From Science To Applications

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ABSTRACT

This paper argues that there is now a gap between the level of maturity of much research into experimental radar techniques and the level which is necessary for a technique to be used in an operational system.

Some possible causes for the emergence of this gap are discussed as part of the argument that the gap is real.

The paper argues that the principal requirements which must be met before a technique can be considered suitable for operational use are first that the performance must be predictable in all the scenarios in which the system will be used, and also that it must provide a real, cost effective, operational advantage over current technology.

1 INTRODUCTION

The course of radar development has always been characterised by the application of new techniques, for example novel microwave hardware, antenna designs, signal processing schemes and analogue or digital processing hardware. In the last decades, however, the continued advance in radar theory combined with decreasing budgets for new developments and procurements has meant that a whole range of techniques have been studied using mathematical analysis, software simulation and basic experiments, often for many years, in research laboratories and in universities, and it is not until many years later, if at all, that they become incorporated into the designs of operational systems. Other evidence which also supports this belief is obtained from the observation that a lot of work is published to demonstrate basic principles, but much less in published about the experiments which are required to prove that a system is suitable for operational use.

This is leading to a gap between the stage at which the technique is believed by the research community to have been well-proven, and the higher degree of proving which is required for the risk of incorporating it into a system design to be low enough to be acceptable.

Whilst in the earlier days of radar developments the link between the research, development and user communities was short and direct, since it has now become longer, it seems sensible to recognise explicitly that moving from first principles to detailed analysis is a step which needs to be made along the process of maturing the technique and describe how it must happen. The aim of this paper is therefore to point out what is necessary for an experimental radar technique, of which Passive Radar is a good example, to come to be used in an operational system.

1.1 Published Work Suggests a Lack of Interest in Development

One reason why publications seem to concentrate on ‘first principles’ may be that many conferences, and journals’ judgement of the quality of potential contributions require novelty as a *sine qua non* without which publication is impossible, and this is easier to demonstrate with the first publication of a principle than with its elaboration into something which can be offered for sale to customers. Another reason for the emphasis on ‘first demonstration’ may be that companies and government establishments are more reluctant to allow the details of the implementation of a system to be published than they are for its basic principles. A third reason may be that those who work on the detailed development may be less keen to publish their work than those whose speciality is more at the 'research' end. Yet a fourth reason is that early-stage research is more often carried out by people at universities, who have a great incentive to publish in order to further their careers, a motivation which although shared in part by many of those who work in government establishments, is much less common in industry.

The reasons listed above may be discussed further, but are not the subject of this paper, which is motivated rather by the possibility that one reason for this apparent emphasis on early-stage research is a lack of understanding within the 'Research and Development' community of what is required for a concept to become 'marketable' within an operational system.

Although this paper uses 'marketable' and similar adjectives to indicate a level of maturity, the requirements which are discussed are also applicable in many cases to other procurement models. The exceptions probably relate to those cases where the risk of the system not performing as anticipated is underwritten by the customer because the potential benefits are so great. Although this is now rare it is true that a government can accept a more 'open ended' risk, if the potential benefits are great enough, although someone still has to (or ought to) make a decision as to whether the degree of risk in going ahead would a project is balanced by the potential benefits.

Although much of the argument made in this paper is very generic, it is really intended to discuss the development of radar systems and the subject of this meeting, Passive Radars, in particular. It is also timely that the paper should consider particularly those passive radars using communication and broadcast transmissions as these are the subject of much current research and are at the stage where the basic research and demonstrations have largely been performed and the next stage has to be the serious, careful, development work.

1.2 Difficulties of Implementing Demonstrations with Passive Radars

With radars in particular, yet another reason why early-stage research may be much more common that more extensive technology demonstrations is that the latter are much more expensive. This makes 'research' at a system level much harder than at the level of a 'proof of principle,' which in turn means that moving to a next stage of 'realism' often requires the developer to convince sceptical funding sources to support the next stage, so it is often easier, or necessary if the funding is refused, to go back and instead research the next idea.

This problem of the cost of demonstrators is particularly severe for airborne radars, in part because of the difficulties of certifying equipment even for experimental flight, and in part because of the logistical difficulties of organizing the trials, especially if cooperative moving targets are needed.

These issues, however, are mitigated for passive bistatic radars by the fact that the developers do not need to provide a transmitter. For many such systems the simplicity of receiver antennas also helps - at lower frequencies these can comprise a small number of elements and also avoid the need for mechanical scanning.

1.2.1. Simulation is not Enough

It could be argued that these days simulation tools are sufficiently sophisticated and comprehensive that all that is required to reduce the risk of a new development to an acceptable level is to prove by simulation that it will work. Unfortunately a simulation is only as good as the data going into it, and may not pick up any erroneous assumptions unless they make the simulation internally inconsistent. This is why 'real' demonstrations are required - effectively to prove that nothing significant has been left out of the simulation. It is also important that this is done at 'full scale' because sub-scale simulations may not be subject to limitations which come into effect when the full system performance is required. A good example of this for any radar using continuous wave (CW) signals, including passive radars, is the need to be able to handle the transmitted signal within the receiver, as well as the signals reflected from the targets. This may limit the dynamic range, but such a limitation may not be detectable if reduced sensitivity is adequate for an initial demonstration. On the other hand, it is a good idea to 'start off small' and build up the capability of the system through a series of increasingly 'realistic' demonstrations, so that any effects which limit the performance can be encountered, and hopefully solved, one by one, instead of all appearing at once, in which case it can become almost impossible to distinguish the different effects which limit the performance.

1.3 Requirements for Marketability

The paper will argue that in order to be marketable, a new technique must meet the following criteria

- It must be able to be implemented with current hardware.
- It must work not only sometimes, but essentially always.
- It must do something better than existing systems do it.
- It must meet a real need.
- There must be a market for it now.

Each of these points is expanded in the subsequent sections of this paper.

Most modern design, procurement and indeed tendering processes require that any system concept has to pass a series of 'gates,' within both the customer's procurement process and within the supplier's tendering process. At these 'gates' the viability of the use of novel techniques should be tested. The aspects tested and the questions asked will be dependent on the actual requirements, but this paper explores the more general principles.

On the positive side, it should be the case that if these requirements can be met, it would be possible to persuade industry's development managers and governments' procurement managers to develop and procure the system. This is worth noting because it appears that another factor which increases the gap between experiment and operation is an increasing reluctance to accept the risk of fielding new technologies. This is in part due to the increasing pressures to minimise development costs and also to the fact that as in-service radars become capable, the benefits offered by new technology must be more compelling in order for it to be adopted.

2 NOVEL HARDWARE REQUIREMENTS

The most obvious requirement is that the equipment using the new technique must be manufacturable. It is commonly recognised that the processing algorithms, which can in principle be as complex as the engineer's mind can imagine, must actually be able to be implemented in hardware, the complexity of which is compatible with the expected cost and complexity of the whole system.

Some more subtle points which can affect a number of proposed systems, but which are not always appreciated, include:

- it must also be possible to get all the data where it can be processed at the right time. Algorithms which allow the data to be partitioned tend to be more practical than those where an operation requires access to all the data.
- the design must not make excessive demands on the stability of the analogue components and
- all the required analogue components must be able to be procured.

This last can sometimes pose a particularly subtle trap. For a basic experiment some components (for example protection of the receiver against other high-powered emitters) may be neglected. Other components may be 'dummied out' for example fixed filters may be used instead of tuneable ones, or 'one off' components may be made in the laboratory, but before the system is practical, one must be sure that the necessary components are actually procurable.

Clearly, the example of receiver protection should not need to apply to systems using broadcast or civil communications transmissions, as it is in the nature of these systems that they can be handled by low-cost consumer receivers without expensive receiver protection. On the other hand, the development engineer has to consider whether such receivers, serviceable and reliable as they may be, formally meet military Electro-Magnetic Compatibility (EMC) considerations. They must also, however, consider whether by using larger antenna apertures, however, they are increasing the dangers of overload.

More relevant to passive radars, especially those using modulated CW signals of opportunity, is the need to maintain a high dynamic range in the receiver. For an experimental system the dynamic range has to be adequate to see targets in a ‘typical’ environment of ‘wanted’ and ‘unwanted’ signals, but a fully-developed system must have enough dynamic range to have the sensitivity to compete with alternative system solutions, in the most unfavourable electromagnetic environments and at extremes of temperature. Whilst it is my judgement that this will still not be an excessively-difficult requirement to meet, that judgement is based more on the performance of military communications intercept receivers than on experience with the Commercial-Off-The-Shelf (COTS) receivers which are often used for experimental passive radar systems.

The use of such receivers in airborne applications poses a further issue because the ‘typical’ direct signal levels seem to rise by about 20dB as soon as one is airborne and thus has direct line of sight to the more powerful broadcast transmitters.

3 PREDICTABILITY

For the argument being presented in this paper, the requirement that a new system must work “essentially always” is not an issue of the reliability of the hardware or of the software, but rather one of whether the system concept is robust against different environments of targets, clutter, interference etc.

The issue of whether a technique is really reliable is in fact a classic example of step from a research experiment to a marketable product. At the opposite extreme from what is required, there is the familiar phenomenon is of the algorithm which only works when applied to the data set on which it was first developed. At a system level, and this is quite common particularly with passive radar demonstrations, there is the issue of the analyses which concentrate on the targets which have been detected but are silent about those which should have been detected but were not. References [1] and [2] are indeed examples of this, but it is firmly intended to extend the analysis, along the lines outlined below, as it is recognised that this is a necessary next step in the maturation of the technique.

3.1 Assessing the Reliability of a Technique

Proving that a target can be detected is indeed an important first step to understanding the performance, but for many types of system that is only a first step. It allows us to go on to define the boundaries between when the system can detect targets and when it cannot, and then to compare the measured results with what theory predicts. For example, the detection performance of a radar is of course statistical and subject to a large number of variables, but the process of evaluating a system’s performance at the research stage is very similar to that which must be undertaken for a ‘production’ system to prove to a customer that it meets its specification [3]. However, the statistical variability of radar detection can be used as an excuse to make any apparent shortfall in performance the subject to ‘special pleading’ for a long while. Such issues will, however, have to be addressed eventually, so they should really be addressed as soon as possible. Of course, the issues which have been expressed above in terms of detection probability also apply to other information obtained from the radar, most obviously, perhaps, location accuracy.

Bistatic radars are inherently more susceptible to coverage limitations than monostatic because a target must be visible to both transmitter and receiver to be detectable. Multistatic systems, with both multiple transmitters and multiple receivers may, however, have better coverage than a single monostatic radar, although of course if multileration or multi-angulation is required to locate the target, the coverage is effectively limited to the area within which the target can be seen via enough paths to obtain the location to the required accuracy. For lower frequencies the performance is improved, at the cost of complicating the understanding because detections can also be obtained via non-line-of-sight propagation, which is not significant at microwave frequencies.

Systems using CW transmitters of opportunity are also probably more susceptible than many others to the effects of the environment because of the need to handle all the signals, from the strongest clutter, and the direct signal, to the weakest at the same time, an issue which is also made harder for many passive radars by the fact that the receivers have wide beamwidths.

While a system which has been developed to the stage where it is ‘marketable’ has to be able to work in any environment, it is not, of course, practical to trial it in all possible environments, particularly in the earlier ‘research’ phase where budgets are limited. The researchers should therefore search diligently for those anomalies which are apparent within the data which they can gather, and take pains to understand them. Although showing the best results is good for gaining publicity, showing that care has been taken to understand the anomalies is important for convincing people that the idea should be taken further.

3.2 Predictability, not Necessarily Perfection

It should be noted that we have called this requirement ‘Predictability.’ Imperfections in performance are often acceptable provided that they are understood so the users can plan how to make the best of the system. Indeed, one is reminded of Watson-Watt’s advice to choose the third best solution to a problem - since the ‘best’ solution never materialises and the ‘second best’ comes too late.

It is usually very desirable that a novel system works under at least the range of conditions where existing solutions work, but at the least it is necessary to know, and therefore be able successfully to predict to the customer, how it will behave under any circumstances, so that the user can be confident that its behaviour is predictable, and it must be possible to show that when it does not work well the consequences are operationally acceptable (and hopefully that they lead to only a negligible degree of degradation in its utility).

3.3 Customer Perception

Another qualification, however, which is worth making at this point is that the purveyor of a ‘new’ system is that the new system is really required by the customer not to work as well as the old system did, but as well as he thinks it did. This latter perception is often biased by knowledge of how to work around its weaknesses of the original system, so that 'different' may be interpreted as 'worse' through lack of familiarity.

Users may also have an overoptimistic view of the capabilities of their current equipment. This can arise because either the sensor is used in scenarios where no ‘ground truth’ is available to the operators so they cannot ‘calibrate’ the performance of the system, or else they know the ‘ground truth’ before they use the sensor, which can influence the way they interpret its output. Both these cases can lead to an over-estimate of the sensor’s performance, whereas the less optimistic, but objective, approach of comparing the sensor’s performance with the ground truth after the measurements have been made is used only when judging the new system.

3.4 Summary of What is Needed

In summary, we can propose a table like that shown below as an example of the sort of the information which would be required to be available in order to claim that the performance of a system is understood.

Criterion	Short Range (0..x km)		Long Range (x..y km)	
	Large	Small	Large	Small
Expected Plots (based on 'Air Truth')	..	..	..	..
Percentage Seen	..	..	..	..
Percentage Hidden by Terrain	..	..	..	..
Percentage Mislocated	..	..	..	..
Percentage Missing	..	..	..	..
TOTAL	100%	100%	100%	100%
Targets seen but missed by Air Truth	..	..	..	..
False Alarms	..	..	..	..
r.m.s. location accuracy	..	..	..	..
Location accuracy within 95% probability	..	..	..	..

Table 1: Typical Data Needed to Characterize a Radar System

A full system specification will, of course, also require a lot of additional information, but if this is available we will have gone a long way towards obtaining a complete understanding of the system's performance.

4 NEED TO IMPROVE ON EXISTING CAPABILITIES

There are always overheads in employing a new technology, such as:

- the most obvious overhead is the financial overhead in development costs and in early-life support costs since a lot of the experience gained with the previous design has to be 'thrown away' and be reacquired for the new one. Another way to look at this is from the point of view of the well-known 'learning curve,' where starting with a new technology means that the manufacturer has to start again at the top of the curve.
- Another overhead is the need to convince the customer that the new system is indeed in some sense 'better' than the previous solutions to his problem. This is accompanied by a commercial risk that, maybe for rational reasons or maybe from prejudice, the customer will remain unconvinced and instead procure the 'older' solution from a competitor.
- Another overhead which must, in practice, be taken into account is often a fear of novelty within the supplier's own organization, beyond the 'rational' issues raised above, which would make him rather stick with 'the devil he knows' rather than be subject to unidentifiable risks.

It follows, therefore, that there is thus no point changing a technology for the sake of it, but only if some benefit comes to the operator from its use.

It is apparent that the benefit of the new technique need not be in performance. It could, for example, be in initial cost, reliability or through life costs (for example reduced support costs or the ability for it to be used by fewer or less-skilled operators). Conversely, strengths in one of these areas should not, if at all possible, be compromised by weaknesses in other areas. In fact, a major strength in one area is often, valuably, supported by secondary improvements in other areas. It then becomes important to distinguish the advantage(s) which are substantial enough in themselves to justify the introduction of the new technique from those secondary advantages which would not justify introducing, but which are still considered benefits which can be enjoyed once the new technique is chosen for other more compelling reasons.

In the case of Passive Bistatic Radar the primary benefit appears to be the fact that it can operate without requiring an allocation of spectrum. The fact that it avoids the cost, weight, power consumption, etc. of a transmitter seems to emerging as another useful benefit, one which is secondary to the spectral efficiency. Other capabilities, such as the anti-stealth capabilities, appear at the moment to be tertiary. Whilst this ranking may change with the emergence of new urgent threats the ability to operate without a frequency allocation appears at the moment to be the ‘key’ advantage.

Amongst the ‘risks’ which are perceived for Passive Bistatic Radars, the lack of control over the transmitter is often seen as a significant problem, but the reliability of broadcast transmissions is probably much better than that on many radar transmitters. In fact the ability to reliably and unambiguously trilaterate all the targets in a dense signal environment may be the more significant issue.

5 MEETING A REAL NEED

The requirement that a new system must meet a real need sounds very close to the requirement to ‘do something better’ which was discussed above. It is expressed as an additional, separate, requirement, however, because the successful innovation must not only do something well, but it must be something for which customers have a real need. Performing a novel task elegantly is not enough - this was for long a problem for passive radars, that of all their capabilities, the key developmental problem has been to find one for which he customer has a need. Although the Electronic Counter-Counter Measures (ECCM) capabilities of passive radars are seem ‘real’ they are capabilities which can be obtained in other ways, but the ability not to occupy spectrum does currently seem to be something for which there is a real need.

Whereas it was noted that the argument for the need to ‘do something better’ was driven by the increased development costs which would have to be justified, the requirement to meet a real need is perhaps a touchstone of the difference between academic and industrial research.

It can well be argued that “academic” research can justifiably study something because of its elegance alone, either from a suspicion that it will lead to something important, or because it will serve as a suitable test-bed for educational purposes or for the exploration of a new technique without adding extraneous complications in the experiments. Industrial research, however, must have an application in mind and should even at its early stages have some idea that the application is something for which there will probably be a real need. In practice this distinction is often not as clear as this. Of course a lot of “academic” research can clearly be seen to be addressed to meeting a recognised need - a “capability gap.” More confusingly, there is a trend for funding bodies to require that academic research is indeed addressed towards meeting a practical need. This trend is confusing because it leads to the development of spurious “application-oriented” justifications for what is really “pure” research.

This maybe leads towards the question of whether Engineering Research should ever be “pure” in the sense we are using it here, and, if it should not, then to the wider question of whether “Engineering” should actually be an academic discipline.

But to stay closer to our subject - it remains true that a successful product must meet a real need, and hence an important step on the way from research to application is that a real need for the application must be apparent before it is justifiable to invest in a full-scale development.

6 MARKET PULL

The need for there to be a market for the new technique is again related to the two previous points, but is a significantly distinct for it to be considered separately.

Even if potential customers recognise the superiority of the new technique, they may not be in a position to make use of it. Even if it fills an acknowledged capability gap, the available money may be needed to meet other, even more urgent, needs, or the customer may have just procured another (inferior) system to do the job and will not be able to write off that investment and start again. If one potential customer is in that position, one can, of course, look for others, but in the military radar market there are only relatively few customers and most suppliers have access only to a fraction of those. If none of the potential customers are 'buying,' then the new idea will have to wait until they are, which is disappointing but does not necessarily represent a failure of the research.

A related problem for military systems is that it is still generally difficult to sell equipment to an export customer unless the development has the support of one's own MoD.

It could be argued that, from an economic point of view, this requirement is actually the same as the previous one, in that the price which a consumer is prepared to pay for something is actually the best judgement of how much he really wants it. This latter argument is not the whole story, however, because it does not distinguish between the case where something will never give enough benefit to be procured and the case where the idea is ‘saleable,’ but just not at the current moment. In the former case, research and development should be discontinued (unless the research continues as a purely ‘academic’ exercise). In the latter case, the development should be delayed. There are, of course, three reasons why development should not be undertaken before the product can be sold:

- for the economic reason that the development costs will not be recovered until the product is sold, so if there is a delay between one event and the another, the developer is having to ‘borrow’ the development costs, and pay interest on the borrowed money, until the product is sold, and this always costs money. It depends on the financial structure of the organization as to whether this delay involves actually borrowing money from a bank or otherwise raising capital, or whether it just deprives the organization of funds which could otherwise be used to support other developments, but it is still a cost.
- There is a technical reason for delay in that if a product is developed in advance of its market it will not be able to make use of the latest developments which come available at the time when the market appears, and is thus likely to be less competitive than a product which is developed by a competitor at the ‘last minute.’
- There is also a ‘marketing’ reason for delay because the exact way in which the customer wants to use the product may change, either slightly or substantially, over time, so a product which is developed too soon is likely to be a less-good fit to the customers’ aspirations than one which is developed just before it is needed,

It can be argued, on the other side, that the development should not be left too late, or else the product will not be available when the market appears. This is a more subtle issue than it may first appear, however, since it is, or should be, a deliberate business decision as to whether one wants to be first to the market. The first seller in the market can usually demand a premium in the price, but at the cost of greater risk since he will be ‘experimenting’ with what the market is actually like. The ‘first to market’ is also driven to take a different approach to innovation than the ‘follower.’ The innovator has to answer for himself all the questions about the design, whereas the follower can make use of the solutions developed by the innovator. The innovator, however, has greater freedom in his design, allowing him some choice in what features to include in the product. The follower, however, must include at least all those features which the customer likes about the innovator’s product, and must then also include something else to give his later product a distinctive advantage.

Much of what has been written above applies to products marketed to either consumer or to ‘professional’ /military customers, but here a difference appears: to take an example of a consumer product we can see how Apple’s ‘iphone’ managed to define the ‘look and feel’ of the ‘follow on’ products. This level of influence does not appear (at least not so obviously or so significantly) with military radars, but a novel product may serve as an ‘existence proof’ that a technology is practical, which may then allow the ‘followers’ to take the risk of developing another product using a similar technique, which they might not have dared to do unless the 'innovator' had proved that it was possible. In another field of radar the PILOT navigation radar[4] may be thought to have served as an 'existence' proof for FMCW radars and to have pioneered a market for 'LPI' radars. The work done to prove that PILOT would work properly in all environments also led to the development of approaches to the analysis of radar performance, concentrating on the effects of transmitter leakage and of the simultaneous presence of returns of very different amplitudes within the receiver, which have been useful for the understanding of Passive Radars.

In the field of Passive Bistatic Radars the principal product which has played the role of innovator must be Silent Sentry[5]. As an interesting aside, it could be argued that both PILOT and Silent Sentry are products of the end of the Cold War, but PILOT seems to have become mature just a few years earlier, and may also be a cheaper product, which will thus allow the decision to procure it to be made more easily, so PILOT/SCOUT seems to have been more commercially successful than Silent Sentry, although the latter has certainly served as a proof of principle of passive radar using communications transmitters. Vera-E should also be mentioned as another proof-of-principle of about the same period for an alternative approach to the use of passive sensing to obtain air-picture infomation[6].

7 CONCLUSION

This paper has examined the principal factors which influence the decision to develop a technology into a product. It will be noted that although the 'marketability' and the related need that the product should do something that is wanted, and do it better than the alternatives, are 'necessary' requirements for a product to be successful, they are underpinned by the need for the product actually to be manufacturable.

The 'key' requirement would seem, however, to be that which has been characterised as 'Predictability,' because without this one does not really know what the 'product' is.

It is fortunate that this key requirement is one which completely in the province of the technical community. As was discussed in the body of the paper, obtaining the understanding to be able to predict how a Passive Radar design will behave in any scenario requires a modelling capability, which is currently quite practical, as much experimental data as is practical, and, to leverage the value of the costly experimental data, meticulous investigation and resolution of all the observed anomalies between the measured and modelled behaviour.

Whilst this approach is required for any radar, and presumably for any sensor, the issue is made more complex for Passive Radars by the greater sensitivity to environmental shadowing, the generally-high level of the direct signals and the need to maintain the dynamic range which can cope with the simultaneous presence of returns with widely-different power levels. Whilst obtaining predictable performance in such a case is a challenge, it is necessary and, I believe possible.

This brings us back, however, to where we started, with the observation that the published literature on Passive Radar systems still concentrates on proving 'first principles' and on the search for novelty, rather than on the detailed understanding. Academic and industrial research organizations often seem uninterested in completing their understanding, although studies with more developed systems often seem to have access to the information which would give them such an understanding, the publications arising from their work often only emphasise the 'positive' without looking at the limits of performance. Whilst this may arise from a desire not to say anything 'negative,' such an approach seems over-optimistic and scientifically naive.

As noted above, I believe that the required level of understanding will be achievable, and I do believe that it will show that Passive Radar is able to do provide capabilities which are needed and are affordable. If we can fill in the table shown in section 4.4, that would give the evidence that Passive Radars can live up to our expectations.

8 REFERENCES

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