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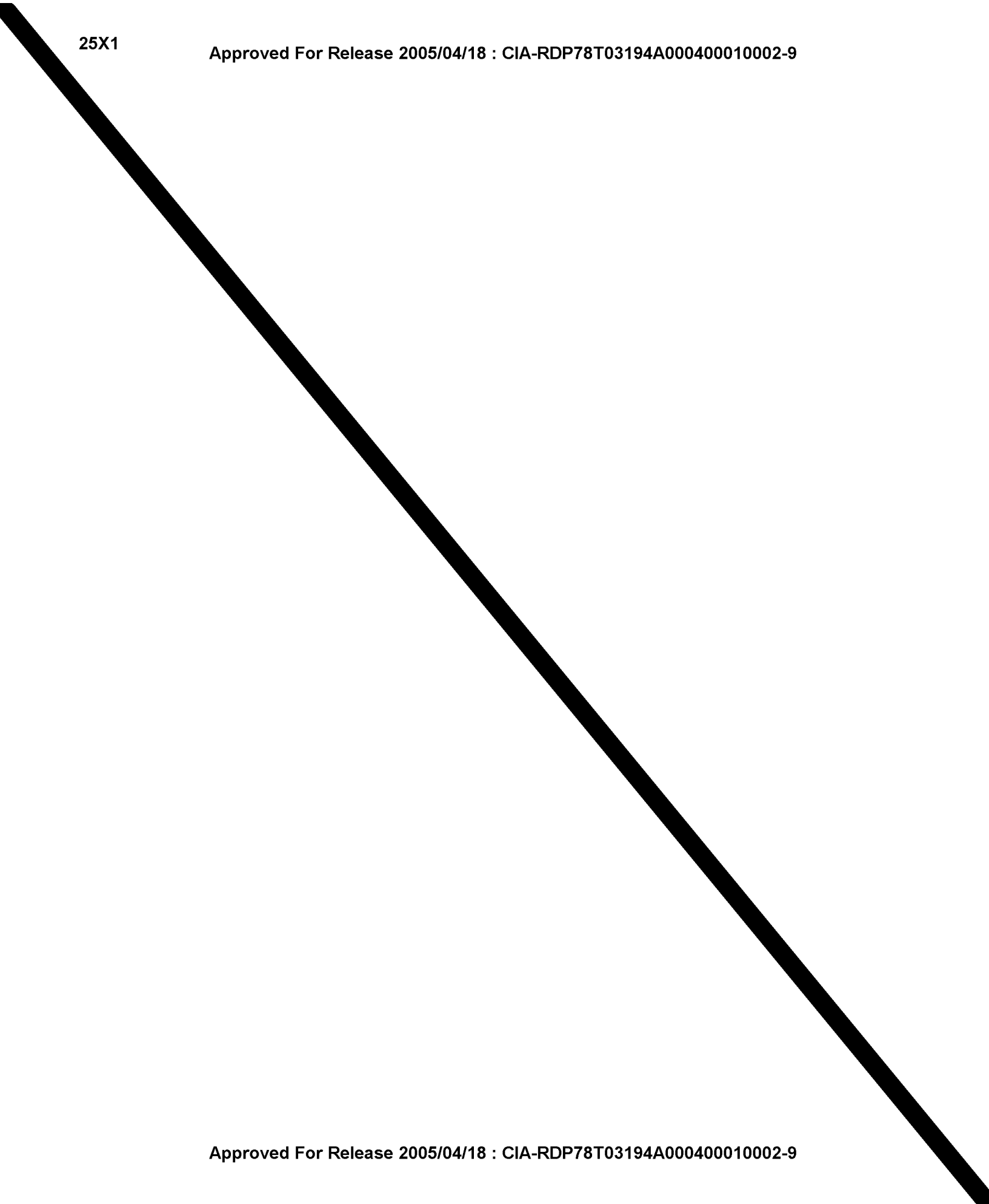

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This issue of

STUDIES IN INTELLIGENCE

is dedicated to

RICHARD HELMS

Director of Central Intelligence
30 June 1966 – 2 February 1973

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RICHARD HELMS

A unique involvement of intelligence with policy-making

STRATEGIC ARMS LIMITATION AND INTELLIGENCE

Richard Helms*

Several of my senior associates will be joining you next Monday to discuss CIA, what its role is, and how it relates to the rest of the intelligence community. In my own appearance here, I will try to give you an appreciation for our work by describing one of our major intelligence problems and how we try to cope with it in practice. I hope that our two visits will give you a full picture of what we do and persuade you, when you return to your own departments, that our efforts are worthy of your cooperation and support.

The problem I'd like to examine today is one which has been with us on and off for almost two decades. Since 1969, however, it has grown so rapidly in importance and urgency that it now is one of our foremost continuing concerns. This is the problem of the Strategic Arms Limitation Talks, commonly shortened to SALT.

It will be immediately obvious to you that intelligence has major roles to play in this matter. We are responsible for defining the Soviet strategic capabilities which are to be limited in any treaty. After any agreement is signed, we will be even more involved in continually monitoring whether the Soviets are observing those limits. Beyond that, the subject has a further interest for intelligence professionals. It illustrates an involvement of intelligence with policy-making which—in its thoroughness, its intensity, and its duration—is in my experience unique. All right-minded men subscribe to the theory that sound intelligence should be one of the fundamental bases of foreign policy, one of the starting points in the policy-making process. The unusual thing about SALT is that the process is truly working that way. And this leads to some problems for the intelligence officer which I will touch upon in a few moments.

Despite endless lip service from all sides, arms control has made precious little progress in this century. One of the key roadblocks has been finding a reliable way to monitor any agreement. The issue is usually referred to as that of verification, although "monitoring" is a more precise term. In brief, we have insisted that any agreement must contain built-in ways of making sure, on a continuing basis, that the Soviets are living up to it. Clearly the preferred way would be to have the right to visit and inspect any facility which we suspected was in violation. But they on their side have refused, very firmly, to permit on-site inspection of a kind we would regard as useful. And so there the matter has rested, by and large, until we could develop means which would satisfy our concerns about possible cheating without running afoul of their objections to foreign inspectors on Soviet soil. In other words, an agreement as wide-ranging as the one contemplated at SALT has had to await the advent of a reliable, repeatable means of verification from outside the USSR.

This brings me into an area in which I must tread with the greatest care. I am talking, of course, about satellite reconnaissance. Everyone knows that this activity is going on. And yet we still go to considerable lengths—and endure

*This is the text of the DCI's address to the National War College on 13 October 1971.

considerable inconvenience—to maintain a security barrier around it. There are two excellent reasons for this. One is that certain details of the program still must be kept from the Soviets if it is to remain fully effective. The second is that the Soviets themselves are very anxious that it not be discussed. They are aware of what we are doing, although not of the extent of our success, and they have a vigorous program of their own. In fact, last year* they launched about three times as many reconnaissance satellites as we did. But they have made it clear that they are unwilling to agree *explicitly* to anything which would appear to some as an infringement of territorial sovereignty, a matter on which they are extremely sensitive. So we draw no more attention than is necessary to this activity. If a treaty is finally achieved, you will find this point covered in language like "national technical means of verification, operating within the generally accepted principles of international law." There will be no misunderstanding between Washington and Moscow about what is meant. But we'll avoid a lot of problems by saying it that way.

Since the development of this capability has been so crucial in bringing about the possibility of a major arms control treaty, let me give you a few benchmarks in the program. We did not await the end of the U-2 flights over the USSR before starting on a successor. In the mid-1950s, not long after the propulsion breakthrough which led to the Atlas ICBM, the go-ahead was given. Working in the closest cooperation with the Air Force, we had to break new ground in a whole variety of systems and subsystems relating to propulsion, guidance, camera performance, and command and control. The first five years were full of discouragements and setbacks, and I must say that I am tremendously impressed with the courage and perseverance of my predecessors, and the ingenuity of our contractors, in their repeated trips back to the drawing board. As a result, the first full-systems success came in 1960, almost overlapping with the last U-2 flight over Soviet territory. Since then, reliability has become excellent. The performance of the system, as well as the quality of the product, has dramatically improved. It has come to embrace electronic, infrared, and other kinds of intelligence in addition to imagery. We have reached the point where we can give to the President some definite assurances about just what sort of treaty provisions we can and cannot monitor with confidence.

And may I remark that, as an old hand in an Agency which is often accused of housing inveterate Cold Warriors, I will be extremely gratified when the day comes, as I think it will, when real limits can be placed on the arms race on the basis of this work of ours.

This possibility began to take on some reality in the summer of 1968, when the United States and the USSR jointly announced their intention to begin talks on reducing both offensive and defensive strategic weapons. In the next month, however, the Soviets invaded Czechoslovakia, and President Johnson had no possibility of taking up negotiations before he left office in the following January. This hiatus was extended when President Nixon decided that the government had not really done all its homework thoroughly, and that we were not adequately prepared for true negotiations with the USSR. Some of my people, I recall, were reluctant to accept this at the time. But when they went back over the ground in detail—and particularly when they saw the sorts of problems which actually emerged once we began talks with the Soviets in November 1969—they were frank to admit that not enough had been done.

The way in which President Nixon's administration addressed this task has been dubbed the "building block approach." As a method, it foresees prolonged

*In 1970.

negotiations, for which it will not suffice simply to construct a U.S. position and then try to get the Soviets to buy it. Instead, we have taken each strategic weapons system in isolation. For example, we took ICBMs or ABMs, and explored all the issues that would be involved in their limitation. This involves, in the first instance, defining what limitations we could verify unilaterally. These building blocks are then combined in various alternative models, which are examined from the standpoint, not only of overall confidence in our ability to verify, but also of the impact on the strategic posture of both sides.

It will be evident that this way of going about it involves a lot more work. We have to cover the waterfront. In the process we have studied many subjects which clearly are not going to be in any agreement reached in the foreseeable future. But at the same time we have clarified a great many uncertainties, and many of our results, though not relevant to the present phase of the talks, may well become so in the future.

When I say "we," I'm referring to a considerable mechanism which has been created to prepare for the negotiations and oversee them once they start. It will surprise none of you to learn that this is done by an inter-agency committee. This group* is chaired by Dr. Kissinger and includes Secretary Irwin from State, Secretary Packard of Defense, Admiral Moorer of the JCS, Philip Farley of ACDA, and myself for CIA. Its name is the SALT Verification Panel, which testifies to the priority given to this concern in formulating our position. Its job is to produce background studies and provide the National Security Council with a set of options from which the U.S. position is finally evolved. Naturally, it has spawned lesser bodies where the work is done, notably the Verification Working Group and the Backstopping Committee, on which all the same departments sit. These groups have been in operation for over two years now, and the end is not in sight.

This brings me to the concern which I touched upon earlier. Frankly, I am made a little uneasy when large numbers of our officers find themselves working, week after week and now year after year, as members of inter-agency groups which are heavily concerned with policy-making. Make no mistake about it, there are plenty of hot policy fights in these groups. The structure of the Executive Branch guarantees that this will be so. ACDA's mission, for example, is to prepare and negotiate arms control treaties, and they need people with a commitment to that objective if they are to do their job effectively. The Pentagon's mission is to make sure that the nation is militarily as secure as it can be, and this encourages a different perspective. In some ways it is an adversary system, and the hope is that out of it shall come one final position which best satisfies all the elements, not just of the bureaucracy, but of the national interest.

But when departmental missions lead to something with elements of an adversary system, CIA is definitely *not* meant to be one of those elements. The Agency as an institution is neither "for" nor "against" an arms control treaty. I make sure that all our officers understand that they are not to involve themselves in this kind of position-taking, which lies outside the purview of intelligence. It is absolutely crucial for us that none of the policy-making departments should have any reason to doubt the objectivity of the intelligence input. There must never be any grounds for suspicion that intelligence is bending its conclusions to suit some policy preference. If we ever lose our reputation for honesty in this matter, we lose all our usefulness along with it.

*As of October 1971.

I said a minute ago that I had some uneasiness on this score. It is not because anyone has ever challenged our objectivity, or hinted at suspicions about it. But this long and intense involvement with policy makers is unusual for us, and I simply feel obliged to worry that one or another of our people will get so deeply embroiled in the *intelligence* angles of some particular controversy that he will forget himself and step over the line into the *policy* aspects of the fight. It is a matter of maintaining professional discipline against the inherent temptations of human nature. I am confident that we have stayed clean so far, and I mean to ensure that we continue to stay clean.

Let me give an example. The Soviets have a defensive missile system which we label the SA-5. Everyone agrees that it is an effective system against aircraft. Some believe that it has capabilities against ballistic missiles too, or that it could be upgraded to acquire such capabilities. Obviously, this has a lot to do with the U.S. position on ABM limits. If the SA-5 has no real value or potential in the ABM role, we need not worry about it in drafting limits on ABM systems. If it does, then ABM limits must be accompanied by some kind of controls on the SA-5.

Clearly, we have a major input to make, as an intelligence agency, on the facts of the matter. It is also clear to us that it is natural for the policy-making departments to divide on this issue—according to their hopes and fears—and to derive conflicting recommendations about the U.S. negotiating position from it. We cannot remain innocently ignorant of these implications. What we can do is remain steadfastly indifferent to them, stick to the facts, share the facts and our reasoning about them with all concerned, give our best judgment, and leave the policy decision to others.

There is one area of policy, however, in which CIA has an inescapable responsibility. That is in reaching a finding of whether a given limitation can be monitored by our own means. CIA does not reach these findings unilaterally, but rather in conjunction with our brother departments sitting on the Verification Panel. But this matter is our special competence as intelligence officers, and our view carries corresponding weight. As to whether a given limitation is *desirable*—whether it advances U.S. interests—we let the others argue about that. But we expect to be held responsible by the President for monitoring any agreement which is reached. So we want to be very sure that the agreement is clear and precise about what is limited, that it is restricted to those areas in which we can subsequently supply assurances that the USSR is complying—or conversely that we can testify definitely to any violation.

Some examples may give a clearer idea of the factors involved here. At one end of the spectrum, we have good capabilities for observing large distinctive objects. That is to say, we can count ICBM silos and launch pads. We can count aircraft. So we can monitor an agreement which provides that thou shalt not deploy more than a stated number of these items. It would be tougher, by the way, but probably not impossible, to monitor an agreement requiring reductions in these categories.

At the other end of the range is the problem of controlling, say, what's inside an object. MIRV is the famous example. No one has yet figured out a way to determine, from 100 miles up, how many individual warheads may be inside the re-entry vehicle on top of a Soviet ICBM. We cannot precisely verify a warhead's nuclear yield, nor its accuracy, although we think our estimates are not far off. In general, the area of qualitative factors—what are called performance characteristics—is very much more difficult to monitor. It is not altogether impossible to bring these factors within the scope of an arms control agreement. But to do so would require something quite drastic. It might include a ban on all flight

testing which would freeze the state of the art at its present level. And the Soviets, who see themselves as behind in several of these areas, have made it clear that they are not now prepared to give up testing.

In between, there are a lot of problem cases. Mobile ICBMs are a case in point. After a lot of study, we have concluded that, should the USSR embark on such a program, we could detect that they had done so. And we could get some broad fix on its size. But this fix would be nothing like the precision we can obtain on fixed land-based missiles. So the verification study on this weapon system leads to the conclusion that we can either allow it within an over-all numerical total, and accept a considerable area of uncertainty about compliance, or ban it altogether. A further conclusion is that a total ban is verifiable, because there would be little point in the USSR jeopardizing the whole agreement with small violations, and we could detect large, strategically significant cheating. And lastly in cases like this we also have to supply a well-based estimate of *how soon*, after the Soviets began a forbidden program, we could catch them at it. In the case of mobile systems, our estimate is it would take us as long as a year or so.

This kind of consideration has led us into another area of work which we didn't foresee, the writing of military definitions. It's easy enough for everyone in Washington to agree that SALT should cover, for example, strategic bombers. And so that problem is solved until some smart fellow comes along and says, all right, what is a strategic bomber? Is it defined by its size? Its weight? What about range? and when that comes up, one wants to know: range from what starting point? These things finally get sorted out, and then one comes up against the Soviets and *their* definitions. Naturally, it turns out that each side has framed its definitions in ways which embrace as much of the other fellow's forces as possible, while exempting as much of his own as he can. And there are plenty of differences in force structure which leave room for this sort of game-playing. So we find ourselves in the unexpected position of composing a glossary of terms, a process which is next door to drafting treaty language. This is an uncommon role for intelligence officers, but our knowledge of Soviet weapon systems makes us natural contributors to this effort.

As veterans of the Washington bureaucracy, you will all assume, and correctly, that SALT has consumed a good many man-hours and generated quite a bit of paper. The bookshelf in our SALT vault is now over six feet long, and our commitment of personnel since January 1969 is pushing toward 100 man-years.* Obviously, the priority of the task means that we have had to devote our top-quality officers to it. Within CIA, I have chosen not to set up a large permanent mechanism for this job, on the grounds that SALT will probably be with us for a long time and has to be integrated into our regular commitments. We do have a small full-time staff of four officers, but beyond this we have made SALT a continuing priority concern of our most able people.

We also send a three-man team to the talks themselves in Helsinki and Vienna. This group provides on-the-spot expertise on verification problems and on current developments in Soviet strategic forces. It also extends intelligence support and general assistance to Ambassador Smith and the delegation. Our chief adviser at the talks is a senior Agency expert, but in keeping with the distinction between intelligence and policy-making, he is not a delegate.

One of the useful aspects of the talks is the opportunity they provide to engage a number of Soviet officials directly, on formal and informal levels, in a continuing dialogue on strategic matters. As one would expect, they practice good security. None of them has let drop any top secrets. But these contacts have

*As of October 1971.

served to clarify or confirm a few general propositions about the Soviets. For one thing, it is clear that the two countries do share a common body of strategic concepts. When we talk with them about deterrence, first and second strikes, and so on, we discover that the implications of nuclear technology have impressed themselves on the two sides in fairly similar ways. It is also clear that Moscow keeps the Soviet delegation on a very tight rein, which is consistent with our picture of how that bureaucracy works. We have also been treated to illustrations of how far the Soviets carry the concept of security compartmentation. Their delegation is very unevely informed. They have confessed that only a few of them are privy to facts about Soviet systems and programs which are well known to the entire American delegation. On the day in which Ambassador Smith set forth some details about Soviet ICBMs, eyebrows shot up on the other side of the table, and notes were busily taken.

This sort of compartmentalization is something we're quite familiar with from our work against the Soviet target. It has the sad consequence for us that almost any Soviet source we acquire will have less knowledgeability than his American official counterpart. This brings up the question of how human sources fit into our plans for monitoring a SALT agreement. There is far too big an element of luck in the agent business for me to promise the President that he can rely upon agents as an important means of verification. At the same time, however, when one turns the problem around, the Soviets can never be entirely sure that we don't have an agent placed so that he could report on cheating. And this, I think, will serve to reinforce the inhibitions upon Soviet deceit.

Cheating is of course the key problem for us—for the U.S. Government and particularly for CIA. If I could just sum up how I see it at the moment:

> The United States is determined not to agree to any limitations which it cannot, with real confidence, monitor unilaterally.
>
> The Soviets do not fight us on this. They acknowledge that any agreement would lose its validity if either side lost this ability to verify.
>
> We now have a pretty clear picture of what we can and cannot verify, that is, of what is eligible and ineligible for inclusion in a treaty.
>
> Presumably the Soviet Union will not sign any treaty which does not conform to its interests, and therefore it will have an interest in keeping it in force. Cheating would have a high risk of detection, and getting caught would be a major political setback which—they would have to recognize—might very well set off a new arms push by the United States.

But one cannot eliminate all the unknowns forever in a world of rapid technological change. With both sides continuing—perhaps even accelerating—their research and development, new weapons—or important variations in old ones—are bound to come along. In thinking about this, it has become clear that one cannot write an arms limitation treaty now, one which can be unilaterally verified, which will cover weapon systems which have yet to be invented. What about an ABM system, for example, based on lasers? I cannot promise to monitor a ban on such a system until you can tell me what it looks like.

There are two answers to this. The first, in the SALT context, is to recognize the problem, not to try to write a treaty that will stand up forever, but to make provision for a continuing dialogue, even a continuing negotiation, which can try to grapple with new technological developments as they occur. In fact, what the two delegations are seeking now is a very limited agreement, covering only a few systems, with the stated intention of proceeding on to a wider treaty later. This approach lays the groundwork for a further extension, embracing new

systems, which do not fit the categories of the initial treaty. Without such an extension, it is hard to imagine that a strategic arms treaty could remain viable for very many years, without the security of one side or the other being undermined by technological change.

The second answer, in the intelligence context, is to direct our future efforts even more vigorously toward the problem of new Soviet weapons. This means trying to anticipate them, to spot them, and to develop a capability to monitor them closely enough, and in time, to meet treaty standards. Up to now, our job has been the filling of intelligence gaps, and the tools developed for this task have turned out to have major additional benefits in the verification field. In the future, we have to consider verification as a priority in and of itself, and to look for collection techniques tailored to this particular task. We will also find that the frequency of intelligence coverage will be determined more by the requirement to monitor an agreement than by the need to fill traditional gaps. This will mean that coverage has to be regular, reliable, and I suspect, at times, more frequent than we would otherwise need.

One last point on the future. The SALT proceedings envision that, as part of any agreement, a Standing Commission would be created. In this commission, either side could raise questions about the other side's compliance. The other side could then provide explanations if it wished. This would be a sort of bilateral Verification Panel, if you will, and I would expect that our Agency would have a great deal to do with its work. In broader terms, such a Commission will be a good test of how well the two sides can get along in maintaining a stable strategic arrangement. If it works well, this will doubtless increase the chances for wider agreements in the future. But if the Soviets prove uncooperative here, we will have to think harder about entering into broader obligations with them.

Let me end on the note with which I began. This is rather new work for intelligence officers. It is immensely challenging, and has brought us into new involvements. I know that I have had to learn a great deal; I can now hold my own in a discussion of laser technology—for the first thirty seconds. It has forced us to learn how to stay very closely engaged with the policy makers, without sliding over into policy-making ourselves. It will be with us for a long time to come, and it will be constantly changing. I think we do it well, and I mean to make sure that we do it even better in the future.

In a larger sense, these are the goals we try to reach in all our work. Specific cases vary enormously. But in all of them we strive constantly to be relevant to the needs of the policy maker. We strive to be objective, to make the most of our unique advantage among Washington bureaucracies—the advantage of not being responsible for making policy. These two qualities—relevance and objectivity—are the core of what we mean by professionalism in the intelligence business. To the extent that we serve these principles, we believe we serve the Republic.

IN MEMORIAM

Rear Adm. SIDNEY WILLIAM SOUERS, USNR

Director of Central Intelligence
Central Intelligence Group

23 January — 10 June 1946

16 January 1973

Mrs. Sidney W. Souers
625 South Skinker Boulevard
St. Louis, Missouri 63105

Dear Mrs. Souers:

For myself and on behalf of all those who have served with the Central Intelligence Agency, I extend you our deepest sympathy on the death of your husband. At the same time I wish to commemorate the great contribution he made to our country in the development of the concept of central intelligence after World War II. This concept was new to our system of Government and for months was a matter of widely divergent and strongly held views among the top advisers to the President. Eventually, in the early days of 1946 President Truman called on Admiral Souers to draft the final papers for his action. This resulted in the Presidential Directive of 22 January 1946 which established the National Intelligence Authority and under it the Central Intelligence Group. Your husband was then appointed Director of Central Intelligence as a leading figure in the intelligence structure and head of that group.

From these events came the establishment by law of the National Security Council and the Central Intelligence Agency, enabling our Government to meet its critical intelligence needs through the most turbulent times of the cold war period and the even more critical intelligence requirements to meet the current period of detente and disarmament negotiations.

We who inherited your husband's concept are particularly aware of what our country owes him in the field of national security. We hope the knowledge of this debt will be of some comfort to you in your loss.

Sincerely,

Richard Helms
Director

A glimpse of a
"bamboo Bastogne"

FIVE WEEKS AT PHALANE

Edwin K. Stockinger

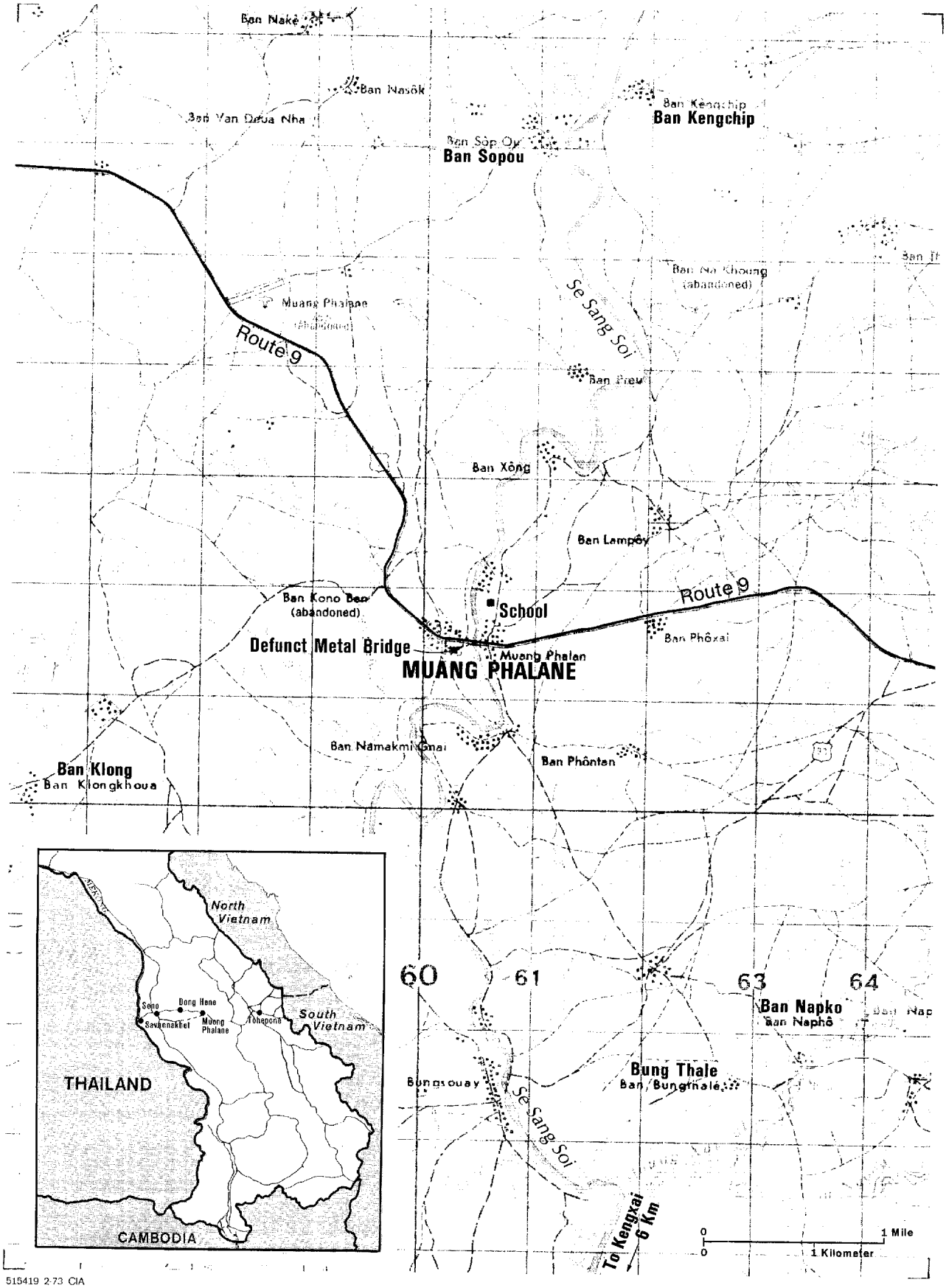
Between 24 March 1971 and 4 May 1971, two understrength paramilitary battalions of ethnic lowland Lao captured, occupied, defended, and finally lost the Route 9 town of Muang Phalane in southern Laos. During those five weeks, their operations encompassed a little bit of counterintelligence, a considerable amount of covert action, some effective intelligence collection, and some very hard fighting. They took heavy casualties, and in the end were overrun and shattered. But the survivors came back with their honor, and with a smug conviction that they had actually won the battle. These were not the feeble Lao troops made infamous by the press. Their story should be told.

In 1970, Muang Phalane was a small district capital and market center on Route 9, about midway between Savannakhet and Tchepone. It was the last stop on the taxi bus run from Savannakhet, and the easternmost point in the bulge of territory controlled by the Royal Lao Government which is loosely called the Savannakhet Plain. There was a District Chief's office, a new U.S. AID dispensary, a three-building school, a small Lao Army (FAR) garrison, and a string of shops on both sides of the main street. Main Street was also Route 9, and was shady and neat. The Se Sang Soi River flowed southward on the east edge of town, spanned by a defunct metal bridge. The town had changed hands a few times in the past, but for two years had been more or less firmly in government hands.

During December 1970, North Vietnamese Army forces began to operate closer and closer to Muang Phalane, and in January 1971, they rocketed and burned the FAR position southwest of town. The garrison withdrew, followed by public officials and traders. Most of the farmers in the district stayed with their land. One irregular battalion (Bataillon Guerrilla, or "BG") continued to operate in the neighborhood, but by mid-March had been forced back toward Dong Hene, 30 kilometers west of Muang Phalane.

In mid-March, rockets and recoilless rifle fire began falling on the airstrip and FAR garrison at Dong Hene, and it appeared that the NVA seriously intended to drive westward through Dong Hene toward Seno. Traders in Dong Hene began preparing to evacuate, and the FAR garrison nervously shuffled its feet.

A newly formed irregular battalion took to the field with the mission of cooperating with FAR and the irregular battalion still east of Dong Hene, in a joint effort to retake Muang Phalane. This green battalion quickly fragmented with leadership problems. Further, the original irregular battalion was itself close to exhaustion, and had to be relieved in place by BG 302, commanded by Major Thong Khoun. A sister battalion, BG 301, commanded by Major Mouy, joined BG 302 four days later. The two battalions together made up half of Groupement Mobile ("GM") 30, and the GM 30 deputy, Major Vathsana ("Vath"), assumed overall command. The two-battalion task force totaled about 540 men.



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At this time, the joint FAR/irregular attack was still on the books, but prospects looked bleaker after the FAR commander at Dong Hene bet Major Vath seven cows that the attack would never reach its objective. When he told Major Mouy that he expected to see Mouy back in three days with his "feet in his ears," it was clear that if any attack was to be made on Phalane, it would be made by the irregulars alone.

Late March is in the middle of the Laotian dry season, when streams dry up and fires in the woods burn unchecked for days. In the late afternoon of 23 March, Vath told [redacted] that he intended to kick off before dawn, and that he would move until he found water or was stopped by the enemy. By 10:30 in the morning, advance elements had moved nine kilometers and were still moving. As expected, the FAR unit on the right flank limited its attack to leaning forward in its foxholes, and the joint operation became a purely irregular one.

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Muang Phalane Retaken

At 8 p.m., the GM 30 Commander, Colonel Touane Boudahara, and the GM 30 [redacted] overflew the two battalions in a light plane and found the main force poised at the old Muang Phalane airstrip, three kilometers northwest of the town, and 17 kilometers east of the morning's jump off point. Vath reported, "My children are visiting the town, and everything is quite." [redacted] then dropped a string of homemade firefight simulators to the northeast of Muang Phalane and laid a string south of town for good measure. By sunup on the 25th, the task force was dug in at the school yard, and company-sized units were east of the Se Sang Soi, flushing demoralized NVA companies out of the villages. Villagers told Vath that the sudden move into Phalane had taken the NVA totally by surprise, and that the air-dropped simulators had turned their faces (and their defenses) toward the northeast.

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By the morning of the 26th, however, the NVA had regained their poise. They counter-attacked with an excess of confidence. They came with three battalions totaling 1,800 men, attacking straight across open paddy fields. The irregular outposts were driven in, but joined the main position in town without difficulty. Lao Air Force T-28's and some U.S. fighter bombers hit the enemy in the open, and began to take a heavy toll. Vath later said that it was as if the enemy were taking shelter from the air strikes by crowding right up against the irregular perimeter. They came nose to nose with the irregulars, couldn't breach the perimeter, and couldn't withdraw back across those open fields. The North Vietnamese had advanced their three 75-millimeter recoilless rifles abnormally close to the irregular position, losing one to air strikes and leaving a second behind when they finally disengaged. Two Porter aircraft dropped ammunition into the irregular position throughout the day, and drew small arms fire on every pass. One aircraft, with Colonel Touane aboard, was hit, and landed back at Savannakhet with a hole in the belly and a flat tire.

During the night of 26/27 March, the NVA were active around the perimeter retrieving their dead and wounded, while Lao Air Force AC-47 "Spooky" gunships fired into them. On the morning of 27 March the enemy tapped the irregular positions again, but without enthusiasm, and by noon had broken all contact. The irregulars had suffered 10 killed and about twice that number wounded, and reported that they had killed about 50 enemy. The GM commander [redacted] went into Phalane by chopper, picked up the casualties and about 30 captured weapons, and raised the Lao flag over the town. Colonel Touane stayed in Phalane to assume command, and [redacted] brought a lightly wounded NVA prisoner back to Savannakhet, where he promptly and perversely died.

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Three days later, a former Pathet Lao lieutenant attached to the task force overheard the NVA commander radioing a report that he had lost 375 men killed and "many" wounded. Later, airborne tactical radio intercept picked up the same report. The retreating enemy told villagers that they had been repulsed not by Lao soldiers but by a "special SEATO force" brought in especially to recover Muang Phalane. The irregular troopers told and retold this story with great relish. With Phalane in friendly hands, the shelling of Dong Hene of course promptly stopped.

In early March, the world's sloppiest (and unluckiest) intelligence officer had been killed by GM 33 about 30 kilometers southeast of Muang Phalane. This officer, an NVA lieutenant, had been carrying among other things a list of 21 names under the heading "Secret Agents to Contact in the Muang Phalane Area." The list obligingly contained each agent's home village, the Laotian equivalent of street address. This document emerged from the Savannakhet translation mill at about the same time the GM 30 task force was establishing an outpost line at a radius of eight kilometers around Muang Phalane. Every village on the list fell within this radius, and in a matter of days 15 agents on the list had been detained and the other six accounted for. ("Went away with the Pathet Lao last year. . . . Married and moved away" . . . etc.) All but one admitted to being NVA informants. One woman insisted throughout that she had been pointed out by her village because she was a chronic borrower and troublemaker. The interrogation center where she was kept came, sadly, to agree. The irregulars never located the real agent; she probably had faded away when the troops entered the village.

NVA Problems

Without an intact informant system, the NVA had to rely even more heavily than usual upon reconnaissance patrols. As these patrols reported copiously by radio, the irregulars were able to make good use of tactical radio intercept throughout the weeks of their occupation. The intercept radio flew aloft in the back seat of a Piper Cub, two sorties per day, for the duration of the operation. The second sortie each day landed at Savannakhet by sundown, and the translators pounced on the take. By 11 p.m. most nights, [redacted] could pick up a clear-text English version of the enemy's transmissions of the day. When he choppered into Phalane at 7 a.m. the next morning, he could hand the task force commander a sterilized resume. When it appeared from the traffic that the NVA had finally pinpointed an irregular position, the task force commander shifted the position a kilometer or more. Some testy exchanges often appeared in enemy traffic following these shifts (to the glee of the task force officers), and to the end, the enemy never had information good enough to make heavy weapons fire really effective.

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The villagers of the Muang Phalane area welcomed the GM 30 task force as heroes and spoiled them with gifts of food, Lao Lao (moonshine), and pretty girls. They also brought the gift of information. Usually the irregulars had prompt notification of enemy moves as far away as 10 or 15 kilometers. Villagers some distance from Muang Phalane would send information by a relay of runners who passed the information by word-of-mouth to the runner in the next village. In mid-April, villager information placed two NVA battalions in an assembly area along a stream south of Phalane. T-28's struck the position the same day, and that night [redacted] hit it twice with firefight simulators. The next morning, two companies of BG 301 approached the position, and saw fires and smoke.

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Thinking they had found the NVA cooking breakfast, the irregulars fired off a magazine apiece and advanced. They found piles of field equipment and medical supplies burning in an empty position, with many tracks heading southeastward. The enemy had cut about 90 bamboo poles on the site which the irregulars hoped were to carry dead and wounded. The combination of good information and timely air strikes worked well several times, and the irregulars began to talk about the possibility of holding Phalane until the rains came in July.

What was really needed was a spoiling attack to the east, to disrupt enemy preparations for the big attack they were virtually obliged to make. But no additional troops could be spared, and GM 30 had to be satisfied with the two battalions it already had in the field. Colonel Touane continued his patrols and ambushes, and threw out limited sweeps outside his eight-kilometer radius.

After a small patrol action near Ban Kengchip, five kilometers north of Phalane, villagers told a company of BG 302 that their opponents had been Pathet Lao troops of the 27th Ekarath (Regional) company. Savannakhet units had encountered the 27th again and again over the years, and had always found it to be a nuisance but a pushover in a fair fight. [redacted] broke into a Savannakhet FAR office on a Sunday afternoon and mimeographed 500 copies of an open letter to the 27th Ekarath. The letter offered amnesty and jobs with the irregulars, and was distributed in villages, left alongside trails, and conspicuously hung on bushes. There was one taker: a squad leader who wanted to bring his eight men to join the irregulars. But first, he wanted to talk to his brother, who was a lieutenant in BG 306, another of GM's four battalions. BG 306 was just finishing a retraining cycle outside of Savannakhet, and the brother was duly sent, with a four-man bodyguard, to talk the 27th into crossing the line. But he never found them.

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A 27th Ekarath soldier (who hadn't seen the letter) later walked into Phalane, and said that the 27th had just been withdrawn from the Kengchip area and sent east to help prepare for "the big attack." He also said that he was 20 years old, had been a PL soldier since he was 13, had never been paid, and was damned sick of it. After release from the interrogation center, he joined GM 30 and began drawing his pay on schedule.

Villagers continued to visit GM 30's "official" command post in the USAID dispensary building. (After the villagers left, in time to walk home before dark, the GM 30 staff would retire to the real command post 600 meters north of town.) During one of the gossip sessions, villagers from Ban Klong, four kilometers southwest of town, described some enemy misbehavior in the village wat (pagoda) during the NVA occupation just ended. Two Pho Bans (village chiefs) from the Klong area volunteered to tell the story on the radio. A chopper picked them up and flew them to Savannakhet, where they taped a 45-minute interview. They described how the NVA had burned wooden images of Buddha for firewood, how they had dipped bronze Buddhas in paint of various colors and hung them upside down in trees, how they used the holy books for toilet paper. All in all, pretty strong stuff, particularly as it was broadcast, and hopefully replayed, on the eve of the world Buddhist conference in Ceylon. The two old men were wined and dined under the electric lights of Savannakhet, and then taken home by chopper.

In addition to gossiping visits, the villagers paid several more formal calls on GM 30. On the occasion of Phi Mai (Lao New Year) they staged a full-scale bacci (a fairly alcoholic semi-religious ceremony). In attendance was a former

informant of Colonel Touane's, who hated Americans and Vietnamese alike, and who was also the PL-appointed Pho Ban of Ban Kengxai, 12 kilometers south of Muang Phalane. He thought that Colonel Touane ought to know that the North Vietnamese had sent a white-haired general, also named Tuan, to recapture Muang Phalane.

The Enemy Counterattack

The Kengxai Pho Ban described a rally at which General Tuan said that the NVA had lost a lot of men and a lot of face at Phalane, and that he was coming with a large enough force to take it back, or else he wouldn't go back to North Vietnam himself. The Pho Ban filled in some other details, and accepted a gift of five sacks of salt for his people. As it turned out, the Pho Ban's information was good: General Tuan used six identified infantry battalions, an antiaircraft battalion, and other attachments, and he did indeed take Phalane back.

On 28 April, BG 306 finally was made available to reinforce the two battalions in Phalane. On 29 April, the battalion was mustered and equipped, but the trucks failed to appear. On 30 April, it disembarked west of Phalane and started walking to Ban Klong to join GM 30. It ran into an enemy force that night, and recoiled with five dead and 11 wounded. At about the same time, outposts of BG 301 and 302 reported enemy contact, and "the big attack" was under way. BG 306 was just one day too late and wouldn't be any help.

By 6 a.m. on 1 May, all five major outposts of BG 301 and 302 were engaged. Captain Inthesorn's company (Co. 2, BG 301) at Bung Thale and Ban Napho, five kilometers southeast of Phalane, found itself surrounded and heavily pressured. Company 3, BG 301, left its position south of the east end of the old metal bridge and counterattacked to spring Company 2 free. Company 3 then returned to its old position near the bridge, and Company 2 dug in just to the south of them. On 2 May, an NVA battalion advanced on them across those same open paddy fields. They came in parade ground formation, at sling arms, with three hand-held bull horns blaring. Three T-28's and a flight of US F-4's caught them in the fields and laid CBU bomblets directly on them. Not many reached the safety of the ditches along Route 9. A later intercept identified this unit as the 2nd NVA battalion, and said that the survivors had "bad morale and would have to be re-educated."

With the 2nd battalion laid to rest, the T-28's and F-4's wheeled for home. They were no more than out of sight when another battalion emerged from the tree line and came across the paddy field. By sundown on 2 May, both Companies 2 and 3 of BG 301 were surrounded again. Their perimeters were small, and ground fire was intense. Resupply aircraft tried to drop supplies to them with ground-impact-delay parachutes, but half of the 'chutes failed, and all but one of the rest missed and went to the enemy.

During the night of 1 May, the NVA had moved large quantities of antiaircraft guns into the area with the infantry units, and on 2 May these guns were to reap a harvest unprecedented in Savannakhet irregular operations.

During the day four Lao T-28's and one U.S. F-4 were hit. One T-28 made one final pass at the enemy in the paddy field. His guns were empty, and he said that he would try to bluff the remaining enemy into the ditches. He must also have been curious about the results of his previous runs because his last transmission was a count of enemy dead. "There's more than a hundred of them lying there!" He was hit by a 37MM round and fell burning west of Ban Klong. A BG 306 patrol recovered his remains later in the day. Of the other T-28's, one reached Savannakhet streaming oil, another landed safely at Seno, and the third

bellied in on the old Phalane Southwest airstrip, 12 kilometers southwest of the battlefield. An Air America chopper picked up the pilot, but the enemy burned the aircraft during the night. The USAF F-4 was hit by a 23MM while on a CBU run against the tree line east of the big rice paddy. He landed safely in Thailand with one wing afire.

The Breakout Begins

North of Phalane, Company 3 of BG 302 moved eastward to Ban Sopou, four kilometers due north of Phalane, where one of its platoons was pinned down. The company managed to extricate its platoon, but then found that its way south to the main position was blocked by about one enemy battalion. The next day, 3 May, Colonel Touane ordered Company 3 to break out to the northwest, and start for Dong Hene.

Meanwhile, south of town, 3 May found Companies 2 and 3 of BG 301 in real difficulty. The companies were separated, and each was surrounded. When Touane ordered them to rejoin the main position, Company 2 was able to break free and cross the Se Sang Soi, but found its way north blocked by about 200 NVA dug in around the school yard. Touane then ordered Inthesorn to take Company 2 back across the river and try to free Company 3. Inthesorn made his try at a bend in the river just south of town, and was beaten back. At that point Touane ordered Company 2 to make for Dong Hene. Later in the day, the

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[redacted]ocated Inthesorn west of Ban Klong. He asked for orders, and the sent him north to Route 9 with instructions to hold the back door open.

Company 3 was running out of ammunition and not making any headway towards a breakout. At 11 a.m., 3 May, their last transmission was: "We're fighting hand to hand. No ammunition. We will call you back later." They never called back. About 15 men escaped, seven of whom made it back to safety. In all, 62 men of Company 3 were killed or captured on the position.

During the morning of 3 May, when it was needed most, tactical air support dried up and stopped. USAF aircraft were busy farther east over the Trail, and the four remaining T-28's in Savannakhet took the morning off while the pilots attended a memorial service for the pilot killed the day before. On the night of 2 May, the Air Attaché in Vientiane had ruled that the skies over Phalane were too hostile for the 0-1 spotter aircraft, and there was a hitch in the rules that forbade the American Forward Air Controllers from working from a T-28. By the afternoon, things had been ironed out, and the fighters came back to work.

At the GM 30 command post, 600 meters north of town, there was a goodly amount of incoming fire but the enemy still had not made a ground attack. Most of the incoming fire was absorbed by a highly conspicuous dummy CP made of parachute tents on the bank of the river. The enemy poured fire into these empty tents, and never really zeroed in on the real CP until after it had been evacuated. There had been a small firebase 400 meters southwest of the CP, but it had been overrun on the morning of 3 May. Somchan, the former PL lieutenant, and an irregular master sergeant were captured there. Captain Southeng, commander of Company 1, BG 301, had been cut off there as well, but Southeng's brother had brought a platoon to his aid, getting wounded in the process.

At 1400, 3 May, it still appeared to Touane that the situation could be saved. Most of the outposts had come in, or escaped to the west, or had been written off. BG 306 was lost and panicky, but was nearby and maybe could be found and brought into the main perimeter. Touane told BG 306 to fire one round of M-79, so he could guide them toward the CP. BG 306 fired the round

(they were two kilometers to the north), and immediately came under heavy mortar and recoilless rifle fire. BG 306 broke and dissolved completely, and there were no more serious thoughts of repelling the attack, except—perhaps—in the mind of the GM deputy, Major Vath. At 1600, the GM [redacted] overhead raised Vath on the radio. 25X1

"*If I can get an air strike . . .*"

"Vath, what do you think?"

"Ah, well, sir," Vath answered. "I would like to try just one more time. If I can get an air strike on those guys in the school yard . . ."

"Vath, my good old friend, get out of there. You can come out with honor now. Never mind the school yard. Come home."

"Well, sir, I'll go see what the Colonel says."

There would be no strikes on any school yards. The rules of engagement forbade an air strike anywhere near a structure of any kind, and there definitely would be no air strikes on that school yard. And there weren't.

At that moment, there were three Porter aircraft overhead, all loaded with ammunition. T-28's were supposed to have covered them while they dropped their supplies into the position, but the T-28's had come and found something to bomb (not the school yard), and had flown away. The three civilian Porter pilots, one American and two Thai, discussed the situation by radio in English. Then Captain Lickett broke into Thai, and told Captain Mi: "Screw it. They need this stuff." Mi said, "OK." And they dived through the ground fire and put all four parachutes directly on the position.

25X1 [redacted] sitting beside the American pilot, said, "OK. Let's go drop." "Where are the T-28's?" was the answer, and they carried their ammo back to Savannakhet. A few days later, Captains Lickett and Mi found bottles of good Scotch whiskey in their lockers.

The NVA made three heavy ground attacks on the CP position during the afternoon, each preceded by a harangue on a bullhorn. The NVA called the GM 30 officers by name, urging them to surrender or be killed. As Touane told the story later, some irregular troops in the line shouted back: "Bo mi ban-ha! No problem!" The bullhorn answered, "OK. Here we come!"

The people on the CP began preparing to slip away, intending to make their break at 8 p.m. At 6 p.m., a very heavy attack fell upon the CP, supported by 82-millimeter mortars, 75-mm. recoilless rifles, and 12 B-40 rocket launchers firing in salvo. The 12.7-mm. antiaircraft guns around the perimeter depressed their muzzles and raked the position, but fired too high to do any real damage. The attacking NVA took casualties from their own supporting fire, and the irregulars could hear them cursing their gunners. The irregulars had plenty of ammunition, thanks to Lickett and Mi, but they just couldn't stop this attack.

The CP force broke out in three parties. Captain Southeng, carrying his wounded brother, led his company. Major Mouy led another group, and Colonel Touane led a third. As Southeng left the perimeter, he was shot through both legs and fell to the ground with his brother. Survivors later reported seeing Southeng pull the pin on a grenade and hold it, destroying himself and his brother, and knocking down several converging NVA troops. Mouy was knocked down by a B-40 round, and reported killed. He showed up at Inthesorn's "back door" position at noon the next day, exhausted and scratched up, with his trouser legs full of holes from the B-40 fragments.

As the irregulars fled, NVA troops swarmed over the position, but their supporting fires did not lift. The irregulars could hear them, still cursing their

gunners, as their own rounds dropped among them. In the rapidly falling darkness, the irregulars were able to mingle with the enemy. They crossed three separate skirmish lines. Most of the enemy troops held their fire, uncertain of their targets. Civilian Operations Assistant Som said that he jumped over a foxhole and a crouching enemy soldier. The man shouted but did not fire. At this point, Som dropped his knapsack full of captured documents, and ran a little faster.

There were many enemy bodies along the escape route, some of them at least two days old. Enemy weapons were scattered about the field, but there were no wounded on the ground.

By noon on 4 May, most of the survivors had passed through Inthesorn's position, and Major Mouy had been accounted for. Air America choppers landed along Route 9, picked up the wounded, and tried to pick up stragglers. The chopper pilots later said that they were surprised by many of the unwounded who refused rides, telling the flight mechanics that the choppers were for wounded, and they could walk very well, thank you. The choppers were able to pick up Inthesorn and the remaining 17 men of Company 2, BG 301. As he disembarked at Dong Hene, Inthesorn flashed his cocky grin and shouted [redacted] that "Company two is Number One!" 25X1

In the days that followed, GM 30 men continued to trickle back. On 5 May Somchan, the former PL, walked into Dong Hene. He and the master sergeant had been tied up on the firebase, and marched away guarded by a wounded NVA who had lost an eye and whose weapon was empty. The sergeant refused to try an escape, but Somchan broke away and hid for three hours while he worked his ropes loose. He was caught again on the bank of a stream, and claimed he was a farmer. One of the NVA said that they were looking for a fat guy anyway, and that Somchan was too skinny. They turned him loose, and he started walking west.

It is hard to say what had been accomplished by it all. General Tuan had Phalane. He had probably paid more for it than he intended, but he did, after all, have it. Colonel Touane's GM was a shambles, but was thoroughly pleased with itself. The NVA eventually made their grab for Dong Hene, but too late in the dry season to consolidate before they were washed away by the rains. Probably those five weeks in Phalane had made the difference. Maybe five weeks were just long enough.

Tracing an ingredient for the Soviet atomic bomb

CHASING BITTERFELD CALCIUM

Henry S. Lowenhaupt

In December 1946 a chemical engineer from the former I. G. Farben plant at Bitterfeld in East Germany volunteered in Berlin that this plant "had started in the past few weeks producing 500 kilograms per day of metallic calcium. Boxes of the chemical are sent by truck every afternoon to Berlin, labelled to Zaporozhe on the Dnieper. Calcium is believed to be used as a slowing agent in processes connected with the production of atomic explosive."

This was the lead we in the Foreign Intelligence Section of the Manhattan District Headquarters had been waiting for. We had read the technical investigation reports from FIAT (Field Information Agency/Technical) on the production of uranium at the Auergesellschaft Plant in Berlin/Oranienburg. We also knew that Dr. Nikolaus Riehl—with his whole research team from Auergesellschaft—had met the Russians, volunteering to help them make uranium for their atomic bomb project. We knew from intercepted letters that the group was still together, writing from the cover address PO Box 1037P, Moscow.* We knew Auergesellschaft during World War II had made the uranium metal for the German Uranverein**—the unsuccessful German atomic bomb project—by using metallic calcium to reduce uranium oxide to uranium metal (not as "slowing agent"). We had analyzed the two-inch cubes of uranium metal from the incomplete German nuclear reactor which the Alsos Mission*** had found in the minuscule village of Stadtilm in Thuringia. We knew German uranium was terrible—full of oxides and voids, though it was fairly pure otherwise by non-atomic standards. The files also disgorged that in 1945 the Russians had started to dismantle and take to Russia the small calcium plant at the enormous Bitterfeld Combine, in addition to the big magnesium facility.

Cables went out immediately to the European Command in Germany via G-2 and directly to Col. Edgar P. Dean, Manhattan District representative in London, to locate and interrogate all engineers who had fled Bitterfeld to the West or were currently willing to sell information on their unloved masters. We wanted to know how much calcium was to be produced, what its specifications were, and where it was to be shipped. We wanted to know what non-atomic normal German industries used calcium, and in what quantities. We wished Col. Dean to keep our British colleagues in the Division of Atomic Energy, Ministry of Supply, informed.

At home, the Scientific Division of the Office of Special Operations in the newly-formed Central Intelligence Group was also apprised of our needs. Col. Frank A. Valente of our section was asked to take time out from his task of organizing an atomic detection system† to talk to the U.S. Atomic Energy

*See "On the Soviet Nuclear Scent," *Studies* XI/4.

**See David Irving's *The Virus House*, William Kimber, London, 1967.

***Code name for teams interrogating Italian, French and German scientists in the final months of World War II.

†See "The Detection of Joe-1," *Studies* X/1.

Commission in depth about the use in the U.S. program of calcium to reduce uranium salts to uranium metal. Major Randolph Archer, also of our office, was asked to talk to U.S. firms making calcium metal, and find out what it was used for and in what quantities.

As so often happens, the people involved and their experience were crucial ingredients. On the American side was the Foreign Intelligence Section of the Washington Liaison Office of the Manhattan District, then in the process of transferring as a unit to the newly formed Central Intelligence Group. It was headed by Col. L. E. Seeman, a career Corps of Engineers officer who had run the American engineering forces of the CBI theater during World War II and would go on to become major general. The section was staffed with a few career Corps of Engineers personnel, several officers and civilians trained in science, and the remainder trained in investigative procedures in the Counter Intelligence Corps.

The orientation toward engineering on the part of our management led directly to a pragmatic approach—do what works, and get on with the job. The engineering orientation also led materially toward the estimative method of technical evaluation. Engineering officers are accustomed to laying out engineering tasks to find out how long they will take at a minimum—and then to evaluate likely slippage. They think in quantitative terms—man days, truckloads, cubic yards. The scientific side of the section, Col. Valente, Mr. Charles Campbell, Mr. Donald Quigley, and I learned gradually to ferret out the crucial technical facts, the bottlenecks as it were, that could be used in these engineering-type evaluations.

A remnant of the wartime cooperation in the atomic field was the direct liaison at that time with the Intelligence Section of the British Division of Atomic Energy of the Ministry of Supply. Col. Dean, Assistant Military Attaché, was our representative in London. This cooperation was normalized gradually into more regular country-to-country liaison channels after our section was deployed to the newly formed CIG early in 1947. The Atomic Energy Act of 1946, which restricted much atomic data to "cleared" U.S. personnel, also tended to perpetuate differences between the U.S. and UK intelligence efforts already in being in 1946 because of the "nationalistic" policies on the parts of both General Leslie R. Groves, Manhattan District Commander, and Sir John Anderson, head of the UK atomic effort.

The British office was staffed with technical personnel, much as our own was. Mr. David Gattiker, their liaison to our section, had been a chemical engineer with Imperial Chemicals Incorporated before World War II. Mr. Kenneth Townley, one of the London members, was a geologist by profession with some experience in uranium prospecting. Its leader, Commander Eric Welsh, however, was also a career member of MI-6. Commander Welsh had masterminded the sabotage of Norsk Hydro in Norway in 1943 to prevent the Germans from getting heavy water and completing an operating reactor at Stadtilm. In 1940 he had been instrumental in smuggling the great nuclear physicist Niels Bohr out of occupied Denmark. And in the thirties he had been a chemist at Bitterfeld.

Returning to the calcium problem, by mid-January 1947 the Bitterfeld activity was definitely confirmed, and indeed amplified: Russian requirements were for 30 tons of metallic calcium per month, and distillation was needed to achieve adequate purity. A number of former Bitterfeld engineers were soon interviewed, especially by Major Paul O. Langguth working for Col. Dean in London. As we learned more, some were even re-interviewed. I remember, for instance, flying to Wright Patterson Airbase in late 1947 to talk once again to a

Bitterfeld metallurgist whom Langguth had previously interrogated, and who had in the interim come to the U.S. as a member of the Air Force's Operation PAPERCLIP.

These interviews also soon established the non-atomic usage of calcium: during the war Bitterfeld had produced about 5 tons per month of 95% pure calcium metal for use by the Osram and Philips Companies to eliminate the remnants of oxygen and nitrogen from radio tubes. Some 20 tons per month of calcium aluminum and calcium-zinc alloys were produced for bearings for the German railroads, and the German Navy and Air Force bought calcium hydride for use in inflating balloons. The concept of 30 tons per month of calcium so pure it had to be distilled was clearly foreign to German industrial practice.

At home, Col. Valente selected Dr. Frank H. Spedding at the Institute of Atomic Research, Ames Laboratory, Ames, Iowa, as the man who would know most about uranium metallurgy—having been concerned with that aspect of atomic energy since the early forties. Spedding was quite firm, to make uranium metal for reactor use, the U.S. normally reduced uranium fluoride with magnesium metal—because it was cheaper. The magnesium had to be made by the Pidgeon process, in which dolomite is reduced with ferro silicon at very high temperatures; normal magnesium produced from sea water by electrolysis was not pure enough.

Reduction of uranium oxide with calcium, Spedding continued, always gave a poor product. However, reduction of uranium fluoride with calcium gave properly liquid melts, and an excellent product on cooling. The calcium had to be distilled for adequate purity. Elements like boron, vanadium, manganese, should they become incorporated into the uranium metal even in minute amounts, would tend to absorb neutrons and stop the nuclear reaction. Thus these elements also had to be kept to exceedingly low amounts in the calcium used to make the uranium metal. He gave Col. Valente a list of maximum allowable impurities in U.S. uranium metal used for our Hanford reactors, and in U.S. atomic-grade calcium. Of these, the worst actor was boron.

Major Archer reported that in the United States, only Union Carbide and Carbon and New England Lime made calcium metal, and only three to five tons per year at that for non-atomic uses.

Informed of the Russian calcium project at Bitterfeld, our British colleagues became quite active. Several Bitterfeld chemical engineers chose to resettle at I. G. Farben plants in British-occupied Germany, thoughtfully taking with them copies of reports on calcium production written for the Russian management. The British also followed our lead in making a thorough survey of non-atomic uses of calcium both on the continent and in Britain, to make absolutely sure this was no red herring.

Meanwhile, the general intelligence net was far from idle. The U.S. Army interviewed a border-crosser, Dr. Adolf Krebs, and learned that he had been taken to Moscow by the Russians for an interview with several MVD colonels and one MVD General "Kravchenko." In the course of these interviews he went to Elektrostal, a town some 40 miles east of Moscow, where the best crucible steel plant in all of Russia is located. Here he was interviewed by Dr. Riehl of Auergesellschaft fame, who was "segregating uranium on a production scale using a new process which utilized electric furnaces." On return to East Germany (after declining the position offered) Krebs fled to the West, fearing reprisals. Confirming this story was the word from the British that Frau Blobel, Riehl's former secretary at Berlin/Oranienburg, had mentioned to an agent that Riehl's last letter to her had been postmarked 7 October 1946 from Elektrostal in the USSR, rather than the usual 1037P Moscow. A search of the files on Elektrostal

quickly disgorged a British report of the preceding autumn indicating three carloads of uranium ore had been sent from the famous Jachymov (Joachimstal) Mines in Czechoslovakia to Electrostahl (sic) in the USSR. The circumstantial evidence that Elektrostal was the site of the Russian uranium metal plant was becoming impressive.

Our "Summary Report of the Status of the Russian Nuclear Energy Program" on 1 June 1947 reflected this, stating that the "indication from metallic calcium production . . . appears to be the construction of two plutonium producing reactors . . . with 500 megawatts (MW)" of total power.* "It is particularly significant that a project of this size cannot be supported by the estimated reserves of uranium ore available to the Russians . . . 514 tons uranium oxide already available and 2200 tons of uranium in reserves. . . . The best information indicates that this program is not proceeding well, and in fact uranium metal appears to have been produced in insufficient quantity to operate more than a very small pilot reactor, such as that first operated in this country in December 1942. Thus, if it is assumed in the worst case that Russian progress from this date will proceed at a rate comparable to that of the American project . . . then to produce a single bomb, January 1950 represents the absolute lower limit."

Not a single thought that—just possibly—the Russians were planning in the light of full engineering information, and that our estimates of their expected available uranium were low. The Greeks called it "hubris"—unreasonable pride.

In mid-1947 our earlier discussions with G-2 and OSO began to pay off. First, engineer [redacted] one of the [redacted] sources on Bitterfeld and by then a resident at Leverkusen in the British Zone of Germany, decided to cash in on a good thing by selling his research papers on calcium distillation to the American S-2 in Berlin as well as to the British. Aside from the delicate problems with the British raised by this particular sale, [redacted]'s information indicated that the Bitterfeld people had developed a new copper-calcium alloy process for making calcium electrolytically which was much more efficient than the old electrolytic "carrot" process. It was this alloy that was partially distilled at high temperatures to give the very pure calcium metal needed, the reject alloy going back into the electrolytic baths. Further, bottlenecks had been developing in obtaining the high-temperature sicromal or similar type steel needed for containers, and the firm Pfeiffer in Wetzlar in the American Zone of Germany was tardy in manufacturing needed vacuum pumps.

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Headquarters, European Command forthwith stopped all further shipments of vacuum pumps and sicromal to the East Zone. We in the U.S. had already put vacuum pumps on the "COCOM" export control list in April 1946, thereby stopping a tidy order recently placed by AMTORG, the Russian trading organization in New York. Thus export control pressure against the Russian atomic program was being applied as rapidly and as forcefully as we could arrange it. How much, if at all, it slowed the Russian atomic program down is problematical, but it certainly forced Russian and Bloc industries to widen the scope of their manufactures rapidly.

Of more importance from an intelligence viewpoint were the samples of raw and distilled calcium which Miehe gave to S-2, Berlin. These found their way to Col. Valente of our office, who passed them to the AEC for shipment to Dr. Spedding at Ames. By late 1947 we had his detailed analysis of the calcium the Russians were using, along with a comparison to U.S. atomic-grade calcium and

*Presumably based on our graphite plutonium producing reactors at Hanford, which were then rated and operated about 250 MW apiece.

U.S. specifications: the Bitterfeld distilled calcium was quite adequate by our atomic standards.

Simultaneously, OSO produced a winner—a reporting source at Bitterfeld itself who had access to the firm's records. He brought in documentary evidence that on 26 July 1947 three rail cars carrying metallic calcium—consignment No. 179-4363—left Bitterfeld for "Elektrostahl Moskau," Post Box 3, Kursk Railroad. The shipment of carload lots of both calcium and uranium ore to Elektrostal confirmed it as the site of a production-sized uranium metal production facility, and not just the location of a research effort under Nikolaus Riehl.

Digressing a moment, we turned out to be lucky—or wise—in accepting Elektrostal as the destination. We eventually had enough destinations to keep the most eager analyst busy. The initial report mentioned Zaporozhe on the Dnieper—which turned out to be where the magnesium plant cells were being sent. Later, air shipments to Leningrad were mentioned. It was said the Russian calcium electrolysis plant would probably be erected at Magnitogorsk, the distillation plant at Kiev, Dzerzhinsk or "Samarov." Knowing mention was made of two German technicians, Drs. Springmann and Kroesel, said to be capable of supervising the erection of a calcium plant, and who reportedly wrote letters from Dzerzhinsk in the USSR.

An early January 1948 report from the UK, for example, indicated that "those German scientists who were deported from Bitterfeld and who had knowledge of the production of pure metallic calcium ore are at Sverdlov near Gorki." Welsh, according to a handwritten note, later "reviled this report," for there is no town of Sverdlov near Gorki. Just to prove that old analysts fade gradually, I took this report the other day to the appropriate section of CIA's Central Reference Service, and out popped the famous explosive manufacturing and shell loading plant "Sverdlov" in the town of Dzerzhinsk, just west of Gorki. Next to Sverdlov is the Chemical Plant "Kalinin," which makes the sulfuric and nitric acids used at Sverdlov for the production of explosives, and the chlorine which would be needed for calcium chloride production for feed for a calcium plant. I would not be surprised to find that the special calcium chloride plant designed at Bitterfeld for erection in the USSR was actually built at either the Kalinin or Sverdlov plant in Dzerzhinsk.

In addition to the "hot tips" on destinations mentioned above, Russian bureaucracy coupled with security produced another bizarre batch for us to unscramble. First, I. G. Farben Bitterfeld became Elektrochemisches Kombinat, Bitterfeld, of the Aktiengesellschaft für Mineraldünger. Later the overall administration was changed to Abteilung der Staatlichen S.A.G. "Kaustik." Initially, the official consignee was c/o Raznoimport, Moscow. By 1948 both "Verwaltung der Aktiengesellschaft für Elektrochemische Industrie 'Kaustik,' Moscow" and "Verwaltung GUSIMZ, Moscow Chkalov St. 36," were used as addresses on two shipments in a single freight car. Possibly there really were two different destinations for raw and distilled calcium metal, but it seems doubtful. By 1950 the address became simply APN 27301, Frankfurt/Oder, although the same type of Ministry of Foreign Trade order and transshipment numbers that had been attached from the very beginning continued in use. Again OSO helped immensely when it tapped banking and trade circles in East Germany who understood in exhaustive detail that Soviet property abroad (GUSIMZ) was subordinate to the Ministry of Foreign Trade, just like the older subordinate trade sections such as Raznoimport. Further, they made it clear that the Trade Representation in Berlin and Amtorg in New York were vehicles or umbrellas in each foreign country under which all these trading or property organizations were "housed." A misassigned German POW (yes—the Russians also make security

mistakes) returned to *West* Germany and told us about APN 27301: it was just the Russian equivalent of a military post office number, in this case simply the address of a labor battalion at the transshipment yards in Frankfurt/Oder which handled the transshipment* of all special atomic goods going east or west.

Another facet of the problem investigated thoroughly was the possibility that the Russians would eventually turn from calcium to magnesium, much as the U.S. had done. Unfortunately, the Germans had been developing a Pidgeon-type process for making magnesium during the war, and there was evidence from Bitterfeld that experimentation on making calcium by this process was under way. There were, indeed, reports that one of these furnaces had been sent to the atomic people. Then we learned that Soviet technicians had been intensely interested in the similar Hansgirg process furnaces at the Hungnam Chemical Complex in North Korea when that country came under Soviet control in 1945. In the end, all too much effort was spent on this red herring of our own devising.

Returning to the calcium problem itself, Commander Welsh in the UK decided in mid-1947 that clandestine penetration of the Bitterfeld calcium program was *the* way of getting at the Soviet atomic program from East Germany. He felt he had the assets and the official backing from the MI-6 hierarchy.

He also attempted to force U.S. agreement to lay off Bitterfeld, allowing the British a free hand and reducing the possibility that too many (American) cooks would alert the Russians. When full agreement was not forthcoming, he tried to use an (unwitting) attempt by an American Army officer as a for-instance case to back up his plea. Col. Seeman and Charles Campbell on the American side spent hours discussing the matter with both OSO and G-2 representatives, but in the end legitimate self-interest forced the decision that the Americans would try not to use the same sources as the British. Actually Welsh's fear of American "clumsiness" was misplaced. His sources at Bitterfeld were never in jeopardy from American actions; indeed we may have helped. What saved him—and us—was his penchant for operating directly for "C,"** [redacted] 25X1

[redacted] His real danger lay in the Soviet penetration of MI-6 and the British Foreign Office: Donald McLean, secretary to the Combined U.S.-UK (atomic) Policy Committee, and "Kim" Philby, MI-6 representative to CIA at the time, were later both shown to be active members of the Russian Intelligence Service.

Welsh's confidence in his Bitterfeld penetration, however, was not misplaced at all. From its inception it produced long sheets of monthly shipment statistics on a box-by-box basis. Selected product analyses were received periodically, and Russian specifications and requirements as they occurred. These data were interpreted in the light of the design reports which the British (and to a lesser extent we ourselves) had already received. In addition, the agent usually added comments as needed for understanding. Indeed it is fair to say that as far as the technical side of the Bitterfeld calcium operation was concerned, by 1948 the British (and in turn we ourselves) knew as much about it as the Russians did.

Information on what was going on in Russia, however, came hard. Through mid-1950, the only additional informant on Elektrostal was Dr. Hans Kerschbaum, who had been arrested and interned in the USSR, gone to Elektrostal for an interview with Riehl, instead worked on electronics at Shchelkovo near

*From Russian standard gauge (five British feet inside-to-inside on the rails) to European standard gauge (five Roman feet center-to-center on the rails). The British, of course, designed both.

**"M" to James Bond fans.

Moscow, returned in early 1949 to East Germany, and then fled to the West. He merely confirmed that Riehl was reducing uranium with calcium, though he did add that he thought it was from uranium fluoride, rather than uranium oxide. The Russian defector "Icarus" in July 1950 confirmed many of our conclusions about Elektrostal, Bitterfeld, transshipment offices, etc., but his information was primarily non-technical.

At Bitterfeld, Russian security about atomics in the USSR was almost absolute. There were, however, rumors in March 1948 that the Soviets had a calcium distillation plant in operation in the USSR at that time. In 1949 a high-level Soviet official at Bitterfeld was reported as saying "that the USSR was engaged in the production of calcium by electrolysis and distillation." In 1950 a "very accurate" source stated "we have been informed that crude calcium is not being used and the quantities delivered by us would be distilled in Russia." Finally, there was a rumor in East Germany in December 1948 to the effect that Riehl had received a 100,000-ruble Stalin Prize. Useful, but hardly earthshaking.

Our only recourse was to infer what was going on in Russia from requirements and specifications given to Bitterfeld, a straightforward, though far from simple technical intelligence problem. To this end, Major P. O. Langguth, temporarily back in the United States, early in 1948 visited both the main offices of the Union Carbide and Carbon Corporation in New York City, and their laboratories and calcium production plant at Niagara Falls, N.Y. He took with him such detailed specifications as were by then available on the Bitterfeld calcium operation. As had happened on previous occasions, Union Carbide was most cooperative. They produced full technical data on their own process, requiring only that it be kept within CIA "and not given to the USAEC," to whom Carbide was selling the majority of its calcium and all its high-purity product. They studied the Bitterfeld data and either judged technical factors (such as efficiencies) in the light of their own experience, or estimated these factors if they were missing from the Bitterfeld data. The analysis extended to the technical factors and material efficiencies involved in producing uranium by calcium reduction of the fluoride. As a result, Major Langguth returned with a comprehensive technical understanding of the Bitterfeld operation and of the expected performance of both calcium and uranium metal facilities designed for Russians for erection in their country. Finally, Carbide's files produced, of all things, a translation of a 1938 paper in Russian entitled "Regarding certain questions on the founding of the calcium industry." Because of the almost certain interruption by the war of any 1938 plans for new facilities, this report settled negatively the question whether the USSR had had any sizable native calcium industry.

Incidentally, I have often been asked "Does technical intelligence help American industry?" Usually I have had to hedge the reply because customarily we have had all too little technical data. For metallic calcium, however, we had detailed design data on a new, definitely more efficient process, copper-calcium electrolysis followed by distillation. Carbide did not want it in 1948, nor has the AEC been interested subsequently. The reason given has always been the same: the (then) current operation was a small one with no expectation of significant expansion. Any major change would have been economically disadvantageous over any reasonable amortization period. Thumbs down.

We, however, gleefully accepted all the data, drawings, plans, evaluations, and specifications we could assemble on the Bitterfeld operation, attempting to collate it in every way we could think of in the hope of squeezing out one more drop of information on the Russian atomic program. We turned to surprisingly complete photointerpretation reports written in 1943 on the Bitterfeld complex to locate the calcium facilities and get exact building dimensions—something the

sources never seemed to have available. In the precess we discovered just how enormous an operation the Bitterfeld Combine really was. No wonder bombing had never completely halted operations.

Events pertinent to calcium were placed in chronological order. The process was properly described and quantified. The names of Russian and German personnel were arranged alphabetically with intelligence data attached. Process yields were evaluated and recorded. Shipment data were tabulated and quantified. The results were eventually all pulled together and published by Donald M. Brasted as a Scientific Intelligence Report early in 1952 after the Bitterfeld operation had been mothballed.

In 1948 and 1949, however, these collations were being used in estimates as fast as they were being made. We lined up a sequence of events dominated by (a) the orders of April 1946 to expand production severalfold, to 30 tons per month of calcium with essentially normal specifications, (b) the coincidence in October of air shipments of raw calcium to Moscow with the Russian decision to build a large calcium distillation plant at Bitterfeld, (c) the arrival in November of production specifications based simply on the theoretical neutron absorption rates of impurities, followed in February 1947 by (d) much more realistic (and adequate) specifications, and in March by (e) orders to draw up engineering plans for Russian calcium facilities. This sequence was interpreted much as follows:

> "... In August 1945 there was no coordinated plan of action for the development of nuclear energy. About January 1946 the USSR decided it was necessary and desirable to use for atomic purposes the production capacity of the occupied countries. ... By June 1947 uranium metal appears to have been produced in sufficient quantity to operate no more than a very small pilot reactor...."

Actually, we could have pointed up our conclusions: as we later learned, Russia's first "Fursov" research reactor went critical at the Moscow Institute of Atomic Energy at 6 p.m. on Christmas Day 1946.* That was why the realistic specifications were sent to Bitterfeld in February 1947. At the time we thought this change in specifications came from Russian measurements on how the chain reaction fell off in an exponential pile (a portion of a reactor mockup about 1/10th size) after the neutron source was removed. Our guesses were running six months too late.

On the designs for Russian calcium facilities drawn up at Bitterfeld between October 1946 and March 1947, we knew that the calcium chloride feed plant had a capacity of 15 tons per day, "with the possibility of being doubled"—enough for a calcium electrolysis plant producing 30 tons per month. The raw (carrot) calcium electrolysis plant was supposed to be similar (or identical) to the 30-ton-per-month plant at Bitterfeld, and the distillation plant would have had to match at 25 tons a month, to give 50 to 60 tons per month uranium metal capacity. Brasted in his 1952 paper arrived at better founded, but not materially different estimates, through a more sophisticated analysis which melded actual Bitterfeld production data with the extreme design possibilities for the Russian plants. These were used in the mid-fifties as basic data.

In the estimates of 1948 and 1949 (by then these were interagency estimates by the Joint Atomic Energy Intelligence Committee under CIA chairmanship) the prime conclusion was that the Russians were headed at least for a plutonium bomb. Even as late as the mid-1949 estimate, it was recognized that if the uranium fuel were irradiated at reasonable values to yield between 200 and 400 grams of plutonium per ton of uranium, then one could assume by analogy a Russian

**Soviet Atomic Science and Engineering*, p 48 (Atomic Energy Publishing House, Moscow, 1967).

long-range target of perhaps two 250-MW graphite-moderated Hanford reactors using about 500 tons of uranium per year, with an additional 200 tons for initial loading. These numbers always conflicted with the uranium ore estimates, which tended to be lower. In mid-1949, for instance, the distilled calcium stockpile of 680 tons easily could have produced 1500 tons of uranium metal, compared to the 850 tons probable and 1320 tons possible of uranium as judged from the ore estimate. So in the mid-1949 estimate, one 250-MW Hanford-type graphite-moderated reactor was assumed as one alternative, a heavy-water-moderated reactor being assumed as the other one. Any additional later reactors were subsumed in the stated errors. These estimates consistently placed mid-1950 as the earliest possible date for the first Russian nuclear test, with mid-1953 being more likely. The general feeling that the first Russian pilot reactor went operational in mid- to late-1947 was, of course, crucial to the minimum estimate.

Actually, from the quantitative point of view the estimates weren't too bad: we learned in 1956 that the first reactor had been a graphite-moderated 100-MW reactor. This one was soon followed by additional reactors reaching perhaps a total of 700 to 800 MW by the mid-fifties. None of us even guessed in 1949 that that "possibility of doubling the calcium chloride plant" was the clue we really should have followed in long-term thinking.

Inasmuch as the final Russian calcium specifications and the American analysis of the Bitterfeld calcium product both were adequate for graphite-moderated reactors, it is odd that no conclusion was ever drawn that the initial reactor must be of that type, especially as it was known that heavy water reactors could tolerate many more impurities. The reason, I suppose, was that having learned how to make really pure uranium, we would and did use that purity for our heavy-water-moderated research reactor in Chicago as well as the graphite-moderated production ones at Hanford. We assumed the Russians might well act the same way. But the reverse would not have been true. Had the Russians established "reasonable" specifications for heavy water reactors, the resultant uranium would have stopped a graphite reactor in its tracks. In actual fact, the Russians did not even get a heavy-water-moderated research reactor operating until 1951.

I believe we were correct—with regard to the estimates—in assigning the cessation of calcium distillation at Bitterfeld between 1 July 1948 and August 1949 to the start-up of the Russian distillation plant, and simply putting the equivalent uranium into stocks during the rather high distillation rate in the August 1949–November 1950 period. I'm sure that widely varying needs for stocking reactors or letting them "cook" for an initial period of four to six months without any additional uranium played its part in the actual course of events. However, without additional specific data, or at least hints, it is pretty hard to take these vagaries into account.

Operation Spanner is perhaps of more interest: in the spring of 1948 Eric Welsh was musing along with Charlie Campbell—presumably, as usual, about which way the cat would jump—when he broached the idea of sabotaging the Russian plutonium effort. Both were aware of the Russian specifications on distilled calcium which called for less than one part per million of either boron or cadmium. These substances simply soak up neutrons, thereby tending to stop nuclear reactions. Welsh was all for dropping in a pinch of boric acid and "buggering the works." But he had been a chemist and was afraid his man at Bitterfeld would be caught through routine batch analysis. Then Charlie Campbell remembered that in 1944 the Manhattan District had a really secret plant for making boron enriched in the neutron-catching boron-10 isotope. In nature,

boron contains 20% boron-10. The Manhattan District product was nearly 90% pure. It should work fine.

Col. Seeman liked the idea and arranged for discreet inquiry to be made at the appropriate levels of the AEC. The answer came back they'd be glad to loan us some excellent material, but if we lost any of it, not to admit it!

Then came the usual period of agonizing detail. British analysis of 1947 Bitterfeld calcium disagreed with U.S. analyses in the boron content. Who really knew how to analyze accurately for boron, and could the other laboratory learn to reproduce the method? What really was the accuracy of the Bitterfeld analyses? How much boron would they really let pass? Could anyone obtain reliable isotopic analyses of millionths of a gram of boron?

Professor A. J. Demster of the University of Chicago indicated he had just received a mass spectrograph which could handle these small amounts of boron. He knew a microchemist who could prepare the samples for his analysis. It was agreed on 11 June 1948 to exchange old Bitterfeld calcium samples with the UK, and analyses as well.

All went well at first. The analytic problems were worked out. The amounts of enriched boron per batch of distilled calcium were worked out. Calculations indicated there was some risk, but the Russians would be hard put to conclude anything except some extra boron contamination in the billets—there was no indication they had mass spectrographs of the sophistication of Prof. Demster's. If, however, a simple neutron absorption test—a routine test in the U.S.—were performed, it would reveal that a whole batch of uranium (that made from the contaminated calcium) was bad. It was decided to go ahead. The sabotage chemical was transferred without records to us, and then on to the British.

Then calcium distillation at Bitterfeld stopped for a year. Welsh's agent started to worry. On grounds that he would be caught, he refused to add to the raw calcium the amount of enriched boron needed to make sure that enough contamination would pass through the distillation process into the uranium.

Finally, in August 1949, the Russians detonated their first plutonium bomb secretly. The Air Force Technical Applications Center intercepted the radioactive debris almost by happenstance. The July issue of *Novy Mir* carried a symbolic poem:

> "You shuddered. The distant hollow rumble of your carriage sounded like a wind.
> Sleep my baby. . . .
> At the pre-arranged hour, the explosion occurred.
> The granite was blown asunder to dust,
> The Taiga around the mountain was illuminated
> By golden radiance. . . .
> . . . Sleep, my baby"

Admiral Hillenkoetter, Director of Central Intelligence, established a Nuclear Intelligence Panel to determine why we had estimated mid-June 1950 as the earliest possible date, when in fact it occurred in August 1949.

There was no longer any sense to Operation Spanner. The prepared material came back from Bitterfeld to London to Washington, finally being reinserted onto a shelf somewhere in the AEC, again without any paper work. All was as if it had never been. The Russians put the Bitterfeld calcium plant in mothballs in December 1950. In September of 1961 I threw the last pieces of Bitterfeld calcium into the Potomac River, watching the water boil with the reaction.

Practice to deceive

DECEPTION

In April 1972, the Joint Chiefs of Staff sponsored a week-long Strategic Planning Seminar concentrating on the question of deception. Seminar presentations by participating U.S. Government departments and agencies, and by the Syracuse University Research Corporation (SURC) under contract to the Advanced Research Projects Agency, have been summarized in JCS's *Strategic Planning Seminar 17–21 April 1972, Vol. I* (SECRET/NO FOREIGN DISSEM). They appear in full in a 525-page *Volume II* which is TOP SECRET/NO FOREIGN DISSEM. *Studies in Intelligence* reproduces the presentation made by Euan G. Davis, Director of the National Indications Center, and prepared in collaboration with Cynthia M. Grabo of the NIC staff, because it relates the question of deception and the entire scope of the seminar to the intelligence warning function.

As an introduction, we also summarize a preceding paper by Prof. Barton S. Whaley, of the Fletcher School of Law and Diplomacy at Tufts University, on *Deception and Surprise—the Lessons from History.*

Dr. Whaley has analyzed the element of surprise in 168 battles in 17 wars from 1914 through 1968.* He comes up with some impressive statistics on the efficacy of surprise:

> Out of 50 battles in which intense surprise was achieved, 17 far exceeded the objectives of the initiators, and only one ended in defeat.
>
> Conversely, out of 50 battles fought *without* the advantage of initial surprise, 30 ended in defeat for the initiators, and only one substantially exceeded the attacking commander's expectations.
>
> The average mean casualty ratio in favor of the attacking force was 1-to-15 when surprise was achieved, but only 1-to-1.7 without surprise.
>
> How, then, to achieve the desired surprise? The classic security precautions? Dr. Whaley finds that in 61 battles which achieved strategic surprise, this could be attributed to passive security measures by the attacking force in only four instances. Of 54 cases of tactical surprise, seven at most could be attributed to effective security.
>
> Deception, however, was either the main cause or a significant factor in 82% of all cases of strategic surprise, and 57% of the tactical surprises. "The greater the effort put into the deception plan," Dr. Whaley notes, "the greater the degree of surprise gained."
>
> Thus, Whaley summarizes, "Your chances of obtaining or exceeding your goals are almost four times better if you can achieve at least some degree of surprise. Your chances of gaining surprise are eight times better if deception planning is used. And finally, you can greatly improve on even these most favorable odds, the more comprehensive and sophisticated is your deception."

Another participant in the same seminar cited a statement by Princeton football coach Jake McCandless, worthy of the late Herman Hickman: "An

*Whaley's *Stratagem: Deception and Surprise in War* was issued as a manuscript by Massachusetts Institute of Technology in 1969. It will soon be published in book form.

ounce of deception is worth a 240-pound tackle." The language of the gridiron may be unfathomable to potential enemies of the United States, but there is nothing to prevent such enemies from performing the same calculations Dr. Whaley has made, and arriving at the same attractive odds. Indications intelligence officers, accordingly, expect any opposition undertakings to seek maximum deception and surprise.

The Editor

STRATEGIC WARNING AND DECEPTION

Euan G. Davis
and
Cynthia M. Grabo

I welcome this opportunity—a rare opportunity, I might add—for some of us in the intelligence field to meet with the operational planners on a subject of mutual interest and great importance to us all: deception.

The subject is a two-faced problem. It may be important for the security and success of our own operations in many cases that we have an effective deception plan. But it may be equally important, and sometimes more important, that we understand what the enemy's deception capabilities may be and what deception he may be practicing at the moment. The latter is peculiarly the function of the intelligence community—and particularly of those elements of intelligence which are concerned with warning. For the perception of the enemy's deception plan, and even the recognition that he may be practicing deception at all, clearly is a most important element in the warning process. In some cases, it could be the most important element in warning, and particularly of strategic warning, of the recognition of the enemy's intention to attack.*

In his manuscript, Mr. Whaley identifies five general types of deception, noting that there is more than one approach to this problem. The military planner, seeking surprise, may attempt to conceal or mislead as to his:

Intention, that is, that he is preparing to attack at all.
Time of attack.
Place of attack.
Strength of the attacking forces.
Style of the attack, that is, the form the military operation will take, or the weapons that may be employed.

Now, we in the strategic warning business today are not unconcerned with matters of the time, place and strength of enemy attacks. We do deal from week to week with questions such as a North Vietnamese attack on Long Tieng, or Israel's response to new attacks by the fedayeen. We deal with these because this is the type of problem which comes up from day to day.

But this is not our primary function. Our primary function is to assess the *intentions* of our enemies to attack us at all, anywhere, at any time in the foreseeable future. We are concerned above all with whether the USSR, the People's

*On the general subject of warning, see Davis, "A Watchman for All Seasons," *Studies* XIII/2; on the timing factor in strategic warning, see Grabo, "Strategic Warning: The Problem of Timing," *Studies* XVI/2.

Republic of China, North Korea or some other potentially hostile country has begun preparations for, or has taken a probable decision to, attack the United States, our forces overseas, or one of our allies. In practice, we also are concerned with whether they might be preparing to attack someone else—with whether the USSR may attack Communist China, or invade Romania. And we also are concerned with measures short of overt attack which might gravely threaten U.S. interests or alter the balance of power—such as the Soviet attempt to introduce strategic missiles into Cuba, and the potential combat role of Soviet forces in Egypt.

In short, we are concerned above all with the strategic intentions of our enemies and potential enemies, on what they are planning to do at all, not primarily when they may do it or what forces they may commit—although we will also be concerned with that as a secondary priority.

It need hardly be said that the greatest warning failures, and greatest national military disasters, are those in which the intelligence services and/or the national decision makers failed to perceive that an attack was coming at all, and therefore had not taken the requisite counter-preparations either to forestall the attack or to reduce its impact. The recognition that Japan intended to attack Pearl Harbor or other U.S. territory at all obviously could have saved much of the U.S. Fleet. An acceptance that Communist China was preparing for a major offensive in Korea in November 1950 could have resulted in a halt to our offensive and the taking of defensive preparations against such an attack, which could have reduced its impact, and might in fact have forestalled the Chinese offensive altogether.

Since strategic warning is concerned primarily with strategic intention, it will also be concerned above all with strategic deception. In actuality, we attempt to deal with deception, no matter what form it may take. But our greatest worry must be our enemies' broad capabilities for strategic deception, the measures which they might employ and are probably holding in reserve for the day when grave national interests or even national survival are at stake. These are the measures which we have not seen yet, or only in small part. We can make some estimates, or guesses, as to what they might include from our knowledge of their military theory, doctrine and exercises, political and diplomatic practice, propaganda techniques in critical situations, and particularly from what they may have done in certain crisis situations in the past. But at best we will probably have only a vague and inadequate understanding of what the real deception capabilities of our enemies may be.

There is a widespread popular opinion that the USSR and other Communist nations are so continually engaged in deceitful practices that we never believe anything that they say, and that the intelligence analyst and policy maker alike constantly are expecting and allowing for Soviet hypocrisy in all things. This exaggerates the case. It is true that the USSR and all closed societies are highly security conscious and routinely conceal all sorts of information which is common knowledge in open societies. It is also true that Communist philosophy does not hold objective truth, as we understand it, to be either desirable in principle or practicable in application. It is further true that the historical traditions of Russia and of the countries of Asia which are Communist today are so different from ours that most of us do not really understand them, that they are from our viewpoint all more or less "inscrutable." And finally, it is of course undeniably true that it is much easier for the dictator or leadership of a closed society to plan and to implement a deception program than it is for us. In every way, from the smallest deception gimmick—such as the planting of misinformation in the

press—up to secrecy on the national decision-making process, they hold enormous advantages over us.

Now, we do expect and we do allow constantly for certain types of secrecy, security, and day-to-day deceit on the part of the Soviet Union, and perhaps even more so from the Asian Communist nations. No one expects the Soviet budget to reveal actual defense expenditures, or that the USSR will tell us the true unit designations of its forces in East Germany, or their strengths. The USSR nearly always denies travel to Western attachés and diplomats when it is deploying forces, and it has never revealed anything publicly about the buildup of its forces along the Chinese border or even that it has any troops in Mongolia. It has attempted gross deception on the strength of its strategic forces when it knew we had no means of verification. And so forth.

But the USSR is not engaged constantly in an active, positive deception program designed totally to mislead us as to its intentions and objectives. To do so would be counterproductive to its own interests, and moreover would undermine the effectiveness of a positive deception program when it would be important that we accept it. A prerequisite for effective deception is to establish some degree of credibility. The Soviet Union cannot afford constantly to lie to the President of the United States. It is only because it does so rarely that it could expect that its denials concerning the introduction of strategic missiles into Cuba would carry a degree of credibility.

To cite another and more recent example, the USSR in the summer of 1968 announced a series of military exercises in Eastern Europe simultaneous with, and as cover for, the various deployments of forces and other preparations prior to the invasion of Czechoslovakia. Many analysts accepted these announcements more or less at face value, and duly reported these Soviet "exercises" in current intelligence publications and briefings. This uncritical acceptance of these Soviet statements probably resulted in large part from the fact that for years the USSR had made a practice of announcing major Warsaw Pact exercises in Europe, and sometimes major exercises in the USSR as well, and that these announcements had always been accurate—that is, some type of exercise always had been conducted at the time and in the area specified. Thus, the analysts had become conditioned to accept this type of announcement, which had never proved false in their experience. It is of interest that this conditioning carried over even into the period after the invasion and into post mortems, some of which persisted in referring to these so-called exercises as if they had really occurred—even to the extent of reporting the alleged scenarios based on information derived entirely from the Soviet press.

In fact, the entire Soviet deception effort for the Czechoslovak invasion was elementary by any sophisticated standard. It involved little positive military deception, relatively little political deception, no disinformation effort by the KGB, and no true strategic deception, that is, no attempt at concealment of the Soviet objective, which was the restoration of orthodox Communist control in Czechoslovakia. Even military security was not drastically tightened for this operation. There were good reasons for this, which we will not go into here, but the point is that it was probably not a typical Soviet performance or representative of what the USSR might attempt in the field of deception if it were preparing for an attack on NATO.*

In the Cuban missile buildup, the Soviet deception operation was considerably more sophisticated, and more effective, than for Czechoslovakia. It was also

*For a further discussion, see Grabo, "Soviet Deception in the Czechoslovak Crisis," *Studies* XIV/1.

much more important to the USSR that the deception succeed; indeed, the success of the operation in Cuba was largely dependent on misleading the United States as to Soviet intentions. Nonetheless, the deception plan itself was not very complicated, and involved only two types of actions: the issuance of falsehoods and misleading statements, directly and indirectly, concerning what the USSR was doing in Cuba; and rigid security on the nature of the military shipments to Cuba. No one can deny that the plan was superbly executed up to the time we finally discovered the missiles. Even by Soviet standards, it was a masterpiece of security, involving not a single specific leak as to the nature of Soviet plans and decisions, or the below-deck cargoes of the ships. Nonetheless, there was little active military deception of the type which we should expect the USSR to employ in other circumstances. The measures used to conceal the movement of this relatively small military force give us only slight insight into what the USSR might attempt in the way of security and deception in the event it was preparing for a major military operation against NATO, or even Communist China.

Those of us in the warning business are concerned about how little we know of—and how little research has been done on—the deception practices and capabilities of our potential enemies. We feel that we have not seen anything yet, and we are only slightly consoled by Mr. Whaley's conclusion that the USSR has shown itself to be relatively unsophisticated in deception—at least in comparison with Great Britain and Israel. This may be true with respect to what they have revealed to us so far, but there have been some clear indications that Soviet planners and theoreticians are studying the problem. It would be foolish to conclude that the USSR has not learned some lessons from some recent successful deception operations and surprise attacks—including the Israeli blitz in the Six Day War.

I would like to take a few minutes here to explain briefly how warning or indications intelligence actually functions in the U.S. intelligence system today. There is widespread misunderstanding on this, and it is important to set the record straight. Contrary to what many believe, warning intelligence is not a separate element of the intelligence community. It is not to be compared with current intelligence or estimates or military capabilities offices, all of which have large staffs which turn out finished intelligence in large quantities and which are the recognized experts in their fields. There does not exist in the intelligence community a semi-independent group of indications and warning analysts who report their analysis and conclusions to higher authority. There are in the office which I head, the National Indications Center, nine analysts plus a director and deputy director who may be classified as indications analysts. A very few of them have had enough experience that they might be said to be experts on the subject of warning, insofar as there are any experts on this subject. The major intelligence agencies, CIA, DIA and NSA, have very small indications staffs—three or four people, literally—who serve as liaison and coordinating staffs and provide the administrative support, and sometimes the members, for the U.S. Watch Committee. But the substantive intelligence and backup for the Committee and for the warning effort is drawn from the regular intelligence elements of these organizations. This involves primarily their current intelligence personnel with such backup and expertise as may be required from other components of the organization, such as order of battle, technical intelligence, and so forth.

The National Indications Center produces indications or watch items in draft form for the weekly Watch Report, and it has turned out a variety of other indications papers and analyses. But the final review, revision or acceptance of these drafts is a community function. The Watch Report, and such other papers as may be approved by the Watch Committee, represent a community view,

and it is as such that they are forwarded to our immediate superior, the United States Intelligence Board, for consideration and approval.

Thus, it will be evident that indications analysis, and with it deception analysis, is widely diffused in the intelligence community. There are both advantages and disadvantages to this. The primary advantage is that the substantive knowledge of numerous desk experts is brought to bear on the warning problem. The primary disadvantage may be that these substantive analysts, qualified as they may be in their fields, may not necessarily know much about indications analysis, still less about deception.

The average U.S. intelligence analyst today is almost totally unprepared to cope with an enemy deception effort—and this will likely be true also of his supervisor and the policy planner. Our experience of recent years justifies a conclusion that the U.S. Government, at both its intelligence and policy levels, is vulnerable to deception. Is there anything we can do about this, or must we resign ourselves to the fact that the masterful enemy deception planner almost surely will succeed?

The information scientists have offered some suggestions that various analytic techniques will help the analyst in such circumstances, such as Bayes' Theorem.* The computer people and particularly salesmen for the computer companies have been leading proponents for the argument that various types of ADP systems are the answer or at least partial answer to our problems.

I am not trying to disparage these efforts. I believe, however, that there are some other methods which are even more important and which we should be considering first. And, furthermore, they won't cost much money.

One major reason that analysts and their supervisors alike are so little prepared to deal with live warning crises and enemy deception is that they lack experience with such problems. They have neither learned the lessons of history from a live experience with a warning crisis—and nothing really can take the place of the live experience—nor have they had any education in intelligence or military schools of the nature of such problems and how to cope with them. Analysts receive some training, and often extensive training, in almost any other field of intelligence before they are permitted to proceed on their own. No one would dream of turning an order of battle analyst loose without some training in the traditional and venerable techniques by which enemy units are finally "accepted" into the order of battle. It is ironic that in the field of warning—which is both the most important and the most difficult of intelligence functions—there is little provision for the training of analysts.

Unlike other established fields of intelligence research and analysis, the chances for on-the-job training in indications and warning are poor. Unlike other fields in which there is a continuing flow of live and pertinent information from which the analyst can learn, the true warning problem from which the analyst may gain experience is infrequent—and, with the relaxation of tensions with both our major Communist adversaries, it is likely to become more infrequent. Aside from the continuing indications problems in Southeast Asia and such relatively minor conflicts as the Indian-Pakistani war last December, the intelligence system has not had any significant warning problems since the Sino-Soviet border crisis in 1969, which did not lead to major conflict. In 1968, we had a major warning problem—the invasion of Czechoslovakia. The last significant indications problem prior to these was the Arab-Israeli conflict of June 1967. Note that these various areas are widely separated, and that few current or order of battle analysts would have researched more than one of these problems, and

*See Jack Zlotnick, "Bayes' Theorem for Intelligence Analysis," *Studies* XVI/2.

that only a handful of so-called indications analysts in the government have an appreciation of the information which was available in all of them. Virtually none of the analysts working these warning problems in different areas profited from the experiences of the others. Although there is much to be learned on both warning and deception from all of these crises, almost none of the benefits of such an education have accrued to the intelligence community as a whole.

Nor are the intelligence schools doing much to make up this serious gap. Until now, the Defense Intelligence School has offered scant training for analysts on indications and warning, although some of the lectures are highly valuable and related to this subject. At least on the overt side of the house—I do not speak for the covert—the Central Intelligence Agency also is offering minimal training for analysts in warning and the perception of the intentions of the enemy. Very little has been written in the way of training manuals or theory to help the analyst understand what warning is all about, and how indications analysis may differ from current analysis in a crisis situation.

Even our military libraries have done little to help the analyst find the relevant historical literature. There is not a single entry in the card catalogue of the Pentagon Library under either Warning or Deception.

This is one reason that Mr. Whaley's manuscript, hard as it is to obtain, has been so widely read, and that its publication is so eagerly awaited. Some of us want to make it required reading in the intelligence community. I am happy to say also that the Defense Intelligence School next year is planning to incorporate a little more instruction on both warning and deception. So some progress is being made, even though we still have a very long way to go. And, perhaps almost more important, we need to find some means to insure that the supervisory levels and consumers of intelligence, including the operational planners, have a better understanding of both warning and deception, and of what they can reasonably expect and should be asking from the intelligence community.

Some great strides have been made in intelligence collection in the past several years. Although we have lost some good sources, we are also technically better off than we have ever been to provide some of the hard military data on enemy forces which the planner needs. There has also been a considerable improvement in the sophistication of the human collectors, particularly in the weeding out of unreliable sources and the more careful evaluation of material. This we owe primarily to the CIA.

Insofar as warning has failed us over the past decade or so—and failure is a relative term—it has not been just for want of data. This does not mean that collection has been perfect, or that we could not have done with more high-class information, particularly some penetration of the enemy's decision-making councils. But usually, we have had lots of information and lots of indications which would have pointed to the final action as a reasonable, if not likely, course of action. In large measure, our problem in all crises is one of analysis of the data, of perception of what the enemy is most likely to do. Some fundamental analytic errors have been repeated in more than one crisis. At the same time, the growing size of the intelligence establishment, and the number of echelons which separate the working-level analyst from the people at the top, have made it increasingly unlikely that the minority view, and the facts and indications which might support that view, will filter upward to planner and policy maker who may most need to know them. Only those who have worked a live warning problem at the desk level can appreciate how many indications get lost in crisis situations and are never reported to the higher levels of intel-

ligence, let alone the policy maker. This is likely to be true particularly if the view is unpopular or contrary to the accepted "climate of opinion." Needless to say, the analyst who may perceive the enemy's deception plan will quite likely be in the minority.

Particularly in crisis situations, it is imperative that something be done to bridge the gap, one might say chasm, which so often separates the intelligence analysts and the policy planners. We need to improve the communications between them—so that the operational planners on the one hand will know better what information the intelligence analysts really have at hand and what they really think, and whereby the intelligence system on the other hand will better understand what the operational level really needs to know. It would probably be too much of a breach of the bureaucratic process to suggest that analysts and operational planners just talk to each other informally, although this might be tried as a last resort. There are at least two other means which will help, however.

The first is for the operational level to ask the right questions, including requests for detailed listing or analysis of all available indications. No amount of diligent initiative at the working level will begin to take the place of the right questions from the top. Where there is reason to suspect enemy deception—as there will likely be when a crisis is brewing—specific questions from those who have experience in deception may elicit useful information which would otherwise not be reported to them.

Secondly, the operational level must recognize that intelligence cannot always anticipate its needs if it does not know what the operational people are doing. The secrecy which surrounds most operational planning and of necessity will surround any deception plan may present critically difficult communications problems. The analyst who does not know that anything is going on by our side will tend to overlook or set aside information which he would regard as important, perhaps critically important, if he really knew what was happening. There have been some historic incidents of the calamitous consequences of such a breakdown of communications between collector, analyst and planner, and the potential consequences in the future could be even more disastrous. The policy level of the government has recognized this in theory, and there is in existence a National Security Action Memorandum (No. 226) which states in part: "all appropriate departments and agencies of the Government are authorized and directed by the President . . . to keep the Watch Committee of the USIB informed concerning significant diplomatic, political, military, or other courses of action by the United States, approved for immediate implementation or in process of execution, which might bring about military reaction or early hostile action by the USSR, or its allies, thus endangering the security of the United States." Unfortunately, the existence of this directive has not guaranteed its implementation, and it has often been honored in the breach. We can only hope that the operational and policy levels of the government would recognize the importance of adequate communication with the intelligence community in a crisis where national security interests were at stake.

I would conclude by saying that this seminar in itself represents a real step forward in this field of communication between us, and that the inclusion of representatives from the intelligence community, and particularly its warning element, is particularly welcome to us. May we hope that there will be more such communication in the future. Thank you for your attention.

Through a glass darkly

THE PREDICTION OF SOVIET INTENTIONS

Robert M. Gates

The record of U.S. intelligence in anticipating Soviet tactical and intermediate-range intentions, understanding them, and putting them in proper perspective is not particularly distinguished. We were unable (except, of course, for the then DCI) to predict the Soviet intention to put missiles into Cuba until we saw the photographs of them already there. We failed to anticipate the construction of the Berlin Wall, the ouster of Khrushchev, the timing of the invasion of Czechoslovakia, and other events of importance.

More significantly, we often have failed to understand—or at least have not conveyed to the policy maker—the larger meaning of major Soviet initiatives, and to give proper perspective to Soviet actions. We were much too tardy, for example, in coming to realize the seriousness of differences between China and the USSR, and the effect of these differences—particularly in the mid-1950's—on Soviet policy. Similarly, we were slow to recognize the importance and scope of the Soviet "peace program" in the late 1960's, even after its formal approval by the 24th Party Congress.

The conclusion is inescapable that—while intelligence assessments of Soviet military and economic capabilities have been remarkably accurate—treatment of Soviet political intentions and decisions has not measured up.

Why We Have Done Poorly

Our failure to anticipate or even interpret these and other developments better should come as no surprise. It derives in no small way from the difficulty inherent in trying to predict how political leaders perceive situations, and how they will react in given set of circumstances. It is a very difficult task in a free society; it is that much harder in a closed one, where little if anything is known of the personal lives and psyches of individual leaders, or of internal battles at the top.

The Soviet Union is such a society. It has no free press to bare state secrets or personal rivalries, to expose options under consideration by the leadership, or any of the other juicy tidbits familiar to the American newspaper reader. Except for occasional glimpses in the press of internal institutional disputes, discussion of state policy and intentions is carried on in secret—and there are few leaks. Moreover, instead of a single decision maker, the Soviet system has a 15-man Politburo and a Central Committee of several hundred members, in both of which constantly shifting balances can make or undo any plan or intention.

Perhaps the most difficult challenge is analyzing correctly the Soviets' perception of problems and opportunities, both foreign and domestic. There is a wide cultural gap between a college-educated analyst in the West and the Soviet leadership. As Robert Conquest has stated, "the Soviet leaders are not to be treated as though their motives and conceptions were in our sense natural and rational. The particular leadership now in control in Russia derives from a tradition which is alien in both aim and method to our own." Not only are they

the products of a centuries-old system of absolutist rule; they are far more isolated from Western ideas and experience than even their Tsarist predecessors. Few among those at the top level have traveled widely, much less spent any period of time in the West. Their narrowness is difficult to comprehend. The Czech Communist leaders, returning from Moscow in late 1968, remarked that they had expected narrow dogmatists, but not "vulgar thugs." While that is perhaps too strong, the fact remains that our perception of situations is probably widely divergent from the Kremlin's perception of those same situations. The Soviet Union is a strange and idiosyncratic polity, not to be understood or dealt with without considerable conscious effort. And often even that is not enough.

Another factor complicating our assessment of Soviet intentions is the role of error and irrationality in the Kremlin. No political leadership is immune to mistakes and, indeed, the Soviets have made their share. Just as important, however, is the mental attitude of the leadership. For example, the Soviets, lacking reliable allies, throughout their history have had a certain siege mentality. Moreover, they are clearly concerned about their relative backwardness, a point underlined by Khrushchev's admission of a sense of inferiority over his smaller plane as he flew into Geneva in 1955 and, even now, their continued insistence on dealing on the basis of "equality." Thus, there is the possibility that after analyzing all the facts and alternatives, the Soviet leadership will react out of personal spite, a sense of psychological or cultural inferiority, or fear.

In discussing the vagaries of personality and differences in culture, we have just scratched the surface of the difficulty of predicting Soviet intentions. For example, one invaluable legacy bequeathed by Lenin to his successors was a sense of political expediency and opportunism probably without modern equal. Stalin and his successors were relieved by historical determinism of the need to be concerned about the final victory or defeat of Communism. Their main task has been the survival of the "home of socialism" and the furthering of its interests. For that task, Lenin's legacy was essential, imbuing Soviet internal and foreign intentions with almost unrivalled flexibility—and unpredictability.

Internal Politics

The changeability of Soviet intentions, foreign and domestic, is a natural product of the internal political process. The Soviet Union, like other countries, is continually beset by minor crises. In that dictatorial, ultra-centralized system, however, the number of these time- and energy-consuming problems demanding the attention of the top leadership is magnified many times. As a result, the Politburo probably can only rarely take the time—and then only some members of it—to reflect on future problems or future opportunities, and then come to a decision on how best to meet a problem or exploit an opportunity. Consequently, it seems likely that few Soviet "intentions" emerge as the result of a conscious attempt to formulate long-range or even middle-range plans.

Those few intentions which do receive lengthy consideration and require a clear-cut decision by the leadership generally concern large objectives often intimately related to economic or military issues. The difficulty in reaching decisions even on these is well illustrated by the quinquennial travail over the Five Year Plan. Bruising bureaucratic struggles over resource allocation, priorities for various industries, and even the general direction of the Plan—whether to emphasize heavy industry, the consumer, or agriculture—are involved in a preparation process drawn out over months and even years. Yet after a decision is made, the plan is still subject to alteration and modification throughout its

existence. A similarly difficult time probably attends decisions on long-range military intentions. A debacle, such as the 1962 Cuban missile crisis, can perhaps galvanize wide support to build a strategic force equal to that of the United States, but other decisions on particular weapon systems, the strength of conventional forces, and so forth no doubt call forth the worst demons of bureaucratic and political rivalry.

Less cosmic issues, however, may never demand a conscious decision by the Politburo. In many instances, plans or intentions have a life of their own, drifting along earlier guidelines until circumstances force a change. Intentions are sometimes the product of an internal political trade-off or compromise in which one faction agrees to support another's program in return for a similar favor. There are also intentions decided by events, in which the leadership finds itself in a situation where national pride, internal politics, or commitments preclude bailing out, and leave only the course of pressing ahead. Soviet Middle East policy in early 1970 could be an example of this. Moreover, some Soviet intentions probably are born full-grown because of the actions of a representative or client which the Kremlin may find either too embarrassing or too inconvenient to disavow.

Yet another element in calculating Soviet intentions is the fact that one intention can evolve into another—with attendant changes in rationale. As an example, the Moscow anti-ballistic missile, apparently originally intended as a defense of the capital against major attack, was scaled down because of its inadequacy in the face of increasingly sophisticated offensive systems. Over time, both the intention and the rationalization evolved into something quite different from those originally envisaged. A political equivalent is suggested by Soviet initiatives for a conference on European security. First broached in the 1950's as an anti-German measure, such a conference has more recently been seen by Moscow as a means to insure Soviet involvement in Europe in the future and to speed the reduction of U.S. influence and its military presence on the Continent.

A change in leadership also can significantly alter intentions, as evidenced by the replacement of Khrushchev in 1964. Soviet intentions in a number of areas both at home and abroad were modified, in some cases substantially, in his wake. Changes at even less exalted levels also can influence the direction of Soviet intentions, whether it be the death or replacement of a high-level economic baron, a military leader, or a political/party figure.

Finally, the bureaucracy can affect the interpretation and implementation of a given intention. Aside from sheer incompetence, bureaucracies can drag their feet in putting policies or intentions into practice, and can even actively obstruct the will of a political leadership—particularly if special bureaucratic interests are at stake. Moreover, bureaucratic inertia can also thwart the intentions of the leadership.

External Influences

The plans and policies of the USSR, like those of every country, are subject to external forces—the initiatives of other governments, foreign aggression, internal turmoil in client or subject states, and so forth. Soviet sensitivity to the actions and intentions of other powers is particularly acute in view of the new relationship between the United States and China.

It is frequently argued on the one hand that Soviet intentions are formed in reaction to outside influences or pressures, or on the other hand that they are planned well in advance and are ruthlessly implemented. Both of these formulations are too simple. For example, the same intention can be both reactive and

assertive, depending on the perspective. Current Soviet initiatives and intentions in Western Europe can be seen as a reaction to the Chinese problem and the possibility of closer U.S.-Chinese relations. Yet in a strictly European context, those same intentions are quite assertive. The important lesson, however, is that external influence—whether it be an opportunity to exploit or a problem to be dealt with—significantly affects Soviet intentions and substantially increases their mutability.

Capabilities

Another factor affecting Soviet intentions is that of capability. If there are no troops or installations on the Sino-Soviet border, a large-scale ground attack on China clearly is not a near-term Soviet intention; if there is no missile in service or under development accurate enough, or with a warhead big enough, to destroy a Minuteman in its silo, then there is probably no intention of a first strike. The absence of capability thus can effectively preclude intention. Unfortunately for the analyst, the reverse is not true: the existence of capability does not necessarily indicate the intention to use it. A good example of this was the Sino-Soviet border situation in 1969, when some analysts believed that the Soviets would attack China because they had the capability. This was a failure to predict Moscow's intention accurately. The task of analyzing Soviet intentions can only become more difficult as Soviet military capabilities are expanded to a point where Moscow has numerous options in a given situation.

In assessing Soviet intentions, a point often overlooked is that political as well as military capabilities must be considered. The Soviet system itself imposes certain limitations on the leadership. It would be unthinkable, for example, for the Politburo to contemplate dismantling the system of collectivized agriculture. Even though that would benefit the country economically, it is an unacceptable alternative for political and ideological reasons. Similarly, removing censorship is also beyond the political, though not the physical, capability of the leadership. The limitations posed by political and ideological capabilities—or lack thereof—often narrow the alternatives or intentions open to the ruling elite in internal affairs.

On the other hand, in the Soviet system political capabilities in foreign policy broaden rather than limit the range of possible intentions. Answering to none but those in power, inseparably tied to no ally, the Soviet Union politically is capable of justifying—and doing—virtually anything. The Soviet Union has never been inhibited from collaborating with another power because it would demand forsaking ideological principles or the interests of an erstwhile ally.

Scope and Time

So far, we have elaborated a number of factors which together make Soviet intentions extremely changeable and therefore quite elusive. Internal politics, external influences, and a host of other pressures all render "intentions," even the most fundamental, a mixed and constantly changing bag of expediency, compromise decisions, indecision, expressions of personal influence, and opportunism. As if that did not make them baffling enough to sort out, they also vary according to their scope and time frame.

The most important intentions, and therefore those relatively less flexible, are the ones concerning long-term strategy. These broad intentions are generally expressions of Soviet national interest and are consequently relatively durable and predictable—although the means of their achievement are remarkably

flexible. Attempts to achieve military parity with the United States, the political and military neutralization of Western Europe, and the military and political containment of China are examples of durable strategic intentions. The Soviets have in mind specific methods for fulfilling each, yet are aware that their accomplishment—if possible at all—will take years.

Intentions of lesser scope and of shorter range may be considered tactical, and they often relate to the specific means of achieving strategic intentions. Referring to the same examples cited above, the development of a specific weapon system such as the SS-9 is a tactical move intended to help realize the strategic intention of parity; the Conference on Security and Cooperation in Europe and discussions of mutual force reductions reflect tactical intentions to achieve West European neutralization; and the friendship treaties and military/economic aid in South Asia, a tactical means to contain China. Tactical intentions also encompass sudden military deployments, VIP visits or tours, friendship treaties, and so on. These kinds of intentions are especially subject to expediency, opportunism, and chance; they are easily altered or eliminated, and replaced by something more likely to help achieve the larger objective. They may take a few years to achieve, or only a few months.

Of course, some Soviet intentions are a mix of strategic and tactical intentions. The invasion of Czechoslovakia involved all kinds of tactical aspects, including the military preparation, the date of invasion, and political action. At the same time, however, the invasion fulfilled a strategic intention which was to limit, if not destroy, the influence of Czech reformism on the other satellites and the USSR itself.

Priority and Action

Two final factors affecting intentions need to be mentioned. Most strategic intentions are by definition vitally important to the Soviet Union. But shorter-range tactical intentions have widely differing priorities. For example, shipping Soviet military equipment and personnel to Egypt in early 1970 for a time clearly had a higher priority than sending military aid to other third world countries.

A second factor is the frequent gap between intention and action. The best-laid plans often go awry, and for a multitude of reasons intentions can fail to become accomplished deeds. Any of the variables cited in this essay can consign an intention to oblivion. By the same token, it would be attributing too much foresight to the Soviets to assume that all their actions flow from intentions, to believe that every move is calculated and planned. Often the Soviets are caught in situations not of their own making, where they must act without prior planning. Their intercession in the Jordanian-Syrian crisis in September 1970 is a good example of this, as were their reactions to the first Chinese border incursion in March 1969 and to their expulsion from Egypt in July 1972. These responses had not been programmed beforehand; they were last-minute reactions to critical situations.

All the foregoing hopefully suggests the enormous pliancy and complexity characteristic of Soviet intentions. Such intentions are decided, develop, evolve, or simply spring forth in a myriad of ways, and even the most important are subject to alteration. They clearly are not decided for the coming year or decade by 15 men in Politburo assembled and voting for the record. Soviet intentions are far more elusive, both in formulation and practice, than that. Lenin's legacy, that sense of political expendiency and flexibility, is plainly still with us—and perhaps becoming ever more important.

Our Limited Assets

The problems posed to the analyst in predicting Soviet intentions could be somewhat diminished, one might think, by the unique assets available to intelligence. Yet, in reality, our intelligence collection capabilities are not very adept at obtaining accurate or reliable information on the thinking of the Soviet leadership. U.S. intelligence resources and the overt press are best suited to collecting intelligence on military hardware and the economy. Except for occasional bits of special intelligence, defectors, and unique finds like Penkovskiy, these collection methods only rarely provide the access to the Kremlin necessary to analyze Soviet intentions with assurance. Thus, in predicting Soviet intentions, we work in an area where our special assets are of only marginal assistance.

Equally as important as our collection problems are our intellectual problems in analyzing the Soviet Union. As the Soviet leaders follow a certain policy for a period of time, the analyst perceives a pattern of response. It is within the context of that pattern that the political analyst interprets and predicts Soviet behavior. All too often, however, when the policy and hence the pattern change, there is a lag between that change and the analyst's perception of it. Indeed, where we consistently fail to measure up is in detecting such changes soon enough to help the policy maker.

What is needed, in effect, is "near-real-time" political interpretation. The analyst must somehow perceive a change in policy between the time the decision is made in Moscow and the time when that change is manifested in action—such as the building of the Berlin Wall or the dispatch of missiles to Cuba. Without our ever-yearned-for source on the Politburo, this is indeed a difficult task. And whatever chance of success we have is further diminished by the simple fact that the analyst sitting at his desk day in, day out becomes complacent, his perspective narrow, and his perceptions stale.

Our Analytical Weapons

Despite this litany of analytical woe, we are not altogether helpless. We know something, for example, about the Soviet leadership. We know something about their personalities and their methods of cooperation. We know generally how Brezhnev's techniques and style differ from those of Khrushchev, and how Khrushchev's were different from Stalin's. If we are not in the position of being able to read their intentions at a given moment, we still have a reasonable knowledge of the motivations and attitudes which will go to form those intentions. We are, it may be said, "in the position of a general, who naturally does not know his opponent's intentions, but knows the style and traditions of that opponent's army and his personal style of fighting."

In addition, as mentioned above, we in intelligence have the invaluable asset of knowing a good deal about our opponent's capabilities. Our assessments of his military strength, present and future, have been proved accurate time and again. We are helped by a detailed knowledge of his economy and its capabilities and limitations. And knowledge of the Soviet system gives us a rather accurate reading of his domestic and foreign political capabilities. We have some good insights into how that system works, into what makes it tick.

We also have the important asset of experience in looking at Soviet affairs. The lessons learned during years of analysis have been passed down, along with an enormous body of information collected on the USSR. Moreover, we have individual analysts whose long experience provides them with useful insights into Soviet actions and intentions. Finally, the ability for frequent assembly of specialists in Soviet propaganda, internal affairs, foreign policy, the military,

and the economy, to focus on a particular subject or to exchange ideas and information, gives intelligence perhaps a unique institutional capability.

Doing the Job Better

Nevertheless, in view of our past record, to say we will keep plugging away at the problem is not enough. Specific steps can be taken to improve our ability at least to offer the policy maker a more accurate appraisal of the options open to the Soviet leaders in a given situation, and to provide a better estimate of their more likely choices. Moreover, there are ways to improve our ability to understand and to report the significance of Soviet actions, and to place them in perspective in relation to larger Soviet aims.

At relatively little expense and inconvenience, the following remedial measures could be undertaken:

As stated earlier, there is a tremendous cultural and historical gap between the USSR and the West. An analyst trying to understand the mentality of the Soviet leaders or their approach to or perception of problems is seriously handicapped without some background in Soviet history and, in particular, Russian history and culture. The importance of understanding this Russian heritage in analyzing present Soviet thinking and behavior can hardly be overemphasized. Intelligence agencies should take steps to insure that future analysts have training in Russian and Soviet history and culture. Analysts now in place without such training should be sent to school to acquire it.

To encourage originality of thought and analytical imagination, and generally to stimulate greater cross-fertilization of ideas, there should be instituted a regular rotation of Soviet political analysts and supervisors among offices with current, estimative, and in-depth research responsibilities. The perspective each could bring to the others would undoubtedly improve the analysis of all.

Further to stimulate analytical imagination, originality, and perspective, periodic—but frequent—exchanges should be arranged between intelligence analysts of Soviet affairs and provocative specialists on the USSR outside the government, e.g. Adam Ulman, George Kennan, Robert Conquest. Such men are experienced and well-versed in Soviet affairs; it is a terrific waste of a valuable asset not to be able to probe their minds. While the views of some outsiders on Soviet intentions would doubtless be unorthodox, the exchanges would certainly provoke intelligence analysts to re-think their own views and allow them to pick up new ideas and information.

If the U.S. intelligence community is to retain a corps of well-trained, expert Sovietologists, it must provide material and psychological incentives for them to remain as Soviet analysts and not to move on to non-Soviet-oriented positions. Inexperience and constant turnover of analysts are hardly conducive to obtaining a better grasp of Soviet intentions. Possible incentives might include promoting analysts to higher grades without assigning them administrative responsibilities. Greater opportunities to travel and meet with other Soviet specialists would be yet another incentive, as would greater encouragement to write for outside publications and to speak before internal and outside gatherings.

More attention should be paid by political analysts to the Soviet perception of U.S. and Chinese intentions and actions. These two countries are without any question among the most significant influences on Soviet intentions and actions. We must be prepared to report that certain U.S. actions or plans will affect the Soviet leadership, and to estimate how they will affect it. At the same time, U.S. policy makers should be made aware that accurate and useful intelligence judgments on Soviet intentions cannot be made without some knowledge of the substance of high-level exchanges.

No one should be permitted the luxury of deference on substantive matters. There should be far more insistent probing and questioning at all levels to assure that all possible Soviet options in a given situation have been investigated. Those options generally outside the current pattern of Soviet behavior should be given special attention. In sum, the analytical atmosphere must be made more lively.

> Finally, a better channel should be established to convey speculative and/or unorthodox views of experienced analysts to the upper echelons of the various intelligence agencies. This might be done by means of gists of only a paragraph or two. Acquaintance with such views could provide officials with a better grasp of Soviet options and also serve to warn them of possible Soviet actions or intentions. Too often, perceptive yet highly speculative analyses remain unknown beyond the analyst level—only to be revived in a post-mortem.

Intelligence cannot realistically assure "near-real-time" identification of changes in Soviet policy. We simply are not able to read the minds of the Politburo. But we can improve our performance by encouraging fresh thinking, imagination, and originality. At the same time, we—and those we serve—should be aware of our limitations, and willing to admit that the political analyst is neither seer nor mind-reader. The most we can promise is to interpret how we think the Soviets perceive problems and opportunities, to set out fully the Kremlin's options and, after vigorous discussion, to offer our analysis on the most likely course of action. This is what we have tried to do in the past with generally unsatisfactory results. To do better, we must consider some changes in the way we do business.

THE PROBLEM OF CHINESE STATISTICS

Leo A. Orleans

The military intelligence analyst responsible for assessing the capabilities and potential of the People's Republic of China is a professionally frustrated individual. Much of his frustration stems from the basic paradox that China represents. On the one hand, it is a country that has a nuclear capability, is developing a variety of modern weaponry, and represents a potential threat; on the other hand, it is a country that is overwhelmingly rural, essentially underdeveloped, and lacking a data base that one normally expects a nuclear power to have. For all practical purposes, Peking has published no national statistics for more than a dozen years (and only inadequate ones before then). Although the experienced analysts will not use figures published in a Chinese source or given by a Chinese official without some caveat or reservation, in this age of computers the hunger for figures is so great that there is occasionally a tendency to become careless, grasp at the few figures that may be reported, and assign more validity to them than they merit. After all, one is apt to hear, the Chinese must know; they must have figures for their own use; considering the progress the Communist regime has made in other areas, isn't it almost certain that they have been able to establish a statistical system that would provide them with the necessary data?

Consideration of the problem of China's statistics leads to these conclusions:

1. It is not only Chinese security considerations that limit the flow of statistical data from China. Much of the body of data we are searching for is not available even within China, while some of the statistics necessary for the more sophisticated analysis are not even missed by Peking.
2. During the past 22 years the Chinese statistical system has had its ups and downs, but even during the better years, in most fields it was not capable of collecting and processing statistics that would meet even the minimal standards for accuracy.
3. One of the major problems has been the inadequate supply and poor training of statistical personnel, and the traditional casualness toward accuracy of figures by the masses in China.
4. These problems are so serious that even with the best of intentions, China's statistics in many fields will continue to be defective and incomplete for some time to come.

Traditional Attitudes Toward Numerical Accuracy

In order fully to appreciate the problem of statistics in China, it is necessary not only to look at the present, but also to go back in time and consider some of the traditional attitudes and concepts of the Chinese people as they relate to statistics—a problem that is completely overlooked by almost all observers of the China scene.

People rightly wince at any generalization that relates to a whole race or nationality. Nevertheless, national traits are demonstrable and scientifically

acceptable to anthropologists. One authority who described the "remarkable trait of the Chinese psychology" as it relates to numbers stated that there is a "complete indifference to the idea of *quantity* and a total disregard for any quantitative measurement in Chinese philosophical thinking. . . . The Chinese conceived of numbers as emblems . . ." [1] The wording of this statement may seem dogmatic, but the point is well taken and, over the centuries, the thinking of the elite toward numbers has permeated and, in a sense, reinforced the attitudes of the Chinese peasant. This has nothing to do with ability or some inherent racial deficiency. On the contrary, as one nineteenth century traveler observed:

> The Chinese are as capable of learning minute accuracy in all things as any nation ever was—nay, more so, for they are endowed with infinite patience—but what we have to remark of this people is that, as at present constituted, they are free from the quality of accuracy and that they do not understand what it is.[2]

The fact that the Chinese are known for talent in mathematics and other sciences should not be confused with their deficiencies in statistics. Sciences pursued by scholars are immune from the numerical nonchalance of the masses; statistics, on the other hand, are a product of many individuals, and can only be as good as the training and the attitudes of hundreds, thousands, or sometimes even millions of people responsible for collecting and processing the figures.

It is important to make a clear distinction between lack of precision and corruption. Imprecision has been a permanent characteristic, while corruption tended to fluctuate over time: ". . . there were in Chinese history a good many examples of corrupt ages having been succeeded by periods of high moral tones." [3] Foreign travelers who visited China over the past couple of hundred years were apt to complain loudly and picturesquely about corruption. Statements such as "We must ever recollect, in dealing with the Chinese, that the shibboleth of Western Chivalry—the scorn of a lie as a cowardly and dishonest thing—is to them unknown," [4] or that "the Chinaman delights in wrapping his mind in a tissue of false suggestion and deceit, for the pure love of misleading those with whom he came into contact" [5] appear in numerous accounts of traders and travelers. But a native of any country is apt to take advantage of a foreign visitor or an inexperienced businessman, so that these somewhat derogatory statements do not really apply to the disregard for precision rather than premeditated falsification.

The most concrete illustrations of the lack of precision in Chinese daily life relate to the use of weights and measures. "Foreigners find, to their great exasperation, that a foot, a pint or a pound is *about* a foot, *about* a pint, or *about* a pound." [6] The Chinese measure of area, the *mou*, varies not only from region to region, but also over time. Harvard's Professor Perkins points out some of these variations in his historical study of Chinese agriculture.[7] For example, at present the *mou* is equal to .1647 of an acre, but over the past centuries it has fluctuated between .1133 and .1669 acres per *mou*—a difference of more than 45 percent. Perkins also lists eight regional variations in the size of the *mou* that existed during the 1929-33 period alone, such as spring wheat area—.152 acres; Szechwan

[1] Amaury de Riencourt, *The Soul of China*, Coward-McCann, New York, 1958, p. 83.
[2] Arthur H. Smith, *Chinese Characteristics*, Fleming H. Revell, New York, 1894.
[3] Carl Crow. *The Chinese Are Like That*, Harper Bros., New York, 1939, p. 197.
[4] G. W. Cooke, *China*, G. Routledge, London, 1859, p. 413.
[5] Alexis S. Krausse, *China in Decay*, Chapman & Hall, London, 1900, p. 47.
[6] Crow, *op cit.*, p. 88.
[7] D. H. Perkins, *Agricultural Development in China, 1368-1968*, Aldine, Chicago, 1969, pp. 220-221.

rice region—.177 acres; winter wheat region—.205 acres, etc. In discussing this problem, D. K. Liu, Director of Statistics under the Kuomintang government, described how the Bureau of Statistics attempted to overcome this problem in its agricultural surveys:

> Since the linear units in which the local mou is measured also differ widely, a slip of paper representing the standard footrule is attached to the schedule, and the informant is asked to give the equivalent of the local unit in terms of the latter.[8]

A similar situation exists with the *catty*—the Chinese unit of weight, and the *shih*—a capacity measure. It is certainly easy to imagine the problems one is likely to encounter in any comparison of crop yield in various parts of China, when expressed in *catties* per *mou*.

In traditional usage, the Chinese unit of distance—the *li* (in theory, the equivalent of half a kilometer)—was also a flexible measure. The number of *li* between two points was often determined by the relative difficulty of traversing the particular terrain. If the road was uphill or over difficult ground, the distance was considered to be longer than if one were walking downhill or over flat land. This reflected the practical nature of the Chinese. Rather than varying the price per *li* of transporting goods over different types of terrain, they varied the distance depending on whether the porter was to walk up or downhill. Because of these local variations, the distances between several intermediate points frequently did not add to the stated distance between the two end localities.[9]

The Chinese indifference to time is, of course, much more understandable; after all, how many Chinese had watches? It was much more of a problem to visiting Westerners who were accustomed to living by the clock. In the words of one exasperated observer writing in the late 1930's, "Three o'clock does not mean to the unsophisticated Chinese the exact point when the hands of the clock stand at that hour, but a more flexible term, 'the third hour,' which is any time during the period of sixty minutes before or sixty minutes after the clock strikes three." [10]

A person's age is also treated very casually. It is true that precise knowledge of one's age is usually characteristic of a literate population. Peasants in backward societies seldom know their exact age. The Chinese system of reckoning age, which considers all infants to be one year old at birth and two years old with the coming of the Chinese new year, further complicates things. Very likely a Chinese will know the animal symbol under which he was born (which reappears every twelve years), but if asked for his exact age he is most likely to give it by "tens," e.g., 30, 40, 60, etc., or simply "a few tens" or perhaps "ever so many tens." The habit of reckoning by tens, hundreds, thousands, tens of thousands, and so forth is widespread, and extends to all types of measurements. In some instances, these general expressions of quantity may be quite adequate; in others, such generalizations seem completely out of line, but apparently greater precision is not expected and round figures adequately meet the needs of daily communication.

It could truly be said that pre-1949 China was "a land where the statistician may perish for want of a few figures, where records are more romantic than mathematical." [11]

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[8] D. K. Liu, *Statistical Work in China*, Shanghai, 1930, pp. 25-26.

[9] Smith, *op. cit.*, p. 52.

[10] Crow, *op. cit.*, p. 84.

[11] Bernard Martin, *Strange Vigour*, Kinnikat Press, Port Washington, N.Y., 1970, p. 3.

The Statistical Foundation

In moving from the past to the present, it is important to keep in mind that if the lack of exactitude is a national trait and an outgrowth of Chinese cultural and philosophical traditions (as suggested in the preceding section), it is also to some degree characteristic of the pre-modern period. Although China, with her early scientific, commercial, and cultural accomplishments, could not be considered primitive, the attitude of most of the population was nevertheless molded by living conditions under which there were neither the tools of precision, nor the need for accurate measurement. The term "statistics" itself is a phenomenon of a modern society, and there was no effort to collect "statistics" in pre-Republican China. The only conspicuous exception was in the field of population, where China has some of the oldest figures on record. Their accuracy is not relevant here; it is enough that they presumably met the need of the Chinese emperors who insisted on some form of population registers to maintain social control over the people, the better to tax them, to conscript them for military duty or peacetime labor service, and to maintain order.

It was not until the twentieth century—and more specifically since the establishment of the national government in Nanking in 1927—that the requirements for statistical data became especially apparent. No doubt influenced by Western advisers and practices, the government finally established the Bureau of Statistics in 1931 under the Directorate General of Budgets, Accounts and Statistics. Even after the creation of this bureau, however, the need for statistics was not fully appreciated by most of the policy makers, especially since the data that were available were never up to date and admittedly inaccurate. Given the indifference of the people and a lack of understanding or push from the top, it is not surprising that the statistical system never developed beyond the rudimentary stage, and almost never reached down to the *hsien* (county) levels. Under these conditions, it is not surprising that "there was no demand, and indeed no need, for accuracy and adequate coverage," [12] and that only a few of the independent government agencies, such as the Customs Bureau, the Ministry of Railroads, the Bank of China—most of them still under foreign administration—managed to collect some adequate data in their specialized fields.

Building the New System (1949-57)

When the Communists took control over the mainland of China, they were preoccupied with much more urgent problems than whether or not they had usable statistics. Nevertheless, as part of the new government structure, they did establish the Department of Statistics and some regional offices, and attempted a few national surveys, almost entirely limited to the urban economy. As a result of these nominal efforts, the new leadership quickly recognized that the statistical system they inherited was extremely weak, and that all the handicaps which kept it from becoming more efficient in the past—such as size, diversity, and backwardness of the country, and the inadequacies in the number and training of the statistical personnel—were still there for them to overcome.

The responsibility of centralizing and standardizing all statistical work in the country was finally vested in the State Statistical Bureau in August 1952. Soviet advisers who were helping the Chinese prepare for the upcoming First Five Year Plan were undoubtedly stressing the need for a system that would provide the authorities with a statistical base and make it possible to measure

[12] Choh-Ming Li, *The Statistical System of Communist China*, U. of California Press, Berkeley, 1962, p. 5. This book remains the only comprehensive study of the Chinese statistical system during the first dozen years of the Communist regime.

the accomplishments of the new economic policies. The urgency of the effort was soon to become quite evident.

Peking was very conscious that the chief prerequisite for the establishment of an effective statistical system was an adequate body of qualified personnel who would be capable of organizing and managing such a system, as well as training the necessary support personnel, formulating the problems, designing the standard forms and surveys, and analyzing and presenting the data. The small nucleus of statistical personnel was concentrated in the cities. In the rural areas, the overwhelming majority of the population was illiterate, while most of the small businessmen and petty officials who might have been able to keep simple books and records were considered too contaminated by capitalist ideas to be trusted with the responsibility for collecting and handling socialist statistics. The expanded educational facilities since 1949 might reasonably have been expected to furnish a much greater number of trained statistical personnel than had previously been available, but the supply never approached the demand. College and secondary level graduates in finance and economics (the departments charged with training statistical personnel) constituted only a small fraction of the total number of graduates, and only a small proportion of these specialized in statistics. As the First Five Year Plan (1953-57) progressed, the demand for statistical data increased, and the shortage of qualified personnel became more and more apparent.

The Chinese frequently admitted that notwithstanding the effort, the overwhelming majority of persons used in the procurement and handling of data continued to be poorly trained. The problem was summarized by Vice Premier Po I-po at the Sixth National Statistical Work Conference in October 1957:

> Most of our statistical cadres, especially those responsible for guidance work and general statistical operations, have not studied the science of statistics, and they lack a systematic theoretical knowledge of the science of statistics. This is a problem in the development of China's socialist statistical work that must be solved. In other words, our statistical workers are now just a team at a rather low theoretical level. It will be difficult for this kind of team to fulfill the important task of the nation's statistical operation as long as their knowledge is not increased through training . . .[13]

In any case, the effort to establish a working statistical system was apparenly serious enough to show some moderate results. Gradually the organization worked its way down from regional and provincial levels to the *hsien* and *hsiang*, although there is no doubt that it continued to be much more effective in the cities than in the countryside. Despite the many problems admitted by the Chinese themselves, by 1957 (the end of the First Five Year Plan) the embryonic statistical system matured to provide the leadership with some of the best statistics that had, until then, been available to any Chinese Government. This may be damning with faint praise, but it was nevertheless impressive progress.

The Great Leap and the Undoing of Statistics

The effects of the Great Leap Forward on the Chinese economy in mid-1958 and particularly on the statistical system are well known to anyone who has been following the developments in the People's Republic. The hope for an economic miracle was shattered by the unrealistic goals, poor planning, ineffective management, and uncooperative weather. At the same time, the statistical system, painfully built up during the preceding five years, also fell by the wayside. Politics were in command, and professionalism of any kind was denigrated. Plans and

[13] *Tung-chi Yen'chiu* (*Statistical Research*) No. 1, 23 Jan 1958, Joint Publications Research Service (JPRS) 960-N, 15 Dec 1958.

quotas had to be fulfilled and overfulfilled, and fantastic statistics were reported by the press and the radio to prove these accomplishments. Chia Ch'i-yun, the new head of the Statistical Bureau, expressed the temper of the times as follows:

> Statistical work is a weapon of class struggle and of political struggle. Our statistical reports must reflect the great victory of the party's general line and the progress of all the works guided by the party. They certainly should not be a mere display of objective facts.[14]

And they were not.

Because the creation of the communes in 1958 and the sweeping rural reorganization that they represented were particularly disruptive to the still shaky rural statistical system, the best-publicized piece of Leap Forward exaggeration was related to the production of grain. Grain production in China in 1957 was reported to be 185 million tons; the Great Leap called for a doubling of this figure. Anxious to fulfill and overfulfill this goal, the Ministry of Agriculture (in charge of these statistics) reported an incredible increase in production to 375 million tons for 1958; and only in August of 1959 was this figure finally revised back to 250 million tons—still much inflated, but certainly a more credible figure. A similar sequence of events occurred in the reported production of many other agricultural and nonagricultural goods.

During the Great Leap, the regime also managed to "solve" the shortage of statistical workers. The responsibility for statistics was shifted from the small number of trained personnel to the "broad masses" and, in the final analysis, to the political cadres. Attacks on intellectuals and experts paralleled a campaign to instill in the masses a belief that with proper political thoughts there was nothing beyond their reach. To prove to them the fallacy of the statement that "statistical work can only be done by a few experts and is beyond the ability of the masses," the regime initiated an extensive training program. Representative both of the nature of this program and of the statistics of the period were reports that "statistical personnel" in Shansi Province alone were increased from 10,000 to 110,000 in a few months,[15] or the boast that "tens of millions of people participated in statistical work" throughout China.[16]

Summary of the First Ten Years

Here, an evaluation of the first ten years of the People's Republic of China seems in order. We know that the statistical foundation in 1949 was barely functional, that Peking made a valiant but less than successful effort at overcoming innumerable problems in trying to establish a statistical system, and that what little was built up was knocked down during the Great Leap. The problems that were encountered and the status of the system itself are best described through quotations from Chinese publications.

As already mentioned, the First Five-Year Plan placed heavy demands on more and more people for an ever greater amount of information. A good example of this acceleration, as well as the complexity and confusion that had become

[14] *Chi-hua Yu T'ung-chi* (*Planning and Statistics*) No. 8, 1959, as quoted in *China News Analysis*, No. 324, 20 May 1960.

[15] *Chi-hua Yu T'ung-chi*, No. 5, 23 May 1960. JPRS 4067, 1 Oct 1960.

[16] *Ibid.*, No. 2, 23 Feb 1960, JPRS 4023, 7 Sept 1960.

part of the statistical system, may be seen in a statement of the Planning Department of the Ministry of Construction:

> Forty-one statistical forms and regulations are required from each construction organization and its related industrial enterprises. . . . The tables now used for periodic statistical reports by this Ministry (excluding annual reports and occasional surveys) are of 15 kinds and total as many as 118 pages. In addition, a construction organization has to prepare at least 12 other kinds of statistical reports of 200 pages for labor and personnel departments, provincial or city statistical offices, and plant administrations. Furthermore there are too many details required. For instance on the grade-table of types of construction there are 74 items to be filled in. Ministry offices need 15 days to post these items in the proper books, to tabulate and audit them. . . .[17]

The imposition of unrealistic statistical requirements during the 1950's was apparently just as prevalent in the rural areas, where persons qualified to fill out the forms were even more difficult to find. It is therefore not surprising that:

> According to a survey conducted in 1955, of 1,023 reports submitted to the State Statistical Bureau, a total of 596, or 58 percent, were late. A total of 71 percent of the trade statistical reports and 87 percent of the finance reports were overdue. . . . In 1955, for example, of the 141 agricultural reports submitted to the Peking Municipal Statistical Bureau, 80 percent were delayed; of the total of 642 agricultural reports submitted to the Honan Provincial Statistical Bureau in the same year, 72 percent were late and 9 percent were not submitted; of the 650 mutual aid and cooperative reports, 63 percent were late and 10 percent were not submitted.[18]

The great burden on the less than competent statistical personnel to fill out the numerous forms and to submit them on time was further exacerbated by the persistent pressures to report only statistics of achievement. Each enterprise, be it urban or rural, was assigned a stated production goal which had to be met; each statistical report, in effect, was a report card. The degree of fear and insecurity of officials at the local level varied over the years, but it was always there. Personal advancement was likely to take place only with proven achievement. Once again quoting Vice Premier Po I-po at the Sixth National Statistical Work Conference in 1957:

> Statistics must reflect actual conditions. I have been told that in reporting their material inventory to higher levels, some provinces did not honestly declare the entire inventory. This might be due to the shortcomings of our work in the past, for the comrades at the lower levels feared that if they declared the entire inventory we would order them to transfer the stock for other purposes or would refrain from distributing new material to them. . . .[19]

All these factors contributed to a situation in which accurate reporting became the exception rather than the rule. Referring to reporting failures in 1959, Hang Chien-chih, Chief of the Division of Agricultural Economics of the State Statistical Bureau, complained that "the handling of statistical work in a perfunctory way must be avoided; it is wrong to set up a primary record merely for the purpose of filling out statistical tables required by higher offices."[20] An article in a statistical journal pointed to still more serious dereliction in "working style":

> There are still districts and basic-level units which neglect the accuracy and reliability of certain urgently needed statistical data. . . . Falsification and blind estimates must be resolutely curbed. . . . Crude methods and lack of responsibility should be checked and corrected. The working style marked by crude work and irresponsibility is serious in some departments of statistics. . . . They go after quantity and neglect quality.[21]

[17] *T'ung-chi Kung-tso* (*Statistical Work*) No. 10, 29 May 1958.
[18] *Ibid.*, No. 10, 29 Apr 1956, JPRS 678, 29 Aug 1958.
[19] *T'ung-chi Yen-chiu*, No. 1, 23 Jan 1958, JPRS 960-N, 15 Dec 1958.
[20] *Chi-hua Yu T'ung-chi*, No. 2, 23 Feb 1960, JPRS 2023, 7 Sept 1960.
[21] *T'ung-chi Kung-tso*, No. 23, 14 Dec 1958.

A newspaper article saw one cause of unreliable statistical reporting in the fact that "some people are of the opinion that a slight discrepancy in figures does no harm, and that an inaccurate figure is better than not having any figures at all, and they therefore adopt unscientific methods for working out statistics by making rough estimates, by averaging, or by reasoning."[22] But perhaps the most damaging admission came from the Party Secretary and Governor of Shansi Province, at the provincial statistical conference in 1959:

> At present, in some places and some fields, statistical figures are so lacking in accuracy, with estimates made without the necessary basic data, that some figures are changeable at will. In some cases the statistical worker, afraid that he might be criticized for rightist conservatism, even prepared two different sets of figures representing two different levels of growth and let the user choose between them.[23]

These citations are most descriptive of the conditions prevailing in the fifties and need little additional comment. The only question a reader might ask is how representative are they? The answer must, to some extent, be subjective, but considering the volume and sources of criticism, there seems to be no alternative to accepting them as both valid and representative.

Rebuilding the System

Partly because of the destructive effects of the Great Leap policies on the statistical system, and partly to conceal the effects of that period on the economy, a virtual blackout of statistical information began in 1960. Any quantitative analysis of the developments in China has had to rely on reported claims, on meaningless growth rates, but most of all on the finely developed art of reading between the lines of Chinese publications. The same situation holds for any effort to describe the developments in the statistical system itself. It has to be much more speculative and intuitive, relying only on occasional tidbits that relate specifically to record-keeping.

The national crisis that came about as an aftermath of the Great Leap appeared to be so severe at the time as to have long-term effects on the economy of the People's Republic. Despite the many dire predictions, however, China managed to pull out of the trough much more rapidly than anticipated by most observers. By 1963 the economy was back to its pre-Great Leap level, but notwithstanding a concerted rebuilding process, the disruption of the statistical system was too severe to permit such rapid recuperation.[24] Nevertheless, professionals were once again essentially in control of the nation's development, most of the statistical workers did return to their positions of authority, new efforts were made to have statistics reflect a modicum of reality, and schools resumed limited training in statistics. Chances are, however, that because of other priorities, the supply of trained personnel in the 1960's continued to be well below the demand, even though China's statistical system does not rely on persons with higher education to the same degree as Western systems. The number of persons with higher degrees in statistics was very small, probably not more than a few hundred a year between 1961 and 1966.[25] A considerably larger number must have graduated in statistics from full- and part-time secondary

[22] *Ta-Kung Pao*, 30 July 1960, Survey of China Mainland Press No. 2803.

[23] *Chi-hua Yu T'ung-chi*, No. 6, March 1959, as quoted in Choh-ming Li, *op. cit.*

[24] See Arthur G. Ashbrook, Jr., "China: Economic Policy and Economic Results, 1949-71," in *People's Republic of China: An Economic Assessment*, Joint Economic Committee, Washington, 1972. p. 47.

[25] From 1961 through 1966 China graduated an annual average of about 5,000 persons in finance and economics from her universities. Considering the diverse programs included in this department and the numerous national requirements, an estimate of "a few hundred" seems reasonable.

schools, but inasmuch as there are literally millions of basic accounting units in China, their number in terms of national needs would also be quite inadequate.

The problems of resurrecting the still shaky statistical system after the severe damage it suffered during the Great Leap were extremely difficult to overcome. As might be expected, the urban-industrial system of statistics which was more solidly based in the past revived more rapidly; in the rural areas, progress in establishing or revitalizing any statistical controls was much slower. Most of the complaints were familiar, differing little from those heard in the 1950's. Probably quite typical was the situation in one commune in Szechwan Province where in 1962 "of the sixty-one production teams in the commune, only a few had sound records. Most of them had incomplete books."[26] The plan throughout China was to convert to a much simpler "Chinese bookkeeping method" which the peasants would easily understand, but apparently a couple of years later most of the country continued to have difficulties with statistics. Blaming all the problems on class enemies was no solution. In the fall of 1965, the Ministry of Agriculture, the Ministry of Finance, and the Agricultural Bank of China called a joint national conference on improving the accounting systems of rural communes and production teams. The commune members who participated in this conference scored the "three excesses"—too many accounting books, too much to learn, and too much repetition of accounts—while the conference called on the communes "in a revolutionary spirit, systematically, and group by group—to improve and change the clumsy and cumbersome accounting system into a simple, appropriate, easy-to-grasp accounting system." [27]

The Cultural Revolution and Since

The "clumsy and cumbersome" accounting system was never changed; instead, it had to undergo yet another setback with the initiation of Mao's Cultural Revolution. There is little doubt that for approximately three years, from mid-1966 to mid-1969, the conditions on the mainland were not conducive either to the collection of statistics or, for that matter, to concern about them. Just as during the Great Leap, much of the responsibility for recordkeeping was again shifted to the workers and peasants:

> Before the Cultural Revolution, the factory relied on a few people to do the accounting "behind closed doors." After the Cultural Revolution veteran workers were put in charge of economic accounting (worker accountants), thus solving many of the problems in the plant's system of economic accounting.[28]

At the same time, most of the professional statistical personnel once again had to undergo the cleansing experience of labor, and the Chinese made a strong case for the advantages that accrued to the statistical system by giving statistical personnel firsthand experience in production:

> There personnel have corrected their past erroneous attitude of being divorced from labor and have taken part in labor together with the workers. They have concentrated their efforts on accounting work at the squad and group levels. They have helped to improve the accounting system, to reduce the number of accounting items, and to simplify accounting procedures.[29]

Even though it may be quite true that both the professional statistician and the squad record keeper benefited from such an experience, on balance the Cultural Revolution must have been more destructive than beneficial to the gathering and processing of statistical data, so that China was once again faced with a rebuilding process.

[26] *Ching-chi Yen-chiu (Economic Research)*, No. 2, 1966, JPRS 34,873, 4 Apr 1966.
[27] *Jen-min Jih-pao (People's Daily)*, 28 Oct 1965, JPRS 33,020, 24 Nov 1965.
[28] *FBIS*, 15 Mar 1971.
[29] *FBIS*, 3 June 1971.

In 1970, Peking saw fit to release some production figures for the first time in ten years,[30] but this is not enough evidence to presume a smoothly operating statistical system. In the first place, the figures were admittedly estimates, and in the second place, production figures of such goods of primary national significance as steel, crude oil, chemical fertilizers, and cotton cloth would most likely by-pass the Statistical Bureau and move up through the channels of the appropriate Ministry. In terms of statistical efficiency, these figures are more than counterbalanced by a New China News Agency report a few years earlier that "in China's vast rural areas, there are some 70,000 or 80,000 people's communes"[31]—clearly showing either the ignorance or the statistical indifference of a major central agency about what would seem to be a most elementary piece of information.

In 1958 there were complaints by statistical cadres "that there is not much to be accomplished in statistical work, that the usefulness of statistical data is limited, and that there is not much prospect for those engaged in such work."[32] More than a decade later, after several years of criticizing the bourgeois statistics of the pre-Cultural Revolution period, the complaints sound all too similar:

> . . . some comrades at present have only a vague understanding of statistical work. After criticizing and repudiating the revisionist theory that statistics are able to do everything, some departments and factories have again generated the theory that statistics are useless. . . . One must never consider correct statistical figures as merely a game with figures.[33]

Apparently the regime has encountered serious problems in its efforts to have enterprises provide the necessary statistics. From some of the official statements, it may be assumed that in terms of personal security, the managements of many plants and factories feel that in the long run it is safer to submit no figures than to risk possible critical reaction to the reported figures. They argue that statistics are useless because "production can be carried out without statistics, and it is success in production that counts."[34] One of their fears has undoubtedly been generated by the struggle against "economism" and the criticism of "profits in command" philosophy. Since profits can only be identified through records, it is safer not to keep records. As a result, there are:

> . . . no plans regulating income and expenditures, no record of working hours, no cost accounting records, no control of supply or materials, and no quotas on consumption, the lack of which cause serious losses to the state.[35]

How widespread or how lasting this attitude was is difficult to determine, but undoubtedly it was of considerable concern to the regime. It is interesting to note, however, that in trying to correct these problems and in urging the enterprises to keep accurate records, the authorities do not even mention the need for national statistics for economic planning purposes:

> Can we do without economic accounting? No, we cannot, because this would cover up shortcomings in enterprise management, cover up the difference between the advanced and less advanced, cover up the struggle between the two classes and the two lines in the enterprise and even open the door to waste, corruption, and theft.[36]

It certainly would appear that the Ministry of Accounting, which authorized this statement, is concerned about records more as a means of control than as a means of obtaining important data.

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[30] These figures were reported by Edgar Snow after his conversations with Chou En-lai.
[31] *FBIS*, 24 Mar 1966.
[32] *T'ung-chi Yen-chiu*, No. 1, 23 Jan 1958, JPRS 960-N, 15 Dec 1958.
[33] *FBIS*, 1 Nov 1971.
[34] *FBIS, Ibid.*
[35] *FBIS*, 3 June 1971.
[36] *FBIS*, 8 Dec 1970.

Thus, the battle for statistics continues. While trying to overcome the many difficulties inherent in any effort to establish a statistical system in a large, densely populated, developing country, the regime periodically introduces artificial crises, which may do wonders for the political purity of the masses, but only retard any progress in the accumulation of meaningful and useful figures. It is probably fair to suggest that it will be a long time indeed before any kind of statistical data reported by China would be accepted by professional observers without subjecting the data to considerable scrutiny.

Evaluating the Present

Before considering the situation as it now exists, it is probably in order to repeat an earlier caution that the focus of this paper is on statistics and not on science. The fact that the Chinese are apparently producing general-purpose digital computers and that scientists are doing research on lasers, super-conductivity, nuclear magnetic resonance, and other contemporary fields of science has absolutely no transfer value when it comes to the collection and processing of statistical data.

In reading about China's efforts to develop a viable statistical system, it must be fairly apparent both from the discussion of the problems and from the cited examples that among the many impediments, one of the most prominent continues to relate to the original theme of this paper—the attitude of the Chinese people toward numerical precision. The feeling that "a slight discrepancy in figures does no harm" remains prevalent in China, particularly at the lower levels. And yet, statistics do originate at the bottom, and their accuracy rests with the workers and peasants who—with minimal formal education—are responsible for the records of a particular production unit, or of one aspect of its operation. It is these millions of part-time reporters and handlers of statistics who are asked to "create" primary data, supplying certain basic figures or entering them on specified forms. It is therefore important to return to the question of attitude and consider why in a country where everything else seems to have changed during a twenty-year period, the regime has apparently been unable to change, to any significant degree, the casual approach to statistics on the part of the masses.

The answer to this question is not obvious; at least in theory, it is easier to find reasons why China's statistics should be much improved. Foremost among them is the increased literacy rate. Despite its ups and downs, over the years the expanded educational system has absorbed a large proportion of children of primary school ages, and it is estimated that four-fifths or more of the population over 15 and under 35 years of age are now able to read basic texts. With increased literacy, there should be an increase in the individual's facility in using and understanding numbers, thus making for more competent handling of figures and statistical forms.

Education in China cannot be separated from indoctrination, and there are numerous indications, at least in interpersonal relations, that Chairman Mao's counsel—"without adopting an honest attitude it is fundamentally not possible to accomplish a number of things in the world"—has been heeded. Through either education or coercion or both, China has become a country where petty crime has largely been abolished and where there is considerable trust in relations between people. Once again, however, it is important to make a clear distinction between personal honesty and its almost incidental relationship to the characteristically casual approach toward accuracy in statistical reporting. Undoubtedly there has been and continues to be some outright falsification in recordkeeping—the

examples presented above clearly reveal this fact. But probably more important in terms of the overall effect on the collection of data are the inaccuracies that are not premeditated—that are almost completely unconscious, subconscious, or perhaps semiconscious.

Peking's current policy of involving as many of the production personnel as possible in recordkeeping has to be detrimental to the statistical system. Despite better education, greater overall awareness, and some specialized training, the average worker or peasant still carries with him many of the attitudes of his ancestors, and simply does not understand why approximations will not suffice. Furthermore, he undoubtedly remembers the Great Leap years when the fabrication of statistics was actually sanctioned by the leadership.

As statistics move up the line from the basic production units, they are probably handled by individuals with increasing degrees of competence, although probably not completely devoid of the tradition of casualness in reporting figures. At each administrative level, however, statistical workers are faced with two basic problems. On the one hand, they have very little faith in the abilities of the compilers below them and are therefore well aware of the limitations of the statistical data that reach them. On the other hand, they continue to be subject to the ever-present pressure from the top of reporting only statistics of achievement. Squeezed from both the bottom and the top, they undoubtedly feel compelled to make "adjustments," "corrections" and outright "estimates," and it is very probable that as the statistics move up to people with more and more statistical sophistication, they are subjected to more and more "corrections." With the advantage of having past records available to them, personnel up the line feel they are in a better position to know "what should be" and at the same time meet the criteria of what may be expected. It is true that a factory manufacturing tractors, for example, would be hard put to misreport the number of units produced over a given period of time. But there are hundreds of other records which are not subject to such obvious controls. Statistics from the rural areas are almost impossible to verify, while social statistics not only are subject to a great degree of error, but also are plagued by problems of concept and definition.

The conclusion is almost anticlimactic. China's competitiveness as a nation and as a representative of a rather unique philosophy make her extremely sensitive to anything that could be interpreted as a failure or even a weakness. Since statistics are the basic measure of success, their publication is closely controlled. It would be wrong, however, to conclude that the absence of statistical data from China and about China is only an aspect of security or a manifestation of pride. At least as important is the fact that China has been unable to establish and maintain a system which would produce these data, and therefore the government itself is very short on information which a more advanced nation would consider indispensable. Naturally, Peking has access to masses of statistics not available to anyone else, but most of these data constitute approximations and are adequate only for internal use. Aside from any security considerations, China is no more likely to publish such mediocre statistics for foreign consumption than she is to publish statistics of mediocrity. Generally, released figures are not intentionally falsified by the authorities. Figures and percentages that in no way reflect reality are sometimes intended as internal propaganda to produce confidence and enthusiasm among the masses, but for the most part they simply reflect the general lack of sophistication on the part of the originators and the publishers of such statistics.

Statistics and Planning

Having considered the statistical system, the problems, and the attitudes of the people, the reader must be left puzzled by a major unanswered question: if the statistics in China are really so inadequate, then how does the central government, with its various ministries, agencies, bureaus and other institutions and components, accomplish its essential planning and managerial functions? In the West, where the supply and demand of the free market and the public pressures tend to dictate many of the economic and social policies and decisions, statistics are still considered to be indispensable for almost all decision making. How does a country which presumably has a planned economy manage, for example, to perform such vital functions as the allocation of capital investments, skilled manpower, and other essential resources if the statistical data base is either unavailable or inaccurate? Not as efficiently as it might but, surprisingly, better than one might expect.

The whole subject of planning in China is too complex to be considered here and would take us far beyond the scope of this paper. Nevertheless, some comments on the questions raised above are in order.

In theory, both long- and short-term plans are first formulated by the primary national planning agencies such as the State Planning Commission, the State Economic Commission, the Ministry of Finance, the Ministry of Allocation of Materials, and the State Technical Commission. Farther down the line, plans are made by the various ministries, by the special business bureaus, by provinces, cities and other administrative units, and eventually by the smallest teams and workshops. The effectiveness with which all this planning is being accomplished naturally fluctuates both with policies relating to the statistical system itself and with the other policies and programs affecting the functioning of government administration. At all levels and at all times, however, it has been a most frustrating experience.

Peking has made many planning mistakes that have resulted in major economic problems. Even reasonable plans have often been dislocated by the ever-present (and periodically intensified) political considerations. Furthermore, it is not always easy, even for the Chinese themselves, to know just how good or bad their planning might be, because there is no built-in system for enforcement of planning directives or for checking whether a particular plan has been fulfilled. Expert bargainers, the Chinese utilize an ancient gimmick of setting higher targets in order to insure desired production. The only problem is that these targets are increased at each descending administrative level to make sure that the target passed on from above will be met. By the time the prescribed targets get to the person responsible for the actual production, they may be so unrealistically inflated that even the most enthusiastic cadre could not take them seriously.

Since the Cultural Revolution, there has once again been a trend toward local self-reliance which means that the authority and the responsibility for all activities, including planning, are to be located closer to the source of information. This should certainly increase efficiency for—given the problems of the statistical system—it is clear that the closer the planners and the controllers are to whatever they may be planning or controlling, the easier it is for them to determine what is possible, and then to implement the plans. This does not mean that Peking is out of the planning picture. Of course not. The State and party reins are still held tightly in Peking, whence all the broad policies and directives will continue to emanate, as will the controls over the production and distribution of the basic industrial products of national significance. But balancing the allocation and

supply of materials for local industries and agriculture, for example, can certainly be done much more realistically in a particular province or even a lower administrative level (which is closer to the source of the necessary information) than in the far-off capital. How well the job is done at the lower levels must fluctuate sharply with the competence and experience of the available personnel, but familiarity with the local conditions should, to some degree, compensate for professional inadequacies.

The whole problem of planning has been succinctly described by Audrey Donnithorne, a noted student of the Chinese economy:

> China's economic planning has been restricted mainly to the setting of targets, to drawing up lists of resolutions. It does not attempt to effect close integration of different economic sectors, nor is it much concerned with optimum allocation of resources. Throughout, and this can scarcely be stressed too much, economic planning in China is constrained by the deficiencies of the information on which it has to work, as well as by weaknesses in the administrative and supervisory organs charged with implementation of plans and with checking this implementation.[37]

This statement is just as true today as it was when it was written in 1966.

Statistics and Problems in Intelligence

Considering the previous discussion on Chinese statistics, what can be said about the availability and nature of the statistics that are of particular interest to the military analyst? Here are five examples of the many problems:

1) *Males Available for Military Service.* In most countries, drawing off a sizable number of males into military service is likely to create labor problems on the economic front, and there is a constant tug-of-war between the requirements of the two sectors. Considering the ratio between the estimated size of the People's Liberation Army (PLA) and the manpower pool, it can truly be said that the supply of men is not a problem to the Chinese government. Because manpower is so plentiful, the fact that Peking has only approximate figures on the age and sex distribution of the country's population does not constitute a problem to the regime.[38]

Statistics on the "quality" of the manpower—the educational achievement of the Chinese youth—are also quite incomplete. The regime does not have accurate figures on the number of persons with completed primary and secondary education because most of the responsibility for these levels is almost entirely in the hands of the local administrative units. The number, however, is large enough to present no problem to the military recruiters. Although statistics on education have not been published for more than a dozen years, it is estimated that of some 125 million Chinese who have completed the six years of primary education, 30 million are males between

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[37] A good discussion of planning in China may be found in Audrey Donnithorne, *China's Economic System*, Praeger, New York, 1967.

[38] Actually, because the sex composition and age groupings in a population always relate to each other in roughly the same way (think of a population pyramid), it is possible to make relatively good estimates of China's age-sex structure even without any reported data. Thus, on the basis of one such model for China, it is possible to say with some confidence that there are now well over 150 million males between the ages of 16 and 44, and about 40 million males between the ages of 18 and 22. (John S. Aird, *Estimates and Projections of the Population of Mainland China, 1953-1986*, U.S. Bureau of the Census, Washington, 1968.)

15 and 19.[39] The overwhelming majority of the recruits, however, are drawn from the very much larger number of youths who have less than six years of primary school. Furthermore, because the PLA does much of its own training, on balance the army returns more skills to the economy than it siphons from it.

2) *Industrial Production.* As a general rule, it may be said that Peking has more accurate statistics on heavy industry than on light industry, on centrally controlled industries than on local industries, and on modern industries as opposed to those using primitive technology. Another generalization that usually holds for all categories of industrial statistics is that the greater the number of administrative plateaus which serve as resting places for statistics as they are moved up the line, the less accurate are the figures. The central government still controls the output and transfer balances to and from provinces of major industrial products, so that it should have fairly accurate data on the production of iron and steel, petroleum, the output of the major machine-building industries, and other basic commodities. Undoubtedly it can also account for materials for the armed forces, armament industries, and other priority production which require that requisitions for raw materials continue to be submitted through the appropriate central authorities. On the other hand, because production of most of the small-scale industries has been removed from the centrally planned balances, the central government is likely to have only approximate figures on the production of consumer goods and other products of local significance. There are still other products that fall somewhere in between. Building materials and chemical fertilizers, for example, are produced both at major industrial installations and in relatively small rural enterprises that are supposed to meet local requirements. Fairly accurate statistics from major enterprises are probably adjusted for the inclusion of the production in small factories and workshops.

3) *Agricultural Production.* Accurate estimates of agricultural production are difficult to come by even at the local level, so that there is little doubt that the central government has large information gaps. Production estimates, particularly of major crops, do rely heavily on sample surveys. The government's main concern is not so much with the total grain production of a particular province as with the delivery of a specified quota for interprovincial transfer or for export. Only very approximate estimates would be available in Peking on the production of produce for local consumption.

4) *Transportation.* Statistics on transportation fall into two categories: those relating to the modern sector, and those relating to the traditional sector. For obvious reasons, data on the modern sector are among the best in China. The miles of railroad track, of all-weather roads, or of navigable inland waterways can be determined and accurately maintained with relative ease; the inventory and annual production of locomotives, railroad stock, motor vehicles, and large water craft are undoubtedly very accurate. Also well known to Peking is the volume of freight moved by the modern sector—most of it between provinces for domestic distribution or foreign trade, and all of it under the control of the Ministry of Communications.

[39] Leo A. Orleans, "China's Science and Technology: Continuity and Innovation," in *People's Republic of China: An Economic Assessment*, Joint Economic Committee, Washington, 1972, p. 206.

The fact that such data are not always available outside China is for reasons that have nothing to do with statistics per se.

Statistics on the very important traditional sector are much more tentative even in Peking. Information on secondary roads may be adequate, but far fewer data are available about dirt roads and trails, built with local labor and maintained by counties or communes, over which a large proportion of the local transport is carried. Only approximate figures are available on the number of sampans, for example, or of vehicles using animal or human power for hauling, as well as on the volume of goods moved by these methods.

5) *Research and Development.* There are no R&D statistics, as we know them, in China. Current Chinese science policies dictate an emphasis on development rather than research. Some research and virtually all the development take place primarily in conjunction with actual production, and in most instances are difficult to isolate as independent activities. Because of this, and because the whole concept of R&D in the People's Republic differs from that in the West, estimating R&D expenditures or in any way quantifying R&D activities becomes a rather hopeless exercise. If statistics are left aside, however, it is possible to follow policy statements relating to science, technology, and education; to speculate on the training and availability of manpower qualified to work in research and development; to identify institutions and enterprises where R&D is probably taking place; and in this way to identify priorities and goals and, to some extent, to evaluate performance.

Conclusions

As difficult as it may be for us to grasp the notion, there is no doubt that the Chinese manage their country with only a fraction of the statistical information which we consider indispensable for planning and decision making. Most of the statistical data thought to be hidden behind combination locks in secret files of the Chinese bureaucracy simply do not exist. Never having had an adequate statistical base, however, the Chinese leadership has been able to work around this handicap, but at the same time work toward an improved situation.

What does all this mean to those of us who are responsible for the day-to-day analysis and interpretation of the developments of the People's Republic of China?

The answer is fairly obvious. We, too, must learn to work around the absence of specific numerical data, accepting the fact that China will continue to be an enigma statistically—as she is in so many other respects. Although it is possible some improvements in statistical reporting may take place, they could only be relative. The high hopes for an accelerated flow of information as a result of China's entry into the United Nations are likely to remain unfulfilled. More to the point, even if Peking should surprise us by releasing quantitative information which we have not previously had, it is doubtful that we could accept such data at face value—without appropriate caveats and inevitable adjustments. Man-made and manhandled Chinese statistics are not likely to become less "romantic" for a long time to come.

A machine-assisted approach
to a pressing language problem

CETA: CHINESE-ENGLISH TRANSLATION ASSISTANCE

Fenton Babcock

After 12 years of uncertain trickle, open-source materials from China are again becoming available in significant quantity and variety. The pre-1960 volume of some 400 journals and up to 500 newspapers may not be reached again, but fairly steady increase can be expected. Much of the material may become widely available outside the United States intelligence community, and may be of direct concern to wide segments of the academic and business communities, in contrast to the past two decades. The task of translating and interpreting these materials has certainly been complicated by tremendous social change in China and by Peking's emphasis upon technological advancement and political conformity, the roots of much new terminology and usage. Fortunately, new methods are being developed for better processing of such material, and new channels are being opened for closer cooperation between government and private specialists in China research. The Central Intelligence Agency took considerable initiative in using the borrowed time of the 1960's and early 1970's to prepare for what now is a pressing problem: processing open-source materials efficiently and effectively for research and analysis on China.

CETA stands for Chinese-English Translation Assistance—the name of a Washington interagency group that is becoming known through its current activities to the community of China specialists in the United States and abroad, and also to officials concerned with the People's Republic of China. The CETA Group of the 1970-72 period grew out of a 1966 DCI initiative which is traceable farther back to 1964. In that earlier year, the then Foreign Documents Division of CIA decided to do something about the dangers and difficulties of using badly outdated dictionaries to translate the rapidly changing Chinese language into English. At about the same time in 1964, an embryonic interagency group concerned with foreign area research began to identify the need for closer cooperation between private and government research efforts on China, and to define the broad problem of processing China research materials. The CETA story includes the perspective of those years as well as the recent movement of the CETA group into the vacuum of communication and coordinated effort on that problem. This record, however, is mainly one of careful, measured approach to that problem through the development of the CETA system. It tells of informal but effective cooperation and coordination among eight components from all four of the CIA's Directorates and among the 12 U.S. Government agencies belonging to CETA.

The Origins of CETA

In mid-1964, government sent academia a list of government "needs," or gaps, in China research. Government, in this case, was the China Committee of the interagency Foreign Area Research Coordination Group (FAR); the contact point in the academic world was the Joint Committee on Contemporary China (JCCC), established in 1959 under the auspices of the Social Science Research

Council and the American Council of Learned Societies, and funded by the Ford Foundation. The late John Lindbeck of Harvard, then chairman of the JCCC, in his reply to the government initiative made it very plain that the scholars he represented (covering most of the private China studies programs in the United States at that time) had had enough vague indications of general interest in government-private cooperation. He rejected most of the topics on the list, such as "Minority Groups in China" and "The Relationship between China's Foreign and Domestic Policies," as non-starters in developing truly meaningful government-private cooperation.

Lindbeck focused instead on a lowly project near the end of the list: "Development of a Comprehensive Dictionary of Modern Chinese Terms." Here, he said, was an area where government and academia simply had to work closely together, if the need were to be met, and time was running out. Six years later, at the end of a gruelling, worldwide survey of China studies that probably contributed much to his untimely death in January 1971, Professor Lindbeck reported to the Ford Foundation that his findings gave the "highest priority" to such a Chinese-English dictionary development effort. He was much encouraged to learn just before his death what CIA had done about this problem in the interim.

Beginning in 1964, the former Foreign Documents Division (FDD), now under the Foreign Broadcast Information Service (FBIS), assembled materials for use in compiling a general-purpose listing of contemporary Chinese terms with English translations. One of the source materials was a Chinese/Japanese dictionary containing many Communist Chinese terms; others came from China, or from U.S. Government institutions including the Joint Publications Research Service and the Foreign Service Institute. The original FDD plan was to merge six dictionaries and glossaries, with computer assistance, and to have the resultant compendium published ultimately in thoroughly-researched, commercial dictionary form.

In February 1966 the then DCI/China Intelligence Activities Coordinator, Maj. Gen. John Reynolds, convened an ad hoc working group to sharpen the focus of the CIA effort and coordinate it with those of NSA and the Air Force, which were working on separate dictionaries of Chinese scientific and technical terminology. The DDI accepted responsibility for seeing the CIA effort through to a logical conclusion, and the task was assigned to the executive secretary of the China Task Force, Walter E. Bass, who had originated and pursued the dictionary idea in FDD. By concentrating on simple translation of the Chinese/Japanese dictionary entries into English, some 100,000 terms were prepared on 3x5 cards by mid-1969. By this time the then Chief, DDI/Special Research Staff (SRS), John Kerry King, had been made responsible for getting good results from the dictionary project in good time. He was assisted in this by Charles M. Otstot, who at the time was special assistant to the DCI/China Coordinator.

Early in 1969 the author, then King's special assistant, was assigned as the dictionary project manager, and worked directly with the concerned parties in all the agencies involved with Chinese/English translation or its products to bring about further definition of the problem and design of a process to meet it. Development of a "living," looseleaf dictionary that could be continuously updated by computer assistance came to be seen by all as the best approach to the existing and projected need of the intelligence community for effective and efficient processing of Chinese language materials. In order to meet the problem of producing a truly useful dictionary, voluntary provision of linguistic and lex-

ical expertise from the private sector clearly had to be obtained, along with the input of new Chinese terms wherever and whenever they were encountered by translators and researchers in the United States and abroad. Finally, this development of a "living" dictionary had to proceed in the light of relevant information on what was being done generally in dictionaries, computers, and Chinese-to-English translation (human and machine), if the effort were going to be soundly based and efficient. This called for more than an ad hoc working group, and the China Committee of the interagency FAR saw it just that way. In March 1971 the China Committee endorsed the CETA concept, and on 25 May that year representatives of 12 government agencies voted themselves into organizational existence as the Chinese/English Translation Assistance (CETA) Group.

The Development of CETA

CETA then organized itself into functional committees, under a rotating convenor of the plenary group, and took over the CIA Chinese Dictionary Project and another one on which the Analysis Division of the DD/S&T/Office of Research and Development (ORD) had been working since 1968. This latter Chinese translation survey and machine assistance test project had already produced a valuable set of academic and government contacts, which Hal Ford, as DCI/China Intelligence Activities Coordinator, and subsequently Chief, DDI/SRS, used to help foster the CETA concept. The marriage of these two CIA projects under CETA thus went very smoothly, and I was elected chairman of CETA's new Coordination Committee. Its function became primarily that of setting CETA's course and coordinating the work of CETA's other two committees, on linguistics (dictionary production) and computer support. With Lee Ohringer, of ORD's Analysis Division, as chairman of the Support Committee, Norman Wild of NSA chairing the Linguistics Committee, and the head of China research in the State Department's Bureau of Intelligence and Research, Stanley Brooks, serving as CETA's first convenor, the intelligence profile was high indeed. Initially unavoidable, this served to get the new organization off the ground and start the flow of services and funds required to accomplish something.

ORD financed the CETA executive secretary, Jim Mathias, until the CETA Group had gotten on its feet. ORD also provided the extensive computer programming required to get the CETA dictionary file ready for testing and use. The Central Reference Service had covered the cost of keypunching the file of 80,000 entries (handwritten on 3x5 cards), after it had been winnowed in 1969 from the original collection of 100,000 entries. With the file in usable, but still very rudimentary form, the Office of Computer Services took on the difficult task of programming for dictionary production. This required close coordination with other CETA members, particularly NSA, which had undertaken to produce microfilm from the computer file. With this film in hand, CIA's Printing Services Division demonstrated its efficiency and "CETA spirit" by putting out the CETA dictionary on 1 September 1971, the target date which had been set by the interagency group at its inaugural meeting in May of that year. As its contribution, the Labor Department provided special looseleaf binders for the 70 copies of the 2,000-page two-volume dictionary that were printed. The Commerce Department, another CETA member, has served as the distributor to the selected recipients, who first must commit themselves to active improvement of the master file through a quota of additions, deletions, and changes in the Chinese entries or the English meanings. In return, the dictionary holders receive new pages as the improvements that have been screened and accepted by a human process are incorporated into the machine process and printed out.

The total number of regular, active individual or group participants in the CETA dictionary improvement process is currently 50 (22 government and 28 academic).

The flow of real life into the new interagency group, through the fulltime work of its executive secretary, was started with the help of the Domestic Contact Service, the O/DDI, and the former National Intelligence Planning and Evaluation (NIPE) Staff of the DCI. ORD continued to make CETA's executive secretary, Jim Mathias, available to CETA by extending his personal services contract for a few months, but Mathias also needed a desk and telephone in Washington, D.C. from which to arrange the many face-to-face contacts at the administrative and working levels in the government bureaucracy that got CETA going. DCS met these material needs for several months, and then the O/DDI helped extend Mathias' contacts beyond Washington by paying the new CETA office phone bill on an interim basis. The CETA Group had directed its executive secretary to open up and develop an academic dimension within the CETA system, and the strong positive reaction from many institutions and individuals throughout the country made it clear that a continuing, meaningful response from Washington would be essential.

Joint funding of CETA's basic needs had been pressed by the Coordination Committee chairman as a goal, because of CIA's heavy past expense with CETA's dictionary. Time and momentum were needed for achieving that goal, and there was as yet only one logical provider—the new China Intelligence Activities Coordinator on the NIPE Staff, George S. Mallory. Through the good offices of the executive secretary of the Intelligence Handling Committee of USIB, J. Neil Wallace, the sum of $21,000 was obtained from NIPE for CETA at a critical time in its very young life. One string was attached—redefinition of the problem and reassessment of ends and means.

The 1972 Workshop

A CETA Workshop in March 1972 was designed for just that purpose, and it produced the guidance and momentum that led to joint funding of CETA by 9 of its 12 member agencies beginning with the current fiscal year (FY 1973). The plan for mixing political scientists, librarians, linguists, lexicographers and translators in one workshop-type conference ran into opposition in the CETA Coordination Committee initially, but there was final agreement on the need for broad perspective within which to examine the evolving CETA system and its stated purpose of dictionary production. In two days of panel and open discussion in the informal workshop atmosphere, the mixed group of 20 government representatives and 24 academic participants (from 20 private institutions) worked smoothly together. After a well-pitched keynote address by E. Raymond Platig, Director of the State Department's Office of External Research, which had funded the workshop (including travel from all over the United States and from three foreign countries), the CETA dictionary effort was explained along with the varied projects of 16 other institutions that bore on the Chinese materials processing problem under discussion. At the end of the discussions, which many described with some emotion as the most fruitful they had ever known in a conference situation, four main conclusions were reached:

1) Steady increase in the flow of materials from China for research on China was likely.

2) Efficient processing of those materials in Chinese and English for both government and private use in research was a definable problem of considerable importance.

3) The CETA man/machine system should be encouraged to attack not only definition of that problem but also its solution.

4) Futher development of CETA's "living" dictionary with appropriate purpose and quality should retain first priority among the Group's efforts.

Before the Workshop ended, five key academicians had volunteered for and now serve on a CETA Dictionary Quality Management Subcommittee. At its first annual meeting in May 1972, CETA acted on other specific recommendations that flowed from the Workshop. A Funding Subcommittee is now part of the regular organizational structure of the CETA Group, and two ad hoc subcommittees are operating as part of the system. One is conducting a worldwide survey of direct machine translation programs (Chinese-to-English), so that CETA can better assess the continuing need for its process of machine assistance to the human translator. Another ad hoc subcommittee is looking into possible assistance through the CETA system in developing a centralized data bank of unclassified, open-source materials on China (including the CETA dictionary file), which could be used with computer assistance in the service of private and government researcher alike. This would mean using CETA's wide academic and government connections, and its key capability for project follow-up, to tie together the bits and pieces of this elusive data bank problem that has been discussed with no meaningful results in numerous gatherings for years, at considerable expense to the taxpayer.

CETA's Support Committee has undertaken to respond to the encouragement received at the Workshop for further testing of the so-called CETA on-line, interactive system. Since the original demonstration staged by ORD for the FAR China Committee in March 1971, more than 150 government and private China specialists, including those at the Workshop, have now seen the operation of this computer system for direct translator use of the CETA dictionary file. From his terminal, the translator obtains access to the dictionary data and retrieves for display on his terminal screen the English translation, and also his selection from many more translator aids than appear in the printed version of the dictionary under each entry. By this means, and by interacting with the file through the computer, the translator can increase his speed and efficiency of translation. The spectacular results achieved by selected FBIS and NSA translators with this system in 1970 testing under ORD contracts (by Jim Mathias, then with Technical Operations Inc., and William Fender, presently with the firm of Chase, Rosen and Wallace) need to be retested in a real production translation situation. Again CIA, through ORD, has come forward informally to offer CETA the financial means for such a pioneering effort. This time, however, the initiative has been based upon the demonstrated willingness of the other agencies to join CIA in funding CETA on a regular basis, and on the strength of CETA's demonstrated capability for taking a careful and systematic approach to the problem of efficiency in Chinese-to-English translation.

CETA's Contribution

In October and November of 1972 CETA's executive secretary travelled to eight private institutions in the United States and 24 in Japan, Hong Kong and Taiwan, where he talked with a total of 140 specialists concerned with translation and other processing of open-source materials for China research. Mathias' written report to the CETA Group showed that he had repeatedly established communication among people who had been working essentially in isolation on dictionary and other Chinese materials processing projects that demanded contrast and comparison, interchange and sharing of ideas, materials and results. Following up the CETA Workshop of March 1972, arrangements were made on the trip for a group discussion of CETA in conjunction with the annual Association for Asian Studies meeting in Chicago in March 1973. Continuity of communication among all these contacts is provided for in the *CETA Bulletin,* which is published from time to time as information is accumulated

on: glossaries and other data collections; various techniques being developed for the input, output and computer processing of Chinese characters; the compiling or publishing of Chinese dictionaries by institutions and individuals in various places; and the activities of the CETA Group itself, including the progress of its "living" dictionary.

On 15 January 1973, Mathias briefed some 50 people in the United Nations Secretariat who are directly concerned with efficient translation of Chinese to English and English to Chinese. Predictably, attention was given in the discussion to the presence of Peking's official romanization in CETA's dictionary file, along with the two common American ways of rendering the characters' pronunciation in English (the Wade-Giles and Yale methods). CETA's plans for adding to the computer file the abbreviated character forms that are now commonly used in China were also discussed by the UN group, and arrangements were made for continuing contact between CETA and some of the UN translation staff members.

The CETA Secretariat has remained within its basic $30,000 budget, but no accounting has yet been made of the considerable time and effort devoted to CETA's background, development and current activities by the Group's members, such as Cyril P. Braegelman, in charge of Chinese translation in the Asia/Africa Division of FBIS, and Gustave Blackett, Chief, Joint Publications Research Service (in the Department of Commerce), nor of the extensive computer and other technical support rendered especially by CIA and NSA. This considerable albeit informal expenditure has proceeded on the member agencies' understanding of the problem, as now defined by CETA, and in expectation of substantial return on the dollar in the fairly near term. Certain accomplishments can be weighed already:

A. The CETA dictionary file of some 100,000 entries has been built up, completely reviewed, updated and greatly improved through the changes made by the wide network of participants to whom it was parceled out in 1972.

B. Through this process a system has been developed and tested which will keep that file current indefinitely, and put it in the hands of translators and researchers around the world in a hard-copy form that will move steadily from its present glossary state toward that of a thoroughly-researched dictionary.

C. A system has been developed which, after further testing, may serve to put that file at the fingertips of those translators and researchers through computer display terminals, with attendant increases in speed and accuracy of translation.

D. The CETA approach succeeded where other efforts were failing because it overcame a communication gap which for some 20 years had wasted human and material resources in duplicated efforts and projects that led nowhere.

With its intelligence profile now much reduced, its basic finances flowing in regularly, and its network of contacts spreading steadily, CETA came officially to the attention of the NSC Under Secretaries' Subcommittee on Foreign Affairs Research (USC/FAR) at the end of 1972. This authoritative interagency group, headed by Ray S. Cline, Director of the State Department's Bureau of Intelligence and Research, replaced the earlier FAR—the Foreign Area Research Coordination Group—in 1971. The USC/FAR has taken particular note of CETA as an example of joint agency funding and government-academic cooperation in the field of China studies.

The late John Lindbeck's report of January 1971 on China studies makes it clear that CETA's development came none too early. For the period since the Korean War, Lindbeck put the contribution to China studies in the United States by private foundations alone at $28,000,000, and he did not attempt to estimate the total government funds that went into Chinese language training, China studies fellowships and Chinese materials processing during that 20-year period. In his extensive survey, Lindbeck found the results to be

generally disjointed, unbalanced and unproductive of a new generation of China specialists well-attuned to China's momentous changes. Lindbeck saw this problem compounded by the long hiatus in China's own research, study and publication on the social, economic and political aspects of that country's development. Under these circumstances, the achievement of efficiency and effectiveness in the processing and interpretation of both Chinese and English language open-source materials on China has indeed emerged as a major intelligence problem. The CETA story stands as an example of a cooperative approach to one aspect of that problem and some of the benefits to be derived.

INTELLIGENCE IN RECENT PUBLIC LITERATURE

OSS: THE SECRET HISTORY OF AMERICA'S FIRST CENTRAL INTELLIGENCE AGENCY. By *R[ichard] Harris Smith.* (University of California Press, Berkeley and Los Angeles, Calif., 1972. 458 pages.)*

On its face, at least, this work on the OSS has made and will continue to make a good impression. Even a cursory glance reveals the diligence of the young author who has done a large amount of research and who writes engagingly. Several discerning readers have given him very good marks for his effort, among them Arthur Schlesinger whom the author thanks for his pre-publication tour through the entire manuscript and for his helpful comment and criticism. Other OSS alumni were consulted about parts of the book in which they had a notable role, and some of them thought the effort commendable and said as much. One at least was well pleased at Mr. Smith's approach to his subject, which he saw as an implicit rebuttal of the cynical interpretation of American foreign policy which revisionist historians of the New Left have been touting. (More about this approach later.) I must confess that my own first reactions were favorable; to be sure I found a number of errors in the chapters whose content was most familiar to me and a number of surprising omissions, but as some readers will, I charitably concluded that other chapters—the ones whose substance lay not within my personal OSS experience—were probably sounder than the ones I knew about. The earmarks of scholarly endeavor which stuck out all over the book were an earnest of the author's training in systematic research.

A word about these earmarks—because in most works on a secret organization like OSS they are signally lacking. Spread through the book's 11 chapters and 353 pages of text there are 823 references which will lead you ultimately to a bibliography of 344 items (books, articles, and documents—published and unpublished). They will also lead you to a list of 75 persons whom the author interviewed (man-to-man or by telephone) and 103 other persons from whom he received written communication. Practically all of his respondents were OSS alumni. In addition to this display of scholarly apparatus, there are 238 proper footnotes at the bottom of the page which furnish important bits of information about people and things. Some of this is striking in its detail—even in the case of relatively minor figures. It is added evidence of Mr. Smith's busy researches and his tact in not revealing a present CIA connection of certain individuals who would prefer it that way.

All of the above, taken with the style of the writing and the scope of the book—it covers the field activities of OSS wherever they took place: Africa, western and southeastern Europe, China, and Southeast Asia—is bound to incline a reader to a ready-for-the-best frame of mind. I regret that a little time invested in a careful and critical second reading leaves me with a very different impression. Almost every way I look at it, I find the book wanting in most of the attributes of quality. My objections can be covered under four headings: general approach, approach to sourcing and the sourcing itself, errors, and omissions.

As one takes the book in big gulps, he gets the distinct feeling of reading about an institution whose inner soul—and outward conscious policy—was one

*With this, all thanks to Walter Pforzheimer and his associates, Linda Benton and Corinne De Lisle, for a lot of invaluable help.

in sympathy with the world's leftist movements. At a guess this is the residue of an earlier essay on OSS which Mr. Smith submitted to a graduate school for an advanced degree. Its central theme was that OSS was disciple and leader of what Mr. Smith called "social idealism" and that OSS had a conscious policy of backing—say—the extreme left against the extreme right, and the left of center versus the right of center, in all its major initiatives. Another guess is that someone who knew better tried to set Mr. Smith right in the matter. They explained that there were all kinds of people in OSS, ranging from Serge Obolensky (who wore his Tsarist ribbons) at one end of the political spectrum, to some ideological Marxists and self-professing Communists at the other. They also must have explained that just because a lot of OSS field officers were sympathetic to foreign leftist causes, and a lot more were unsympathetic with rightist ideologies related to that which we were trying to extirpate in general war, there was little reason to coalesce these individual (and in the circumstances quite normal) attitudes into a formal OSS party line. Those who knew General Donovan best knew that he had one overriding goal for his agency, and that was to do the enemy the greatest hurt in as many ways, in as many places, and as fast as possible. They also knew that he was a stickler for observing broad lines of national policy where they had been clearly established, such as, for an obvious example, the maintenance of the solidarity of the alliance. As to other lines of national policy—especially those on non-war issues, and those affecting the post-war world—few indeed had been clearly defined anywhere, and Donovan's people were no less free to roam this undemarcated area than the officers of other departments and agencies of the government. Many roamed freely. Mr. Smith's second effort, the book under review, shows that he was aware of the great diversity of people in OSS, but that he could not bring himself to reject out of hand his cherished dream thesis. The result is that you get both points of view throughout the book—with the emphasis on the earlier thesis.

It seems gratuitous to call attention once more to the fact that Americans everywhere were conscious of the political, social, and economic goals of the New Deal and that many Americans, especially young Americans, were sympathetic to them. It seems unnecessary to state that innate distrust of Soviet Communism was inevitably softened by the realization that the USSR was an ally, and an ally which was absorbing the overwhelming proportion of the Nazi war potential, and that, after all, the enemy *was* the Fascist Axis. As if these were not in themselves enough to account for the left-leaning posture of many young (and old) officers of OSS, there were also compelling pragmatic reasons. To be pro-monarchy in Italy was not merely to back a royal family which had gone along with Mussolini, but also to back a sure loser. To be pro-Tito and for the Communist irregulars in northern Italy and in France was perhaps far less to be accounted for on ideological grounds, than upon a realization that among the sketchily reported hordes of underground warriors these stood out for their organizing ability, their courage, skill, and resolve. That another OSS group had a close relationship with Ho Chi Minh in northern Indochina is not so much to be cited as evidence of OSS's moral devotion to anti-colonialism, as evidence of the fact that the principal task of the group was to collect intelligence, and they found Ho's apparatus an admirable source of supply. I do not wish to suggest that Mr. Smith is ignorant of how these practical considerations influenced OSS Chiefs and lone Indians, but I do wish to say that he plays down their importance to nurture the alternative thesis.

The saddest result of all this turns up in the final chapter, where—to clinch his point about OSS—he chooses to contrast that agency with what he would term its direct lineal descendant, the CIA. According to Mr. Smith, critics of the present organization

> have often wondered how an amateur secret service that once gave hope to Ho Chi Minh's guerrillas could have evolved into an "invisible government" of the Cold War era. The answer is simple. The CIA is no aberrant mutation of "Donovan's dreamers" [the heading of Smith's first chapter]; it is in many ways the mirror image of OSS.

The comparison thus drawn between the two institutions must be weighed in terms of Mr. Smith's license to speak usefully about either. As is already clear, I cannot share one of his basic thoughts about OSS, and I am not ready to admit that he knows much more about CIA. While awaiting proof to the contrary, we must guess that his store of knowledge of the Agency derives from a nine months' tour in headquarters at a junior grade and the reading of the materials cited in his bibliography like the books of Tully,* Wise and Ross,** and the long article of Fred Cook which constituted a special issue of the New York *Nation.****

There is another aspect of Mr. Smith's general approach which calls for comment, and this is the undue stress he gives to the lack of organization and discipline in OSS. It is just irresponsible journalism to decorate Chapter One with pixie tales of General Donovan's supposed impatience with organization diagrams and administrative detail. Whatever example of this sort the General may have set, it stopped right there. It did not go beyond the Buxtons, Chestons, Magruders, the branch directors, and on down the line. Administration at headquarters and in the large units overseas was no more chaotic than anywhere else in a wartime government, and a lot less than in many another war agency. With a few notable—and to me inexplicable—exceptions, Donovan's principal lieutenants were an able no-nonsense group. What went on in tiny units operating far afield in friendly and especially in enemy territory obviously could not be controlled as closely as a headquarters company. Even so, there was a lot less irresponsible free wheeling than Mr. Smith and the other romanticizers of OSS like to pretend.

"Insubordination became a way of life for OSS officers, but Donovan was unconcerned," begins a paragraph in the first chapter. The theme then continues for the best part of three pages and is recalled elsewhere in the book. If the reader comes away with the feeling that indiscipline was basic to the institution, this is not because he has misread Mr. Smith. Yet how wrong he was. And how better to illustrate his wrongness than his own account of General Donovan's peremptory handling of Robert Solborg's and Arthur Roseborough's disregard of instructions. This sort of response is far closer to the way most OSS alumni will think of the matter of indiscipline than in terms of the whimsical anecdotes of uncaught and unpunished culprits.

Now about the sources for Mr Smith's book and the way he used them. The first thing to say on this subject is that Mr. Smith, like his recent predecessors, did not have access to the official OSS archive. He knew that this was the way it was going to be before he started, and that if he was to write the book he would

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**CIA, The Inside Story* (Morrow, New York, 1962).

***The Invisible Government* (Random House, New York, 1964) and *The Espionage Establishment* (Random House, New York, 1967).

****The Nation* (Special Issue, 24 June 1961).

have to use the next best thing. A more discriminating student would have perceived that the next best thing—with a few exceptions—was a pretty poor substitute, and that a book resting upon it was doomed from the start. Solid workmanlike history has seldom, if ever, been written from the stuff that leaks out around the edges of a secret organization.

Have a look at what Mr. Smith had to work with. The very best of it consists of papers which some officers of OSS took with them into private life and then turned over to public repositories. There is, for example, the collection which Preston Goodfellow, a chief of the Special Operations branch and trusted Donovan lieutenant, bestowed upon the Hoover Institution at Stanford (supposedly under seal until 1980)! Judging only from Mr. Smith's footnote references, the documents touch upon a number of OSS matters and are highly illuminating. There are also the papers of Joseph Hayden, who began life in OSS as a member of the Board of Analysts of the Research and Analysis Branch (he was a political scientist by trade, with a specialty in the Far East), and moved over to one of the clandestine branches and served in China. There are the papers of Captain (later Vice Admiral) Miles, famous as the American deputy director of the Sino-American Cooperative Organization and for 10 months the Chief of OSS in the Far East; the papers of Francis P. Miller (one of the principal officers in the Sussex operation) and among which Mr. Smith says he found a copy of the official history of Sussex. Lastly there are the papers of Harley Stevens (who, among other jobs, was commander of the OSS detachment in Chungking); of Leland Rounds (one of the control officers in North Africa nominally in place to police the U.S.-North African economic accords and in fact one of the purveyors of highly important intelligence prior to the Torch operation of 1942); and of DeWitt Poole (Chief of the Foreign Nationalities Branch of OSS).

There can be no question of the value of this sort of material to a historian, but one suspects that it cannot illuminate more than a tiny fraction of the vast screen of total OSS activities.

So it will be with the next echelon of material. This consists of books and articles written by OSS officers at a time when their memories were still fresh, or perhaps later with the aid of letters or diaries; plus similar publications of other civilian officials and military men whose business took them into contact with this or that part of OSS. I would include in this group books like Mr. Dulles's *Secret Surrender,** which, though published in 1966, rests solidly upon a long memo for the record which Mr. Dulles and his colleague Gaevernitz wrote in 1945; Carlton Hayes's *Wartime Mission in Spain,*** and General Stilwell's *Papers.**** I would include some others like the books of Donald Downes and Peter Tompkins with a warning about their reliability. These books, with the documents of my first category, would constitute what a critical scholar would call his primary materials. When taken all together and stacked against what one ought to have at hand to describe the multifarious real-life activity, the myriad undertakings, and signal successes and failures of the OSS, you have something perilously close to nothing at all.

Of course, Mr. Smith must have been aware of just that, and so he dropped down to the next category of written testimony. This is the material that constitutes the bulk of the bibliography and an odd lot it is. Some of it has no more

*Harper & Row, New York, 1966.
**Macmillan, New York, 1945.
***Sloane, New York, 1948.

than a whiff of relevance to Mr. Smith's task. For example, he includes some books which recount the adventures of some British Special Operations Executive operatives in Italy, perhaps to compensate for what Mr. Smith could not find out about OSS's special operations in this theatre. He includes the book of Joyce Lussu (wife of the liberal Italian political figure) which was published in 1969 and is seemingly an undocumented reminiscence. The book is about the turmoil in Italy and about Lussu; on one page the author adverts to Benedetto Croce's presence on Capri in the fall of 1943. Since it was at this time that Croce recommended to Donovan that the Americans put the Italian General Pavone in charge of a combat legion of anti-Fascist Italians to fight at the side of the Anglo-American forces in Italy, the Croce incident becomes relevant to the OSS story, and the Lussu book which relates to Croce becomes, by extension, a legitimate item of the bibliography. There are a good number of entries whose claim to notice are as tenuous as this one.

Others have a much better claim, in that their content and their authors are closer to the subject and to OSS activities. But the bulk of this group of books was written long after the fact, largely on the basis of unaided memory—few if any reveal any other source and to this reviewer seem in large part trivial, self-serving, or grossly inaccurate, or all three. Take for example the two books of Robert Alcorn. Mr. Alcorn in 1962 and 1965 wrote as if he had been a privileged and important OSS insider, while in fact he held a modest administrative position in London which afforded him no more than observer status—and that, usually, at several removes from the action. In these circumstances I for one am not surprised to find a number of Mr. Smith's passages written upon Alcorn's authority that I know to be aimless woolgathering or inexcusably erroneous.

The peril of relying upon this sort of published material was nowhere better illustrated than in Mr. Smith's use of the book of James Dugan and Carroll Stewart (cited in a footnote only) on the Ploesti raids. In a section devoted to the evacuation of downed American flyers from Rumania, the authors take a breather from their main task to recount how an OSS team came to recently-liberated Bucharest to lend a hand in the rescue, to inventory the damage to the Ploesti refineries, and to pick up any materials of intelligence interest which the retreating Germans had left behind. The Dugan/Stewart account is full of jocular fantasy, most of which Mr. Smith found out when he checked the story with Frederick Burkhardt—one of the OSS team members. Despite what nonsense Burkhardt was able to purge from Mr. Smith's original understanding, perhaps a dozen significant errors remain in Mr. Smith's single paragraph.

Mixed among the irrelevancies and the balderdash of the bibliography are perhaps a score of books written after the events they describe which are sober, sound, and careful. I note them with pleasure as a judicious corrective to what has just gone before. But let me add that there is no critical phrase anywhere in the bibliography which will identify them. In your innocence you will be left to flip a coin as to which is the better book: the Corey Ford, Alistair McBaine *Cloak and Dagger*,* which contains scarcely a paragraph without some dismal error of omission or commission, or the Roger Hall *You're Stepping on my Cloak and Dagger*,** which is a humorous and at the same time accurate account of an OSS man's training for irregular warfare. If Mr. Smith knew the difference, he does not let on.

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*Grosset & Dunlap, New York, 1946.

**Norton, New York, 1957.

Beyond the written word there was that vast reservoir of oral testimony to be tapped, and Mr. Smith exploited this with commendable zeal. He uncovered an astonishing number of OSS alumni (and less than half a dozen alumnae), got in touch with them, and pumped. Some of what he got from them is cited in the footnotes, but no one save the author can know what else they contributed. Where his informants requested anonymity, they got it. There can be no question of the value of the information Mr. Smith acquired from these interviews and exchanges of correspondence. Important parts of the book could not have been composed without them. Yet in his use of oral testimony Mr. Smith was up against two disadvantages. The first is the obvious one that these memories which he tried to journey through were already 25 years stale. The second is that, however good or poor individual memories were, Mr. Smith approached them without the basic tool of the interrogator's trade—namely, as full a pre-knowledge of the subject as only the official or other authoritative sources could afford. Without access to the OSS archive, and without an inventory of solid background information, Mr. Smith was at best at a very serious disadvantage. The results are clear. Holes and misremembrances in this or that man's recollections are all but impossible to cope with, and the other non-additive episodic memories which were tapped led more surely to misconstructions and distortions than to the detached overview that Mr. Smith sought. In fact, the business of trying to patch together scores and scores of flawed and disparate bits of oral testimony is, I fancy, one of the reasons that the book is as it is.

Surely it will account for some of the errors, not all. Irrespective of their source, there are far too many of them. Some of the most damaging are the errors of innocence. Not one of these is much in itself; in fact, most would be wholly unimportant if they did not underscore the fact that no part of what Mr. Smith knows of his subject derives from his personal experience. OSS was one year dead before Mr. Smith took his first breath, and as far as he is concerned, OSS might almost as well have existed in the forties of the nineteenth or eighteenth centuries. That there were living witnesses around to query seems not to have compensated for his unfamiliarity with the institution or its era. How, for example, could anyone professing a knowledge of intelligence matters put the headquarters of the Gehlen organization in an "OSS compound near Frankfurt" and have the Gehlen group "fed and clothed by Donovan's officers"? How could a serious investigator of OSS put Rudolph Winnacker, Milwaukee- and Madison-educated and an American citizen since young manhood, among the recent German emigrés on the R and A staff, or speak in the same phrase of Edward Mason and Walt Rostow as if they were co-equals? In the early 1940's, Mr. Mason was already a senior Harvard professor and among our country's half dozen leading economists; Walt Rostow, still in his mid-twenties, was a junior research assistant to Mason. How could one mention Harold Macmillan three times with three different points of reference without once connecting him to the man with the same name who became the British Prime Minister. Is it possible that Mr. Smith did not know? When he speaks of someone in "OSS uniform," another "with the assimilated rank of OSS Major," and identifies the Purple Gang with Philadelphia, one is nudged to fear the worst.

The damage which this sort of error does to Mr. Smith's credibility on more important matters is augmented by the multitude of simple run-of-the-mill mistakes. There are hundreds of them. One group of them has so incensed a reader that he has muttered about legal action. Another, surrounding the murder of William Holohan in northern Italy, by ignoring the findings of the Italian court and accepting the story of one of those found guilty by it in absentia, has stirred justifiable muttering of another sort. Among the many other errors there

are two at least of special concern to readers of this review: one is the canard which identifies Mr. Dulles as one of the directors of the J. Henry Schroder banking concern (which is correct) and which then links that concern with that of the German financier, Kurt von Schroeder, who was one of the Nazis' financial angels (the linkage is dead wrong). Through this line of argument, Mr. Dulles becomes part of the circle of Hitler's early well-wishers and financial backers. This malevolent and silly story began in one of the publications of the Soviet propaganda mill in a pamphlet called *Falsificators of History*,* published in 1948 in Moscow in English. Fred Cook borrowed it (without attribution) for his previously cited article on CIA in the *Nation*. Inasmuch as the Moscow piece does not appear in Mr. Smith's bibliography and the Cook article does, one must assume that the source was Cook. Incidentally if Mr. Smith has concluded that the OSS was motivated by the spirit of liberal idealism, his reading of Fred Cook would cast the CIA as its mirror image, and make no mistake.

Another error which offers more important hurt to the reputation of OSS is the allegation (p. 6) that

> unaware that a top secret [U.S.] naval intelligence team had broken the Japanese military code, OSS men in Portugal secretly entered the Japanese embassy and stole a copy of the enemy's code book. The Japanese discovered the theft and promptly changed their ciphers. Washington was left without a vital source of information, and the Joint Chiefs of Staff were irate.

Mr. Smith's source for this is a popular book, *Cloak and Cipher*, by Dan Moore and Martha Waller.** What he learned from this book he incorporated in that paragraph of Chapter I which begins "Insubordination became a way of life for OSS officers, but Donovan was unconcerned," and in which he goes on to list a few places where OSS operatives got out of line without bringing down directorial reprimand or punishment. The story from Moore and Waller so nicely fitted the requirements of the paragraph as perhaps to inhibit more research into the matter which might add confirmation (unnecessary?) or denial (unwanted?). In all events, there is no evidence that Mr. Smith knew that the primary source was a far more credible and weighty affair than the slight offering he seized upon. The true source was part of a long letter which none other than General George C. Marshall addressed to Governor Thomas Dewey (dated 27 September 1944) and which a year later General Marshall introduced in his testimony to the Congressional committee investigating the Pearl Harbor attack. (The committee subsequently published it.) Toward the end of the communication is the following:

> . . .some of Donovan's people (the OSS), without telling us, instituted a secret search of the Japanese Embassy offices in Portugal. As a result the entire military attaché Japanese code all over the world was changed, and though this occurred over a year ago, we have not been able to break the new code and have thus lost this invaluable source of information, particularly regarding the European situation.

The occasion for the letter was General Marshall's concern that Governor Dewey, in the heat of the presidential campaign, would reiterate the allegation of President Roosevelt's complicity in the Pearl Harbor attack and in so doing build his case on Roosevelt's familiarity with Japanese diplomatic traffic and the U.S. capability to read it. To avoid the horrendous consequences of such a disclosure, General Marshall sent the Governor what has been called "the most

*Soviet Information Bureau, Moscow, February 1948.
**Bobbs-Merrill, Indianapolis, 1962.

revealing single document in the annals of cryptology."* In it he told of the stunning successes of the U.S. crypt-analytic effort on Japanese communications, and how they had made possible a number of decisive American naval actions against the Japanese. Wishing above all to underscore the sensitivity of the source, he noted that the report of Justice Roberts on Pearl Harbor had had to be purged of all reference to our ability to read the Japanese traffic before it was made public. And then just to cap the point, he included the two sentences I have quoted about OSS in Portugal.

Most readers of this review will know that General Marshall's initiative was successful; that Governor Dewey with some reluctance read the letter and then scrupulously observed its injunction. Few readers will know that General Marshall had been previously misinformed about the incident regarding OSS in Lisbon, and had made an incorrect charge which the likes of Moore and Waller and now Richard Harris Smith perpetuate. No one without access to the OSS and other intelligence records could possibly set the matter to rights.** The short of it is that whatever the OSS people in Lisbon got out of the Japanese naval attaché's office was not related to the attaché code, and that whether or not the Japanese were alarmed at the scent of an OSS penetration of the Lisbon premises, they did little or nothing to make the attaché communications system more difficult to read. We were reading virtually all of it both during and after the events at issue. With this I hope that OSS is exculpated from what would appear the most damaging charge ever made against it.

As I have remarked, little personal blame should be attached to Mr. Smith for his (unknowingly, to be sure) going along with General Marshall in this particular error. His omissions, on the other hand, are not so easily pardoned. From something that he wrote to one of his contacts—and which is borne out by the general character of the book—his concern focused on the overseas field operations of OSS. His successes, such as they are, lie largely in his accounts of the doings of the secret intelligence (SI) and special operations (SO) branches and the operational groups (OG). But the other field activities—X-2 (counter-espionage), MO (morale operations), MU (Maritime Unit), even the FP (field photographic), all of which had substantial duties (and successes) overseas—get little notice or none at all. But this is not the worst.

Far more serious is the omission of almost all reference to the entire Washington scene. If this was a well-formulated intention, it would have been considerate of him to have put some such confession into his subtitle instead of the garish "The Secret History of America's First Central Intelligence Agency." It would have been prudent of him to have given the matter a paragraph in his preface. But he did not, with the result that the studious reader is left to wonder about what formed, directed, and nurtured these overseas empires, who received what they reported, and what was done with it.

Setting his sights thus, Mr. Smith is under no obligation to examine the fundamental problems of the OSS: things like General Donovan's leadership where it counted—his relationship with the President, the Congress, the Joint Chiefs, the Services and their intelligence branches, and the rest of the war agencies. There is no obligation to look to the principal executives and administrators (the men I have mentioned earlier and a number of others), to their tasks, and how they performed them. There is no need to look at recruitment, training, cover, commo, the medics, security, the vast logistics service, personnel and its problems with both the Civil Service Commission and the military, and the OSS

*David Kahn, *The Codebreakers*, Macmillan, New York, 1967, p. 605.

**My thanks to Mr. Thomas F. Troy who led me to the appropriate OSS folders.

budget. With the bathwater, out went these babies plus another which is to the reviewer painful on both personal and professional accounts. This is the Research and Analysis Branch, which to be sure gets its marginal mentions (often wrong) in the context of its people in the overseas posts, but nothing else.

The large majority of R and A's staff stayed in Washington, and the bulk of its work was designed for high-level consumption within the national government and the Pentagon building. Clearly, then, it was not within Mr. Smith's terms of reference, but in leaving it out he denied himself the chance to discuss a "first," and a distinguished one, in American intelligence history and an important contribution to OSS's remarkable record. Incidentally, of all the branches, R and A was the most overt, the one most authoritatively commented upon by former insiders, and the one whose substantive output has been largely declassified. Leaving it out of the story was to leave out the part most manageable to a diligent student working without benefit of the closed official record.

Is this the kind of book that OSS is owed? The answer is, of course, no. Is this the best that can be expected? The answer here is both no and yes. In the first place, given Mr. Smith's diligence, his book did not have to have this one's grievous shortcomings. With more skepticism about what he read and what he heard and more caution about what he decided to commit to print, he could have written a much better book. But it would still be far short of what the subject requires. It would be because the job is bigger than a two-man/year stint with the materials which Mr. Smith used, and bigger than a 20-man/year stint with access to the official OSS archive. Other scholars have looked at the task, plumbed its magnitude and turned to more rewarding projects when they learned that the archive would not be opened to them.

Our cousins have made a start of doing things differently; they have liberated part of the archive of the British "Special Operations Executive" and made it available to a mature and critical scholar, M. R. D. Foot. Mr. Foot's book, *SOE in France: An Account of the Work of the British Special Operations Executive in France, 1940-1944** is a praiseworthy example of an outsider trying "simply to explain what happened, without conscious bias in any direction," and endeavoring to write a healthy corrective to "the turmoil of under-informed publicity that has surrounded what has so far appeared in English about secret operations in France. . . ." The writing of the book's first draft took more than two years (note: for SOE in France, *only*). What happened next—namely, the clearance by "a number of people who had a claim to be heard on what it said," the consequent "further research and . . . some changes and amplifications of text," plus the actual publication—took another four years. From the book's beginning, when Prime Minister Macmillan authorized some research on the subject, through an official announcement of the project made to Parliament, to the actual printing, which was done by Her Majesty's Stationery Office, it had the standing of a piece of government-sponsored business. This is one way to handle an important bit of national history whose sources were highly classified 30 years ago. There are many other ways, any one of which would probably better serve the national interest than the one adopted by the author of *OSS*.

Sherman Kent

*Her Majesty's Stationery Office, London, 1966.

THE DOUBLE-CROSS SYSTEM IN THE WAR OF 1939-1945. By *J. C. Masterman*. (Yale University Press, New Haven and London, 1972. 203 pages.)

THE COUNTERFEIT SPY. By *Sefton Delmer*. (Harper & Row, New York, 1971. 256 pages.)

These two books deal largely with the same subject. The most important theme of each is a description of the various Allied deception efforts—particularly in the preparations for the Normandy invasion—in which the Allies were extraordinarily successful. One of the primary factors in achieving these deceptions, Masterman asserts, was that from June 1940 onward, the British controlled *all* German espionage agents in the United Kingdom. Through those who became double agents, they were able to feed the Germans a great deal of misleading material. Masterman's book deals with the whole British double agent operation of World War Two from the start, and describes all deception efforts springing from that operation. Delmer's book centers on one double agent, a Spaniard the British called Garbo, the Germans Cato, and whom Delmer calls Jorge Antonio. This Spaniard, a long-time double agent, was an important part of the Normandy invasion deception effort, which Delmer describes in considerable detail. Garbo's case is also described by Masterman.

Masterman was with the double agent side of MI-5, while Delmer was chief of the main British black radio propaganda effort, Soldatensender West.

Masterman wrote his book in the period July through September 1945, his last months with MI-5. It was the official history of the double agent operations, and so he had access to all pertinent documents. He explains this briefly in the preface and in an article in the Yale Alumni Magazine of February 1972. He took a copy of his (Top Secret) work with him when he retired to civilian life, and—as the years went by—started efforts to get permission to have it published. He felt very strongly that this should be done to "improve the image" of the Secret Service, which had suffered so many blows in the security flaps of the 1950's and 1960's. He did not succeed in getting it past the authorities until he turned it over to the Yale University Press.

In reading Masterman's book one is constantly struck by the fact that here is (1) a wonderful book describing (2) an astounding intelligence achievement, and one is continually getting the two mixed up. Perhaps it would be best first to describe the achievement in rough outline. The British intelligence services, military and civilian, the Foreign Office, the Home Office, etc., later on to be joined by an OSS representative, formed a committee to handle double agent cases. In war, double agent spells deception faster than in peace, and is much more important, so the committee was a vital affair. It was called "The Twenty Committee" after the Roman numerals XX, which also stand for double-cross. At the start there was the usual interdepartmental bickering, but after the committee was formed in January 1941, it met weekly until May 1945. Its chairman—Masterman—was appointed by the chief of MI-5. It had its teething troubles, but despite the fact that it had very little in the way of formal guidance and rules of procedure, "the organization had the supreme merit of working."

During the war the British gained control of some 120 German agents dispatched by the Germans. Many were of no importance and were not developed, but 39 were developed into double agent cases, some lasting three or four years, through whom a wide variety of deception material was fed to the Germans. As the British gradually and incredulously came to believe, they controlled all German agents in the U.K.

So much for the achievement, and now to the book. It is an excellent one. The British seem to be able to turn out superb writers as if from an assembly line. Masterman, a historian and novelist, is no exception and so the book is well written. The subject is complex—should one take each case separately and follow it through to the end? No, the answer is that you treat the double-cross operation as a chronological whole, since many of the agents play a role in the same deception operation and so the cases are interwoven. The book is very well organized and is in the form of a text book for the CI officers of the next war. In other words, it is a magnificent official history. It sets forth the events and also the lessons to be learned from them, and I cannot find any real fault with it. We may never, hopefully, find ourselves in the position of doubling enemy agents in our country in time of war, but if we ever do, we could do no better than to read the lessons, experiences, and techniques set forth in *The Double-Cross System.*

Perhaps the most important general lesson which Masterman pounds home is that there is no point in brilliantly capturing 120 German agents and doubling 39 of them unless you get a product which can be put to good use. No operations for operations' sake for him.

The book doubtless has a number of mistakes which a thorough CI analysis, drawing on information collected in the last 27 years, would reveal. But such an analysis would be of no interest to the general reader of this review. Therefore instead of delving deeply into individual cases, I will make only general comments and cite items of interest—and the book is chock full of them.

1) The British would not let their double agents keep the salaries paid by the Germans. The agent was paid a salary by MI-5, and turned his German remuneration over to his British case officer. The total amount collected by the British in this way amounted to £85,000, which made the whole operation almost self-supporting!

2) Communications intelligence played the most important role in the collection of intelligence. Masterman states this categorically. What he naturally skirts around is the fact that it also played a vital role in the collection of counterintelligence—was, in fact, the crucial element of the whole operation.

3) One of the prime reasons for the failure of the Abwehr espionage effort (and the success of the British double-cross effort) was the great independence which an Abwehr base, say in Lisbon, would have in recruiting and handling agents. This is an old and well-established German modus operandi—you give the subordinate the money and the requirements, and he recruits the agents and supplies the intelligence with no questions asked. This is an over-simplification, but is essentially true. The weakness of this system is that the case officer will tend jealously to guard his agent from all headquarters snoopers, and will thereby inevitably have a greater tendency to fall in love with his agent, hanging onto the man long after he should have been terminated. The German case officers did this, and their cases evidently were not subjected to hard-eyed CI reviews by others.

4) The trick of running a good double agent with the end view of deception is to build him up slowly. Once the luckless German case officer had come to accept the information sent to him by his agent (prepared by the British and containing enough truth to be credible) nothing would shake his faith. Even after an agent should presumably have been discredited for having reported extensively, for instance, on preparations for an invasion of Norway in 1944 (to take the German eyes off Normandy), the Germans went on believing him. "In short, it was extremely, almost fantastically,

difficult to 'blow' a well-established agent." One time the British tried to blow one in order to increase the credibility of the others, but no luck.

5) In an appalling (for them) number of cases, the Germans would instruct an incoming agent to contact one who was already established in the UK and communicating with them (via MI-5). The consequences are obvious.

6) Masterman did not consider all German case officers to be boobs, for he had a high regard for some aspects of their agent handling. But these skills foundered upon:

(a) the success of the British communications intelligence effort;
(b) the fatal German error of failing to compartmentalize their agents;
(c) a German need and tendency to accept intelligence uncritically from their agents; and
(d) the fact that Britain was an island, which made it much easier to control ingress.

7) Masterman feels that the best kind of agent is a notional one. Thus in the spring of 1944 Garbo, the Spaniard, had 14 agents and 11 contacts—all notional—reporting assiduously through him.

8) In the fall of 1940 more than 25 German agents landed in the UK either by parachute or small boat. All were picked up and none of them would have been able to operate their radios without the help of MI-5!

9) Masterman feels that "in some ways the most important practical lesson which we learned" was that if they tried to lure the Germans with an obviously disaffected Briton living in, say, Lisbon—perhaps a cashiered Army officer with an interest in Fascist organizations—"we almost always failed." The lesson is, don't try to create candidates.

10) Masterman stresses the need for meticulous record keeping, but urges a regular review and purging of files since the 50 volumes on Garbo were obviously too much!

11) The most important deception operations in which these double agents played a role (many other devices such as dummy landing craft, dummy tanks, fake radio traffic were used) were:

(a) the North African landings;
(b) the Sicilian landing (The Man Who Never Was came under the purview of the Twenty Committee);
(c) fake invasion preparations for Norway at the time of Normandy;
(d) fake invasion preparations for the Pas de Calais;
(e) fake minefields which forced U boats to make detours; and
(f) fake reports on the accuracy of V-1's and V-2's.

12) The effort with the V weapons caused much heart-burning. The British found the V-1's fairly well grouped, that is, they landed in a relatively small area of London. So their agents reported that they were landing 10 miles north of where they actually did; the Germans shortened the range and the missiles didn't hit London but the Kentish suburbs and countryside—which was tough on the Kentish men.

13) One agent, Tait, whom the British recruited in September 1940, received his last Abwehr message from Hamburg on 2 May 1945, only a few hours before the fall of that city.

There is an interesting quotation, a conclusion, to be found in the last chapter:

> So we come to this provisional—and admittedly theoretical—conclusion: in peacetime, espionage is easy and profitable, counterespionage is difficult and unrewarding; in

> wartime espionage is difficult and usually unprofitable; counterespionage is comparatively easy and yields the richest returns.

Masterman's peacetime-wartime theory is correct—in the Anglo-German context. For instance the Germans ran a very neat CI operation (Nordpol) in Holland where, for a couple of years, they captured and doubled all the 50-odd Dutch agents the British parachuted into that country. It was a brilliant success.

But the theory does not hold in the Anglo-Soviet context. Soviet peacetime espionage successes in the UK alone became increasingly apparent during the 1950's and 1960's. At the same time Soviet and other Communist successes at counterespionage during peacetime, while shrouded in a good deal of mystery, have become quite apparent; certainly the difficulty of conducting espionage operations in the Communist countries has. Therefore it would have been better for Masterman to have added a 1972 footnote to his theory, for his wartime conclusions are so brilliantly proven that the unwitting may come to accept his peacetime ones in all contexts too.

I have probably written too much about Masterman's book, but it is that rare thing which comes along once in an age where one cannot say enough. It rates an A Plus and should be read by every intelligence officer, and counterintelligence specialist, including those who are engaged in writing official histories.

For the normal reader, Delmer's book is hard to assess. It is eminently readable, indeed its style is almost too racy—he was not the star reporter of Beaverbrook's "*Daily Sexpress*" for nothing. As a sort of novel-style tradecraft manual, it is one of the best, and it deals with the Garbo case in far greater detail than Masterman with his five to six pages on the subject. We are regaled with fascinating details of dead drops, personal meetings, radio messages, and so forth. But is it worth much as history? The German side of the story comes from the West German Federal Archives, and from conversations with former Abwehr officers, as he acknowledges. But he neither cites documents nor acknowledges access to files so far as the British side is concerned; he mentions only people who helped him. And there are so many details.

Well, this writer happened to discuss the matter with a retired member of our organization who was with the OSS liaison group with MI-5 during the war. So I can report that Delmer had access to an official history of the Normandy invasion deception operation, which certainly had the details of that operation (which Delmer treats at length) and presumably of the Garbo operation, which was such an important part of it.

If I were writing a history of those times, however, I would quote Delmer with care, I certainly would not quote any of the conversations he quotes, and I would add a cautionary note when listing my sources. I am convinced he takes too many liberties.

That does not mean that it is not an enjoyable book to read, or that there are not good tradecraft lessons to be learned. Let me just cite a couple from the Garbo case.

Garbo was a young, well-educated Spaniard, who hated both Fascists and Communists. In January 1941, while in Madrid, he offered himself to the British as a double; he would go to work for the Germans, but would report to the British. They turned him down. He went to the Germans anyway with an offer to spy on the British, and was dispatched to London. He went no further than Lisbon, however, where he went into hiding, and for nine months sent the Germans reports presumably emanating from England! These were accepted even though Garbo had never been to England, had only very limited research materials and did not even know how many pennies there were in a shilling, and

shillings in a pound! Garbo told the Germans that his reports were carried from England to Lisbon by a BOAC air steward. They were then dead-dropped in a safe deposit box in a Portuguese bank. But never once did the Germans stake out the bank or check on the steward. In April 1942 when the British finally woke up to the fact (probably through radio intercepts) that here was an excellent agent (who had persuaded the Germans to have U-boats lie in wake for a fictitious convoy) they smuggled Garbo to England, and then timed the loading of the Lisbon drop with the arrival of a particular (unwitting) steward; the message actually went in the pouch and the drop was loaded by a Portuguese working for the British Lisbon station. The Germans never checked anyway! Garbo operated successfully until May 1945, and the reader will be happy to learn that not only did he receive an MBE (and an Iron Cross II) but the British gave him a bonus of £10,000.

For those intelligence officers who are familiar with the hard feelings which German intelligence officers had for Delmer's muckraking journalism in the 1950's (he had a series in the *Daily Express* on Gehlen which had headlines like "Nazi General Works for Uncle Sam"), there are some interesting examples of fence-mending. In thanking Frau Jodl for her help, he speaks of her husband as "an honourable officer and gentleman who was made a scapegoat for the crimes of the Third Reich." Then of Colonel Alexis Baron von Roenne, Gehlen's counterpart as chief of Army intelligence for the Western Front, who plays a considerable role in Delmer's book, and who was hanged for participating in the 20 July plot, Delmer says: "this courageous officer, true to the highest traditions of the much-maligned Prussian aristocracy . . ." Sefton has come a long way.

Frank Wooley

C.I.A. THE MYTH AND THE MADNESS. By *Patrick J. McGarvey.* (Saturday Review Press, New York, 1972. 240 pages.)

It may appear superfluous for a partisan of CIA to review this book when a critic of CIA* has already dismissed it as "bad writing, bad taste, and bad logic," but Pat McGarvey has loaded his book with references to his "14 years in intelligence," and thus presents himself to the reading public as the wise old insider purveying the lowdown on what goes on within the Agency. That makes it necessary to examine his credentials and make some evaluation of the contents.

To come to grips with those 14 years, the first eight of them were spent rising to Staff Sergeant in the U.S. Air Force Security Service, performing voice intercept at Pyaeng Yang Do in the Yellow Sea, Kimpo Air Base near Seoul, and another air base near Tokyo. As McGarvey describes it (pp. 43-48), these eight years were devoted largely to goldbricking and scheming to goof off. In May 1963, he joined CIA as a Junior Officer Trainee. He spent 16 months on the OTR strength, and then in October 1964 was assigned to Saigon for nine months. Finally, he worked in the Office of Current Intelligence from July of 1965 until his resignation in June of 1966. Inasmuch as his book purports to be about CIA, one must prune his 14 years down to something less than two years of what might overgenerously be called productive work for CIA. (He notes that while in Saigon he also found it possible to goldbrick by stretching one contact report over several days' reporting, to give the impression of frenzied activity.)

After this study in depth of CIA, McGarvey then put in a final three years at DIA, and one of the banes of this book is that he switches his narrative back and forth between CIA and DIA with little warning. Not only did a substantial number of the incidents he purports to relate occur at CIA when he was no longer there to observe them, but on quick reading there is a tendency—particularly in view of the title—to attribute his gripes about DIA lifestyles to the Central Intelligence Agency.

This is a slipshod book, slipshod from the research through the writing and the editing to the proofreading. On four separate occasions McGarvey complains that as a trainee he received four solid weeks of lectures on the organization of the intelligence community, and then proceeds to demonstrate he would have been better off with eight. "I still do not comprehend fully after 14 years' experience the complex structure of intelligence," he writes. He places the President's Foreign Intelligence Advisory Board *over* the National Security Council, lists Scientific and Technical Intelligence as Basic Intelligence, refers to the Current Intelligence Weekly as "well-bound" and the Current Intelligence Digest as still extant, and misnames the Guided Missiles and Astronautics Intelligence Committee and the Joint Atomic Energy Intelligence Committee, with both of which he must have dealt while in OCI. McGarvey can hardly blame the proofreaders for his consistent misspelling of Admiral Raborn's name throughout the book, but one can perhaps let him off the hook for the allegation that "the JSC" recommended an air strike when the North Koreans seized the *Pueblo* "and interred its crew."

McGarvey's leitmotif is probably best expressed on page 116:

> The intelligence community is a bureaucratic morass, a fragmented, disjointed effort in which no one seems responsible for momentous decisions, where vested interests are coldly played off against one another, where men of varied expertise stifle the unorthodox and opt for wretched half-measures or compromises so weakened by consensus that they would be better not taken at all.

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*Thomas B. Ross, co-author of *The Invisible Government,* in a 31 December 1972 review for the *Washington Post.*

A St. George who hacks away at the multiplication of committees, and distortion of conclusions to serve a vested interest or accepted thesis, and the "truck kill" mathematics of Southeast Asia—this is normally a man who deserves one or two more cheers than does the dragon. McGarvey blows it, however, by overkill, by purple prose—everything seems to be "infamous" or "notorious"—by questionable writing techniques, and by just plain misrepresentation.

Follow one of his recurrent themes, and learn that CIA and the whole intelligence community have "little or no central direction" (page four) and "lack central direction and control" (page 16). For counterpoint, however, behold Richard Helms as the Machiavelli who terrorized his predecessor, destroyed his rivals, scoffs at the President's Foreign Intelligence Advisory Board, holds Congress at arm's length, and has the nation's press in his pocket. McGarvey's Helms is also responsible for the Army's acquisition of dossiers on private American citizens, although some time in those four belabored weeks they *must* have told McGarvey that the CIA's charter provides no police power and no domestic mission. McGarvey's Helms, in short, doesn't emerge as a man who would be remiss in central direction and control.*

There is other evidence that McGarvey neither wrote this book in one chunk, nor edited it that way. On page 185, for instance:

> The promotions within the clandestine service are considerably slower than anywhere else in intelligence . . . Analysts in the production side of the house, on the other hand, rise faster than their contemporaries in dirty tricks,

while on page 115,

> The only place to be in intelligence these days is in collection. There is a ten-to-one leverage between a collection program and the production of intelligence . . . Any smart guy in intelligence today will get out of the production side as soon as he can. He's on a dead-end street if he doesn't.

And after devoting a major part of the book to flaying bureaucracy, McGarvey concludes on page 224 that the answer is to get rid of a professional intelligence man as DCI in favor of a "manager and administrator."

The book is so full of misstatements and distortions that a few examples will have to suffice:

1. The Deputy Director, Intelligence, who failed to predict the ouster of Khrushchev "was himself ousted and exiled to Taiwan . . . for his misdeeds." (Ray Cline, DDI when Khrushchev fell in October 1964, had gone to Taiwan some five years earlier, and came *from* Taiwan to the DDI position.)
2. The staff of the President's Foreign Intelligence Advisory Board "was drawn from the upper staff echelons of CIA. . . . Their impartiality can be seriously questioned." (The "staff" at PFIAB, over the period McGarvey discusses, consisted of one man, an ex-FBI agent.)
3. "Compare, if you will, the *New York Times*' lust to uncover any scandal it can about the Pentagon with its absence of critical reporting on CIA." (In May, 1966, before McGarvey resigned from CIA, the *Times* ran a week-long exposé amounting to at least one full page a day.)
4. At CIA, "Every single top-management position is filled by a WASP. . . . It was the subject of a very close Inspector General study in

*McGarvey's final chapter shows (pp. 228-233) that he finished the book in the awareness of The President's directives in late 1971 providing for greater centralization of control, so he must also know that until 1972, the DCI's authority over the intelligence community amounted to *coordination* rather than any form of *direction*. He makes no allowances for this, however, when blaming CIA for the duplication, lack of centralization, etc.

1964." (There was never any such study, and those familiar with the DCI's morning meetings can attest that the attendance is far from WASP-ish.)

5. "CIA's involvement with the Green Berets in political assassination in Vietnam even threatened the fiber of the American system of justice." (CIA's "involvement" consisted exclusively of telling the Green Berets they shouldn't execute the man, and then helping a witness blow the whistle when they did anyway.)

6. Finally, there is McGarvey's charge that Congressional overseeing of CIA is a "fiction." He acknowledges that the DCI reports to four special subcommittees in the Senate and House on CIA matters, in addition to the substantive intelligence briefings given routinely to other Congressional committees, but dismisses this with a purported quote from Senator Fulbright:

> It is all very hush-hush . . . The director of CIA spends most of the time talking about the Soviet missile threat and so on. The kind of information he provides is interesting, but it really is of little help in trying to find out what is going on in intelligence. He actually tells them only what he wants them to know.

I recall two hearings to which I accompanied two directors. The first was a substantive briefing for the Senate Foreign Relations Committee by Admiral Raborn, during which the Chairman, Senator Fulbright, spent a good part of two days trying to persuade the DCI to discuss a CIA operational policy which Fulbright feared might impinge on his Fulbright scholars abroad. Admiral Raborn, as required by both Congress and the National Security Council, repeatedly informed the senator that he was not authorized to discuss operational methods and policies with that particular committee.*

Subsequently, I was present at the first meeting Senator Fulbright attended of the late Senator Richard Russell's CIA subcommittee—a joint subcommittee of Armed Services and Appropriations, with three invitees from Foreign Relations added as observers. Ultimately it was Fulbright's turn to ask questions. He had just begun a careful introduction to the question I had heard him ask Raborn so often, and so fruitlessly, when Helms pulled a paper from his briefing book and handed it to him. It was a complete and detailed answer to Fulbright's question, including the original text of the policy directive involved. The senator had no further questions.

As for some of McGarvey's writing techniques, there is the Bellman's Approach (what I tell you three times is true), the Creeping Confirmation, and the Composite Witness. I have already mentioned his repeated references to those four weeks of organization charts. There are also three citations of what McGarvey claims is an old DIA folk saying, "If you want it real bad, you're gonna get it real bad." I didn't really want it that bad.

Creeping Confirmation, an old Goebbels technique, consists of moving from a planted rumor to "bekanntlich" to a flat statement of fact. Thus on page 23, for instance, Congressman Jamie Whitten has "heard" that the total intelligence expenditure "goes in excess of four to five billion dollars a year." By page 32, it is McGarvey's established fact that "intelligence spends four to five billion dollars a year amassing information" and by page 234, no doubt allowing for inflation and rising costs, the taxpayers should protest to the President because, flatly, "it is they who are paying the five billion dollars a year."

*The exchange, spread over a Friday and a Monday, was at times so heated that it was difficult to believe Raborn and Fulbright had been partners on the golf course Saturday in fleecing a couple of pigeons.

Throughout the book, McGarvey has horror stories of the Brilliant Young Junior Analyst (BYJA) who makes a deduction, establishes a truth, or reaches a startling new conclusion, and then can't get past his boss to publish it because it might make waves, or disagrees with accepted views. Invariably the BYJA is reprimanded, fired, or reassigned as motor pool officer at Akureyre, Iceland.

A considerable effort has been made to identify some of these BYJA's from McGarvey's clues as to time, subject, location, personality, field of competence, etc. Generally, it developed that McGarvey's protagonist was an amalgam of fact and fiction, and a composite of several people. This is a handy way to avoid libel suits, but hardly constitutes factual reporting.

One of McGarvey's training officers, for example, was Ray, who according to the book had run the base from which the Bay of Pigs operation was launched, was an expert in amphibious operations, drank, and had family trouble. For the facts, there may have been an officer at the training center at that time who had been involved in the Bay of Pigs operation. But Ray, who had run a Cuban operations station *after* the Bay of Pigs, was a maritime instructor, sober and with no family problems, while yet another instructor there at the time had had a dust-up with his wife and was prone to mid-day tipsiness.

Then there was the persnickety supervisor from Cornell, newly in charge, who exiled "Ed," one of his BYJA's, to Training because the BYJA wore white socks to work. This BYJA could be fairly readily identified by other evidence. The supervisor was actually from Syracuse. They had a harmonious relationship of nearly two years before the BYJA accepted an offer from Training because he appeared to have more room for promotion there. And the BYJA deposes that his wife won't let him wear white socks, except to athletic events.

"George," the DCI's Special Assistant for Vietnamese Affairs, on page 187 enraged Walt Rostow at the White House with his assessment of the January 1968 Tet offensive. Within a month he was assigned to London for a year's study. McGarvey notes that because George was upper echelon rather than BYJA, he was exiled to a "cushy overseas post," but nevertheless exiled. In this case, both SAVA and his then deputy answered to "George," and both admit to having had brisk exchanges with Rostow, but George Carver never went to London, and his deputy, George Allen, says his dust-up with Rostow was in the fall of 1967; at that time, he had already been nominated for the Imperial Defense College, was held over in his job for another year because he could not be spared, was promoted shortly after Tet, went to London, and now is Director, Imagery Analysis Service.

"Barry" on pages 193-194 detected evidence of Chinese Communist preparations for a nuclear shot two weeks before their first test on 16 October 1964, and predicted it in a paper refuting existing estimates. His unappreciative boss pigeonholed the paper until after the detonation. In actual fact, the Joint Atomic Energy Intelligence Committee concluded as early as May 1964—five months *before* the test—that a test was planned for the reasonably near future. The Office of Scientific Intelligence *Surveyor* reported on 24 September that construction at the test site had been completed. Secretary of State Dean Rusk issued a press statement on 29 September that the Chinese were approaching the point where they might be able to detonate a nuclear device. OSI's *Surveyor* on 15 October reported further evidence that the test was imminent.

"Tom" wrangled with his unfeeling boss over a paper disputing Air Force estimates of MIG strength in Eastern Europe. He confronted the Air Force with his analysis, ultimately appealed to the Bureau of the Budget which sustained Tom over both the Air Force and his boss; for this, two weeks later he was transferred to a "staff job processing paperwork" with "little hope for promotion." "Tom"—actually Doug—in fact wrote a paper in 1969, three years after

McGarvey's departure, challenging the Air Force/DIA conclusions cited by McGarvey. The Bureau of the Budget was not involved, his superior did not thwart him, the paper was accepted and published, and shortly thereafter Tom/Doug received a promotion.

"Bill" on pages 190 and 191 invented the science of "cratology." All of his superiors had the glory of briefing at the White House, all received commendations and promotions, one of them received a $1,000 award, while "Bill" got drunk at the ensuing celebration, told off four layers of bosses above him, and received a formal letter of reprimand. "Bill" is surely Victor Marchetti, and while he played a prominent part in developing "cratology," his role was no greater than those of Harry Eisenbeiss, then chief of the Current Support Staff of ORR; John Yeo, whom Marchetti replaced when Yeo was assigned to Hawaii; and Thaxter Goodell, the staff's specialist on arms traffic and the Soviet shipping account.* During the Soviet build-up in Cuba, Eisenbeiss, Goodell, and Marchetti were all briefing on almost a daily basis. Eisenbeiss did in fact receive a Certificate of Merit with Distinction with an award of $500 for his over-all performance during the flap, and he blew the whole $500 on a party for the staff before he found out he would have to pay taxes on the award. Marchetti may indeed have gotten "smashed," but he made no speech, didn't tell off four layers of bosses—Eisenbeiss was the only superior there—and there was *no* reprimand, oral or written. Instead, Marchetti got a reassignment to the Office of National Estimates which led to a promotion.

These, then, are some of the "myths" McGarvey has created, and they bring us pretty close to the "madness." "Bill" isn't quite Bill, "Tom" isn't quite Tom, "Ed" isn't quite Ed, and "Barry" just isn't. Could it be they are all just surrogates for Pat, an embittered BYJA, frustrated because some of his views were questioned by his superiors and challenged in coordination? Therein lies "The Myth and the Madness."

Clinton B. Conger

*Goodell, "Cratology Pays Off," *Studies* VIII/4.

THE RUSSIAN SPACE BLUFF. By *Leonid Vladimirov*. (Tom Stacey, Ltd., London, 1971. 192 pages.)

Books about the Soviet space program generally originate from one of two sources: the Soviet press corps, or "knowledgeable" writers in the West. Those written by Soviet journalists—most recently Evgeny Ryabachikov's *Russians in Space*—present the Soviet space program in the best light possible. The facts are carefully selected so as to present only successes, no failures. The western writer, on the other hand, usually is woefully lacking in facts of either category. Myths, inaccuracies, and biases abound. Consequently, neither of these sources provides an accurate assessment of the program.

The Russian Space Bluff is another matter. "Leonid Vladimirov," (actually Leonid Vladimirovitch Finkelshteyn) is a refugee from the USSR who settled in the United Kingdom in 1966. An engineering graduate from a Soviet University, in 1960 he became a writer for a Soviet magazine specializing in scientific subjects, including the space program. This position apparently gave him the expertise with which to write his book. His description of the manned spacecraft, the details he provides on some of the space flights, and the names and positions cited for various important figures within the program can all be confirmed from intelligence sources, providing a ring of authenticity. There is no doubt that Vladimirov's contacts provided information that he would not have been allowed to publish within the USSR.

The one apparent weakness in Vladimirov's technical background is in the area of space booster development. He regularly attributes the inadequacies of the space program to the lack of reliable boosters. In fact, the exact opposite is true: the one major lead the USSR had over the United States in the early days was in its development of space boosters.

This failing, however, should not discredit Vladimirov's conclusions. He asserts that the USSR is far behind the U.S. in space technology at the present time and that the Soviet Union could not now undertake a program like Apollo. This appraisal of the situation today is unquestionably correct. Indeed, the author goes one step further. He states that the Russians never were ahead, even in the days of Sputnik I. This conclusion, seemingly in error at first glance, probably is not far from the truth. Despite the appearance of superiority that the Soviet space program had at the time, the effects of the backward technology used can be seen, in retrospect, as early as Vostok I, the first manned flight.

The Russian Space Bluff is built around the early Sputniks and the man-in-space program. These are the prestige portions of the overall Soviet space program and those which, as an editor, Vladimirov most likely came in contact with. He states that Sputnik I was launched after only three months of preparation, in order to be "first." He also observes that the "lead," once established, had to be maintained. He then contends that the Soviet leadership, aided by its own policy of strict secrecy coupled with the American policy of announcing plans, was able to achieve an image of "first" after "first" despite vastly inferior technical capability.

That the Soviets did achieve a series of "firsts" despite inferior technical know-how can hardly be disputed. The remaining question is that of program planning. Vladimirov has given an exaggerated picture of a poorly organized space program, with missions chosen only in reaction to U.S. plans. In reality, this is probably not the case. No program of the size and scope of the Soviet space program could be as successful as it has been without careful planning.

If there is a main character in the book, it is Academician Sergei P. Korolev. Korolev, a top Soviet designer, is pictured as the individual who overcame the

backward technology and the conditions imposed by the regime, and who put the USSR into the lead in space. Vladimirov believes that Korolev ran virtually a one-man show until his death in 1966. This characterization of Korolev is probably quite accurate. There is ample evidence that he was at least the guiding light of the early program. The number of major problems that the Soviets have encountered since his death attest to his great importance in the space program. For example, the Soyuz program has produced only two successful flights in seven attempts in almost six years. The failures have included the deaths of four cosmonauts. In addition, Soviet ambitions in unmanned lunar exploration have been slowed significantly since 1966 by booster development problems.

The Russian Space Bluff is interesting reading for anyone who wishes a fresh view of the Soviet space program. It can be read quickly and is a fairly good presentation of the condition of the Soviet program as we know it today.

Robert A. Anderson

DECEPTION GAME: CZECHOSLOVAK INTELLIGENCE IN SOVIET POLITICAL WARFARE. By *Ladislav Bittman.* (Syracuse University Research Corporation, Syracuse, N.Y., 1972. 246 pages.)

A defector who happens to be an accomplished author has written the first inside story on "disinformation" to emerge from any intelligence service in the Soviet Bloc. Ladislav Bittman tells his own story with lively directness, humor, and clarity.

Bittman was a professional intelligence officer in the Czech service from 1954 to 1968. During the "Prague Spring," he was a supporter of Dubcek's reform Communism. Shortly before the Soviet invasion of Czechoslovakia, Bittman submitted plans for reorganizing the foreign intelligence service and removing it from jurisdiction of the Ministry of Interior.

Not the only Czechoslovak intelligence officer whose life was radically and irrevocably changed by the Soviet invasion, Bittman tells a story most of which jibes with hard data and is thoroughly credible. Bittman once had a lot going for him. By 1968 he was under cover as First Secretary and Press Attaché in Vienna. He held two degrees from Charles University in Prague, was author of two books, and spoke five languages. He had a Communist Party record which began as a boy activist two years before the 1948 coup. He had been to Korea with the Neutral Nations Reparation Commission; worked for several years as an analyst in Czechoslovakia's DDI; handled agents as a case officer in Berlin; and he had traveled abroad with the chief of service. In the mid-1960's he helped to create and then serve as Deputy Chief of Czechoslovakia's first functional deception unit (called the "disinformation and active measures department").

"Deception Game" is mainly about Czechoslovak deception operations. Secondarily, it is about Soviet exploitation of the Czechoslovak intelligence service. Between the lines it is also about one man's past illusions of Communism. The prose is delightfully direct. Some awkward Communist usages may jolt the reader, but the text probably will make immediate sense to Kremlinologists and non-Kremlinologists alike. Long-range planning, personnel management, incentives, the cagey hand of the KGB, and Party approvals—all are presented in the context of actual cases. A chapter entitled "Scapegoat for the World's Troubles" concentrates on operations against the United States—forged letters to and from American ambassadors, and a forged questionnaire.

Inexpensive and technically crude operations succeeded because they were developed from ideas submitted to Prague by officers who had been deep into the political scene where they were stationed. Officers who could identify rivalries, gullible editors, or bearers of grudges and could pick up some hot gossip were able to win cash bonuses by suggesting tricks that seemingly worked. A policy bias in favor of cheap "paper operations" evolved (not entirely from success). The service had suffered an expensive fiasco in 1963. It had tried to stage a massive "Continental Congress for Solidarity with Cuba" (codename "DRUZBA") in Brazil. On instructions from the KGB, however, it had relied solely upon the Communist Party of the Soviet Union to see to it that the international front organizations such as the World Peace Council would shepherd the star attractions to Brazil. On the appointed day, no Cubans and only one Soviet, a newspaperman, showed up! Bittman wrote:

> Latin America was not ripe for any qualitative changes. . . . A return to Operation DRUZBA was impossible. Whenever I mentioned DRUZBA to my colleagues, their reaction was accompanied by such comments as "Russian bastards." Operation

> Thomas Mann,* representative of the so-called paper operations which consisted of the production of falsifications and their anonymous distribution through postal delivery rather than live agents, set patterns for future operations. . . . Repeated refutation of the Mann and Hoover forgeries by American authorities went unheeded by the leftist press. . . . It is interesting that the Communists and the leftist Latin American press became primary victims, since the conservative media did not trust and were not deceived by the disinformation message of Operation Thomas Mann.

Some operations mounted by the German Department of the Czechoslovak service in the mid-1950's were violent and sensational. Two were package bombings, designed to substantiate fears of resurgent Nazism in West Germany. In 1964 came "Operation Neptune," in which Bittman arranged that a television crew filming a travelogue would discover a phony cache of hidden Nazi documents by the trunkful. Bittman in his Scuba gear finally ended up on film in a documentary which won a special award of the International Radio and Television Organization at the Leipzig Film Festival. Operation Neptune earned Bittman a bonus too, but only after months of work including an all-night session with a Minister of Interior who almost got cold feet on the eve of a crucial international ceremony and press conference. The Minister, incidentally, was Dr. Lubomir Strougal, Czechoslovakia's present Prime Minister.

The "Billy Palmer" operation in 1965 was meant to "prove" that an American businessman in Jakarta was working for CIA on a plot to overthrow President Sukarno. The "Gilchrist letter" forgery was one of several vehicles used to deliver intermeshing disinformation messages to Sukarno. Other vehicles used during the course of several months were journalists in Jakarta and a philandering Indonesian ambassador in Europe with whom the Czechoslovaks had cultivated a symbiotic relationship. The operation succeeded, but hardly had the toasts to it been drunk in Prague than the "Movement of 30 September" tried to exploit the mood it had created. In consequence, several hundred thousand Indonesians and the Indonesian Communist Party were destroyed.

Bittman was the Czech disinformation specialist who established liaison with Hungarian and East German deception experts, and for one week in 1965 he escorted the late Gen. Ivan Ivanovich Agayants, then was Chief of the KGB's Disinformation Directorate. Bittman prudently refrains, however, from excessive generalization about the Soviets; he admits that the Soviet advisors kept their cards close to the chest.

The public and the media in Western Europe had become supersensitive to forgeries by the early 1960's. Many of the official successes of the mid-1960's were generated by cheap forgeries and stereotyped gimmicks in Third World countries. These were often timed and calculated to deepen existing prejudices. Bittman concluded, however, that these operations often antagonized but seldom influenced Western leaders. Deceiving Western leaders was supposedly the highest form of the game, but the Czechoslovaks settled for inciting "moral isolation" of the United States; promoting nationalistic divisiveness in NATO; and aggravating distrust of West Germany.

Bittman and Agayants may have enjoyed a golden era for deception operations. Their successors may find it more difficult to operate in a world which has become wiser and warier. In any event, Americans who contemplate what the Soviets are up to will find most of "Deception Game" both entertaining and rewarding.

Thomas F. Meeksbroth

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*A forgery indicating that U.S. Assistant Secretary of State Thomas Mann had issued notification of a new hard line on aid to Latin American countries.

BRIEFLY NOTED

THE CHAMPAGNE SPY. By *Wolfgang Lotz.* (St. Martin's Press, New York, 1972. 240 pages.)

First person account by Ze'ev Gur Arieh, alias Wolfgang Lotz, of his seven years as an active operative of the Israeli Intelligence Service in Cairo. The author, exchanged after three years in prison and now living near Tel Aviv, cleared his book before publication with the IIS. As a result, both he and the IIS appear overly infallible, but the book is entertaining and worth reading. "Lotz" posed as a wealthy Nazi German horse breeder, and successfully penetrated high Egyptian society.

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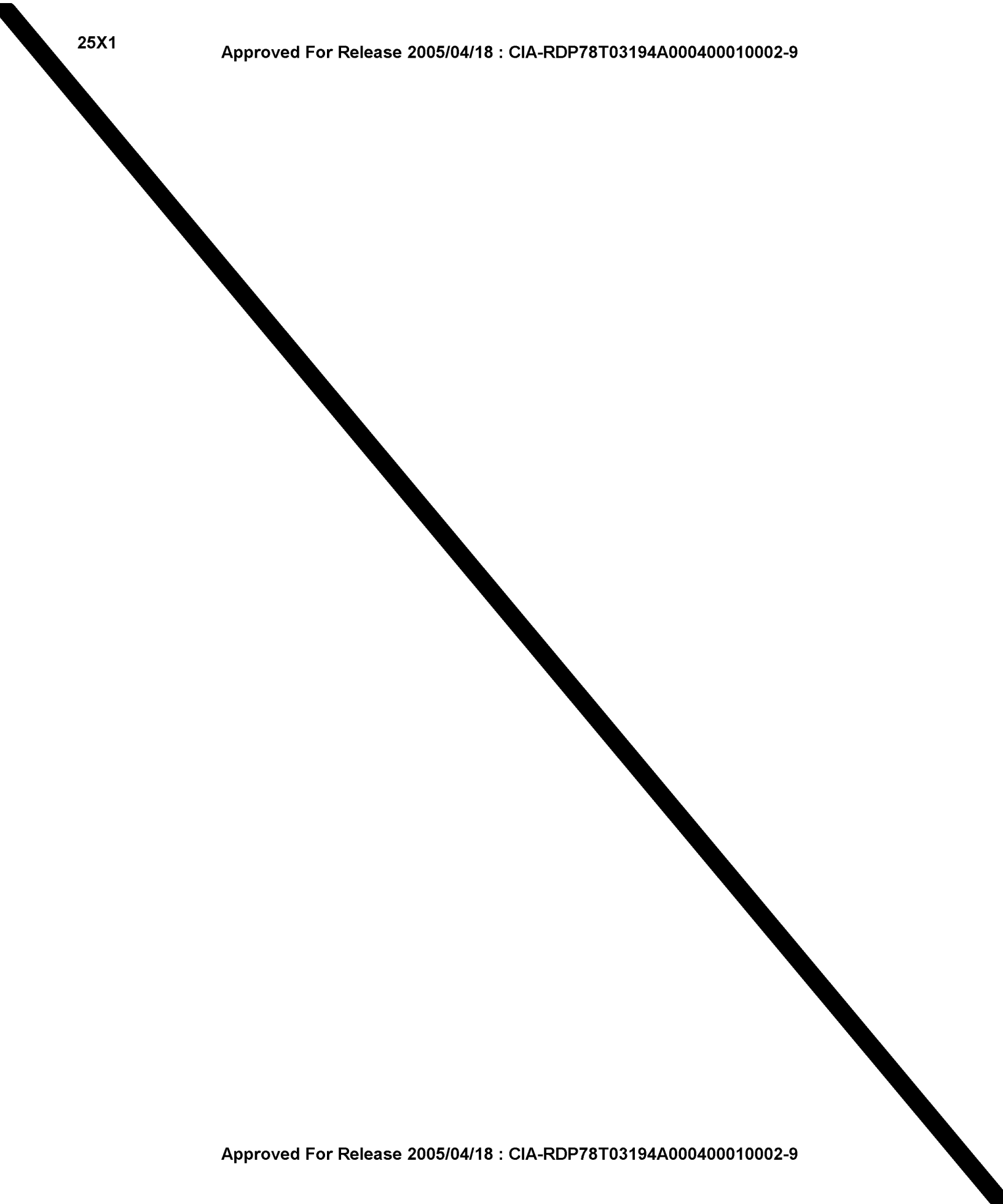

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March 1973

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The first photographic reconnaissance satellite

CORONA

Kenneth E. Greer

When the U-2 began operating in the summer of 1956, it was expected to have a relatively short operational life in overflying the Soviet Union—perhaps no more than a year or two. That expectation was based not so much on the likelihood that the Soviets could develop the means of shooting it down, as on their ability to develop a radar surveillance network capable of tracking the U-2 reliably. With accurate tracking data in hand, the Soviets could file diplomatic protests with enough supporting evidence to generate political pressures to discontinue the overflights. As it turned out, the United States had underestimated the Soviet radars, which promptly acquired and continuously tracked the very first U-2 flight over Soviet territory. The Soviets filed a formal protest within days of the incident, and a standdown was ordered.

For nearly four years, the U-2 ranged over much of the world, but only sporadically over the Soviet Union. Soviet radar was so effective that each flight risked another protest, and another standdown. Clearly, some means had to be found to accelerate the initial operational capability for a less vulnerable successor to the U-2. Fortunately, by the time Francis Gary Powers was shot down near Sverdlovsk on 1 May 1960 (fortunate for the intelligence community, that is—not for Powers), an alternative means of carrying out photographic reconnaissance over the Soviet Union was approaching operational readiness. On 19 August 1960, just 110 days after the downing of the last U-2 overflight of the Soviet Union, the first successful air catch was made near Hawaii of a capsule of exposed film ejected from a photographic reconnaissance satellite that had completed seven passes over denied territory and 17 orbits of the earth. The feat was the culmination of four years of intensive and often frustrating effort to build, launch, orbit, and recover an intelligence product from a camera-carrying satellite.

At about the time the U-2 first began overflying the Soviet Union in 1956, the U.S. Air Force was embarking on the development of a strategic reconnaissance weapons system employing orbiting satellites in a variety of collection configurations. The program, which was designated WS-117L, had its origins in 1946 when a requirement was placed on the RAND Corporation for a study of the technical feasibility of orbiting artificial satellites. The first real breakthrough had come in 1953 when the USAF Scientific Advisory Board reported to the Air Staff that it was feasible to produce relatively small and light-weight thermonuclear warheads. As a result of that report, the ATLAS ICBM program was accorded the highest priority in the Air Force.

Since the propulsion required to place a satellite in orbit is of the same general order of magnitude as that required to launch an ICBM, the achievement of an ICBM-level of propulsion made it possible to begin thinking seriously of launching orbital satellites. Accordingly, General Operational Requirement No. 80 was levied in 1955 with the stated objective of providing continuous surveillance of pre-selected areas of the world to determine the status of a potential enemy's war-making capacity.

The Air Research and Development Command, which had inherited the RAND study program in 1953, assigned the satellite project to its Ballistic Missile Division. The development plan for WS-117L was approved in July 1956, and the program got under way in October 1956 with the awarding of a contract to the Lockheed Aircraft Corporation for the development and testing of the system under the program name [redacted] 25X1

The planning for WS-117L contemplated a family of separate systems and subsystems employing satellites for the collection of photographic, [redacted] 25X1 and infrared intelligence. The program, which was scheduled to extend beyond 1965, was divided into three phases. Phase I, the THOR-boosted test series, was to begin in November 1958. Phase II, the ATLAS-boosted test series, was scheduled to begin in June 1959 with the objective of completing the transition from the testing phase to the operational phase and of proving the capability of the ATLAS booster to launch heavy loads into space. Phase III, the operational series, was to begin in March 1960 and was to consist of three progressively more sophisticated systems: the Pioneer version (photographic and [redacted] 25X1 the Advanced version (photographic and [redacted] 25X1), and the Surveillance version (photographic, [redacted] 25X1 and infrared). It was expected that operational control of WS-117L would be transferred to the Strategic Air Command with the initiation of Phase III.

It was an ambitious and complex program that was pioneering in technical fields about which little was known. Not surprisingly, it had become apparent by the end of 1957 that the program was running behind schedule. It also was in trouble from the standpoint of security. The U-2 program was carried out in secret from 1956 until May 1960. Its existence was no secret to the Soviets, of course, but they chose to let it remain a secret to the general public (and to most of the official community) rather than publicize it and thereby admit that they lacked the means of defending their air space against the high-flying U-2. WS-117L was undertaken as a classified project, but its very size and the number of people involved made it impossible to conceal the existence of the program for long. The press soon began speculating on the nature of the program, correctly identifying it as involving military reconnaissance satellites, and referring to it as BIG BROTHER and SPY IN THE SKY. The publicity was of concern, because the development of WS-117L was begun in a period when the international political climate was hostile to any form of overflight reconnaissance.

It was against this background that the President's Board of Consultants on Foreign Intelligence Activities submitted its semi-annual report to the President on 24 October 1957. The Board noted in its report that it was aware of two

advanced reconnaissance systems that were under consideration. One was a study then in progress in the Central Intelligence Agency concerning the feasibility of a manned reconnaissance aircraft designed for greatly increased performance and reduced radar cross-section; the other was WS-117L. However, there appeared little prospect that either of these could produce operational systems earlier than mid-1959. The Board emphasized the need for an interim photo reconnaissance system and recommended that an early review be made of new developments in advanced reconnaissance systems to ensure that they were given adequate consideration and received proper handling in the light of then-existing and future intelligence requirements. The Executive Secretary of the National Security Council on 28 October notified the Secretary of Defense and the Director of Central Intelligence that the President had asked for a joint report from them on the status of the advanced systems. Secretary Quarles responded on behalf of himself and Mr. Dulles on 5 December with a recommendation that, because of the extreme sensitivity of the subject, details on the new systems be furnished through oral briefings.

As a consequence, there are no official records in CIA's Project CORONA files bearing dates between 5 December 1957 and 21 March 1958, but it is clear that major decisions were made and that important actions were undertaken during the period. In brief, it was decided that the photographic subsystem of WS-117L offering the best prospect of early success would be separated from WS-117L, designated Project CORONA, and placed under a joint CIA-Air Force management team—an approach that had been so successful in covertly developing and operating the U-2.

The nucleus of such a team was then constituted as the Development Projects Staff under the direction of Richard Bissell, who was Special Assistant to the DCI for Planning and Development. Bissell was designated as the senior CIA representative on the new venture, and his Air Force counterpart was Brigadier General Osmond Ritland, who, as Colonel Ritland, had served as Bissell's first deputy in the early days of the Development Projects Staff and later became Vice Commander of the Air Force Ballistic Missile Division.

Bissell recalls that he first learned of the new program and of the role intended for him in it "in an odd and informal way" from Dr. Edwin Land. Dr. Land had been deeply involved in the planning and development of the U-2 as a member of the Technological Capabilities Panel of the Office of Defense Mobilization. He continued an active interest in overhead reconnaissance and later headed the Land Panel, which was formed in May 1958 to advise on the development of OXCART, the aircraft planned as the successor to the U-2. Bissell also recalls that his early instructions were extremely vague: that the subsystem was to be split off from WS-117L, that it was to be placed under separate covert management, and that the pattern established for the development of the U-2 was to be followed. One of the instructions, however, was firm and precise: none of the funds for the new program were to come from monies authorized for already approved Air Force programs. This restriction, although seemingly clear at first glance, later led to disagreement over its interpretation. CORONA mangement expected that the boosters already approved

for the THOR test series of WS-117L would simply be diverted to the CORONA program; this proved not to be so. As a consequence, CIA had to go back to the President with an admission that the original project proposal had understated the estimated cost and with a request for more money.

Roughly concurrent with the decision to place one of the WS-117L subsystems under covert management, the Department of Defense realigned its structure for the management of space activities. The Advanced Research Projects Agency (ARPA) was established on 7 February 1958 and was granted authority over all military space projects. The splitting off of CORONA from WS-117L was accomplished by a directive from ARPA on 28 February 1958, assigning responsibility for the WS-117L program to the Air Force and ordering that the proposed WS-117L interim reconnaissance system employing THOR boost be dropped.

The ARPA directive ostensibly cancelling the THOR-boosted interim reconnaissance satellite was followed by all of the notifications that would normally accompany the cancellation of a military program. The word was passed officially within the Air Force, and formal contract cancellations were sent out to the prospective suppliers. There was much furore when the cancellations went out: contractors were furious over the suddenness of the action; Air Force personnel were thunderstruck at the abandonment of the WS-117L photographic subsystem that seemed to have the best chance of early success. After the cancellation, very limited numbers of individuals in the Air Force and in the participating companies were cleared for Project CORONA and were informed of the procedures to be followed in the covert reactivation of the cancelled program.

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After Bissell and Ritland had worked out the arrangements for the [redacted] they then began tackling the technical problems associated with the design configuration they had inherited from WS-117L. The subsystem in point contemplated the use of the THOR IRBM as the first stage booster and, as a second stage, Lockheed's modification of a rocket engine that had been developed by Bell Aircraft for take-off assist and auxiliary power applications in the B-58 HUSTLER bomber. It was referred to as the HUSTLER engine during the development phase of WS-117L but soon came to be known as the AGENA—the name it bears today.

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One of the very early CORONA plans called for spin stabilization of the payload, with the camera scanning as the payload rotated. The contractors working on this subsystem design were Lockheed on the space vehicle, and Fairchild on the camera. The camera was to have a focal length of six inches, without image motion compensation. Ground resolution was expected to be poor with

this short focal length, particularly if combined with the readout techniques envisaged by WS-117L.

Several important design decisions were implemented in this organizational period of CORONA. Recognizing the need for resolution to meet the intelligence objectives, it was concluded that physical film recovery offered the most promising approach for a usable photographic return in the interim time period. This resulted in the addition to the design of a recovery pod or capsule with General Electric selected as the recovery vehicle contractor. In retrospect, the decision on film recovery would prove to be one of the most important made in U.S. reconnaissance activities, in that all photo reconnaissance systems developed up to the current time have relied on physical recovery of film.

Another major decision for the new CORONA Program came in late March 1958, following a three-day conference in San Mateo, California, among representatives of CIA, Air Force Ballistic Missile Division, Lockheed, General Electric, and Fairchild. The discussion revealed that, while work was going forward, the design was far from complete. The senior Lockheed representative reported that they had investigated the possibility of building a satellite vehicle shaped like a football, a cigar, or a sphere. They had finally decided, for the original drawings at least, on a football-shaped pod slightly elongated at each end to correct the center of gravity. There was discussion of the need for immediate contractual arrangements with the various suppliers. Bissell remarked that he was "faced with the problem at present of being broke" and would need estimates from all the suppliers as soon as possible in order to obtain the necessary financing to get the program under way. The suppliers agreed to furnish the required estimates by the following week.

The project quickly began taking formal shape following that meeting. Within a span of about three weeks, approval of the program and of its financing was obtained, and the design of the payload configuration evolved into a concept quite different from the spin-stabilized pod. It was at this point in late March and early April 1958 that major complications had arisen in the technical design of the Fairchild camera. Interest shifted to a competitive design submitted by the Itek Corporation, a spin-off of Boston University. Itek proposed a longer focal length camera scanning within an earth-center stabilized pod. The Itek design was based on the principle of the Boston University Hyac camera. Bissell recalls that he personally decided in favor of the Itek design, but only after much agonizing evaluation. The decision was a difficult one to make because it involved moving from a proven method of space vehicle stabilization to one that was technically more difficult to accomplish. It did, however, standardize on the 3-axis stabilization being pursued on the WS-117L AGENA development, and which has been a part of all subsequent photo reconnaissance systems.

Bissell's first project proposal, which was completed on 9 April 1958, requested approval for concurrent development of both the Fairchild and the Itek systems, with the Fairchild configuration becoming operational first and the Itek configuration being developed as a follow-on system. Within two days, however, Bissell had made the final decision to abandon the Fairchild spin-stabilized configuration entirely. He rewrote the project proposal, taking note of the earlier

configuration and giving his reasons for favoring the Itek approach (principally the better resolution attainable, the lower overall cost, and the greater potential for growth). The proposal was rewritten a second time, retaining the Itek configuration but raising the cost estimate from [redacted] to [redacted] Of the 25X1
25X1 total estimated cost, [redacted] represented "a rather arbitrary allowance" for 12 each THOR boosters and Lockheed second stage vehicles, and was to be financed by ARPA through the Air Force. The remaining [redacted] was for 25X1
25X1 [redacted] by CIA of the pods containing the reconnaissance equipment and the recoverable film cassettes.

The final project proposal was forwarded to Brigadier General Andrew J. Goodpaster, the President's Staff Secretary, on 16 April 1958 after having been reviewed by Mr. Roy Johnson and Admiral John Clark of ARPA; Mr. Richard Horner, Assistant Secretary of the Air Force for Research and Development; Brigadier General Osmond Ritland, Vice Commander, Air Force Ballistic Missile Division; and Dr. James Killian, Special Assistant to the President for Science and Technology. The proposal was approved, although not in writing. The only original record of the President's approval reportedly was in the form of a handwritten note on the back of an envelope by General C. P. Cabell, the Deputy Director of Central Intelligence.

Although it may have been the original intent that CORONA would be administered in a manner essentially the same as that of the U-2 program, it actually began and evolved quite differently. It was a joint CIA-ARPA-Air Force effort, much as the U-2 was a joint CIA-Air Force effort, but it lacked the central direction that characterized the U-2 program. The project proposal described the anticipated administrative arrangements, but it fell short of clarifying the delineation of authorities. It noted that CORONA was being carried out under the authority of ARPA and CIA with the support and participation of the Air Force. CIA's role was further explained in terms of participating in supervision of the technical development, especially as regards the actual reconnaissance equipment, handling all [redacted] 25X1
25X1 [redacted] The work statement prepared for Lockheed, the prime contractor, on 25 April 1958 noted merely that technical direction of the program was the joint responsibility of several agencies of the Government.

The imprecise statements of who was to do what in connection with CORONA allowed for a range of interpretation. The vague assignments of responsibilities caused no appreciable difficulties in the early years of CORONA when the joint concern was primarily one of producing as promised, but they later (1963) became a source of severe friction between CIA and the Air Force over responsibility for conducting the program.

Bissell, the recognized leader of the early CORONA program, gave this description of how the early program was managed:

> The program was started in a marvelously informal manner. Ritland and I worked out the division of labor between the two organizations as we went along. Decisions were made jointly. There were so few people involved and their relations were so close that decisions could be and were made quickly and cleanly. We did not have the problem of having to make

compromises or of endless delays awaiting agreement. After we got fully organized and the contracts had been let, we began a system of management through monthly suppliers' meetings—as we had done with the U-2. Ritland and I sat at the end of the table, and I acted as chairman. The group included two or three people from each of the suppliers. We heard reports of progress and ventilated problems—especially those involving interfaces among contractors. The program was handled in an extraordinarily cooperative manner between the Air Force and CIA. Almost all of the people involved on the Government side were more interested in getting the job done than in claiming credit or gaining control.

The schedule of the program, as it had been presented to the CORONA group at its meeting in San Mateo in late March 1958, called for a "count-down" beginning about the first of July 1958 and extending for a period of 19 weeks. It was anticipated that the equipment would be assembled, tested, and the first vehicle launched during that 19-week period, which meant that the fabrication of the individual components would have had to be completed by 1 July 1958. By the time Bissell submitted his project proposal some three weeks later, it had become apparent that the earlier tentative scheduling was unrealistic. Bissell noted in his project proposal that it was not yet possible to establish a firm schedule of delivery dates, but that it appeared probable that the first firing could be attempted no later than June 1959.

It is pertinent to note here that there was no expectation in 1958 that CORONA would still be operating over a decade later. The CORONA program got under way initially as an interim, short-term, high-risk development to meet the intelligence community's requirements for area search photographic reconnaissance pending successful development of other, more sophisticated systems planned for WS-117L. The original CORONA proposal anticipated the acquisition of only 12 vehicles, noting that at a later date it might be desirable to consider whether the program should be extended—with or without further technological improvement.

Having settled on the desired configuration and having received Presidential approval of the program and its financing, the CORONA management team moved forward rapidly with the contractual arrangements. The team of contractors for CORONA differed from the team on the WS-117L subsystem as a consequence of selecting Itek's earth-center stabilized approach. Itek was brought in as one of the two major subcontractors to Lockheed (General Electric being the other). However, to soften the financial blow to Fairchild, Itek was made responsible for the design and development of the camera subsystem with Fairchild producing the camera under subcontract to Itek. This contractor team continued throughout the CORONA program, although later in the program, the relationship was changed to that of associate contractors. The contractor relationships on the CORONA program were as friendly and cooperative as any that could have been set up, and this team dedication to the success of the program is one of the primary reasons for the success the program enjoyed. The final contractors were selected on 25 April 1958 and a work statement was issued to Lockheed on that date. The contractors began systems design on 28 April

and completed them and submitted them for first review on 14 May. The designs were frozen on 26 July.

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[redacted]

Thus, by mid-1958, the program was well down the road—on the contractors' side—toward meeting the goal of a first launch no later than mid-1959. The Government side, however, was running into difficulties. The first problem was money, the second was cover, and the two were inextricably intertwined. The [redacted] cost estimate for the 12-vehicle program had assumed that the cost of the THOR boosters would be absorbed by the Air Force by diverting them from the cancelled WS-117L subsystem. That assumption proved to be incorrect. An additional [redacted] had to be found to pay for the 12 THORs. Further, it had been decided that an additional four launch vehicles would be required for testing of launch, orbit, and recovery procedures and that an additional three would be required for biomedical launches in support of the CORONA cover story. ARPA could not see its way clear to making Defense Department funds available merely for testing or for cover support when there were other DoD space programs with pressing needs for money. Consequently, CORONA management had to go back to the President for approval of a revised estimate.

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By August 1958, it had also become apparent to the project's managers that the original, but as yet unannounced, cover story conceived for the future CORONA launchings (an experimental program within the first phase of WS-117L) was becoming increasingly untenable. WS-117L had by then become the subject of fairly widespread public speculation identifying it as a military reconnaissance program. It was feared that linking CORONA to WS-117L in any way would inevitably place the reconnaissance label on CORONA, and—given the hostility of the international political climate to overflight reconnaissance—there was the risk that the policy level of government might cancel the program if it should be so identified. Some other story would have to be contrived that would dissociate CORONA from WS-117L and at the same time account for multiple launchings of stabilized vehicles in low polar orbits and with payloads being recovered from orbit.

It was decided, therefore, to separate the WS-117L photo reconnaissance program into two distinct and ostensibly unrelated series: one identified as

DISCOVERER (CORONA – THOR boost) and the other as SENTRY (later known as SAMOS – ATLAS boost). A press release announcing the initiation of the DISCOVERER series was issued on 3 December 1958 identifying the initial launchings as tests of the vehicle itself and later launchings as explorations of environmental conditions in space. Biomedical specimens, including live animals, were to be carried into space and their recovery from orbit attempted.

The new CORONA cover concept, from which the press release stemmed, called for a total of five biomedical vehicles, and three of the five were committed to the schedule under launchings three, four, and seven. The first two were to carry mice and the third a primate. The two uncommitted vehicles were to be held in reserve in event of failure of the heavier primate vehicle. In further support of the cover plan, ARPA was to develop two radiometric payload packages designed specifically to study navigation of space vehicles and to obtain data useful in the development of an early warning system (the planned [redacted] infrared series). It might be noted here that only one of the three planned animal-carrying missions was actually attempted (as DISCOVERER III), and it was a failure. ARPA did develop the radiometric payload packages, and they were launched as DISCOVERERs XIX and XXI in late 1960 and early 1961.

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The photo reconnaissance mission of CORONA necessitated a near-polar orbit, by launching either to the north or to the south. There are few otherwise suitable areas in the continental United States where this can be done without danger that debris from an early in-flight failure could fall into populated areas. Cooke Air Force Base* near California's Point Arguello met the requirement for downrange safety, because the trajectory of a southward launch from there would be over the Santa Barbara channel and the Pacific Ocean beyond. Cooke was a natural choice, because it was the site of the first Air Force operational missile training base and also housed the 672nd Strategic Missile Squadron (THOR). Two additional factors favored this as the launch area: the manufacturing facilities and skilled personnel required were in the near vicinity, and a southward launch would permit recovery in the Hawaii area by initiating the ejection/recovery sequence as the satellite passed over the Alaskan tracking facility.

Unlike the U-2 flights, launchings of satellites from U.S. soil simply could not be concealed from the public. Even a booster as small as the THOR (small, that is, in comparison with present-day space boosters) launches with a thunderous roar that can be heard for miles; the space vehicle transmits telemetry that can be intercepted; and the vehicle can be detected in orbit by radar skintrack. The fact of a launch could not be concealed, but maintenance of the cover story for the DISCOVERER series required that the launchings of the uniquely configured photographic payloads be closed to observation by unwitting personnel. Vandenberg was excellent as a launch site from many standpoints, but it had one feature that posed a severe handicap to screening the actual launches from unwanted observation: the heavily traveled Southern Pacific railroad passes through it. The early launches from Vandenberg had to

*Cooke AFB was renamed Vandenberg AFB in October 1958.

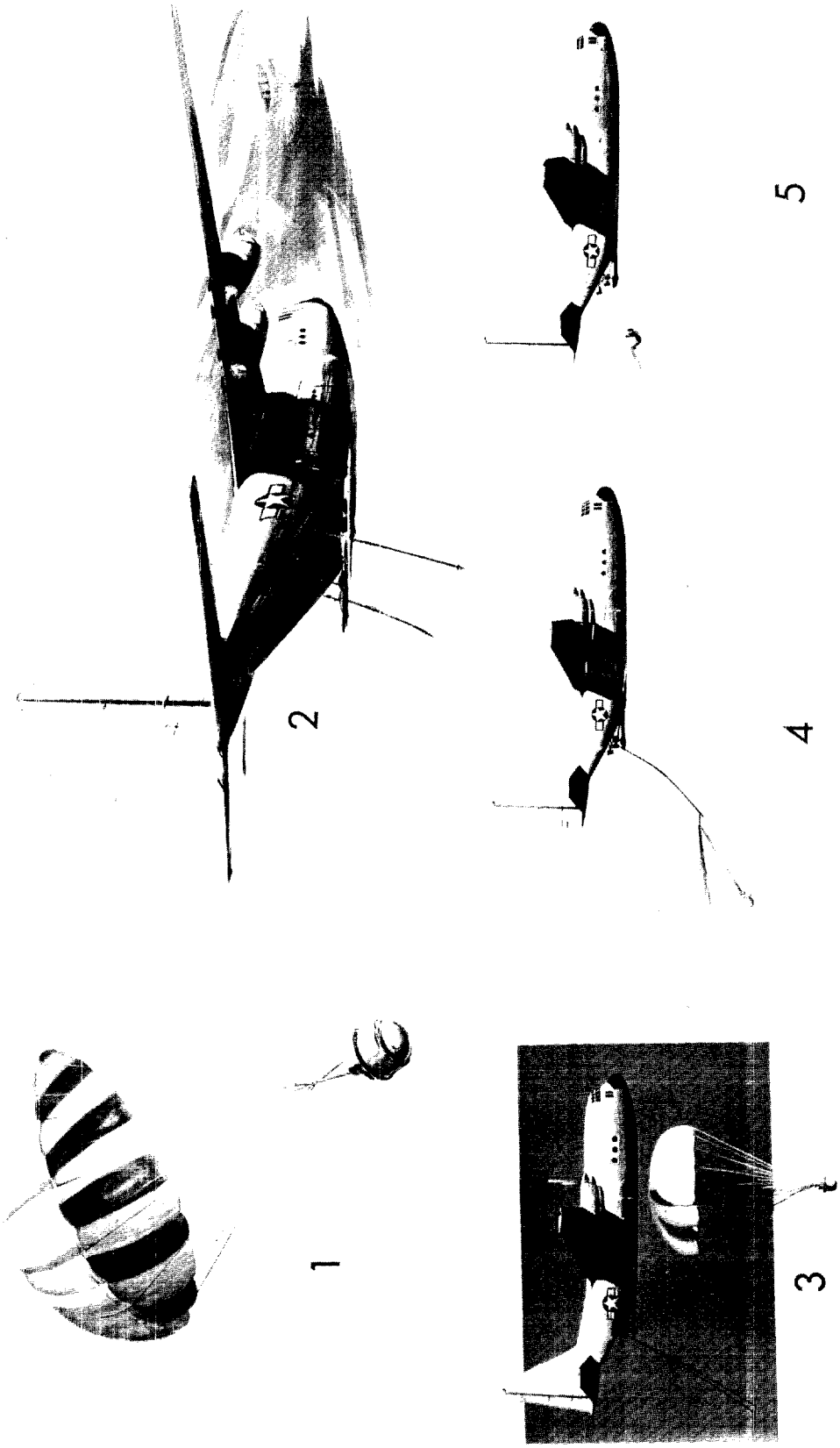
be timed for early afternoon,* and the Southern Pacific schedule broke this period into a series of launch windows, some of which were no more than a few minutes between trains. Throughout its existence, the CORONA program at Vandenberg was plagued by having to time the launches to occur during one of the intervals between passing trains.

The planned recovery sequence involved a series of maneuvers, each of which had to be executed to near-perfection or recovery would fail. Immediately after injection into orbit, the AGENA vehicle was yawed 180 degrees so that the recovery vehicle faced to the rear. This maneuver minimized the control gas which would be required for re-entry orientation at the end of the mission, and protected the heat shield from molecular heating, a subject of considerable concern at that time. (Later in the J-3 design when these concerns had diminished, the vehicle would be flown forward until re-entry.) When re-entry was to take place, the AGENA would then be pitched down through 60 degrees to position the satellite recovery vehicle (SRV) for retro-firing. Then the SRV would be separated from the AGENA and be spin-stabilized by firing the spin rockets to maintain it in the attitude given it by the AGENA. Next the retro-rocket would be fired, slowing down the SRV into a descent trajectory. Then the spin of the SRV would be slowed by firing the de-spin rockets. Next would come the separation of the retro-rocket thrust cone followed by the heat shield and the parachute cover. The drogue (or deceleration) chute would then deploy, and finally the main chute would open to lower the capsule gently into the recovery area. The primary recovery technique involved flying an airplane across the top of the descending parachute, catching the chute or its shrouds in a trapeze-like hook suspended beneath the airplane and then winching the recovery vehicle aboard. C-119 Aircraft were initially used with C-130 aircraft replacing them later in the program. The recovery vehicle was designed to float long enough, if the air catch failed, for a water recovery by helicopter launched from a surface ship.

While the vehicle was still in the construction stage, tests of the air recovery technique were conducted by the 6593rd Test Squadron—with disheartening results. Of 74 drops using personnel-type chutes, only 49 were recovered. Using one type of operational drop chute, only four were recovered out of 15 dropped, and an average of 1.5 aircraft passes were required for the hook-up. Eleven drops with another type of operational chute resulted in five recoveries and an average of two aircraft passes for the snatch. Part of the difficulty lay in weak chutes and rigging, and in crew inexperience. The most serious problem, however, was the fast drop rate of the chutes. Parachutes that were available to support the planned weight of the recovery vehicle had a sink rate of about 33 feet per second. What was required was a sink rate approaching 20 feet per second so that the aircraft would have time to make three or four passes if necessary for hook-up. Fortunately, by the time space hardware was ready for launching,

*The early THOR-AGENA combination limited film to enough for a 24-hour mission of 17 orbits, seven of which would cross denied territory. Requirements for daylight recovery and for daylight passage over denied areas with acceptable sun angles dictated the afternoon launch time.

AERIAL RECOVERY OF CAPSULE



a parachute had been developed with a sink rate slow enough to offer a reasonable chance of air recovery.

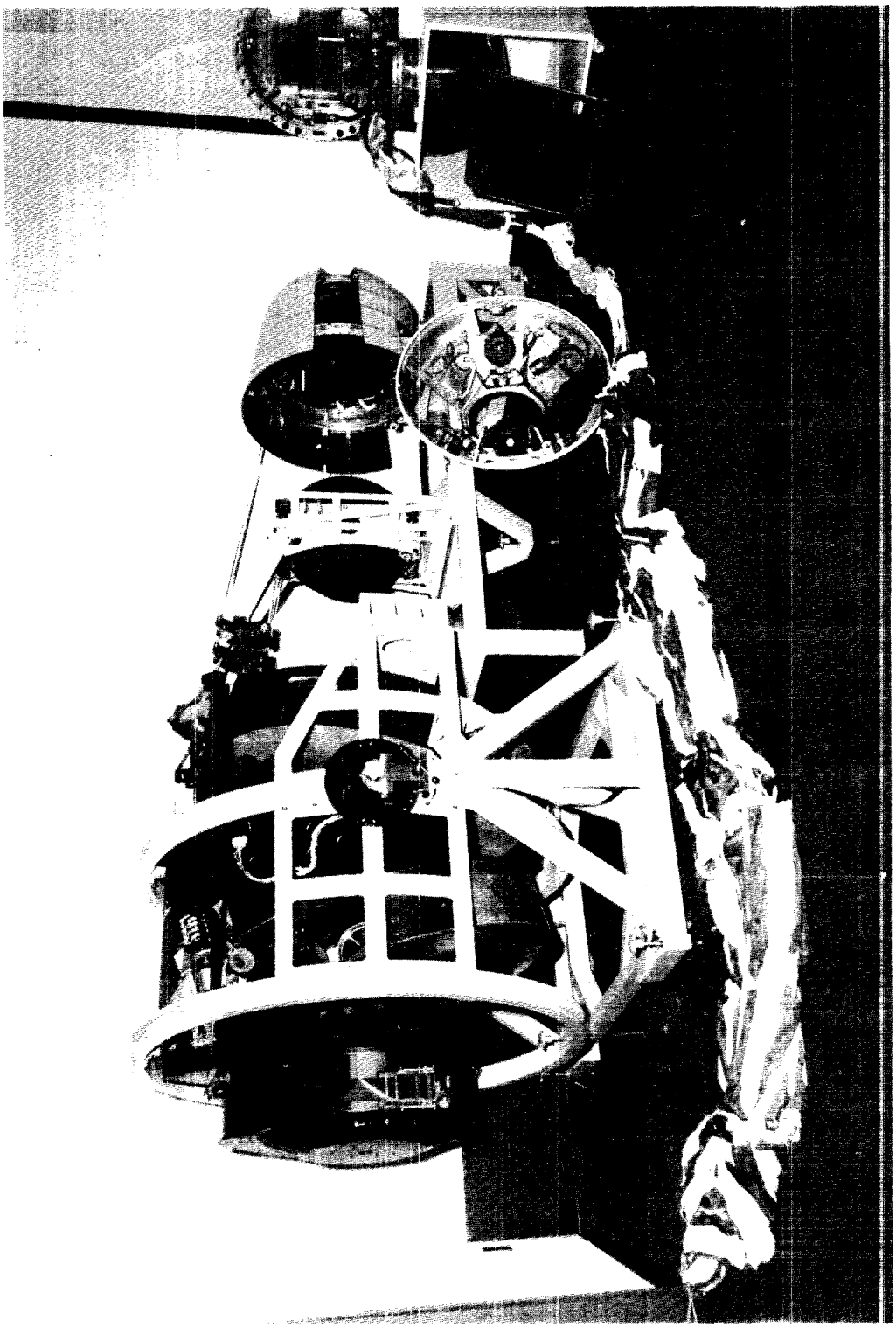
The launch facilities at Vandenberg AFB were complete, and the remote tracking and control facilities which had been developed for WS-117L were ready for the first flight test of a THOR-AGENA combination in January 1959. The count-down was started for a launch on the 21st; however, the attempt aborted at launch minus 60 minutes. When power was applied to test the AGENA hydraulic system, certain events took place that were supposed to occur in flight but not while the vehicle was still sitting on the launch pad. The explosive bolts connecting the AGENA to the THOR detonated, and the ullage rockets* fired. The AGENA settled into the fairing attaching it to the THOR and did not fall to the ground, but appreciable damage was done.

A program review conference was held in Palo Alto two days after the launch failure to examine the possible causes of the abort and to assess its impact on the planned CORONA launch schedule. Fortunately, the problem was quickly identified and easily corrected, and it was felt that the system was ready for test launches at the rate of about one per month.

At the review conference, General Electric surfaced a new problem having to do with the stability of the nose cone during re-entry. The cone was designed for a film load of 40 pounds, but the first missions would be able to carry only 20 pounds. GE reported that about three pounds of ballast would have to be carried in the forward end of the cone to restore stability. The program officers decided to add an instrument package as ballast, either for diagnostic purposes or for support of the biomedical cover story, thus converting what could have been dead weight into a net plus for the test program.

The test plan contemplated arriving at full operational capability at a relatively early date through sequential testing of the major components of the system—beginning with the THOR-AGENA combination alone, then adding the nose cone to test the ejection/re-entry/recovery sequence, and finally installing a camera for a full CORONA systems test. Just how much confidence the project planners had in the imminence of success cannot now be discovered; however, if the confidence factor was very high at the start, it must soon have begun to wane. Beginning in February 1959 and extending through June 1960 an even dozen launches were attempted, with eight of the vehicles carrying cameras, and all of them were failures; no film capsules were recovered from orbit. Of the eight camera-carrying vehicles, four failed to achieve orbit, three experienced camera or film failures, and the eighth was not recovered because of a malfunction of the re-entry body spin rockets. These summaries of the initial launch attempts illustrate the nature and dimensions of the problems for which solutions had to be found.

*Ullage rockets are small solid propellant rockets attached to the AGENA. These rockets are fired just prior to ignition of the AGENA engine after its separation from the THOR to insure that the liquid AGENA propellants are pushed against the bottom of the tanks so that proper flow into the pumps will occur.



DISCOVERER I

The on-pad failure of 21 January was not assigned a number in the DISCOVERER series. DISCOVERER I was launched on 28 February 1959 with a light engineering payload as a test of THOR-AGENA performance. No recovery was planned. For a time there was uncertainty as to what had happened to it because no radio signals were received. At the time, it was believed to have obtained orbit with speculation that the protective nose cone over the antennas was ejected just before the AGENA fired and that the AGENA then rammed into the nose cone, damaging the antennas. Today, most people believe the DISCOVERER I landed somewhere near the South Pole.

DISCOVERER II

The second vehicle was launched on 13 April 1959. Orbit was officially announced about two hours later, along with a statement that the capsule carried a lightweight biomedical payload (as indeed it did). The Air Force reported on 15 April that plans to recover the capsule near Hawaii had been abandoned and that the capsule might descend somewhere in the Arctic. The announcement slightly understated the known facts. The capsule had ejected on the 17th orbit as planned, but a timing malfunction (actually a human programming error) had caused the ejection sequence to be initiated too early. The capsule was down, probably somewhere in the near-vicinity of the Spitsbergen Islands north of Norway. In fact, there were later reports that the falling capsule had actually been seen by Spitsbergen residents. The Air Force announced on the 16th that the Norwegian government had authorized a search for the capsule which would begin the following day. Planes scoured the area, and helicopters joined the search on the 20th. Nothing was found, however, and the search was abandoned on the 23rd.

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DISCOVERER III

Much publicity attended the launching of DISCOVERER III: some of it planned and some uplanned (and unwanted). This was the first (and only) DISCOVERER flight to carry animals: four live black mice. Black mice were chosen in order to ascertain the possible hair-bleaching effects of cosmic rays. The mice were members of the C-57 strain, a particularly rugged breed. They had been "trained," along with 60 other mice, at the Air Force's Aeromedical Field Laboratory at Holloman AFB. They were seven to ten weeks old and

*The incident inspired a book by Alistair MacLean, *Ice Station Zebra,* and a 1968 movie of the same name, but the fictional version gave little cause for concern that some CORONA alumnus was serving as technical consultant. In the movie, a U.S. nuclear submarine is heading for the North Pole to rescue British meteorologists on a disintegrating ice floe. Special agents on board are after a missing capsule with coverage of all U.S. missile sites, snapped by a Soviet satellite equipped with a stolen U.S. camera. Enter Soviet paratroopers, second- and third-country spies, etc., etc., etc.

weighed slightly over an ounce apiece. A three-day food supply was provided, which consisted of a special formula containing peanuts, oatmeal, gelatin, orange juice, and water. Each mouse was placed in a small individual cage about twice its size, and each had a minuscule radio strapped to its back to monitor the effects of the space trip on heart action, respiration, and muscular activity.

The lift-off on 3 June 1959 was uneventful, but, instead of injecting approximately horizontally into orbit, the AGENA apparently fired downward, driving the vehicle into the Pacific Ocean and killing the mice. Looking back on the mission, the attempt to orbit the mice seems to have been jinxed from the very beginning.

Just before the first try at launch, telemetry indicated a lack of mouse activity. It was thought at first that the little fellows were merely asleep, so a technician was sent up in a cherry-picker to arouse them. He banged on the side of the vehicle and tried catcalls, but to no avail. When the capsule was opened, the mice were found to be dead. The cages had been sprayed with krylon to cover rough edges; the mice had found it tastier than their formula; and that was that.

"The Mouse That Poured"

The second try at launch several days later, with a back-up mouse "crew," was a near-abort when the capsule life cell humidity sensor suddenly indicated 100 percent relative humidity. The panic button was pushed, and troubleshooters were sent up to check. They found that when the vehicle was in a vertical position the humidity sensor was directly beneath the cages, and it did not distinguish between plain water and urine. The wetness dried out after a while, all was forgiven, and the vehicle was launched—unhappily into the permanent 100 percent moisture environment of the Pacific Ocean.

Also, the timing of the launch was unfortunate. The monkeys, Able and Baker, had survived a 300-mile flight in a JUPITER nose cone on 29 May in connection with another, unrelated test program. However, Able died during minor surgery on 3 June to remove an electrode that had been implanted under his skin. (This was the date of the DISCOVERER III launch.) The British Society Against Cruel Sports made a formal protest to the U.S. Ambassador, and the press raised quite a stink about the fatal mice flight—comparing it unfavorably with the Russians' successful launching of the dog, Laika, in SPUTNIK II back in November 1957, and demanding that orbit and recovery procedures be perfected before attempting further launches of mice or monkeys.

DISCOVERERS IV-VIII

DISCOVERER IV on 25 June 1959 was the first to carry a camera and thus the first true CORONA test, but the payload did not go into orbit. DISCOVERER V, again with a camera, attained orbit but the temperature inside the spacecraft was abnormally low and the camera failed on the first orbit. The recovery

capsule was ejected at the proper time, but never showed up; early in 1960 it was discovered in a high near-polar orbit with an apogee of 1,058 miles. Failure of the spin rocket had caused the retro-rocket to accelerate rather than de-boost the package. DISCOVERER VI went into orbit six days later, but the camera failed on the second revolution, and the retro-rocket failed on the recovery attempt.*

DISCOVERER VII on 7 November did not go into orbit. DISCOVERER VIII on 20 November went into an eccentric orbit with an apogee of 913 miles, and the camera failed again. The recovery vehicle was ejected successfully, but the parachute failed to open.

It had become plain by the end of November 1959 that something (or, to be more precise, many things) had to be done to correct the multiple failures that were plaguing the CORONA system. Eight THOR-AGENA combinations and five cameras had been expended with nothing to show for the effort except accumulated knowledge of the system's weaknesses. The project technicians knew what was going wrong, but not always why. Through DISCOVERER VIII, the system had experienced these major failures:

One misfired on the launch pad.
Three failed to achieve orbit.
Two went into highly eccentric orbits.
One capsule ejected prematurely.
Two cameras operated briefly and then failed.
One camera failed entirely.
One experienced a retro-rocket malfunction.
One had very low spacecraft temperature.

A panel of consultants reviewed the various failures and their probable causes and concluded that what was needed most was "qualification, requalification, and multiple testing of component parts" before assembling them and sending them aloft. This called for more money. Accordingly, Bissell submitted a project amendment to the DDCI on 22 January 1960 asking approval of nearly [redacted] additional to cover the costs of the testing program. He apologized to General Cabell for submitting a request for funds to pay for work that was already under way: "Although such a sequence is regrettable, there has been con-

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*One of these early launches tested a system for concealing the tell-tale payload doors from inquisitive eyes near the launch pad. The scheme was to cover them with paper, fastened over two lengths of piano wire with pingpong balls at the front end. The air flow at launch would use the pingpong balls and wire as "ripcords" to strip away the paper. The idea was tested on the side of a sports car simulating launch velocity as nearly as possible on the Bayshore Freeway late one evening. The test proved that the ripcords worked, and that Freeway patrolmen could overhaul a vehicle going only 90 m.p.h. Unfortunately, the ripcords malfunctioned on the next actual launch, and there was no consensus for another test round with the Freeway police.

siderable confusion in this program as to what the amount of the overruns would be and this has made it difficult to obtain approvals in an orderly fashion in advance."

As of the fall of 1959, major problems remained to be solved in achieving an acceptable orbit, in camera functioning, and in recovering the film capsule. These were the more serious of the specific failures that were occupying the attention of the technicians:

The AGENA vehicle was designed for use with both the THOR and the ATLAS boosters. The ascent technique used by the AGENA vehicle was essentially the same in both combinations, but there were significant differences in the method of employing the booster. In the CORONA program, in order to conserve weight, the THOR booster followed a programmed trajectory using only its autopilot. Also, the THOR thrust was not cut off by command at a predetermined velocity (as in the ATLAS); instead, its fuel burned to near-exhaustion. This relatively inaccurate boosting profile, coupled with the low altitude of CORONA orbits, required great precision in the orbital injection. At a typical injection altitude of 120 miles, an angular error of plus or minus 1.1 degrees or a velocity deficit of as little as 100 feet per second would result in failure to complete the first orbit. This had happened repeatedly. Lasting relief from this problem lay some distance in the future: a more powerful AGENA was being developed, and the weight of instrumentation for measuring in-flight performance on the early flights would be reduced on later operational missions. The short-term remedy lay in a drastic weight-reduction program. This was carried out in part (literally, it is said) by attacking surplus metal with tin snips and files.

The system was designed to operate without pressurization (again to conserve weight), and the acetate base film being used was tearing or breaking in the high vacuum existing in space and causing the camera to jam. A solution for this problem was found in substituting polyester for acetate base film. The importance to the reconnaissance programs of this achievement by Eastman Kodak in film technology cannot be overemphasized. It ranks on a level with the development of the film recovery capsule itself. The first orbital flight in which the camera was operated with polyester film was DISCOVERER XI (Mission 9008) in April 1960. Although recovery was not successful, one of the major space reconnaissance problems had been solved.

The equipment was built to work best at an even and predetermined temperature. To save weight, only passive thermal control was provided. The spacecraft's internal temperature had varied on the flights thus far, and it was much lower than desired on one flight. An interim solution for this problem was found in varying the thermal painting of the vehicle skin.

The spin and de-spin rockets used to stabilize the recovery vehicle during re-entry had a tendency to explode rather than merely to fire. Several had blown up in ground tests. A solution was found in substituting cold gas spin and de-spin rockets.

> One of the most intractable problems, which was to persist for many months, was that of placing the satellite recovery vehicle (SRV) into a descent trajectory that would terminate in the recovery zone. This required ejecting the SRV from the AGENA at precisely the right time, and decelerating it by retro-rocket firing to the correct velocity and at a suitable angle. There was very little margin for error in this phase: each one-second error in ejection timing could shift the recovery point five miles; a retro-velocity vector error of more than ten degrees would cause the capsule to miss the recovery zone completely.

One might ask why the CORONA program officers persisted in the face of such adversity. The answer lay in the overwhelming intelligence needs of the period. The initial planning of CORONA began at a time when we did not know how many BEAR and BISON aircraft the Soviets had, whether they were introducing a new and far more advanced long range bomber than the BISON, or whether they had largely skipped the build-up of a manned bomber force in favor of missiles. There had been major changes in intelligence estimates of Soviet nuclear capabilities and of the scope of the Soviet missile program on the basis of the results of the relatively small number of U-2 missions approved for the summer of 1957. However, by 1959, the great "missile gap" controversy was very much in the fore. The Soviets had tested ICBM's at ranges of 5,000 miles, proving they had a capability of building and operating them. What was not known was where they were deploying them operationally, and in what numbers. In the preparation of the National Intelligence Estimate on guided missiles in the fall of 1959, the various intelligence agencies held widely diverse views on Soviet missile strength. Nineteen Sixty ushered in an election year in which the missile gap had become a grave political issue, and the President was scheduled to meet with Soviet leaders that spring without—it appeared—the benefit of hard intelligence data. The U-2 had improved our knowledge of the Soviet Union, but it could not provide area coverage and the answers to the critical questions, and it was increasingly becoming less an intelligence asset than a political liability. It was judged to be only a matter of time until one was shot down—with the program coming to an end as an almost certain consequence.

DISCOVERERS IX-XII

A standdown was in effect in CORONA from 20 November 1959 until 4 February 1960 to allow time for intensive R&D efforts to identify and eliminate the causes of failure. On 4 February, DISCOVERER IX was launched and failed to achieve orbit.

The first recovery of film from a CORONA vehicle occurred with the launching of DISCOVERER X on 19 February 1960, but in a manner such that no one boasted of it. The THOR booster rocket began to fishtail not long after it left the launch pad and was destroyed by the range safety officer at 52 seconds after lift-

off. The payload came down about a mile from Pad 5, was located by helicopter, and the recovery was made by a crew that rode to the scene by Jeep.*

DISCOVERERs VII through X carried only a quarter of a load of film (10 pounds) to permit the carrying of additional instrumentation for testing vehicle performance. DISCOVERER XI was launched on 15 April 1960 carrying a camera and 16 pounds of film. A reasonably good orbit was achieved (380 miles at apogee and 109.5 miles at perigee), and the camera operated satisfactorily.** All of the film was exposed and transferred into the recovery capsule. Unfortunately, the problem of the exploding spin rockets, which had been observed in ground tests, occurred during the recovery sequence, and the payload was lost.

Another standdown—a major one—was imposed following the failure of DISCOVERER XI. As of mid-April 1960, there had been 11 launches and one abort on pad. Seven of the launches achieved orbit, but no capsules had been recovered. DISCOVERER XII was planned as a diagnostic flight—without camera payload—heavily instrumented to determine precisely why recovery of capsules had failed previously. The vehicle was launched on 29 June 1960, but the AGENA failed to go into orbit.

DISCOVERER XIII—Partial Success

The next flight, on August 1960, was launched as a repeat of the no-orbit DISCOVERER XII diagnostic flight, without camera and film. The vehicle was launched and successfully inserted into orbit. The recovery package was ejected on the 17th orbit, and retro-firing and descent were normal—except that the capsule came down well away from the planned impact point. The nominal impact area was approximately 250 miles south of Honolulu where C-119 and C-130 aircraft circled awaiting the capsule's descent. The splash-down occurred about 330 miles northwest of Hawaii. The airplanes were backed up by surface ships deployed in a recovery zone with a north-south axis of some 250 miles and an east-west axis extending about 550 miles to either side of the expected impact point. Although beyond the range of the airborne recovery aircraft, the DISCOVERER XIII capsule descended near enough to the staked-out zone to permit an attempt at water recovery. A ship reached the scene before the capsule sank

*This was one of the few launch failures for the remarkable Douglas team which prepared the THOR boosters at Vandenberg Air Force Base. The early CORONA launches provided many exciting moments for the Douglas crew, however. Several of the crew were holdovers from the V-2 "broomlighters," who on V-2 launch days would actually ignite reluctant rocket engines with kerosene-soaked brooms. At Vandenberg AFB they did not have to resort to this tactic, but they were required on numerous occasions to return to the launch pad as late as T minus 15 seconds to unfreeze valves with the touch of a sledgehammer. Other members of the blockhouse crew would marvel as the "Douglas Daredevils" would race their vehicles in reverse the entire way from the launch pad to the blockhouse, arriving just as ignition would begin.

**This was the first mission on which the camera operated successfully throughout the mission, primarily because of the change from acetate base to polyester base film.

and fished it out of the ocean. Much of the credit for the success was attributed to the inauguration (on the unsuccessful DISCOVERER XII launch) of the cold gas spin and de-spin system.

For the first time ever, man had orbited an object in space and recovered it. This American space "first" beat the Russians by just nine days. The Soviets had tried to recover SPUTNIK IV the previous May but failed when the recovery capsule ejected into a higher orbit. They did succeed in de-orbiting and recovering SPUTNIK V carrying the dogs, Belka and Strelka, on 20 August 1960.

Arrangements were made for extensive publicity concerning this success in recovering an object from orbit—in large measure to support the cover story of DISCOVERER/CORONA as being an experimental space series. News photos were released of the lift-off from Vandenberg, of the capsule floating in the ocean, and of the recovery ship *Haiti Victory*. President Eisenhower displayed the capsule and the flag it had carried to the press, and it was later placed on exhibit in the Smithsonian Institution for public viewing.

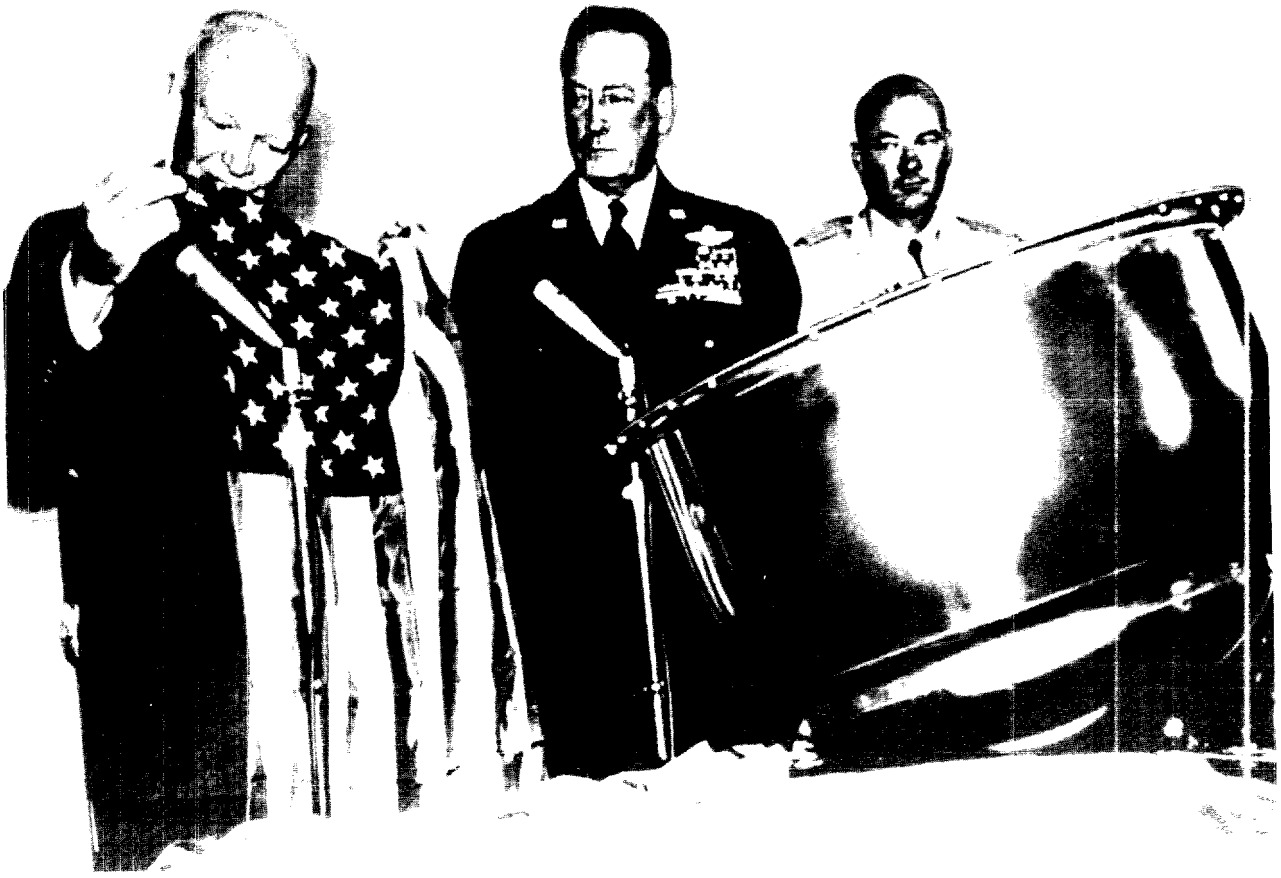
In anticipation of the first recovery being a reconnaissance mission, a plan had been developed under which the capsule would be switched in transit through Sunnyvale. Since DISCOVERER XIII was a diagnostic flight, the project office was spared the necessity of executing a clandestine switch of capsules prior to shipment to Washington, and the President and Smithsonian received the actual hardware from the first recovery.

We have all watched television coverage of the U.S. man-in-space programs with the recovery of astronauts and capsules after splash-down in the ocean. A helicopter flies from the recovery ship to the floating capsule and drops swimmers to attach a line to the capsule. After the astronauts are removed, the helicopter hoists the capsule from the water and carries it to the recovery ship. What most of us don't realize is that the recovery technique was developed for and perfected by the CORONA program as a back-up in event of failure of the air catch.

DISCOVERER XIV—Full Success

Success! ! ! DISCOVERER XIV was launched on 18 August 1960, one week after the successful water recovery of the DISCOVERER XIII capsule. The vehicle carried a camera and a 20-pound load of film. The camera operated satisfactorily, and the full load of film was exposed and transferred to the recovery capsule. The AGENA did not initially position itself in orbit so as to permit the recovery sequence to occur. It was on the verge of tumbling during the first few orbits, and an excessive quantity of gas had to be used in correcting the situation. Fortunately, vehicle attitude became stabilized about midway through the scheduled flight period, thus relieving the earlier fear that recovery would be impossible. The satellite recovery vehicle was ejected on the 17th pass, and the film capsule was recovered by air snatch.

Captain Harold E. Mitchell of the 6593rd Test Squadron, piloting a C-119 (flying boxcar) called Pelican 9, successfully hooked the descending capsule on



First Recovery from Space

Dwight D. Eisenhower

his third pass.* Upon arrival at Hickham Air Force Base, Hawaii, with his prize, Captain Mitchell was decorated with the Distinguished Flying Cross, and members of his crew were awarded the Air Medal for their accomplishments.

25X1 The film was flown to the [redacted] for development and was then delivered to PIC (now known as NPIC) for readout and reporting. The resolution was substantially lower than that obtainable from the U-2, but the photography had intelligence value, and it covered areas of the USSR which the U-2 had never reached. This one satellite mission, in fact, yielded photo coverage of a greater area than the total produced by all of the U-2 missions over the Soviet Union. The only major deficiencies in the photography were plus and minus density bars running diagonally across the format. Some were due to minor light leaks, and others were the result of electrostatic discharge known as corona. These marks showed that the program security officer had had great insight when he named the program. There are two types of corona markings, a glow which caused the most difficulty, and a dendritic discharge which is more spectacular in appearance. 25X1

A press release announced the success of the mission but naturally made no mention of the *real* success: the delivery of photographic intelligence. The announcement noted that the satellite had been placed into an orbit with a 77.6 degree of inclination, an apogee of 502 miles, a perigee of 116 miles, and an orbital period of 94.5 minutes. A retro-rocket had slowed the capsule to re-entry velocity, and a parachute had been released at 60,000 feet. The capsule, which weighed 84 pounds at recovery, was caught at 8,500 feet by a C-119 airplane on its third pass over the falling parachute.

Progress and Problems

The program officers did not take the success of DISCOVERER XIV to mean that their problems with the system were at an end, but many of the earlier difficulties had been surmounted. The orbital injection technique had been improved to a level at which vehicles were repeatedly put into orbit with injection angle errors of less than four-tenths of a degree. The timing of the initiation of the recovery sequence had been so refined that ejection of the DISCOVERER XI SRV occurred within five seconds of the planned time. Parachute deceleration and air catch of the capsule had been accomplished repeatedly with test capsules dropped from high-altitude balloons. The last two cameras placed in orbit had operated well.

There were other critical problems, however, that remained to be solved. Foremost among them at the time was that of consistently achieving the correct retro-velocity and angle of re-entry of the recovery vehicle. The DISCOVERER

*Mitchell had been patrolling the primary recovery zone for DISCOVERER XIII, which was fished from the water by a recovery ship after Mitchell's plane missed it. The Air Force, pride stung, assigned Mitchell to the boondocks some 500 miles downrange for DISCOVERER XIV. The capsule overshot the prime recovery area, where three aircraft were chasing the wrong radar blip. When Mitchell first tried to report his catch, he was told to keep off the air in order not to interfere with the recovery operation.

First Aerial Recovery

With best regards —

XIV capsule was the only one thus far that had descended in the designated impact zone. This was a problem that was to receive major attention during the next few weeks.

Four more cameras were launched within the next four months, with one success and three failures. DISCOVERER XV was sent aloft on 13 September. The vehicle was successfully inserted into orbit, and the camera functioned properly. However, the recovery vehicle re-entered at the wrong pitch attitude, causing the capsule to come down outside the recovery zone and demonstrating that the technicians' concern over the retro-firing sequence was well founded. The capsule was located, but it sank before a recovery ship could reach it. DISCOVERER XVI was launched on 26 October, but the AGENA failed to go into orbit because of a malfunction of a timing device.

The first ten camera-equipped vehicles carried what was known as the C camera: a single, vertical-looking, reciprocating, panoramic camera that exposed the film by scanning at a right angle to the line of flight. DISCOVERER XVI carried the first of a new series of cameras known as the C Prime (C′). The C′ differed only slightly from the original C configuration and was essentially little more than a follow-on procurement of the C camera.

The DISCOVERER XVII mission was launched on 12 November and went the full route through successful air catch—except for one mishap: the film broke after 1.7 feet of the acetate base leader had fed through the camera. There is an inconsistency in the records on this and the succeeding mission. The press release concerning this mission announced that the AGENA B, a more powerful second-stage engine, was used for the first time; the project files record the first use of the B vehicle on the following mission. In either event, it was the first of the two-day missions. The capsule was recovered on the 31st orbit.

DISCOVERER XVIII was launched on 10 December 1960 carrying 39 pounds of film. Orbit was achieved, and the camera worked well, exposing the entire film load. The recovery vehicle was ejected on revolution number 48 after three days in orbit, and the capsule was retrieved by air snatch. This was the first successful mission employing the C′ camera and the AGENA B second stage. There was fogging on the first, second, and last frame of each photo pass due to mirror light leaks, but image quality was otherwise as good as the best from DISCOVERER XIV.

CORONA in 1961

Of the next ten launches, extending through 3 August 1961, only four were CORONA missions. DISCOVERERs XIX and XXI carried radiometric payloads in support of the CORONA cover story, and they were not intended to be recovered. DISCOVERER XXI included an experiment that was to be of major significance in the later development of CORONA and other space programs: the AGENA engine was successfully restarted in space.

There was another "first" during these 1961 launches. When the film was removed from one of the capsules, the quality assurance inspector found three objects that should not have been there: two quarters and a buffalo nickel. Early

capsules had contained a flag, so that there would be one to present to President Eisenhower after the first successful recovery. This had apparently inspired program personnel at Vandenberg to make their own payload additions during flight preparation. The Washington program office sent a sharply worded message to the West Coast project office charging it with responsibility for ensuring that the practice of souvenir-launching be stopped. (Years later NASA would find itself in the same position after the Apollo moon flights.)

DISCOVERER XX was the first of a dozen launches extending over a period of three years carrying mapping cameras, a program sponsored by the U.S. Army, which the President had approved for inclusion within the CORONA project. The purpose of the mapping program, which was known as ARGON, was to obtain precise geodetic fixes and an extension of existing datum planes within the Soviet Union. DISCOVERER XX was a bust on a number of counts: the camera failed; there were no shutter firings; and the orbital programmer malfunctioned. This last-named failure led to an important change in control procedures for CORONA. On this and all prior flights the recovery sequence was initiated automatically by an ejection command cut into the program tape. The program timer failed temporarily on orbit 31 of this mission, causing the entire sequence to be about one-half cycle out of phase. The automatic initiation of the recovery sequence was eliminated from the program tape on subsequent missions. Thereafter, the positive issuance of an injection command was required.

Of the four CORONA missions attempted between December 1960 and August 1961, two did not go into orbit as a consequence of AGENA failures, and two were qualified successes. DISCOVERER XXV was launched on 16 June and exposed its full load of film. The air catch failed, but the back-up water recovery was successful. The camera failed on revolution 22 of DISCOVERER XXVI, which was launched on 7 July, but about three-quarters of the film was exposed and was recovered by air catch.

Going into August 1961, a total of 17 camera-carrying CORONA missions had been attempted, and usable photography had been recovered from only four of them. These four successful missions, however, had yielded plottable coverage of some 13 million square miles, or nearly half of the total area of interest.

Camera Improvements

The first substantial upgrading of the CORONA camera system came with the introduction in August 1961 of the C Triple Prime (C′′′) camera. The original C camera was a scanning panoramic camera in which the camera cycling rate and the velocity-over-height ratio were constant and were selected before launching. Image motion compensation was fixed mechanically to the velocity-over-height ratio. A brief explanation of these terms may be helpful in understanding the nature of the problems with which the camera designers had to cope.

> A means must be provided for matching the number of film exposures in a given period of time (camera cycling rate) with the varying ratio between vehicle altitude and velocity on orbit (velocity-over-height) so that

the ground area is photographed in a series of swaths with neither gaps nor excessive overlapping in the coverage.

If the subject moves just as a snapshot is taken with a hand-held camera, and if the camera shutter speed is not fast enough to "stop" the motion, the photographic image will be smeared. To a camera peering down from an orbiting CORONA space vehicle, the earth's surface appears to be passing beneath the camera at a speed of roughly five miles per second. A camera photographing the earth's surface from a satellite moving at that speed would yield smeared photography if some means were not provided for stopping the relative motion. The technique used in accomplishing this is known as image motion compensation.

The C Triple Prime was the first camera built totally by the Itek Corporation. The C′′′ was also a reciprocating camera with a rotating lens cell, which exposed the film during a segment of its rotation. The new camera had a larger aperture lens, an improved film transport mechanism, and a greater flexibility in command of camera and vehicle operations—especially as regards control of the velocity-over-height factor. The larger aperture lens permitted use of slower film emulsions, which, combined with the improved resolving power of the lens itself, offered the prospect of resolution approximately twice as good as the C and C′ cameras.

The first C′′′ camera system with a 39-pound film load was launched on 30 August 1961. The mission was a success, with the full film load being transferred and with ejection and recovery occurring on the 32nd orbit. All frames of the photography however, were out of focus. The cause was identified and was corrected by redesigning the scan head. Seven more missions were launched during the last four months of 1961, three with the C′ camera and four with the C′′′. Six of them attained orbit, and the cameras operated satisfactorily on all six. Film was recovered from four of the missions. The last of the four, which carried a C′′′ camera system, was rated the best mission to date. It also had a cover assignment to carry out: the injection of a secondary satellite, dubbed OSCAR (orbital satellite carrying amateur radio), into a separate orbit. OSCAR was a small radio satellite broadcasting a signal on 145 megacycles for pick-up by amateurs as an aid in the study of radio propagation phenomena.

Slowly but surely the bugs were being worked out, but it seemed that just as one was laid to rest another arose to take its place. Perhaps what was actually happening was that various sets of problems existed simultaneously, but the importance of some of them was masked by others. The elimination of a particular problem made it possible to recognize the significance of another. The recent successes had resulted largely from correcting weaknesses in the payload portion of the system. At the same time, difficulties in the AGENA vehicle began to surface. Of the last seven missions in 1961, four experienced on-orbit difficulties with the AGENA power supply or control gas system.

Power system components for general use in satellite systems were designed, developed, and tested in the CORONA program. Foremost among those components were the static electronic inverters used to convert direct current

battery energy into the various alternating current voltages required by the other subsystems. Static inverters, which were first flown aboard CORONA vehicles, were considered essential, because they had half the weight and double the efficiency of their rotary counterparts. Unfortunately, they are rather temperamental gadgets. The history of inverter development had been marked by high failure rates in system checkouts on the ground. Despite the lessons that had been learned and the improvements in circuit design that resulted from them, the recent on-orbit power failures demonstrated a need for further research and development.

The Last DISCOVERER

The AGENA failed on DISCOVERER XXXVII, launched on 13 January 1962, and the payload did not go into orbit. It was the last mission to carry the C′′′ camera system, and with it the DISCOVERER series came to an end. After 37 launches or launch attempts, the cover story for DISCOVERER had simply worn out. With the improved record of success and the near-certainty of an even better record in the future, it seemed likely that there would be as many as a dozen and a half to two dozen launches per year for perhaps years to come.

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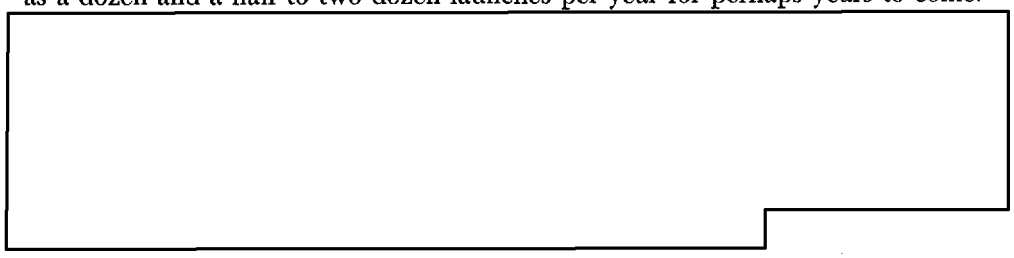


CORONA Goes Stereo

The 1961 R&D effort was not confined to improving the performance of the existing system. A major development program was concurrently under way on a much better camera subsystem. A contract was awarded on 9 August 1961, retroactively effective to 20 March, for a new camera configuration to be known as MURAL. The MURAL camera system consisted essentially of two C′′′ cameras mounted with one pointing slightly forward and the other slightly backward. Two 40-pound rolls of film were carried in a double-spool film supply cassette. The two film webs were fed separately to the two cameras where they were panoramically exposed during segments of the lens cells' rotations and then were fed to a double-spool take-up cassette in the satellite recovery vehicle. The system was designed for a mission duration of up to four days.

The vertical-looking C, C′, and C′′′ cameras had photographed the target area by sweeping across it in successive overlapping swaths. The MURAL concept involved photographing each swath area twice. The forward-looking camera first photographed the swath at an angle 15 degrees from the vertical. About a half-dozen frames later, the backward-looking camera photographed the same swatch at an angle also 15 degrees from the vertical. When the two resulting photographs of the same area or object were properly aligned in a stereo-micro-

scope, the photography would appear to be three-dimensional. Simultaneous operation of both instruments was required for stereo photography. If either camera failed, photography could still be obtained from the other, but it could be viewed in only two dimensions.

The first MURAL camera system was launched as program flight number 38 on 27 February 1962. On the first M flight, an anomaly occurred during re-entry. The RV heat shield failed to separate and was recovered by the aircraft along with the capsule. This anomaly provided valuable diagnostic data on the re-entry effects, which served the program well in later years, when program stretchouts caused shelf life of the heat shields to be a major concern. The twenty-sixth and last in the MURAL series was launched on 21 December 1963. Twenty of the SRV's were recovered, 19 of them by air snatch. The one water recovery was of a capsule that splashed down a thousand miles from the nominal impact point. An interesting aspect of this recovery was that the capsule turned upside down in the water, causing loss of the beacon signals. It was located during the search by an alert observer who spotted the sun shining on the gold capsule. Of the six vehicles that failed, two malfunctioned in the launch sequence, one SRV failed to eject properly, and three capsules came down in the ocean and sank before they could be recovered. Twenty successes out of 26 tries appeared to be a remarkable record when viewed against the difficulties experienced only two years earlier.

The three capsules that sank came down in or near the recovery zone, indicating that the problems previously encountered in the reentry sequence had been solved. They were not supposed to sink so quickly, however. (One of them floated for less than three minutes.) To minimize the chance that a capsule might be retrieved by persons other than the American recovery crew, the capsules were designed to float for a period ranging originally from one to three days and then to sink. The duration of the flotation period was controlled by a capsule sink valve containing compressed salt, which would dissolve in sea water at a rate that could be predicted within rather broad limits. When the salt plug had dissolved, water entered the capsule, and it sank—ingenious but simple.

More Problems, More Answers

Other significant improvements in the CORONA program were inaugurated during the lifetime of the MURAL system. One of them was an aid to photointerpretation. In order to read out the photography, the photointerpreter must be able to determine for each frame the portion of the earth's surface that is imaged, the scale of the photography, and its geometry. In simplest terms, he must know where the vehicle was and how it was oriented in space at the precise time the picture was taken. Until 1962, the ground area covered by a particular frame of photography was identified by combining data provided on the orbital path of the vehicle with the time of camera firing. The orientation or attitude of the vehicle on orbit was determined from horizon photographs recorded at each end of every other frame from a pair of horizon cameras that were included in the CORONA camera system.

Beginning with the first of the MURAL flights, an index camera was incorporated into the photographic system, and a stellar camera was added a few missions later. The short focal length index camera took a small scale photograph of the area being covered on a much larger scale by successive sweeps of the pan cameras. The small scale photograph, used in conjunction with orbital data, simplified the problem of matching the pan photographs with the terrain. Photographs taken of stars by the stellar camera, in combination with those taken of the horizons by the horizon cameras, provided a more precise means of determining vehicle attitude on orbit.

The photography from program flight number 47, a MURAL mission launched on 27 July 1962, was marred by heavy corona and radiation fogging. The corona problem was a persistent one—disappearing for a time only to reappear later—and had become even more severe with the advent of the complicated film transport mechanisms of the MURAL camera. Corona marking was caused by sparking of static electricity from moving parts of the system, especially from the film rollers. The problem was eventually solved by modifications of the parts themselves and by rigid qualification testing of them.

25X1

The boosting capacity of the first-stage THOR was substantially increased in early 1963 by strapping to the THOR a cluster of small solid-propellant rockets, which were jettisoned after firing. This Thrust Augmented THOR, or TAT as it came to be known, was first used for the launching of the heavier LANYARD camera system. LANYARD was developed within the CORONA program as a film recovery modification of one of the cameras designed for the SAMOS system and, with its longer focal length, was expected to yield better resolution than the CORONA cameras. It had a single lens cell capable of stereoscopic coverage by swinging a mirror through a 30-degree angle. Three flights were attempted, only one of which was partially successful. The camera had a serious lens focus problem, which was later traced to thermal factors and corrected. The LANYARD program was initiated as an interim system pending the completion of a high-resolution spotting system then under development. It was cancelled upon the success of the spotting system. The TAT booster itself was a significant success, permitting the later launching of heavier, more versatile CORONA systems.

The Two-Bucket System

Program flight number 69, launched on 24 August 1963, introduced the first two-bucket configuration—the next major upgrading of the CORONA system.

(The film recovery capsule is commonly referred to as a bucket, although it more nearly resembles a round-bottomed kettle.) The new modification, which was known as the J-1 system, retained the MURAL stereoscopic camera concept but added a second film capsule and recovery vehicle. With two SRV's in the system, film capacity was increased to 160 pounds (versus the 20-pound capacity of the first few CORONA missions). The two-bucket system was designed to be deactivated or stored in orbit in a passive (zombie) mode for up to 21 days. This permitted the recovery of the first bucket after half of the film supply was exposed. The second bucket could begin filling immediately thereafter, or its start could be delayed for a few days. A major redesign of the command and control mechanisms was required to accommodate the more complicated mission profile of the two-bucket system.

As with each of the major modifications of CORONA, the J-1 program had a few early bugs. On the first mission, the shutter on the master horizon camera remained open about 1,000 times seriously fogging the adjacent panoramic photography, and the AGENA current inverter failed in mid-flight, making it impossible to recover the second bucket. Also, the J-1 system initially experienced a rather severe heat problem, which was solved by reducing the thermal sensitivity of the camera and by better control of vehicle skin temperature through shielding and varying the paint pattern.

Back in 1960 and 1961, the successful recovery of a CORONA film bucket was an "event." A mere two years later, with the advent of the J-1 system, success had become routine and a failure was an "event." By the end of 1966, 37 J-1 systems had been launched; 35 of them were put into orbit; and 64 buckets of film were recovered. There were *no* failures at recovery in the three years following 1966: 28 buckets were launched, and 28 buckets were recovered. Also, mission duration was greatly expanded during the lifetime of the J-1 system. A mission in June 1964 yielded four full days over target for each of the two buckets. Five full days of operation with each bucket was attained in January 1965. In April 1966, the first bucket was recovered after seven days on orbit. A 13-day mission life was achieved in August 1966, and this was increased to 15 days in June 1967.

The increased mission life and excellent record of recovery resulted from a number of successive improvements that were incorporated into the J-1 time period. Among them was a subsystem known as LIFEBOAT, a completely redundant and self-contained apparatus built into the AGENA that could be activated for recovering the SRV in event of an AGENA power failure (which still happened occasionally). Another improvement was the introduction of the new and more powerful THORAD booster. A third was the addition of a rocket orbit-adjust system. The CORONA vehicles were necessarily flown over the target areas with quite a low perigee in order to increase the scale of the photography, and this led to a relatively rapid decay of the orbit. The orbit-adjust system compensated for the decay. It consisted of a cluster of small rockets, known as drag make-up units, which were fired individually and at selected

intervals. Each firing accelerated the vehicle slightly, boosting it back into approximately its original orbit.

A Maverick

The CORONA camera system was to undergo one more major upgrading but we cannot leave the J-1 program without giving an account of one mission failure of truly magnificent proportions. Program flight number 78 (CORONA Mission Number 1005), a two-bucket J-1 system, was launched on 27 April 1964. Launch and insertion into orbit were uneventful. The master panoramic camera operated satisfactorily through the first bucket, but the slave panoramic camera failed after 350 cycles when the film broke. Then the AGENA power supply failed. Vandenberg transmitted a normal recovery enable command on southbound revolution number 47 on 30 April. The vehicle verified receipt of the command, but nothing happened. The recovery command was repeated from various control stations—in both the normal and back-up LIFEBOAT recovery modes—on 26 subsequent passes extending through 20 May. The space vehicle repeatedly verified that it had received the commands, but the ejection sequence did not occur. After 19 May, the vehicle no longer acknowledged receipt, and from 20 May on it was assumed that the space hardware of Mission 1005 was doomed to total incineration as the orbit decayed.

But Mission 1005, it later developed, had staged its own partial re-entry, stubborn to the end. At six minutes past midnight on 26 May, coinciding with northbound revolution No. 452 of Mission 1005, observers in Maracaibo, Venezuela saw five burning objects in the sky.

On 7 July, two farm workers found a battered golden object on a farm in lonely mountain terrain near La Fria in Tachira State, southwestern Venezuela, a couple of miles from the Colombian border. They reported it to their employer, Facundo Albarracin, who had them move it some 100 yards onto his own farm and then spread the news of his find in hopes of selling it. Albarracin got no offers from the limited market in Tachira, however—not even from the smugglers with access to Colombia—so he hacked and pried loose the radio transmitter and various pieces of the take-up assembly to use as household utensils or toys for the children.

Ultimately word of the find reached San Cristobal, the nearest town of any size. Among the curious who visited La Fria was a commercial photographer, Leonardo Davila, who telephoned the U.S. Embassy in Caracas on 1 August that he had photographed a space object. It was the first bucket from Mission 1005, with one full spool of well-charred film clearly visible.

A team of CORONA officers, ostensibly representing USAF, flew to Caracas to recover the remains. The capsule was lugged out by peasants to a point where the Venezuelan Defense Ministry could pick it up for flight to Caracas. There the CORONA officers bought the crumpled bucket from the Venezuelan government, and quietly dismissed the event as an unimportant NASA space experiment gone awry.

The story rated only a dozen lines in the *New York Times* of 5 August, but the local Venezuelan press had a field day. *Diario Catolico,* of San Cristobal, along with a lengthy report, published three pictures of the capsule showing the charred roll of film on the take-up spool. The *Daily Journal* handled the story in lighter vein with this parody of Longfellow:

I shot an arrow into the air.
It fell to earth I know not where.
Cape Kennedy signalled: "Where is it at you are?"
Responded the rocket: "La Fria, Tachira."

The CORONA technicians who examined the capsule after its arrival in the States concluded that the re-entry of the SRV was a result of normal orbit degeneration, with separation from the instrument fairing caused by re-entry forces. The thrust cone was sheared during separation but was retained by its harness long enough to act as a drogue chute, thus preventing the capsule from burning up during re-entry and stabilizing it for a hard, nose-down landing.

The Final Touches

The final major modification of the CORONA system got under way in the spring of 1965, when about a dozen and a half of the two-bucket J-1 systems had been flown. The J-1 was performing superbly, but it had little potential for within-system growth. The new CORONA improvement program was begun with a series of meetings among representatives of Lockheed, General Electric, Itek, and the various CORONA program offices to examine ways of bettering the performance of the panoramic and stellar/index cameras, and of providing a more versatile command system. These were the resulting design goals established for a new panoramic camera:

Improved photographic performance by removal of camera system oscillating members and reduction of vibration from other moving components.

Improvement of the velocity-over-height match to reduce image smear.

Improved photographic scale by accommodation of proper camera cycling rates at altitudes down to 80 n.m. (the minimum J-1 operating altitude was 100 n.m.).

Elimination of camera failures caused by film pulling out of the guide rails (an occasional problem with the J-1 system).

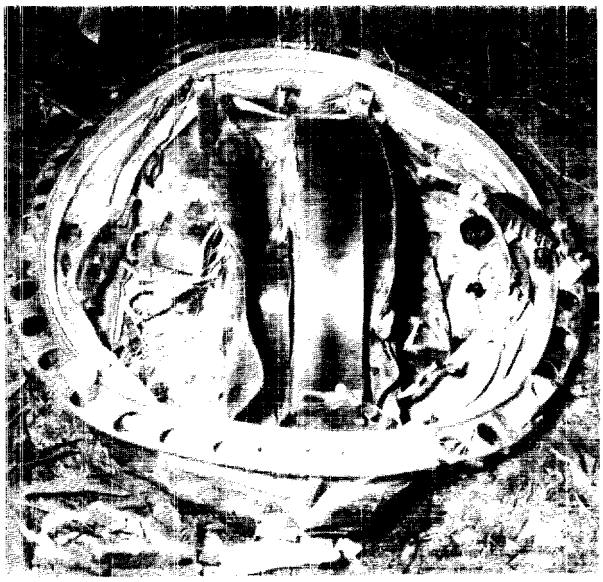
Improved exposure control through variable slit selection. (The J-1 system had a single exposure throughout the orbit resulting in poor performance at low sun angles.)

Capability of handling alternate film types and split film loads. An in-flight changeable filter and film change detector was added for this purpose.

Capability of handling ultra-thin base film (yielding a 50% increase in coverage with no increase in weight).

The panoramic camera that was developed to meet those design goals was known as the constant rotator. The predecessor C''' camera employed a com-

SRV being carried out of La Fría on foot by Campesinos



On location in La Fría, Táchira

Sold to the U. S. Air Force

bination of rotating lens cell and reciprocating camera members. In the constant rotator, the lens cell and the balance of the camera's optical system is mounted in a drum, and the entire drum assembly is continuously rotated, thus eliminating the reciprocating elements from the camera system. The film is exposed during a 70-degree angular segment of the drum's circular sweep. The capability of using ultra-thin base (UTB) film was one of the design goals, but the camera design was not to be constrained by requirements to accommodate the thinner film. UTB was successfully flown on several flights but ground test results showed a loss of reliability and attempts to use it in the contant rotator were eventully abandoned. In all other respects, however, the constant rotator was a resounding success. It yielded substantially better ground resolution in the photography. It also permitted versatility in operation far exceeding that available in the earlier cameras.

The stellar/index camera in use was a delicate instrument with a short (1.5″) focal length and a history of erratic performance. The efforts at upgrading the performance of the stellar/index camera resulted in an instrument with a 3″ focal length (like ARGON) and a dual-looking stellar element. The new camera had the jaw-breaking designation of Dual Improved Stellar Index Camera, commonly referred to by its acronym: DISIC.

The new payload system, which was designated the J-3, consisted of a pair of constant rotator panoramic cameras, a pair of horizon cameras, and a DISIC. The J-3 system naturally retained the stereo capability begun with the MURAL cameras and the two-bucket recovery concept of the J-1. Apart from the improved picture-taking capability of the hardware itself, the most significant advance of the J-3 was the flexibility it allowed in command and control of camera operations. Any conventional area search photographic reconnaissance system is film-limited. (When the film runs out, the mission is finished—assuming, of course, that other mission-limiting components of the system survive that long.) Consequently, the ultimate goal of all the CORONA improvement efforts was to pack the maximum of the best possible quality of photography of important intelligence targets into each roll of exposed film. The built-in flexibility of the J-3 system greatly increased the variety and degree of controls that could be applied to camera operations, thus substantially boosting the potential intelligence content of the photography.

The first J-3 system was launched on 15 September 1967, and it proved to be the one major modification with no bugs in it. In its nearly five years of operation, it yielded even better photographic intelligence and higher reliability than the remarkably successful predecessor J-1 system.

An early series of tests demonstrated the unusual flexibility of the J-3. It could not only accommodate a variety of film loads, including special camouflage-detection color and high-speed, high-resolution black and white; the camera also had two changeable filters and four changeable exposure slits on each camera.

These tests drew such interest throughout the intelligence community that a CORONA J-3 Ad Hoc Committee was formally convened by the Director of the National Reconnaissance Office on 4 December 1967, and formally constituted in February 1968. Its purpose was to analyze and evaluate the experiments con-

ducted on these five test flights. Specific findings of the Committee included the recommendations that further testing of color films and techniques should be conducted, against specific intelligence requirements and that a special subcommittee of the Committee on Imagery Requirements and Exploitation (COMIREX) should be constituted to evaluate the utility of satellite color photography; and that a well-planned color collection program be worked out with the close cooperation of the system program offices, the Satellite Operations Center (SOC), the intelligence analysts, and the photo interpreters.

In Retrospect

Looking back on CORONA, it is not always easy to keep in mind that it was merely an assemblage of inanimate objects designed and put together to perform a mechanical task. The program began as a short-term interim system, suffered through adversity in its formative years, and then survived in glory throughout a decade. Those who were associated with the program or came to depend upon its product developed an affection for the beast that bordered on the personal. They suffered with it in failure and revelled in its successes.

The technological improvements engineered under CORONA advanced the system in eight years from a single panoramic camera system having a design goal of 20 to 25 feet ground resolution and an orbital life of one day, to a twin camera panoramic system producing stereo-photography at the same ground resolution; then to a dual recovery system with an improvement in ground resolution to approximately 7 to 10 feet, and doubling the film payload; and finally, to the J-3 system with a constant rotator camera, selectable exposure and filter controls, a planned orbital life of 18 to 20 days, and yielding nadir resolution of 5-7 feet.

The totality of CORONA's contributions to U.S. intelligence holdings on denied areas and to the U.S. space program in general is virtually unmeasurable. Its progress was marked by a series of notable firsts: the first to recover objects from orbit, the first to deliver intelligence information from a satellite, the first to produce stereoscopic satellite photography, the first to employ multiple reentry vehicles, and the first satellite reconnaissance program to pass the 100-mission mark. By March 1964, CORONA had photographed 23 of the 25 Soviet ICBM complexes then in existence; three months later it had photographed all of them.

The value of CORONA to the U.S. intelligence effort is given dimension by this statement in a 1968 intelligence report: "No new ICBM complexes have been established in the USSR during the past year." So unequivocal a statement could be made only because of the confidence held by the analysts that if they were there, CORONA photography would have disclosed them.

CORONA coverage of the Middle East during the June 1967 war was of great value in estimating the relative military strengths of the opposing sides after the short combat period. Evidence of the extensive damage inflicted by the Israeli air attacks was produced by actual count of aircraft destroyed on the ground in Egypt, Syria, and Jordan. The claims of the Israelis might have been discounted as exaggerations but for this timely photographic proof.

In 1970, CORONA was called on to provide proof of Israeli-Egyptian claims with regard to cease-fire compliance or violation. CORONA Mission 1111, launched on 23 July 1970, successfully carried out the directions for this coverage, which brought the following praise from Dr. John McLucas, Under Secretary of the Air Force and Director, NRO, who said in a message to the Director of Special Projects, DD/S&T, on 25 August 1970:

> I extend my sincere thanks and a well done to you and your staff for your outstanding response to an urgent Intelligence Community requirement.
>
> The extension of . . . Mission 1111 to 19 days, without benefit of solar panels, and the change in the satellite orbit to permit photography of the Middle East on 10 August provided information which could not be obtained through any other means. This photography is being used as a baseline for determining compliance with the Suez cease-fire provisions.

CORONA's Decade of Glory is now history. The first, the longest, and the most successful of the nation's space recovery programs, CORONA explored and conquered the technological unknowns of space reconnaissance, lifted the curtain of secrecy that screened developments within the Soviet Union and Communist China, and opened the way for the even more sophisticated follow-on satellite reconnaissance systems. The 145th and final CORONA launch took place on 25 May 1972 with the final recovery on 31 May 1972. That was the 165th recovery in the CORONA program, more than the total of all of the other U.S. programs combined. CORONA provided photographic coverage of approximately 750,000,000 square nautical miles of the earth's surface. This dramatic achievement was surpassed only by intelligence derived from the photography.

In placing a value on the intelligence obtained by the U.S. through its photographic reconnaissance satellite programs between 1960 and 1970, a first consideration, on the positive side, would be that it had made it possible for the President in office to react more wisely to crucial international situations when armed with the knowledge provided by these programs. Conversely, it can be said that without the intelligence which this program furnished, we might have misguidedly been pressured into a World War III.

The intelligence collected by the reconnaissance programs makes a vital contribution to the National Intelligence Estimates upon which the defense of the U.S. and the strategic plans of the military services are based. Principal among those estimates are the ones which deal with the Soviet and Chinese Communist strategic weapons, space, and nuclear energy programs.

The intelligence from overhead reconnaissance counts heavily not only in planning our defense, but also in programming and budgeting for it. It helps to avoid the kind of floundering that occurred during the time of the projection of the "Missile Gap." Without the kind of intelligence which the CORONA program provided, the U.S. budget for the defense of our own territory, and for military assistance to our allies, would doubtless have been increased by billions.

The total cost for all CORONA activities of both the Air Force and the CIA over the 16-year period was [redacted]

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The CORONA program was so efficiently managed that even the qualification models of each series were refurbished and flown. As a result, there was little

hardware available at the termination of the program when it was suggested that a museum display should be set up to illustrate and to preserve this remarkable program. Using recovered hardware from the last flight, developmental models from the J-3 program, and photographic records from the memorable flights, a classified museum display was set up in Washington, D. C. In his speech dedicating the Museum, Mr. Richard Helms, the Director of Central Intelligence said:

> It was confidence in the ability of intelligence to monitor Soviet compliance with the commitments that enabled President Nixon to enter into the Strategic Arms Limitation Talks and to sign the Arms Limitation Treaty. Much, but by no means all, of the intelligence necessary to verify Soviet compliance with SALT will come from photoreconnaissance satellites. CORONA, the program which pioneered the way in satellite reconnaissance, deserves the place in history which we are preserving through this small Museum display.
>
> "A Decade of Glory," as the display is entitled, must for the present remain classified. We hope, however, that as the world grows to accept satellite reconnaissance, it can be transferred to the Smithsonian Institution. Then the American public can view this work, and then the men of CORONA, like the Wright Brothers, can be recognized for the role they played in the shaping of history.

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