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POLITICAL AND MILITARY INTENTION ESTIMATION: A Taxonometric Analysis

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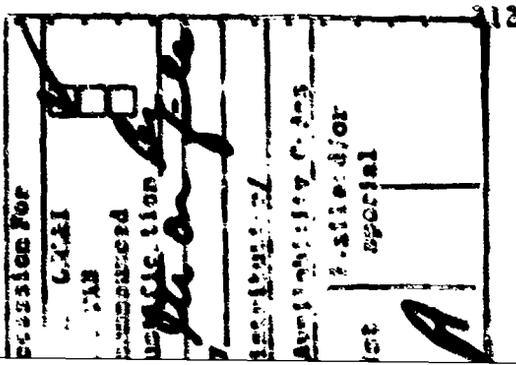
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SECTION 1

INTRODUCTION

INTRODUCTION

This report deals with intention estimation in the foreign policy arena and how governmental policymakers and analysts estimate the political and military intentions of other nations. While this subject has received sporadic attention (primarily in the wake of surprise attacks or other intelligence failures) it has not received sustained or extensive investigation.[1]

However, there are signs that the estimation of intentions has begun to receive greater official interest. Former U.S. intelligence officers acknowledge that, although our modern technical collection systems produce a flood of information, translating this information into relevant intelligence on intentions for government policymakers remains a formidable task that technology has done little to simplify and perhaps much to complicate. Military leaders in the United States are concerned with the adverse ratio of forces in Western Europe and the Soviet Union's open, self-avowed interest in military "surprise." The experience of the 1967 and 1973 Mideast wars demonstrates that tomorrow's wars may be much faster moving, harder hitting and briefer than wars in the past. Faced

[1] For example, a comprehensive computerized search of master's theses and doctoral dissertations uncovered only three dissertations which included the word "intentions" in the title. All of these were historical studies which did not address the subject of intention estimation. Furthermore, no dissertations were uncovered on the topics of military intentions, political intentions, intelligence failures, the attribution of political or military motives, or the misperception of intentions. The fact that the author is aware of at least two dissertations in this area (see Wasserman, 1960, and Whaley, 1969) which were not retrieved by the computer suggests the search technique is not foolproof; nevertheless, the near absence of research in academia in these areas betokens the need for studies such as this one.

with a numerically superior, wily opponent, U.S. military leaders are deeply concerned with how they can "win the first battle." One major shift in U.S. military thinking overturns decades of adherence to an "estimate capabilities only" doctrine for tactical intelligence officers: the U.S. Army's most recent field manual on "Operations" (which describes itself as setting forth "the basic concepts of U.S. Army doctrine," and the "capstone" of the Army's system of field manuals) for the first time states that "enemy intentions must be considered along with capabilities and probable actions." Intentions have become more important to the U.S. counterintelligence community as well. The military services have begun to stress operations security (OPSEC, or the concealment of one's own operational plans and intentions from possible enemies), and the Federal Bureau of Investigation, in a widely distributed pamphlet on security, asserts (FBI, 1978: 4) that "of particular importance to a foreign intelligence service is the identification of what constitutes our vital political, economic, and military intentions, and the theft of America's military and scientific secrets." But while the estimation of intentions has begun to be considered a vital element of intelligence and national security, comparatively little effort has been devoted to studying it. Furthermore, recent research in areas such as decision-making under uncertainty, the social psychology of attribution, and the psychology of cognitive processes which relate to the estimation of foreign intentions has not been applied to this subject. Finally, the extensive literature on intelligence operations and intelligence failures and successes has not been mined for information on the task of intention estimation. This report attempts to start filling this gap by explicitly investigating the phenomena of intention estimation.

There are two main objectives in this study. The first is to determine how intelligence specialists and policymakers estimate the intentions of a foreign nation, and more importantly, how they make mistakes in doing so. The second objective is to compare the tasks and processes of intention estimation to research on related cognitive, social, and organizational behavior to determine where the estimation tasks and processes are inherently weak and how they might be reinforced and strengthened. While the author made no a priori assumptions about how the estimation process could be improved, it was assumed from the outset that improvement is both possible and desirable. The ultimate hope for this research project is that it will lead to useful and realistic guidance on steps that would improve the intention estimation process.

This study is divided into ten sections, with this introduction being the first. The essential working definitions of the key terms used throughout this study are found in Section 2. The third section examines various views and opinions (often more implicit than explicit) on the two questions of whether intention estimation should be done and whether it can be done. Section 4 begins the work of determining how intention estimation is done and how it fails by reviewing three models of intelligence failure. These models focus on individual factors in intention estimation failures, organization factors, or political factors. This section closes with an examination of the status of a "theory" of intelligence failures. Section 5 describes a sociological theory of disasters such as dam failures, mine accidents, large fires, etc., which shows considerable promise as a working model for intelligence disasters. This section concludes with an application of this model to the Yom Kippur War intelligence failure.

The generality of this disaster model is further supported by Section 6 which relates this theory to Kuhn's paradigmatic analysis of scientific revolutions to show that the basic processes underlying the development of disasters resemble the process of science prior to scientific revolutions. This section argues that pre-revolutionary science, pre-disaster institutions, and pre-failure intelligence agencies share universal problems deriving from the basic cognitive, social, and organizational processes involved in producing knowledge.

Section 7 complements Section 4 by reviewing successful intention estimation experiences and contrasting successful intelligence with intelligence failures. The second main objective of this study is begun in the eighth section which relates recent work on cognitive processes and attribution psychology to the tasks of intention estimation. The main conclusions of these comparisons are summarized in Section 9 in a diagnostic of intention estimation vulnerabilities and weaknesses. The final section examines the problems and prospects of translating the diagnosis of estimation weaknesses into useful and realistic prescriptions for strengthening the estimation process.

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The views expressed in this study are those of the author and do not necessarily represent views held by the Office of Naval Research, Mathematica, Inc., or MATHTECH, Inc.

SECTION 2

DEFINITIONS

DEFINITIONS

> One problem when we talk about intent is definition.
>
> William Colby, 1978

> Man converts his words into idols that darken his understanding.
>
> Francis Bacon

> Above all, ... insist upon having the meaning of a word clearly understood before using it.
>
> John Stuart Mill, 1867

This section examines definitions of three key terms used in this report. It is felt appropriate to begin with definitions rather than adhering to the tradition of appending a glossary because one finds many different definitions of such concepts as "intention." Since these are broad and ambiguous concepts, with a wide variety of meanings, it is best to begin by demonstrating that the ground this report covers can be viewed from various perspectives.

Intelligence: Intelligence on a foreign nation differs from information; intelligence is evaluated and interpreted information which is significant to a nation's plans, policies, and operations. Intelligence is subordinate to the formulation of policy and plans, it helps determine feasible policy objectives with respect to other nations and provides a basis for developing methods to attain them. Intelligence in this most general sense is evaluated, policy-relevant information on another nation.

David Kahn (1978: 39-41) makes a useful distinction between "physical intelligence" and "verbal intelligence." Physical intelligence is derived from things, physical entities. Natural resources, physical

installations, the numbers of weapons available, volume of commercial trade, are the subject matter of physical intelligence. Verbal intelligence derives from words: plans, orders, morale, perceptions, intentions, estimates, promises, motives; these are the objects of verbal intelligence.

Kahn points out that this distinction does not depend on the perceptual means by which information is acquired, or the methods used to acquire the information, or on the means by which the information was transmitted. The distinction rests on the object of the intelligence. It is a useful distinction because war and politics (the main concerns of the consumers of intelligence and hence of intelligence producers) involve a physical and a mental component. On the one hand, Clausewitz said "war is an act of force" while on the other, Sun Tzu observed that "all warfare is based on deception." There are two facets of strategy: as Liddell Hart (1967: 337) wrote, one entails overcoming resistance with force, the other's purpose "is to diminish the possibility of resistance ... by exploiting the elements of movement and surprise." Both components achieve political and military objectives, intelligence must cope with both.

Intelligence is not necessary to diplomacy or war. But when it is available it "magnifies strength" (Kahn, p. 40) or, in modern jargon, it is a "force multiplier." Knowing what physical resources are available to the other nation allows the state which possesses physical intelligence to adapt its own physical resources to the situation. But physical intelligence cannot provide the state the time needed for such adaptation, it can only outline what adaptations are necessary. The mental component, the plans and intentions of the other, take time to translate into physical reality: plans are changed more quickly than men and guns can be

moved, or weapons built. Verbal intelligence gives the state the time needed to adapt resources, and by moving them to the best places, increase strength. Kahn writes (p. 40):

> The time that verbal intelligence gives a commander puts his knowledge of the enemy ahead of the present situation -- in effect, it foretells what the enemy will do. Physical intelligence, on the other hand, just reports on the present situation. The fundamental difference between them is that physical evidence merely confirms enemy intentions, while verbal evidence predicts them.

Intentions: There are two different ways of defining intentions: as physical objects of physical intelligence, i.e., as entities (although these are future entities), or as mental objects of verbal intelligence, i.e., as statements about the future. It is common to find the two ways confused: Washington Platt (1957: 62) writes that intentions are "what the other nation will do" but also that "forecasting intentions ... is in part an exercise in mind reading." Pettee (1946) writes of "decisions" and "operations" -- the first is mental, the second physical --

> Intelligence bearing on strategic decisions ... concerning the overall conduct of war, the scale and timing of major operations, the scale and scheduling of production and transport.

Some definitions clearly fall into one or the other category. George (1959: 16) defines intentions (emphasis added) as "future actions ... decided upon and the objectives behind current actions." In other words, intentions are something that has been decided upon (mental) for the future, not the actions (physical) that occur in the future; and the objectives (mental) behind the current action (physical). George considers "action" as something different from "intentions;" actions are "the specific moves undertaken in order to further these objectives."

He considers intentions and actions to be closely linked to "national policies," the rules which govern [a state's] use of military, economic, diplomatic, political, and propaganda instruments of power for the purposes of reaching certain objectives, domestic and foreign." Intentions are mentally contemplated, decided-upon future actions and the mental objectives or justification of present action. Actions are physical moves. Policies are mental rules governing action, presumably influencing intentions, and used to attain objectives.

In contrast, Jervis (1976: 48) defines an actor's intentions as "the actions he will take under given circumstances," and he contrasts his definition with those which "use intention to refer to what the actor plans to do or what goals he hopes to reach." He describes the "utopian intentions" of a state -- what it would do in the absence of external constraints, and contrasts these with "basic intentions," which include the state's considerations of the costs and risks implied by external constraints. Jervis writes (p. 48-9) that "intentions cannot be totally separated from the concepts of resolve and willingness to run risks." Jervis thus adds several mental dimensions to the physical concept of intention as future action. First, his definition emphasizes that state X acts, not in a vacuum, but in the awareness that states Y, Z, etc., will impose certain constraints on X, depending on what they perceive X to be doing. Second, in formulating plans, state X estimates what the reactions of Y, Z, etc., might be and what costs to X would result from these reactions. X must determine a balance between the value of obtaining its likely objectives and the likely costs of putting its plans into effect. Intentions somehow reflect state X's (mental) estimate of Y's, Z's, etc., likely responses, its evaluation of possible costs and

risks, its weighing of possible gains against possible losses, willingness to gamble, and its commitment or resolve to gaining its objectives, but intentions are first still, according to Jervis, actions.

George (1959: 15) defines these mental aspects separately, as "estimates" and "expectations" distinct from intentions, actions, and policy. State X's estimates are what it believes state Y can and will do, as well as X's predictions of its general future environment. State X's expectations are what it forecasts about its own actions and their consequences and outcomes, as well as X's forecasts of Y's and Z's policies and actions. Expectations are conditional predictions held by X with varying degrees of certainty.

Jervis' definition of intention includes what George labels estimates and expectations. He notes several reasons why state X's actions may not follow from its own plans and goals, e.g., the expectations and estimates of state X may be inaccurate and, as the future unfolds, state X will be forced to act in ways it did not plan. Consequently, Jervis' definition implies that intentions include not just those actions guided by what state X expects and estimates, but also those reactions of state X to the unexpected and to things it did not estimate. Jervis implies that state X's estimates and expectations may or may not be relevant to its future actions.

Although Jervis makes many useful observations about the elements involved in perceiving a state's future behavior, he seems to be wrong in defining (p. 48) intentions as "the actions he [the actor, or state X] will take under given circumstances." Trivial though it is to say it, state X's actions are its actions, and its intentions are something else. We do not know, with Jervis' definition, whether "intentions"

are the object of verbal or physical intelligence, but it seems most likely that Jervis would associate them with the physical component. Jervis justifies (p. 48) his definition, and the distinction he makes between intentions as actions and goals or plans, on the basis that "the collection of actions the state will or would take ... is what others are trying to predict." It is true that other states do try to predict these collections of actions of state X -- but they also try (and, we will argue in Sections 6, 8, and 9, they need) to estimate the intentions of state X, and it is possible and perhaps necessary to keep the prediction of action and the estimation of intentions intellectually separate.

We are going to follow the guide of the philosopher Anscombe (1969) and use intention to mean a form of verbal prediction and the object of verbal intelligence as suggested by Kahn. Such predictions include orders, commands, plans, estimates, expectations, pure prophecies, as well as expressions of intention. All of these are verbal statements about the future, and are thus objects of verbal intelligence. They differ in terms of their referents, their potential accuracy, and their psychological constituents.

When person A gives another person B a command or order, e.g., "Get out of the way," it implies A believes, to some degree, that B can (is able) and will carry out the instruction. A has reason to believe his command will be carried out by B. A can make some estimate about the future state of the world on the basis of his knowledge of B's ability and interests and the difficulty of the assignment. Finally, A has some stake in the outcome. Such orders do not necessarily reflect any intention by A to act. A similar argument applies to requests, pleas, etc.

If a command to B is explicitly or implicitly made contingent with a predicted action by A, then it becomes a threat or a promise, depending on whether the action A is predicting is something A thinks B will want or avoid. That is, A says "If you get out of the way, then I will reward you," or "If ..., then I will punish you." A is making himself the referent of the contingent prediction and hence is indicating a contemplated, decided upon future (in this case conditional) action. In George's definition, A is making a statement of (conditional) intention when he makes a threat or promise.

It is important to note the difference between a prediction or prophecy and an intention. A might say "I feel I am about to go crazy and if B is present then I may hurt him." This prophecy is about a future action A has not decided upon; merely one that A estimates will happen. It is not intentional action but it is nevertheless a future action which others, e.g., B, would want to predict.

Jervis would define A's future crazy action as an intention even though it may be an action which A would try very hard to prevent. This confusion between a future action and intention leads Jervis (p. 54) to make some unusual assertions:

> Because we have defined a state's intentions as the actions it will or would take under given conditions, intentions are sometimes different from what the state's decision-makers think they will or would do. This definition is useful because observers must try to predict how the actor will behave, not how he thinks he will behave. Although one might think that the actor can always accurately predict his own behavior, this is not true. Indeed observers may know the actor's intentions better than does the actor himself.

What Jervis is asserting about an actor's and an observer's ability to predict future action is true. But what he is asserting about knowing

intentions is patently false and confusing. As Anscombe noted (p. 9): "the question what a man's intentions are is only authoritatively settled by him." And we cannot agree that observers will not try to predict how an actor thinks he will behave. Jervis' definition seems more confusing than useful. This suggests one problem with a definition of intention as a physical object such as action, rather than as a verbal or mental object. Intentions come to mean behavior that was not intended, decided upon, or planned, as well as behavior that was. Anything done in the future becomes an intention.

If state A gives a command or order to one of its agencies, it is, in effect, commanding itself, and these commands and orders become part of state A's intentions, an expression of its plans. Similarly if a state A estimates or expects some future action from some part of itself, these estimates or expectations may reflect the state's intentions if the future action is a response to, or result of, the state's orders or plans. Of course, the state may estimate or expect actions or events will occur of which it was not an agent, that it did not order, or even desire but which it can nevertheless expect to happen. These estimates and expectancies would not be part of the state's intentions, i.e., mentally contemplated, decided-upon future actions. Similarly, we would argue that the threats and promises state A makes are intentional statements, but any prophecies or predictions of unplanned events would not be, even though the referent of all these might be state A.

The philosopher, Anscombe, author of the monograph Intention (1969: 6) defines an expression of intention as

> a description of something future in which the speaker is some sort of agent, which description he justifies (if he does justify it) by reasons for

> acting, reasons why it would be useful or attractive if the description came true, not by evidence that it is true.

This definition seems consistent with ours and George's of intention as a decided-upon, planned future action, as distinct from prophecies, expectations, estimates, and the future actions themselves. With respect to determining what a person's intentions are, Anscombe (p. 9) writes:

> In general we are interested, not just in a man's intention of doing what he does, but in his intention in doing it, and this can very often not be seen from seeing what he does.

Furthermore, an intention can be "a purely interior thing," if the individual does nothing or is prevented from carrying it out.

However, actions and behavior are important elements of estimating intentions. Anscombe notes (p. 8) that:

> If you want to say at least some true things about a man's intentions, you will have a strong chance of success if you mention what he actually did or is doing. ... the greater number of the things which you would say straight off a man did or was doing, will be things he intends.

Anscombe writes (p. 21) of an intention as a "forward-looking" motive, where the intention is what the state aims at or chooses and the motive is what causes or determines the aim or choice. But he also notes (p. 36) that "a man's intention in acting is not so private and interior a thing that he has absolute authority in saying what it is." There must be some reasonable link between the interior aspect of intention and the action that is taken in aid of the intention. He quotes (p. 45) Wittgenstein's observation:

> Why do I want to tell him about intention too, as well as telling him what I did? ... because I want to tell him something about myself, which goes beyond what happened at that time.

This is also the reason why intelligence must focus on intentions, not just what has been done or will be done. By examining both intentions and what the state does, intelligence learns something about that state which goes beyond what happened -- it learns how the state makes the reasonable link between its interior aims and objectives and its behavior.

Capabilities: In the parlance of the military, capabilities are the courses of action open to a military force. If a military force has the men and weapons available to launch an attack, and such action is not ruled out by the environmental factors (e.g., weather or terrain), the force is said to have an "attack capability." The notion of capability refers generally to unopposed courses of action. Even though the force in the attack example might be outnumbered twenty to one, for instance, it is still said to have an attack capability. The aggregation of all possible courses of action are the force's total capability. Obviously in aggregating, mutually inconsistent courses of action [illegible] considered: e.g., the force might have an attack capability or a defense capability, but not both at the same time. Because it is rarely possible to determine exactly which subsets of capabilities a force may have at any time, all probable courses of action are estimated and a decision is made as to the more probable courses of action. Improbable courses of action are usually ignored by intelligence (see e.g., U.S. Army, Field Manual 30-5, Combat Intelligence, p. 3-10).

This focus on the physical means of the other state and its physical environmental situation (e.g., weather, terrain) avoids the use of verbal intelligence. Observations of these physical components, e.g., physical intelligence, comprise the greater proportion of all intelligence efforts. Sometimes these observations provide information on intentions.

David Kahn (1978: 101-2) gives an example (see Figure 2.1) from the Russian front in World War II: the German intelligence officer of the 102nd Division noted that the Soviet soldiers behaved one way when they were on the defensive and another way while on the offensive. Such patterns, or stereotyped actions, are typically termed indicators. The wearing of caps or helmets is a pattern indicator in Figure 2.1. Indicators may cue an observer to deduce the identity, capabilities, or intentions of an opponent. In addition to patterns, which are activities, intelligence may note signatures, or particular displays of key equipment or facilities, which identify a military unit or future activity. In Figure 2.1 the construction characteristics of dugouts and the artillery and anti-aircraft artillery positions are signature indications. A profile is a picture formed of future activity through the combination of signatures and patterns (U.S. Army, Training Circular 30-24, Tactical Counterintelligence, p. 11-2).

Capabilities versus Intentions: If future activities can be assessed solely on the basis of physical elements, which cannot be changed or disguised as easily as plans and orders, would it not be more conservative to estimate intentions entirely on these profiles? There are several reasons why a complete reliance on physical intelligence for the estimation of intentions would be unwise.

First, while the physical components which form the basis for an estimation of capabilities cannot be as easily disguised as a plan can be changed, they can, nevertheless be camouflaged, and used in feints, ruses, spurious demonstrations and displays (see Reit, 1978, and U.S. Army, Training Circular 30-1, Tactical Cover and Deception, p. 7-10). Just as it is possible to "plant" false plans, i.e., letting them fall into

Figure 2.1. Example of Indications of Intentions Based on Physical Observations (From Kahn, 1978: 102).

INDICATIONS OF RUSSIAN INTENTIONS

Offensive	Defensive
wearing of helmets	wearing of caps
artillery firing for adjustment	harassing fire of uniform density at regular intervals, such as morning and evening
increased observation posts without noticeable increase in fire	roving pieces firing at intervals from many positions
clearing of mine fields and barbed wire	laying of mine fields and setting out of barbed wire
construction of lightly built dugouts	construction of heavily built dugouts
construction of real artillery positions unoccupied or with dummy pieces just behind the front lines, especially for antiaircraft artillery	construction of dummy artillery positions with no point of concentration; antiaircraft artillery only at traffic centers
inconspicuous traffic near front	sporadic appearances of tanks in same sectors
continuous visible traffic in open columns and motor noises toward the front for long periods	no increase in traffic
increase in enemy patrols	no increase in patrols
nervous behavior of soldiers with movement across areas under fire, indicating new arrivals	no change in behavior
change in mess and guard hours	no changes in schedules
new faces and languages in front lines	same faces and languages
carrying of packs but not gas masks	carrying of gas masks but not packs

enemy hands, in such a way as to convince the enemy of the plan's authenticity (e.g., Montagu, 1953) so it is possible to create and sustain notional military forces and capabilities (Reit, 1978). Neither verbal nor physical intelligence is immune from deception, and verbal and physical deception will often be integrated with each other.

Second, the physical intelligence estimation of intentions will provide, other things equal, less time than will verbal intelligence. When the physical profiles of an enemy activity are sufficiently clear to be observed by intelligence the enemy's capability for the indicated action is largely in place and ready for use. A warning based on these profiles provides little reaction time. In contrast, intelligence on the enemy's plans may be received even before the enemy has set those plans in motion, i.e., before any physical events occur which might be observed.

Psychologist Charles Schmidt (1976: 57) points to a third problem with an estimate of intention based on physical intelligence: the physical events will be observed chronologically while the enemy's plan will be ordered logically.[1] Actions, patterns, and signatures which relate to a variety of different future activities will be observed as occurring in a linear order through time and there may be few criteria for knowing which

[1] Schmidt writes (p. 58, slightly rearranged):

> First, acts that are part of different plans may be temporally contiguous and acts that are part of the same plan may be quite separated in time. Second, plans group actions into temporally disconnected sets of actions. Consequently, plans have a logically definite beginning and ending point. Finally, because plans have a logical progression, if the end or goal is known then it is possible to predict the kinds of actions that are to occur if the plan is completed ... some plans ... may involve no action at all but simply a decision not to act ... plans are opaque unless the goal is known.

profiles go with which future actions. For any particular enemy plan, only some smaller set of these events has any relevance. The physical intelligence observer is thus faced with the task of determining which events are relevant to the various possible courses of action the enemy may take without knowing which course the enemy intends to take. The verbal intelligence observer, who has some access to the enemy's plan, is less dependent on temporal order, and thus less misled, by the flow of observations over time.

The issue of whether to estimate capabilities, or intentions, or intentions from capabilities takes us beyond definitions. This issue is considered in the next section. One final comment on capabilities is in order here.

Capabilities versus Weapons: The traditional military usages of "capabilities" stress those physical actions a force might take, what the force is capable of doing given its own means and the limitations posed by environment (but not by an opponent, i.e., they are unopposed capabilities). An increasingly common usage of "capabilities" connotes nothing more than the number of weapons of various kinds. This is an unfortunate simplification of the term since the possession of a weapon by no means denotes a particular capability with it. However, in modern usage, weapons inventories are frequently used to denote capability. In the traditional usage of "capabilities" weapons are only a component and other "order of battle" factors are equally important: manpower, skill and training level, esprit de corps, doctrine, tactics, leadership, logistics, intelligence and security, etc. And in addition, these factors must be assessed against a backdrop of environmental constraints. Especially in the context of estimating intentions the reduction of

"capabilities" to nothing more than "weapons" is a misleading expedient, which is examined in the next section.

SECTION 3

INTENTION ESTIMATION: SHOULD IT BE DONE? CAN IT BE DONE?

INTENTION ESTIMATION: SHOULD IT BE DONE? CAN IT BE DONE?

> The most difficult and most crucial element in the intelligence craft lies in estimating the enemy's intentions.
>
> Avi Shlaim, 1976

> Intelligence on enemy intention is never clear.
>
> H. A. DeWeerd, 1962

This study assumes that estimating the intentions or the probable courses of action of an enemy or a foreign nation is an essential task of intelligence and diplomatic agencies, and that anticipating the actions of adversaries and allies is a prerequisite of statecraft and generalship. Although these claims have been made by intelligence officers, policy-makers, and scholars, there is by no means universal agreement that intelligence analysts can assess foreign intentions or that they should attempt to do so. In this section we examine opinions on how necessary intention estimation is as an intelligence task (should it be done?) and how difficult a task it is (can it be done?).

Intention Estimation: Should it be done?

> An open society like ours hands hostile governments virtually everything that exists about our strategic forces, tactical forces, and intents, while we're asked to play the game without corresponding information about our enemies. Good intelligence is therefore absolutely essential.
>
> George Bush, 1978

> The principal function of intelligence is to anticipate major foreign developments and changes in policies.
>
> The Church Committee, 1976

Raymond Garthoff, a U.S. Foreign Service Officer with extensive experience estimating Soviet intentions, has written recently (Garthoff, 1978: 24) of a "common fallacy" that analysts should "never estimate intentions, only capabilities," which is prevalent especially among the military. This view asserts that only the measurement of absolute, and perhaps relative, military capabilities is an appropriate task for intelligence analysis. Garthoff sees this view as fallacious because estimating future capabilities in practice only occurs against a background of implicit assumptions of (1) hostile intentions and (2) intentions to maximize those future capabilities. Similarly, Jervis (1976: 68) notes that decisionmakers "frequently assume . . . that the arms of others [capabilities] indicate aggressive intentions . . . An increase in the other's military forces . . . is taken to show that the other is not only a potential threat but is actively contemplating hostile actions."

An implicit assumption of hostility conceals the logic underlying the assessment of hostility and creates a second fallacy of "when in doubt, assume the worst," and this worst-case reasoning combines with assumption (2) above to produce overestimates of capabilities and an emphasis on "all-out" or maximum development, production, and deployment of capabilities.

Underestimates of military capability can be as misleading as overestimates if extrapolated to estimate intentions. For example, the British Navy underestimated Germany's military buildup in the 1930s and consequently was reluctant to accept Germany's willingness to go to

war. The United States underestimated Japanese Army and Naval capabilities prior to Pearl Harbor. On the other hand, the British Air Staff overestimated German bombing capability against Britain.[1]

The fallacy of "just the facts." The fallacy of "never estimate intentions" can be related to another fallacy which Garthoff terms "just give me the facts." Because the measures of capabilities and the military effectiveness of weapons are complex, multidimensional and often incommensurable, it is possible to develop comparisons and

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[1] A fascinating example of the tenuousness of estimates of enemy intentions extrapolated from estimates of enemy military capabilities is given in McLachlan's (1968) account of British Naval Intelligence before and during World War II. In 1936, when the Germans were constructing their great battleship Bismarck, the consensus of British diplomatic opinion was that Germany would adhere to the Anglo-German Naval Treaty of 1936, which in effect limited German battleship size to 35,000 tons. This consensus provided the basis for the design of the British battleship King George V. The dimensions of the Bismarck and Tirpitz as released by the Germans indicated that if these ships did in fact displace only 35,000 tons as the Germans claimed, they were of much shallower draft than the British ship. Although the lower tonnage was doubted by Naval Intelligence nontechnical officers, intelligence opinion was divided. From the inferred characteristics (shallow draft and 35,000 tons) the British Naval Plans Division concluded "The present design of German capital ships appears to show that Germany is looking towards the Baltic with its shallow approaches more than in the past" (quoted by McLachlan, 1968: 136), that is, the German ships were aimed more at Russia than at Britain. In fact, Bismarck and Tirpitz were designed to be roughly 45,000 tons, and the Germans released false figures to the British, depending on British belief in the readiness of the Germans to honor the 1936 agreement. The Germans read their opponent well; the British Director of Plans at the time wrote, "our principal safeguard against such an infraction of treaty obligations lies in the good faith of the signatories" (p. 137 in McLachlan). Not only had Germany deceived Britain as to her capabilities, she had the additional, unintended benefit of an erroneous British estimate of Germany's naval intentions. Similar underestimates based on similar German deceptions regarding submarines, cruisers, and battlecruisers also occurred prior to World War II and may also have misled British estimates of Germany's willingness to engage British naval power with what the British took to be a far less capable navy than Germany in fact possessed.

Similarly, U.S. underestimates of the range and performance of the Japanese Zero and the estimate that shallow water torpedo attacks were infeasible probably contributed to the Japanese surprise in attacking Pearl Harbor (Wohlstetter, 1962: 394).

Fear of the Luftwaffe's strategic bombing capability, overestimated by the Air Staff, inflated British Cabinet estimates of Germany's willingness to go to war over the Czechoslovakia crisis and contributed to Chamberlain's motivation to capitulate to Hitler, according to some historians (see Bracken, 1977).

arguments about trends in which the "facts" support virtually any conclusion. If the "facts" developed are premised on a form of worst-case assumption (what Garthoff terms the fallacy of "When in doubt, assume the worst"), they will support a foregone conclusion rather than provide new, independent evidence regarding the adversary. The weapons "selection and comparison" process, as Garthoff terms it, tends to stress present or future inventories, rather than capabilities to achieve objectives. This process is fallacious because the intelligence consumer leaves the selection of "facts," the definition of concepts and categories, and the interpretation of data to an implicit rather than to an open process or else the decisionmaker is merely swamped with "facts."[2] Consequently facts can be consciously or unconsciously selected to fit a thesis and then used to impute intentions.

The "just the facts" fallacy hinges on what Betts (1978) terms the "ambivalence of judgment," the fact that in intelligence there is usually some evidence to support any prediction, and on the "ambiguity of evidence," the fact that there exists a high volume of raw data, analysis, and estimates from which to construct a variety of judgments.[3] Betts

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[2]The most famous case of the "just the facts" fallacy was Winston Churchill who, when he first became Prime Minister, instructed "I do not wish such reports as are received to be sifted and digested by the various intelligence authorities," and ordered that he was to be "shown everything," whenever possible in the original documentation, so that he might draw his own conclusions (Churchill, The Second World War, Vol. 3, p. 319, quoted in McLachlan, 1968: 359).

[3]A well-known consequence of these two aspects of the fallacy occurred in the early 1960s when President Kennedy sent a Foreign Service officer and a general to assess the situation in Vietnam. The general returned with so highly encouraging a report, and the diplomat with such deeply pessimistic views that the President asked "were you two gentlemen in the same country?" (Halperin, 1974: 171).

asserts that "ambiguity is exploited by wishfulness" and the greater the ambiguity, the greater the impact of preconceptions.

Competing facts. Two examples demonstrate the problems of the "just the facts" fallacy. Garthoff (1978) notes that between 1965 and 1975 the Soviet Union built 205 "major combat ships" (defined as ships over 1,000 tons) to 165 for the United States, according to Pentagon figures. If "major combat ships" are defined as ships over 3,000 tons (thus not counting those smaller ships more suitable for the coastal defense missions which are vital for the Soviets but less so for the United States) the conclusion is reversed, the United States has built more "major" ships. A recent study for the Defense Nuclear Agency by the Santa Fe Corporation (DNA, 1978) which found the Soviet Union exceeding the United States on all but eleven of forty-four measures of strategic nuclear capability, was attacked by Congressman Robert Carr (Aviation Week, 1979) as misleading, since the set of measures chosen tended to favor the Soviets while measures favoring the United States were excluded.[4] Betts also has noted "the problem of innumerable and endlessly refined indices of strategic balance, and the dependence of assessments of capabilities on complex and variable assumptions about the doctrine, scenarios, and intentions that would govern their use" (1978: 69-70). Baldwin (1976: 19) suggests that this problem permeates the Washington intelligence establishment: ". . . in the past decade, the CIA and the DIA have offered policy-makers two alternative -- and unfortunately competing -- sets of facts, or derivations from those facts."

[4] A recent Mathtech survey (1978) of strategic measures found they cluster into two groups representing countervalue capability (useful against area targets) and counterforce capability (useful against point targets), and that the Soviets have tended to lead in the former while the United States leads in the latter.

A further difficulty with basing estimates of intentions explicitly or implicitly on estimates of just enemy capabilities is that assessing new or improved enemy capabilities is no simple matter. One's own technical experts will have great difficulty perceiving in a flow of incomplete intelligence novel concepts or ideas which they have not themselves already considered, or solutions to problems which they have not yet solved. Such underestimation frequently occurred before and during World War II (Wohlstetter, 1962; McLachlan, 1968; Jones, 1978). The consequence of the opposite, worst case, bias, that is, crediting the enemy with more capability than he really possesses, has been to produce the various missile, bomber, etc., "gaps" with resultant overreactions (Bottome, 1971; Dick, 1972; Gray, 1972; Licklider, 1970).

Responsibility for intention estimates. Is there a wide-spread tendency among intelligence analysts (especially in the military) to "never estimate intentions," as Garthoff suggests? Historically, both British and American military staff officers entered World War II having been trained that the staff officer should set down and examine all courses of action open to an enemy but that none should be selected as more likely than the rest since such a choice would give a wishful slant to assessments of courses taken by one's own forces (McLachlan, 1968: 252-3).[5] Gradually, the practice of estimating the enemy's most probable course of action came into practice, but as late as 1944 American Staff officers in Eisenhower's headquarters still observed the principle of not selecting the enemy course of action that intelligence

[5] Whaley (1973: 241) quotes the elder Von Moltke as saying "I have noticed that there are always three courses open to the enemy, and that he usually takes the fourth."

showed to be most likely. British officers had cast off this reticence in 1941 (McLachlan: 409). Wohlstetter (1962) observes that the U.S. Director of War Plans in 1940 refused to let the Director of the Office of Naval Intelligence (ONI) have any hand in estimating enemy intentions, although the head of ONI argued that his job should include "interpreting possible enemy intentions" (pp. 317-19).

More recently, the Senate Select Committee to Study Government Operations with Respect to Intelligence Activities (the Church Committee) notes that the intelligence community in the 1960s and 1970s, "confronted with the challenge to exploit the new sources of intelligence [technical, especially satellite intelligence] . . . turned away from the more speculative task of understanding Soviet purposes and intentions, even though insight into these questions was central to a greater understanding of the technical information being acquired in such quantity" (Fain, et al., 1977, p. 21). The Committee pointed out that analysts could furnish fairly complete and reliable reports on tangibles, such as military inventories, but are "not as good" at assessing motives or intentions; "in particular, some policymakers feel that intelligence analysts have not been especially helpful . . . on the more subtle questions of political, economic, and military intentions of foreign groups and leaders" (Fain, et al., 1977: 250, see also Hilsman, 1956: 46-51).

John Huizenga, former chairman of the CIA's Board of National Estimates, made the same point more strongly:

> There is a natural thrust in military intelligence to maximize threats and to oversimplify the intentions of potential adversaries. It is also quite naturally true that military professionals tend to see military power as the prime determinant of the behavior of states and of the movement of events in international politics." (Fain, et al., 1977: 45)

General Daniel Graham, who spent 16 years in various military and civilian intelligence posts including duty as head of the U.S. Defense Intelligence Agency, wrote (1976a: 10) that the "essential intelligence needs" of military policymakers are "strength, capability, and disposition" of the enemy. He writes that in the 1950s and 60s, the prime national intelligence question was "What are the military capabilities and intentions of the Soviet Union?" but that this question has "become calculable today with considerable precision" and today's questions involve more complex political and economic relationships among various states and regions. Elsewhere Graham (1976b: 65-66) wrote that "estimating the military capabilities of a potential enemy is to military intelligence men a much more critical function than it is for his [sic] civilian colleague" and that "the military intelligence officer when in doubt -- that is, when evidence is ambiguous -- tends to choose from the more prudent, more pessimistic, range of analytical results."

These opinions of outsiders (the Church Committee) and insiders (Huizenga and General Graham) seem to reflect a tendency to estimate military capabilities more often, more readily, and with more success than to estimate intentions. Furthermore, Graham clearly sees the "when in doubt, assume the worst" policy to be a prudent, "professional but honest" bias rather than the estimation fallacy Garthoff labels it.

Military doctrine. Unofficial and official military doctrine at least until recently tends to support Garthoff's assertion that military intelligence analysts tend to estimate only capabilities, i.e., the pre-World War II staff principles are still acknowledged.[6] The unofficial history of U.S Army Intelligence (Powe and Wilson, 1973: 97) reiterates

[6]E.g., U.S. Army Field Manual 30-5 "Combat Intelligence," 1951: "Commanders must be certain that they base their actions, dispositions, and plans upon estimates of enemy capabilities rather than upon estimates of enemy intentions."

a dictum well known to officers that military intelligence analysts should "evaluate the enemy's capabilities and avoid guessing as to his intentions." The recent official Army manual on combat intelligence (Department of the Army, 1973) describes the duties and methods of the combat intelligence officer without using the term "intention." Instead it prescribes (pp. 6-9) that "The primary purpose of the intelligence estimate is to determine the courses of action open to the enemy . . . and, if possible, the probable order of their adoption." That is, the military analyst avoids predicting or forecasting a particular intent or objective, and instead describes every "likely" action available to the enemy and, "if possible," attempts to determine which are most consistent with the enemy's situation and capabilities. To assess such probable courses the analyst is instructed to depend on the estimation of capabilities, i.e., what Garthoff termed "just the facts."

However, U.S. military doctrine on the estimation of intentions is shifting. For example, the most recent field manual on Army operations (Field Manual 100-5, 1976) emphasizes that enemy intentions must always be considered along with capabilities and probable actions. The manual instructs that "commanders must think of the enemy in terms of the enemy's tactics and doctrine, and seek to detect indicators of his intentions as well as his capabilities." Furthermore, the manual explicitly acknowledges that the military commander will have to base his estimate on ambiguous data and that risks are inherent in decisions based on such estimates.

If there has been on the one hand the tradition that the enemy's intentions cannot be known, that enemy capabilities will indicate the gamut of his possible actions, which argues that intention estimation

should not be done, there is also on the other hand the more recent syndrome (also largely military) which implies that explicit intention estimation should not be done, that it is at best unnecessary and at worst misleading, because the enemy himself will unwittingly signal us his intentions. This psychology has been termed "The Ultra Syndrome."

Intention Estimation and The Ultra Syndrome

> You always assume that the military and intelligence people have some secret skill not available to ordinary mortals.
>
> John F. Kennedy

> We often give our enemies the means of our own destruction.
>
> Aesop

Although the history of codes and ciphers is ancient, and their interception and solution nearly as old, the scope of signals intelligence and cryptanalysis and their impact on modern warfare have been publicly related only recently. The invention of wireless communication and its widespread adoption by military forces and governments brought not just the ability to operate and communicate rapidly and at great distance, but also the opportunity for enemies to intercept and decipher one's private messages. (David Kahn's (1968) massive compendium is the best extant public treatment of the general subject of codebreaking.)

The first widespread use of enemy signals for the purposes of ascertaining enemy intentions coincided with the first widespread use of radio in warfare. The intercept and analysis of German signals by British Naval Intelligence in World War I has been told in some detail (Ewing, 1939; James, 1956). Perhaps the central cryptological event in this war was the intercept and decipherment by British Naval

Intelligence of the telegram by the German Foreign Minister to Mexico, inviting that country and Japan to join Germany in attacking the United States. Tuchman (1958) examines the impact the release by the British of this intelligence to Woodrow Wilson's government had upon America's decision to enter the war.

Reading the enemy's mind. It was not until World War II however that radio communication became so ubiquitous in diplomacy and warfare that it offered the potential of serving as direct access to the enemy's thoughts and plans. Recent accounts of the solution of the German Enigma Ciphers in Europe (Lewin, 1978; Montagu, 1978; Winterbotham, 1974) and of the decryption of the Japanese Purple Cipher System (e.g., Clark, 1977; Kahn, 1968) make clear that these triumphs of codebreaking and security did indeed produce an unprecedented (and perhaps unique) access to enemy plans, estimates, and intentions. R. V. Jones, Scientific Advisor to the RAF during the war, writes (1978: 530): "the confidence with which Enigma decodes could be used in constructing or testing theories of enemy intentions was outstanding among all the sources available to us."

These accounts raise the issue of what Lewin (1978: 123) terms the Ultra Syndrome (Ultra was the British and later American code-name given to Enigma-based intelligence reports): "The assumption that because the intercepts . . . so authentic and so eloquent, told so much they must tell everything." McLachlan (1968: 28) made the same observation:

> Experience on both sides in two wars and in peacetime has shown that an intelligence organization which lived on cryptographical expectations alone became spoiled. It lost the skill and application that make the fullest use of other sources . . .

There are perhaps two main reasons that the Ultra syndrome occurs (that it has occurred is well documented in the public record of such intelligence failures as Pearl Harbor, the Arnhem offensive, the Ardennes Offensive, the Battle of the Atlantic, the North Vietnamese offensive of 1975, and others; see sources noted above and Strong 1968; Wohlstetter, 1962; Ryan, 1974; Snepp, 1977).[7] First, signals intelligence to be at all effective must be held very closely: only the most necessary figures can be allowed to know of its existence and only one's most highly trusted allies can be informed lest the enemy learn of this mortal breach of his security and close it. The facts that the Ultra secret was well kept for over 30 years, and that the U.S. Army did not circulate information on signals intelligence for its common soldier until 1976 (TC 30-20, 1976) betoken the secrecy which surrounds these methods. Second, the yield from these methods has been extremely good: for example, in the North African campaign Montgomery knew of Hitler's instructions before Rommel did (Winterbotham, 1974; Lewin, 1978). The net effect of these two factors is that those privy to this

[7] For example, Snepp (1977: 179) writes of the 1975 North Vietnamese offensive from his perspective as a CIA analyst in Saigon: "The recent report from [a North Vietnamese] defector point[ed] to the shift of the 320th NVA Division . . . to Ban Me Thuot [where the NVA offensive was to begin] . . . I finally dismissed it as false, principally because of the recurring radio intercepts that seemed to place the 320th in its normal operating area. . . Unfortunately, all of us in the analytic business in Saigon had come to rely excessively on such electronically obtained intelligence, in lieu of human-source data, in fast-moving crisis situations." Similarly, Eisenhower fired his intelligence officer, Mockler-Ferryman, for overrelying on Ultra and underestimating Rommel's intention to attack in the Kasserine Pass. Eisenhower's aide, Butcher, wrote: "Ike insists we need a G-2 who is never satisfied with his information, who procures it with spies, reconnaissance, and any means available" (Lewin, 1978: 273-4).

special source of information seemingly posses a magical insight into the enemy mind, and the successes this knowledge produces lead to the atrophy in other analytic skills noted above by McLachlan.

Secrecy and accuracy also limit challenges; such special sources come to have a cachet of omniscience because they are never identified to those without access, and so cannot be directly criticized. Perhaps the most severe problem engendered is that top policymakers are almost always given total or near-total access to these sources while only a handful of intelligence officers are included on the restriction lists and none are included on them all. Daniel Ellsberg related to a joint Senate Hearing his comments to Henry Kissinger in 1968 when the latter was about to become President-elect Nixon's national security advisor (Testimony reprinted in Fain, et al., 1977: 501-514) which eloquently details this problem:

> You have written articles and rubbed shoulders for a decade with people who had these clearances and access to information that you didn't know existed, and you will feel like a fool because you didn't know it. You will feel like a fool for having written all that without having this special information on which to judge. You will realize that the people that you talked to had it and you didn't.
>
> But that feeling will only last for a week or two, because after a week or so of having four-star generals, or at that time one-star generals . . . bring you in special brief cases, special pouches, books that are available only to you and your boss and a few other people . . ., and certainly not to members of the public, you will forget that you were once a fool and remember only that everyone else is a fool who does not have this information.
>
> Moreover, in signing agreements to have this information, you will come to understand that the only way of keeping secrets this well is to lie . . .

> You have the information, they don't; they don't even have the wisdom to know what they don't know; therefore they have no legitimate role. You will become unable to learn from anyone who does not have these clearances.

The policymaker who comes to rely on these little-known secret sources of seeming perfect accuracy regarding the enemy's plans will see little need for further estimation of the enemy's intentions, and will see little point in questioning the assumptions which these sources foster and sustain.[8]

Intention Estimation: Can it be done?

> In a world of perfect information, there would be no uncertainties about present and future intentions . . . information, however is bound to be imperfect . . . the intelligence community can at best reduce the uncertainties and construct plausible hypotheses.
>
> James Schlesinger, 1971

> Unless something is totally conclusive, you must make an inconclusive report.
>
> Ray Cline, 1975

> If a man could say nothing against a character but what he can prove, history could not be written.
>
> Samuel Johnson

There are several examples in history of successful efforts by intelligence to "read" the intentions of the enemy in war or an adversary

[8] The Ultra Syndrome infrequently cuts the other way, i.e., those in the know may be pilloried because their defense rests on secrets they are unable to reveal. British Air Chief Marshall Sir Hugh Dowding, Commander-in-Chief of RAF Fighter Command, was sacked and Air Vice-Marshall Keith Park, Commander of Fighter Command 11 Group (which bore the brunt of the Battle of Britain), was transferred to a training job as a result of a "deplorable" Air Staff meeting after the battle at which Park's rival, Leigh-Mallory, and one of Leigh-Mallory's squadron leaders challenged Dowding's and Park's tactics. Dowding's conduct was based on his knowledge of Luftwaffe plans obtained via Ultra, but he did not reveal this basis for his highly effective tactics because present among his attackers were several who knew nothing of the Ultra secret. (See Lewin, 1978; and Sir John Slessor's introduction to Winterbotham, 1974).

in peace but there are many more examples of failures of intelligence, which were basically faulty estimates of intentions. This section lists some well-known intelligence successes and failures in order to draw some general observations about strategic surprise and strategic success.

Successes and failures. Even a brief listing of the best known instances of intelligence successes and failures (Table 3.1) reveals several useful observations. In the table each event is evaluated from the viewpoints of the defender and the attacker. Those surprise events which the defending country's leaders failed to anticipate are judged to be intelligence failures. When the surprise event was anticipated the event is judged as an intelligence success. From the viewpoint of the attacker each event is judged to be a strategic success if it subsequently led to the long-term outcomes desired by the attacker. If the surprise event is followed by strategic disaster for the attacker the event is judged to be a failure. Thus the Pearl Harbor attack was an intelligence failure because U.S. leaders failed to anticipate it, but the event was also a strategic failure because it strengthened rather than weakened U.S. belligerence and military efforts and led to Japan's defeat. On the other hand, the Chinese intervention in Korea surprised the U.N. command but was also a strategic success in that it prevented the collapse of the North Korean regime and ultimately restored the two Koreas' ante bellum boundaries.

Trumpeting of failures. This summary of surprise events indicates first that intelligence failures (13/16) are certainly better known than intelligence successes (3/16). Several factors will tend to make this true regardless of the efficiency of intelligence systems.

Table 3.1. Some Well-known Examples of Strategic Surprise

Event	Defender's Intelligence S	Defender's Intelligence F	Attacker's Strategy S	Attacker's Strategy F
1939 Germany Attacks Poland		x		x
1940 Germany Attacks Norway		x	x	
1941 Japanese Attack Pearl Harbor		x		x
1941 German Attack on Russia		x		x
1942 Japanese Attack on Midway	x			x
1944 German Ardennes Offensive		x		x
1950 North Korean Attack on South Korea		x		x
1950 Chinese Intervention in Korea		x	x	
1956 Franco-British-Israeli Attack on Egypt		x		x
1961 Bay of Pigs Invasion	x			x
1962 Russian Installation of Missiles in Cuba		x		x
1967 Egyptian-Syrian Attack on Israel	x			x
1968 Vietnamese Tet Offensive		x	x	
1970 U.S. Intervention in Cambodia		x		x
1973 Egyptian-Syrian Attack on Israel		x	x	
1975 North Vietnamese Offensive		x	x	

Defender's Intelligence	Attacker's Strategy: Success	Attacker's Strategy: Failure	
Success	0	3	3
Failure	5	8	13
	5	11	16

(1) It is virtually impossible to conceal the consequences of a major intelligence failure -- the results are too disastrous. Furthermore, responsibility for intelligence failures usually is readily ascertained; faulty estimates and predictions persist after the disaster and point fingers of blame. On the other hand, what we have termed intelligence success events might exclude many more such events that have never been identified publicly.[9] That is, one cannot identify the surprise attack that was canceled as easily as one notices the surprise attack that succeeds. Attackers will bide their time, change their plans, or cancel their operations if they learn that the defender's intelligence has uncovered their surprise plans. Ironically, intelligence in these circumstances may be accused of having issued a false alarm, since nothing happened, rather than the alarm that forestalled the attack: the "great paradox" noted by Shlaim (1976) that intelligence's "greatest successes may be indistinguishable from failures." (2) The defender whose intelligence has successfully warned of a surprise attack may wish to keep the details of this success confidential since similar techniques if kept secret will probably have future applications. The defender, as well as the attacker, may wish to foster the myth (or cover story) that the attack failed or was postponed for any of a variety of innocuous reasons other than the defender's intelligence skills. There is thus a strong motive to keep successful intelligence secret. (3) Over the long run, successful intelligence will reduce the probabilities of war and conflict by providing the state with accurate appraisals of the future

[9] John F. Kennedy noted of his intelligence chief "your triumphs are unheralded, your failures trumpeted." (Quoted in Colby, 1978: 456.) Chan (1979: 173) writes: "we should refrain from overemphasizing the ubiquity of strategic surprise, since the universe of successful warnings is largely unknown." Infrequently, the record of a nation's successes and failures in estimating foreign intentions becomes public; such is the case in the instance of Nazi Germany. Kahn (1978: 60-69) concludes that the Nazi intelligence agencies were wrong more often than right in their estimates of foreign intentions.

goals and actions of other states so that the state's policy can effectively influence the others and the state can arrange mutually satisfying accommodations and compromises. This subtle and quiet role of intelligence as the handmaiden of effective foreign policy will tend to go unnoticed, whereas the failure of intelligence to accurately estimate the objectives of other states or their willingness to take risks leads to those conflicts and subsequent surprise events which make intelligence failures so conspicuous.

The prevalence of strategic failures. A second observation to be made of Table 3.1 is that, despite the preponderance of intelligence failure over intelligence success (i.e., despite the attainment of surprise) strategic failures (11/16) are more common than strategic successes (5/16). Intelligence failure is perhaps a necessary but certainly not a sufficient condition for strategic success. In the analysis of intelligence failures below, it is argued that intelligence failure is likely to be followed by strategic failure because the most surprising plans are those which are assessed as likely to fail and, in fact, such plans do surprise because they will eventually fail.

The tenuous advantage of surprise. A third observation is that intelligence surprise tends to be followed by strategic failure (of 13 cases of intelligence failure, or strategic surprise, 8 are followed by strategic failure while 5 are followed by strategic success). Intelligence success may be sufficient to cause strategic failure, but it is not necessary. Of 11 strategic failures, 8 were preceded by intelligence failures and 3 were preceded by intelligence success. This suggests there may be three "types" of strategic action: (1) those that will fail even if launched in surprise, (2) those that will fail unless they are launched

in surprise, and (3) those that cannot fail, surprise or not.[10] From the defender's point of view the first type of event will be the most difficult to foresee (because it is so risky), while the third type will be the easiest (because it has no risks). That is, in the first and third types intelligence failure (surprise) is neither necessary or sufficient for strategic success, whereas in the second type intelligence failure is necessary but not sufficient for strategic success. In the first type the intelligence analyst is unlikely to anticipate such seemingly "irrational" and risky action (although he/she can and should attempt to "expect the unexpected"). In the third type the intelligence analyst's estimate may be unnecessary since the enemy's course may be obvious, and, at any rate, the provision of warning would have no impact on the outcome. It is in actions of the second type that intelligence can play a regular and

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[10] Logically there is a fourth "type" of strategic action: those that succeed only when intelligence succeeds in eliminating their surprise and which fail if surprise is achieved. Such "nonsurprise attacks" seem improbable at first glance because they may not occur in situations of hostility between states. Such a type may take place in the context of efforts to better relations, such as negotiations. For example, the state which makes a radically new and unexpected proposal (i.e., achieves surprise) may fail to have the proposal accepted whereas a gradual unfolding of the proposal preceded by prolonged preparation (such that the proposal would be amply telegraphed to the other state) might succeed. Israeli intelligence analysts, just before Sadat's peace initiative in 1977, were reportedly preoccupied with how they could determine friendly Arab intentions. Sadat nevertheless surprised Israeli intelligence again (Hareven, 1978; 18, fn. 7). However, failures to estimate friendly intentions are less critical (although they may still be costly) than are failures to estimate hostile intentions; consequently this fourth type of strategic action will receive no further discussion.

Furthermore, it is worth noting that cryptographic knowledge of the enemy's intentions does not automatically eliminate intelligence failure, as the Pearl Harbor episode demonstrates (Wohlstetter, 1962). Perfect knowledge of enemy plans and intentions, i.e., intelligence success, may be followed by strategic success of the enemy if the defender is unable to make use of his knowledge. The German conquest of Crete is a case in point (see Lewin, 1978: chapter 6).

useful role, that is, in forestalling actions that require intelligence failure (surprise) for their success.

The uncertain trumpet.[11] A fourth observation can be made of these well-known instances of strategic surprise. In nine of the thirteen cases of intelligence failure, either hostilities were initiated (e.g., German attack on Russia), or a previously nonbelligerent country entered the war (e.g., China intervened in Korea), or a previously neutral nation was attacked (e.g., U.S. intervention in Cambodia, German invasion of Norway). Only three of the intelligence failures took place in the midst of war (Ardennes, Tet, and North Vietnamese offensives). Only one failure took place entirely in a peacetime environment (Cuban missile crisis). This suggests that intelligence failures (at least those that are well-known) are particularly likely to occur regarding when war will break out and regarding what nations will be attacked or will join in the war. Especially suggestive of this are the facts that in World War II, the British declaration of war surprised Hitler, Germany's attack on Russia surprised Stalin, and Japan's attack on the Americans and British surprised Roosevelt and Churchill; that is, the events that brought each of the major belligerents into this war were strategic surprises.

A final observation may be of small comfort to the intelligence services which have failed; any service seems to be subject to failure. Several countries which succeed in surprise attacks are also victims of them (Israel, Egypt). It is unlikely that intelligence failure or

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[11] If the trumpet give an uncertain sound, who shall prepare himself to the battle. Corinthians 14: 8.

strategic surprise are the province of any nation, culture, service, or period. There is a universality about intelligence failure.

Intention estimation successes. Although this cursory examination of intelligence successes and failures suggests the latter are far better known to the public than the former, detailed histories of successful estimations of enemy intentions can be found, such as George's study summarized below. George's history of propaganda analysis provides another reason why intelligence failures will tend to be more conspicuous than intelligence successes. That is, determination of an intelligence success requires careful analysis of the product of the intelligence agency and comparison of this product to historical records and data to verify or disconfirm the estimates and predictions made. In contrast, as noted above, determining the occurrence of intelligence failures is a simpler and more direct process requiring no historical or scholarly skills.

After World War II George (1959) assessed the accuracy of intelligence analysts working for the U.S. Foreign Broadcast Information Service (FBIS) of the Federal Communications Commission in estimating German and Italian policies, intentions and goals from Axis propaganda. George compared a random sample of two months of inferences and estimates by the FBIS analysts with verifying evidence in captured Axis war records, diaries, and orders. The analysts were scored correct on 15 of 18 verifiable inferences dealing with elite policies and intentions, on 10 of 10 inferences dealing with elite expectations, on 7 of 8 inferences dealing with elite estimates; i.e., in the random two month sample they were able to accurately estimate

32 of 36 issues of elite behavior for which verification could be obtained (George, 1959: 264). George cautions that the FBIS analysts may have been less willing to make estimates of elite behavior than to assess Axis propaganda goals (another of their tasks), and thus made the former inferences only when they were highly confident. Nevertheless, their ability to draw correct estimates of enemy intentions from propaganda alone was impressive. George concludes (p. 268) that the analysts made correct estimates on a wide variety of questions of interest and importance to decisionmakers; that such estimates included (1) Nazi intentions, (2) the calculations and estimates underlying Nazi policies, and (3) the situational factors influencing Nazi policy and actions; and the analysts produced consistently reliable analysis of intelligence problems over time. George's assessment of the methodology used by the FBIS analysts to estimate intentions is analyzed in Section 7.

A similar claim for accuracy has been made by McLachlan (1968: 248), who summarizes the four year experience of the Joint Intelligence Committee which produced British intelligence estimates during World War II: "Forecasts of enemy strategy and intentions were mostly accurate, although at times the language of their reports would have been firmer had they not been obliged to reconcile the views of five departments." However, McLachlan relies on anecdote and example, and the successful conclusion of the war as evidence

for his assertion.[12] Similarly Jones (1978) summarizes his successes in using technical intelligence and Ultra in predicting German air operations.

[12]In all fairness it should be pointed out that George, McLachlan, and Jones were wartime members of the intelligence agencies whose abilities to estimate enemy intentions they subsequently evaluated so positively. While the natural bias of a "loyal alumnus" seems almost certainly to have been avoided in the case of George, and relatively inconsequential in the case of Jones and McLachlan, it cannot be totally ruled out as affecting their views.

SECTION 4

WHY ESTIMATES OF INTENTIONS FAIL: STRATEGIC SURPRISE AND INTELLIGENCE FAILURE

WHY ESTIMATES OF INTENTIONS FAIL: STRATEGIC SURPRISE AND INTELLIGENCE FAILURE

> Intelligence failures have probably created the biggest wars in this century.
>
> William Colby, 1978

> Oh, where hath our intelligence been drunk, where hath it slept?
>
> Shakespeare, King John

In this section the literature on intelligence failures and the failure of policymakers to utilize intelligence is reviewed. This review utilizes the three levels of analysis used in Allison's (1971) examination of the Cuban missile crisis. That is, we characterize the literature on strategic surprise and intelligence failure as focusing on the level of 1) the individual actors, 2) the organizational actors, or 3) the political decisionmaking channels. Little of this literature uses more than one of these levels of analysis; most studies identify the central causes of failures as occurring at one of these levels. Within this level of analysis framework our aim is to identify what various observers of intelligence failures see as the basic causes of the failure as well as the component elements and processes that underlie failure. This review throughout emphasizes estimates of intention; however, failures of other estimates which seem to affect intention estimation are also reviewed.

Allison's examination of the Cuban missile crisis utilized three conceptual models or paradigms to determine the motives and intentions behind Soviet and American actions. He labeled these the rational actor paradigm (or "classical model"), the organizational process paradigm, and the bureaucratic politics paradigm. We employ these

paradigms not as Allison did, to examine general policymaking, but rather to organize the explanations of strategic surprise and intelligence failure. That is, whereas Allison uses these paradigms to answer questions such as "Why was a particular policy chosen?", we use them to organize the various answers to the questions "Why were the estimates of intentions wrong; why was the government surprised by its adversary?"

The Individual Model of Intelligence Failure

> The history of international relations is full of . . . wishful assumptions which have come unstuck.
>
> Benno Wasserman, 1960

> We can easily represent things as we wish them to be.
>
> Aesop

The basic unit at this level of analysis is the individual intelligence analyst or policymaker. This model assumes that the basic causes of intelligence failures originate with individuals and that the component elements and processes behind failures involve the problems of the individual actor trying to estimate intentions.

Logic versus perception. The literature on intelligence failures which emphasizes the individual divides into two schools, that which sees intelligence analysts and policymakers as essentially logical and rational but nevertheless occasionally fooled (which we can term the rational hypothesis), and that which views the individual as a fallible, biased, and illogical victim of his own distorting perceptions (which we can term the perceptual hypothesis). While each school stresses the role of the individual, they point to distinctly different causes as leading to failures.

Knorr (1964) argues for what we might call the rational hypothesis school of the individual model. While he acknowledges that intelligence analysts develop a "set of expectations" of a target country's likely patterns of behavior, Knorr writes that these are different from (but perhaps affected by) the "national image" one society has of another. The intelligence analyst's "belief system" is more sophisticated than the general social belief system and, Knorr asserts, analysts "apply to (their) images tests of proof and disproof that will modify or dispel them. These specialists . . . can be expected to rise above (society's) stereotyped views" (p. 461). Knorr concludes that the analysts' expectations, having withstood the tests of previous estimates (i.e., having been confirmed by experience in the past), or having been modified as a result of such experience, "should form a solid basis for intelligence" (p. 461).

In contrast to this quasi-scientific image of the intelligence analyst, the perceptual hypothesis stresses the analyst's cognitive limitations and the critically debilitating role of perceptions and images in biasing analytic judgment. For example, George and Smoke (1974: 574) write:

> Individuals . . . are capable of engaging in various strategems for diluting or discrediting information that challenges the structure of existing expectations. . . discrepant information. . . is often required, in effect, to meet higher standards of evidence and to pass stricter tests to gain acceptance than new information that supports existing expectations. . . the equivalent of the scientist's null hypothesis is rarely available or welcomed in policymaking . . . it is relatively easy for intelligence specialists and policy-makers to discredit discrepant information or to interpret it in such a way as to save a preferred hypothesis or policy.

Similarly, Shlaim (1976) asserts that images of the international situation and especially the intentions of other nations play an "all-important" role in the making of intelligence estimates. In forming these estimates intelligence officers use "over-simplified images embodied in long-standing ideological stereotypes" rather than applying varied concepts derived from an open-minded process. Shlaim believes intelligence analysts "can easily become prisoners of their theories, with those theories acting as blinkers to exclude any evidence that does not conform to their expectations" (p. 357-8). Stalin may have put it most succinctly: "An intelligence hypothesis may become your hobby-horse on which you will ride straight into a self-made trap" (quoted in Whaley, 1973: 222).

Knorr

The rational actor model of foreign policy processes is described by Allison (1971: 26-8) as having a broad and deep impact on U.S. scholarship. However, with the exception of Knorr, analyses of intelligence failure and strategic surprise eschew the image of the intelligence specialist or policymaker as classical rational actors. A brief review of Knorr's analysis demonstrates the difficulties created by assuming a rational actor in assessing the causes of strategic intelligence failures.

Although analysts' expectations are more sophisticated than those of the public, Knorr acknowledges that intelligence specialists do get surprised. He attributes this to what he terms "technical surprise," "behavioral surprise," and "apparent surprise." The first occurs when the analysts' expectations are correct regarding the other country but

the opponent succeeds in concealing a particular course of action or capability.[1] This would seem to limit "technical surprise" to rather minor intelligence failures. If the course of action or capability which the intelligence specialists overlook has critical consequences it would be hard to allow that events could nevertheless be deemed compatible with the specialists' expectations.

"Behavioral surprise" may result from three causes: (1) the enemy country may act irrationally, (2) the intelligence agencies may be incompetent, or may be unduly influenced by national images based on myths or wishful thinking rather than on objective perception, or (3) the opponent country may change its pattern of behavior so that the intelligence experts' expectations are out of date. The first cause is certainly compatible with Knorr's view of the analyst as a rational, means-ends estimator of enemy intentions. If the enemy does not utilize rational calculations to reach decisions, the rational analyst can hardly be expected to anticipate the decisions reached, although he would presumably be prepared for exotic and unusual decisions. The second cause is clearly incompatible with Knorr's view of the intelligence analyst. An intelligence agency which succumbed to the

[1] Perhaps the most extraordinary example was the double technical surprise on 9 March 1862 when the steam powered ironclads Monitor and Merrimac (or Virginia as the latter was rechristened by the South) battled in Hampton Roads, forever ending the reign of wooden ships. The success of the Merrimac the previous day over the wooden sailing ships of the Union Navy had taken the North by complete surprise (there was panicky talk of abandoning Washington) while the appearance of the Monitor on the ninth took the South (and the Merrimac's crew) utterly unawares. Both sides expected the other, in general, to eventually build ironclads at some point during the war, and each was racing to be the first. That the race could end in a tie seems to have been anticipated by neither.

third cause may be considered rational in some respects but its obsolescence, rigidity, and staleness implies a nonrational, and certainly nonoptimal, adjustment to change. Knorr rejects behavioral surprise as the cause of the Cuban missile crisis, but acknowledges its role in other intelligence failures, which suggests he sees intelligence specialists as having only limited rationality, i.e., as being occasional victims of biased or rigid perceptions.

Knorr's "apparent behavioral surprise" can be best described as a double intelligence failure. In the case of the Cuban missile crisis this consisted of the Soviets underestimating the risks of U.S. reaction to Soviet missiles in Cuba (the Soviet intelligence failure, or "behavioral surprise") and U.S. intelligence and policy agencies assuming the Soviets would continue to follow a conservative policy (the U.S. intelligence failure, or "apparent behavioral surprise"). The U.S. analysts' expectations regarding the means-ends calculations of the Soviets Knorr believes were correct, but the analysts did not foresee the information and estimates provided by Soviet intelligence to the Soviet decision-makers. While the part played by U.S. intelligence agencies (as seen by Knorr) is consistent with an image of a rational intelligence analyst, his description of the Soviet intelligence performance is not; the expectations of the Soviets regarding the U.S. reaction were clearly inadequate if events occurred as Knorr speculates.

In short, a review of Knorr's analysis of intelligence failure suggests that he believes the expectations of intelligence analysts may be more objective than general national stereotypes, but these expectations are at times inadequately assessed against reality. Knorr

seems to be arguing that intelligence agencies are more rational than the public but are still occasional victims of incompetence, wishful thinking, and faulty assumptions.

The Perceptual School

> Genius, in truth, means little more than the faculty of perceiving in an unhabitual way.
>
> William James, 1890

> This difficulty of seeing things correctly, which is one of the greatest sources of friction in war, makes things appear quite different from what is expected.
>
> C. von Clausewitz

While the analysis of intelligence failure from the perspective of the rational intelligence specialist has few spokesmen, the perceptual hypothesis has many. Three main themes ranging from specific to general appear in this school of the individual model: (1) a focus on the impact of specific images of the other and of self, (2) a focus on the filtering effects of perceptions on information processing, and (3) a focus on the limitations of human cognition in general.

The impact of specific images. Lloyd Etheredge (1978) traces failures and errors in policymaking to the personal motives, the individual behavioral patterns, and the fears of the top leaders. These internal structures shape leaders' beliefs about external reality and limit any objective examination of the appropriateness of these images. This process generates a (false) sense of confidence and consistency which leads leaders to believe their decisions are right and rational. Benno Wasserman (1960) makes a similar assertion, that policymakers are surprised because they lack and do not seek the knowledge of foreign

affairs which would prevent surprise "because intelligence tends to be geared to an uncritical conceptual framework. . . its estimates of the intentions of other countries are based on inapplicable assumptions" (p. 157). H. A. DeWeerd (1962) expanded this basic cause to "the political-military climate of opinion" which prevents policymakers from "drawing proper conclusions from . . . intelligence collections" (p. 451). Using Korea as a case study, DeWeerd claims the policymakers

> refused to believe what our intelligence told us was in fact happening because it was at variance with the prevailing climate of opinion . . . (they) also refused to believe our intelligence because it would have been very inconvenient if (they) had: (they) would have had to do something about it.

Etheredge, Wasserman, and DeWeerd seemingly agree that particular assumptions of policymakers about the enemy prevent them from objectively employing intelligence,[2] but they differ in their assessments of the component elements underlying this basic cause of intelligence failures. Etheredge sees the perceptual blindness of policymakers as a consequence of the personality of those who succeed in political life; i.e., the characteristics of the successful politician are also likely to produce a highly biased outlook on foreign affairs.[3]

Wasserman

Wasserman sees the basic cause of intelligence failures as resulting from the "official theory of intelligence" and from the relation

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[2]This issue is also analyzed by Lampton (1973), reviewed below.

[3]Dixon (1976) similarly argues that military life attracts authoritarian personalities, and military training and traditions reinforce the rigidities, projection, and denial aspects of thought that produce military blunders and catastrophes.

of intelligence to policy. The "official theory" of intelligence is a shared, implicit community of attitude among policymakers, intelligence analysts, and officials, and scholars of intelligence, which Wasserman finds in Hilsman's (1956) observations in Strategic Intelligence and National Decisions. There are four elements of this "official theory:" naive realism, inductionism, the notion of a determined future, and the concept of a weak and dependent role of intelligence in policy.

Naive realism refers to the belief that knowledge and intelligence consists of "unvarnished" facts which admit of only one interpretation. The purpose of intelligence is the accumulation of ever more data so that policy will be based on "all" the facts. Inductionism is the belief that knowledge is induced by unbiased observation of all the facts which precludes preconceptions or the need for thought. Hilsman's observations of the depreciation of intellectualism, theorizing, and reasoning and the appreciation of simplism, activism, experience and "know-how" result from these beliefs that knowledge is "facts divorced from thought or interpretation." The consequences of these beliefs are an emphasis on (1) collecting facts at the expense of analyzing them, (2) current intelligence and day-to-day reporting at the expense of long-term analysis,[4] (3) encyclopedic accumulation of details at the expense of subtle reasoning, richness in qualification, experimentation, or scholarship (Hilsman,

[4]Former Director of Central Intelligence, William Colby (1978), describes with pride one of the major contributions during his tenure, the birth of the newspaper-like National Intelligence Daily, as "the focus of my effort to present our intelligence better and produce it better" (p. 354). Colby frequently joined the nightly editorial conferences that decided the next day's product. In fairness, it should be noted that Colby also initiated the system of National Intelligence Officers, twelve experts on various crucial areas whose responsibilities are primarily for long-term, in-depth analysis.

p. 58). These beliefs also inspire the attitude that intelligence failures could be prevented if the "right" people had the facts (or more facts), and if collection were greater or dissemination more efficient.[5]

Clearly, Wasserman's observations foreshadowed Garthoff's (1978) analysis of the fallacies of "just the facts, estimate capabilities and not intentions, and worst-case" in the estimation of intention.

The notion of a determined future derives from a belief in objective facts uninfluenced by humans, so that the job of intelligence is to determine "what the facts have in store," i.e., absolute rather than contingent or conditional prediction. Predictions based on the "objective, entire facts" are perceived as ultimate explanations which preclude alternative predictions. The emphasis is thus on accumulating details and avoiding bias rather than on critical hypothesis testing, speculation, evaluation, and measurement.

Wasserman's recommendation that naive realism, inductionism, and determinism be replaced with "the notion of an a priori or deductive testable knowledge" and the explication, testing and modification of hypotheses, reflects his preference for a "scientific" intelligence, and his belief that strategic surprise "in theory. . . should not be possible" (p. 157). However, Wasserman relies on a version of science (c.f., Wasserman, 1960: 161, fn. 1) which is examined and challenged

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[5] Former Deputy Secretary of Defense (and now Governor of Texas) William P. Clements, Jr., is quoted as saying (Beecher Report, in Fain, et al., 1977: 320): "In every instance I know about where there was a horrendous failure of intelligence, the information was in fact available to have averted the problem. But the analysts and the system didn't allow the raw data 'o surface."

later in this paper. This "normal version" of scientific theory as testable creates problems for science which will also affect intelligence should Wasserman's recommendations be adopted.

The fourth element, the weak and dependent role of intelligence, is related to the second factor that leads to intelligence failure, the relations between intelligence and policymaking. There are two problems which arise from these relations: First, because intelligence is supposedly separate from policy it becomes, on the one hand, remote from policy, and, on the other hand, because it is subordinated to policymaking, intelligence is used to rationalize or justify the policy line. As Wasserman points out (p. 161) intelligence is both too separate and not separate enough from policy.

The Mosaic Theory. Wasserman's study was based on Kent's (1949) and Hilsman's dated observations of intelligence. The intelligence community still displays the naive realism, inductionism and determinism that Wasserman labeled the "official theory" of intelligence. Heuer (1978), an observer of intelligence analysis, terms this the "mosaic theory," noting that it continues to characterize intelligence officers. He writes:

> The function of intelligence [according to the mosaic theory] is to collect small pieces of information that, when put together like a jigsaw puzzle, eventually enable us to see a clear picture of reality. Accurate estimates depend primarily upon having all the pieces. . . it is important to collect and store the small pieces. . . we never know when it will be possible to fit a piece into the puzzle . . .

Lewin (1978), in his history of signals intelligence in World War II, uses the same imagery (p. 91):

> All intelligence work involves assembling a mosaic whose key pieces are missing while others are broken or defaced.

Intelligence too separate. The Church Committee investigations (Fain, et al., 1977; 42-52) of the CIA provided several recent examples of the impact of intelligence "too separate" or "not separate enough" from policy. An example of the former occurred in 1970 when CIA Director Richard Helms judged it to be inappropriate to forward to the White House a memorandum on future developments in Indochina since its authors did not know of the planned U.S. military intervention in Cambodia. Although the analysts (without knowing of U.S. plans) had included an appraisal of the possible consequences of U.S. intervention in Cambodia, their views (subsequently shown to be valid) that such intervention would not thwart North Vietnamese control in Indochina were kept from senior U.S. policymakers.

Not separate enough. An example of intelligence "not separate enough" occurred in 1969, according to the Church Committee, when the White House and Secretary of Defense "indirectly pressured the DCI [Director of Central Intelligence, i.e., Helms] to modify his judgments on the capability of [and intentions behind] the new Soviet SS-9 strategic missile system" (Church Committee Report, in Fain, et al., p. 46). Defense Secretary Laird pressured Helms to modify the draft National Intelligence Estimate (NIE) on Soviet strategic forces to conform to his own views on Russian capabilities and intentions. Garthoff (1978: 23) used this episode as an example of the consequences of estimates of intentions from "worst case" estimates of capabilities:

> . . . Laird in 1970 estimated a Soviet SS-9 ICBM buildup to be some 60 percent larger than the one finally completed. . . and then

> concluded [and pressured the NIE to conclude] and publicly stated that the Soviets were "going for a first strike capability" -- based on his own (erroneous) worst-case capabilities estimate.[6]

DeWeerd

Like Etheredge, DeWeerd sees the basic cause of failure to be the responsibility of policymakers, but he points to several components of the failure process due to both policy and intelligence. Thus DeWeerd's is not an analysis of intelligence failure but it sheds additional light on the role of intelligence estimates of enemy intentions in strategic failures.

DeWeerd points (p. 452) to three problems in the warning process: (1) separating signals from noise, (2) efficiently coordinating and assessing intelligence signals, and (3) determining appropriate responses to assessments; the first two being the province of intelligence. He states that the climate of opinion is the "main factor" affecting the success of each of these steps.

His analysis of the component elements of the strategic failures in the Korean War reflects this emphasis. He notes that the North

[6]The policy consequences in this case were that (1) U.S. missile capability was greatly increased by the addition of MIRV (multiple) warheads, (2) no MIRV limits were included in the Strategic Arms Limitations Talks, and (3) the United States embarked on an expensive and controversial Anti-Ballistic Missile (ABM) building program. Each of these decisions has been found in retrospect to have been flawed: U.S. MIRV deployment was excessive and premature given the Soviet threat then existing and probably led the Soviets to MIRV; the opportunity to limit MIRV was lost, and the ABM program was abandoned as unworkable. The failure of intelligence to provide a counterargument against the assumptions underlying these choices contributed to these errors.

Korean attack and the Chinese intervention were both preceded by clear intelligence warnings and estimates but that these were rejected or ignored at the highest policy levels for the reasons shown in Table 4.1. (The categories on the left are drawn from the Turner disaster model which is described in Section 5.)

The impact of intelligence in the Korean failure was minor, according to DeWeerd; its responsibility was limited primarily to its failure to sound warnings forcefully and in concert. Policymakers were able to disregard intelligence warnings and estimates presumably because intelligence did not attempt to challenge the prevailing climate of opinion. Other reviews of this episode reach similar conclusions. For example, George and Smoke (1974) conclude that the Korean and Chinese attacks were not intelligence failures in the narrow sense of failing to assess capabilities and intentions. However, they suggest (p. 208) that "intelligence appraisers were evidently subject to some of the same psychological influences and misperceptions that distorted the views of top-level leaders . . . the most sober and valid CIA estimate came too late to goad top-level decisionmakers into correcting policy errors already made." Nevertheless, CIA estimates from 9 to 24 November (the day before the major Chinese offensive) became increasingly ominous, ultimately predicting the intensity of the coming attack. But George and Smoke note that warning came "too late" (p. 212) to change policy made largely without intelligence inputs.

Intelligence and policy. Several observations about the interrelation between intelligence and policy emerge from the factors listed in Table 4.1. The post-Pearl Harbor assumptions by policymakers

Table 4.1. Elements of Strategic Surprise in the Korean War (DeWeerd)

Initial Beliefs and Norms	Post-Pearl Harbor assumptions regarding intelligence: 1) if we collect everything, we will be reasonably sure of not missing key items, 2) improved coordination and wider dissemination of intelligence will safeguard against surprise attack, 3) because strategic warning is necessary, it will somehow be provided, and we will make the necessary responses.
Incubation Period	
a. Erroneous Assumptions	1. North Korean capabilities were underestimated, South Korean capabilities overestimated by top policymakers. Were correctly estimated by intelligence but ignored.
	2. Policymakers assumed U.S. nuclear umbrella would deter aggression by everyone who mattered, i.e., Russia and China, and everyone else was harmless.
	3. Intelligence warnings of North Korean attack were "crying wolf", i.e., meaningless.
	4. The question of limited wars had not been considered, Korea was not the "right" war, i.e., the war we were ready for.
	5. General belief that next war would be all-out affair with Soviets, and main threat in Far East was subversion; direct enemy military action was ruled improbable.
	6. Erroneous assumptions about why the Chinese would not enter the war: a) war might weaken internal Communist control b) entry holds no real advantage for the Chinese c) entry would weaken China's international position.

b. Information Handling Difficulties	1. Intelligence and warning responsibilities were widely divided among agencies prior to the war and shifted during the war.
	2. Ad hoc groups usurped analysis and decisionmaking functions from established and experienced organizations.
c. Lack of Standards and Precautions	1. No war plans for war in Korea existed.
	2. No estimate on the Korean situation existed.
	3. U.S. forces in Korea and the Far East were unprepared to defend South Korea.
d. Minimizing Emergent Danger	1. Gradual buildup of Communist forces immunized us against drawing proper conclusions from intelligence. Costs of reacting to warning would have been too high given current climate of opinion.
	2. Warnings from India and China of Chinese intervention were disregarded.
	3. Chinese and North Korean military capabilities were underestimated by MacArthur while he overestimated the capabilities of his own forces.

about what was required for adequate warning reflect the prevalence of the naive realism, inductionism, and determinism noted by Wasserman. The varying estimates of capabilities held by the policymakers and intelligence suggest a disregard of intelligence at the top, i.e., intelligence "too separate," although it is not clear whether this was due to the low-ranking of intelligence officials within the hierarchy, a general disregard of intelligence, or a tendency to ignore outsiders. The facts that direct warnings from Indian and Chinese spokesmen were ignored, and that intelligence was excluded from policymaking circles tend to support the last explanation.

The diversity of intelligence agencies and ad hoc policy groups making estimates (e.g., CIA, State, NSC, MacArthur's Far Eastern Command) permitted decisionmakers to choose those estimates or opinions most in keeping with their own outlooks, a problem noted by Wohlstetter (1962) of the pre-Pearl Harbor era. The observation by the then Secretary of Defense that intelligence had "cried wolf" implies an attitude by policymakers that intelligence warnings, strategic or tactical, will be unambiguous and absolute. Finally, DeWeerd notes that acting on warning carries high costs as well as the high risks that the warnings are false alarms, or that warning followed by action will forestall the threat, and the warning thus seem to have been a fake alarm. Although ignoring warnings may greatly increase the ultimate costs, it eliminates the risk of acting prematurely or on false alarm. The latter risks are likely to be more salient to the policymaker than the risk of a surprise attack. Responding after a surprise attack needs no justification whereas taking action on warning

entails the risk of false alarms, needless anxiety and tension, and thus requires strong justification.

In effect, failure to act on warning is the opposite of "worst-case" motivated behavior. In the worst-case situation, the policymaker accepts the most pessimistic possible course of action as the basis for planning and action. DeWeerd describes the more likely case, that warning will not specify the worst possible case but rather what seems to be the most probable range of enemy action, and that policymakers will not entirely accept the warning as the basis for action. Consequently, the decisionmaker will demand unambiguous warnings which intelligence will be unable to provide.

Perceptual Filters

> So much of what we see, so much of what we perceive, so much of what we experience is in truth what we conceive.
>
> Robert L. Sinsheimer, 1971

> Ye cannot find the depth of the heart of man, neither can ye perceive the things that he thinketh.
>
> Judith 8: 14

Several scholars of intelligence failures have focused on the perceptions of the intelligence specialist as well as policymaker. This theme differs from that just reviewed in emphasizing the widespread impact of perceptual filters on all actors in the foreign policy process. Whereas Wasserman and DeWeerd suggest that intelligence estimates of foreign intentions are, or were, relatively objective but were rejected by biased decisionmakers, this second theme examines the pervasiveness of perceptual biases in both intelligence and policy.

Avi Shlaim (1976), using as a case study the failure of Israeli intelligence to foresee the Egyptian-Syrian surprise attack of 1973, argues that "failure of an intelligence prediction can be reduced, in the last analysis, to a misunderstanding of the foreigners' conceptual framework -- i.e., a failure to understand properly the assumptions or interpretations of the situation upon which foreigners base their decisions" (p. 363). David Lampton (1973) examined the importance of U.S. presidents' "image of the opponent" (in this case China) in shaping the definition of the foreign policy situation and presidential evaluations of alternatives. Robert Jervis (1968) discusses in detail the hypothesis that policymakers must develop an image of the other state and of its intentions, but "this image may, however, turn out to be an inaccurate one; the actor may, for a number of reasons, misperceive both the other's actions and their intentions" (p. 464).

Shlaim

Shlaim concurs with Wasserman and DeWeerd that failures to correctly estimate the intention of an enemy to launch a surprise attack cannot be attributed to a lack of information. However, despite the volume of data on enemy moves and preparations, and, in the case of the 1973 October War, considerable data on military capabilities, intelligence specialists reach the wrong conclusions about intentions for psychological and institutional reasons. By the latter Shlaim means the relationship between intelligence and policymakers. He makes the case that in the October War the monopolistic structure of Israeli intelligence permitted the Military Intelligence heads to promulgate an unquestioned (but unfortunately erroneous) "conception" of Arab intentions which was accepted without challenge by the Meir Cabinet. In

contrast to Wasserman (1960), who emphasizes the subordination of intelligence to policy, Shlaim points to the danger of a monopolistic intelligence structure which comes to dominate all analysis of foreign states' behavior. He concludes that this structure in Israel "was largely responsible for the narrow, dogmatic, and monolithic thinking that characterized the estimates presented to (the Meir Cabinet)" (p. 369).

"All-important" images. Shlaim's primary focus is on the psychological causes of intention estimation failures. He writes that the "most difficult and most crucial" (p. 362) task of intelligence is the estimation of enemy intentions, but these are "notoriously elusive" and the analyst must deal with incomplete and unreliable (although voluminous) data. Knowledge of capabilities does not yield an infallible estimate of intentions. Such estimates, according to Shlaim, require that the analysts employ "models, theories, or conceptions that are functional equivalents of an ideology . . . the commonest mistake is to make an uncritical interpretation of the enemy's intentions by applying a theory or model based on assumptions that may be correct about oneself, but are not necessarily correct about the enemy" (p. 363). This "mirror image of the enemy" (Garthoff, 1978) is identical to what Wasserman (1960: 167) termed "the uncritical interpretation of foreign states' actions in terms of one's own framework." However, Shlaim points the finger of blame at intelligence as well as policy. Inasmuch as Shlaim relies heavily on the previous work of Jervis (1968), Wohlstetter (1962, 1965), and Whaley (1973), which are reviewed below, further review of Shlaim is deferred until the section below which uses the October War as a test case of a tentative model of intelligence

failure. Shlaim's basic point (p. 357) is that "images, beliefs, ideological bias, wishful thinking, natural optimism or pessimism, confidence or the lack of it" all influence the data the analyst attends or ignores, the weight he attaches to the facts he selects, the theories he creates, and the conclusions he draws. In short, images "play an all-important part" in the making of intelligence estimates of intentions.

Lampton

Lampton's (1973) paper deals with intention estimation indirectly, by questioning how the theories or images of foreign policy analysts influence policy outcomes. Specifically, he examines the image of the opponent (China) held by Truman, Eisenhower, and Kennedy (and their advisors) to determine how these images were influenced by the policies of previous administrations and in turn how the images influenced policy. Lampton's three case studies provide insights into the psychological process stressed by Shlaim. (See Ben-zvi 1978, for a similar approach.)

The image of the opponent is conceived by Lampton to consist of (1) the image of the opponent's operational code, and (2) the image of the opponent's strength. The notion of operational code is taken from the work of George (1969) and refers to several characteristics of the opponent: the control the opponent believes he has over history and the course of events; the opponent's concept of political or military conflict; the opponent's views on effective political means; and the opponent's views on risk. The image of the opponent's strength is composed, according to Lampton, of perceptions of the opponent's capabilities, and perceptions of the vulnerabilities of the opponent to one's own power or influence.

Lampton notes (as have others) that in crises and periods of high stress, presidents and other leaders tend to form close, tight-knit decisionmaking circles of trusted advisors and central actors (e.g., Secretaries of State and Defense, Chiefs of Staff). In these situations, according to Lampton, "the more fundamental images and stereotypes of the enemy are likely to be particularly important in policymakers' evaluation of available information and their selected responses" (p. 48). Lampton concludes that the accuracy of perceptions regarding China played a crucial role in the policies of the three administrations he studied.

In the case of the Truman administration the image of the opponent was faulty in overemphasizing an underlying mutual friendship between the United States and China; and in underestimating the importance of U.S. military moves in Asia to Chinese security interests, Chinese willingness to take military action, and China's military and ideological power. The consequence of this faulty perception of China's operational code and her power was the failure to attend the signals that China would intervene in Korea. According to Lampton, the "lesson" of Korea was overgeneralized by Eisenhower and Dulles in the case of French Indochina. Their perceptions of China failed to recognize that China's policy was conducted in political and diplomatic terms as well as in the military dimensions which Washington overstressed. On the other hand, Lampton sees Kennedy as correctly perceiving China as having various interests in Southeast Asia, and as having learned appropriate lessons from the two previous administrations' failures which were successfully applied in the Laos crisis.

Lampton's decomposition of the perceptions of policymakers into operational code and power conforms to the dimensions actually used by these figures in their statements; additionally, these dimensions also characterize intelligence estimation of intentions (see, e.g., Wohlstetter, 1963, 1965, or George, 1959). That is, the train of logic which seems to apply in both cases starts with a general image of the opponent's self-concept and philosophy of world affairs, his motives, his outlook on risk, and his power capabilities and vulnerabilities. The composite of these elements (the image of the opponent) becomes the perceptual filter through which incoming information is screened, and ultimately combined into an appreciation of the opponent's intentions.

Jervis

In a highly influential article, Jervis (1968) discussed fourteen hypotheses on misperceptions of other state's intentions due to an actor's image of the other state and of self (see Table 4.2.). As Jervis notes these hypotheses were developed from social and cognitive psychology and from Lewis Richardson's analysis of arms races. These hypotheses clearly go beyond the "image of the opponent" or a simple working hypothesis of the enemy which happens to be wrong because it is too heavily based on ethnocentric assumptions. Jervis's analysis probes the interplay between actors in the international arena, considering how signals, gestures, and messages are created and intended, how they are transmitted and received, and how the psychological characteristics of the actors tend to distort such signalings. Hypotheses 5-7 and especially 10-13 deal with the misperceptions that occur during interactions.

Table 4.2. Jervis's Hypotheses on Misperception of Intentions

1. Decisionmakers tend to fit incoming information into their existing theories and images, which in turn determine what they notice.

2. Decisionmakers tend to be too wedded to the established view and too closed to new information and unwilling to alter their theories.

3. The image of the other is less affected by contradictory information which arrives bit by bit than by discrepant information that is considered all at once.

4. Concepts and categories about the other state's behavior which are new are the most difficult for the decisionmakers to learn and include in the images of the state, while the application of concepts and categories learned for other states to a new case is far less difficult.

5. Misunderstanding tends to result when messages are sent between parties who differ in their backgrounds of concerns and information.

6. The greater the time spent in making a plan or a decision the greater the tendency to believe that the messages about it the sender wishes to convey will be clear to the recipient.

7. Actors often do not realize that actions intended to project a given image may fail to have the desired effect because the actions themselves do not turn out as planned.

8. Overall, decisionmakers tend to see other states as more hostile than they are.

9. Actors tend to see the behavior of others as more centralized, disciplined, and coordinated than it is, and complex events are unjustly squeezed into a perceived pattern.

10. Familiarity with the other state's Foreign Office tends to lead decisionmakers to believe that the position of the Foreign Office is the same as the position of the other state.

11. When other states act in accordance with an actor's desires, the actor tends to overestimate the degree to which the other has been influenced by the actor; when the behavior of the other is undesired it is usually seen as derived from internal forces.

12. When actors do not attempt to conceal intentions they assume their intentions are accurately perceived by others.

13. It is difficult for an actor to believe that others see him as a menace, and even harder to see that issues important to him are not important to others.

14. Actors overlook the fact that evidence consistent with their theories may also be consistent with other theories.

The image hypothesis. Jervis seems to place his major emphasis however on what might be termed the image hypotheses (1-4, 8, 9, 14) which assert that decisionmakers and analysts of intentions are strongly influenced by their image of the opponent. Jervis does not assume that the adjustment of incoming information to fit and confirm preexisting hypotheses is necessarily irrational, but argues instead that this is consistent with empirical logic (although the mental processes employed in a particular case may be completely illogical). That is, theories and hypotheses are essential for interpreting and identifying facts and "pure empiricism" (analysis without theories or hypotheses) is "impossible." Jervis also clearly differs from Wasserman's (1960) recommendation that intelligence be based on deductive testable knowledge, pointing out that scientific theories are not so much tested as replaced by better theories. Further, while a particular theory is extant, it is scientifically important that observations be fitted into it if possible; conversely, a theory that successfully accounts for a large number of observations should not automatically be rejected because of a few discrepant findings. Wasserman, in other words, does not provide a solution to the problem of images in the estimation of intentions, according to Jervis' argument, but rather one-half of the dilemma which confronts scientist and intelligence analyst alike: how "open" should one be to new information (especially when the opponent is able to manipulate information in attempts to deceive), and how closely should one hold one's theories (especially since these may be erroneously based on ethnocentric assumptions). Jervis' hypothesis 2, that theories tend to be too strong and analysts too closed to new information, and hypothesis 14, that analysts do not apply new information objectively to all theories,

accords with Shlaim's (1976) stress on the negative effects of "the conception" Israeli intelligence officers had of the Arabs and Wasserman's emphasis on "uncritical frameworks" of policymakers.[7]

Safeguards. Jervis makes several recommendations regarding the dilemma of being too open or too closed to new information. He presents five "safeguards" to reduce the problems of being too wedded to a given theory. The first is that analysts should be aware that they do not make unbiased interpretations of each piece of information. Such an awareness, according to Jervis, should lead them to examine discrepant information more closely. Second, analysts should determine whether their theories contain elements which are not logically linked, since unlinked elements may be held to be true because they are psychologically consistent with each other (e.g., if I know a nation is hostile to a nation friendly to me, and I know the first nation is strong, I may tend to also believe that nation constitutes a threat to me) while being unsupported by evidence. Third, decision-makers and analysts are advised to make their assumptions, beliefs, and predictions as explicit as possible and to determine before events occur which events would tend to support these beliefs and which are inconsistent with them. The fourth safeguard is to prevent an individual's or an organization's functional identity and health being tied to or dependent on specific images and theories (the frequent accusation that the Defense

[7] Joseph de Rivera and J. M. Rosenau (1968) use many of these hypotheses in their analysis of the psychological dimensions of U.S. foreign policy during the Korean War. While they do consider organizational and political factors, they emphasize the role of individual perceptions and especially the tendency to fit information into expectations or else reject it.

Department overestimates the Soviet threat reflects this problem). The fifth recommendation is that analysts and agencies develop a willingness to play with material from a variety of angles and entertain unpopular as well as popular hypotheses. Specifically, decisionmakers should explicitly structure conflicting biases into the policy and decision-making process.

Jervis notes that these safeguards may divert resources from other tasks and would increase internal dissension. However, he does not note that his recommendations require in effect that decisionmakers become less influenced by their perceptions. That is, Jervis' recommendations seem to suggest that analysts and decisionmakers can stop being victims of their perceptions when they begin to act like rational actors (Kinder and Weiss, 1978). For example (second recommendation), it seems quite likely that individual analysts would have no trouble establishing what seem to be logical and valid links between elements of their theories and assumptions (which might nevertheless be erroneous). Playing with material (fifth recommendation) from a variety of angles and entertaining various hypotheses can be a means of opening one's mind to new information, but it can also serve to "innoculate" one's attitudes so that one finds it easier to discount or explain away discrepant information (McGuire, 1968). Even if decision-makers create an environment of compensating biases (which implies the decisionmakers can somehow identify their original biases so as to compensate for them), they must also anticipate that their perceptions will lead them to prefer the views of analysts with similar perceptions. It is probably much easier to create devils' advocates then to listen to them.

The Signal-Noise Theme

The theme that decisionmakers' and intelligence specialists' perceptions intervene between them and the realities of the international environment has been refined by Wohlstetter (1962, 1965), Holst (1966), Whaley (1973), George and Smoke (1974), and Handel (1976) using concepts from information processing theory. These writers emphasize that perceptions do not distort an "objective" reality; rather they act as filters which separate what seems to be information from that which seems to be confusion (or signals from noise in the terminology of information theory). The determination of just what is a signal (and therefore important and necessary for the analyst to notice and explain) and what is noise (and therefore irrelevant and possible to ignore or disregard) is a psychological process which occurs consciously, subconsciously, and perhaps unconsciously according to individual tendencies and predispositions that in turn depend on perceptions and expectations. Without these perceptions and expectations the process of translating diverse stimuli into useful information cannot proceed. Furthermore, these perceptual processes require consistencies and routines that tend to produce and reinforce the "closedness" to novel information and the "weddedness" to tested theories and assumptions which Jervis stressed.

Wohlstetter

Roberta Wohlstetter's study of intelligence failure in the Pearl Harbor attack defines strategic warning (1962: 389) as: (1) having a signal (defined as "a clue or a sign or a piece of evidence that tells about a particular danger or a particular enemy move or intention"),

(2) perceiving the signal as a warning, (3) communicating the warning unambiguously, and (4) acting on the warning. She makes clear (p. 391) that, in the Pearl Harbor attack, operational as well as intelligence agencies were responsible for failures to perform steps 2, 3, and 4. Responsibility for the failure of strategic warning is, in her view, shared by intelligence and policymakers.

The central cause of such failures is an inability to perceive a clear pattern of enemy intentions. Just as Wasserman notes, the problem is not insufficient data, rather, Wohlstetter writes (1962: 397),

> The roots of this (Pearl Harbor) surprise . . . (are found) in the conditions of human perception and stem from uncertainties so basic that they are not likely to be eliminated . . . the relevant signals . . . will be partially obscured before the event by surrounding noise . . . in conditions of great uncertainty people tend to predict events that they want to happen actually will happen.

Wohlstetter (1965: 691) defines "noise" as:

> the backdrop of irrelevant or inconsistent signals, signs pointing in the wrong directions, that tend always to obscure the signs pointing the right way.

This basic relationship: the difficulty of creating a clear image of the adversary's intentions from an incoherent mixture of signals and noise given the strong human tendency to perceive what is expected and preferred (oversimplifying, we might term these the signal-noise problem and the wishful thinking bias) leads to eight specific factors which Wohlstetter sees as underlying intelligence failure (1962: 393-5, 1965). These factors are listed and briefly described in Table 4.3.

Table 4.5. Factors in Intelligence Failures (Wohlstetter)

1. Noise and Ambiguity:	Masses of conflicting evidence supporting alternative and equally reasonable hypotheses create an environment of noise that complicates coherent analysis. Piecemeal and day-to-day analysis produce additional confusion and incoherence.
2. Expectations and Preferences:	Tendency of analysts and policymakers to select hypotheses which accord with expectations and preferences and to favor supporting evidence, obscuring alternative hypotheses.
3. Security and Deception:	Successful enemy security reduces signals and indications of intentions, while successful enemy deception creates noise that appears to the analyst or policymaker to be a signal.
4. Cry-wolf Phenomena:	High-tension levels and repeated false alarms numb attention and vigilance. High costs of acting on warning reduce motivation and freedom to attend to warning.
5. Impact of Policy:	Other areas and interests may have captured the attention of the policymaker. Cautious policy led to cautious and ambiguous warnings. Intelligence unaware of its own nation's actions and policy.
6. Technical Surprises:	Ability of the enemy to rapidly change and keep concealed technical and logistic capabilities leads to inapplicable assumptions in estimation.
7. Organizational Blocks:	Delays in transmission or evaluation, or restrictions on dissemination of intelligence and warnings prevent effective prevention or defense. Organizational inefficiencies and rivalries, and low status of intelligence services impede effective analysis and dissemination. Policymakers were uninformed of military vulnerabilities.

8. Underestimate of Opponents' Willingness to take Risks:	Utilization of one's own utility calculus to estimate enemy intentions from risks facing enemy is misleading.

Unlike Wasserman (1960), who reasons that strategic surprise should be impossible "in theory," Wohlstetter (1962: 397) describes strategic warning as intrinsically uncertain and consequently in history strategic surprise is frequent. She states (1965: 707) that "we cannot guarantee foresight." Despite (or perhaps because of) the depth of her research and her elaboration of these dimensions on which intelligence problems occur, Wohlstetter offers very few recommendations for reducing intelligence failures. She is not hopeful: noting (1962: 400) "we cannot count on strategic warning." In fact she closes her book (1962: 401) on Pearl Harbor with the admonition that "we have to accept the fact of uncertainty and learn to live with it." She does, however make (1965: 707) three brief recommendations, the first two of which apply to intelligence analysis: (1) make a thorough and sophisticated analysis of observers' reports, (2) make frameworks and assumptions explicit and tentative, and (3) make the range of responses to crises selective, refined, and divisible so that the responses can be flexibly tailored to the situation.

Wohlstetter (1965) makes two additional and very cogent observations: (1) that failures to foresee and forestall catastrophes are very frequent and are not limited to the political-military arena, and (2) that both science and intelligence depend on implicit as well as explicit theories and hypotheses which may produce signal-noise problems for analysis. Her first observation relates intelligence failures with other "failures of foresight," an idea further explored by Wilensky (1968) and developed more fully in the next section. The second observation suggests the basic problem with Wasserman's

(1960) naive empiricism and his recommendation that intelligence be conducted more scientifically: namely that scientists themselves rely on implicit assumptions and hypotheses that are remarkably resistant to contradictory data and are discarded only after scientific revolutions (which share many characteristics with intelligence failures). This observation, also made by Jervis (1968), is explored more fully later in this paper.

Holst

Johan Holst (1966) relies heavily on the Wohlstetter "signal-noise" formulation in his analysis of "the prism through which the Norwegian authorities received and evaluated the incoming warning signals" (p. 33) of the Nazi surprise attack on Norway in the spring of 1940, and why the relevant signals failed to impress the Norwegian leaders. In his view, the basic problem in intelligence failure is that

> even if warning signals of a strategic or tactical nature should become available to the decision-making authorities . . . (they) may not be able to perceive the relevant messages in the flow of contradictory information . . . (T)he "right" signals were apt to be ambiguous, lending themselves to a variety of reasonable and rational interpretations . . . (S)ignals which confirm existing expectations are much more readily accepted than those which constitute a challenge (to the existing predispositional framework of the perceiver) (p. 33).

Holst follows Wohlstetter's framework closely, underscoring the similar features of the intelligence failure in Norway and at Pearl Harbor.

Whaley

Barton Whaley (1973) accepts Wohlstetter's concept of signals but, from his analysis of the German surprise attack on the Soviet Union (Operation Barbarossa), argues that the basic cause of the Russians' intelligence failure was not the ambiguous "background noise" competing with the signals which Wohlstetter emphasized in the case of Pearl Harbor. Whaley rejects Wohlstetter's communications theory (although he uses its terms and framework) and proposes his own theory of strategic surprise. He argues that the abundance of signals available to Stalin (he lists 84 separate warnings) from a wide variety of sources and agencies do not permit an explanation of Stalin's unpreparedness in terms of the ambiguity and uncertainty of the information which reached him. Whaley suggests that in fact there was very little ambiguity or uncertainty in Stalin's mind regarding Hitler's intentions.

The bodyguard theme. Whaley argues that Stalin was victimized, not confused,by a massive German effort to reduce Soviet uncertainties and misdirect Stalin's perceptions by means of "the greatest deception operation in the history of war" (p. 172). He presents evidence that Germany created a complex but plausible cover story which would explain their preparations for Barbarossa without alarming Stalin as to its real intent. This orchestrated plan had four parts; each mythical but consistent with German actions and with what was known of German goals; invasion of Britain; defense against Russia; buildup against the Balkans; and an impending ultimatum by which Germany would make demands on Russia (and thereby provide a warning prior to any

attack). Whaley argues that Stalin accepted at least the last myth,[8] and was surprised "by the deliberately false signals and not by the ambiguous signals, much less the distracting noise" (p. 242).

Whaley's thesis gains credence from the many instances of similar deception operations through history, beginning with the Trojan Horse, but especially as conducted by the British and Americans in World War II, which successfully fooled the Germans throughout the war. Cave Brown (1975) provides a detailed account of the London Controlling Section and the Bodyguard Plan which coordinated these deceptions (so-named from Churchill's dictum that "in wartime, Truth is so precious that she should always be attended by a bodyguard of lies"). Among these operations was the work of the Double Cross (i.e., XX, or Twenty) Committee which intercepted all German agents in Great Britain and turned them, that is, used them to feed false information back to their Nazi masters (Masterman, 1972). Another operation entailed creating the identity of a high-level courier for a corpse which was released off the coast of Spain to be washed up so that the doctored and misleading papers it carried would find their way to the Germans (Montagu's 1953 account of "Operation Mincemeat" became the well-known book and film, The Man Who Never Was). The most extensive Allied tactical deception plan (codenamed "Fortitude"), designed to mislead Germany as to the time and place of the cross-channel invasion of Europe, included the creation of two fictitious British Armies in the

[8] Which is doubly remarkable in that the Germans seem to have used this trick twice with success; first against Norway in the spring of 1940 and then in the spring and summer of 1941 against Russia.

Mediterranean, one in Scotland, and the phantom First U.S. Army Group opposite Pas de Calais, all of which the Germans swallowed.

While Whaley clearly rejects Wohlstetter's stress on the importance of noise in masking signals, introducing instead the concept of deliberate deceptive disinformation (which, as Table 4.3. shows, Wohlstetter considers but does not emphasize) it is also clear from Whaley's analysis and from the Allied experience that deception plans must be tailored to the enemy's perception and expectations. For example, Lewin (1978) concludes that the information on German expectations provided by the Twenty Committee's double agents and the intelligence from Ultra "enabled the deceivers to read the enemy's mind and twist it" (p. 322). Montagu, who represented British Naval Intelligence in the Allied deception operations, made the same point about the value of Ultra and double agents:

> . . .it (was) invaluable to any officer who had to try to deceive the German Intelligence, the Abwehr, if he knew what the Abwehr were already thinking and what other investigations they were making into a particular subject (1978: 49).

The success of strategic deception and, in the case of Barbarossa, strategic surprise depended heavily on knowing and manipulating the enemy's perceptions, theories, images, and expectations. Thus, while Whaley differs from Wohlstetter as to the importance of background noise, his analysis, like hers, focuses on the central roles of cognition and perception in intelligence failures.

George and Smoke

In their comprehensive attempt to systematically analyze the principles of American foreign policy, Alexander George and Richard

Smoke, in their study, Deterrence in American Foreign Policy (1974), propose what they term the response theory of intelligence failure. This theory is part of their overall theoretical analysis of deterrence in foreign affairs. Response theory deals with how the defender, relying on deterrence to achieve policy goals, decides to respond or not to indications that an opponent may attempt to challenge that deterrence, i.e., the problem of response to warning indicators. George and Smoke acknowledge that various psychological, social, organizational, and political mechanisms may hamper receptivity to and evaluation of warning indicators. However, response theory points to perceptual and cognitive factors as the basic causes of failures to correctly estimate the opponent's intentions. George and Smoke write that "recognition of intention indicators . . . requires complex cognitive interpretations" (p. 573) and that even intelligence that is "plentiful, consistent, and relatively free of noise" on the opponent's intentions does not speak for itself. Interpretation of such intelligence requires hypotheses about how the adversary approaches political conflict and theories or models about the opponent's behavioral style. They note that "one's image of an opponent affects one's interpretation of available intelligence . . . an incorrect or defective model . . . can distort even reasonably good factual information" (p. 583). They also agree with Knorr that even a good model of the opponent does not guarantee an accurate estimate of the opponent's intention if the model requires that the opponent's perceptions be undistorted.

Among the most common perceptual problems in postwar American foreign policy found by George and Smoke in their historical case studies is the tendency of U.S. policymakers to attribute to the adversary their

own line of reasoning, the "mirror image" tendency also noted by Wasserman, Shlaim, Lampton, and Jervis. But in addition to pointing to the role of ethnomorphic reasoning, George and Smoke also describe how the policy background at the time warnings are received can attenuate the reaction to these signals. For example, there is a reluctance to confront the possibility that deterrence has failed, or to reopen policy questions that were resolved only after arduous debate. Analysts tend not to challenge positions and policies that are "established" and have gathered their own momentum. In this regard George and Smoke address the same issues raised by DeWeerd.

George and Smoke reiterate the importance of the eight factors listed by Wohlstetter (see Table 4.3.) as causes of the Pearl Harbor failure and add to these four more: underestimation of the opponent's motivation, underestimation of the opponent's capabilities, the costs of responding to warnings if they are taken seriously, and the influence of domestic and organizational politics within the executive branch. These twelve factors are compared to six crises in Table 4.4. Some factors (such as the "cry-wolf" syndrome) they assess as having played no appreciable role in any of the crises. Only two factors are credited with contributing to all six intelligence failures: expectancies and preferences (i.e., perceptions and wishful thinking), and inadequate appraisal of opponent's risk calculations (i.e., inadequate image of the opponent). Clearly, George and Smoke view cognitive and perceptual processes as central and perennial features of intelligence failures.

Handel

Of all writers in the perceptual school, Michael Handel (1976), in his monograph, Perception, deception and surprise: the case of the

Table 4.4. Causes of U.S. Intelligence Failures Leading to Surprise

(+ = factor played a role; 0 = no appreciable role; ? = uncertain, difficult to interpret)

	Pearl Harbor	*Berlin Blockade*	*Korean War*	*Chinese Intervention*	*Berlin 1958*	*Cuban Missile Crisis*
1. Technological or logistical surprise	+	0	0	+	0	+
2. Delays in transmission/evaluation of intelligence	+	0	0	0	0	+
3. "Cry-wolf" phenomenon (*a*) withholding or downgrading intelligence from higher-up or theater authorities	+	0	+	0	0	+
(*b*) Nonreceptivity of higher authorities to intelligence warnings of "cry-wolf" type	0	0	?	0	?	+
4. Expectancies and preferences	+	+	+	+	+	+
5. Influence of policy background	+	0	+	+	+	?
6. Successful deception	+	0	0	?	0	+
7. "Noise"	+	?	+	0	+	+
8. Underestimation of opponent's motivation	+	0	+	+	?	0
9. Underestimation of opponent's capabilities	+	0	+	+	0	+
10. Inadequate appraisal of opponent's risk calculations	+	+	+	+	+	+
11. Problem of response if warning is credited	?	+	+	+	0	?
12. Influence of domestic and organizational politics	+	+	+	+	0	+

[a] From George & Smoke

Yom Kippur War, presents the most complex conceptual framework of intention estimation failures. This framework consists of two parts; an analysis of three "noise barriers" which affect the flow of information to analysts and policymakers, and the interactions between estimates of capabilities and estimates of intentions. Handel derives five paradoxical propositions (Table 4.5.) from this framework which he sees as evidence of "the unavoidable contradictions inherent in any intelligence work and . . . the impossibility of ever reaching fail-safe answers in strategic intelligence evaluations" (p. 7). Handel's pessimistic outlook devolves from his assessment of the centrality of cognitive and perceptual factors in intention estimation:

> Surprise is almost inevitably the result of human nature, of subjectivity in the final analysis, the psychological perceptions, wishful thinking, and the dialectic nature of conflict. These qualities can victimize the most sophisticated actor who unwittingly unintentionally and unwillingly plays into his adversary's hands and helps to set his own trap (p. 7-8).

While Handel's is clearly a model of failure at the level of the individual actor, and while he focuses on perceptual and cognitive factors, his formulations also suggest organizational factors which go beyond the individual and contribute to failure as well. Wohlstetter and George and Smoke discuss some organizational shortcomings that played roles in intelligence failures, but do not examine them in detail; nor do they relate them directly to their psychological central focus. Handel thus serves as a bridge from the individual model to the organization model of failure which is described below. Furthermore, because Handel stresses the general limitations which human cognitive processes place on the perceptual and estimative tasks of intention estimation

Table 4.5. Handel's Five Paradoxical Propositions

1. As a result of the great difficulties in differentiating between "signal" and "noise" in strategic warning, both valid and invalid information must be treated on a similar basis. In effect, all that exists is noise, not signals.

2. The greater the risk, the less likely it seems, and the less risky it actually becomes. Thus, the greater the risk, the smaller it becomes.

3. The sounds of silence. A quiet international environment can act as background noise which, by conditioning observers to a peaceful routine, actually covers preparations for war.

4. The greater the credibility of an intelligence agency over time, the less its reports and conclusions are questioned; therefore, the greater the risk in the long run of overrelying on its findings.

5. Self-negating prophecy. Information on a forthcoming enemy attack leads to countermobilization which, in turn, prompts the enemy to delay or cancel his plans. It is thus impossible -- even in retrospect -- to know whether countermobilization is justified or not.

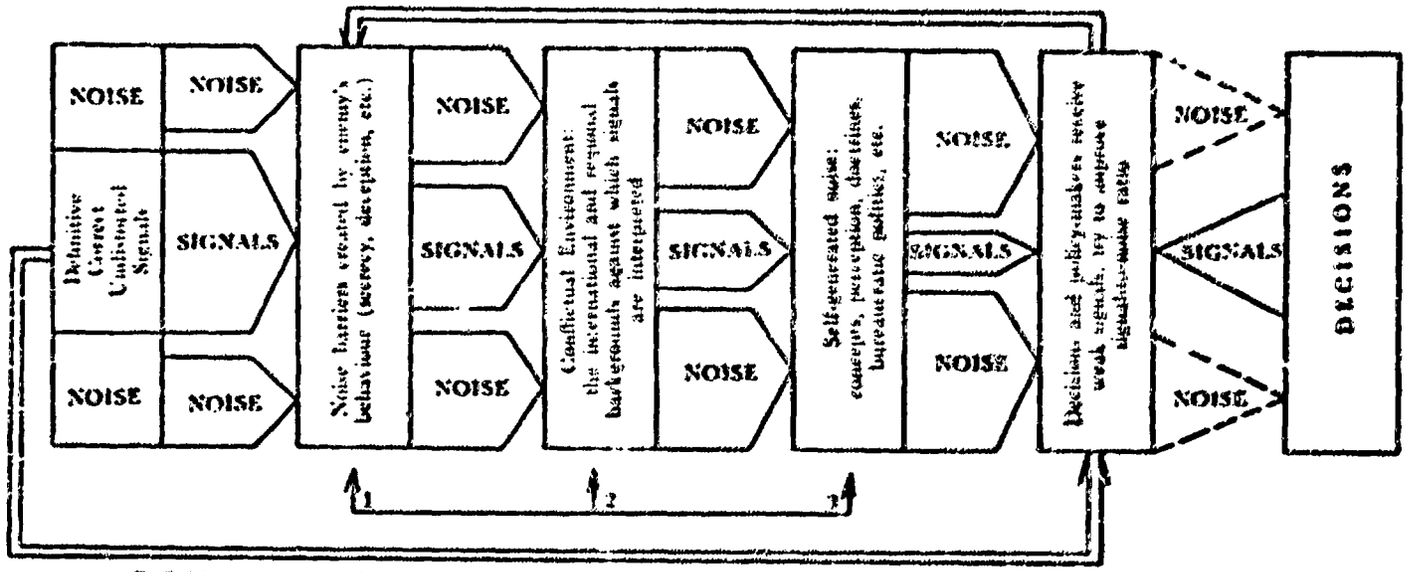
(rather than focusing on specific perceptions or on only signal-noise processing problems), Handel is also representative of the last theme of the perceptual school, namely that human cognitive faculties are generally unsuited for the tasks of preventing surprise by determining intentions.

Noise barriers. In his formulation of the role of three noise barriers in intention estimation failures Handel relies heavily on Wohlstetter's communications theory. He postulates three partially overlapping barriers (Figure 4.1.) which correspond to the levels of the foreign policy system: the global environment, the regional environment (i.e., the enemy), and the domestic environment (i.e., the images and perceptions of the defending country). Most information passes through these barriers and is distorted, weakened or attenuated. Furthermore, the barriers generate their own noise as well as passing on or perhaps amplifying noise from the previous barrier.

Enemy noise. In discussing the barrier created by the enemy Handel stresses that clear and unambiguous signals may not be forthcoming because the enemy himself may be uncertain about goals or courses of action, or may hold conflicting goals or options which are not resolved until just before the moment of action. Similarly enemy doctrines may shift or the opponent's leadership may change, making estimates of intentions problematic. The circumstances surrounding the enemy's decisions may change unbeknownst to the intelligence agency attempting to probe enemy intentions. Furthermore, the enemy's security and secrecy efforts will conceal intentions while enemy deceptions will attempt to disguise them.

Figure 4.1. Three Barriers that Reduce the Signal-to-Noise Ratio

In a conflictual environment no signals can ever simply be accepted as definitive and correct; therefore signals are received suspiciously, and are often re-examined through the three noise barriers.



Definitive, correct and undistorted signals transmitted directly to decision-makers.

The thickness of the noise or signal arrows indicates their relative strength. Note how the signal arrows become thinner as they cross the three noise barriers, while the noise arrows thicken.

*From Handel

Handel also notes that the force planning an operation has the advantage of putting the opponent on the horns of a dilemma by threatening him in two or more places and consolidating forces at the last possible moment. If plans are made so the actual attack might be launched from any of the planned directions, the opponent is forced to either defend all avenues, or, less plausibly, prepare a defense with greater responsiveness and flexibility than that possessed by the attacker, or, least likely, hope to penetrate the attacker's plans in advance. Similarly, a country planning to initiate war might arrange that several countries seem equally likely to be the target.[9]

Handel's emphasis on enemy uncertainty seems somewhat overdrawn: although enemy uncertainty or confusion will certainly complicate the intelligence analyst's task, the more threatening problem would seem to be the enemy who is certain of his plans and goals and who can carefully guard and disguise his efforts from his opponent. This latter case is especially impenetrable by intelligence, according to Handel, because each piece of information may be valid, or it may be a piece of the

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[9]Such tactics are frequent in combat. For example, Pershing concealed the objective of the American Army in the summer offensive of 1918 by having one of his corps commanders, Bundy, unwittingly prepare an offensive farther to the south. When Bundy was ultimately told that the southern attack was a ruse, he correctly observed "If it isn't a fake, we are ready to attack as soon as our troops arrive. If it is a fake, it's a good one, and I hope it works." Pershing's German opponents were well aware that Bundy's plans could be a deception intended to mislead them as to the place of attack but they noted "there is nothing to indicate that it is not the real point of attack," and they consequently reinforced the sector with three more divisions (The Editors of the Army Times, 1963, 61-75). Similarly, American official estimates of Japanese intentions prior to Pearl Harbor were misled by her quite real capabilities and actions which suggested she might first attack Thailand, Indochina, or Siberia (Wohlstetter, 1962, chapter 5).

enemy's deception plan. Handel's first paradoxical proposition modifies Wohlstetter's signal-noise principle and makes it more consistent with Whaley's emphasis on the impact of deception (see Table 4.5.), asserting that the reservations raised in the analyst's mind by the possibility of enemy deception force the analyst to view each piece of information as if it were deception, even if it is not.

In short, there is the noise the enemy unconsciously generates (e.g., by his indecision), and the noise the enemy creates intentionally (e.g., for deception purposes) as well as the enemy's efforts to mask possible signals (security). Handel discusses an additional difficulty, that created by an enemy who is ready to take "greater than usual risks" (p. 15). While the underestimation of the enemy's willingness to take risks is a component of intelligence failure (as several of the writers reviewed above have noted), it is not a problem created by the enemy so much as a faulty image of the opponent held by the intelligence specialist, thus it and paradox two are reviewed below as elements of the domestic noise barrier.

International noise. The international environment creates noise barriers for the intention estimation analyst in two ways, according to Handel. In the first case multiple hot spots, areas of tension or potential conflict, capture the attention of analysts and especially policy-makers and divert them from the direction the attack actually comes. In the second case a calm international background muffles indicators of conflict, leading to Handel's third paradox.

The notion that surprise attacks can come like a bolt from a calm blue sky is perhaps Handel's weakest assertion. He must stretch

and distort the case of the Yom Kippur War to make it fit his "sounds of silence" paradox. While it is probably the case that Israeli experts did not expect the tensions they were monitoring to turn into war, the environment prior to the October attack was hardly "quiet." To the degree that the Israelis expected the Soviets to restrain the Arabs from attacking Israel, as Handel suggests, the analysts' fault was not in being "conditioned to a peaceful routine" but in overestimating the Russians' influence and underestimating Arab initiative and independence. Handel's third proposition is not paradoxical so much as just untrue.[10]

Self-generated noise. Logically, underestimation of the opponent's willingness to take risks and Handel's second proposition are elements of the third noise barrier: self-generated noise. The central component of this barrier is the "conceptual framework for action" of the intelligence service and the decisionmakers. This framework consists of hypotheses and assumptions concerning the enemy's intentions and capabilities which are compared to one's own capabilities and intentions to determine the probability of conflict in the short-, medium-, and long-term future. Such conceptual formulations are necessary to interpret data and constitute the final goal of analysis itself. However, they create problems because they may become too rigid, dogmatic, and inflexible, with the consequence that "all data are deterministically interpreted and forced into the mold of the existing concept, gradually leading to a growing gap between the objective and the perceived reality, while wishful thinking on the part of the intelligence unit prevails" (p. 17-18).

Perhaps the most critical problem created by self-generated noise is the inability of the intelligence specialist to assess correctly

[10] Handel makes several incredible assertions, e.g., "one can influence the enemy's mind, i.e., his intentions to go to war: but one

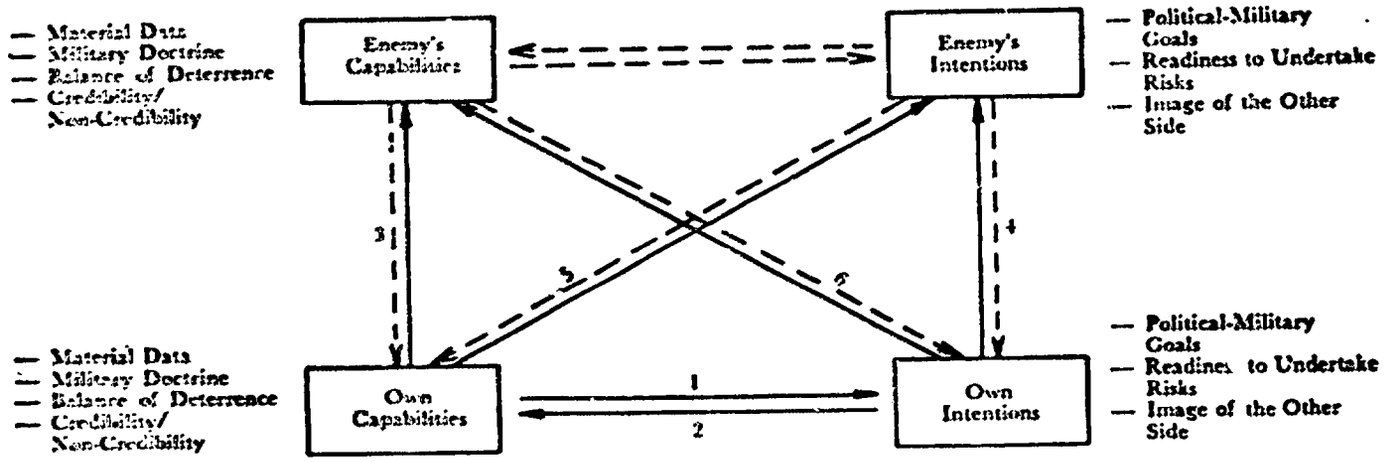
10 (continued) cannot have any impact on his capabilities" (p. 62). If true, negotiations would prevent all or most wars, and the outcomes of all wars would be predictable in advance on the basis of existing capabilities. In fact the opposite seems to be true: it is usually impossible to have an impact on the intentions of an enemy bent on war but one can deter by reducing the effectiveness of the enemy's capabilities by taking countermeasures. If anything is likely to deter the enemy's willingness to wage war, it is the possession of an invincible capability to destroy the enemy's first blow. For example, the North Vietnamese decision on whether to launch a partial or an all-out offensive in 1975 was contingent on whether the United States would resume large-scale bombing following the initial NVA attacks (Snepp, 1977). While a military leadership may risk defeat in the long run if there is a high probability of success in the first attack (e.g., Pearl Harbor, Barbarossa), they are far more cautious when the reverse is true (e.g., Bay of Pigs). Examples demonstrate the falsity of Handel's assertion: On the one hand, the Hull-Nomura negotiations failed to make any significant impact on the Japanese intentions; Chamberlain's and Stalin's appeasements failed to stop Hitler's attacks; while on the other hand, the British battleship Dreadnought made the battleship capabilities of all the world's fleets (including the British) obsolete almost overnight; radar, sonar, and the convoy system put Hitler's U-boat blockade out of business; British electronic warfare ended the Luftwaffe's night bombing capabilities; etc., etc. Deterrent forces must have a credible impact on capabilities if they are to have any impact on enemy intentions (George and Smoke, 1974, chapter 1), instances of direct influence on enemy intentions are far less common than instances of impacts on enemy capabilities.

the enemy's views of the risks involved in the options open to them. Handel, among others, notes that historically analysts have failed to perceive the international environment from the enemy's perspective and have underestimated the enemy's willingness to take risks and/or overestimated the risks of options later chosen by the enemy. Handel's second proposition leads to the conclusion that risk assessment by the enemy and the analyst tend to be highly uncertain. Our own analysis of intelligence failures and strategic failures (Section 3) and Wohlstetter's analysis of Japanese planning prior to Pearl Harbor suggest a corollary to Handel's proposition: The plans most likely of all to surprise are those so risky that they are certain, in the long run, to ultimately fail (perhaps this could be called the "sic transit gloria surpri" corollary).

Handel's formulation of the self-generated noise barrier is clearly drawn from Jervis (1968) and Handel outlines the dilemma of conceptual models "too open" to data or "not open enough" in the same terms as Jervis. Handel's recommendation favoring competing intelligence agencies with different and "relatively open concepts" is also similar to one of Jervis' safeguards.

Intentions and capabilities. The second part of Handel's framework depicts the balance between estimates of own and opponent's capabilities and intentions. These interrelationships are shown in Handel's diagram (Figure 4.2.). Erroneous estimates of enemy intentions can arise from the six sources shown at the bottom of this figure. First, one's own intentions influence the perception of enemy intentions. Handel advances a projection rule: when a country's intentions are peaceful it is reluctant to

Figure 4.2 The Mutual Balance of Capabilities and Intentions: The Basis of Evaluation of the Probability of the Outbreak of War by Each Side.



1. One's intentions are determined; in the short range, however, they are limited by available means.
2. Capabilities are developed according to intentions, and can be expanded in the longer run.
3. One's capabilities are also built and designed to match or surpass the enemy's capabilities and are continuously evaluated against the enemy's capabilities: balance of deterrence.
4. The enemy's intentions and willingness to undertake risks are evaluated. The enemy's intentions also influence one's own.
5. One's own capabilities are adapted to enemy's intentions.
6. One's intentions are constrained by enemy's capabilities. Enemy's capabilities must be taken into account before his intentions.

*From Handel

perceive hostility in an adversary; when its intentions are aggressive it tends to attribute similar intentions to other nations. Such projection, as well as satisfaction with the status quo, can lead to wishful thinking about the enemy's plans. Second, misunderstanding of the opponent's character and willingness to take risks, what Handel terms a "cultural noise barrier," prevents the intelligence analyst from accurately picturing how the opponent approaches this balance between capabilities and intentions. The opponent's logic and reasoning may be internally consistent and rational but may appear irrational to the outside observer who does not understand the assumptions behind the opponent's reasoning. Consequently, what seems to the analyst to be an impossibly risky option may be for the opponent the least costly remaining course of action. Similarly, projecting one's own military doctrine onto the opponent introduces further cultural noise.

Third, the enemy's intentions depend on his understanding of our own intentions. However, it is often difficult for the intelligence agency to determine what the opponent knows or believes about the capabilities of the agency's country. Ignorance about what the enemy knows about us makes it easier to misestimate the enemy's willingness to take risks. Fourth, information of the enemy's capabilities is used to assess enemy intentions. However, the enemy can hide his capabilities (security) or provide misleading data about them (deception). Fifth, there is a tendency for decisionmakers to overestimate the capabilities of their own country while underestimating the capabilities of the opponent, particularly when the opponent appears threatening. There is also the tendency to overestimate the enemy's capability (e.g., bomber gaps,

missile gaps) especially when decisionmakers are attempting to convince their countrymen that the collective image of the opponent is not yet threatening enough.

Sixth, if the intelligence agency correctly anticipates the hostile action of the opponent and issues a warning which is translated into defensive mobilization, the opponent may cancel his aggressive plans. This has the effect of making the correct estimate of the intelligence agency appear to be a false alarm. It is possible for the opponent to play a war of nerves with the intelligence agency in hopes of prompting enough such alarms to dull the decisionmakers to subsequent warnings ("alert fatigue"). Consequently, both the intelligence agency and the policymakers adopt increasingly stringent criteria for what they will now consider to be positive indicators of an impending attack as opposed to further false alarms. This sequence produces Handel's fifth paradoxical proposition (Table 4.5.) and also Shlaim's (1976) assertion that intelligence success may be indistinguishable from intelligence failure. Ultimately, reports of enemy capabilities and actions which might otherwise suggest an impending attack are discounted as part of the enemy's posturing.

This last element and Handel's fourth proposition (Table 4.5.) imply that organizational factors play important roles in the perceptual aspects of intelligence failures. That is, his fifth proposition implies that while some elements of the intelligence agency will respond to warning signals, higher elements may become habituated to such signals, because of a war of nerves or just a high overall level of tensions, and thus stop paying attention to new data. The fourth proposition implies that the success of one agency (or element within an agency) leads to

reduced influence for other agencies. The resulting monopoly on credibility prevents organizational checks and balances from influencing intelligence estimates, with the result that one organizational outlook predominates.

The Cognitive Limitations Theme

The last theme in the perceptual school stresses the general limitations of human cognitive dynamics for processing information of complex foreign relations or for estimating future events such as enemy intentions. This theme goes beyond the emphasis on perceptual filters to examine the process of reasoning involved in all decisionmaking.

Steinbruner

John Steinbruner (1974) has proposed a theory of decisionmaking which depicts a nonrational, essentially cybernetic process governed by the feedback of nonidiosyncratic cognitive processes. Universal cognitive tendencies, established by experimental psychology, are viewed by Steinbruner as determining how information is considered and analyzed. These cognitive dynamics enable the individual to resolve uncertainty by means of a tendency to generalize from the particular situation. This generalization proceeds however, without confronting, and indeed often in order to avoid confronting, the complexities of the situation. Strong beliefs substitute for evidence from empirical observation in initiating and sustaining certain beliefs produced by the cybernetic cognitive dynamic process. These strong beliefs enable the individual to successfully ward off conflicting information which might otherwise seem overwhelming. As Steinbruner writes:

> . . . cognitive theory readily accounts for the existence of firm, categorical, nonprobabilistic beliefs in the presence of intense uncertainty.

> The cognitive processing mechanisms of the mind provide a number of ways in which beliefs become established, independent of the weight of objective evidence . . . As a general matter, cognitive theory makes the assumption that structure will be imposed on certain situations, and uncertainty thereby resolved, not by probabilistic judgments but by categorical inferences. (p. 110)

Before examining Steinbruner's model as it might apply to failures in the process of estimating enemy intentions, it is useful to note how this conceptualization of the individual relates to the rest of the perceptual school descriptions. The earliest of these formulations of intelligence failures point to specific beliefs and perceptions which tend to distort information on particular foreign policy issues. Later writers broadened this concept into the notion of perceptual filtering, the tendency to distort information because of pre-existing expectations and hypotheses. This theme was in turn refined to the signal-noise theme which highlights the dilemmas of accepting and rejecting information. Steinbruner attempts to incorporate these successive refinements by returning to the first theme: i.e., the signal-noise problem and perceptual filtering are manifestations of the general and universal cognitive feedback processes which create and maintain specific erroneous beliefs and perceptions.

Basic cognitive processes. Steinbruner describes five basic principles from experimental psychology regarding the process of mental operations. These principles describe mental mechanisms which are probably universal and which influence all information processing to some degree. However, because they largely operate both prior to and independent of conscious mental operations, they are not obvious or intuitive and may not be apparent even with intense introspection. The explication of these basic processes is due to empirical experimentation.

The first principle Steinbruner terms "inferential memory" to highlight the fact that memory is a reconstructive process rather than a matter of simple recall. Memories must be reconstructed or recollected by reutilizing the rather complex decompositions of images that are involved in the process on remembering. This storage process entails abstracting meaningful and organized concepts from perceptions and organizing these concepts both hierarchically and laterally. However, because this storage process entails simplification and restructuring of the original material, differential memory decay can occur; most commonly the overall concepts are remembered easily while specific components and details are recalled only with great difficulty, if at all. Memory is thus largely a process of inferring what was actually present in the original perceptions from the recollection of the general concepts that the mind uses to store these perceptions. That is, at the most simplistic level, the mind is forced to reason from the general to the specific.

The memory process requires a core of stable, underlying beliefs and concepts which serves to organize perceptions and give them structure and meaning so they can be effectively summarized for storage. The second principle Steinbruner describes is "consistency," the tendency of the mind to operate in such a way as to keep these internal beliefs and concepts consistent with each other. This strain toward consistency affects both perception and memory. However, psychological consistency differs significantly from logical consistency. The most easily recognized evidence of this occurs whenever an individual perceives what are commonly termed "optical illusions." Such illusions occur because they suggest

(falsely) the presence of some basic structure of the physical world. This suggestion influences how we see the illusion but in fact the basic structure which we assume to be present is absent, hence our senses are fooled by our falsified assumption. Because the basic structures we assume to be present are so widespread and common in our perceptual world it is difficult or impossible to not assume their presence in the optical illusion. So, to maintain a consistency of perception, we mentally extend these assumptions to all cases with the result that we are misled when confronted by unusual perceptual events such as illusions. Optical illusions reflect inference mechanisms which are tied by the consistency principle to common perceptual phenomena. Mental structures more complex than perceptions are also influenced by the consistency principle. Receipt of information inconsistent with existing attitudes and beliefs will generate mental efforts to reduce the inconsistency.

The operations of the mind are constrained to some extent by reality in important ways, which has been termed the "reality principle," and the mind does react to stimuli that are clearly and unambiguously presented in ways that any other mind would respond.

The fourth and fifth principles noted by Steinbruner deal with mental economy, the tendency of mental processes towards simplicity and stability. Cognitive mechanisms operate to keep the structure of beliefs and concepts as simple as possible. Consequently the mind is highly selective about the information it perceives and attends. Furthermore, information that is attended is regularized, filled in, completed, organized and otherwise manipulated to fit the simplest concepts needed to cope with a situation. In addition, the mind attempts to keep the core of basic beliefs and concepts stable and resists change in this core

structure. There is consequently a strong bias against any information that would tend to change the components of the major belief systems.[10]

Decisions under uncertainty. Steinbruner examines the implications of these principles for decisionmaking under conditions of complexity, as for example when incommensurate values are involved. While in many cases problems of this type are resolved analytically, that is, by constructing and weighing trade-offs and computing expected values of various possible outcomes; in cases of great uncertainty such value integration tends not to occur. In these latter cases cognitive mechanisms tend to eliminate trade-offs from the belief system as violations of the consistency principle thus preventing the analysis of expected values. Because uncertainty implies that the reality principle will be relatively weak due to ambiguities as to what is reality, the principles of stability and simplicity allow the mind to reject or deny information that challenges existing beliefs or otherwise introduces inconsistency. Steinbruner reasons that under these circumstances decisionmakers will deny the trade-off relationship and will assume they are pursuing the separate values simultaneously and independently, or may even see the incommensurate values as mutually supporting.

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[10] Biologist Robert L. Sinsheimer makes a similar point in his splendid article, "The brain of Pooh: an essay on the limits of mind:"

> Our brains too are really very limited compared to the complexity about us . . . the construction of our brains places very real limitations upon the concepts that we can formulate. Our brain . . . simply lacks the conceptual framework with which to encompass totally unfamiliar phenomena and processes.

It is not difficult at this point to extend Steinbruner's general analysis of decisionmaking to the specific case of intention estimation. Clearly, in an uncertain environment the intelligence specialist or policymaker could be confronted by a conflict between values. For example, the policymaker might believe that the opponent state would be unwilling to risk war, but he might also be confronted by reports that the opponent is preparing its military forces and is making threats. The latter information can be made consistent with the former beliefs by denying the reliability of the source of the information, or by believing the information but asserting that these moves and statements are in aid of defensive rather than offensive purposes. The policymaker may go so far as to make no reaction at all in the belief that the opponent's goals are peaceful and any action would be misunderstood as aggressive. If the opponent in fact is planning an offensive, the policymaker becomes the victim of a "surprise" attack because of the tendency to maintain consistent beliefs in conditions of uncertainty.

A cognitive organizational model. Like other theorists of the perceptual school Steinbruner goes beyond the individual and examines organizational factors which might contribute to failures in decision-making. However, Steinbruner also relates these organization problems directly to his cognitive model, making them an element of his framework rather than an addendum. He argues that because the cognitive dynamics which form the heart of his model seem to be nonideosyncratic they should create stable, recurring patterns of behavior in organizations. He outlines three modes of organizational thinking which derive from the mental processes described above.

Grooved thinking. The cybernetic feedback character of the Steinbruner model points to the mechanism of organizational routine: the tendency to systematically attend a small number of variables and apply consistent decision criteria to them. Steinbruner terms this "grooved thinking" to underscore its narrowed perspective and inflexibility. This type of thinking occurs in organizations which have well-established operating procedures and wide experience with a certain range of tasks. Repeated encounters with related problems and responsibilities for producing some solution reinforce the use of highly stable patterns of reaction based on well-determined decision rules and very narrow empirical grounds. The chief cognitive characteristic of grooved thinking is the stability which results from long experience with its routines. Steinbruner writes (p. 127):

> This experience, recorded and weighted in memory, offers powerful analogues for new decisions and protection against the variance of new decision problems. The burdens of responsibility - uncertainty, political pressure, heavy workloads, potential controversy over the outcome - are handled unusually well by this sort of thought process, precisely because of the ready-made, well-anchored structure to which new problems can be fitted.

This type of thinking is strongly influenced by the reality principle because the decisionmaker is forced to focus on the most obvious, immediate, and observable aspects of the situation. Further, the process operates with a short time horizon and a very low level of abstraction. Short-range components are separated from complex problems and independently dealt with by separate agencies. The results are then filtered independently to higher echelons. The simplicity principle leads each agency to conceptualize its piece of the problem in very simple and stereotyped terms.

Uncommitted thinking. The second syndrome occurs at the top of organizations where different lines of communication, information, and responsibility converge on a decisionmaker. The incumbents of these positions cannot rely on grooved thinking because they lack the expertise with the routines and procedures of such methods, and they must contend with a broader range of problems with greater scope than can be handled by stereotyped decisionmaking. Top-level decisionmakers receive inputs from agencies which compete and contend with each other, preventing a routine response. Steinbruner also notes that many such top decisionmakers have very little experience with the organizations they lead or familiarity with its problems and they "quite literally do not know what to think" (p. 129). These forces result in what Steinbruner terms "uncommitted thinking"; the tendency for top decisionmakers to adopt generalized concepts embedded in larger, theoretical belief structures which are associated with particular sponsors. But further, the decisionmaker tends to adopt different belief patterns for the same problem in sequence. Because of the uncertainty surrounding the longer-range settings of the decision he makes, the top-level decisionmaker has difficulty protecting and reinforcing his beliefs from competing ideas, especially since the main source of support for these beliefs, the organizational sponsors, tend to differ and disagree. The overall pattern of uncommitted thinking is: "an oscillation between competing belief patterns" (p. 130), with the decisionmaker siding first with one group and then with another on a given issue.

Theoretical thinking. The third syndrome Steinbruner labels "theoretical thinking" to characterize the elaborate, extensive, consistent and stable belief pattern that the decisionmaker develops over time

and to which he is greatly committed. In resolving particular problems the theoretical thinker relates concrete alternatives to general propositions about the environment, and very general values are linked to highly specific objectives. The theoretical thought of this type of decisionmaker insulates him from the reality principle by providing a long-range framework and extensively anchored beliefs with which he can fend off inconsistent information. Furthermore, the theoretical thinker is relatively unaffected by uncertainty and is able to react quickly with confidence in fluid situations when others seem to be at sea. Over the longer time frame theoretical thought tends to flounder as contradictory evidence accumulates which cannot be refuted or denied. Theoretical thought requires time and a supportive environment to flourish, hence it is found, according to Steinbruner, in organizational contexts which foster small, close knit groups which interact with each other about issues of common concern on a regular basis.

The Organizational Model of Intelligence Failure

> intelligence failures are built into complex organizations.
>
> Harold L. Wilensky, 1967

> Then came all the king's wise men: but they could not read the writing, nor make known to the king the interpretation thereof.
>
> Daniel. 5:8

The basic unit of analysis of this model is the intelligence or policy agency. The basic assumption is that the underlying causes of intelligence failures are the consequences of organizational factors

and the components of failure are due to the behaviors, structures, and functions of bureaucratic agencies.

Perspectives. Writers focusing on the organizational aspects of intelligence failure range from Betts (1978) and Steinbruner (1974), whose perspective is largely psychological to those with a sociological outlook (Janis, 1972; Janis and Mann, 1977; Ben-zvi, 1976, 1977, 1978) and to those whose outlook is basically structural (Wilensky, 1967; Allison, 1971; Downs, 1967). The psychological perspective, introduced in the above paragraphs in our review of Steinbruner's cognitive theory, stresses perceptual and conceptual behavior in the organizational context. The basic assumption is that cognitive and psychological processes which affect information processing by individuals can also influence information flows and decisionmaking by intelligence organizations. The sociological approach considers the social relations and forces among the individuals in groups as important influences on intention estimation. For example, Janis (1972) examines the effects of conformity pressures on decision-making in groups. The emphasis in the structural approach is on the impact of bureaucratization, hierarchical organization, specialization, centralization, routine, communication patterns and so forth on the intelligence process. The basic assumption here is that the characteristics of individuals or social relationships are less important than the rules which describe organizational relationships.

Betts

Richard Betts' (1978) recent and comprehensive article attempts to answer the question raised in its subtitle: why intelligence failures are inevitable. His article makes it clear Betts does not see intelligence failure as organizational; he writes that failure is "political and psychological more often than organizational" (p. 61). However, it is argued here that the basic causes of and contributing factors to intelligence failure which Betts describes are the psychological and political relationships of organizations rather than of individuals. Because we are in the embarrassing position of questioning Betts' perception of his own conclusions, we examine Betts' arguments and language in some detail.

Betts begins by stressing the role of the appreciation of intelligence rather than its acquisition or analysis. He asserts that it is the policy-makers who are to blame for most intelligence failure, occasionally the analysts of intelligence, and only rarely the collectors of information. This leads him to the conclusion quoted above that failure is psychological and political more often than organizational. However, in the sentence preceding this conclusion Betts writes (p. 71):

> Policy premises constrict perception, and administrative workloads constrain reflection.

Even at this early point Betts is highlighting the constriction of perception (which is indeed largely psychological and political) but also the constraints of administration (which is organizational). Furthermore, because

intelligence and policymaking are kept separate administratively (and when they are not, as in the reign of Secretary of State/National Security Advisor Kissinger, there are intense pressures to reseparate them) the process of appreciation, which Betts emphasizes, entails the relationships between intelligence agencies and policymaking agencies.

Betts goes on to discuss three concepts of the intelligence failure problem, the first two of which are relatively unimportant. The first (discussed earlier in section 3) notes that it is difficult to determine the ratio of failures to successes in intelligence and thus it is impossible to know if perhaps the failures that can be noted are not offset by many more successes that cannot be known. Betts rightly notes that even one failure can be catastrophic and thus this concept is perhaps reassuring but nonetheless largely irrelevant to the question of the causes of failure. The second concept is explicitly organizational; that intelligence breakdowns are due to communication pathologies. Betts notes that this concept leads to recommendations that future blunders be averted by reorganization and changes in operating procedures. While this concept is organizational, we include it in the structural category (briefly described above and discussed more fully below).

Basic causes. The most crucial concept of the intelligence failure problem according to Betts, and the concept which we interpret to be organizational (albeit with a psychological perspective) is what Betts

terms the "paradoxes of perception" (p. 63). Despite this label, he writes:

> the roots of failure lie in unresolvable trade-offs and dilemmas. Curing some pathologies with organizational reforms often creates new pathologies or resurrects old ones.

Betts then lists a number of conflicting organizational aspects of intelligence production and consumption; of warning and reaction to warning; of analysis and decisionmaking; as well as one psychological aspect: the need for strategic preconceptions by leaders. Since individuals generally do not play these conflicting roles of producer/consumer, warner/warned, analyst/decisionmaker, Betts has noted paradoxes in the organizational process of intelligence and its use. Rephrasing the problem Betts has underlined may make clearer why it is actually an organizational problem. Intelligence performs basically the functions of perception and conceptualization for the state. The product of these functions serves (with other products generated elsewhere) as inputs to the decision and action functions of the state. Failures arise because the agencies performing these functions of intelligence and decision are organizationally separate, and more importantly, their functions are to some limited but inescapable degree incompatible with each other psychologically and politically but also organizationally. Betts stresses related points. He writes that "it is usually impossible to disentangle intelligence failure from policy failures" (p. 66) (which,

again implicates several actors and agencies) and argues that intelligence and policy functions are inseparable, interactive rather than sequential, processes. The paradoxical and unresolvable trade-offs and dilemmas of which Betts writes concern the relations among agencies and within agencies more than the psychological processes of individuals. Betts' main point is not that failures would cease if the intelligence and policy functions were done by a single agency. Rather, it is that the same types of problems that face the individual attempting to perform both functions also affect the intelligence-policy system. The demands of the separate functions are incompatible. In this sense he is correct that the basic problem is psychological. Our point is that Betts applies this basic point to the organizational level.[11]

Factors of failure. Turning to the component elements of intelligence failure Betts notes four factors, the first of which is

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[11] The refusal of Betts to associate his analysis with "organizational" factors in intelligence failures is not difficult to explain. Much of his article is devoted to examining why the reforms which have been enacted in the past as a result of previous intelligence failures have had so little discernible effect. Most of these reforms are "organizational" in the sense of improved flows of communications, larger staffs, new watch and warning mechanisms, changes in responsibility and procedure, and so forth. He argues that such reforms tend to have only minor impact because they do not circumvent the "basic barriers." With none of this do we disagree with Betts; we only disagree that these basic barriers operate at just the psychological level and are not also organizational in a fundamental sense.

explicitly organizational. Drawing on the work of Wilensky (1967, reviewed below) Betts notes that the structural characteristics of hierarchy, centralization, and specialization; the structure of authority; and the allocation of time and resources all may hinder the intelligence function. The remaining three factors Betts characterizes as "more fundamental and less remediable intellectual sources of error" (p. 69). As we review these factors we attempt to point out that Betts has focused on organizational applications of factors which also affect the individual actor.

Ambiguity. The incoherent environment of foreign affairs creates the obstacle of ambiguity to accurate analysis. Both the lack of information and an excess of information compound ambiguity: the lack of data by increasing uncertainty, the excess by creating problems of information overloads, signal-noise, and signal-deception detection problems. The consequence of the ambiguity obstacle Betts describes in terms of the relations between the analysts and the policymakers:

> To the degree they reduce uncertainty by extrapolating from evidence riddled with ambiguities, analysts risk oversimplifying reality and desensitizing the consumers of intelligence to the dangers that lurk within the ambiguities; to the degree they do not resolve ambiguities, analysts risk being dismissed by annoyed consumers who see them as not having done their job. (p. 69)

While the individual "intellectual" factor of ambiguity precipitates these problems, it is the organizational relationship between the analyst and

the consumer that produces the actual intelligence failure. As Betts notes "because it is the job of decisionmakers to decide, they cannot react to ambiguity by deferring judgment" (p. 70) yet deferring judgment in the face of ambiguity may be the best (or the only) course for the analyst.

Betts points to other aspects of ambiguity that lead to failure which can also be seen as at least partially organizational: confused environment; overload of conflicting data; lack of time for rigorous assessments. He concludes that "intelligence can fail because the data are too permissive for policy judgments rather than too constraining ... (A)mbiguity is exploited by wishfulness." This process can operate in two ways. The individual analyst, faced with ambiguous information, may reach conclusions which are largely wishful. Alternately, the policymaker, faced with seemingly unambiguous but highly conflicting intelligence appraisals from various (and perhaps competing) intelligence agencies, may select those appraisals which most closely conform to his preconceptions (similar to Steinbruner's "uncommitted thinker"). The first process has been discussed above under the individual model and is almost entirely psychological, whereas the second process is both psychological (in that the agencies reach differing conclusions from similar data and the policymaker chooses among them on the basis of his perceptions) but it is also organizational (in that the agencies

emphasized different interpretations and are competing for the attention and belief of the policymaker). Since Betts offers examples of the impact of ambiguity which are the second type and not the first, he is implicating organizational as well as psychological factors in failures.[12]

Ambivalence. The intelligence agency often must choose to report ambivalent judgments to the policymaker. These may lead to failure because, again, the policymaker may fashion out of them the conclusions he wished to find. Furthermore, the hedged judgment, while honest, may fail to "shock consumers out of wishfulness and cognitive insensitivity" (p. 71). As with ambiguity, Betts characterizes this problem in terms of the relations between intelligence and policymakers and sees the dilemma arising out of the differing goals of these separate organizations.

Reforms. The final factor which Betts stresses is the atrophy of reforms, the fact that lessons learned from previous mistakes and failures are decreasingly applied or taken seriously as time passes. He writes: "If the reforms ... do not fulfill day-to-day organizational needs--or if, as often happens, they complicate operations and strain the organization's resources--they fall into disuse or become token practices" (p. 71-2). Betts has obviously returned his attention to the structural organizational characteristics of intelligence with which he began his examination of the components of intelligence failures.

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[12] The competition among bureaus and agencies is a characteristic of Allison's (1971) bureaucratic politics model and the role of ambiguity as described by Betts seems to be both organizational and bureaucratically political.

Of these three barriers to analytic accuracy, Betts concludes: "they are inherent in the nature of intelligence and the dynamics of work" (p. 72). While the nature of intelligence certainly includes psychological aspects (as discussed above under the individual model), the dynamics of work just certainly include organizational aspects. The factors Betts identifies and the examples he uses can be understood as resulting from an interplay of both psychological and organizational processes that is perhaps best described by Betts:

> The interaction of analytic uncertainty and decisional prudence is a vicious circle that makes the segregation of empirical intelligence and normative policy an unattainable Platonic ideal (p. 88).

Having judged the basic causes of intelligence failure to be "unresolvable trade-offs and dilemmas" between the analytic and the policy functions, Betts offers no panaceas for improving the intelligence process. His strongest recommendation is that "intelligence professionals anticipate the cognitive barriers to decision makers' utilization of their products" (p. 84). But Betts is fundamentally pessimistic about the prospects for preventing strategic surprise: like Shlaim and Wohlstetter he believes there is "no guaranteed prophylaxis against intelligence failures" (p. 84) and intelligence failure is inevitable and natural (p. 88). He entitles the final section of his article "Living with Fatalism."

Two further points made by Betts are worth noting: like Wohlstetter and Jervis, Betts compares intelligence failures to scholarly errors. This concept is further explored in Section Six below. Betts

also relies heavily on the image of intelligence failures as disasters, beginning and ending his article on this theme. The relation between intelligence failures and disasters is discussed below in Section Five.

Janis and Mann

Janis and Mann (1977) have proposed a conflict theory which postulates that errors can result from the motivational consequences of decisional conflict imposed on decision-makers by the need to make a choice. This theory is largely psychological and their descriptions clearly indicate that Janis and Mann view the central processes in their model as affecting primarily the individual. However, the two extended illustrations of high-level policy failures they provide (a school desegregation plan and the Pearl Harbor disaster) are both failures of decision-making groups. Since the clearest application of their theory to the issue of intelligence failure is described at the group rather than the individual level, and because several social features of policy-making groups play important (but not central) roles in their description of intelligence failure, Janis and Mann's conflict model is included as an organizational rather than an individual approach to explaining intelligence failure.

While Janis and Mann assume that the stress induced by the need to decide is a major cause of errors, they acknowledge other factors: information overload, limitations of human information processing, group pressures, prejudice, ignorance, organizational constraints, and bureaucratic politics. They postulate five basic patterns of coping with decisional conflict each of which is associated with a specific set of antecedent conditions and a characteristic level of stress. These patterns and the related stress symptoms are shown in Table 4.6. The

Table 4.6. Manifestations of conflict and related symptoms of stress for each of the five basic patterns of decision making.

Pattern of Coping with Challenge	Subjective Beliefs (indicators of mediating psychological conditions specified in figure 3–2)	Level of Stress	Degree of Vacillation of Preference for Alternative Courses of Action
1. Unconflicted adherence	· No serious risk from current course of action	*Low*: persistently calm	No vacillation
2. Unconflicted change	· Serious risk from current course of action · No serious risk from new course of action	*Low*: persistently calm	No vacillation
3. Defensive avoidance	· Serious risk from current course of action · Serious risk from new course of action · No better solution can be found	*Variable* from low to high (predominantly pseudo-calm, with breakthrough of high emotional arousal when signs of threat become salient)	Little or no vacillation (except when signs of threat are salient)
4. Hypervigilance	· Serious risk from current course of action · Serious risk from new course of action · A better solution might be found · Insufficient time to search for and evaluate a better solution	*High*: persistently strong anxiety	Very high rate of vacillation, but occasionally practically none as a result of perseveration
5. Vigilance	· Serious risk from current course of action · Serious risk from new course of action · A better solution might be found · Sufficient time to search for and evaluate a better solution	*Moderate*: variations within intermediate range, with level depending upon exposure to threat cues or reassuring communications	Moderate to high rate of vacillation (depending on content of new information)

*From Janis and Mann

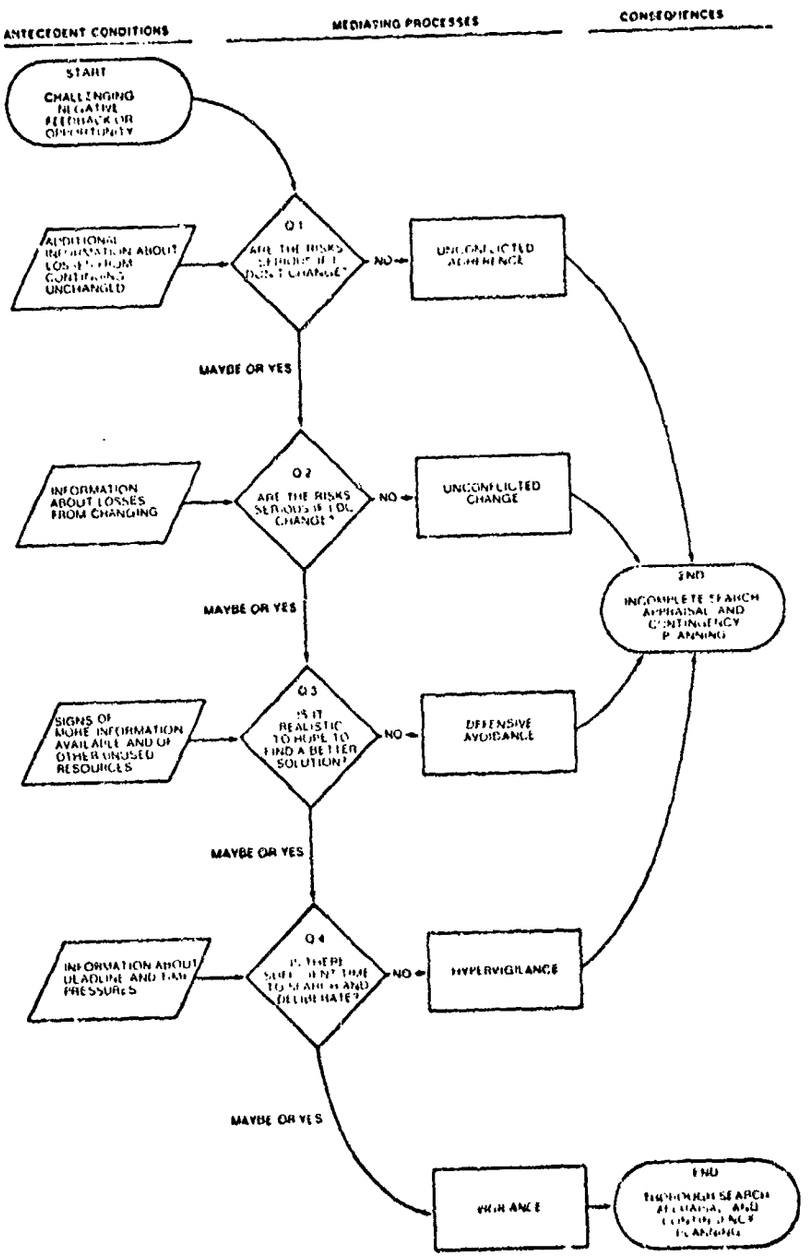
antecedent conditions, mediating processes and consequences are shown in Figure 4.3.

The conflict model involves a sequence of four questions confronting the decisionmakers regarding the procedure for reaching a choice. The first issue (Q. 1 in Figure 4.3) involves the risks of adhering to the existing policy; if these seem to be low the decision is simply to maintain the status quo. Janis and Mann label this "unconflicted adherence" and note that this choice may be well-founded or the assessment of low risk and the decision to continue the present policy may constitute complacent ignorance of the risks of continuing present policy. (Table 4.7 compares each of the five basic patterns with seven criteria for unbiased and thorough decision making.)

The second issue arises when the decisionmaker rejects current policy as high risk and then assesses the risks of changing policy (Q. 2 in Figure 4.3). In what is labeled "unconflicted change" the decisionmaker uncritically adopts whichever new course is salient or most strongly represented, and perceives the risks of this choice as low. Clearly, in terms of intelligence failure, the psychological impact of strategic surprise following either unconflicted adherence or unconflicted change should be great inasmuch as these decision patterns entail low stress levels, low assessments of risk, and no vacillation prior to the surprise (see Table 4.6).

The third issue faces the decisionmaker who sees high risks in current policies and in new policies, namely whether it is realistic to hope for a better solution (Q. 3 in Figure 4.3). If the decisionmaker concludes that there really is no hope for a better policy, what Janis and

Figure 4.3. A conflict-theory model of decision making applicable to all consequential decisions.



From Janis and Mann

Mann label "defensive avoidance" results in the decisionmaker evading the conflict by procrastinating, shifting responsibility to someone else, or wishfully rationalizing a selected policy by bolstering, i.e., by exaggerating its favorable consequences, overestimating its prospects of success, and underestimating or denying its negative consequences and its prospects for failure. All three aspects of defensive avoidance may occur prior to the decision.

When intelligence failure results from defensive avoidance surprise may or may not result. If the decisionmaker is able to shift responsibility for the decision to others he may not be surprised by the consequences of intelligence failure. On the other hand, when bolstering occurs, the decisionmaker may convince himself he has dealt with the challenge and may be totally surprised by failure. In the case of procrastination the decisionmaker is very likely to be surprised inasmuch as the rationalization that there is no urgency implies that the decisionmaker perceives no impending threats which would demand a decision and action.

The fourth issue confronts the decisionmaker who does hope for a better solution (Q. 4 in Figure 4.3). If deadlines and time pressures preclude sufficient search and deliberation then the decisionmaker resorts to "hypervigilance;" a frantic, panicky, impulsive grasping for whatever solution can be hastily contrived. On the other hand, if the decisionmaker reaches this stage and determines that there is adequate time, "vigilance" produces a painstaking search for appropriate information, unbiased weighing of alternatives and consequences resulting in an adequate decision.

Table 4.7. Predecisional behavior characteristic of the five basic patterns of decision making.

Pattern of Coping with Challenge	*Criteria for High-quality Decision Making*							
	(1)	(2)	(3)		(4)	(5)	(6)	(7)
			Careful evaluation of consequences					
	Thorough canvassing of alternatives	Thorough canvassing of objectives	*a. of current policy*	*b. of alternative new policies*	Thorough search for information	Unbiased assimilation of new information	Careful re-evaluation of consequences	Thorough planning for implementation and contingencies
Unconflicted adherence	–	–	–	–	–	+	–	–
Unconflicted change	–	–	+	–	–	+	–	–
Defensive avoidance	–	–	–	–	–	–	–	–
Hypervigilance	–	–	±	±	±	–	–	–
Vigilance	+	+	+	+	+	+	+	+

KEY + = The decision maker meets the criterion to the best of his ability.
– = The decision maker fails to meet the criterion.
± = The decision maker's performance fluctuates, sometimes meeting the criterion to the best of his ability and sometimes not.

All evaluative terms such as *thorough* and *unbiased* are to be understood as intrapersonal comparative assessments, relative to the person's highest possible level of cognitive performance.

[a] From Janis and Mann

Intelligence failure following hypervigilance is unlikely to cause the decisionmaker great surprise, in effect the surprise occurs with the realization that time has run out. As Table 4.6 suggests, hypervigilant behavior is accompanied by high stress, strong anxiety, and uncertainty as to whether the decisions and actions taken will succeed. In this case the decisionmaker's intelligence function can fail and he may be unprepared but his hypervigilant behavior has prepared him for this possible failure and its occurrence comes as no surprise.

Janis and Mann use the Pearl Harbor attack to illustrate the defensive avoidance pattern of coping with decisional conflict. They label this analysis "Admiral Kimmel's Failure at Pearl Harbor," but they stress that (1977, p. 120):

> All too often an advisory group spontaneously takes on the role of a protective shield, reinforcing the leader's efforts to protect himself from having to face unpalatable facts and agonizing choices. The Pearl Harbor fiasco is an example of how leader and group mutually bolster each others' misjudgments, thereby protecting one another from the discomforts of a painful decision.

Janis and Mann describe the following pattern prior to December 7: Kimmel (Commander in Chief of U.S. Forces in the Pacific and the top-ranking officer in Hawaii) was considerably worried about the adequacy of his plans and preparations in the face of increasingly ominous signs of war with Japan. However, he was reassured by his staff that the threats and warning being noted were not aimed specifically at Hawaii, and that precautionary plans were adequate and were being carried out. Consequently, Kimmel and his staff were able to bolster their course of action, denied or reduced the import of warnings and

indicators of hostilities, and achieved a state of "pseudo calm" at the expense of a thorough search and appraisal of their situation.

Janis and Mann note (p. 122) that Kimmel failed to check with Washington to determine whether the 27 November "war warning" message implied a possible attack on Hawaii. Assuming such an attack was not possible, Kimmel nevertheless assumed (erroneously, and neglected to verify) that the Army had responded to the war warning by placing its air defenses on full alert. (In fact, the Army response to the war warning dealt primarily with sabotage, and Army radar and anti-aircraft forces were not informed of the war warnings.) Janis and Mann portray Kimmel as neglecting to take inexpensive precautions (such as checking to see that the Army air defense was ready) because he was unwilling to alert all forces, a costly step which would have squandered his manpower and material resources if no attack came. Because Kimmel and his staff decided the warning did not apply to Hawaii, lower echelon forces, which might have provided clues that an attack was imminent had they been alerted, failed to do so. For example, the radar signals noted early on 7 December of the Japanese planes on their way to Oahu were ignored because the duty officer knew nothing of the war warnings and was expecting a flight of U.S. bombers at about that time. Similarly, two minesweeper commanders, who spotted an unidentified, presumably Japanese, submarine in an unauthorized area, failed to report their sighting, and the command center radio officer who overheard their discussion of the sub failed to report the incident to Kimmel's staff. (All this despite instructions from Kimmel less than two months earlier that any encounter with a Japanese sub was to be regarded as a sign of extreme danger because

it might be a screening element for a Japanese aircraft carrier force.) Finally, although warned an hour before the attack that an unidentified submarine had been sunk near the Pearl Harbor entrance, Kimmel and his staff did not treat the incident as a warning or an emergency and were awaiting confirmation of the sinking when the Japanese bombers arrived.

The cognitive defenses used by Kimmel and his staff included, according to Janis and Mann: misjudging relevant warnings, inventing new arguments to support the chosen policy, failing to explore the ominous implications of ambiguous events, forgetting information that would enable a discrepant event to be correctly evaluated, and misperceiving signs of the onset of actual danger. As a result of the general bolstering and cognitive defensive avoidance at the top, lower echelons were not alert and unprepared to notice or report the signs and indicators that might have allowed the top to take at least hypervigilant responses. Consequently, despite their high anxiety and concern, and what they thought were adequate preparations, Kimmel and his Staff were completely surprised when the Japanese blow fell upon them rather than elsewhere as they had expected.

Janis and Mann label the collective pattern of defensive avoidance which characterized Kimmel and his staff an instance of "groupthink" --

> ...defensive avoidance tendencies on the part of the leader of an organization are encouraged when he receives social support from advisors who concur with his judgments and share in developing rationalizations that bolster the least objectionable choice (p. 129).

Groupthink is likely in small, highly cohesive, usually unofficial circles of top political or military figures. Other antecedent social conditions

for the groupthink syndrome are: insulation of the group from outsiders, lack of methods or standards for information search or appraising the adequacy of conclusions, directive leadership, and high stress and low hope for finding a better solution than that favored by the leader or an influential clique.

Janis and Mann (1977; 130) describe eight symptoms of groupthink:

1. A shared illusion of invulnerability, causing excessive optimism and encouraging extreme risks.
2. Collective rationalizations and discountings of warnings which might prompt reconsiderations before a recommitment to past policy decisions.
3. Unquestioned belief in the group's inherent morality and a tendency to ignore ethical or moral consequences.
4. Stereotyped views of the enemy: too evil to permit genuine negotiation; too weak or stupid to counter whatever risky attempts are made to defeat the enemy's purposes.
5. Direct pressure on in-group members who argue strongly against the group's stereotypes, illusions, or commitments; branding dissidents disloyal.
6. Self-censorship of deviations from the group consensus, minimizing the importance of doubts and counterarguments.
7. Shared illusion of unanimity, assumptions that self-censorship is unimportant and silence implies agreement.
8. Emergence of self-appointed and group-tolerated "mindguards" -- members who protect the group from adverse information.

Thus, while the Janis and Mann conflict model can operate when decisions are made by individuals or by groups, groupthink social conformity pressures will aggravate and strengthen the tendency toward the defensive avoidance pattern when decisions are made in a group or organizational context, especially when certain antecedent group characteristics are present.

Ben-zvi

Abraham Ben-zvi, in a series of articles (1976, 1976-77, 1977, 1978), challenges the perceptual school of the individual model of intelligence failure (as exemplified by Wohlstetter, 1962; Whaley, 1973; Jervis, 1968; and George and Smoke, 1974) as being monistic and based entirely on hindsight, offering "n suggestions for predicting or preventing surprise attacks" (Ben-zvi, 1976: 383). He proposes (1976: 383-4) a multifactor theoretical framework to conceptualize the problem of surprise.

The observing state, B, perceives some information about the observed state, A, as "inherently valid" -- namely those "dimensions and characteristics which are beyond the ability of State A to control and manipulate." Using this information, State B assesses State A's possible courses of action ("assumptions of possibilities") and infers State A's actual course ("assumptions of actualities"). The assumptions of possibilities are the strategic and tactical "explicit and implicit assumptions . . . about the conditions and circumstances under which State A would strike." Assumptions of actualities are the strategic and tactical assumptions that have become "realities in the eyes of the observing state" (1976: 383-4).

Based on his analysis of several cases of strategic surprise and intelligence failure, Ben-zvi argues that "in each case . . . tactical indicators of the impending attack did exist -- but they simply did not bear out the strategic assumptions about the intent and capability behind them" (1976: 394). However, decisionmakers tend to "give priority to the strategic assumptions of possibilities over the tactical assumptions of actualities" and to attribute their own line of reasoning to the enemy so that accurate tactical indicators are ignored. Ben-zvi's main conclusion (1976: 395; 1976-77: 90-01; 1978: 43) is that tactical field information needs to be evaluated independently of a priori strategic assumptions (which inevitably reflect the top policymakers stereotypic and usually erroneous image of the enemy) and that tactical actualities should be weighted more heavily when they vary with the assumptions of strategic possibilities.

In other words, Ben-zvi sees the perceptual filter of top decisionmakers as the primary factor in the preference for strategic assumptions. These perceptions are particularly important when decisions are made at the top under high, ambiguous and uncertain conditions (Ben-zvi, 1978: 42). However, he views these misperceptions as only one factor "in a complex picture of inefficiency, inconsistency and sluggishness" (1976-77: 83) with a variety of dimensions: bureaucratic, organizational, technical, communications, and political, which combine to suppress or attenuate tactical information. In lieu of the single-factor perceptual theory, Ben-zvi (1976-77: 90) recommends a framework that encompasses "the multitude of interwoven factors, dimensions and stages that constitute the process by which nations come to grips with threatening situations." He also urges a theoretical account which deals with the

"post-perceptual" phases of strategic surprise -- the problem that takes place after the threat is recognized. One example is the needed analysis of strategic surprises as "trigger events" which produce drastic changes in perceptions (1978: 39). Thus, for Ben-zvi, State B's failure to correctly appraise State A's technical capabilities, as in the U.S. failure to appreciate the aerial torpedo capability of Japan prior to Pearl Harbor or the Israeli failure to foresee Egyptian anti-tank tactics, may contribute as much to surprise as the misperceptions of the top leadership. Ben-zvi (1976-97: 86) considers such factors as Knorr's (1964) "technical surprise," i.e., failure to detect technical or logistic capabilities, an important contributor to intelligence failure, but not the exclusive cause.[13] Table 4.8 summarizes the various factors Ben-zvi identifies in his analyses of several cases of intelligence failure and strategic surprise. However, he does not attempt to determine the reasons why these factors sometimes contribute to failure and sometimes do not, i.e., Ben-zvi's framework includes no description of process. It identifies possible vulnerabilities but fails to specify how one might determine which are, in fact, likely to fail. Clearly, Ben-zvi sees organizational pathologies (such as the U.S. Navy's prohibition on estimation in the Office of Naval Intelligence during Pearl Harbor) as a major impediment to accurate tactical intelligence.

[13] It bears remembering, however, that Knorr (p. 462) specifically defined "technical surprise" as "not incompatible with the (analyst's) prevalent set of expectations," i.e., the analyst correctly perceives the enemy's intentions but is wrong about some enemy technical capability. Ben-zvi's use of the term seems more consistent with Wohlstetter's (1962: 394) concept of technical surprise as one more "barrier to accurate perception."

Table 4.8. Strategic and Tactical Factors in Intelligence Failure (Ben-zvi)

Errors of Strategic Assumptions	Barbarrosa	Pearl Harbor	Yom Kippur War
Enemy's Modus Operandi	x		x
Enemy's Willingness to Accept Risks		x	x
Enemy's Capabilities	x	x	x
Enemy's Major Concerns			x
Tactical Problems			
Organizational Pathologies	x	x	x
Equipment Vulnerabilities	x	x	
Lack of Experience	x		
Inflexibility of Procedures	x		x
Service Rivalries		x	
Communication Vulnerabilities		x	x

Given Ben-zvi's high opinion of the ability of tactical intelligence to infer enemy intentions, it is worth noting how he thinks this is done. For example, he writes (1976: 387):

> . . . although the accumulating field information signalling imminent danger was subject to more than one interpretation, there had emerged, particularly within the lower and middle echelons of military intelligence, a coherent and alarming picture which called for vigilance. The tactical perception of danger was the outcome of a continuous process through which many pieces of information were pieced together to form a menacing whole . . . the presence of confusing "noise" did not obfuscate or eliminate the relevant warning signals . . .

Ben-zvi's opinion of "piecing together" contrasts sharply with the rather low opinion of the "jig-saw" or "mosaic" method held by other analysts of intelligence (e.g., Wasserman, 1960; Hilsman, 1956; Heuer, 1978).

Wilensky

Harold Wilensky (1967) approaches intelligence failure from an organizational structure perspective. He defines (p. ix) intelligence failure as "the inability to muster the intelligence needed for successful pursuit of organizational goals . . . the relevant information is not in the organizational system as a result of the lack of appropriate search procedures." His analysis of such failures emphasizes the "institutional threats" (p. 7) which prevent adequate warning by intelligence. In contrast to Steinbruner, Betts, and Janis and Mann, Wilensky does not view the organizational aspects of intelligence failure as a matter of individual psychology extended to a larger context.

Wilensky notes (p. 39ff) that intelligence agencies are characterized by large size, extensive specialization, centralized management, and

heterogeneity of membership and goals. This structure dictates heavy reliance on experts and highly compartmentalized efforts. These characteristics introduce sources of failure which are structural, doctrinal, or related to specific approaches to problems. He writes (p. 42):

> Intelligence failures are rooted in structural problems that cannot be fully solved; they express universal dilemmas of organizational life that can, however, be resolved in various ways at various costs. In all complex social systems, hierarchy, specialization, and centralization are major sources of distortion and blockage of intelligence.

Other factors which affect the quality of intelligence are: prevailing concepts of intelligence in the organization, the problems being solved, the organization's stage of growth, and the contexts (economic, political, cultural) of the problem. Wilensky discusses each of these factors in detail; they are summarized in Chart 4.1.

Hierarchy. While necessary for organizational control, hierarchy facilitates and reinforces concealment and misrepresentation of information. "Bad" news tends not to flow upward, especially if it reflects upon the evaluations of those who would convey it. Conversely, hierarchy permits low-level information and personnel to be ignored easily by the higher levels. Hierarchies inhibit innovation, narrow the range of communicated ideas, foster defensive cliques, and self-serving coalitions. Wilensky notes (p. 46) the operation in hierarchies of what Peter and Hull (1969) term the Peter Principle: that successful experts tend to be promoted beyond their sphere of competence, their talents are thus missed at lower levels and misapplied or irrelevant at their new, higher position.

CHART 4.1.

Roots of Intelligence Failure, Typical Effects, and Organizational Defenses

ROOTS OF FAILURE	MAIN EFFECTS ON INTELLIGENCE	ORGANIZATIONAL DEFENSES AGAINST INFORMATION PATHOLOGIES
Structural attributes that maximize distortion and blockage		
Many ranks in hierarchy, emphasis on rank in style and symbolism. A tall pyramid narrowing sharply at the top, providing long promotion ladders for a few.	Blocks upward communication. More effort to create organization men via loyalty criteria in recruitment, indoctrination, etc. Keeps experts in their "place" (subordinate, isolated). [But hierarchy eases internal control, motivates hard work.]	Team or project organization. Investigation and inspection machinery. Communicate out of channels. Rely on informed outsiders. Diversify channels. Develop general advisors at the top. Accent persuasion, manipulation in administrative style.
Great specialization and interdepartmental rivalry. A large number of organizational units involved. Specialization on geographical basis.	Parochialism—much irrelevant or misleading information. Expert too distant from policy. Agreed-on estimates conceal strong dissent, obscure issues and alternatives. [But specialization increases efficiency in knowledge production and if problem of upward communication can be solved (see hierarchy), rivalry makes top alert to diverse perspectives.]	Recruit managers from staff. Rotation. Conferences of diverse specialists (but avoid consensual judgments or agreement by exhaustion). Career lines from field to headquarters. Examine multiple sources firsthand. Encourage constructive rivalry. Create liaison groups (e.g., between research and development).
"Overcentralized" intelligence.	Top out of touch, too overloaded. [But if intelligence is scattered, the dysfunctions of hierarchy and specialization are maximized.] Expert with data too distant from policy use. After move away from decentralization, unified consensual judgment fosters intelligence fantasies, gives illusion of reliable information.	Develop interpretive skills, integrate collection and evaluation at every point where important decisions are made. Strike balance, depending on purpose.
Doctrines that maximize distortion and blockage (e.g., misleading dichotomies)		
"Facts" to "fill in gaps" vs. "evaluated facts" or "interpretation."	Collection kept subordinate and separate from interpretation. Experts excluded from policy deliberations. Pathologies of specialization and hierarchy maximized. More recruits who are raw empiricists, or conventional "backstoppers." More anti-intellectualism (resistance to new ideas, unfamiliar questions, outsiders plus exaggerated belief in practical experience).	Develop interpretive skills and staff. Set up study commissions (e.g., Royal Commissions), review boards, with men of independent mind and stature.
"Intelligence" ("information gathering" or "research") vs. "operations" (clandestine operations).	Fact-gathering attracts naïve realists with weak interpretive abilities. Secret operations attract adventurers—unreliable, hard to control.	Integrate research and operations. Accent research. Rotation. Make secret agencies accountable to competent (strong, independent) authority. Restrict clandestine action.

Chart 4.1. (continued)

"Overt" vs. "covert" intelligence. The notion that secret sources are superior. The "right to know" (vs. executive privacy in decision process and accountability for effects).	Accent on secrecy (1) necessitates loyalty-security systems (recruit cautious mediocrities) and segregation of operations from research (breakdown in communication); (2) impairs critical judgment, dulls sense of relevance, blinds executive to superior open sources; (3) creates poor conditions for intellectual work, blocks recruitment of independent-minded experts, top scholars; (4) can demoralize an organization. Even good information if gathered by secret means is treacherous or unusable; could be enemy plant or work of double agent. Debilitating, punishing publicity and crisis journalism. Blocks private expression of unpopular views.	Make full use of overt sources. Minimize loyalty-security criteria, use only for very sensitive positions, when clear danger, with due process. Avoid invasion of employee privacy. Use institutionalized adversary procedures or equivalents. Perform competitively (spying on rivals unnecessary); with efficient innovation, loss of secrets not costly. Insure media competition and diversity. Individual access to defensive publicity.
Prediction or estimate vs. analysis and orientation.	Prediction inappropriate where identity of enemy is unclear, organizational goals ambiguous or conflicting, policy alternatives poorly defined. Boss asks the impossible, expert wastes time. Demand for short, speedy journalistic estimates of future diverts experts from proper work. Failure of short-run predictions reinforces anti-intellectualism. "Cry wolf syndrome."	Train executives in uses and limits of experts in various fields. Recruit better-trained experts, who will limit claims and maintain professional autonomy. Invest more in general orienting analyses.
Types of problems and processes that maximize distortion and blockage		
Decision is not urgent, but involves heavy costs, great risks or uncertainty, and significant changes in goals and methods.	More time and motive to search for information but more weight for established policy and vested interests. Policy discussion is more formal, rank-oriented. (I.e., distortions of hierarchy, rivalry more prominent; doctrine accenting "facts to fill in gaps" more salient.) More chance for paralyzing delays? More chance for building case to confirm mistaken in-group preconceptions?	See defenses against structural and doctrinal roots of failure, above.
Problems are those of established organization with slow growth rate, "stable" environment.	Policy discussion is more formal, rank-oriented.	See above
Frequent, institutionalized succession (i.e., no succession crises).	Bias toward continuity of established policy, official prejudice. Short time perspective.	See above

Wilensky notes several defenses against the hierarchical effects on intelligence: use of ad hoc teams or task forces, communications out of channels, special advisors at the top, etc. (see Chart 4.1). However, each of these mechanisms introduces new problems at the same time that it solves hierarchical problems. For example, task forces are outside normal chains of command (thus hard to control and ambiguous in authority), informal (thus difficult to command or fit into institutional niches), with diffuse sources and channels of information (thus difficult to censor or monitor). They also tend to institutionalize themselves and thus sacrifice some or all of their advantages while shedding some of their disadvantages.

Specialization. Specialization, according to Wilensky, abets rivalries and restrictions on information flow. Subunits of the organization become "guardians" of their particular mission, skills, standards, sources, etc. Loyalty and secrecy interfere with rapid flow of intelligence to areas where it is needed. Wilensky notes (p. 48-9) that "the history of intelligence failures . . . hints that the foreign office, the military, and the intelligence agencies seldom if ever form an effective three-way communication network." However, the major problem with specialization, according to Wilensky, is parochialism -- the production of misleading or irrelevant information too remote from the needs of the intelligence consumer.

There are further problems inherent in the solutions Wilensky proposes to specialization pathologies. For example, the fostering of constructive rivalries undercuts the authority and morale of the institutionalized specialists. "Multiple advocacy" (George, 1972) has

been recommended as an institutional alternative to specialists that would produce a balanced, open debate, subjecting the assumptions of all the experts to examination and question. However, Betts (1978: 76) notes that such mechanisms are usually found in organizations (c.f., Steinbruner's (1974: 129) "uncommitted thinker") and may add ambiguity rather than reduce it while providing "an aura of empirical respectability" which allows the leader to choose policies according to predisposition. Rivalries may also lead to unresolvable differences and paralysis.

A recent effort by the U.S. Intelligence Community to structure rivalry into the estimation of Soviet objectives, the so-called Team A-Team B exercise was seen by Congress (see Senate Select Committee, 1978) as being "not desirable," since the rival B Team had a predetermined outlook on the issue. (The Select Committee urged continued use of outside expert reviews however.) Further, the character of the rivalry was only loosely monitored and the interchanges between the Teams tended to be somewhat unfair and inconclusive. Finally, despite this attempt to broaden the outlook on the issue, the range of questions addressed by the Teams and scope of their concerns remained too narrow and parochial according to the Committee to clearly illuminate Soviet objectives.

Another consequence of defending against specialization is the redundancy that may result. Overlapping responsibilities may overcommit limited resources, or misallocate them. Conversely, if rival groups ostensibly share responsibilities, each may silently and unofficially concede the field to the other, leaving unnoticed gaps in the intelligence coverage.

One particular conclusion on specialization which Wilensky reaches (p. 55) is especially interesting in view of the organization of U.S. intelligence efforts, namely that "intelligence failures are greatest if location [geographic specialization] is emphasized." Wilensky supports this conclusion by noting that geographic specialization: 1) depends on arbitrary political boundaries, while good intelligence focuses on problems and disciplines, 2) overelaborates administrative apparatus and inhibits resource transfers, 3) dilutes scarce technical staff and encourages duplication, and 4) leaves intelligence in less able hands. Since both the U.S. State Department and the U.S. Central Intelligence Agency are largely organized on regional bases, Wilensky's remarks are particularly provocative.

Centralization. Wilensky is highly critical of centralization as a panacea for intelligence failure (p. 58): "too few officials and experts with too little accurate and relevant information are too far out of touch and too overloaded to function effectively." He lists several problems with centralization: 1) data collection too remote from policy usage, 2) consensus estimates conceal strong disagreements and diverse opinions, 3) competition for scarce resources and personnel between center and peripheral agencies, 4) fosters an illusion of reliability and security and conceals fantasies at the top. Wilensky offers several examples (e.g., U.S. Forest Service) of organizations with a widely shared ethos which can operate decentralized effectively without falling prey to the problems of hierarchy and specialization. This generally is possible within organizations with a single, stable goal; organizations with more diverse goals are less able to decentralize.

Doctrines of Intelligence. The concepts held by the producers and consumers of intelligence on what intelligence is and how it should be used condition their relation and the quality of intelligence. Wilensky notes several views on intelligence already summarized above as contributing to intelligence failure, viz., Wasserman's (1960) "naive realism" and Hilsman's (1956) anti-intellectualism reflect a belief in "facts" which speak for themselves; suspicion of analysis, interpretation, or estimation; and an exaggerated belief in the value of "practical" experience. The consequences of these views are to weaken analytic elements, to attract "fact-gatherers," and to inhibit creativity. These views also lead to a simple-minded demand for predictive estimates of what the facts hold in store for the future, rather than a view of the future as contingent and dependent on a variety of forces which may change and fluctuate. The consequent failures of inappropriate efforts at prediction (or the more appropriate defensive hedging) reinforce the anti-intellectualism of consumers.

The necessary security surrounding intelligence work tends to be exaggerated and extended, according to Wilensky (p. 67), so that secret sources seem more valid and infallible than open sources:

> The more secrecy, the smaller the intelligent audience, the less systematic the distribution and indexing of research, the greater the anonymity of authorship, and the more intolerant the attitude toward deviant views and styles of life.

These problems were examined above in Section Three under "The Ultra Syndrome."

As with Wilensky's other recommendations, the defenses he offers against the pathologies of intelligence doctrines bear costs as

well as benefits. For example, he recommends an investment in "general orienting analyses" as a defense against the emphasis on predictive estimation. Overly general "surveys" tend to be subject to parochialism and unneeded encyclopedism.

Allison

Graham T. Allison, in his book Essence of Decision (1971), uses Wohlstetter's (1960) research on Pearl Harbor to illustrate the "organizational process paradigm" of governmental decision-making. Like Ben-zvi (1976-77) and Whaley (1973), Allison is willing to use Wohlstetter's historical analysis without accepting her theory of intelligence failure. Instead of explicating the Pearl Harbor disaster as due to the blinding effects on perceptions of noise and wishful thinking, as does Wohlstetter, Allison outlines the failure as resulting from the standard operating procedures and routines of government organizations.

The basic unit of analysis in Allison's model[14] is the output of governmental organizations. Historical events are composed of actions taken by organizations. Leaders must operate within the constraints posed by existing organizational routines and present physical capabilities. Organizational outputs shape the situation and the leaders' choices by raising the problem, providing the information and taking

[14] While in these paragraphs we term the organizational process paradigm "Allison's model," this is for shorthand purposes only. Allison outlines three models of governmental decision-making (as described at the introduction of this section) and makes clear that he espouses none to the exclusion of the others. Indeed, Allison's main point is that there exist several competing models of decision-making which all have some validity and explanatory power.

initial steps that color the issue; in a sense "pre-deciding" the outcome for the decisionmaker. The options open to the leader are narrowly limited by the ways various involved organizations are able to function.[15]

This model explains intelligence failures as being a consequence of the programmed character of organizational activity and the limitations imposed by pre-established routines on how organizations view problems and enact solutions. Problems are dealt with by the subunit most concerned in terms of the constraints it takes to be most important, which permits conflicts among organizational constraints to persist. The subunit evolves routines and standard operating procedures (or SOPs) to cope reliably with the problems that it defines to be its province. These SOPs are intended to be simple, unambiguous, and slow to change. They

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[15]The organizational analysis of intelligence failure would be incomplete without reference to the general literature (both serious and serio-comic) on bureaucracy and its pathologies. For example, Downs (1967) addresses the biases common to all bureaucrats (e.g., information favorable to oneself is exaggerated as it is passed upwards while unfavorable information is minimized), the behavior of organizations (e.g., the greater the effort made to control subordinates, the greater the efforts of subordinates to evade control), and communications (e.g., the quantity and detail of reporting required rises steadily over time, regardless of the amount or nature of activity monitored). Parkinson (1957) notes the tendency of work to expand "so as to fill the time available for its completion," and the tendency of incompetence and jealousy to paralyze an organizational hierarchy. The prevalence, nay universality, of incompetence in high places is argued by Peter and Hull, who claim that "in a hierarchy every employee tends to rise to his level of incompetence." Gall (1975), building on these predecessors, writes that "Any large system is going to be operating most of the time in failure mode," that "a system can fail in an infinite number of ways," and that "the mode of failure of a complex system cannot ordinarily be predicted from its structure." Consequently, "the crucial variables are discovered by accident."

tend to be created to deal with the important classes of problems the subunit faces daily and consequently they accept a limited range of impacts, permit only certain processes, and define specific outputs. The net result Allison terms a "program:" a set of rehearsed SOPs for producing specific actions. Consequently, the best explanation of an organization's future action is its present action, what its programs allow it to do now. Allison uses this model to show that Pearl Harbor resulted from behavior on December 7 that was virtually identical to previous behavior, i.e., the government programs were operating according to highly rigid and inappropriate routines.

Because SOPs are designed to handle standard (i.e., common) situations, programs (or complexes of SOPs) fit a current problem approximately, depending on how common the problem is, and programs are chosen which seem to be closest in approximation for the problem at hand. Thus, because the Army in Hawaii had institutionally solved the problem of sabotage, when warned on 27 November to expect "hostile activities" (by which was meant attack from without), the Army instituted the program (countersabotage) which it felt appropriately fit the problem. The available solutions defined the meaning of the problem. Because the competition on Army resources prior to Pearl Harbor ruled out an extensive air defense or anti-aircraft program, the problem could not be defined in those terms.

The existence of successful SOPs and programs thwarts efforts to modify the organization's behavior. Old programs resist change and incorporate new routines into themselves. Having elaborately "solved" the subversive and sabotage problem the Army was unable to develop a current estimation capability in the Far East as instructed by the Chief of Staff in May 1940. The expansion of personnel intended to supply

estimators instead led to the expansion of anti-subversion efforts and the estimate function was rudimentary on December 7.

The organizational emphasis on solving common and routine problems leads to the institutionalization and ultimate disregard of long-range planning. As far back as 1936 war games in Hawaii postulated a Japanese air attack on Pearl Harbor and an Army anti-aircraft defense against such attack. But actual operations and training proceeded independently of this planning exercise and Army units were unprepared and unequipped to carry out these Army plans.

The Navy air elements in Hawaii were assigned two wartime missions: to attack Japanese mandated islands, and carry out reconnaissance of enemy activities. Shortage of aircraft made preparation for both missions impossible so only the first was conducted seriously, avoiding hard choices among goals and sequential goal seeking. Consequently, reconnaissance was never adequate and tended to be totally absent during weekends.

The prepotency of the goal of organizational maintenance and resistance to "poaching" by other agencies led the Navy War Plans Division to resist efforts by the Office of Naval Intelligence to turn intercepted Japanese signals into estimates of intentions. Being the older and more powerful agency Plans won the day although it lacked the personnel and expertise to perform the function adequately.

The tendency of organizations to perceive and respond to problems only in terms of the routines and programs it has available means that the interaction of two organizations is largely limited to whatever programs they have created to maintain such liaison. As the

situation leading to December 7 worsened, despite Washington's warnings and assumptions of Army-Navy teamwork in Hawaii, the two services in the Islands were not exchanging information or intelligence (the Army's chief intelligence officer had none of the most informative intelligence available to Navy cryptographers, nor was he allowed to have it had he even known of its existence), both services had their own differing estimates of what the Japanese were planning, each service had its own emergency plans which were poorly linked to the plans of the other. Even in Washington the services could not agree what to tell the theater commanders, how to word that information, what situations warranted an alert, or the level of alert that would be appropriate.

The issue Allison raises, that organizational routines resist changing and focus incoming problems and information into existing programs, tends to complement Steinbruner's (1974) model of "grooved thinking" and Janis and Mann's (1977) analysis of the "defensive avoidance" by top decisionmakers and the role of "mind-guards" played by top-level advisors. That is, the reluctance of the top leadership to make their fears and concerns widely known reinforce the organizational tendency to treat a steadily worsening situation as merely routine and to respond with limited and inadequate programs to increasingly threatening events, which tends in turn to reassure the leaders that their concerns are really groundless. At both the leadership level and within the organization there is a tendency to swiftly reduce uncertainty, in the former case by cognitive defenses, in the latter case by applying approximate solutions in the form of SCPs and programs, which precludes adequate analysis of the signals which might hint at an enemy's intentions.

The Bureaucratic Politics Model of Intelligence Failure

> You can't believe everything in intelligence reports.
>
> Josef Stalin, 1941

> O true believers, take your necessary precautions against your enemies.
>
> The Koran

The central unit of analysis in this model is the bureaucratic actor, i.e., that set of individuals who represent a particular bureau or governmental agency during action on a particular decision. As elaborated by Allison (1971) and Halperin (1974) this model assumes that governmental decisions are the result of a bargaining process (or "game") between actors with different goals, courses of action, and consequences. Decisions emerge from this conflict and decision-making is ultimately a process of building consensus on key issues among competing bureaus from conflicting information.

Actors enter the decision-making process with different interests ("stakes"). Variations in bureaucratic pressures and capabilities (deadlines, budgets, channels of influence, etc.) affect the way the actors perceive the issues. The consensus building process depends on the amount of power the various actors bring to bear on the issue. The actors' positions constrain them to particular "action-channels" (regularized means of taking specific action on specific kinds of issues) which pre-select the actors, determine the points at which they can enter the game, and which agencies will play what part in the execution of the decision. The rules of the game (some unwritten, some part of the laws and regulations that govern the bureaucracy) determine influence paths (e.g., contact with

the chief executive), power resources (e.g., control over critical information), constrict the range of decisions, and sanction or prohibit various actions.

Implications. While neither Allison nor Halperin applies the bureaucratic politics model specifically to intelligence failure, both outline propositions regarding the implications of this model for decision-making. These propositions suggest several ways in which intelligence estimation of enemy intentions might be disturbed by the operation of bureaucratic politics.

Allison notes (p. 175) that the bureaucratic actor focuses on the decision (especially the decision preferred by that actor) and not on the total strategic problem, i.e., players do not focus on what the strategic analyst is focusing. The players' problems are "both narrower and broader than the strategic problem" (p. 175). The outlook of the bureaucratic "chiefs" differs from that of the "Indians," according to Allison: the chiefs' concerns are with preserving options and leeway until uncertainties are resolved or a decision is necessary. Indians attempt to get other Indians committed to their views and to gain the chief's confidence.

These varying outlooks on strategic problems will tend to foster competing estimates of enemy intentions by the different Indians involved in the game and a tendency by the chiefs to withhold acceptance of any one estimate, while attempting to build some kind of consensus behind a policy solution. Furthermore, competition for the chiefs' and Indians' time permits only limited attention to any one issue and generates a misexpectation that someone else will 'help with the problem'. Consequently elements of the intention estimation task which are left to others may

never be done. The competition for time and attention also forces indians to be more positive and confident about their estimates than is truly warranted by the internal evidence of their analysis. Chiefs are thus presented with estimates which are possibly ill-founded but highly convincing. In turn, chiefs may use the misperceptions of other chiefs of these estimates to engineer a consensus behind their chosen policy. This strategy is abetted by the level of miscommunication which the pace and noise of simultaneous bureaucratic games tends to generate. Players believe they have spoken clearly and that acceptance by others results from agreement with the arguments they have espoused, rather than from the arguments the others have heard. Conversely, players believe they understand what other players have said while they actually understand only what they have heard.

Manipulation. Halperin (1974) discusses how the manipulation of information serves the goals of bureaucratic players. Players transmit information that protects their interests and withhold information that would impugn their stands or reduce the power of their bureaus. For example, he writes (p. 145):

> Intelligence officials in the various services and agencies wish to demonstrate that they are doing a good job and that competing organizations in the intelligence field are less effective.

DeRivera and Rosenau (1960: 56) note that intelligence agencies ("indians") put a high cost on failure to provide a warning or signal while consumers of intelligence ("chiefs") put a high cost on false alarms or falsely reporting a nonexistent signal. The consequence is that chiefs receive many warnings and signals which they treat as nonserious, intelligence agency hedging. But Halperin implies intelligence agencies

(among others) deliberately manipulate information to support their stands and defend their stakes in the bureaucratic game. He portrays intelligence players as serving primarily as 'backstoppers,' providing the facts and rationale to support decisions already made. He lists eleven "maneuvers" commonly used to manipulate information (see Table 4.9.), and argues, in effect, that intelligence players use their control over information as a source of power to attain the bureaucratic goals of the intelligence services.

The Cuban Missile Crisis

> The President bears the burden of responsibility. The advisors may move on to new advice.
>
> John F. Kennedy, 1962
>
> The best test of truth is the power of the thought to get itself accepted in the competition of the market.
>
> Oliver Wendell Holmes, Jr.
>
> Practical politics consists in ignoring facts.
>
> Henry B. Adams

Allison (1971) applies the bureaucratic politics model to the Cuban missile crisis. His analysis of events leading up to the discovery on U-2 photography of offensive Soviet missiles shows how political games may lead to intelligence failure.

Whether the Cuban Missile Crisis was a U.S. intelligence failure (Wohlstetter, 1965), a double intelligence failure (Knorr, 1964), or the U.S. intelligence community's 'finest hour' (Cline, 1974-75), or none of these, can all be debated. What cannot be argued is that U.S. intelligence uncovered the Soviet missile emplacement well after they had been begun but well before these bases were operational. The U.S. Government was

Table 4.9. Information Manipulation Maneuvers (Halperin)

1. Report only those facts that support the stand you are taking.
2. Structure reporting of information so that senior participants will see what you want them to see and no other information.
3. Do not report facts which show dangers or difficulties in an operation.
4. Prepare a careful and detailed study to present facts in what appears to be an authoritative manner and to discover new facts which may bolster your position.
5. Request a study from those who will give you the desired conclusions.
6. Keep away from senior participants those who might report facts one wishes to have suppressed.
7. Expose participants informally to those who hold correct views.
8. Get other governments to report facts which you believe will be valuable.
9. Advise other participants on what to say to the senior participants.
10. Circumvent formal channels.
11. Distort facts if necessary (and if you can get away with it).

surprised by what it discovered on 15 October (the NIE of 19 September and high officials testifying to Congress and addressing the press said the Soviets were not and probably would not put offensive weapons in Cuba) but it was not taken by surprise by a Soviet fait accompli. The impacts of the various bureaucratic and political games surrounding the discovery contribute to an understanding of why the missiles came as a surprise and why they were not detected sooner, but also to an understanding of why they were discovered in time to have them removed by the minimally threatening device of a U.S. naval blockade.

The primary question was why the Kennedy administration was unready to detect the missiles sooner; that is, why early warnings (prior to 16 October) that Soviet military efforts might be offensive as well as defensive tended to find unreceptive and unresponsive audiences. The secondary question was why the regular (weekly) U-2 surveillance of Cuba failed to cover the western end of the island (where the missile bases were most obvious), that is, why the flights stopped for three weeks just as these bases were becoming observable.

Partial answers to both questions can be found in political events. Kennedy had established a clear policy for his presidency of reining in the Cold Warriors and relaxing tensions between the United States and the Soviet Union. With the 1962 Congressional elections impending Cuba was the dominant issue and his policy had come under attack, forcing Kennedy and his national security officials to take the positive stand that the policy in general was working and in particular the Soviets were placing only defensive weapons in Cuba (which was bad enough for Kennedy but which he saw realistically as both justified and reasonable given the Bay of Pigs fiasco of the previous year), but not

offensive weapons (which would demonstrate the Kennedy policy was not working and was foolish because the Soviets could not be "trusted"). Further, Kennedy had Khrushchev's assurance that the Russian would not complicate Kennedy's domestic election problems and would not put offensive missiles in Cuba. Cuba, Kennedy's bete noir ever since his ill-considered judgment on the Bay of Pigs invasion, was a sensitive issue both inside and outside his administration and he realized that advocates of a harder line against Cuba and Russia had legitimacy and support that was being exploited politically against him and his party. Kennedy was thus forced to defend a vulnerable and questionable policy which had little tangible benefits to show and he chose to do so, typically, "with vigor," which meant resisting all efforts to shift policy to a blockade of Russian aid, an invasion, or worse. The various rumbles, hints, indicators, and signals that the Soviet aid was more than just defensive weapons had to be suppressed for the Kennedy policy to proceed. (Special measures were taken to insure that if evidence of offensive weapons did appear the information would be kept in the inner Kennedy circles.) The signals would have to be very strong indeed to penetrate the top echelon because, if true, they would disrupt the central element of Kennedy's foreign policy and upset the off-year election applecart, perhaps leading to a Republican House. Under the circumstances it was easy to attribute the signals and intelligence to perennial cold warriorism or Republican efforts to sabotage the Kennedy election strategy. Inferential evidence, no matter how voluminous or well-constructed was too circumstantial to challenge this policy. For the intelligence agencies there was no stake in bucking the White House on this issue, to the contrary, there were points to be made in backstopping the line that the Soviet effort in Cuba was defensive.

In fairness, it must be noted that DCI John McCone argued as early as August that the Soviets might be preparing to move offensive missiles to Cuba. But McCone was a "non-professional," a businessman with little intelligence or political background and no expertise on Russia or the mysteries of the Kremlin. To Secretaries Rusk and McNamara it was highly unlikely that the Soviets would take such a provocative risk. To Kennedy, McCone was a suspicious professional anti-Communist. Even his standing within the intelligence community he headed was diluted by McCone's amateur status and his absence from Washington during the crucial weeks of September.

Furthermore, the "indians" in the intelligence community continued to address the question of offensive missiles despite the contrary stands of the "chiefs." While the Kennedy administration was unreceptive to the tentative signals, it did not prevent others from investigating the possibility of offensive weapons. Colonel Wright in DIA noticed unique patterns of Soviet surface-to-air missile (SAM) sites which were associated with offensive missiles. Navy analysts noted the high volume, low density cargoes (suggesting something large and bulky like missiles) of Soviet ships going to Cuba. CIA analysts compiled refugee stories of Soviet missiles and missile crews. On the other hand, the "hard" evidence of U-2 photography showing offensive missiles was lacking. Furthermore, Kremlinologists made a strong case that emplacement of nuclear missiles outside the Soviet Union would be highly uncharacteristic (probably unique) and the provocative step of placing them in Cuba was a far riskier one than could be expected from the Kremlin. The Kennedy administration, in consciously supporting efforts to develop contrary

views, while at the same time ignoring those views, could reassure itself and its critics of its "objectivity."

The story of why the U-2's did not fly from late September until the middle of October also fits with the bureaucratic politics model. After the Soviets shot down Powers' CIA U-2 over Russia, the U.S. intelligence community was highly sensitive to the physical and political risks involved in overflights. The stepped-up emplacements of SAMs in western Cuba increased the risk of a U-2 loss which (1) might force the President to stop the flights and (2) could ultimately derail the President's political strategy. The former would deprive the agencies of their best source of data while the latter would eliminate the President's confidence in the intelligence chiefs. The agencies were especially sensitive to these stakes because a U-2 had strayed over Siberia in early September and on 9 September a U.S. U-2 flown by a Nationalist Chinese was shot down over mainland China. Despite Colonel Wright's hypothesis and other signals that the SAMs in western Cuba might mean the presence of offensive missiles, the risk those SAMs posed to the U-2 s was deemed too great, there was more at stake than just finding out what might be going on. The intelligence chiefs decided to avoid the western SAM area and minimize the probability of a U-2 loss. When McCone returned to Washington the Wright hypothesis received a hearing and on 4 October the issue of a western overflight was raised again, with McCone and CIA backing. Ten days elapsed while the arguments were made for and against the flight. On 14 October McCone and Wright prevailed and the U-2 mission detected the offensive missiles. The hesitancy of those who opposed risking the U-2 was not unjustified, however; while the 14 October mission returned safely, on 27 October, at the height of the

crisis, Soviet SAMs on the western end of the island shot down a U-2. But by then both the physical and political stakes had changed: none of the players could afford not to have all the reconnaissance information it was possible to obtain, even at the cost of a U-2 and its pilot's life. Had the chiefs not kept the U-2 s away from the SAMs in late September and early October, this loss might have come before the missiles themselves were detected -- the intelligence agencies might have been blinded until the missile sites were completed and the crisis might have begun with the Soviet fait accompli which would have ruled out the successful blockade strategy and the Kennedy-Khrushchev negotiations that ended the crisis.

Hareven

Alouph Hareven (1978) argues that the cause of the Israeli intelligence failure in the Yom Kippur War was the "disturbed hierarchy" of Israeli intelligence agencies. He points (p. 5) to the

> . . . failure of an entire hierarchical system . . . because the interaction between the personalities of key officials disrupted its effective operation . . . when officials at successive levels lack judgment, their mutual influence is liable to produce disturbance rather than control and balance.

Hareven's analysis, however, goes beyond the analysis at the individual level of, for example, Etheredge (1978), in examining how these actors reach their critical positions and how the interactions among them lead to disturbances of the entire hierarchy. These latter aspects of his focus are largely political and thus warrant inclusion of Hareven's thesis as a bureaucratic politics model.

Focusing on Moshe Dayan (Israeli Minister of Defense in 1973), Lt. General David Elazar (Chief of Staff), and Major General Eliahu

Ze'ira (Chief of Military Intelligence), Hareven asserts that this "hierarchy of heroes," all highly successful military commanders, tended to be overconfident of Israeli intelligence's capability, and the ability of the Israeli defenses to defeat any attack the Arabs might mount.

Elazar and Ze'ira were appointed lacking the support of their superiors: Dayan did not select Elazar, and Ze'ira, who was close to Dayan, was not Elazar's choice for the top intelligence spot.[16] Thus the chain of intelligence communication which would filter intention estimates to the Meir cabinet was marked by uneasy personal relations. Dayan also tended to act as his own intelligence officer and, to some extent, his own chief of staff -- often obtaining "raw" intelligence and operational data from commanders in the field. However, neither Dayan nor Elazar attempted to bypass Ze'ira to gain access to all the analyses or raw reports in the summer and fall of 1973 which might have suggested to them that the Syrians and Egyptians were planning something unusual. Hareven writes (p. 14):

> . . . apparently the head of Military Intelligence did not give the Chief of Staff all the data in his possession; . . . the Chief of Staff himself does not appear to have demanded it. This is a classic example of the functioning of a "hierarchy of heroes": (Ze'ira) felt obliged to spare his superior the trouble of going into detail; (Elazar) relied on the head of Military Intelligence to tell him all the salient facts.

The "essence of the intelligence failure" (p. 15), according to Hareven, was the blockage of the intelligence hierarchy imposed by Ze'ira's interpretations and unwillingness to submit different views to his superiors. At least in part Ze'ira was motivated by his desire (understandable given his command rather than staff experience) to

[16] Hareven does not mention it but Dayan did not approve the election of own boss, Golda Meir (Meir, 1975: 435).

present "clear and sharp" evaluations to his chiefs, unclouded by the ambiguities of the Arab reality.[17] Ze'ira was aware (at least before

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[17] It is noted frequently that the highly successful military commander is rarely a highly successful intelligence officer. Dixon (1976) writes of the widespread abhorrence of intelligence by commanders apparent in military history:

> Indeed it is no exaggeration to say that an absence of adequate reconnoitering, the refusal to believe intelligence reports and a general horror of spying have tended to keep our armies wrapped in cocoons of catastrophic ignorance.

Even such seeming exceptions as General William Donovan (winner of the Medal of Honor in World War I and founder of the U.S. Office of Strategic Services in World War II) appear on closer examination to be far better organizers and leaders than analysts or estimators. Many who recall Churchill's often brilliant intelligence insights during World War II forget that he was acting as his own intelligence officer unofficially (and could claim credit for his successes but was not responsible to anyone for his errors) and was probably as often wrong as right (cf. McLachlan, 1968, e.g., about the course of the U-boat war he was wrong more than right) or that in World War I he was officially responsible for that horrendous intelligence and operational calamity, the invasion of Gallipoli, which cost him his position at the time.

The successful military commander takes the initiative by going on the offensive and avoiding the defensive. As Clausewitz noted, intelligence, especially knowledge of the enemy's intentions, is crucial to the defense, while security (hiding one's plans) is elemental to the offense. Being offense-minded, successful commanders may de-emphasize or neglect intelligence. Conversely, intelligence officers may be relatively more defensive-minded, inasmuch as their role is greater under such conditions.

It may be that the traits of the brilliant commander: keen intuition, ability to make instantaneous decisions, willingness to take great risks without hesitation, and supreme self-confidence, are wholly inappropriate to the tasks of the intelligence analyst. Good soldiers and spies may be cut from similar stock but good intelligence officers appear to be a different breed. It is certainly no accident that the Prussian Staff system which separated the intelligence and operational staff functions from each other as well as from command has enjoyed near universal adoption in armies (Millis, 1956: 123).

October) of the risks this course entailed, commenting to a Knesset Committee in May 1973 (p. 15):

> Of course, the clearer and sharper the evaluation, if proven to be a mistake -- then the mistake also becomes clear and sharp, but that is the risk which the head of Military Intelligence takes.

Hareven characterizes Ze'ira's stand as translating the estimation process into a "game" of "who's best at intelligence evaluations." He faults Ze'ira for his unwillingness to admit his (or his agency's) lack of knowledge, uncertainty, and doubt as to Arab intentions, instead adopting a commander's posture of decisiveness, self-confidence, certainty, and readiness to be the final judge of Israeli intelligence.

Although Hareven raises but does not answer the question of why Dayan, who persistently acted as his own intelligence officer and constantly kept personally in touch with the frontline situation, accepted Ze'ira's evaluations in October (especially after suspecting an Arab attack in 1971 and 1972), such behavior is consistent with the Allison-Halperin bureaucratic politics model. Ze'ira was Dayan's man, appointed over the reservations of Ze'ira's direct superior, Elazar. Dayan, regardless of his reservations about Ze'ira's estimates had a stake in defending his commitment to the man. On his side, Ze'ira 'owed' Dayan at least a performance of duties that would satisfy Dayan's and Elazar's expectations: to Ze'ira this meant "clear and sharp" estimates. Given the risk of his investment in Ze'ira, Dayan in a sense would be questioning his own judgment if he questioned Ze'ira's estimates, while Ze'ira was unlikely, given his commander's character, to introduce doubts in the minds of his superior or his mentor. And Hareven assesses Elazar as lacking the

capacity for curiousity and doubt (again the attributes of the dynamic commander) that would have led him to question Ze'ira or challenge his estimates. To this we might add a chief of staff's unwillingness to be seen as challenging a minister of defense's protege without clear justification. Ze'ira apparently provided no openings for such a challenge, and Elazar (who was not Dayan's choice for chief of staff) contented himself with the status quo. The fact that after the 1973 war Elazar repeatedly questioned the validity of the Military Intelligence estimation process suggests, that when not restrained by the mentor-protege link and given an opening, he was prepared to question Ze'ira's wisdom. In short, while Ze'ira was the central figure in the failure, Hareven argues that the relationship of Dayan, Elazar, and Ze'ira allowed the circumstances to develop which led to failure.

To this we would add that Dayan's and Elazar's stands in October 1973 and their stakes, which would be jeopardized if they sought out discrepant signals from below, tended to maintain the unfortunate hierarchy of heroes relation which prevented valid indicators of Arab intentions from reaching the top echelon and prevented the top echelon from developing a more comprehensive intelligence picture. The production of a better balanced intelligence estimate would have carried heavy political costs for the three actors who alone could have demanded it.

In a sense Hareven's analysis brings this review of the literature on intention estimation and intelligence failures full circle, returning us to an awareness of the impact of individual personality in such failures, the theme espoused by Etheredge (1978) noted early in this review. The final part of this Section attempts to synthesize the lessons learned from this review and to address the question of whether a "theory" of intelligence failure can be constructed from these analyses.

A Theory of Intelligence Failure

> A sophisticated theory of intelligence . . . could be of considerable service to the profession both in improving its modes of operation, and . . . in protecting it from excessive demands and from being tagged with blame for failing to live up to unrealistic expectations.
>
> Klaus Knorr, 1964

> The need still remains to convert "post-mortems of past intelligence performances" into an operational theory of intelligence.
>
> Abraham Ben-zvi, 1976

> Theories of intelligence are virtually nonexistent.
>
> H. H. Ransom, 1974

Do the numerous analyses of failures to adequately estimate intentions permit us to synthesize a theory of intelligence failure, as has been called for repeatedly (e.g., Knorr, 1964: 465-6; Ransom, 1974: 144-6; Ben-zvi, 1976: 383)? At the very least, certain of the "lessons learned," as revealed by the postmortems, and the concept of different levels of analysis, as exemplified by our trichotomization of models into individual, organizational and political, strongly imply that an adequate theory will be both multi-factored (i.e., include several variables) and multi-layered (i.e., include several levels of aggregation).

Pessimism. Scholars of intelligence generally are pessimistic about the possibility of adequate warning against surprise. They also have only limited expectations that a theory of intelligence can be explicated in sufficient detail to do much toward solving the intelligence failure and surprise attack problems.

For example, Knorr (1964: 460) asserts that surprise is not necessarily inevitable and that it would be possible to "improve the 'batting average' - say from .275 to .301." Shlaim (1976: 370) notes the "intrinsic uncertainty of strategic warning," but asserts that the frequency of intelligence failures might be reduced if certain precautions were observed. Wohlstetter (1965: 707) writes that foresight cannot be guaranteed but "we can improve the chance of acting on signals in time to avert or moderate a disaster." In contrast, Handel (1976: 7) sees "little chance, despite the availability of adequate information, ultra-sophisticated technologies, and all human effort invested, to prevent or forestall an impending surprise attack." Betts (1978: 88) writes that intelligence failures are "not only inevitable, they are natural." On the other hand, Ben-zvi (1976: 395) appears to believe that greater use of tactical intelligence may help to prevent strategic surprises, although he is critical (1977) of those who espouse exclusively technological solutions to the problems of warning (Andriole and Young, 1977; Belden, 1977). Because surprise attacks are the most conspicuous instances of intelligence failures, they provide the grist for the majority of studies of intelligence failures. However, such failures are rare, though conspicuous and costly, whereas other failures of intelligence, which may be far more prevalent and hence more costly in the long run, receive little attention. Hence a theory of intelligence constructed from the literature on major failures and strategic surprise may capture only a narrow spectrum of intelligence performance. Looking a wider analytic literature on other types of intelligence failures, we must be content with this caveat that only intelligence theories with narrow applications are possible at the moment. Since our major concern is with estimates of intentions, however,

and because the surprise attack seemingly provides the most strenuous test of the ability of intelligence agencies to perceive the intentions of a possible opponent (seemingly often failed), the analytic literature may prove adequate for a tentative theory of the intention estimation aspects of intelligence failure.

Scholars of intelligence have only limited expectations for the utility of an intelligence theory at this time; while they seem to urge that one be developed, they offer little encouragement that it will solve many problems. The most pessimistic about warning are also the most pessimistic about the utility of theory.

Ransom (1974: 145) suggests that without theories of intelligence measures of the effectiveness of intelligence in policy-making may be impossible to develop, reiterating a similar argument of Knorr's (1964). However, Ransom perceives many impediments in the path of the theory-builder (e.g., secrecy, bureaucratic complexity, issues of definition) and speculates that "possibly it is overly ambitious . . . to call for theories." A decade earlier, Knorr (1964: 155) argued that "historians and social scientists have developed and are developing skills that should permit substantial contributions to the theory . . . of intelligence," although he too noted the impediment of secrecy. Knorr suggests the theory would be informal, fragmentary, and developed from cumulative experience. Some possible uses of such theory he suggests include distinguishing events in terms of their inherent predictability, assessing intelligence efficiency, and developing indicators and behavior profiles of foreign leaders. Shlaim's (1976: 300) views echo Knorr's.

Handel (1976: 9) takes Knorr's assertions one step further, arguing that scholars have failed "to apply [their] theoretical insights

and empirical knowledge "to the problems of intelligence failure. He reasons that the "theory of surprise" being necessarily ex post facto, has strong explanatory power, but weak predictive power, and holds little value for the decision-maker: "even the most refined theory . . . cannot guarantee against surprise." Similarly, Betts (1978: 62) argues that while descriptive theory of intelligence failures abounds, there is no normative theory and that the hindsight analyses of failures have produced no material improvement in intelligence performance.

Ben-zvi (1976: 381-3) also faults existing theories as being hindsighted and proposes "an operational theory . . . which will categorize the key variables associated with . . . surprise and develop a cluster of 'sophisticated expectations about surprise'" which might reduce failures. Wohlstetter (1962, 1965) and Jervis (1968) derive safeguards from their analyses which they suggest may help to reduce failures and surprise.

Theory levels. An intelligence theory of intention estimation will have to consider at least three levels of activity. First, the environment external to the estimating nation, B, includes the background activity of all world affairs as well as the activity of the hostile nation, A, whose intentions are B's concern (Handel, 1976; Ben-zvi, 1976). The external environment creates distractions, noise, as well as signals relevant to the hostile nation's possible future behavior. The hostile nation itself and its allies create noise, signals, and deceptions (noise masquerading as signal, and vice versa). The activity of this first level cannot be perceived directly, but many observers and agencies beyond the intelligence services of a given country monitor and assess it, so that, at least in hindsight, a variety of perspectives on this level could hypothetically be constructed as a baseline against which to compare events at the other two levels.

The second level of activity which intelligence theory must consider is the conscious, ongoing effort of various official and unofficial agencies in B to employ their knowledge and information on A to estimate A's plans, goals, future behavior, and intentions. Perceptual, organizational, and bureaucratic factors all become involved in this activity. The intellectual assumptions and cognitive operating characteristics of analysts and other central actors affect how data on A are shaped into estimates. Organizational relationships, SOPs, communication mechanisms influence the approach to the problem, the flow of data and other aspects of the business of requesting, producing, and delivering an estimate. The bureaucratic politics of the various agencies and actors involved in this business, their various stakes and stands and the action channels available to them affect their contribution to the estimation process.

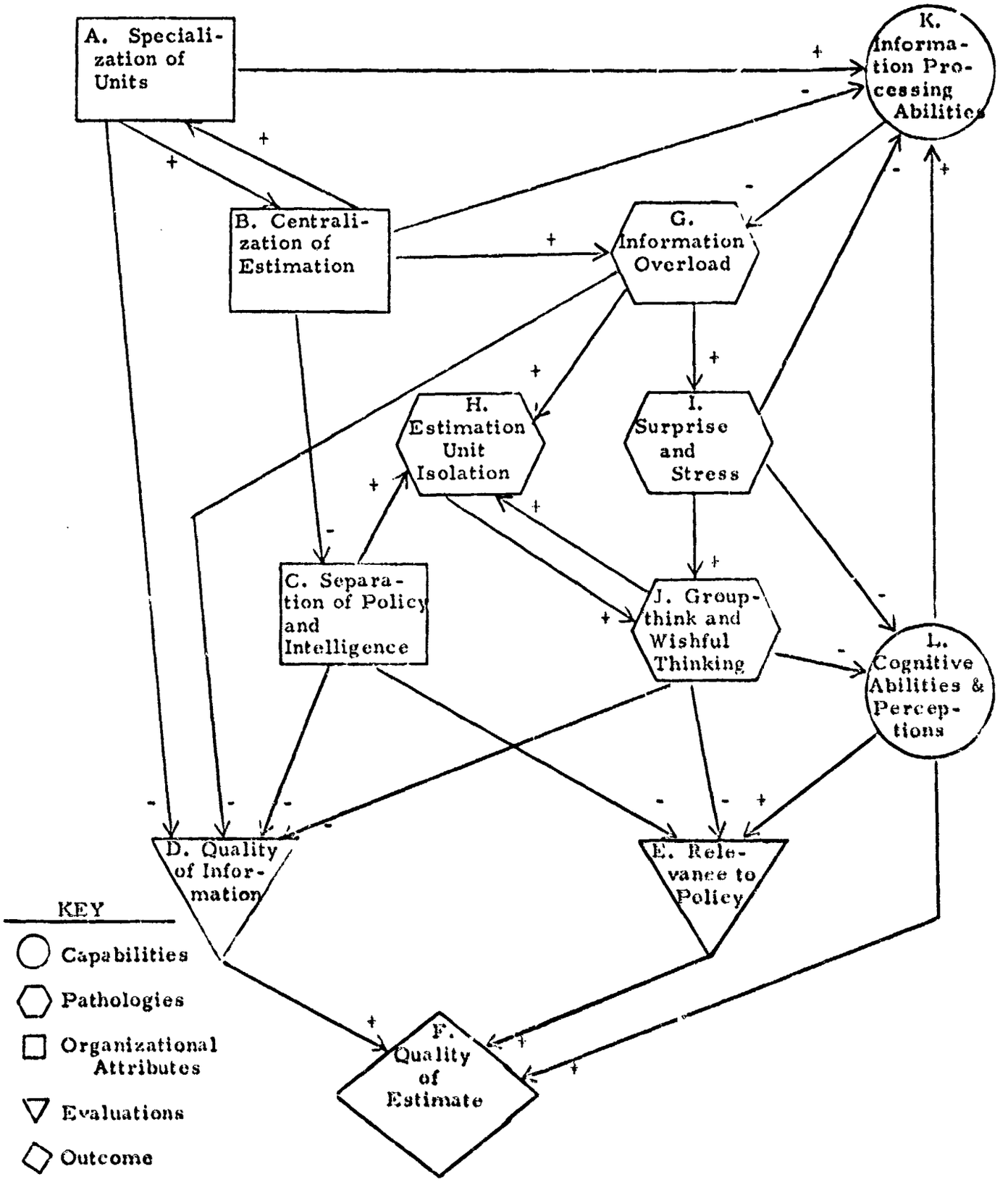
At this second level, the perceptual and organizational factors seem to be the most immediate and most tangible influences on the construction of the estimates. Certainly these are the influences most often cited as responsible for intelligence failures. However, the work of estimation in the intelligence agencies does not take place in an internal vacuum but against the backdrop of various ongoing bureaucratic games being fought out within an intelligence agency, or between agencies, between the intelligence and the policy levels, or finally, and most typically, between differing policy camps. The impact of these bureaucratic fights (which may or may not be closely related to B's estimates of A's intentions) will tend to have extremely pervasive but subtle effects on the estimation business. Such effects are probably difficult to detect except in rare and blatant cases (e.g., Laird's pressure on CIA to change the SS-9 estimate) and are perhaps often attributed to personality rather than politics when suspected.

The third level of theory then is the internal environment against which intention estimation is conducted. This level must treat the perceptual, organizational, and political factors surrounding the use and manipulation of intelligence by the policy-makers. Because so much of the real business at the policy level is conducted in informal, ad hoc fashion, with unofficial action channel rules applying, organizational factors (which emphasize official, bureaucratic rigidities) become less conspicuous or important. Personalities, perceptions, and politics become the primary explanatory mechanisms at this final level.

Theory factors. The review of intelligence failures implicates a wide variety of variables which seem to contribute to faulty estimates of foreign intentions. However, the relationships among these variables are rarely described; single-factor theories are the norm. Some relationships among several candidate variables are diagramed in Figure 4.4 (derived from Smart and Vertinsky, 1977: 640). The unit of analysis in this scheme is the intelligence estimation unit, any intact organized group estimating intentions. There may be several such groups within an intelligence agency, and many operating in a nation at any given time. Such a unit may range from a single individual to a large team.

Several organizational variables are suggested in the diagram (Boxes A, B, and C), but these are not exclusive, others (e.g., hierarchy, organizational attitudes toward intelligence) could have been included. We have kept the diagram as schematic as possible to demonstrate the possibility of relating variables to each other and to some dependent variables. For example, while specialization of estimation units (Box A) fosters some estimation capabilities (e.g., information processing, Box K) it may reduce the quality of information (Box D) by fostering

Figure 4.4. Hypothetical Interrelationships Among Multiple Factors



parochialism (Wilensky, 1967). Too much specialization ultimately produces pressures for centralization (Box B), but this tends to complicate information processing abilities and to foster information overload (Box G). On the other hand, centralization may reduce the tendency of policy and intelligence to be kept separate (Box C).

This schematic diagram implies some rough, tentative relationships between capabilities (Boxes K and L), estimation pathologies (Boxes G, H, I, and J) and organizational variables (Boxes A, B, and C). The impact of these variables is shown on two evaluative dimensions (Boxes D and E) which characterize the overall product (Box F). Note that even a limited schematic example such as this one is complex and speculative. Adding more factors (e.g., variables from the bureaucratic politics model) would only increase the complexity and uncertainty. At this point in the development of a theory of intelligence it would seem to be useful to outline the simplest schematic concept consistent with the three models of failure described above. Figure 4.4 indicates that an effort to capture even some of the candidate variables may be too complex to be productive. A more promising approach is to focus on a small number of variables that are intellectually consistent with the main elements of the single-factor failure models which have predominated. The next section outlines such a (relatively) simple model.

SECTION 5

DISASTER THEORY

DISASTER THEORY

> Small-scale failures can be produced very rapidly, but large-scale failures can only be produced if time and resources are devoted to them.
>
> Barry Turner, 1976

> Great blunders are often made, like large ropes, of a multitude of fibres.
>
> Victor Hugo

The Turner Disaster Model

Barry A. Turner, an industrial sociologist at the University of Exeter, has formulated a model of disasters which is easily extended to intelligence failures. Borrowing Wilensky's (1967) term, Turner (1976a, b) characterizes disasters as organizational "failures of foresight" and asserts that they are the product of an "incubation" period in disaster development. The common causal features of this period are erroneous assumptions, information handling difficulties, unattended violations of existing precautions, and a reluctance to fear the worst outcome. These features lead to operational and procedural errors which accumulate unnoticed until a precipitating event produces the disaster and a degree of cultural collapse. Cultural readjustments after the disaster allow the ill-structured problem which led to the failure to be absorbed into the culture in a well-structured form.

Because Turner's disaster model bears closely on such "failures of foresight" as intelligence disasters, and because it provides a potential basis for a "theory of intelligence surprise" (Knorr, 1964, p. 466; Shlaim, 1976, p. 380), we present it in detail and compare it with appraisals of intelligence failure. Later we will apply Turner's model

to an intelligence failure example which stemmed from errors in the estimation of the enemy's intentions. Our aim is to use the Turner model as an aid in developing a taxonomy of intention estimation processes.

Definitions

Failure of foresight. Turner defines this central characteristic of disaster development as a collapse of precautions previously regarded as culturally adequate, producing alarm, disaster, or catastrophe, and requiring widespread reevaluation of previously accepted precautions and defenses.

Disaster. Disasters are distinguished from accidents by Turner. Disasters disrupt the social context by unleashing destructive forces in the physical environment and by producing psychological disruptions which provoke the question "How could such a thing come to happen?" Disasters threaten a major part of society with unwanted and unpredicted consequences as a result of a failure of foresight. A disaster may consist of a single disruptive event followed by many repercussions, or a series of events in close succession producing successive surprises and the need for successive readjustments. In contrast, accidents do not provoke a cultural reevaluation of precautions. Even though accidents may result from failures of foresight at the individual level, accidents caused by individual errors or failures to adhere to accepted precautions do not provoke reevaluations of cultural defenses; nor do even large-scale accidents which occur in situations of recognized hazard and high risk. Accidents are either explainable within preexisting cultural contexts or are foreseeable and predictable to some extent, while disasters

can be neither predicted nor explained before or immediately after the fact. Turner asserts that disasters are potentially foreseeable and avoidable but this potential is not recognized until wide-spread re-examinations of the disaster's antecedents and consequences have been made. This potential for prediction and avoidance exists because disasters, unlike accidents, are the result of long, complex chains of errors, inaccuracies, and inadequacies in accepted norms, beliefs, and practices. Turner writes (1976a, 395) that disasters "are not created overnight . . . (they) need the unwitting assistance offered by access to the resources and resource flows of large organizations and time . . . Small-scale failures [accidents] can be produced very rapidly, but large-scale failure [i.e., disaster] can only be produced if time and resources are devoted to them."

Variable disjunction of information. Turner relates failures of foresight to the communication difficulties of large systems. He defines variable disjunction of information as a state facing an organization of high complexity and uncertainty for which there is no single authoritative description. Individuals in the organization have access to different sets of information and each tends to construct different theories about problems and solutions. Scarcities of time, money and energy prevent adequate handling of information which might reconcile conflicting aspects of the various sets of information. Consequently information gathering, processing, distribution, and communication are selective and diverse across elements of the organization.

Assumptions

Turner's model assumes organizations cope with uncertainty by relying on heuristics, rituals, habitual patterns and by setting goals

and making plans to reach them. Action on such plans is premised on a collective adoption of simplifying assumptions about the environment which produce a framework of "bounded rationality" (Simon, 1957; Steinbruner, 1974).

Basic Issue

The basic question facing organizations is whether the simplified diagnosis of present and likely future situations is accurate enough to achieve organizational goals without encountering unexpected catastrophic difficulties.

Central Problem

The chief difficulty lies in discovering which aspects of the problems facing the organization are prudent to ignore and which should be attended and how the organization can establish a reasonable criterion level of safety. Dealing with this problem requires that the organization recognize what Wilensky (1967) termed "high-quality" intelligence, i.e., clear, timely, reliable, valid and wide-ranging information relevant to plans and goals. Failures to recognize such intelligence leads to failures of foresight, which in turn produce disasters.

Common Features of Disasters

Turner (1976b) analyzed over 400 accidents and disasters reported in British Parliamentary papers between 1966-1976. Common to all failure of foresight disasters were the following features:

1. The organizations involved were coping with a large, complex problem with hard-to-specify limits.

2. The problem was being dealt with by a number of groups and individuals in separate organizations and separate departments in organizations.

3. Some combination of errors resulted from

   a. erroneous assumptions,

   b. information handling difficulties,

   c. lack of accepted standards, regulations or precautions,

   d. reluctance to fear the worst.[1]

Disaster Development Sequence

Turner (1976a, b) has constructed a sequence of disaster development that includes the events which lead to disaster and identified specific features of predisaster stages.

Stage 1: Initial beliefs and norms. At this stage events are reasonably "normal." Culturally held beliefs about hazards are sufficiently accurate to enable individuals and groups to deal successfully with problems. The community adheres to normative prescriptions (embodied in laws and codes of practice, "common sense," and folkways) which are consonant with accepted beliefs and which enable the community to cope with the world. When unfortunate consequences result from violations of these prescriptions there is no need for cultural readjustment, such incidents are labeled as accident, are not surprising, and the attribution of their cause to violations serves to strengthen the force of existing prescriptions. This stage is the notional starting point for disaster development.

Stage 2: Incubation period. This period starts when events which are at odds with the accepted beliefs about hazards and the norms for their avoidance begin to accumulate unnoticed. An "incubation

[1] There is nothing nationally specific about Turner's analysis. The characteristics he identifies for British disasters apply as well for disasters in the United States, e.g., the Buffalo Creek flood disaster (Nugent, 1973; Stern, 1976) or the New York City power blackouts (Boffey, 1978; Metz, 1978).

network" is formed consisting of a chain of discrepant events, or several chains, which are misperceived or unnoticed. Hazards are covertly delineated and amplified. These events are (1) not known by anyone, or (2) known but not fully understood by all concerned, so that the implications of the events are not understood as they will be after the disaster occurs. Errors accumulate for four reasons:

a. Erroneous assumptions: Events may be unappreciated because no one expected or was alert for such phenomena, in part because of institutional rigidities of belief and perception as to what the critical features of the hazard really are.

-- The significance of events may be missed because "decoy phenomena" draw attention from more serious problems. A well-defined problem or source of danger may be dealt with but such action may draw attention from dangerous but ill-structured problems in the background. [N. B. Turner's concept of "decoy phenomena" can easily be extended to include deception activities in intelligence failures.]

-- Complaints of danger from nonexperts outside the organization may be dismissed on the assumption that such persons are uninformed alarmists, cranks, power-hungry, attention-seekers, etc. An organizational assumption that it knows better than outsiders may lead to high-handed dismissive responses which close off this channel of warning and alternative perception. [N. B. The use and misuse of "devil's advocates" from within and outside the intelligence community share some of these problems.]

-- Organizations may have overly rigid stereotypes of the problems they should be attending and overly restrictive definitions of their areas of responsibility.

b. Information handling difficulties: These difficulties may stem largely from the nature of the information itself or from the nature of the organizations handling the information.

-- Excessive amounts of information lead to an inability to screen, translate, interpret, distribute and analyze data.

-- Large amounts of ambiguous information ("noise") can conceal crucial messages ("signal").

-- Large or multi-agency organizations create many information handling difficulties. The variable disjunction of information in such organizations leads to ambiguities about orders, procedures, responsibilities and controls. The preoccupations and routine practices of message handlers and processors may lead to an oversight of critical, emergency data. The different subcultures within the organization have differing assumptions regarding the significance of warnings, information-sharing, and responsibility. Organizationally protective rules restrict information distribution so that some of the wrong people get critical information and some of the right people do not. When dealing with prolonged tasks, the rules, goals, responsibilities, and administrative roles in the organization change thus altering information handling processes. When dealing with rushed tasks critical information is neglected, inadequately processed or not distributed. Interpersonal difficulties may result in poor transmission or the transmission of wrong or misleading information or unintentional distortions. Information may not be integrated over time, agencies, or individuals properly. Institutional rigidities in information sharing lead to reliance on informal contacts at the expense of formalized procedures, introducing new ambiguities to the transmitted information.

c. Lack of accepted standards and precautions: Where formal precautions are not fully up-to-date violations of formal rules and regulations come to be accepted as normal. Individuals fail to realize that existing regulations apply. Since regulations may seem inapplicable to ongoing changes and new situations, appropriate and universal standards cannot be applied. Instead, ad hoc solutions are applied piecemeal without the benefit of guidance from well-considered and widespread formal precautions.

d. Minimizing the emergent danger: The natural human tendency to assume invulnerability and to maintain a self-protective positive psychological equilibrium leads to a reluctance to fear the worst outcome. Consequently recognized hazards are underestimated, the capabilities to cope with hazards are overestimated, and warnings are minimized, disregarded, or not perceived. Emergency equipment, methods, and procedures are either not used when appropriate or are used "too little and too late." Fear of sounding false alarms prevents the alarm being given, more so if those witnessing the hazard feel inadequate or ill-informed to perform the alarm function. Bystanders often erroneously assume that others have already given the alarm. There may be ambiguity and disagreement among several parties about the status and significance of evidence pointing to danger which produces an undervaluing of the evidence, particularly if the more complacent group is also more powerful.

The incubation period ends when one or several of the incubation network error chains culminates in a precipitating event or incident. This event produces what is immediately termed the "disaster." In the

final stage of disaster development this initial definition is expanded to include the accumulated errors of the incubation period.

Stage 3: Precipitating event. This event transforms the hitherto covert, subliminal, and latent structure of incubation error chains into a perceptible form. The precipitating event (1) has immediate physical characteristics and consequences which cannot be disregarded and which are unambiguous (but perhaps confusing), (2) immediately creates general recognition of the need for a new interpretation of the events in the incubation period, (3) gains force from being unpredictable and from the unpreparedness it reveals. (The latter two characteristics distinguish the disaster from the accident.) The precipitating event will be limited (in retrospect) to many of the error chains of the incubation period although not necessarily to all the error chains that are eventually uncovered.

Turner (1976b) notes that these characteristics of the precipitating event tend to prevent disaster precaution. That is, a noncatastrophic event which could potentially reveal the incubating chains of errors will not release sufficiently strong and unambiguous forces to compell a new interpretation. He writes (p. 762) "only when a non-catastrophic realization is achieved by a powerful and prestigious body can cultural redefinition arise without the impetus of a large-scale physical precipitating event." [The intense and elaborate attention to security which surrounds visits of heads of state and the initiation of surprise attacks usually results in survival of the former and the success of the latter because noncatastrophic realizations are achieved when catastrophes are explicitly considered before hand by supreme authorities.]

Stage 4: Disaster onset. The direct and unanticipated consequences of failure occur and the immediate consequences of the collapse of cultural precautions become apparent.

Stage 5: First adjustment-rescue and salvage. The main features of the immediate post-collapse situation are recognized in ad hoc adjustments and redefinitions which permit the work of rescue and salvage to be started. In addition, action is taken to place and shift blame and quasi-magical solutions, shock, and denial processes are manifested which may impede mopping up.

Stage 6: Full cultural readjustment. On subsidence of the immediate effects a more leisurely and detailed assessment of the incident is undertaken, attempting to determine how culturally approved precautions could have been so inadequate, tracing the now-revealed pattern of events which developed in the incubation period, and considering adjustments to beliefs, assumptions, laws and codes needed to prevent reoccurrences. Turner notes (1976a, 393; 1976b, 764) that such reappraisals deal with the events which led to the disasters as they are later revealed and not as they presented themselves to those involved at the time. In other words, post hoc reviews treat the well-structured problems defined and revealed by the disaster, rather than the pre-existing, ill-structured problem. [This tendency is characterized in psychological research as the "hindsight bias" and its implications are examined below.] These transformations yield a new configuration

of the incubation period which has different meanings and interpretations than existed prior to the precipitating event.[2]

Turner's work (1976b) contains an heuristic device useful in examining intelligence failures, a flow diagram of the key event preceding the precipitating event of a disaster and an overlay which identifies errors and links them in the causal error chains. This flow diagram heuristic is similar to the event trees and fault trees used in operations research. Although used post hoc by Turner (1976b), such trees are often used by experts to make predictions or assess risks. The advantages and disadvantages of such analytical adjuncts are discussed below.

[2]Even though disasters such as dam failures, impound-induced earthquakes, landslides, power failures, etc., are historically not uncommon, the benefit-cost analysis of dams, water projects, and so forth rarely include an explicit examination of such failures. The low-probability character of these events tends to lead to their being completely neglected, i.e., treated as if they were zero-probability events (Mark and Stuart-Alexander, 1977).

The Yom Kippur Disaster

Egypt and Syria attacked Israel on the afternoon of 6 October inflicting an unparallelled military defeat on the Israeli Defense Forces (IDF) which was reversed only after several desperate battles. The political, economic, and diplomatic aftershocks of the attack jarred Israel as badly as the initial military defeats; e.g., the oil embargo alone had world-wide repercussions which worsened Israel's diplomatic position. The Israeli military disaster can be divided into three main parts, each of whi was, for the Israelis, unexpected and costly: the attack itself (the strategic surprise), and two technical surprises (Knorr, 1964); the immobilization of the Israeli Air Force (IAF) by the Arab surface-to-air missile (SAM) systems, and the massive losses inflicted on Israeli armor by the Egyptian Second Army (or Second Corps) "tank traps" on the east bank of the Suez Canal ("beyond a doubt ... the worst defeat in the history of the Israeli Army," Dupuy, 1978: 433). Although strategic surprise was the key, necessary element to Sadat's plan for a limited attack, it is important to realize that surprise was not a sufficient condition for the success of his plan. Furthermore, the strategic surprise was in no way complete; many Israeli officers expected the attack. Nor did the Egyptians count on the surprise being as complete as it was (e.g., they expected 50 times as many casualties as they actually suffered in the Canal crossing, Safran, 1978: 146). But the surprise obtained was adequate to disrupt the Israeli General Staff and Cabinet and to delay mobilization long enough for the Arab air

defense barriers and tank traps to be established. This Israeli disaster is analyzed in terms of the Turner disaster model.

Disaster Theory. Turner's model of disasters includes two devices which are applied to the Yom Kippur disaster. The first is the six stage model of disaster development. The second device consists of a flow diagram of the main events preceding the central event which precipitates the disaster, and an overlay diagram which identifies (with the benefit of hindsight) the errors and links between them that constitute the causal error chain. The event flow diagrams in this case will contrast the Israel interpretations and explanations of events before the 1973 war with the actual implications of the events as known from the Egyptian and Syrian war plans.

It is argued that the application of disaster theory is not necessarily post hoc and that the construction of event flow diagrams can aid in the prevention of disasters by making clear potential erroneous assumptions and possible causal error chains. However, the present case is developed from hindsight with all the attendant biases entailed, for example, exclusion of events now known to be unimportant. This account is thus not satisfactory as a means of providing a definitive analysis of the pre-war intelligence problems facing the Israelis (i.e., it is not an equivalent of Wohlstetter's encyclopedic review, Pearl Harbor). The objective here is to show that disaster theory provides an analytic tool which deduces what seem to be the central errors

in failures, thus highlighting intention estimation processes which are weak. Further, it inductively determines the interrelations of various errors and analytic weaknesses to each other so that the critical path of causal error links can be traced in a particular case, and other potential critical paths can be noted. For these purposes the extreme schematization of the event flow diagrams is appropriate. [A more detailed flow diagram is available from the author, but it too is still highly schematic, as must be any useful record.]

The intelligence analyst might attempt, using this tool, to specify potential disasters and relevant assumptions and, by outlining possible causal error chains between the assumptions and the disasters, be alerted to conclusions and inferences which may be dangerous and should receive close attention. This use of disaster theory as a prescriptive method is developed more fully in Section 6.

The Six Stage Disaster Process

Stage 1: Initial beliefs and norms. Following the 1967 Israeli victory over the Arabs certain assumptions formed the core of Israel's defense concept (see, e.g., Monroe and Farrar-Hockley, 1975; Safran, 1978; Shlaim, 1976, 1978; Ben-zvi, 1976). Foremost among these was the overwhelming Israeli belief in Arab inferiority in all things military, and conversely, what Safran (1978: 135) euphemistically labels the Israelis' "proneness to excessive self-assurance." The Israeli defense plans required at least a 48-hour warning of an impending Arab attack, but

this warning was "guaranteed" by the Military Intelligence Branch. Although several intelligence agencies exist in Israel, Military Intelligence was the only one with an estimative and evaluative mission. Given the assumption that ample warning would be received, the secondary assumptions in the Israeli defense concept were reasonable: Israeli reserves would be mobilized, the IAF would destroy enemy air defenses just before or at the outset of the enemy attack, and Israeli armor would be in position to stop an advance on the Golan Heights or a Canal crossing when the Arab attack was launched.

The IDF strategy prior to 1973 was defensive. The long borders won in the 1967 war gave the IDF the freedom, for the first time since independence, to trade space for time, especially in the Sinai expanses. The Bar-Lev defensive line along the Canal, constructed at great expense during the 1969-70 War of Attrition, was intended to withstand heavy artillery harassment and to repel commandos crossing the Canal, as well as to provide the farthest outpost on the Egyptian frontier, but not as a final defensive line against an Egyptian Canal crossing. Instead the mobilization of Israel's tank corps was expected to precede an Egyptian attack, and these tanks, ready and waiting, would keep the Egyptians on the West Bank of the Canal. Defense of the 1967 borders against all-out attack required mobilization of all reserves, but several additional factors contributed to the high Israeli confidence that such mobilization would occur before the Arab attack was launched.

Sometime during 1971 or 1972 Israeli intelligence obtained authoritative information on the thinking of the Egyptian military chiefs under War Minister Sadeq (Safran, 1978: 138). This information formed the basis for "the conception," the term given by the Agranat Commission Report (the official Israeli post-mortem of the Yom Kippur War) to the Israeli assumptions that: (1) if the Arabs attacked they would seek a total military victory, (2) without air superiority over Israel, Egypt would not attack unless it could paralyze the IAF by air attacks deep in Israel, and (3) Syria would attack only jointly with Egypt.

These assumptions -- Arab inferiority, Israeli superiority and terrain advantage, an IAF that could not be stopped by the Arabs, that an effective Arab coalition was impossible, and that the Israelis could force the Arab ground forces into a battle of mobility -- combined to produce the Israeli conclusion that the Arabs were incapable of a combined attack or of a total victory and thus the Arabs did not intend to attack. In fact, as Dupuy (1978: 388) notes:

> Sadat came to the conclusion that it would be better and more satisfactory for the Egyptian people to fight a war and lose, than not to fight at all simply because defeat was likely ... honorable defeat was a preferable alternative to an inglorious peace.

While the Israeli assumptions regarding Arab strategic intentions ('total victory') were based on erroneous conclusions about the Arab's general capabilities (insufficient for their strategic objectives), the

Israelis also underestimated the specific weapons and tactics the Arabs used in 1973. The Israelis were wrong about what the Arabs hoped to gain and also about their capabilities to gain a military advantage. While the Israelis knew the Syrian and Egyptian Armies were equipped with modern, effective Soviet antitank and antiaircraft weapons, the IDF was unprepared for the numbers of weapons they encountered in the opening battles. Equally unexpected were the coordination and integration of the various weapons systems which made the Egyptian defensive strategy in the Sinai and the Syrian assault of the Golan Heights so costly to Israel. By avoiding tank-to-tank and air-to-air encounters, the Egyptians neatly parried the Israelis' initial counterthrusts and delivered a crippling riposte in the opening days of the war from which the Israelis recovered only by great ingenuity and fortitude (and sweeping changes of commanders). The weight and intensity of the Syrian assault was unexpected, as were the Israeli casualties in men and materiel. The carefully laid Israeli plans for swift counteroffensive lay in shambles as the IDF was forced to improvise defensive tactics from day to day until a large-scale counteroffensive could finally be mounted.

At the organizational level, prior to the October war Israel profited from the resourcefulness of the world's most respected intelligence apparatus. Credited with near-infallibility on the basis of its many coups in war and peace, Israel's Military Intelligence

Branch had a reputation virtually unchallengeable by Israelis or their allies. As the October war approached, this reputation seemingly improved as a result of the premature mobilization in May 1973 ordered by Defense Minister Dayan which was opposed by Intelligence Chief Ze'ira. After this success it became even more difficult to question intelligence judgments. The Military Intelligence Branch had "guaranteed" the IDF at least 48 hours warning of an all-out attack, more than enough time for another preemptive air strike that, as in the 1967 war, would give the IDF control of the sky and allow it to destroy the Arab attack. Also, Israeli intelligence enjoyed close contacts with U.S. intelligence agencies, giving it access to the most modern technical inteiligence-gathering resources as well as those the Israelis themselves developed. Among these were mechanisms which Israeli intelligence believed would provide incontrovertible proof of an attack early enough to permit the "guaranteed" warning. This assumption was not questioned until October. [Hareven's (1978) analysis of the personalities of the central actors provides additional, psychological reasons why Ze'ira's estimates were not challenged seriously until they were obviously wrong, see Section 4.]

Stage 2: Incubation period. Turner asserts that this period begins when events accumulate which are at odds with accepted beliefs about hazards and the norms for their avoidance. An "incubation network" develops, i.e., a chain or chains of discrepant events

which are unnoticed, misperceived, or ignored. Four factors facilitate this process: erroneous assumptions, information handling difficulties, lack of standards and precautions, and minimizing the emergent danger. Each of these factors played a central role in the Israeli disaster and several "incubation networks" of error chains led to the precipitating event of the disaster, the Arab surprise attack.

A. Erroneous Assumptions. The Israelis made a variety of erroneous assumptions regarding Arab tactics, capabilities, weaponry and intentions. Some of the major errors, summarized below, contributed to the error chains that developed.

(1) "All-out war": The Israelis assumed the Arab military objective of the next war would be "all-out" victory (Shlaim, 1976; Ben-Zvi, 1976; Safran, 1978). Several major clues available to the Israelis prior to 1973 suggested the Arabs might seriously pursue objectives more in keeping with their capability such as a limited military action aimed at forcing Israel to a diplomatic solution by imposing heavy costs on the IDF. While the 1968 al Ahram article by Nasser's confidant, Mohammed Haykal, argued for the type of war eventually launched in 1973, a variety of war scenarios appeared regularly in the Arab press (Safran, 1978: 134). However, the objectives of Nasser's War of Attrition clearly foreshadowed the aims of the October War (Shlaim and Tanter, 1978). Perlmutter (1975:451) writes

> Dayan misperceived Sadat's goal, which was essentially political, not military: to establish a symbolic and real Egyptian stronghold on the east bank of the canal. Dayan's failure ... stemmed from not absorbing Sadat's message during the war of attrition--a war waged for political purposes using military means.

However, the Egyptian tactical failure in that effort tended to divert the Israelis from appreciating the Egyptians' intentions, and convinced them that Egypt could not make an attrition strategy pay off (Shlaim and Tanter, 1978).

The Israeli assumption of "all-out" Arab intentions was based on authoritative intelligence reflecting the thinking of Egyptian War Minister Sadeq and his military chiefs. When Sadat fired Sadeq in November 1972 (because, it was learned after the 1973 War, he opposed Sadat's limited war strategy) a major basis for the Israeli assumption was removed, but the assumption was not reexamined when Sadeq was replaced by Ismail (Safran, 1978: 138).

(2) "Available warning": The "guarantee" of at least 48 hours warning was essential to the Israeli defense strategy. In large part this "guarantee" was based on confidence in the technical intelligence sources (presumably communications intelligence) available to the Israelis which, they believed, would provide incontrovertible proof of impending attack. Unclassified sources are still ambiguous as to when this reliable signal was received. Some writers (Handel, 1976; Shlaim, 1976; Safran, 1978) note that the reliable signal was received

by the Israelis in the early morning hours of 6 October (Handel, 1976: 37, and others claim 0300; Safran, 1978: 140, 0430; Golda Meir writes that she got word "at about 4 a.m.," 1975: 410), far too late to provide the guaranteed 48 hours warning. Furthermore, Ze'ira's interpretation at the time was that the signal reflected a 1800 attack hour; in fact, the Arabs struck at 1400, four hours sooner. The Israelis consequently actually had about 10 hours of warning rather than 48, and they thought they had 12 to 14 hours to prepare. On the other hand, testimony before the U.S. House of Representatives Select Committee on Intelligence (Pike Committee, 1975) by U.S. intelligence officers indicates U.S. intelligence was aware of Egyptian alert mechanisms as early as 26 September (p. 655). Handel (1976: 32) reports in his chronology that on 21 September

> "The CIA, DIA and NSE (sic; NSA?) have collected enough evidence to indicate the possibility of a combined Syrian-Egyptian attack on Israel. Israel intelligence apparently does not agree that the probability of war is high enough to warrant special concern."

The Israeli intelligence chief rejected this warning however, and attributed the unusual activity to exercises. Whether the Israelis were wrong in rejecting the U.S.-derived signal, or wrong in believing the signal of 6 October would occur at least 48 hours before the attack, the Israelis mistakenly had staked the fate of their defenses on a signal that could not possibly have warranted the sense of certainty it generated.

(3) "All-tank tactics." The overwhelming success in the 1967 War produced an Israeli commitment to the doctrine of "all-tank" tactics; high armored mobility and rapid, shifting engagements (Luttwak and Horowitz, 1974; Safran, 1978). This doctrine required the Israelis to have large armored formations ready to move when the Arabs struck, i.e., it depended on adequate warning and mobilization. Further, it assumed a war of movement; that the Arabs would attack to seize considerable terrain. In the Egyptian case the Israelis seemingly assumed the attack objectives would extend at least as far as the major passes (Gaddi, Mitla, Khatmia). Finally the Israelis assumed that, because the fighting would be fast-paced and wide-ranging, Arab infantry would not keep up or would coordinate poorly and would pose little threat to the Israeli all-tank formations. Arab infantry would in any case be easily dealt with, the Israelis believed, especially since the Israelis would have air superiority over their counterattacking forces and since the Arab soldier was known to be a good defensive fighter but a poor attacker. All these assumptions were wrong in the Sinai, and most were wrong on the Golan.

The Egyptian plan stressed surprise explicitly to keep the Israeli armor out of the fight until the bridgeheads over the Canal were established and Ismail's tank and air defense traps were ready. Ten hours after the attack began the Egyptians had pontoon bridges and ferries in operation

(half the time estimated before the war by Israeli intelligence), anti-tank guided missile (Sagger ATGM) and RPG-7 antitank rocket teams deployed; and SA-2, SA-3, SA-6 and SA-7 SAMs and gun antiair defenses sealed off the Egyptian field forces from the IAF. When the Israeli tanks counterattacked on 7 October, Ismail's "meat grinder" defenses knocked out over 140 and General Gonen's crippled spearhead withdrew back into Sinai (Monroe and Farrar-Hockley, 1975; 22). "It was not until the afternoon of Monday, 8 October," write Luttwak and Horowitz (1975: 353), "that Mendler [Sinai Armored Commander], Gonen [Southern Commander] and their staff understood the new threat posed by the Egyptians."[3] From the initial crossings on 6 October until late on the 8th, Military Intelligence kept informing Gonen that the Egyptians were ready to "break" at any moment. Gonen's repeated orders of attacks can be attributed in part to the continued underestimation of the enemy by military intelligence (e.g. Dupuy, 1978: 428, 431).

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[3] Safran (1978: 167) writes

> there was much amiss behind the fact that Israeli armor was so heavily punished by the enemy's antitank missiles. The relevant Israeli military authorities knew the characteristics of those weapons, were aware that the enemy possessed them in large quantities, and had even devised ways to combat them; but somehow the practical conclusions of this information in terms of the composition of forces and battlefield tactics were not followed up ...

Israel had underestimated the effectiveness of the Arab defensive weapons and tactics and had assumed Arab tactics would be offensive. Although the Israelis lost tanks to the Arab ATGMs before (as early as July 1967), they continued to assume their tactics could deal with this threat and that the low quality of Arab infantry ruled out an effective, integrated antitank capability. Luttwak and Horowitz (1974: 316) label this "a basic error of judgment." Nor did the Israelis anticipate the Arabs' effective use of night vision devices in night operations. In generally assuming the Arab attack could not get started, that the Bar-Lev defenses and tanks would prevent a Canal crossing, that Egyptian water-crossing efforts would be awkward and slow, the Israelis were unprepared to deal with the defensive tactics they faced in Sinai. And while they were better prepared to face the Syrian thrust (which was similar to what the Israelis anticipated) the Israeli forces were still surprised by the tactical integration and weight of the Syrian attack, its effective air defenses and tank-killer infantry tactics, and the Syrian night fighting. The "all-tank" concept, so effective in 1967, was obsolete in 1973.

(4) "The invincible IAF": The story of the IAF success over the Arab air forces in 1967 had but one peer (the Nazi destruction of the Soviet Air Forces in a single day in 1941). No one on either side anticipated the remarkable surprise and effectiveness of the Israeli pilots' preemptive strikes. The overall success of the IAF in the

War of Attrition and several impressive dogfight victories before the 1973 War (e.g., on 30 July 1970 the Israelis shot down four Soviet-flown MIG-21Js in a single encounter, on 13 September 1973 Israeli jets shot down thirteen Syrian fighters while losing a single plane) reinforced the collective impression that the IAF could not be defeated, that Israel would have air superiority over the captured territories, and that Israeli counterattacks on the ground would have full air support. Only the first of these assumptions proved true.

The IAF was not defeated, but the Arabs mobile SAMs and guns, which thrust forward a defensive umbrella over the attacking ground forces, prevented the IAF strafing attacks needed to keep the Arab antitank forces from trapping Israeli armored columns. Furthermore, the surprise of the attack permitted Egyptian jets to make short, sharp strikes at the Israeli tanks, artillery, and communications which supported the Bar-Lev defenses. Although lacking air superiority, the Egyptian's jets nevertheless contributed to the Canal assault, then swiftly ducked behind the air defense screen. When the IAF finally responded, it confronted an unexpectedly dense air defense network. The integration and complex organization of the Arab air defenses and its ability to fully absorb and dilute the IAF, comprised one of the unexpected elements of the Arab challenge to IAF superiority. The IAF plan for dealing with the Arab air defenses required total concentration on this task, ruling out ground support. The critical need to block the Arab ground attacks prevented this plan from being put into effect. Other surprises

were the massive, simultaneous Syrian and Egyptian thrusts which decoyed the IAF from Sinai to the Golan, and the many technical surprises posed by the electronic capability of the Arab SA-6 and ZSU-23/4 radars and infrared guidance system of the SA-7. Only at a high cost in time, planes and pilots did the IAF learn to counter these capabilities, and only after the ground forces overran the SAM sites could the IAF again fly with impunity.

While the incredible IAF successes of the 1967 and early War of Attrition supported the "invincibility" assumption, events from late 1970 to October 1973 should have weakened the dependence on IAF total superiority. The success of the IAF against the Egyptian and Syrian air forces during the War of Attrition, especially the bombing attacks near Cairo, led to a massive influx of highly sophisticated Soviet SAMs, guns, radars, and interceptors, forcing the Israelis to halt its widespread bombing so as to avoid directly confronting Soviet crews (Shlaim and Tantner, 1978). Gradually through 1970 the Soviet-Egyptian SAM belt was pushed closer and closer to the Canal. In late 1972 and early 1973 the Soviets added the ZSU-23/4, SA-6, SA-7 weapons (to the extent of stripping Soviet units) to the older systems already in Arab hands, thus completing an air defense belt that extended over both sides of the Canal.

(5) "Decoy Phenomena." Turner notes that "decoy phenomena" may draw attention from more serious problems. In the case of the October War, several Arab deceptions and a few coincidences provided decoys for Israeli attention.

The most successful Arab deception was the high level of tension and military mobilization and exercises they created in the spring of 1973 which caused Dayan and Elazar to overrule Ze'ira and order a partial mobilization at great expense to Israel. Since no attack followed, Ze'ira's estimate that the Arabs had had no intention of attacking was confirmed. Sadat's war of nerves dulled Israeli sensitivity to the Arabs' attack preparations and rehearsals, and created the "wolf cry" which strengthened the most complacent Israeli intelligence figure.

The frequent conferences of senior Egyptian, Syrian, and Jordanian political and military figures in the spring and summer of 1973 was attributed by the Israeli Military Intelligence Branch to the reopening of Jordanian diplomatic relations with Egypt and Syria. Consequently the shift of Syrian troops from the Jordanian border to the Golan line was seen in Israel as a goodwill gesture by Assad to Hussein, rather than a buildup prior to an attack. Similarly, the Syrian reinforcement of tanks and artillery in late September was explained as a reaction to the Israeli-Syrian dogfight on 13 September in which thirteen Syrian jets were shot down.

On 26 September Egyptian Foreign Minister Zayat arrived in Washington to reactivate the American mediation role in the Middle East. On 28 September Arab terrorists held up a trainload of Russian Jewish immigrants enroute to Vienna and demanded that the Jewish transit camp at Schoenau be closed. The Austrian acquiescence to these

demands became major concerns in the Israeli Cabinet and Prime Minister Golda Meir was in Strassbourg and Vienna from 30 September to 2 October. Zayat's efforts were part of Sadat's deception plan, the terrorist incident may have been although perhaps it was fortuitous. Israel's preoccupation with international and domestic terrorism between the 1967 and the 1973 wars was a constant distraction for the military and the Cabinet.

Finally, Sadat's plan also included the more traditional tactical deceptions: all senior commanders were ostensibly to go on religious pilgrimages on the day of the attack; landline communications were installed along the Canal to permit the attack planning to proceed while the Egyptian radios were used to discuss (for the benefit of Israeli eavesdroppers) "Exercise Tahrir 41," the supposed cause of extensive Egyptian military activity along the Canal. While not all of these ploys succeeded in fooling the Israelis (Heikal, 1975: 17, implies most of them failed) they tended to make it easier for Ze'ira to continue defending the "conception" when the contrary signals became stronger as war approached.

(6) "Rejection of Outside Advice." Turner writes that advice and warnings of danger by nonexperts outside the organization often precede disasters and are rejected by the organizational insiders whose duty it is to forestall the disaster. The erroneous assumption in these cases is that the organization knows the situation best and is better able to evaluate information than amateurs or outsiders.

The Israeli Military Intelligence Branch received many warnings from "outsiders" which it rejected. Within a month of Egypt's issuance in May 1973 of the plan for the October attack, "Operation Badr," the U.S. CIA had obtained a copy and passed it to the Israelis. Both agencies concluded at the time that an attack was unlikely. Junior intelligence officers as well as Dayan and Elazar warned of an attack in April and May after Sadat began major war exercises, triggering the partial premature mobilization. In September, Dayan, Elazar's deputy, as well as the Northern Commander questioned Ze'ira's estimate on Syrian intentions. The U.S. intelligence community warned of a possible joint Syrian-Egyptian attack. The head of Israeli Naval intelligence concluded war was in the offing since the Soviets had begun removing their ships from the Egyptian port of Alexandria and had stepped up patrols by intelligence collecting trawlers off Israel's coast. The unusual military movements along the Canal in late September and early October led several junior intelligence officers in the Southern Command to predict war but these predictions were excised from the Southern Command reports to the General Staff (Handel, 1976: 34; Ben-Zvi, 1976: 393; Shlaim, 1976: 354). Golda Meir noted the similarity between the departure of Soviet advisors and families on 4 October and a similar exodus prior to the 1967 War: the implication of her observation was generally ignored (Handel, 1976: 63; Meir, 1975: 408). Both lower echelon (junior officers) and higher echelon (Northern Commander,

Dayan, Meir) warnings were ignored by the insiders in the Military Intelligence Branch. Nor were amateur warnings lacking; predictions of war in the autumn were made as early as May by Gabriel Cohen and Yair Evron, commentators on Israel's international relations.

B. Information Handling Difficulties. Although mishandling of information was not a primary factor in the October War disaster, several instances of information handling difficulties aggravated the situation. Perhaps the foremost problem was the monopoly on intelligence evaluation of the Military Intelligence Branch, and its role as sole guardian of intelligence received from abroad and from other Israeli intelligence gathering agencies. This created a bottleneck in the flow of signals from below and outside to the Cabinet-level decision-makers, fostered the "conception" and reinforced it by self-censorship, such as the deletion of war warnings by Southern Command. The Agranat Commission conclusion is quoted by Shlaim (1976: 370):

> independent evaluation of political strategic, operational and tactical intelligence was prevented through centralization in one organization and under one authority on the one hand and the absence of a special intelligence advisor to the Prime Minister on the other ...

Perhaps the most paradoxical effect of this centralization is the apparent fact that the mounting signals from the Southern Command were not integrated with those from the Northern Command. Such integration of information from disparate sources is one of the foremost

justifications for centralized intelligence systems. Herzog (1975: 278 quoted in Handel, 1976: 58-9) commented of the October War, however:

> "one of the incredible facts of the period is that at no stage and at no level, so far as can be evaluated from available materials, did any element link the Syrian buildup in the north with the unusual Egyptian activity and concentration in the south."

Handel's explanation (p. 59); a "bureaucratic miscoordination between the Egyptian and the Syrian desks of Israel intelligence headquarters," seems plausible especially in light of the U.S. warning of a joint attack on 24 September.

Handel (1976: 56) notes a second difficulty in handling intelligence, the one-way flow of information from bottom to top. In other words, field tactical units collected and passed on information to General Headquarters and the Military Intelligence Branch but received little intelligence support in return: "It lacked information, maps, air photographs --all of which were readily available to higher echelons." When the high command and the Cabinet finally agreed war was imminent warnings failed to reach the tactical units which were struck first: detachments on the Bar-Lev line were not warned and some soldiers manning the Mount Hemon lookout posts were sunning themselves when the Syrian artillery opened fire (although the Northern Command defenses were far more alert and prepared than the Southern Command and Bar-Lev defenses). Israeli artillery and tanks behind the Bar-Lev

line were destroyed by the Egyptian air and artillery attacks and Israeli communications stations in Sinai were destroyed. Even with the limited warning the Israelis had the lowest levels were not alerted. As the Israelis attempted to halt the Arab offensive the breakdown in communications aggravated their already acute tactical problems. Safran (1978: 145) noted the extent of this:

> The Israeli forces had an excellent, highly sophisticated system of communications designed to keep the various levels of command fully informed ... yet the system somehow failed again and again to work as intended ... the divisional command and other higher echelons were under the misconception that the armored counterattacks (on October 6) were succeeding on the whole, and had an erroneous impression of the magnitude of the losses they were suffering.

The combination of the "fog of war," resulting from the failure of tactical communications, and the unexpected course of the Arab attack generated widespread confusion and frustration in the Israeli high command and produced frequent disagreements on tactics and strategy.

C. Lack of Accepted Standards and Precautions: Turner's disaster model postulates that formal and active precautionary steps which might head off disaster are usually not taken by responsible agencies and out-of-date procedures and violations of formal rules come to be accepted as normal. Ad hoc solutions and remedies are applied in piecemeal and uncoordinated fashion to novel situations and problems. A lack of formal precautions characterized several aspects of the Israeli disaster, primarily the failure to plan to fight more than one type of war.

The Israeli Cabinet relied heavily on their intelligence warning system even though the adequacy of this system to predict an Arab attack had been questioned at the highest level. Israeli defense plans were premised on adequate warning which, in turn, was predicated on the operation of technical intelligence mechanisms (whose details have not yet been publicly explained). Yet the IDF mobilization in the spring of 1973 ordered by Dayan and Elazar indicates they were not complete believers in the adequacy of the Military Intelligence Branch's precautions. The adequacy of any warning system could not be realistically evaluated unless the Arabs often attempted to attack Israel; the fact that Dayan and Elazar did not share the confidence placed in the warning mechanism by others was only reasonable given this inability to actually test the system. However, no formal mechanisms existed (1) to arbitrate disagreements about warnings between the Defense Minister and Chief of Staff on the one hand, and the intelligence chief on the other, and (2) to determine what mobilization or preemption should follow the receipt of certain warning. Formal standards seemed lacking for both the warning and the operational elements of Israeli defense plans. In the first case, after Dayan and the Northern Commander questioned Ze'ira's estimate of Syrian intentions on 24 September Dayan reinforced the Golan defenses despite Ze'ira's complacent evaluation. Similarly, after the evacuation of the Soviets on 3 and 4 October, Dayan and Elazar ordered

a "C" level alert for the army (which did not call up reservists) and a full alert for the IAF (which mobilized reserves). (The Israeli Navy was put on a war footing on October 1.) Nevertheless Ze'ira persisted in his estimate that the probability of war was "lower than low." Since the Cabinet accepted this appraisal Dayan and Elazar could take no further steps.

The Israeli preemption and mobilization plans, based on the assumption of 48 hours warning, left the Israelis having to improvise when the warning mechanism failed. No contingency plans provided for the actual events: a warning that the attack would come in 15 hours (in fact, the attack came in 11 hours). That is, when faced with a far less timely (and even then, inaccurate) warning than expected, the Cabinet was forced to debate what steps to take. Dayan and Elazar differed on mobilization and preemption: Elazar favoring all-out mobilization and a preemptive air strike, Dayan a partial mobilization and no air strike. Dayan favored warning Egypt and Syria that their attack plans were known and they had lost surprise, in hopes of forestalling them; Elazar was opposed. The debate was taken to the Prime Minister and the issues resolved by 0930; that is over three hours were lost in last-minute arguments over the Israeli response. (The Israeli Navy, in contrast, was ordered to battle stations at 0500, only 2 hours after the "certain warning" signal was received. It, however, had been expecting war.)

The Israeli defense plans called for the ground forces (presumably forewarned and completely mobilized) to contain the Arab ground attack while the IAF (presumably aided by striking preemptively, that is, before the Arab attack kicked off) would devote itself exclusively to destroying the Arab air defenses and air forces (Safran, 1978). By October 1973, however, it had become politically impossible for the IAF to preempt as it had so effectively in 1967. The U.S. reaction to the IAF's deep bombing strikes into Egypt during the War of Attrition had made clear to the Israeli Cabinet that Israel would have to absorb the first blow or risk losing U.S. support (Shlaim and Tantner, 1978). These political realities seem not to have much influenced the IDF's precautions; when the October attack began the IAF was immediately pressed into support of the failing Israeli ground defenses, and was too preoccupied with halting the ground attacks to deal effectively with the air defense threats. The original plan for IAF invincibility was useless, there were no others, and the IAF was committed to battle day-by-day, even hour-by-hour until the Israelis had turned the tide. Similarly, Israel's plans for her armored forces were inapplicable (because the attack came unexpectedly) and inflexible and unworkable (because the attack that came was not what was expected). The ground forces were forced to learn how to fight the war under fire.

This lack of tactical precautions was not due to a lack of an understanding of the Arabs weapons or the capabilities of the weapons. Nor was it due to Israeli ignorance of the tactics the Arabs would employ (which were largely Soviet-based). Indeed, between the 1967 and 1973 wars the Soviets' military tactics were a major concern of the IDF, a source of many IDF doctrines, and in the case of Dayan, a virtual preoccupation (Luttwak and Horowitz, 1974). Nevertheless, the Israelis obviously failed to prepare themselves for the possibility that the Arabs might use Soviet weapons as the Soviets intended them to be used. And despite the Israelis' preoccupation with the possibility that they might have to fight a Soviet intervention force, the events of October show no indication that the IDF was prepared to do so. Handel (1976: 47) concludes, for example, that the IDF 'planned in a vacuum, not realizing the new battlefield weapons' devastating impact on their freedom of action." Had the Russians been dragged into or 'volunteered for" the fighting (Safran, 1978: 139 writes that Dayan anticipated this possibility) the IDF would have faced the same devastating tactics executed not only by the students but by their masters as well. Israel's greatest military successes resulted when the IDF forced the Egyptians to fight a war of movement and foresake the Soviet defensive tactics that worked so effectively in the Sinai.

As in so many other respects the story of precautionary failures does not apply to the Israeli Navy. After the 1967 war this force was

completely rearmed with Gabriel cruise missiles mounted on Saar-class and Reshef-class fast attack boats and its tactics were revamped to deal with the Soviet supplied Osa- and Komar-class missile boats in the Syrian and Egyptian navies. The IN was totally successful in dealing with the enemy's tactics and equipment in the October War, destroying 19 Arab vessels including 10 missile boats without suffering a single loss (Safran, 1978: 166).

D. Minimizing the Emergent Danger: The last element of the incubation stage of a disaster is the tendency to assume invulnerability, underestimate hazards, and overestimate the capabilities for dealing with them. When the occurrence of the disaster becomes undeniable, emergency measures are applied "too little and too late." Fears of false alarms prevent alarms being sounded. Disagreements about the significance of evidence pointing to danger leads to the undervaluation of warnings, particularly if the more complacent group is most powerful. Each of these problems contributed to the Israeli disaster.

The Israelis clearly minimized the danger of an Arab military attack, so much so that in assuming an attack would be suicidal they assumed as well that the Arabs would not attack. Because the IDF evaluated the Arabs as incapable of coordinated, surprise, and swift assaults both the Golan and Bar-Lev defense lines were thin relative to the forces facing them. When lower and higher echelons and outsiders

questioned his estimate of the situation, Ze'ira stoutly "explained away" evidence discrepant with his position: Syrian reinforcements were a goodwill gesture to Jordan or a reaction to the Syrian Air Force defeat, Egyptian activity was a multi-division exercise, conferences of senior Arab commanders were efforts to reduce disunity and restore diplomatic relations. The financial costs of mobilization and the political costs of preemption prevented the cabinet from responding to these warning signals. When the "certain" warning signal finally arrived, a preemptive air strike was not launched, and the general mobilization was so hurried that forces had to be committed to battle piecemeal and underequipped.

Stage 3: Precipitating Event. This event, according to Turner's disaster theory, concludes the incubation period and: (1) is unpredictable in the sense that it is generally unexpected (although many may have forecast it), (2) it cannot be disregarded, because its immediate consequences and its unexpectedness arouse attention, (3) it reveals the latent structure of the events of the incubation period, uncovering a different set of properties of these events than were previously assumed (which further motivates attention), (4) it creates the need for a reexamination and reinterpretation of the events of the incubation period, (5) it reveals unpreparedness and failures of foresight, (6) it is ultimately found to be linked with many of the chains of discrepant events in the incubation period.

Turner (1976b: 761) asserts that the precipitating event "makes it inevitable that the general perception of all the discrepant events in the incubation period will be changed."

Precipitating events and their following physical consequences may be either "instantaneous" or "progressive" (Turner, 1976b: 763), i.e., in the former case the event is followed immediately by disruptive consequences, in the latter case a series of precipitating events follow each other and produce a series of surprises and successive readjustments. The Yom Kippur disaster was of the second type; the Israelis faced a series of surprises: the attack itself, the Arab alliance, the increased effectiveness of the Arab tactical techniques and fighting forces. These surprises had separate precipitating events which kept the Israeli leaders and the IDF command off balance for several days. This "progressive" character of the disaster made it all the worse; as each surprise was comprehended and readjustments made, another came along bringing more confusion and dismay, creating the impression of an ever expanding catastrophe. Many commentators and witnesses and virtually all the major Israeli leaders noted the acute and widespread depression in Israel resulting from the Arab surprises. Golda Meir, for example, wrote (1975):

> The shock wasn't only over the way in which the war started, but also the fact that a number of our basic assumptions were proved wrong ... the circumstances [in the opening days of the war] could not possibly been worse (p. 412) ... [T]he word "trauma" ... most accurately describes the national sense of loss and injury [that persisted through and after the war] (p. 437).

Stage 4: Onset. The immediate consequences of the failure follow the precipitating event(s). These occur at varying rates and intensities and over areas of varying scope. This stage initiates Stage 5, salvage and rescue operations.

The onset of the consequences of the Yom Kippur disaster started with Ze'ira's "certain warning" in the early morning of October 6, immediately shaking the faith of Israel's military elites in the capability of Military Intelligence. However, the consequences of the various Israeli failures continued to emerge throughout the war and extended beyond the military and the war itself into Israel's diplomatic, economic, social and political affairs.

Stage 5: Rescue and Salvage: Turner describes this stage as the "first stage adjustment" (1976a: 381), the application of ad hoc adjustments to the post-collapse situation so that the major features of the failure can be recognized and dealt with. He writes (1976b: 763) "prolonged analyses are not undertaken, but only the minimal recognition of changed circumstances necessary to deal with the immediately pressing problems." Some of the afflicted cannot make even these adjustments and continue to deny the failure.

The story of how the Israelis improvised their defenses in the face of the Arab assaults and regained the military initiative in the October War bears witness to the adequacy of their rescue and salvage efforts. The details of this readjustment have little bearing on the

application of disaster theory to intention estimation failures. Nevertheless, the ad hoc nature and uncertainty characteristic of this stage are reflected in several events, e.g., Dayan and Elazar's differences on mobilization and preemption, Ze'ira's repeated predictions on October 7 and 8 that the Egyptians were ready to run from Gonen and Mendler's tank charges, the piecemeal commitment of reserves to the Golan defenses, the shifting tactics of the IAF. Only the Navy seems to have stuck to its pre-war plans and tactics.

Stage 6: Full Cultural Readjustment. Turner's theory predicts that, once the immediate effects of the disaster subside, a major inquiry or assessment is conducted, and beliefs and precautionary norms are adjusted to fit the newly gained view of the world. The knowledge which permits these revisions was available prior to the disaster but its significance unrecognized; the catastrophe transforms it into new configurations. The inquiry results in cultural redefinitions aimed at restoring adequate precautions and norms so that the disaster "can never happen again." The success of this effort depends on the agreement among concerned groups on the effectiveness of the proposed readjustments. Turner notes that such inquiries tend to uncover error chains which did not cause the disaster itself, but which could eventually have done so had the disaster not occurred when it did. He also observes (1976a: 339) that such inquiries deal with

> the problem that caused the disaster as it was later revealed and not as it presented itself to those involved beforehand. The recommendations, therefore, treat the well-structured problem defined and revealed by the disaster, rather than with preexisting, ill-structured problems.

The Yom Kippur disaster was assessed by the Agranat Commission, formed to investigate the intelligence failures before the war as well as the conduct of the Israeli military leadership. The Commission attributed the intention estimation failure to (1) the stubborn adherence of Military Intelligence to the "conception," (2) the false guarantee of 48 hours warning, and (3) the incorrect evaluation of evidence, contrary to the "conception," which suggested an Arab offensive. Shlaim (1976: 354) notes that the Commission explicitly attempted not to fall victim to the tendency to be wise after the event, but he concludes that

> to some extent they succumbed to the all too human tendency of distinguishing more clearly between signals and noise with hindsight than would have been possible at the time; they dwell on the signals which after the event are clearly seen to have heralded the attack; paying insufficient regard to the plethora of conflicting signs which pointed in the wrong direction.

Chan (1977), Betts (1978), and Fischhoff (1975) have similarly noted the limitations imposed by this "hindsight bias" (as well as other biases) on the utility of retrospective inquiries. This bias is examined in detail in Section 8.

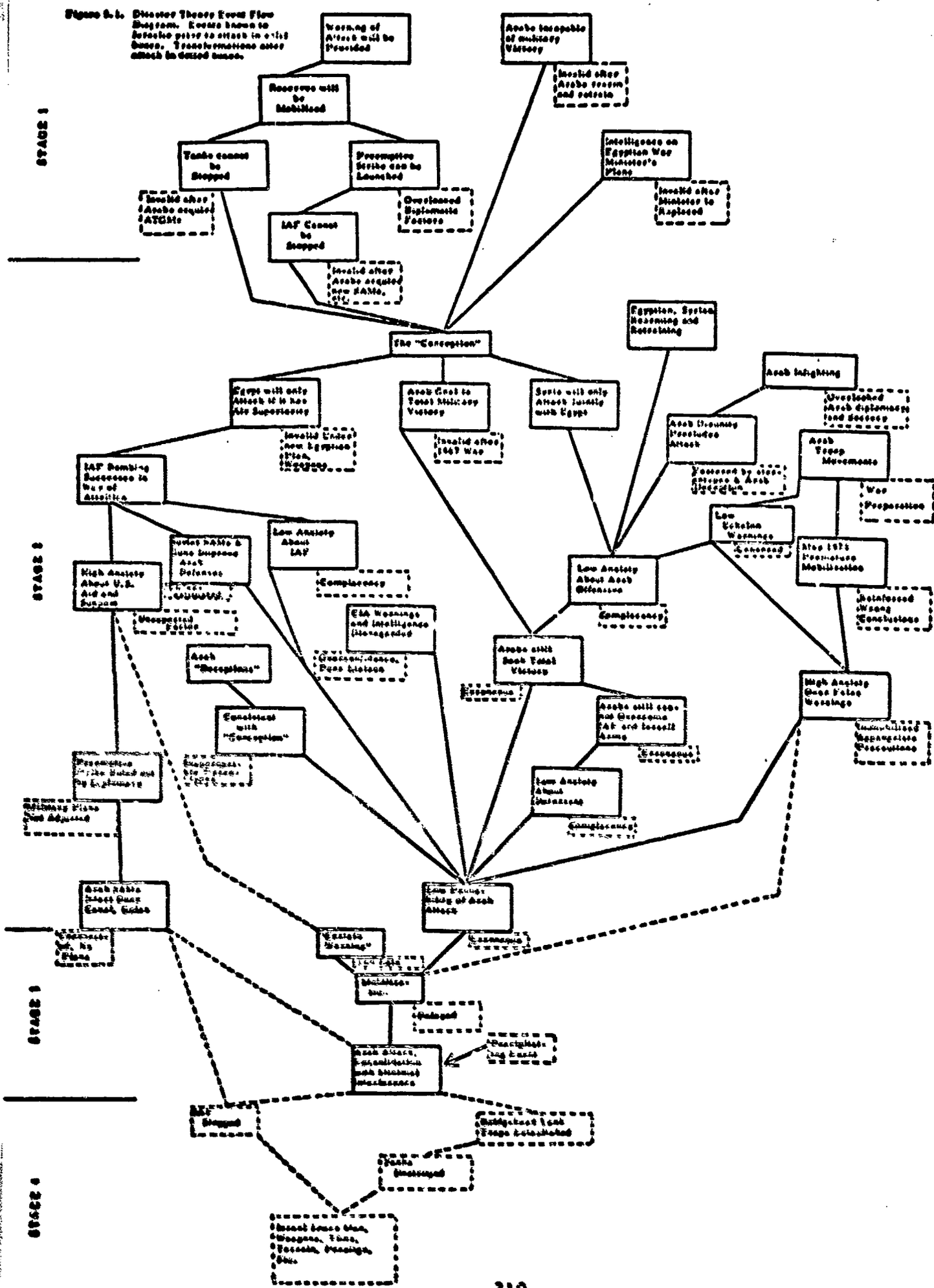
The Event Flow Diagrams

The six stage sequence model applied above to the Yom Kippur Israeli failure to correctly estimate Arab intentions can be supplemented with a flow diagram of the events leading up to the onset of the disaster.

Figure 5-1 is a highly schematic map of the events known to the Israelis prior to the war (shown in solid boxes and lines) as well as the transformation of this knowledge, or the latent events, which the precipitating event revealed (dotted boxes and lines). [The detailed schematic on which Figure 5-1 is based is available from the author.]

In Stage 1, the notionally normal stage, shows the beliefs regarding the Arabs' intentions before the discrepant events of the incubation stage began to accumulate. At this stage, the norms and beliefs of the Israelis were appropriate, but only so long as the Arabs made no changes. For example, the Israeli assumption that the IAF could not be stopped was accurate in the 1967-1970 period, but became increasingly less realistic as the Arabs acquired the Soviets' most modern air defenses, and Soviet-Egyptian SAM crews moved the air defense umbrella up to the Canal. While the Israelis were well aware of these changes, they seem not to have realized the implications on their overall assumptions and plans.

Stage 2 shows the incubation of numerous error chains as various events accumulate which are discrepant with the existing beliefs, norms, and precautions. The accumulation of these events produces more


STAGE 1
STAGE 2
STAGE 3
STAGE 4
The "Conception"
Egypt will only Attack if it has Air Superiority
Arab Unlikely to Total Military Victory
Syria will only Attack Jointly with Egypt
Egyptian Syrian Reasoning and Forecasting
Arab Infighting
Overlooked Arab diplomacy and Secrecy
Arab Disunity Precludes Attack
Arab Troop Movements
Assessed by Israel Defense & Arab Unreliable
War Preparation
Low Scholastic Warnings
Assessed
Also Lists Premature Mobilization
Misinterpreted Wrong Conclusions
IAF Bombing Successes in Way of Assaults
Invalid Under new Egyptian Plan, Weapons
Invalid after 1967 War
Low Anxiety About IAF
High Anxiety About U.S. Aid and Support
Complacency
Unexpected Factor
IDF Warnings and Intelligence Disregarded
Arab "Deception"
Inconsistent with "Conception"
Low Anxiety About Arab Offensive
Complacency
Arabs still Seek Total Victory
Arabs still Hope and Overcome IAF and Israeli Arms
Low Anxiety About Unreasons
Complacency
High Anxiety Over False Warnings
Unjustified Aggressive Precautions
Arab Attack, Consolidation and Unlimited Maintenance
Participate and Assist
Stopped
Withdrawal Until Troops Demobilized
Unstopped
Israel Loses Men, Weapons, Time, Terrain, Warnings, Etc.

competing estimates of the Arab intentions which are rejected, disregarded, or censored. The implications of combinations of various error chains are not noted, e.g., the increased Arab air defenses, diplomatic concern over preemptive air strikes, and the dependence of Israeli defense plans on IAF air supremacy seem not to have been considered in combination.

The first precipitating event (the "certain warning" in the early hours of October 6) ends the incubation period and begins to reveal the latent structure underlying the discrepant events of Stage 2. For example, the Arab troop movements over which much controversy had revolved from May through October were now clearly attack formations and not routine defense or training exercises, as had been accepted.

Stage 4 represents the onset of the disaster, the immediate consequences of the failure begin occurring. For example, the IAF found it could not totally destroy the Arab air forces and air defenses (as it had in 1967) and it had to focus on supporting the ground forces despite its heavy losses to the Arab SAMs and guns. The Arabs had stopped the IAF from dominating the battlefield and the skies.

SECTION 6

INTELLIGENCE FAILURES, DISASTERS, AND KUHNIAN SCIENCE

INTELLIGENCE FAILURES, DISASTERS, AND KUHNIAN SCIENCE

> I see no reason to believe that political decision-makers are less rational, sophisticated, and motivated to understand their environments than are scientists.
>
> Robert Jervis, 1976

> 'Pon my word, Watson, you are coming along wonderfully. We have really done very well indeed. It is true that you have missed everything of importance, but you have hit upon the method.
>
> Sherlock Holmes, "A Case of Identity," 1891

The theory of disasters outlined in Section 5 as a tentative theory of intelligence failures is related to Kuhn's analysis of science. That is, the stages of disaster resemble the stages of scientific progress and revolution. This section argues that pre-revolutionary science, pre-disaster institutions, and pre-failure intelligence agencies share universal problems of observation and analysis. Intelligence failures may result from an effort to be "scientific" in the analysis of intelligence data, but intelligence successes also seem to result from a "scientific" approach.

The Traditional View of Science: In the conventional, everyday image of science progress is made by moving from superstitious ignorance toward final scientific truth by the successive accumulation of facts. Scientific progress is built fact by fact, much as a bricklayer builds up a wall, or in the image favored by intelligence, fitted together like a jigsaw puzzle or a mosaic. In this view, science is a strictly logical process. Scientists propose theories based on inductive, logical observations of nature. They then confirm or refute by experimental

tests hypotheses drawn deductively from these theories. When hypotheses are refuted old theories fail and new theories and hypotheses are proposed and adapted because of their greater explanatory power. Through this process of rejecting the false and adapting what is experimentally true, science slowly and surely completes the entire picture of natural reality.

The Kuhnian View of Science: In the past few decades historians and the philosophers of science have utterly discredited this commonplace image of scientific methods. These scholars of the scientific process no longer view science as the heartless pursuit of objective information through experimental falsifications of erroneous hypotheses, but rather as a creative, intuitive human activity, shaped by its historical context and the psychologies of the scientists. In this view the theories and beliefs about reality of scientists are not changed as a consequence of new discoveries or experiments. New theories are adopted because they are more complex than old theories, not because they are any closer to reality. Instead of a cumulative acquisition of knowledge, this view sees science as a series of peaceful interludes punctuated by intellectually violent revolutions. The pattern of peaceful, normal science evolving toward a violent, revolutionary switch from one theory to another parallels Turner's (1976a, b) descriptions of disaster development. The foremost exponent of this modified view of science is Thomas Kuhn (1970).

Kuhn argues that during the peaceful interludes between revolutions scientists' work is guided by a set of consensible (potentially affirmable) and consensual (in fact affirmed by most qualified scientists) theories, standards and methods. Kuhn refers to this agreed upon set as a "paradigm."

The paradigm defines which problems are interesting and which are not, which methods are reliable, which data are worth seeking. The paradigm is the basis of a research tradition: "men whose research is based on shared paradigms are committed to the same rules and standards for scientific practice" (Kuhn, p. 11). These paradigms govern the peaceful interludes of what Kuhn terms "normal science."

The paradigm provides an exploration plan for the investigation of nature's complexities. It points to important puzzles and guarantees that, if the paradigm is followed, the parts of the puzzle will be filled in. Because the paradigms of normal science do work, they attract adherents from other, competing, scientific activities. And because the puzzles the paradigm suggest are sufficiently open-ended, normal science engages scientists usefully and profitably. Because the paradigms of normal science provide scientists with a means of consensual discourse, progress is rapid in these periods of normal scientific activity. Many of the jigsaw puzzles are satisfactorily fitted together. A paradigm protects science from accepting as "scientific facts" data which are only coincidental and artifactual.

Kuhn argues that these peaceful eras of normal progress do not last. Scientists eventually attempt to extend the paradigm to puzzles which they cannot solve. These efforts yield anomalies which the paradigm cannot solve or even explain. Often such anomalies are present from the outset of the paradigm but are ignored because they are inconvenient in the effort to reach an agreed-upon set of operating principles. In fact, during normal science, scientists often try to suppress novelties so that time and effort are not wasted on unpromising paths. Yet against the backdrop of the paradigm, the anomalies begin to accumulate and

stand out. Eventually they cannot be ignored and the paradigm enters a stage of crisis. In the language of Turner's disaster theory, normal science provides the first two stages of initial beliefs and norms (paradigm) which leads to the incubation period (the gradual accumulation of anomalies, discrepancies, and events which are at odds with accepted explanations).

Normal science, Kuhn argues, is not only successful in developing efficient and effective solutions to the puzzles which it poses to itself, but it is also a "uniquely powerful technique for producing surprises," (Kuhn, p. 52) i.e., new and unsuspected phenomena which normal science itself cannot immediately assimilate. While the field will ignore or suppress these surprises, some scientists are nevertheless aware of the anomalies, and will begin an extended exploration of the area of the anomaly. Gradually, there is an observational and a conceptual recognition of the anomaly by these few scientists and a change of paradigm categories and procedures to extend the exploration of these novelties. These adaptations are often accompanied by resistance from those scientists who suppress novelty for the sake of agreement on the scope of the paradigm, and its protection from unsound and ephemeral "discoveries."

The "crisis" of normal science occurs when the study of the anomalies begins to demonstrate that the paradigm is failing to solve its own traditional problems and is increasingly vague and decreasingly useful in solving the puzzles it has developed. When several pronounced failures of normal puzzle-solving emerge, a proliferation of new theories, in direct response to the failures, takes place. Normal scientists then turn increasingly from puzzle-solving to the discussion of fundamentals, i.e., the paradigm itself. New paradigms are suggested

which build on the works of those who have explored the anomalies of the old paradigm. In short, the crisis of the old paradigm occurs when problems which had long been recognized by the paradigm and were believed to be solvable are found to be in fact unsolvable within the paradigm. But more important (because puzzles are never, even in "normal" times, easily solved) to the onset of crisis is the discovery of normal scientists that the solution to the unexpected failures had been anticipated, at least partially, during the period when there was no crisis, by those scientists who had focused on unexplained, extra-paradigmatic, anomalies. The solutions are seen to have been present before the crisis generated by the unexpected failures of the paradigm, but were ignored. Because the new paradigm seems to explain better, or explain more, it wins converts and the old paradigm loses its grip on scientists.

The unexpected and unanticipated failure of the paradigm to solve a problem of the sort it had traditionally solved can be related to Turner's "precipitating event" and "disaster onset." A precipitating event is (1) immediate and physical, cannot be disregarded, and is unambiguous, if perhaps confusing, (2) creates the recognition that new interpretations of events are needed, (3) gains force from being unpredictable and from demonstrating an unexpected incapacity in the old methods of understanding. In the "disaster onset" the consequences of failure occur and the collapse of cultural precautions become apparent.

Kuhn argues that when confronted with even severe and prolonged anomalies scientists never renounce their paradigm and treat anomalies as counterexamples. Kuhn directly challenges the falsificationist theory of science propounded by Karl Popper, which holds that theories cannot

be confirmed, only refuted and, when refuted in any serious instance, are abandoned. Kuhn argues that theories are never rejected, they are exchanged for more satisfying theories:

> Once it has achieved the status of paradigm, a scientific theory is declared invalid only if an alternative candidate is available to take its place. No process yet disclosed by the historical study of scientific development at all resembles the methodological stereotype of falsification by direct comparison with nature... The decision to reject one paradigm is always simultaneously the decision to accept another, and the judgment leading to that decision involves the comparison of both paradigms with nature and with each other. (Kuhn, p. 77)

The consequences of crises as described by Kuhn are quite similar to Turner's description of the "full cultural readjustments" that occur in the wake of disasters. Kuhn notes (p. 84) that the effects of crises may not be recognized consciously by scientists. The transition to a new paradigm is sudden and complete: "paradigm changes ... cause scientists to see the world ... differently" (p. 111). Kuhn uses the visual gestalt shift as an "elementary prototype" of what occurs in full-scale paradigm shifts: the scientists do not see something as something else, instead, they simply see it.

> Looking at the moon, the convert to Copernicanism does not say, "I used to see a planet, but now I see a satellite." That locution would imply a sense in which the Ptolemaic system had once been correct. Instead, a convert to the new astronomy says, "I once took the moon to be (or saw the moon as) a planet, but I was mistaken." (Kuhn, p. 115)

What had been confusing and chaotic before the paradigm shift is now perceived as well-defined by the new paradigm.

Even though scientists with a new paradigm will be confident that the crisis-producing anomalies are now mastered, they still confront new anomalies. Even the evidence that ostensibly supports the new

paradigm may be unruly: as Kuhn puts it (p. 135), the scientists will have "to beat nature into line," with the new paradigm.

Science and Other Fields: Kuhn (pp. 207-40) cautions that although science may experience cycles of tradition and revolution like other fields, it is inappropriate to conclude that science itself is like other fields, e.g., art. It is appropriate then to ask how science is like disasters and how disasters and science may be like intelligence failures.

Disasters may be thought of as failures of social engineering, i.e., a failure in the application of physical and social sciences to practical everyday problems. Similarly, intelligence failures can be thought of as parts of (because players other than intelligence actors are involved in what come to be called intelligence failures) a failure of foreign policy engineering, i.e., a failure in the application of social and political sciences to practical problems of diplomacy and analysis. In all three cases of Kuhnian science, disaster, and intelligence failure, previously successful and "empirically confirmed" theories and methods are discovered to be inadequate and, after a crisis/disaster/strategic failure a new perspective develops which provides a more satisfactory explanation of events than that provided by the old theories (i.e., paradigm). In all three cases, the new paradigm tends to be anticipated before the crisis by a small minority that is ignored or suppressed by the majority. After the crisis and the adoption of the new world view, a tradition-building "normal" process begins again, in which a paradigmatic attention to scientific norms, procedures and rules guides the practitioners. The "old" confusing observations now make sense, and the "new" confusing anomalous data are ignored or suppressed, until the next crisis. In each of these three areas there is the strong

motivation to behave scientifically, to treat problems dispassionately, and to consider data objectively. What Kuhn argues, and what we are extending to two other "applied scientific" areas, social engineering and intelligence, is that the normal contexts which permit progress and successful problem-solving also produce confusing anomalies which are not easily attended or understood within those contexts until they accumulate into a crisis.

Coping with the Normal Context: The normal science protects science from spurious, coincidental and artifactual data. By explicitly judging some research as extra-paradigmatic and excluding its results from a serious hearing, normal science preserves itself for the most promising pathways. Thus normal science at the same time insures effective progress and insures that scientists will tend to overlook some anomalous research findings that will eventually lead to crisis. Is there any way around this paradox that the most efficient path seems to lead to an ultimate disaster?

Two observations offer some prospect that normal science can operate in such a way as to adapt to anomalies at the same time that it guards against extraneous, irrelevant data. That is, normal science may be able to postpone some crises by anticipating them through a gradual accommodation of the paradigm to anomalies. In essence these observations recommend alterations in the institutional practice of normal science which yield such science better (but not entirely) able to cope with anomalous results. Given that scientists (and social engineers and intelligence officers) must have frameworks, these observations suggest how scientists might see beyond the limitations on their vision that these frameworks create.

Translations: Kuhn (1965: 266) argues that the crisis of paradigms involves a process of persuasion and conversion and that this process is impeded by "incommensurable" languages. That is, the adherents to the old, "normal" paradigm, and the advocates of the new theory share much in common as scientists, researchers, and even as judges of theories. However, they do not share a common viewpoint on the matter under debate and they cannot use the language of their own theory to refute or negate the theory of the other. Although this problem becomes obvious as opponents attempt to communicate, it is more than a linguistic problem and cannot be resolved by stipulating a dictionary of troublesome terms. Kuhn (1970: 201) describes the means scientists use to overcome this problem of incommensurable languages:

> The techniques required are not, however, either straightforward, or comfortable, or parts of the scientist's normal arsenal. Scientists rarely recognize them for quite what they are, and they seldom use them for longer than is required to induce conversion or convince themselves that it will not be obtained.
>
> Briefly put, what the participants ... can do is recognize each other as members of different language communities and then become translators.

Kuhn sees this as a process of determining which terms are the foci of differences and attempting to use common terms to see how the other would respond to various controversial elements. This effort involves becoming a good predictor of the other's behavior: "each will have learned to translate the other's theory and its consequences into his own language and simultaneously to describe in his language the world to which that theory applies" (Kuhn, 1970, p. 202). While such a translation process does not automatically lead to persuasion or conversion, it seems to clear the path for both.

Kuhn discusses this translation effort as taking place in the context of crisis. If frameworks are to be made less restrictive before crises occur, this translation process would have to take place more often during the "normal" operation of science. The question then is how to "move up" translation from its usual position as a consequence of crises to a more foreward position as a routine part of normal science. [An analysis of "devil's advocates" in intelligence work in Section 10 will develop this translation concept.]

Strong Inference: John Platt (1966) recommends a form of scientific investigation which seems to include some steps toward the translation process which Kuhn describes. Platt defines strong inference as:

> ... applying the following steps to every problem, formally and explicitly and regularly:
>
> (1) devising alternative hypotheses;
> (2) devising a crucial experiment (or several of them), with alternative possible outcomes, each of which will, as nearly as possible, exclude one or more of the hypotheses;
> (3) carrying out the experiment so as to get a clean result; and
> (1') recycling the procedure, making sub-hypotheses or sequential hypotheses to refine the possibilities that remain; and so on.

Platt acknowledges that the testing of multiple hypotheses is not new but is frequently neglected in favor of "method-oriented" rather than "problem-oriented" research. Although these methods are taught widely, he argues they are not systematically applied and do not form the core of research except in a few fields of science.

A central aspect of this strong inference process according to Platt is that it takes place in "high-information" fields. These are characterized by vast detail and complexity, and research undertaken

without any pre-analysis of various theoretical outcomes would lead to years of low-payoff research. Because experiments in these fields can be so potentially unproductive, scientists attempt to "think through" each experiment in detail from all theoretical perspectives before actually conducting it. The consequence is a more definitive test of the various theoretical views involved, and less wasted (i.e., ambiguous) experimental effort.

The experimental consideration of multiple hypotheses has the advantage of forcing the practicing "normal" scientist to translate various theories into corresponding terms, so that disproofs and disconfirmations take place in a single context rather than in separate paradigms. The scientist is also forced to go beyond a single method in his effort to clarify a single problem. As a result, a variety of techniques are brought into a paradigm and put to use. In addition, the strong inference method subjects all experimental methods to a critical scrutiny so as to eliminate artifactual data. This loosens the scientists' commitment to the details of the paradigm. Most importantly, it focuses attention on the logic of falsification and disproof, and deemphasizes confirmation and efforts to support a hypothesis. The scientist's efforts are concentrated on anticipating the reasons why his observations will have one form or another and not on fitting the data to a particular hypothesis. [The successful use of multiple hypotheses in the estimations of intentions is examined in Section 7.]

SECTION 7

AND COME TO LIGHT: EXAMPLES AND ANALYSIS OF SUCCESSFUL ESTIMATES OF INTENTIONS

AND COME TO LIGHT: EXAMPLES AND ANALYSIS OF SUCCESSFUL ESTIMATES OF INTENTIONS

> It is very difficult to make accurate predictions, especially about the future.
>
> Niels Bohr

> What enables the wise sovereign and the good general to strike and conquer, and achieve things beyond the reach of ordinary men, is foreknowledge.
>
> Sun Tzu

> For nothing is hid that shall not be made manifest, nor anything secret that shall not be known and come to light.
>
> Luke 8: 17

This section complements Section 4, which assessed intelligence failures; here we analyze intelligence successes. Specifically, three examples of successful estimations of enemy intentions are described in detail. Each example is then analyzed to determine the methods, procedures, and mental processes which seem to have contributed to the success. This section closes with a summary of the features common and unique to the three examples.

While there are many detailed assessments of intelligence failures, there are few descriptions of intention estimation successes. And although there are many examples of successful estimation of enemy intentions, there are very few descriptions of these successes that provide sufficient methodological details to shed any insights into the intellectual or organizational processes and methods that contributed to the success. The three examples used in this section were selected because they were written by participants and describe in detail the

estimation process. It is our belief that these are the most complete methodological accounts of successful intention estimation available.[1] Nevertheless, even these accounts leave many methodological issues unaddressed and none was written with the purpose of comparing successful estimation to efforts which fail, or with ultimately assessing why some methods succeed while others do not.

The three accounts which provide the examples for this section are George's (1959) Propaganda Analysis: A Study of Inferences Made from Nazi Propaganda in World War II, Beesly's (1977) Very Special Intelligence: The Story of the Admiralty's Operational Intelligence Centre 1939-1945, and Jones' (1978) The Wizard War: British Scientific Intelligence 1939-1945. All three authors participated in the intention estimation successes they describe and each attempts to explicate how such successes were attained. Beesly's and Jones' accounts were written as histories. George's account is a social science study which attempts to describe the inferential methods used by propaganda analysts.

While each of these accounts describes estimates of enemy intentions, different types of intentions were being predicted in each case. The propaganda analysts focused primarily on the propaganda, strategic and political intentions of the Nazis. The Operational Intelligence Centre of the British Admiralty was attempting to predict future German U-boat moves. R. V. Jones was attempting to predict future German weapons and how and where they would be used.

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[1] Should the reader be aware of descriptions of intention estimation successes which contain details of the methods, inferences, deductions, procedures, routines, etc., which contributed to the estimates, the author of this paper would be extremely grateful if these were brought to his attention. Holmes (1979) and Hinsley (1979) were received too late to be included.

ESTIMATING NAZI INTENTIONS: GEORGE PROPAGANDA ANALYSIS

> We live in an age of intense propaganda... to turn a deaf ear is to court disaster... Better to sharpen the wits in order better to appreciate truth that lies concealed in many a verbal distortion.
>
> C. C. Pratt

> One must know how to color one's actions and to be a great liar and deceiver.
>
> Niccolo Machiavelli

During World War II the task of monitoring and reporting on the mass communications of other nations fell to the Foreign Broadcast Intelligence Service (FBIS) of the U.S. Federal Communications Commission.[2] In addition to reporting enemy broadcasts, FBIS subjected them to propaganda analysis to summarize the content and to interpret "the intentions, strategy, and calculations behind propaganda communications" (George, 1959: vii).

The FBIS Accomplishments: George served as a junior analyst in the Analysis Division of FBIS and contributed to some of the studies which he undertook to analyze in detail after the war for the RAND Corporation. To determine the accuracy of the FBIS inferences he selected a sample of estimates of Nazi propaganda and matched them against relevant historical evidence of Nazi conduct during the war. George found 85 percent of the inferences which could be checked in this manner were correct. Of those estimates which dealt with Nazi elite policies and intentions 81 percent were correct (p. 264). (Only

[2] FBIS continues this work today, now under the title of Foreign Broadcast Information Service and the auspices of the Central Intelligence Agency.

these estimates are considered here.) A variety of Nazi intentions were estimated in George's sample. The plans for the use of the V-weapons, German preparations for hostilities with Brazil, German plans regarding Spain and Spanish Morocco, the prospects for German offensives against Russia, the possibility that Germany would denounce the Geneva prisoner-of-war convention, were among the Nazi elite actions predicted on the basis of propaganda analysis. George's study attempted to "codify the procedures they [FBIS analysts] followed in making inferences" (p. xi). It was possible, according to George, to do this for the procedures followed to infer future enemy actions, but it was "less feasible" to characterize how the analysts inferred the meanings behind Nazi propaganda. However this poses no problems here as it is the analysts' efforts to estimate intended actions that are the subject of this section.

The Diagnostic Approach: George likened the work of the FBIS analysts to medical diagnostic techniques and concluded that his codifying efforts made only some aspects of the FBIS techniques more explicit. Further, he noted that the analysts began with "no blueprint of procedures for drawing inferences about the intentions and calculations of the propagandist from his communications," and that the methods ultimately developed were not "entirely systematic" and "rested to a considerable extent on the intuitive skill and judgment of [the] . . . expert" (p. x-xi). Despite never being fully articulated during the war, the FBIS techniques, although creative and intuitive and lacking a systematic basis or doctrine, did adhere to several methodological regularities, and, George concluded, were based on an explicit model of the Nazi propaganda process.

Estimation Objectives: Three types of predictions made by the FBIS are relevant to the estimation of intentions, using "intentions" in the broad definition of Section 2: enemy (1) estimates, (2) expectations, and (3) policies, plans and actions. George labels the latter intentions (p. 16).[3] FBIS estimates included what the Nazis believed were the Allied capabilities and intentions as well as the Nazis' estimates of the various situations and problems they faced. Nazi expectations included the forecasts and predictions the Nazis used to make their plans, and included forecasts regarding events the Nazis controlled as well as predictions about events over which the Nazis had little influence. The Nazi "policies, intentions, and actions," George defined (p. 16) as:

> national policies ... to govern the use of military, economic, diplomatic, political, and propaganda instruments of power for the purpose of reaching certain objectives. The specific moves undertaken ... are ... "actions." The term "intentions" ... indicate(s) both future actions which the regime decided upon and the objectives behind current actions.

Assumptions: The FBIS analysts made several basic assumptions about the relationship between the Nazi propaganda system and the political decision-making system (p. 20). First, propaganda content was a resultant of the operations of both systems and, second, that the former system operated in a subordinate, auxiliary relation to the latter. Three additional assumptions were made regarding this relationship:

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[3] Estimates of these three types comprised 22 percent of the 729 inferences in George's sample from one two-month period, March-April 1943. The remaining estimates dealt with events affecting Nazi behavior and propaganda and with propaganda strategy, George, 1959: 17 fn. 2.

1) Propaganda was used as an instrument of policy in a highly purposive and deliberate way to implement policy goals.

2) The goals and strategies for the propaganda were closely coordinated with Nazi policy calculations, estimates, expectations, and intentions.

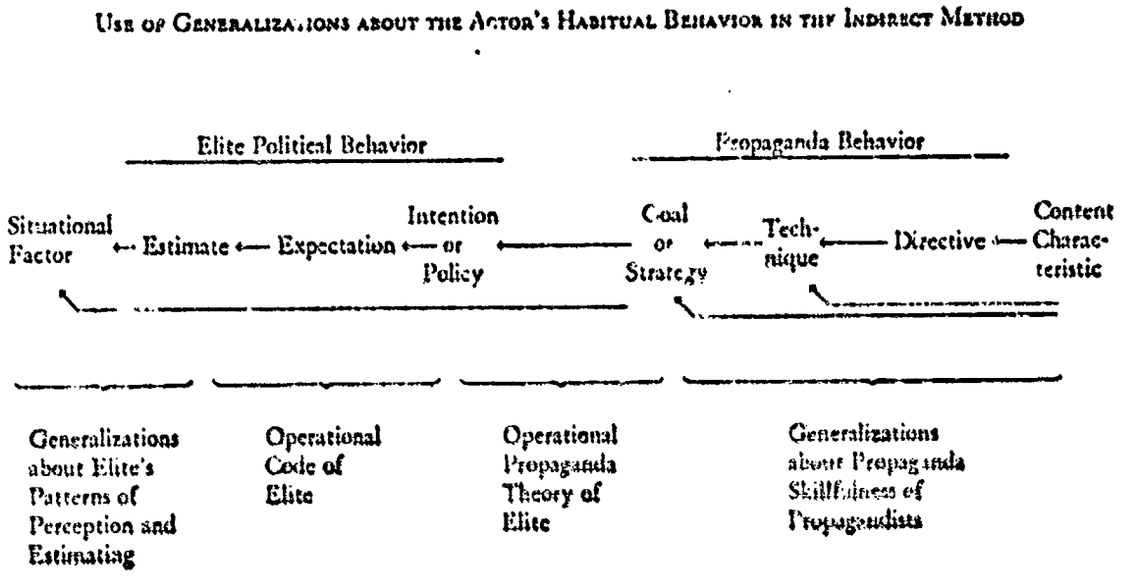
3) All propaganda was under the centralized control of the Propaganda Ministry.

A further assumption was also made regarding the nature of the influences on this policy -- propaganda relationship: it was assumed that (p. 24)

> Variations in the content of Nazi propaganda were more likely to reflect changes in situational factors and in Nazi policy calculations than changes in the basic ideological and cultural determinants of behavior.

The FBIS Analysts' Model: The aspects of Nazi action which the FBIS analysts were attempting to predict were seen as "the major unstable variables" in the propaganda system. The analysts, using their expert knowledge of the stable elements of the Nazi system -- the culture, ideology, values, habits, predispositions, and perceptions of the Nazi leaders -- placed the unstable aspects into a coherent relationship to each other and to the stable "background." The main relationships are shown in Figure 7.1. On the bottom line of this figure are the relatively stable background factors. The diagram shows how these were believed to influence the seven unstable elements which produce the propaganda content and which the FBIS analysts were attempting to estimate (shown in the middle line of the figure). As the arrows indicate, the analyst's task was to infer from the content characteristics of the

Figure 7.1. Conceptual model relating propaganda system to policy-making system used by FBIS analysts. (From George, 1959: 53)



propaganda back to the unstable intention or policy. Intervening between the content and this conclusion were inferences about the propaganda behavior of the Nazi system.

This figure shows two stable background aspects as directly influencing the inference of intention and policy: the operational code. and the operational propaganda theory of the elite. The former refers to the general rules of political strategy and conduct which the elite employ and to the enemy elites' image of the opponent. The latter refers to the enemy elites' and propagandists' beliefs about the capabilities and limitations of propaganda as an instrument of policy and their general image of the audiences which they addressed.

The figure also makes clear that the unstable element of propaganda behavior was believed to intervene between the behavior the FBIS analyst was observing (propaganda content) and the element the analyst was estimating, namely intentions and policy. Situational factors would influence both the propaganda strategy and the intentions; separating the relative impact of the situation on these two aspects of the Nazi system posed a major challenge.

The Estimation Process: The first step in the FBIS technique was "to establish the propaganda goal or strategy underlying the specified content" (p. 40, 48), i.e., to explain the purpose of the propaganda behavior. As Figure 7.1 suggests three sources of information are applied to this inference: the nature and content of the communication, the analyst's knowledge of the situational factors affecting the Nazi system, and his knowledge of the Nazis' propaganda habits and skills.

Having explained the propaganda goal, the analyst turns to the task of inferring the Nazis' policies or intentions behind the propaganda.

Here the analyst applies his knowledge of the elite operational code, the elite operational propaganda theory, and the situational factors. The first and second of these are based on the analyst's observation of the enemy elite, in effect, the analyst has answered two questions: how has the elite translated expectations and estimates into policy and plans in the past (determined from a study of elite writing and action), and how have plans and intentions been turned into propaganda (derived empirically from past Nazi propaganda efforts). With this background image of the elite's habits and his knowledge of the situation which confronts the elite, the analyst infers the enemy policy or intentions, and how the enemy perceives the present situation it faces, i.e., the analyst infers the enemy's expectations and estimates.

Basically the analyst is reasoning from either end of Figure 7.1, using the Nazi propaganda behavior and also the situational factors as clues to the unstable factors such as intentions. Using his background knowledge of stable Nazi habits, beliefs, and skills, the analyst "formulate(s) alternative hypotheses or inferences against which to weigh available evidence ... [or] to delimit the range of types of behavior which that elite is likely to display" (p. 51).

George describes how this essentially circular mental model of the Nazis' policy and propaganda systems was utilized:

> the investigator [analyst] attempts to approximate the logic of experiment by means of a mental rehearsal of hypothetical outcomes. Changes in the value of one or more variables are postulated ... in order to appraise the consequences, if any, for other variables. (p. 58)

Since the analyst knows (or believes he does) the values of some of the unstable variables, and he knows the general relationships between the unstable and stable variables, he can ask how the enemy actor might

behave given these known factors. The analyst attempts to determine what novel features of the present situation might influence the enemy's view of the situation, which leads, in turn, to the analyst's estimate of how previous generalizations about how the enemy usually behaves must be modified in view of the situation at hand. George describes this process:

> The propaganda analyst examines ... the propaganda communications ... to identify content features which may be the consequences of a particular choice of action in that situation. The scanning of propaganda communications proceeds ... in intimate conjunction with the rehearsal in the analyst's mind of alternative inferences as to antecedent conditions. ... (V)arious content features of the propaganda may be tentatively regarded as indicators of various possible action responses. In many cases, content characteristics which "indicate" (permit the analyst to infer) the propagandist's goal or strategy can be readily spotted. The task then becomes to infer other unstable components, or antecedent conditions, of the action. (p. 60)

Nevertheless, as George warned (p. 91), "inferences as to what the propagandist is trying to say and why the propagandist is trying to say it are not neatly discrete."

Coping with Circularity: The FBIS analysts made no sharp distinctions in using this model between the descriptive and the inferential phases of their estimation effort (p. 93). Nor was there a commitment to use some variables only as dependent and others as independent. The circular approach FBIS employed risked "analytic bias," the possibility that an analyst's hypothesis, formed early in the description of the propaganda content, determines what he subsequently regards as significant (p. 93) in the estimate of intentions.

George described (p. 93-4) how the analysts attempted to offset this potential weakness in their use of a mental model of Nazi propaganda:

> ...(T)he disciplined analyst guards against it in several ways. He does not read through the propaganda materials just once but rereads as many times as necessary to satisfy himself that the inference he favors is consonant with all of the relevant portions and characteristics of the original propaganda material; he considers not just one inferential hypothesis in reading and rereading the original propaganda materials but also many alternatives to it; and he systematically weighs the available evidence for and against each of the alternative inferences.
>
> Thus the results of his analysis explicitly or implicitly include not merely (a) the favored inference and the content evidence for it but also (b) alternative explanations of that content evidence, (c) other content evidence which may support alternative inferences, and (d) reasons for considering one inferential hypothesis more plausible than others.

Trend Analysis: The analyst's generalizations about Nazi habits played a central role in the estimation of specific intentions. These generalizations were constructed and updated by analyzing trends and tendencies in the Nazi use of propaganda. They formed the basis to answer these questions: when and for what types of actions propaganda was used, which audiences were addressed for which types of action, what propaganda goals were pursued for different audiences and actions, which communication channels were associated with different types of action (p. 133). These empirically verified generalizations formed the background information against which the analysts assessed specific propaganda behavior. To these patterns of observed Nazi behaviors the analysts added other possible propaganda patterns which might be linked to various enemy actions.

The V-weapons, A Comparative Example: George compares the efforts of a British propaganda analyst (who used the FBIS technique described above but under different circumstances) to predict Nazi intentions and actions regarding the V-weapons with the efforts of FBIS analysts. This comparative example demonstrates how the FBIS method was most effectively applied as well as illustrating some of its potential weaknesses. The British report, shown in Figure 7.2, was based solely on German propaganda and was the result of a specific request for a retropsective analysis on secret weapon propaganda.

George checked the accuracy of these estimates against historical records. Inferences 1 a-e accurately reflected the Nazi expectations for the new weapon. This estimate that the secret weapon threat was real[4] was based on the fundamental observation, frequently verified, that Nazi propaganda never deliberately misled the German people about an increase in German power. Therefore, claims being made for the secret weapons to the home audience could be accepted as reflecting the elites' own expectations. Infer nce f was also accurate.[5] Inferences g and h were based on the fact that references to reprisal weapons stopped suddenly after 19 August and again after 10 September. Just prior to these dates Allied bombers attacked the research facility

[4]That rocket weapons were a potential threat had been accepted by the British War Cabinet meeting of 29 June (Jones, 1978: 343-6) at which it was decided to bomb Peenemunde, the Nazi rocket research center.

[5]George was unable to confirm f, but Jones' (1978: 350) account indicates this estimate was largely correct. A spy in the German Army weapons office reported in August that the rocket attack would begin 20 October. This report was made before the RAF raid on Peenemunde on 17-18 August.

Figure 7.2. Report of British propaganda analyst on Nazi secret weapons, issued 8 November 1943, seven months before the V-weapons were used (from George, 1959: 142-3). The "D-day" in the report refers to the day on which the Nazis planned to make first use of the secret weapon.

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1. It is beyond reasonable doubt that Germany possesses an offensive weapon which her leaders believe:

   a) Is of a type unknown to the Allies.
   b) Cannot be countered within a short period.
   c) Will be used for the first time on a scale sufficient to produce very striking results.
   d) Will create in British cities havoc at least as great as that in German cities, and probably much greater.
   e) Will have a more shocking effect upon civilians than air-bombing on present scales.

2. It is further highly probable that:

   f) By the end of May preparations for the use of this weapon were past the experimental stage.
   g) Something occurred on or a little before August 19th which substantially postponed D-day.

3. It is further probable that:

   h) Something occurred between the 3rd and about the 10th of September which further postponed D-day.
   i) The schedule for the offensive weapon has lagged in relation to that for a type or types of defensive weapon, and Germany's leaders now expect a diminution in the weight of Allied air attacks to precede German retaliation.

4. It may be tentatively estimated that Germany's leaders expect this offensive weapon to come into use not before the middle of January, 1944, and not later than the middle of April. There is unlikely to be an error of more than a month each way in the first of these estimated dates, but there might well be an error of two months either way in the latter.

   The estimate for the earliest date of use is based partly upon estimates of the schedule existing in June, in early August, and in early September. If these estimates from propaganda can be confirmed by independent evidence, it would be possible to regard the final estimate (mid-January) with slightly less caution.

   The estimated schedule at these earlier periods was:

In June:	Earliest use, mid-September.
In early August:	Earliest use, beginning October.
In early September:	Earliest use, beginning December.

and the launching sites, respectively, for the V-weapons, slowing down the production of the rockets, although the analyst was unaware of these raids or their effects.

Inference i was based on a change in German propaganda after 19 August in which the Nazis predicted new anti-bomber defenses would be ready before the use of the secret offensive weapons against England. Prior to 19 August the retaliation weapons were not mentioned together with defensive developments. The conjunction of the two after 19 August suggested that the priority of the defense was in excuse for lagging offensive developments rather than due to a speedup in defensive developments.[6]

Estimate 4 George labeled (p. 145) "amazingly accurate." Using data from George and Jones (1978: 339, 350, 374) the dates predicted by the Nazis for D-day at various times (and reported to Jones by secret agents in the Third Reich) can be compared with the analyst's reconstruction from Nazi propaganda:

Date of Nazi Prediction	Nazi Prediction of D-day	British reconstruction of Nazi Prediction from Propaganda
June 1943	end July 1943	mid-September 1943
August	late October-Nov.	early October
September	?	early December
November	March-April 1944	mid-January 1944 (± one month) to mid-April (± two months)

These deductions of the Nazi leaders' estimates were based on the analyst's general observation of one element of Nazi operational

[6]German defenses against British night bombing did improve in 1944 due to new methods and equipment (Jones, 1978, ch. 41).

propaganda theory; namely that Propaganda Minister Goebbels was "careful not to give the German public a promise of retaliation too far ahead of the date on which the promise could be fulfilled" (George, 1959: 145), an insurance against disillusionment on the homefront. The analyst calculated Goebbels would allow about three months in advance of the fact in describing a retaliation capability.

FBIS estimates of the V-weapons were of "lower calibre" than the British report, according to George (p. 146), for several reasons. First, FBIS did not attempt to go much beyond description of V-weapon propaganda. It was assumed that inferences about the existence, nature, targets, and operational dates of new weapons were the province of other intelligence agencies. Further, FBIS implicitly assumed "that it is simply not possible to deduce from mere propaganda threats whether such weapons actually exist" (George, 1959: 168 fn. 17).

This lack of an effort to draw more insightful inferences from propaganda was reinforced by organizational relations. FBIS was not asked to coordinate its efforts with other agencies whereas the British analyst was requested to construct his estimate so that it could be compared to other intelligence.

Second, the FBIS effort was periodic, undertaken to meet weekly deadlines, while the British report was a retrospective analysis undertaken at a single time of all Nazi propaganda on the V-weapons. The trends in Nazi propaganda habits were probably much clearer to the British analyst, surveying all the material, than to the FBIS analysts, dealing with each week's material with no explicit pressure for an overall perspective.

Third, a general reluctance to go beyond description inhibited the FBIS effort to construct and test various hypothetical estimates of the German secret weapon effort. Although FBIS analysts were aware of the fact that propaganda was never used to mislead the Germans about German power, FBIS did not note the relevance of the Nazi propaganda commitment to retaliate with secret weapons.

George also faults FBIS for not using trend analysis as did the British analyst, especially with respect to the negative (nonoccurrence) evidence of the lack of reprisal threats after 17 August and 10 September. Again, it is highly likely that the piecemeal practice of FBIS, in contrast to the systematic retrospective method of the British, led FBIS to rely on their impressionistic analysis rather than the more quantitative British approach. Negative vidence is impossible to detect except against the perspective of a long background of normal activity.

Both the British and FBIS were aware that Nazi propaganda was preoccupied with German morale and that public skepticism tended to set in if the Nazis were unable to deliver in a reasonable time on their propaganda threats. However, Goebbels' propaganda operational theory was neither noted or applied by FBIS to the V-weapon case to determine the Nazi predictions for D-day. The reluctance of the FBIS analysts to formulate estimates and check competing inferences, their isolation from other intelligence efforts, and their week-by-week production schedule all combined in their overlooking this central evidence and its implication for deducing the Nazis' estimates.

Characteristics of the FBIS Method-Strengths: The FBIS technique was based on explicit models of Nazi political and propaganda behavior. These models were built up from empirical observations and reduced to

"stable" generalizations. Against this explicitly formulated background the FBIS analyst then assessed a specific piece of propaganda and, using what George terms the "logic-of-the-situation" (that is, the likely impact of situational forces) attempted to infer the unstable variables which produced the propaganda.

Using these models, the analysts "tested" a variety of intention estimates against the generalizations and the logic-of-the-situation to determine how well each alternative fit. Although the methodology tended to be circular, this express attention to competing explanation was seen as a safeguard against analytic bias.

This methodology had the advantage of being able to cope more rapidly with changes in political or propaganda strategy by the Nazis than would a complete reliance on a quantitative method (i.e., what would amount to using only the generalizations to draw inferences while ignoring the situational and unstable parts of the models). Because the models included the background generalizations, the FBIS analyst had the benefit of the trend and tendency information (including negative evidence) these generalizations provided, as well as the flexibility (which the unstable part of the models provided) to deal with a wily and deceptive opponent propagandist, whose tendencies or strategies might change.

George (p. 61, 134) likens the FBIS method to mosaic reconstruction, a fairly common analogy (along with jigsaw puzzles) for intelligence analysis. He writes (p. 61):

> The analyst's reasoning takes the form of filling in, or assigning a value to, each of the major unstable variables which are not already known, and supporting this reconstruction both by generalizations and by logic-of-the-situation assessments.

This analogy of mosaic or jigsaw puzzle solution has been attacked by scholars of intelligence failures (see Section 4) as a method leading to analytic biases: the analyst picks pieces of evidence to fit his preconceptions and ignores evidence which conflicts with his preferred hypothesis. That is, the analyst creates the mosaic (jigsaw) he wants to create. There are two aspects of the FBIS method as George describes it which would tend to prevent it from being such a mosaic process.

First, the explicit use of (and repeated validation of) both the generalizations and the logic-of-the-situation assessments led to the detailed model shown in Figure 7.1. This model drew the outlines of the mosaic, so to speak, and constrained the analyst's choice of evidence -- the analyst could not create any mosaic; he had to work within the bounds of a specific model which had previously been tested and found sound.

Second, George stresses that the analysts tested all logical hypotheses against the available evidence rather than selecting pieces of evidence to create a preferred hypothesis:

> The analyst rehearses in his mind the different possible versions of each particular missing variable ... trying to decide which version is most plausible, given the known value of the content ... and ... other antecedent conditions (p. 61).

This process of comparing hypotheses conforms to the "strong inference" method described in Section 6, and made it more difficult (but not impossible) for the analyst to deprecate a worthy hypothesis merely because the analyst personally favored another explanation.

A final strength of the method, discussed below, was its narrow, intensive focus on a single category of enemy behavior.

Characteristics - Limitations: George notes (p. 131-2) some of the limitations of propaganda analysis in the prediction of enemy elite actions. First, the major focus of FBIS was on propaganda, not intention estimation (although such estimates were an important product). Consequently, the FBIS analysts may have attempted to make estimates of intentions only when they were especially confident that their judgments were correct. George (p. 275) considers this possibility largely conjectural. Nevertheless, propaganda analysis was explorative, not systematic, and was not applied to every intention estimation question, hence the FBIS analysts, through a selection of the questions that were addressed, may have inflated their success rate beyond what it would have been if such questions had been assigned to them systematically. It may well be that the propaganda analysis method of intention estimation succeeds only over a narrow range of issues (although George's examples show that that range spanned many critical intentions).

Second, these methods can only apply to intended actions by an opponent which involve propaganda preparation to enhance or justify the intended action. Spontaneous actions or reactions could not be inferred specifically, although rough estimates might be made from the generalizations. Nor could the analysts anticipate elite action, prepared in secret with no advance propaganda preparation. The enemy may prepare in secret, execute the secret action, and then employ propaganda after the fact, excluding any intention estimation based on propaganda (although other useful estimates are still possible, e.g., what the enemy expects to accomplish). As in the first limitation, the consequence is to limit the range of applicability.

Third, while the methods George describes employ many scientific, quantifiable procedures (e.g., strong inference, trend analysis), they are primarily subjective judgments based in part on intuition, experience, and an unquantifiable "feel" for the problem. Analyses which depend heavily on the subjective opinions or estimates of judges, no matter how expert and experienced the judges may be, have several limitations which are explored in Section 8. Notwithstanding George's observation that the FBIS experts had far more successes than failures, methods now exist which improve upon a heavy reliance on subjective judgment. The implication of this is that the methods as described by George may be less reliable than they could be.

Finally, as practiced in World War II, propaganda analysis was largely a "collateral" intelligence operation, that is, it was based on information which is widely or publicly available and accessible to many. In contrast, the intention estimation successes described in the remainder of this section were based on "all-source" methods, i.e., the integration of intelligence from all sources, ranging from the enemy's propaganda and public press to the most secret codebreaking of the enemy's closely guarded communications. It is not essential that propaganda analysis be conducted without close interaction with other sources of intelligence or other analytic methods and it is likely that the FBIS analysts would have been much more effective if closer interaction had been practiced. This limitation, which was put upon this method rather than being inherent in it, points out a final strength of propaganda analysis -- the remarkable number and the high quality of inferrences on enemy intentions which can be obtained by an exhaustive, methodical examination of a single source of intelligence over a long period of time.

PREDICTING THE U-BOATS' MOVES: BEESLY
VERY SPECIAL INTELLIGENCE

> We repeatedly checked our security instructions in order to ensure ... that our intentions were not being betrayed ... Our ciphers were checked and rechecked ... and on each occasion the head of the Naval Intelligence Service ... adhered to his opinion that it would be impossible for the enemy to decipher them.
>
> Admiral Karl Doenitz, 1958

> The Tracking Room could claim, with justice, to know more about the U-boats' deployment than Admiral Doenitz's own staff.
>
> Ronald Lewin, 1978

> Former Head of German Naval Signal Department: If the Allies could read it all, why didn't they win the war sooner?
> An American historian: They did.
>
> David Kahn, 1979

Just before World War II, in August 1939 the British Admiralty's Naval Intelligence Division created the Operational Intelligence Centre (OIC), described later by Patrick Beesly (1977: xv), an alumnus of the OIC, as the "nerve centre" of the maritime war against Germany. The brain within this nerve center was the Submarine Tracking Room where the movements of all enemy U-boats were estimated, plotted, and, by the middle of the war, predicted.

This part of Section 7 summarizes the methods developed by the Tracking Room to predict the moves of the U-boats and to anticipate the plans and reactions of the German U-boat commander, Admiral Karl Doenitz.[7]

[7] Beesly's description of the Tracking Room's methods is most detailed, but he also describes the intention estimation successes of the OIC in the areas of the Nazis' surface fleets and commerce raiders, and its coastal convoys.

The success of the Tracking Room: The climax of the Battle of the Atlantic came in March 1943, when the British Naval Staff observed that "the Germans never came as near to disrupting communications between the New World and the Old as in the first twenty days of March 1943" (Lewin, 1978: 218). In those twenty days the Allies lost 97 ships (627,377 tons, Lewin, p. 216). The Germans lost 16 U-boats (Beesly, p. 180) but were building an average of almost 24 per month (Doenitz, 1959). Nonetheless, from May 1942 to May 1943, the height of the Battle of the Atlantic, the Tracking Room (and its American counterpart) accomplished the following (Beesly, p. 185, from Rohwer, The Critical Convoy Battles of 1943):

	Number	Percent
Convoys routed clear of U-boats	105	57
Convoys sighted but not damaged	23	13
Convoys suffering minor losses (4 or fewer ships)	40	22
Convoys suffering major losses (5+ ships)	16	9
	184	100

In contrast, the German U-boat losses, averaging 16 per month in February and March 1943, increased to 28 per month in April and May. On 24 May Doenitz withdrew the wolfpacks from the North Atlantic and "never again would the U-boats make a significant comeback" (Lewin, p. 218). By June, July, and August 1943 more U-boats (74) were sunk than Allied merchant vessels (58, Lewin, p. 220), and orders were given that no ships were to be routed against the advice of the Tracking Room without the express permission of the Assistant Chief of the Naval Staff (Beesly, p. 166).

Obviously, the staffs of OIC and the Tracking Room by themselves sailed no convoys and sank no subs. These feats were the work of the Allied naval and maritime crews at sea and in the air. But the successful routing of convoys around the U-boat threat and the targeting of the U-boats were the accomplishment of the Tracking Room.

The Tracking Room Sources: Six sources of intelligence provided the information used by the Tracking Room staff to make its intention estimates. The quality and availability of the intelligence from these sources varied widely throughout the war. Only at rare moments were all of these sources operating at their maximum potential.

Long before the war began a network of radio listening posts on British territory intercepted signals from vessels at sea and obtained bearings to the signals. These high frequency radio direction finding (H/F D/F) stations then radioed the bearings to the Admiralty. By triangulating the bearings from several stations the Admiralty could potentially locate any ship's radio at sea nearly as soon as it operated. In the first year of the war, however, this embryonic system was slow and had only limited coverage of the oceans. As the war progressed the extent and efficiency were greatly increased.

The British long neglected aerial photographic intelligence (photint) during the interwar period but rapidly expanded its capability as the war progressed. However, the competition for the RAF reconnaissance capabilities and the distances to the Baltic prevented continuous photographic coverage of key naval targets in Germany for the early war years. Later, regular photint on the docks and building yards gave the Tracking Room a complete inventory of the operational and under construction U-boat fleet. With photint it was also possible to accurately estimate

the production schedule of the U-boat yards and thus predict how fast the boats were built or repaired.

Any Allied sighting or contact with a suspected U-boat produced an operational report, or oprep, which reached the Tracking Room. As the Allies obtained long range surveillance aircraft and anti-submarine escort aircraft carriers, and as the Allied radar systems improved, it became impossible for a U-boat to approach the convoy routes or the coastal areas on the surface without being sighted. While some Allied ships were sunk without giving any word, most ships attacked by U-boats were able to get off at least an S-S-S (attack by submarine) distress message. Even these brief opreps were fed to OIC and, compiled in the Tracking Room with other sources, helped estimate where the U-boats were and to predict where they would be next.

Throughout the war secret agents working for the Allies provided information on the comings and goings of the U-boats in the building yards, the training bases, and the coastal harbors. Reports of the attitudes, morale, and unguarded remarks of the U-boat crews also reached the Tracking Room. While not a high quality source in the sense of providing readily usable intelligence (human intelligence, or humint, reporting is often by slow and circuitous courier routes -- a report from a spy in a French port across the Channel from England might have to go to the Tracking Room via Lisbon) or in the sense of solving the major riddles of the U-boat operations (because it is often incomplete and unreliable), the humint sources nevertheless provided a steady flow of useful insights which rounded out the Tracking Room's other data.

A second humint source is the POW, prisoner of war. Many German sailors were captured and questioned by the British, although

few U-boat Arm seaman survived to be captured (fully two-thirds of all who served in the German U-boats lost their lives in World War II, Lewin, p. 219).

Another source of information was provided by captured German equipment and documents. While, in general, this source played a secondary role in the Tracking Room's operations, it furnished much of the information needed to improve Allied defenses and countermeasures. Several especially important captures, of the trawlers Krebs, Munchen, and Lauenberg, and especially of the U-boat U-110 provided important clues in the solution of German Naval ciphers (Beesly, p. 71).

The most important source for the Tracking Room was Ultra, the decrypted German coded radio messages. Quite simply, Ultra was, as David Kahn recently quoted one of its veterans as saying, "the most important sustained intelligence success in the history of human conflict" (Kahn, 1979: 141). The solution of the Germans' most secret ciphers allowed the Allied intelligence officers to know, repeatedly and often simultaneously, what the German commanders and leaders knew and planned. The Tracking Room, through Ultra, was privy to the orders, instructions, destinations, and missions given the U-boats by Doenitz, and to the U-boats' sea reports of their operations.

Good as Ultra was, it was not easy to get. "Breaking" into the German codes was a daily task and sometimes the effort succeeded but often it was slow or a failure. In addition, much of the Germans' most secret messages could be sent by telephone landlines which were secure from Allied interception. A crucial cipher might go "uncracked" for months. On the other hand, the Allied cryptographers could sometimes "unbutton" a message faster than its German recipients. Ultra was by

no means a steady or constant tap into the Germans' communications and it was only by combining its other sources with Ultra that the Tracking Room was able to effectively estimate German U-boat intentions. Nevertheless, Ultra was central to this ability; when it was "blacked out" (by the inability of the Allied codebreakers to unscramble the ciphers) the Tracking Room's effectiveness fell off (see Lewin, ch. 8). But sporadic as Ultra often was, it deserved the description given it by Sir John Slessor, Marshall of the RAF: "a real war-winner" (Lewin, p. 219).

The Tracking Room Products: The objective of the Tracking Room was to maintain as complete a record of U-boat plans and dispositions as that kept in Doenitz's headquarters. As McLachlan (another OIC alumnus) wrote (1968: 59) of the OIC:

> The ideal is ... to reproduce the enemy's mental processes by participating so far as possible in his information and bringing to bear on it one's own complete information about the intentions and strength and position of one's own forces ... [thus] a good operational intelligence man would read his enemy's intentions.

To achieve this end of bringing together the intelligence on the enemy with information on those parts of one's own forces which the enemy was attacking, the Submarine Tracking Room was located directly opposite the Admiralty Operations and Trade Main Plot, on which all Allied naval and commerce movements were tracked (Beesly, p. 165). Those officers responsible for the conduct of Allied convoys and escorts and the operations of the Allied fleets made a nightly visit to these two rooms for a "last look" at the situation before retiring. Each morning the head of the Tracking Room, Commander (later Captain) Rodger Winn, would confer by scrambler with the staffs of Western Approaches and RAF

Coastal Command. Winn would then brief in the Tracking Room the Admiralty heads of Trade, Operations and Anti-submarine Warfare. At ten each morning Winn briefed the Naval Staff (Beesly, p. 167-8). Furthermore, Winn and his opposite number in America maintained a secure teletype line to each other on which they could exchange (in confidence from their own superiors) personal views of the situation (p. 169).

In addition to these daily contacts and moment-to-moment messages to various headquarters during fast-breaking developments, the Tracking Room produced forecasts of U-boat operations 24 and 48 hours into the future and a fortnightly forecast every two weeks. Each week a "Report on U-boat Activities" summarized the U-boat situation (Beesly, p. 170-1). Finally, a variety of special and regular appreciations were produced. For example, the former might entail an estimate of how the U-boats would be used against Allied invasion forces. The latter included complete wrap-ups on the U-boat effort against each Allied convoy (Beesly, p. 202).

The Prediction Process: The first head of the Tracking Room, Commander Thring, did not believe that intelligence should attempt to estimate intentions, adhering to the traditional military view (see Section 2):

> He refused to accept that there was any possibility of predicting what the U-boat might do next. To reconstruct a U-boat's cruise by working backwards from one incident to the previous one, yes that was possible, but to attempt to go forward and try to guess its future movements was not only impossible but either useless or ... positively dangerous. (Beesly, p. 55-6).

When Winn replaced Thring, this traditional view was discarded and the Tracking Room evolved methods to predict the U-boats' moves and to anticipate Doenitz's plans.

In the early years of the war the British cryptographers were unable to "read" the Enigma codes used by the German Navy, the first break-ins not occurring until May 1941 (Beesly, p. 64). However, Winn had other sources and, as the Tracking Room followed the U-boats' movements, "certain patterns of behavior [were] established. Certain types of U-boat signals were ... associated with certain situations" (Beesly, p. 57). A major "theory" of the head of OIC, Captain Norman Denning, was that "valuable intelligence could only be obtained by first establishing a 'norm' and then looking for any deviation from it" (Beesly, p. 41). As the R/F D/F, photint, and Ultra intelligence improved, this experience in U-boat pattern analysis was applied to these much richer sources of data.

Beesly (p. 253) describes the essential elements of this norm creation process: the patient collection of "hundreds of scraps of information" from various sources; the analysis, indexing, and filing of these scraps; and "the ability of a particular individual expert to assemble the relevant facts, to judge, possibly, what was abnormal and to fill in the gaps which even current Special Intelligence [i.e., Ultra] almost always left."

Winn described this method as "a working fiction -- what could only be an estimate and a guess was taken as a fact and acted upon" (quoted by McLachlan, p. 115). The plotting of a U-boat's moves began from a variety of "known" and "suspected" information:

(1) Which U-boats were at sea and where,
their past behavior, possible intentions,
and their probable fuel and torpedo states.

(2) Which U-boats were in which ports, lengths of port calls, scheduled departures. Which training crews were nearing graduation and which new boats were finishing sea trials.

(3) The general methods (tactics) and physics (e.g., fuel capacity) of U-boat operations.

Each U-boat's departure from its home port's channel could be observed by the British with their improving sources. From this last known location its progress was estimated every twenty-four hours, considering such factors as the weather and sea states of various routes, probable targets, where the Allied air and sea patrols were strong and weak, the changing fuel state of the U-boat. Against these daily "working fictions" the Tracking Room would attempt to fit its R/F D/F location fixes (made whenever the U-boats transmitted a message to its base), sighting and other opreps.

However, each piece of evidence could be associated with any of several U-boats. The Tracking Room estimated every possibility and followed through with them until some fact or observation ruled out a possible course. The impossible course estimates were dropped and new course probabilities developed which fit the pattern of facts. Courses were re-estimated over and over until the pattern of observations was consistent with the Tracking Room's reconstruction. Nevertheless, ambiguous cases were frequent. Beesly writes (p. 164):

> ... there were many occasions when ... two or more solutions were apparently possible ... In most such doubtful cases all the various possibilities were shown on the plot.

The OIC Tracking Room, like the FBIS analysts, used multiple hypotheses built on a basis of facts and strong suspicions. As data were received inconsistent with these hypotheses, they were eliminated or modified. The remaining hypotheses were then extrapolated until further inconsistent data led to a questioning of them. Beesly stresses that Winn had no reluctance to maintain an estimate based on "fragmentary and nebulous evidence" when faced with skepticism and a lack of evidence (p. 145) but in the face of inconsistent evidence, Beesly notes (p. 114), Winn

> ... insisted on complete honesty and, no matter how involved and painstaking the process of re-estimating, fresh pins [i.e., submarines] could not be added to the plot to account for awkward events, or old ones removed just because there had been no recent evidence to support that U-boat's presence where we were showing it.

Beesly's and McLachlan's accounts of how Winn deduced Admiral Doenitz's long-range intentions are less complete than their descriptions of the U-boat predictions and it is possible only to speculate on the precise factors underlying these long-range estimation successes. Winn's prescient estimates of Doenitz's plans began during the height of the Battle of the Atlantic, January to May 1943. Winn's earlier estimates, e.g., of the U-boat threat to the Allied "Torch" invasion of North Africa in the autumn of 1942, were often made without the benefit of Ultra intercepts of the U-boat communications. The Torch estimate was a "worst case" prediction, which Beesly labeled (p. 149): "an accurate estimate of the possibilities, but ... the precise outcome largely depended on the enemy's reactions and these could not be known in advance."

Later, when Winn had access to TRITON, the U-boats' Enigma-coded radio traffic with Doenitz, he was able to estimate not only the

possibilities, but also Doenitz's reactions. Beesly (p. 175) describes a February 1943 estimate by Winn of U-boat strategy for May-August 1943 as "a truly remarkable forecast, showing not only an understanding of how Doenitz was thinking at that very moment, but how he would react when the adverse conditions, which Winn clearly anticipated, actually arose."

By the time of the climax of the Battle of the Atlantic, in March 1943, Doenitz had had repeated experience with the Allies' improving anti-U-boat capabilities and tactics. He was forced increasingly to explain and justify his tactics and failures to the U-boat crews as well as to Hitler.[8] Winn's access to these explanations, even when they were deciphered too late to be of operational use, provided an illuminating insight into how Doenitz responded to the changing faces of battle. And Doenitz's choice of wolfpack tactics required extensive communications between the boats at sea and his headquarters. The OIC gained not only the locations of the U-boats and their instructions but frequent glimpses into the logic of their leader.

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[8]The sincerity of Doenitz's dialogues with the U-boat crews is evident on reading his memoirs (1958) or any of the accounts written by the U-boat crewmen themselves, e.g., Herbert Werner's Iron Coffins. Furthermore, although Doenitz often suspected the German ciphers had been broken by the Allies, he seemingly faithfully accepted the constant reassurances he received of the codes' security. Beesly (p. 67) notes that as late as 1973 (before the publication of Winterbotham's The Ultra Secret and the subsequent outpouring of previously unknown cryptologic triumphs) Doenitz "was apparently still loath to accept that most of his ciphers had been consistently and thoroughly penetrated." Doenitz's unguarded explanations to his crews must have had great value to Winn in his efforts to penetrate to the mental processes and reactions of his opponent.

In addition, as the Battle of the Atlantic shifted in favor of the Allies and the pressure upon the U-boats increased, Doenitz was forced from his earlier strategy of initiative into a pattern of reaction. The moves and capabilities of the Allied anti-submarine forces became a major influence on Doenitz's behavior and plans. Since Winn had intimate knowledge of this Allied factor, indeed he was guiding its actions on the basis of the Tracking Room's U-boat predictions, he was in an excellent position to observe his opponent's reactions and draw inferences about his future plans.

Three factors then may be suspected as contributing to Winn's long-range intention estimates: the access (via Ultra) to Doenitz's explanations and justifications; the shifting pattern of U-boat strategy from autonomous action to dependent reaction; and Winn's access to the German and Allied constraints on Doenitz's plans. This information was probably central to Winn's insights into Doenitz's reactions to these constraints. A fourth factor underlying Winn's success was probably his self-reliance; he thought through the logical steps of his estimates himself. Beesly, who worked by Winn's side in the Tracking Room, wrote (p. 168-9):

> ... delegation did not come easily to him. Nor did the nature of the job readily lend itself to this. The man in charge had to see and consider every scrap of information himself; he could not rely on readymade solutions to individual parts of the puzzle presented to him by his staff.

Limitations: The estimates of the OIC were not invariably accurate, and convoys were erroneously routed into the lurking U-boats. Beesly noted several reasons for these failures.

Sometimes the codebreakers at Bletchley Park could not unbutton signals in time for the Tracking Room to make operational use of the information. Similar problems might be caused by inaccurate or untimely D/F locations (Beesly, p. 156-7, 179).

Sometimes the OIC had in hand timely and accurate information, but could not solve the puzzles posed by Nazi security measures. For example, Doenitz introduced a grid zone location system which allowed him to transmit instructions without compromising the U-boats' future locations. It was some time before the Tracking Room could reconstruct the arbitrary grid system (Beesly, p. 162-4).

At other times the OIC wrongly assumed the U-boat commander understood Doenitz's instructions as well as they in the Tracking Room did. On one occasion the Tracking Room routed a convoy around the grid zone Doenitz had assigned a particular U-boat. The U-boat commander however, having less success than the Tracking Room in penetrating the complexity of Doenitz's grids, got lost and stumbled across the rerouted convoy (Beesly, p. 164).

Late in the war, following the Normandy invasion, Doenitz could not maintain strict control over the U-boats. Several factors prevented him from controlling them: Increased Allied anti-submarine tactics kept the U-boats submerged and on the run from base to base. Doenitz's own headquarters in Lorient was under attack. With the U-boats operating independently, Winn could only guess at their likely targets, but the U-boats were at such a great disadvantage that little harm was done by them, and many were lost to the anti-submarine patrols.

Finally, on some occasions the U-boats might be estimated to be in one of a few places, but no greater certainty could be put on one

definite estimate. When two or more hypotheses remained all the Tracking Room could do was pick one and hope for the best. Sometimes they picked the wrong possibility and routed the convoy into the waiting U-boat line (p. 164). The use of multiple hypotheses was not a perfect predictive method.

ESTIMATING LUFTWAFFE INTENTIONS: JONES THE WIZARD WAR

> Do not think what you want to think until
> you know what you ought to know.
>
> "Crow's Law," R. V. Jones, 1978

> Their stores, magazines, and other preparations furnish the best intelligence concerning the real designs of the enemy.
>
> Marshall Saxe

While FBIS was estimating German intentions from propaganda and the OIC was predicting German U-boat actions, R. V. Jones (1978) was attempting to predict future capabilities and intentions of the German Air Force, the Luftwaffe. Jones served from 1939 to 1945 as the chief scientific intelligence officer first for the British Secret Intelligence Service, and later as well for the Air Staff. His efforts as an estimator spanned several aspects of the Nazi war effort: air defenses, the V-weapons, atomic developments, among others. However, in this section we summarize only his estimations of Luftwaffe targets and tactics during the Battle of Britain and the Blitz, based on his analysis of German aircraft navigation beams, widely known as the "battle of the beams." His other estimation experiences followed much the same pattern as this first instance in the opening years of the war. Since the beam battle estimates were based on fewer sources of intelligence, the description of the logic and mental processes behind them is thus made much simpler. Furthermore, Jones' efforts to assess these particular Luftwaffe intentions were unique, other agencies did not pursue the same problems or clues, hence the description of his inferences and deductions is unclouded by competing or complementary

efforts by others. Later in the war, when Jones' efforts were meshed with those of other intelligence offices, the process of intention estimation is less easily reconstructed.

Despite the paucity of information on which to base such estimates Jones' intention predictions were nevertheless accurate and provided the Royal Air Force (RAF) the information needed to effect successful countermeasures and defenses. In Their Finest Hour Winston Churchill wrote of "the evermemorable and decisive part" played by "British science and British brains" in this episode (cited in Jones, 1978; 181). But perhaps the more eloquent tribute to Jones' estimative talents was paid by the Luftwaffe itself: on the basis of Jones' prediction that the next target of the German fire raids was to be Wolverhampton, the British Anti-Aircraft Command so increased defenses in that region that the Luftwaffe reconnaissance noted the preparations. Knowing the British were ready and waiting, the Germans canceled the attack and never bombed Wolverhampton. An accurate estimate of intentions had deterred the enemy.

The general problem posed for Jones as chief scientific intelligence officer was how the Germans would apply science to the war effort against Britain, especially how the Luftwaffe and the German air defenses would exploit science. This general problem entails a variety of questions, not all of which involved intentions, but which all ultimately influenced Jones' estimates of intentions. Among the questions regarding the use by the Luftwaffe of night bomber radio navigation beams (the first major issue Jones confronted) were the following: did the Luftwaffe have the scientific resources for such a program, how would such systems function, what operating properties would characterize them,

how would the Luftwaffe employ such beams, what countermeasures would be possible and how would the Luftwaffe react, what could be determined about future Luftwaffe targets from the analysis of their beam systems.

Jones had five basic sources of information available to him: (1) the physical characteristics of the navigational beam systems and British technical exploitation of captured equipment and documents; (2) the Luftwaffe operations involving these systems as observed by British radar, Fighter Command, the RAF Radio Listening Service or Y-service; (3) intercepts of radio traffic to and from the navigational beam stations, deciphered by the British from the German Enigma codes; (4) interrogations of and eavesdropping on German bomber crew prisoners of war (POWs); and (5) reports from British agents inside the Third Reich. These sources are listed roughly in order of the volume of information they provided for Jones' deductions. This same ranking does not apply for the significance of these sources. For example, the British had no agents in Germany early in the war with access to Nazi scientific information. However, an unsolicited and anonymous report passed to the British Embassy in Oslo in late 1939 included the first sketchy details indicating the possibility of a beam navigation system in the Luftwaffe. The Oslo Report, as Jones (1979: 70) labeled it, provided just enough details to convince Jones that such a method was physically possible and within the capacity of German science. The first question then was whether the Germans were actually developing beam navigation systems.

Jones began his search with several strong, explicit models. First, he outlined (on 7 December 1939) the stages involved in the

adoption by the enemy of a fundamentally new weapon and, opposite this, the collection capabilities of British intelligence to penetrate German security and obtain the information he needed (p. 73-4). These two outlines, comprising in essence an intelligence estimate, are shown in Figure 7.3. This estimate led Jones to conclude that stage one in the adoption of new weapons would be the easiest to observe, while gathering information on stages three and four would be more difficult but far more important to the prediction of stage five. The sources available to him at the beginning of the beam battle were largely of types one and three. This assessment led Jones to formulate concrete recommendations for specific additional intelligence collection, e.g., systematic monitoring of German radar transmissions, as well as allowing him to provide guidance to available sources (POW interrogators) as to the nature of information bearing on high priority questions (e.g., what things were the Germans doing of the stage four sort?). In addition to this fairly explicit estimate and plan of the intelligence aspects of the problem, Jones' pre-war training and experience (in the physics of radar and infrared aircraft detection) provided him with strong physical models of the phenomena involved in aircraft radio navigation as well as background on the requirements for the detection and measurement of such phenomena.

Radio navigation was not new in 1939. Before the war navigators were able to obtain nearly simultaneous bearings on two widely separated radio signals of known location and by plotting the back bearings on a map from the known locations could find by intersection their own position. Such traditional radio navigation could not, in 1939, provide sufficient accuracy for precision night bombing (because of the

Figure 7.3. Intelligence estimate formulated by R. V. Jones prior to the "battle of the beams." (From Jones, 1978: 73-4)

Stages of Adoption of New Weapons	Intelligence Collection Capabilities/Sources
1. General scientific work of an academic or commercial nature which causes	1. Accidental indiscretions (including deciphered messages).
2. Someone in close touch with a Fighting Service, and who is aware of Service requirements, to think of an application of the results of academic research. If this application be considered promising	2. Indiscretions encouraged by alcohol and/or mistresses.
3. Ad hoc research and small scale trials are performed in a Service laboratory. If these are successful	3. Information that cannot be kept secret and yet can give useful information, e.g., radar transmissions, loss of apparatus to the enemy.
4. Large scale Service trials are undertaken, which may lead to	4. Direct acquisitions of information by placing agents in Military Research Departments.
5. Adoption in Service.	5. Information from disaffected nationals.

imprecision of airborne radio direction finding equipment) and was in any event inferior to other techniques such as celestial navigation.

The Oslo Report however provided Jones details (p. 70) of a different and potentially more accurate system of radio navigation which, instead of using omni-directional beacons, would provide a narrow beamed signal to the aircraft, providing it with the desired azimuth of attack. A receiver in the aircraft would modify and retransmit the signal back to the ground station. By comparing the transmission and retransmission signals the ground station could compute the distance to aircraft, and when the aircraft was at the correct range (i.e., over the target) would radio the bomber a signal to drop its bombs.

A second prewar radio navigation system, the Lorenz beam, was used for blind aircraft landings, i.e., for extremely short ranges. This system involved a ground station with two antennae at a slight angle to each other, each capable of transmitting a fairly wide directional beam. The ground station would transmit dashes from one antenna interspersed with dots beamed from the other antenna. Because the overlap of the directional beams from the two antennae was quite narrow (even though the two beams themselves were wide) the overlap provided an accurate navigation aid. The aircraft crew used the beam by flying to one side when it received only dashes and to the other if it received only dots. When the aircraft received a steady tone, with the dots fitting exactly into the spaces between the dashes, the aircraft was within the narrow overlap of the two antennae and on course. By mounting the two antennae on a turntable the Lorenz beam could be aimed in any direction. Before the war it was an open question scientifically whether short wavelength radio beams could bend around the earth's surface

sufficiently to make a Lorenz-type system operational at long range. Jones realized that if such beams could propagate at long range, a system of two intersecting Lorenz-type beams would provide a highly accurate night bombing system. He also realized the potential vulnerability of this system to jamming (by spurious dots or dashes) or "bending" (by spurious synchronized dots or dashes). This background suggested to Jones that he should look for various radio beam systems (p. 85).

This possibility was reinforced by two further pieces of evidence obtained early in 1940. First, two Luftwaffe POWs were overheard discussing the "X-Gerat" or "X-Apparatus" which involved pulses, which Jones presumed were radio pulses (p. 84-5). In March a fragment of paper was recovered from a downed Heinkel bomber which mentioned "Radio Beacon Knickebein from 0600 hours on 315°" (p. 85). Jones observed this bearing was meaningless if the Knickebein (or "Crooked Leg") were a beacon of the traditional sort, but made sense if "Knickebein might be some kind of beamed beacon which that day had been set to transmit in a north-westerly direction" (p. 86). Using this clue, the POW interrogators obtained the further information that Knickebein was something like the X-Apparatus and that a short wave beam was transmitted that was no more than a kilometre wide over London. This was consistent with physics since a narrow beam required a short wavelength. Furthermore, Jones learned of rumors from the French that the Germans had been setting up some kind of radio beam station on their frontier (p. 86). Based on physical theory, Jones believed that centimetric wavelengths could propagate around the curvature of the earth sufficiently to be of navigational use over Britain, and also rendering

them subject to interception by the British. In his first report to the new Prime Minister on 23 May 1940 Jones described the possibility that the Germans "have developed a system of intersecting radio beams ... (with) accuracy of location expected by the Germans ... something like 1/2 mile over London from the western frontier of Germany" (p. 89). The next step was to determine whether in fact the Luftwaffe was using such beam systems.

That the Luftwaffe beam capability in fact existed was determined in the ten days 12-21 June 1940. Four sources provided the evidence: POW interrogations, technical evaluations of captured Luftwaffe equipment and documents, intercepts and decipherment of German Enigma communications, and finally the actual interception of the beams themselves by specially equipped RAF beam-hunting listening aircraft (See Figure 7.4 for a chronology). Since the night bombing of Britain intensified in August and began in earnest in mid-September, the detection of the Knickebein beams and the compromise of their operating characteristics in June gave the British the time needed to have countermeasures ready when the night attacks finally came (e.g., between 7 September and 3 November an average of 200 bombers attacked London nightly, Lewin, 1978; 97). In fact, the compromise came so early (probably somewhere in mid-stage four of Figure 7.3) that doubts were raised in the RAF that the Luftwaffe actually intended to adopt the Knickebein capability (p. 123).

It fell to Jones to determine the Luftwaffe intentions regarding this night bombing capability he had discovered. On 27 July two pieces of information completed an emerging picture of Luftwaffe plans: a captured document noted the availability of special Luftwaffe squads to fine tune

Figure 7.4. Chronology of the search for the Knickebein beams. Jones' inferences and estimates in parentheses. (Based on Jones, 1978, ch. 11 and Lewin, 1978, ch. 3.)

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12 June 1940 Enigma decrypt: "Cleves Knickebein is confirmed at position 53° 24' north and 1° west," from Flieger Korps IV. (Confirmed the suspected location of Knickebein ground station at Cleves.) Jones determined FK IV equipped with Heinkels 111s of Squadrons 4 and 27. POWs of these units termed the Knickebein receivers unfindable. (Jones concluded receivers were so obvious that they would be overlooked.) British technical exploitation of downed Heinkels radio gear indicated it was unremarkable except for the extreme sensitivity of its Lorenz receiver. (Jones concluded the Lorenz gear was the Knickebein receiver and determined the frequency range as 28-35 megacycles with preset frequencies at 30, 31.5, 33.3 mc.)

13 June Lindemann, convinced by Jones of beam threat, warned Churchill, who ordered a thorough examination.

14 June POW reported Knickebein is a bomb-dropping device using two intersecting radio beams transmitted from Lorenz-like towers. (Supported earlier Jones hypothesis of intersecting Lorenz-type beams.) Air Minister appointed coordinator for beam investigation.

15 June Night Interception Committee focused on organizing intelligence collection effort against the beams: ordered formation of beam-finder aircraft, beam receivers mounted on radar towers, development of jammers and detectors.

17 June Intelligence collection tasks issued to operating units, teams of beam-hunters organized.

18 June Beam-hunter teams briefed. Captured Luftwaffe document located second Knickebein ground station in Schleswig Holstein, gave Cleves frequency of 31.5. (Confirms Jones' estimate of 12 June.)

19 June Beam-hunters failed to find beams.

20 June Beam-hunters failed to find beams.

21 June At War Cabinet meeting Jones convinced Churchill of the beam threat but RAF remained unconvinced and threatened to cancel beam-hunter flights. Captured document confirmed Schleswig Holstein frequency as 30 mc. (confirmed estimate). Jones insisted on beam-hunter flight, guessed Derby as the target area.

Figure 7.4. (Continued)

22 June 1940	Beam-hunters detected two Lorenz-type beams intersecting in the vicinity of Derby operating on frequency of 31.5, one beam emanating from Cleves, the other from Schleswig Holstein. (Confirmed beam characteristics, frequencies, locations.)

the Knickebein receivers for long range operation, and an Enigma intercept stated that one of these squads was requested by Luftwaffe Bomber Group 54 beginning 5 August. Jones combined this with three additional facts and put the new information into a context which permitted him to forecast the future night threat: he knew Bomber Group 54 operated in the Liverpool-Manchester-western England area, out of range of the Cleves Knickebein station. A month earlier an Enigma message noted a proposal to set up Knickebein installations at Cherbourg and Brest, which would be in range of western Britain. On the basis of these facts Jones deduced (p. 123) "the information implied that the foreshadowed Knickebein beam near Cherbourg was coming into operation early in August. I was able to give this warning on 4th August: major night bombing appeared imminent." The Luftwaffe night attacks began towards the end of August but by then the British countermeasures were ready to jam the Knickebein system. By the end of September "the knowledge that Knickebein was jammed spread through the Luftwaffe" (p. 129).

Lessons Learned: The triumph over the Knickebein beams provided several useful lessons which were promptly applied by Jones and his growing scientific team to other Luftwaffe beam systems. The detection of Knickebein on 21 June confirmed the soundness of Jones' method of carefully matching intelligence collection capabilities to key intelligence questions. This method required Jones to keep all his various sources informed daily of the significance of the information they had already provided him and [redacted] essential pieces of information that were still needed to determine exactly what type of system the Knickebein was. Knowing what to look for focused the efforts of the

intelligence collectors: the POW interrogators, the decipherment experts at Bletchley Park, the RAF Y-service interception teams, the technical exploitation experts. Jones' formulation of the physical aspects of the problem provided the information needed to create new intelligence gathering capabilities. For example, his inferences about the operating frequencies of the beams and the location of the first two Knickebein stations allowed the RAF to quickly equip intercept aircraft able to intercept the beams. Jones was forced to guess where these "beam hunters" should look on 21 June, but his estimate that the night bombers' target would be Derby was informed as well as lucky. The Luftwaffe tactic at that time was to put the RAF out of the war and Derby was the center of Spitfire and Hurricane fighter engine production.

The [illegible]f characteristics of Jones' estimation methods were (1) the consideration of several explicit models of the Luftwaffe threat (e.g., single beams, multiple intersecting beams) which could be disconfirmed or supported by existing or future intelligence sources, (2) an estimate and a plan for the application of various collection assets to those parts of the problem which needed clarification, (3) a prediction of what the missing elements of the problem would be like, (4) thorough pursuit of leads using those sources most likely to provide further information, and (5) continuous monitoring of sources for significant deviations or developments. It is also important to note that Jones did not attempt to exploit every possible source of information, e.g., photographic intelligence and British secret agents played no role in the detection of the beams or their exploitation for intention estimation. Jones concentrated on the most rewarding sources and scanned them for the details he knew would eliminate the remaining mysteries of the Knickebein.

Using the same analytical techniques that he had developed in the Knickebein battle Jones made several important estimates of Luftwaffe intentions in the battles against the X-Apparatus and the Y-Apparatus, successors to Knickebein. Three of these estimates of German intentions are briefly sketched.

Predictions of nightly bombing targets. By October 1940 the characteristics of the X-beam system were well enough in hand that, if the nightly beam settings radioed to the ground stations could be deciphered, Jones would be able to provide two to three hours warning of the Luftwaffe's targets. By late October the Bletchley Park code-breakers had developed the ability to decode the beam setting on about one night out of three. On these nights Jones was able "to tell the Duty Air Commodore at Fighter Command the exact place of attack, the time of the first bomb to within ten minutes or so, the expected ground speed of the bombers, their line of approach to within 100 yards, and their height to within two or three hundred metres" (p. 139). These estimates were derived entirely from the established characteristics of the X-beam system and, of course, Jones was able to confirm his predictions by determining the following day where and how the Luftwaffe had actually attacked.

Predictions of the "Moonlight Sonata" - Coventry fire raids. By November 1940 the path-finder bombers equipped with the X-beam system were conducting nightly raids against two targets. On 9 November a long Enigma decode referred to plans for a major operation named "Moonlight Sonata," and increased Luftwaffe radio traffic since 3 November also portended a large operation. A second decode on 10 November

instructed the beam stations to prepare for operations against three targets which Jones identified as Wolverhampton, Birmingham, and Coventry. The bearings for these targets were sixty times less accurate than was the German's practice. Furthermore, three targets were more than ever noted before and never had there been advance warning of more than a few hours to the beam stations while this time notice was given several days in advance. Jones deduced (p. 147) that the path-finders would drop flares or incendiaries, which require less accuracy than conventional explosives, and would be followed up by large numbers of bombers. But neither the beam evidence nor the Enigma data permitted a prediction of the target, only a warning that Luftwaffe tactics were changing. Jones provided the correct frequencies used on the night of the Coventry raid (the first of the big fire raids), based on the pattern of settings of previous nights (p. 149). This lucky prediction did not help the British countermeasures effort however, since, although the RAF used the correct jamming frequency, its signal was 1500 cycles per second whereas the X-beam was 2000 cps. The fact that the X-beam receivers were fitted with frequency filters which effectively screened out the British jamming was discovered only after the Coventry raid.

Although Enigma decipherment allowed a prediction that the next target was Birmingham, the warning came too late to provide any augmented defenses. However, this left only the last target, Wolverhampton, and Jones convinced the Anti-Aircraft Command to prepare. The fact that no attack came was initially taken to mean that Jones' prediction had been wrong. But within a few weeks two POWs, overheard discussing the Coventry and Birmingham raids, mentioned that a similar raid against

Wolverhampton had been planned but had not come off. Subsequently German discussions of the security of the X-beam stations were intercepted which revealed that Luftwaffe photoreconnaissance had detected the increase in anti-aircraft batteries just prior to the scheduled Wolverhampton raid. In light of this serious breach of their security, the Luftwaffe canceled the attack. As Jones writes (p. 153), "in contrast to all the other 40 or so targets for which we had intercepted the beam instructions, target No. 51 (Wolverhampton) was never attacked." Jones had just barely missed becoming a victim of what Handel (1976, see Section Four) terms the "self-negating prophecy" -- the fact that a valid countermobilization taken on the basis of a warning of an attack may prompt the enemy to delay, or cancel the attack, making it impossible to know if the warning was correct and the countermobilization justified. Enigma provided not only the information to predict German intentions, but also assisted in determining how accurate and effective such predictions were.

Pre-empting Luftwaffe counter-countermeasures. Neither the Knickebein nor the X-beam systems conformed to the model predicted in the Oslo report; a single beam that would provide both bearing and range to the target and send a signal for bomb release. Throughout 1940 Jones kept watch for such a system, accumulating several fragments pointing in that direction. On 6 October a strong clue was finally obtained: an Enigma intercept gave a bearing for the "Y" system which, unlike the Knickebein and X-beam bearings, referred to a single ground station. The RAF beam-hunters also reported signals on a new set of frequencies, unlike those of the Knickebein and X-beams. Jones and the RAF had enough information to begin preparing a counter to the Y system.

In January a document found in a downed bomber confirmed the Oslo report of a single beam system which combined bearing and range, and it also provided the locations of two of the Y stations. By this time the Luftwaffe had readied the Y-beams for operational use, having concluded that the British were effectively jamming the X-beams. On the first night that the Luftwaffe chose to replace the X-beam with Y-beam pathfinders, the British also, but coincidentally, chose to try out their Y-beam countermeasure. This countermeasure had the effect of ruining the accuracy of the Y-beam system without itself being detected. Jones describes (p. 176) the results:

> The effect was very satisfactory. One aircraft became involved in a puzzled exchange with the ground station, which informed him that he must have a wire loose in his receiver ... Over the next few nights, we gradually turned up the power ... and the Germans realized the system was unusable ... we had effectively countered the system from the very first night on which it was to be used on large scale, and this by itself completely shook the German confidence.[9]

Self-analysis. Jones made several observations about his own intelligence methods and recommendations regarding the organization of intelligence. He recommends that intelligence analysis organizations should be as small as possible with as high a level of talent as can be obtained. Collection of information and its collation and analysis must be the responsibility of a single head (p. 494, 523) whom Jones recommends, should have the authority of a military commander (p. 517).

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[9] George noted (1959: 143), assessing the British propaganda analysis of the V-weapons, that "Hitler was particularly gratified by the fact that the V-1 flying bombs did not depend upon radio beams for their aiming, a fact that made it technically impossible for Allied defenses to deflect them from their course." Hitler's gratification is further tribute to the British "beam-benders," but his faith was misplaced: Jones deflected the V-weapons by passing false impact data to the Nazis via the doubled Nazi agents controlled by British intelligence (see Jones, 1978: 416, 420 and Masterman, 1972: 253-5).

Dual responsibility or overlapping terms of reference are to be avoided: the intelligence agency should have an exclusive charter for its operations (p. 515).

With respect to his own analytic methods Jones uses two analogies: the human brain and its senses, and the "chain of evidence." Intelligence uses a variety of collection senses (sources), when one of these detects an indication of interesting activity in its own domain, the intelligence organization then turns other senses onto this activity to supplement the first (p. 333). The analysis of intelligence entails converting information, initially categorized by source, into information categorized by subject (p. 493), as does the brain. Jones seems to see the chain of evidence as central to this conversion process. He writes (p. 353):

> My real strength had been ... that ... I had done all my own work myself, and had forged out every link in the chain of evidence, so that I knew exactly what its strength was. Everyone else ... had had to depend on work done for them by (others) ...

and elsewhere (p. 523):

> You must employ as few links as possible between the source [of the intelligence] and the operational staff who make use of the information.

According to Jones the "cardinal principle of intelligence" is Occam's Razor: "hypotheses must not be multiplied unnecessarily" (p. 523). The simplest hypothesis formed the strongest chain with the fewest links:

> Time after time when I used Occam's Razor in intelligence it gave me the right answer ... But every now and again it will be wrong ... By accident you may just have collected a set of facts that can be explained by a simpler hypothesis than what is really occurring; the

> answer is never to be satisfied but always to search for fresh facts and be prepared to modify your hypothesis in the light of those facts. But in general Occam's Razor gives much the greatest chance of establishing the truth. (p. 373)

Jones' scientific intelligence analysis, like the work of the OIC, was "all-source," all intelligence information was potentially available to him. But, like the OIC, Jones did not attempt to direct all of the various intelligence collection methods onto a problem. Rather he used each within its own "domain" and supplemented it as anomalies or interesting developments were noted. Jones seems to have made no attempt to amass all data from every possible source, to the contrary he frequently pursued a problem to a successful conclusion without using some sources at all. The beam battle estimates were made virtually without photo intelligence or secret agent sources, the V-weapon discoveries were made almost entirely without the assistance of the Enigma-based Ultra intelligence (p. 348). In part, this success in selective application of all-source capabilities seems due to Jones repeated application of Occam's Razor: favoring the simplest model that fits what is known and also provides an answer to the problem.

A second element of Jones' success appears to be his ability to extract extraordinary information from single sources and reach significant conclusions from seemingly innocuous reports. This ability to exploit sources for the maximum information, "milking them dry" so to speak, resulted from his familiarity with the problem he was pursuing and with his sources. He knew the key elements to the solution of the problem and he knew which sources could provide those keys and how reliable the information would be. In short, Jones was fully prepared

to recognize significant information and place it into context. His scientific background allowed him to solve mentally most of the problems he faced. However, usually more than one solution was possible. But Jones would mentally calculate all the simplest solutions, and with these hypothetical results in mind, it was largely a matter of waiting for the best-suited sources to provide the Germans' own answers, which Jones was ready to recognize at once.

FEATURES OF INTENTION ESTIMATION SUCCESSES

Several features are common in these three descriptions of intention estimation success. These commonalities can be analyzed to determine why they may have contributed to the success.

Wartime: Each of the three accounts related estimation successes during the course of World War II. As was noted in Sections 3 and 4, many intention estimation failures are failures to predict the outbreak of war, while fewer failures seem to occur during war itself. That is, the environment of peacetime may be far less conducive to intention estimation than the environment of war.

There are several reasons why the intentions of another nation may be easier to estimate in wartime. There are also many reasons why intention estimation may be more difficult during hostilities. For example, during war the security and secrecy of a nation's plans and intentions receive much higher priorities than during peace. Even within an open, democratic nation, in wartime a veil of secrecy is drawn over many facets of government. Embattled people acknowledge the need for secrets and loyally aid the state's efforts to maintain tight security. (The thirty years of silence that preserved the Ultra secrets despite the thousands who were "in the know" are one measure of the depths of this widespread dedication to security.)

In addition to secrecy, war brings physical barriers which slow or prevent the flow of information and people: borders are closed, the press and media are censored and restricted, travel and trade are impeded or stopped, mails no longer move between belligerents, contacts between nations are ruptured along a spectrum ranging from the

state to the individual. Spies for the belligerents flock to neutral capitals to restore in some slight way this lost window on the opponent. These things make intention estimation more difficult in wartime.

But war also makes intention estimation easier. Although war may be too serious to be left to the generals, its execution, that is, its tactics must be placed in the hands of the naval and military professionals. The professional conduct of war tends to be universal -- few are the tactical tricks that have never been tried before. And war is prosecuted by military forces which must operate within the constraints of their capabilities. This bounds the problem for the wartime intention estimator: he knows what the enemy can generally do and he knows generally how the enemy might proceed, he need not ask if the enemy's intention will be hostile, he knows the broad objective of the opponent is the defeat of his own nation. In war there are few debates about the charity or reasonableness of one's opponent. (This fundamental assumption of the singlemindedness of the enemy makes estimation of enemy intention difficult when the time approaches for war to end. This is when the debates are renewed: will the enemy fight on madly in an Alpine Redoubt, die to the last man defending the Emperor? Is any form of surrender that is less than unconditional reasonable? Will the enemy, after the war, still pose such a threat that his nation must be divided, or "agrarianized," or bombed back into a stone age? Who must be held responsible for the war crimes of the enemy if the surrender is to be fully complete: the Fuhrer, the Duce, the Emperor, the generals, the common people? These debates occur because there are doubts about the general intentions of the post-war enemy.)

The instruments and physics of war are generally known to all; the dimensions of war behavior are constrained by the forces and weapons available. These can be used in many ways against many targets but the wartime analyst can be reasonably confident that the instruments of enemy behavior will be limited to these military capabilities (whose characteristics can be generally estimated) and will probably not extend far into the realms of culture, trade, and diplomacy. Nor can the enemy "hide" a military capability very long by not using it. The peacetime analyst has no such confidence. In this sense, it might be said that the ways of war hold few surprises; indeed the two surprises which changed the "physics" of World War II by orders of magnitude were at least anticipated by all the belligerents. The warring states all knew the atomic bomb and Ultra-like codebreaking were possibilities, although not all states thought of them as realities.

Many of the belligerent states neglected intelligence of all kinds in the interwar period. (Britain had no official photo reconnaissance service, America had no central intelligence or cryptologic service, Japan had built virtually no human intelligence networks which could be expected to survive in wartime America or Britain, etc.) Peacetime diplomacy was (and still is) often conducted without the aid of explicit efforts to estimate the intentions of other nations. Statesmen intuitively "knew" what to expect from the Germans, or the Americans, or the British, or the Japanese. War destroyed these illusions and forced the belligerents to turn to intelligence to prevent more surprises. Intention estimates, seen as unnecessary or impossible before war, were in great demand.

The interplay between belligerents is far more intense and concentrated than the interrelationships of states at peace. Acts which in peacetime seem inconsequential take on great importance in war. The fact that German leaders never lied to their people about German power is a cultural curiosity in peacetime, in war it is the stuff out of which one estimates what weapons the Germans have. With the wartime demand for intelligence and intention estimates come expanded legions of analysts to watch and ponder the closely-woven fabric of combat interplay. The constant thrusts and paries which were the daily, worldwide character of World War II provided abundant grist for each intention estimator's mill. In peacetime, the analyst may see too little behavior between his own nation and another to make any reasonable estimates of intention. In war the analyst will see so much that his role may become highly specialized and focus on one small aspect of enemy behavior (e.g., propaganda). This abundance of behavior provides the analyst a fast-moving and varied picture against which to compare his hypotheses of what the enemy will do.

The density and rapidity of warfare do not, by themselves, insure successful or easy intention estimation. An organizational machinery for the collection and processing of data on the flow of events is necessary. David Kahn (1978: 528-531) argues that intelligence and, in particular, estimates of the enemy's intentions are essential characteristics of the defense, and only contingent characteristics of the offense. In each of the three examples examined above the estimators' nations were on the defense. Intelligence was essential to this defense and it received resources and priority to develop sources and methods which could penetrate enemy intentions. For Britain and America in the early war years it was essential to determine where the next blow would

fall to be ready to parry it. Without intelligence no defense could succeed. Kahn (p. 528) writes: "Knowledge of enemy intentions, is necessary for success in defense." So even while the Allies neglected intelligence before the war, they soon devoted a high level of effort to it and especially to learning the enemy's plans. Having developed a successful intelligence structure during this opening defensive stage, the Allies had the advantage of large, functioning intelligence systems when the opportunities came to go onto the initiative:

> When the German tide of war ebbed, and the Allies seized the offensive, they reaped all the advantages that this extensive organization and greater experience gave them. Their information, in contrast to Hitler's, was high-level, voluminous, and reliable. And they used it to speed their victory. (Kahn, 1978: 530)

War summons human efforts and sacrifices which in peacetime would seem irrational. The intention estimators, no less than the front line warriors, gave heroic efforts and called for selfless dedication from their colleagues. In many respects the strain of war on the estimators was like that on the generals, admirals, and heads of state: they were well aware that the lives of many innocents hung on their decisions. One reads their accounts of these wartime efforts and concludes that the intention estimators gave their very best.

One cannot conclude that war favors the work of intention estimator and peace does not. But the tasks of estimation differ in the two environments as do the resources available. Until detailed accounts of peacetime intention estimation successes are available it will remain an open question whether the mental processes and methods of the estimators also differ in the two contexts.

Models: Each of the three accounts specifically mentions the use of explicit models of enemy behavior. George's outline of the propaganda analysts' model was the most detailed; Jones' mental models were based largely on applied physics; the Tracking Room's model was founded on U-boat communications, capabilities and tactics. These models formed for the analysts the rough outlines of what the enemy could do in the future and how he would act. As current intelligence was fitted into these models, specific opposing hypotheses were drawn. The models also allowed the analysts to extrapolate specific enemy action from the various hypotheses, preparing them to recognize the significance of the actual action when it was taken.

Strong Inference: The use of explicit models facilitated the formulation and testing against each other of alternative hypothetical estimates of enemy intentions. Each of the three accounts stressed this pitting of competing hypotheses against the evidence. Two of the three accounts (George and Beesly) noted the possible problems of analytic bias which the use of an explicit model might engender, but both also noted that, by elaborating and testing multiple hypotheses, this problem was largely overcome. Jones added his belief that, in testing hypotheses against each other, parsimony and simplicity were important criteria in selecting the best prediction. A similar attitude seemed implicit in the approach of the Tracking Room, that predicted courses which fit the known facts were not to be discarded even if the facts were dated.

Self-reliance: Both Jones and Winn believed that intention estimation was an individual, not a committee or team, process. While George does not assess the organizational aspects of the FBIS estimation effort, it seems clear that each FBIS analyst composed his

own estimates on the basis of personal analysis. However, the stress on this "one-man rule" was far stronger in the two "all-source" instances of Winn and Jones than in the case of the propaganda analyst.

The all-source estimators had to master the intricacies of each of the intelligence sources available to them. They had to know its strengths, weaknesses, blind spots and potential capabilities. Winn and Jones had to know how to assess and weigh each fact against their knowledge of the source and the problem at hand. As Jones put it (p. 353), they had to forge each link in the chain of logic personally to know just how strong an estimate they could actually make. Knowing the capabilities of their sources, Winn and Jones knew which questions each source could be expected to answer and with what degree of certainty. By piecing their estimates together themselves, they knew how strong a case they had.

By doing all of the analysis himself, the intention estimator was also better able to note discrepancies and inconsistencies in the pattern of any one aspect of enemy activity or in any one source of intelligence. This sense of the "norm," as Denning termed it, would probably not emerge as easily in a team or committee effort: in fact, as demonstrated in George's contrast of the FBIS analysts and their British counterpart in the example of the V-weapons, even the individual may miss a deviation from the norm if his attention is only sporadically turned to the activity or the source. Each of the three accounts dramatically conveys the quality of estimates and inferences which can be based on a methodical evaluation and exploitation of patterns in a single intelligence source.

Intimate familiarity with all aspects of the estimation process seems also to have assisted Jones and Winn with the "front end" of the process: guiding the collection of intelligence. Because Jones and Winn had explicitly formulated the intention questions they were attempting to answer and knew the type of answers they expected to get, they were better able to guide the collection effort to secure the data which would give them their answers. In Kuhnian terms (Section 6), they were able to utilize the transformations of vision which a full familiarity with a scientific paradigm brings (Kuhn, 1970: 111).

For example, Jones knew the Germans were developing rockets and he suspected several of the Nazi radar stations, whose messages he could read via Ultra, would be tracking the rocket tests. By focusing a collection and analysis effort on this set of radar sites Jones obtained a complete record of the rocket test flights. Had he not known that the tests and tracking were necessary and where they were likely to be, he would not have known where to look for the right data, or, it is likely, have recognized it if it had later fortuitously appeared. A second example of the pay-off of self-reliance occurred when Jones examined an aerial photograph of Peenemunde which had been previously analyzed by an expert photo interpreter. Unlike the expert, Jones noted the first image of a V-2 rocket, because he knew what to look for and he suspected where it would be located. The expert was also looking for rockets but it is doubtful that he had as clear an image of this never-before-seen

object as did Jones,[10] who had already sketched its outline on the basis of his background knowledge and other intelligence sources. Furthermore, Jones was able to prepare other analysts to better recognize the answers he sought. On the basis of radar trackings of the V-1 tests he knew when and where to expect the next test of the V-1. From various agent reports he had a firm idea of how the V-1's were launched and what they looked like. He was thus in a position to request a photo reconnaissance mission over the right place at the right time and when he got it, the photo interpreter found the V-1 in the place it was expected to be.

Influence and Charter: Jones and Winn had an extremely high level of influence over the military and political leadership of wartime Britain. Their estimates were heard in the highest councils and formed the basis for major military operations.[11] Furthermore, the Tracking Room and Jones' Scientific Intelligence Division received an implicit charter for the work they were doing: although other agencies might

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[10] Jones (p. 339) makes clear that the photo interpreter was "less ready" to see the rockets than someone who knew what to expect: "... (W)hat one could see in a photograph was often a matter of subjective interpretation. ...(T)he principal interpreter assigned to the task supplemented his powers of observation by a remarkably fertile imagination. What were in fact catapults for flying bombs were, for example, interpreted as 'sludge pumps,' a theory perhaps coloured by the interpreter's previous experience as an engineer with a river Catchment Board after his Cambridge Ph.D. thesis on classical hydraulic engineering ..."

[11] Beesly notes (p. 259) that "O.I.C. had no executive authority: it merely provided information and appreciations in the light of which other departments took action. Plans, Operations and Trade Divisions were, in theory, perfectly entitled to ignore the advice proffered to them: the fact that they so rarely did so is an indication of the influence which O.I.C. exerted." Elsewhere (p. 253) he notes that "no maritime operation" ever took place without consultation with the OIC staff.

participate in the solution of a particular puzzle, Britain's leaders insisted that these estimators be consulted and kept informed. This positive reinforcement from the top levels demonstrated to Jones and Winn the high respect and value given to their estimates. It also demonstrated to others the high priority their estimation efforts enjoyed with the nation's leaders. Jones was frequently encouraged by Churchill to use the Prime Minister's name if he found his intelligence work impeded. Jones noted how the attitude of the RAF photo interpreters on whom he relied became far more cooperative when they discovered that Jones was in a position to include favorable references to their work by name in reports going directly to Churchill (p. 108, 134).

It would be inappropriate, however, to attach too much significance to this high influence and exclusive charter which supported Winn's and Jones' intention estimation efforts. The FBIS analyst enjoyed no access to the high circles of policy and strategy but still produced consistently timely and accurate estimates of Nazi intentions. (But note that one of the very best estimates reviewed by George, the British V-weapons analysis, was specially commissioned by top intelligence leaders.)

Nevertheless, the similarity in the way in which Jones and Winn first emerged in the limelight and the similar tactics they used to resist efforts to displace them from their positions of influence are noteworthy. This similarity of the two patterns seems unlikely to be purely coincidental -- they are probably important components to the political aspects of intention estimation.

Jones took his post the day World War II began and was immediately called on to assess a statement by Hitler which seemed to betoken some secret German weapon that might be used to attack the British

Isles. With the assistance of some linguists Jones concluded Hitler was referring obliquely to the Luftwaffe. This early, convincing defusing of an important flap in the first panicky days of war put Jones in a favorable light with his superiors in both the Secret Intelligence Service and the Air Staff. He passed this baptismal test successfully.

Jones was a former colleague and student of the two most influential scientists in wartime Britain: Tizard and Lindemann (later Lord Cherwell). Both knew and respected his prewar work in applying physics to air defense problems, and Tizard was responsible for Jones' appointment with SIS and the Air Staff. Having these highly placed associates was less critical to Jones' influence than his relationship with them later, during critical crises. There were many other scientists, equally close or closer to Tizard and Lindemann, who never shared the measure of confidence Churchill and the British military leaders placed in Jones. But his access to these men brought Jones' work to Churchill's attention.

Jones stepped into the limelight during the beam battle. On 21 June 1940 this 28 year old scientist was summoned (largely at Lindemann's urging) to Churchill's cabinet meeting to explain why Jones believed the beams existed (see chronology, Figure 7). The meeting followed two days of fruitless searching by the RAF beam-hunters, equipped according to Jones' own specification. At the meeting Jones found confusion over the nature of the beams and opposition from his mentor, Tizard. In twenty minutes Jones related the history of his beam intelligence effort and the counterbeam measures he contemplated or had set underway. Churchill later described to Jones the effect this meeting had on him (Jones, p. 108):

> ... (H)aving surveyed our position in the early weeks of June 1940, he thought that we ought just to be able to hold the Luftwaffe by day. And then, when this young man came in and told him that they could still bomb as accurately by night, when our nightfighters would still be almost powerless, it was for him one of the blackest moments of the war. But as the young man went on the load was once again lifted because he said that there could be ways of countering the beams and so preventing our most important targets being destroyed.

That same night the RAF beam-hunters found the beams exactly as Jones had predicted, at the place he had guessed they would be. This personal triumph Churchill seems never to have forgotten: thereafter he recalled Jones as (quoted in Jones, p. 516): "the man who broke the bloody beam." It was not that Jones had made so many correct estimates and predictions, but that he made this one crucial prediction directly to Churchill despite his mentor at the exact moment it was needed, and he was right. Jones henceforth enjoyed an unshakable heroic reputation with Churchill.

Beesly's account of Winn's first days as head of the Tracking Room is remarkably similar to Jones' first success, albeit, pitched at a lesser level. While still Thring's deputy, Winn had argued for the possibility of predicting the U-boats' movements. Thring's disbelief in estimating intentions has been quoted above. How Winn resolved this dispute is not exactly clear, but several factors suggest the following story holds the key. Beesly writes (p. 57) that the truth of the story is hard to vouch for as he could not interview the participants.

> ... Captain Edwards [Head of the Admiralty's Operations Division] came down to the Tracking Room and observed on the plot that there were two valuable tankers homeward bound and in close proximity to each other. Some distance ahead of them on the plot was a tab indicating the position, deduced from a D/F fix, of a U-boat. Edwards asked Winn whether he had any ideas as

> to the U-boat's future movements and whether it would endanger the tankers. Winn's view was that, in view of the length of the signal the U-boat had just made, it was probable that it was about to start its return to base, and that if his theory was correct, it might well intercept one or both of the tankers. Edwards decided to make an experiment, and obtained Trade Division's somewhat reluctant agreement; one tanker was diverted to a route that in Winn's opinion would take her clear of the probable path of the U-boat; the other tanker, in accordance with Trade Division's wishes, was left on its original direct Great Circle course to her destination. Next morning Winn's guess, for it was no more, was proved right. The tanker which had been diverted was unmolested but her unlucky sister was sunk.
>
> Whether it was due to this episode or not, the decision was taken ... that ... Thring must be moved to less onerous duties and that Winn should be ... appointed in his place.

The fact that this story has survived all of its participants and "fits" the pattern noted in Jones' case suggests it might have occurred more or less as Beesly related it. Such lucky first impressions seem to be extremely memorable and instrumental in establishing influence.

In the case of both Jones and Winn the initial success was swiftly followed by more remarkably accurate and useful predictions. Nevertheless, it seems likely that it was their initial impressions that made their superiors attend closely their subsequent efforts, and coming to appreciate their highly accurate track records, to forgive them their occasional lapses.

Both Jones and Winn defused many of their critics by giving them all the available intelligence information and letting their critics try to draw the proper inferences from the data. The Naval Staff rarely challenged the Tracking Room experts but when they did, Winn dealt with the criticism directly, as Beesly (p. 157-8) describes:

> ... Admiral Sir Max Horton, ... Commander-in-Chief Western Approaches, ... was dissatisfied with appreciations given him by the Tracking Room which had led to an unsuccessful convoy battle, and attacked Winn at one of the fortnightly U-boat Warfare meetings ... Winn accepted the criticism in good part (indeed he had no option), but suggested that if he could be given half an hour, he would lay out all the intelligence available to the Tracking Room at the time the appreciation was made. The Admiral could then examine it himself and decide what different conclusions he would have come to. When Horton arrived in the Tracking Room he was confronted with a mass of Special Intelligence signals, D/F fixes, sighting reports and the last confirmed positions of the U-boats concerned. 'It's all yours, Sir,' said Winn, 'and your Chief of Staff in Liverpool is in a devil of a hurry for the answer.' Horton settled down, but after a period of intense study turned to Winn and ... 'confessed that most of it was outside his province...'he held out his hand and said, "Goodbye, Rodger -- I leave it to you." And thereafter he did.

Jones' technique for dealing with his critics was similar, much less direct but perhaps more effective. When he expected he would have to present a controversial estimate or prediction which was likely to be challenged, Jones would insure that all the raw intelligence was in the hands of all concerned, including his possible challengers. He would hold back his own interpretation of these facts until his challenger had expressed his estimations. These were usually hypotheses which Jones had already considered in detail and he was now in a position to show how his opponent's logic could not fit what was known as well as would an alternative hypothesis. The fact that Jones was sometimes challenged by his longtime colleagues, Tizard and Lindemann, probably added to his credibility when he succeeded in challenging their logic. At one conference on the V-weapons which pitted Jones against Lindemann, as Jones made each of his various points demolishing Lindemann's case,

Churchill would turn to Jones' former professor and remark: "another point against you, and remember, it was you who introduced him to me!" (cited in Jones, p. 345). Because he personally undertook the analysis of each problem in its entirety, Jones was far more familiar with the strengths and weaknesses of various alternative estimates than were his critics, who usually relied upon staffs to develop their estimates.

Source Exploitation: In all three examples of successful intention estimation one can infer the importance of following a particular source of information methodically and patiently, in order to determine the background pattern or norm of activity against which anomalous events might occur. However, the estimators had to pick sources to monitor which had a high probability of detecting an anomaly in a significant area of behavior -- methodically collecting data from a trivial source had no prospect of useful payoffs. The estimation experts had thus to decide what forms of anomalous behavior would be significant and which sources would best detect the shifts in patterns.[12]

For example, the British propaganda analyst anticipated the significance of Nazi references to retaliation weapons and to German power. Focusing on these themes he noted the significant interruptions in the former in August and September and the consistency and veracity of the latter. In another example, R. V. Jones' knowledge of existing radio navigation techniques and the warning of the Oslo Report led him to listen for certain wavelengths and signal characteristics, and to watch for certain kinds of transmitters in the locations where the Germans would be likely to put them in view of their transmission physics.

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[12] Jones (p. 124) and Beesly (p. 168-9) both remarked on the powerful memory that they developed to cope with this monitoring process.

In addition to noting trends, this source exploitation permitted the estimators to use negative evidence in their logic. All three accounts include significant instances of negative evidence used to support one hypothesis and reject others.

Finally, with a methodically collected series of observations on a particular significant aspect of enemy behavior, the estimators could check their explicit models of the enemy against this accumulated record. The models could then be adjusted or the sources could be rechecked if a descrepancy between them was noted. Gradually the models became more complete and the estimators gained a firmer understanding of the limits of their various sources.

In the next section we review theories and experiments in the fields of perceptual, cognitive, and social psychology and organizational behavior to determine why successful and unsuccessful estimates of intentions have the characteristic features noted in Section 4 and here in Section 7.

SECTION 8

EMBARKING ON THE BRAIN OF POOH: EXPLAINING WEAKNESSES IN THE ESTIMATION OF INTENTION

EMBARKING ON THE BRAIN OF POOH: EXPLAINING THE WEAKNESSES IN THE ESTIMATION OF INTENTION

> "Now then, Pooh," said Christopher Robin, "where's your boat?"
>
> "There!" said Pooh, pointing proudly ...
>
> "But it's too small for two of us," said Christopher Robin sadly.
>
> "Three of us with Piglet."
>
> "That makes it smaller still. Oh, Pooh Bear, what shall we do?"
>
> And then this Bear, Pooh Bear, Winnie-the-Pooh ... -- in fact, Pooh himself -- said something so clever that Christopher Robin could only look at him with mouth open and eyes staring, wondering if this was really the Bear of Very Little Brain whom he had known and loved so long.
>
> "We might go in your umbrella," said Pooh.
>
> For suddenly Christopher Robin saw that they might. He opened his umbrella and put it point downwards in the water...
>
> "I shall call this boat The Brain of Pooh," said Christopher Robin, and The Brain of Pooh set sail forthwith in a south-westerly direction, revolving gracefully.
>
> A. A. Milne, 1926

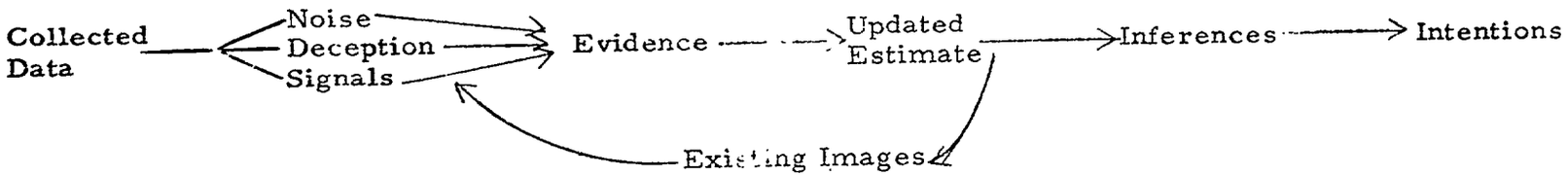
In this section intention estimation is decomposed into constituent tasks which are analyzed from the perspectives of cognitive, social, and organizational psychology. Failures of intention estimation tasks have already received some analysis: e.g., Jervis (1976) examines the role of psychological mechanisms in the formation of perceptions and misperceptions, Wilensky (1967) notes some of the

sociological reasons why organizations may fail to produce good intelligence. It is not the aim to repeat these efforts, but to cover more completely other aspects of the intention estimation task which have had insufficient analysis. Various weaknesses of the tasks are identified and compared to the methods utilized in the successful episodes of intention estimation (Section 7), to identify compensating techniques.

It is important to reassert that, although the analysis of intelligence failures and this section tend to dwell on one task at a time, we believe it is much more likely that faulty estimates of intentions result from complex "causal error chains," as described by Turner (1976a, b) which lead to disasters (see Section 5). That is, while an analyst may make an incorrect judgment about some aspect of an intelligence problem, this mistake alone is rarely sufficient to cause an intelligence failure. Such failures more likely result from concatenations of many mistakes, which culminate finally in an erroneous estimate of intentions. Consequently, to understand these error chains this report has attempted an overview of the intention estimation task, as a whole, as well as an analysis of the strengths and weaknesses of each of its various parts.

Weaknesses of the Individual Analyst. Shown in Figure 8.1. are the various interpretations of intelligence failure at the individual level. There are two main themes in the individual model of failure: the difficulty analysts have in separating signals from deception and noise;

Figure 8.1. Models of Individual Failure



Signal/Noise	Mosaic Theory	Deception	Perceptual Filters	Risk	Climate of Opinion
Wohlstetter Holst	Wasserman	Whaley Wohlstetter	Jervis Shlaim	Handel Wohl- stetter	DeWeerd Wasserman
George & Smoke Handel		Handel	Lampton DeRivera Knorr		

Cognitive Feedback	Self-Negating Prophecy
Steinbruner	Handel Shlaim

and the tendency to fit incoming signals into the context of existing perceptions and images. Some other themes include the official, "mosaic" theory of intelligence, the inappropriate estimation of risks, and the cognitive feedback mechanisms which reduce uncertainty. In the following paragraphs we examine some of the psychological mechanisms underlying these problems, and describe several additional cognitive limitations which are related to the estimation of intentions.

Access to Cognitive Process. Psychologists have taken two approaches to the investigation of how experts make estimates and choices, a mathematical statistical approach and a process tracing approach (Einhorn, Kleinmuntz and Kleinmuntz, 1979; Hogarth, 1974). In the first approach, the psychologists obtain from expert judges a list of the important dimensions of a decision. The psychologists then combine data on these variable dimensions using simple mathematical formula (e.g., linear regression) to predict a choice or produce a judgment. That is, the experts determine the variables and the formulas integrate the information. These actuarial or statistical judgments have been found consistently to be superior to the judgments of the experts themselves (Dawes, 1977; Goldberg, 1968, 1970; Meehl, 1954; Slovic, Fischhoff, and Lichtenstein, 1977).

Process tracing models are developed from the verbal protocols of expert judges and are expressed as computer algorithms (Newell and Simon, 1972). Process tracing is much more concerned with the judges' cognitive processes than is the statistical approach. Furthermore,

while the statistical approach and process tracing focus on information integration and decision, process tracing goes on to attempt to model other phenomena such as attention to cues, cue sampling, and the intersubstitutability of cues.

Intuitively the process-tracing approach appears more promising as a means of determining how expert judges really do solve problems. However, an important paper by Nisbett and Wilson (1977) suggests that, when people attempt to report on their cognitive processes and how they use information to reach their conclusions, they do not report on the basis of any true introspection into their own mental reactions. Instead, these reports are based on a priori, implicit causal theories, or judgments, about the extent to which a particular piece of information is a plausible basis for a particular conclusion. This suggests that even though judges may not be able to directly observe and report on their own cognitive processes, they will nevertheless sometimes provide accurate protocols whenever the information which the experts in fact used to form their conclusions is also a salient and plausible basis for their judgments. When the experts use cues which are not salient or plausible causes for their judgments, they are inaccurate reporters on their own mental judgment process. Nisbett and Wilson's paper implies that asking expert judges how they solve problems will yield uniformly accurate information on how the judges think they are making judgments, but misleading information on how they actually do form conclusions. For example, Goldberg (1968, 1970) found that clinical judges believe they are performing

complicated multivariate judgments when objectively their information-processing policies can be represented by simple linear combinations of evidence using a fairly small number of variables. The general and widespread result of these studies is for people to overestimate the importance they place on cues which actually play a minor role in their decisions and to underestimate their reliance on the few variables which are the major influence (Slovic and Lichtenstein, 1971). As Fischhoff (1976; 432) summarizes: "people tend to exaggerate their information-processing sophistication."

Einhorn, Kleinmuntz, and Kleinmuntz (1979) report a process tracing experiment involving judgments of the nutritional value of breakfast cereals. All the various cereals had similar calories and thus this cue could not discriminate among the cereal brands (calories did not correlate with other cues either). Nevertheless, the judges paid close attention to this cue and believed it was important to their decisions, although logically it was not (because of its low variance over brands) and a statistical analysis showed this cue received no weight in the judges' actual decisions.

In summary, Nisbett and Wilson's (1977) review of a variety of psychological studies indicates that there are signals and cues which may be very influential for an expert intelligence analyst but the analyst may be unaware of this influence. The analyst may be unaware of the signal or cue itself, dispite its effects. Finally, the analyst may perceive some cues and signals as having a great impact on his

judgments when they actually have no discernable relationship to the analyst's conclusions.

This difficulty, of experts being unable to accurately assess which signals are important for their decisions and which are not, was demonstrated in Slovic and Lichtenstein's (1971) review of the literature on the correspondence between the actual and the subjective weights people place on evidence in judgment tasks. Slovic and Lichtenstein found that a wide variety of judges (including many experts) have poor insights into the weights they assign evidence and that their subjective reports tend always to differ from objective assessments of what weights are actually given to evidence. Slovic and Lichtenstein found, however, that there was some correspondence between objective and subjective weights, leading Nisbett and Wilson (1977: 254) to note that this is virtually the only evidence in psychology that "people can be at all accurate in reporting about the effects of stimuli on their responses." However, Slovic, Fischhoff and Lichtenstein (1977: 8) note that many studies of choice behavior have validated the introspective reports of judges or experts against theoretical predictions or data from nonintrospective sources. One problem with this conclusion may be what Nisbett and Wilson (1977: 254) term the lack of "causal theory controls," that is, the expert's subjective weights may merely reflect cultural or subcultural rules which prescribe how evidence is to be used to form judgments. Since the experts sometimes do use formal rules of evaluation, experimenters may occasionally find evidence that

subjective and objective weights in judgment tasks may agree as a result of the judge adhering to a formal evaluation rule (e.g., a military analyst might be taught that concentrations of forces always precede an attack and might apply this formal rule whenever he estimates the probability of an attack, similarly, a college admissions officer might always attend to test scores as important predictor information regarding success in college). Nisbett and Borgida (1975: 4) suggest that "perhaps in fact, it is only when we have rather well-rehearsed schemata [i.e., rules] for dealing with certain types of abstract, data-summary information that it is used in a fashion that the scientist would describe as rational."

Making such rules explicit and open to examination is a frequent recommendation for improving intelligence estimation of intentions (Betts, 1978; Jervis, 1976; Shlaim, 1976; see Section 4), and one which was followed by successful intention estimators (see Section 7).

Information Overload. Clearly one problem for the intelligence analyst is the monitoring of more information sources than can be usefully integrated into an estimate. However, a more subtle problem occurs when the analyst concludes that because he is successful in monitoring more channels, he is necessarily making better estimates than he would if he monitored fewer. Research by Slovic and Corrigan (reported in Slovic, 1979: 19-20) indicates that with more information, horse race handicappers became more confident in their judgments, but the consistency of their judgments decreased as the number of information sources increased, and the accuracy of their predictions was unchanged whether they used 10, 20, or 40 pieces of information.

Up to a small number of information channels, prediction accuracy actually increased, but beyond that point having more sources of information decreased consistency and led to unwarranted overconfidence, with no increase in accuracy. These results reinforce the doubts about the widespread "all-source" approach to intelligence, and the "official theory of intelligence" that an analyst should have "all the facts" before making an estimate raised by Hilsman (1956) and Wasserman (1960). See Section 7 also.

Biases of Memory and Recall. Since the early work of Bartlett in the 1920's and 30's, psychologists have been aware that human memory of complex events is best characterized as a process of reconstruction rather than one of recollection (Norman, 1969). Bartlett found that accuracy of report was the exception when people attempted to recall stories, arguments, or drawings and that what typically occurs in memory is a reconstruction from a general "schema," or an active organization, of the original material. This schema depends heavily on whatever the person perceives to be the isolated and striking details in the original material. Consequently, recall of complex data tends to be shorter than the original, more modern, more coherent and consequential, and these errors increase with time. With increased time there is more "constructive remembering," or invention, and people are often most pleased about and certain of those items they invented than those they accurately recalled.

Posner (1969), Franks and Bransford (1971) and Bransford and Franks (1971) demonstrated that subjects appear to abstract a schema from complex visual stimuli or sentences and use these for subsequent recognition judgments. Further, subjects rated themselves most

confident of having seen schematic sentences even though such sentences were actually never shown the subjects in the original sessions. In related experiments (Barclay, 1973; Bransford, Barclay, and Franks, 1972) it was found that subjects go beyond the information given in complex data and store not only information from the data but also implications and inferences from the data.

Intelligence officers are not immune from the tendency to recollect instances that reflect schematic constructions but which did not exist in the original material. Holmes (1979) reports that U.S. Navy intelligence officers in the Pacific prior to the Pearl Harbor attack had set a watch for the famous "East Winds Rain" code signal from the Japanese. In subsequent investigations of the Pearl Harbor failure some officers recollected having seen the signal, but all available evidence suggests it was, in fact, never sent.

As noted in Section 7, expert intention estimators report developing surprising memories for the material they handled, when they otherwise had no surprising mnemonic talents. It seems quite likely that this experience parallels the highly accurate recall and rapid memory storage feats noted in expert chess players. Such experts can study a chess board for only a few seconds and recall each piece's location, and retain these memories for weeks or months. Such recall is possible, however, only for meaningful board positions; chess pieces which are placed randomly are no more readily remembered by the experts than they are by novices. Studies of the eye movements of the chess experts show they literally focus on the most important strategic relations between the pieces. Similarly, the excellent recall of the successful intention estimators probably resulted from their abilities

to fit each piece of data into a meaningful context, or to determine its promising, potentially relevant features, so that when a new context was formed they could recall the earlier data which could now be fit into the new context (Klatzky, 1975).

In summary, memory scholars (e.g., Klatzky, 1975; Norman, 1969; Posner, 1973) observed that for material to be stored in long-term memory, it must be integrated within the existing structure and fit within the schema created by previous material. This schema-linked storage may bias retrieval, however. For example, Funkhouser (1968) had people classify objects according to color or shape. Subsequently, the people were better in speed and accuracy of recall when allowed to use the original classification scheme than when forced to use a new set of categories. In other words, memory is searched effortlessly provided that the context at the time of retrieval (problem context) matches the organizational classifications made at input, i.e., if the data at retrieval are "context addressable" (Shiffrin and Atkinson, 1969). If the context at retrieval requires a new classification, memory search is more difficult. Typically, however, the problem context matches the input schema only for easy problems. When the two differ, inappropriate data may be recalled (those with input schema similar to the problem context but not necessarily relevant to the problem), or appropriate data may not be recalled, or the problem context may be skewed to match what seem to be appropriate data in a different schema. Schema tend to strongly affect perception as well as memory and are the basis of many well-known perceptual illusions. Illusions result from the tendency (bias) to see all stimuli as reflecting our typical rectilinear world. A vivid example of how perceptual schema in turn influence memory comes from studies

of people's mental maps. Parisians, who normally perceive the Seine from ground level as gently and slowly curving through the city, typically recollect the river as making a shallow 145° bend rather than the sharp, 85° angle of its actual course, which is accurately perceived by aviators and geographers (Milgram, 1977).

Two organizing mental structures commonly used in memory are ordered lists and hierarchies. Both of these structures can bias recall. Many personal experiences appear to be organized in memory as lists, however multidimensional events tend not to be ordered in memory on lists of several dimensions but rather along a single dimension. DeSoto (1961) noted that peoples' impressions of others on a variety of dimensions, such as voice quality and intelligence, tend to be highly correlated: i.e., those judged high in one quality are also judged high on others. This tendency to see various qualities as co-occuring has been termed the "halo effect" by social psychologists. The implication is that, for certain discriminations and judgments, people tend to collapse various dimensions into one, and to generalize from that one dimension to others.

There is strong evidence that memory of concepts has a hierarchical structure (e.g., Warren, 1972). More importantly, statements which are not represented by hierarchical structures tend to be altered in memory so that they can be (Dawes, 1966). For example, Dawes compared memory of statements of the form "Some X are Y" with those of the form "All (or No) X are Y" and found that "some" statements are more likely to be recalled as "all or no" statements than the reverse. This preference for absolute and concrete concepts ("all or no") relative to relativistic or ambiguous concepts ("some") pervades other analytic tasks as well. A further characteristic of hierarchical organization is that properties of

classes are often represented at higher levels (e.g., "birds fly") and these properties are then applied to lower levels (e.g., "eagles fly"). This can lead to misapplication of properties (e.g., "ostriches fly"). Further, properties are often assigned a class on the basis of observations of a subclass or single member (e.g., "eagles are killers," therefore "birds are killers"). Such thinking is termed "stereotyping" and may be due to the organizational structure of memory. The implications of hierarchical memory structure are that class assignments are sometimes inappropriate, and inferences from properties of individuals to properties of classes and vice versa are not always thought out.

Because isolated and striking details of data are important in creating prototypes and schema in memory, there is a strong tendency to recall or recognize unique or striking information with little loss of detail even over long periods of time (Rock and Englestein, 1959). Mnemonists (individuals with greater than usual memory capability) attribute their prodigious memory, in part, to their ability to experience each particular datum as a unique instance (Luria, 1968). There are several implications of this characteristic of memory. First, non-occurrences of events and negative instances are rarely as striking or unique as occurrences or positive instances, and will tend to be less well-remembered. Second, because unique and striking events tend to be well-remembered, relative more commonplace events, they tend to be over influential when estimates are made (see the discussion of "availability" below). Third, the perception of physical stimuli tends to be affected by the so-called "central tendency of judgment," that is, smaller stimulus values are overestimated and larger ones are

underestimated. These inaccurate sensation judgments are compounded when psychophysical sensations are stored in memory, i.e., smaller, weaker values are further overestimated and larger, stronger values underestimated. Since "smallest, least, weakest," and "strongest, largest, most" are striking and unique, they tend to be well-remembered in terms of detail but not in terms of actual magnitude. Further, the "law of sense memory" suggests that the more extreme the stimuli, the more distorted the memory of its magnitude relative less extreme stimuli. In short, perception compresses magnitude, memory does so even more, and compression is most severe for those stimuli most likely to be recalled. Fourth, the distinctiveness of striking and unique data tend to isolate them from other data, thus improving their memorability, but also reducing the ability of the individual to integrate these data with others. In effect, the classification of data as unique protects it from forgetting or interference while in memory, but may also isolate it from further cognitive integration, unless such integration explicitly involves other distinctive cases.

To summarize, the following characteristics of memory will tend to bias intelligence analysis:

o Memory reconstructions streamline, condense, and modernize input data, fitting it to preexisting schema.

o "Constructive remembering" (invention) increases with time and people are often most pleased with and confident of the fidelity of such memories. These constructions tend to reflect the schema used by the individual to organize the input data.

o Recall and recognition tasks which match in context the organization schema of memory inputs can be performed relatively effortlessly. Recall contexts which do not match memory schema require greater mental effort.

o Memory lists fail to capture the multidimension attributes of stimuli, producing the "halo effect," and the assumption that favorable or unfavorable qualities co-occur.

o Hierarchical organization of memories is often misapplied to nonhierarchical inputs, or too rigidly applied, producing stereotypes, the assumption that attributes of members of a class extend to the entire class.

o Striking details tend to be best recalled but also tend to be most compressed in magnitude toward less impressive data. The distinctiveness of striking details may prevent their integration with other data in memory. Since negative instances are generally less striking than positive instances, the former are less memorable and less available for further cognitive work. Striking details will be readily integrated only with other striking details.

Information Integration Biases. The schema which guide memory are composed of "concepts," or the describable regularities of events or objects (Bourne, Ekstrand and Dominowski, 1971). Concept formation is perhaps the most critical element of "information integration:" the ability to combine information from a variety of sources into a single judgment.

Mere exposure to a representative instance of a concept, in general, is not sufficient to allow a person to describe future instances. Rather, the person must experience the crucial dimensions of the

concept changing to perceive the variation of the dimensions across positive and negative instances of the concept. Such experience over a variety of cases allows the individual to progress from knowledge of individual patterns of various dimensions to an abstraction of prototypes, and finally to a definition of the dimensions along which the instances differ. Such a definition provides the concept, and integrates the information provided by the varying dimensions and differing instances into a coherent judgment.

Unfortunately, there is mounting evidence that people do not attend a wide variety of dimensions to form concepts, nor do they attend the variation of patterns and instances sufficiently before forming concepts.

Several studies of judgments by medical and psychological clinicians (Goldberg, 1968, 1970; Meehl, 195'), radiologists (Hoffman, Slovic, and Rorer, 1968), stockbrokers (Slovic, 1969), business managers (Hamner and Carter, 1975; McCann, Miller, and Moskowitz, 1975), admissions officers (Dawes, 1971), financial officers (Ashton, 1974; Libby, 1975), and U.S. Senators (Wainer, Zill, Gruvaeus, 1973), indicate that information integration judgments can be made at levels of validity equal or better than that of the "experts" with extremely simple additive combinations of relevant dimensions (Dawes and Corrigan, 1974). Several of the studies (e.g., Dawes, 1971, Goldberg, 1970) show that it is difficult for experts to be perfectly consistent in applying implicit integration and decision rules over many cases. More, or equally, accurate judgments are made when actuarial formula derived from the expert's behavior ("policy capturing") are applied to judgments, a procedure termed "bootstrapping" (Dawes, 1971). The

actuarial method does not necessarily represent the methods actually employed by the experts, although extensive research by Anderson (1970, 1971, 1973) suggests that it probably does; i.e., Anderson finds individual's impressions tend to be the weighted average of the values of the various component impressions. Goldberg (1968) reviewed research that indicates expert's accuracy is unrelated to either the amount of information on hand or the level of professional training and experience.

Why are experts (and everybody else) ineffective integrators of multidimensional data? Several cognitive heuristics seem to be applied which lead to nonoptimal utilization of information.

Restricted Dimensional Usage: Slovic (1966) found subjects employed only two out of nine dimensions related to success when judging overall intelligence, and when these two dimensions were inconsistent for a case, one was ignored (the preference for redundant, consistent cues rather than independent cues is discussed further below). Typically one or two dimensions are used as a focus and small corrections are made by reference to other dimensions. In Hoffman et al.'s (1968) study of radiologists, six dimensions were deemed important in judging ulcer malignancy, but in fact only two dimensions were used by most radiologists in making actual prognoses. Slovic and MacPhillamy (1974) found that when faced with multidimensional alternatives, people are excessively influenced by commensurable dimensions, those that can be readily compared across choices. In effect, choice decisions were made on the grounds of comparisons of comparable dimensions even when these dimensions were unimportant for the choice. Other heuristics are described below.

Biases of Concept Formation. Psychological experiments on concept formation bear a strong resemblance to the intelligence analyst's task of separating signals from noise. The subject in the concept formation task is confronted with many objects or events varying on many attribute dimensions, only some of which are related to the required categorization problem, the remaining dimensions are "noise." The subject solves the problem by selecting the relevant attributes and the rule for combining them.

As the review of literature above indicates, the difficulty of the concept formation problem increases as the number of attribute dimensions increases. The more attributes, the more difficult it is to find the ones which are relevant to the solution. If more than one combination of attributes can be used to solve the problem (i.e., if attributes are redundant), the rate at which the concept is formed is increased. (However, the application of redundant information has important drawbacks, as is discussed below under "Representativeness" and "the illusion of validity.")

Testing Hypotheses. Once a person has formed a tentative concept, it is necessary to test the concept as a hypothesis against the stream of evidence. There is an extremely strong tendency to detect a pattern or rule underlying a sequence of events, and events that are randomly connected are often seen as following some form of rule. This sensitivity to order means that when people are confronted with an orderly sequence of events they quickly detect the regularities, but when confronted with a disorderly sequence they again detect regularity.

People rarely eliminate all the possible hypotheses that the available range of evidence would permit them to reject. In contrast,

there is a tendency to formulate a hypothesis, test it until it is proven wrong, and then discard it and formulate a new hypothesis. When, occasionally, people select multiple hypotheses to test, they tend to reject some, but not all, of those not consistent with the information (Levine, 1966).

Hypotheses shape the information people remember, and the choice of information to attend, reducing strain on memory, but limiting processed information to just that which is relevant to the current hypothesis. If the current hypothesis proves inadequate, information may be lost which would have suggested new hypotheses. The tendency to form hypotheses before adequate supporting data are available (e.g., when the data are random) interferes with hypothesis testing, since these early hypotheses tend to be held too long, i.e., long after sufficient evidence has been seen to reject them. In an experiment in which subjects expected false as well as valid information against which to test hypotheses, there was a strong tendency to regard data confirming their hypothesis as valid and data weakening their hypothesis as false. The subjects' confidence in their hypothesis increased with any confirming data they received, but confidence did not decrease when negative evidence was received; evidence against the hypothesis was simply rejected as false (Kozielecki, cited in Posner, 1973: 75).

Jervis (1968, and especially 1976) has emphasized how expectations affect the perceptions of statesmen and intelligence analysts. As noted in Section 4, a central principle guiding the process of separating signals from noise is the tendency to fit incoming information into existing theories and images, which in turn determine what is noted. Jervis also asserts that statesmen overlook the fact that evidence

consistent with their theories may also be consistent with other theories. Finally, decisionmakers tend to be too wedded to their views and too closed to new information. George and Smoke (1974: 574) write that "discrepant information ... is often required, in effect, to meet higher standards of evidence and to pass stricter tests to gain acceptance than new information that supports existing expectations."

Consequences. There are three main consequences of this limited information processing capacity. First, perception of information is not comprehensive or even-handed but selective. The person's anticipations of what he will perceive determine to a large extent the small part, out of the greater information environment, that is actually perceived. Second, because man does not have the capability to make optimal use of information, he resorts to heuristics and simplification mechanisms to ease the cognitive strain. Third, since he cannot simultaneously integrate a great deal of information, man is forced to process information sequentially.

Estimates Based on Noisy Signals: The signals the intention estimating analyst responds to are typically incomplete (important data are missing, e.g., key words in a message may not be decoded, reconnaissance missions may not be flown on schedule), signals are vague or confused (reconnaissance photos may be blurred), or otherwise randomly imperfect. Such signals might be completely reliable and valid, or they might include unreliable or invalid information. Given all these possible causes of uncertainty (what parts are missing, incoherent, unreliable or inaccurate?), the analyst must nevertheless form some predictions as to the opponent's intentions.

Although the analyst may be highly sensitive to the uncertainty produced by unreliability and invalidity in the signals he attends, he may nevertheless overlook the uncertainty contributed by randomness alone, the noise within a signal channel. That is, the analyst may assume that signal channels have perfect fidelity, which is impossible. Psychologists have found that people have a very poor conception of such random disturbance, rarely recognize it when it is present, and offer deterministic explanations for random phenomena (Kahneman and Tversky, 1972). In short, the analyst, convinced a signal is from reliable and valid sources, may assume it contains no error. One consequence of this tendency to overlook random error is that, when people formulate and test hypotheses using uncertain signals, they keep searching for deterministic rules which will account for all the signals they are attending; when their hypotheses fail to produce perfect predictions, people frequently change hypotheses, apply them inconsistently, and tend to reuse hypotheses which were previously discarded. Even when people form the correct hypothesis they have trouble applying it consistently (Slovic, et al., 1977: 13). This basic difficulty, of not efficiently excluding hypothetical inferences when data are uncertain and contain random error or noise, is perhaps a major stumbling block to the application of the "strong inference" method described in Section 6 and demonstrated in Section 7. The difficulty is two-fold: holding onto a discredited hypothesis too long because some noisy data seem to support it, and rejecting a true hypothesis because noisy data seem inconsistent with it. The tendency to check hypotheses inconsistently against the signals compounds the difficulty.

The review by Slovic et al. (1977) of literature on judgments provides evidence that people make inferences that are too extreme given the uncertain data they have. These too-extreme estimates have been related to three judgmental heuristics; representativeness, availability, and anchoring, which provide efficient and sometimes valid methods for reaching conclusions about noisy data, but which can also lead to persistent, large, and serious estimation biases.

Representativeness. Many estimative tasks require the analyst to judge the probability that some behavior, X, observed now indicates an intention to attempt behavior Y in the future. Tversky and Kahneman (1973, 1974, Kahneman and Tversky, 1973) demonstrate that people tend to make such estimates by examining the features of X and Y and assessing the similarity between them, the degree to which behavior Y is representative of the process X which might be generating it. If Y is very similar to X then the estimated probability that X "indicates" that Y will occur is deemed high.

The representativeness heuristic implies that the analyst may estimate the probability that a particular event indicates another event, e.g., the opponent is dramatically increasing military capabilities implies the intent to attack, by comparing his impressions of the build-up with his stereotype of a surprise attack. If the information that capabilities are rapidly improving is representative of the analyst's image of a surprise attack, the analyst estimates that an attack is probable. The problem with this inference is that it overlooks information which is more relevant to accurate probability judgment, namely how often do nations (all nations or the one in question) which improve military capabilities then launch surprise attacks? The representativeness

heuristic overlooks this "base-rate" data, that is, data on the distribution of outcomes in similar situations, and overemphasizes "case" data, that is, evidence on the particular case under consideration. This tendency is enhanced by anything which increases the perceived uniqueness of the case at hand. One consequence is that rare events or extreme values are predicted if they happen to be representative. A second consequence is that estimates based on representativeness ignore the unreliability of case data, i.e., the fact that a single case or piece of information is not likely to represent the total universe of cases. However, Tversky and Kahneman (1971) find people (laymen and scientists alike) use the "law of small numbers," and regard a small random sample drawn from a population as highly representative and similar to the population in all essential characteristics. Tversky and Kahneman found even mathematical psychologists underestimate the error and unreliability inherent in small samples of data.

An equally disquieting finding in Kahneman and Tversky's (1973) series of experiments is that when judges were given no information about specific cases except base-rate data, the base rate influenced their judgments. However, when worthless information was given on a particular case along with base-rate data, the judges inferred from the worthless case data that no judgment was possible, even though they also had base-rate data which would allow them to make an estimate. In other words, very noisy (worthless) data on a <u>particular</u> observation may lead an analyst to make no predictions even though the analyst may have data on <u>general</u> past patterns which would permit him to calculate base-rate tendencies and thus make an estimate or prediction derived from the average of past behavior. Clearly, a

possible ploy for deception would be to "feed" the opponent so much noisy data on a specific case that the opponent's analysts would overlook base-rate data and conclude no estimates were possible because the case data are too noisy.

A third consequence of the representativeness heuristic is its tendency to produce nonregressive predictions, that is, predictions which ignore regressions to the mean. Kahneman and Tversky (1979) note that intuitive predictions follow a simple matching rule. The predicted value is selected so that the standing of the case in the distribution of possible outcomes matches its standing in the distribution of the analyst's impressions. For example, the hypothetical analyst may note that an opponent's rate of buildup of capabilities is greater than 95 percent of all buildups ever observed. He might then estimate the probability that this buildup indicates the intention to initiate hostilities at .95. The analyst has been excessively influenced by the extremeness of the singular case (a small sample of one) and insufficiently influenced by the average probability that buildups of capabilities lead to attacks. Only if military buildups were perfect predictors of attacks would the analyst be justified in letting his prediction of an attack match his impression of the buildup. To the degree that buildups are imperfect predictors of attacks, the analyst should lower (regress to the mean) his prediction toward the average for the class. If, on average, military attacks follow military buildups in one case out of ten, the analyst should lower his prediction, from .95 toward .10, by an amount proportional to the uncertainty of predicting attacks and the possible unreliability of the data on the particular buildup examined.

Since representativeness implies a disregard for the unreliability of information, one would expect that judges, given unreliable information, might make the same predictions as when given highly reliable information. This is what Kahneman and Tversky (1973) found in their experiments. People given data they knew to be unreliable but representative of academic performance (the data were on mental concentration) made the same predictions of academic performance as were made by judges given highly reliable information on academic standing. Even though the former group was aware of the unreliability of the cues they were using, they used the cues as representative of the situation they were predicting.

The hypothetical analyst example above is not entirely farfetched. Bracken (1977) describes how the British Air Staff in the 1920's and 1930's based its estimates of bombing casualties on the worst raid of World War I rather than on the average of all raids. The Staff's predictions of the probable casualties from Luftwaffe bombing were consequently vastly inflated and wide of the facts in World War II. Similarly, Jervis (1976: 266-271) describes how intelligence and policy estimates and predictions tend to be excessively influenced by the last war fought while data on other previous wars tends to be disregarded. Similarly, he notes that policies which succeed are repeated while policies which fail are avoided (even though the success or failure of a single case can be due to chance factors, pp. 275-279) that is, unrepresentative instances are treated as if they were representative of a whole class of policies.

Kahneman and Tversky (1973) find what they label "the illusion of validity," the widespread intuition that consistent information allows

greater confidence in predictions and estimates than inconsistent information, and that extreme predictions (e.g., a given enemy attack will either succeed or fail) are made with much higher confidence than are intermediate predictions. This illusion is quite significant for intelligence estimation. That is, an analyst will tend to have greater confidence in an estimate based on signals which agree than he will have in an estimate based on signals which have inconsistent content. People tend to discount evidence which conflicts with existing impressions (Anderson, 1972). Furthermore, analyst confidence will tend to increase with the correlation between signals and be highest when the signals are perfectly correlated. This means however that the analyst is treating what amounts to one signal as if it were two independent, mutually confirming signals. However, given valid signals, the strength of the prediction that can be based on them is related statistically to the inverse of the correlation between them, not to the direct correlation, as most people assume. The analyst tends, as Kahneman and Tversky (1973: 249) write, to be "more confident in a context of inferior predictive validity." If signals, which all point to a certain intention, are redundant or highly correlated to each other, the analyst would do well to ask if they are, in fact, from independent sources of information, and in any circumstances, to have less confidence in them. On the other hand, signals which do not correlate with each other, but which nevertheless suggest the same intention, are less likely to be emanated by the same source, and are a better statistical basis for analyst estimates and confidence.

The fact that people tend to have more confidence in predictions of extreme values than in predictions of intermediate values is

inconsistent with the statistical phenomenon of regression to the mean. Intuitive predictions tend to be insufficiently regressive, i.e., they are too representative of case data and insufficiently representative of the base-rate data on the mean, or average, for the class of that case. The discrepancy between predictions and outcomes is worst when extreme predictions are made. Thus, people are most confident in their predictions when their predictions are most likely to be wrong.

Kahneman and Tversky (1973: 249) write:

> Factors which enhance confidence, for example, consistency and extremity, are often negatively correlated with predictive accuracy. Thus, people are prone to experience much confidence in highly fallible judgments ... like other perceptual and judgmental errors, the illusion of validity often persists even when its illusory character is recognized.

Obviously, the illusion of validity tends to reinforce what Wasserman (1960) termed "the official theory" of intelligence, especially "naive realism," the notion that intelligence consists of "unvarnished" facts which admit of only one interpretation and the purpose of intelligence is the accumulation of ever more data so that policy is based on "all the facts." One consequence, as Hilman (1956: 58) and others have noted is an emphasis on encyclopedic accumulation of sources at the expense of analysis, and a belief that "all-source" intelligence centralization automatically leads to adequate intelligence. By way of contrast, our review of some successful intention estimators (Section 7) found that, even though they had "all-source" data, the estimators determined which sources were independent of each other (uncorrelated) yet still valid and reliable indicators of intentions. They did this primarily because they could not depend on any one source to last (e.g., the

Nazis might at any point have changed their enciphering policy and destroyed the Ultra source), and it was important to have other independent sources to take the place of those that might be compromised or destroyed. But secondarily, these estimators seemed to realize the inherent random error present in even the best sources, and attempted to offset this by triangulating on intentions with several sources whose errors were uncorrelated and hence would offset each other. Jones (1978: 529) explicitly noted this uncertainty with respect to Ultra decodes:

> Care was of course necessary -- although any one decode was likely to be one hundred percent reliable, it might well contain much less than the whole truth -- a fact that must always be borne in mind regarding information from any source, however reliable.

Availability. A second heuristic used is to judge those events most probable for which it is easiest to imagine or recall relevant instances. In general, the most probable events are also the most available to recall, but not invariably: availability is affected by other factors (familiarity, recency, emotional salience) which reduce the accuracy of predictions and estimates based on this heuristic. The consequence is that systematic biases may result from judgments based on the availability heuristic (Tversky and Kahneman, 1973).

The availability heuristic is related to the bias Chapman and Chapman (1967, 1969) term "illusory correlation," the marked over-estimation of the co-occurrence of natural associates. That is, Chapman and Chapman found people incorrectly judged natural associates (e.g., "lion-tiger") as appearing more frequently in lists of word pairs than unnatural pairs ("lion-eggs") which appeared equally frequently. Similarly, Chapman and Chapman found naive undergraduates "discovered"

the same associations between clinical symptoms of psychopathology which clinicians in clinical practice believe exist, but which do not in fact characterize psychopaths. Both the undergraduates and the clinicians estimate that a diagnosis of paranoia is accompanied by the patient's drawing of figures with peculiar eyes. However, studies of drawings by paranoids show no such relationship.

Tversky and Kahneman (1973: 223) offer the availability heuristic as an explanation of this illusory correlation. People seem to know that co-occurrence increases the associative memory bond between concepts (perhaps the oldest principle in memory study). The availability heuristic exploits the inverse of that principle, that strength of association is an index of frequency of co-occurrence. However, repeated co-occurrence is only one factor that increases associative strength. other factors include the ease in retrieving instances, the effectiveness of the search for instances, and the ease in imagining instances. Readily imagined instances of association, such as peculiar eyes and suspicious paranoids, seem to lead people to conclude that these events frequently co-occur. Unfortunately, the illusory correlation has been found to be extremely resistant to contradictory data, persists even when the perceived positive correlations are in fact negative, and it prevents people from detecting relationships that are in fact present.

Hamilton (1976:) suggested that illusory correlation and availability are, in part, responsible for the psychological formation of stereotypic perceptions. Noting the psychological tendency to infer that rare events have rare characteristics, and that common events have common characteristics, Hamilton suggests that members of groups whose numbers are rare in a population (i.e., minority group

members) may be perceived as having characteristics which are unusual in the population (i.e., deviant tendencies), because both are uncommon and distinctive. In contrast, majority group members are associated with common, normal characteristics. Research by Hamilton and Gifford (1976) supported this hypothesis. The implication for intelligence analysts is that distinctive characteristics may be attached to distinctive objects (states, leaders, or events) without any evidence of actual covariation between the characteristic and the object. The hypothetical analyst might conclude, for example, that the behavior of North Korean leader, Kim Il-song, is distinctly different than that of any other modern leader, and then attribute this to Kim's unpredictable, irrational, psychopathic character.

The intention estimation analyst may have a constellation of signals which form a pattern suggestive of several possible enemy intentions. Suppose one such possible intention is to launch a sneak attack. The analyst's task is to assess the probability of the various possible intentions against the pattern of the evidence. In making such estimates the analyst may attempt to recall similar patterns in the past, and, by determining what intentions past patterns led to, predict the likelihood that the present pattern indicates one intention or another. The availability heuristic suggests that past instances which are easily remembered, retrieved, or imagined will have more influence on the analyst's estimate than less available instances.

One factor influencing availability is salience. Instances with which the analyst is personally familiar will be recalled more readily. More recently observed patterns will be recalled more easily than historical ones. The analyst will tend to recall the patterns which led

to the most dramatic outcomes -- past patterns which led to attacks are better remembered than patterns which led to no dramatic changes. Previous occurrences are recalled better than nonoccurrences. Some past patterns will be easier to retrieve, e.g., those noted within the analyst's own agency can be searched out more readily than those noted by a rival agency, or an agency in an Allied country.

On the other hand, the analyst may view the present constellation of evidence as so unique that past history is not relevant to the estimation of possible intentions. In attempting to predict intentions the analyst may construct scenarios, stories that lead from the present pattern to a different target event or intention. The plausibility of the scenarios that come to mind, or the difficulty of producing them may be used as cues for the likelihood of a particular prediction. If no reasonable scenario comes to mind for a hypothetical intention, that intention is deemed unlikely or impossible. If many scenarios come to mind, or if one scenario is especially compelling, the intention in question appears probable. Particularly compelling scenarios are likely to constrain future thinking, i.e., once an uncertain situation is seen in or interpreted in a particular fashion, it is quite difficult to view it in any other way. As Bruner (1957: 129-30) writes:

> The greater the accessibility of a category [or scenario], (a) the less the input necessary for categorization to occur in terms of this category, (b) the wider the range of input characteristics [i.e., signals] that will be "accepted" as fitting the category in question, (c) the more likely that categories that provide a better or equally good fit for the input will be masked [i.e., not attended].

Availability is thus related to several psychological phenomena that influence estimations: "perceptual readiness" to accept certain signals

or consider certain scenarios; the "set" [or einstellung], that is, the tendency to perceive signals as fitting a particular scenario; and selective attention to only those signals which fit a particularly favored scenario.

Anchoring and Adjustment. The third error-prone heuristic involves picking a natural starting point (or anchor) for an estimate as a first approximation and then adjusting from this anchor point to accommodate the implications of additional information. For example, a natural anchor point for an intelligence estimate is the previous estimate on that subject. Typically, people make adjustments which are imprecise and insufficient, and different starting points yield different estimates which are biased towards the initial values. For example, Tversky and Kahneman (1974) found that, when asked to estimate the value of 8! (= 40,320), one group which had the question posed as 8 x 7 x 6 x 5 x 4 x 3 x 2 x 1 gave a median estimate of 2,250, while a second group, given the problem as 1 x 2 x 3 x 4 x 5 x 6 x 7 x 8, gave a median estimate of 512.

The consistent underestimates by American intelligence analysts of future Soviet missile strength, observed by Albert Wohlstetter (1974), may possibly have been due to an anchoring and adjustment bias based on a too-low anchor value. Similarly, the "missile gap" and "bomber gap" overestimates of Soviet capabilities may have originated in highly salient (and deceptive) Soviet displays and claims which then formed anchors for further U.S. intelligence estimate adjustments (see, e.g., Bottome, 1971; Dick, 1972; Licklider, 1970). Hogarth (1975: 284) notes "the common, almost hypnotic, weakness of commitment to one's first hypothesis" and the fact that people usually require "much information to change this."

Anchoring and adjustment can be used to explain the common tendencies to overestimate the likelihood that plans will be executed successfully and to underestimate the likelihood of failures in complex systems. People tend to overestimate the probabilities of conjunctive events (e.g., drawing a red marble seven times in a row from a bag of 90 red marbles and 10 white marbles) and to underestimate the probability of disjunctive events (e.g., drawing one white marble at least once in seven draws, with replacement, from the same bag). The success of the first draw in the red marble case is high, .9, while the probability of getting the white marble on the first draw is low, .1, forming anchors for the typical prediction that getting seven consecutive reds is more likely than getting at least one white. However, the overall probability of getting seven reds in a row is less (.48) than the probability of getting one white in seven tries (.52). People tend to see these probabilities as reversed since they are adjusting insufficiently from the .9 and .1 anchors.

In several studies Wyer (1974; Wyer and Goldberg, 1970) found people consistently overestimate the likelihood of the conjunction of events and Slovic, Fischhoff, and Lichtenstein (1976) found that the estimated probability of compound events may be greater than the probability of the constituent events.

Similarly, the successful execution of plans typically entails conjunctive events; many events have to go right for the plan to succeed, but if one event goes wrong, the whole plan may fail. Although the probability of the various events alone may be high, the overall probability that they will all succeed is quite low if the number of events is large. High single event probabilities may form inappropriate

anchors for estimating the overall probability of success. Conversely, risks are typically characterized by disjunctive events. A complex system may malfunction if any one essential component fails. Even though the probability of a single component's failure may be very low, the probability of system failure is high if many components are involved. Because estimates of failure may be anchored to the very low probability of a single component failure, the estimate of system failure may be inappropriately low.

Intelligence analysts often acknowledge the uncertainty of their estimates by providing, not a single point estimate for a quantity, but a range within which the actual value should be found. Psychologists have repeatedly found that these confidence intervals are overly narrow. When people estimate they are 90% certain they have given the right prediction or that the actual value lies within the range they have estimated, well over 10 percent of the time they are in fact wrong, or more than 10 percent of the actual values lie beyond the estimated range (see Lichtenstein, Fischhoff and Philips, 1976, for a review and Cambridge and Shreckengost, 1978, for evidence that intelligence officers are not immune from overconfidence). It is not unusual to find forecasts of estimated ranges which are widest for present values (for which firsthand knowledge is available) and much narrower for values in the distant future (for which only speculations on future uncertainties and unknowns are available). This narrowing of uncertainty is equivalent to greater confidence in predictions of the future than in estimates of the present, confidence which is clearly unwarranted except in naturally asymptotic situations and even then, only if the rate at which the situation approaches asymptote can be predicted

(e.g., I am more confident my health will probably be poor in 50 years than I am about predicting my health now, I could set a narrower range on my eight-year-old son's probable height at twenty five than I could on his height at sixteen).

Anchoring and adjustment effects tend to explain the widely noted phenomena that information which is processed slowly and in small amounts tends to be assimilated to the existing theory, image, concept, etc., while the same information, presented in a single, sudden mass tends to effect an accommodation process, and thus has a greater impact on changing theories and images. Jervis (1968) noted this phenomena explicitly in his third hypothesis on the misperception of intention: the image of the other is less affected by contradictory information which arrives bit by bit than by discrepant information that is considered all at once. Turner notes similar phenomena in the incubation of disasters (see Section 5), while Kuhn describes how this effect occurs in science (Section 6). Successful estimators of enemy intentions, such as Jones (1978), stoutly resisted feeding information to decision-makers piecemeal for this reason, writing (p. 334):

> We are sometimes criticized for withholding information, ... we reserve our right to do so because (1) to spread half-truth is often to precipitate erroneous action ... and (2) the steady and immediate broadcasting of each insignificant and uncollated fact automatically and insidiously acclimatizes the recipients to knowledge of enemy developments, so that they feel no stimulation to action. The presentation of the complete picture of an enemy development is the best way of stimulating the appropriate authority to action.

The converse argument is that habituation is an excellent means of deception. By slowly and gradually changing some element of behavior, the opponent is led to gradually adjust to the altered level

of behavior without becoming alerted. The high level of military action along the Suez Canal from 1967 to 1973, especially following the War of Attrition, seems to have acclimated the Israelis to such a degree that the Egyptians' 1973 war preparations were insufficiently different to serve as a warning.

Overconfidence. Anchoring and adjustment provide one explanation for the widespread tendency for laymen, judges, and experts to be overconfident in the accuracy of their predictions and estimates. Such overconfidence can also be related to the other heuristics; i.e., nonregressive predictions, disregard of base rates, disregard of small sample size unreliability, are representativeness and availability biases which will tend to produce overconfident judgments (Slovic, et al., 1977: 6).

Research by Lichtenstein and Fischhoff (1977) suggests that expertise decreases overconfidence and that experts may become underconfident (right more often than they expect to be) on very easy problems. However, they found experts no better than nonexperts in the ability to distinguish problems which are unlikely to be correctly solved from problems which probably will be solved.

Slovic, et al. (1977) and Kahneman and Tversky (1979) suggest these overconfidence effects are widespread because the environment rarely shows the limits of human predictions, i.e., errors in estimates are hard to detect, and often the estimator receives no feedback at all. Even when feedback is available, however, it is often the case that the estimator can convince himself that what occurred is what he had estimated, regardless of the actual estimate made. People overremember past successes (Langer and Roth, 1975) and exaggerate in

retrospect the predictability of significant events (Walster, 1967). Fischhoff (1975, Fischhoff and Beyth, 1975) has labeled this phenomenon the "hindsight bias" and "knew-it-all-along" effect.

Hindsight Bias. The hindsight judge knows how things actually turned out, knowledge the foresight judge lacks. In a series of studies Fischhoff (1975; 1977; Fischhoff and Beyth, 1975; Slovic and Fischhoff, 1977) demonstrates that outcome knowledge increases the perceived inevitability of the outcome reported, but hindsight judges remain unaware of this change in their perception. As a result, they believe they and others had in foresight insights which they actually had only as a result of outcome knowledge. In other words, hindsight judges overestimate what they would have known without outcome knowledge, underestimate the informativeness of the outcome knowledge itself, and overestimate what others actually did know without outcome knowledge. Telling hindsight judges of this bias and exhorting them to avoid it fails to have any impact. As Fischhoff (1976: 430) writes "by exaggerating the predictability of the past, people underestimate what they have to learn from it."

The hindsight bias clearly limits the utility of post mortems on intelligence failures, as has been explicitly noted by Wohlstetter (1962), Fischhoff (1976), Betts (1978), and Chan (1979). Furthermore, the hindsight bias seemingly influences the popular impression of normal science and makes it easy to underestimate the importance of scientific revolutions and paradigm shifts (see Sections 5 and 6). Perhaps the most damaging consequence for intelligence of the "knew-it-all-along" effect is that intelligence consumers will tend to underestimate the impact intelligence information has on their images, decisions, and

policies, and thus will place too little value and priority on intelligence since they believe it never provides any surprising information. Ultimately, the decision-maker comes to believe, because of the hindsight bias, that he could get along as well without intelligence, and intelligence becomes "too separate" from policy, as Wasserman (1960) warned (see Section 4).

As was noted in Section 7, successful estimators of intentions have overcome the impact of the hindsight bias by withholding their estimates until decision-makers have formed their own impression of the data and have expressed them, i.e., the decision-maker becomes publicly committed to a prediction before knowing the outcome (i.e., the intelligence estimate). Having been publicly committed, the decision-maker was less able to assert that the intelligence information contained no surprises.

Perseverance of False Impressions. The hindsight bias seems to entail an immediate assimilation of outcome knowledge into all that is known about the event. In other words, the hindsight judge attempts to make sense, or a coherent image, out of what he knows of the event. Such assimilation should increase the perceived similarity between the reported outcome and the situation that preceded it. Because the prior events thus appear, in retrospect, representative of the outcome, the probability of prior events producing the outcome is subjectively increased. Similarly, the availability heuristic tends to foster the hindsight bias: the judge who knows what happened, and has adjusted his perceptions in the light of that knowledge, may well find it difficult to imagine how things could have turned out otherwise. And knowledge of the outcome may lead the judge to assign it a certain probability

anchor and to insufficiently adjust downward the probability that the outcome could be predicted from prior events. A disturbing aspect of this assimilation of outcome information is that it also takes place when the outcome information is false, and leads to false perceptions which persist even when the information on which they were based is known to be false. Even when the false information is totally discredited by the recipient it continues to produce residual effects on the recipient's perceptions. Ross (1977) suggests that once the false information is assimilated it becomes autonomous from the assimilation process and becomes part of the overall perception. The erroneous perception may survive the discrediting of its original evidential basis because the impression comes to be supported by additional evidence that is seemingly independent of the now-discredited basis. In fact, the discredited information may be all that sustained the evidence which is now perceived as the independent support for the perception.

Safran (1978) noted an instance of such perseverance in the case of Israeli intelligence before and during the 1973 War (see Section 5). The Israelis received authoritative intelligence before 1973 that Egyptian War Minister Sadeq approved of war only if its aim was "all-out" victory. After Egyptian President Sadat fired Sadeq in November 1972 the Israelis did not review their assumption that Egyptian war aims would be total victory, and this view persevered right up to the 1973 War (Ben-Zvi, 1976; Safran, 1978; Shlaim, 1976). After the 1973 War the Israelis learned that Sadat had fired Sadeq because the latter opposed Sadat's limited war aims strategy (Safran, 1978: 138).

Similarly, Bracken (1977: 291) notes how the French strategic planning for the Maginot Line in the 1920's depended on a Franco-Belgian

alliance against Germany and a French deployment into Belgium. The development in the 1930's of the Maginot Line however shifted the war plans for the French Army from the planned deployment to Belgium, to deployment in the Maginot fortresses. Alarmed by this shift, Belgium declared neutrality in 1936, but French war strategy remained unchanged, allowing the Nazis to "end run" the Maginot Line in 1940.

Attributions of Intentions

The psychological theory of attribution is concerned with how people understand the causes and implications of past events, how people attribute causes to and explain the past. Since interpretations of past trends in behavior seem to influence successful estimators' predictions of future intentions (see Section 7) it is important to examine how such interpretations might tend to be formed. Additionally, knowing something about how people tend to attribute causes to past events and attribute dispositions and intentions to actors should provide some clues as to how intelligence analysts might tend to attribute the causes and intentions for future actions.

Attribution theorists describe the process of attribution as having four steps: the observation of action, the judgment of intention, the making of a dispositional attribution, and the prediction of outcomes and behavior (see, e.g., Shaver, 1975: 26-29).

A basic distinction is made in this literature between behavior or action which is caused primarily by the environmental, situational factors confronting the actor, and behavior which is caused by the disposition, personality, and intentions of the actor. Understanding how people determine from observing an action whether behavior is situational or dispositional in origin is a major aim of attribution research.

The Observation of Action. An important aspect of the psychological discrimination of signal from noise is the rendering of a continuous, undifferentiated stream of physical stimulation impinging on the various senses into discrete, discriminable, describable entities. The same problem occurs in intelligence work, as Jones (1978: 493) observed:

> A fundamental difficulty of intelligence work is that input is by source, and output is by subject. A changeover has thus to occur inside the intelligence machine.

Attribution theorists (e.g., Heider, 1958) note that the perception of behavior of others is discrete, rather than continuous. People are seen as performing a series of discrete actions. These discrete actions divide the stream of information into segments or units. Newtson (1976) found several regularities in the way people segment the stream of information on the behavior of others; i.e., how they tend to divide their perceptions into a series of discrete actions. For example, people observing highly organized, step-by-step action, with a clear hierarchy of subordinate and superordinate goals, tended to segment the action into grosser units, while much briefer and finer segmentations are made perceptually for irregular, loosely organized action sequences. Furthermore, as people watch predictable, highly organized action they perceive it in longer duration segments, whereas when presented with unexpected action they resort to shorter units of action. The perceptual organization of action becomes extremely fine-grained immediately after an unexpected or unpredicted significant event occurs.

An interesting finding by Newtson, replicated but still controversial, is that, as the perceived behavior becomes more important

to the perceiver, grosser units of analysis are used. These results are consistent with Kahneman's (1973) finding that arousal causes a tendency to focus on a few relevant cues, i.e., decreases the range of attention while amplifying its intensity. The tendency of intelligence analysts to rely heavily on a single source of information when under stress was noted in Section 3, under the "Ultra Syndrome." Janis and Mann (1977) term this behavior "hypervigilance" in their conflict theory of decision-making (Section 4).

Time is one of three dimensions which attribution theorists emphasize as important to the perception of action (Heider, 1958). Objects and events are organized into action perceptions along space and formal substance dimensions as well (Koffka, 1935). A fundamental principle in the formation of perceptual units is that the degree of perceived similarity between characteristics of objects and events determines which will be perceived as connected in a unit. Decreasing the time, distance, and deviations between discrete objects or between discrete objects and events increases the tendency to perceive changes and movements as a single action.

Fischhoff (1976: 431) notes that peoples' poor conceptions of randomness (see above) may lead them to perceive causal actions when in fact only random phenomena exist. What seem to be meaningful patterns of actions may only be another manifestation of the widespread tendency to offer deterministic explanations of random events.

As was noted in Section 2, Schmidt (1976) observes that actions which are parts of different plans may be temporally or spatially contiguous, while acts that are part of the same plan may be quite separated in time. Because people tend to perceive actions which are temporally or spatially contiguous as being causally related, actions which are in fact

unconnected may be seen as resulting from a common plan. And because people have only a limited ability to retain unconnected events in memory, plans may be difficult to infer from actions unless the goals of the actions are known. Finally, some plans will be even more difficult to perceive because they contain both actions and nonactions.

Curious Incidents: Nonactions and Nonoccurrences.

> Sherlock Holmes: And then there is the curious incident of the dog in the night-time.
> Watson: The dog did nothing in the night-time.
> Holmes: That was the curious incident.
>
> A. Conan Doyle, "The Silver Blaze"

Among the most difficult information the intelligence analyst has to contend with is negative information, i.e., data that something has not occurred. In Section 7 several examples were given of the successful use by intention estimators of negative information. At least as many examples can be found of negative information which was neglected, or, when properly monitored and assessed, was not accepted by the decision-maker. For example, Admiral Pound's actions in the battle of the ill-fated convoy PQ17 were based on his belief that the German ships Tirpitz, Hipper, and Scheer were at sea. British naval intelligence had a reliable monitoring system which would report any move of the Tirpitz from the Norwegian fiords. Since no signals of movement were received, naval intelligence estimated (correctly) that Tirpitz had not sortied. Pound, however, rejected this evidence and, on the worst case assumption that Tirpitz was abroad, dispersed the convoy, which was then destroyed piecemeal by U-boats and bombers (Beesly, 1977: 137). McLachlan (1968: 150) writes that "valuable though negative information can often be, fighting men generally distrust it."

and there is a "prejudice in favor of what the 'watchkeeper' has seen" (p. 37) which dominates inferences from negative information.

There seems to be a strong psychological tendency to attend to actions or occurrences when forming inferences but to neglect the information conveyed when particular responses or events do not occur. For example, when experimental subjects are given information in the form of a four-fold presence-absence table, only the "present-present" cell strongly influences subjects' inferences of covariation. Logically, the frequency of that cell is no more relevant than are the frequencies in the other three cells, including the "absent-absent" cell. Ross (1977) suggests that nonoccurrences are rarely as salient or as cognitively available to the attributor as are occurrences. Consequently, recognition, storage, retrieval, and interpretation are less likely with nonoccurrences than with actions.

Einhorn and Hogarth (1978) note that the tendency to ignore data unless it falls in the "present-present" cell of the contingency table has the effect of biasing feedback on the accuracy of judgments. If only the confirmed predictions are attended (while false predictions, and correct and incorrect nonpredictions are ignored), the tendency will be to assume far greater accuracy in judgments than is actually the case. Einhorn and Hogarth suggest that the difficulty people have in searching for disconfirming information to test their judgments leads to persistence in the illusion of validity and overconfidence in judgments. An important consequence of the tendency to overlook disconfirming evidence is the result that the wrong lessons are learned from experience.

Attributing Causes and Intentions. The perception of action is followed by the second step of attribution, causal judgment: the

perceiver seeks to identify the cause, or set of causes, to which an action or outcome may most reasonably be attributed. A fundamental link between this step of attribution and the next, that is, the making of dispositional judgments about the actor, is the perceiver's judgment as to whether a given action was intentional or accidental. That is, only intentional acts serve as the basis for making attributions of disposition to an actor.

Theorists of the attribution process provide different interpretations of how the judgment of intentionality is made. In each case, theorists assert that an initial judgment must be made of whether an action is due to personal (dispositional) or environmental (situational) forces. Theorists subdivide the components of these personal and environmental forces differently.

Heider (1958) theorizes that motivation and ability are personal forces, perceived along with task difficulty (the environmental force) as forming the basis for inferences of intention, effort, and possibility. The combination of intention, effort and possibility of the action are believed to determine whether the action is successful or not. The perceiver infers intention from the outcome of the action, the possibility of that outcome, and the perceived level of effort. If the level of effort is judged very low and the possibility of success is judged low, the inference based on a successful action is likely to be that the outcome was due to luck rather than intention. Likewise, if ability is judged low and the task difficulty is perceived as high, successful actions are more likely to be attributed to luck than to intention, even if effort is perceived as high. Intention will tend to be attributed to successful action when task difficulty is consistent with

ability such that successful action is judged possible, and when the effort or exertion is high. If the action is a failure, the attribution tends to be that this was due to luck rather than intention.

The tendency to attribute failure to luck rather than intention when task difficulty and ability are compatible, and exertion is apparently high, underlies the famous "lost plans" deceptions of World Wars I and II. That is, Richard Meinertzhagen seemed to accidentally stumble upon a Turkish patrol, and while making a desparate escape, feigned being shot and dropped a dispatch case containing plans which led the Turks to believe the British attack was coming on the wrong flank. Heavy British patrolling subsequently in the same area gave credence to the belief that something of value had been lost. By "failing" because the task was difficult (escape while shot) even though effort was high (the gallant and dashing ride back to British lines and the later patrols), the dropping of the plans had the character of accident rather than design. Similarly, in World War II, Montagu (1953) of Naval Intelligence and Cholmodeley of the British Secret Service successfully passed off on the Nazis false hints regarding the Sicily invasion by planting them on a dead body, a seeming victim of an air crash, and having the body "happen" to float ashore. The desired and obtained impression was of a dutiful courier stayed from his round by misfortune. Strong British efforts to regain the lost briefcase and prevent its contents from falling into Nazi hands reinforced the impression that bad luck caused the incident, not intention, and lent credence to the false clues that Sicily was not the objective.

On the other hand, failure at a task with low difficulty, or when ability is present but effort is absent is likely to be attributed to an intentional effort to fail.

Jones and Davis (1965) suggest that intention will be perceived under fairly constrained circumstances; when an action has rather unique effects suggestive of a distinct choice by the actor, when the actor has the knowledge and ability to create the uncommon effects, and when these effects deviate from the social or group norm of conventional behavior (i.e., out-of-role behavior is more informative than in-role behavior). Intentionality is inferred when actions are unusual, have limited and negative consequences, and are within the capability of the actor.

Kelley's (1967, 1971, 1972) attribution theory hypothesizes that attributions vary in three dimensions: persons, time/modality, and entities. The principle of covariation between potential causes and effects of actions is the central theme of Kelley's attribution theory. Intentionality will tend to be attributed whenever the actor (but not other persons) tends to perform a particular action consistently but only in a very limited variety of situations or in a variety of ways (time/modality), and the given action tends to produce negative outcomes (entities). Intention will tend not to be attributed to an actor if the action is performed by many others, if it is performed randomly or in a wide variety of contexts, or if the action's effects would be rewarding to the actor.

All three theories recognize that a judgment of intention is a necessary precondition for an attribution of a disposition, but they differ in the degree to which they hold intention to be sufficient for personal attribution. Kelley restricts intentional actions to only those actions which are entirely dispositional and personalistic and could not have been forced or elicited by environmental factors. At the

other extreme, Heider sees intention as necessary but not sufficient for a dispositional attribution; effort or exertion in the direction of the intention (that is, to obtain rewarding outcomes) must also be present. Jones and Davis hold a middle position, that intentional action is less likely to lead to personal dispositional attribution if the action effects are highly desirable.

The theory most compatible with the task of the intelligence estimator of intentions is obviously Heider's. Both Kelley and Jones and Davis tend to rule out estimation of intentionality in cases where the outcome rewards the actor. In contrast, Heider would require the analyst to assess the outcome (successful or not) against the effort to determine if the action was intended. However, other aspects of the Kelley and Jones and Davis theories are of interest in the analysis of intention estimation. Kelley's theory provides a framework for organizing information on actions along useful dimensions of consensus (people), consistency (time/modality), and distinctiveness (entities), and underlines the principle of covariation. Jones and Davis' theory underscores the significant impact which unusual events can have on estimates and attributions.

For example, Jones and Davis predict that expert advice would be most convincing when it is out-of-role, i.e., when CIA officials discourage covert action, when State Department officials recommend military rather than diplomatic measures, etc. In fact, Betts' (1977) research on military advice seems to bear out Jones and Davis' prediction: "military advice [to U.S. Presidents] has been most persuasive as a veto of use of force and least potent when it favored force" (p. 210) and "soldiers have exerted the greatest leverage on [military]

intervention decisions in those instances where they vetoed it" (p. 5).

Bias in Attributions. A commonly observed bias in attribution is the tendency for actors to attribute success to their own abilities, efforts, or dispositions while attributing failure to luck, task difficulty, or other factors, while observers of the action give the actors less credit for success and more blame for failure. While such asymmetric attributions have an obvious egocentric explanation, it can also be explained in nonmotivational terms. Success is intended, planned and anticipated by the actor and congruent with his past experience, whereas failure is an unintended, unusual event which occurs despite the actor's plans and efforts. Observers are much less aware of the actor's intentions, plans, efforts and expectations than is the actor.

The most frequently noted bias of attribution (Heider, 1958; Ross, 1977) is the fundamental attribution error, the tendency for observers to underestimate the impact of situational, environmental factors and to overestimate the role of dispositional factors in controlling behavior. An explanation of this bias parallels the explanation of the asymmetric attribution of success and failure, namely the differential perceptions of actors and observers (Jones and Nisbett, 1971). The actor is focusing on the situation surrounding an action whereas the observer is focusing primarily on the actor. It appears that this "perceptual focusing," that is, whatever or whomever we focus our attention on becomes more apt to be cited as a causal agent (Duval and Hensley, 1976; Storms, 1973; Taylor and Fiske, 1975) underlies the actor-observer asymmetries.

The implication of the fundamental attribution error is that observers too readily infer broad personal dispositions and expect consistency and predictability in behavior across a wide variety of situations and contexts. There is a tendency to draw hasty conclusions about dispositions while overlooking relevant environmental forces and constraints. Several of Jervis' (1968) hypotheses on the misperceptions of intentions (see Section 4) stem from this bias: the fact that statesmen tend to see the behavior of others as more centralized, disciplined and coordinated than it is; and the tendency to perceive the position of a state's Foreign Office as the position of the state.

Jervis (1968) postulates a further hypothesis that seems indirectly related to the fundamental attribution error: that when states interact, a state will overestimate the degree to which desired behavior by a second state is due to the influence of the first, and will overestimate the degree to which undesired behavior by the other state is due to internal forces. That is, states generally see other states as behaving negatively for dispositional reasons and behaving positively because of influence. In this latter event the perceiving state overemphasizes its own influence in bringing about the positive behavior. These tendencies have been noted in psychological research. Taylor and Koivumaki (1976) found that people are perceived as the cause of positive, desired outcomes while situational factors are regarded as causing negative outcomes, which they labeled the "positivity effect." Snyder, Stephan, and Rosenfield (1976) found an egotistic tendency to make attributions that put oneself in the best possible light, attributing good outcomes to one's own skills while bad outcomes are attributed externally. Finally, there is a tendency toward what Jones and Nisbett (1971) term egocentricity, the

tendency to assume others see the world the same way one does. Ross (1977) notes that laymen tend to perceive a "false concensus," that is, to see their own behavioral choices and judgments as relatively common and appropriate to existing circumstances while viewing alternative responses as uncommon, deviant, and inappropriate. Furthermore, there is the tendency to judge those responses that differ from our own as more revealing of an actor's stable dispositions than those responses which are similar to our own. Jervis (1968) notes that it is difficult for an actor to believe that others see him as a menace, and even harder to see that issues important to him are not important to others.

Ross (1977) suggests that the egocentric bias explains the asymmetry of actor and observer attributions: when others behave differently than the observer would act, the observer makes a dispositional attribution, and since others are likely to behave differently from us on at least some occasions, as observers we tend to see others as having more distinguishing personal dispositions than ourselves.

These attributional biases have many implications for the estimator of intentions. First, there may be a tendency to focus only on behavior of other states that differs from the behavior of one's own state and to assume that similar behavior is understandable and needs no explanation. Furthermore, when discrepant behavior is examined there is a tendency to seek dispositional explanations for it and to assume that other observers will also perceive the behavior as different, needing explanation, and probably due to dispositional causes. The egotism and positivity effects would make it very difficult for an observer to attribute the negative behavior of another state to the impact the observer's state may be having on the observed state: instead of noting the negative behavior as a

reaction to situational forces (i e., the impact of the observer's state), the observer will tend to make a dispositional explanation. Similarly, when a state fails to make the desired impact on another state the failure is more likely to be attributed to the disposition of the other state than to the insufficiency of the effort to make an impact.

Conflict and Attributed Intent. Thomas and Pondy (1977) report the tendency for parties to a conflict to perceive themselves as cooperative and reasonable, but to attribute competitiveness and unreasonableness to the other party. Both parties tend to translate the ongoing events into their own frame of reference and into the terms of their expectations. Each party tends to be aware of the role pressures on himself and the conditions which influence the party's ability to satisfy those pressures. Each party sees his behavior as flowing rationally from these pressures and conditions, while the other's behavior is apt to be most significant in terms of its actual or potential frustration of the party's own concerns. Having no access to the other's reasoning process, each party perceives the other's frustrating behavior as arbitrary and unreasonable. Attribution of intention becomes especially salient when relative power is a central feature of the relationship (Maselli and Altrocchi, 1969). A party is more likely to be concerned with the intent of the other when he thinks the other is more powerful, and more likely to attribute the actions of the other party to intent.

Jones and Nisbett's (1971) observation that actors and observers focus on different cues and have access to different information also applies to a conflictual relationship. In conflict both parties are occupied with their own behavior -- choosing the next moves and responses. The parties have relatively little opportunity or motivation to attend to the

effect of their own behavior, and are relatively unaware of the provocative elements of their own behavior during conflict, but highly aware of them in the other party. Coupled with this relative unawareness of the negative elements in their own behavior, individuals are likely to be especially sensitive to uncooperative cues from the other party. Both the threat of loss implicit in noncooperative behavior and the high salience of uncooperative acts tend to lead individuals to selectively attend to, and recall, uncooperative cues from the other party, producing exaggerated attributions of hostility, an observation Jervis (1968) makes of diplomats.

Failure to use Base-Rate Data. Kelley's attribution theory predicts that if many actors perform a given behavior, observers will interpret this concensus as indicating situational forces rather than dispositional causes underlie the action. Reviewing the experimental literature Nisbett and Borgida (1975) and Nisbett, Borgida, Crandall, and Reed (1976) conclude that there is little evidence that people are sensitive to consensus information, rather, people fail to use information about what most people do in a particular situation, and instead focus on individuating information on the case in hand. As Nisbett, et al. (1976: 114) write;

> People are largely unmfluenced in their causal attributions by knowledge of the behavior of others. Knowledge that the actor's response is widely shared seems not to prompt the inference that the situation rather than the actor is the chief causal agent. Conversely, knowledge that the actor's response is unique seems not to prompt the inference that the actor rather than the situation is the chief causal agent.

Nisbett and Borgida and Nisbett, et al., note the parallel between this

failure to use consensus information and Kahneman and Tversky's (1973) finding that people fail to use base-rate information when making predictions.

Nisbett et al., suggest that base-rate or consensus information is statistical, remote, pallid, and abstract while target case data is vivid, salient, and concrete, and the former "may simply lack the clout to trigger further cognitive work." Concrete, emotionally interesting information may have greater power to generate inferences because they serve as representative cases which aid in retrieving similar information.

However, Tversky and Kahneman (1977) demonstrate that base-rate data are used in making predictions or inferences when they induce a causal model which explains the base-rate and applies to the individual case. Their research has a wider application to attribution theory however. They find that it is easier for people to reason from causes to consequences than to reason from consequences back to causes. It is the latter problem, that of perceiving actions and attempting to determine the causes for the action, that attribution theory emphasizes. Tversky and Kahneman hypothesize that people create causal schemes to make coherent explanations of the causes of events, and have difficulty in using base-rate data in these causal schemes, but naturally use representative case data. They find that when people are able to give base-rate data a causal, rather than diagnostic, interpretation the base-rates affect judgments; while base-rates that do not fit into causal schemes or which conflict with a causal scheme are given little or no weight.

Tversky and Kahneman's results suggest that even though the intelligence analyst has excellent base-rate diagnostic data, he may be unable to utilize it in his estimates unless it can be given a causal explanation, i.e., until it can be interpreted as a propensity that is causally related to a target outcome. Knowing that military capability buildups of a nation are followed by surprise attacks in one out of twenty cases is diagnostic. Knowing that a given nation had twenty capability buildups and only one surprise attack followed is causal and more likely to be influential. The encoding of base-rate data tends to determine how it is used.

In the concluding section of this report, recommendations on how intelligence might be improved are evaluated against the evidence reviewed above and the studies of failure and success (Sections 4 and 7).

SECTION 9

DIAGNOSIS OF WEAKNESSES IN INTENTION ESTIMATION

DIAGNOSIS OF WEAKNESSES IN INTENTION ESTIMATION

This section presents three check lists of major problems with intention estimation and their characteristics and symptoms. The symptom patterns vary greatly from problem to problem. These checklists parallel the individual, organizational, and political models used in Section 4. Details on these diagnosed weaknesses are found in Sections 4 and 8, and, to a lesser extent, in Sections 2, 3, and 7. When these problems are linked with each other, major failures tend to result (see Section 5).

Individual Model

Major Problems	Characteristics and Symptoms
Personal Motives of Policy-Makers	Estimates and decisions follow self-interests. Premature closure of estimation. Estimates fit preconceptions. "Yes" men dominate dissidents.
"Official Theory" of Intelligence ("Mosaic Theory")	Intelligence consists of "unvarnished" facts, facts "speak for themselves," the future is predictable if the "right" people get "all the facts." Intelligence estimates echo the policy line, or tend to be ignored. Policy groups usurp intelligence functions, no clear authority or responsibility for estimation.
Images of the Enemy	"Mirror imaging" of the enemy. Salient and dramatic images form the basis of estimates. Preparing to fight the last war. Alternative images not considered. Data evaluated by how well it supports existing images. Estimate revisions are either minimal or revolutionary. False alarms (crying wolf) dulls reactions.
Memory Biases	Salient, dramatic, memorable instances overwhelm less dramatic data. Images that "make sense" are better recalled than ambiguous or confusing information. "Some X" is recalled as "All X" or "No X." Dramatic data "shrink" in magnitude, grow in memorability.
Hypothesis Testing and Concept Formation Biases	Hypotheses are accepted and rejected on the basis of insufficient evidence. Early intelligence "fixes" the image and confirming intelligence is accepted, disconfirming intelligence is disbelieved. Only one or two of many dimensions are monitored at a time. Conjunctions are basis for most hypotheses. Negative intelligence ignored. "Hit rate" sole criterion of success, false alarms ignored.

Major Problems	Characteristics and Symptoms
Estimation Psuedo-Sophistication	Estimates believed to be based on many variables in fact depend on only a few. Estimators wrongly identify the "main" variables. Use of more information increases confidence but decreases consistency of estimates, leaves accuracy unchanged. Stress on "all-source" system, getting "all the facts." Resistance to estimates done by "outsiders," or checking past estimates against criteria.
Noise Conceals Signals	Estimates are ambiguous or ambivalent. Estimators differ on predictions. Analysts differ with each other and policy-makers over what is "relevant." Piecemeal and day-to-day shifts in opinions occur. Each analyst sticks to a single hypothesis at a time. Mirror imaging of uncertain elements such as enemy risk function.
Deception Conceals Signals	Estimates have high certainty, consistency, and support at high levels. Low-level intelligence varies from high-level estimate. Estimate of enemy intention has close fit to top level preconceptions. Tactical intelligence dominated by strategic estimates.
Risk Estimation	"Impossible" risks become possible because they are unexpected. Risk calculus tied closely to capabilities, not possible future outcomes. Rejections of "suicidal risks" ignore possible short-term payoffs. Mirror image rationality, logic.
Estimation Heuristics	
"Representativeness"	Small samples are believed to be as good as large samples. Estimates are deterministic, random error is ignored. Estimates are not regressed to the mean. "Good" case studies form the basis for estimates. Unreliable intelligence yields identical estimates as reliable intelligence. Worthless intelligence masks base-rate intelligence. Consistent indicators are

Major Problems	Characteristics and Symptoms
	preferred to inconsistent indicators. Analysts have highest confidence in most extreme estimates. Estimates for distant future more certain than present estimates.
"Availability"	Memorability used as index for co-occurrence. Illusory correlations accepted without checking. Distinct events seen as co-occurring. "Likely" scenarios form the basis of selective attention and estimation.
"Anchoring and Adjustment"	Current estimate based on previous estimate. First approximations used as starting points. Successes overestimated, failures underestimated. Confidence bounds are too narrow, analysts overconfident in estimates. Many "surprises" and intelligence failures unexplained. Hindsight biases conceal lessons of failures, "knew-it-all-along" effect leads to rejection of intelligence estimation. False estimates persevere discrediting of data.
Attribution Biases	Behavior, estimated to be intentional, may be either random or situational. Behavior like our own needs no explanation, behavior different from our own needs intention explanation. Deviant behavior seems more complex than "normal" behavior. Orderly behavior less closely attended than disorderly behavior. Negative evidence ignored. Behavioral interdependence overlooked. Attributions of hostile intentions predominate. Causal data used while base-rate data are ignored.

Organizational Model

Major Problems	Characteristics and Symptoms
Inconsistency of Policy and Intelligence Goals	Policy perspective narrows intelligence perspective. Demands for certainty generate ambivalence; ambivalence fosters predisposition, bolsters confidence. Ambiguous data lead to intuitive analysis, leaves predispositions unchallenged. Multiple intelligence channels foster ambiguity. Reforms atrophy because they fail to meet organizational needs.
Uncritical Change or Maintenance of Policy/ Policy Change under Stress	Alternative actions and objectives not assessed. Current versus new policy consequences not evaluated. No search for new information. No new planning for implementation or contingencies. Shifting to new policy leads to evaluation of current policy but not future policies. Changes under pressure lead to shifting responsibility to others, bolstering selected policy by wishful thinking, exaggerating positive outcomes and minimizing negative possibilities, procrastination. Narrowing of cognitive processes, premature concensus from limited alternative generation. Decision unit and advisor circle shrinks, reduces alternative viewpoints, insulation from system. Limited information sought or accepted from fewer sources. Leadership promotes preferred solution. Short-range issues drive out long-range planning and analysis. Trade-offs of values not considered, hard choices among options reinterpreted into single choice.
Groupthink	Illusion of invulnerability, excessive optimism, excessive risktaking. Collective rationalizations and discounting of warnings. Belief in group morality. Stereotyped view of enemy. Pressures against deviant opinions and self-censorship. Illusion of unanimity, silence means agreement assumption. "Mindguards" tolerated and fostered to defend group from contrary views.

Major Problems	Characteristics and Symptoms
Hierarchy Effects	Increased time delays in transmission of information and intelligence. Concealment and misrepresentation of information. Lower level analysts and those closest the problem ignored. Innovation inhibited, communication restricted to narrow channels, defensive cliques and coalitions fostered. Experts are promoted out of their area of competence. Transmission of bad news upward inhibited and policy information fails to flow downward. Analytic talents compete with others for promotion. Easy to isolate inconvenient experts.
Specialization Effects	Stress on loyalty and secrecy interfer with information flow. Units adopt guardian role to protect resources, foster rivalries. Parochial intelligence fostered, too remote from policy needs. Diffe·ing unit estimates produce ambiguity and ambivalence, and paralyze policy choices. Redundancy among units wastes resources, consumes policy-makers' time and attention. Multiple intelligence sources generate information overload at policy level. Uncovered gaps result in intelligence functions or coverage.
Centralization Effects	Consensus estimates hide alternatives and disagreements. Resources spread too thin between center and periphery. Illusion of reliability and security fostered. Too many layers intervene between data collection and policy usage.
Secrecy Effects	Secret sources seem more valid and infallible than open sources. Information evaluated by fewer experts. Authorship anonymous. Distribution, indexing, referencing less systematic. Irritating critics removed from access.

Major Problems	Characteristics and Symptoms
SOP Effects	Rigid response patterns to information may be unsuited to changed conditions. Resistance to changing established procedure. SOP's determine what problems can be solved, which information is available, what methods can be used. Available routines define the meaning of problems. Communications between agencies and within agencies depend on routinized channels and liaison, limiting what is communicated, the scope of information, and its impact.

Political Model

Major Problems	Characteristics and Symptoms
Effects of Differing Stakes and Action Channels	Focus on decision from unique viewpoint of bureaucratic stand. Intelligence is politically selfinterested. Conflicts result from competition for leadership attention and credence. Competition fosters excessive certainty and confidence. Emphasis on salesmanship rather than analysis. Bureaus control information and resource flow to make points in strategic competition with other bureaus.
Personality Effects	Political personality clashes disrupt intelligence hierarchies. Operations at one level depend on attitude of leadership at the next higher level. Estimates by proteges are protected by mentors from criticism. Poor personal relations within the chain of command impede upward and downward communication, stifle feedback, foster competitions. Similarities in background reduce novel ideas and approaches.

SECTION 10

PROBLEMS AND PROSPECTS FOR PRESCRIPTIONS

PROBLEMS AND PROSPECTS FOR PRESCRIPTIONS

A wide variety of prescriptions, reforms, and reorganizations have been proposed to cope with the weaknesses which are listed in Section 9 (see, e.g., Fain, et al., 1977: 239-259; Barnds, 1974). This section assesses some of the most popular recommendations, makes a few new ones, and relates them to the successful estimation experiences reviewed in Section 7.

Three features characterize many recommendations. First, there is an overwhelming stress on identifying the causes of the failures and shortcomings of intelligence. The tendency is to focus on a particular failure or defeat and to identify those critical features which seem central. Rarely is there a parallel analysis of successes in comparable situations to determine if the seemingly central features of the failure actually were absent (as would be expected) during the successes. If the features highlighted for change or reform were also found to be present during successes, one would have much less optimism that the proposed reforms will really make much difference. Barnds (1974: 36) recommends more nonfailure post-mortems be conducted. Second, there is a tendency among reformers to exhaust their critical energies on the features they wish to change and to downplay the costs and problems introduced by their recommendations. Enumerating the weaknesses of proposed reforms is often left to those who resist change, and who defend the status quo; often those who have been charged with failure. This weakens the impact of their opinions on the proposed reforms. Third, the causes of failures are usually identified along one of the dimensions used here, individual, organizational, or political.

Having diagnosed the problem on one of these dimensions, the recommendations are often on another dimension. For example, one recent critique, by Szanton and Allison (1976), lists the primary deficiency of the intelligence community as "inadequate analysis" resulting from "bias, irrelevance, and a judgmental rather than analytic orientation;" problems of the individual analyst and the psychological aspects of intelligence organization (e.g., intelligence having too little impact on policy). However, Szanton and Allison's recommendations are addressed to the structural aspects of intelligence organization. They suggest a separate analytical agency with no collection or operational responsibility. Colby (1976: 53), in a rebuttal, asserts that "tinkering with the organizational structure" is the first and easiest recommendation but "it is also a panacea for infinite problems." Rarely do reformers demonstrate how, for example, changing the structure will alter the psychological features of organizational behavior which they identified as the problem. Too often, one suspects, whatever the nature of the problem, the recommended reforms will tend to be whatever seems easiest to accomplish, i.e., reorganizing.

To practice what is being preached and by way of an example of how reformers might anticipate the weaknesses of their own recommendations, a brief counterargument against intention estimation is outlined.

A Case Against Intentions. We have argued in this report that intention estimates should and can be done, that the problems which prevent accurate estimates can be determined, that the underlying psychological mechanisms of estimation tasks are generally known, and that we can learn many useful lessons from the episodes of successful estimation in the past. In making this case it is easy to overlook the impact of the

present context of intelligence on this issue, and to assume that more or better intention estimation, ipso facto, solves the major problems of intelligence. To the contrary, in slightly altered contexts, intention estimation might have little value or even be counterproductive.

Among these contextual factors is the present high capability of intelligence agencies to estimate military and economic capabilities and the other major elements of physical intelligence (see Section 2). Because we know, with considerable certainty, what our potential enemies could do, we are far more curious about what they will do. If our ability to determine capabilities were less robust, we would put a much higher priority on developing capabilities estimation methods and much less stress on intentions.

History and recent experience bear this out. Prior to World War II the British War Office regularly complained that the Secret Service failed to provide urgently needed factual information about Germany's military capabilities, equipment, preparations and movements because too much of its limited resources were diverted to or distracted by the collection and distribution of speculations on Germany's immediate intentions (Hinsley, 1979: 55-6). Similarly, when the U.S. monitoring stations in Iran were lost after the overthrow of the Shah, the debates in the U.S. Senate over ratification of the SALT II treaty made a sudden shift from concern over Soviet intentions to concern over verification of Soviet strategic capabilities.

Because we estimate capabilities well, we might also be able to estimate intentions. Should we lose some or all of our capabilities estimation ability, we would be far less able and far less desirous of estimating intentions. Intention estimation can occur only against this context of a high ability to estimate capabilities.

A second contextual factor is the essentially defensive, reactive nature of U.S. foreign policy in peacetime. To put it too simply, the main post war U.S. policy concern has been maintaining the world status quo. Intention estimation is a central element of defense but a secondary element of offense (see Section 2). If U.S. policy were to become more expansionistic and aggressive, the need for intention intelligence would contract and the demands for capabilities intelligence would increase. By the same token, the policy of America's primary adversary and potential wartime enemy, the Soviet Union, has been (again, being too simple) largely expansionistic, and, from the U.S. perspective, aggressive. Such behavior and perceptions dictate the need for intelligence on Soviet intentions as well as on Soviet capabilities. If, for some reason, the Soviets became militarily or economically weaker, or less active and troublesome outside their borders, U.S. interest in Soviet intentions would shrink; if the Soviets become more active still, interest will grow.

Thus, while the present context makes improved and increased intention estimation a sound goal, shifts in any of several contextual factors could lessen the value of achieving that goal. In recommending intention estimation, we note that its value depends on our continued ability and skill in estimating capabilities, and the continuance of the recent relative world outlooks of the United States and its principle adversary.

With this principle in mind, that all recommended solutions introduce their own new problems, we now turn to some recommendations for improving intention estimation.

Awareness. A common recommendation is that decision-makers and intelligence analysts should be aware that they do not make "unbiased"

choices or estimates or process information in an unbiased manner (e.g., Jervis, 1968: 462; Chan, 1979: 179). Such an explicit awareness, it is argued, would do much to offset the negative effects of the "official" theory of intelligence" and the "images of the enemy." For example, a more critical attitude toward "fact-gathering" and single hypothesis estimates might result. Estimators would seek more aids to offset biases (e.g., greater reliance on strong inference) and more critical reviews of their estimates.

There is strong evidence in psychological studies, however, that informing people of their biases and exhorting them to do better has little impact (e.g., Fischhoff, 1977; Kahneman and Tversky, 1979; Tversky and Kahneman, 1974); people do not become less biased after someone tells them they are. Nor do policy-makers readily adopt aids even when a case is made that they are needed (Brown, cited in Slovic, et al., 1977: 27). On the other hand, decision aids are sometimes adopted as showpieces and selling tools, rather than as direct aids to managing decisions (cf. Sapolsky, 1972). Nevertheless, analytic aids are constantly being accepted in various fields and a major outstanding question is how to get someone who needs help to accept it, and to use the help as it was intended.

Multiple Advocacy. The emphasis on the role of preconceptions and "images of the enemy" shaping estimates of intentions has prompted many critics of intelligence to recommend that the intention estimator surround himself with advocates of many differing viewpoints and hypotheses. Wohlstetter (1962: 302) recommends the intention estimator adopt "a willingness to play with material from different angles and in the context of unpopular as well as popular hypotheses." The review of

hypothesis testing and concept formation in Section 8 suggests that it is extremely difficult for a single analyst to do this, although the use of explicit formal models which yield competing predictions, coupled with strong inference hypothesis rejection techniques seems to have served well the successful intention estimators reviewed in Section 7.

Jervis (1968, 1976), Shlaim (1976) and others imply that, because the individual analyst cannot escape the hypnotic effect of his own preconceptions or early hypotheses, having many people with conflicting biases, or using large, diverse groups rather than small, homogeneous groups will facilitate multiple advocacy and the probing of information from different perspectives.

This recommendation can be evaluated against the research on how group performance compares with that of individuals (see Kelley and Thibaut, 1969, for a review). This research suggests that whether the group or the individual performance is superior depends on the problem undertaken. Groups perform as well as their best members when working on problems where (a) the solution has plurality support at the outset, implying that the problem was easy enough for many but not all the group members to solve it independently, and (b) the solution is attained by members with reputations of high competency, implying the group has had prior experience with problems of the same type and with the abilities of its members. Groups perform better than their best members when the problems involve (a) multiple parts and no one member has the entire solution, (b) the group members have uncorrelated deficiencies in capabilities so that member shortcomings offset each other, and (c) member relationships are highly cooperative and lack conflicting vested interests. If disputes become intense the

less aggressive members (not necessarily the less correct) give in, or the conflicting parties compromise on third alternatives (which are almost always wrong) to protect feelings and relationships. Groups perform worse than the most proficient member on tasks that require thinking through a series of interrelated steps or stages, applying a number of rules at each point, and always keeping in mind conclusions reached at earlier points. The verbalizations of several members who started at different points and pursued different lines of reasoning are mutually disrupting. Multiple-stage (in contrast to multiple-part) problems require the individual problemsolver to place in proper relation a number of ideas and pieces of information before he can see the answer. This implies the correctness of a given answer is not a simple matter. On problems of this type group processes handicap the most proficient member, all members tend to contribute to the discussion, whether their comments are helpful or not, and the difficulty of demonstrating the solution seems to prevent the most proficient members from making their due contribution.

An estimate of intentions might be any of these three types of problems. Some estimates will have group plurality support at the outset, in which case multiple advocacy may expose minority views, but these are unlikely to make any impact on the group solution. Multiple advocacy will be most effective on problems of the second type, if the group atmosphere is highly cooperative, otherwise a nonoptimal compromise solution tends to result. Estimates of this second type would be ones in which results from many fields of expertise would have to be interrelated and integrated into an overall estimate. For example, the Senate Select Committee on Intelligence (1978: 3) found the multiple

advocacy technical subteams of the Team A-Team B estimative exercise

> ...were the most rewarding: there was a mutual give-and-take, and these B Teams clearly made a constructive contribution. By contrast, the discussions concerning Soviet objectives were more controversial and less conclusive. The B Team on Soviet Objectives contributed some useful critiques concerning certain technical intelligence questions, but there was not much give-and-take on broader issues.

It seems likely that the technical aspects of Soviet strategic capabilities were multiple-part problems and were approached with a dispassionate, cooperative attitude, whereas the problem of Soviet Objectives was more likely a multiple-stage problem which was, the Senate Report makes clear, treated as a competitive versarial proceeding. Multiple advocacy is likely to be counterproductive when applied to multiple-stage estimation problems. George (1975: 95-6) seemingly agrees that multiple advocacy requires rather narrow conditions to work effectively. The three successful intention estimation methods reviewed in Section 7 were all multiple-stage procedures, i.e., would tend to yield worse results if conducted in a multiple advocacy environment.

In short, multiple advocacy will probably expose diverse perspectives and viewpoints in any case, but only on certain types of problems will these diverse views have an effective impact on the group's estimate. For other problems, multiple advocacy is unlikely to yield an effective estimate, and instead may produce either a nonoptimal compromise (nevertheless endowed with the cachet of a multiple advocacy team solution embodying "all viewpoints"), or else an incomplete resolution of the intention issue. In the latter case, a cautious, uncertain estimate will probably result, and, as Betts (1978: 71) noted,

"a wishful decision-maker can fasten onto that half of an ambivalent analysis that supports his predisposition," while the more objective official may consider the estimate as useless because it merely describes uncertainty but does not resolve it.

Szanton and Allison (1976: 191) note that U.S. national intelligence estimates are products of a multiple advocacy process, i.e., are composite judgments of CIA, DIA, INR and the military service intelligence agencies. Szanton and Allison fault this process for leading to compromises among these perspectives that often lead to "estimates that reflect an exaggerated, military-oriented view," delivered in "an ex cathedra fashion" that make it "impossible for policy-makers to uncover the analytic basis for the judgments offered, or ... the grounds for disagreement." Betts (1978: 76) argues that multiple advocacy is usually present in the intelligence and policy process, but it may highlight ambiguity rather than resolve it when the problem context includes data overload, uncertainty, time constraints, and differences of power or of opinion among experts. Further, he notes that redundancy and competitiveness found within the intelligence community may serve the function of "multiple advocacy" better than specially organized groups. A major defect with relying on the competing intelligence offices is that each agency's estimate may be perceived as serving the organizational or political interests of that agency. Since the agencies compete for resources and influence, competitive estimation efforts might be expected to generate much more heat than light. Analyst coordination across agency boundaries will probably be less competitive, but restrained by organizational rules.

Finally, multiple advocacy transfers the problem of data integration from the intelligence agencies (where the time and experts are

available to do such integration) to a higher level. The policymaker, confronted with multiple advocates, may feel as overwhelmed as he would if confronted with the raw intelligence data. If we assume the policymaker should not function as his own intelligence agency, why should we assume he should function as a magistrate for advocates. There is no reason to believe the policymaker is better equipped to integrate various hypotheses than are lower echelons, in fact, because time at the top is limited, the policymaker is perhaps ill-equipped to adjudicate among advocates. It is one thing to present policy makers with new perspectives, which require no decisions, but quite another to force them to resolve a decision out of the arguments and evidence of various advocates. Placing such a burden on the decision-maker removes one of the primary benefits of staffs, namely, the translation of arguments into comparable and commensurable terms for presentation to the decision-maker.

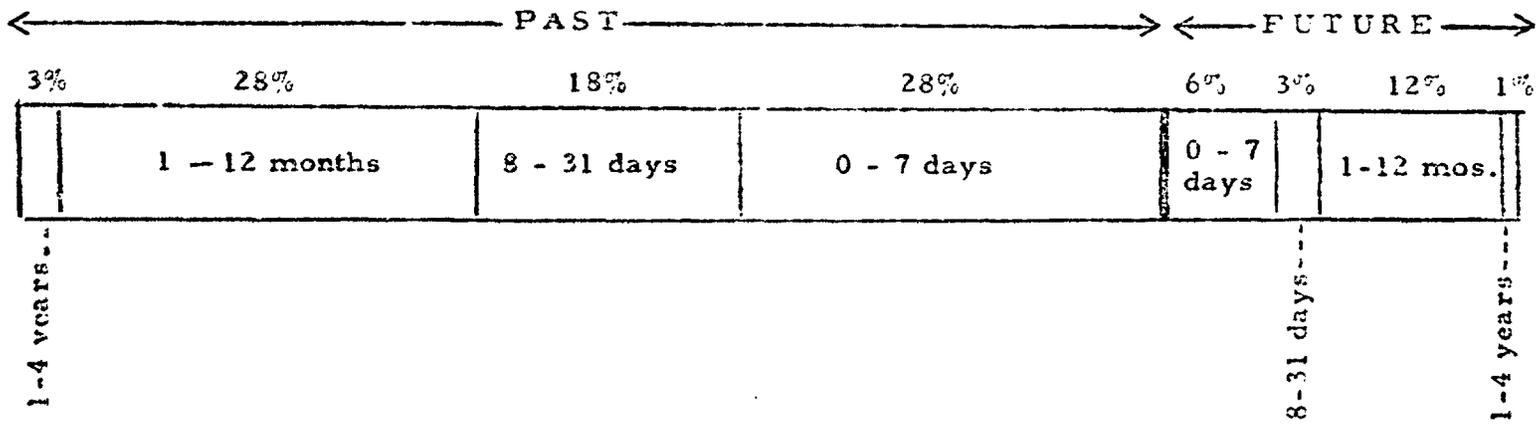
The Nature of the Effort. A recent assessment of intelligence production in the State Department's Bureau of Intelligence and Research (INR) categorized 504 intelligence products in various ways (O'Leary, Coplin, Shapiro, and Dean, 1974; O'Leary and Coplin, 1975). Of the 504 products, less than half were forecasts, and most of these were "descriptive-explanatory-forecast" or "descriptive-forecast." Only 31 of 504 (6%) products were classified as "forecast." Of the 504 reports, 41 percent made no reference to the future, and 36 percent more made an unspecific reference to the future; only 23 percent mention a specific future date. Of the 298 documents which referred to the future, only 2 percent referred to a date one to four years in the future, 38 percent refer to the short term (i.e., 0 to 365 days into the

future), while 62 percent have an unspecified future time reference. References to the past show a parallel bias which avoids specific reference to events more than one year in the past; only 3 percent of the references were this historic. In fact, if one takes these data on past and future time references, the INR "time perspective" can be portrayed as Figure 10.1, in other words, analysts rarely projected more than a year into the past or the future, and the most frequent references (74 percent) are to events in the past year. With so much backward looking it is hard to produce foreknowledge.

In short, INR products rarely make specific predictions or forecasts, and when the rare, specific forecasts are made, they tend to concentrate on the immediate future (i.e., the next week, or, more often, the next year).

It is interesting to compare the relative avoidance of specific long-range forecasting with the record of the FBIS propaganda analysts during World War II (George, 1959: 264, and Section 7). George sampled 729 inferences during a two-month period in 1943 and found 85 (12 percent) predicted Nazi policies and intentions, 76 (10 percent) predicted Nazi estimates and expectations; thus, almost one-quarter of the total inferences tended to be predictive. (Many specific predictions on strictly propaganda issues were included in the 729 inferences but not among the 161 inferences noted above, i.e., much more than 22 percent of the FBIS inferences were actually forecasts, but it is impossible to distinguish descriptive and explanatory inferences from forecasts for the propaganda inferences sampled by George.) These forecasts on the Nazis military and political actions were not coded by George as to whether the references to the future were general or specific, but the case studies

Figure 10.1. Time perspective of INR reports, based on number of reports with references to specific periods of time in the past and in the future. Number reports = 510. Based on O'Leary, et al. (1974).



he selected show a high proportion of predictions of specific times (see Section 7 for one example). Specific predictions seemed to have been the norm, especially in the later years of the war. On the other hand, the FBIS analysts, like INR, tended to make short-term predictions.

In summary, although the INR and FBIS analysts were dealing with similar intelligence problems, and although the FBIS analysts were limited to much more restricted sources of information (propaganda) than the INR analysts, significantly more of the FBIS products were specific forecasts. The contrast is even more striking if INR is compared, for example, to the CIC (see Section 7). Virtually all of the OIC estimates were predictions of future Nazi naval moves, and these predictions were highly specific, e.g., an estimate of where a particular U-boat would be, for how long, and what it might do. These comparisons lead to a specific point, that the less frequently an agency makes predictions of the future, and the less specific its predictions, the less accurately can the agency gauge the effectiveness of its analytic methods. Vague forecasts will rarely seem wrong or right, and the agency making such estimates has no effective way of applying feedback from future events. The agency which frequently makes specific predictions increases the probability that it will be wrong, but it is also able to apply the lessons learned from its failures to improving its predictive and estimative methods. This suggests that intelligence estimation of intention is a skill that must be practiced, mistakes have to be made and corrected, and that without practice, learning cannot result. We would recommend that, in general, the more specific predictions of intentions that were made, the better. (Belden, 1977: 193-6, offers some suggestions on the format and frequency of such predictions.)

However, this recommendation implies several problems. Unless intelligence agencies and consumers can tolerate and accept early mistakes as a cost for making improvements, analysts will probably restrict their efforts to the easiest problems and avoid predicting the "tough" ones. If the aim is to improve capabilities to call the "tough" ones, some tolerance has to be shown the analyst and agency that tries and fails. Rather than a batting average, a more appropriate benchmark might be the learning curve.

There may be both organizational and political costs involved in any effort to improve intention prediction through greater practice. Organizations might be judged, and perhaps even funded, on the basis of their hit rates or false alarms. This would probably be counterproductive since different agencies and offices tend to deal with problems of varying complexity, information quality differs, and a host of other factors make comparisons odious. Nevertheless, comparisons of the learning curves might be an appropriate tool for the allocating of resources and efforts: if two offices are working on different problems of roughly comparable priority, input resources (e.g., manpower, talent, internal analyses, etc.) should be allocated to the slower learner, while output resources (e.g., promotions, cash bonuses, high visibility, decorations, invitations to high councils, etc.) should be allocated to the faster learner. Obviously, bureaucrats could build empires by merely being the perennial slowest learner, so some circulation of manpower and talent rather than simple resource additions would be necessary. Certainly much good would come of determining how the fastest learners are doing it and applying these lessons to the slower learners. Practice in predicting intentions will not "make perfect" but it may make progress toward improvements.

Formal Models. A frequent recommendation, that analysts make greater use of formal assumptions, explicit hypotheses, and quantitative techniques, tends to follow from the recommendation that analysts and intelligence offices simply should make more frequent and specific predictions. Specific predictions are easier to make if formal models of intentions are employed, and such models make it easer to determine why predictions turned out wrong or right, thus avoiding hindsight biases to some extent.

Intellectually and psychologically, the use of formal models is very difficult advice to apply. Formalizing models requires logical rigor, statistical and mathematical sophistication, a willingness to employ such aids as experimental or quasi-experimental designs, as well as substantive expertise. Furthermore, there are the dual problems of just how many competing hypotheses one can test, or should test; and just how open to be to various data sources (cf. Jervis, 1968). No general recommendations can serve every instance.

Jones (1978) offers many insights into how the expert intelligence analyst can use his knowledge and formal models to limit hypotheses and lines of evidence. For example, when Jones attempted to determine the characteristics of the V-2 rocket (p. 447-8), he knew that only liquid fuels could chemically provide the necessary thrust-to-weight ratio needed to power a practical rocket (solid fuel technology was, at that time, too primitive). The only candidate liquid systems that would chemically work involved liquid air or liquid oxygen. Jones thus ruled out many hypothetical rocket designs which used neither. Furthermore, he assembled only those reports which mentioned either liquid air or oxygen, and in them found a highly consistent set of characters reported,

although agreements on individual details were imperfect. Although he started with many hypotheses about the rocket design, Jones quickly narrowed them and, by selecting evidence which included the liquid air or oxygen "touchstone" of truth, eliminated the "noisy" data, and collated only the accurate reports.

Heuer (1978) and O'Leary, et al. (1975) provide more recent examples of the use of formal models in intelligence estimation. Axelrod (1979) offers an example of a mathematical treatment of the surprise attack problem. Brams (1977) and Zagare (1979) offer game theoretic methods as a means of coping with deception problems.

There are severe drawbacks to using formal models. Their use is counterpsychological, counterorganizational, and counterpolitical. That is, most people feel able to think their way through tough problems without the employment of high-powered logic, statistics or technical aids (see Section 8). Barnds noted (1974: 29) "subtlety of thought about complex issues is seldom a noteworthy trait of any large organization." Organizational pressures toward consensus, compromise, and conformity mitigate against tests of multiple hypotheses. Specialization in organizations limits the number and distribution of experts who will comprehend any given formal methodology. Other experts will argue that the issues and their analyses "represent such complexity that no single quantitative work ... could even begin to test their validity (O'Leary, et al., 1974: 228), although, as was seen in Section 8, it is with such multidimensional problems that people tend to do worse unless assisted by formal methods. On the political level, few successful politicians have the technical, scientific, or quantitative backgrounds to readily comprehend the elements of formal hypothesis testing.

Although legal training seems to facilitate the use and comprehension of logic, the tendency of lawyers to rely on words may interfere with comprehension of the quantitative aspects of formal models.

Despite these hurdles, greater official emphasis on the use of quantitative, formal methods in nontraditional areas has led to the formation of such offices as the Methods and Forecasting Division of the Office of Political Analysis in CIA (Heuer, 1978). Heuer concludes (p. 8), on the basis of several years' experience in applying formal models to political analysis, that

> ...the kinds of analytic techniques which seem most useful ... are those that help to trace the logical consequences of subjective judgments, extend the mental capacity of the individual analyst, force the analyst to make his assumptions explicit, or help to organize complexity.

While Heuer acknowledges the modest success of the Methods and Forecasting Division (which he formerly headed), he notes that formal methods analysis "is only a very small part of the total political research effort." The centrality of formal models to the work of the successful intention estimators (reviewed in Section 7) suggests that, despite the problems in their application, formal methods should be a major part of attempts to estimate intentions.

Coordination: Much has been made of the increased coordination between intelligence analysts in the U.S. intelligence community (Barnds, 1974; Belden, 1977; Betts, 1978). For example, Belden (1977) describes the National Operations and Intelligence Watch Officers Net (NOIWON) and the National Operations and Intelligence Analysts Net (NOIAN), and comments that (p. 193) "although improved communication techniques are not a panacea, they might help solve some of the organizational difficulties surrounding ... indications, warning, and crisis." Betts p. 87)

comments that the period of testing of these new coordination mechanisms is as yet too brief to demonstrate their effectiveness.

Some (e.g., Huizenga, 1974: 43; Church Committee in Fain, et al., 1977: 253-4) see coordination as a problem rather than a solution. Huizenga argues that "community coordination works against product quality unless there is adequate competence in the analytic effort of the participating agencies." The Church Committee notes that high-level coordination of estimates may produce "a reinforcing consensus" in which divergent views are "submerged in a sea of conventional collective wisdom." The coordination process seems to the Church Committee to yield estimates which lack clarity of judgment, are "waffly or delphic," hedged, fuzzy, or imprecise.

Jones (1978) argues that coordination of estimates produces poor analysis and that the essential form of coordination is between the analyst and the intelligence collectors on one hand and with the intelligence consumers on the other. By keeping in close contact with collectors, the analyst learns the strengths and weaknesses of his sources; by working closely with the intelligence consumers, he learns what type of estimates are most needed and most wanted, thus is better able to guide his own efforts and the efforts of the collectors. Jones does not believe that committees, analyst coordination, or centralization can produce meaningful integration of data from all-sources. In his wartime work, Jones kept his staff small, in the belief that

> ...the larger the field any one man can cover, the more chance there is of those fortunate correlations which only occur when one brain and one memory can connect two or more remotely gathered facts (p. 32.).

Similarly, Winn, in the OIC Tracking Room (see Section 7), believed in tight coordination between collectors, analyst, and operators, with the analyst performing all phases of evaluation, integration, inference and estimation. Since OIC's charter was unchallenged, Winn's only significant analytic coordination was with his American and Canadian counterparts on the other side of the Atlantic.

Today there are organizational and bureaucratic restraints on coordination between analyst and collectors and organizational and political restraints on analyst-consumer liaison which restrict or prohibit the analyst from the free-wheeling methods of Jones or Winn. Today's collection systems seem far more complicated and expensive to task than those of World War II. Many more physical phenomena, people, platforms, services and organizations are involved. Collectors serve a much wider variety of customers and agencies. The need to protect "sensitive sources and methods" limits who is even permitted to know of the existence of a collection program. These factors all argue for some measure of isolation of collection from analysis, and some buffer between analysts and collectors is always necessary to prevent waste and duplication and to make the most efficient use of the sources. Nevertheless, removing some of the bureaucratic barriers and organizational restraints between collectors, processors, and analysts is a reform that should receive greater consideration. Coordination among analysts is no substitute for analyst familiarity with the sources of his information. In view of the extensive and expensive efforts being devoted to linking analysts together via computer networks (e.g., ARPANET, SAFE, COINS, NAIWON, NOFWAN, etc., are some noted by Phillips and Hayes, 1975) the need for direct coordination channels between

analysts, processors, and collectors becomes more apparent, while the greater difficulty of controlling the latter channels is also apparent since the management of collection resources is involved.

The problem of coordination between analysts and intelligence consumers is equally complex but for different reasons. A variety of issues are related: consumer guidance and evaluation of intelligence; the work overload of analysts and consumers; and the relationships between policy, operations and intelligence.

The tendency of presidents, cabinet officers, prime ministers, and commanders-in-chief to act as their own intelligence officers (or worse, as their own intelligence services) is often noted (e.g., Church Committee, in Pain, et al., 1977: 249; Barnds, 1974: 30; McLachlan, 1968: 359). These intelligence consumers, whose time is very limited, nevertheless work as their own analysts, particularly in the context of the role of "crisis manager." Under such pressure, the conclusions these top level decision-makers draw from intelligence are likely to be badly flawed. In calmer periods, the policy-maker turns to other interests, and longer range intelligence analysis and assessments are ignored.

Several factors contribute to this phenomenon. First, policy-makers frequently accuse the intelligence community of irrelevancy and of lacking insightful judgments on the more subtle but useful issues, such as the intentions of foreign groups and leaders (Church Committee, op. cit.: 350). Given the policy-makers' private access to other heads of state, the conclusions of analysts who do not share in these private conversations may indeed seem pallid or irrelevant to the policy-maker. Furthermore, the intelligence analyst may have no knowledge of

important policies or actions of the decision-maker which affect foreign intentions. Finally, the policy-maker may complain but offer no guidance on what is wanted.

Certainly this problem reflects the fact that the relationship between intelligence and policy is a two-way street. Intelligence officers need sufficient familiarity with the environment of policy to determine where intelligence is needed. Policy-makers can express their wants but may be imperfectly aware of their needs. But access to the policy-maker is not a simple matter. The intelligence officer has to convince the policy-maker that intelligence has some unique and valued service to provide.

Policy-makers cannot expect good service and quality production from intelligence if they ignore that staff and supplant it with their own efforts. While the attitudes and personalities of the major actors (i.e., President, Director of Central Intelligence) play an important role in this relationship (as Huizinga and Barnds suggest), intelligence has seemingly aggravated its own problems by avoiding the "tougher" estimates of intentions, and instead, accentuating its role as "bean counter" (i.e., estimator of weapons inventories) and as a reporter of "current events" (Church Committee, op. cit.: 255). In part, the "current intelligence syndrome" arose from the bureaucratic competition among intelligence agencies all striving to be the first to deliver the latest tidbit. Since most daily reports are now CIA products, competition is reduced. The tendency of the policy community to act as its own intelligence analyst fostered and sustained this heavy flow of daily "factlets." In its efforts to satisfy this demand, intelligence often cuts into its own long-term estimative resources. When the intelligence

community is required periodically to improve its in-depth and long-term analysis, it is rarely allowed to cut back its current intelligence services. The present emphasis on crisis centers, command centers, watch centers, warning centers, etc., expands the demand for both short- and long-term analysis. While intelligence has a clear responsibility to provide warnings and alerts, it is far less clear that intelligence should be obliged to furnish a daily stream of what amounts to hurriedly analyzed or even raw information. If intelligence were to attempt to merely reduce this hemorrhaging, or confine it organizationally, the unavoidable demands for current events would soon undo the efforts as they have in the past. If this function were removed entirely from intelligence production, other agencies would probably swiftly take it over. Intelligence might still retain the responsibility for warnings and alerting the community to dangers, and stand ready to comment on (but not to report) current events, including "no comment," if events warrant nothing more.

Such a drastic step is probably impossible. It would mean the intelligence community as collector would be feeding current events to some other agency for collation, processing (and perhaps evaluation), and dissemination, and then would become a consumer of this product. These other agencies would soon become competitor intelligence producers, expanding their "product line" to longer-term analyses. On the other hand, it is difficult to see how the intelligence community can deliver a high-quality, long-term estimate (which must be superior to the policy community's own efforts if it is to be accepted in policy circles) if most of its resources are consumed with servicing the policy community's appetites for semi- and unevaluated information for policy-makers' efforts at intelligence analysis.

Given this demand for current intelligence, there are neither the resources nor the support from policy-makers for long-term estimates of intentions. Consequently, crises and surprises multiply and policy-makers have even less faith in intelligence to provide the necessary warnings and alerts. The policy community becomes even more reliant on itself for intelligence analysis, increasing its demands for current events. In trying to perform thoughtful and careful analyses as well as making decisions under the stresses of crises, policy-makers find themselves even less able to anticipate and foresee events, lowering still more their opinion of the intelligence services they receive, and increasing their demand for more "relevant" intelligence, the "right" facts, more "timely" information, etc. Ultimately a failure occurs and the debate begins as to whether it was an intelligence or a policy failure. And the cycle starts again.

The methods of the successful estimators of intention (Section 7) included withholding raw data and the latest reports until an adequate evaluation of them could be made. The temptations to report on current events were resisted until an estimate of the situation and a useful forecast could be produced. The high quality estimates that resulted led the operators and policy-makers to rely on intelligence because its products were superior. Intelligence quality led policy to grant intelligence an estimation charter and to rely on these estimates in planning operations. The close relationships between the consumers and producers of intelligence led to mutual exchanges of relevant forecasts: the policy-makers keeping intelligence informed of future plans, intelligence keeping the policy community informed of the enemy's future plans. By working together, policy and intelligence came to

know and appreciate each other's strengths and weaknesses; consequently, requests were reasonably within the capability of intelligence and responses were tailored to the needs of policy. Intelligence producers could guide and reward intelligence collectors. Policy and intelligence understood the real causes underlying the other's failures. Working together on the crises that came out favorably and those that did not, both learned to appreciate the element of uncertainty inherent in estimating intentions and acting on such estimates. This too was a cycle, but a benign one. It developed in the crucible of the most complex and threatening crisis of this century and it dealt successfully with challenges far more severe than any since faced by governments. It provides lessons we have still to relearn and a goal we have yet to seriously pursue again -- how to make and use estimates of intentions.

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20. ABSTRACT (Continue on reverse side if necessary and identify by block number)

Political and military intention estimation should and can be done, but little systematic research has been devoted to intention estimation methods. The problems preventing accurate estimates are psychological, organizational, and political. The cognitive, social, and organizational mechanisms underlying intention estimation are fairly well understood, but little has been done to reduce the identified weaknesses or to capitalize

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STRATEGIC SURPRISE

BLOCK 20. Abstract (continued)

on the known strengths. Much less is known of the political aspects underlying successes and failures. Estimation weaknesses are diagnosed and the problems and prospects for prescriptions are outlined. Most importantly, every reform reduces old weaknesses but creates new weaknesses in the estimation process, as well as adding new strengths. Improving intention estimation requires applying the lessons learned from past successes as well as the lessons of past failures. Three episodes of successful intention estimation were analyzed and the common features noted. These strengths were compared with the weaknesses of intention estimation failures, identified by application of a tentative theory of intelligence failure based on the sociology of the origins of disasters. A case study of the Yom Kippur War intelligence failure was analyzed with the disaster theory. This theory was shown to be related to the common problems of knowledge generation experienced by scientists.