TO THE EDITOR:

In his review essay, "Being Intelligent About Secret Intelligence Agencies," Harry Howe Ransom wrote:

Until disproved, the following hypotheses, partly intuitive, appear to represent reality: intelligence systems tend to report what they think the political leadership wants to hear. . . (p. 147).

The view that intelligence caters to the preconceptions of the political leadership seems widespread in both academic and journalistic circles. Intelligence practitioners know it to be untrue.

One factor does, unavoidably, exert pressure toward conformity on the individual intelligence analyst. That is the need to defend and argue cogently for whatever position one takes. If an analyst takes a position that is controversial or counter to the conventional wisdom, that analyst will have to do more. He or she will have to do more research, defend the position in greater detail, and the process of review, approval, and coordination will be longer and slower. That is obviously the way it should be; analysts should not be able to rip off an unconventional analysis and send it to the White House without the most careful review. But when analysts are working under extreme time pressure, as most analysts are, it means they have to make a decision whether or not they have the time to do what is required to defend a novel or minority view.

Obviously, this does exert some pressure toward conformity, but several points are important in interpreting the significance of this fact. This is not necessarily conformity to the views of the political leadership. It is, first of all, pressure toward agreement with analysts in competing centers of analysis, e.g., for the CIA analyst, the tacit or active support of colleagues in the State Department and Defense Intelligence Agency who might otherwise seek to undermine one's analysis.

Secondly, it is conformity with the preconceptions of one's own most immediate supervisors. Pressures to reflect one's supervisor's views or to support departmental interests differ in different agencies. They are perhaps strongest in the Defense Department and almost certainly weakest in CIA, where a long tradition of analytical independence supports the individual analyst's freedom to express unconventional or unpopular views as long as they are well documented. Analysis biased in support of long-standing departmental interests is not the same as analysis that tells the political leadership what it wants to hear; students of bureaucratic politics are well aware that departmental interests frequently lead to intelligence assessments that diverge from what the political leadership wants to be told.

Analysts are subject to many countervailing pressures. The need for self-esteem and pride in one's work is, in most cases, at least as strong as pressures for conformity. In 28 years' service with CIA, I found that analysts generally did not hesitate to advance a novel or minority view even though it might make a few waves or cause them more work. Disagreement is, in fact, endemic to the intelligence community, and is far more prevalent than conformity. Most analysts enjoy challenging the superficial or conventional wisdom of the policy makers or other analysts, and they look for opportunities to do so as this is how they can be most useful. Analysts gain the greatest job satisfaction from presenting and defending their own views under precisely those circumstances when, according to Ransom's hypothesis, they should be catering to the preconceptions of the political leadership.

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Reply

TO THE EDITOR:

Most generalized observations of political behavior fail to encompass the complexity of reality. The generalization to which Richards J. Heuer objects comes from my diligent reading of non-secret sources over the past quarter-century. From this evidence and from conversations with intelligence professionals, I intuitively suggest the hypothesis that intelligence agencies, even if unconsciously, tend to report what they think decision makers want to hear. Heuer's experience is different from mine and he reaches a contrary conclusion. He has had the advantage, and disadvantage, of the inside view of a secret agency. I respect his observations.

Yet how does one explain the monumental intelligence failures beginning with Pearl Harbor? How does one interpret the shock in Washington at the first Soviet atomic bomb explosion in 1949; the dual surprises in the Korean War—the initial
attack and later Chinese intervention; the Bay of Pigs failure; the Cuban missile surprise; the colossal failures in Vietnam, early and late; the frequent failures in forecasting Soviet strategic military power; and the more recent intelligence failures regarding Iran and Afghanistan? Surely an information pathology is at work. Admittedly, the concept of "intelligence failure" presumes a theory of intelligence that remains underdeveloped.

Failure can occur at any number of points in a complex series of steps, from the setting of requirements to the use of intelligence by decision makers. But in the examples cited, the decision makers in each instance were misinformed and in most cases were pleased to hear the intelligence estimates given them.

The inference I draw from these and other examples is that tendencies exist in bureaucratic structures that lead to misinformation or worse. I agree with Mr. Heuer that such tendencies may be less pronounced in the CIA than in department intelligence reports. Certainly such an objectivity was an important reason for creating the CIA in 1947. The practice in recent times of appointing CIA directors anew, with each new incoming presidency, however, invites a dangerous politicization of central intelligence, exacerbating the problem of objective reporting.

The record clearly indicates that major intelligence estimates have been tragically wrong about many major events over the past 30 years. Doubtless numerous variables are involved in these failures. Pressures on, and tendencies of, intelligence professionals to bring good news to political leaders appear to have been important causes of such failures.

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My contrary claim was, I grant, worse than a fault; it was a mistake and one which I shall now put right. Replace

\[
\frac{d^2x}{dt^2} = -a \quad \text{with} \quad \frac{d^2x}{dt^2} = k \frac{dy}{dt} - a \frac{dx}{dt}
\]

from which

\[
\frac{dx}{dt} = ky - ax + g
\]

can be derived using, pace Mr. Schrodt, freshman calculus. Even Schrodt's target-hitting arrow, however, falls short of a bull's-eye since the whole point of the discussion of that section (pp. 108-09) was to argue that political scientists who think of "explanation" in international politics as conforming to a highly idealized hypothetico-deductive model misconceive both the nature of explanation—which lies outside positivist science—and international studies—which lies outside positivist conceptions of it. Given the validity of the corrected notation and Schrodt's silence about my general point, I shall assume that my argument holds good. Indeed, the notion that a "theory" of international politics ought to conform to some single criterion of rationality is, on my view, a self-protective piece of petty legislation which precludes the possibility of counterexamples by stipulating that there are none. This point is not new but in light of the undeserved popularity of the H-D Model among some political scientists, it bears repeating.

As to Schrodt's second point concerning my having attributed to Richardson a deterministic Weltanschauung, the arrow is crooked. I never said that Richardson was a determinist. What I said was that he had committed himself, whether he realized it or not, to a version of scientific determinism, and this still seems right to me. From the fact that Richardson "sold his physics books" to learn about statistical proof from Karl Pearson it does not follow that he was not committed to a version of scientific determinism. As determinists have long argued, mere indeterminacy or randomness goes no way toward providing the possibility of free will. To have free will requires that an agent could have acted otherwise.

Let me briefly reconstruct my argument at pp. 112-13 of the article in a more cryptic form. Suppose, as the quotation from Richardson suggests, there are only two alternatives for nations—namely, either they follow fixed traditions mechanically or they stop and think. Then we have to choose between the second conjunct whose truth-conditions can never, so far as we can ever tell, be decided upon (leaving to one side the far-fetched), and the first conjunct. This is no choice at all except in a rhetorical way and that leaves the first conjunct. But what does it mean to say that na-