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STUDIES in INTELLIGENCE



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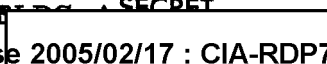
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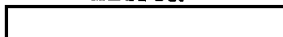
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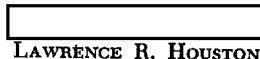


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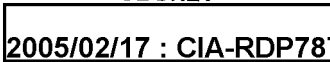
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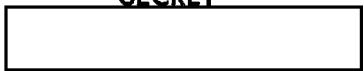
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CONTENTS

	<i>Page</i>
Penkovskiy's Legacy and Strategic Research Len Parkinson	1
<i>The latest strategic information is not in all cases the most useful checkpoint. SECRET</i>	
The Cuban Missile Crisis of 1962: Presenting the Photographic Evidence Abroad Sherman Kent	19
<i>Inside story. SECRET</i>	
Bayesian Analysis	
Bayes' Theorem for Intelligence Analysis Jack Zlotnick	43
<i>More on probability - I. UNCLASSIFIED</i>	
The Sino-Soviet Border Dispute: A Comparison of the Conventional and Bayesian Methods for Intelligence Warning.	53
Charles E. Fisk	
<i>More on probability - II. SECRET</i>	
The Origins of National Intelligence Estimating	63
Ludwell Lee Montague	
<i>In the beginning. CONFIDENTIAL</i>	
Intelligence Implications of Disease	71
Warren F. Carey and Myles Maxfield	
<i>Genesis of a project. SECRET</i>	
Strategic Warning: The Problem of Timing . . . Cynthia M. Grabo	79
<i>On assessing timing. SECRET</i>	
Intelligence Support to the US SALT Delegation	93
Howard Stoertz, Jr.	
<i>Letter from Helsinki. SECRET</i>	
Notes and Comments	107
Intelligence in Publication	113

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The editorial board will welcome readers' nominations for awards but reserves to itself exclusive competence in the decision.

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CONTRIBUTORS TO THIS ISSUE

Len Parkinson is a member of CIA's Office of Strategic Research.

Sherman Kent is presently engaged in historical research.

Jack Zlotnick was formerly with the Office of Current Intelligence and

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Charles E. Fisk is a member of CIA's Office of Economic Research.

The late Ludwell Lee Montague was a retired member of the Board of National Estimates.

The late Dr. Warren F. Carey was a member of CIA's Life Sciences Division, as is his colleague, Dr. Myles Maxfield.

Cynthia M. Grabo is a member of the National Indications Center, and a contributor to *Studies in Intelligence* in the past.

Howard Stoertz, Jr., former member of the Office of National Estimates, is now the director of CIA's Imagery Analysis Service, and was on the scene for the activity he describes.

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is not in all cases the most useful
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PENKOVSKIY'S LEGACY AND STRATEGIC RESEARCH

Len Parkinson*

Why do we in the Directorate of Intelligence continue to research the documentary material Colonel Penkovskiy photographed in the early Sixties with his Minox cameras?

For one thing, we have concluded that most Soviet military practices and strategic theories are slow to change. We have, therefore, found it useful to identify as many of these practices and concepts as possible, because this helps us in analyzing genuinely new Soviet strategic doctrines, and in evaluating how the Soviets are reacting or might react to particular political and military events.

Secondly, the development of some key weapons systems requires long lead-times. For many weapons in the Soviet Navy, as an example, the average is about ten years. Furthermore, additional time is required to work out the operational concepts for the use of some new weaponry. As a result, the discussions of some new systems in IRONBARK—the code name for the bulk of Colonel Penkovskiy's photographs—retained usefulness for strategic researchers through the late Sixties and early Seventies. A large number of hardware developments observed in the last several years of the last decade can be traced to discussions in the IRONBARK documents.

A third reason for repeated immersion in the thousands of pages of IRONBARK, even though much of it is now ten years old, is realistic training for intelligence analysts. A survey conducted by the Office of National Estimates in 1970 concluded that several offices in the Central Intelligence Agency continue to value the collection, particularly as an aid in the training of new researchers. The ONE poll concluded that there is no better source from which to gain a basic insight into the way the Soviets think about military philosophy and doctrine.

*Adapted by the author from his presentation before the February 1971 Midcareer Executive Development Course (Number 27).

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So, for all these reasons, it is rather evident *why* we still value the Penkovskiy material as a solid reference aid.

How we use it is a more complex question to answer. I will address that question by examining five general research areas in which the IRONBARK proved to be a coup of the first magnitude. These five main areas, in which it is still quite useful as a checkpoint, are: first and foremost, *military doctrine*—in particular, the IRONBARK is critical background for our current research on Soviet perceptions of the nature of an East-West war in Europe; second, *military organization*—a research area which particularly involves the subjects of combat readiness, reinforcement, and mobilization in the Soviet Ground Forces; third, *hardware*—our research on this currently centers on the characteristics of anti-ballistic missiles (ABMs) and surface-to-air missiles (SAMs), and anti-submarine and anti-carrier weapons, the need for which were outlined or hinted at in the IRONBARK; fourth, the Penkovskiy material is useful for research on *bureaucratic behavior*, an analytical field which involves a combination of our research on doctrine, organization, and hardware; fifth, and last, the IRONBARK remains useful in researching the rather exotic field of Soviet procedures for maintaining control of their nuclear weapons. In the popular literature this is called “fail-safe,” but it really ought to be called “*positive control*,” and toward the end of the article I will examine the question of who pushes the button.

One: Doctrine

Several months ago, a document on Soviet offensive operations in the European theater came across my desk. I was, at that time, responding to a request from the Pentagon to prepare a memorandum on the significance of one of the earliest IRONBARK documents, which also examined Soviet offensive operations in the European theater.¹ The two documents on my desk were not only dated a decade apart, but they advocated sharply different approaches on the proper manner to wage a European war. Thus, my analytical task became an effort to assess which one more closely reflected current accepted Soviet military thinking on this important matter.

And here is a good example, I think, of the current value of understanding the totality of the Penkovskiy reports. By studying *all* the documents in the IRONBARK series, we and our counterparts

¹ An article by Lieutenant-General V. Baskakov, Special Collection of Articles of the Journal *Military Thought*, 1960, First Issue.

in British Intelligence were able in the early and mid-Sixties to identify a number of *patterns* and signs of evolution in the doctrinal discussions. The identified patterns, in turn, have helped us to evaluate the reports that we have received *singly* from other sources since Penkovskiy was apprehended in 1962.

Some of the IRONBARK material which Penkovskiy passed to us in 1961 and 1962 revealed a sharp military debate concerning Soviet military concepts and organization needed for nuclear warfare. There was general agreement in the writings that the existing doctrine and organization were obsolete and inadequate for the era of modern nuclear weapons. But there was wide disagreement on what changes were necessary and how best to accomplish them. The central issue in the IRONBARK debate in the early Sixties was the force structure question of whether nuclear weapons should support massive conventional combat operations in Europe—or replace them.

The IRONBARK document that the Pentagon wanted our comments on called for forces which could practically vaporize NATO countries—their national command centers, economic and strategic targets, and armed forces—by nuclear strikes carried out by the Strategic Rocket Forces. This strategy gave the Soviet Ground Forces the subordinate task of marching through the rubble. No “battle” was to take place, and there was little indication that a systematic conquest of NATO Europe was conceived, not any attempt to exploit its resources in the interests of the Soviet Union. (The cold calculation, presumably, was that ashes were not really worth occupying.) This theory, which we dubbed the “more rubble for the rubble” strategy of former party leader Khrushchev, was endorsed by only two other military writers in the IRONBARK collection.²

The Khrushchevian conclusion that nuclear weapons would replace massive conventional combat operations in Europe promptly provoked a sharp reaction from a wide variety of senior professional Soviet officers. These officers proceeded to lay out the main themes of their more orthodox, traditionalist line in subsequent issues of the IRONBARK material. The more orthodox writers argued that the indiscriminate use of nuclear weapons in the European Theater was wrong (one general rebutted that such saturation strikes do not “conform with Marxist dialectics”³), that nuclear-missile weapons should

² Colonel-General A. I. Gastilovich and Lieutenant-General I. A. Tolkonyuk, Special Collection of Articles of the Journal *Military Thought*, 1960, First Issue.

³ General of the Army P. A. Kurochkin, Special Collection of Articles of the Journal *Military Thought*, 1960, Second Issue.

be used only within the limits of expediency, and that the selection of enemy objectives to be destroyed in the tactical and operational zones is the prerogative of the troops of the Front, *not* the Strategic Rocket Forces.⁴

In the view of the orthodox camp,⁵ sufficient reason remained to draw up conventional plans to blitz to the Rhine and beyond. These plans were based firmly on the traditional judgment that a land battle would be fought in Europe which would require adequate ground and air forces. One traditionalist writer argued that "the *dominant* role in an operational-tactical plan will quite often belong to the Ground Forces. . . ." ⁶

The weight of this orthodox counter-barrage was so heavy that the very radical, Khrushchev-like views practically disappeared from the IRONBARK debate. As a result, Khrushchev initially failed to sustain an imaginative airing of military arguments in favor of his defensive policy in the published material supplied by Penkovskiy.

At this point, the singular nature of some of the IRONBARK, particularly the *Special Collection of Articles of the Journal Military Thought*, merits some explanation. The Soviets classified the documents top secret, but, most significantly, they were in fact unofficial. The unusual nature of the *Special Collection* arose from the fact that it was established, in early 1960, as an *ad hoc* forum for the airing of frank, controversial and far-ranging views of senior military officers. According to an editorial note, the articles expressed only the opinions of the authors.

The articles selected for publication in the *Special Collection* were evidently regarded as too sensitive for publication in the secret *Collections of Military Thought* articles, or in the more widely circulated monthly *Military Thought*.⁷ The circulation of the *Special Collection* was limited to army commanders and higher. The contributing writers, for the most part, were drawn from the same small circle of military elites. Numbered among the contributors were the Minister of Defense, the deputy ministers of defense, military district com-

⁴ General of the Army V. V. Kurasov, *Special Collection of Articles of the Journal Military Thought*, 1960, Third Issue.

⁵ In addition to Kurasov and Kurochkin, it included Marshal of Armored Troops P. A. Rotmistrov, Colonel-General N. O. Pavolvskiy, General of the Army A. V. Gorbатов, Colonel-General A. Kh. Babadzhanyan, Colonel-General I. I. Gusakovskiy, and Colonel-General G. I. Khetagurov.

⁶ Kurochkin, *op. cit.* Emphasis supplied.

⁷ This version was restricted to "Generals, Admirals, and all officers of the Soviet armed forces."

manders, senior staff officers, chiefs and officials of military directorates, and military academy heads and theorists.

Since the *Special Collection* constituted a forum principally for the exchange of unofficial or individually held viewpoints, the materials contained numerous recommendations for the planning and conduct of strategic and front operations in a future general war. And the articles varied in quality. Some were distinguished for the care and thoroughness exercised in their preparation. Other articles were disjointed, naive, incomplete, extreme.

The very extreme nature of the views put forward in the IRONBARK document on my desk (the one the Defense Department had requested more information on) was probably part of the reason for the failure of Khrushchev's attempt to gain many adherents for the foundations underlying the logic in his military philosophy. His premises were that, first, any direct confrontation in Europe over vital interests would quickly escalate into an all-out nuclear exchange that, second, would leave little room for a land battle and, therefore (to repeat), little need for a massive, multimillion man conventional force.

The fireball philosophy of Khrushchev—and this appears to be the salient point—involved much more than military strategy. It was closely tied to his long term program for domestic economic development. His program required increased resources for domestic investment and consumer goods, which he hoped to obtain in large measure through economies in the military. At the expense of conventional capabilities, he advocated a military policy based on a minimal nuclear deterrent. His strategic policy was in part dependent on deceptive statements and Soviet secrecy, in the sense that it rested at that time largely on the US intelligence community's inflated assessment of the numbers of Soviet intercontinental ballistic missiles (ICBMs).

In brief, Khrushchev's considerations on the nature of a future war were simple and cheap. The bulk of the professional Soviet military's arguments in the *Special Collection*, by comparison, was costly and complex.

The outcome of the debate exposed in the IRONBARK was greatly influenced by two developments: the introduction of US satellite photography, which subsequently exposed Khrushchev's missile gap deception; and the failure of Khrushchev's last "cheap" attempt to employ the strategic threat for policy gain by trying to position medium range missiles in Cuba in 1962. The post-Khrushchev leaders apparently concluded that past deficiencies in strategic power were in part responsible for foreign policy fumbles, and that a policy

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Penkovskiy's Legacy

of minimal deterrence was too risky for the Soviet Union. In other words, the new leadership decided to purchase the security—in both the strategic and conventional forces—that Khrushchev tried to finesse.

In this sober vein, the theoretical pitch of that new document (the one which I was contrasting with the ten-year-old IRONBARK article) reflected the orthodox consideration that in order to strengthen and make the Soviet deterrent more effective, the Soviet Union must make serious and costly efforts to prepare for all kinds of threats. This view was central to other post-Khrushchev classified articles which discussed offensive operations in the general vein of the more traditional advocates in the 1960–62 IRONBARK *Special Collection* series.⁸

Two: Organization

The knowledge of the doctrinal debate in the IRONBARK turned out to be doubly important, because the various articles on the subject also provided considerable new insight into the key subjects of combat readiness and mobilization.

The IRONBARK evidence on combat readiness⁹ indicated that in peacetime Soviet authorities viewed most of their divisions as gen-

⁸ This lengthy footnote is composed for those readers who wonder what happened to the “orthodox” and “radical” strategists of the early Sixties. The quick answer is that their subsequent careers appear to have been largely unaffected by the points of view expressed in the debate. The longer answer, starting with the last known positions of the conservative writers, is as follows: P. A. Rotmistrov, a general inspector of the Group of General Inspectors of the Ministry of Defense; N. O. Pavlovskiy, deceased, last held the post of Deputy Chief of the General Staff; V. V. Kurasov, inactive, last held the post of Member of the Joint Supreme Command for Warsaw Pact Forces in East Germany; A. V. Gorbatov, probably inactive, last held the post of Deputy Chief of the General Staff; A. Kh. Babadzhanyan, presently Deputy Commander in Chief of Ground Forces and Chief of the Armored Troops; I. I. Gusakovskiy, probably inactive, last held the post of Chief of the Main Personnel Directorate of the Ministry of Defense; G. I. Khetagurov, probably inactive, last held the post of Commander of the Baltic Military District; P. A. Kurochkin, may be inactive, his last post (terminated by 1970) was as Member of the Joint Supreme Command for Warsaw Pact Forces in East Germany. The last known positions of the “radicals” are as follows: A. I. Gastilovich, inactive, last held the post of Senior Professor at the Academy of The General Staff; I. A. Tolkonyuk, last identified in 1969 (but since replaced) as the First Deputy Commander of the Siberian Military District; V. Baskakov, presently a Colonel-General and the Deputy Chief of the Main Directorate for Military Training Institutions. N. Khrushchev, the last of the important radicals, “retired” in mid-October 1964 and died on 11 September 1971.

⁹ In particular see Major-General Ya. Shchepennikov, *Military Thought*, 1961, Third Issue. Also see Major-General A. Klyukanov, *Special Collection of Articles of the Journal Military Thought*, 1961, First Issue.

erally falling into three classes, based on level of strength and availability for use. The first class consisted of units "in a full state of combat readiness" and available for immediate use. The second class of units were frequently termed "of increased combat readiness" requiring a "short mobilization period" and capable of being moved to the theater of operations "within hours" or up to "several days." The strength and availability of the third class were the least clearly defined in the documents. The third class units were described as either "at reduced strength" or "in cadre status," and their availability was expressed in days or (sometimes) weeks.

Evidence over the last ten years supports this three-way breakdown. The only refinement some of us would make would be the addition of a fourth class of division, one in skeleton form.

Most of the IRONBARK writers who wrote on combat readiness were in agreement that at least three classes of divisions were expected to participate in the hypothetical campaign to seize Western Europe. And most Soviet authorities in the early Sixties considered that the campaign would be finished in about two to three weeks. In the schemes of the General Staffers, the campaign was to end with the arrival of Soviet forces at the English Channel within 10 to 20 days. On the timetable issue, we have evidence that the Soviets' current planning for the blitzkrieg campaign against Western Europe is essentially as ambitious as it was at the time the IRONBARK documents were published, including both timing and composition of the Warsaw Pact forces to be involved.

The capability to accomplish such a dazzling deployment depends in large part on the effectiveness of the mobilization system. In this connection, information on the Soviet system and its capabilities generally echoes assertions in a 1961 IRONBARK article which maintained that large units from the western part of the Soviet Union could complete their mobilization and reinforcement in about 10 to 12 days.¹⁰ For example, in the case of their performance during the 1968 Czechoslovak crisis, when the Soviets could set the pace themselves, a partial mobilization and reinforcement was accomplished in about two and a half weeks.

Three: Hardware

A third research area where the IRONBARK remained valuable for many years was in the identification and analysis of some of the characteristics of several new weapon systems.

¹⁰ Major-General P. Stepshin, Collection of the Journal *Military Thought*, 1961, Sixth Issue.

At the outset, however, it may be helpful to highlight the critical information provided by Penkovskiy on the fairly old medium-range and short-range ballistic missiles. This information helped us to evaluate readiness conditions. For example, it enabled us to inform the intelligence community that at least part of the medium range (MRBM) force was in an increased state of readiness during the Soviet invasion of Czechoslovakia in August 1968, when other sources indicated that crews working on the SS-4 were performing certain critical work revealed in the Penkovskiy material.

Significantly, other intelligence sources helped us determine at an early stage of the Soviet buildup of forces along Czech borders that this deployment of forces was *not* directed against NATO. In other words, the increased state of readiness of some of the Soviet MRBM force was a precautionary move—part of a contingency plan against the rather remote contingency that the invasion of Czechoslovakia would spark a general European war.

How has the IRONBARK helped us in our research on new strategic defensive weapon systems—ABMs and SAMs?

Two articles published in the IRONBARK series in early 1962 reflected Soviet consideration of low altitude intercept of ballistic missile reentry vehicles.¹¹ Both articles rejected the concept of using atmospheric sorting as a means of identifying the reentry vehicle prior to its engagement and destruction. The engagement phase of the Soviet ABM system should take place in outer space, not in the atmosphere.

The two articles in the IRONBARK recognized the need for sorting, but one discarded the atmospheric approach on the basis of the limited reaction time available after target identification (and in fact it *is* literally counted in seconds). The other IRONBARK article warned of the risks to ground targets if ICBMs and intermediate range ballistic missiles (IRBMs) were allowed to penetrate to altitudes below 40 to 50 kilometers before attempting intercepts. When the first generation Soviet ABM system was deployed around Moscow, technical analysis of the system by CIA's Science and Technology Directorate was consistent with the IRONBARK exoatmospheric conclusion. Subsequent U.S. progress with endoatmospheric interception (with the Sprint missile) alerted us to look for any possible evidence that the Soviets were reconsidering their earlier rejection of atmospheric intercepts.

¹¹ Colonel-General I. Podgornyy and Colonels V. Savko and N. Maksimov, Special Collection of Articles of the Journal *Military Thought*, 1962, First Issue.

Another 1962 IRONBARK article¹² cited the chief of Soviet strategic air defense (*PVO Strany*) on the need for a long-range surface-to-air missile system. Such a system would permit a change in the Soviet organization of air (*not* missile) defense from defense of *points* to defense of *zones*. This, of course, alerted us to look for the development of a long-range SAM. When one appeared with characteristics which seemed to fulfill the zonal requirement, the IRONBARK statement formed part of the evidence used in assessing the role of this new SAM system.

Several articles discussed the problems of air defense for the ground forces.¹³ These stressed a need for highly maneuverable weapons which existing Soviet strategic SAM systems (SA-1, SA-2, SA-3) could not provide. This alerted us to watch for development of mobile SAMs, which we first saw in 1964. The IRONBARK articles also indicated that the Soviets probably would not deploy large numbers of their SA-2 and SA-3 systems with tactical forces, and they have not.

Another important field of weapons development discussed in the IRONBARK dealt with the Soviet Navy. However, IRONBARK was not clear regarding the role of Soviet ballistic missile submarines. Apparently the small force then in existence was targeted against naval bases and ports and not on cities or military targets farther inland. The best deduction is that the role and future of the ballistic missile submarine were under debate in 1960-62, but at the highest level and was too sensitive a topic to be within IRONBARK material.

With the exception of this gap in the IRONBARK, the material helped us to understand at least two important missions of the Soviet Navy—the anti-carrier mission and the anti-Polaris mission.

The IRONBARK admirals saw the US attack carrier as the greatest strategic threat at that time. New anti-carrier equipment was entering the Soviet fleets but major problems of its strategic and tactical employment remained to be solved. And the cruise missile, delivered by aircraft and submarines, clearly emerged in the Penkovskiy papers as the primary anti-carrier weapon. This knowledge helped US intelligence discern the purpose of the SS-N-3 and other missiles, a navy bomber, and two classes of cruise missile sub-

¹² Lieutenant-Colonel Ye. Ryukin, *Military Thought*, 1961, Sixth Issue.

¹³ In particular, see Colonel-General S. Mironov, Special Collection of Articles of the Journal *Military Thought*, 1962, First Issue.

marines.¹⁴ All of these weapons were exercised in the annual Norwegian Sea exercises during the Sixties. These exercises have continued into this decade and have followed the strategic and tactical lines set out in one of the *Military Thought* articles.¹⁵

This article and other IRONBARK papers were our principal guide for interpreting these important exercises and establishing the estimate of the SS-N-3 missile as, primarily, an anti-ship weapon—not a weapon intended for strategic attacks on bases and other targets ashore.

The IRONBARK admirals, exhorted to look ahead, foresaw that the Polaris submarine would replace US attack carriers well before 1971 as the primary strategic threat from the sea. One admiral¹⁶ revealed that the Soviet Navy was assigned its anti-Polaris mission in 1957, and another¹⁷ outlined a rather comprehensive anti-Polaris program. In retrospect it is clear that many essentials of the plans in the IRONBARK were accepted. Due to the long lead-time required for development of many anti-submarine warfare (ASW) systems, some of these just recently showed up in operational versions. This is a case in which the hardware value of IRONBARK is particularly relevant to today's strategic researchers.

The IRONBARK admirals, however, were divided on the proper direction for the submarine mission. One admiral¹⁸ advocated multipurpose submarines for anti-ship as well as anti-Polaris mission, while another¹⁹ argued for several classes of specialized submarines with designs optimized for specific tasks. Consequently IRONBARK, while suggestive, is not a definitive aid in sorting out several new classes of attack submarines now under construction. There was general agreement, however, in the Penkovskiy papers on the value of nuclear propulsion for submarines and for the priority development of better sonar and torpedoes.

¹⁴ For example, the Kennel and Kipper missiles, the naval TU-16 bomber, and the E-class and J-class cruise missile submarines.

¹⁵ Captain First Rank Ye. Mamayev, Collection of Articles of *Military Thought* (the SECRET version), 1962, Third Issue.

¹⁶ Admiral N. Kharlamov, Special Collection of Articles of the Journal *Military Thought*, 1962, First Issue.

¹⁷ Rear Admiral O. Zhukovskiy, Special Collection of Articles of the Journal *Military Thought*, 1961, Fourth Issue.

¹⁸ Admiral V. Platonov, Special Collection of Articles of the Journal *Military Thought*, 1961, Second Issue.

¹⁹ Admiral Yu. Panteleyev, Special Collection of Articles of the Journal *Military Thought*, 1961, Third Issue.

The total impression given by IRONBARK was that, while anti-Polaris killer submarines held the greatest promise, aircraft and surface ships had crucial stalking roles to play. For example, one paragraph in a *Special Collection* essay²⁰ assisted in our early (1967) assessment of the Moskva helicopter cruiser as an anti-submarine warfare ship. Other admirals advocated ASW cruisers and destroyers with strong air defense armament to protect them while hunting Polaris far at sea. We believe this concept is behind the appearance of surface-to-air missiles on the Moskva and on five other classes of ships whose NATO designations all begin with "K": the Kresta, Kashin, Kanin, Kotlin, and Krivak classes. The joint SAM-ASW concept may also be behind additional new classes of major combatants under construction.

In the main, the ASW aircraft force, with its improved detection and weapons systems, developed along the lines laid out in IRONBARK. In fact, the increasing emphasis to the ASW mission in naval aviation, like the use of the helicopters on the deck of the Moskva helicopter cruiser, was foreshadowed in the *Military Thought* articles.

Four: Bureaucratic Behavior

Another current research area in which we still use the IRONBARK involves a combination of the three subjects just discussed—doctrine, organization, and hardware.

We call this research on bureaucratic behavior, and the Penkovskiy material is extra rich because several critical features relating to doctrine, organization, and hardware were all in sharp focus by 1960. In addition, a number of important decisions were begging for resolution, such as the proper role of armor in a nuclear war.

Thirteen articles appearing in the IRONBARK's *Special Collection* constituted the main vehicle for an intramilitary assessment of the armor question.²¹ The authors of these articles ranged from technical

²⁰ Rear Admiral N. Zvyagin, *Special Collection of Articles of the Journal Military Thought*, 1961, Second Issue.

²¹ See articles by Colonel-General A. Kh. Babadzhanyan, Colonel-General A. I. Gastilovich, Marshal of the Soviet Union R. Malinovskiy, Colonel-General P. Poluboyarov, Marshal of Armored Troops P. Rotmistrov, Major-General L. Sergeev, Lieutenant-General A. Shevchenko, Lieutenant-General M. Shaposhnikov, Major-General G. Zavizion, Colonel V. Zemskov, General of the Army A. Zhadov, and Major-General C. Zimelev. For a good analysis of these articles, see the Rand study *The Role of Armor: Case Study of a Soviet Bureaucratic Decision Pattern* (Secret, April 1969, RM-5814-PR/ISA).

specialists who detailed fine points of tank design and troop organization, through senior branch-level officers who dealt with more comprehensive concepts, up to the Minister of Defense, who summarized the course and content of the professional military's discussions. The Minister of Defense (Marshal Malinovskiy at that time) also indicated the main directions he thought worth pursuing in the armor field.

For a strategic intelligence researcher, these articles—which span practically the whole period embraced by the Penkovskiy material—are engrossing, because their contents testify to a vigorous exchange of views and an examination of alternative choices.

One choice that the Soviets made resulted in the machine which they call the Infantry Combat Vehicle or ICV. The guidelines for the ICV were set out in considerable detail in one of the articles on the tank debate.²² This advanced weapon was first seen in the Moscow parade in November 1967, and when analysts in the Intelligence Directorate began to study its characteristics, they were already on first base, thanks to the 1961 guidelines in this one IRONBARK article. The guidelines called for an amphibious, lightweight, low silhouette vehicle mounting a turreted cannon and an antitank guided missile. The guidelines added that the vehicle was to carry a squad of men and provide hatches at the rear of the vehicle for safe entry and exit under fire. The Infantry Combat Vehicle meets all these requirements.

Interestingly, from a bureaucratic point of view, one choice that the Soviets apparently did *not* make was a super-sophisticated tank described in the IRONBARK by the top man in the Soviet military, Defense Minister Malinovskiy.

The *Special Collection* materials on the role of armor, supplemented by Soviet open press writings and by our own esoteric communication analysis, enable the researcher to reconstruct, practically blow-by-blow, the institutional, bureaucratic realities in which some major Soviet decisions were actually made.

Another subject in which research on bureaucratic behavior is currently making use of the Penkovskiy papers concerns the organization of the Strategic Rocket Forces. The SRF was established only a few months before Colonel Penkovskiy made his first contact with us. And the key fact about the SRF in the 1960 to 1961 period was that it was still in the process of formation: jurisdictional responsibilities were being defined and redefined, relationships with the General

²² Colonel General Poluboyarov, *ibid.*

Staff were being determined, personnel acquired, and major directorates being transferred to the SRF.

On the subject of the SRF's organization, the researcher can set aside the IRONBARK volumes and make use of the colorful CHICKADEE series. CHICKADEE is the codename for the tape recordings Penkovskiy made and the reports he himself wrote.

One of the particularly interesting series of CHICKADEE reports concerned a dispute between two important rocketry officials in the Soviet military, Marshals Varentsov and Moskalenko. Penkovskiy reported that beginning in early 1961 there were rumors at responsible levels of the Soviet General Staff that the strategic missile command under Moskalenko (then Commander-in-Chief of the Strategic Rocket Forces) would be combined under a new command headed by Varentsov, a close associate of Penkovskiy. Varentsov, then responsible for tactical missiles, had openly referred to his rival, Moskalenko, as a "stupid old sheep." Agitation for the incorporation of Moskalenko's strategic missiles under Varentsov's command apparently existed throughout 1961, but by January 1962 Penkovskiy reported that the final decision on this matter had been taken in Moskalenko's favor.²³ But in the Intelligence Directorate, the reasons behind Moskalenko's success and Varentsov's disappointment in early 1962 remain another mystery of Moscow's byzantine-style politics.²⁴

Politburo-level politics and policies, and particularly those dealing with research on the highest level military decision-making bodies in the Soviet Union, constitute another research target in which the Penkovskiy material retains value. Here the CHICKADEE series provides useful background on the rough-and-tumble way Khrushchev ran his Higher Military Council—the rough equivalent of our National Security Council. These reports also provide a useful contrast with the comparatively phlegmatic management style of the current party

²³ Marshal Varentsov was reduced in rank following the discovery of Colonel Penkovskiy's activities. (Penkovskiy was a frequent visitor in the Varentsov home, and had full access to the Marshal's missile and artillery headquarters.) Varentsov never regained his former lofty rank of Marshal of Artillery. A final indication of his disgrace was the terse, unsigned notice of his death in *Red Star* on 4 March 1971. This is the sort of obituary usually given to relatively obscure veterans.

²⁴ The mystery is "solved," if you are willing to accept the authenticity of Khrushchev's story in his "memoirs." Khrushchev, by CPSU (and Mafia) standards, would have owed a debt to Moskalenko. According to the "memoirs," Moskalenko (like a sheep) followed Khrushchev's extra-legal orders soon after Stalin's death by arresting a Kremlin goon of the period (Lavrenty Beria) in the Politburo inner sanctum. "I could see that Comrade Moskalenko would do what was necessary for the Party cause." *Khrushchev Remembers* (1970, Little, Brown and Co., Inc.), page 338.

boss, Brezhnev. Penkovskiy's tape recording sessions also provided another chapter on the energetic style of the present Defense Minister, Grechko.²⁵

In short, the IRONBARK and CHICKADEE material have been invaluable in our research on bureaucratic behavior. This material, in part, helped us modify the simplistic Cold War notion of the Soviet Union as a monolithic system directed by a unified central power.

Five: Positive Control

A fifth current research area concerns the sensitive subject of Soviet procedures for maintaining control over their strategic nuclear weaponry.

Here it is important to emphasize that the IRONBARK and CHICKADEE provided a wealth of essential information, previously unknown and unavailable to us through other collection efforts, concerning Soviet strategic missiles. Through another IRONBARK series—the Top Secret *Information Bulletin of the Missile Troops*²⁶—we saw for the first time how the Soviet strategic missile units were organized and structured, what the functions of the various staffs in each unit were, how these units were linked through the chain of command to the military high command in Moscow, and what the activities of missile units were at the different levels of combat readiness. Through the CHICKADEE series, we received for the first time detailed technical data on the missiles themselves, on the yields of their warheads, on the method by which the missiles were oriented toward their targets, and on the types of priority targets to be attacked by strategic missiles, that is, military targets, industrial and administrative centers, and the like.

But regarding all this data, there are analysts in the intelligence Directorate who now maintain that the Penkovskiy material and ancient history are beginning to have much in common. Indeed, the IRONBARK is aging, particularly in light of the stunning changes in the makeup of the Soviet strategic missile force since the last of the Penkovskiy papers were acquired in 1962. For example, the force of Penkovskiy's time was composed almost entirely of medium and intermediate range ballistic missiles. The Soviets in the past few years have begun to deactivate these missiles. Only a handful of intercontinental

²⁵ An earlier chapter on Grechko's management style was derived from our telephone tap in the Fifties, when he was Commander-in-Chief of the Soviet Forces in East Germany.

²⁶ In contrast to the *Special Collection*, the *Bulletin* was a technical journal which did not carry controversial or unofficial articles.

missiles were available when the IRONBARK documents were written, and these were located at vulnerable soft sites (i.e., above ground level). Since then, well over one thousand ICBMs have been deployed, principally in single, dispersed, hardened silos. Advanced systems for command and control of the force have, according to Soviet sources, been put into use in the same period to centralize control of all strategic weapons. In addition, multiple warheads are now being introduced into the force, and production of Polaris-type submarines has been underway for more than five years.

Nevertheless, some of the information in the Penkovskiy material continues to be pertinent to intelligence research being undertaken today in the Office of Strategic Research. And a prime example is in the area of research on Soviet measures for control of strategic offensive weapons.

This research examines measures the Soviets have taken to achieve what is called "positive control"—preventing accidental or unauthorized use of nuclear weapons while maintaining a capability for quick, measured nuclear strikes. It also examines questions about Soviet awareness of the need for such control and the evolution and present status of the Soviet national command mechanism; who in the Soviet hierarchy gives the order to launch a nuclear attack? How is an order communicated to the launch sites? This research draws upon open Soviet sources. A wide variety of technical collection systems are also used. But the contribution from the Penkovskiy material remains conspicuous and significant.

Unexpectedly, it is not always what the Penkovskiy documents *say* that is important for our detective work in this research area, but what they *do not* say.

Until quite recently, there was evidently a prohibition against discussing in the Soviet open press the dangers of unauthorized or accidental use of *Soviet* nuclear weapons. Significantly, the Penkovskiy papers did not discuss this issue, which indicated that the blackout extended even to classified military publications. The chief reasons for the blackout might have been Soviet super-sensitivity toward the subject—that is, security concerns may have outweighed other important considerations, including that of keeping foreign governments informed about the adequacy of Soviet precautions. The IRONBARK editors may also have believed that the more one can learn about Soviet safety precautions, the more one can infer about Soviet preparedness and capabilities—and in the early Sixties, unlike the early Seventies, the Soviets had little of either preparedness or capabilities in the strategic-missile field.

It is also possible to reinterpret and gain new clues from the Penkovskiy material on the basis of what we have subsequently learned about the Soviet command network from other sources. In this connection, some of the IRONBARK documents addressed the need to make greater use of computers and automation in the command and control process as well as in the actual firing of missiles. Although these particular documents did not describe the computer and automation systems needed to do the job, they did reveal the types of command and control problems the Soviets were experiencing in 1960, 1961, and 1962, and the types of proposals they were considering to correct these problems.

The documents indicated that the Soviets would seek, through improvements in communications technology, automation, and data processing, to reduce the reaction time of their strategic forces and increase the versatility and reliability of their strategic command and controls systems.²⁷ Knowing these were the Soviet goals, we are placed in a better position to evaluate the present state of the Soviet strategic command and control network.

The Utility of the Penkovskiy Reports In the Seventies

Several intelligence researchers maintain that the evolution of Soviet strategic forces, combined with the inflow of technical and documentary evidence during the last few years, has converted the Penkovskiy papers into "just historical" documents, with no lasting relevance to the situation in the mid or late Seventies. Regarding documentary material, and aware of the "apples and oranges" situation, some believe that the Soviet statements at the Strategic Arms Limitations Talks (even though they are skewed by the multi-lateral arena in which they are voiced) have developed into a collection of evidence on Soviet strategic thinking more valuable than major parts of the Penkovskiy collection (prepared for a far different audience and not reflecting in all cases the agreed upon, prevailing doctrine). Other contributors to *Studies* have highlighted the kinds of detailed information we have received over the last ten years and can expect from technical collection systems in the Seventies. So, with the premise and prognosis of my colleagues that the IRONBARK will continue to be buried by a flood of high quality technical and documentary strategic information, I will conclude with a few words on the tremendous analytical mileage which has accrued from the Penkovskiy contribution.

²⁷ A "single centralized system of communication with wide-scale use of multichannel radio relay and wire links" was referred to in General Kurochkin's article, *op. cit.*

The ten-year-old IRONBARK information stands as one of the most valuable collections in the history of strategic intelligence.

The IRONBARK documents covered a period when the Soviets were preparing for a major revision in the three key areas touched on in this *Studies* article—Soviet military doctrine, organization, and weaponry. The documents were composed at a time when the last major revolution was taking place regarding Soviet perceptions on the nature of a future war, and on the type of weapons and command and control procedures needed to wage that hypothetical conflict.

This period represented a major watershed in the transformation of Soviet military thinking away from the Stalinist preference for massive conventional forces, to new patterns of thinking, calling for brand new forces equipped with highly sophisticated, modern weaponry.

Much of the revolutionary IRONBARK material grappled with concepts which the Soviets did not begin to implement until the mid or late Sixties. A lot of what has taken place in Soviet military doctrine in recent years has only been a footnote to the intense debates in the information provided by Colonel Penkovskiy. Thus, while the ten year old material has less value than it did when it was ten months old, its continuing utility as a checkpoint for our current research is clear.

In sum, it is probably going to require another revolution in Soviet military thinking to reverse the present situation, reduce the IRONBARK itself to footnotes, and relegate the Colonel's legacy to "just historical" documents.

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Inside Story

**THE CUBAN MISSILE CRISIS OF 1962:
PRESENTING THE PHOTOGRAPHIC
EVIDENCE ABROAD**

Sherman Kent

It was 0737 in the morning of Sunday 14 October 1962 when Major Richard Heyser began the crossing of Cuba in his U-2. He flew almost due north—on a course some 60 miles to the west of Havana—and passed over the northerly beaches six minutes later. In that brief timespan he took 928 pictures, which covered a swath 75 miles wide. The resolution of his best shots was a matter of three feet.

Once past the target, he headed for McCoy Air Force Base near Orlando, Florida. There the exposed film was transferred to special shipping containers, loaded into a courier aircraft, and flown with all deliberate speed to the Naval Photographic Interpretation Center at Suitland, Maryland. It was late in the day when the film arrived; from then on and through the night the Center developed the original negatives and began making duplicate positives—not the usual kind of photoprints on opaque paper, as we amateurs might think, but a special kind of print on clear acetate that the pro's could study over a light table.

The first of these duplicates reached the National Photographic Interpretation Center (NPIC) just before 1000 on the morning of 15 October. By 1600 that afternoon the photointerpreters (PI's) were almost certain that they had identified large surface-to-surface missiles; in another hour or so they were sure enough for Arthur Lundahl, the Director of NPIC, to pass the word to CIA Headquarters. Headquarters, in turn, reached McGeorge Bundy about 2100 that evening. It was his decision to give the President a night's rest and the PI's a night's more labor before putting the earth-shaking evidence before his chief.

SECRET

The President and his principal advisors were informed the next morning.¹ This left the question of what to do—a matter which was resolved after five days of debate and deliberation in favor of a “strict quarantine on all offensive military equipment under shipment to Cuba.” Once the President reached this basic decision, he had a myriad of second-line but still important decisions to make. Just to touch on one—and incidentally the one that triggered the subject of this essay—consider that word “quarantine.” The President used it to avoid the more provocative word “blockade,” but no matter what he called it, the other man was free to take grave offense. Neither would go down easily with the USSR. In fact it was possible that the quarantine and its enforcement would lead to that well-known series of actions and reactions so often cited in intelligence papers as the unintentioned stairway to general conflict. Though the odds favoring this progress of events were small, they were by no means negligible. Even if events stopped a long way short of the cataclysm, there was still room for a thundering crisis, the outcome of which would depend in significant measure upon the way in which our allies would respond—whether they would support us or back away.

During the seven days between the President’s learning of the Soviet’s emplacement of medium- and intermediate-range ballistic missiles in Cuba and his speech announcing it, a few score principal officers of the Executive Branch worked endlessly and in unpenetrated secrecy. Except for the President, the members of the so-called Ex Comm (the ad hoc executive committee of the NSC), and the top echelon of the intelligence community, few indeed of our fellow countrymen knew what was going on and why, and practically no one in the governments of our allies. Until the President was ready to act, the Russians must not know that we knew their secret, and, when we were ready to act, our allies should know our chosen course before our adversaries. It was to this end that the Ex Comm drafted for the President’s approval a time-table of consecutive actions which included the briefings of the chiefs of government of our principal allies.

¹ A good bit has been written on the subject of the missile crisis. The best full account is still Elie Abel, *The Missile Crisis* (Philadelphia and New York, 1966). Mr. Abel’s material comes in very large part from oral testimony—taken while events were still fresh in mind—from most of the major American policy officers and a few of the British. Robert Kennedy’s *Thirteen Days* (New York, 1969) is an important first-hand account.

At A hour of D day (a time which became 1900 EDST Monday 22 October) the President was to tell publicly what was wrong in Cuba and what the US government proposed to do about it. At about A minus 12, the British were to receive formal advance notice, about four hours later the French and the Germans, and later still the Canadians.² Our ambassadors were to call upon the chiefs of government, deliver personal letters from the President and a copy of the speech to be delivered that night, and make whatever oral comment was appropriate. Each of them was also to have copies of the air photos and (for the presentations to the British, French, Germans, and Canadians) an intelligence officer from CIA headquarters to brief and answer questions as necessary.

Of our ambassadors to the UK, France, the Federal Republic, and Canada, only Mr. Bruce was at his post in London. Mr. Dowling was not in Bonn; he was in Georgia on compassionate leave. Mr. Bohlen, our ambassador-designate to Paris, was on his way on a boat in mid-Atlantic,³ and Mr. Butterworth, the ambassador-designate to Ottawa, was not to assume his functions until after the New Year.

In Mr. Dowling's case there was a remedy, a speedy termination of his leave; as for Mr. Bohlen and Mr. Butterworth, there was no remedy but that of finding worthy substitutes. For the group heading for Europe there was to be a presidential aircraft (Air Force One) which would transport Mr. Dowling, Mr. Acheson (the substitute for Mr. Bohlen), the documents, the pictures and their CIA security courier Edward Enck, and the three CIA men to do the intelligence briefing. Chester Cooper, who had had a tour of duty in London, was to be with Mr. Bruce; R. Jack Smith (who was AD/CI at the time) was to go on to Bonn with Mr. Dowling; and I had the honor to be with Mr. Acheson. In place of the absent Mr. Butterworth, the President called from private life Mr. Livingston Merchant (who a few months earlier had resigned as our ambassador to Canada and left the Foreign Service). He and William Tidwell, his CIA intelligence briefer, made their separate ways to Ottawa.

There is some evidence that first planning in the Ex Comm did not envisage that the intelligence briefing of the chiefs of government

² The Turks and Italians were also to receive advance notice.

³ Between ambassadorial assignments Mr. Bohlen had been keeping Soviet matters under special scrutiny for the benefit of the President and Secretary of State. His appointment to Paris had come only shortly before the discovery of the missiles in Cuba. After this turn of events, President Kennedy was torn between keeping Bohlen at his side in Washington or releasing him to take up his duties in France. The result was some temporizing which led to Mr. Bohlen's late departure.

would take place simultaneously with the ambassadors' presentations of the case. Rather the technical intelligence colloquy was to take place on a service-to-service basis soon after the principals had met. I mention this to indicate that the Ex Comm did consider the intelligence aspects of the multi-national maneuver and came to attach a high importance to it.

Whether the Ex Comm worried about the credibility of photographic evidence (it was the only solid evidence there was) I do not know, but I do know that a few very important officers of the Agency did. Accordingly, Cooper, Smith, Tidwell, and I were urged to pay particular attention to the way in which our audiences responded to the photographs and to record these reactions in our memos for the record. We were also urged to make these memos as full and detailed as other demands on our time would permit.

Cooper and I did find the time to write up our experiences at length. Smith, who did not, spent some time last June (1971) giving me the benefit of his remembrance of the events almost nine years back. Tidwell wrote only a short memo, of which more later, since the magisterial Memorandum of Conversation which Mr. Merchant filed with the Department of State covered the subject with depth and thoroughness. In these communications there is much of interest to the intelligence calling. But let the memos speak for themselves.

First from a shortened version of Chester Cooper's "Memorandum for the Record" of 29 October 1962:

The Prime Minister

On Monday, 22 October [1230 London time] I accompanied the Ambassador to the Admiralty to assist him in briefing Mr. Macmillan on the situation in Cuba. The letter from the President had been sent to the Prime Minister's office earlier in the day. We delayed our session with the Prime Minister for half an hour, hoping to bring with us an advance draft of the President's message.

The Prime Minister was alone except for his Private Secretary. It was evident that the Prime Minister had some advance general knowledge of the developing situation in Cuba (as indeed he should have since we had briefed various members of the British intelligence community several days before in Washington). However, Mr. Macmillan obviously had no idea of the extent or precise nature of Soviet offensive capabilities in Cuba. His first reaction, which he addressed more to himself than to the Ambassador, was to the effect that the British people, who had been

living in the shadow of annihilation for the past many years, had somehow been able to live more or less normal lives and he felt that the Americans, now confronted with a similar situation would, after the initial shock, make a similar adjustment. "Life goes on somehow."

The Prime Minister was obviously aware that this might be misinterpreted, and went to considerable length to explain to the Ambassador that this was more of a philosophical commentary on human nature than any indication on his part that he was not sympathetic with the US position or shocked at the news.

After my recitation of the present Soviet offensive strength in Cuba, Mr. Macmillan said that, if the President were convinced that a meaningful offensive capability were present, "That was good enough for him." He did not spend more than a few seconds on the photographs. Although the Prime Minister did not develop this theme in my presence in detail, he did indicate that he felt that a blockade would be difficult to enforce and that the US would have problems in getting solid UN support. He also ruminated about whether it would not have been better to have confronted Khrushchev privately with our evidence and given him a private ultimatum.

Lord Home then joined the Prime Minister and the Ambassador for a discussion of policy matters and I was excused. I was quickly followed by the Private Secretary who stressed the necessity for making our evidence as convincing as possible to the British public. . . .

Members of the Shadow Cabinet

Cooper also briefed Hugh Gaitskell and George Brown of the British Shadow Cabinet. He, Ambassador Bruce, and two embassy officers met with them on Tuesday evening. Cooper told the story and showed the photographs. Gaitskell, who up until that time had feared that the President was confusing the issue of the Soviet buildup by making it appear that surface-to-air missiles were offensive weapons, confessed his earlier apprehensions and acknowledged that they were ill-founded. He was visibly shaken by the evidence of the long-range missiles.

He made much of the analogy between Cuba and Turkey and brushed aside most of the standard arguments about the difference between the two. However, he seemed much impressed with the fact that the Cuban missiles were outside the BMEWS sys-

SECRET

Missile Crisis

tem. He felt that this did, in fact, represent a change in the *status quo* and in the "balance of terror" question.

George Brown was concerned as to whether the United States had deployed more or fewer Jupiter missiles in Turkey than the Soviets were putting into Cuba and as to the Soviets' capability for early warning of the firing of these missiles. Cooper said he would try to get enlightenment for Brown on both matters. Brown's point, and one to which Gaitskell assented, was that if the United States did indeed have fewer missiles in Turkey than the Soviets would have in Cuba and if the Soviets did have an early warning capability, the argument about the equivalence of the Turkish and Cuban bases would be weakened.

Gaitskell said that he had been with the Prime Minister just prior to our discussion and that the Prime Minister expressed annoyance about the lack of advance knowledge of US actions. I pointed out to Gaitskell in fairly strong terms that there were two aspects to the question of advanced knowledge: one was the developing situation in Cuba and the other was US intentions with respect to Cuba. In connection with the former, I told Gaitskell that we had occasion to discuss Cuba with several important people in the British intelligence community who happened to be in Washington during the week of 15 October, and that several of them had been given a formal briefing on Friday, 19 October. We could only assume that they notified their government of the developing situation in Cuba. With respect to US intentions, I noted that we had hoped to get an advanced copy of the President's statement to the Prime Minister 12 hours before the broadcast, but that this was not possible because the President himself had not decided on the precise language of his statement until fairly late in the day. . . . This was unfortunate, but in the nature of the circumstances, was all that could have been done. . . .

The British Intelligence Community

Ambassador Bruce and Cooper agreed that it would be wise to give the briefing to the British Joint Intelligence Committee, and [] [] got in touch with Sir Hugh Stephenson (the JIC Chairman), who set the time for 1000 Tuesday morning.

There was no evident skepticism of the validity of our evidence, but it was clear that the Air Ministry was anxious to get the photo take for analysis by their own PI's (a team of Air

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25X1

Ministry officers was provided an opportunity for closer examination of the photos later in the afternoon). There was, naturally, considerable speculation as to Soviet motives. To the extent that there was any consensus in the JIC, it was very much along the line propounded by Sir Dick [White] the previous evening... [Namely: that the Soviet aim was to confront the President late in November with a *fait accompli* in Cuba, a vantage point from which Khrushchev could bargain for a definitive settlement of the Berlin question and the question of US foreign bases in general.]

The Press

Because of the adverse or skeptical press reaction to US claims that the USSR had offensive missile bases in Cuba, the Ambassador and the Public Affairs Officer were anxious to have a press briefing as early as possible on Tuesday. At 5:00 p.m., Tuesday, a press conference was held for representatives of all the dailies, BBC, and ITV. The conference was chaired by Evans, the PAO, and attended by Minister Jones and myself. After indicating the ground rule ("backgrounder," no attribution, etc.), Mr. Evans briefly described the situation in Cuba and indicated that I, a Department of Defense consultant, would show the photographs and explain some of the background of the build-up. I did this, guided by the instructions I had received from Washington. The questions which followed were friendly and I had the feeling after the conference was over (it lasted about an hour) that the press representatives were genuinely convinced of the US case. I released the photographs, without the identification of their precise locations, to the press. (A fuller description of the circumstances of the release of the photographs is attached at Annex.)

Later Tuesday evening both the BBC and ITV had major programs dealing with the Cuban crises. The BBC broadcast the Foreign Minister's speech [which indicated strong support for the US position and a condemnation of the Soviet Union] and documented his remarks by the use of the photographs which I had supplied to the BBC.

*Annex—Release of Pictures to Press*⁴

The following consideration influenced my decision to release the photographs of the Soviet build-up to the British press:

Immediately following my briefing of the Prime Minister, Philip De Zuluetta, the Prime Minister's Private Secretary, expressed serious concern about the reception any strong Government statement would have in the absence of incontrovertible proof of the missile build-up.

On Tuesday morning (23 October) the British press was almost universally skeptical of the President's claim that the USSR had established offensive bases in Cuba. References were made to the forthcoming election and to the "failures" of past US intelligence efforts re Cuba.

On Tuesday morning, also, there was some uncertainty as to whether, at the DOD press conference following the President's broadcast, the press was *shown* the pictures or whether it was given the pictures.

After my briefing of key Embassy officers at noon on Tuesday, the PAO and the Minister urged the necessity of providing the British press with a clear and authoritative story on the build-up. I was asked to do this (the Ambassador subsequently expressed his own desire that this be done) and was also urged to show the pictures on a special BBC television program scheduled

⁴ Elie Abel (*op. cit.*, p. 138) has the following comment on the release of the pictures to the British press. The last line is in conflict with Cooper's testimony, as well as the fact that the London TV of Tuesday night showed the photographs, and the London press of Wednesday morning was loaded with them.

Sir David and Lady Ormsby Gore had received a pre-crisis invitation to join the Kennedys that evening [Tuesday 23 October] for a private dinner-dance. The dance, of course, had been canceled. But Mrs. Kennedy invited the Ormsby Gores to bring to dinner some Embassy guests who had arrived from New York too late to be forewarned of the cancellation. The British Ambassador found the President in no mood for social chatter. The two went off together for a talk about the day's events and what the morrow might bring. Sir David was worried about the skeptical British press reaction. Even the President's friend, Hugh Gaitskell, leader of the Labor Opposition, had talked of "so-called missiles" in Cuba. The Ambassador felt it was most important that the missile-site photographs be published, especially those that would most readily persuade laymen that the Soviet missiles were indeed installed. The President sent for the photographs and together the two re-examined them closely. Ormsby Gore's plea, reinforcing the direct appeal of Ambassador Bruce in London, helped the President decide to publish the pictures next day.

SECRET

for Tuesday night. I refused to appear on television, agreed to participate (but not sponsor a press briefing) and requested Headquarters' permission to have the pictures shown on BBC.

I received permission to have the pictures shown on television on the basis of the Ambassador's urgent request. The localities of the sites were to be removed and the press and the television audiences were to be told that these were *typical* sites but were not to be informed of the *number* of sites.

After consultation with Embassy officials, I agreed that since the pictures were going to be shown on television (it subsequently developed that ITV as well as BBC was going to have a special Cuba program) we could release sanitized versions of the photographs to the press for publication Wednesday morning.

I informed Headquarters at my first opportunity (which was after the Gaitskell briefing at 2100) of this release.

Sometime after midnight I was in telephone communication with the White House (Forrestal) and explained briefly the circumstances of release.

R. J. Smith in Bonn

Air Force One—which had left Cooper at Greenham Common Air Force Base in the United Kingdom and had left Mr. Acheson and me at Evreux, an air base in France used by the USAF—flew on to Cologne in the Federal Republic and disembarked Ambassador Dowling, Edward Enck the courier, and R. J. Smith. The time was well on towards Monday's dawn (22 October).

The meeting with the Chancellor, who had been electioneering in Hanover all day, did not take place until 1900. Herr Adenauer received Dowling and Smith in the Chancellor's official residence. He had provided the interpreter. As Smith remembers it, Ambassador Dowling gave the Chancellor the personal letter from President Kennedy, and with the reason for the meeting clear, introduced Mr. Smith of the CIA who was to show the evidence for the President's concern. The Chancellor's first response was characteristic: it showed perhaps his amused annoyance at the Gehlen organization's habit of using pseudonyms even within the official family and certainly something more than a trace of his legendary suspicion of everything. "Are you sure your name is Smith? Perhaps you have two names,"

SECRET

27

he said, just by way of getting things straight at the start.⁵ Unruffled, Smith said that his name was really Smith and began the briefing with the photographs, which were contained in an outsized carrying case. The Chancellor asked him if he slept in it, but Smith pushed on. They were seated at a low table, Smith and Herr Adenauer side by side, with Ambassador Dowling across. As the dramatically illustrated story unfolded, Adenauer was an attentive listener. Seemingly concerned to indicate his general familiarity with the sort of military intelligence being laid before him, he asked questions such as one regarding the state of readiness of the surface-to-surface missiles. (As it came through the interpreter, it was to the effect "were they warm or cold?")

There was no question but that he was impressed with the evidence. Far from showing any incredulity, he indicated that he was not at all surprised to hear of these Soviet doings. His tone was one of "this is what one must expect of them." Nor did he leave any doubt in Ambassador Dowling's mind that he would support the President's adopted course of action. "You may assure your President that I will be useful" is the way Smith remembers his reassuring comment.⁶

⁵ Some three weeks after the dialogue in Bonn the Chancellor and key members of the German government made a state visit to Washington. As R. J. Smith recounts the incident,

the White House decided that one of the features of the program for the Germans should be a briefing which would detail for Chancellor Adenauer precisely how the Russian missiles were removed from Cuba. Smith was asked to perform this chore, the venue for which was the Cabinet Room in full panoply. The German Chancellor sat on one side of the table, flanked with his defense and foreign ministers; President Kennedy sat across from him, flanked by Secretaries Rusk and McNamara. Smith sat behind the Chancellor and, on signal from the President to begin the briefing, stood up and placed the first briefing board on the table before Chancellor Adenauer. As he did so, he said, "Chancellor Adenauer, I am Mr. Smith." Adenauer looked up, his ancient face impassive, and said, "Immer," which the translator rendered as "still." This cracked Smith up and the Chancellor chuckled, whereupon Smith felt obliged to explain the joke to the distinguished group. The President smiled frostily and urged Smith to continue.

⁶ High officers of our government thought that there would be no harm in reinforcing the Chancellor's decision to be "helpful." Knowing of his warm personal friendship with Mr. Acheson and his high respect for General de Gaulle, they asked Mr. Acheson to pass through Bonn on his way home and discuss the situation anew and tell of de Gaulle's reaction to the President's chosen course of action. This is worth a footnote if for no other reason than to set a woefully confused chronology straight. Washington sent a night action cable to Mr. Acheson Monday night 22 October; it reached his attention in the small hours of Tuesday (23 October). He went to Bonn during that very day, and with Mr. Dowling saw the Chancellor for two hours late in the afternoon. Needless to say, the mission was a great success.

Neither the official memorandum of conversation, nor Mr. Acheson's memory of the interview - as reported in C. L. Sulzberger, *The Last of the Giants* (New York, 1970), p. 931, mentions the photographic evidence.

Missile Crisis

SECRET

Paris—From My Memorandum ⁷

Mr. Acheson and I with Mr. Dowling and Smith flew on from our UK stop to Evreux where we were met by Cecil Lyon, the chargé in Paris, Ambassador to NATO Mr. Finletter, and Edward Ryan, [redacted] and an armed courier. It was then about 0130 local time. Mr. Acheson, with Messrs. Lyon and Finletter, proceeded directly to Lyon's residence. Ryan, the courier, and I went to the Embassy to put the materials in the vault.

About noon (Monday 22 October) there was an assembly at Mr. Lyon's house of high-ranking officers from the Embassy, from our delegation to the North Atlantic Council, and from among our military men in France. I gave the intelligence briefing using the photographs.

Meeting with President de Gaulle at the Elysée Palace

At 4:40 Laughlin Campbell, [redacted] and I again appeared at Mr. Lyon's residence where two modest automobiles from the Elysée Palace awaited us. Mr. Acheson and Mr. Lyon, with a presidential escort officer, took one; Campbell and I (with the photographs) the other. We entered the Elysée through the regular entrance on the Rue du Faubourg St. Honoré. Once within the first courtyard we followed a tortuous course from court to inner court to inner court and were finally brought up to an unprepossessing doorway under guard.⁸ We proceeded down small

⁷ The memo was dictated on 28 and 29 October 1962 and typed up a couple of weeks later.

⁸ In short, the French neglected nothing in assuring that Mr. Acheson—a recognizable man in almost any corner of the world—would not be recognized by a casual bystander. His meeting with the President had to be kept secret until "A" hour which would have been about midnight in Paris.

Mr. Acheson's well-known powers as a raconteur were stimulated by the route we took; I kept getting playbacks from third parties which became harder and harder to recognize. The penultimate version occurs in C. L. Sulzberger's book, already cited, p. 930. He says he got it from Paul Nitze, who said he got it from Mr. Acheson, and it involved "Acheson [being] smuggled into de Gaulle's office by an underground tunnel from across the street." Apparently so high was the credibility of this unlikely story that ace newsman Sulzberger who had lived in Paris some twenty years and knew the environs of the Elysée as well as those of the White House swallowed that secret tunnel without even a footnote.

If perchance the reader happens to be the studious sort who checks references, he may be disturbed to read Sulzberger's two sentences following the one about the tunnel. They go: "Acheson went in alone except for the Elysée interpreter. Not even Sherman (See footnote on following page.)"

SECRET

29

25X1

25X1

corridors, up small corridors, up small stairways, through more corridors and stairways until we finally arrived at a large room adjoining the President's private office. My guess is that if this were not the Cabinet Room it served some such purpose. There was a very large oval table which would have seated perhaps 20 people. The four Americans and the escort officer were here joined by another Frenchman who turned out to be an emergency interpreter. After a few minutes' wait—which would have been a minute or so after—Mr. Lyon and Mr. Acheson were ushered into the General's office. Mr. Lyon has reported by cable on what took place. Campbell and I waited for perhaps 20 minutes; then the two of us were invited in. After I had completed the first draft of this memorandum, I saw Mr. Acheson, who told me the following about his discussion with de Gaulle. When he had conveyed his message he told the General that there was an intelligence officer waiting outside to brief him on the evidence. General de Gaulle's response was that he needed no such evidence; he was satisfied with Mr. Acheson's account; after all, President Kennedy obviously would not have sent a man of Mr. Acheson's eminence to give him misinformation. Mr. Acheson said he thought the General would be interested.⁹

The presidential presence was awesome. I was prepared for the height but not for the bulk. At the moment of shock he seemed to be about twice the size of normal men. His eyes too were somewhat unnerving, shielded as they were behind the thick lenses made necessary by the removal of cataracts. I can recall a feeling of

(Footnote 8 continued)

Kent was allowed." May I assert that this is another error (either Nitze's or Sulzberger's—certainly not Mr. Acheson's); that I did go in; and that the memo for the record which you are now reading is not a self-serving fabrication.

The peak occurs in Kenneth Harris' write-up of an interview with Mr. Acheson (*Life*, 23 July 1971, p. 52). The operative passage runs thus:

"So he [General de Gaulle] sent two small French cars, and we drove down into the garage basement of the palace and were led up through the basement past the wine closets. There were all sorts of steel doors with little eyelet holes in them, and people would look through and give a password. I had a very amusing CIA friend along with the photographs. Halfway through this, he said: 'D'Artagnan, is that saber loose in the scabbard?' And I said, 'Aye, Porthos.' And he said: 'Be on the alert. The Cardinal's men may be waiting.' Finally, we were brought up into the cabinet room, where an old friend of ours, whose name was Lebel, greeted us. . . ."

⁹ Elie Abel (*op. cit.*, p. 112) has a slightly different version whose primary source was almost certainly Mr. Acheson. It goes: "Then Acheson offered to show the photographs. De Gaulle swept them aside. 'A great government such as yours does not act without evidence.'"

SECRET

despair that came with the realization that the evidence which we were about to present was wholly visual evidence. (As it turned out my fears were groundless.)

As Campbell and I entered, he rose from his small desk—not much larger than our photographs—and gravely shook hands. He gave me the nod to begin.

Campbell handed me the large photograph of the map of Cuba which I put before the General. Still standing, he bent over it as I began to talk about the defensive phase. I mentioned first the arrival of large numbers of Soviet personnel, quantities of transportation, communications and electronic equipment. Next I came to the SAM's, pointing out the SAM symbol on the map. To my great comfort he at once identified the symbol and with his own finger pointed to a number of the others. I then showed him the photograph of a SAM site which he seemingly identified at once. I passed on the photograph of Santa Clara airfield, pointing out the MIG-21's. There was a reading glass which he picked up and put into the proper position, looked at the swept-wing aircraft, and indicated that this was a remarkable photograph. I quickly showed him the Komars and the surface-to-surface cruise missiles. The word "cruise" was the only technical term [which the interpreter] did not cope with instantly. He snapped a finger in annoyance and then realized that salvation lay on the graphic itself for this photograph had as an inset a diagram of the little winged missile.

I then [took up] the offensive phase, showed him the IL-28 crates being carried as deck cargo, showed him the San Julian airfield, pointed out the crates, the assembled IL-28 and the two uncrated fuselages. Again he picked up the reading glass and examined the picture carefully. I then went to MR-1 [Medium Range Ballistic Missile site called number one] at San Cristobal and the MR site at Sagua la Grande. Next came the IR [Intermediate Range Ballistic Missile] site at Guanajay. Coming back to the map again I totted up the number of confirmed sites, the number of probables plus the possibles at Remedios. I then went over our estimates of degree of readiness and gave him a worst case estimate as of the moment of speaking and another worst case as of early 1963. I discussed briefly nuclear warheads, the fact that we could not positively identify any but noted the high degree of probability that they were in Cuba and the highly suspicious storage areas being readied. I called his attention to the storage site at Guanajay. I noted our estimate of the yield

SECRET

31

of these warheads as two to three megatons for the MR's and three to five for the IR's. I closed with a reminder that as of early 1963 our worst case estimate could augment present Soviet first strike capabilities with missiles by some 50%.

Not once in the course of my briefing was there any hint of incredulity on the part of the General. If he was not perfectly satisfied that the pictures were scenes from Cuba and the weapons those which I asserted them to be, he gave me no inkling of doubt. Furthermore, if he had expressed doubts to Mr. Acheson and Mr. Lyon [after Campbell and I had left the room], I am sure they would have reported it. . . .

Meeting with the North Atlantic Council

During the day [] received the USIB-approved briefing note to be read to the NAC. Mr. Acheson got a copy and had read it. Meanwhile we hopefully awaited the full text of the speech which the President would deliver at midnight local time. The NAC meeting was scheduled for 10 PM. By the time I had to leave the Embassy only Part 1 of 4 had been received.

The Acting Chairman [of the North Atlantic Council] was Colonna of Italy. He introduced Mr. Acheson as needing no introduction to the group, noting that he was on a special mission for the President of the US. . . . Mr. Acheson began by briefly discussing the nature of [his] mission, read some excerpts from the portion of the President's speech that he had at hand and then indicating that he wished to read a statement, introduced me as Assistant Director, CIA, who was there to answer questions when he finished reading his prepared text. He then read the text. There were a few questions on the estimated performance of the MR's and IR's, a general question about their state of readiness, and after the meeting an aide of the German permanent representative followed us to Mr. Finletter's office to ask the estimated yield of the warheads. . . .

As per USIB instruction, I used no graphics whatever with one exception. I passed around an unclassified map. It showed what portions of North, Central, and South America the MR's and IR's could reach. Among the metropolitan areas of the US under the gun were New York, Philadelphia, the District of Columbia, Chicago, San Francisco, etc., and in this distinguished company, one found Oxford, Mississippi. It had been spotted on the map by a roguish CIA man to show Robert Kennedy, who had wondered out loud if Oxford (then

much on Mr. Kennedy's mind as the place where bitter racial controversy had enveloped the state's university campus) was within range. I never recovered the map and have often wondered how some analyst of one of the NATO intelligence services explained how Oxford, Mississippi came to be listed among the great metropolises. From the council there were no questions about the sources of our information and no questions whatever to indicate any doubt that Mr. Acheson's story was not in fact a true story. The meeting adjourned just in time for the members to hear the President's speech which began at midnight Paris time.

Next day, Tuesday 23 October, under instructions from Headquarters, Campbell and I made the trip to the SDECE (the French secret intelligence service) and briefed the Director, General Jacquier, and a few of his principal officers. Our experience was much like Cooper's with the British JIC. There was great interest and no hint of doubt with respect to the genuineness of the photographs. There were no questions even about the pictures of the IR site and the surface-to-surface cruise missiles, the identification of which required a lot more faith than simple good eyesight.

The Briefing of the French Press

We returned to the Embassy by about 3:30 to find that USIB had authorized the briefing of the French Press, had supplied a briefing text and instructions with respect to the use of the graphics. John Mowinkle, the Public Affairs Officer, under instruction from the chargé called the press conference for 10:30 the next morning, Wednesday, 24 October. Mowinkle himself was not to do the briefing but was to entrust the job to an assistant who had a greater familiarity with military matters than Mowinkle himself. It was further decided, and this was entirely satisfactory with me, that I would make no appearance before the newspaper men but would confine my activities to reading the assistant in on the subject and making sure that the graphics were keyed into his spoken statement in fool-proof manner.

It will be recalled that USIB's instructions re this briefing were as follows: the briefer was to follow a USIB-approved text which was at hand. The briefer was to refer to certain stipulated graphics. The number was perhaps no more than half of the total number . . . in the kit. All place names, locational data, and numbers were to be removed from the graphics. Members of the press could study the graphics but could not reproduce them. Graphics were not to be allowed outside the Embassy building.

In the light of these instructions I personally selected the graphics as indicated, cut off the headings at the top of the prints, removed the little box in each photograph which contained the orientation map of Cuba with its designating arrow, the classification, and where indicated obliterated locational information and numbers.

Two graphics had to be improvised. These were a map of the Western Hemisphere showing approximate ranges of the MR's and IR's (the map I had not recovered from the NAC) and a map of Cuba showing what Cuban air space was under protection of the SAM's.

I went over the briefing notes carefully, patched up a needless obscurity in one paragraph and keyed the graphics to the text.

24 October

With my breakfast arrived a copy of the International edition of the *New York Herald Tribune*. To my very considerable surprise, smack in the middle of the top half of the front page and three or four columns wide, was the photograph of the SAM site referred to in the briefing note. A few minutes later, upon arrival at the Embassy, I was informed that the whole kit of photographs had been released to the British press the night before, that they were appearing in the London papers this morning and indeed had appeared on two British TV programs last night. A few minutes later I was shown two Paris morning papers, one of which carried the SAM site above mentioned, the other, the picture of the SAM support area which I had not been authorized even to show to the French Press.

I conferred with [redacted] as to the best procedure and we agreed that I should call Washington for permission to release reproductions of the graphics which were to be shown to the French Press at 10:30 this morning. There was some difficulty in getting through to Washington and it was not until about 9:50 AM local time that I reached the CIA Watch Office. Ten or fifteen minutes later they called back authorizing the release if satisfactory to the chargé. He agreed to the release of four pictures. An Embassy pressman accordingly scotch-taped the four pictures in question (MR-1, IR-1, the IL-28's at San Julian and the MIG's at Santa Clara) to the floor and photographed them. Enlarged prints of these shots went to the French press.

Briefing of André Fontaine of Le Monde

André Fontaine, one of the important feature writers of *Le Monde* (France's leading afternoon paper)

had had time to hear and study the President's speech of midnight, 22 October, and to write an unsympathetic front page column on US policy toward Cuba.¹⁰ His articles are usually signed; this one was not. The second paragraph banged into the credibility of the evidence. "One would like to be sure of the accuracy of the information" upon which the President has acted. "But unhappily, experience shows that the American intelligence services sometimes make mistakes." This set the tone. Later on he again obliquely challenged the evidence in the fourth paragraph which contains the sentence, "If the Russians have not really delivered and do not have the intention of delivering..." In short, M. Fontaine was from Missouri and had rather persuasively set forth his doubts about the evidence and his views—totally unsympathetic to the US—for the edification of France's best educated and probably most conservative reading elite.

Mowinkle who knew Fontaine well was most anxious that I see [him] and go over the script and graphics with him. The [chargé] agreed. I was presented to Fontaine under a pseudo as a Department of Defense civilian temporarily in Paris. Accordingly I gave him the word.

I began by calling his attention to the fact that neither he nor I were expert enough in the PI's art to identify the terrain as Cuban or some of the weapons and sites as to what they really were. I told him that if he thought that I was about to embark upon a snow job with fabricated graphics I was prepared to call it off right there; that if he were willing to take on faith the fact this countryside was Cuban and the weapons in fact were what I said they were, we would proceed. Interestingly, he then said, "No. I am prepared to believe you because Castro himself in a speech of yesterday proclaimed that American aircraft had been violating Cuban air space. This is good enough evidence for me to believe that you have been overflying Cuba and photographing it from the air." With these formalities over, I ran through the exercise with the sanitized pictures. Almost the only question he asked was the altitude from which the pictures were taken. He presumed that this was secret. I indicated that it was indeed secret and let it go at that. I left Paris before *Le Monde*, dated 26 October, was printed.

¹⁰ This appeared in *Le Monde* of Tuesday afternoon 23 October. For reasons best known to the publisher, the paper is dated one day ahead, thus this issue of *Le Monde* is one bearing the date 24 October 1962.

In this issue M. Fontaine grudgingly acknowledged that the missiles were in fact in Cuba, citing that both the British government and his colleagues of the British press believed the photographs and furthermore Castro himself had lent credence to the matter by denouncing American photo reconnaissance flights as violations of Cuban air space.

William Tidwell in Ottawa

Livingston Merchant, President Kennedy's special emissary to the Canadian government, William Tidwell, the Agency officer told off to do the intelligence briefing (with the photographs), along with our chargé d'affaires, and [redacted] met at 1700 (22 October) with Prime Minister Diefenbaker and his Secretaries for External Affairs and Defense, Messrs. Green and Harkness. Mr. Merchant described the situation in Cuba and handed the Prime Minister the text of the President's speech (to be delivered in two hours). Mr. Diefenbaker read it rapidly and passed it to these two cabinet colleagues. He then asked Mr. Merchant to summarize the main points, which Mr. Merchant did, and then he read the whole speech aloud. Apparently two matters bothered the Prime Minister. One was the use of two words "dishonest" and "dishonorable" which in the draft speech were applied to Gromyko's statements to the President when the two had met on 18 October; the other was the credibility of the evidence of the missiles in Cuba. He made an abbreviated note to remind himself of the two points which read "1. *Dishonest and dishonorable*/withdrawal of/Ambassador" [and] "2. How to present proof of/threat to/UN or OAS."¹¹

The first of these he straightway took up and with repetitions and some vehemence. They were unnecessary and provocative words; they might result, for example, in the Soviet Union's withdrawal of its ambassador in the United States, he thought. He hoped that they would not be used. The second he seems not to have got around to. Most likely the reason was his viewing of the photographs which

¹¹ Mr. Tidwell wrote a memo to the Curator, Historical Intelligence Collection which reads in part:

2. During the briefing session Mr. Diefenbaker made several notes as reminders to himself. At the conclusion of the briefing he tore up the notes and threw them on the floor. In the course of my security check of the room after the briefing, I picked up the fragments of his notes. They are forwarded with this memorandum for retention in the Historical Intelligence Collection.

The notes read as I have rendered them above.

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Tidwell presented.¹² The three Canadians were clearly impressed and asked a range of questions which, far from indicating incredulity, were of the sort which showed a ready acceptance of the evidence. Indeed it seemed to the Americans that the photographs themselves may have had much to do with a lightening of the Prime Minister's mood, which at the beginning had been that of a worried and harassed man. At the end, he left Mr. Merchant with the impression that he would support the President and he complimented Tidwell on the quality of the intelligence briefing.

Tidwell stayed behind to give the briefing to half a dozen of the next most important officers of the Canadian government involved in the foreign affairs of the country. Like similar groups in other friendly states, they believed what they saw and they were impressed.

¹² It may be, as Mr. Tidwell himself suggests in the memo, that Mr. Diefenbaker's self-addressed query about "how to present proof of the threat to the UN or OAS" derived from his half-formulated thought to ask a group from among the eight unaligned members of the 18-nation disarmament committee to make an on-site inspection and to furnish "a full and complete understanding of what is taking place in Cuba." This thought, which he quite fully developed only a few minutes later to the Canadian House of Commons, he had not even hinted to the Americans. They noted that he had not said that he would support the President in the chosen course, but they were very considerably surprised at his presentation to the Commons.

It is perhaps noteworthy that his remarks to the Commons contained no mention of any special audience, UN, OAS, or other. Nor did his remarks to the Commons next day, when he did a little reconsidering:

In connection with the suggestion I made last evening that a group of nations might be given the opportunity of making an on-site inspection in Cuba, lest there be any doubt about my meaning in that connection, I was not, of course, casting any doubts on the facts of the situation as outlined by the President of the United States in his television address. The government had been informed of and it believes that there is ample evidence weapons have been constructed in Cuba and exist in sufficient quantities to threaten the security of this hemisphere.

The purpose I had in mind in suggesting a United Nations [his remarks of the previous day made no specific mention of the UN. The 18-nation disarmament committee did, however, have an association with the UN] on-site inspection was to be ready to put in motion steps which could be taken in the United Nations general assembly in the event of a Soviet veto, or if the Soviet Union denies the existence in Cuba of offensive ballistic missile bases. (Canada, Parliament, *House of Commons Debates*, 22 and 23 October 1962, pp. 805-6 and 821.)

In the light of these utterances, it seems to me that Diefenbaker's note about convincing the UN and OAS more likely derived from a certain incredulosity which possessed him before a look at the photographs dissipated it.

The Credibility of Photographic Evidence

As a source of information, overhead photography has always won high marks. From the nineteenth century, when daring men took cameras aloft in balloons, to our day with its more sophisticated approach, all who have worked at the intelligence calling or used its findings have recognized the extraordinary virtues of photographs taken from the air. The reception of the U-2's pictures of Cuba in 1962 was proof of more of the same.

Any viewer of an air photo is likely to bring with him some associative apparatus. For example, he has seen airfields from above and he can tell the difference between a picture of an airfield and one of a freight yard; he may even be able to tell a parked transport airplane from a puddle jumper. Some of the non-PI viewers of the Cuban pictures had had a fairly rich experience with, say, air photos of Soviet installations in East Germany and when they saw small aircraft known to be Soviet models on Santa Clara airfield in Cuba, they could tell the difference between the MIG-17's and the delta-wing MIG-21's. When they saw a bit of the Cuban landscape marked off in the design of a perfect six-pointed star, they instantly recognized the unmistakable signature of the Soviet SAM—the second-generation surface-to-air missile. All viewers, however, took on faith or on the say-so of the purveyors that the pictures were what they claimed to be: scenes from Cuba taken a few days past.

When it came to photos of less obvious things than the aircraft and the SAM's all viewers but those indispensable middlemen, the photo-interpreters, had to take virtually everything on faith. In the big glossy prints of the surface-to-surface missile sites, the privileged but nonetheless amateur viewer could discern a number of man-made objects—some looked like long cylindrical tanks, some like oil trucks. He could also see bits of equipment parked in or about what "appeared to be no more than the clearing of a field for a farm or the basement of a house."¹³ More than this even the witness who could tell one MIG from another could not possibly tell.

Of course, the PI could and did. To begin with, what he looked at were better pictures. He had the duplicate positives printed on clear acetate that carried what the camera lens had seen, minimally degraded by the processing. More, he could arrange these prints in stereoscopic pairs on a light table and study them in three dimensions through a multi-power stereoscopic viewer. When he found something whose exact dimensions he had to know, he could turn to sensitive and

¹³ Quoted in exactly this context from Robert Kennedy, *Thirteen Days*, p. 24.

complicated measuring devices and get the answer in feet and fractions of feet.

Much more important than all of these was his experience. At a glance he would know that some of the objects resembling commonplace things of everyday life just could not be what they seemed. Such objects would never occur in these numbers and in this particular constellation of physical surroundings. They had to be something else. He might remember a similar puzzle in other photographs of another time and place and the way it had been cracked. He might recall the process by which he had reduced several competing hypotheses to two and how the final solution had come, not with more photography, but with a wholly different sort of information. With this, he had made a confident estimate that the object in question was a large surface-to-surface missile with its carrier. If such an experience had not been his, perhaps one of his colleagues had a recollection that would help. And if not, he could begin from scratch, summoning from the vitals of a computer the vast wealth of its electronic memory. If in the course of the history of his organization's work it had met cognate puzzles, everything about them could be speedily put on the table.

With the deployment of the critical tools, the PI's moved from hypothesis to tentative estimate, and, as they became more confident that what they thought they might be seeing was indeed an all-but-dead certainty, they were ready to take their judgment to their chief, Arthur Lundahl. When they convinced him and he convinced himself, and when he could answer President Kennedy's question "Are you sure that these are offensive missile sites?" with "Mr. President, I am as sure of this as a photointerpreter can be sure of anything . . .," and when the President, reminded of the accuracy of past interpretations, accepted this one, that was it.

By their actions Mr. Macmillan and General de Gaulle underscored this fact. As Cooper noted, Macmillan "did not spend more than a few seconds on the photographs;" and except as Mr. Acheson urged him to have a look, General de Gaulle would not have given the photographs even the "few seconds." Their credibility was not at issue: what was was that of Ambassador Bruce and Mr. Acheson and especially that of the man who had sent them, President Kennedy himself. Obviously this elite audience did not think that the President was playing games with them.

From what we know of the reaction of civil officials a notch or two below the chiefs of government, they were much the same as those of their masters. For much the same reasons Ormsby Gore (the British Ambassador in Washington), Lord Home, and Sir Burke Trend,

Gaitskell, and Brown, and others in London, and Messrs. Green and Harkness in Ottawa accepted the photographs at face. We know nothing of the reactions of the officials in Paris and Bonn to whom de Gaulle and Adenauer confided.

With highly placed foreign intelligence professionals the case was much the same. They brought to the pictures a certain amount of critical expertise (but still well below that of a card-carrying PI), and practically to a man they were both amazed and convinced. Sir Kenneth Strong, long Britain's first intelligence officer, General Jacquier, Director of the French SDECE, Mr. McCardle, the Chairman of the Canadian Joint Intelligence Committee—none of them queried the validity of what they looked at. As Cooper notes in his memo, intelligence officers of the British Air Ministry apparently wanted copies of the photos for their own PI's to examine. But, Cooper assures us, there was nothing necessarily angular about their request.

How different the response of those who spoke for others. Mr. Zuluetta, the private secretary of Mr. Macmillan, according to Cooper's testimony, was worried about how a statement of the British government in support of the American decision would go down "without incontrovertible proof of the missile build-up." Next morning the skeptical tone of the British press showed him to have been on the right track. After the release of the first batch of pictures to British newsmen, an important British intelligence officer besought American officials in London to release more pictures and more information about the first ones. He said he was confronted by a great skepticism on the part of the press which was muttering about possible forgeries and expressing doubt that the terrain was in fact Cuba. He felt that the US government should release precise information about the location of the missiles and show a photo of a missile even if it were under canvas. All of this to make the case credible.

The Public Affairs officer in our embassy in Paris was worried about the French press and had very much in mind those snide sentences that André Fontaine had written in *Le Monde*. Mr. Diefenbaker seemed to have been concerned about how proof of the missiles could be demonstrated to the "world."¹⁴

How much beseeching the press did in its own behalf and how much in behalf of the "world," is another story. The press usually beseeches

¹⁴The operative sentence in Mr. Diefenbaker's remarks of 22 October was: "As to the presence of these offensive weapons, the only sure way that the world can secure the facts would be through an independent inspection." (Canada, Parliament, *House of Commons Debates*, p. 806.)

most eloquently when it senses good front-page copy, and there could be no doubt about the news appeal of this story.

The difference between what public relations men asked in behalf of the press and what the press asked in behalf of its readership—the difference between this and what it got, let alone what it gave—is of course well-nigh incalculable. In the first place, the very best prints of the most important installations in Cuba (those which chronicled the presence of the long-range surface-to-surface missiles) conveyed next to nothing in themselves. If you were to use a powerful reading glass you might be sure that you perceived some things common to your range of normal experience (the context might offer some passing difficulty, but only if you thought about it), but you would have no valid appreciation of their size, let alone their ominous function. Who, for example among the uninitiated, could have identified a thing resembling a big tent as the air-conditioned structure necessary for the complicated check-out of the missiles?

Such being the case, what do you think of the chances of the British subject who first got his information from his television set, a reproductive process which had robbed the original glossy prints of at least half their definition? Where do you rate the chances of the still less fortunate Frenchman? He was introduced to the Soviet secrets in Cuba via some half-tones in his morning paper. If you had made a half-tone from the original negative, the loss of definition would probably be as severe as that via TV. Still the Frenchman had no such luck. His was the opportunity to look at half-tones made from enlargements of 35 mm shots of the glossy prints. The amateur photographer who took the shots probably used a good camera with proper lens and film, but he took them in the natural light that filtered through an embassy window, and he did not use a tripod. In these circumstances the man who saw the pictures in next morning's *Figaro*, even if he were the country's leading photointerpreter, might have had trouble telling whether the camera had been pointed down at Cuba from a high-flying aircraft or pointed up a soundly-positioned proctoscope.

No one can ever know how many of the people whose acquaintance with the Cuban pictures was limited to television and press reproductions felt that they were being had. The one thing we do know is that if there were any such people, there were not enough of them to cause the slightest political ripple. All over the world the great majority of people who knew and cared about such things must have looked at the appallingly deficient copies of the original pictures and concluded that their chiefs of government had acted on the basis of incontrovert-

ible evidence. Those who disagreed with the course of action which the US had adopted, did so because of the risks which it involved, not because they did not believe the story that the pictures told.

Of the millions of people of many nations who saw the pictures that fourth week of October, only a handful, and these were PI's, knew exactly what it was that they were looking at. It was their testimony which convinced the high officers of their government, and from there on out the credibility of the photo evidence was established. What happened in October of 1962 had happened many times before and has happened many times since. To paraphrase once again a famous remark—never have so many taken so much on the say-so of so few.

More on probability—I

BAYES' THEOREM FOR INTELLIGENCE ANALYSIS*

Jack Zlotnick

The intelligence interest in probability theory stems from the probabilistic character of customary intelligence judgment. Intelligence analysis must usually be undertaken on the basis of incomplete evidence. Intelligence conclusions are therefore characteristically hedged by such words and phrases as "very likely," "possibly," "may," "better than even chance," and other qualifiers.

This manner of allowing for more than one possibility leaves intelligence open to the charge of acting the oracle whose prophecies seek to cover all contingencies. The apt reply to this charge is that intelligence would do poor service by overstating its knowledge. The very best that intelligence can do is to make the most of the evidence without making more of the evidence than it deserves. The best recourse is often to address the probabilities.

The professional focus on probabilities has led to some in-house research on possible intelligence applications of Bayes' Theorem. At the time of my participation in this research, I was an analyst in the Central Intelligence Agency, which sponsored the scholarship but took no position of its own on the issues under study. My personal views on these issues, as elaborated in the following pages, have no official character.

The Bayesian Approach

Bayes' Theorem in its odds-likelihood form served participants in our test program as their diagnostic rule for appraising new evidence. The odds-likelihood formulation of Bayes' Theorem is the equation

$$R = PL$$

R is the revised estimate of the odds favoring one hypothesis over another—the estimate of the odds after consideration of the latest item of evidence. P is the prior estimate of the odds—the odds before consideration of the latest item of evidence. There is no escaping some starting estimate of P. However, after the starting estimate was in

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hand, the participating analysts offered no judgments about P. It was a value carried forward in machine memory from previous analysis. R, the result of the mathematical processing, was what went back into machine memory to become the value of P used in consideration of the next item of evidence. The participating analysts offered judgments only about L, the likelihood ratio.

The likelihood ratio was the analyst's evaluation of the diagnosticity of an item of evidence. Evidence is diagnostic when the chances of its appearing are different if one hypothesis is true than if another hypothesis is true. Suppose intelligence is asked to estimate the comparative merits of two hypotheses—one of imminent war, the other of no imminent war. The estimate is to be expressed in terms of the odds favoring or disfavoring the war hypothesis. The latest evidence is deployment of foreign troops to a border area. Is the deployment deemed to be say two times more likely if the war hypothesis is true than if the no-war hypothesis is true? Then the evidence is certainly diagnostic. The value of L, a judgment of the analyst communicated to the machine processor, would in this case be the fraction $2/1$.

Three principal features of Bayesian method distinguish it from conventional intelligence analysis. The first is that the intelligence analyst is required to quantify judgments which he does not ordinarily express in numerical terms. This requirement to quantify probabilistic judgment is the feature that perhaps draws most of the critical fire against the Bayesian approach in intelligence analysis. A debating point of the critics is that analysts are bound to disagree in their opinions of the exact figure that should represent the diagnostic value of an item of evidence. The Bayesian rebuttal is that disagreement among analysts is just as much a characteristic of traditional method and is no less serious for being implicit rather than explicit in the analysis. The critic returns to the debate by observing that the typical analyst, being a verbal and not a mathematical man, finds it inordinately difficult to express his degree of belief to the precision implied by a numerical value. The partisan of Bayes, for his part, takes the position that people have been quantifying probabilistic judgments since the beginning of time—whenever they offered or accepted betting odds on the outcome of any doubtful issue.

The second distinguishing feature of Bayesian method is that the analyst does not take the available evidence as given and draw therefrom his conclusions about the relative merits of opposing hypotheses. He rather postulates, by turns, the truth of each hypothesis, addressing himself only to the likelihood that each item of evidence would

appear, first under the assumption that one hypothesis is true and then under the assumption that another hypothesis is true. The analyst is under no ego-supporting need to hold to positions previously taken on the merits of the respective hypotheses; he does not feel called upon to reinforce his self-esteem by reaffirmation of opinions previously put on the record.

The third distinctive feature of Bayesian method is that the analyst makes his judgments about the bits and pieces of evidence. He does not sum up the evidence as he would have to do if he had to judge its meaning for final conclusions. The mathematics does the summing up, telling the analyst in effect: "If these are your readings of the individual items of evidence, then this is the conclusion that follows." The research findings of some Bayesian psychologists seem to show that people are generally better at appraising a single item of evidence than at drawing inferences from the body of evidence considered in the aggregate. If these are valid findings, then the Bayesian approach calls for the intelligence analyst to do what he can do best and to leave all the rest to the incorruptible logic of a dispassionate mathematics.

The Bayesian approach was not studied with any idea of its replacing other approaches in intelligence analysis. The responsibility of intelligence is to depict, as best it can, the current and prospective state of international affairs. The intelligence estimate is a closely-reasoned analysis of such important matters of interest as the top political leadership of a foreign country, evolving popular attitudes in that country, changing force structures in its military establishment, its levels of scientific achievement, and the hard choices it is making in allocation of resources to the guns and butter sectors of the economy. The intelligence estimate is sketched in all the lights and shadows of descriptive, narrative, and interpretive commentary. This task is not reducible to terse statement of the odds favoring one particular hypothesis over another.

There are, however, areas of intelligence analysis where Bayes' Theorem might well complement other approaches. One crucially important area is that of strategic warning—the analysis directed to uncovering any pattern of activity by a foreign power suggestive of a major and imminent threat to US security interests. The patterns of events leading to Pearl Harbor in 1941 and to the Communist invasion of South Korea in 1950 are cases in point. Strategic warning analysis focuses primarily on just the problem that Bayes' Theorem addresses—the odds favoring one hypothesis (say imminent attack) over another hypothesis (no imminent attack).

The Research Task

One way to test the usefulness of Bayes' Theorem for intelligence analysis is to replay intelligence history. This means going back to international crises of years past. It means assembly of the evidence which was available before the outcomes of the crises were known. It means reading the old intelligence estimates and other studies in order to find out how the analysts of the day interpreted the evidence. It means assignment of L values—likelihood ratios—that honestly reflect these analyst evaluations of the evidence at the time and not our present hindsight knowledge.

Another way to test Bayes' Theorem is on current inflows of evidence. The advantage of this kind of testing is that hindsight knowledge does not intrude; Bayes' Theorem is pitted fairly and squarely against the conventional modes of analysis. Offsetting this advantage for honest research, however, is a disabling disadvantage.

The disadvantage derives from the very nature of the hypotheses at interest in strategic warning. The alternative hypotheses are commonly of two types. One stipulates continuation of the status quo. The other stipulates sudden change from the status quo. Usually the situation today is pretty much what it is going to be a week from today. The status quo hypothesis, in other words, usually turns out to be the true one in strategic warning analysis. But the main test of strategic warning effectiveness is the capability to give forewarning of the sudden changes that occasionally do occur in the status quo. The intelligence interest in Bayes' Theorem is primarily in how well the Bayesian approach to strategic warning would meet this main test of performance in situations of general surprise, without chronic resort to cry-wolf false alarms. Unfortunately, intelligence research cannot be speeded up by focus on the particular current issues which will turn into occasions of intelligence surprise. If intelligence could pick out in advance the issues on which it was going to be surprised, it would by definition never be surprised, and it would have no interest in the possible contributions of Bayes' Theorem to improved analysis.

The outlook, then, is that many tests of Bayes' Theorem on current inflows of evidence will be needed to get the few interesting occasions that show Bayesian performance in circumstances of general intelligence surprise. And just a few interesting examples are not enough to make the case for or against the Bayesian approach, which may do better than conventional method sometimes and not as well other times. A large enough sample of interesting examples is needed to justify confident findings of comparative performance on the average.

The results of the testing so far have been interesting enough to make a good case for further testing of Bayes' Theorem in intelligence analysis. Among the interesting results has been an uncovering of problem areas that flank the path of intelligence analysis and that are not very easily outflanked.

The Life-Span of Evidence

One such problem area has been called nonstationarity. In situations of nonstationarity, that is, when hypotheses are being effectively altered by the passage of time, evidence will have a limited life-span. An intelligence hypothesis about current Soviet policy is not exactly the same hypothesis on January 15 that it is on February 15. The date has changed, so the hypothesis is to a degree different; and evidence back in January which had a certain bearing on the hypothesis of what was then current Soviet policy does not have the same bearing on the hypothesis of what is current Soviet policy a month later.

Consider, for example, some evidence which was available to intelligence and to the public at large in the summer of 1962, before photographic confirmation was received of missiles in Cuba that could reach targets deep in the United States. Soviet leaders gave public assurances during this period that the expanding military aid to Cuba was for defensive purposes only. Now an analyst's appraisal of this kind of assurance will depend partly on how honorable or dishonorable he believes Communists to be. But whatever his views about the honor of Communists, he would certainly not consider any government's assurances to constitute a commitment for all eternity. Governments do make new decisions and reconsider old ones. This amounts to saying that the diagnostic value of evidence bearing on hypotheses about current government policy tends to erode over time. A mathematical logic for strategic warning analysis has to be attentive to this erosion. Perhaps the analyst can specify the expected rate of erosion when he first encounters an item of evidence. If he cannot or prefers not to, the Bayesian approach does not quite attain the mechanistic ideal that would require of the analyst only his one-time attention to each item of incoming evidence. The analyst instead finds himself looking back from time to time at his whole body of past evidence, to consider whether its diagnostic value, as recorded in machine memory, is still valid and not out-dated.

Causal Evidence

Another problem area spotlighted in the testing is the occasional reversal in cause and effect relationship between hypotheses and data.

The disease generates the symptoms of the disease, and so the physician can infer the disease from the symptoms. Similarly in his surveillance of the Soviet scene, the intelligence analyst in Washington can infer from Soviet actions a good deal about Soviet policy. But the analyst also has his eye cocked for relevant data other than Soviet actions, data which have less a derivative than a causal relationship to Soviet policy. I draw again on the Cuban missile crisis of 1962 for my historical example.

On several occasions that year, President Kennedy publicly warned that the United States would take a grave view of strategic missile emplacements in Cuba. How would a Bayesian analyst evaluate President Kennedy's warnings for their relevance to opposing hypotheses about Soviet missile shipments to Cuba? If the analyst were a mechanical, uncritical Bayesian, he would say to himself: "President Kennedy is more likely to issue these statements if the hypothesis of imminent Soviet missile shipments to Cuba is true than if the hypothesis of no such missile shipments to Cuba is true. My L in the Bayesian equation $R = PL$ is greater than $1/1$, and so my mathematics works out to an increase in the odds favoring the missile hypothesis."

Well, the analyst in this case is surely not reasoning as President Kennedy reasoned. The President no doubt felt that the clear communication of American concern would either have no effect on Moscow or, hopefully, would dissuade the Soviet leadership from shipping strategic missiles to Cuba. He thought, in other words, that his statements would tend to reduce, not increase, the odds favoring the missile hypothesis.

The complication for the Bayesian analyst is the causal character of President Kennedy's statements. Soviet actions are direct derivatives of Soviet policy. President Kennedy's statements were not. They were important primarily for the chance that they would affect, not reflect, Soviet policy.

It can be shown that, in principle, Bayes' Theorem is as applicable to causal evidence as to derivative evidence. In practice, Bayes' Theorem often offers slippery ground to the analyst appraising causal evidence. In practice, the analyst does better by putting a little sand in his tracks. He gets better mental traction in this case by making a direct judgment about the impact of the causal evidence on the comparative merits of his hypotheses. He says to himself: "If the odds were even-money in favor of the missile hypothesis before receipt of the causal evidence, what would the odds be now after receipt of this evidence?" When the prior odds are even-money (that is, $1/1$), the revised odds equate to the likelihood ratio, according to the Bayesian

equation $R = PL$. So, by making a direct judgment of revised odds following a stipulation of even-money prior odds, the analyst obtains an effective likelihood ratio to give the computer.

This is an approach which respects the mathematics of Bayes but does violence to the spirit of Bayes. One of the attractive features about Bayesian method in its pristine purity is that the analyst need address himself to the merits of the hypotheses only at the very beginning of his analysis. In principle, he does not thereafter reaffirm his first opinion, admit to a change in opinion, or criticize anybody else's opinion on the subject. He is supposed to make a judgment, instead, of quite another sort, a judgment about the evidence which postulates the truth of each hypothesis in turn, a judgment which does not involve him again in debate about the merits of each hypothesis. His encounters with causal evidence, however, often do not allow him to keep quite this detachment from the hypotheses. He finds himself addressing R, not L.

Catch-All Hypotheses

Another problem area encountered in our research has been examined in Bayesian literature as the nonindependence issue. Nonindependence enters into analysis as a complicating feature when the likelihood ratio—the L value of an item of evidence—is affected by the previous pattern of evidence.

Nonindependence is an arcane subject to analysts who are new to probability mathematics, mainly perhaps because items of evidence which are independent if one hypothesis is assumed true can be nonindependent if another hypothesis is taken as true. Analysis is easier when items of evidence are independent (or to put it more properly, conditionally independent)—that is to say, when the likelihoods of their being received do depend on which hypothesis is assumed true but when these conditional likelihoods hold regardless of the previous pattern of evidence. Intelligence analysts have their way of reaching for conditional independence, whether or not they have ever heard of the nonindependence issue. They reach for a new hypothesis to do service for some hypothesis that no longer seems suitable as originally worded.

Such an unsuitable hypothesis could be the one postulating continuation of the status quo in the strategic warning problem. This catch-all hypothesis can be divided into two or more subhypotheses (and it can be divided different ways into different sets of subhypotheses). For an illustrative example, take any case in history of a big power

threatening its much smaller neighbor and finally invading the little country when threats alone did not avail.

Suppose the invasion is preceded by reports that the big power is moving its troops toward the border. Considered later in time from the vantage point of hindsight, the troop movements certainly would seem to be strong evidence, which ought to have tipped the odds substantially in favor of the invasion hypothesis. But the analyst of the day would probably find himself reflecting on at least two relevant subhypotheses of the no-invasion hypothesis. Subhypothesis A might be that the big power will not invade the little country but will apply very strong pressures—psychological, political, and other—just short of military invasion. Subhypothesis B might be that the big power will neither invade nor apply other extremes of pressure against the little country.

Now the analyst using Bayes' Theorem introduces an initial opinion about the hypotheses when he begins his analysis. He must similarly introduce an opinion about the subhypotheses if he comes to make them explicit elements in his analysis. By the time he receives the reports of troop movements, the previous evidence will have inclined him to the opinion that subhypothesis A—strong pressures against the little country—is the only reasonable interpretation of the no-invasion hypothesis. The events leading up to the troop movements (the grim warnings, the shrill propaganda, the military alerts) will constitute such virtual contradiction of subhypothesis B—no extremes of pressure—as to give it a near-zero probability. If this is the analyst's view, then the troop movements toward the border must seem almost as likely under the no-invasion hypothesis as under the invasion hypothesis. His L is just about $1/1$. His Bayesian approach has done virtually nothing to change his current odds.

This undiagnostic character of incoming evidence near the climax of international crises may seem novel to novices; it is familiar enough to experienced intelligence analysts. The more experienced they are, the more rueful they are likely to be in their recollections of evidence that was ambiguous to contemporaneous vision but became telling in retrospective inquiries.

False Evidence

Perhaps the most difficult problem area is the suspect character of some evidence. The intelligence analyst gets his information in accounts from sources of varying reliability. He does not know for sure which accounts to believe and which to disbelieve. So he has to appraise his evidence, not only for its bearing on the hypotheses, but

also for its probability of being accurate. The estimated probability of accuracy will enter into the analysis and will affect final results.

Unfortunately, an analyst's opinion about a report's probable accuracy or inaccuracy will be influenced by his current opinion about the hypotheses. Does he find it hard to give credence to reports from Cuban refugees who claim to have seen objects resembling medium-range missiles near Havana? If he is skeptical, it may well be because he finds it hard to give credence to the hypothesis that the USSR will do anything so foolish as to ship such missiles to Cuba. So once again, we have a case of information not doing the work which critics later, in all the wisdom of hindsight, will say it should have done.

The Research Promise

My exposition of these problem areas is not meant to imply that they muddle only the Bayesian approach; they plague with fine impartiality all types of intelligence analysis—traditional method as well as Bayesian method, verbal logic as well as mathematical logic. Traditional method also must cope with the eroding diagnostic value of past evidence as it recedes into history. Traditional method also finds it harder to draw probabilistic conclusions about the state of the world from causal evidence than from derivative evidence. Traditional method also sometimes explains away evidence that can be explained away by a favored subhypothesis of a catch-all hypothesis. Traditional method also has to contend with the implausibility of evidence that is not in character with the climate of prevailing opinion.

My purpose in expanding on the problem areas is to show that much of the difficulty in intelligence analysis is not the difficulty to which the Bayesian approach is addressed. The Bayesian approach seeks to insulate analysis from frailties of logic in aggregating the evidence. The working world of intelligence, however, is concerned not only about possible inconsistency in everyday thinking between the conclusion drawn from the body of evidence considered as a whole and the conclusion that should logically follow from judgments about the evidence considered item by item. Intelligence views with concern also the possibilities of mistaken judgments about individual items of evidence. The intelligence pragmatist is wistful about evidence which almost speaks for itself, evidence to which most people will attribute much the same probability values because the values can be documented by, say, actuarial statistics or other such extrinsic authority. The pragmatist feels that an increase in the amount of this kind of evidence would do more to help men reach sound conclu-

sions than could any formal logic—Bayesian or other—for reasoning from uncertain propositions about the evidence.

Conceding this point, the Bayesian responds that intelligence must still do the best it can with what it has. In a world of fallible judgments about evidence, the Bayesian approach is not a path to perfection; it can be at best only a path to improvement. The promise of the research on Bayesian method is a mathematical logic to which intelligence can have recourse for substantiating or contradicting the verbalizations of the traditional analysis. When the different approaches lead to discrepant conclusions, intelligence should perhaps undertake to rethink, recalculate, and if possible reconcile. The research interest at this time should be to find out whether such a Bayesian cross-check on other reasoning would significantly improve the quality of analysis.

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More on probability-II.

**THE SINO-SOVIET BORDER DISPUTE:
A COMPARISON OF THE CONVENTIONAL AND
BAYESIAN METHODS FOR INTELLIGENCE
WARNING**

Charles E. Fisk

Problems of "indications analysis" or "intelligence warning" are essentially questions of how to assign probabilities to hypotheses of interest. For example, a problem of indications analysis occurred in August 1969 when two hypotheses arose; namely, the conjecture (H₁) that within the next month the USSR would attempt to destroy China's nascent nuclear capabilities, and the alternative hypothesis (H₂) that such an attack would not occur.

A method of indications analysis is a rule for eliciting probability judgments from intelligence analysts, and alternative methods for this purpose have been studied within the Agency since 1967.¹ The usual and most direct method is simply that of asking analysts to make either verbal or numerical probability judgments about hypotheses of interest. As an alternative to the conventional approach, the so-called Bayesian method does not require analysts to assign probabilities to the main hypotheses of interest; instead, analysts are asked to specify values for certain "conditional" probabilities, from which one can infer judgments about the main hypotheses.

It has been argued² that the Bayesian method is better than the conventional approach to problems of intelligence warning. This article will illustrate the two alternatives, and will then explain the results of an experiment that was designed to test the assertion of the Bayesian method's superiority.

¹ Two examples of these studies are *A Mathematical Model for Intelligence Warning* (Intelligence Report No. 1396/67, November 1967), and *Bayes' Theorem in the Korean War* (Intelligence Report No. 0605/68, July 1968). For references to various studies done outside the Agency, see *A Bibliography of Research on Behavioral Decision Processes* by Ward Edwards (University of Michigan, Human Performance Center, Memorandum Report No. 7, January 1969).

² A detailed exposition of this argument is offered by Ward Edwards *et al.* in "Probabilistic Information Processing Systems: Design and Evaluation," *IEEE Transactions on Systems Science and Cybernetics* (Vol. SSC-4, No. 3) September 1968. Further expositions have been put forth by Jack Zlotnick in "A Theorem for Prediction," *Studies in Intelligence* (Vol. 11, No. 4) Fall 1967.

SECRET

53

MORI/HRP PAGES 53-62

The Conventional Method of Intelligence Warning

The conventional approach to intelligence warning begins when a set of hypotheses first comes under active scrutiny. For example, during August 1969 several intelligence officers warned that the USSR would probably launch a major attack against China within the next month. This warning spawned various hypotheses, two of which were (H_1) that the USSR would begin the offensive during September 1969, and (H_2) that there would be no attack.

For a large class of hypotheses, the problem of indications analysis remains essentially the same: certain Agency officials must first elicit from qualified analysts judgments about the hypotheses, and then must synthesize these judgments into a warning. The officials obviously cannot pore over every bit of evidence observed by each analyst, so analysts must focus and summarize their views.

Generally, then, the first step in the conventional method involves the gathering of either verbal or numerical probability estimates. On 30 August 1969, for example, each of six senior analysts from six Agency offices was asked to estimate the probability of the war hypothesis H_1 . Their estimates—i.e., values for $P(H_1)$ —appear in Table 1. As time passes, further estimates are elicited, and previous warnings are either amplified or damped on the basis of the new estimates. Clearly, then, a key question is how an official ought to elicit probabilities from analysts. The conventional approach suggests that an official should simply ask analysts to state the probabilities whenever the official wants to reconsider his warning.

As part of an experiment that was designed to compare the conventional method with an alternative system (the Bayesian) for eliciting

Table 1

Analyst	The Probability of H_1 on 30 August 1969*
A	.20
B	.85
C	.40
D	.25
E	.35
F	.20

*The symbol H_1 denotes the hypothesis that during September 1969, the USSR would launch a nuclear attack against China.

ing probabilities, each of the six analysts mentioned above was asked on 5 September 1969 to re-estimate the probability that a Sino-Soviet war would erupt before 29 September. On 12 September the analysts were asked again, and so forth for each week in September. As a result of this process of questioning, each analyst produced an "intuitive" probability track such as the one shown in Figure 1. Each point on the illustrated track denotes the best probability judgment that Analyst D could offer after reading the all-source intelligence available to him.

On the basis of a considerable amount of research involving simulated questions of intelligence warning, however, Edwards,³ Zlotnick,⁴ and other proponents⁵ of the Bayesian method for eliciting probabilities would argue that the sequence of estimates shown in Figure 1 was *not* the best sequence that Analyst D could have specified. They claim that an official who had asked "the right questions" could have obtained from Analyst D—and from each of the other analysts—a better sequence of probabilities. This alternative method of questioning will be explained in the following section.

Probabilities Stated by Analyst D

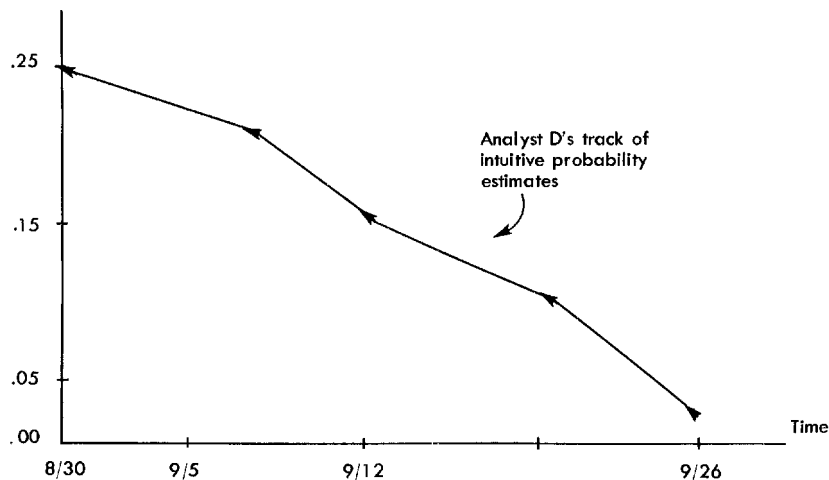


Figure 1

³ *Ibid.*

⁴ *Ibid.*

⁵ See the bibliography cited in Footnote 1.

The Bayesian Method of Intelligence Warning

There is no unique "Bayesian method": dozens of systems, each slightly different from its predecessors, have been proposed and tested on simulated problems of intelligence warning. Most of these systems, however, involve substantially similar steps. The steps taken in the Sino-Soviet Experiment to obtain from each of the six analysts a Bayesian track that could be compared with the analyst's intuitive track are as follows:

(a) On 30 August 1969 each of the six analysts was asked to estimate a value for $P(H_1)$, which at that time denoted the probability that the war hypothesis H_1 was true. This first step duplicated the first step in the conventional method discussed above, so each analyst's estimate for $P(H_1)$ appeared as in Table 1.

(b) In contrast to the conventional method, on 5 September the Bayesian approach did not require the analysts to re-estimate $P(H_1)$. Instead, each analyst was asked to list the major events whose occurrence during the previous week had influenced his opinion about the war hypothesis. For example, during the week Analyst D might have observed that no men in the Soviet reserve army had been called for active duty. This event of "no calls" could have been denoted by E_1 . And, since Analyst D might have believed that a call-up during the previous week would precede the event of a Soviet attack in September, the event E_1 might have lowered his intuitive probability judgment concerning the chance of war. Similarly, E_2 might have denoted the event of no increases in Soviet propaganda against the Chinese, and so forth for other events that an analyst might have thought relevant to the war hypothesis H_1 .

(c) A majority of the analysts listed virtually the same set of relevant events, although some analysts' views had been influenced by events that other analysts had not listed. From the separate lists, a master event list was compiled, such that the events E_1, E_2, \dots on the master list exhibited two properties; namely, (i) each event proposed by each analyst was reflected in the master list; and (ii) each master event was, roughly speaking, independent of each other master event.⁶

⁶ The notion of independence can be illustrated as follows: suppose that Analyst D has listed the event of "a high-level diplomatic probe by the USSR to ascertain probable US reactions to a Sino-Soviet war," while Analyst E has listed "a war-related contact between US and Soviet officials." These two events clearly refer to the same thing, so the master list would contain only one event referring to a diplomatic probe. In some cases, however, the two properties of inclusiveness and independence were difficult to achieve in compiling the master list.

(d) When the master list had been compiled on 5 September 1969, some of the analysts asserted that certain events suggested by other analysts had not actually occurred. Such differences over raw intelligence were recorded as each analyst estimated a probability of occurrence for each of the events E_1, E_2, \dots on the master list.

(e) In addition to specifying probabilities of occurrence, each analyst estimated various conditional probabilities on 5 September. For example, with respect to the event no reserve calls during E_1 of the previous week, Analyst D was asked to specify a value for $P(E_1|H_1)$, which denotes the probability that E_1 would have occurred, given the assumption that the war hypothesis (H_1) was true. Moreover, Analyst D was asked to estimate $P(E_1|H_2)$, the probability of E_1 on the assumption that the no-war hypothesis (H_2) was true. For each of the other events on the master list Analyst D specified a similar set of conditional probabilities, as did each of the other analysts.

(f) A modified version of Bayes' Theorem was then used on 5 September 1969 to calculate for each analyst a "revised" probability of war.⁷ This probability was called an analyst's Bayesian estimate, and was plotted on the same graph as his intuitive probability. Thus for Analyst D in particular, on 5 September 1969 the two probability tracks shown in Figure 2 had been obtained—one track by the conventional method, and one by the Bayesian approach.

(g) On 12 September 1969 the Bayesian procedure outlined above was repeated, with the exception that the "prior" probabilities used in the revision process were the Bayesian probabilities of war that had been obtained on 5 September 1969. Thus after two weeks, a typical analyst's probability tracks appeared as in Figure 3.

(h) After the Bayesian procedure had been repeated at weekly intervals during September, the Bayesian tracks derived from conditional probabilities specified by Analysts A, B, and D appeared as in Figure 4. The Bayesian and intuitive tracks compiled for Analysts C, E, and F resembled the tracks shown for A and D, in the sense that for five of the six analysts, the Bayesian track always fell below the intuitive track.

A Criterion for Comparing Probability Estimates

A criterion for comparing methods of probability elicitation can be illustrated with reference to Figure 3. In retrospect, we know that the

⁷ This method of calculating revised probabilities is sometimes called a "roll-back" procedure. See *Applied Statistical Decision Theory* by H. Raiffa and R. Schlaifer (Harvard Business School, Division of Research, 1961).

Probabilities Stated by Analyst D

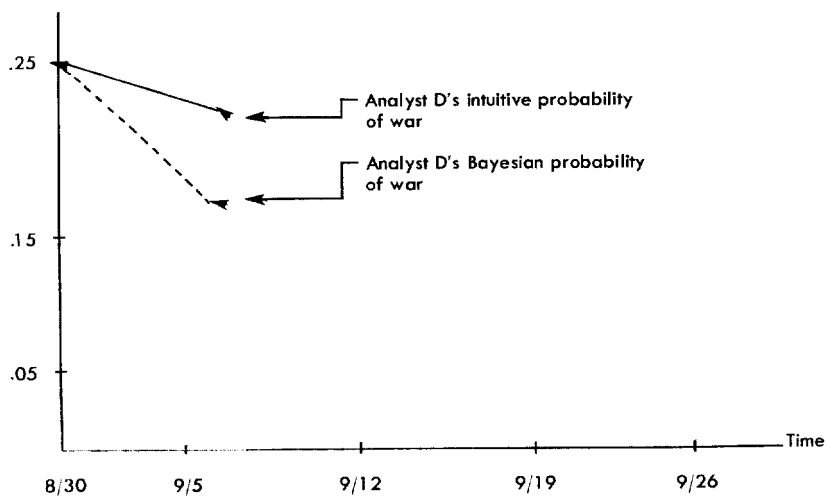


Figure 2

Probabilities Stated by Analyst D

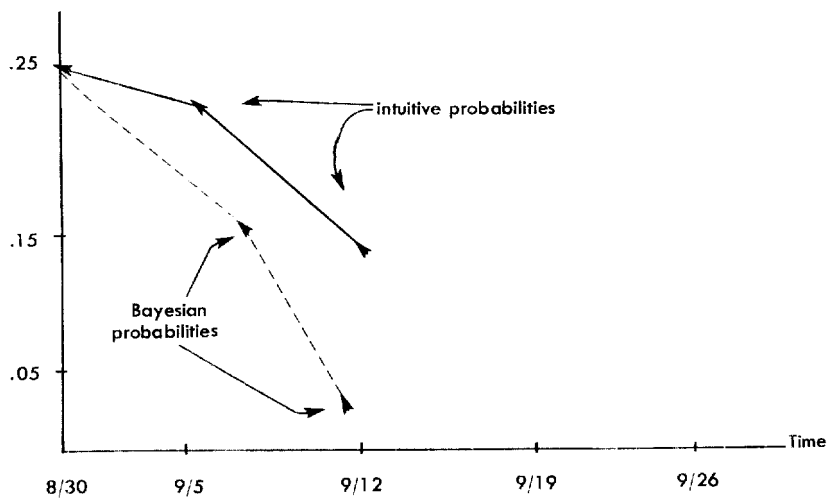


Figure 3

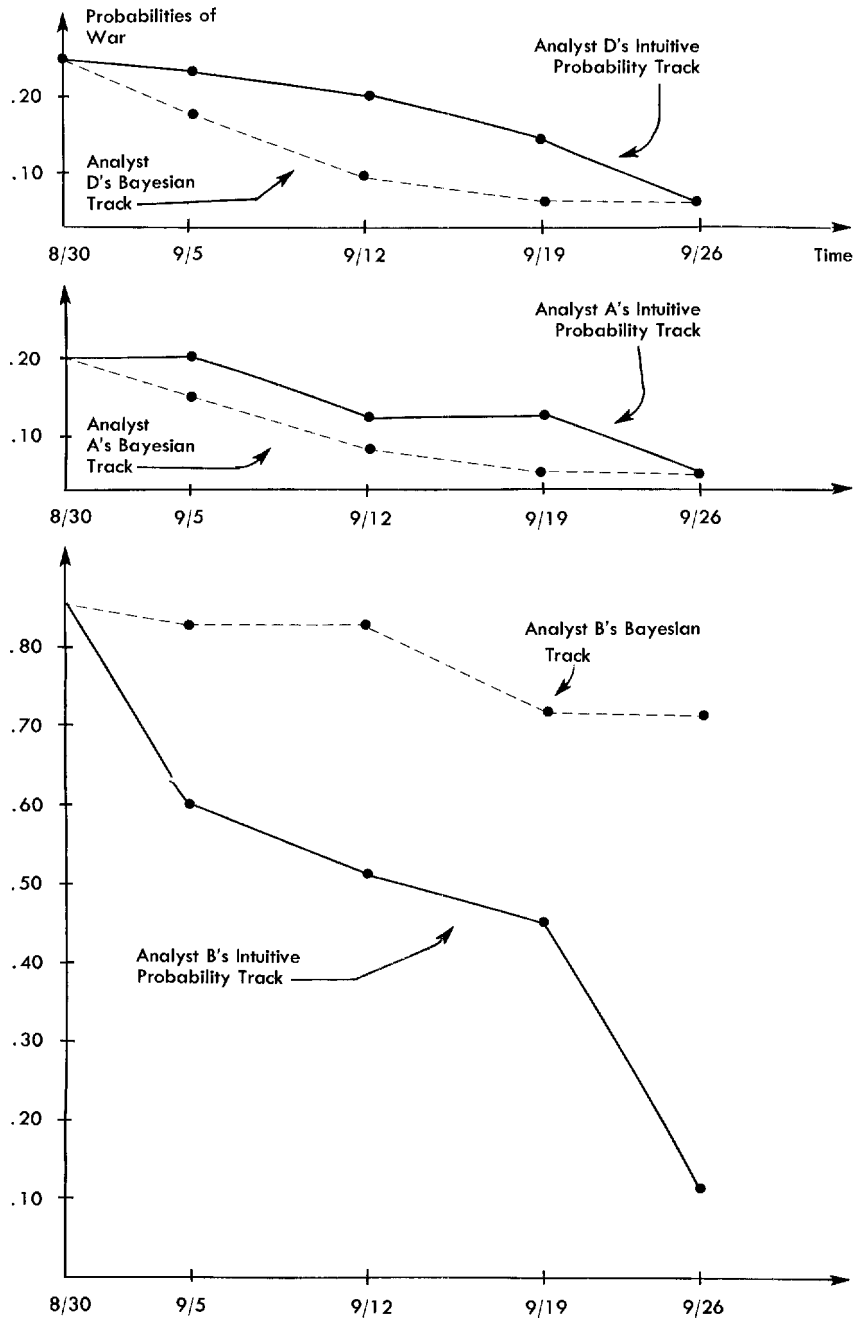


Figure 4

hypothesis H_1 was false: Russia did not attack China. Thus if an analyst's "probability tracks" had actually appeared as in Figure 3, then on 12 September 1969 an official would have acted more wisely on the basis of the Bayesian sequence of estimates. In other words, if one had been forced to gamble according to either the Bayesian or the intuitive tracks shown in Figure 3, one would in retrospect have preferred the Bayesian sequence.

Of course, if Russia had attacked China, and if a typical analyst's probability tracks had appeared as in Figure 3, then one would have preferred to have acted according to the analyst's intuitive track. But according to the advocates of Bayesian analysis, such a preference for an intuitive track will seldom occur: if Russia had attacked, then—according to the Bayesian proponents—prior to the attack the Bayesian track for a typical analyst would have been above his intuitive track, such that in retrospect the Bayesian method would again have been preferred. As is evident in Figure 4, Analyst B proved to be an exception to this assertion: his Bayesian track always fell above his sequence of intuitive estimates.

This criterion of "retrospective superiority" has served as the basis for dozens of experiments⁸ in which researchers have compared the Bayesian method with alternative techniques for eliciting probabilities, and in most cases the Bayesian approach has triumphed. But there is no firmly established analytical justification for the method. Bayes' Theorem is a mathematical truism, but there are no axioms from which one can infer that repeated applications of the theorem to conditional probabilities specified by analysts will yield superior intelligence warnings. Thus, in the fall of 1969, it was of considerable interest to review the Bayesian method's effectiveness in the context of the actual intelligence problem posed by the chance of a Sino-Soviet war.

The Sino-Soviet Experiment

As explained above, the six analysts met at weekly intervals during September 1969 in order to re-estimate the probability of the war hypothesis H_1 , and to specify the conditional probability estimates that were processed according to the Bayesian method. In October 1969 (when the war hypothesis H_1 was known to have been false) the probability tracks derived from the two methods were compared as in Figure 4. The primary result was that for five of the six analysts, the Bayesian track had always been below the intuitive sequence of probabilities. Thus in retrospect, an official would have preferred to

⁸ See the references cited above on page 53.

have acted according to the Bayesian estimates, rather than according to the analysts' best intuitive judgments concerning the war hypothesis.

An Evaluation of the Bayesian Method

Several results of general interest emerged from the Sino-Soviet experiment. First of all, when the experiment began the analysts differed widely in their views concerning the chance of a war; but the reasons for their differences were murky at best.

A typical argument between two analysts would arise when one would accuse the other of having ignored certain crucial facts in estimating the likelihood of war. The accused would then respond that he had indeed considered all relevant information, and that his estimate was based on facts that other analysts had overlooked. Such arguments were difficult to evaluate, since there was no record of who had considered what, or of how each analyst's probability estimate had evolved over time.

Once the Sino-Soviet experiment had begun, however, one could easily determine the relative importance that an analyst had assigned to any given event. For example, it was evident from Analyst B's conditional probability estimates that he had considered the event of Kosygin's visit in September 1969 to Peking as being irrelevant to the war hypothesis. In contrast, Analyst E had regarded the meeting as a profound indicator that war would not occur. The issue of whether Analyst B exercised good judgment in this respect remains an open question; but at least his assessment of the Peking trip had been recorded and could be evaluated.

Thus the Bayesian approach provided a kind of accounting system for intelligence analysis. If such a system were implemented for other questions of indications analysis, a significant class of disagreements among analysts might be resolved. And to the extent that such disagreements would persist, an official who must synthesize warnings on the basis of analysts' estimates could discern and evaluate causes for the disagreements.

A second contribution of the accounting system was the fact that after the system's inception, the analysts definitely did consider the same relevant events. In particular, Analyst E wrote the following review of the experiment.

In the case of Office E, interchanges with other offices are usually on an unsystematic *ad hoc* basis. The Bayesian experiment afforded an opportunity to bring these interchanges into focus on a systematic basis. Its particular merit lies in the manner in which participants are led to identify the factors

influencing their estimates and to present these for critical review by others approaching the question from varying angles. I would emphasize the value of focus, though perhaps no less valuable is the exposure of participants to lines of analysis—as one analyst noted—of which they are dimly if at all aware.

Similarly, Analyst C wrote:

The meeting was a useful forum for the interplay of ideas and the exchange of information which might otherwise not occur. Interchanges would take place in the absence of such a meeting; but they would be limited because of their bilateral nature (in most cases).

In summary, an improved system of accounting for analytical judgments is needed. Although it cannot be said categorically that the Bayesian method excels as a forecasting device, the Sino-Soviet experiment indicates that it might provide a means for such accounting.

In the Beginning.

THE ORIGINS OF NATIONAL INTELLIGENCE ESTIMATING

Ludwell Lee Montague*

Most of what I have to say on this subject is a matter of personal recollection. I was "present at the creation," though without power to control the event.

My story begins in October 1940, when I was ordered to active duty in the Military Intelligence Division of the War Department General Staff. At that time, now thirty years ago, there was no common conception of *any* kind of an intelligence estimate, much less of a *national* intelligence estimate.

In our language, the word "intelligence" originally meant communicated information: that is, information reported from elsewhere, as distinguished from information known by personal observation. You will find the word used in that sense by Shakespeare. That was still the prevailing sense of the word in 1940. Indeed, public comment shows that, even today, most laymen regard us only as gatherers of information. The Press, which is itself a primitive intelligence organization, shows almost no comprehension of the function of estimating the meaning of the information gathered, apart from the expression of personal opinion by individual columnists whose "authority" varies with their personal prestige.

In this primitive sense, the entire Department of State was, in 1940, an intelligence organization. It had its own network of reporters who sent it information from abroad—but the evaluation of that information occurred only intuitively in the minds of the desk men who read it. The Department had no conception of intelligence research, much less of any organized process of estimating.

The Navy was one degree more sophisticated. It had an Office of Naval Intelligence, the function of which was to compile NIS-type information of Naval interest. Just the *facts*, man! Navy doctrine strongly held that it was *not* a function of Intelligence to estimate the *meaning* of the facts. Only the Admiral could do that—which may go some way to explain Pearl Harbor.

*This article is the text of an address delivered by the late Dr. Montague, a retired member of the Board of National Estimates, at the first meeting of the Intelligence Forum, 11 May 1971.

Only the Army conceived it to be a function of Intelligence to estimate the capabilities and intentions of foreign powers. That was Army doctrine, but the Military Intelligence Division did little to practice that art. Like ONI, it spent the year before Pearl Harbor producing "strategic handbooks," a primitive, single-service, NIS.

During that year "Wild Bill" Donovan burst upon the scene as the President's Coordinator of Information. He was a man of many pregnant ideas. Just one of them was that the President should be better informed than the State, War, and Navy Departments, acting separately, could possibly inform him. Donovan assembled a group of eminent scholars, men knowledgeable of foreign affairs and practiced in the techniques of research and analysis in a way that regular Army, Navy, and Foreign Service officers could not be. Donovan's Research and Analysis Branch would assemble all of the information in the possession of the Government, not only in the State, War, and Navy Departments, but also in the Library of Congress and other places, and would prepare for the President a fully informed and thoughtful analysis of any situation of interest to him.

Let me observe at this point that the analyses actually produced by this R&A Branch were not estimates. They were academic studies, descriptive rather than estimative, more like an NIS than NIE.

Donovan had no idea of coordinating these studies with anyone. He was responsible only to the President. One can readily imagine how professional Army, Navy, and Foreign Service officers reacted to the idea that a lot of johnny-come-lately professors would be telling the President what to think about political and strategic matters.

Gen. Raymond Lee, who had recently served as military attache in London, proposed to head off Donovan's intrusion into the mysteries of military intelligence by the creation of a Joint (Army and Navy) Intelligence Committee, in imitation of the British JIC.

Significantly, the task of defining the functions of this US JIC was assigned, not to the Chiefs of Intelligence, but to the Chiefs of Army and Navy Plans. There arose at once a doctrinal controversy between the Army and the Navy. The Army wished the JIC to "collate, analyze, and interpret information with its implications, and to estimate hostile capabilities and probable intentions." The Navy wished it to present such factual evidence as might be available, but to make no "estimate or other form of prediction." Inasmuch as the Army desired to have a joint committee, for which the Navy's agreement was indispensable, the Navy's view prevailed. Thus the first US inter-departmental intelligence organization came into existence expressly forbidden to make estimates!

I dwell upon this episode because it has contemporary relevance. Now, thirty years later, we hear again that in certain high quarters the idea prevails that the function of Intelligence is to produce evidence, not estimates. Conclusions as to the meaning of the evidence will be drawn by the interested policymakers to suit their policy predilections.

There was, of course, a scuffle between the Army and the Navy for control of this new joint committee. Before the war, normal promotion was faster in the Navy than in the Army. The Army was shocked to learn that the senior Naval officer assigned to the joint committee, a youngish commander, actually outranked the older lieutenant colonel assigned by the Army. And that commander's mission was to see to it that the joint committee did nothing except by direction, particularly that it did no estimating. But the Army, in the midst of a massive mobilization, had developed a faster system than the Navy's for making spot promotions. The Army made the lieutenant colonel a colonel before the Navy could make the commander a captain. And the first thing that the new colonel did, on taking over from the commander, was to order the immediate preparation of the first US joint intelligence estimate, in flagrant violation of the JIC's charter!

The subject of that first US interdepartmental intelligence estimate was the strategic consequences if the Japanese were permitted to seize control of the Netherlands East Indies. (Singapore and Bataan were then under attack, but had not yet fallen.) The answer was obvious: that would be a Bad Thing. The policy implication was also clear: it should be prevented. Since that policy implication suited the Navy, it did not protest the illegality of making that estimate.

At this point, Mr. Winston Churchill came to town with two purposes in mind. The first was to commit the United States to give the war with Germany priority over the war with Japan, not an easy proposition in the face of the US reaction to Pearl Harbor and Bataan. The second was to establish the Combined Chiefs of Staff in order to insure for Britain a more or less equal voice in the conduct of both wars.

The Combined Chiefs of Staff organization included a Combined Intelligence Committee modelled after the British JIC in London. Since the Foreign Office and the Ministry of Economic Warfare were important members of the British JIC, the US JIC, the US side of the CIC, had to be enlarged to include representatives of the State Department, the Board of Economic Warfare, and the Office of Strategic Services. Since the sole function of the CIC was to produce combined "appreciations" as a basis for combined war planning, it

automatically became the primary function of the US JIC to produce US joint intelligence estimates. Thus it was the Prime Minister of Great Britain who created the wartime US JIC and put it into the estimating business.

At that time, and for many years thereafter, the British JIC held the highest reputation in Intelligence. Let me therefore say a word about British joint intelligence estimates. They were joint only in the sense that all of the members of the JIC subscribed to them. It would never have occurred to a British Army officer to question the political judgment of the Foreign Office, nor would it have occurred to a Foreign Office representative to question the Army's order of battle. Consequently British JIC estimates were nothing more than a set of departmental estimates fastened together.

The situation was somewhat different in the US JIC. The State Department was incapable of making any contribution, but felt free to criticize the political contributions of OSS. The Foreign Economic Administration (formerly BEW) generally deferred to OSS in economic matters. The Air Force and the Navy generally stuck to their technical specialties, although the Navy was ever ready to defend the interests of Admiral Nimitz against any supposed Army favor toward General MacArthur. But the Army and OSS both claimed a universal competence: they would argue with anybody about anything, and chiefly with each other. The Army had no hesitation about contradicting an OSS political or economic estimate. OSS delighted to expose deficiencies in the Army's order of battle.

In these circumstances, the Joint Intelligence Staff, the full-time working staff of the JIC, performed a real service in working out an agreed joint text from conflicting contributions, particularly those of the Army and OSS. These were not split-the-difference compromises. Despite their different departmental origins, the members of the JIS were a band of brothers who lived and worked together; they could reach agreement amongst themselves on the basis of reasoned consideration of the evidence. The estimates that they prepared were truly joint estimates.

The trick then was, of course, to obtain the concurrence of the members of the JIC, who were surrounded by advisers who had never participated in joint consideration of the subject. But the members of the JIS were the personal representatives of the members of the JIC for this purpose. They had equal access to them, and could generally persuade them to adopt the joint view.

The defect of the JIC system was that the Committee was composed of six sovereign powers. No one represented the national

interest, as distinguished from departmental interests. No one held a power of decision in case of disagreement. Since there was no acceptable way of registering a divergent view, unanimous agreement was required. In the case of a real controversy, that could be obtained only if someone backed down, or, as happened more often, if someone could devise an ambiguous formulation acceptable to both sides in the controversy. Thus, joint estimates tended to become vague or meaningless precisely at points of critical importance.

The members of the JIS agreed that a headless joint committee was the worst way of producing national intelligence estimates. During the autumn of 1944 they developed their idea of a more effective interdepartmental intelligence system. Since every department would require its own intelligence organization to meet its specialized departmental needs, there would have to be an interdepartmental committee to bring together the heads of those departmental organizations to deal with common (that is, national) problems. But that committee should have an independent chairman, appointed by the President and responsible only to him. And, in the case of estimates, that chairman, having heard all of the evidence and argument bearing on a disputed issue, should have the power to decide what the text of the estimate would say, subject only to the notation of the dissenting opinion of any chief of a departmental intelligence agency. That idea, developed by the men who then had the most personal experience in the coordination of interdepartmental intelligence estimates, is the key to the present system for producing national intelligence estimates.

In the autumn of 1944 others were thinking of a postwar intelligence system. The Department of State had a plan. It was premised upon the exclusive responsibility and authority of the Secretary of State for the conduct of foreign relations, subject only to the direction of the President. It assumed that the military intelligence services would be interested only in technical military matters. It contemplated the creation of an "American (i.e., National) Intelligence Service" within the Department of State. This Service would maintain "close liaison" with the military intelligence services and would obtain through liaison whatever military inputs it required for its own estimates. The military, however, would have no voice in those estimates. Produced under the exclusive authority of the Secretary of State, they would provide the intelligence foundation for national policy.

Somehow, a working copy of this State Department plan came into the possession of General Donovan, the Director of Strategic Services. He moved to forestall State by proposing to the President the creation of a "Central Intelligence Service" in the Executive Office of the President. His point was that departmental intelligence estimates were by their nature self-serving. The President should have in his service an intelligence organization wholly free of the influence of departmental policy advocacy and special pleading. It would make full use of departmental intelligence resources, but would produce its own independent intelligence estimates, as the basis for national policy and strategy.

Let me stress that neither the State Department Plan nor the Donovan Plan contemplated any interdepartmental coordination of these "national policy intelligence" estimates. The military intelligence services would contribute "factual" data to them, but would have no voice in their estimative judgments. They would be produced under the sole authority of the Secretary of State, in the first case, or of the Director of the Central Intelligence Service in the second.

The Donovan Plan was referred to the JIC for comment and the fat was in the fire. After a month of fierce contention, the JIS worked out a compromise plan, JIC 239/5, 1 January 1945. In that paper the JIC recommended the establishment of a Central Intelligence Agency which, among other things, would produce national intelligence estimates. With regard to such estimates, however, the Director of Central Intelligence was required to consult with a board composed of the heads of the departmental intelligence agencies and to report their individual concurrence or dissent. In short, this was the scheme developed by the members of the JIS during the autumn of 1944, to which reference was made above.

After a year of vicissitudes that I shall not take time to relate, President Truman adopted the plan set forth in JIC 239/5. In January 1946 he established the Central Intelligence Group which, in September 1947, became the Central Intelligence Agency.

It is true to say that without William Donovan's initiative, in 1941 and again in 1944, there would have been no Central Intelligence Agency. All honor to him for that. But it is a mistake to suppose, as is commonly done, that CIA was based on the Donovan Plan of 1944. General Donovan himself knew better than that. Instead, CIA is based on JIC 239/5, which General Donovan stubbornly opposed.

The Central Intelligence Group set out to produce national intelligence estimates in accordance with the concept embodied in JIC 239/5. It was frustrated in that intention by the departmental agencies.

For reasons that I shall not take time to explain, they refused to contribute to CIG estimates, or even to meet with CIG to discuss them. The result was that CIG, later CIA, produced estimates based solely or primarily upon its own research and sent them to the heads of the departmental agencies for concurrence or dissent on a take it or leave it basis. The IAC (the predecessor of USIB) never met to consider an estimate.

That certainly was not what had been intended. In 1949 the Dulles Committee blamed CIA for it.

This matter was not straightened out until General Walter Bedell Smith became DCI, in October 1950. Through positive leadership, he then developed a cooperative relationship with the IAC. He established also the Office of National Estimates with the sole mission of producing national intelligence estimates in the manner that had been intended in JIC 239/5—that is, on the basis of departmental contributions, independent evaluation of those contributions, working level consultation with the contributors, and final consideration by the IAC (USIB).

In this context, the specialized research offices of CIA should be regarded as contributors on the same basis as the departmental agencies. ONE, working solely for the DCI as the Chairman of USIB, has no more commitment to them than to, say, DIA. They are represented in USIB by the DDCI.

This system has now worked well for 20 years, which proves that it is soundly conceived. Let me close by pointing out its particular virtues from the point of view of the user of the NIE.

First, it assures him that all of the intelligence resources of the Government have been brought to bear on the problem, and that every intelligence authority in the Government has been consulted.

Nevertheless, the power of decision with regard to the content of an estimate resides in one man, the DCI. This is, or should be, a protection against the evasions and obfuscations that characterize joint estimates. It should work to clarify any real differences that may exist among well informed men.

Third, any dissenter is forced to dissent within the context of a generally agreed discussion—not in an *ex parte* paper circulated separately.

Finally, the user has consequent assurance that all of the intelligence considerations bearing on his problem are contained in this one paper, under one cover.

After 20 years, these virtues may seem commonplace—but sometimes I sense that they are not fully understood and appreciated nowadays. To appreciate them fully, one has to know what it was like 30, or even 20, years ago. I have endeavored to give you some feeling for the difference between the present system and what went before.

It is written that those who disregard past experience are condemned to repeat it.

Genesis of a project

INTELLIGENCE IMPLICATIONS OF DISEASE

Warren F. Carey and Myles Maxfield

Outbreaks of meningitis in China are not unusual, but the winter of 1966–1967 was something else again. It began innocently enough with a few reports of school closings in Canton. News of this routine precaution turned out to be the signal for one of the worst series of epidemics to hit China in many years, and the beginning of Project IMPACT. The concept of this project—forecasting disease problems and epidemics, and the assessment of their effects on military and civilian activities—had hardly scratched the surface of implementation in the CIA's Office of Scientific Intelligence (OSI); but the opportunity was present in December 1966. China was in turmoil as millions of its people were participating in the Great Cultural Revolution. The demonstrations, riots, large dislocations of the population and general chaos attendant on this revolution were, epidemiologically speaking, some of the best ingredients for a successful epidemic. On the other hand, this mass upheaval had no precedent, there was no up-to-date quantifiable disease information of any sort on China and the status of China's public health conditions and medical capabilities were uncertain to say the least.

In the early stages of the project there was even uncertainty over the actual cause for the school closings in China. Two disease names, meningitis and Japanese B encephalitis, were being cited in reports describing the same outbreak in Canton (some reports combined both diseases into one—"Japanese B meningitis"). The confusion of reporting terminology was soon clarified. Distinct but similar Chinese words were being used to describe the disease; but which disease was it? Encephalitis is a viral disease, transmitted by mosquitoes, and is usually associated with seasonal periodicity of occurrence in warm weather. With the advent of colder weather the mosquitoes die and the disease subsides. By contrast, meningitis is a bacterial disease, having in temperate climates its greatest prevalence during cooler weather; although large outbreaks have occurred in hot, dry climates. The disease is mainly one of children and young adults and is more common where living conditions are crowded, as in barracks and institutions. The key to the correct diagnosis was a

report that cited the specific use of antibiotic nosedrops to treat encephalitis. The disease was thus remotely diagnosed as meningitis because antibiotics are not effective against viral encephalitis.

Identification of the etiology of this outbreak was crucial to our forecasting—meningitis had the greater potential for spreading rapidly from person to person by discharges from the nose and throat of infected persons. A significant point too was that the general pattern of behavior of meningitis epidemics tends markedly to repeat itself over a two to three year cycle. Thus it appeared that China was going to have an extended disease crisis. The first intelligence assessment was made in an OSI publication in January:

“It is becoming increasingly evident that Communist China is being confronted with a serious disease control problem. Factors suggest a breakdown of public health measures under the impact of mass movement of people, and perhaps the beginning of a series of new disease problems.”

Subsequent reports on the magnitude of the epidemic exceeded the prediction: travellers arriving in Hong Kong reported meningitis raging throughout Kwangtung Province, Radio Canton repeatedly advised people to guard against exposure to the disease—but it was too late. By mid-January, the epidemic in Canton was out of control, as supplies of sulfadiazine used in the prevention as well as the treatment of the disease became depleted. Red Guards took over the hospital facilities to care for their personnel only and some additional 900,000 visitors in Canton with the Cultural Exchange Program were exposed to meningitis. As the epidemic gained momentum, the entire public health infrastructure began to collapse.

A pattern of spread began to develop primarily to the north of Kwangtung Province. It became possible to predict a chronological sequence from one province to the next by tracing the movements of Red Guard units. In mid-January the epidemic was reported in Fu-ch'ing, Fukien Province (bordering Kwangtung Province on the northeast) and a Red Guard unit from Chi-mei diverted its march at this time to avoid Fu-ch'ing. At Ching-kang Shan, over 60,000 Red Guard each day were visiting the cradle of the Chinese Communist Revolution. Following an outbreak of the disease, the area was placed under quarantine. So it went—little being done to restrict mass movements until an outbreak occurred. In almost perfect order, meningitis infected one province after another all the way to the northeast Soviet border, and, as it struck, the movement and activities of Red Guards were hampered.

At this point, OSI analysts knew the identity of the disease and where it was going. The question now was how to quantitatively estimate the impact on the Chinese population? The only reports received were general descriptions such as "many sick and dying"; "many dead"; "no drugs"; "hospitals overcrowded"; "quarantines"; and "the most serious thing that has happened since the liberation." An attempt was made to model the epidemic on paper based on an analysis of outbreaks that have occurred in Western countries. In such disease outbreaks a very high percentage of people are known to carry the infection and about one-half to one percent of these will become ill with the disease. Given the estimated Chinese population in the infected provinces and also the ones in the path of the epidemic, a range of about 2.5 to 5.0 million cases was arrived at. It was an impressive range but descriptive accounts of the epidemic still appeared to be in excess of calculations.

The medical situation was presented to analysts in the Office of Economic Research (OER) who were able to complement the analysis. Projected population figures showed that there were 130 million children in the 0-4 age group and in the 0-24 age group about 500 million. Well over half of China's population consisted of young people—the very ones most "at risk" in a meningitis epidemic. It became apparent that in addition to the actual epidemic problems, considerable alarm and panic was being generated which could impede control of the disease. Real and imagined symptoms would initiate frantic appeals for medical assistance and drugs, thereby disrupting internal distribution systems. OER analysts also indicated that in addition to producing sulfadiazine, China imports small amounts of this drug to meet the normal requirements. Overall, there existed a close balance between supply and demand. The amounts needed for treatment based on the calculated incidence rate was small in comparison to that needed to provide broad prophylactic protection to a large segment of the population.

State Department officials were advised of these new developments. It was clear that an excellent opportunity was present to help "reduce tensions" between the U.S. and Chinese Governments by rescinding the U.S. ban on exports of drugs and other medical supplies. A formal offer to assist China in controlling the epidemic was made by the State Department. China did not respond to this gesture. Nevertheless, by February, shortages of sulfadiazine began to occur, with reports of many Chinese resorting to ineffective traditional medicines and urgent calls for sulfadiazine being placed on higher echelons by local health units. Soon thereafter China solicited

Western European and Asian pharmaceutical companies to make available substantial quantities of sulfadiazine. An accounting of the total amounts imported to China was attempted but much of the information was related to negotiations on purchase prices. At least several hundred metric tons were known to have been shipped between February and April to supplement China's internal production. Calculations based on chemoprophylactic dosage requirements (0.5 grams for children, 1.0 grams for adults each 12 hours for four doses) indicated that enough had been imported to protect about 100 million persons.

Chinese authorities broadcasted many appeals for "masses" of doctors and nurses to act in halting the contagious disease that was erupting and flowing from place to place. They then attributed the epidemic to medical workers who had not followed Chairman Mao's orders for the care of the country's 700 million persons. In retrospect, the "barefoot doctors" program to provide medical services and disease reporting in rural areas was a logical outgrowth of this massive epidemic. Whether the ensuing decline of the disease was due to the extensive use of sulfadiazine or to the normal decline of the epidemic cycle was never ascertained. It was followed by other predicted disease outbreaks (i.e., hepatitis, measles), and a recurrence of a much less severe meningitis epidemic in the winter of 1967-1968. As a postscript, China's failure to prevent and control the spread of diseases was viewed by the USSR as a fundamental weakness of the Chinese health services and the Soviet Ministry of Health abruptly rescinded the 1960 Sino-Soviet agreement on mutual abolition of vaccination requirements for travellers between these countries.

Project IMPACT went global in the summer of 1968 when a new strain of influenza rolled out of China and within a short period of time affected one out of every four persons in the world. The strain was not an unusually lethal one but it was only by chance that it was not. Again, various Agency sources provided the first indication of the beginning of this worldwide pandemic when the disease moved from China via travellers to Hong Kong in late June. An estimated 500,000 cases resulted in Hong Kong alone including 30 percent of the personnel at the American Embassy. At this time a unique opportunity was available to review statistical data on influenza (a program to computerize disease information to derive trends, cycles and predictions had already been initiated under a CIA Project called BLACKFLAG); the current epidemic in Hong Kong was causing the highest incidence since the first Asian Type A2 epidemic of 1957. While the epidemic appeared to be progressing in a new way, initial

curiosity subsided when a laboratory report from Hong Kong identified the strain as the common Type A2 variety.

Soon, however, separate reports from laboratories in Japan, U.S., and England identified antigenic (genetic) changes in specimens isolated in Hong Kong. Investigators at the Japanese National Institute of Health identified the Hong Kong influenza virus as a new Asian Type A3. In the U.S., isolates of the disease showed a magnitude of antigenic dissimilarity which had not been observed previously with Type A2 specimens. The World Health Influenza Center in London also noted an antigenic shift from previous A2 strains. Summarized findings noted: "the emergence of a new strain occurs every 10-15 years and together with rapid transportation, and in the absence of specific vaccines, leads us to believe that the disease may cause extensive outbreaks throughout the world in the coming months." Medical members of the Scientific Intelligence Committee were informed of these developments. The Defense Intelligence Agency member, in turn, alerted representatives of the Army Surgeon General's Office and following their conference with scientists at the Communicable Disease Center, an overall emergency plan was approved. Orders were issued to produce as rapidly as possible, large quantities of vaccine to protect military, public health and Government personnel, and civilians in high risk categories. The World Health Organization in August officially designated the new virus strain as Hong Kong/A2/68.

The race began in many countries to manufacture vaccine before the disease struck. Data was available on earlier flu epidemics from which could be derived a projected pattern of an eastward movement across Europe enabling a forecast of this spread. The disease would be in the Soviet Union about February, 1969, some two to three months after it reached Europe. Thus, the Soviets had an estimated seven month lead time, and reports on their progress in manufacturing and distributing Hong Kong flu vaccine were anticipated. Instead, the Soviets continued to vaccinate the urban population (about 75 percent) with the standard A2 vaccine which was shown even in August, to have very little protective value against Hong Kong flu (this decision later was reported to be based on their inability to make the new vaccine in less than a year and their gamble that A2 vaccine would help). By late January, the flu was present in many Soviet cities and incidence rates began to increase sharply. Central Asian areas also were facing their worst winter in 90 years as record snow fall and cold temperatures helped to disrupt medical assistance plans. A massive educational campaign on TV and local news media

was initiated in Soviet cities on how to avoid the disease. "Flu stations" were set up on corners to dispense cold remedies, but in the absence of specific prophylaxis, this effort was largely academic. Workers were given an extra day of sick leave in addition to the usual five days granted for flu cases. About 25 percent of the Moscow population was stricken (about 30 percent in Leningrad) and it was assumed that comparable figures occurred in most other population centers known to have been infected. The disease produced an ever widening ripple of effects on military and civilian activities (i.e., disruption of military training and industrial production schedules, which were costly in terms of sick relief payments, medical assistance, etc.). As the effects were felt in the Soviet Union they called the disease "Mao's flu." The direct and indirect cost of the epidemic was calculated to be several billion rubles.

Soviet health officials were criticized for their inept handling of the epidemic which caused considerable harm to the economy and to the health of the people. It caused five to six times as much illness as the total of all other infections. In response, health officials in the USSR recommended that they be freed from "petty supervision by dozens of incompetent authorities." The Soviet Medical Gazette in an excellent review of the controversy noted that in the absence of more specific preventive measures, scientists, doctors, and particularly the Soviet population, are still indebted to the practical health workers.

Influenza also reached Southeast Asia and project IMPACT was applied to forecast, and quantify the effects upon Viet Cong and North Vietnamese Army (VC/NVA) forces. A chronology of the times and locations of outbreaks was made from reports over the 1968-1970 period including any quantifiable figures on the rates of sickness and the frequency of VC/NVA requests for drugs and other medical supplies. There evolved a pattern which showed that the occurrence of influenza was a function of traffic density and personnel moving south from North Vietnam and coincided with the dry season when the bulk of all military supplies moved down the Ho Chi Minh Trail. Incapacitation rates ranged from about 40 to 70 percent and there was very good evidence that except for the isolation and quarantine of patients, no capability existed to specifically protect their military personnel by mass vaccinations. In December 1970, reports of outbreaks among VC/NVA forces in North Vietnam-Laos border area began to be noted with increased frequency—the stage was set for the beginning of the 1971 influenza epidemic there.

Staff personnel of the Special Assistant/Vietnam Affairs (SA/VA) were consulted, and together with their data on traffic routes, troop

concentration, and locations of waystations (Binh Trams), made it possible to construct a model of the direction of the influenza epidemic. Tchepone was a key junction on the Communist roadnet which extends into Southern Laos—if Tchepone became infected, the disease would move from Binh Tram to Binh Tram north and south in Laos and back to North Vietnam (see Figure 2). In late December there were indications that the NVA 4th and 16th AAA Battalions at Tchepone had become infected. It was estimated that in the primary infected area of Quang Binh Province the epidemic peak would occur about 30 January 1971 and in the secondary infected area south of Tchepone the peak would be about mid-February 1971. An overall 50 percent infection rate was calculated for VC/NVA personnel in those areas and it was estimated that one-half of those infected would be incapable of performing normal duties for about a week.

A warning was sent to indigenous intelligence teams operating in Laos and Cambodia to take special precautions during these peak influenza periods. Inasmuch as vaccination was not practical, an anti-flu drug, amantadine HCL, which had been shown to help prevent the disease was recommended for these teams. During February 1971, South Vietnamese army units entered Laos and conducted extensive operations near Tchepone and other areas in and near the primary infectious zone. Unfortunately, these operations took place just after the predicted time for the peak incidence. Combat effectiveness of committed VC/NVA forces probably was affected to a lesser degree by the declining incidence rate of influenza during February. This aspect was, however, difficult to quantitate.

The Future

Keeping ahead of meningitis and influenza required an extended all-out effort to assess, in each case, the disease with its special conditions so that the epidemic consequences could be projected. Analysts in what appeared to be completely unrelated fields of interest, all had significant bits of data to support and extend the forecasts. Disease intelligence can provide an initiating and vital role in the more familiar political, military and economic categories of intelligence. Project IMPACT clearly indicated that nothing is more international than diseases which recognize no political boundaries and few natural ones. Human diseases move freely across national frontiers and spread as conditions permit from one area to another. Even in the case of diseases of plants and animals, there is little doubt today that pathogenic organisms themselves are either already globally distributed or can rapidly become so. The appearance of something new

like Hong Kong influenza or the recent and costly spread of Venezuelan equine encephalomyelitis into the U.S. from Mexico can have demonstrable intelligence implications. Such disease events undoubtedly will occur in the future, and they will be much nastier to all facets of human activity.

Disease impact predictions require the retrieval and analysis of immense amounts of unclassified and classified data. This must be done in a very short time period if it is to be responsive to the current world disease situation. The techniques learned in working out the basic approaches on a few selected situations has led the Office of Scientific Intelligence to initiate an extensive effort to develop computer assisted working tools to retrieve the desired data quickly and to calculate statistical summaries and the probability of an epidemic spread. Mathematical models also are being designed for a multitude of epidemic diseases to give a rapid up-date and display capability. Project IMPACT depends upon such systems, but its best asset is still the cooperation of analysts in varied disciplines who help in the predictive processes.

On assessing timing

STRATEGIC WARNING: THE PROBLEM OF TIMING¹

Cynthia M. Grabo

A widely held concept about warning is that, as the hour of the enemy attack draws near, there will be more and better evidence that enemy action is both probable and imminent. From this, the idea follows naturally that intelligence will be better able to provide warning in the short term and will, in the few hours or at most days prior to the attack, issue its most definitive and positive warning judgments. Moreover—since there is presumed to be accumulating evidence that the enemy is engaged in his last-minute preparations for the attack—this concept holds that intelligence will likely be able to estimate the approximate if not the exact time of the attack. Therefore, if we can judge at all that the attack is probable, we can also tell when it is coming.

This concept of warning—as a judgment of imminence of attack—has strongly influenced US thinking on the subject for years. As of this writing, the official definition of strategic warning in the JCS Dictionary is, “A notification that enemy-initiated hostilities may be imminent.” More explicitly, the US national warning estimate of 1966 concluded: “Intelligence is not likely to give warning of *probable* Soviet intent to attack until a few hours before the attack, if at all. Warning of increased Soviet readiness, implying a *possible* intent to attack, might be given somewhat earlier.”

However logical these suppositions may appear in theory, they are not supported either by the history of warfare nor the experience of warning analysts, and in recent years more realistic assessments of this problem have begun to appear in warning papers and estimates.

For the fact is that warning judgments are not necessarily more accurate or positive in the short term and that assessing the timing of attack is often the most elusive, difficult and uncertain problem which we have to face. It is simply not true that the last few days or hours

¹ This article is adapted from a chapter of *A Handbook of Warning Intelligence* which the author is preparing for the training of intelligence personnel in analytical problems of strategic warning.

prior to the initiation of hostilities are likely to bring more and more specific indications of impending attack which will permit a better or more confident judgment that attack is likely or imminent. In many cases experience shows that the reverse will be true, and that there will be fewer indications that the attack is coming and even an apparent lull in enemy preparations. This can be quite deceptive, even for those who know from experience not to relax their vigilance in such circumstances. Those who do not understand this principle are likely to be totally surprised by the timing—or even the occurrence—of the enemy action. They will probably feel aggrieved that their collection has failed them and they will tend to believe that the remedy for the intelligence “failure” is to speed up the collection and reporting process, not appreciating that the earlier collection and analysis were more important and that a judgment of probability of attack could have been reached much earlier and should not have been dependent on highly uncertain and last-minute collection breakthroughs.

Principal Factors in the Timing of Attacks and the Attainment of Surprise

Nearly all nations, except in unfavorable or unusual circumstances, have shown themselves able to achieve tactical surprise in warfare. History is replete with instances in which the adversary was caught unawares by the timing, strength or location of the attack—even when the attack itself had been expected or considered a likelihood. Even democracies, with their notoriously lax security in comparison with closed societies, have often had striking success in concealing the details (including the timing) of their operations. To cite the most conspicuous example, the greatest military operation in history achieved tactical surprise even though it was fully expected by an enemy who potentially had hours of tactical warning that the massive invasion force was approaching. It was the Normandy invasion, in which deception played a major role in misleading the Germans.

It is not only by deception, however, that tactical surprise is so often achieved and that last-minute preparations for the attack can be concealed. A more important and more usual reason is that the indications of attack which are most obvious, and discernible to us are the major deployments of forces and large-scale logistic preparations which are often begun weeks or even months before the attack itself. Once these are completed, or nearly so, the enemy will have attained a capability for attack more or less at the time of his choosing, and the additional preparations which must be accomplished shortly

prior to the attack are much less likely to be discernible to us or may be ambiguous in nature. Staff conferences, inspections, the issuance of basic loads of ammunition and other supplies, and the final orders for the attack all are measures which require little overt activity and are not likely to be detected in time except by extraordinarily fine collection and rapid reporting—such as a well-placed agent in the enemy's headquarters with access to some rapid means of communications, or the fortuitous arrival of a knowledgeable defector. Even the final deployments of major ground force units to jumpoff positions for the assault may be successfully concealed by the measures which most nations take to insure tactical surprise—including rigid communications security and night movements. Thus, unlike the major deployments of troops and equipment which almost never can be entirely concealed, the short-term preparations have a good chance of being concealed, and quite often are. And, even if detected, there will often be minimal time in which to alert or redeploy forces for the now imminent attack, still less to issue warning judgments at the national level. Such tactical warning usually is an operational problem for the commander. Ten minutes or even three hours warning does not allow much time for the political leadership to come to new decisions and implement them.

Another facet of the problem of assessing the timing of attack is the difficulty of determining when the enemy's preparations are in fact completed, and when he himself will judge that his military forces are ready. It will often be particularly difficult to make this judgment with regard to logistic preparations. In fact, I can recall no instance in my experience in which it could be clearly determined that the logistic preparations for attack were complete, particularly since heavy supply movements usually continue uninterrupted even after the attack is launched. There has often been a tendency for intelligence to believe that all military preparations are completed earlier than in fact is the case—the discrepancy usually being attributable to the fact that the major and most obvious troop deployments had apparently been completed. Thus, even when intelligence has come to the right judgment on enemy intentions, it has sometimes been too early in its assessment of the possible timing of the attack.

In addition, the enemy command for various reasons may not go through with an attack as soon as the forces are fully prepared, or may change the date of the attack even after it has been set. A recent study has compiled some data concerning the frequency with which D-Days are not met, and the effects of this on the adversary's judgments. Of 162 cases analyzed where D-Days applied, almost half

(about 44 per cent) were delayed, about five per cent went ahead of schedule, and only slightly more than half (about 51 per cent) remained on schedule. The most common reasons for delay were weather and administrative problems, presumably in completing or synchronizing all preparations. Some attacks have had to be postponed repeatedly. For example, the Germans' Verdun offensive of 21 February 1916 was postponed no less than nine times by unfavorable weather.²

Such changes in plans have sometimes had notable effects on the opponent's assessments, particularly when he has gone through one or more alerts of impending attack which failed to materialize. Whaley notes that the finding that procrastination can help to generate surprise is explainable by the "cry-wolf" syndrome—whereby the false alert, and particularly a series of them, breeds skepticism or downright disbelief of the authentic warning when it is in fact received. "Moreover, the trend is that the greater the number of false alerts, the greater the chance of their being associated with surprise. . . . [The] Aesopian moral seemingly holds . . . , the false alarms serving mainly to undermine the credibility of the source and dull the effect of subsequent warnings. . . . It is ironic that . . . some of the D-Day warnings were quite authentic, the enemy having merely unexpectedly deferred the operation. The consequence was, of course, that several superb intelligence sources including Colonel Oster, Sorge and Rossler received undeserved black marks on the eve of their subsequent definitive alerts."³

Of all aspects of operational planning, the easiest to change and most flexible is probably timing. Once troops are in position to go, orders to attack usually need be issued no more than a few hours ahead, and the postponement of even major operations rarely presents great difficulties to the commander. Attacks have been postponed—or advanced—simply because there was reason to believe that the enemy had learned of the scheduled date. Obviously, among the simplest of deception ruses is the planting of false information concerning the date of operations with the enemy's intelligence services.

In addition to general preparedness, tactical factors and surprise, operations may be delayed for doctrinal reasons or to induce enemy forces to extend their lines of communication or to walk into entrapments in which they can be surrounded and annihilated. The delayed counteroffensive, designed to suck enemy forces into untenable advanced positions, is a tactic which the Communists have employed

² Barton Whaley, *Stratagem: Deception and Surprise in War* (Cambridge, Mass., MIT Center for International Studies, April 1969), pp. 177-78, and A-69.

³ *Ibid.*, pp. 187-188.

with devastating effect. Obviously, misjudgments of the enemy's intentions in such cases have been heavily influenced by the seeming delay in his response, which induces a false sense of security that he will not respond at all.

Political factors also may weigh heavily or even decisively in the timing of operations. This, of course, will be particularly true when (as is often the case) the nation in question intends to resort to military operations only as a last resort and hopes that the threat of such action will induce the opponent to capitulate. Obviously, in such cases, the decision of the national leadership that the political options have run out and that only force will succeed will be the determining factor in when the military operation is launched. In this event, operations may be deferred for weeks beyond the date when military preparations are completed, and the assessment of the timing of the attack may be almost exclusively dependent on knowledge of the political situation and insight into the enemy's decision-making process.

Still another political variant which may affect the timing of attack is when one nation is attempting to induce the other to strike the first major blow and thus appear as the aggressor. In this case, a series of harassments, border violations and various clandestine tactics may be employed as the conflict gradually escalates until one or the other power decides to make an overt attack. Clearly, the point at which this may happen will be very difficult to predict.

Apart from the various reasons noted above, there may be other largely tactical considerations which will affect the timing of attack. Weather, as already mentioned, is one of these—not only visibility, but in some cases winds, tides, moonlight or lack of it. Conditions of roads and terrain of course have been a major determining factor in when some operations will be launched. Military operations and logistic movements of Communist forces in Southeast Asia have traditionally been greatly slowed, if not halted altogether, at the height of the rainy season, and spring thaws on the plains of central Europe have delayed many operations. In cases where weather effectively precludes overland movement, it is of course highly probable that attacks will not occur. Nonetheless, there is always a chance that an enemy may choose to attack even in highly adverse conditions in the interests of achieving surprise.

As is well known, many attacks are initiated near dawn, for two reasons: the nighttime cloaks the final deployments of the attacking units, and the hours of daylight are desirable to pursue the operation. Several Communist nations, however, have shown a marked

favoritism for attacks in the dead of night. This has been particularly true of North Vietnamese and Viet Cong forces, which have shown themselves highly adept in night penetration operations and assaults. The USSR also has often launched attacks or other operations hours before dawn: the operation to crush the Hungarian revolt began between about midnight and 0330; the Berlin sector borders were sealed about 0300; the invasion of Czechoslovakia began shortly before midnight.

The USSR also has shown some favoritism for Sunday, both the Hungarian and Berlin operations having occurred in the early hours of a Sunday morning. It would be dangerous, however, to assume that this would be the case. The invasion of Czechoslovakia occurred, for instance, on a Tuesday night, slightly to the surprise of some who had come to expect Soviet operations to begin on Sundays. Whaley has found some preference for Sunday operations among Communist states but not in a majority of cases; it was true in only about one-fourth of the operations which he studied.⁴ Among other nations, there does not appear to be any evident preference for particular days of the week. In cases where Sunday is chosen, it is not for any anti-religious reason, but because the alert status of most Western nations is then usually lowest. The Japanese selected Sunday for the Pearl Harbor attack because their observations had shown that most US ships would then normally be in port.

Some Examples of Problems in Assessing Timing

Because of space limitations, discussion of more than a few examples is precluded, and even these must be covered briefly. There is considerable military historical writing, particularly on World War II, which may be consulted by those who wish to study this aspect in more detail, as well as the many examples in Whaley's previously cited work. Since much of this material is readily available, the examples below include only two from World War II with the remainder drawn from more recent intelligence experience.

The German Attack on Holland, Belgium and France, May 1940

World War II had been under way for eight months before Hitler finally launched his offensive against Western Europe in May 1940, the long delay in the opening of the western front having generated the phrase "phony war." All three victims of the final assault had ample and repeated warnings, and indeed it was the redundancy of

⁴ Whaley, *op cit*, pp. 180-181.

warnings which in large part induced the reluctance to accept the final warnings when they were received. The "cry-wolf" phenomenon has rarely been more clearly demonstrated—Hitler is said to have postponed the attack on the West 29 times, often at the last minute.

Owing to their access to one of the best-placed intelligence sources of modern times, the Dutch had been correctly informed of nearly every one of these plans to attack them, from the first date selected by Hitler, 12 November 1939, to the last, 10 May 1940. Their source was Colonel Hans Oster, the Deputy Chief of German Counterintelligence, who regularly apprised the Dutch Military Attache in Berlin of Hitler's plans—and of their postponements. Although in the end Oster provided one week's warning of the 10 May date, and there was much other evidence as well that the German attack was probably imminent, the Dutch ignored the warnings and failed even to alert their forces prior to the German attack. The Belgians, more heedful of the numerous warnings received, did place their forces on a general alert. The French, having also experienced several false alarms of a German attack, seem to have ignored the repeated warnings of their own intelligence in early May, including a firm advisory on 9 May that the attack would occur the following day. These instances also illustrate two fundamental precepts of indications intelligence: "more facts" and first-rate sources do not necessarily produce "more warning," and intelligence warnings are useless unless some action is taken on them.

The Soviet Attack on Japanese Forces, August 1945

This is one of the lesser studied World War II examples, but clearly shows the difference between strategic and tactical warning. The Japanese watched the buildup of Soviet forces in the Far East for about seven months (December 1944 through July 1945). They correctly judged that the USSR would attack the Japanese Kwantung Army in Manchuria, and they were able by July to conclude, also correctly, that the status of Soviet military preparations indicated that the USSR would be ready to attack at any time after 1 August. Despite this expectation which almost certainly must have resulted in a high degree of alert of the Japanese forces in Manchuria, the Kwantung Army had no immediate warning of the timing of the attack, which occurred about midnight on the night of 8–9 August.

The North Korean Attack on South Korea, June 1950

This was a notable example of both strategic and tactical surprise, and indeed one of the few operations of this century which truly may

be described as a surprise attack. Neither US intelligence, at least in its official publications, nor policy and command levels had expected the attack to occur, as a result of which there had been no military preparations for it. The South Koreans, despite many previously expressed fears of such an attack, also were not prepared and had not alerted their forces. Since strategic warning had been lacking, the short-term final preparations of the North Korean forces (insofar as they were detected) were misinterpreted as "exercises" rather than bona fide combat deployments. In considerable part, the warning failure was attributable to inadequate collection on North Korea—but the failure to have allocated more collection effort in turn was due primarily to the disbelief that the attack would occur. In addition, the "cry-wolf" phenomenon had in part inured the community—for at least a year, there had been about one report per month alleging that North Korea would attack on such-and-such a date. When another was received for June, it was given no more credence than the previous ones—nor, in view of the uncertain reliability and sourcing of all these reports, was there any reason that it should have been given greater weight. Although we can never know, most and perhaps all of these reports may have been planted by the North Korean or Soviet intelligence services in the first place. The attack is a notable example of the importance of correct prior assessments of the likelihood of attack if the short-term tactical intelligence is to be correctly interpreted.

Chinese Intervention in the Korean War, October–November 1950

Among the several problems in judging Chinese intentions in the late summer and fall of 1950 was the question of the timing of their intervention. Based on the premise that the less territory one gives up to the enemy, the less one's own forces will have to recover, the Chinese can be said to have intervened much "too late" in the conflict. And this conception of the optimum time for Chinese intervention strongly influenced US judgments of their intentions. From the time the first direct political warning of the Chinese intention to intervene was issued on 3 October (to the Indian Ambassador in Peking) until the first contact with Chinese forces in Korea on 26 October, all Communist resistance in Korea was rapidly collapsing as the US/UN forces were driving toward the Yalu. As the Chinese failed to react and the Communist prospects for recouping their losses appeared increasingly unfavorable, the Washington intelligence community (and probably the Far East Command as well) became increasingly convinced that the time for effective Communist intervention had passed. In the

week prior to the first contact with Chinese forces, the US national warning committee (then known as the Joint Intelligence Indications Committee, the predecessor of the Watch Committee) actually went on record as stating that there was an increasing probability that a decision *against* overt intervention had been taken.

Once the Chinese forces had actually been engaged, there was an interval of a month before they became militarily effective and launched their massive attacks in late November. Thus in this period the intelligence process again was confronted with the problem of assessing the timing of any future Chinese operations, as well of course as their scope. The four-week period produced many hard indications, both military and political, that the Chinese in fact were preparing for major military action. But there was virtually no available evidence when such action might be launched, and even those who believed that the coming offensive was a high probability were somewhat perplexed by the delay and were unable to adduce any conclusive indications of when the attack would occur. As is well known, tactical surprise was indeed achieved.

Even in retrospect, we cannot be sure whether the Chinese delayed their intervention and their subsequent offensive because of political indecision, the need for more time to complete their military preparations, or as a tactical device to entrap as many UN forces as possible near the Yalu. I believe that military rather than political factors probably delayed the initial intervention and that both preparedness and tactical considerations accounted for the delay in the offensive, but I cannot prove it. Others may argue—and they cannot be proved wrong—that the Chinese may not have decided inevitably on intervention by 3 October, and/or that negotiations with the USSR and North Korea may have delayed the intervention as much as military factors.

The Arab-Israeli Six-Day War, June 1967

There were many indications of the coming of this conflict. From 22 May, when Nasser closed the Gulf of Aqaba to Israeli shipping, tensions had been mounting, and the possibility of war was universally recognized. Both sides had mobilized and taken numerous other military preparedness measures. Before 1 June US intelligence was on record that Israel was capable of and ready to launch a preemptive and successful attack with little or no warning, and that there was no indication that the UAR was planning to take the military initiative. The US predictions of the likelihood and probable success of an Israeli

assault were highly accurate, although the precise timing and tactics of the operation, of course, were not known to us.⁵

The Israelis nonetheless achieved almost total tactical surprise against the Arabs in their attacks on the morning of 5 June, particularly in the decisively effective air strikes. The Israelis screened their plans by a combination of rigid security (there was no leak of their decisions or final military preparations) and an exceptionally well-planned and effective deception campaign. There were several facets of the deception plan, one of which was to lead Egypt to believe that the attack, if it occurred, would be in the southern Sinai rather than the north. In addition, numerous measures were taken in the several days prior to the attack to create the impression that attack was not imminent. These included public statements by newly appointed Defense Minister Moshe Dayan that Israel would rely on diplomacy for the present, the issuance of leave to several thousand Israeli soldiers over the weekend of 3-4 June, public announcements that concurrent Israeli cabinet meetings were concerned only with routine matters, and so forth. In addition, the attack was planned for an hour of the morning when most Egyptian officials would be on their way to work and when the chief of the Egyptian Air Force usually took his daily morning flight.⁶

The Invasion of Czechoslovakia, 20-21 August 1968

This case well demonstrates the impact on intelligence assessments of the seeming deferral of a military operation beyond the date when the forces appear to be ready, and when the intelligence community is psychologically most ready to accept the likelihood of such action. As will be recalled, the major deployments of the Soviet and Warsaw Pact forces for the invasion had largely been completed by 1 August, and it was at this time that US intelligence reached its firmest judgments—i.e., that Soviet forces were in a high state of readiness to invade if it was deemed necessary. When the Soviet Union did not invade in early August but instead reached a tenuous political agreement with Czechoslovakia, a letdown occurred and intelligence assessments almost immediately began placing less stress on the Soviet capability to invade. In fact, that capability was being maintained

⁵ See J. L. Freshwater, "Policy and Intelligence: The Arab-Israeli War," *Studies in Intelligence*, Winter 1969, for a discussion of the assessments made by the US intelligence community prior to the outbreak of this conflict.

⁶ A great deal of material on the Israeli planning has been brought to light, much of it unclassified. An excellent, unclassified summary of the techniques of deception and tactical surprise has been prepared by the Syracuse University Research Corporation, Syracuse, New York.

Warning

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and actually was increasing. Logistic activity was continuing at a high level—the USSR did not announce until 10 August the conclusion of its so-called rear services “exercise” which served as the cover story for the mobilization and forward deployment of the invasion forces. Moreover, substantial additional forces from the Baltic and Belorussian Military Districts were continuing to deploy into Poland in the first half of August. So long as this buildup continued, or was being maintained, the possibility of course was in no way reduced that the USSR sooner or later would exercise its military capability. Nonetheless, the psychological peak of our readiness for the invasion had passed well before it occurred. Since there was very little last-minute warning (such indications as there were mostly reached us too late), the USSR achieved effective tactical surprise against both the Czechoslovaks and ourselves.

The Czechoslovak case provides an outstanding illustration of the critical importance for warning of the judgment of probability of attack and of the lesser likelihood that intelligence will be able to assess the timing or imminence of attack. US intelligence in this instance, as in others, placed too great weight on short-term or tactical warning, and too little on the excellent strategic intelligence which it already had. Moreover, many persons (including some at the policy level who were aggrieved that they had not been more specifically warned) tended to place the blame on the collection system which in fact had performed outstandingly in reporting a truly impressive amount of military and political evidence, much of it of high quality and validity, bearing on the Soviet intention. The intelligence community, while clearly reporting the USSR's capability to invade, deferred a judgment of whether or not it would invade in seeming expectation that some more specific or unequivocal evidence would be received if invasion was imminent. On the basis of historical precedent and the experience derived from numerous warning problems, this was a doubtful expectation; an invasion remained a grave danger, if not probable, so long as the military deployments were maintained, while the timing was far less predictable. The history of warfare, and of warning, demonstrates that tactical evidence of impending attack is dubious at best, that we cannot have confidence that we will receive such evidence, and that judgments of the probable course of enemy action must be made prior to this or it may be too late to make them at all.

North Vietnamese Attacks in Laos and South Vietnam, 1969–70, 1971–72

As a final example of problems in timing, three instances of North Vietnamese attacks in Laos and South Vietnam provide quite striking

evidence of the problems of assessing timing of attacks even when the preparatory steps are quite evident and firm judgments of probable attack have been made.

Traditionally, in the seesaw war in northern Laos, the Laotian government forces have made gains in the Plaine des Jarres area during the rainy season, and the Communist forces (almost entirely North Vietnamese invaders) have launched offensives during the dry season (November to May) to regain most of the lost territory and sometimes more. In the fall of 1969, evidence began to be received unusually early of North Vietnamese troop movements toward the Plaine des Jarres, including major elements of a division which had not previously been committed in the area. As a result, the US Watch Report beginning the first week of October unequivocally forecast a major Communist counteroffensive. After eight consecutive weeks of this conclusion (qualified in later weeks by the proviso "when the Communists have solved their logistic problems"), it was decided to drop it—not because it was considered wrong, but because consumers were beginning to question repeated forecasts of an enemy offensive which had not materialized yet, and the impact of the warning was beginning to fade. In mid-January, evidence began to become available that preparations for an attack were being intensified, and a forecast of an impending major offensive was renewed. The long-expected offensive finally came off in mid-February, or four months after the troop buildup and the initial prediction of the attacks. The delay was not a surprise to experienced students of the area, who had learned that the North Vietnamese meticulously plan and rehearse in detail each offensive operation and that their attacks almost always were slow in coming.

Two years later in the fall of 1971, a very similar repetition of the North Vietnamese buildup in northern Laos began, again in October and again involving the same division, although this time there were indications (such as the introduction of heavy artillery) that an even stronger military effort would be made. Intelligence assessments again forecast major North Vietnamese attacks in the Plaine des Jarres but for the most part avoided any firm judgment that they were necessarily imminent. There was almost no tactical warning of the attacks which this time were launched in mid-December in unprecedented strength and intensity. Within a few days, all Laotian government forces were driven from the Plaine, and within three weeks thereafter, the North Vietnamese launched an offensive against government bases southwest of the Plaine.

Concurrently, the North Vietnamese were preparing for their major offensive against South Vietnam which finally kicked off on 30 March 1972 after months of buildup and intelligence and public predictions that an offensive was coming. Initial expectations, however, had been that the attacks most likely would come some time after mid-February, possibly to coincide with President Nixon's visit to China later that month. Once again, timing proved one of the most uncertain aspects of the offensive, and we remain uncertain whether Hanoi originally intended to launch the attacks earlier and was unable to meet its schedule, or never intended the operation to come off until the end of March. In retrospect, it appears that the forecasts of another "Tet offensive" in mid-February probably were somewhat premature, since the deployments of main force units and other preparations continued through March. Nonetheless, the intelligence forecasts were essentially right, and it could have been dangerous on the basis of the evidence available in mid-February to suggest that the attacks would not come off for another six weeks.

Growing Recognition that Warning is Not a Forecast of Imminence

It is from experiences like these (which are truly representative and not selected as unusual cases) that veteran warning analysts have become extremely chary of forecasting the timing of attacks. They have learned from repeated instances, in some of which the timing of operations appeared quite a simple or obvious problem, that this was not the case. In most instances, attacks have come later and sometimes much later than one might have expected, but even this cannot be depended on—sometimes they have come sooner. But except in rare cases any forecast of the precise timing of attack carries a high probability of being wrong. There are just too many unpredictable factors—military and political—which may influence the enemy's decision on the timing and a multitude of ways in which he may deceive you when he has decided.

This experience has finally borne fruit at the national estimative level. The last estimate to address possible warning of Soviet attack in Europe reversed the previous estimate (cited on the first page of this article) that warning of probable attack could not be given until a few hours before. It concluded instead that, once deployments and other military preparations had been largely completed, the chance of obtaining evidence of further military preparations would be greatly reduced, and that final warning that attack was imminent could likely be dependent largely on chance or other unpredictable factors.

For strategic warning, the key problem is not when attack may occur, but whether the enemy is preparing to attack at all—a judgment which we have a good and sometimes excellent chance of making with accuracy. Judgments often can be made, with less confidence in most cases, that all necessary preparations have probably been completed. A little less confidence still should be placed in forecasts as to when in the future all necessary preparations may be completed. At the bottom, and least reliable of all, will be the prediction of when the adversary may plan to strike.

Strategic warning is not a forecast of imminent attack. *Strategic warning is a forecast of probable attack* and it is this above all which the policy official and commander need to know. If we recognize the uncertainties of timing, we will also be less likely to relax our vigilance or alerts because the enemy has not yet attacked even though he is seemingly ready.

Letter from Helsinki

INTELLIGENCE SUPPORT TO THE US SALT DELEGATION

Howard Stoertz Jr.

SALT, as readers will know, means Strategic Arms Limitation Talks. The talks, which are now under way between the US and the USSR, represent an effort to put a brake on the competition in nuclear delivery systems which has been a major aspect of the arms race between the two countries over the past quarter of a century. The SALT talks began in November 1969, and many of the people involved have been at it almost continuously since then. On 20 May 1971, after the most searching discussions ever held between the US and USSR on subjects of such vital importance to their national survival, the two governments agreed to concentrate for the rest of this year on working out an agreement to limit ABM systems, and, together with concluding such an agreement, to agree on certain measures with respect to the limitation of strategic offensive arms.

In the session now under way here in Helsinki, the two delegations are seeking to carry out this guidance. The present session, called SALT V to signify the fifth round, began in July. It has now lasted nearly eleven weeks and is almost over. We will soon have a recess of perhaps a month for consultation in capitals, and will then reconvene at the alternate site of the talks in Vienna for yet another round.

There are a number of considerations which have brought the two countries to the conference table to discuss strategic arms limitation. Among the most important are: first, the emergence on each side of a perception of mutual deterrence and rough strategic parity; secondly, an awareness on each side that another round in the arms race, while costly in terms of human and material resources, would almost certainly not bring greater security to either country; third, the availability on each side of intelligence collection systems capable of monitoring the military programs of the other country without infringing on its territory. At SALT, intelligence resources fitting the foregoing description are called "national technical means of verification." In the SALT context, such systems appear to be tolerable to the USSR, whereas aerial overflight and on-site inspection continue to be anath-

MORI/HRP PAGES 93-105

SECRET

93

SECRET

SALT

ema to the Soviet leadership. Thus developments in intelligence techniques and capabilities, required by the imperatives of the race in strategic armaments, have helped create conditions in which the two sides can move toward verifiable limitations on such armaments.

I am not at liberty to discuss the substantive details of the SALT negotiations. Respect for the privacy of the talks by all participants on both sides has been one of the characteristics which signify the serious and business-like way in which the two sides have proceeded. When the outcome is known, readers will be able to judge the results for themselves. My purpose in this report is to describe the delegations, the negotiating procedures, and the role of the CIA representation.

The Delegations

Someone around here once said "This isn't a delegation, it's a convention." By that he meant that the various members of the US delegation represent the several constituencies in the US government which have an interest in strategic defense programs and policies. He also meant that these constituencies have a certain independence (based on their statutory responsibilities), have certain points of view, and very definitely do not agree on certain issues. Participation by more than one constituency also characterizes the Soviet delegation.

The head of the US delegation, Ambassador Gerard C. Smith, is a lawyer with long experience in atomic energy and arms control matters. Smith is Director of the US Arms Control and Disarmament Agency. The four other presently active delegates are: Ambassador J. Graham Parsons, a career foreign service officer from the Department of State; Mr. Paul H. Nitze, former Secretary of the Navy and Deputy Secretary of Defense and now a consultant to the Secretary of Defense; Lt. Gen. Royal B. Allison (USAF), Assistant to the Chairman, Joint Chiefs of Staff, for Strategic Arms Negotiations; Dr. Harold Brown, president of Cal Tech, former Secretary of the Air Force and now a member of the General Advisory Committee of ACDA. There are two inactive members: Ambassador Llewellyn Thompson, former US ambassador to the USSR; and Mr. Philip Farley, Ambassador Smith's deputy at ACDA.

On the USSR side, the head of the delegation is Minister V. S. Semenov, the senior Deputy Minister of Foreign Affairs. (There is a first deputy, so Semenov is number 3 in seniority in the MFA.) The four other active delegates are: R. M. Timerbaev, a Deputy Chief of the International Organizations Division of the MFA and

head of its U.N. component, who has taken over the SALT responsibilities of the ailing chief of the disarmament component of that division (there is no independent agency in the USSR equivalent to ACDA, and arms control matters are handled within the MFA); Lt. Gen. K. A. Trusov, a member of the Soviet General Staff; A. N. Shchukin, member of the USSR Academy of Sciences and chairman of its Scientific Council for the Propagation of Radio Waves; P. S. Pleshakov, a Deputy Minister of the USSR Radio Industry. There are two inactive members, Col. Gen. N. N. Alekseyev, a Deputy Minister of Defense, and Col. Gen. N. V. Ogarkov, First Deputy Chief of the Soviet General Staff.

The delegations are teams representing their governments, but as their positions indicate, each delegate can also be said to represent a constituency and point of view within his government. This is also true of the advisors. On the US delegation at Helsinki there are 21 advisors and four interpreters. Six of the advisors are from ACDA, 3 from State, 5 from OSD, 4 from JCS, and 3 from CIA. That is to say, 3 of the 6 listed as State are from CIA—at the outset of the SALT talks serious consideration was given to acknowledging the CIA representation openly, but it was decided that it would be preferable not to do so, taking into account both Soviet sensitivities and CIA's general policy with respect to its employees overseas. The USSR delegation at Helsinki has 17 advisors and 4 interpreters; of the advisors, 12 are listed as MFA and 5 as Ministry of Defense. No one is listed as KGB or GRU, but our records show that two advisors are known or believed to be from these organizations.

The two leaders of the US advisory group are Ray Garthoff, an authority on Soviet strategic affairs who is Deputy Director of the Bureau of Politico-Military Affairs at State, and Sid Graybeal, a missile expert who is Deputy Assistant Director, Bureau of Science and Technology, ACDA. These men are, respectively, Executive Officer and Alternate Executive Officer of the delegation. Both were once employed by CIA, where they dealt mainly with estimates and analysis of Soviet strategic capabilities and policy. Incidentally, the general secretary of the Soviet delegation, N. S. Kishilov, whose role corresponds to Garthoff's is a former military attache and GRU officer.

The foregoing sounds like a couple of pretty compact little groups, 30 on the American side and 26 on the Russian. Readers may be surprised to learn that the American contingent at SALT in Helsinki actually comprises 85 people, counting all those others without whom the delegates, advisors and interpreters could not function. There are secretaries, administrative and security officers, documents officers,

communicators, Marine guards who provide 24-hour protection for our offices, and even a press officer whose job is to fend 'em off. The Russian contingent is somewhat smaller. This is mainly because the US delegation runs an office complex which is independent of the US embassy in Helsinki (and about 5 miles from it), whereas the USSR delegation moves into the large and very well staffed Soviet embassy, making use of its space and supporting personnel. Also, for some reason, the Russians do not seem to feel any need for a press officer.

The Negotiations

The fate of nations may once have been decided by ministers plenipotentiary, but not any more. The US delegation receives its guidance directly from Washington on an almost daily basis. This extremely close contact is attributable to the importance of the subject, the participation of various constituencies in Washington, the NSC system as operated by the President and Dr. Kissinger, and the efficiency of modern communications. We have three separate communication setups here—State commo serving the delegation as a whole, and in addition State and ACDA; JCS commo serving JCS and OSD; and CIA commo serving CIA, the head of delegation, and the White House. Through these communications means, which include an excellent secure voice telephone link manned by State but available to us all, we keep in constant touch with our masters and helpers back in Washington.

The US delegation negotiates on the basis of both broad and detailed guidance in the form of National Security Decision Memoranda staffed through the NSC system and approved by the President. The Washington staffing is performed by the same agencies that are represented here, and CIA has an active role in that work. The role of the delegation here is to carry out the guidance, make the best possible case for the US point of view (talking to the USSR delegation and through it to Moscow), receive and interpret and where necessary rebut the Soviet arguments, expand areas of agreement and narrow areas of difference in the positions of the two sides, and, on all important issues, report to Washington and get new guidance. The delegation recommends to Washington that the US stick, compromise, fall back, or try a new tack as it considers appropriate.

All of this takes time, in varying amounts depending on the difficulty and importance of the issue, the unanimity or lack of it within the delegation and among the various agencies in Washington, the need for homework, and the competition of non-SALT matters for the time and attention of the top US policymakers. These factors are at work

on the other side as well. A Soviet representative once said it took Moscow at least three weeks to respond to a major new proposal. Once recently, when we had every reason to believe the USSR delegation already had a prepared response in its pocket, they delivered it in a week. We have the impression that the Russians here limit their activities a bit more strictly to negotiating than we do, and that they rely more exclusively on staffing from Moscow. (One indication is that nine of the US advisors have scientific degrees, whereas at most three of the Soviet advisors have such training.) On the other hand, the USSR delegation once made clear that a new proposal they tabled had been originated by the delegation and approved by Moscow. Be that as it may, they are certainly skilled negotiators.

Until recently, the meetings themselves have been held two or three times a week in plenary session. At a plenary session the heads of delegations each make formal set-piece statements, often on unrelated topics because varying time intervals are required for staffing. Such a session is always followed by informal discussion in which ideas and argumentation are swapped by all participants. This technique alleviates the problem caused by the fact that the formal speeches tend to pass each other like ships in the night. Much of the business of the negotiations is thus conducted in informal conversation, with everybody rushing back to the office afterwards to write up his memorandum of conversation.

Formal sessions are invariably handled through interpreters, as are informal discussions between Smith and Semenov. The other informal discussions use a fascinating combination of methods to bridge the communications gap. The military delegates and advisors congregate in one corner using military interpreters. Nitze, Brown and Shehukin use French, Parsons and Timerbaev English, Garthoff and Kishilov Russian, and the rest of us patch a conversation together with whichever language we can. Ten of the Soviet advisors speak English (a consideration in their appointment?) while only four of the American advisors speak Russian well enough to use it in conversation, so English is used most of the time. At each of the plenary meetings just described, the CIA advisory group has a seat, mostly for purposes of observing and self-fulfillment, and we have shared this opportunity among us.

Recently, to break the formality and get more accomplished at this stage of the negotiations, the heads of delegations have tended to replace the plenaries with more frequent, smaller meetings of delegates (called "mini-plenaries" and "troikas") and working groups. Representation at such meetings has been limited to delegates and

SECRET

SALT

advisors from the policymaking agencies. While we CIA representatives have not been invited to attend the meetings, we participate actively in the preparation of papers and provide intelligence support to those attending. The pace has become quite intense as the Helsinki session draws to a close: in the past two weeks there have been 13 scheduled meetings with the USSR side, plus a number of unscheduled preparatory and cleanup meetings.

The fact is that some of the most important negotiating has occurred not in scheduled meetings at all, but in private meetings between Smith and Semenov, including some held during recreational trips arranged for the two delegations by the governments of Austria and Finland. Key exchanges between our two chiefs have occurred on a railroad train in Finland, on a boat trip in the Carinthian Alps in Austria, and on another boat trip on Lake Saimaa in Finland. In Carinthia the exchange was so important that the heads of delegation requested that the boat, with the rest of the delegations and our Austrian hosts aboard, make another loop around the lake, so they could keep talking. This prompted one member of our delegation to remark that he had heard of a negotiation going in circles but this was the first time he had literally engaged in one. It illustrates, however, the dedication of Smith and Semenov to their task and one of the many contributions made by the host governments.

CIA Representation

We comprise three intelligence officers, a secretary, and three TDY communicators. The communicators man the shack at the office and beef up the capability [] to handle the volume of relay traffic.

At SALT I, the first of the five sessions held so far, CIA was represented by one intelligence officer who was accredited as an advisor and one who was assigned TDY [] and acted in support. Later, the two participating directorates, DDI and DDS&T, each sent co-equal advisors. A watch officer was added to handle incoming current intelligence materials and other reports. The representatives were changed at each session, and also in the middle of the very long second session. The present pattern evolved from this background during SALT III and obtains now at SALT V.

I am a DDI officer and am the senior CIA representative. I am signed on for the duration. There is an experienced DDS&T officer, chosen for his knowledge of SALT and of the scientific fields expected to be at issue in this session, and an OCI officer with a background in Soviet affairs who has previously supported the SALT effort from the

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Washington end. These individuals will be exchanged for others at the next session. This combination seems to work well; it represents the interests and capabilities of the participating directorates, it maintains continuity of senior representation, and through the rotation it allows a number of people to feel the pulse of the negotiations.

Intelligence support to the delegation does not begin and end here in Helsinki. The CIA communications I have described lead back to the SALT support unit in Langley, presided over by an assistant to the director of OSR. This unit comprises an OSR officer, a DDS&T officer, and an OCI officer, who are the funnel through which passes our requests for support and the information and analysis generated for our use by the analytical and estimative components at headquarters. The headquarters unit also coordinates CIA's staffing of SALT-related studies within the NSC system in Washington.

Role of CIA Representatives

The CIA advisors here at the site of the negotiations perform four main functions: we report to the delegation on the latest intelligence information and analysis; we respond to requests by delegates or advisors for intelligence backup; we provide advice and assistance to the delegation in its general work, including the preparation of statements, talking points and other negotiating material; we keep our headquarters in Washington informed about what is going on here. In addition, we have certain special responsibilities in the fields of security and communications.

In carrying out these functions, we take as our model the role CIA plays within the NSC system in Washington. In the NSC, the agency is a participant with limited and clearly-defined responsibilities, not to make or recommend policy, but to contribute an objective view of the facts, to project from the facts to estimates of present and future capabilities and intentions, and to evaluate the consequences of given courses of action. We play it down the middle and we try where possible to represent the views of the intelligence community as a whole. This is in keeping with the DCI's role and reflects the special responsibility we bear as the only intelligence representatives here with the delegation.

While CIA does not make policy, it has a role in carrying it out, and this obtains with special force in the field of strategic arms limitation. The prerequisite for Soviet participation in any agreement is that compliance will be monitored by "national technical means of verification." This makes it essential that the terms of any agreement be consistent with the actual capabilities of US intelligence to monitor

Soviet activities without the aid of any on-site inspection within Soviet territory. Nobody expects intelligence to be able to supply 100 percent assurance of compliance with an agreement—the requirement is reasonable assurance that non-compliance could be detected and recognized in sufficient time so that US security would not be jeopardized. But in a game where the stakes are as high as in this one, reasonable assurance needs to be high assurance indeed. On this matter, we too represent a vitally interested constituency and reflect its point of view.

To fulfill the task of *reporting information and analysis*, we receive a daily flow of current intelligence issuances electrically, we receive twice-weekly pouches containing analytical reports selected by the Langley support team, and we receive pertinent NIE's (conclusions electrically, full texts by pouch). Our day starts with a rush. We screen the Central Intelligence Bulletin (CIB) and other current support traffic, select items of importance, and hand-carry a briefing book to the delegates and the two leaders of the advisory group. The hand-carrying is partly to provide prompt, personal service, partly to permit an opportunity for dialogue, and partly to retain physical custody over the most highly classified materials. The briefing rounds start when Ambassador Smith arrives, usually at 0830, with the objective of getting to all recipients before the first delegation meeting of the day, usually at 1000. For what it's worth, the time difference (Helsinki is six hours ahead of Washington) means that the US SALT delegates see the day's CIB long before their colleagues at home.

For the daily briefing we choose items related to strategic arms and arms limitation, items dealing with the USSR and China generally, Europe and the Mid-East, and occasionally other items of interest. We aim not to overload and not to duplicate the excellent State daily summaries and the press services. The Finnish News Bureau, for example, supplies both delegations with excellent four-page summaries of press highlights in English and Russian, which are delivered to our hotels each morning before breakfast. We aim, however, to keep the delegates abreast of developments they are specially interested in and of the intelligence reports their colleagues and superiors back home will be reading when they arrive in their offices.

Using the less highly-classified materials, we publish a Daily Intelligence Summary for delegates and advisors each morning. FBIS transmissions of key Soviet statements and articles are also culled for this publication. The FBIS Washington and London offices keep us in mind when we are in the field, and have given us fast service on such things as a recent Red Star series on US military programs and

a recent article by the head of the USSR's American Institute evaluating the new departure in US-Chinese relations. The more highly classified materials, both current intelligence and intelligence memos and reports, are kept on a reading table in our offices for use as needed by other advisors.

One other current reporting service we perform is to brief the delegates and advisors promptly on an all-source basis on the products of US technical intelligence collection programs as they are acquired, using the CIA preliminary assessments of the intelligence significance of the acquisitions together with supplementary materials supplied in response to our specific questions. These briefings serve to keep the US delegation up to the minute on the strategic situation. They also serve to keep the delegation aware of what intelligence can and cannot do in collection and analysis corresponding to that which would be used in monitoring a strategic arms limitation agreement. The briefings are real case studies—not just staff studies—of our unilateral verification capabilities.

In *responding to requests*, we burn up the wires to Washington. Many needs can be met with our own resources—we brought 450 pounds of files with us to Helsinki, and between the three of us we have a pretty good collective memory. But in the eleven weeks of this session we have cabled 51 messages to the Langley support team and received 69 messages from headquarters in addition to the regular flow of current intelligence materials. One reason for the discrepancy in numbers is that the Langley group anticipates some of our needs and some of our requests are phoned in.

The topics covered in these exchanges range from the trivial to the crucial. For example, a recent day's traffic to headquarters included a request for an aerial photograph of the town of Grand Forks, North Dakota (near a US Safeguard ABM site under construction), a request for comments on a self-initiated analysis of a new Soviet proposal on ABM limitations, and a warning that we will soon need new guidance about whether or not CIA can live with a restriction the USSR delegation insists be included in the draft provision on verifying an agreement. Our policy is to accept most any request with an intelligence flavor, and to initiate some ourselves, in order to be of maximum assistance to the delegates and advisors from the other agencies. We take full advantage of the fact that CIA is an incomparable storehouse of information, its analytical capability is unsurpassed, and the rapidity of its response is unbelievable. (Here the time difference works in our favor—we can send in a request in the afternoon at Helsinki, it can be researched all day at headquarters, and we can have

SECRET

SALT

the answer at opening of business the next day.) Our aim is to stay a couple of jumps ahead of the negotiations, to anticipate likely needs, and to have the answer available when the question arises.

Providing *intelligence advice and assistance* to representatives of policymaking agencies is a tricky business. Every intelligence officer who has dealt with policymakers has faced the question of when to speak as an intelligence representative, when to speak as an individual, and when to keep silent. The distinction between intelligence support and policymaking must be consciously maintained by the intelligence representative. One's judgment about the proper role of the agency plays a part, as does one's personality. It should also be noted that there is a difference between the DCI's credentials at the NSC and ours here. The DCI is appointed by the President and by decision of the NSC stands in the relationship of permanent advisor to that body. The SALT delegates were appointed by the President, but the senior CIA representative at SALT was not, and the fact of CIA representation is not acknowledged to the other side. So we stick pretty close to our knitting and remain in the background.

In providing general advice and assistance to the delegation, the CIA group here at Helsinki divides its tasks as follows: Ron Stivers, the OCI man, handles the current intelligence account; Maury Lipton, the DDS&T man, represents us at meetings of the advisors' working groups; I represent us at delegates' meetings. The advisors' working groups draft and polish speeches and talking points for use in negotiating with the other side. The delegates' meetings initiate these papers, give them a final review, and review delegation proposals or requests for guidance to Washington (these are drafted mostly by Garthoff). We CIA representatives frequently draft and always comment on papers dealing with Soviet capabilities and programs and papers dealing with monitoring compliance with an agreement. As members of the delegation with experience in SALT matters, we comment and make suggestions on papers dealing with other subjects as well.

I am not included in those delegates' meetings which deal with the tactics of the negotiations (neither are other advisors, except Garthoff). I don't really think CIA should devote much attention to tactics, especially in light of the fact that the US SALT delegates are by now much more familiar than anyone else with the attitudes and reactions of their Soviet counterparts. But I am bound to say that meetings which start out on tactics often end up covering a very broad range of topics on which I think afterwards that a CIA contribution might have been helpful. This exclusion is also a potential handicap in that if we don't watch out, we can get left behind on delegation

thinking about important issues. We guard against this danger by hustling and maintaining good contacts.

We keep headquarters informed about what is going on here by supplementing the flow of communications which the delegation sends to Washington via State channels for distribution to the NSC agencies. The cabled output of the delegation is very rich: there is always a cable summarizing each plenary or mini-plenary meeting, cables are prepared on each Smith-Semenov informal discussion and on other informal discussions of substantive importance, and delegation thinking about the state of play is often summarized in cables requesting further guidance from Washington. We alert headquarters to particularly urgent or sensitive delegation cables, send selected memos of conversation electrically to speed up their receipt by the agency, add nuances about issues and views gleaned from our participation in meetings or talking with participants afterwards, and offer our own opinions about important developments.

In performing this service, we are looking after the interests of our constituency. The aim is to help our colleagues at home participate as effectively as possible in the ongoing work on SALT within the NSC machinery, and to alert them to developments which affect CIA's very special interests in the verification of an agreement.

In this connection, it should be noted that we have no intelligence collection assignments here, nor do any other delegates or advisors. Of course the very process of discussing strategic arms limitations with the Soviet delegation is a source of information about Soviet strategic thinking, policy, and technology, and we keep on the alert for revelations of potential intelligence significance. But we are here to assist in the negotiations, not to service collection requirements.

With respect to *security* of classified information, in addition to routine duties like maintaining physical custody of sensitive intelligence documents, we have the special responsibility of advising the delegation about what intelligence information can be disclosed to the other side. With our help the delegation strikes a careful balance between the need to protect US intelligence sources and methods (for example, by being circumspect about precisely what we know and don't know) and the need to conduct a dialogue which will lead to verifiable arms limitation.

It must be obvious that consideration of the ability of the two sides to monitor various activities by using "national technical means of verification" has had to play a part in the dialogue about what is to be included in an agreement. It is equally obvious that a dialogue requires a base of common understanding and terminology. The USSR's

pathological concern for security allows the Soviet delegation almost no latitude in these matters. They cannot disclose state secrets, which include even such things as the names the USSR gives its missiles. The two delegations therefore discuss Minuteman and SS-9 ICBM's, BMEW's and Hen House radars, and Spartan and Galosh ABM's, using the US terms for the Soviet systems. When necessary, the US side gets facts about Soviet forces and programs onto the table, to help make our points or to ensure common understanding by both delegations. The other side never confirms these facts, and there is strong evidence that our information about the USSR's forces is often news to the civilian members of the Soviet delegation.

So the responsibilities of the CIA representatives include making judgments—in consultation with headquarters when necessary—about the gains and risks of disclosing various items of intelligence information before they are released. In this connection, we review and clear all statements provided to the Soviet side by the US delegation. We sometimes tease that the texts we approve for release should be reclassified "SECRET NOFORN EXCEPT USSR."

Finally, CIA *communications* serve the head of delegation by providing him with a direct, secure link to the White House. Since messages on this link are EYES ONLY and are not read by the CIA advisors, we are involved only peripherally—for example, when the communicator wants advice about how to handle an IMMEDIATE which arrives in the wee hours. This communications link, however, is an important supporting service and one for which Ambassador Smith gives CIA high marks.

In Conclusion

Why are we here? Because CIA has committed itself just as firmly to contributing to verifiable arms limitation as it has been committed over the years to providing intelligence support for adequate defense programs. Because it is understood that the national interest requires the US negotiating team to be armed with up-to-date information and objective analysis of Soviet capabilities, programs and policy. Because it is essential that the terms of any SALT agreement be compatible with the collection and analysis capabilities of US intelligence.

What will be the outcome? The arms race will not be stopped by any measures we agree on in this phase of the negotiations. There is a good chance, however, that after 25 years of strategic arms competition between the US and the USSR, the SALT talks will result in the application of a brake. If this is the outcome, the investment of time and energy will have been worth it.

What will be the implications for CIA? Intelligence will have acquired still another responsibility—that of monitoring the USSR's compliance with a set of agreed limitations on strategic armaments. In the agreement, the USSR will have acknowledged explicitly the acceptability of "national technical means of verification" and will have pledged not to interfere with the use of such means. The actual task of monitoring will probably not require much adjustment in our collection and analytical priorities, which are already designed to maintain close watch on those Soviet military programs that will be the subject of any SALT agreement. But intelligence will be confronted with the difficult challenge of being able to advise the policymaker with high assurance as to whether or not the USSR is complying with the terms of the agreement.

Helsinki, 17 September 1971

Postscript—

My article, written in September 1971 during SALT V, does not accurately reflect the situation at SALT VII, the session which concluded with the signing of the ABM Treaty and Interim Offensive Agreement in Moscow on 26 May 1972.

At SALT VII, the US Delegation functioned as a unified team, and a highly effective one at that. In the final stages of the negotiations, the differences among constituencies within the Delegation and in Washington were minimal. This trend was actually emerging before September 1971, but I failed to recognize it at that time.

Also, at SALT VII the senior CIA advisor was invited to participate in virtually all deliberations of the US delegates. The minor problems I felt earlier, arising from my exclusion from certain meetings, no longer existed.

Finally, in the last days of the negotiations, the role of CIA communications became much more important than indicated in the article.

Helsinki, 31 May 1972

NOTES AND COMMENTS

MEMORANDUM FOR: Editor, *Studies in Intelligence*

SUBJECT: CIA's Role in Combatting the Desert
Locust in Africa and the Middle East

REFERENCE: "Intelligence in the Ecological Battle,"
Studies in Intelligence, Vol 14, No 2,
Fall 1970, p 123-126

Having read the article, "Intelligence in the Ecological Battle," I must commend the OCI analysts for describing how CIA can make a significant contribution to an environmental problem. On the other hand, I was chagrined at the major oversight of recognizing some of the major contributors to the intelligence effort. While I do not wish to discredit OCI, I feel the role of all those that contributed should be placed in proper perspective.

The major significance of this particular episode was the excellent example of how the intelligence community can work together through USIB with CIA as the focal point. This demonstrated the ability of the intelligence community to react to a problem requiring a coordinated input of scientific, economic and political information. It also established a mechanism for formal and informal workable cooperation with non-USIB Government Agencies.

OCI did write the fortuitous item which initiated the subsequent fervor the authors described. The subsequent events, however, were shaped mainly through the combined coordinated efforts of CIA, Air Force, State Department and the Department of Agriculture. This work was accomplished through the aegis of the Scientific Intelligence Committee/USIB, and in particular the Biomedical Intelligence Subcommittee/SIC. Because of this, the Life Sciences Division/Office of Scientific Intelligence/DDS&T played the role of coordinator of this intelligence activity. This role was served from the time the US Air Force became interested through the period of actual Air Force spraying operations in Saudi Arabia. BMIS designated OCI as the coordinator of cable traffic for community use. OCI also wrote further timely *OCI Weekly* items staying abreast of the current situation.

MORI/HRP PAGES 107-108

SECRET

107

Summary of significant events relating to the problem:

- a. 23 May 1968—Gordon Torrey/OCI published initial item in the *Central Intelligence Bulletin*.
- b. Early June 1968—The Undersecretary of the Air Force requested, through the Office of the Surgeon General, that the intelligence community assess the desert locust problem.
- c. 17 June 1968—First of a series of meetings of BMIS/SIC/USIB in response to the Undersecretary. Participants included selected BMIS members who had a direct interest plus invited guests from DDI/CIA, USDA, State and AID.
- d. June 1968—Intelligence Memorandum—"The Desert Locust Threat"—CIA/BGI/GM 68-5.
- e. 22 July 1968—OSI-STIR/68-17, "The Desert Locust Threat in the Middle East and North Africa." Substantive contributions came from OBG and OER. This served as the intelligence community's response to the Undersecretary's request.
- f. 13 January 1969—Special briefing for USAF Desert Locust Survey Team assigned to Saudi Arabia. Briefers were from OSI/DDS&T and USDA.
- g. 19 February 1969—OSI participated in debriefing of USAF Survey Team.
- h. March 1969—USAF spray team conducted a successful spraying operation in Saudi Arabia.

It should also be pointed out that while OCI indicated in October 1968 that Saudi Arabia was the major obstacle to the antilocust effort, the coordinated OSI-STIR described the importance of Saudi Arabia in July 1968.

Again, OCI should take its credit due; however, the coordinated community effort was by far the most significant in terms of elucidating the problem and getting the job done. It is unfortunate that this point was not made clear in the published *Studies in Intelligence* article. I hope this information will serve to set the record straight.

RICHARD C. HOSLER

MORE ON "LUCY"

The article "The Rote Drei: Getting Behind the Lucy Myth"* is an admirable contribution to the literature of an important case. However, it is unlikely that its readers will feel that they have been taken very far behind the myth; instead, the article's effect is to perpetuate it. Even the author seems to entertain doubts, since a sentence on his penultimate page would tend to dismiss much of his previous argument. (I will return to this sentence later.)

Part of the article's argument, and symptoms of its weakness, are to be found in three phrases:

(1) "The record clearly shows that Lucy had four important sources." (P. 63).

The author is, of course, referring to the record of the transmissions from Switzerland, which however bulky is still inadequate for any such conclusion. There is no record of the transmissions from Germany to Switzerland, which would offer a more solid base from which to speculate on the identity of the sources. Any material received from Germany—in whatever form—could have been edited in Switzerland, perhaps partly to hide the identity of the source or sources.

(2) "Rudolf Roessler *did* divulge the identity of his sources . . . to a trusted friend." (P. 64).

In addition to the author's apparent assumption that the four "divulged" by Roessler are identical with the four "clearly shown by the record," other assumptions underlie this phrase, some of them perhaps naive:

- the assumption that (Roessler) had sources,
- the assumption that he knew their identities, and
- the assumption that he divulged them truthfully.

Note the coincidence that those "divulged" were, with one (unidentified) exception, well-known resistance figures who had been the subject of guesswork concerning "Lucy's sources," probably for years before Roessler revealed their identities to his intimate friend. Even without this coincidence, one should be more skeptical than the author about Roessler's own statements. Roessler's postwar silence and relative obscurity are well known; the motives for his silence, which stands out oddly in an era of

* *Studies*. Vol. 13, no. 3, 1969.

memoirs and revelations about the very network with which he worked, must have been strong ones; such motives might have led him to throw out red herrings.

(3) “. . . the characteristics of the Lucy messages and of their transmission from Germany to Switzerland suggest that Werther and the others probably had Abwehr communications channels at their disposal. There seems to be no plausible alternative theory.” (P. 67).

One searches the article in vain for any substantiation of this theory. It would seem to be more guesswork, although one must admit that it is at least more acceptable than the inference, in some published books, that the refugee Roessler and his sources must individually have operated agent transmitters and receivers. There seems to be equally little justification for attributing as much as the article does to Gisevius' travels (and why would Gisevius' information go to the refugee Roessler?).

I would suggest that we examine, as the article does not despite its title, the possibility that “Lucy” really *was* a myth, and a purposeful one. This might help explain why the secret of his sources has never been nosed out despite a generation of publicity and journalistic curiosity, and might also lead us to the “plausible alternative theory” which the article denies on page 67.

I refer to a possibility alluded to (in passing, surprisingly) on page 88, the penultimate page of the article: “Only the Swiss know today whether the vital information coming from Germany went first to Lucy and then, via Haussmann, to Masson, or whether the Swiss received the bulk of the information from their sources in Germany and passed it to Sedlacek for relay to the British and to Lucy for relay to the Russians.” The article also allows, on the preceding page, that Bureau Ha might have been created to free the operation from the shackles of neutrality.

More of the truth about “Lucy,” I think, lies in these two sentences than in all the rest of this long article. If so, much of the rest of the article loses its pertinence.

The following would seem more plausible, professionally:

That the “Lucy” sources were reporting to Swiss intelligence in Berlin, not to Roessler in Switzerland.

That there is no compelling reason, given the large number of dissident, anti-Hitler officers in Berlin, to believe that the “Lucy” sources were involved in known resistance groups such as Oster's.

That the information was transmitted from Berlin to Switzerland not by agent transmitters or agent couriers (nor by

Abwehr communications) but by Swiss staff communications channels.

- That Roessler was nothing more than a front man “created” along with Bureau Ha by Swiss Intelligence as a mechanism to pass the information to the Allies while preserving Swiss neutrality.
- That Roessler neither had sources of his own (at least not the key sources in Berlin) nor knew their identity nor controlled their commo.
- And that Roessler was inclined or compelled to hide these facts. (I believe he was paid again for his silence by the light punishment he received for his later espionage in 1953, to which the article does not pay enough attention.)

Even the opening words of the sentence on p. 88, which I cite above, is probably misleading: one cannot assume that “only the Swiss know.” The Soviets might also know: they got all the messages, and with their capture of Berlin and informed interrogations of key individuals, they could have identified the sources.

Switzerland’s continued interest in preserving a neutral history, and the well proven discipline of its people, do not offer much hope for disclosures. But I agree with the article’s contention that our continued interest in this case goes beyond our duty to Clio. Among other reasons, we should try to identify the Lucy sources because if the Soviets did, they may have been able, by pressure, to gain assets who might have provided later access to sensitive levels in postwar West Germany. The version above might offer new and promising lines of investigation. (For example, who were the Swiss MA’s, in prewar and wartime Berlin? Who were their closest German friends?) But the Lucy *myth* will only lead us into dead ends.

Andrew K. Megaris

INTELLIGENCE IN PUBLIC LITERATURE

In Quest of Justice, *Abraham Brumberg*, editor. Protest and Dissent in the Soviet Union Today. Praeger, New York, 1970. 477 pages. Short biographical notes on 83 individuals taking part in protests and on officials denouncing them are appended.

A compilation of documents in support of any socio-political thesis, regardless of how well organized and representatively selected, frequently constitutes a tedious exercise for the reader. When the assembled papers interweave the pros and cons of an issue, repetition is less likely; when their bulk is chosen to elaborate and support one set of ideas, repetition normally crowds the compilation. Papers written to uphold the same principles, regardless of the intellectual levels of the authors, tend to reiterate essentially the same story.

Brumberg's sizable and interesting assembly of documents illustrates the open and secret, at times only latent, growth of dissent—literary, social, and to a very feeble degree political—in the Soviet Union in the 1960's. Commentaries on the documents are written by Brumberg himself, perhaps best known as the former editor of *Problems of Communism*; Sidney Monas, of the University of Rochester; Stephen Weiner, a lawyer and former teaching fellow at Boston College Law School; George Luckyj, of the University of Toronto, who specialized in Ukrainian literature; and Peter Reddaway of the London School of Economics and Political Science, who has been deeply involved with the analysis and presentation of Russian “underground” literature for several years now. These literary efforts are technically neither “underground” nor “illegal” in terms of the Soviet constitution, which guarantees freedom of communication. They are, however, mainly unauthorized; and, as we all know, administrative regulations are often broken with greater risk than legal ones.

Unfortunately, from our point of view, none of the commentators takes up the question of even the possibility of KGB control of any of the underground authors, their *samizdat* or self-publishing facilities, or their channels of transmission to Western outlets. When the KGB is mentioned at all, it is simply as a repressive police force, with no apparent thought of KGB use of the well-known techniques of penetration, provocation, manipulation, etc. Monas does point out that “to some degree each poet has become his own KGB,” because the

repression of the past has been so deep that it is hard for today's writer to rise above it, but that is a different concept. It is obvious from the material concerning trials (above 100 pages) that the KGB has penetrated these "underground" circles. It follows that some of its agents must be producing some of the literature and helping in its dissemination, just as the agents of the Tsarist *Okhrana* helped man the revolutionary printing presses in order to collect evidence for arrest. Its task may be made more difficult by the wider availability of typewriters and carbon paper, but it is hard to believe that it is beyond KGB resources. It seems more likely that what the West is seeing in *samizdat* is to a great extent what the Soviet government has decided we should see, the decision being a political one balancing the danger of the literature to the internal stability of the USSR on the one hand against the danger of the people's reaction to KGB repression on the other. After all, as Brumberg points out, dissent in the Soviet Union in 1968-69 not only lacked an organizational framework, it did not seek one; "still relatively small, it confronts not only a hostile regime but also a largely indifferent population, vast segments of which are imbued with a traditional suspicion of 'smart-alecky' intellectuals and 'Jews'. It would therefore be wrong ... to see it as a serious political challenge to the regime."

Most Soviet *apparatchiki* probably still see the literature of dissent, when they see it at all, as the predictable and unfortunate result of the opening of Pandora's box that came with Khrushchev's "secret speech" criticizing Stalin, rather than as any great threat to the Soviet state. In addition, the number of its readers may well be greater outside the USSR than inside. This fact creates a time-tested ideal situation for the KGB: the appearance of resistance, vis-a-vis the West, without the substance of resistance, or little of it, inside the USSR. This condition has perhaps been best achieved in the past by such Soviet fictions as the *Trust* and *Win*, designed to fulfill the two-fold function of leading the West to believe internal resistance would bloom soon enough if only let alone, while attracting to them some Western attention and support. Most of Brumberg's selections date from 1968, with a few in 1969 (they originally appeared in *Problems of Communism*). At that time *The Chronicle of Current Events* was just beginning to appear in the West and, as noted above, there was little if any evidence of organization within the dissent and its literature, although several trials involving small groups had been reported. Of course, intelligence approaches from the *samizdat* milieu may have been made on the covert level unbeknownst to Brumberg (and this reviewer). In any case, it will be surprising if the KGB does not

at least take advantage of the opportunity, if indeed it is not partly creating it, to offer the West the organizational framework the movement ostensibly now lacks. In fact, some manifestations of this possibility have appeared since Brumberg's book was written, perhaps setting the stage for the entrance of players whose attraction will be hard for the West to resist, the minds of its political leaders having been conditioned by the "literature of dissent."

The ninety documents included in the book in no sense suggest that the protesters are non-communists or adverse to communism as a basic principle of governmental structure of the USSR. They protest against the revival of Stalinism and its harsh methods of repression. In their descriptions of court trials of writers, poets, artists, demonstrators, or religious and national minority oppositionists, they appeal for justice as guaranteed in the civil rights of the Stalinist constitution. They condemn illegal searches, long detentions, and harsh interrogations as identical with the excesses under Stalin; and they expose the court procedures as illegal and contrary to constitutional guarantees.

The protest literature engendered by the political trials (Sinyavsky, Ginsburg, *et al.*) focused for the most part on "constitutionality." The judges and the entire judicial machinery were accused of acting according to habitual party policy in total disregard for the legal rights of the accused. Stephen Weiner, concentrates on "Socialist Legality on Trial." After discussing the volume of protests about the handling of political offenses by the Soviet courts, he suggests that the protesters, by constantly harping on constitutionalism, have aimed at preventing the Party from ideological interpretation of the existing statutes on civil rights. He concludes that the ability of the Soviet system to produce an independent judiciary without radical political change may depend upon gradual development of this type of "mythical" (constitutional) restraint on political power.

One set of documents covers dissent in the Ukraine. George Luckyj comments on this group, while Peter Reddaway discusses the authors or origins of the open and underground documents on "Freedom of Worship and the Law." In each instance, the two writers point to a return to the old Stalinist practices of persecution. The latter is a bit optimistic in stating that the dissent has achieved much toward moderating persecution and state interference in internal church affairs. He believes that dissent on this score is apt to grow unless the regime makes very large concessions. "As the spiritual vacuum of Soviet life becomes more intolerable," he writes, "religious dissent, along

with other forms of social defiance, might increase in both scope and intensity.”

The documents appeared predominantly in western publications which received them through clandestine deliveries from the Soviet Union. Only a few stem from underground or illegal press and open publications in Russia. Added also are a few short stories ridiculing Stalin's regime, and a score of poems. The substance of most of the latter could be considered as dissent in some transcendental sense, but Brumberg obviously included them as literary samples from forbidden publications in Russia.

The volume is no doubt worth reading by students of contemporary social history of Russia. It is not, however, very encouraging. The documents dealing with court procedures and sentences meted out reveal clearly that at this stage dissent in the USSR can still not be considered as much more than so many isolated cries in the wilderness.

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