore based pool of flying squad maintainers might be acceptable in peacetime but would hardly meet the requirements in the event of hostilities.

Until the Utopia is established R.N. ships must carry skilled maintainers, and since it is logical to make full use of the staff available, a high level of utilization of skilled personnel is always required at sea.

No-one would suggest that because a skilled man is carried in the ship he should be deprived of modern aids simply in order to keep him occupied—but the situation in a ship is in direct contrast to most other maintenance situations.

On line testing of a/c equipment can be automated right to the point where a tester is only required to replace a faulty unit—tuning and repair can be carried out by specialist, skilled men at base. The time of an a/c mission, for which systems must have a high availability, is negligible compared with the average ship's mission.

The same sort of conditions apply to tanks and other army vehicles; but at sea the skilled maintainer has always been required to cope with any situation; there is a strong psychological objection by R.N. maintainers to any form of go/no go indication, they are, with much justification, very averse to leaving "out-of-tolerance" decisions in the hands of the designer; regular maintenance exercises are beneficial to maintainers who need

practice to retain their skills; too much automation breeds unfamiliarity and apprehension in the very man the ship may need to depend on in a crisis.

What Degree of Automation ?
Past and Present Examples

The first part of this paper was concerned with the Navy's need and dealt briefly with a few areas in which A.T.E. could be of enormous value; later remarks balance the picture and dealt equally briefly with a few of the factors which militate against a wholesale standardized A.T.E. policy for R.N. ships.

Unless they were related to particular equipment installations, philosophical discussions on the relative merits of manual and automated testing have rarely been profitable in the past; the final decision is never a straight one of choice but always one of degree and the optimum degree can only be determined in context at the present time; advantages and disadvantages are often so finely balanced that the decision may go either way, not only for two different equipments in the same ship, but often for two parts of the same equipment.

However automation has been steadily increasing in R.N. ships, and, although aids to the "user" have far outstripped aids to the "maintainer," the latter is now beginning to get more consideration.

This section then will give a very few examples of past and present equipment testing philosophies and although the small sample is not necessarily representative of shipborne maintenance they will suffice to show that R.N. shipborne A.T.E. is evolving even if the revolution is not at hand.

Guided Missiles

The development of R.N. guided missiles and the attendant problems of shipborne missile testing provide a splendid example of the interdependence of technique and philosophy.

The three examples chosen, *Seaslug* Mk. 1, *Seaslug* Mk. 2 and *Sea Dart*, followed each other in the given chronological order; the immediate R.N. needs and the benefits of A.T.E. were appreciated in the very early stages of *Seaslug* Mk. 1 conception, the R.N. were provided with a highly automated M.T.E. (missile test equipment); this system was fully engineered and fitted, with its automated M.T.E., into four operational ships of the *County* class.

Meanwhile during the long time scale associated with development, production, ship-building and ship fitting, *Seaslug* Mk. 2 development was well under way, and this time the agreed M.T.E. policy was a pronounced step backward from automation to an almost entirely manually operated M.T.E.

Meanwhile during the time that *Seaslug* Mk. 2 was being fitted into ships *Sea Dart* development

was well under way, and, because there was no good proven reason for changing the *Seaslug* Mk. 2 policy, the *Sea Dart* M.T.E., to be fitted into ships is also almost entirely manually operated.

The interesting, and one hopes, ultimate sequel to this fascinating saga of the shipborne missile test equipment is that the " hoped for " reliability of the missiles in ships of the future may be so high that shipborne M.T.E. may no longer be required at all !

So an example of shipborne equipment apparently ideally suited for the application of A.T.E. *viz.* a large number of identical equipments requiring regular routine testing, may pass through and out of the shipborne testing phase in two decades, with one failure and no successes being marked up for A.T.E.; one reason for this, it is suggested, is simply the anachronism between the philosophy and the technique " state of the art ".

It is quite possible that the automated M.T.E. philosophy of the Mk. 1 *Seaslug* was right but the capability of the technique, or " state of the art " current at that time could not justify the decision; for many reasons, some of them totally divorced from automation as such, the faith of the R.N. in A.T.E. was further reduced, hence the choice, with perfectly valid, rational reasons, of manual M.T.E. for *Seaslug* Mk. 2 and *Sea Dart*.

The probable ironic final sequel, again resulting from the interaction of technique capability and philosophy, is, that although the A.T.E. techniques have now advanced to the point where a perfectly satisfactory automated M.T.E. could be produced, missile design has also advanced to the point where shipborne M.T.E. can probably be dispensed with altogether.

The following quotations, from the report of the study which reversed the decision to automate M.T.E., are more than just historically interesting:—

" The missile test system will be manned on the operator/maintainer principle and should make maximum use of the man's brain power . . . even if a fully automatic system requiring no operators were proposed the ship would still need to carry maintainers . . . a man using an elementary machine would be unsuitable for complex accurate measurements, but a fully automatic machine would require more equipment and could result in lower test equipment reliability and more complex repair problems ".

After a study in some depth of the man/machine relationship involved in this shipborne task it was recommended that . . . " means which require the man to play an intelligent part should be used to perform the task ".

A recommendation, it is suggested, which might well be endorsed to-day, nearly 10 years later, but which is unlikely to be made for this task in any situation other than on board ship.

Weapon Radars

The development of test equipment for weapon radars shows a pattern not greatly dissimilar from that already outlined previously for the missile test equipment, and the author suggests that some reasons for this pattern are identical in both cases.

Designers and development engineers are strongly motivated to work as closely as possible to the " frontiers of science "; the philosophy always has a considerable lead on the engineering capability and the two are often allowed to get out of phase.

The three examples chosen are again from the same weapon systems, *Seaslug* Mk. 1, *Seaslug* Mk. 2 and *Sea Dart*. In the *Seaslug* Mk. 1 trials ship, H.M.S. *Girdle Ness,* the R.N. were provided with a completely centralized, quasi-automatic radar monitoring system, for its day, the mid-1950s, very sophisticated.

The sophisticated system failed; the engineering capability could not justify the philosophy; clumsy electro-mechanical switches, poor inter-connection techniques, unreliable thermionic valves and let's face it, undisciplined circuit design and escalating cost, all combined to destroy what was in fact a perfectly sound philosophy.

However, from the A.T.E. point of view, the shipborne radar story indicates a brighter future than the shipborne missile story—radar reliability is not even keeping abreast of increased complexity !

The step backwards in the mid-50s from centralized automatic monitoring was not too pronounced and the shipborne radar monitoring equipment for both *Seaslug* radars not only retains the better features of the earlier development but also provides a good basis for further automation; in particular, the conversion of all a.c. and pulse waveforms to d.c. allowed the maintainer at sea to make quantitative out of tolerance decisions on *all* types of signal on the simple indication of a built-in meter.

The *Seaslug* radars used the engineering techniques of the early and middle 1950s, thermionic valves and large discrete components, and it is suggested that the monitoring and testing philosophy was just about in phase with the engineering capability.

The built-in monitoring and test equipment of the *Seaslug* radars was a success, and continues to be so to-day.

Ironically, the very success of the *Seaslug* radar manually operated test and monitoring equipment, allied to the relatively high reliability of the radar design, has made progress to further automation

more difficult; how do you counter the argument, " if a system, or a philosophy, works well, why change it ? "

However, the progressives, amongst whom the author will stand to be counted, prevailed; the new *Sea Dart* radar will go to sea with a completely centralised, highly automated system of A.T.E. The considerations which led to this decision are discussed in some detail later.

The author has but one apprehension concerning the *Sea Dart* radar A.T.E—it happened with missile testing, it happened with earlier radar test equipment, it even happened with operational data handling computers—the philosophy might yet be too far ahead of the technical and engineering capability and when that happens even a sound philosophy takes a long time to recover its status.

Torpedoes

The patrol of the modern submarine may now be measured in months rather than days. Its armament may include underwater missiles (torpedoes) at least as sophisticated as most air flight missiles, and these have to be available at short notice with a high degree of reliability. The penalties associated with firing faulty missiles may be operational as well as economic. The fitting of an automatic tester allows the filtering out of faulty weapons by periodic checks.

A.T.E. is currently designed to carry out pre-firing tests on a fully assembled torpedo in its tube.

A fixed programme of 200 tests is defined in a core store and each test defined by a word of 32 bits. Part of the word contains instructions to the tester to provide certain stimuli to the torpedo and a further part contains the correct responses expected. The actual responses are compared with the correct ones. If the data agree the tester proceeds to the next test automatically, but disparity results in the tester stopping with a fault indication on the display. Tests may be overridden or particular tests cycled. Very limited diagnostic aids are provided.

The high premium on space and the extreme difficulty of access in this situation are added incentives to a remote centralized A.T.E.

Considerations which led to one Sophisticated Shipborne A.T.E.

This section was produced from notes prepared for a talk given by the Author in 1965. As such it represents only a few points culled from the surface and condensed from a number of very complex relationships.

It is considered that the material is unaffected by the intervening three years.

Complexity

This paper will make considerable use of comparisons between the monitoring systems of the *Sea Dart* radar Type 909 and the *Seaslug* radar Type 901. The monitoring system of the Type 901 Radar was considered to be somewhat revolutionary a few years ago and its complexity was the butt of very widespread objections at that time. However, the Type 909 Monitoring System is expected to contain twice as many components as the whole of the Type 901 Radar. In view of this fact, this paper will not attempt any sort of technical description but will merely make a few simple points of a philosophical nature. No apology is made for the simplicity of these points because it is considered that they all have a direct bearing on the major aspect of Type 909 maintenance which is the decision to automate many of the maintenance tasks.

Now two whole 901 radars just to monitor a Type 909 radar seems a bit much and will undoubtedly provoke such stock phrases as " the tail's wagging the dog " and " what monitors the monitor ? ".

To anticipate these phrases, the complexity problem will be examined in a little more detail.

Table I is a very brief comparison of the complexity, failure liability and cost of the monitoring systems in Type 901, Type 901M and Type 909. Type 901M is the radar for the *Seaslug* Mk. 2 weapon system.

The first two columns illustrate very clearly the increase in radar complexity during the last few years, and it can be seen that the Type 909 Radar may have eight times the component population of Type 901 and that the Type 909 Monitoring System may have more than twice the component population of the whole of the Type 901 Radar.

However, in the third column, which gives the Monitoring System component population as a percentage of the total radar component population, there is a remarkable similarity between the three equipments.

It is fair to say that the many objectors of a few years ago to this 30% figure have been remarkably silent since Type 901 has been in service. The author has experienced the personal pleasure of hearing senior Type 901 maintainers say that it was difficult to fault the system from a maintenance point of view and on reports so far it would be difficult for anyone to say that 30% was not a reasonable amount of monitoring for Type 901.

Thirty per cent is a fantastically high figure and in a way it is a pity that the figures in column three of Table I show such close agreement, because it may give the impression that 30% is right for all equipments. There is not a right

TABLE I—Complexity and Cost of Monitoring

	Total Components	*Monitoring Components*	*Monitoring to Total*		
			% Components	*% Failures*	*% Cost*
Type 901	12·5K	4K	30	<10	20
Type 901M	25K	6K	25	<10	15
Type 909	100K	30K	30	10	10

figure for all equipments and it is an illusion to think you can pick a figure out of the air before design commences and say that the monitoring must only be so much of the total.

By way of a bonus, the figures in column three give us a very clear illustration of the way in which this 30% figure can be reduced. By doubling the number of components from 901 to 901M we reduce the monitoring proportion from 30% to 25% and if the Type 909 Monitoring System could be coupled to three or four equipments, as it easily could be, the monitoring proportion will fall quite dramatically.

However, that is looking into the future and we can revert to the past and present in columns four and five of Table I.

Reliability and Cost

The main purpose of the last two columns of Table I is to soften the blow of the 30% figure discussed above and to correct a common misunderstanding which results from the curse of generalizations.

The misunderstanding is that cost and failure liability can always be equated to component population. That this is not necessarily true is evident from Table I.

Much as the author would enjoy saying so, this is not because monitoring equipment designers are better than the rest but simply because they are able to make most use of the reliable and cheap high population components.

In Type 909, with its automatic Monitoring System, these percentages should be even more favourable than in Type 901 because most of the monitoring components are used in repetitive binary computer type circuits and if that type of circuit is not more reliable and cheaper than the rest everybody will be too busy with A.D.A. to worry about Type 909 A.T.E. (A.D.A. is the ship's operational data handling computer).

Now it must be emphasized that all the 901 and 901M figures in this Table are facts and are as accurate as this sort of figure is ever likely to be: component populations were counted, failure rates

carefully measured and production costs based on bills which have mostly been paid.

For Type 909, the figures are predictions and although, like the author, the reader may be sceptical about predictions in absolute terms, there is no doubt that the percentage must be reasonably accurate.

So it is hoped that the figure left ringing in the reader's head is not the 30% component population but the 10% cost and failure liability, which is of course what we are really interested in.

Before leaving cost and complexity, it is worth considering briefly the physical dimensions of this complexity.

The increase in complexity of 8 : 1 of Type 909 over Type 901 must be associated with a packing density increase of something like 40 : 1 in the cabinet which contains the 30K monitoring components.

Availability

Leaving the trend of increasing complexity another familiar trend worth some consideration is the increasing demand for higher orders of reliability and availability.

Now requirements cannot be quoted but for the sake of the argument it is intended to assume that the difference between Type 901 and Type 909 is that a hypothetical requirement for four hours operation without failure has been stepped up to four days and see if we can be more specific about what this would mean in terms of reliability and availability.

From Table II it can be seen that Type 901, if it had a mean time between failures of 100 hours, would have a 96% chance of surviving four hours without a failure. However, if we want to get this same 96% chance with Type 909 for four days it would need an m.t.b.f. of 2300 hours.

Furthermore if it was decided that the highest practical design aim for Type 909 was Type 901's assumed 100 hours m.t.b.f., and even this means an overall improvement in component failure rates of eight times over Type 901, then, from Table II it is apparent that the probability of

TABLE II—Reliability

	Probability of Survival with no Failures	*Period of Operation*	*Mean Time between Failures*
Type 901	96%	4 hours	100 hours
Type 909	96%	4 days	2300 hours
Type 909	38%	4 days	100 hours

operating for four days without a failure slumps to 38%. We might now feel justified in saying that since we may have to tolerate the failures we must have automatic fault diagnosis to bring up the availability.

However, Table III gives an estimate of the sort of improvement we might expect from automation:

From this Table we see that reducing the mean time to repair from 20 min. to 5 min. takes the availability from 99·7% to 99·9% and if anyone wants to be sufficiently specific about the requirements to say that 99·9 is acceptable but 99·7 is not, then he can be reasonably sure that no one will ever prove him wrong. These sort of figures only exist in mathematical formulae and with radars of modern weapon equipments there is virtually no chance ever of measuring availabilities of that order to that accuracy.

There may be some objection to the repair times quoted in Table III, these are based on the author's personal experience and as such are open to controversy. However, the manual diagnosis time might be achieved by an improved and extended version of the Type 901 system and it is evident that any improvement in the automatic diagnosis repair time will only add 9s of continually decreasing significance to the probability of 0·999.

It must also be said that the order of improvement quoted in Table III is in fact better than that quoted in the U.S. Maintainability Specification MIL-M-2331A (Ships), a document often used with great authority.

With regard to our problem of whether to automate testing, we have to consider whether the increased development cost of automation is justified by improving the availability from 99·7% to 99·9%. From the aspect of maintainer convenience, this decision must also be influenced by the very favourable reception given by the user to the Type 901 Manual Monitoring System and by the well known disadvantages of automation, such as the low level of utilization of skilled personnel, lack of build-up of intimate knowledge and experience, inflexibility to changes, and others.

Although the figures in this paper have been given rough and ready treatment, there is little doubt about the order of trade-off to be made— the additional complexity and problems of an automatic Monitoring System against an 0·2% increase in availability.

If the decision to automate the monitoring had to be made solely on this criterion, the author would be a very worried man and wild horses would not have persuaded him to try to justify it even though intuition suggests that the decision is sound. Fortunately this aspect of availability is not the sole criterion and now it is intended to go on and say why.

Automatic Testing

While discussing availability, we were concerned largely with fault location and fault repair, but before he gets to that stage the maintainer has to know whether or not a failure exists. With this problem in mind, we can go back to our comparison of Type 901 and Type 909.

In Type 901, with its mainly Manual Monitoring System, the Daily Routine, pared down to its absolute minimum and performed by experienced experts, might still take half an hour to give a reasonable guarantee of correct operation.

TABLE III—Availability

	Mean Time between Failures	*Mean Time to Repair*	*Availability (Probability of working when wanted)*
Manual Diagnosis	100 hours	20 min.	99·7%
Auto Diagnosis	100 hours	5 min.	99·9%

Now if we apply the 8 : 1 complexity factor it could take four hours with the Type 909 radar just to find out whether it is working, and four hours to check out just a portion of one of a modern fighting ship's defensive weapons is really too absurd.

Having been somewhat severe in condemning generalizations the author feels he must justify the use of the 8 : 1 component population increase as a complexity factor. There are many reasons why 8 : 1, from a testing viewpoint, is not only justified but optimistic; one which has a major effect on the maintainer's load is the automation of many functions previously performed by the operators.

For one function Type 901 has a couple of displays and a couple of men and not only does this provide relatively simple operational circuits but it also provides very simple and quick manual test facilities.

Contrasting that with Type 909, we have not only lost the manual test facilities but we now have a vast number of parallel circuits each of which must be tested independently to check for satisfactory system operation.

Automatic testing then is not only justified but is absolutely essential if the maintainer, and even more important, A.D.A., is to know whether the system is working or not—and it may not be an oversimplification to say that auto-testing is justified by the operational automation alone.

The Sea Dart *Radar A.T.E.*

A lot of time has been used on the auto/manual choice of monitoring system because this probably has more repercussions on R.N. maintenance than any other but there are many other equally controversial choices to be made.

However, since auto-testing is now a major branch of the electronic industry and acres of paper have been written on these choices, this paper will conclude with a brief summary of some other features of Type 909 A.T.E. The system is integral rather than auxiliary, is centralized rather than localized, is digital rather than analogue, is quantitative rather than qualitative, uses pure binary rather than codes, and has self-test facilities.

System repair by unit replacement, the provision of spare units in the office and servicing desk facilities for unit repair are all very similar to those in 901.

Now we come to the major compromise, we have a central unit which can carry out 640 tests, but what happens when faults are detected ? We have our immediate fault warning and we can check out the system rapidly but having put all this stuff in just to find out there is a fault, it is obviously economical to add a bit more to locate the faulty unit.

But the question is, how far do we go ?

To do it all automatically would require storage capacity for the results of the 640 tests, a considerable amount of logic to define the area to be tested in more detail and a complex programme to deal with detailed tests in all possible areas. Furthermore, as was shown earlier in this paper, all this might possibly improve the availability from 99·7% to 99·9%.

The compromise we propose is to use a 640 lamp display panel as a store, to provide the logic in a special fault-finding handbook, and to ask the maintainer to provide the programme.

This should not be as formidable as it sounds; a lamp pattern of one to three or four lights will indicate within 1 sec. one page in the handbook and we expect that for at least 50% of the failures, this page will indicate immediately the geographical location of the faulty unit.

In the other 50% of failures, other pages will have to be referred to and more automatic tests will have to be initiated.

Probably, the strongest argument in favour of this auto-manual compromise, apart from cost and complexity, is that it provides maximum flexibility in an area where flexibility is most desirable. Remember, we are trying to anticipate faults in a large new system, always a pretty unrewarding occupation, and if all this programme and logic was built in, the costs of modification even during development could be high. With our maintenance handbook, however, amendments will be cheap and easy even after the system is in service.

Three features which arise from the form of Monitoring System chosen are worth mentioning. Firstly, because signals are brought to the central unit in their original analogue form and accurately measured, it should be possible to carry out most of the testing and tuning, setting-up and detailed performance measurements without recourse to any external equipment.

Secondly, because all the tolerances are stored in binary form a simple register shift of one place up or down will double or halve all the test tolerances. Halving the tolerances will provide the maintainer with a quick method of checking setting-up accuracies and drifts, and may be helpful in locating incipient failures. It has been suggested that " Forever Amber " may be a good code name for this monitoring system since the one lamp which indicates the presence of a failure will be coloured amber and in this situation, when many of the 640 fault lamps are burning simultaneously, the ability to extinguish some of these lamps by doubling the test tolerance should be useful, enabling the maintainer to concentrate on signals which are really badly out of tolerance.

Thirdly, since all signal measurements will be present in digital form in the central unit, it will

be possible to provide, at little cost, the facility for obtaining a permanent record on punched tape of all the test measurements. The tape can be printed out at a machine already provided for A.D.A.

Summary

Type 909 A.T.E. could contain twice as many components as the whole Type 901 radar.

These components will have a packing density in the Monitoring Cabinet which is about 40 times greater than in an average Type 901 Cabinet.

The system is expected to have about 10% of the total radar failures and cost about 10% of the total radar cost.

Automation is not easy to justify simply to reduce repair time but is absolutely essential to reduce testing time and to provide immediate fault warning.

The A.T.E. is centralized, digital and testing is automatic but we have an auto/manual compromise for fault location and our choices have been made, to use the current O.K. phrase, on a system cost/effectiveness basis.

Possible Degree of Future Automation

The benefits of the well known C.R.E.T.E. range of test equipment have already been mentioned. A wide range of signal generators and basic measuring equipment, used by men with a broad based technical training, assisted by adequate documentation has in the past provided the basis for a very cost-effective maintenance policy.

The C.R.E.T.E. equipment has been designed or selected specifically for the shipborne environment; the range of equipment has been updated and increased to meet the increase in complexity of operational equipment; the incentives to recruitment and standards of training have been improved to meet the demands on maintenance effort; the amount of documentation has been increased to cope with equipment complexity; the cost of test equipment has been minimized by standardization, resulting in relatively high production quantities for each item of test equipment; those are some of the features of the old maintenance policy with its C.R.E.T.E. concept of manually operated test equipment.

Some of the disadvantages of the old policies have been mentioned in Section 2 of this paper, it has been suggested that the policy is stretched beyond its limits in all areas, but in no area is it more apparent than the question of the ship's survival.

Few will argue with the proposition that the old maintenance policies are totally inadequate to meet the present situation or that future needs of the R.N., as outlined, call for a reappraisal of maintenance policies.

In fact, as the examples clearly show, the situation has been well appreciated at project level for many years; most large shipborne equipments have for some time past incorporated a high proportion of integrated, built-in test equipment and much of this has been automated.

The C.R.E.A.T.E. Concept

The author makes no apology for creating yet another acronym in an area where acronyms abound since it seems to be a prerequisite of any new concept. Starting from the present situation, as already outlined in this paper, a logical step in the evolution of shipborne A.T.E. would be the establishment of a Common Range of Electrical A.T.E. functional modules, a C.R.E.A.T.E. policy meeting many of the needs, overcoming many of the problems of standardization and making most use of the existing situation.

It is not just by good fortune that the Type 909 A.T.E. provides a good example of how a C.R.E.A.T.E. policy might be developed, future needs of the R.N. were borne in mind during the formulation of that system.

The Type 909 A.T.E. is completely centralized and the central unit controls and programmes all the automatic tests; it provides active and passive test programmes; it accepts a wide range of analogue and digital signals, converts them to pure binary and from then on all programming, storing, counting, comparing, *etc.* is carried out by binary computer techniques; the signals to be measured are accepted in their original form, though some scaling is necessary, as indeed it would be for any form of built-in manual monitoring; the measurements are absolute and are made by comparison with stored values of signals and tolerances; automatic go/no go alarm annunciation is provided but so are facilities for manual control and measurement; automatic self-test facilities are incorporated; diagnostic programme could be added.

These features, among others, overcome many of the objections to standardized A.T.E.; this flexible, centralized system permits the design, or adoption, of standard central modules in isolation from the main equipment; conformity of packaging with modern techniques, though desirable, is no more an essential requirement for centralized A.T.E. than it is for a portable oscilloscope or Avo; finally any project embarking upon a new equipment design must reduce costs with standard A.T.E. modules on the shelf—analogue/digital convertors, counters *etc.* would be universal, individual system facilities would be " special to type ".

Although the author has used the system features of the *Sea Dart* radar A.T.E. to point the way to a C.R.E.A.T.E. concept, it is not

suggested that the engineering features of this system are necessarily a good starting point for a C.R.E.A.T.E. range; due to the usual project constraints of cost and time, the Type 909 A.T.E. was necessarily designed exclusively for one large but specialized operational system and it was just too early to reap much benefit from I.Cs and micro-miniaturization. It is well known that many industrial organizations with A.T.E. divisions or departments can provide, off the shelf, a wide range of A.T.E. but for R.N. ships a flexible A.T.E. system design based on common modules would appear to have overwhelming advantages. Indeed most modern electronic techniques, integrated circuits, printed wiring, computer assisted design *etc.*, cry out for mass production policies; modular design of all R.N. systems, not only A.T.E., would meet this need and reduce the maintenance task.

This, then, is one approach to the evolution of shipborne A.T.E.; it is relatively safe in that each advance can be based on a previously proved development and that each project would use only the modules they need and would have to prove the necessity of diverging from the standard range; the performance and engineering features of the standard range could be modified and updated in the same way that the old C.R.E.T.E. range used to be, though modern techniques should make the need less frequent; nothing need be lost or discarded until it is demonstrated to have no further value, but at least the maintainer would be getting a tool which compared with oscilloscopes and Avos might be as a Ford truck to a wheelbarrow.

The System A.T.E. Concept

A S.A.T.E. policy would appear to the author to be quite compatible with a C.R.E.A.T.E. policy as already outlined, and in no way need the two policies be mutually exclusive.

It has already been pointed out that one frigate may have over 60 separate, identifiable functions, and for no other reason than that the author is familiar with it, the function associated with the *Sea Dart* weapon system will again be used to point the way to a S.A.T.E. policy.

In the context of this section the Type 909 radar must be considered as a sub-system of the *Sea Dart* G.W. system, this G.W. system will contribute to the function of defence against air attacks and this function will itself contribute to the role of air defence.

The trend in the development of modern weapon systems is towards integrated concurrent development of all the sub-systems comprising the complete weapon system; in a typical case the sub-systems would include the missile itself, missile handling and testing, missile launching, missile

setting, one or more radars and, significantly, a large amount of digital computer capacity.

No elaboration is necessary on how the features of the *Sea Dart* radar A.T.E. might be adopted for an overall G.W.S.A.T.E. system, particularly when it is borne in mind that the operational computer itself might provide the basis for many of the A.T.E. modules.

Even in the *Sea Dart* system the possibility of using some of the storage and program capacity of the operational A.D.A. computer for the radar A.T.E. was considered, and only ruled out by the project constraints of capital cost and time schedule; the A.D.A. computer is used however to carry out automatic overall tests of the radar system and of the interface links during the pre-action period.

The author's own opinion is that at present R.N. ships would be too vulnerable with too much centralized A.T.E. that the essential back-up facilities would cancel any gain, but that a C.R.E.A.T.E. policy applied to each sub-system combined with a central interrogation and display unit might well be acceptable now.

The Ship System A.T.E. Concept

Even in our present permissive society the author decided that acronyms arising from the heading of this section might be too embarrassing; however our U.S. colleagues have for some years been providing us with, not just food, but a feast for thought in this area.

Starting with D.A.T.I.C.O. designed to monitor a complete Polaris weapon system, through N.A.R.A.T.E. to monitor all types of Navy radars, to T.E.A.M.S. which is a multi-purpose A.T.E. to monitor all types of system and finally to A.L.E.R.T. which is designed to monitor, control and use the information from up to 10 T.E.A.M.S.

To avoid accusations that this paper ignored the contribution of our own fine U.K. industry, such systems as T.R.A.C.E., M.A.T.E., E.A.T.E. and many others are conceptions on a scale no less grandiose than those emanating from the other side of the Atlantic.

However to develop the line of thought so far pursued throughout this paper the author will stick his neck out with his own prediction for the future of R.N. shipborne A.T.E.

It will soon become apparent that the repeated design of similar but new A.T.E. for each new development of shipborne equipment is not cost-effective and that some form of C.R.E.A.T.E. concept could be adopted; from then on shipborne A.T.E. will become self-generative; the attraction of automatic monitoring of a complete system, independent of human skill (or intuition?), will be obvious; the ability of a ship's command to

obtain information on the availability of all ship's systems at one glance will prove irresistible and some day some ship's captain may yet obtain the information " all systems GO " from one solitary light.

It would seem unlikely that a revolutionary change to one large centralized A.T.E. monitoring a number of ship systems, will find early favour for a variety of reasons, some of which were discussed in Section 3.

However IF a C.R.E.A.T.E. policy leads to a S.A.T.E. policy and numbers of autonomous A.T.Es, containing many common modules, begin to appear all over the ship, then undoubtedly, argument concerning the merits of centralization versus decentralization will occur; this type of argument has flourished in the operational field ever since the introduction of digital computers to handle the ship's data, control the ship's weapons *etc.*; in the operational field there are many protagonists for both sides of the argument, and in the testing field no doubt both sides will find support; the author would be happy to take part in such an argument if his premises were established, but at the present time, in the present climate, arguments for massive A.T.E. centralization are more likely to delay the evolution of shipborne A.T.E. Autonomous sub-system and system A.T.Es with centralized interrogation and display looks a more acceptable immediate policy, A.T.E. is not a panacea to the R.N. maintenance problems but it could provide quick relief until the R.N. design and maintenance philosophy has been adjusted to the rapidly changing electronics technology and could also provide even greater benefits thereafter.

Acknowledgments

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NAVIGATIONAL RADAR

P. J. Phillips, R.N.S.S.

Admiralty Surface Weapons Establishment

Introduction

Shipborne radar originated in the military environment of the Royal Navy during the late 1930s, and the war years saw the derivation of navigational radar from these early weapon system radars. Thus in 1946 the well established techniques were made available, for commercial use in the United Kingdom. The Admiralty, together with the Board of Trade produced a specification for Merchant Marine Radar and demonstrated its application with a specially constructed ' X ' band model. British industry then commenced development of marine radars which have subsequently been universally fitted in Merchant Fleets, and widely applied in the military sphere.

The B.O.T. Marine Radar Performance Specification has remained the basis for commercial radar design in the U.K. It was revised in 1957 and again in 1968 to ensure full advantage is being taken of modern techniques and development. Practically all the Merchant Radars marketed in the United Kingdom up to and including the present extensive range have met, and indeed considerably improved upon the basic requirements of this specification. The Admiralty Surface Weapons Establishment provides a service to industry by type-testing the manufacturers pilot models prior to quantity production, and when this independent authority is satisfied that the specified performance and engineering standards have been met, a certificate is issued by the Board of Trade. This qualification is most acceptable to customers not only for its guarantee of performance, but because it ensures the equipment has been tested under environmental conditions at least as severe as those which will be met at sea.

The Frequency Band

With few exceptions, Marine Navigational Radars operate in the 9,400 MHz region or X-band. It is argued that lower frequencies suffer less from clutter, but this problem has been largely overcome in modern radars by the use of anti-clutter circuits and techniques such as circular polarization. X-band discriminates more readily and is able to provide a picture of greater accuracy for Navigation and Pilotage with the ability to track ships in close proximity and detect small targets such as marker buoys. Further, the physical size of the X-band radio frequency components including the aerial is smaller, and the steady improvement in the design of the power source such as the magnetron has resulted in a considerable increase in the life and reliability of the transmitter. Coupled with this, the changeover from thermionic devices to transistors has resulted in significant reductions in the size of the equipment, its primary power consumption, and hence its heat dissipation.

Main Design Features

Some of the main design features of X-band navigational radars are summarized in Fig. 1 and they are relevant to equipment used by either military or merchant navies.

The frequency will be in the band 9,375 to 9,445 MHz. Alternative transmitter powers are available from 3 up to 50 kW.

Two Pulse Repetition frequencies are usually employed in the region of 1,100 and 2,200 pulses per second.

Two or three Pulse Lengths are used. The shortest, 0·05, 0·1 microseconds for instance, being used for short ranges and the longest, up to 1·2 microseconds for long ranges.

A minimum scanner rotation of 20 revolutions per minute is maintained under severe wind conditions.

Range scales are provided from a quarter nautical mile by steps up to 48 nautical miles.

The range accuracy is at least as good as plus or minus one per cent of the scale in use.

The bearing accuracy and discrimination is one degree or less.

Typical sizes of the main units which comprise a modern navigational radar are shown in Fig. 2. Although the dimensions are not related to any particular system they are indicative of the compactness being achieved with modern techniques. The size and weight of a system will vary with the degree of complexity and duplication employed, but approximately 150 to 200 kilograms is representative of an average system.

Fig. 3 shows a 25 kW bulkhead mounted transmitter receiver available on the commercial market, and one of the associated display outfits is shown

Frequency	9375→9445 Mcs
Transmitter Power	3→50 kW
P.R.F.	1000→1200 and 2000→2400 P.P.S.
Pulse Length	0·05→1·2 Micro secs
Scan Rate	20 R.P.M.
Range Scales	¼ N.M.→48 N.M.
Range Accuracy	±1% of the scale
Bearing Accuracy	1° or better
Bearing Discrimination	1° or better

FIG. 1. Typical Radar Data.
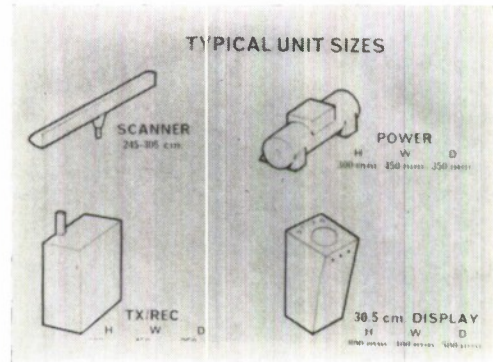
FIG. 2. Typical Unit Sizes.
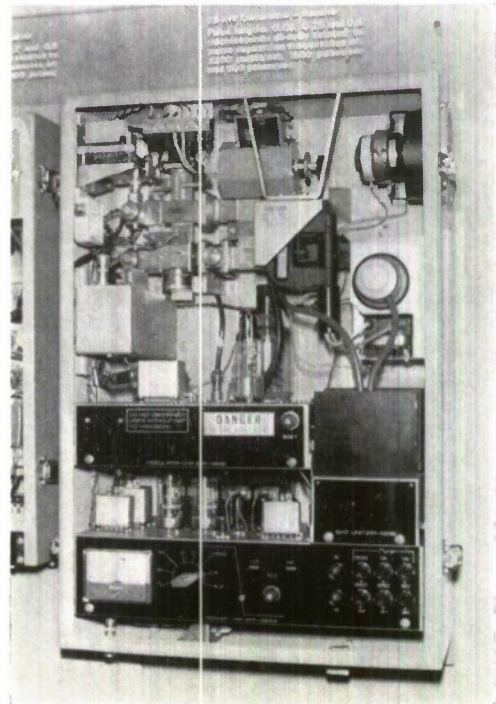
FIG. 3. 25 kW Transmitter-Receiver.

in Fig. 4. Although there is some degradation of quality in reproducing Fig. 5, it can be seen that a high definition can be obtained from this type of display. The picture shows an area of the River Thames on a $1\frac{1}{2}$ mile range scale, *i.e.* radius of the circle.

General Applications

With the addition of an extra unit the standard Relative Motion display can be changed to a True Motion presentation, in which the ship's motion can be seen compared to the surrounding land which is stabilized. The large display console illustrated in Fig. 6 has its True Motion computer on the left-hand side. This facility can be fitted to most sizes of display outfit except the smallest, and is being added to R.N. radars Type 978 and 975.

Considerable use is made of X-band navigational radars in conjunction with transponders. The Racon, or Radar Beacon is one widely used system. The transponder, installed for instance in lighthouses and lightships, replies to the radar beam and provides a distinct and recognizable trace on the P.P.I. picture. The transponder sweeps over 200 MHzs in the X-band thus ensuring that whatever the centre frequency of the radar, a suitable response will be given once every 70 seconds. Transponders are now available which are small enough to be installed in marker buoys and which will reply to low power radar transmissions.

Accurate ship's position fixing, to within 20 metres, can be obtained by attaching a precision ranging unit to the standard radar. Known as the Decca "Alpine" system it is shown in Fig. 7 mounted on the top rear of a Decca Transar display. This system plots the ship's position in relation to two fixed targets, and again transponders can be used to provide the targets in conditions where the radar may present a confusing picture.

Military Applications

One military application of the transponder and navigational radar is the control and identification of shipborne helicopters. An airborne transponder is triggered by the radar and transmits a coded response which enhances the echo and provides an identifiable trace. Fig. 8. The transponder signal frequency is shifted slightly beyond the radar frequency but is received by the radar scanner in addition to the normal radar echo. The response is extracted and processed in a special reply receiver and superimposed on the P.P.I. either with or without the radar picture. The reply receiver is shown in Fig. 9, bulkhead mounted, left centre, in a ship's 978 office.

There are a number of variations to the display which are important in military use. Target range

and bearing information transmitted to a weapon system may be derived from a navigational radar in a small ship where more sophisticated equipment is not carried. The information may be required in analogue or digital form and by manual or automatic extraction from the radar. Conversely in large ships, the surface picture may be viewed on a photo-plot display (Fig. 10). This console provides a large photographically projected picture suitable for viewing in high ambient light on which can be plotted a tactical situation as well as navigational data. The displayed information is continuously updated every three seconds with a running time of eight hours.

In contrast, the difficulties of submarine pilotage are being met by the use of a small portable display. This unit is particularly suitable in the restricted space of a conning tower. (Figs. 11 and 12). It is fed from the radar office as a slave display, thus the command need not be dependent on information relayed from the radar operator below.

Radar in submarines has for many years been provided by the Type 1000 series of equipments shown in Figs. 13 and 14. The twin transmitter-receiver cabinet (left) and ranging cabinet (right) together with the Display Outfit occupy a considerable part of the limited space available for radio/radar equipment. The introduction of modern transistored units based on commercial designs should make a significant saving in weight and space.

In ships carrying an I.F.F. interrogator (Identification Friend or Foe) the ability to display the responses on a short range surveillance radar can also be useful, and indeed if synthetic markers or other processed information is also added the basic navigational display can become an important part of a ship's integrated weapon system.

Two other features often associated with a military radar are sector selection and mutual interference suppression. In the first, the aerial is prevented from radiating except over a selected arc, usually variable, so that hostile detection of the transmitter is minimized. In the second, immediately prior to the transmitter firing, a special pulse is generated and fed to other electronic equipment to inhibit their receivers and prevent interference by the radar.

Using Commercial Radar in the R.N.

The foregoing are some of the specialist requirements which have been successfully applied to navigational radar in the military environment. In the R.N. adaptation of commercial marine radar has been accepted practice for many years. This policy, converting civil marine radar to military naval use, is probably more applicable today than

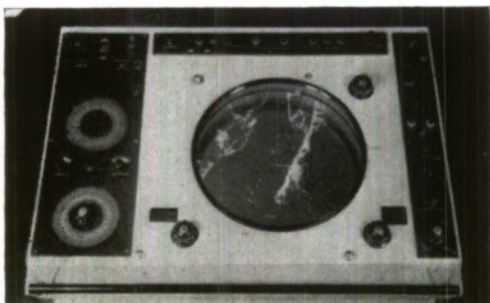FIG. 4. Display equipment.

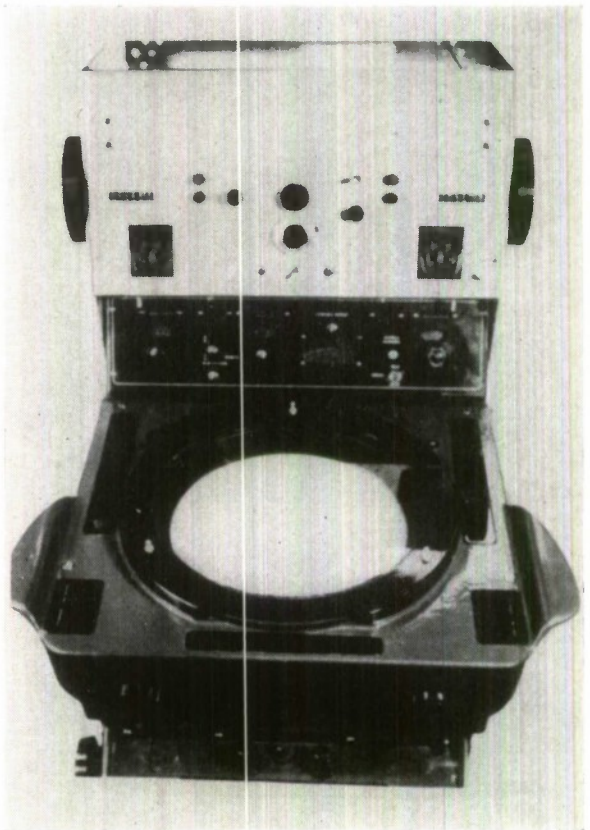FIG. 5. A typical trace.

FIG. 6. True motion display.

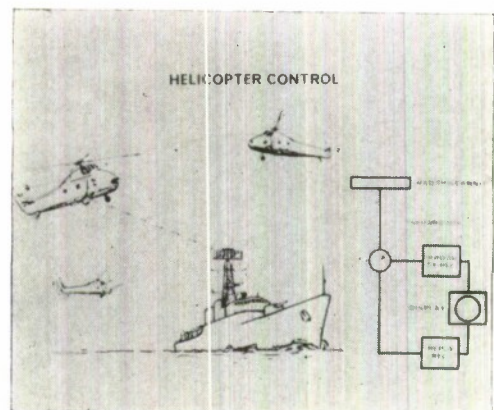FIG. 7. The Decca "Alpine" precision ranging unit.

FIG. 8. Helicopter control.

FIG. 9. Reply receiver.

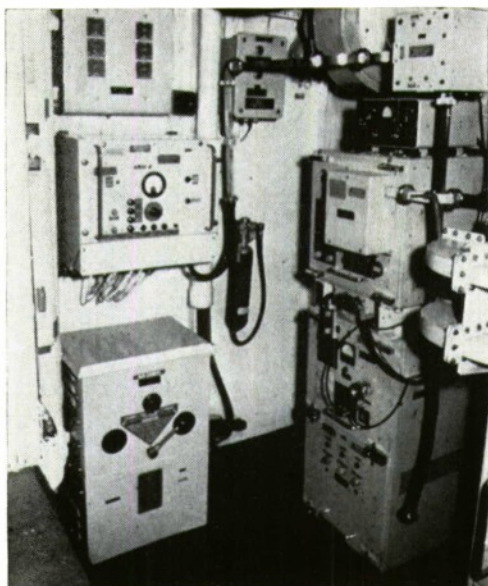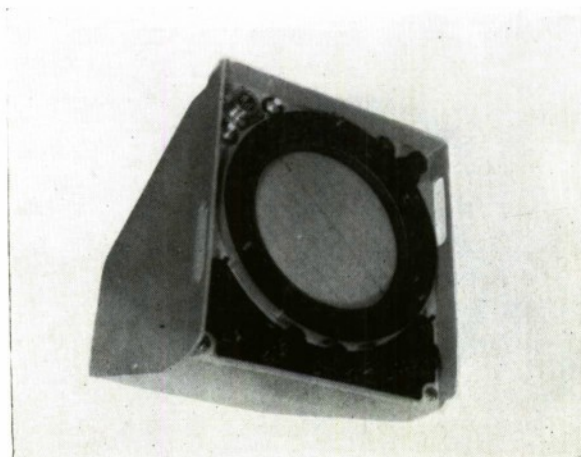FIG. 12. Slave display unit.

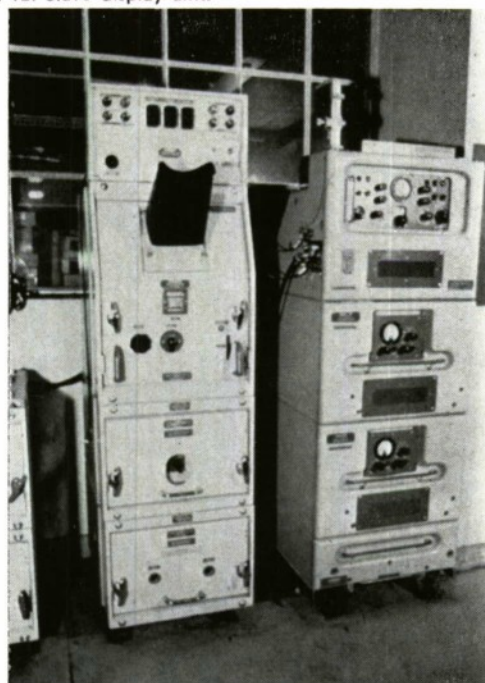FIG. 10. Photo plot display.

FIG. 13. Type 1000 series.

FIG. 11. Portable display unit.

FIG. 14. Submarine display.

ever before in the field of navigational radar. How then should one assess modern commercial products, bearing in mind one may be required to satisfy the needs of different classes of fighting ships, both surface and sub-surface as well as naval auxiliary craft.

May I suggest the following criteria (Fig. 15) are important in the assessment, though the list is not necessarily in order of priority.

PERFORMANCE

RELIABILITY

MAINTENANCE

MONITORING

SIZE

ADAPTABILITY

COST

FIG. 15.

A high standard of equipment *performance* has been attained by all United Kingdom radar manufacturers, coupled with a flexibility of system to suit all classes and sizes of ship. It should not be necessary to insist on higher standards for what is after all the same fundamental requirement in a civil or military vessel. *Reliability* is a subject in itself and capable of many interpretations. Obviously the highest mean time between failures should be obtained with immediate operational readiness and no fall off in performance. Some indication of the reliability of modern equipment can be obtained from an assessment recently made by industry on operational British Merchant radars, where it was shown that on average no more than one or two servicings per year per set are necessary. But one should also remember that a warship is expected to be completely self-reliant for longer periods than a merchant ship, and the ability to effect on-board repair and *maintenance* is still most important. Even the skilled maintainer can-

not be efficient without the means of *monitoring* performance, and assistance in fault diagnosis. Which leads me to the question of *size*. A compact, space-saving design, whilst desirable, does conflict with the need for sub-units which are accessible and easily replaced. A reasonable compromise can be achieved, using printed circuit boards and connectors, without a significant increase in size or the number of connections between sub-units.

It is to be expected that any *adaptation* of a navigational radar to perform additional military tasks will lead to extra financial expense, but a prudent choice of the basic radar can minimize the *cost* of modification and provide equipment equally efficient but considerably cheaper than that specially designed to a military specification.

Future Trends

Finally, what advances in navigational radar may be expected in the future. One important addition will be the use of collision avoidance devices. The A.S.W.E. in collaboration with the Ministry of Transport is engaged in the development of a Closest Point of Approach computer. This will be associated with the navigational radar and will provide accurate predictions of ships courses, eliminating misinterpretations and confused situations which can still arise in reading navigational radar data. The change from thermionic to solid state devices is already apparent and the next stage will be the elimination of all thermionic components except for the magnetron and cathode ray tube, and greater use of encapsulated and packaged circuitry. There appear to be no technical reasons why this should not be achieved in the comparatively near future bringing with it considerable advantages for both civil and military use particularly with regard to maintenance and support. Eventually a completely solid state radar with its attendant high reliability may lead to a completely automated computer controlled navigation system.

NORTH ATLANTIC TREATY ORGANIZATION
Defence Co-operation In The Field Of Electronic Parts

H. Lewis, R.N.S.S.
Services Valve Test-Laboratory

Since the inauguration of the NATO Alliance in 1949, there has been continuous technical collaboration between member countries on almost every aspect of military equipment for use by the joint armed Forces. This includes electronic equipment and hence electronic parts. This article attempts to describe the aims and procedures involved in collaboration within a section of this latter area of interest.

General directives on policy and technical programmes in each field are determined and prepared by an Armaments Committee (or more recently in some fields by the Conference of National Armaments Directors) comprised of senior representatives of each member country. The general duties for the organization of the collaboration systems are undertaken by the NATO International Staff and Secretariat, Defence Support Division. The main relevant term of reference of this Division is:—" the promotion of the efficient use of the resources of NATO for the equipment and support of its Forces. This especially involves

(a) the study of problems concerning the standardization of weapons and equipment, their development and production and their supply and maintenance within the framework of the defence plans of the Alliance, solutions being sought on a co-operative basis between member countries, and

(b) the organization of exchanges of information related to these problems ".

This Division is also responsible for the distribution of all official papers, for making arrangements for meetings and the provision of secretarial assistance.

In the field of Electronic Parts, co-ordination of technical programmes is supervized by the Group of Experts on Electronic Parts (AC/67). The specified mission of this Group is:—

(a) To increase the availability of adequate quantities of assured-quality parts for electronic equipment of NATO nations and to foster economy in the development and production thereof.

(b) To facilitate the operations of NATO Forces by encouraging the use of a minimum number of selected parts having the maximum practical degree of interchangeability.

To accomplish this mission, the group of Experts is empowered to create Special Working Groups to deal with specific categories of electronic components *e.g.* Electron Tubes, Resistors Capacitors, Semiconductors, Frequency Control devices etc. These Special Working Groups have general terms of reference which (in abbreviated form) are:—

(a) To exchange technical information on research and development of electronic parts within their scope and to report on either the lack of suitable development or unnecessary duplication in the field.

(b) To propose recommendations for tests and test methods which will be mutually acceptable to member countries.

(c) To propose a uniform system of marking and methods of marking which will facilitate the identification and use of parts.

(d) To prepare Standard Lists and Electronic Parts Recommendations e.g. Preferred Lists etc.

(e) To exchange technical information on methods of production, inspection and related matters.

(f) To undertake other studies as deemed necessary by the Group of Experts.

These terms of reference can be fulfilled in various ways, and in this article the work of Special Working Group SWG/8 (Electron Tubes) is described to illustrate the procedures and practices used to achieve the required collaboration in the specialist field. Some typical items on which agreements have been achieved and implemented or are under constant review are listed.

In some instances when a restricted or very specialized area of work is to be examined, this is dealt with by a Sub-Committee or an Ad-hoc Restricted Working Group. Members of these are usually specialists in the subject under consideration rather than individual National representatives. They may also be further supported by advisers from Industrial Organizations concerned with the subject being discussed. The recommendations of each Ad-hoc group are then submitted to their parent Special Working Group for formal action.

The formal results of NATO technical collaboration are usually published in one of two forms:—

(a) As a STANAG *i.e.* Standardization Agreement. This is an obligatory agreement in which every member country undertakes to implement the terms and conditions within his Military (or Government) Organizations. Such documents are signed by the Government Ratifying Authority of each country and are subsequently published by the NATO Military Agency for Standardization for promulgation within member countries. Typical examples of STANAGS are:—

(i) STANAG 4012—The Designations and Marking of Electron Tubes. This STANAG defines the type numbers (designations) to be used for identification purposes as marking on the actual tubes and in all reference documents used by NATO countries. It further lists, in a supplement, the code markings to be put on a tube and its packing to indicate the specification used for acceptance, the qualification code, the manufacturers identification code, the date of acceptance and any other markings deemed necessary.

(ii) STANAG 4093 applies to all classes of electronic parts. It is an agreement between member countries that each will recognize the other's Qualification Approval of an electronic part and will, in general terms, accept such approval when purchasing such parts from another country, "without recourse to further Qualification Testing".

(iii) STANAG 4107 is an agreement that member nations will provide a Government Quality Assurance Service (Inspection) and make this available, within defined limits, to member countries for use or orders in all areas of defence supplies.

(b) As a NEPR i.e. NATO Electronic Parts Recommendation. These contain agreed information or technical data to be used as far as practicable by all NATO Government and Military Organizations. They are not obligatory in the same sense as STANAG, but member countries are expected to make every endeavour to align their parts and procedures to ensure harmonization of parts and rationalization of the number of variants of parts used in military equipment. To achieve this NEPR's have been agreed and published on a number of different aspects, viz:

(i) NEPR 18 is the NATO Preferred and Guidance List of Electron Tubes including the recommendation that, "unless impossible for valid technical reasons", only those tube types listed in the NEPR should be used in military equipment being designed for use by NATO forces.

(ii) NEPR 40 (Prepared by an AC/67 Sub-Committee) is a comprehensive document describing Environmental and Durability Tests for Electronic Parts.

(iii) NEPR 60 in a document containing "Recommendations on the preparation of National Basic Specifications for Electronic Tubes". This contains details of procedures and test methods compiled from information in all currently existing Military Specifications, together with any additional recommendations available from sources such as publications issued by the International Electrotechnical Commission. It is in fact based on the U.S.A. specification MIL-E-1 but has been amended and expanded as found necessary. NEPR 60 is intended to

form the basis of National Specifications prepared and issued internally by member countries. It is expected that this NEPR will be supplemented in the near future by the addition of a further NEPR on " Recommendations for the preparation of National Detail Specifications (TSS) for Electron Tubes ".

Note—Documents containing technical recommendations will in future be known as NETSR's.

Exchange of technical information on new developments is a subject of importance to NATO countries. In earlier years when developments in the fields of electron tubes were many and varied, discussion on these, followed by an exchange of reports and technical data, occupied an appreciable amount of the working time of the Special Working Group. In recent years however new developments have been less numerous and are mainly in the area of microwave tubes and electro-optical devices for special military purposes.

A considerable exchange of information has taken place over the period of existence of these technical groups. The smaller countries, especially those without development or manufacturing resources for electronic parts, lean heavily on those larger countries having these resources thus their main contribution consists of their technical knowledge and experience as users of parts. These same smaller countries adopt and use all of the NATO agreements and recommendations without modification and therefore they publish STANAGS and NEPR's directly as their official military standards. This is not, in fact, always feasible for larger countries with established systems and organizations for their military needs, some adaptation being occasionally necessary to satisfy special local requirements.

It may be informative to describe briefly some typical problems encountered during the collaboration discussions on electron tubes. Not infrequently, differences in approach and interpretation of matters which, outwardly, appear to be quite straightforward have resulted in quite unexpected and lengthy discussion before agreement could be achieved. In some instances it is found that the apparent difficulties are caused by language translations or interpretation of meaning rather than by real technical differences. In other cases the national attitudes and procedures for implementing the same practices varied markedly between countries.

There have been widely differing views on what it to be understood by the expression " functional interchangeability ". One view is that minor electrical differences appearing in tubes manufactured to similar but not identical acceptance test specifications should be tolerated provided that the tubes can be shown to operate satisfactorily in the equipment for which they are required. The opposing view is that such differences should not be allowed and that tubes manufactured by all countries must be identical in every respect, within narrow limits, so that functional interchangeability can be automatically assured.

Another problem has been to reach agreement on the differentiation between essential and non-essential features when attempting to determine the content of acceptance test specifications. One view is that non-essential aspects such as second-order physical dimensions need not be specified or subject to inspection. The alternative view is that all measurable dimensions should be the subject of acceptance inspection whether or not they have any effect on mechanical fitting in equipment. Blemishes in glass envelopes for tubes have also caused differences in opinion in this respect.

It is obviously essential that the work undertaken by NATO collaboration groups should be critically examined from time to time to ensure that the achievements and results obtained justify the effort and cost involved. The Special Working Groups are directed to ensure that any proposals for new work are carefully scrutinized before being added to their programme. The programme of work of each Special Working Group is revized annually by the Group of Experts and is confirmed, or modified if necessary, before any proposed new work is commenced. It appears probable that the NATO Alliance conditions may be the subject of a review in 1969—its 20th anniversary—and it therefore remains to be determined whether these activities continue as hitherto or whether, in the light of 20 years collaboration, changes will be deemed to be desirable by the needs of present day defence requirements or by the rapidly changing developments in the field of electronic parts.

TECHNOLOGICAL INNOVATION AND LONG RANGE PLANNING

M. Zvegintzov

National Research Development Corporation

Technology is the application to production and services of science-based information and methods. It is thus, in the broadest sense, the role of the engineer. Whether we like it or not, we live in an increasingly intensive scientific and technological world. Science is expanding rapidly, because man is an inventive creature with an insatiable thirst for knowledge and the urge and power to create. Innovation in the context of this talk is the large scale reduction to practice of scientific discoveries in the national economy. Such a process is 'ex hypothesi' long term, and pursuit of it—particularly of radical novelty—must inevitably involve long range planning. One must probe into the future and exploit success, as well as broaden it, bearing in mind possible ramifications. Because all the possibilities of a discovery are not immediately visible the long range potential of any immediate success must continue to be explored. But a system and discipline are needed, otherwise one risks dissipating effort on by-products—often illusory. (There is growing evidence that the value of so-called 'fall out' or 'spin off' from main programmes of research and development is marginal).

So one has to assume that science and technology will continue to expand and become increasingly major factors in developed economies. Accordingly, greater cross-references and interpenetration, i.e. increasingly complex systems, will emerge. So an integration principle—scientific, technological, economic, social and educational—will have to be applied in the overall strategy alike of business enterprises and of the public sector, including central Government. Quite a number of quantification techniques aimed at achieving this are now available, such as cost/benefit analysis, input/output matrix, and so on. However, the creative element in innovation introduces an unknown and in part unknowable component, and one asks how can this be quantified? The unknowable component can vary widely and thus affect considerably the factor of uncertainty—so when dealing with innovation all attempts can only be approximations. This, after all, is in accordance with scientific method: as a somewhat old-fashioned experimental scientist, I believe there is no absolute law of nature but only a number of working hypotheses assumed to be correct until there is firm experimental evidence to the contrary. So there is no absolute continuing certainty but only a number of fairly firm assumptions, such as the Second Law of Thermodynamics. One should base oneself on the main relatively simple scientific principles but continually test knowledge and feasibility and be on the alert for possible deviations. If such are reliably noted this is a warning for prompt and thorough investigation. The atomic and nuclear science fields are fruitful in such problems. We may laugh at perpetual motion, but proposals involving a subtle form of it—perpetual motion

of the second order—occur and may be very difficult to identify. Force of gravity is generally assumed, but possible indications of anti-gravity, just as of anti-matter, do seem to appear.

However sophisticated and apparently quantified techniques of assessment and prognosis may be for innovation trends and specific projects, the inevitable unknown component introduces a a qualitative factor, which may even be dominant. It is important, therefore, to identify where in the evaluation process such factors occur. Probably the most complete and sophisticated methods of quantification are those of the Pentagon (serviced by computers and other resources of the Rand Corporation). Not long ago three Pentagon representatives engaged in novel Defence project planning came to see me to exchange ideas: they were extremely intelligent, enthusiastic and friendly (although I would not like to have met one of them on a dark night in a Soho backstreet). They outlined 30 to 40 quantified systems, based on about 15 primary factors. The use of computers and other high-speed methods, a great deal of ' jobbing backwards ' and processing experimental data were possible for rapidly refining the systems. As we talked it quickly became clear that certain crucial qualitative assumptions — call them ' hunches ' — were involved. This they readily admitted, and that a form of qualitative matrix built on subjective judgements was inevitable. At Rotterdam last autumn Professor de l'Estoile presented, with the clarity and inevitability of French Cartesian education, a brilliant and precise paper on the evaluation system he used. At a later conference on strategic studies I attended, he gave another paper introducing political and strategic judgements—which, I must say, sounded much less convincing.

As long range planning is usually deemed to be mainly economics, I must introduce some words of warning. It is dangerous to rely on extrapolation of economic trends as reliable forecasts—particularly in a rapidly changing scientific and technological environment. For instance, national and international fuel supply forecasts some years ago, based on consumption and production projections, all pointed to acute fuel shortage and resulted, in part, in some very rash capital commitments. These forecasts ignored two factors—the inevitable trend of technological improvement which drastically reduced fuel consumption for a given job, and the spur for greater productivity which these forecasts of shortages stimulated. (The creation of NIFES, for instance, achieved quite remarkable economies in industrial fuel use). I discussed this dilemma with Professor Stone in connection with his input/output matrix: he admitted that technological changes could seriously affect the mathematical future model of the economy. His hoped for remedy appeared to be qualitative discussions with knowledgeable people to try and see what technological advances may take place, and then give ' pro forma ' values to them. The ' reductio ad absurdum ' of trendomania is extrapolation of current curves for civil service expansion, production of physicists by the universities, and drug addiction. According to these data, at the beginning of the twenty-first century everyone in Britain will be a civil servant and a physicist—a highly desirable target for introducing science and technology to the civil service, but unfortunately they will also be drug addicts. I recommend to the reader the Melchett Lecture for 1966 of the Institute of Fuel recently delivered in this auditorium by Sir Owen Saunders, the Vice Chancellor of the University of London, with his sarcastically pointed illustration of what graphs can do.

However, lest I offend the economists, there is also the other side. The wonders of science can rightly claim to be able to achieve anything: as a German scientist once said to me, one can always make coal from diamonds. It is a long, complicated and expensive process—not only because of the cost of the raw material—but it can be done. Few things are more expensive and damaging in the long run than the scientific and technological ' sacred cows '. There is thus need for constant mental cross-reference and feed-back.

In planning innovation the time factor for markets is of immense importance: unless innovation can be ' reduced to practice ' and become useful to the economy—which means full-scale sales to customers—it is worse than useless. If you will forgive me for ' plugging ' my own organization, the 1966/67 Annual Report of N.R.D.C. gives in Appendix III a list of our failures and the reasons why: inability to establish a market is a usual feature. One particular item—acetylene production—is an interesting example of timing. As the starting point for a wide range or organic chemicals, acetylene is a versatile and highly reactive raw material. Its production, unless one has cheap energy, is relatively expensive. There was a possibility, however, of adapting a gas turbine technique for direct recovery of the large amounts of low grade heat evolved in the hydrocarbon cracking process for acetylene. It was assumed that for thermal efficiency reasons advances in gas turbine design as prime movers to cope with higher inlet temperatures (essential for acetylene) would be taking place anyway. So N.R.D.C's effort was concentrated on developing the requisite chemical engineering rig. Unfortunately, progress in gas turbine engineering was much slower than expected, and although through

various modifications a reduction in acetylene costs could be achieved, progress meanwhile in alternative raw materials and methods made the market prospects no longer worth while. It is interesting to note that when jet propulsion/gas turbines as prime movers were first being considered, by a fortunate timing alloys suitable for the turbine blades working at the temperatures then envisaged became available, thus enabling working units to be manufactured.

To be commercially viable, innovation requires, first, techno-economic selective forecasting and then a degree of assessment. A twofold approach is indicated:

(a) identification of needs to be met and problems solved and

(b) possible practical applications arising from scientific advances.

(a) is relatively easy compared with (b), much of which must be crystal balling. For (a) one should ask oneself, "Where does the shoe pinch ? Why ?" and then go on, " Does any technology exist, could be adapted or even invented, to achieve the desired end ? " Therefore the sort of " systems approach " mentioned earlier should be adopted. However, before going into considerable detail, one should get a rough idea of the areas and priority importance of the needs, and accordingly start with a few relatively simple basic criteria of the British economy. To give a few examples:

The population trend: from about the middle 1950s to 1970, because of past birth rates, there is an inevitable tightness in manpower of working age. This could have been roughly foreseen as early as about 1955, with the inevitable consequences of manpower shortage in rapidly escalating labour costs etc. The age group numbers and distribution for the late 1970s and 1980s are much better, but meanwhile their effects on demands for housing and education are felt, because these age groups are not yet in the productive range. In this sort of long term projection great accuracy is neither possible nor necessary. The order of magnitude is enough to start with, which with time is constantly up-dated and refined. Recently the Ministry of Labour published very interesting detailed statistics on the above which, to my great satisfaction, I found reasonably closely confirmed our own very rough " guesstimates " made about 1955/56, which had led us to certain qualitative conclusions on techno-economic progress needs. Another basic factor is the balance of payments— not only concentration on exports but also on imports saving (I am glad to see, by the way, that the Government has now publicly said so).

It is well known that a larger quantity of material is used for a given job than the intrinsic strength and quality of the material, given good

design, demand. This is an urgent (and rewarding) problem for the design engineer in co-operation with the materials scientist.

World trends in food supplies are unfavourable, so concentration on more " unconventional " methods is increasingly called for, because the time factor is rapidly shortening.

The rate of capital formation is crucial: expanding technology is capital-intensive, so increasing investment (including trained manpower) is a continuing " must ". The output of scientists as compared with engineers in this country is at present out of balance, with the ratio scientist : engineer significantly greater than in the U.S.A.: as the amount of science available is now more than enough for practical application, the relative lack of techno-economic progress in Britain may well in part be due to this unbalanced output of trained manpower.

A few years ago energy appeared to be a bottleneck: now the picture is entirely different with an almost embarrassing range of choices. This, however, again emphasizes the time scale effect and the need to make timely decisions: unfortunately again and again experience is " Too little and too late " with delays altering the relevant factors on which ultimate decision is based.

For innovation projects, the cost ratios 1:10:100 can be taken as a rough yardstick of research: development: full-scale commercialisation, with a time factor of seven to 10 years before full-scale operation. Experience shows that at present resources are ample for research and development, and it is at the next stage—the first full-scale venture into the unknown—that bottlenecks seem to occur. (This is also the experience abroad). When once, on the basis of main criteria, areas and priorities for potential innovation have been picked, they should be broken down and studied in greater detail, using the sort of quantified methods already mentioned. From such studies more derivative factors will evolve and can then be selected to fit the time scale and size of any ultimate effort.

In a complex economy most identified needs are " straddling problems " across a number of disciplines: to tackle them " combined operations" must be mounted, requiring first and foremost the power of mental cross-reference to grasp all the various factors—technological, economic, social and political—involved.

As regards (b)—useful application of new scientific and technological possibilities—this is much more difficult. One should try to identify the advancing sciences and the reasons for this, then, after breaking them down into more detailed sectors, to select those where a crucial novel element appears making further study worth while.

One possible procedure, particularly for long term purposes, is a form of brain-storming exercise—collecting the views, however wild, of scientists, technologists and forecasters. Many suggestions will be out of court, but past experience seems to show that ultimately useable applications do manage to crop up unexpectedly, however unlikely they may appear at the time. Next comes a shifting process—to reject what clearly cannot be done or, because of gaps in essential knowledge, the time span is wrong. The residue then can be more thoroughly investigated. However, the rejection criteria should continue to be watched, as any significant indication of change must be followed up.

Thus at this stage one has to look for factors of kind rather than degree. A few examples from the recent past or even the near future may be quoted—also some " famous last words ".

From our own experience — the antibiotic Cephalosporin, one of the financially most successful of our projects, was originally chosen because it came from a good stable (the late Sir Howard Florey, as he was then), also, because of the growing menace of penicillin-resistant bacteria, the chase for new antibiotics was a ' must '. The crude product appeared to be stable to the enzyme which destroys the penicillin molecule, although its molecule is of similar shape and therefore likely to have comparably useful properties. So we went all out— it took a long time, but the result is a major success. The fuel cell was selected because ' prima facie ' it is electrolysis in reverse and avoids the thermodynamic limitations of the heat engine cycle. This is still a very long pull with formidable engineering obstacles. Quite recently there has been much talk about magnetohydrodynamic power generation (or MHD) which again is a means of increasing the thermal efficiency of generation. The principle of using ionised gas flowing between magnets as a dynamo is ingenious, but problems of sufficiently high ionisation and of construction materials in view of the high temperatures involved are formidable and possibly prohibitive. In actual fact, efforts were made to develop it, but even assuming success the cost involved as compared with progress in atomic power stations make it unattractive. Fluidised electrodes give a possibility of very high current densities as well as very large catalytic surfaces, thus introducing a new dimension into practical electrolysis. Lasers are enormously high energy pulses of very short duration, accurately beamed, offering very precise energy input—possibly for initiating reactions. The selective weedkiller was based on an elegant study of plant metabolism chemistry, showing that certain categories of weeds were killed by a particular chemical degradation product which was harmless to others. So the next stage was to synthesize a compound which, when absorbed by the plants and metabolised, gives that particular degradation product.

Quite often a ' prima facie ' innovation, when further explored, reveals gaps in fundamental knowledge and thus stimulates further background research. This happened in fuel cells where absence of important electrochemical knowledge was revealed, and academic work to that end is now in progress. In the early days of Hovercraft it became apparent that the aerodynamics and hydrodynamics in this sort of confined air cushion space were not fully known, nor being studied, and research had to be mounted. In the flexible towed tanker, the *Dracone,* trouble was experienced with the laminated fabric—which had to stand up to repeated flexures, salt water, air, sunlight and (internally) hydrocarbons. It emerged that proofed fabrics are mostly made by rule of thumb and much remains to be done on the fundamental knowledge of lamination and adhesion. An interesting policy of approach is by the U.S.A. space research organisation—N.A.S.A.— where projects often reveal subsidiary components presenting a challenge which must be solved before the main project can proceed: thus the very fact of an advanced scientific innovation creates the sort of shoe-pinch referred to earlier. (By the way, for N.A.S.A., rivalry with the Russians seems to be a built-in shoe pinch).

All innovation, not least in the ' wonders of science ', requires a strong sceptical atmosphere— both positive and negative. Among ' famous last words ' can be quoted those of Professor Kipping, the discoverer of silicones, who thanked God he had never discovered anything of any possible use. Silicones were much used for specific purposes in the War and are now essential components of many water-repellent surfaces and proofed fabrics. Lord Rutherford is said to have pronounced that he could not see any possible use for atomic energy.

In tackling projects, whichever the way of selection, two important factors must be borne in mind.

The time scale: selection may be too late for useful application, or too early involving too much ancillary work to mobilise the essential information and resources. Therefore successful innovation requires an essential element of luck.

The second factor is that of danger: innovation must ' fail safe '. There is already a significant accumulation of technological disasters, by no means all in the nuclear field.

On timing, I have already mentioned the acetylene project, which was both too early and too late. To early—because gas turbine technology had not kept pace and would have involved com-

plicated and unacceptable additional work if we had become involved; and too late—because when significant useful advances had been achieved the market was no longer there. (My parallel example of gas turbines as prime movers showed a distinct element of luck as both the right achievements appeared at the right time, added to which, of course, there was the additional spur of war-time need).

As regards 'fail safe', even when all possible precautions are taken, new unexpected phenomena can appear—the more sophisticated the innovation the greater the risk. The Winscale nuclear accident is a good case in point which comes to mind. The disasters to the two early Comets, although in the end traced to the well-known phemenon of metal fatigue, were, it is said (even by American aircraft experts), to have been unpredictable at that particular date, because the combination of vibrations and pressure differences at the said points of failure produced novel and unassessable stresses. In underwater technology the well-known need for gradual decompression is adequately dealt with, but a new physiological defect seems to be appearing—bone marrow damage associated with prolonged pressures even when no work is being done. The causes are still unknown but research is in progress. This sort of risk is always present in new drugs—the Thalidomide disaster is a case in point. Even with increasing knowledge of biochemistry, physiology and metabolism phenomena, the emergence of delayed side effects can be a quite unpredictable time hazard. Nor is there any real substitute for the human guineapig. So the best one can do is to locate where the likely or possible weak points are and concentrate on these. An interesting example of this sort of analysis and, as it turned out, its correct experimental verification, was the introduction of automatic computer control on the French railways, whereby both tracks can be used in either direction for the purpose of allowing faster trains to pass slower ones. The computer works out safety distances and time margins and then automatically switches slower trains onto available sections of the opposite track. For this purpose accurate monitoring of train positions and speed calculations fed into the computer is necessary. It was known that the automatic system must fail occasionally, but the chances of this were calculated as very much less than with human control, and the calculated risk was accordingly taken. On a particular occasion a major disaster occurred because the fast train was switched by mistake onto a side track. On examination, this was found to be due to a weak human link in the chain—monitoring had been entirely automatic except for one observation point where a human

observer mistook the fast for the slow and signalled the computer accordingly, which then did the rest correctly. One should remember, however, that the 'unknowable' component in innovation must introduce a quantitatively unassessable factor into risk.

I now come to that all important aspect of planning: decision making. As you have seen from what came earlier, this must inevitably be a blend of quantified evaluation and subjective qualitative factors—'hunches', to use a simple word. As these latter permeate the whole, the price of success must be constant refining through feedback—i.e. eternal vigilance. Unfortunately there is a growing tendency in Britain, and in certain other developed countries, for 'science' to be regarded as good in itself—a 'sacred cow'. Accordingly there is a multiplication and expense of trained personnel and an increasing burden is put on resources for 'clever men and women to have fun'. So Finance Ministers are now taking a hard look and applying squeezes. The popular slogan 'More research and development for progress', may be obvious by itself but so far no clear-cut quantitative links between R. & D. effort and economic growth have been established, even though many attempts have been and are still being made. Recent progress such as cost/benefit analysis of research is probably a useful discipline, and the various other quantifying techniques evolved should be applied wherever possible. But the pendulum has perhaps now swung too far the other way, with growing neglect of the unknowable component. The fashion of 'd.c.f.' calculations for instance is fast moving into the 'sacred cow' category. The volume of scientific and technological data now available is probably greater than the ability to process it effectively and to select proper priorities. Britain is conspicuously backward in striking a proper balance, partly because the administrative structure here is bad for the purpose and partly because of the disproportionate production already mentioned of scientists as compared with engineers. In the more spacious days of rapid expansion this equilibrium was unnoticed, but now with excessive pressure on all resources and the need to re-adjust, the moment of truth has arrived. This does not mean that background research for the advancement of knowledge should be abandoned or seriously curtailed—indeed, selection and vigorous prosecution of applied projects, as I have already said, often reveal gaps in fundamental knowledge and thus stimulate background research. But the latter thus becomes more, as the Americans say, 'mission oriented', which is, I submit, an essential element of planning and an aspect of the integrated approach which I mentioned at the beginning.

Summing up in conclusion, I should like to emphasize that innovation imposes a particular form of the decision-making process on long term planning, which is essential in national economies of whatever scale. However, it is particularly important (or visible earlier) in national economies of moderate size which, because of their relatively limited resources, are forced into rigorous selection from the outset. Hence for them the particular importance of acquiring the various selection techniques increasingly available. In Britain we are currently spending nearly £1,000 million per annum under all headings (public and private, defence and civilian) in research and development and have a percentage of G.N.P. spent on R. & D. only slightly smaller than in the U.S.A. Even before the present series of financial so-called crises (which I suspect are almost tailor-made through postponement or lack of decisions, and faulty analysis) selection was necessary. Now it cannot be avoided. While arguing the toss or postponing decisions time passes, costs rise and factors change: so ultimately wrong decisions are made for what were once the right reasons. However many sophisticated quantifying techniques are available, the decision ultimately depends on individual human beings who have to exercise a greater or lesser degree of 'hunch'. There is therefore a growing need to evolve a new kind of highly developed creature—the scientist and technologist with maximum powers of wide mental cross-reference and the ability to speak a relatively simple language which widely differing disciplines (including economics, sociology and politics) can understand. Because of the speed of scientific progress and the evolution of techniques this factor is becoming an urgent matter for basic education and for refresher course training in the immediate coming years. We can neither contract out of our increasingly techno-economic environment nor out of our thirst for knowledge and power to create. In the closing scene of his symbolic play, 'Hassan', on man's life quest, James Elroy Flecker puts into the mouths of pilgrims joining the caravan about to start for the fabulous city of Samarkand the words: "For lust of knowing what should not be known we take the golden road to Samarkand".

Mankind is launched on this glittering journey which demands a well prepared caravan led by those who know or can find the way.
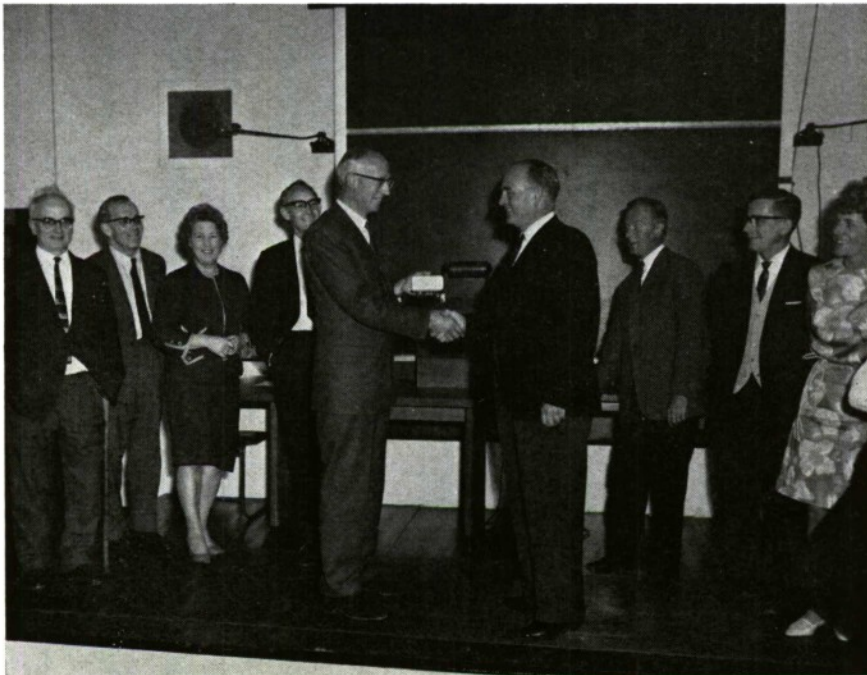
Mr. Colin H. Beaumont (right) receiving the Imperial Service Medal from the Officer-in-Charge, S.E.R.L., Mr. G. P. Wright.

AN ESSAY ON THE ORIGIN OF TIME

R. N. Gould, Ph.D.*

Victoria University of Wellington, N.Z.

Introduction

The great metaphysical systems of the past have by now all encountered serious and possibly insurmountable difficulties. This is hardly surprising; indeed it is a disadvantage of thought on the grand scale that before one can know anything one must, as it were, know everything, a condition mitigating against the success of any such efforts.

The methodology of science, on the contrary, involves investigation by concentration on " isolated " small parts of the Universe, without immediate consideration of the whole. This approach is at the opposite pole, as it were, to that of the great metaphysicians. No doubt many find this second approach to be aesthetically less satisfying than the first, and it is believed that this point formed a criticism by the Cartesian School of the work of Galileo.

The success of the scientific approach, due presumably to the fact that to some extent the phenomena of nature are capable of precisely the kind of isolation required by it, has been possibly the major cause not so much of the downfall of the great metaphysical systems as of their extinction.

However this may be, the fact is that what may be called the piecemeal approach to knowledge has won general acceptance at the expense of more grandiose conceptions. We are now mentally oriented against thought on the grand scale, against the outlook which produced the miracle of the Grecian heritage, and are indeed suspicious of it.

It is therefore with some trepidation that the following incomplete and acknowledgedly inadequate thoughts are put forward on a topic which, whilst being by no means " thought on the grand scale " as in the instances referred to above, is nevertheless too wide to qualify for immediate respectability. A further point of doubt concerns the fact that on the present subject any hypothesis which can be put forward will be very probably wide of the mark and almost inevitably unsatisfactory on grounds of completeness, detail and so forth.

Many however believe that a too rigid adherence to narrow specialism can be as unfortunate as lack of any such restriction. This opinion is by no means confined to philosophers but has been voiced by many great scientists, for example Schrodinger.[1]

In this vein it could be argued that the great systems of the past, though largely discredited, have served more than adequately by the discussion and disputation to which they have given rise. In a smaller way the framing and subsequent discussion of contentious hypothesis on broad topics, such as that treated here, which draw humanity irresistibly by their inherent interest, may give rise to a similar possibility of constructive suggestion and subsequent advance.

To the scientific mind, a hypothesis is only valuable in so far as it is testable (*i.e.* falsifiable) and leads to new testable predictions. But this point could be regarded as a criterion of demarcation between science and metaphysics; genuine metaphysics, whilst being intelligible and logically satisfactory, consists of statements which are untestable. It may be that some of these statements appear to be inherently untestable, in which case, or rather if such can be shown to be the case, the material must inevitably remain metaphysical. However, what is testable is relative; relative for example to the current state of knowledge and technology and possibly to the whole contemporary theoretical structure, so that at least some of what is metaphysical today may well become testable and hence scientific tomorrow. Russell[2] has made a similar point regarding the general content of philosophy. With such points in mind it

* Associate Professor, Sometime P.S.O., R.N.S.S.

could be argued that scientists might do well to pay more attention to metaphysics, even though contemporary metaphysics is necessarily outside their immediate sphere of interest. It would seem from the literature that the new advances in physics are bringing precisely this situation about. On this view it seems probable that metaphysical speculation on topics closely associated with scientific matters, quite apart from inherent interest, may well have some justification from the purely scientific point of view. In so far as the above considerations carry conviction it might be contended that in certain cases the opening of a possibly new line of discussion by means of otherwise unsatisfactory work such as is presented here, may serve some useful purpose. It is in this spirit that following speculations are presented.

The Problem of the Origin of Time

The properties of classical space and time flow largely from their homogeneity. An interesting analysis of this point has been made by Capek.[3] The supposition of the homogeneity of space leads to Euclidean geometry via the Euclidean axioms, which Capek argues are all aspects of the supposed homogeneity. It can be argued that from the homogeneity of time follows its Newtonian character; its independence of physical content, its continuity and uniformity as well as its infinity. With this view its absolute character is a consequence of its independence of physical content and hence of its homogeneity.

Classical space and time possessed basically similar analyses; time has even been considered as essentially similar to space and has been referred to as merely a disguised space. In physics, time was taken to have the relevant properties of a Euclidean line and was represented diagrammatically by such a line. In the Minkowski diagram this representation has been carried over into relativistic physics, although philosophically speaking the Minkowski representation suffers from the grave disadvantage of subjectivising time, a facet which arises from the motion of the observer along his world-line. In addition time is dualised appearing also as essentially a fourth dimension of space. As far as this aspect is concerned time is reduced to a mere epiphenomenon: the future is laid out before us. Our subjective dualised time enables us to pass along the world-line. Philosophically, as it were, this dualisation and subjectivisation of time is quite unsatisfactory.

The independence of classical space and time of one another and of their content gives rise to the need for four basic elements in the fundamental equipment of physics; these are, space, time, matter and motion. On this view therefore, space and time are presumably ontologically prior to matter and motion. Motion is essentially the motion of a vehicle as clearly enunciated by Tyndall and others, for example, Hume before him. Thus, matter is prior to motion. These priorities arise naturally on the classical scheme; it is of the essence of the view presented here that their ordering be rearranged.

Concerning the relative priorities of space and time it might be pointed out that physicists usually subordinate time to space. Indeed, time as a variable is suppressed as soon as possible in the equations which describe the physical situation. In spite of certain complications this subjection of time to space is still prevalent in the treatment of relativistic physics, as is evidenced by the Minikowski representation mentioned above. Nevertheless classical time was, as has been emphasized by Capek, ontologically prior to space, time being the receptacle not only of the changing content, but of the successive individual timeless spaces as well. Such priority is reasserted in special relativity where the ordering of lengths is relative, a situation which does not hold in the case of time spans, since the time order of causally related events is an invariant of the Lorentz transformation.

The ontological priority of time implicit in the above remarks has been plausibly and powerfully argued by Capek and Whitrow[4, 5]. Classically the elevation of space to a position of eminence has lead to the suppression of time as variable in the equations of physics and possibly to a block in progress towards an understanding of it. Latterly it has lead to the relegation of time to a subjective role in the consciousness of an observer crawling along his world-line: in Bergson's words time is reduced to " our incapacity to know everything at once " and it becomes a mere epi-phenomenon.

The subjection of time is a persistent trait; it is often, if not usually felt, that time is an unimportant aspect of things. Thus, Russell regarded time as an unimportant aspect of events[6]. It is here contended that such feelings have a sound basis, and difficulties have arisen, not so much by over emphasis on space, but by the neglect of motion and the inclusion of time in the basic categories.

Returning to the classical schema it could thus be argued, and indeed has been argued, that time is quite fundamental. Moreover, owing to the basic position occupied by the concept of homogeneity in framing the classical notion of time, its infinite extension in both directions, that is towards the past and towards the future, is a necessary implication. This implied infinite extension into the past has met both with acceptance and criticism. Averroes, the Arabian commentator on the Stagyrite saw no need of a creative moment. With this

view St. Thomas concurred, but believed on the basis of revelation, that such a moment had in fact existed. St. Augustine put forward, in effect, the view that time was created with the Universe: a subtle interpretation which is in fact carried further in the present speculations. Kant, whose method, as is well known, involved argument for and against various propositions, inveighed strongly against the possibility of an infinite past, though to him as to others the idea of a commencement of time appeared equally unlikely. Russell contested Kant's thesis concerning the impossibility of an infinite past on the grounds that it was based on a misunderstanding of the nature of infinity[7]. This thesis has not found universal acceptance and the present author agrees with Whitrow[8] in his criticism of it; the essence of the criticism being that although the measure can be adjusted in given circumstances to indicate an infinite or finite stretch at will, and is thus an unreal criterion to which to refer, the core of the Kantian objection refers to a completed sequence of an infinity of events. Russell[9] took a different stand in his comments on Sir Arthur Eddington's discussion of this point.[10]

It is here accepted that there appear to be definite difficulties concerned with the conception of an infinite past, as well as with that of a finite past; at least on the classical scheme. It is the principle stimulus of the present work to suggest, if the situation is accepted as an impasse under present theories, what lines the modification of the theory might take in order to resolve the difficulty.

Before turning to the suggestions themselves, it is worthwhile to consider what modifications in the above position may be entailed by the theories of modern physics. It might be argued that these theories are merely closer approximations to the truth than their classical counterparts and that, basically, little has changed. This thesis would be extremely hard to defend and is rejected by most, including the present author. The basis for such a view may perhaps be sewn by understandable conservatism in effecting an associated pedagogical revolution. Accepting the magnitude of the change in outlook, or better, in the metaphysical foundations of physics, the first impression is that the relevant changes are enormous. From the homogeneity of space and time flowed, classically, their attendant properties. With general relativity such homogeneity has been abandoned; moreover with the special theory, space and time have ceased even to be independent. In Einstein's theory of gravitation, the casual inertness of space has vanished and the concept of the independence of space from its physical content modified. With quantum theory the meaningfulness of the con-

tinued divisibility of space and time has been challenged: as Brilluoin[11] points out, in principle, the measurement of a length of the order of 10^{-45}cm requires the immediate destruction of 100 metric tons of matter, so that the infinite divisibility of space is no longer a testable contention even in principle. Difficulties similarly arise with the divisibility of time, leading to the concept of the chronon—a quantum of time. One could continue in this vein; little being saved, save perhaps, as Born has said[12], the formalism of the equations due to Hamilton.

Despite the subjection of time implied by the metaphysically unfortunate concept of the Minkowski diagram, time nevertheless retains its ontological priority in the relativistic scheme, for as pointed out by Capek although there is no juxtaposition of events which would be sequential for all observers, the class of causally related distant events retain their temporal sequence in all frames of reference.

Nevertheless in spite of the upheaval which has been wrought by the new physics in what may be described as the philosophical foundations of physics, the category of time remains, though in modified form. Although it is usual to speak of space-time, or better but less usually of time-space; as far as the observations of events within a given non-relativistic laboratory are concerned, that is as far as any particular observer's frame of reference is concerned, time remains to a large degree, what it has always been. The difficulties or complexities arise when comparing notes between two such observers in relative motion. An implication is that it is difficult to say what one means by earlier states of the Universe and hence, of an origin of time if only because there is no generally agreed time scale ! However although time measure may differ from one observer to another this is hardly the relevant factor in the present discussion. For each and every observer, however his time scale may be related to that of other observers, the same arguments apply as in the classical case; in particular by-passing the question of the measure as the important criterion, as has been done in the classical case itself, one notes that each observer records a succession of different events which it is presumed can in theory be extrapolated indefinitely backwards. It is therefore not clear, in spite of the problem of the ordering of distant events, why just the same difficulties do not arise as formerly. The Kantian argument against an infinite sequence of completed events being applicable to each and any particular frame of reference. On this assumption the problem of the origin of time may be taken as real: once this is accepted the latter becomes a matter of great interest and importance in the metaphysical realm.

One may still take differing views as to its relevance to science: clearly it can be said that the question of the extrapolation of physical theories, and physical concepts, such as time, into the past is not a matter for the scientist to concern himself about. This view can easily be accommodated from the point of view of local physics: it is more difficult to absorb from the viewpoint of cosmological theories. Although it would be a commonly held view, it would be by no means universal, as can be seen by considering the remarks of Helmholtz[13] who in particular recommended approaches with objectives similar to those suggested here.

The Basic Proposal

The first difficulty that is met in trying to discuss the origin of time is the instinctive feeling that there must be something earlier, a feeling which is, in the temporal realm, analogous to the arguments of Archytas and Locke against the conception of a boundary to space. It appears to be self-contradictory to refer to a " period " before time began. The difficulties associated with the admission of an origin for time have lead to continual attempts to circumvent the idea: a major instance being the discovery of the correlation of the red-shifts observed in the spectra of the extragalactic nebulae with their distances. This might be taken as providing evidence of the evolutionary history of the Universe and indicating a natural origin for time. As stated by Whitrow various hypotheses have been put forward however to bypass the difficulty otherwise involved, for example, the theory of the continual creation of matter, and also the oscillatory theory of the Universe. The present thesis has apparently been partly anticipated by Whitrow, who points out a third alternative according to which the origin of time " is regarded as a primeval limit imposed on the application of the laws of nature to the objects constituting the actual Universe ". He instances the concept of a static Universe containing identical particles in equilibrium until the spontaneous decay of one of them. This instant, though not necessarily a moment of world-creation, would be the beginning of time in the sense that it was the first thing that happened in the Universe. In certain measure this agrees with the thesis presented here in that the primitive objection raised above regarding the difficulty of considering time to be bounded is shown to be semantical rather than logical. This method of dealing with the matter is akin to that by which the difficulties associated with radioactive decay laws can be met. For such laws, unlike for example, the Newtonian law of gravitation, it is required to explain away the question of what happens as the variable "t" is extrapolated indefinitely backwards. The answer could be that the applicability of the laws cannot be so extrapolated; or, alternatively put, that there were no suitable objects to which it could be applied.

There are however certain difficulties about this approach which are absent in the more radical suggestion presented here. Naturally the price paid is much higher in terms of a more thorough embracing of metaphysics; that is, the suggestion made by Whitrow, which no doubt was largely merely illustrative; is yet more scientific. The amount of violence one is prepared to do to prevailing views depend on the difficulties judged to be inherent in them: from the philosophical standpoint the violence inflicted on the classical framework by modern physics has been extreme. In that case however the violence in question was necessitated by experimental fact, hence its adoption was strictly scientific. It may be worth pointing out that although events in fact occurred this way, and on account of the healthy conservatism of scientific men of the day could hardly have been brought about any other way, the wave theory of matter might well have arisen from a more philosophical foundation; namely, theoretically from the observations of Sir William Rowan Hamilton concerning the formal mathematical similarity of Fermat's principle and the principle of least action. Similar contentions could conceivably even be made regarding the origin of the theory of special relativity, which might have arisen on a more philosophical basis than was in fact the case had Mach's pioneering work of the nineteenth century and the deliberations of H. Poincaré been pressed further. The question therefore of whether the presently suggested high price to be paid for the resolution of the problem of the origin of time is acceptable, depends on the depth of the difficulty that is felt to exist in this problem and in the associated difficulties arising from the classical conception of time. Those who on the one extreme feel no difficulty to be involved, will see no reason whatever for the present line of thought; if any feel in company with the present author that the problems pose an impasse to the philosopher, such sweeping changes might be worth considering.

To return to Whitrow's illustration, he maintains, correctly in the present author's view, that the concept of the first moment of time is not self-contradictory, for it can be defined as the first thing that happened, " for example, the spontaneous decay of an elementary particle in a static Universe ". Presumably he intends to imply that this first thing can be defined without regard to temporal considerations, however difficult it may be to state such a relation in ordinary language without appearing to presuppose time. However, ordinary language largely results from a distillation

of ordinary experience and one would therefore expect this to be so: the warning one may validly derive from this situation is not felt to be the danger of discussing a self-contradictory concept, but of pressing the conclusions to be drawn from the analysis of language beyond their domain of application. Such a picture of the first moment of time, if taken seriously, and not illustratively, involves however severe difficulties. Although, again, the use of ordinary language and the associated unconsciously held assumptions render the framing and discussion of these points full of pitfalls and difficulties, the attempt cannot be avoided if progress is to be made. In its simplest terms one might argue that the basic difficulty of the above illustration, when taken seriously, is the concept of the static Universe. It is on precisely this point that the present suggestions diverge utterly. It is not easy to frame the objections specifically, but the very concept of the static Universe raises problems as great as that of the origin of time itself and on the present view such problems are closely connected with the latter. It is implied in the above suggestion that the laws of physics are given and their applicability is brought into question. The first moment, defined by the first event, namely the first disintegration of a fundamental particle in a static Universe, does in fact refer a natural origin to time. The first disintegration was the first event occurring in accordance with these laws. However, it is at this point that one feels it difficult to preserve what might be termed the internal consistency of the suggestion, for it must be decided what is meant by the contention that the first event, the natural time origin is to be regarded as " the primeval limit imposed on the application of the laws of nature to the objects constituting the actual Universe ". Alternatively put the objection made above regarding the difficulties inherent in the concept of the static Universe might be framed, in accordance with this last quotation: what is the origin of the so-called primeval limit ?

At this stage one must consider what one means by the term " laws of nature " and their " applicability ". In accordance with the instrumentalist view one might argue that a law of nature is a mere description of what actually occurs, a handy formula for predictions, which, within the limits of experimental error stands up to varied tests. If laws cease to stand up to such tests, the description is altered by modifying the laws to reinstate agreement between them and observed facts. If this view is adopted and the problems of "reality" thereby avoided, then for the static Universe there are no laws, for nothing is happening to be described. The laws spontaneously arise with the first happening.

The operationist view, ably put forward by Bridgman[14] and now enjoying some popularity amongst physicists, would condemn the question concerning the existence of laws in the static Universe as meaningless. In either case it is difficult to see any distinction between the " primeval limit on the applicability of the laws of nature ", and the non-existence of such laws. Accordingly on these views the hypothesis is modified to read:— the origin of time is fixed naturally by the appearance of the laws of nature, which in principle, arise for the present case with the first event. This re-statement is a not very plausible special case of the view to be presented here. This telescoping of the original statement occurs, because on the operationist and conventionalist views "laws of nature" and their "applicability" are terms which are very much tied together: for the meaning of the term " laws of nature " is defined by their applicability.

This re-statement of the original illustration is not however the only possible interpretation. A more discredited metaphysic would bring the term " reality " into the argument. The term reality is one which has an inherent appeal to scientific people, although they have now learnt through painful experiences to be suspicious of it. On this older basis we might proceed as follows: laws of nature are framed, usually in mathematical terms, to describe the events which take place, the possibility of such mathematical formulation being a result of the observation that any standard series[15] constructed to measure a physical magnitude, can also be used as a measure of number. The fact that the laws achieve their aim implies on this view a correspondence between them and the structure of the Universe. That they do not fully describe the course of events with perfect accuracy implies that the correspondence between them and the structure of the Universe is not exact, but can be made more precisely so by the suitable modification of them in accordance with experimental facts. On this view to the laws there corresponds in reality a certain structural organization. This structural organization would from this view, be present whether events were taking place or not and in this sense therefore, the laws must be considered to exist in Whitrow's static Universe and to be independent of their applicability: on this view time would be defined by the laws in which this category appears as variable, so that although the natural origin of time is automatically defined by the first event, time is not created with it. In this sense time has become absolute as in the Newtonian scheme. With this older metaphysic Whitrow's illustration can now be interpreted to read; a natural origin to time is specified by the first event in a hitherto static Universe. Hitherto is in this case a permissible word because on the

metaphysic accepted here time has become absolute: there is no need to refer to a primeval origin of applicability for the laws which are always applicable; it is merely that in the static Universe there is nothing for them to be applied to. This last picture which Whitrow may himself have intended, besides introducing reality and a metaphysic with which earlier generations of physicists would feel at home, does resolve the question of an origin to time, in Whitrow's sense, just as the so-called " heat death " would resolve a similar but possibly philosophically less difficult question concerning the infinite future. It also has the merit of avoiding the discontinuities in the concept of the " laws of nature " when the illustration is framed in terms of a modern metaphysic. A major disadvantage lies however in the apparent impossibility of ever explaining how the Universe came to be static: that is why, in point of fact, there ever was a first event.

This discussion leads naturally to present suggestions. It is seen from the above that the difficulties of an infinite past are avoided by Whitrow's illustration according to a modern interpretation of the meaning of scientific laws by, in effect, the appearance of these laws in a discontinuous manner. This is less difficult to absorb and distinct from the appearance of the Universe together with the laws in a discontinuous manner as proposed by St. Augustine. It retains therefore the merit of St. Augustine's view whilst avoiding some of the disadvantages. From the point of view of metaphysics it still suffers from the following defects. Firstly, the discontinuity in the concept of natural law which occurs at the time origin. As seen above the metaphysic of the Victorian era avoids this last difficulty at the expense of transferring the discontinuity to the actual occurrence of the events, a transference which on account of the continuity in law is difficult to accept. In other words why should this discontinuity in the occurrence of events ever have occurred ? The second disadvantage is that, accepting the meaningless of laws " prior " to the initial event, or better stated their meaningless as referred to the static Universe; why, with this initial event should these laws arise as " going concerns," for according to this metaphysic they were not implicit in the structure of reality, waiting to appear at the first sign of activity, for the metaphysic avoids reference to reality at all.

All of these difficulties are associated with some form of discontinuity. The basic proposal is to retain the idea of Plato and St. Augustine, as revived by Whitrow, but to remove the discontinuity with which the difficulties are associated. This approach may solve the difficulties of an infinite past and certain other metaphysical difficulties as incidentals, in at least two ways. Both turn on some basic assumption such as the habit-principle propounded below: in the one case, that of a finite number of events to which the principle is to be applied, the main gain is merely a lessening of the severity of the discontinuities discussed above. In the second case, if it can be satisfactorily sustained, the gain is much greater at least as regards the difficulties to be discussed here.

In this second case the solution is obtained not by removing the infinity of events, but by removing any possibility of ordering them and hence obtaining a sequence.

This impossibility of ordering events is qualitatively different from that applying in relativity theory. In that field there is no world-wide instant because of the disagreement of the various observers, but any given observer can order events to his own choosing in a systematic and meaningful way. In the present case, however, such meaningful ordering is not merely impossible on a universal basis, it is impossible also for any particular observer at the local level.

It might be objected that the infinity in time has at best been replaced by an unavoidable infinity in space and in the elements composing the Universe, for on the admission of a past infinity of events, either an infinity of events has occurred at some instant or at an indefinitely large set of different instants. In the latter case one has infinity of time, in the former infinitude in world structure. Such possible objections have been met by the preceding arguments which destroy totally the conceptual basis implicit in the formulation of the objection.

On this view one retains a " past " infinity of events but no past infinite sequence, thus it could be argued that Kant's powerful objections have been by-passed. Moreover the method yields the equivalent of a natural origin to time.

There are no doubt many ways of tackling the problem and of removing the discontinuities referred to above. The present suggestion however whilst fulfilling the objectives implied by the preceding discussions, is framed to explain the appearance of law, or more precisely to reduce the existence of such natural laws to a more fundamental origin. Instead of considering the first moment directly, as in Whitrow's illustration, it is more appropriate to start with the present situation and consider a hypothetical journey back through time. The basic principle on which the " explanation " is achieved involves the concept of an evolving Universe. In this respect it is desired to make an illustrative parallel between the world of physics and that of nature.

In the case of the animal world, the adaptation of one species to another and to the various kind of plant life is via inter-relations of the utmost

complexity and subtleness; so much so that a disturbance in the natural balance by artificial means has far reaching and sometimes unforeseen consequences. The discovery of this complex jig-saw pattern which dovetails together in a remarkable manner, has lead some to regard the apparently miraculous organization as direct evidence of the work of God. In similar fashion, although science initially and inevitably clashed with religious authority, at a later stage some apologists have seen in the discovery of natural law an argument from design for the existence of God. It seems to the present author that this stand is valid on the basis of an expressed inability to proceed to further more basic reduction. The miraculous dovetailing in the world of nature has been explained, or reduced to a more fundamental, that is simple level, by the theory of evolution. It seems not improbable that the existence of design in the physical world may similarly owe its origin to a corresponding evolution of the Universe. If varied hypotheses of evolution are considered, the explanation of the origin of laws in nature is brought within the reach of legitimate investigation and, if appropriate techniques were to be discovered, even within the legitimate province of science itself. Without some such speculation nothing can be said concerning the curious fact of natural law and the field must be left to the theologians.

In order to resolve the impasse of an infinite past, and at the same time to bring the origin of law into the metaphysical scheme, an evolutionary principle is proposed by which both may possibly be accommodated. The suggestion also implies an arrow of time and this solves incidentally the difficulties associated with this question. For want of a better description, this will be termed the habit-principle. The latter could be framed, not very precisely, in the following way:— with each occurrence of a particular type of event further such occurrences become more probable.

The basic assumption is thus to the effect that in some unspecified manner, every event leaves a trace or record on the Universe itself, as each personal event leaves a memory trace in ourselves. Moreover this trace is such as to predispose the repetition of the type of event in question. If this is accepted as a working hypothesis we may consider progress through time in the directions of both the past and future. Initially no difficulty would be experienced in defining and measuring a practical variable corresponding to the absolute time of Newton, and the existence of law would make the concept both meaningful and appropriate. For example, similarly constructed clocks would remain in good agreement and clocks based on different types of mechanism would also agree, lending through their invariance physical impor-

tance to the concept, which is essentially a relation of correspondence between processes. However, as we followed our hypothetical journey into the past, law would become progressively less valid; that is, exception to rules however one tried to re-frame them to meet the new situation, would begin to occur and with increasing tempo. The time variable itself would then become harder to define, since its essence consists in certain relations of correspondence between phenomena. As this correspondence becomes less exact so the variable itself would tend to lose meaning and importance. In effect the same problem exists in practical classical physics because of the problem of " error ". However a metaphysical assumption of the existence of true values obeying exact laws is made in this case, although as Landé[16] points out this is quite unjustified by the observations. Landé takes this situation as basic and attempts to derive quantum formalism from the classically observed results, thus purporting to unify physics and free it from the dualism to which it is at present subjected. The assumption of error and the concept of exact laws consequent upon it, implies the existence of a precisely definable concept " time ", for the relation of correspondence between processes, which is of the essence of time, would hence be precise and definite. This is, in point of fact, emphasized by the possibility of defining time as the independent variable in the basic equations of physics. If the scatter of observations is accepted at its face value however, without recourse to the metaphysic of error, a different situation prevails. Landé's thesis is one development of this issue; a further facet concerns the establishment of the concept of time which is our present concern. From the basis of uncontrollable and unpredictable divergences from an ideal situation in which law is exact and hence time precisely definable, no exact concept of time could be framed. This statement is different from saying that no exact time measure could be made in practice; for in the classical situation the time concept can be precisely framed on account of the exact nature of the laws, whereas accepting on the joint basis of exact law and the nature of actual observations the unavoidable assumption of error, no exact time measure could be achieved in practice.

It is evident that as the hypothetical observer goes " back in time " he will according to the habit-principle meet situations which, for want of a better description become progressively less " lawful ", that is events are less predictable. This will involve a degeneration in the usefulness of the present concept of time and a reduction in the ability to order events by use of the observed regularities; regularities which constitute, or are the essence of time itself.

The situation however has been deliberately oversimplified, since the above deductions imply only a finite number of events from the present situation back to the event or series of events from which the habit-principle is supposed to have operated. This restriction is considerable for it takes away a degree of whatever value the proposals may have. Nevertheless it does not take away all the value since it softens the discontinuities discussed earlier at least in their abrupt form: it becomes the problem of the weaker discontinuity represented by the situations: operation of habit-principle/non-operation of that principle.

The problem therefore is how to envisage the operation of the habit-principle without arriving at a complete rigidity arising from the infinity of past events. A solution of this problem could indeed, in principle, remove almost all the metaphysical difficulties associated with time. A possible satisfactory approach is by consideration of the alteration in the basic constituents and structural relations of the Universe brought about by the operation of the habit-principle itself. Granted the evolutionary process to be in operation, successive stages will produce more complex arrangements and metamorphoses of the basic elements, so that different possibilities arise which were at an earlier stage much less probable or even impossible. As long as this increasing complexity continues to offer new possibilities, or possibilities which because they have become more probable, may become effective in practice, so long can the Universe continue to evolve from the point of view of changing structure. The concept is of course different from, but inclusive of, the concept of a Universe evolving according to given fixed laws. Although at least some of the specific new possibilities referred to above may have been conceivable on a purely random basis, they would presumably have been by-passed according to the habit-principle in favour of more mundane occurrences. However, it may be possible that with the advent of more complex structure arising from the operation of the habit-principle, that some of these otherwise by-passed eventualities suffer an increase in probability of occurrence at some stage and may hence be resurrected. In other words, the probability of occurrence of particular events is influenced at any stage, not merely according to the given principle, that is by the number of occurrences of such particular events, but also by the overall structure developing as a result of the former operation of the habit-principle on the given categories and elements themselves. If it is possible for such a development to take place in this diverging and evolving way, rather than in a converging and restrictive way only, then it may

well be that such evolution would continue through an indefinite number of changes. In this way the infinity of past events can be retained, whereas, on account of the imperfect nature of the laws at any stage, the category of time cannot be precisely defined and hence a sequence of events cannot be precisely determined. In this way although an infinity of past events has occurred they do not form an infinite sequence of events, so that Kant's powerful arguments may perhaps be avoided.

It might again be felt that the resolution is artificial, in that such a Universe would always present a similar aspect because otherwise the assumption of a past infinite sequence of events would have to be violated. It is felt however in accordance with the above analysis that the objection does not necessarily hold, since one must be careful not to presuppose a converging development excluding the possibilities outlined above. The genuine possibility of a continuously evolving model having chaos at one extremity and the present position at the other cannot thus be ruled out. This argument raises difficulties at the origin of time: the state of chaos can presumably only be made consistent with the Halsit-principle by making that principle itself dependent (*e.g. via* the constants defining the new time variable) on the new time variable.

Summary of the Scope of the Basic Proposal

The above proposals suggest broad lines along which the resolution of various philosophic difficulties might be pursued. The high price paid for these advantages can only be warranted, if at all, by the urgency of the impasse otherwise presented. The nature of the difficulties which might apparently be thus resolved are:—

(1) The difficulties of an infinite past sequence of events as championed by Kant and Whitrow.

(2) The alternative difficulty of a finite past sequence of events.

(3) The difficulty of reducing the observed law and order in the structure of the Universe to anything more fundamental, as was achieved by the theory of evolution for the complex inter-dependence observed in the world of animals and plants.

(4) The difficulty of interpreting time's arrow: this point is automatically resolved by the basic assumption.

(5) The reversability of classical laws and the paradoxes of Loschmidt-Zarmelo (see also the discussion in this conclusion).

(6) The difficulties of laws of limited duration. For such laws one must explain   what

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happens as the time variable is extrapolated indefinitely backwards. Particular difficulty might be involved in this respect with, for example, the second law of thermo dynamics.

Priorities of the various categories under present proposals

It is felt that present theories cannot satisfactorily account for any, except possibly, the fourth of the points listed in the preceding section. Whether a satisfactory resolution of this last point has been made is open to doubt (*e.g.* [17, 18] and [19]). The scheme presented here might possibly be able to resolve all the above points, each of which is of definite metaphysical interest, but a different order of priority would be assigned to the basic categories. Existing theories involve the following ontological priorities for these categories: first time, second space, third matter, and fourth motion.

According to previous discussions the new ordering would read:— first, space; second, matter and third motion. It is a mere mistake accepting the present scheme to feel that motion presupposes time. Time would be removed from the basic categories in terms of which explanation is achieved. Nevertheless the concept does not lose its importance; quite the contrary, for time on this view represents the quintessential elements of the relations between phenomena: if nature were such that some concept corresponding to time could not be framed, then science itself would not be possible. From this vantage point the importance of time cannot be overestimated for the solidity of the concept becomes a measure of the orderliness of nature; that is of the rigidity and precision of the latter's structure.

Accepting the primacy of motion over time, as enunciated here, the definition of velocity in terms of space and time via $v = \dfrac{ds}{dt}$ is metaphysically incorrect. A consistent approach would be to define velocity as an extensive variable by the method of fundamental measurement. This would require some basic physical process to define unit velocity, for example the speed of waves along a stretched string prepared according to some given specification. Velocity would then be measured by the fundamental method using a standard series constructed in the normal manner by the composition of the elementary velocities. A priori there is no reason to expect this magnitude to correspond with velocity as defined in the derived terms; we know in point of fact that from the theory of relativity that the two magnitude diverge at velocities such that the

ratio $\dfrac{u}{c}$ cannot be neglected with respect to unity. Defined as suggested here the velocity of light would be infinite. The supremely important fact of nature corresponding to the finite velocity as defined by the alternative method would not, of course, be lost. It would reappear in invariance properties existing between appropriate variables. The suggested method of measurement would be more consistent in that all the basic categories would then be measured by the fundamental method, whereas on the contrary for the present situation velocity is the exception. The reason a derived method of measurement has been employed for the case of speed is no doubt purely utilitarian and not methodological, yet according to present contentions, this choice has epistemological consequences.

The relation between the two velocities for motion in a straight line appears to be $\dfrac{u}{c} = \tanh v$.

As already stated each of the other basic categories, namely, length and mass, and in the classical system, time, are measured by the fundamental method in any case so that the presently proposed procedure would be more consistent. This same question, in a different context, has already been discussed by Bridgman.

Definition of time from sets of Observed Regularities

The problem of defining the concept of time in terms of the relations between sets of observed regularities is basic and must be faced directly or indirectly by the investigator whether he adopts the present view or not.

In point of fact the only unambiguous definition possessing exactitude would appear to be based on the metaphysic of the classical theoretical structure; that is on the assumption of exact law involving departures due to error from a true value. The laws in question whilst not being self-contradictory, would have to be sufficient to determine all events precisely, in principle, at least. This is the classical scheme, though it is not clear what implications, if any, Godel's theorem may have in this connection. It has been cogently argued that the resulting schema of Lapacian determinism is in fact " an academic dream ". Although the present author fully agrees with the analyses and holds no brief for a thorough-going deterministic posture, it is nevertheless felt that all the usual arguments show, is that if Lapacian determinism operates and is valid this can never be known to be so, since investigation loses its essential characteristics; it is not admitted that they show in any way that such a metaphysic could not in point of fact

be operative. Such a deterministic scheme does not appear to represent the present state of affairs in physics. We drag without modification the inappropriate concept of absolute time, generated solely by the fully deterministic scheme, into the present situation. Whilst it is true that modification of the time concept is effected on grounds of relativity, the change is not of the nature presently considered and indeed demanded quite apart from present proposals by experimental facts; it is a mere complication of the concept that is not relevant in the present context. Whether the scheme suggested here is relevant or not will therefore be astounding, at least to the present author, if physics can continue on its present foundations without at some stage running into grave difficulties over the concept of time. This may be expected to occur in the field of microphysics where the approximation to an exact and fully comprehensive scheme of natural law is less applicable than in other fields.

From this point of view, namely, time as the quintessence of correlations between different phenomena, its definition amounts in effect to the discovery of the complete system of laws describing such correlations; in these laws a variable to be identified with all aspects of such correlations must occur. This variable is time, so that its definition amounts to the result of the framing of a complete scientific theory from the given observations. In the classical scheme of fully comprehensive law, this view permits the definition of time as the independent variable occurring in the equations of physics. It is interesting here to see how accepting the classical scheme, a measure of the magnitude associated with the concept could be effected. This measure is merely of a convenient process against which any other processes can be compared. The convenient process sums up all the information concerning correspondence between phenomena which is contained in the structure of the exact laws. It is a standard intermediary through which the correlations existing between different processes can be compared. On the classical basis the procedure might be to try to derive such a measure from the observation of a particular motion, for example, the motions of the solar system. A rough but good guide would be the motion of the earth around the sun. But this motion is not a simple application of the laws for an exactly soluble case, for perturbations are introduced according to the position and motions of other planets. Allowance via the laws would have to be made for these and subsequently for influences external to the system itself. The actual measure so produced would approximate, as far as calculations and observations permitted, to the true time variable, unearthed as it were by successive approximations based on the laws. Theoretically there is no limit to the extent of the minute perturbations which must be taken into account and therefore an exact measuring mechanism could not in practice be produced. Its theoretical existence is however guaranteed by the existence of a sufficiently comprehensive scheme of exact law. This can be seen for the particular attempt given above in which a time measure is produced in theory by allowing for progressively more refined perturbations. For if one imagines this process of correction to be continually applied without reference either to practicability of calculation or experimental error then it is seen that nothing less, eventually, than the Laplacian calculation of the motion of the entire Universe will do. Any theoretical attempt to unearth time by such a thought experiment-cum-calculation, must inevitably continue until the total description of the happenings of the entire Universe is achieved. This is merely another way of saying that the isolation of a system even in the classical scheme is real only in an approximate sense. Of course this sense is fully meaningful in practice because of experimental error. Firstly because of the inability to obtain sufficiently exact data for the super calculation and secondly that predictions from a spread of possible values consistent with the experimental data involves an overlap masking the minor perturbation effects at a relatively early stage of the super calculation.

The inevitable arrival at a Laplacian description of the Universe underlines the reason for the view that time, classically, in effect constitutes the frame work of scientific law, or alternatively put that time can be defined as the independent variable in the law of physics. It also underlines the assertion that it is only in the case of a sufficiently comprehensive exact schema of law that the scalar independent variable representing time becomes appropriate. On this view Universe itself gives in its " operation " a full account of the time variable and becomes a full kind of super clock; a clock which constitutes classically, the Newtonian absolute time.

By introducing error into the structure of the Universe and not merely into our experiments; that is by saying that the fundamental laws expertly framed by a super-being still permit divergent results from given data, the above-mentioned process of extrapolation to the whole Universe defining precisely the time variable could not be carried out definitively. The Newtonian absolute time, carried over into microphysics with suitable complications arising from relativity theory, is inappropriate there on account of the apparently essential indeterminacy observed.

One should it is contended, therefore tackle the problem to hand whether the remainder of the present thesis is accepted or not. In the following an attempt will be made to discuss the formulation of this problem.

Any satisfactory formulation of this problem is quite outside the capability of the present author; it may well be that it is outside the scope of available mathematical techniques. Nevertheless it is relevant not merely to the present thesis, but to modern physics as well. On this account the present subsection of the paper has a status different from that of the remaining portions. It is not known how to proceed, but it is felt to be certain that the essence of the relation in the general case will not be capable of representation in terms of a single scalar magnitude as in the limiting case of a Newtonian Universe. No doubt whatever the entity may be, because of the simplicity of the case of exact and fully comprehensive law, the entity in question loses degrees of freedom so that it is in effect reduced to a scalar variable, or something the variation of which can be represented by such a variable. However, the conceptual difficulties in deriving a procedure for the hypothetical general case are enormous; this is felt to be because the concept corresponding to time is in this case of great complexity, since it inheres as an essence in the formulation of the set of natural laws: its reduction in the classical case to an apparently simpler concept being fortuitous and consequent upon the rigidity of those laws; that is on their exactitude and completeness. Time is here seen therefore as a complex concept arising out of the more elementary categories and their relations. Its ontological priority is therefore low, but its significance, as an essence of the relations of correspondence between various phenomena, is extreme. Because of this deep significance it is tempting to make the concept basic and to define relations in terms of it. It is on the contrary contended here that this is a mistake engendered by the artificial and factually inapplicable case of the metaphysics of the classical theoretical structure, with which however we are familiar and from which it is therefore our habit to think. The transplant of time in its absolutist sense into the equations of Quantum theory being a case in point and one which, as difficulties are inevitably encountered, may well give rise to a resurrection of thought along the presently suggested lines.

Model of the Evolving Universe

Any model, will, in some way, be dependent on the concept of time as abstracted from the correlations in events according to some method referred to above and at present unknown. It is therefore not possible to offer any reasonable model until that problem has been solved. It might, however, not be amiss to give an indication of the type of thing which might be expected.

Firstly, it should be clearly stated that no fully satisfactory model could conceivably be made by the assumption of a time dependence of the fundamental constants. Such an assumption would not in any case be in accordance with the habit-principle, for although artificially one could engender states of increasing chaos and uncertainty by for example making Plancks Constant a monotonically decreasing function of time, it is clear that as 'h' increased and chaos was approached, the final form of the basic laws would still be inherent at all stages of their development. Consequently the basic structure would have had to inhere in the limiting chaotic situation, which is a much stronger supposition than that of the habit-principle alone. Furthermore the various stages would be defined by an entirely inappropriate method; namely an absolutist time for which it would be hard to find a place in the so posited physical Universe purely on the grounds of the observed relations and correlations between events; that is for similar reasons which render inappropriate, in the present author's opinion, the transference of the classical concept of time into the basic differential equation of wave mechanics.

As mentioned earlier it is felt that the generalized concept of time, applicable in any hypothetical Universe where the laws are not exact and complete, will not be capable of representation by so simple a mathematical device as a scalar magnitude. A quite different form of operator or other representation will no doubt be applicable, the limiting form of which alone may be representable in such simple terms. It is hardly therefore meaningful to attempt an even semi-serious proposal under the present heading. The mathematics must be chosen by some means to represent not a situation, but a process. Moreover the " operator " giving rise to this process must be modified by the actual process itself! This process must operate within the framework of the assumed categories: space, matter and motion. Although further speculative conditions can be laid down along these lines, there hardly seems point in this even from the point of view of metaphysics let alone science, for apart from a superficial but clearly inadequate resemblance to some iterative procedure the author at least knows of no similar or appropriate mathematical entities or procedures.

Conclusion

A line of approach has been suggested which with suitable development is alleged to be capable of resolving certain basic difficulties in the meta-

physical bases of science. These difficulties are:

(1) The difficulty of a past infinity sequence of events as put forward by Kant and later supported by Whitrow.

(2) The alternative difficulty of a past finite sequence of events.

(3) The ascription of a direction to time's arrow.

(4) The reduction of the existence of natural law to a simpler fundamental assumption; without such attempts the origin of the framework in which science operates must be left for discussion by the theologians.

(5) The reversibility of classical laws: point (3) above is one of philosophic importance in its own right. It is associated however with the question of the reversibility of classical laws and hence with the paradox of Loschmidt-Zermelo. This latter is assumed by most, but not all authorities, to have been resolved. In the present author's opinion this resolution is acceptable only on a certain metaphysical basis and not on other equally plausible metaphysical assumptions. Certainly part (3) itself, and by implication these associated classical difficulties, would be capable of direct resolution on presently advocated views.

(6) The difficulty of laws of limited duration for which it is required to explain what happens as indefinitely earlier states are considered. In particular the second law of thermodynamics may give rise to difficulties in this connection.

On the view presented time is a distillation of relations of correspondence between different phenomena. In the particular and hypothetical case of a Universe where, in principle, laws are exact and comprehensive (that is any event has a full and precise explanation) the concept, whatever its nature, becomes exact. Its distillation from the relations between all possible events renders it independent of them and hence absolute in this sense. For this restricted case it can apparently be represented by a scalar magnitude; the latter probably being a particular and collapsed form of its intrinsic nature in the more general case. On such a view it is a mistake to take the scalar magnitude called time in to the quantum domain on account of the nature of the laws obtaining there; these laws are not complete and the concept is thus inappropriate. Basic difficulties in this regard must eventually arise if the present view has any foundation.

Once a model is developed by a proper and mathematically suitable analysis of the basic problem the theory could be related to physics in a number of ways: these include particularly astronomical methods, which give some access to observations covering significant time spans. A further possibility is the derivation of quantum theory or rather the fitting of that theory into the model as a " boundary " condition. This development may well require new mathematical techniques because of the unusual nature of the problem and might then permit cosmological use to be made of the theory.

The approach suggests that the ontological priorities of the basic categories should be space, matter and motion. Time arises out of the relations between phenomena occurring in these basic categories. It is a mistake according to the above developed arguments to think either that an origin to time is self-contradictory or that motion presupposes time. Time is, in a sense, more significant than the basic categories themselves, for it is because the concept is valid that the Universe for which this is true can be described according to scientific laws. Time is thus of the essence of the relations between phenomena and its existence implies the possibility of science. Causality is then, as it were, a particular aspect of time.

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FURTHER APPLICATIONS OF THE LOGISTIC FUNCTION

K. W. Harrison, R.N.S.S.

Admiralty Surface Weapon Establishment

Previous papers [1-4] have described applications of the logistic function to problems arising in the assessment of radar performance, and illustrated the versatility and power of the function with reference to biological, physico-chemical, and statistical examples. An analytical technique has been described which utilizes derived parametric relationships between individual members of families of logistic curves. Additionally, it has been shown that the use of the logistic function can give an improved fit to data, while, at the same time, a great simplification in formula is achieved and an acceptable alternative model is provided. In this paper we continue in the same vein with four more examples of the power of the function. We shall consider the function in the form

$$Y = \frac{1}{1 + 10^{F(x)}} \ ,$$

where $F(x)$ is a polynomial in x which is usually linear or quadratic but may sometimes be cubic.

The first example is drawn from the field of industrial chemistry and involves the use of a computer programme written to fit logistic curves to a set of data by transforming it into "logits"[5] or "plausibilities"[1,6]. This transformation gives

$$F(x) = \log(q/p),$$

where q and p are either proportions or probabilities and $p + q = 1$. To the transformed values linear,

quadratic and cubic equations are fitted by the use of Fisher's orthogonal polynomials[7, 14]—thus three logistic curves of increasing complexity are obtained. The least-squares solution is not exact because logarithmic units are used; the exact least-squares solution to data which may be represented by logistic curves is extremely tedious and time-consuming, as stated by J. Berkson[5] and E. S. Keeping[8], but we have shown elsewhere, and will show here, that the method given above yields satisfactory results.

The second example comes from physical chemistry and illustrates another useful method of fitting a curve—this time a modified exponential curve (this becomes a logistic curve if the reciprocals of the Y values are taken). In particular, this example provides another illustration of the method of analysis by parametric relations referred to above.

This method of analysis is also used in the third example. The example is taken from bio-chemistry; it features the oxygenation of haemoglobin and illustrates the relevance of the logistic to *quantum biology*.

Lastly, an example is given which features Brownian motion and the energy distributions which can be derived from classical, Brownian movement principles when a certain assumption is made. Although the random processes which occur in body fluids and tissues clearly have im-

portant biological relevancies (and again the logistic relates to quantum biology) in the paper from which this example was taken Brownian movement is used as a step towards a quantum communication theory.

Example 1.

The method of curve-fitting by orthogonal polynomials used in the programme is given in "Statistical Methods in Research and Production" by O. E. Davies[9]. The example examines the data on Schopper-Riegler freeness tests on paper pulp samples taken at hourly intervals during a beating process. Davies states that it is clear from his figure (reproduced in Fig. 1) that a cubic regression is required to represent the points. However, the sigmoidal shape of the distribution suggests also that a logistic might yield a good fit. The fitted cubic and a skew logistic giving a *closer* fit to data can be seen in Fig. 1. We observe that by fitting a skew logistic (*i.e.* where F(x) is not linear) several improvements can be made. These are:—
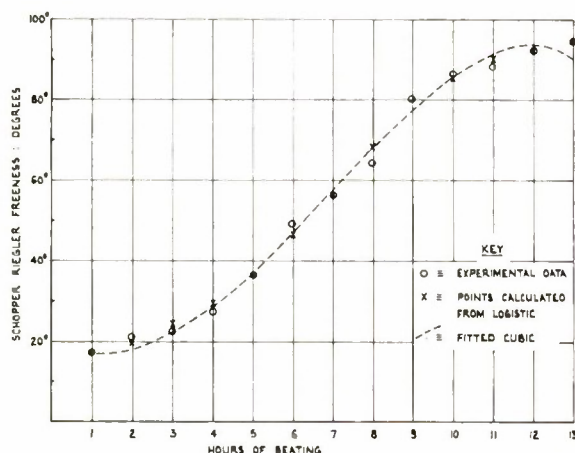
FIG. 1.

(a) The residual sum of squares is reduced from 49 to 43 units;

(b) A more appropriate model is employed— the logistic with its asymptotic levelling-off is more " natural " and to be expected than the arbitrary and constrained cubic (observe how the " tail " of the cubic droops down and away from the data. Also, would the mixture be expected to become less free as this seems to indicate ?);

(c) Potentially more information is to be extracted, *e.g.* the asymptote giving the closest fit is less than 100° on the scale. If, hitherto, 100° had been taken as an obtainable upper limit, some useful knowledge, possibly of economic value, may have been gained.

Example 2.

In a recent edition of *Nature* [10], I. S. McLintock relates how the " Elovich " equation has been extensively applied to chemisorption data and explains how, in his opinion, existing techniques for its application to data are in error. McLintock found in certain cases the Elovich equation applied over the whole range of data could give values which are far from accurate. Therefore, he divided the data into sections and found that by so doing he could represent data more accurately—his example of this is reproduced in Fig. 2. Two weaknesses appear in his method. Firstly, there is no homogeneity among the equations representing different sections, *i.e.* the first section is described by a parabolic equation but the next two are described by Elovich equations. Secondly, he is compelled to use a formula to which, in his own words, " no mechanistic significance is yet attached ".
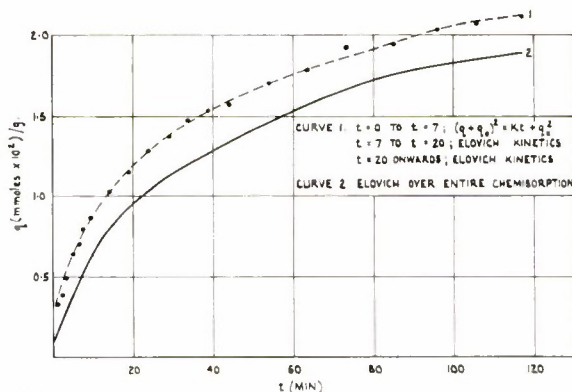
FIG. 2.

The two curves shown in McLintock's figure are very similar to modified exponential curves. The equation for a modified exponential curve is

$$Y = K + a\,e^{bx}$$

(It can be seen that this is a logistic function in terms of the reciprocal Y values). The data, however, contain some abrupt changes of slope (of considerable interest to the physical chemist) and these make it necessary to divide the data into sections. Thus, if we wish to think in terms of modified exponentials we are obliged to consider a succession of such curves, and to look for relationships between the parameters K, a and b, of successive curves; we have shown this to be sometimes possible[2].

To enable us to do this rapidly and accurately there is a graphical method of fitting a modified exponential, devised by Cowden and quoted by Keeping [8], page 349. The method is simply to use the formula

$$K = \frac{Y_0 \, Y_2 - Y_1{}^2}{Y_0 + Y_2 - 2Y_1}$$

where Y_0, Y_1 and Y_2 are three convenient equidistant ordinates, to obtain a tentative value for K and then, by plotting $y_i - K$ on semi-log graph paper, to readjust K slightly by trial and error until the points $y_i - K$ lie on a straight line. From the resulting straight line a and b may be estimated, a being the ordinate at $X = 0$, and e^{bx_1} the ratio of the ordinates at $X = x_1$ and $X = 0$.

Applying these methods to McLintock's data (y_i's being estimated from his figure) we obtain $K = 2 \cdot 3$; and then by plotting the values of $y_i - 2 \cdot 3$, with no need for adjustment, we obtain data which may be represented very accurately by three straight lines as shown in Fig. 3.
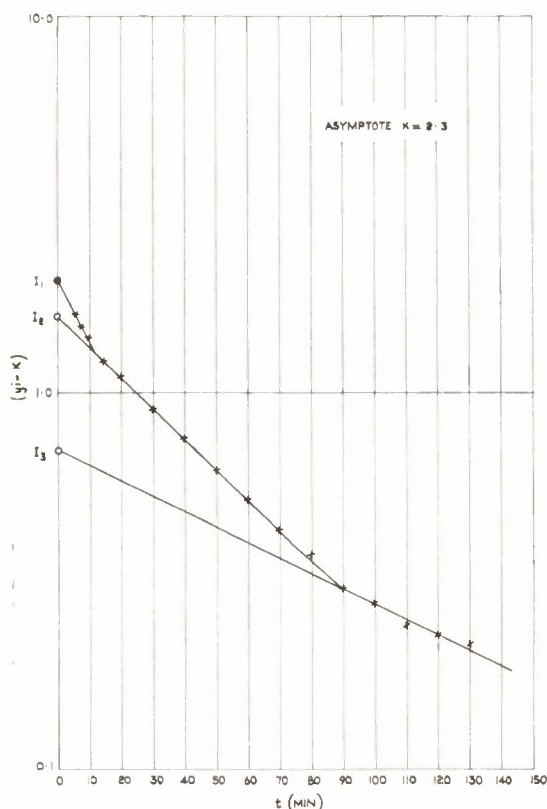
FIG. 3.

Thus, for one overall asymptote ($K = 2 \cdot 3$) we obtain successive values for—a of $2 \cdot 05$, $1 \cdot 6$ and $\cdot 70$, and values for e^{bx_1} of $\cdot 668$, $\cdot 825$, and $\cdot 910$. We note that in the first series differences between successive values are doubled and in the second series differences are halved. There are, therefore mathematical relationships between successive values of the parameters.

To summarize the advantages gained by using modified exponentials in this way:—

(a) We can see from Fig. 3 that the data will be represented no less accurately—possibly more so (we cannot say this for certain because we have estimated the data);

(b) The form of the equation representing successive sections is consistent throughout—only the values of the parameters change—thus we can represent the complete set of data by one equation, specifying the appropriate values for the parameters according to the value of the abscissa;

(c) Values of parameters over successive sections can be related to each other mathematically, hence, either by replication of this experiment under different conditions (*e.g.* different temperature) or by aligning these relations to some other concurrent physical change, further quantitative information may be obtained;

(d) We are left with no empirical observations to account for;

(e) By this method the "transition" points seem to emerge particularly clearly — although they do not seem to agree with McLintock's. This focuses attention on to the appropriateness of the models, etc. In the absence of the original data, and because it is not our prime concern we must leave this (so far is this paper is concerned);

(f) The procedure we have adopted seems more mathematically satisfying (even rigorous) than the methods of McLintock and others.

Example 3

The third example is taken from the field of biochemistry. Dr. G. Bemski describes in *Nature* [11] how comparative experiments on certain human methaemoglobins yield information concerning the presence or lack of interactions between α and β polypeptide chains in Haemoglobin-M Hyde Park. Figure 4a summarizes the results of his electron spin resonance experiments. From the fact that the behaviour of met-met Hb-M H.P. is intermediate between that of met Hb-A and met-oxy Hb-M H.P., Dr. Bemski concludes that normal α chains when in met state undergo a high to low spin transition similar to Hb-A. The β chains continue in the high spin state, as indicated by the remaining amplitude of the electron spin resonance signal at pH $= 9$ and pH $= 10$.

The first impression given by his figure is that this concerns a family of logistic curves. The second impression is that, judging by the symmetry and spacing, the parameters of the logistics might be expected to be simply related to one another.
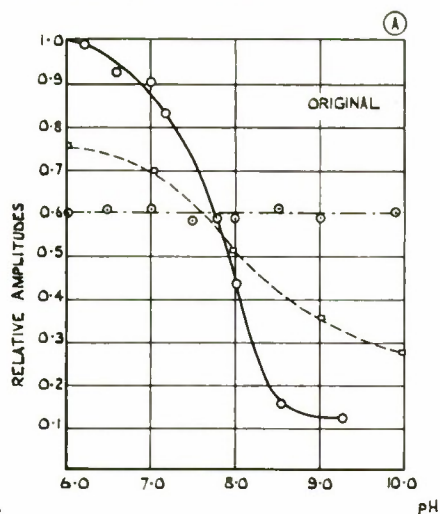
FIG. 4a.

CURVE 1. o——o = MET   HB – A
CURVE 2. o– – –o = MET –MET HB –M HP
CURVE 3. o–·–o = MET – OXY HB –M HP

Thirdly, if these two impressions can be corroborated, further information on a more quantitative basis may be forthcoming.

Before proceeding to details of the curve-fitting we should remark on the data, because the nature of the data governs the accuracy and precision of the fitted curve. In this respect there are several points to be noted:—

(a) The uneven spacing of the measurements makes curve-fitting more difficult, as does

(b) The different number of readings in each curve,

(c) No mathematical expression for the curves seems to be furnished and the implication is that the curves are fitted by eye. Therefore, a mathematical representation of the data might not provide such a good fit,
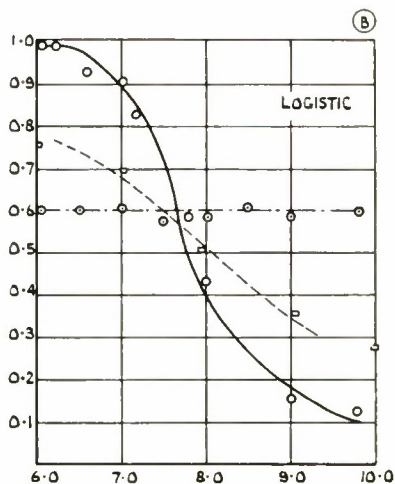
(d) If the curves have been fitted by eye the apparently artificial constraint of curve 1 to the two points at pH=8·5 and pH=9·3 conflicts with the fit of the curve *between* the points of pH=6·5 and pH=7·0: these pairs of points are roughly equidistant from the centre of symmetry. This is supported by the fact that at pH=6·0 the ratio of the distance between curves 1 and 2 to the distance between curves 2 and 3 would then be closer to the reciprocal of this ratio at pH=9·3.

Thus there seems little point in searching diligently for the most exact fit as we have introduced further inaccuracies into the data we are treating. Accordingly, the logistic curves obtained from a first inspection of the data were examined. The family of curves obtained in this way is shown in Fig. 4b. It is considered that this is sufficient for the purposes of our demonstration — particularly as the major coefficients of the quadratic expression denoting a skew logistic can be represented as shown in Fig. 5a and 5b. Relations between the coefficients of the quadratic term are not shown because they become too small to have satisfactory precision—for this reason, however, the effect of the quadratic term upon the computed value of the amplitude becomes small and can be ignored. The values of the coefficients, C_1, C_2 and C_3, are respectively -0.0300, $+0.0020$ and -0.0002. These might be said to form an oscillatory series of regularly decreasing magnitude— if such a conclusion could be justified on three, small, values.

FIG. 4b.
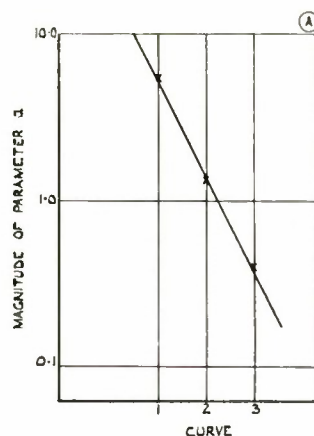
FIG. 5a.

It appears, therefore, that our feelings concerning Dr. Bemski's figure were justified. The data can be represented by a family of logistic
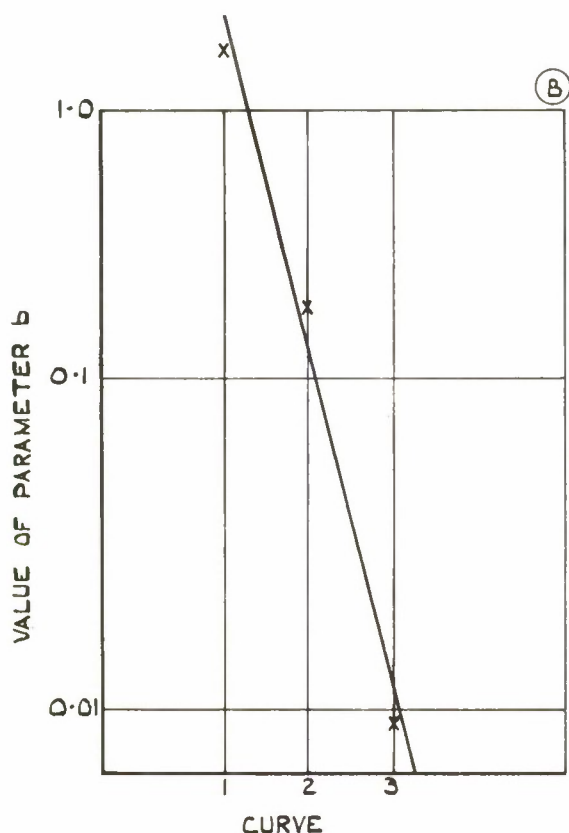
FIG. 5b.

curves and the parameters of successive logistics can be related to each other mathematically. As to the usefulness of these observations, even if no physical or measurable property can be found to associate with curves 1, 2 and 3, it should still be possible to achieve quantitative measurement by replicating these experiments under different conditions (*e.g.* for different concentrations) and observing the resultant changes in the parametric relations.

Example 4

In this example we relate the logistic to quantum mechanics and also to a random process occurring in body fluids and tissues, *i.e.* Brownian motion. The animal body—blood plasma and the protoplasm comprising the tissue cells—is composed largely of mixtures of colloids. Brownian movement is to be seen when a colloid solution is observed with the ultra microscope and W. S. Ament[12] has derived from Brownian movement principles the Fermi-Dirac, Bose-Einstein, and the Maxwell-Boltzman distribution functions. The first two are in the form of a logistic function—the third is a boundary case of transition between the two[3].

(a) Ament comments that the layman's impression is that Brownian-motion principles are a direct development from elementary classical physics, whereas the entropy definition often appears to be given by mystical fiat.

By virtue of what we call the "logit" transformation (after Berkson), which we discovered independently[1] and which was also used by Fisher to transform r (the correlation coefficient) to z (see later), any set of data which can be represented by the logistic may be transformed into subjective measure, *i.e.* plausibility and weight of evidence, and vice versa. This subjective probability measure can be interpreted in terms of entropy and information theory[6]. Hence the logistic provides the link between Ament's derivation of the distribution functions from classical principles and their definition from entropy considerations (and subjective probability subsumes both approaches !).

(b) A rule of thumb which W. S. Ament subsequently formulates is believed to apply to the physical signals, or waves, which form the substantial aspect of communication. Therefore, he suggests, one might seek an appropriate experiment, rather than a theoretical agreement, for testing his prescription.

The famous Brown-Twiss experiment[13] is apparently relevant here but the experiment entails not only the boson statistics of light waves but also the Fermi-Dirac statistics of the photo-electronic currents by which the waves are recorded. Ament asks " Is it conceptually possible to measure the Bose-Einstein statistics of fluctuations of a photon stream without having to allow for Fermi-Dirac statistics in the measuring process ? " (An explanation is given by E. M. Purcell in *Nature*, **178**, 1949). We may briefly note two points here before passing on to consider the major observations we wish to make. Firstly, our experience with the logistic in related fields suggests that the logistic could be of some utility in disengaging the two effects. Secondly, Ament's prescription is very similar to the reduction in Refs.[1, 2] and [3] of a complex theoretical representation of a probability distribution to a simple physical model representing the distribution of electrons in the surface layer of a conductor.

Brown and Twiss give a theoretical analysis of the correlation to be expected between the fluctuations in the outputs of two photoelectric detectors when these detectors are illuminated with partially coherent light. It is shown how this correlation depends upon the parameters of the equipment

and upon the geometry of the experiment. They describe a laboratory test in which two photo-multipliers were illuminated with partially coherent light and the correlation between the fluctuations in their outputs was measured as a function of the degree of coherence. They compare the results of their experiment with the theory and claim that these agree within the limits of accuracy of the experiment. In a figure the experimental points are plotted, together with the theoretical curve and the probable error of each point experimentally determined. It is towards this figure that we turn our attention—its salient features are reproduced in Fig. 6.
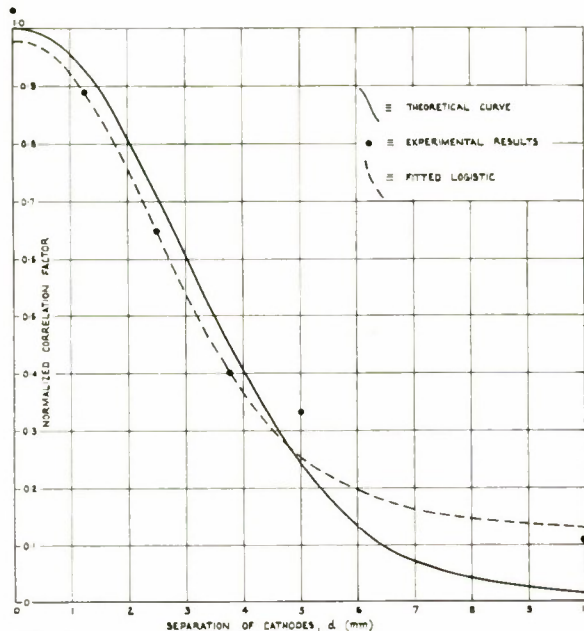
FIG. 6

Brown and Twiss discuss the possibility that no real effect is being measured in their experiment and conclude that the probability of this is negligibly small. The probability that the effect is due to some quite different cause, such as fluctuations in the source intensity, is, they claim, rendered extremely unlikely by the closeness of the agreement between theory and experiment. There remains the possibility that the effect is real, but that its magnitude is not accurately predicted by their theory. This suggestion has been advanced by another researcher but is refuted by Brown and Twiss. They admit, however, that a small systematic error (perhaps due to some quantum effect which has been ignored) could not be detected by present measurements.

We note with interest:—
(i) Brown and Twiss's use of the statistical concept " the probable error ".

(ii) their reference to the closeness between theory and experiment,

(iii) the possible existence of a small systematic error—possibly because of some ignored quantum effect.

Our comments on these points are as follows:—

(i) Probable error as a measure of dispersion is little used now and its use is not to be recommended[9]. It tends to convey a picture of less dispersion than there actually is.

(ii) (a) Fisher used the logistic function to transform a measure of correlation into " plausibility ", i.e. $\frac{1}{2} \log \frac{p}{q} = z$ where $r = 2p - 1$ [14].

(b) If we use the " logit transformation " on Brown and Twiss's data the " logits " obtained lie on a smooth curve which may be a quadratic or possibly a cubic—suggesting that a skew logistic will fit the data.

(c) If we fit a logistic curve to their data (using the programme) we obtain a very much *closer* fit than with the theoretical curve (Fig. 6).

(iii) (a) It seems possible that the difference in fit is a consequence of the omission in the theoretical formulae of some small quantum effect.

(b) This possibility is perhaps reinforced when we consider the formula used to obtain the correlation factor which is

$$\Gamma^2(v_0,d) = \frac{1}{A_1 A_2 \Delta(v_0)}$$

$$\int_{-b/2}^{b/2} dy_1 \int_{-b/2}^{b/2} dy_2 \int_{-\frac{1}{2}(d+a)}^{-\frac{1}{2}(d-a)} dx_1 \int_{\frac{1}{2}(d-a)}^{\frac{1}{2}(d+a)} dx_2$$

$$\times \frac{4J_1^2\{(\pi\theta_0 v_0|c)((x_1-x_2)^2 + (y_1-y_2)^2)^{\frac{1}{2}}\}}{\{(\pi\theta_0 v_0|c)^2((x_1-x_2)^2 + (y_1-y_2)^2)\}}$$

We remark that this great simplification in formula and improvement in fit obtained by the use of the logistic has been noted before in the results of a scattering experiment in nuclear physics and in applications in electronics[2, 3].

(c) Brown and Twiss admit the possibility of a systematic error in their calculation of the correlation, due to some quantum effect which has been ignored. Conceivably, the better fit of the logistic curve to the observed data indicates the inclusion of some quantum effect of this kind. Ament seeks an understanding of how Plank's constant must be "invented" and,

to this end, he introduces into his calculations a term which stands for hitherto ignored terms of quantum-mechanical origin. This term represents deviations of the statistics from the classical Gaussian ideal.

Considering previous comparisons between logistic curve and normal curve, previous applications of the logistic function to particle physics and communication theory[2, 3] and, particularly, the applications in (a) and (b) above, it seems possible that not only understanding but simplification and improvement, as sought by Ament, lie in the further development of the use of the function in these fields.

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OBITUARY

E. J. Burton, M.I.Mech.E.

By the sudden and premature death of Ernest Burton during a game of hockey, ACO has sustained a tragic loss, felt far beyond the R & D community of which he was a valued and respected member.

Ernest James Burton was born in 1913 and received his early education at Bishopsholt Grammar School, Hillingdon, and his practical training in mechanical engineering with the HMV Gramophone Company of Hayes. He held appointments with the Amalgamated Dental Company and Sciaky Welding Machines Limited before entering ACO in 1938 as a Drawing Office Assistant. He became a TEA III in 1939, moving up through the grades to become established as a Senior Assistant (Scientific) in 1947 and was promoted to Experimental Officer in 1951. He was elected Associate Member of the Inst.Mech.E. in 1943.

Up to the mid 1950s when ACO's main preoccupations were gyro and magnetic compasses, he was associated with many projects in both these fields, but particularly with projectors for tank and submarine magnetic compasses. With the expansion of ACO's R & D, marks of his mechanical abilities were left in a wide variety of enterprises including SINS, gas bearings, metrology, polaris navigation and laser gyros. The navigation monitoring computer, now in service at sea, is one of the items in whose design his was the major hand.

Even if "mechanical genius" is too strong an epithet for him, there was scarcely a branch of this class of engineering of which he had no knowledge; and his help and advice in this field was frequently sought and willingly given.

Modesty to the point of self-effacement denied to Ernie the greater recognition which those who knew him better thought he well deserved, but he won the liking, respect and friendship of all who were associated with him, on duty or off.

To his wife Sheila, herself a former member of the Observatory, and to their three daughters, goes the sincere sympathy of all who knew him as a good colleague and friend.

DISTRIBUTION OF MEN WITH RESPECT TO THEIR SUSCEPTIBILITY TO DECOMPRESSION SICKNESS

B. A. Hills, M.A., Dip.Chem.Eng., B.Sc., Ph.D., A.R.I.C., R.N.S.S.

Royal Naval Physiological Laboratory

SUMMARY

The Weibull function is shown to provide a particularly good fit for the distribution of minimum bends depths between individuals breathing air. A reasonable fit is also shown for goats, similarly subjected to hyperbaric exposures, and for resting pilots decompressed aerially. The correlation of these different cases is shown to hold only if the parameter for estimating the imminence of decompression sickness is taken as the volume of gas predicted to have separated from solution in unit volume of tissue. The very practical advantage of this simple statistical function is discussed in relation to its application in modifying any method of calculating diving tables to give a required minimal bends rate.

Introduction

In compiling diving tables, it is highly desirable to have a mathematical expression describing the distribution of men in relation to their susceptibility to decompression sickness. This applies whatever calculation method is employed to predict the behaviour of the average diver, or of any individual of known sensitivity, from the measurable parameters of the dive. Moreover it should also hold whether these methods have been devised empirically or have been derived by fundamental physical and physiological reasoning.

Thus, if one knows the equation(s) for predicting the behaviour of a particular individual of known sensitivity, then it should be relatively simple to adapt a schedule based upon this subject for say a 1% bends rate—once the susceptibility distribution function is known. However, in determining the relevant function, it is essential to select the most realistic parameter for describing individual tolerance and to employ the most pertinent data for testing it.

Relevant Parameters

The parameters determining the imminence of decompression sickness may be classified into two groups:

1. Those defining the environmental changes to which a subject has been exposed, and
2. Those describing the anatomy and physiology of the individual and which are intrinsic to his constitution.

Although the latter determine individual susceptibility, it is not necessary to estimate their separate contributions or even to identify them. For practical purposes it is only necessary to estimate their cumulative effect in determining a frequency distribution of tolerance to decompression, although it is an intriguing academic problem to postulate how these inherent differences between subjects can arise.

The cumulative variation between individuals can be estimated practically from the relative changes in the environmental parameters necessary to produce the same clinical condition. In defining such a state it is little use employing any estimate of the severity of symptoms since these can vary enormously depending upon individual tolerance

to withstand pain, and are open to subjective errors associated with personal descriptions of its intensity. However the body senses are a much better ' yes-no ' indicator between comfort and discomfort. Fortunately this threshold of marginal symptoms is the most relevant state in practice, since the object of decompression schedules is to prevent the diver experiencing any distress at all.

Standard Decompression

Having defined a standard condition for comparing individuals, the parameters which can be invoked to describe the environmental changes necessary to induce this state include:

1. The absolute pressure (P) of the subject which may vary between a maximum absolute pressure (P_1) followed by decompression to a lower absolute pressure P_2.

2. The time of exposure (t) at P_1 and decompression time allotted to pressures between P_1 and P_2.

3. The composition of the breathing mixture.

It is simplest to select air as the breathing mixture in comparing individuals. Moreover to avoid the uncertainties in allowing for time[1] it is simplest to guarantee that each individual reaches an effectively steady-state condition at P_1 and is then decompressed to P_2 as rapidly as possible. Six hours has proven adequate[2] for a subject to become saturated with respect to alveolar inert gas.

Thus the environmental change selected as standard for comparing individuals is a rapid decompression to a pressure P_2 from an absolute pressure P_1 at which the subject has been breathing air for at least six hours. The standard decompression described above is particularly easy to apply since, if P_2 is normal atmospheric pressure, then $(P_1 - P_2)$ is the minimum bends depth of the individual. Moreover, if P_1 is normal atmospheric pressure, then P_2 is the absolute pressure corresponding to the minimum bends altitude of the subject for aerial decompression.

Hence it is particularly convenient to obtain data upon the distribution of susceptibilities of individuals by measuring just one parameter— P_1 for hyperbaric exposures and P_2 for hypobaric decompression.

Relevant Parameter

While P_1 has been used to classify divers, and P_2 to classify pilots, it is felt that a more universal parameter is required which can correlate both. Those which have been used in the calculation of decompression tables for divers and caisson workers include:

A. The decompression ratio (P_1/P_2) as introduced by Boycott *et al.*[3]

B. The decompression $(P_1 - P_2)$ as advocated by Leonard Hill[4].

However both of these parameters provide a very poor correlation between pilots and divers, comparing both the least and most susceptible in each group[1]. This comparison has shown that a better parameter is provided by:

C. The volume of gas (v) separated from solution in a volume of tissue (V) in phase equilibrium[5], where (v/V) is given by:

$$\frac{v}{V} = \left[\frac{(u\,P_0 - 46) + u\,(P_1 - P_0)\psi(t) - P_2 - 74}{(P_2 + 74)} \right] SP_0 \quad \ldots (1)$$

in which all pressures are expressed in mm.Hg, S is the solubility of the inert gas in tissue (expressed as a Henry's constant), P_0 is normal atmospheric pressure and $\psi(t)$ is the function for inert gas uptake by the tissue. Thus for air (u = 0·8) and t > 6 hours, when $\psi(t) = 1$, the constants in the above expression can be grouped to give a ' susceptibility parameter' (F) defined by:

$$F = \frac{v}{Vu\,SP_0} = \frac{(P_1 - 46)}{(P_2 + 74)} - 1·25 \quad \ldots (2)$$

Hence there are at least three parameters which can be used as an index of susceptibility for both hyperbaric exposures and aerial decompressions, *viz.*, (P_1/P_2), $(P_1 - P_2)$ and F as defined in Equation (2).

Selection of Function

With each of the above approaches to estimating the imminence of decompression sickness, symptoms are postulated to occur if the vital parameter exceeds a critical threshold or extreme value. Hence, on the basis of ' renewal theory '[6] the ' extreme value ' distributions seem a likely group of functions to describe the susceptibility of individuals. Of this group of expressions one of the simplest and most popular is the Weibull function. If X is the cumulative fraction of ' failures ', then the ' survivor ' function $(1 - X)$ can be expressed as:

$$(1 - X) = \exp[-(\rho p)\alpha] \quad \ldots (3)$$

where α and ρ are constants and p is the vital parameter causing 'failure'. This function can be tested upon decompression data if X is the fraction of subjects with symptoms.

Suitable Data

The most comprehensive sets of data which could be found, and which conform to the standard decompression defined previously and to the required clinical condition, are the following:

(i) The data of Crocker *et al.*[7] who decompressed a group of 15 men to atmospheric pressure $(P_2 = P_0)$ following six hours of breathing air at constant values of P_1. The

gauge pressure (P_1-P_0), equivalent to the minimum bends depth, was increased by intervals of 2 ft of sea water from 27 ft to 41 ft, eliminating those individuals who displayed symptoms from further exposures.

(ii) The data of Davidson *et al.*[2] who performed the same series of trials with a group of goats breathing air while exposed to pressure for 12 hours. In both sets of data the subjects were resting after decompression.

(iii) The data of Gray *et al.*[8] who recorded the percentage of cases occurring in large numbers of resting pilots exposed to absolute pressures of 175, 158 and 136 mm.Hg to simulate altitudes of 33,000, 35,000 and 38,000 ft respectively.

The data for resting subjects from all three sources has been converted to pressure units of feet of sea water, and is quoted in Table 1.

Such plots are shown for $p = P_1-P_2$ and $p=P_1/P_2$ in Fig. 1 and for $p=F$ in Fig. 2.

Only the latter gave linear plots for all three sets of data, values of $\propto$ from these being 4·73 for divers, 4·73 for goats and 5·59 for pilots. The fit for divers was quite remarkable. If equation 2 is converted to pressure units of feet of sea water, and substituted for $p = F$, Equation 3 gives the distribution of their susceptibility as:

$$(1-X) = \exp\{-[(D-14\cdot3)/25\cdot1]^{4\cdot73}\} \quad \ldots (4)$$

where D is the minimum bends depth (P_1-P_0).

Discussion

The great advantage of equation (4) is that it provides a fit of the frequency distribution of the minimum bends depth of divers irrespective of the mechanism postulated for the occurrence of marginal symptoms. However it is unlikely that the constant of 14·3 ft would have arisen

TABLE I. Data for the ' minimum bends depth ' of divers and goats and the ' minimum ' bends altitude ' of pilots. Values are calculated for (P_1/P_2), (P_1-P_2) and F as the vital parameter.

Data Subjects	No.	P_1 (ft.)	P_2 (ft.)	P_1-P_2 (ft.)	P_1/P_2	F equation 2	Symptoms (%)	$\log_e[-\log_e(1-X)]$
PILOTS	100	33·0	8·60	25·40	4·34	1·641	5·0	−2·970
Gray *et al.* [8]	429	33·0	6·86	26·14	4·81	1·844	9·8	−2·271
	223	33·0	5·91	27·09	5·59	2·150	20·2	−1·489
DIVERS	15	64·0	33·0	31·0	1·94	0·461	13·3	−1·947
	15	68·0	33·0	35·0	2·06	0·572	33·3	−0·904
Crocker *et al.* (7)	15	70·0	33·0	37·0	2·12	0·627	46·7	−0·463
	15	72·0	33·0	39·0	2·18	0·682	60·0	−0·087
	15	74·0	33·0	41·0	2·42	0·737	73·3	0·278
GOATS	10	73·4	33·0	40·4	2·23	0·721	20	−1·501
	10	77·9	33·0	44·9	2·36	0·845	30	−1·030
Davidson *et al.* (2)	10	79·0	33·0	46·0	2·40	0·876	40	−0·672
	10	82·4	33·0	49·4	2·50	0·969	60	−0·088
	10	85·8	33·0	52·8	2·60	1·062	70	0·186
	10	89·1	33·0	56·1	2·70	1·155	90	0·834
	10	94·7	33·0	61·7	2·87	1·310	100	−

Results

The three parameters considered most likely to be the one vital in determining the imminence of symptoms have been calculated in Table 1 for all three of the different sets of practical data.

The Weibull function given in Equation 3 can be re-written in the form:

$$\log p = (1/\alpha) \log [-\log_e (1-X)] - \log \rho$$

such that the plot of log p versus $\log[-\log_e(1-X)]$ should be linear if the function holds and the correct parameter has been selected.

fortuitously, and the appearance of the term $(D-14\cdot3)$ must add some support for selecting F as the most relevant parameter, since Equation (2) predicts $F \propto (D-14\cdot3)$. This, in turn, would favour the thermodynamic hypothesis in so far as the vital quantity in determining the imminence of decompression sickness is the volume of gas separated from solution per unit volume of fully-nucleated tissue[5]. This concept has also offered a better correlation of much published diving data than afforded by critical supersaturation theories mathematically interpreted in terms of a

FIG. 1.

FIG. 2.
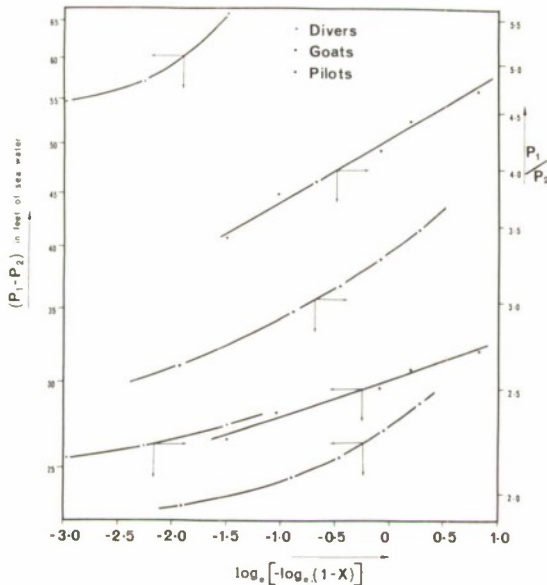
fixed tension differential $(P_1 - P_2)$ or a decompression ratio (P_1/P_2), besides reducing total decompression time[9].

An advantage of the Weibull function is that one can get a clear indication whether it fits a set of data, since it contains only two constants compared with four which can be adjusted in such as the logistic function[10]. The remarkable fit of the practical data (Fig. 2) is interesting since a Weibull function is characteristic of a system with a large number of similar components, similarly ageing, of which failure of any one causes the whole system to fail. This generalization from 'reliability physics' could be analogous to the 'in vivo' case of a series of cells, capillaries or fibres in any one of which the separation of excessive gas from solution could cause symptoms.

Since $\alpha > 3.6$ in each case tested, the distribution exhibits negative skewness[11]. The great practical advantage of the Weibull distribution lies in its mathematical simplicity. Thus, if it is known that a subject whose threshold value of the vital parameter for symptoms is p_1, and he is 'stronger' than a fraction X_1 of a random selection of divers. then it is possible to calculate the limiting value of the parameter (p) for a general bends rate of $100 X \%$ as:

$$p/p_1 = 4.7 \sqrt{\frac{\log (1-X_1)}{\log (1-X)}}$$

This is a very convenient expression for modifying any calculation method to a required bends rate.
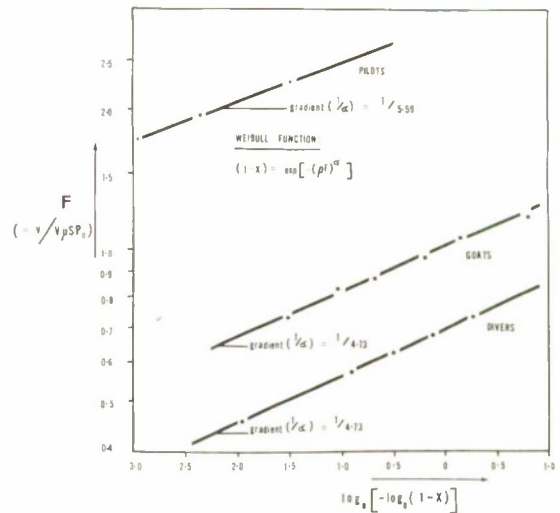
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THE SECOND INTERNATIONAL SYMPOSIUM ON GAS LUBRICATION

Las Vegas, Nevada, June 1968

Reported by A. G. Patterson, M.A., M.I.E.E., R.N.S.S.
Admiralty Compass Observatory

It is perhaps ironic that Las Vegas, home of the wheels of chance, should be selected as the convention venue for scientific deliberations on bearings. The apparent incongruity made it difficult for some delegates to convince their wives that they were going to Las Vegas for a serious purpose. However, at the recent Second International Symposium on Gas Lubrication there, held at the Stardust Hotel, some 46 papers were presented, covering most classes and permutations of gas bearings and representing the " state-of-the-art " as it had developed since the first symposium in 1959.

The symposium was the outcome of co-operative effort between the U.S. Office of Naval Research, which covers most gas lubrication projects sponsored by the U.S. Government, and the American Society of Mechanical Engineers, handling the civil side.

The international distribution of papers was 32 American, six British, three Soviet, two Rumanian, one Danish and one Japanese. Mr. H. J. Elwertowski, Chief Scientist at A.C.O. and Mr. A. G. Patterson were each invited to serve as co-chairmen in the course of the sessions.

In the keynote address, Dr. B. Sternlicht (Mechanical Technology Inc., Latham, N.Y.) emphasized the multi-disciplinary approach now made to gas lubrication and pointed out that over 20 disciplines were involved in the present symposium.

Self-Acting Bearings (Aerodynamic)

The state-of-the-art in this class was surveyed by Professor D. D. Fuller (Columbia University), who included cylindrical, plain and grooved journal bearings, pivoted pad types, compliant surfaces and various thrust bearings in his review. Subsequent papers in this session dealt with theory, design and mathematical analyses of the self-acting class.

In the concluding paper of this session, Dr. H. Marsh (Cambridge University) whose work on gas bearing stability is internationally recognized, discussed the effects on stability of a non-circular bearing member in the presence of flexibility.

Instrument Applications

Moving into the field of instruments, Mr. W. G. Denhard (MIT), in a joint paper with Dr. C. Pan (MTI), discussed applications of self-acting bearings to gyro spin-axes, of pressurized bearings to gyro support axes and, in addition, included the relatively new squeeze film bearings.

The development of gas bearings for gyros in the United Kingdom was reviewed by Mr. A. G. Patterson (ACO, Slough).

Design of Externally Pressurized Bearings (Aerostatic)

This class of bearing, perhaps the most widely known, was reviewed by Dr. W. A. Gross (Ampex, Redwood City), who listed the many permutations

in the group. Various subsequent authors including L. G. Stepanyants, N. D. Zablyotsky and I. D. Sipenkov (Leningrad Polytechnic Institute) dealt with some of the theoretical analyses and associated concepts and put forward a method of theoretical investigation into this class.

Process fluid bearings, including steam, which were actively being examined in both Britain and America, were the subject of a paper by Mr. F. K. Orcutt (MTI).

Turbo-Machinery Applications

A decade of progress in applying gas bearings to turbo-machinery was reviewed by Dr. B. Sternlicht, who made an appraisal of the many ubiquitous advantages of gas bearings in turbo-machinery, including high temperature and cryogenic applications in turbines, compressors, generators and motors. This was a practical engineer's paper and very well illustrated.

An experimental study of particular interest was described by Mr. R. Y. Wong (NASA, Cleveland). The Brayton cycle system for space auxiliary power uses a gas turbine system to convert heat into electrical energy. To eliminate the need for oil lubrication, a 38,500 r.p.m. turbo compressor with three pivoted pad gas journal bearings was designed for a two-shaft 10 Kw space station. Experimental data on such a system were presented.

A Soviet contribution on spiral grooved thrust bearings and another on jet engine compressors and seals by two American authors were followed by two British papers.

In the first, Mr. H. L. Wunsch (NEL, East Kilbride) surveyed the advantages and limitations of air bearings in the machine tool and measuring instrument fields. He gave examples of applications to cylindrical grinding, with the resulting enhanced geometrical precisions, drilling machines and machine tool slideways, in the development of which NEL has played a major role. In measuring instrument applications, Mr. Wunsch described a recent numerical read-out workshop inspection machine, a profile form trace projector, a roundness-measuring machine and a form-measuring instrument, all utilizing pressurized air bearings.

In the second British paper in this session Dr. J. W. Powell (Westwind Turbines Ltd., Poole) in a joint paper with Mr. M. C. Tempest presented a paper discussing the properties of rubber "O" rings in the suppression of self-excited whirl of rotors supported in resiliently-mounted air bearings. This is of vital importance to the aerostatic air bearing dental turbine, which operates at about 500,000 r.p.m. and of which over 20,000 have been produced since 1962.

Self-Acting and Externally Pressurized Design

The effects of molecular mean free path and slip-flow with reduced bearing clearances can be of significance in gas bearings and their effect in spiral grooved thrust bearings was discussed by Mr. F. G. Hsing (MTI) in a joint paper with Mr. S. B. Malanoski. The stiffness and damping properties of gas bearings were considered by Mr. J. W. Lund (Copenhagen Technical University). Mr. J. Pirvics (Columbia) and Professor V. Castelli (Columbia) reviewed numerical methods used in gas bearing analyses.

A new and interesting permutation of the pressurized class, porous bearings, was the subject of two papers. The first, by Mr. H. J. Sneck (Rensselaer Polytechnic) surveyed the class and the second by Mr. E. P. Gargiulo (DuPont, Wilmington) and Mr. P. W. Gilmour gave a numerical design solution.

Special Topics in Gas Lubrication

The anonymous title of this session masked two types of gas bearings of great significance—foils and herring-bone journals. In addition a new concept was introduced of electrical influences in gas bearings.

Mr. M. Wildman (Ampex) summarized the principles of foil bearings, which are finding applications in computers and magnetic tapes and Dr. A. Eshel (Ampex) gave a mathematical technique for disturbance solutions.

With regard to herring-bone grooved journal bearings, their effects on stability were discussed by Mr. R. A. Cunningham (NASA, Cleveland) in a joint paper with Dr. D. P. Fleming and Mr. W. J. Anderson. An analysis of this type was presented by Mr. D. J. Foster (AC Electronics, Wakefield) with Mr. D. Carow and Mr. D. Benson.

The electric charge in gas bearings was featured in two "iron curtain" papers. Professor V. N. Constaninescu (Bucharest Inst. Fluid Mechanics) with Mr. F. Dimofte examined the prospects of magnetodynamic boost to the load capability of gas bearings and Mr. M. V. Korovchinski (Inst. Machine Study, Moscow) described the behaviour of ionised gas bearing under electric and magnetic fields.

Materials

Dry friction problems and fine clearances, as well as the paramount importance of good geometry, make materials study a subject of vital significance in gas bearings. Five papers were devoted to this theme.

Mr. A. G. Patterson (Admiralty Compass Observatory, Slough) in a joint paper with Mr. G. D. Galvin and Mr. D. W. Morecroft ("Shell"

Research) described current work on discharge cleaning and boundary lubrication of gas bearings, marking a point where the two sciences and several other disciplines merge.

Material problems encountered in tilting pad gas bearings at temperatures up to 1400°F were discussed by Mr. S. F. Murray (MTI) in a joint paper with Mr. M. B. Peterson. Significant changes in dimensions which could result from material processing were enumerated by Mr. R. Maringer (Battelle Labs., Columbus) with Mr. A. Ingram.

The important part being played by ceramics in gas bearings was brought out by Mr. H. H. Rowe (MIT). In another British paper by Dr. N. S. Stoloff (Univ. Birmingham) process-fluid lubricated bearings materials operating at over 1000°F were discussed.

Special Topics

The outstanding feature of this last session was undoubtedly the subject of squeeze film bearings (vibratory). The load support capability of these was the subject of a paper by Mr. C. L. Strodtmann (Lear Siegler, Grand Rapids), with Professor J. M. Beck. The same authors with Mr. W. G. Holliday, analyzed a flat disc squeeze film bearing;

and Dr. C. Pan (MTI) with Dr. T. Chaing considered the behaviour of spherical bearings in this class.

Conclusion

The overall picture left in the mind by this symposium was of a decade of steady progress in all classes of gas lubrication. No single class of gas bearings had surpassed the others in the many and diverse aspects of gas lubrication which featured at the meetings.

However, among the application areas where great potential seems to exist should be mentioned those of turbomachinery, machine tools and instruments. The growing importance of the computer in modern life may foster wider applications in foil bearings and the porous bearing also seems to be ripe for further development in rail transport application. A new concept in Britain, the squeeze-film bearing, is beginning to awaken interest as an inexpensive low-friction replacement for the pressurized class of bearing.

The symposium was notable for its forward-looking outlook and enthusiasm of all concerned to continue to expand the applications of gas lubrication as the cheapest and in many cases the best of all lubricating fluids.

PROBABILITY, JUDGMENT AND MIND

The Nature of Mind—Part 2

K. W. Harrison, R.N.S.S.

Admiralty Surface Weapons Establishment

THE PHYSICAL UNIVERSE

" It may be premature to believe that the present philosophy of quantum mechanics will remain a permanent feature of future physical theories; it will remain remarkable, in whatever way our future concepts may develop, that the very study of the external world led to the conclusion that the content of the consciousness is an ultimate reality "—E. P. Wigner.

In the preceding section[1] we considered a plexus of equations, each of which represented, at different levels, the way in which a living organism adapted to, and regulated, its environment, *i.e.* by organization, by sensation and by probability judgments. We remarked that these could be compared with physical processes which may be associated with the mathematics of the transfinite. Featuring in every process, biological or physical, was the operation of change of sign or *reversal of direction*. It seemed almost as if the relation between life and mind could be expressed as a balance between order and electrical energy on the one hand and mathematical space, or the transfinite, on the other. The observations were made in support of three suggestions concerning the existence and the nature of a psychical universe. It was remarked that as a consequence of these relations attention becomes focused on the thermodynamics of mental activity and the nature of psychological time.

We shall now leave theory for practice. In this section we shall consider some of the experimental evidence from particle physics and parapsychology which further supports the " suggestions " and their corollaries. It will be necessary, of course, to consider some of the theories offered in explanation of experimental results by physicists and psychologists. These (for our purposes) are concerned mainly with the direction of time and the geometrical nature of space-time and of particles.

Counter-worlds have been suggested by F. R. Stannard[2] and the late Dr. J. Waddell[3] to compensate for the asymmetries of Nature that we find in our world. They provide an explanation of the preferred direction of time. In such worlds a tendency for entropy to decrease would be observed. These theoretical concepts of counter-worlds can be supported by direct and indirect evidence. There may exist other places in the mathematics where counter-worlds may be postulated but Dr. Waddell urged that the counter-world defined as the space orthogonal to ours be designated as the " faustian " counter-world as this makes most sense. He held that this counter-world seems to hold the most theoretical promise as a working tool; there is no difficulty, for example, in giving a mathematical explanation as to why the real world has an electric current but no magnetic current. Between the two worlds there is no ex-

change of energy but there is an *exchange of causality* caused by the emission or absorption of photons from the "other" world. Dr. Waddell indicated that this causality exchange would be seen in observations of real atoms, not in thermal equilibrium, and that the *presence of faustian matter would decrease the emission of a thermal detector (or atom)*. He described, also, the "population ratios" of emitting atoms to absorbing atoms, and a *reversal in direction* of the expansion of the universe obtained by replacing the usual states of emission and absorption by the faustian ones. This process resembles the process of change from electrical conduction to heat conduction, described in Reference [4] with an attendant reversal of direction and an accompanying change from denumerable to non-denumerable, or transfinite, mathematics.

When we consider Professor Burt's suggestion of interpenetrating and interacting systems of psychical and physical universes the above concepts are intriguing. If we supposed these relationships to obtain in Burt's universes some fascinating consequences would ensue and stimulating questions would be posed. Consider the following:—

(a) Exchange of causality but not energy could be an excellent mechanism for the correlation between mind and brain. Both Professor Burt[5] and Professor Wigner[6] (similarly to Dr. Waddell) talk in terms of photon interactions within and between universes. Professor Eccles[7] has made the suggestion that certain neurons in a critical state can be fired by some external influence which seems to be of the nature of psycho-kinesis. The "influence" arranges that these critically poised neurons are fired at the correct instant and in the correct order. Now in order to be discharged a neuron has to receive signals from two or more other neurons. Thus the firing of the "critical" neurons would release discharges from other groups of neurons and the "influence" might thus guide the pattern of a volitional process in the motor cortex. E. G. Soal and F. Bateman[8] suggest that, coming as they do from an eminent neuro-physiologist, these suggestions might well be the starting-point of a new theory of the working of the brain which might supersede the old mechanical theories.

(b) The views of Professors Burt and Wigner are in accord with this. They see the function of the brain not to "produce" conscious phenomena but to "detect" them selectively. The brain's neuronal network (according to Burt) would act like a network of receiving aerials, not generating psychic activity, but amplifying, transforming, and directing it, so that it can perform a useful function in a physical world. Such psychic activity is what he referred to when he wrote of a "field of information" in "Mind and Consciousness"; in this we are also assured that Professor Eccles, our greatest living authority on the working of the brain, has said that the actual structure of the brain suggests that it could be operated in this way.

(c) From the point of view of extra-sensory perception, Soal and Bateman point out that in truth our total ignorance of the correlation, if such there be, between cerebral activity and specific thoughts makes any wave-theory of telepathy an unprofitable speculation to-day. But they think it a mistake to insist, as so many prominent writers on telepathy have done, that physical radiation theories are impossible or even absurd. They quote Dr. A. J. Robertson[9] who has pointed out that many of the stock objections such as the necessity for a code by which the percipient discovers the meaning of the wave patterns which impinge upon his cortex, or the fact that telepathy does not appear to obey the law of inverse squares, are, upon analysis, not so insuperable as at first sight appears. For instance, Robertson observes that physical and biological systems are known where the effect produced by a given stimulus does not depend upon the size of that simulus, provided it is above a certain threshold value. Thus, when a single nerve fibre is stimulated, the resultant electrical disturbance transmitted along the fibre is independent of the size of the stimulus. A decline in the intensity of the radiation would not necessarily imply any loss of intelligibility. Or again, there might be in the brain a receptor mechanism having a response that is *proportional to the logarithm of the stimulus magnitude,* so that a large decline in intensity of radiation would produce only a small change in the magnitude of the response. Soal and Bateman discuss other observations of Dr. Robertson's but they point out also that there are serious objections to such arguments. They refer to Dr. Grey Walter's book "The Living Brain"[10] to which we also will refer later.

(d) In common with what has previously been said, an approach to techniques of experimental psychology involving cerebral heat measurement (analagous to thermography)

is suggested—this will be the subject of a separate paper.

(e) Coldness would be an indication of a psychic presence. (It would be interesting to know the percentage of reports on psychic phenomena that include mention of a fall in temperature).

(f) Would there be a balance between psychical and physical elements constituting an entity, or, perhaps, a proportionality which changes with age? (In the latter case the feeling that time goes faster as one gets older might be based on fact.) Eddington[11] in 1925 expressed his feeling that " ... there is something as yet ungrasped behind the notion of entropy—some mystic interpretation, if you like—which is not apparent in the definition by which we introduce it to physics. In short, we strive to see that the entropy-gradient may *really* be the moving on of time (instead of *vice-versa*)."

(g) By a transformation into " plausibility "[4] we have described a ratio changing in value from 0 to 1 by the logistic function. If there is a changing proportionality between psychical and physical elements, could the logistic function be used to describe the life of an organism? Stannard pointed out that if matter could in some way be transferred between the faustian and the real world then a mutually regenerative process could be established! If so we might be able to represent reincarnation mathematically as a series of logistic curves spliced on to one another in a way very similar to that by which Croxton and Cowden describe the representation of series of cultural and social epochs in human populations[12]. Perhaps it is as well at this point to remind ourselves of the law of growth which the logistic curve describes. This is stated by Raymond Pearl [13] as follows: " In a spatially limited universe the amount of increase which occurs in any particular unit of time, at any point of the single cycle of growth, is proportional to two things, *viz:* (i) the absolute size already attained at the beginning of the unit interval under consideration, and (ii) the amount still unused or unexpended in the given universe (or area) of actual and potential resources for the support of growth."

(h) A balance, or ratio, of this nature would be consistent with such things as Jung's Racial Memory and Universal Unconscious[14], extra-sensory perception, Polanyi's " tacit knowledge "[15] and even (it could be suggested) immunological memory. Soal and Bateman point out that we may expect to discover that processes akin to clairvoyance and psycho-kinesis are at work continually in the biological processes of morphogenesis and in the psychological processes of perception and memory. G. Tyrell's words[16] are appropriate " ... if the human being is the vastly complex structure that psychical research is beginning to reveal (and not merely complex, but, as regards its higher phases, impenetrable to thought and of unknown profundity), there may be surely a great deal of it which does not show . . . There may well be only a specialized part of it that does show . . . There may well be a third factor, a pre-existing self; in fact there may well be many factors in the subtle woven personal complex, of whose independent existence we can see no trace from without." Those who are mechanistically inclined could combine the creed of those who explain all learning as a conditioned response with the " mountain range " description of Jung's concept of mental life and suggest that life is merely a vehicle for mind—an uninspired form of pantheism similar to that of Pope. (The Romantics could, however, develop a Wordsworthian pantheism!).

(i) In Gestalt psychology a phenomenon called the " phantom limb " is sometimes discussed. If a limb has been amputated sensation may be felt for sometimes as long as years afterwards from where the limb used to be. The sensation will come, apparently, from within a solid obstacle if that happens to be where the limb would have been. Katz[17] describes how " . . . with the passage of time characteristic changes occur in the phantom limb. It must be assumed that they are caused by the dynamics of the residual field. The phantom limb tends to shrink. The change does not depend on nerve tissues of the stump, but on the residual fields of the brain . . . There is much to be said for the view that the phantom limb's regression throws light on the original structuring of arm and leg impressions in the life of each individual. It does this by *reversing the growth process* ". Here is a factual, biological, example of time-reversal, of negative values for sensations as conceived by Fechner, and of the fields of information associated with the brain conceived by Burt.

(j) Could the phantom limb be detected thermographically?

Another attempt to explain time-reversal (of

established relevance to scientific inference) is the theory developed by Richard P. Feynman (from a suggestion by Professor Wheeler), for which he shared the 1965 Nobel prize in physics. This has a profound effect on notions of causality and would seem to be a step towards Professor Burt's " neutral monism ". Feynman developed a mathematical approach to quantum theory in which an anti-particle is regarded as a particle moving backward in time for a fraction of a microsecond. When there is pair-creation of an electron and its anti-particle the positron, the positron is extremely short-lived. It immediately collides with another electron, both are annihilated and off goes a gamma ray. Three separate particles—one positron and two electrons—seem to be involved. In Feyman's theory there is only *one* particle, the electron. What we observe as a positron is simply the electron moving momentarily back in time. Because our time, in which we observe the event, runs uniformly forward, we see the time-reversed electron as a positron. We think the positron vanishes when it hits another electron, but this is just the original electron resuming its forward time direction. The electron executes a tiny zig-zag dance in space-time, hopping into the past just long enough for us to see its path in a bubble chamber and interpret it as a path made by a positron moving forward in time.

Hans Reichenbach, in his book " The Direction of Time ", calls Feynman's positron theory " the most serious blow the concept of time has ever received in physics. Not only does it reverse the *direction* of time for parts of the world, Reichenbach points out, it also destroys the uniform topological *order* of causal chains. Thus we see that not only does the theory have a relevance to the foregoing discussion, but it relates directly to the problems of teleological and mnemic causation considered by Professor Braithwaite[18]. Dr. Good discusses a somewhat similar situation in an article " Speculations on Precognition "[19].

Professor Wheeler had imagined one electron, weaving back and forth in space-time, tracing out one world line. The world line would fill the entire cosmos in one instant like a gigantic ball of string. A cross-section through cosmic space-time, cutting at right angles to the time axis would show a picture of three-dimensional space at one instant of time. On this cross-section there would be millions upon millions of points each corresponding to a spot where the tangled world line was cut. If the cut occurs at a spot where the electron is moving forward in time, the spot is an electron. If it occurs at a spot where the particle is moving backward in time, the spot is a positron. All the electrons and positrons in the cosmos are, in Wheeler's idea, cross-sections of the path of this one particle. Since they are all sections of the same world line, naturally they will have identical masses and strengths of charge. Their positive and negative charges are no more than indications of the time direction in which the particle at that instant was weaving its way through space-time."

Although Professor Wheeler did not outline this idea as a serious theory, there have been serious theories put forward in similar vein. For example, in England Dr. P. F. Browne, of Manchester University has suggested[20] that the universe can be explained in terms of positrons and electrons. Objections have been raised to theories which would lead to the need for the numbers of electrons and positrons in the universe to be equal but it has been shown recently[21] that to account for the creation of galaxies equal amounts of matter and anti-matter must be postulated (possibly in an original " ambi-plasm ") so this difficulty might be resolved.

Theories such as these are of interest to us because they relate to our discussion on the nature of mind and on the correlation between mind and brain, but also because they have relevance to problems concerning determinism and free-will (clearly of prime importance in probability consideration) which must be touched upon if in the next paper of this series we are to come to any conclusions about the purpose of, or reason for, mind. For example, differences between the theories of Einstein and H. Weyl amount to differences between a deterministic or a non-deterministic universe. Weyl realised that the relativistic geometrization of matter should be extended to the microcosmic. If the proposition that matter is merely a local wrinkling of space-time is true, then it must be true even of the microphysical constituents of matter, *i.e.* of electrons; but, according to Weyl, this is possible only if we give up not only the Euclidean character of space, but also its continuity, which the space of the general relativity theory still possessed. Weyl regards the electron as a sort of " gap " or " hole " in a non-Euclidean continuum. Professor Capek [22] discusses in some detail the continuity of space and time and the invention of the new names " chronon " and " hodon " for designating the atoms of time and space respectively. He attributes Einstein's reluctance to depart from determinism to certain unconscious or semi-conscious metaphysical predilections of the kind we discussed in the first paper of this series. (It may be, of course, that his comments contain bias !) In particle physics to-day the two colleagues John Wheeler and Robert H. Dicke, of Princeton University, have conflicting theories which clearly set in focus the sharp contrast in opinion over the value of Einstein's general theory

of relativity. At stake is Einstein's unique formulation of motion and the gravitational field as implicit properties of the geometry of space-time. According to Einstein " Matter which we perceive is merely nothing but a great concentration of energy in very small regions. We may therefore regard matter as being constituted by the regions of space in which the field is extremely intense . . . There is no place in this new kind of physics both for the field and matter *for the field is the only reality.*" From this point of view, the electron represents that region of the electromagnetic field where the field intensity is incomparably higher than its surroundings; but according to Einstein (in contrast to Weyl) the same field equations (though certainly more complex ones than the classical equations of Maxwell) hold **inside** the electron as well. The triumph of either Wheeler's scheme—to unify Einstein's geometrical interpretation with quantum mechanics and generate a " quantum relativity "—or Dicke's plan—to replace Einstein's construct with an alternative, non-geometrical formulation which better describes Dicke's own recent astronomical observations—will mark a profound transition in man's conception of the universe.

The most significant aspects of modern particle physics, however, were spoken of by Professor Wheeler at a meeting of the American Physical Society (he was the retiring president). He referred to the need to unify the principles of general relativity with those of quantum mechanics[23], and mentioned some phenomena which are of particular relevance to our discussion. " The end of time " was the way he described a strange phenomenon within distances of less than 10^{-33} cm infinitely removed from the resolving powers of modern physics. Within these distances it seems there is *neither sense of time nor substantial mass* but only—as in general relativity—an intrinsic " geometry ". This means that in this ultra-microscopic world, where all phenomena are subject to quantum mechanical fluctuations, physical events are " some sort of resonance hybrid of any number of conceivable fluctuating geometries " in the same way that chemists see molecules as resonance hybrids of any number of possible molecular configurations. Wheeler visualizes a particle as a manifestation of the violent small scale fluctuations in geometry which are continually taking place throughout all space. It is not itself a wrinkle in the geometry. It is not in itself a 10^{-33} cm fluctuation in the geometry. Instead, it is a fantastically weak alteration in the pattern of these fluctuations extending over a zone containing very many such 10^{-33} cm regions. He sees space geometry, despite appearances, as not being frozen in a Euclidean cast. Instead there is a

probability amplitude for this, that and the other geometry, each differing from the other at the 10^{-33} cm scale of dimensions. Professor Wheeler points out that nobody is inventing these fluctuations in geometry. They are forced upon us by the very mathematics that go with general relativity and the quantum principle.

Several features of Professor Wheeler's address and of modern experiments on fundamental length are worthy of remark:—

(a) Wheeler's mention of scales of dimension reminds one of the suggestion that the Universe bounded by the radius, R_u, is an elementary particle in a super-universe, which would be an isolated system on an enormously greater scale and would involve scaled up constants. An infinite system of such isolated systems at different levels is defined, and spin momentum, $\propto^n h$, where $\propto$ is an enormous dimensionless constant, is associated with nth. level system. The universe, the electron and the neutrino are identified with the systems, $n=+1$, $n=0$ and $n=-1$, respectively. These we recognise to be the values held by the parameter of the logistic functions representing, respectively, the Fermi-Direc, the Boltzman, and the Bose-Einstein distributions with which we associated the realm of the denumerably infinite, a pure gas of electrons, and the realm of the non-denumerably infinite, or the transfinite[4]. Such notions of infinite chains of worlds within worlds reappeared most unexpectedly in the thoughts of Alfred North Whitehead only a few weeks before his death (he will be remembered in relation to panpsychism in Part 1). As Professor Capek[22] says, " It is certainly astonishing to hear the thinkers who so emphatically stressed that ' reality is incurably atomic ' sink back into the Pascalian vision of the infinitely divisible universe."

(b) C. A. Ramm, head of the Nuclear Physics Apparatus Division at C.E.R.N., has recently said[24] that in another five years experimental neutrino physics will have passed from being technically unfeasible to one of the most important experimental fields in high energy physics. Ramm says that the neutrino is one of the most prolific particles in the world. The question is, according to Ramm: What is its role in our world ?

(c) A recent experiment at the Brookhaven National Laboratory[25] has provided for the first time a critical test of the current concepts of space and time at really short

distances. Lindenbaum and his colleagues have drawn the conclusion from elastic pion-proton scattering experiments that for distances down to the order of 10^{-15} cm there is no evidence whatsoever for a primitive acausal (in the sense of special relativity) region or fundamental length. It has been known for some years[26] that the existence of an acausal region of space-time (across which signals might be propagated at velocities exceeding that of light) leads to a failure of forward dispersion relations for elastic scattering.

(d) According to Soal and Bateman, if mechanical theories have failed to provide any intelligible theory of mental phenomena such as memory or learning, it seems unlikely that they will prove more successful in the case of telepathy. To seek for a *causal* explanation of telepathy may be merely a case of a question that is wrongly posed. We shall probably have to accept non-causal correspondence between mental states as an irreducible fact of nature.

(e) With regard to faster than light particles, Professor Gerald Feinberg of Columbia University has concluded[27] that the possibility of particles whose velocities are always greater than c does not contradict special relativity. Feinberg calls his particles tachyons and predicts the following properties for them:—

   (i) Their velocity is limited to values between c and infinity;
   (ii) The *mass* of the tachyon is *imaginary*;
   (iii) At $v=c$ the tachyon has infinite energy and momentum; as the particle loses energy it speeds up until for $E=0, v=\infty$;
   (iv) Although the tachyon has no spin, it obeys Fermi statistics;
   (v) Tachyons of any energy would not be prohibited from emitting massless particles (photons or neutrinos).

(f) J. H. C. Whitehead suggested to Dr. Good in 1945 that "thought waves" could travel faster than light[19].

(g) We have shown[4] that the logistic function can be of use in representing the results of scattering experiments.

(h) Professor Feinberg considers it unlikely that further discoveries concerning the elementary particles or the addition of new members to that family will shed any real light on the properties of nuclei. The still unsolved problems of detail in that field, he suggests, are more likely to be understood through the discovery of subsystems of nuclei, such as the shell structure of nuclei,

terms of which simple approximations can be made. Bethe has suggested more powerful mathematical weapons for tackling the problem of the nucleus, in reply to which Gunther and Campbell have suggested the use of transfinite numbers. We have already found the logistic function to be useful for approximations in physics, for describing particle distributions and the results of scattering experiments, and for the introduction of transfinite mathematics; it is not inconceivable, therefore, that it might be used in this respect. In particular the method of deriving parametric relationships for the logistic curve[28] might be of use.

(i) Kyoto University's famous Nobel prize-winner, Hideki Yukawa, has produced a new theory concerning the nature of elementary particles (*Scientific Research*, October 1967). Yukawa and his colleague Katayama have compounded a model of the " interior " of elementary particles. It has always been supposed that a theory would emerge that would reveal the causal chain underlying the veiled mechanisms through which collisions and decays produce streams of new particles from old ones, but physicists have been reluctant to make a plunge into speculations on the inner constitutions of fundamental particles. To the " inside " space they first assign the same kind of geometrical structure as Einstein first prescribed for the structure of *all* space, *i.e.* a measure based on curved space-time. With this beginning, they proceed to adapt the essentially classical theory of the way in which deformations of macroscopic bodies alter such physical properties as their charge distribution and spinning motion, to produce a quantum mechanical description of how particles are created and annihilated. One of the most startling aspects of their theory is that when a particle is not excited—that is, when it is in its " ground state "—it cannot be observed. Thus it is akin to a geometrical object rather than a material particle. Only when the particle is excited does it become an observable entity.

(j) As atomic phenomena seem only aspects of an as yet unexplored structure with hidden variables and the objects of quantum mechanics are related to a subatomic world, Louis de Broglie has proposed the notion of the thermodynamics of the isolated particle, attributing to the atomic particles themselves both entropy and temperature.

These are some features of the emergent picture of space-time. We see how consistent they are with the views of Professors Burt and Jung and, indeed, how well they fit into the pantheistic views of Einstein and others. We have seen that questions in particle physics on causality, events, physical and mathematical interpretations of space, time and matter, continuity, *etc.*, have considerable interest, also, for biologists with respect to problems of knowledge, judgment, sensation, experience, and perception, in other words problems of life and mind. There are two large areas to which we have, as yet, paid little attention: these are Emotion and Extra-Sensory Perception. Discussion of the former is more appropriate to the next paper in the series, but the phenomena of the latter—in particular telepathy and precognition—will be discussed in the following, and concluding, section of this paper. If there is some kind of basic psychical substrate, such as we have suggested in the first section, which corresponds to the physical substrate, some early glimpses of which we have attempted to outline in this section, we should expect the region of overlap and interaction, the psycho-physical world as described by Professor Burt, to be manifest in some way and open to experimental investigation. We have already seen glimpses of opportunities for this concerning E.S.P. In the following section we shall take this up more fully.

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Book Reviews

An Introduction to Vectors. By A. E. Coulson. Pp. 170. London; Longmans. Price 12s. 6d.

"This book introduces . . . the basic concepts of vector algebra and vector analysis . . . by the end of the book the student is ready to approach abstract branches of the subject with confidence". This claim is a very fair one, the standard work is introduced and illustrated by plane geometry, electro-magnetics and mechanics, each chapter having a generous number of examples. The last two chapters are brief introductions to vector algebra and field operators. The student who has worked through the book is indeed well equipped to go on to such topics. The style is clear and concise and there is an abundance of clear diagrams.

<div align="right">

H. N. V. Temperley

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An Introduction to Number Scales and Computers. By F. J. Budden. Pp. xiv + 192. London; Longmans, Green & Co. 1965. Price 12s. 6d.

During the last decade, the number of computers in Britain has risen sharply from about 70 to over 2,000. According to recent estimates, there will be a grave shortage of trained personnel to cope with this increase and therefore any means of introducing computers to a wider public is welcome.

The book under review is primarily intended for students at v - vi form level but could be read with interest by anyone of O - level standard.

The first chapters introduce the Octal and Duodecimal scales and then Binary arithmetic is dealt with in detail. The application of the Binary scale to electronic computers is described and the final chapters gives an introduction to programming and is based on Mercury Autocode.

There are many examples at the end of each chapter and these are presented in a lighthearted way which helps to maintain interest.

In a book of this size the subjects cannot be treated at great length and while not a major contribution to computer studies it may encourage the reader to seek more detailed information.

<div align="right">

B. Boothman

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Discrete and Continuous Methods in Applied Mathematics. By J. C. Mathews and C. E. Langenhop. New York and London; John Wiley and Sons, 1966. Pp. xiii + 525. Price 83s.

The emphasis in this book is on the use of mathematics as a source of models of real situations and processes, particularly in operational research, economics and the managerial sciences. There are chapters on discrete probability theory, linear algebra, finite Markov chains, linear programming, capacitated transport networks and stochastic processes as well as several chapters on differential equations. Both authors have experience as lecturers in mathematics at American Universities and their book is intended as the basis of a one

year course for science, engineering and mathematics undergraduates. Presumably, they have found a need to supplement the more traditional applied mathematics syllabus with courses of this kind. They also consider the book useful as a self study text for practising scientists.

Each major topic is introduced by a lengthy discussion of some appropriate mathematical model—a diet problem illustrates the basic ideas of linear programming, a model for traffic flow introduces stochastic processes and so on. Examples of this sort lead the reader into a detailed and rigorous treatment of the formal mathematics. Worked examples and problem sets with hints and answers follow each main section and are used to develop points made in the text. The concepts and notations introduced in the chapters on probability theory and linear algebra are used throughout the book and are essential reading but otherwise, the various chapters can be studied more or less independently.

The methods dealt with are, of course, based on well established theory (all references are to standard texts) and the mathematics appear sound enough. The treatment does, however, seem unnecessarily detailed for a book intended for a subsidiary course at undergraduate level and the text suffers generally from an excessive wordiness. The authors apparently believe that rigorous detail is essential for a full understanding of the subject but all too often they succeed only in obscuring and confusing the main line of an argument. The first two chapters alone take 170 pages to cover fairly standard material such as permutations, combinations, probability functions, matrices, system of equations etc., but despite numerous definitions, theorems and proofs, no really clear ideas emerge as to the importance of the Binominal and Poisson distributions for example, or the implications of Bayes' theorem. At a time when students are having to absorb an increasing amount of information, it is surely important for writers of text books to present their material more lucidly than this. The general layout of the book also adds to the reader's difficulties—the text is cramped, subheadings infrequent and the mathematical notation not always easy to follow.

The main advantages of the book are that it brings together a number of topics not usually found in a single volume on applied mathematics and that some attempt is made to relate the various methods to the development of practical mathematical models. Whether or not these advantages outweigh the shortcomings already mentioned depends largely upon the student's outlook. The mathematically inclined may find the rigorous attention to detail quite acceptable whereas the science or engineering student may prefer a more intuitive treatment. For self study purposes, however, I think that anyone with access to a good library would do better to consult some of the standard books already available.

<div align="right">

J. Turnbull

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Electromagnetic Shock Waves. By I. G. Katayev. Pp. 164 + 11. London; Iliffe Books Ltd., 1966. Price 35s.

This is probably the first book devoted entirely to this relatively new subject, it is claimed to contain much previously unpublished material.

The first chapter explains the mechanism by which electromagnetic shock waves are formed, and certain similarities with the more familiar shock waves of gas dynamics, from which their name is derived. They can arise when an electromagnetic wave propagates in a non-linear medium, whose parameters are functions of the strengths of the fields. The simplest case is when the velocity of the crest of an initial wave is greater than the velocity of the trough, which leads to the

formation of a sharp fronted wave. From a practical point of view suitable non-linear media are ferrites, piezoelectrics, transition layers in semiconductors and the magnetron effect. The present interest in the subject arises because pulses can be generated with sharper sides than can be obtained from conventional electronic circuits.

As might be expected, the general theory of non linear media is eschewed and consideration is confined to modes of propagation which, in the linear case, obey the Equations of Telegraphy. Within this limitation Chapter I considers generally how various forms of non-linearity may be dealt with, but Chapter II concentrates entirely upon the case of ferrite loaded transmission lines, and indeed throughout the book detailed consideration is only given to the case of ferrite media. Chapters I and II occupy the first 104 pages.

Chapter III, occupying the remainder of the book, is practical and is entitled "The Design of Pulse-Shaping Networks for Short Pulse Generators using Shock Waves in Lines containing Ferrite". This considers the conflicting design criteria and contains many interesting numerical examples. As a rough idea of what can be achieved with a ferrite loaded continuous transmission line, we quote pulse rise times of 10^{-9} sec. at about 10 amps and 10^{-10} sec. at 100 A. The same times can be achieved at somewhat lower currents with sectionalized loaded lines. With semiconductors much lower power levels could be reached, but as we said, the book does not deal with this in any detail.

The bulk of this book is a theoretical treatment of a rather difficult subject, and it says much that this is achieved with nothing beyond elementary calculus. Close contact with the physics is maintained, and this is used as a guide to simplifying approximations. The book is fairly concentrated and, although not difficult, the reader unfamiliar with the subject will probably feel he has been provided with a good measure of new material to absorb.

The book is translated from a Russian edition first published in 1963. As far as we can judge the translation is mainly accurate, although on a very few occasions we found we had to guess how the translator had been led astray. There are of course rather more Russian references than we would find in an English book, and the nomenclature of Russian ferrites probably does not mean anything to a Western reader. We believe the book represents good value to anyone either mildly or seriously interested in the subject from either the theoretical or practical point of view.

R. B. R. Shersby-Harvie

Fundamentals of Ultrasonics (Second Edition). By J. Blitz. Pp. x + 220. London; Butterworth's, 1967. Price 38s.

The aim of this book, as stated in the preface, is to provide a reasonably up-to-date general textbook on the study of ultrasonics. This aim is achieved, and the book is eminently readable yet the subject matter is given a sufficiently rigorous treatment to satisfy most of its users. The references provided are many and these are remarkably up-to-date.

The Introduction contains a definition of Ultrasonics, justification of the use of ultrasonic frequencies as opposed to audio frequencies and a historical development of the use of ultrasonics. It concludes with a list of references for further reading, which are grouped under general subject headings and a list of recommended journals which keep abreast of the latest developments in the field.

The Introduction having been designated Chapter 1, Chapter 2 is concerned with general principles of propa-

gation of low amplitude waves. It includes sections on free and forced vibrations, which are discussed theoretically, plane wave propagation and the different types of attenuation. Reflection, refraction, diffraction and stationary waves also receive a reasonable amount of attention. Also included in this chapter are a very short section on acoustic waveguides, succeeded by a section on ultrasonic focusing systems. At the end of this chapter, as is the case with each chapter, there is a list of references specifically mentioned in that chapter. An idea of the way in which the Author has kept abreast of his subject can be obtained by noting that approximately half of the references quoted have been published since 1960.

Chapter 3 is concerned with ultrasonic generators and receivers.

Crystal oscillators are dealt with at great length. These oscillators utilize either the piezoelectric effect or the electrostrictive effect and both effects are defined, particular attention being paid to piezoelectricity. The piezoelectric relations are obtained, these being equations relating the dimensions of the crystal and the electrical voltage and corresponding mechanical strains produced in the crystal. Also concerned with piezoelectricity are sections on the dynamic characteristics of such transducers, and the equivalent electrical circuit of a piezoelectric transducer.

Other types of transducers mentioned are the magnetostrictive, mechanical, electromagnetic and electrostatic types, the limitations in their usefulness being pointed out. These latter four types of transducers are given less space than that devoted to crystals, due presumably to the restrictions on their use, but nevertheless the text provides readers with a good working knowledge of the principles involved and there are many references which take the subject further.

Small sections are devoted to thermal, chemical and optical transducers and the chapter ends with a section on ultra high frequency transducers. These transducers, such as the depletion layer, epitaxial and magnetic film transducers, are designed to work at microwave frequencies.

Chapters 4, 5 and 6 are concerned with low amplitude propagation of ultrasonic frequencies in gases, liquids and solids respectively. The velocities of propagation are investigated for each case, and the mechanisms of absorption and dispersion are dealt with in great detail. Practical measurements are made using adaptations of the pulse technique for each case and other methods which, for various reasons, are suited to only one of the three media.

Chapter 7 details some low power applications of ultrasonics. The Author's background in non-destructive testing being conspicuous by the space allocated to it.

Both this chapter and the final chapter (Chapter 8), which deals with high energy ultrasonic waves, are short, but carry impressive bibliographies, particularly in the case of Chapter 7.

In conclusion, this is a book which is extremely useful, both for the information it contains and for its extensive and comprehensive list of references. The student of physics would be hard put to find better value for money and for the worker in acoustics and ultrasonics it is an excellent reference book.

R. G. Marsh

Introduction to Instrument Transformers. By B. D. Jenkins. Pp. xiv + 282. London; George Newnes Ltd., 1967. Price 50s.

The author's aim is to provide a "concise and comprehensive introduction to both current-trans-

formers and voltage transformers, including basic theory, design approach, types of construction, materials used, testing and the utilization of these instrument transformers in both measuring and protective circuits ". The emphasis in the book is on current transformers and only the last 57 pages are devoted to voltage transformers. The mathematical treatment, where it is used, is simple and many graphs and phasor (vector) diagrams are included.

The opening chapters include the usual derivations of the characteristics of magnetic alloys, design formulae of the current-transformer error equations, a discussion and curves, special theoretical considerations and some operational notes. A number of methods of modifying current-transformer errors are described and a chapter is devoted to " current-transformers in indicating and metering circuits ". The four chapters which deal with protective current-transformers provide a useful introduction to this topic. The chapter on the testing of current transformers is brief but the principles involved are well illustrated in the description of the Petch-Elliott and the Arnold methods.

Most engineers will find that this text is a good introduction to the subject and in particular they will appreciate the sections which deal with design, construction and utilization. Students may find that this book is less satisfactory because of some omissions in the basic theory and because there are no examples or problems (The book was written for designers and not for students).

On page 44 an equation is stated which relates an exciting current under sinusoidal flux conditions with two other exciting currents and their form factors. It is also stated that this equation depends on the existence of a linear relationship between the r.m.s. exciting current and the form factor of the voltage waveform. With this information it is easy to derive the given equation but the linear relationship itself is not so obvious.

On page 74 in a discussion on the separation of eddy current and hysteresis losses, reference is made to the difference between the apparent and the calculated eddy current loss. It is a pity that the author did not discuss further this " anomalous loss ".

On page 224 it is stated that in an ideal voltage transformer the terminal voltages are in precise " phase opposition " to each other. Whether this is true or not will depend on the connections of the windings to the terminals as well as the polarity of the measurements.

The above examples are only minor criticisms of a book which well satisfies its stated objectives.

E. L. Topple

Spectroscopy at Radio and Microwave Frequencies. By D. J. E. Ingram. Second Edition. Pp. viii + 534. London; Butterworths, 1967. Price 95s.

This is the second edition of Professor Ingram's well known book first published in 1955. The new preface explains that although advances since 1955 have been considerable important new developments are expected during the next two or three years. A full revision is to await these developments, meanwhile the second edition consists of the original nine chapters plus three new ones intended to be read in conjunction with various earlier sections. The new material is extensive, 182 pages in the new chapters, additions and corrections to Appendix II (details of commercially available instruments) appropriate revision of the author index and subject index, and references in the original text to the new chapters.

To do the whole book justice would demand too long a review and rather than make a sketchy coverage of the whole it will perhaps be more useful to concen-

trate on the new material with but brief remarks about the rest.

The second and third original chapters cover " The Production and Detection of Microwaves " and " Waveguide Techniques ". Insofar as instrumental limitations bearing upon spectroscopy are discussed the chapters are important, but there are now many books covering the general subject matter and most of the text could with advantage be omitted. The two chapters could be replaced by notes on instrumental limitations and a bibliography, added to the introductory chapter.

The last original chapter "Applications of Radio Frequency and Microwave Spectroscopy " is not fully updated by the applications discussions in the new chapters and I feel that a revision of at least the addition of a short list with references to the literature would have been worth while now.

The first new chapter describes recent work on nuclear magnetic resonance concerned mainly with exploiting the very narrow width of absorption lines in liquids. In this and the other new chapters the character of the original book is maintained; experimental techniques in both the instrumental and sample manipulation areas are clearly explained and the supporting theoretical discussion is sufficient and arises in the natural flow of the text. For those seeking more detail there are extensive reference lists at the end of each chapter.

After describing the more straightforward applications of high resolution N.M.R., using examples very carefully chosen to avoid side issues, Professor Ingram includes interesting sections on Bloch's double nuclear resonance technique and on nuclear precession in low field strengths. The important application of the latter process to measuring the earth's magnetic field is properly stressed.

The next chapter " Recent Advances in Electron Resonance " is an appendix to many earlier chapters. Disregarding superfluous statements about areas in which no recent advances have occurred the chapter holds together remarkably well, and the references back to the original text together with the forward references added therein bring the new material fully into the body of the book. The most important sections are the two on " Free Radical Studies " with the sub-titles " Experimental " and " Molecular Orbital Theory ", a longish one on " Ligand Field Theory on Transition Group Complexes " (for my money the best effort in the whole book as it is a difficult concept to put across), and the final one on " Biophysical and Biochemical Applications ". One hopes that this last section, together with smaller references elsewhere in the new part, may encourage biochemists to read all the book.

The last chapter " Double Resonance—Masers and Lasers " may be a mistake. As in the rest of the book the presentation is impeccable and a large amount of information is painlessly absorbingly and therefore relentlessly administered. Like any good book, you can't put it down. Nevertheless, I feel about this as I did about the earlier chapters on microwaves; the subject is adequately covered elsewhere in the literature and you just can't cram everything into one book. In successfully attempting a miniature treatise on masers and lasers themselves the spectroscopy applications of the techniques fail to come over properly. A pity, because that aspect is not well covered elsewhere and Professor Ingram could do it so effectively.

You will have gathered that I consider this to be a very good book, my criticism being mainly that there is too much in two parts of it. It is well worth buying —trade in the first edition if you already have that and remember that it may be some years before the fully revised version is published.   **J. P. Grantham**

Pendulum Gravity Measurements at Sea, 1936 - 1959.
By J. Lamar Worzel. Pp. xx + 422, 28 Figs., 58
Tables. New York; John Wiley and Sons, Inc., an
Interscience Publication, 1965. Price 210s.

Professor Worzel's book is a collection of 2,865
previously unpublished gravity measurements made at
sea, mainly by the staff of the Lamont Geological Ob-
servatory, Columbia University, New York, using the
Vening Meinesz pendulum apparatus. An uncertainty
of $\pm 3 \cdot 6$ milligals is estimated after an analysis of
measuring procedure and sources of error. The results
are presented both as free-air and simple Bouguer
anomalies, and listed under cruises. A chapter is devoted
to a discussion of their geophysical and geodetic impli-
cations. In order to provide a comprehensive and useful
summary of the data, there are four appendices giving
details of the cruises, cruise charts, base station location,
and a list of references to observations not recorded
in the main part of the book. In the final two appendices,
all available marine pendulum gravity measurements are
plotted in the form of free-air and Bouguer anomalies,
on a series of bathymetric charts.

Marine pendulum measurements require several min-
utes of observation to produce a single value, and these
observations can only be satisfactorily made in sub-
marines, below the disturbing accelerations of the sea
surface. The technique is now being rapidly superseded
by satellite observations and by spring gravity meters
mounted in surface ships. The latter give a continuous
profile of gravity along the ship's track, have a higher
relative accuracy than pendulum measurements, and
reduction of the data is easier. However, comparison of
gravity profiles along intersecting tracks has shown that
systematic errors as large as 20 milligals can occur par-
ticularly in rough seas. Pendulum measurements provide
a useful absolute reference for checking routine gravity
surveys, and when investigating cross-coupling and other
probable causes of these discrepancies. This book will
be a useful reference for absolute gravity measurements
for some years to come. In addition it should prove,
ultimately, a useful documentation of the submarine
pendulum era in the development of gravity measure-
ment at sea.
                                                    D. T. Pugh

Germanium. By V. I. Davydov. Section on Radioactive
isotopes of germanium. P. Rudenko and L. V. Kovtun.
Translated from the Russian by Adam Peiperl. Pp.
417. London, New York and Paris. Gordon and
Breach, Science Publishers. 1966. Price $18.00.

Although there is no introduction, the title implies
that this is a comprehensive review of the element.
However, the major part deals only with the chemistry,
and even then, by aiming to be comprehensive, it does
not provide very detailed information. In particular,
the section on organogermanium compounds is outdated
and, as is general throughout the book, out of the eight
references cited, only two are in well known journals
published outside Eastern Europe.

The Appendix on the radiochemistry of germanium
has not been correlated with the rest of the book, and
as a result there is some repetition of data, for example,
in considering the occurrence of the element.

The style is terse and in places puzzling; although
there is a table of contents there is neither a subject
nor an author index. For a " handbook " on germanium
the number of references (282) is surprisingly restricted
and a high proportion of these are to standard works.

There are a large number of informative diagrams in
the metallurgy and physical chemistry sections. The book
is likely to be of greatest value and use to the metal-
lurgist or physical chemist wanting to survey the Russian
literature prior to 1961 on the subject. **J. A. Douek**

The Depth of Cold. By A. R. Meetham. Pp. 173. London.
English Universities Press Ltd. 1967. Price 25s.

This book surveys almost all topics suggested by the
word cold, from problems of survival in cold and other
biological matters to industrial applications, the lique-
faction of gases and fundamental problems like the
conduction of heat, superconductivity, liquid helium and
the meaning of absolute zero. This wide sweep reflects
the author's own experience, he writes of most of the
topics from first hand knowledge. There are 17 chapters
so each gets about 10 pages. This is only enough for
fairly superficial surveys. In one instance there is a
definite waste of space—the author is beguiled into
giving a long disquisition on lasers when discussing the
concept of negative temperature. The situation could
have been illustrated better by using a magnetized
material as the example.

The style is clear and pleasant but becomes rather
irritatingly chatty in places. Apart from this slight
defect, I recommend the book. There is something for
almost everyone—very few people are experts in all
the many topics dealt with.
                                           H. N. V. Temperley

Energy and Entropy in Chemistry. By P. A. H. Wyatt.
Pp. ix + 192. London; Macmillan, 1967. Price 30s.

Although this book is only claimed to be an intro-
ductory course in chemical thermodynamics it is likely
to be useful to chemists and physicists at all levels.
Many of the fundamental ideas of statistical mechanics
are explained in simple but carefully chosen words.
Thermodynamics is hard to teach, not because of its
intrinsic difficulty, but because no two students fare
alike; one will be held up for weeks by a difficulty that
another scarcely notices. The author deals with this by
providing a generous number of examples with hints
both in the text and in the solutions. The student who
wants to know more about a specific point is gently
guided to advanced works, mainly Lewis and Randall.
The scope of the work is indicated by the chapter head-
ings of which " Measurement of the tendency to change",
" Energy is not enough ", " Entropy ", " Phase equili-
brium " " E.m.f. and the free energies of ions in solu-
tions " are enough to show that all branches of chemical
thermodynamics are adequately covered.

The style is clear and pleasant, the author obviously
both understands his subject and is capable of making
it interesting to others. There is an adequate index and
list of symbols.

I recommend the book thoroughly.
                                           H. N. V. Temperley

Computer Produced Physiological Tables. By G. R.
Kelman and J. F. Nunn. Pp. 50. London; Butterworths,
1968. Price 25s.

This book consists of computer-produced tables for
calculations involving the relationships between blood
oxygen tension and content. The authors present these
tables as a stop-gap measure until on-line computing
facilities become widely available to the medical pro-
fession.

The methods of using the tables are fully explained
together with the rationale and physiological basis upon
which they are constructed. While some laboratories may
find that the extrapolation required between the values
given does not give sufficient accuracy for research
purposes the tables will be very valuable for routine
clinical use and teaching. The purely digital format
allows for greater accuracy than is possible with the
slide-rule type of calculator and this book will be a
useful tool in all departments concerned with blood-gas
calculations.
                                                    J. M. Young

Practical Five-Figure Mathematical Tables. By
C. Attwood. Pp. viii + 88. London, Melbourne,
Toronto; Macmillan, 1967. New York. St. Martins
Press, 1967. Price 8s. 6d.

This book was compiled for the Ford Motor Com-
pany, presumably for training purposes, and the selection
of tables included, although relevant to their particular
needs, might not be suitable for most people requiring
a book of mathematical tables.

Throughout the tables the mean proportional parts
have been calculated by a method that has not been
used before. Each possible combination has been
individually tested and it is claimed that " Over 97%
of the 168,903 combinations of mean proportional parts
produce either no error or an end-figure error of one
unit, whilst less than half per cent produce an error
in excess of three units . . . " The negative mean pro-
portional parts have been printed in red type, which
proves a very effective reminder; also all the arguments
are in bold type.

There are three sections in this book entitled Main
Tables, Subsidiary Tables, and Notes on the Tables.

The first part, the " Main Tables ", includes a page
of multiples and fractions of π and $1/\pi$, cologarithms,
areas of circles with diameters advancing by thousandths,
a comprehensive and useful list of conversion factors,
and factorials and logarithms of factorials amongst
other more usual tables. The most striking omission
appears to be that of Naperian logarithms.

The second part, called " Subsidiary Tables ", com-
prises a system of notes at the bottom of some of the
pages of main tables. For example, the contents list
suggests that to convert minutes to radians the answer
can be found on pages 29, 32, 42 or 47, which is in fact
an inch at the end of each page, awkward to use and
confusing when using the main tables. If these tables
were not felt to be important enough to warrant a
complete and tidy section of the book, then why include
them at all ?

The last 20 pages give clear, detailed instructions on
how to use the tables, forming the third section of the
book.

This book cannot be recommended for more general
use, owing mainly to the rather disorganized layout and
the lack of Naperian logarithms and other data one
might expect to find in so expensive a book. However,
it is ideal for a student who is in the initial stages of
studying mathematics and needs a comprehensive
guidance on how to use the more elementary tables, and
the few people to whom such accurate mean proportional
parts or an attractive front cover are major consider-
ations when buying mathematical tables.

G. Feltham

Applied Hydrodynamics. By H. R. Vallentine. Pp.
xi + 296. London; Butterworths, 1967. Price 50s.

This is a second edition of a book first published
in 1959 and devoted primarily to the dynamics of incom-
pressible, inviscid fluids. For those familiar with the
earlier version, the principal changes are the addition
of a chapter on vortex motion and the inclusion of the
image method of solution.

In contrast to most of the standard works on the
subject, Professor Vallentine has deliberately avoided
the elegance of the classical development of the theory
and the power of the vector notation. Instead he has
concentrated on a presentation in the most fundamental
form; using the simplest mathematics possible and seek-
ing always to emphasize the physical meaning of the
equations. In pursuit of the latter aim, over 50 pages
have been devoted solely to the derivation of the basic

equations and to the introduction of a few basic con-
cepts such as streamline, stream-function and velocity
potential. Even so, some of the equations are derived
only for two dimensional flow and the corresponding
result for three dimensions is simply quoted. One slightly
unusual but very sound feature of this section of the
book is the stressing of the importance of the stream-
function over the velocity potential because of the
greater generality of the former; the potential function
applying only to irrotational flow.

The second and third chapters, continue to establish
and extend the physical basis of the hydrodynamics.
The second follows the modern trend of introducing, at
a very early stage, the flow properties of viscous fluids
in order to show clearly the relation between real and
ideal fluid flows and to establish the limitations and
areas of applicability of the latter. It includes excep-
tionally clear descriptions of boundary layers, both
laminar and turbulent, and of boundary layer separation.
The third deals with approximate graphical, numerical
and experimental methods of solving two dimensional
steady flow problems. Of particular interest is the
section on the graphical construction of flow nets based
on the streamlines and equipotential lines introduced
in the first chapter. The method is applicable to quite
complicated problems in two dimensional flow and the
examples given are for the flow under a sluice gate, and
for the percolation of water through the soil under
a dam. In the latter case the equations for percolating
flow are shown to be identical to the irrotational ideal
fluid equations. The example shows how easily the total
leakage rate under the dam, and the upthrust of the
water on it may be calculated (in the idealized two-
dimensional case). The experimental methods discussed
are the membrane, electrical and viscous flow methods.

The remaining five chapters cover most of the con-
ventional analytical methods of solution and patterns of
flow, for two dimensional problems. Sources and sinks,
irrotational vortices, doublets and conformal transfor-
mations (including the Joukowski transformation and
the Schwartz-Christoffel theorem) are described and
many examples are given. Simple three dimensional axi-
symmetric flows and the motions of vortex streets,
sheets and rings are also discussed. The three most
noticeable omissions are the solution of Laplace's equa-
tion by the separation of variables, solutions by Green's
functions and descriptions of incompressible wave
motions. These subjects however could really only be
included in a rather larger book.

The volume is described in its preface as being
intended as an introduction to hydrodynamics for
students of applied mathematics and a course in fluid
mechanics for post-graduate engineers. These claims
seem a little ambitious for the book is rather lacking
in depth. Mathematics is abandoned for physical des-
criptions whenever the mathematical going becomes at
all rough. In fact the standard of mathematics required
throughout is deliberately minimal, a working know-
ledge of calculus being the principal requirement. Even
complex numbers are introduced and described at some
length.

All in all, the clear and well illustrated descriptions
of why fluids behave as they do, make the book a
useful companion to the standard, rather more mathe-
matical and comprehensive texts. At 50s. however it
does seem rather expensive.

A. N. Hicks

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Names of authors appear in **bold** type

Personal items are shown in *italics*

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NOTICE TO READERS

The Editor extends to all readers of the JOURNAL a cordial invitation to contribute articles of R.N.S.S., Naval or general scientific and technical interest.

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> H. L. R. Hinkley, Editor,
>> Journal of the Royal Naval Scientific Service,
>>> Ministry of Defence,
>>>> Station Square House,
>>>>> St. Mary Cray, Orpington, Kent.

Telephone Orpington 32111 Ext 258

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