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The Cuban Missile Crisis and Intelligence Performance

JAMES G. BLIGHT and DAVID A. WELCH

The foregoing essays collectively raise important questions, not only about what happened in the Cuban missile crisis (and why), but also about intelligence assessment more broadly.1 As we stated in our introduction, we see them as the beginning of a more extended conversation between scholars, intelligence professionals, and (we would hope) policy makers, rather than as the final word on any topic in particular. A proper synthesis is clearly premature. But given the Sisyphean nature of missile crisis scholarship, it may be doubted whether the time will ever be ripe for a true synthesis. Hence in this essay we make so bold as to offer an interpretation of the collective import of the preceding pages, knowing full well that some of it must later stand to be corrected.

We would like to try to grapple with deep and abstract questions having to do with the evaluation of intelligence performance, and the theory and practice of intelligence assessment – though we shall evince some skepticism about the possibility of a ‘theory’ of intelligence assessment per se. While not everything we will have to say on this score will be entirely novel, we hope that the perspective in which we will attempt to put it will be unfamiliar and fresh. These are our chief aims. However, we believe it would be fruitful to ease our way into these topics by first having a closer look at some of the nuts and bolts of intelligence assessment in the missile crisis, both to provide some real-world purchase on more abstract issues, and to flag items that require further exploration. We begin with unresolved or puzzling empirical questions that arise from the preceding essays – questions first about what actually happened, and then about why certain things happened and others did not – and then proceed to examine and evaluate certain important perceptions, judgments, and inferences. This will help us frame and anchor the more abstract discussion to follow.
‘WHAT’ QUESTIONS

Owing to the various dimensions of imbalance that we discussed in our introductory essay, rather more questions about what actually happened in intelligence assessment and intelligence-policy interactions in the Cuban missile crisis arise with respect to the Soviet Union and Cuba than with respect to the United States. This is not to say that the American intelligence story is an open book – far from it – merely that there are relatively few aspects of the American story that remain utterly mysterious to outsiders. The single most important lacuna, of course, is signals intelligence (Sigint), encompassing communications intelligence (Comint) and electronic intelligence (Elint). Whether the United States had broken Soviet and Cuban codes – and if so, what of importance, if anything, they learned from eavesdropping on Soviets and Cubans – are some of the important pieces of information not yet on the public record. Neither do we have details of Soviet or Cuban Sigint, of course – but it is at least ironic that in the Soviet and Cuban cases, we have clear testimony to the effect that their capabilities were limited and not very useful. We do not even have a general characterization of this sort with respect to American Sigint. As Raymond Garthoff notes, one direct reference to signals intelligence in a now-declassified document, coupled with judicious inferences about security deletions from other declassified documents, permits the surmise that Sigint was on balance more useful to the United States than was human intelligence (Humint), but less useful than photointelligence (Photint). This would confirm the sense students of the missile crisis without security clearances have had (ourselves included) that the United States probably did benefit from signals intelligence, but that our understanding of the event would not change radically if the Sigint story could be told in full. But this, of course, remains to be seen.

Although Aleksandr Fursenko and Timothy Naftali have done a remarkable job of painting what was until very recently an almost entirely blank canvas, the Soviet intelligence picture is less complete. Three question in particular arise as a result of apparent inconsistencies between the accounts of Fursenko and Naftali on the one hand, and Domingo Amuchastegui on the other; a fourth question arises out of inconsistencies between the documentary and testimonial records.

1. Did the KGB consider an American invasion of Cuba imminent between the Bay of Pigs and the Cuban missile crisis, and if so, did they report this to Moscow? Fursenko and Naftali maintain that the KGB provided the Kremlin with raw information that could be construed in two quite different ways: suggesting both that there was great danger of an American invasion
of Cuba, and that such an invasion was unlikely. They suggest that the KGB hedged its reports, even at the price of blatant inconsistency, so as to be able to defend itself in the face of any contingency. Nevertheless, the KGB was unable to reach a firm determination of American intentions toward Cuba, and never issued a clear warning that an invasion was imminent. The best and most insightful intelligence suggested that it was not. Aleksandr Feklisov, the KGB rezident in Washington, judged quite perceptively that Kennedy would have to be provoked into invading Cuba and would not willingly attack otherwise. Fursenko and Naftali even imply that in judging an American attack unlikely, the KGB relied in part upon Cuban assessments. Amuchastegui, however, reports that in 1961 Soviet intelligence began furnishing Cuba with ‘constant, unambiguous, dire threat assessments ... for which there was no good-quality evidence’. As Amuchastegui readily acknowledges, the lack of Cuban documents represents a major obstacle for scholars who wish to get to the bottom of issues such as this. It is entirely possible, however, that Fursenko and Naftali on the one hand, and Amuchastegui on the other, are both correct. The KGB may have deliberately skewed its reports to Cuba for some ulterior purpose. Indeed, this is what Amuchastegui reports Cuban intelligence eventually decided. The conclusion Cuban intelligence retrospectively drew from this – that Khrushchev must have conceived the nuclear deployment in 1961, and that he made use of falsified KGB threat assessments to prime Castro to accept it – is logically consistent with, though not logically entailed by, such dissimulation. Khrushchev may have had other goals in mind in pursuit of which it would have been useful to persuade Castro that the Cuban Revolution was in imminent danger from the United States. He might simply have sought to draw Cuba closer to the Soviet Union, for instance.

2. What did the Soviet intelligence community know about the plan to deploy missiles, and when did it know it? Fursenko and Naftali claim that Aleksandr Alekseev was the only KGB field officer who was briefed on the Cuban initiative. Alekseev was summoned to Moscow in May, appointed ambassador, informed of the plan, and dispatched back to Cuba with a delegation led by alternate Presidium member Sharaf Rashidov and Deputy Minister of Defense (and Commander-in-Chief of the Strategic Rocket Forces) Marshal S. S. Biryuzov, whose primary task was to sell the idea to Castro. By all accounts, however, neither Georgi Bolshakov – the GRU’s man in Washington – nor Aleksandr Feklisov knew of the deployment before President Kennedy announced it to the world on 22 October. Semichastny claims he found out about the deployment only when the KGB took custody of nuclear warheads aboard Soviet merchant ships in late
summer. This implies that certain KGB units charged with transport and custody of nuclear munitions must also have been aware of the decision at some point. But without knowing precisely who in the Soviet intelligence community knew of the deployment, when they knew it, and why they were (or were not) informed, it is difficult to come to a refined assessment of the relevance and performance of Soviet intelligence in the episode.

3. Did the KGB orchestrate a campaign to mask the nuclear deployment by funneling accurate information about it to the CIA through Cuban sources? If the KGB was unaware of the missile deployment, then it could not have attempted to mask it by leaking accurate information about it. Yet Domingo Amuchastegui claims that this is precisely what the KGB did: 'This campaign presupposed – quite correctly – that the CIA would discount this information, because they would not consider the individuals and groups peddling it to be credible.' One can only admire the daring and the genius of such a plan. But if Fursenko and Naftali are correct that Alekseev was the only KGB field officer briefed on the deployment, then unless Alekseev personally directed it – and he has never claimed that he did – Amuchastegui must be mistaken.

Here, again, we must await further documentation for a definitive resolution. But it is entirely plausible to imagine that the KGB and Cuban intelligence did cooperate on such a campaign.

One possibility is that the KGB in Cuba did, in fact, know about the nuclear deployment. Indeed, it is difficult to imagine that they did not. Thousands of Soviet troops in Cuba surely were aware of it; why would the local KGB be unaware? If they had not been informed in advance, certainly they would have discovered it in any case (Cuban intelligence did, after all, despite not being informed). But it is difficult to imagine why the local KGB would not have been informed. They would have been the most useful cadre imaginable for protecting the secrecy of the deployment. It is easy to imagine that Fursenko and Naftali err to imply that only Alekseev knew.

It is equally easy to imagine, however, that Amuchastegui errs in recalling – or perhaps in assuming – that the KGB could only have conceived and carried out such a plan if they knew that the deployment actually included strategic nuclear missiles. The KGB officers involved in designing the campaign could have believed that the Soviet Union was only deploying conventional military forces to the island, and yet still would have appreciated the value of tales about nuclear missiles. Their task was to capitalize on the CIA's low estimate of Cuban credibility. Why would they not make use of stories that they, in their ignorance, believed to be false, and as such all the more incredible?
4. What exactly happened between Scali and Feklisov? This particular question is shaping up to become one of the enduring mysteries of the Cuban missile crisis. Fursenko and Naftali do an excellent job of summarizing what we know, and sketch out plausible scenarios. We fully concur with their judgments, and have nothing to add to their discussion. We would like, however, to highlight their conclusion, which underscores the truly important point: 'While it is only speculation that the contradiction between the accounts suggests that Scali and Feklisov misunderstood each other on 26 October, it is certain that the United States and the USSR misunderstood what went on between the two men.' Any positive effect this intelligence side-show may have had was entirely fortuitous.

Two additional 'what' questions arise from Domingo Amuchastegui’s essay, one of which has important implications for the accuracy of the historical record, and one of which does not. The minor issue is whether Cuba accepted Soviet offers of intelligence assistance, as Fursenko and Naftali state, or whether Cuba requested it, as Amuchastegui implies. Nothing of real importance would seem to turn on the answer, though the divergent representations may be evidence of the turbulent undercurrent of national pride that undoubtedly complicated the Soviet-Cuban intelligence relationship, just as it profoundly complicated the Soviet-Cuban political relationship.

The important question is whether Che Guevara traveled to Moscow in April 1962. Amuchastegui provides a detailed account of the mission, and characterizes it as a crucial turning point in Soviet-Cuban relations. According to Amuchastegui, the purpose of the trip was not only to request increased levels of military aid, but also military aid of a ‘qualitatively different’ character. Amuchastegui claims that Che ‘left it up to Khrushchev himself to make a concrete proposal’, and that ‘[a]t no point did Che suggest a deployment of Soviet nuclear missiles, nor did Khrushchev offer them at that time’. Nevertheless, if Khrushchev understood Che to be probing for an offer of nuclear assistance, this could certainly help explain why the idea took root in Khrushchev’s own mind almost immediately thereafter. The timing would be perfect.

The difficulty is that we have been unable to confirm Amuchastegui’s claim that any such trip took place. We cannot locate any reference to such a mission in any of the published literature, and the knowledgeable Cubanologists whom we have consulted on the matter have no recollection of it. Neither Alekseev nor Oleg Daroussenkov – Che’s special assistant in 1961 and 1962, a former staff member and interpreter in the Soviet embassy in Havana, and thereafter the leading Cuba specialist at the Central Committee – can remember such a visit. Nor is there any evidence of it in
the material Aleksandr Fursenko viewed in the Russian Presidential and Foreign Intelligence Service (SVR) archives during the course of his collaboration with Timothy Naftali.20

It is not out of the question that Che undertook such a mission, but it is highly curious that this particular trip would be shrouded in such secrecy when other trips to the Soviet Union by high-ranking Cubans dealing with equally sensitive topics (arguably, more sensitive topics) have been matters of public record for some time. This would suggest that if Che did travel to Moscow in April, something far more sensitive than Amuchastegui suggestions must have transpired—something of the order of an explicit request for nuclear assistance. By all accounts, however, Castro evinced surprise when Biryuzov suggested a nuclear deployment during his trip to Cuba at the end of May.

Might Che have made such a request on his own initiative? Che and Castro certainly had their share of disagreements, but they were very close, and it is difficult to imagine that Che would make such a request without at least vetting it with Fidel. In any case, since the Soviets would not have known the purpose of Che’s visit in advance, there would have been no reason why Alekseev or Daroussenkov would have been unaware at least of the fact that Che intended to go to Moscow. Nor would there have been any reason for the visit to pass entirely without mention in Soviet documents. If the trip indeed took place, then we must wonder very seriously about the recollections of former Soviet officials, the accuracy and completeness of the Russian archives that have been opened to scholars, or both.

Is someone simply lying? We find it difficult to imagine what personal, professional, or national interest would be served by fabricating such a story. Nothing in it smacks of personal or professional accomplishment, let alone triumph. The story does not tend to cast Cuba, Cuban intelligence, Che Guevara, or Khrushchev in either a favorable or an unfavorable light. It provides no basis for explaining away an embarrassment, or escaping some blame. It is simply an interesting story with no particular valence.

It is possible that Amuchastegui has some other trip in mind. There are two possibilities. Che did travel to Moscow in late 1960 for the purpose of requesting increasing levels of military aid, and there are at least some indications that, while in Moscow, he did float a trial balloon about a Soviet nuclear deployment in Cuba.21 However, Amuchastegui distinctly connects Che’s trip and the Escalante affair (March 1962), which means that if he were thinking of Che’s 1960 trip, he would have to be confusing two significant events separated by almost a year and a half. Of course, Amuchastegui’s claim that ‘Khrushchev did not raise the Escalante affair or the expulsion of Kudriavtsev during the course of his meetings with Che’22 would certainly make sense if the trip had taken place in late 1960, rather
than in April 1962, since the Escalante affair had not yet taken place.23 A second possibility is that Amuchastegui is thinking instead of the trip to Moscow by Ramiro Valdés in March 1962. The timing is much better; but on first glance the context would appear to be wrong. Available records indicate that Valdés dealt primarily with KGB officials in Moscow, and that the chief purpose of his visit was to coordinate strategy for promoting revolution in Latin America (a task in which he failed utterly, much to his disgust).24 However, Valdés was certainly a member of Castro’s inner circle, and it is entirely possible that he could have been charged with the task of requesting increased military assistance. He could easily have approached Khrushchev secretly on this, and it would not be surprising that no mention of it appears in the available accounts. It may also be significant that when Rashidov and Biryuzov traveled to Cuba in late May to propose a nuclear deployment, Valdés was one of the five Cuban leaders (along with Fidel Castro, Raúl Castro, Che Guevara, and Osvaldo Dorticós) who met to decide whether to accept the démarche.25 This solution to the puzzle involves the fewest logical hurdles, the smallest number of facts which must be adjusted (only two: the date of the trip, and the lead member of the Cuban delegation), the fewest gaps in the documentary record that must be accounted for, and the smallest number of people whose recollections must be mistaken. Additionally, of the two facts that must be adjusted, one – the date of the trip – requires a trivially minor adjustment.

While we must await further evidence from Cuban (or Russian) sources before we can solve this particular mystery, it is worth noting here what may hang on its outcome: namely, whether Cuba prompted, or Khrushchev proposed, a nuclear deployment. This is not a question that concerns intelligence specifically, but it is one that certainly bears on the genesis of the crisis. If Amuchastegui’s story is correct, Castro may have deliberately sowed the seed that germinated in Khrushchev’s mind.

‘WHY’ QUESTIONS

It is likely that as the documentary and testimonial record grows richer, many more questions about what actually happened will arise. For now, though, we wish to put them aside and ask why certain things happened, and why others did not. All five of the preceding essays do this throughout, of course, in somewhat different ways, and space prohibits a comprehensive review. Nevertheless, we would like to flag and explore some of the more interesting and more important puzzles before we move on to consider deeper issues, such as what constitutes a ‘good answer’ to questions of this kind. For the moment we are still concerned with fairly nitty-gritty matters of historical detail that emerge from the three empirical essays. Later in the
essay we will tackle the larger, more cosmic questions, such as why the three countries so badly misunderstood each other – questions which are the primary focus of the essays by Wirtz and Fischer.

**US Puzzles**

As we explain in our introductory essay, we believe the two most commonly asked ‘why’ questions about American intelligence in the Cuban missile crisis – (1) Why did DCI McConne, but not his analysts, anticipate a Soviet deployment?; and (2) Why did American intelligence discover the missiles only in mid-October? – are both relatively unimportant and fairly easy to answer when understood – as most historians of the crisis have understood them – as questions of historical detail. McConne got lucky (an insight devoid of practical implications for intelligence); the weather in Cuba was bad (ditto). We do not propose to consider them further here. They do, however, raise larger ‘why’ questions which we propose to examine in some detail later: namely, (1) Why did US intelligence, in attempting to estimate the likelihood of a Soviet nuclear deployment in Cuba, use as the basis of their judgment one set of assumptions about Soviet behavior rather than some other available set that might have led to a radically different estimate?; and (2) Why did American intelligence employ such an exacting standard of proof – direct photographic evidence of nuclear missiles – before being willing to conclude that a deployment of the kind President Kennedy had specifically warned the Soviet Union against, both publicly and privately, was, in fact, underway?

To help answer these larger ‘why’ questions, it would be helpful to know the answers to two related nitty-gritty questions. First, why did US analysts ignore the estimate of the size of the Soviet force in Cuba provided by the operational branch of the CIA? Samuel Halpern has testified that Task Force W concluded in September 1962 that there were 45,000–50,000 Soviet military personnel in Cuba. The official American estimate in early October was 4,000–4,500 – an order of magnitude lower. As we now know, the operations people had it just about right. Halpern is certain that CIA analysts received the Task Force W estimate in September, but does not know what happened to it. Why did the estimators dismiss it?

The only defensible reason would be that they had some contrary evidence to suggest that the estimate must have been off by about an order of magnitude. If so, this evidence has never been made public. There are at least three other possible (non-mutually-exclusive) explanations – one bureaucratic, one cognitive, and one motivated:

- The estimators may simply have been defending their turf. Estimates, after all, were their job.
The CIA's presumption about the nature of the Soviet mission – to train Cuban troops – may have skewed their estimate. The CIA did not believe at that time that the Soviets intended to deploy to Cuba a substantial nuclear deterrent and a full battle-capable conventional force to defend the island from American attack. A training mission would have required no more than a few thousand Soviet troops, but many times more would be needed to mount a proper deterrent and defense. They may simply have seen what they expected to see.

CIA estimators may not have wanted to believe they were off by an order of magnitude, because of the painful personal or professional repercussions of being wrong.

We cannot say for certain which (if any) of these explains the error, singly or in combination, although our sense, from discussions with scholars and former intelligence professionals – as well as from a close reading between the lines of the available documents – is that the cognitive explanation is most plausible, the bureaucratic explanation somewhat less so, and the motivational explanation unlikely.

The second question is related, and may be an important part of the answer to the first: Why did CIA estimators so heavily discount information from Cuban sources? Is Domingo Amuchastegui correct to infer that ‘there was a pervasive and unprofessional distrust of Cuban sources’? Garthoff writes:

Most reports were of no value, or perhaps negative value; there were literally thousands of reports of missiles in Cuba in the period before any missiles were actually brought there. Only in late September and early October (after the 19 September SNIE) were a few reports from Opa-Locka received in Washington that, in retrospect, probably were valid sightings of the medium-range missiles – but that could not be determined at the time. The existence of hundreds of other reports that were found to be not valid, and many hundreds of others of undetermined validity, made it ever more difficult to credit the few that were later found to be true.

It is one thing to question the accuracy and usefulness of a report because it is vague, confused, insufficiently detailed, self-contradictory, or inconsistent with other reliable information; it is entirely a different thing to question the accuracy and usefulness of a report because one suspects the motives, competence, or intelligence of the class of persons to which the reporter belongs. The poor quality or suspiciousness of one Cuban refugee report provides no valid logical ground for discounting another, high-
quality, Cuban refugee report; nor do 50 low-quality or suspicious reports provide twice the reason for discounting the 51st report as do 25 for discounting the 26th. Every single report should be assessed on its merits. We would certainly criticize a doctor who based a diagnosis on the frequency of diseases in the general population, rather than on the patient’s symptom profile: the fact that a disease is extremely rare does not mean that any given patient must not have it.

We do know that a few reports from Cuban sources were gems – very precise and very detailed descriptions of what, in retrospect, almost certainly were Soviet MRBMs. We also know that some of these reports reached Washington well before the photographic discovery of missiles on 15 October. We also know that, as a result of numerous false sightings and misinterpretations, ‘CIA analysts had naturally come to view all such reports with a high degree of suspicion.’ It is easy to imagine that the CIA was prejudiced against Cuban sources; that this prejudice delayed the Kennedy administration’s conclusion that a nuclear deployment was underway; and that it prevented the CIA from appreciating the true size of the Soviet expeditionary force.

Soviet Puzzles

Many questions arise with respect to Soviet intelligence, of which clearly the most important – stated most generally – surely must be, ‘Why was intelligence so marginal to Soviet policy making?’ We can ask four more specific questions in an attempt to probe the possible answers.

1. Why was the KGB not tasked to assess the likely US response to a nuclear missile deployment? Khrushchev had a deep and abiding fear of nuclear war, and apparently also genuinely worried that the United States might crush the delicate flower of socialism in Cuba. It is fair to assume that if Khrushchev truly believed that the United States, in response to a covert nuclear deployment, would take measures dramatically increasing the danger of both outcomes, he would not have undertaken it. He must therefore have believed that they would not. Khrushchev could not reasonably consider himself an expert on American politics, or on President Kennedy; so it would be natural – if he were reasonable – for him to ask those better qualified than himself to prepare a sober assessment.

Among those better qualified was his personal aide on foreign policy matters, Oleg Troyanovsky, who, as the son of the first Soviet ambassador to the United States, had grown up in Washington, attended Swarthmore College, and knew the American political scene extremely well. Troyanovsky’s testimony is worth quoting at some length:
Even though I was familiar with all the information that Khrushchev received on foreign policy, I did not immediately find out about his intention to deploy nuclear missiles to Cuba. I recall perfectly well the day when another adviser ... told me that we were contemplating the idea of deploying nuclear missiles in Cuba if the Cuban leadership agreed. I was definitely taken aback with this information, because being someone knowledgeable of U.S. affairs, and realizing the importance of such a step, I knew this would entail serious consequences. I was then faced with the dilemma of discussing this with Khrushchev or not, although my colleagues in the Secretariat told me that there was no sense in discussing this because the decision had been made and a change in the decision would be impossible. However, I was doing this just to calm my own conscience, and when I found the appropriate time I talked to Khrushchev. He said that he was aware that this was a very serious decision, but why should we not do what the Americans had been doing all along? Why couldn’t we deploy missiles in Cuba when we were surrounded by US military bases and US missiles? He even said that the Americans appealed to the Monroe Doctrine every so often, but that the Monroe Doctrine did not just call for the non-intervention of European states in U.S. affairs, but also the non-intervention of the U.S. in European affairs. The United States had already discarded this doctrine. Against this logic, what was I to say, especially since I really did not expect a change in the decision that had been made? That was the end of our discussion.

What is interesting here is Khrushchev’s academic, legalistic, and deductive approach to the problem. In effect, Khrushchev thought that if the Americans were reasonable, they would tolerate the Soviet deployment. The Americans were reasonable; ergo, they would tolerate the deployment. This was clearly unreasonable. Even if Khrushchev’s minor premise were correct, the major premise evinced an astonishing naïveté about American sensibilities. Troyanovsky, Soviet Ambassador to the United States Anatoly Dobrynin, Aleksandr Feklisov, and no doubt other intelligence and foreign service officers who devoted their lives to the American political scene, could have corrected Khrushchev’s astounding misapprehension about American complaisance. But he asked none of them.

Fursenko and Naftali explain the failure to task the KGB to estimate the US response in terms of the organizational culture of intelligence in the Soviet Union:

For Western scholars, it is even more startling that no one in the KGB was asked for a formal assessment of the likely American reaction to the placement of missiles in Cuba. Since World War II, the US
intelligence community had attempted to provide estimates of the intentions and capabilities of important foreign countries. While these estimates often evinced the flaws of committee-generated reports, they at least reflected an effort to coordinate and make sense of secret knowledge on a certain policy matter. In some cases, these reports sought to clarify the murky world of the subjunctive, to illuminate what might happen. Called ‘intelligence estimates’ in the Anglo-Saxon intelligence world, these reports were foreign to Soviet experience. In Moscow, ‘what if’ questions were reserved for the consideration of the Presidium alone. This does not strike us as quite right. ‘What ifs’ are an inherent part of threat assessment. As Fursenko and Naftali themselves note, the KGB was tasked to provide an assessment of US intentions toward Cuba several times – for example, immediately after Aleksei Adzhubei’s meeting with President Kennedy in January 1962 – and in some of these assessments, the KGB attempted to identify hypothetical conditions under which the United States might attack. An especially interesting example of this was a July 1960 estimate that a US invasion of Cuba was unlikely unless the Cubans invaded the US naval base at Guantánamo Bay or unless the Soviets attempted to station nuclear missiles in Cuba. This estimate – which Fursenko and Naftali imply had never formally been withdrawn or revised – was, in our view, a quality piece of intelligence that certainly should have given Khrushchev pause had he not been so convinced (for whatever reason) that the deployment would succeed.

While it may well have been unusual for Soviet leaders to pose direct ‘what if’ questions to the KGB, it was not unusual for the KGB to consider them. But more to the point, the organizational culture of Soviet intelligence cannot explain this particular failure well because the real question is not simply ‘why was the KGB not tasked to assess the likely US response to a nuclear missile deployment?’, but why was nobody asked? Quite simply, Khrushchev did not think it necessary. He was the kind of man – headstrong and brimming with self-confidence – who did not feel the need to ask the experts.

2. Did intelligence play any role in Khrushchev’s judgment that the deployment would succeed? Troyanovsky’s testimony highlights Khrushchev’s deductive approach to forecasting; but there was intelligence available that could have buttressed Khrushchev’s confidence. A ‘highly regarded source’ indicated to Feklisov in March 1962 that Kennedy would not risk military action against Cuba before the mid-term congressional elections in November. This report could have encouraged Khrushchev to
believe that he had an eight-month window of opportunity to act. Of course, as Fursenko and Naftali note, the KGB provided the Kremlin with ample material that could just as reasonably be construed to indicate American hostilities were likely at any time. Therefore, if Khrushchev had been paying attention to Soviet intelligence when attempting to gauge the likely American response, he was clearly paying attention selectively.

3. **Why was the GRU not tasked to assess the prospects for a successful covert deployment?** Quite distinct from the question of whether the United States would tolerate Soviet nuclear missiles in Cuba was the question of whether the Soviets stood a decent chance of transporting them, building facilities for them, and making them operational before the United States could discover them. Khrushchev seems to have believed quite strongly that it was necessary to deploy covertly and to confront the United States with a *fait accompli*. Despite his academic and legalistic turn of mind, he energetically resisted when, in late September, Che Guevara and Emilio Aragonés pleaded with him to go public with the deployment, stressing the sovereign right of the Soviet Union to supply, and of Cuba to accept, any armaments whatsoever that they deemed necessary for Cuba’s defense. They proposed this so as to deprive the United States of a pretext for action. Presumably, Khrushchev thought Americans were reasonable with guns pointed at their heads, but not otherwise.

Soviet military intelligence would have been the most competent body to assess the feasibility of a secret deployment. There is no indication, however, that the GRU was asked to conduct a study and make a report. It would appear that Khrushchev was satisfied to allow Biryuzov to make that determination himself while in Cuba in late May and early June. Biryuzov appears to have concluded that MRBMs could easily be hidden in Cuba, in caves and mountains, or disguised as palm trees. It is unclear what, if anything, Biryuzov thought about the feasibility of hiding the roads, buildings, and launch pads associated with the sites, or of preventing the United States from noticing the removal of already-deployed weapons from the Ukraine and their shipment 8,000 miles into the United States’ backyard. The problem of *maskirovka* was simply left to the planners of Operation ‘Anadyr’ and treated as an operational detail.

As with the first Soviet puzzle, the best explanation for the failure to task the GRU with a more intensive study may be Khrushchev’s over-confidence and commitment. It would certainly appear that the Soviet leadership did not appreciate the difficulties of a secret deployment, which may reflect ignorance of logistical technicalities and/or American capabilities. We do not know whether it would have been unusual for the Kremlin to ask military intelligence to conduct feasibility studies for secret military
deployments, so it is difficult to gauge an organizational-cultural explanation. It would be interesting to know in any case what the GRU would have concluded about the prospects for covert deployment if it had been given the opportunity to study the problem closely.

4. What explains the sloppiness and amateurishness of KGB reporting in the Cuban missile crisis? Fursenko and Naftali give an account of Soviet intelligence during the Cuban missile crisis that is astonishing given the KGB’s reputation for competence. Students of intelligence will know that laymen and outsiders consistently overrate the capabilities of intelligence services; but even when measured against a more realistic standard, the KGB appears to have performed extremely poorly in this episode. Fursenko and Naftali describe an organization obsessed with espionage, unable to integrate secret and open sources, either unwilling or unable to synthesize and interpret, ill-informed about Soviet policy, lacking well-placed informants, reduced to the ignominy of hanging around a parking lot at the crack of dawn to keep a journalist under observation, and – if one of the more plausible versions of the Scali-Fomin story is correct – undisciplined to boot.

While we can certainly blame Khrushchev personally for certain demand-side intelligence failures, clearly he cannot be responsible for supply-side failures. We are inclined to credit Fursenko and Naftali’s organizational culture explanation here. ‘Professional prudence was the principal reason for the inadequacy of Soviet intelligence analysis’, they write, combined with a ‘limiting institutional ethos’ emphasizing the collection of secret information.” This goes quite a long way toward explaining why the KGB fared so poorly. But in addition to this, it is worth noting that the KGB suffered from technical shortcomings that made its performance seem all the worse. The KGB’s inability to fathom the mind of John F. Kennedy did not reflect institutional incapacity per se, since a different ethos, and a different set of procedures, could not have compensated for a lack of quality sources at or close to the White House. But unlike the CIA – which was equally unable to fathom the mind of Nikita Khrushchev, and which appears likewise to have had no good sources close to the Kremlin – the KGB could not fall back on impressive technical collection of intelligence on the adversary’s capabilities in order to contribute to policy making, and to appear busy and productive to national leaders.

Cuban Puzzles

We have so little information on Cuban intelligence – there was essentially none, prior to Domingo Amuchastegui’s essay – that it is difficult even to
speculate about possible explanations for certain obvious questions, let alone hazard tentative answers. Nevertheless, several are worth flagging.

First, Amuchastegui emphasizes the stark disagreement between Soviet intelligence and the Dirección General de Inteligencia (DGI) on the likelihood of an American attack on Cuba. Amuchastegui’s discussion of the various possible explanations for that disagreement is both fascinating and instructive. For the moment, however, it is worth recalling Amuchastegui’s claim that Cuban military intelligence – the Dirección de Inteligencia Militar (DIM) – consistently echoed the Soviets’ dire threat assessments. What, then, explains the stark disagreement between DIM and DGI?

Both organizations had high-level Soviet advisers, so it seems unlikely that the difference can be accounted for simply by a closer professional relationship between Soviet and Cuban personnel in one case than in the other. DIM fell under the Ministry of the Revolutionary Armed Forces (MINFAR) led by Raúl Castro, however, and it is conceivable that Raúl’s pro-Soviet sympathies may have had some influence on DIM’s willingness to accept Soviet assessments. Another possibility is that DIM’s professional focus on military threat assessment led it to rely heavily on American military capabilities as an indicator of political intent. DGI appears to have based its assessment on a broader analysis of political context, and on its reading of the goals of Operation ‘Mongoose’ (certainly a more defensible basis, since military capabilities are not reliable indicators of political intent).

A second puzzle is Fidel Castro’s failure to inform his own intelligence services of the true nature of the Soviet deployment. It is not clear why Castro would wish to keep his own intelligence service in the dark, although one obvious possibility is that he wished to limit dissemination of this information to reduce the danger of leakage. Given the state of unrest in Cuba and the intense activity of counterrevolutionary groups, it would have been reasonable for Castro to fear that the CIA had informants within his intelligence services. Yet Cuban intelligence could not properly fulfill its primary mandate – warning of an American attack – without knowing what the Soviets in Cuba were up to that might provoke one.

This leads directly to a third puzzle: namely, if Cuban intelligence did, in fact, catch on to the nuclear deployment, and did attempt to warn the Cuban leadership of the danger that this might provoke the United States, why did Piñeiro refuse to pass that threat assessment on to Castro? Amuchastegui explains this in terms of Piñeiro’s ‘reluctance to submit to the Cuban leadership assessments that would challenge or even contradict Fidel Castro’s own thinking’. It is certainly possible that Piñeiro worried Castro might shoot the messenger. Yet if this is the case, why did Castro send Che
and Aragónés to Moscow in late August to plead for public disclosure? Surely Castro himself must have been concerned that the Soviet deployment could precipitate an American attack. It is far from clear that on this score the judgment of Cuban intelligence would challenge or contradict Castro's thinking. Piñeiro may have known that, of course, in which case he would have been wise not to pass on a report containing nothing Castro did not already know or believe. This would only irritate Castro, and in Cuban politics, that would be a mistake.

Fourth—and, for moment, finally—what was the basis of Castro's confidence on 26 October that an American attack was imminent? Amuchastegui insists DGI did not provide Castro with any information to that effect. Certainly it is unusual for political leaders to warn intelligence analysts of an impending assault. Amuchastegui is probably correct to infer that the information did not come from Soviet intelligence, since Castro issued precisely the same warning to Khrushchev. It is possible, as Amuchastegui speculates, that Castro drew this conclusion from his own reading of open sources, or, as Fursenko and Naftali suggest, from Brazilian President João Goulart, whose explicit warning of an imminent attack coincided with, and reinforced, an otherwise inconclusive report from Cuban intelligence in New York. As far as we know, Castro himself has never identified a particular source for his judgment. It thus remains a mystery.

EVALUATING INTELLIGENCE PERFORMANCE

The authors of our three empirical essays all make judgments about the performance of the respective intelligence bodies they examine (and, in at least one case, of the adversary's intelligence service as well). Were they to assign grades, we suspect that Garthoff would give the American intelligence community an A–; Fursenko and Naftali would give the Soviet intelligence community at best a C– (if not an 'Absent'); and Amuchastegui would give the Cuban intelligence community a solid B+ (and the CIA a D). The Stennis Committee and the President's Foreign Intelligence Advisory Board (PFIAB) both probably would have given the CIA a grade of B–; DCI McConie, in his response to the PFIAB report, a B+. We know of no official Soviet intelligence post mortems, so we cannot determine how the Soviets assessed their own intelligence performance. Amuchastegui notes that there were major changes to Cuban intelligence after the crisis, indicating some dissatisfaction, but that many of these changes simply sought to reduce Cuba's reliance on Soviet and East European intelligence.

It is natural to try to judge the performance of intelligence communities, yet it is very difficult to know what standards or benchmarks to employ. It
is unrealistic to expect intelligence to know everything, and to know it right away. Omniscience, omnipotence, and ubiquity are out of the question. Even an intelligence service operating at peak efficiency and unhampered by resource constraints will make mistakes and fail to anticipate important events every once in a while. In her essay, Beth Fischer argues that perfectly normal human psychology imposes a ‘performance limit’ on intelligence. Two decades ago Richard Betts eloquently and perceptively identified a number of other insurmountable obstacles to perfection, psychological and otherwise.

How, then, do we know how well an intelligence community is performing? We cannot design meaningful tests of real-world performance, as we can (say) for student drivers. There are two reasons for this. First, we cannot manipulate the international environment. Certainly it would have been an intriguing idea for American officials to ask Nikita Khrushchev to deploy nuclear missiles in Cuba, so that they could test the CIA’s forecasting skills; but there would have been no way to secure Khrushchev’s compliance, and in any case, this would have been perverse. Second, since we cannot predict the future, we cannot know the ‘right answers’ before we administer a test. If we could, we would not need intelligence services.

Occasionally it is possible to know in real time when an intelligence service misses something. From time to time crucial information of the kind it is the intelligence community’s job to acquire reaches national leaders through entirely different channels. This does not, however, provide a basis for determining whether intelligence communities are performing well or poorly in general. An intelligence community that is performing extremely well in general will, unavoidably, miss certain things. This is logically implied by the notion of a performance limit.

Retrospective Evaluation: Three Temptations

Most judgments about the performance of intelligence arise from retrospective evaluations, such as official post mortems and academic studies. Retrospective evaluation has the crucial advantage of a fixed historical record against which to assess analysts’ perceptions, beliefs, and inferences. There is always some irreducible element of uncertainty about what actually happened, of course, but this degree of uncertainty cannot compare with our uncertainty about what is happening now and what will happen in the future. Retrospection is the best we have.

How accurately can we gauge intelligence performance in retrospect? We submit that the answer is ‘not very’, largely for straightforward epistemological reasons. However, we would like to suggest that retrospective evaluation – though imperfect – can be useful if we have a particular understanding in mind of how we can benefit from it. We suspect,
however, that unrealistic expectations about intelligence performance, coupled with understandable but misguided views about what retrospective evaluation is for, typically frustrate the enterprise.

Three common temptations represent obstacles to fruitful retrospective evaluation: the temptation to focus on the specific and the spectacular (a form of selection bias); the temptation to privilege hindsight; and the temptation to try to evaluate performance in terms of a 'rate of success.'

1. Selection bias. It is almost irresistible to focus on particular dramatic failures when assessing intelligence performance retrospectively. The enormous literature on surprise attacks provides a useful example. Admittedly, these are engrossing events. It may be doubted whether studies of intelligence successes could compete for our attention, because one of the chief things that makes a failure spectacular is that it has a dramatic consequence. A properly-exploited intelligence success may lead to the prevention of something spectacular. Moreover, a success does not provide the occasion for public angst about intelligence capabilities, and would not normally spawn public review or discussion. Our fascination with failures is entirely understandable. However, it is clear that by focusing our attention on failures, we may overestimate the weaknesses of an intelligence community — much as aviation disasters incline people to exaggerate the dangers of flying (uneventful flights do not make headlines).

In principle, we could avoid this temptation by making an effort to study a representative sample of intelligence episodes — selecting not on the outcome, but randomly, or by period of time. But this is easier said than done. Successes leave less of a documentary trail than failures, and mundane episodes attract less attention than dramatic ones. There may simply not be enough information available to conduct a truly balanced study. The combination of inherent allure and available information means that spectacular intelligence failures have an almost irresistible gravitational pull.

2. Privileging hindsight. Hindsight tends to color our judgments of how intelligence analysts should have interpreted information. This leads to unrealistic expectations about performance.

Once we know what has happened, we can easily distinguish between meaningful and meaningless indicators: the signals stand out starkly from the noise. But to intelligence analysts attempting to anticipate events, those same signals may be barely perceptible, and quite understandably so. After the Japanese attacked Pearl Harbor, it was easy to see that there was plenty of information available on the basis of which US intelligence might have anticipated the attack. It is tempting to take the further step and fault US
intelligence for not anticipating it. Hindsight provides such a powerful prism that the intellectually frail often cannot resist the conclusion that US intelligence must have known about Pearl Harbor in advance. Thus are conspiracy theories born.57

It is in the nature of Monday-morning quarterbacking to argue that if intelligence had done X rather than Y, they would have plucked the signals from the noise and correctly anticipated events. And it may well be true that if US intelligence had concentrated on certain targets rather than others, relied more heavily on certain methods of collection than others, handled information differently, made use of different working assumptions, etc., etc., they would have reached the conclusion that the Japanese were about to attack Pearl Harbor in time to do something about it. The problem is that we cannot know this, even with the benefit of hindsight. All choices have trade-offs and opportunity costs that are difficult to estimate. Choosing X might have induced other (possibly cascading) errors.

Moreover, to go the extra step to conclude that intelligence should have done X rather than Y begs additional questions. Was X technically possible? Would intelligence have had good reasons for doing X at the time? Would those reasons have been better than the reasons they already had for doing Y? How do we know what reasons are good, what reasons are bad, and, of two good reasons, which is the better?58

3. The temptation to determine a ‘rate of success’. In an enlightening and deservedly well-regarded essay written shortly after the Cuban missile crisis, Klaus Knorr attempted to understand why American intelligence failed to anticipate the Soviet deployment, and to put that failure in a context that would permit a meaningful evaluation of intelligence performance. Knorr quite properly acknowledged the inevitability of surprise. In a now-famous passage, however, he stated that ‘the practical problem is to improve the “batting average” – say, from .275 to .301 – rather than to do away altogether with surprise’.59 We believe this to be misguided.

It is a natural temptation to think of ‘good’ intelligence performance in terms of a high rate of success, and ‘poor’ intelligence performance in terms of a low one. Just as we can be confident that a baseball player batting .350 must be doing things right and a player batting .125 needs serious work, an intelligence agency that fails rarely must be doing well, and one that fails often must be doing badly. It is also natural to think that retrospective evaluation, despite its various pitfalls and difficulties, at the very least permits us to gauge the relative frequency of correct and incorrect judgments. On first glance, this seems straightforward. When an intelligence community correctly anticipates the enemy’s move, we score a hit; when it does not – as the CIA did not, in failing to foresee the Soviet
nuclear deployment to Cuba – we score an out.

The moment we begin to think about operationalizing these assessments, however, we quickly discover that the metaphor is both meaningless and unworkable. Consider SNIE 85-3-62:

The USSR could derive considerable military advantage from the establishment of Soviet medium and intermediate range ballistic missiles in Cuba, or from the establishment of a Soviet submarine base there. As between these two, the establishment of a submarine base would be more likely. Either development, however, would be incompatible with Soviet practice to date and with Soviet policy as we presently estimate it. It would indicate a far greater willingness to increase the level of risk in US-Soviet relations than the USSR has displayed thus far, and consequently would have important policy implications with respect to other areas and other problems.

The estimate equivocated. In effect, it said that a nuclear deployment was unlikely, but not impossible. It did not assign probabilities, but, for the sake of argument, we can imagine that SNIE 85-3-62 meant to communicate that there was a 0.2 probability that the USSR would deploy MRBMs to Cuba, and a 0.8 probability that it would not. When calculating the CIA’s batting average, should we count this as one-fifth of a hit, and four-fifths of an out? DCI McCone estimated the reverse. Was he at bat, too, or only the authors of the SNIE?

Suppose that Khrushchev had consulted with his American specialists earlier in the game, got cold feet, and decided to subtract the nuclear component from the Soviet deployment. In that case, SNIE 85-3-62 would have been right on the money, and we would have to score it a hit (or, at least, 80 per cent of a hit). And yet, in this scenario, there is nothing whatsoever that the CIA would have done differently. Do we want our evaluations of the performance of an intelligence community to turn on extraneous stochastic contingencies? Put another way: why should we not blame Khrushchev, rather than the Office of National Estimates, for the mistaken SNIE?

Consider now Cuban intelligence. The DGI predicted that the United States would not tolerate the presence of Soviet nuclear missiles in Cuba. By ‘not tolerate’, they had in mind a military response. Now, the DGI was correct to predict that the United States would not tolerate the missiles, but it did not anticipate the blockade. How do we score this? Do we count it as two at-bats – one hit and one out for a batting average of .500? Or was the second estimate simply part of the first – a swing-and-a-miss during the one at-bat of consequence, so to speak – for an average of 1.000?

Consider also that from April 1961 to September 1962, DGI estimated
that an American attack was not imminent. During that period, no such attack took place. How do we score this? Do we count one successful at-bat for every day that the United States did not invade? Every week? Every month? Every three months? Our decision will certainly have dramatic implications for DGI's batting average.

It is clear that there is no non-arbitrary, rigorous way to gauge the relative frequency of successful and unsuccessful judgments, even in retrospect. Even if there were, we could not be certain that this would be a valid indicator of the competence of an intelligence community. Sloppy estimates are sometimes right, and careful estimates sometimes wrong, for reasons over which the estimators have no control. The batting average metaphor is therefore misplaced.

Indeed, it is possible to argue that a batting average is an imperfect indicator of skill even in baseball. Batters who play more games on artificial turf, against weaker pitchers, and against weaker defensive teams will have higher batting averages than better batters who play on grass against stronger pitchers and better defensive teams. Over a long season, a batting average can be a fairly reliable indicator of skill for players who play in a league with a balanced schedule, because these circumstances approximate the ceteris paribus condition. But even then a batting average is only a relative measure of skill, and the valid comparison group is limited to other players in the league. Intelligence analysis takes place under comparatively few parametric constraints. Analysts may be called upon to predict anything, anywhere, by anyone. Different intelligence communities may face radically different challenges – as James Wirtz discusses in his essay – and one intelligence community may face consistently more difficult tasks than another. Finally, in intelligence assessment there is no ‘standard equipment’, and corked bats, far from being forbidden, are actively encouraged. Intelligence ‘batting averages’, therefore, would be meaningless measures of relative performance even if it were possible to calculate them in a rigorous, non-arbitrary way.

Knorr might reply that he did not intend for us to take him so literally. Perhaps he simply meant his metaphor to imply that the practical problem is to minimize the number of errors. Some might be tempted to say that this goes without saying. We are not so sure. It may not be the number of errors that best measures intelligence performance, but the kind of errors.

Perils Illustrated: the US ‘Failure’ to Predict the Soviet Deployment

It might be helpful at this point to examine an important judgment that has attracted special attention in the preceding essays (and elsewhere), to see some of these temptations at work, to illustrate the difficulties of avoiding them, and to highlight the difficulties of making rigorous evaluations. This
will help prepare the ground for our discussion of how retrospective evaluation can be used constructively. Because of the richness of the available record, the US ‘failure’ to predict the Soviet deployment will serve well.

The Stennis Report concluded that the ‘predisposition of the [US] intelligence community to the philosophical conviction that it would be incompatible with Soviet policy to introduce strategic missiles into Cuba’ was the primary cause of its failure to predict the deployment. This preconception, according to the report, prevented the intelligence community from giving ‘proper weight’ to ‘indications to the contrary’. The implication is that US intelligence should not have been so firmly wedded to its preconceptions, and if it had not been, it might not have made this important error.

The Stennis Report’s conclusions provide an interesting example of Monday-morning quarterbacking. Did the US intelligence community have ‘good reasons’ for relying less heavily upon the preconception that such a deployment would be incompatible with Soviet strategic policy? Garthoff writes: ‘In the absence of clear evidence of deployment, the estimate [i.e., SNIE 85-3-62] had to rest on an assessment of Soviet intentions, and the past record tended to support the conclusion that the Soviet leaders would not deploy strategic nuclear missiles in Cuba.’ Our sense is that most professional intelligence analysts, and most scholars of the Cuban missile crisis, would concur with this judgment.

Garthoff’s statement has several interesting features. The first is the phrase, ‘In the absence of clear evidence of deployment’. What, exactly, is ‘clear evidence of deployment’? ‘Only direct evidence, such as aerial photographs, could be convincing’, Garthoff writes. As a psychological matter, this was undoubtedly true in 1962. Neither President Kennedy, nor senior members of the intelligence community – DCI McCone included, we suspect, since he was so concerned about stepping up photographic coverage of the island – would have been willing to conclude that the Soviets were deploying missiles to Cuba without photographs of Soviet missiles in Cuba. But ought American officials to have had such a demanding standard of proof? The Stennis Report did not state, but seemed to imply, that the answer was no, concluding that a contributing cause of the failure was the CIA’s tendency ‘to discredit and downgrade the reports of Cuban refugees and exiles’. As we have already suggested, if the CIA discounted reports simply on the ground that they came from Cuban sources, this would at least have been a logical error, and might also have been a pathological prejudice. But would it have been an avoidable one? It is easy to say in retrospect that the CIA should not have been so hard on Cuban refugee reports, but it is difficult to imagine how CIA analysts
working without the benefit of hindsight could have ‘corrected’ for a prejudice of which they were not conscious, and which, though illogical, is perfectly normal psychologically.\textsuperscript{71}

The second interesting feature of Garthoff’s rejoinder is the phrase, ‘the estimate had to rest on an assessment of Soviet intentions’. If the CIA could not see that a deployment was underway, it would have to try to read Soviet minds. The CIA had no sources in the Kremlin, and hence no good information on whether Khrushchev and his colleagues were contemplating a nuclear deployment. How, then, were they to know what Khrushchev was thinking?

Enter the third interesting feature of Garthoff’s statement: ‘the past record tended to support the conclusion that the Soviet leaders would not deploy strategic nuclear missiles in Cuba’. Garthoff here invokes the principle of induction: namely, that the future will resemble the past. Several logical difficulties arise here. First, the CIA was attempting to ascertain the likelihood of something that had never happened before, and to do so relied upon a principle whose strict application will always result in failure to anticipate the unprecedented. Second, the CIA made the logical mistake of inferring intentions from behavior. It is fallacious to infer present intentions from present behavior, let alone to infer present intentions from past behavior. An act, a practice, or a policy may be consistent with a variety of incompatible purposes (the deployment itself was logically consistent with both offensive and defensive motives, for example).\textsuperscript{72} Third, even if past behavior could predict present intentions and future behavior, the inference would be arbitrary unless we could determine which of several possible ways of setting up the induction applied in a given case. It is certainly correct, as the CIA noted and emphasized, that the Soviet Union had never before deployed nuclear missiles beyond its borders. But neither had the Soviet Union ever failed to extend its nuclear umbrella over a friendly Marxist-Leninist state. Nor had the USSR ever passed up an opportunity to deploy land-based nuclear missiles in the Western Hemisphere (Cuba was, after all, the first opportunity to arise). Finally, ‘past Soviet behavior’ was a composite of the behavior of several Soviet leaders, only one of whom was Khrushchev, and he had not yet had much of an opportunity to leave his imprint. As Beth Fischer notes, the CIA might have attempted to base the estimate specifically on an assessment of Khrushchev’s intentions and Khrushchev’s risk-taking propensities, instead of on some generic ‘Soviet’ characterization.\textsuperscript{73} There was no guarantee that Khrushchev would do what Lenin or Stalin would have done had they been in his shoes instead.

Most US officials were shocked by the discovery of the deployment because it seemed to them to be so out of character. Nevertheless, it was
trivially easy for analysts working with the benefit of hindsight to construct explanations of Khrushchev’s behavior that conformed with their preconceptions. Some of these retrospective assessments cut the Gordian knot simply by assigning blame to Khrushchev for failing to appreciate the nature and intensity of American concerns about Soviet forces in Cuba, or for overestimating the likelihood that a covert deployment might succeed. The current record shows that Khrushchev erred, but it is interesting to note that these retrospective assessments – while in this respect quite accurate – themselves made various indefensible logical leaps. Knorr, for example, making use of a constructive distinction between technical and behavioral surprise (that is, surprise that results because someone succeeds in fooling us, and surprise that succeeds because it is, or seems to be, inconsistent with our set of expectations), 74 concludes that there was nothing wrong with the set of expectations about Soviet behavior prevalent in the US intelligence community in 1962, and that the Stennis Report erred in finding fault with it. Instead, Knorr considers the failure to anticipate the deployment a case of ‘apparent behavioral surprise’, where the USSR seemed to have adopted a course of behavior that was inconsistent with US expectations, but, in fact, was not. In support of this conclusion, Knorr reasons that the lengthy period of planning and preparation that must have been involved in the deployment was ‘not conducive to a strong component of emotional or irrational decision-making’, and that Moscow’s underestimation of the risk of the deployment can most probably be attributed to Soviet intelligence failure. 75 Knorr is undoubtedly correct to suppose that there were Soviet intelligence failures; but we now know that the Soviet mistake was over-determined, and that emotional or ‘irrational’ factors most certainly played a role.

Other retrospective assessments downplayed Soviet errors and quite creatively constructed a set of beliefs on the basis of which Khrushchev ‘must’ have been operating that would have made the covert deployment appear to be a rational gamble. These are interesting because they managed to preserve three key assumptions: (1) that Khrushchev was aware of the nature and intensity of the American interest in Cuba; (2) that Khrushchev would not behave fundamentally differently from his predecessors; and (3) that Khrushchev would not accept a poor gamble. These explanations typically relied heavily (for example) on the claim that Khrushchev had a low estimate of Kennedy’s resolve – because of his youth, his ‘liberalism’, his failure to follow up the Bay of Pigs disaster with full-scale American intervention, his overly-intellectual performance at the 1961 Vienna summit, and so on – and/or the claim that Khrushchev thought that his stranglehold on Berlin gave him the leverage he needed to force Kennedy to accept a nuclear missile deployment in Cuba. These assessments also make enormous logical leaps, egregiously demonstrating the fallacy of
inferring motives and calculations from behavior. Even nuanced retrospective analyses that made some allowance for Soviet miscalculations tended to make indefensible assumptions and inferences that we now know, on the basis of a much richer record, to have been in error. Most strikingly, as Garthoff discusses at length, US post mortems consistently neglected or downplayed the extent to which Khrushchev’s behavior was consistent with defensive or deterrent purposes. It is interesting to note that every time US analysts raised the possibility – in real time or in retrospect – that Khrushchev might be operating on the basis of defensive motivations, they let this possible explanation wither on the vine.

The conclusion we must draw, then, is that there was no logically valid way for the CIA to infer Khrushchev’s intentions from the information available. The information was consistent with any number of possible explanations and predictions. The best the CIA could do, as a matter of logic, was to say that Khrushchev might deploy nuclear missiles to Cuba, but that he might not. They could also say, in effect, ‘If we were in Khrushchev’s position, knowing what we know, we would not attempt to deploy nuclear missiles to Cuba.’ We think this is not a bad way of interpreting SNIE 85-3-62. The difficulty is that it tells us a great deal about the CIA, but very little about Khrushchev.

And yet it is difficult to specify exactly what the CIA should have done differently, given existing constraints on information. When the President of the United States asked whether the Soviet Union was likely to station nuclear missiles in Cuba, the CIA had a duty to try to answer. To predict the deployment correctly, the CIA would have had to assume a number of cockamamie things – for example, that Khrushchev would not ask any of his experts about how the United States would react to a nuclear deployment in Cuba, and that Khrushchev could believe that the Soviet military could send 45,000 men and dozens of ballistic missiles 8,000 miles into the Caribbean without the United States catching on. It is easy to imagine how the President of the United States would have reacted to an estimate that stated these two assumptions explicitly.

Even if the CIA had fully factored Khrushchev’s risk-acceptance and his defensive or deterrent motivations into its estimates, it is not at all clear that the CIA would have concluded that such a deployment was, on balance, probable. It was a breathtaking gamble. We are inclined to agree with Garthoff that, even taking into account what was not then known, the estimate might have concluded that Soviet leaders probably would not place nuclear missiles in Cuba, even while giving ‘more weight and attention to the possibility that they would do so.’ But it is unlikely that this would have made any practical difference to a President already keenly intent on monitoring the Soviet buildup in Cuba.
Failures Consequential and Otherwise: How Should we Evaluate Intelligence Performance?

It is ironic that the US intelligence community made such an intense effort to look for something that they did not expect to see. And while it is possible that the United States might have discovered the Soviet nuclear deployment, and detected the true size and nature of the Soviet conventional deployment, somewhat earlier than it did, had it relaxed its expectations, its standards of proof, or both, it is worth recalling – as Garthoff puts it – that ‘intelligence did identify the missiles in sufficient time to allow successful American initiative and action to compel their withdrawal’.

‘Discovery a week or two earlier in October,’ Garthoff writes, ‘... would not have changed the situation faced by the President and his advisers.’

In what sense, then, did the CIA ‘fail’? It certainly failed to make a clear prediction that the Soviet Union would attempt to deploy nuclear missiles to Cuba. But it did not fail American policy makers who deeply dreaded the prospect. The CIA was vigilant, and told the President all that he really needed to know. Certain things the CIA did not know, and could not tell the President, even though he would have preferred knowing them: for example, exactly when the Soviet MRBMs in Cuba would become operational; whether or not nuclear warheads had yet arrived in Cuba, and if so, where they were stored, and whether they had been issued to missile units; and exactly how many Soviet troops were on the ground. We can call these failures if we like; but we are not certain that we see the point of doing so. Only the last was something the CIA could have been, able to divine given its existing capabilities, and given the information at its disposal. The others are very difficult tasks, and the CIA could only have known these had it been lucky in data collection. But in any case, none had a negative policy consequence. As Garthoff notes, the White House, the Pentagon, and the CIA all assumed in prudence that the Soviet missiles were operational and that warheads were present, and given the nature of US contingency plans for military operations against Cuba, it is unlikely that the underestimate of the number of Soviet soldiers there would have materially affected the outcome of an invasion.

The only significant lacuna that we can identify concerns the presence of tactical nuclear weapons. American officials did not presume that Soviet forces in Cuba would be equipped with tactical nuclear weapons, and had Kennedy sent American forces into Cuba, they would not have been equipped with similar weapons themselves. But while this was certainly a lacuna, it is unclear that the CIA should have been expected to discover that the Soviet Union had equipped its forces with tactical nuclear weapons, and it is possible to argue that the CIA had furnished Kennedy with ample
information on the basis of which to conclude that an invasion carried with it unacceptable risks in any case. Since he did not choose to invade Cuba, the CIA's failure to establish the presence of tactical nuclear weapons did not have negative consequences. In fact, as Garthoff argues (correctly, in our view), the CIA's failure to establish the full size and character of the Soviet deployment had a distinctly positive policy consequence: it facilitated the resolution of the crisis. It may be an unusual circumstance when intelligence serves policy better by knowing less rather than more; but this is a useful reminder nonetheless that the task of intelligence is not to know everything, but to serve policy, and that intelligence can serve policy well even when it does not know everything, and sometimes even when it makes mistakes.

What we see on the American side of the Cuban missile crisis, at least, is a constructive intelligence-policy dynamic. Communications between the White House and the CIA were far from perfect, and there was a great deal of uncertainty in the air. But while the CIA did not predict a Soviet nuclear deployment, and did not fully appreciate the size, purpose, and battlefield capability of the Soviet expeditionary force even after identifying MRBM's in Cuba, it managed to avoid communicating any overconfidence in what it did know (or believe) that would have either encouraged recklessness, or undercut the President's respect for, and appreciation of, the intelligence process. Taking in the broad sweep of the CIA's assessments before, during, and after the acute phase of the crisis, we believe that it did quite an impressive job of serving US policy, in the light of its technical capabilities and the inherent limits of intelligence assessment. We can try to learn from its failure to predict the Soviet nuclear deployment, but it should not bear more than marginally on our evaluation of its performance over the course of the crisis as a whole.

What of Soviet and Cuban intelligence? There is a sense in which Soviet intelligence was never really given a chance to fail, at least prior to the acute phase. Intelligence cannot serve policy when policy makers do not take it into their confidence and ask it policy-relevant questions. Once the crisis broke, it would appear that Soviet intelligence strove to give the Kremlin information useful for managing the crisis, but came up with very little. Certainly the KGB and GRU cannot be faulted for not having technical capabilities to rival CIA's or NSA's. But given the Soviet investment in Humint, one might have expected a better performance than Fursenko and Naftali describe. We will never know, of course, what contribution to policy Soviet intelligence might have made had it better integrated and synthesized sources, and had it not engaged in self-censorship. But we do know that these shortcomings deprived it of any real possibility. Our expectations of the performance of Soviet intelligence must be somewhat lower than for
American intelligence to begin with, in view of its technical inferiority and the acute challenges it faced by virtue of the structural and domestic political circumstances under which it operated (as Wirtz discusses at length). Nevertheless, even when measured against a lower standard of reasonable expectations, Soviet intelligence hardly acquitted itself well. We may say in its defense, however, that while it had great difficulty being relevant, at least it cannot be held responsible for disastrous policy.

We cannot hold Cuban intelligence up to a standard of performance appropriate either to American or Soviet intelligence. It was a nascent community operating under severe technical and resource constraints. Moreover, the Cuban leadership was clearly selective in what questions it asked its intelligence community, how it listened, and how it used intelligence. Amuchastegui's account suggests that most of the pathologies evident in the Cuban intelligence-policy relationship lay on the policy side. Nevertheless, as Amuchastegui makes clear, Cuban intelligence did make mistakes. They were not, however, mistakes of the kind that would encourage faulty policy, and Amuchastegui's tale indicates to us that Cuban intelligence had a remarkably mature demeanor throughout the episode in question. It was circumspect, relatively open-minded, and acutely aware of the structural and domestic political challenges it faced. In professionalism, it certainly compared favorably with the KGB.

Evaluating the performance of an intelligence community in this way—rather than focusing intently on specific spectacular judgments (usually misjudgments), second-guessing analysts with the benefit of hindsight, or attempting to gauge a rate of success—has, we believe, at least four merits. First, it forces us to factor into our evaluations some baseline expectation of performance. Clearly intelligence communities differ from each other in raw capability and the challenges they face. Simply counting successes and failures—even if we could do it meaningfully—could tell us nothing about how well they are doing relative to each other. But at least we can determine roughly how well an intelligence community is doing relative to its own performance in the past when we begin by trying to establish a reasonable expectation. Second, while judgments of this kind can never be scientific, and while judgments of what is a 'reasonable' expectation are bound to be somewhat impressionistic, this style of evaluation has the virtue of highlighting the notion of a performance limit. All intelligence communities are going to make mistakes. Third, by forcing us to think about performance over an extended period of time, rather than with respect to a specific judgment, it makes it easier to bear in mind that at the end of the day the crucial judgment we wish to make is how well intelligence serves policy, not how well intelligence performs in purely intellectual exercises such as forecasting. Fourth, by evaluating performance over an extended period of
time, it is possible to discern habits and tendencies to cultivate or to try to eliminate, strengths to exploit in the future, and weaknesses to try to shore up. No clear practical implication follows from the conclusion that the CIA failed to predict the Soviet nuclear deployment. Clear practical implications follow from the conclusion that Cuban intelligence depended too heavily on Soviet and East European technical services, that the KGB relied too heavily on espionage, and that the CIA was excellent at technical monitoring but weaker at political analysis. We learn these things only by taking in the broad view.

Monday-morning quarterbacking and attempting to improve the batting average, then, are misguided approaches to evaluating intelligence performance. We offer instead a third sporting metaphor drawn from the only game harder than life itself - golf. As a metaphor, it has a few obvious weaknesses, but we believe that it also has much to commend it. So as not to try the non-sporting reader's patience unduly, we will simply highlight a few relevant points about the game of golf, leaving readers to tease out for themselves the isomorphisms:

- The best golfers and the worst golfers all play the same courses. There is a sense, then, in which their scores are directly comparable to one another. Moreover, built into the very nature of golf is a performance limit. Excellent golfers will consistently beat par, but no golfer can even in principle score lower than 18 in one round of golf, and certainly no golfer playing a standard par-72 course with regulation equipment could ever hope to score (say) 36. Indeed, scores in the low 60s are rare.

- While all golfers' scores are directly comparable to one another, the overwhelming majority of golfers compete not against others, but against themselves. A 'good' round of golf is one in which one scores lower than usual; a 'poor' round is one in which one scores higher than usual. (Amateur golfers who do compete against each other sometimes factor in their handicaps - constrained averages of their previous scores - so that, no matter what the skill differential may be, they can make a game of it. Notice that even in this case, they are playing against themselves at the same time as they play against their opponents, because the player who scores best relative to his or her own average round wins.)

- A golfer can make what for him or her would be an excellent score despite making some number of poor shots. Indeed, a round without a single poor shot is almost unheard of, even for professionals. The best golfers are those who make relatively few poor shots, and recover from them well.
The game has an important strategic element in addition to its simple mechanics. Players with beautiful swings who consistently choose the wrong clubs or make poor decisions about where to aim – players, for example, who refuse to acknowledge the existence of tall trees and water hazards – may score worse than players whose swings are technically inferior, but who play a smarter game.

Golfers have different styles, and different strengths and weaknesses. Some are risk-averse, and some are risk-acceptant. Some are better off the tee; others are better around the green; still others are better on the green. Some have no difficulty with sand traps and do not take pains to avoid them; others have great difficulty and try to avoid them at all costs.

Golfers rarely play only one course, though typically they become quite familiar with some number of courses. They enjoy the challenge of playing new courses, though they understand that, given two courses of approximately equal difficulty, they cannot expect to score as well on the course that they have never played before.

Golfers are strong believers in post mortems. When they score well, they like to try to identify and explain particularly good shots (often these are recovery shots). When they score poorly, they reflect solemnly upon their poor shots – though they may not choose to do so publicly. Golfers try to cultivate practices that work well, and to eliminate bad habits.

All golfers profoundly appreciate the role of luck.

CONCLUSION: THE 'THEORY' AND PRACTICE OF INTELLIGENCE ASSESSMENT

MENO: How will you look for it, Socrates, when you do not know at all what it is? How will you aim to search for something you do not know at all? If you should meet with it, how will you know that this is the thing that you did not know?"  

In 1978, Richard Betts lamented the lack of a ‘theory’ of intelligence. Betts suggested that ‘[n]egative or descriptive theory – the empirical understanding of how intelligence systems make mistakes – is well developed,’ but that positive theory and normative theory were not. ‘The distinction is significant’, Betts wrote, ‘because there is little evidence that either scholars or practitioners have succeeded in translating such knowledge into reforms that measurably reduce failure. Development of a normative theory of intelligence has been inhibited because the lessons of hindsight do not guarantee improvement in foresight, and hypothetical
solutions to failure only occasionally produce improvement in practice.'

We might quibble with the notion that failure can be reduced 'measurably', but otherwise Betts's point is well-taken, and we see no evidence that scholars have managed to redress the lack of theory that he lamented. Certainly if by 'positive theory' we mean a robust and heuristic set of propositions relating causes and effects - e.g., condition $x$ predicts $y$ with probability $p$ - it is impossible to identify anything in the open literature that would count as a 'theory' of intelligence assessment. And we may justly wonder whether any such thing is possible in principle. The very difficulty of operationalizing the notion of 'success' and 'failure' would seem an insurmountable obstacle. But this is the least of it. Intelligence assessment is not, strictly speaking, a science. It is an art. Intelligence analysts are not in the business of drawing logically-necessary conclusions about behavior on the basis of evidence generally knowable to be sufficient for that task. Leaders of states do not make decisions under constraints sufficiently numerous and sufficiently knowable to make talk of the 'probabilities' of their choosing alternative courses of action objectively meaningful. Such language conveys information not about the target, but about the analyst. When McCone and his subordinates at the Office of National Estimates debated whether Khrushchev was 'likely' or 'unlikely' to deploy MRBM to Cuba, they articulated differences in their own fears and beliefs, and their own judgments of the implications of what American intelligence saw on the high seas and in Cuba. Khrushchev, however, was not literally rolling dice. There was no objectively knowable 'likelihood' that he would do anything in particular. McCone was right for bad reasons, and his analysts were wrong for less-bad reasons, but Khrushchev was Khrushchev - a complicated man with a mind of his own, not some automaton preprogrammed according to a 'Soviet' behavioral algorithm.

Betts is clearly correct that '[n]egative or descriptive theory – the empirical understanding of how intelligence systems make mistakes – is well developed', if by this he means that there are many different bodies of theory upon which to draw in order to try to explain errors. Fischer's essay provides a good example. The style of argument whereby one assesses the 'goodness of fit' of a body of propositions well-established in one domain with the empirical details of a particular case in another does not, however, lead to the kind of cumulation or generalization to which positive science aspires. Nor does the application of any one body of theory to a series of cases of intelligence failures tend to satisfy (e.g.) a Lakatosian criterion of theoretical 'progress'. It is often possible to say that a body of theory can help us make sense of an otherwise puzzling event, because it supplies us with concepts and cause-effect propositions in terms of which to understand it. But the difficulty is that there may be many different bodies of theory that
can do this, or that the body of theory that does the best job of helping us make sense of one puzzle may do a poor job, vis-a-vis some other body of theory, with respect to another.

Without a positive theory of intelligence assessment, it is difficult to know how to go about constructing normative theory. Put another way, there can be no engineering without natural science. This is why it may be unfortunate that ‘official post mortems of intelligence blunders inevitably produce recommendations for reorganization and changes in operating norms’.\(^92\) Not only can we not be certain that we would have avoided one mistake had the organization and process of intelligence been different in the particular case at hand, but our tinkering may well induce other, possibly more serious mistakes in the future.

What then, can we know in general about intelligence assessment, and how can this knowledge help improve the performance of intelligence? Recognizing that there is a performance limit to intelligence, and that intelligence assessment is an art rather than a science, should certainly induce caution against lofty expectations. But there is one general phenomenon characteristic of human judgment and perception the greater appreciation of which can cultivate a useful circumspection in intelligence assessment. Before closing, we will touch upon it and reflect upon how it might bear on practice.

When Meno asked Socrates, ‘How will you look for it ... when you do not know at all what it is?’, he did not make the mere ‘debater’s argument’ for which Socrates chided him.\(^93\) Meno asked a profound question: how can we have knowledge of something without some prior familiarity with it? Once we allow some prior familiarity, we court infinite regress and the disturbing conclusion that knowledge is not possible at all. Socrates’s solution — that all learning is mere recollection, for the immortal soul knows everything\(^94\) — would seem on the face of it to substitute one paradox (the paradox of time) for another (the paradox of knowledge). In any case, the metaphysical presuppositions of Socrates’s answer seem in no less want of justification than the answer itself.

The paradox remains unresolved, though its contours and implications are now clearer, thanks to the efforts of Gadamer, Quine, Chomsky, and others.\(^95\) For purposes of understanding intelligence assessment, the paradox is most enlightening when we throw it into reverse: we do hold certain beliefs (sometimes we like to call this ‘knowledge’), and they do influence how we understand new things. There are many vocabularies to use when describing and exploring the phenomenon and many angles from which to look at it. Fischer describes it in the language of cognitive psychology, but we could just as well do so in the language of structural sociology, semiotics, cultural anthropology, or hermeneutics.\(^96\)
The insight that our own concepts and beliefs affect our interpretation of the behavior of others can incline intelligence analysts to reflect on the perspective they bring to bear on an intelligence problem, and the possibility that, in the absence of very good quality evidence about others’ deliberations and motives, their judgments may say more about themselves than about their targets. This does not necessarily mean that analysts will have an easy time trying to achieve some ‘perspective on perspective’, however. Garthoff writes:

Today, I believe it is clear that there was a failure of estimative empathy, of viewing the situation as the Soviet leaders did as they made their decision to place missiles in Cuba. In particular, even if Soviet considerations of redressing the unfavorable strategic global balance for defensive and offensive purposes was paramount, as I continue to believe it was, it is now evident that deterrence of a US attack on Cuba was also a consideration of major if not equal importance. Yet the failure to see the situation through Soviet (to say nothing of Cuban) eyes blinded the US intelligence community as a whole to that significant factor. American intelligence estimators should have recognized a Soviet incentive to defend Cuba against a plausible American threat well before the Soviet decision to deploy missiles was made.97

Certainly if American decision-makers had appreciated that Khrushchev felt boxed in, they might have considered an attempt to deploy nuclear missiles to Cuba rather more likely. As Garthoff supposes, this might have given the President additional reasons to try to forestall it by means of earlier and stronger warnings.98 But how should we interpret Garthoff’s claim that the CIA ‘should have recognized a Soviet incentive to defend Cuba against a plausible American threat well before the Soviet decision to deploy missiles was made’? As a retrospective lament, we endorse it completely. If Garthoff means to suggest that this was an avoidable mistake, however, we are not so sure. There were many reasons why US intelligence did not and could not easily empathize with Khrushchev. It took the shock of the Cuban missile crisis to demonstrate to American officials that they badly misunderstood him, and even then they did not appreciate exactly how they had misunderstood him.99 It is difficult enough to empathize with an adversary in retrospect; it is asking a great deal of analysts to empathize in prospect.100

The paradox of knowledge must incline us to disagree with Fischer that there is a useful distinction to make between theory-driven and data-driven thinking. There is no such thing as data-driven thinking. Facts never speak for themselves. We can only make sense of the world on the basis of some prior understanding. There is a distinction to make, however, between
people who are tightly wedded to their beliefs, and people who are relatively more open to the possibility that their beliefs and assumptions may be wrong, or who appreciate the potential explanatory power of two or more different sets of beliefs and assumptions.101

Intelligence analysis can never transcend its dependence upon prior assumptions and beliefs. Understanding presupposes them. Yet at the same time, understanding must always, to some extent, be frustrated by them. All intelligence communities would like to minimize the frustration, and to tip the balance between knowledge of self and knowledge of other in the direction of the latter. As a practical matter, though, how do intelligence analysts avoid tripping over their prior assumptions and beliefs without being paralyzed by ambiguity and indecision? How do they steer between the Scylla of overcommitment, and the Charybdis of uncertainty ('an open mind is an empty mind')?102 Scholars and practitioners have been very creative in proposing solutions, but, as Betts effectively demonstrates, the most prominent ones — assuming the worst, multiple advocacy, consolidation, Devil’s advocacy (e.g., ‘Team B’ exercises), sanctions and incentives, and ‘cognitive rehabilitation and methodological consciousness’ — all face important obstacles or have serious defects.103 Betts is certainly correct that intelligence failures are inevitable, and he rather pessimistically concludes that the inherent limits of intelligence assessment suggest that we are unlikely to realize more than marginal benefits from procedural innovation.

Fatalism may be entirely appropriate — if we insist upon thinking of ‘better’ intelligence as an improvement in some rate of success, or as a purely intellectual measure of the accuracy of an intelligence community’s judgments of an adversary. But we may be able to improve the effectiveness of intelligence quite dramatically simply by keying on possible sources of intellectual error. If intelligence failures are inevitable, why not exploit that fact when attempting to serve policy?

Because intelligence assessment is so heavily dependent upon assumptions and specific modes of inference, it stands to reason that if policy makers were more fully aware of them, they would be better able to read, understand, and make use of intelligence. National leaders are often sophisticated analysts in their own right, who operate on the basis of certain assumptions about the world, and who draw their own conclusions on the basis of their own beliefs. There is ample evidence of this on all sides in the Cuban missile crisis, certainly, although of the three leaders Khrushchev appears to have relied upon his own analysis most heavily, and Kennedy least. But all leaders are, to some extent, their own analysts, and are therefore confronted with the task of having to make sense of intelligence assessments which may or may not reinforce their own expectations. Some
political leaders – Castro, for example – have a fascination for intelligence, and like to wade into the assessment process, voraciously seeking details on the source and content of the information their intelligence services acquire. Most, perhaps, are too busy to do this even should they wish to do so, and must be content to receive the intelligence community’s distilled wisdom. But relatively few, we suspect, think hard about their own assumptions, beliefs, and modes of inference, let alone those of their intelligence analysts, and accordingly most have but a dim appreciation of the underlying points of, and reasons for, similarity or disagreement.

A political leader with strong beliefs and a high degree of self-confidence may simply dismiss as flawed an intelligence report dissonant with his or her own judgments. Over time, given enough points of disagreement, this would quite naturally lead to an intelligence-policy rift. If intelligence analysts took pains, however, to flag the assumptions and inference patterns undergirding their estimates, this would not only tend to improve the rigor and consistency of estimates, but it would enable policy makers to see more easily why they agreed or disagreed. In some cases this might even lead to a productive dialogue between policy makers and analysts on crucial assumptions. This was precisely the rationale for an intriguing innovation instituted by the former Chairman of the National Intelligence Council, Joseph S. Nye Jr, who required intelligence estimates to identify and flag the crucial assumptions upon which they were based. The intelligence community initially resisted the requirement, but came to regard it as salutary for helping structure and clarify their own assessments. For policy makers, it provided several points at which to engage an otherwise terse and remote summary document.

As policy makers bear the ultimate burden of responsibility, anything that helps them to better understand both intelligence and the presuppositions of policy represents a potentially important contribution. We can imagine taking Nye’s innovation two steps further: (1) offering incoming national leaders briefings on the nature and limits of intelligence early in their tenure; and (2) wherever possible appending to intelligence estimates analyses of how crucial judgments in it would change if certain key assumptions were modified or relaxed. These practices would help prime policy makers to appreciate the performance limits of intelligence, and to integrate estimates more fully and more easily with their own judgments. At the same time, they would preserve intelligence estimates intact for those policy makers who are only interested in the distilled wisdom of the intelligence community and who have neither the time nor the inclination to peruse appendices.

The main attractiveness of the practice lies in its potential for engaging decision makers in the intelligence-policy process. From a practical point of
view, it makes little sense to conclude, as did Dean Rusk, McGeorge Bundy, and John McCon...
NOTES

1. Throughout we will cite the essays in this volume by author, without title.
2. Fursenko and Naftali, p.79; and Amuchastegui, p.89.
4. Fursenko and Naftali, p.66.
5. ‘Before being sent out to the US to run the KGB’s principal station there, Feklisov had overseen the intelligence assessment in 1960 that Eisenhower was unlikely to invade without a Cuban assault on Guantánamo Bay or the creation of a missile base on the island. Two years later, he still believed this, despite the change in administration and the experience of the Bay of Pigs. In the middle of March 1962, Feklisov reported that Kennedy was unlikely to approve an invasion because a military intervention would undermine the Alliance For Progress.’ Fursenko and Naftali, pp.73–4. Surviving senior members of the Kennedy administration overwhelmingly concur that Kennedy would not have authorized an invasion of Cuba without some serious provocation, such as a deployment of Soviet nuclear missiles to Cuba James G. Blight and David A. Welch, On the Brink: Americans and Soviets Reexamine the Cuban Missile Crisis, 2nd ed. (NY: Noonday 1990); James G. Blight, Bruce J. Allyn, and David A. Welch, Cuba on the Brink: Castro, The Missile Crisis, and the Soviet Collapse (NY: Pantheon 1993) passim.
6. Fursenko and Naftali, p.73.
8. The weight of evidence and testimony available today would strongly suggest that Khrushchev did not seriously begin to consider a nuclear deployment to Cuba until April or May 1962, and that planning for the operation did not begin until shortly thereafter. See, e.g., Gen. Anatoli I. Gribkov and William Y. Smith, Operation Anadyr: U.S. and Soviet Generals Recount the Cuban Missile Crisis (Chicago: Edition Q 1994); Richard Ned Lebow and Janice Gross Stein, We All Lost the Cold War (Princeton UP 1994); Raymond L. Garthoff, Reflections on the Cuban Missile Crisis, rev. ed. (Washington DC: Brookings 1989); Blight and Welch, On the Brink (note 5); Blight, Allyn, and Welch. Cuba on the Brink (note 5). The second reason which apparently led Cuban intelligence to conclude that Khrushchev must have conceived the deployment much earlier was the judgment that the Soviet Union – a notoriously sluggish and bureaucratic state – could not have planned and carried out such an ambitious redeployment in a mere 90 days. It is important to note, however, that Cuban intelligence apparently believed that the deployment was substantially complete by mid-September, and did not appreciate that the first Soviet MRBMs in Cuba only became operational in late October – four and a half months after planning could have begun in earnest following a late-May decision to proceed – and that the IRBMs never arrived. While this would still be a significant accomplishment for a notoriously sluggish and bureaucratic state, it could not be considered downright unbelievably rapid.
9. Fursenko and Naftali, p.84.
10. Blight, Allyn, and Welch, Cuba on the Brink (note 5), pp.77–80. The second task of the delegation was to assess the prospect that strategic nuclear missiles could be deployed in Cuba without the United States discovering them. See p.185, below.
   Fursenko and Naftali are correct to state that Alekseev ‘was [not] consulted on the project’ (p.75) if by this they mean that he was not brought in on the deliberations before Khrushchev had made up his mind to propose the deployment to Castro. However, as Alekseev recounts the tale, once Khrushchev had decided to make the offer, he did ask Alekseev how he thought Castro would react. Alekseev predicted incorrectly that Castro would not agree. Blight, Allyn, and Welch, Cuba on the Brink (note 5) pp.77–8.
11. Timothy Naftali, personal communication.
14. Since Cuban intelligence apparently discovered the true nature of the deployment in due
course, it is conceivable that they simply inferred that Soviet intelligence in Cuba must have been aware of it as well. This might have been an accurate judgment. But even if it were incorrect, it would be natural, because the Cubans—like the Americans—understood the Soviet Union as a coherent rational actor. See our discussion in note 96, below.

15. Fursenko and Naftali, pp.80–82.
16. Ibid. p.82.
17. Fursenko and Naftali, p.70; Amuchastegui, pp.91–2.
20. Personal communications. Naftali adds in another personal communication: ‘It seems odd that Alekseev would have been recalled [to Moscow] in the manner he was if Che was on his way to Cuba. [Also,] I have a clear recollection of the letters prepared by the Presidium for Castro in the first week of May, and not one refers to a fraternal visit by El Che.’

21. Aleksandr Fursenko and Timothy Naftali, *One Hell of a Gamble*: Khrushchev, Castro and Kennedy, 1958–1964 (NY: Norton 1997) p.70. Amuchastegui finds it difficult to imagine that Che would have made such a probe at that time, and recalls that the chief purpose of Che’s visit was to discuss economic cooperation (personal communication).
23. Che’s next documented voyage to the Soviet Union took place in late August 1962. The chief purpose of that trip was to propose making the deployment public. It seems unlikely that Amuchastegui is thinking of this particular trip, as increasing levels of military aid were not on the agenda. Blight and Welch, *On the Brink* (note 5) pp.333–4.

27. See ibid. p.28.
28. Beth Fischer gives good reasons for preferring a cognitive to a motivational explanation in her essay [pp. 28–9].
33. Certainly there is ample evidence that the KGB played at best a minor role throughout. It would appear from the somewhat thinner testimonial and documentary record on Soviet military intelligence that the GRU was marginalized as well. See generally Fursenko and Naftali, *‘One Hell of a Gamble’* (note 21) passim.
34. Fursenko and Naftali, p.75.
35. In his annual message to Congress on 2 Dec. 1823, President James Monroe warned European states against any future attempts to colonize the Americas, declared that the United States would consider any attempt by the nations of Europe to extend their system into the hemisphere ‘dangerous to our peace and safety’, and disavowed American participation in European wars. See Ernest R. May, *The Making of the Monroe Doctrine* (Cambridge, MA: Belknap 1975). While Khrushchev may have believed that American participation in World Wars I and II represented a renunciation of the Monroe Doctrine, Troyanovsky would have known that no one in the United States so considered it.
37. The KGB, despite a lack of highly-placed sources, seems to have had a fairly good understanding of President Kennedy, his attitude toward Cuba, and the constraints under which he operated domestically. Fursenko and Naftali, pp.68–9. The KGB’s reading of the Bay of Pigs—that JFK had been pushed by the CIA and by
the hawks in his administration, and would, if left to his own devices, act less aggressively toward Castro (ibid, p.69) – is extremely interesting, and it would be useful to know exactly why the KGB drew this particular conclusion. Cf. James G. Blight and Peter Kornbluh (eds) Politics of Illusion: The Bay of Pigs Invasion Reexamined (Boulder, CO: Lynne Rienner 1997).

38. Fursenko and Naftali, p.75.
39. Ibid., p.72.
40. Ibid.
41. It is conceivable that Khrushchev chose not to task the KGB with such an assessment in part because he feared it would compromise the secret, as Beth Fischer surmises (p.165). There is no direct evidence to this effect, however.
42. Fursenko and Naftali, p.74.
43. Blight and Welch, On the Brink (note 5) pp.333–4. It is interesting to note that senior members of the Kennedy administration unanimously agree that had Khrushchev attempted to deploy the missiles openly, the United States would have been in a much more difficult position. The secrecy and deception surrounding the deployment enabled the United States to characterize it as sinister and aggressive, and to deflect attention from the undeniable fact that what the Soviet Union and Cuba were doing was entirely legal.
44. Ibid. p.239; Sergei Khrushchev, personal communication.
45. Graham Allison has suggested that organizational routines may explain the failure of the Soviet military to camouflage the missile sites effectively; Essence of Decision, (note 12), p.111. An explanation more consistent with the available evidence is incompetence. See David A. Welch, ‘The Organizational Process and Bureaucratic Politics Paradigms: Retrospect and Prospect’, International Security 17/2 (Fall 1992) pp.112–46.
46. ‘Sharaf Rashidov had reported to the Defense Council that Cuba’s forests would provide just the needed cover for our missiles. Only someone with absolutely no competence in such technical matters could have reached such a conclusion.’ Gribkov and Smith, Operation Anadyr (note 8), p.40.
47. Fursenko and Naftali, p.66.
48. Amuchastegui, p.91.
49. Ibid. p.102.
52. Castro has spoken in general terms about his conviction that the United States, once ready, would pounce, and would seek to do so without warning. Evidently he was determined not to make the mistake Stalin had made in 1941 of failing to prepare to meet an assault that could be launched at any time. See Blight, Allyn and Welch, Cuba on the Brink (note 5) pp.109–10.
55. Richard K. Betts, ‘Analysis, War, and Decision: Why Intelligence Failures are Inevitable’, World Politics 31/1 (Oct. 1978) pp.61–89. Many of the constraints on intelligence assessment that Betts identifies may be understood in terms of the psychological dynamics. Fischer discusses in her essay, although his study predates much of the research upon which Fischer draws. Betts concludes that ‘[intelligence failure is political and psychological more often than organizational’, Ibid. p.61.

Betts also draws our attention to the fact that intelligence communities are often engaged in a zero-sum strategic interaction, whereby success for one logically implies failure for the other. The fact that even in the most celebrated cases surprise is never truly absolute further underscores the sense in which 'the best possible' performance is always something less than 'perfect performance'. Richard K. Betts, ‘Surprise Despite Warning: Why Sudden...

56. See, e.g., Garthoff, p.23: ‘Some ambiguous or equivocal intelligence indications in September-October were noted but did not command as much attention as, in hindsight, they should have. In particular, it was observed that several Soviet merchant ships with large hatches en route to Cuba were riding high in the water, indicating bulky but relatively light cargo, such as missiles. Some of these ships were seen unloading normal cargoes, but others were reported to be unloaded at night, again a possible indicator of a cargo of special sensitivity. After the missiles were discovered, in retrospect it was recognized that these particular ships had undoubtedly brought in the missiles in early to mid-September.’ US intelligence did notice all of this at the time, but it may be doubted whether the CIA had compelling reasons in 1962 to regard it all as more significant than they did.


58. Monday-morning quarterbacking is much easier for football fans than for critics of intelligence, because in football it is possible to play the percentages. We can justifiably criticize a coach for calling a long pass on third-and-inches, because a running play would have a much better chance of success (and a much lower chance of resulting in a turnover). Critics of intelligence do not ordinarily have good-quality base rate information with which to second-guess intelligence professionals.


61. Garthoff notes that some of the authors of the September SNIE subsequently argued that the estimate had been correct and that Khrushchev had been mistaken (p.21). There is a sense in which this is undoubtedly true. One possible rebuttal is that the CIA should have known that Khrushchev would not consult his American specialists, and that they should have factored this into their estimate. This reply, however, begs all of the difficult questions to which we allude under the previous head.

62. Echoing the point of the previous paragraph, it is worth noting that had President Kennedy not chosen to deliberate for a week before choosing his response to the Soviet deployment, he might very well have done as Cuban intelligence anticipated. On 16 Oct., the mood in the White House cabinet room was decidedly belligerent. The delay was crucial for permitting tempers to cool. See generally James G. Blight, *The Shattered Crystal Ball: Fear and Learning in the Cuban Missile Crisis* (Savage, MD:Rowman & Littlefield 1990). Although the timing of the American response was not something Cuban intelligence controlled, it might well have had a significant effect on our retrospective assessment of their performance.


64. According to Amuchastegui, DGI also considered an American attack inevitable (ibid.). Since this was an open-ended prediction, we are technically still not in a position to determine whether that estimate was correct.


66. Ibid. p.11.


68. Ibid., p.23.


70. See pp.181–2 above.

71. Fischer's essay extensively documents the propensity to evaluate information in the light
of pre-existing beliefs. Clearly the large number of false reports of missile sightings by Cuban agents and refugees had led CIA analysts to believe that Cuban sources were unreliable, and that their reports had ulterior purposes.

72. It might be replied here that certain kinds of behavior unambiguously telegraph intentions. For example, we can infer reliably that a leader who masses troops on the border in a full battle-ready posture intends to attack. This behavior, however, is fully consistent with other intentions: e.g., to provoke an attack, to intimidate, to signal acute concern, and so forth. See Michael Handel, *Perception, Deception and Surprise: The Case of the Yom Kippur War* (Jerusalem: Leonard Davis Institute 1976); and Janice Gross Stein, ‘Calculation, Miscalculation, and Conventional Deterrence II: The View from Jerusalem’, in Robert Jervis, Richard Ned Lebow and Janice Gross Stein (eds) *Psychology & Deterrence* (Baltimore, MD: Johns Hopkins UP 1985) pp.60–88.

73. Fischer, p.159.

74. ‘Failures in National Intelligence Estimates’ (note 59), p.462,

75. Ibid. pp.463, 465.

76. The best example is Arnold L. Horelick, ‘The Cuban Missile Crisis: An Analysis of Soviet Calculations and Behavior’, *World Politics* 16/3 (April 1964) pp.363–89. Cf. also Arnold Horelick and Myron Rush, *Strategic Power and Soviet Foreign Policy* (U. of Chicago Press 1966) p.141: ‘Soviet calculations throughout the Cuban missile crisis can be reconstructed with some degree of confidence, because the confrontation with the United States was so open’.


78. Similarly, US analysts and policy makers had insufficient evidence on the basis of which to draw reliable conclusions about what Soviet leaders might be willing to consider by way of retaliation for American military action against Cuba. Garthoff writes: ‘In response to either an air strike or an invasion, the intelligence estimators concluded that because the Soviet leaders “would almost certainly not resort to general war and could not hope to prevail locally, we believe that the Soviets would consider retaliatory actions outside Cuba”, and the most likely place would be Berlin – but “major harassments”, not a military move on West Berlin. As for a countering strike against some US base around the world (such as the missiles in Turkey, although not specified), a Soviet retaliatory move of this kind was considered “possible but in our view unlikely”. Again, this judgment was never tested, but although the estimated Soviet response (to “consider” retaliation by a non-combat harassing action) was much more mild than many (including Secretary McNamara and some other members of the ExComm) believed likely, and was certainly a prudent caution, it appears from all we now know to have been the most the Soviet leaders would have done, if that.’ Ibid. pp.30–1.

79. Ibid. p.21. N.B. that Garthoff argues, on the basis of present knowledge, that this is what the estimate should have concluded,

80. Klaus Knorr therefore seems to us to be splitting hairs unduly finely when he states, ‘The question is: why did the intelligence community fail to warn the government earlier that such a Soviet move was distinctly possible, if not probable, instead of estimating that it was improbable, though not impossible?’ ‘Failures in National Intelligence Estimates’ (note 59), p.456.

81. Garthoff, p.18. We would choose some word other than ‘compel’, in view of the fact that the United States agreed secretly to withdraw its Jupiter missiles from Turkey as a quid pro quo.

82. Ibid. p.24.

83. See, e.g., Mark Kramer, ‘The Cuban Missile Crisis and Nuclear Proliferation’, *Security Studies* 5/1 (Autumn 1995) pp.171–9; and James G. Blight and David A. Welch, ‘On Historical Judgment and Inference: A Reply to Mark Kramer’, ibid. 5/4 (Summer 1996) pp.172–82. We should qualify this last statement by noting that we do not know whether US signals intelligence had collected anything that would have clarified any of these matters.

84. Garthoff, pp.27–9.

85. We appreciate that how one bounds the relevant period may be a choice of some
consequence. Defining ‘an event’ spatio-temporally is, to some extent, arbitrary, and is perhaps usefully understood as a function of socio-cultural propensities. We have remarked before upon the different understandings of the crisis in the United States, the Soviet Union (now Russia), and Cuba, and the significance of the different names by which it is known in each country. We have also argued that culturally-rooted differences in problem-representation help account for its genesis and greatly complicated its resolution. See Blight, Allyn, and Welch, Cuba on the Brink (note 5) pp.318-72. We do not believe that this represents a serious obstacle for the present point, however: as long as it is possible to conceptualize an intelligence encounter as having taken place over some period of time, and as consisting of some number of discrete phases or episodes, it should be possible – within limits – to arrive at some evaluation of performance, even if those conceptualizations are arbitrary, and the episodes in fact linked. Thus by contrasting US intelligence performance with respect to Cuba over the period 1958-61 and 1961-62 – with the Bay of Pigs and the Cuban missile crisis as foci – we can meaningfully conclude that the CIA performed poorly in one period and well in another, and we can learn something of practical consequence from the contrast.


88. Consider, for example, the following: ‘Castro in March had moved to purge key old Moscow-line communist leaders from his regime, and some in the US intelligence community saw this as causing a significant rift in Cuban-Soviet relations. Military shipments from the USSR, after a buildup in 1961, were lower in the first half of 1962. The evidence before July 1962 thus tended to support an estimate that a major Soviet military buildup in Cuba was not likely.’ Garthoff, p.22. US intelligence apparently inferred from the Escalante affair that Soviet-Cuban relations would deteriorate; yet, as Amuchastegui explains, the purpose of Castro’s purge was precisely the reverse: to prepare the ground for a closer security relationship (Amuchastegui, p.92). The purge itself was consistent with diametrically opposite motivations, and US intelligence simply leapt to the conclusion that it implied one in particular. Garthoff considers the US judgment ‘intuitive’, but this, of course, is merely another way of saying that it was more consistent with suppressed assumptions.

89. This is a complicated claim whose full exegesis would take us very far afield. The gist, however, is that leaders of states make decisions on the basis of idiosyncratic beliefs and judgments about the lessons of history and how the world works, and do so consciously in reaction to the historical record, often precisely so as to surprise. Important choices are often unprecedented, or have important unique features, such that the historical record provides no basis for estimating a probability distribution. Among the important works that touch various aspects of this issue are Robert Jervis, ‘The Future of World Politics: Will it Resemble the Past?’ International Security 16/3 (Winter 1991/92) pp.39-73; Yuen Foong Khong, Analogies at War: Korea, Munich, Dien Bien Phu, and the Vietnam Decisions of 1965 (Princeton UP 1992); and Richard E. Neustadt and Ernest R. May, Thinking in Time: The Uses of History for Decision-Makers (NY: Free Press 1986). See also Masato Kimura and David A. Welch, ‘Specifying “Interests”: Japan’s Claim to the Northern Territories and Its Implications for International Relations Theory’, International Studies Quarterly 42/2 (June 1998) pp.213–44.

90. Among political scientists there is a widespread impression that ‘expected utility theory’ can serve as the basis for intelligence assessment. Bruce Bueno de Mesquita claims a successful point-prediction rate of over 90 per cent across a variety of political decision-making domains for a particular expected-utility model based upon Black’s median voter theorem. Bruce Bueno de Mesquita, David Newman and Alvin Rabushka, Forecasting Political Events: The Future of Hong Kong (New Haven, CT: Yale UP 1985); Duncan Black, Voting in Committees and Elections (Cambridge UP 1958). Bueno de Mesquita’s model is not, however, an ‘expected utility’ theory at all, merely a computational device for aggregating subjective judgments of actors’ weighted preferences on a given issue. Its predictive power – whatever that may be (many of the details are classified) – depends
essentially upon the quality of the inputs, which are themselves generated by area specialists in a non-scientific way. For further discussion, see Janice Gross Stein and David A. Welch, "Rational and Psychological Approaches to the Study of International Conflict: Comparative Strengths and Weaknesses", in Alex Mintz and Nehemia Geva (eds) Decision-Making on War and Peace: The Cognitive-Rational Debate (Boulder, CO: Lynne Rienner 1997) pp.51–77.

91. Imre Lakatos, 'Falsification and the Methodology of Scientific Research Programmes', in idem and Alan Musgrave (eds) Criticism and the Growth of Knowledge (Cambridge UP 1970) pp.91–196; idem The Methodology of Scientific Research Programmes, Vol.1 (ibid. 1978). To simplify a bit, Lakatos argued that we should prefer theory \( x \) to theory \( y \) when \( x \) explains everything \( y \) explains, plus additional facts.

93. ‘I know what you want to say, Meno. Do you realize what a debater’s argument you are bringing up, that a man cannot search either for what he knows or for what he does not know? He cannot search for what he knows – since he knows it, there is no need to search – nor for what he does know, for he does not know what to look for.’ Meno, 80e, p.69.
94. Ibid. 81b-d, pp.6–70.
96. For applications of a competing perspective (which the author calls ‘critical constructivism’) on the phenomenon as it bears on problem-representation in the Cuban missile crisis, see Jutta Weldes, Constructing National Interests: The U.S. and the Cuban Missile Crisis (U. of Minnesota Press forthcoming 1998). For a critical cultural perspective as it bears on intelligence assessment, see Robert B. Bathurst, Intelligence and the Mirror: On Creating an Enemy (Oslo: PRIO 1993).

It would certainly appear that what might be thought of as ‘intellectual culture’ has a profound effect on political judgments of all kinds, including intelligence assessments. In the Western world, at least five stock assumptions about how international politics works, and about how leaders make decisions – roughly congruent with the ‘Realist’ understanding of international politics – tend to dominate. Among the most important of these assumptions are (1) that all states always seek ‘power’; (2) that leaders are ‘rational’; (3) that all leaders understand, and respond to, threats and incentives; (4) that the adversary’s behavior is always intentional; and (5) that the adversary’s actions are always coordinated and meaningful. Typically, when leaders and analysts attempt to make sense of an adversary’s behavior, they infer evil intent from actions they dislike, and dismiss as disingenuous and instrumental arguments adversaries make that appeal to notions such as justice or fairness. Often these are important mistakes. These assumptions exert a powerful spin on ambiguous data, and can lead to tragic misjudgments. For extensive discussion, see David A. Welch, Justice and the Genesis of War, Cambridge Studies in International Relations 29 (Cambridge UP 1993); and idem, ‘Remember the Falklands? Missed Lessons of a Misunderstood War’, International Journal 52/3 (Summer 1997) pp.483–507.

97. Garthoff, p.46.
98. As we suggest in our introductory essay, these may not have been effective in the absence of credible offers to alleviate Khrushchev’s anxieties, and it is difficult to imagine what those offers might have been, given the political realities of the day. Moreover, this is not an intelligence failure: Kennedy did not need the impetus of an intelligence assessment to communicate more clearly with Khrushchev. Yet, conceivably, had the CIA raised the alarm earlier, it might have been able to serve policy even better than it did.
99. There were glimmers of understanding: ‘In seeking to understand why Khrushchev and the other Soviet leaders believed they could succeed’, Garthoff writes, ‘the ORR study concluded (in my view correctly) that Khrushchev, “having lived restively under the shadow of U.S. strategic bases for more than a decade”, and now having his first opportunity to do likewise, “may have felt confident that the U.S. would understand the rules as he did – that military bases on the opponent’s periphery are facts of great power
life which fall far short of a provocation to war." Ibid., p.39. But as Garthoff also notes extensively, even after the shock of the missile crisis, retrospective evaluations consistently underestimated Khrushchev's deterrent and defensive motivations.

100. It seems to us undeniable that the self-awareness of the US government – perhaps of the entire country – was so deficient, and so discrepant with Cuban perceptions of the United States, as to be laughable were not the stakes too high for laughter. In all the posturing, no one seems to have asked two questions: (1) Are our actions scaring the Cubans as well as deterring the Soviets? (2) If so, might Cuba's sense of insecurity drive the Soviets to undertake desperate measures on its behalf? From the American perspective, Cuba was simply absent from the Cuban missile crisis.

While it is possible to imagine that some American official or commentator might have posed these questions and answered them affirmatively in 1962, it is difficult to imagine this having had much effect on America's Cuba policy, given the grip that the rational deterrence/Cold War perspective had on the American imagination at the time.

101. See, e.g., Janice Gross Stein, 'Political Learning by Doing: Gorbachev as Uncommitted Thinker and Motivated Learner', International Organization 48/2 (Spring 1994) pp.155–83. Notwithstanding their judgment that the United States would eventually try to destroy the Cuban Revolution, Cuban intelligence concluded that an American attack was not imminent after the Bay of Pigs because (a) the pattern of activity conducted under the rubric of Operation 'Mongoose' seemed ill-designed to prepare the ground for an American invasion; and (b) Cuban intelligence believed that the United States would not invade Cuba without adequate diplomatic 'cover' and the sanction of other Latin American states. Amuchastegui, pp.96–7. Clearly DGI drew heavily upon assumptions about American behavior when attempting to predict American behavior. We disagree with Fischer that '[d]ata-driven thinking could have led [Khrushchev] to the (correct) conclusion that the Kennedy administration was not planning an attack in 1962.' Fischer, p.156. Since American behavior was consistent with different interpretations, data-driven thinking, even if it were possible, would have led to no particular conclusion at all. Instead, we would suggest that Khrushchev would have reached the same conclusion as Cuban intelligence if he had relied upon a different set of assumptions with which to interpret ambiguous American behavior.

Fischer also writes: 'It is also interesting to note that while Cuban and American intelligence reached the same (inaccurate) conclusion about Soviet motives, they appear to have done so via different methods of information processing. American analysis appears to have been heavily theory driven. Beginning with the assumption that the Soviet Union was a cautious, opportunistic aggressor unlikely to risk a nuclear deployment to Cuba, American analysts concluded that the purpose of the deployment was to give the Soviets a strategic advantage over the United States once the deployment became evident. Cuban intelligence, on the other hand, inferred Soviet motives from the available data – specifically, the discrepancy between the dire Soviet assessments of the American threat and Cuban assessments that such a threat was receding. This finding suggests that there is no necessary relationship between one method of information processing and the accuracy of inferences. Data-driven and theory-driven information processing can both lead to inaccurate assessments.' Ibid. pp.163–4. We understand Cuban judgments here as no less theory-driven than American judgments.

According to Amuchastegui's account, Cubans had been somewhat suspicious of Soviet intentions all along, and therefore, among the available set of working assumptions about Soviet behavior, was the hypothesis that Soviet policy with respect to Cuba was driven by narrow self-interest rather than altruism. Cuban intelligence was never wedded to the altruism assumption and had no difficulty assimilating the available evidence to an alternative assumption. Note also that Soviet dissimulation did not logically imply that the Soviet motive in deploying nuclear missiles to Cuba was to gain a strategic advantage over the United States. Many possible purposes could be served thereby, as Americans recognized – including the defense of Cuba (Khrushchev may simply have believed that the Cubans would never take what was necessary for their defense unless he exaggerated the American threat). See., e.g, Allison, Essence of Decision (note 12) pp.43–56.
102. Betts, ‘Analysis, War, and Decision’ (note 55) p.84.
103. Ibid. pp.73–84.
104. We are not clear on the chief reason for the resistance, though it is possible to imagine that estimators feared it would be onerous, distracting, or superfluous.

105. One of the striking features both of US intelligence assessment and White House decisionmaking during the Cuban missile crisis was the partial success achieved in relaxing certain common assumptions and articulating the implications – specifically, the assumptions that the adversary is a ‘unitary’ and ‘rational’ actor. Garthoff writes that ‘Two special national intelligence estimates issued on 19 October (SNIE 11-18-62) and 20 October (SNIE-11-19-62) ... [recognized] “the possibility that the Soviets, under pressure to respond, would again miscalculate and respond in a way which, through a series of actions and reactions, could escalate into general war”,’ and that ‘in any use of military force by the United States, the Soviet leaders would be “alarmed and agitated” (SNIE 11-18-62) or at least “surprised and probably alarmed” (SNIE-11-19-62).’ Garthoff, p.30. US policy makers, similarly, ‘prudently allowed for the possibility that even if Moscow instructions prohibited initiative of Soviet forces in Cuba to fire the nuclear missiles there, in case of an American air strike or invasion any surviving Soviet missiles might be fired either with or without authorization from Moscow. Both the general assumption of tight Moscow control, and the recognition that it nonetheless might fail, were properly taken into account.’ Ibid. p.54. In our view, US policy makers were encouraged to relax these assumptions about Soviet behavior in part because of their own experience during the crisis – and particularly at its climax – of American command-and-control problems. See generally Blight and Welch, *On the Brink* (note 5), passim. Contrast Argentine leaders’ rigid (and illogical) ascriptions of unity and rationality to British actions prior to and during the Falklands/Malvinas war of 1982 (Welch, ‘Remember the Falklands?’ note 96); Alberto A. De Vita, *Malvinas 82: Como y Por Que* (Buenos Aires: Instituto de Publicaciones Navales 1994).


107. Note also that it would have helped minimize the dangers of post-hoc rationalization characteristic of several prominent retrospective studies that attempted to make sense of Khrushchev’s calculations. See the discussion on pp.196–7, above.

108. Amuchastegui, for example, writes: ‘Not only did Cuban leaders privilege Soviet over Cuban assessments, but Cuban intelligence was instructed to treat the inevitable confrontation with the United States as its guiding strategic assumption shaping every aspect of data collection and interpretation’ (p.98), and that ‘in June 1962, Piñeiro had given strict instructions not to reflect upon the topic of Soviet forces and missiles in Cuba in any Cuban assessments’ (p.100).