Inevitable and some perhaps remediable lags in the highest-priority scientific intelligence.

INTERACTION IN WEAPONS R&D

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Intelligence may be thought of as having two missions in relation to military planning and weapon system development. The primary mission is to provide information on the weapon systems of potential enemies so that counterweapons may be developed. That is, if we can gather valid and timely intelligence for projections of the capabilities and vulnerabilities of the opponent's military systems, we can develop weapons capable of penetrating his defense and blunting his offense. A secondary mission is to furnish information on the foreign development of weapon system components which parallel our own designs. Here, the objective is to take advantage of their R&D to produce better weapons for ourselves, thus gaining time and reducing our own development costs.

In what follows I will examine the way these intelligence missions are accomplished, primarily in the field of strategic attack and defense systems, and how the results affect U.S. research and development. I will try to point out the difficulties in generating satisfactory intelligence hypotheses on the basis of thin data, validating them by independent checkout on the part of the several analytic groups involved, and then feeding them back to the R&D community, difficulties which make the process less than ideally effective. A few illustrative case histories will show just how it has or has not worked well and suggest where it might be tinkered with.

Time and Pride

The problem is time. Too much time is usually consumed in generating and validating intelligence on weapon R&D to feed it back into U.S. planning effectively. True, the weapon-counterweapon process is a sequential one, so that limited time lags in the feedback loop can be

[1] Adapted from the author's presentation before the September 1966 Intelligence Methods Conference [redacted]

tolerated. Intelligence output, however, is usually much too late to cause the initiation of counterweapon developmental programs or to affect them directly at their initiation. While it may be frustrating to proponents of so-called interaction analyses, a look at the decision dates for some of our existing systems proves that almost all strategic counterweapons reflect recent advances in the state of the art rather than reaction to a specific opposing system. They are designed basically against hypothetical threats founded upon broad assumptions as to the capabilities of the adversary. Intelligence findings are more likely to be used to refine developmental programs already well under way, or sometimes as grounds for eliminating alternative lines of development previously undertaken to "cover all bets."

With respect to the secondary intelligence objective, our parallel weapons development program has benefited only to a trivial extent from intelligence because of the truly crippling effect of the time lag coupled with a fundamental human barrier—"Thanks, but we are doing it better." Generally, two systems being developed in parallel by two countries, given about equal capabilities in both, are likely to mature at the same rate. By the time Country A has discovered that some feature of Country B's system is superior to the corresponding one in its own, it has paid out its development money and concludes that it cannot make changes. Convincing researchers that someone else has solved a problem in a better way is not easy; there is frequently the necessity to overcome an inborn chauvinism, or at least pride of invention, which blunts the feedback mechanism.

Collection Difficulties

The first problem is to collect the intelligence. Ideally, we would like to know as soon as research and development starts on a new weapon system. The fact is, however, that information is inadequate on Soviet military R&D programs and almost totally nonexistent on Communist Chinese programs. While general information showing large increases in Soviet technical capability is available, as well as evidence of an expanding scope of military R&D, specifics are too fragmentary to allow a precise definition of the future threat. Basic research can be followed in some detail through analysis of scientific papers published in the technical journals, but the possible final uses of the research in terms of weapon systems are many and nonspecific, and only trends can be detected. The end result is a lack of sufficient accurate intelligence on R&D projects in their early stages.

In the face of these uncertainties, we usually credit the Soviets with the ability to solve many scientific or technological problems without specific evidence of this ability. This may often be reasonable; the Soviets have the advantage of getting much information on Western advances, and their progress may well parallel ours in many instances. Nevertheless, there is a tendency to estimate greater Soviet progress as the West becomes more certain of success in the solution of corresponding scientific problems. Our assessments of the status of specific R&D programs thus may be uncertain in the near term and increasingly tenuous as estimates are projected into the future. The loss of lead time for U.S. planning of counterweapon development and an inability to present unanticipated alternatives to the U.S. R&D community is a serious consequence of these perpetual intelligence problems.

Once the adversary has made a decision to capitalize on the results of basic research and build a specific new weapon system, he goes underground with the entire process of preliminary design, laboratory test, and prototype construction up to flight (or other equivalent final) test, which is therefore secure from most forms of intelligence collection. Indeed, a study of the technical literature to see when reporting on some particular area of basic research suddenly stops is advocated as a way of learning that a military development program has been started in that area. Proving an exclusion theorem is always a difficult matter, however, and even its proof would hardly be a clear signal as to exactly what was going on.

In the development of some key weapons the final flight test phase is open to intelligence collection, but this is likely to take place three to five years after the start of preliminary design. This is the lag for long-range ballistic missiles and space systems. ABM systems, on the other hand, could well be flight-tested over a period of years without our knowledge.

Analysis, Consensus, Feedback

Let us assume that in spite of these difficulties we have finally collected a body of raw data on a new weapon system. The next step, the analysis of the data, is likely to consume a large amount of time because it is a difficult process. This is particularly true when the opponent has developed a system fundamentally different from our home-grown variety. There is generally a lack of background data for the intelligence findings and a reluctance on the part of analysts and consultants to accept the idea that someone else was able to solve the problem differently from our solution. A short-circuit sometimes removes this obstacle when actual hardware is collected, but unfortunately this happens only very rarely, and when it does it seldom involves a major weapons system.

After the analytic efforts seem to have generated satisfactory hypothetical conclusions, we still need to validate these by having independent researchers check them out. The intelligence community needs to reach a consensus on their validity. Different analysts working separately on the same data are quite likely to come up with differing answers, particularly when the data base is thin to begin with. While we would all probably agree on the desirability of having more than a single individual or a single group take a crack at the problem, we would also hope that they communicate with each other at frequent intervals. I am encouraged by what seems to be a better rapport nowadays among the various analytical groups in the community and a greater speed in coming to an agreed conclusion than heretofore. Even so, the validation process still takes a significant length of time, in some instances as long or longer than the analysis process itself.

Now let us engage the problem of feedback. There are many channels through which this takes place. The CIA and DIA put out reports and estimates to consumers on a regular basis, many of which contain details of weapon system developments. These agencies also routinely brief officials in Defense, NASA, the Bureau of the Budget, and the White House, along with many other decision-making elements of the government. There are a number of key members of the scientific establishment, both industrial and academic, who participate in a variety of government-sponsored panels and committees and receive intelligence briefings in their fields of cognizance. These people, however, are at the policy and top management level; it is likely that little direct intelligence-derived guidance filters through them to the laboratories.

Security restrictions inhibit wide dissemination of intelligence down to the scientists and engineers at the design level, but perhaps little is lost, as the briefings at the policy level are seldom detailed. They are usually condensed to a point that a consumer would have difficulty detecting, say, a significant breakthrough in the development of a subsystem. For instance, we are prone to tell policy-level audiences that a missile has a particular accuracy but are less likely to tell them why. Finally, even if we could get around the security restrictions in our contacts with industry, we still need to be circumspect. In our capitalistic society we lay ourselves open to severe criticism if we provide information to industry in a way that gives one company a competitive advantage over others.

Case History: ICBM Guidance

Having reached the end of the process thus still beset with difficulties, I would now like to summarize briefly some case histories which illustrate them. The first one is of parallel development work in the United States and the USSR on the same problem, the guidance system for an ICBM.

Solving this problem is one of the most difficult ingredients of an ICBM development program. It is clear now that we and the Soviets started in parallel, in about 1955, and arrived at strikingly different solutions. The U.S. systems use a simple rocket engine operating at a fixed thrust but couple with this a very complex guidance system containing an airborne digital computer. During powered flight the guidance system continuously computes the proper burnout position for the rocket so that the warhead will impact acceptably close to the target. The Soviets, on the other hand, fly their missiles on a precalculated trajectory with a somewhat more elaborate variable-thrust engine but a very much simpler guidance system having virtually no on-board computation capability.

Both systems work fine; but we would probably now admit that the advantages inherent in the Soviet system, simplicity and quick achievement of satisfactory reliability, would have been very appealing to us had we considered such a system during the design phase. The fact is that we did not begin to collect useful flight test data on the Soviet system until 1959, that we did not really perform enough analysis work to understand it until 1963, and that the results of this analysis did not begin to be disseminated outside the intelligence community until 1965. By this time, of course, the U.S. ICBM guidance design had long since been completed. It may be said in all candor that this particular intelligence program had no effect at all on parallel systems at home.

An interesting sidelight illustrative of our problems is that analysis of one aspect of the Soviet data showed that whereas all U.S. missiles use rate gyros or angular accelerometers to keep the control system stable, the Soviets devised a much simpler system that dispenses with these fragile and expensive instruments. The analyst who made this determination was successful in getting a Soviet-style system incorporated into a proposal to NASA for a new launch vehicle—only to have the feature rejected by that agency with the comment that although the scheme looked quite feasible, there hadn't been enough test experience to justify its adoption. The NASA people were not aware of the fact that the Soviets had been testing it for many years because they had never received feedback in the detail required and at the right level in their organization.

ABM Radar

An interesting case history in the weapon-counterweapon field is the intelligence analysis of a large Soviet radar and its effect on the U.S. program for developing penetration aids. When thought first turned in the United States to the problem of penetrating Soviet radar defenses with ICBM warheads, there immediately arose the question of the operating characteristics of these radars. About the only contribution intelligence could make were some excellent U-2 photographs taken in 1960 of some very large radars in the Sary Shagan area which seemed to be logical candidates for the role of spotting ballistic missiles as ABM targets.

Unfortunately, deriving a radar's characteristics from a photograph of it is rather difficult. Various analytical groups looking at the same photos came up with different answers. The consensus seemed to be that the largest radar—nicknamed Hen House—was the keystone of the defensive system, and that it would operate in a UHF frequency band, at about 1,000 MHz; after all, our developers were working on radars in the 1,000-2,000 MHz region in our equivalent programs at the time. This was the word that went out to U.S. agencies engaged in devising penetration schemes.

Then in 1962 the Russians tested some nuclear warheads at high altitudes in the Sary Shagan region. Nature was kind, and we received radar signals reflected from the ionized cloud. These included a new train of signals in the much lower VHF band, which it soon developed must have come from a very high-power radar in the Sary Shagan area.

For a long time, however, there was a singular lack of interest in the peculiarity of these signals. It may be that too many analysts had already decided that the Hen House frequency would be much higher. It seems they had convinced themselves that there was no merit in a long-range radar operating at VHF because such a radar would be blacked out by a nuclear cloud. Nevertheless, analysis efforts did go forward, and two things became increasingly clear: first, the VHF signal characteristics were such as to prove that they could have emanated only from a radar with the physical dimensions and orientation of Hen House; and second, that our work on nuclear blackout effects was based on tests in the tropics, whereas the Soviets were likely to deploy radars such as these in the Arctic, where the blackout problem was not nearly so severe.[2]

A final impediment to the fast functioning of the system was the fact that the validation step took an unconscionably long time. Although by late 1964 most of the various analytical groups had arrived at the correct solution, there was one that was still unconvinced, and its opposition prevented the firm feedback of this information to the R&D community. As a result, another two years went by before the message got back to the laboratories in an effective way.

We are now finally on the tracks. A serious review of our offensive capabilities against VHF radars is now under way. A VHF radar has been built at White Sands to allow us to see what the Soviet radars will see. Our own Nike ABM program is at the same time looking at ways of using VHF radars for our defensive systems. However, I would not begin to estimate the millions of dollars which could have been saved by a more speedy functioning of the intelligence process.

Outlook

To recapitulate, I have presented so far a rather pessimistic picture of our capability to influence weapons research and development, and I have given two examples illustrating how the long times needed to collect, analyze, validate, and feed back information serve to make our efforts ineffective. Ideally, this would now be the proper place to unveil a blueprint for the future, in which clear solutions to these problems are presented.

I have no such blueprint. I know of no way to collect intelligence on weapon systems at the early R&D phase except by clandestine deep

[2] For a more detailed account of this case history see the next following article in this issue.

penetrations of the adversary's laboratories and design bureaus. Those responsible for clandestine operations know this as well as we on the technical side do, and presumably they are doing the best they can. Similarly, I see no easy way of speeding up the analysis process. Massive infusions of people or computing machines are not likely to expedite the process. Key analytic breakthroughs will generally come from the application of brainpower by a few gifted and dedicated individuals working at their own pace.

The last two steps in the process, validation and feedback, seem to me to be the places where we should focus our attention. The effectiveness of these steps depends wholly on how we structure the setup so as to allow people to communicate and interact with one another. Perhaps it is time to take a fresh look at the mechanisms by which intelligence information is provided to researchers, particularly in the final phases. This, it seems to me, is an area where no new inventions are required, no multimillion dollar expenditures are called for, and yet where there is promise both of improving our defensive and offensive capabilities and of saving millions of dollars.