(1948b), 197–202.
———. *Arms and Insecurity: A Mathematical Study of the Causes and Origins of War.* Pittsburgh, Pa.: Boxwood Press, 1960a.
———. *Statistics of Deadly Quarrels.* Pittsburgh, Pa.: Boxwood Press, 1960b.
ROPER, ELMO. "The Fortune Consumer Outlook," *Fortune*, 42 (1950), 57–60.
SUCHMAN, E. A., R. K. GOLDSEN, and R. M. WILLIAMS, JR. "Attitude toward Korean War," *Public Opinion Quarterly*, 17 (1953), 171–84.
TOPPING, SEYMOUR. "Russians Uneasy on Cuba Dispute," *New York Times*, Sept. 12, 1962.

Comments

ED. NOTE: *Because the research reported in the preceding article is clearly tentative, its authors approved of its publication along with the following critical comments by two readers of the manuscript.*

First Comment

ANATOL RAPOPORT
Mental Health Research Institute,
The University of Michigan

The difficulties evident in the paper by Campbell and Cain stem from the corresponding weakness in Richardson's work on war moods, namely, the bulkiness of the mathematical theory, which is unusable for lack of data to put more than a very small and rather inconsequential portion of the model to a test.

Richardson's model of war moods dynamics is a system of six nonlinear differential equations. The dynamics of such systems are enormously involved. An examination of the phase space alone will reveal all kinds of singularities (in case the system is unstable, which it probably is), and so the behavior of the system may depend crucially on minute fluctuations of the parameters around some threshold value.

None of this is treated by Richardson. Instead, he trivializes the problem, reducing it to simple logistic equations. The authors of the present paper do no more. It is, of course, not my purpose to criticize either Richardson or the authors for what they have not set out to do. My point is merely that the psychological theory proposed by Richardson nowhere enters the working out of the mathematical model. Richardson himself only *wrote down* the equations governing the war moods, using the overt-covert states as the variables. When it comes to drawing even rough theoretical conclusions from the model, Richardson forgot about the overt-covert state theory and considered only a simple logistic epidemic model from which the heart and soul of the original psychological theory had disappeared. The authors repeat this mistake. They estimate the parameters pertinent to Richardson's drastically simplified (logistic) model, and one is left to wonder what the relevance of the overt-covert theory is.

For this reason even the crudest conclusions either for or against the contagion theory are unwarranted. From the data marshalled in the paper it is clear that war moods fluctuate with events. This does not support the contagion theory, nor does it refute it. The events themselves may be influenced by the prevailing political climate. For example, is it unthinkable that Truman's decision to commit troops in Korea was a reflection of the mood prevalent in 1949 and 1950, while Eisenhower's decision not to commit troops in Vietnam was a reflection of the mood prevalent in 1954?

The authors note the relation between events and war moods, but not throughout. They speak of war-weariness growing in the fall of 1944 and the winter of 1945. However, in the fall the Allies' drive across France petered out and in December there was a serious setback—the Battle of the Bulge. War-weariness disappeared in the spring. It is, of course, well known that war moods change drastically with victories and defeats. This makes the assessment of war-weariness extremely difficult, and it also means that no clean way exists for testing the self-propelled dynamics of the war mood theory.

As long as the theoretical discussion of war moods is kept on the level of qualitative speculation, numerical data are not of much help. They can be interpreted in many ways and so can be cited to support even contradictory assumptions. This is especially so when one brings in "covert" moods, which are unobservable in principle. A theory of war moods would be pushed forward if we could somehow isolate a sequence of events about which data pertinent to the formulated theory could be obtained. I still have no idea how this can be done.

Second Comment

PHILIP E. CONVERSE
Survey Research Center,
The University of Michigan

The paper by Campbell and Cain raises a number of important issues surrounding the general relation between public opinion and governmental decisions as to the initiation and termination of wars. Stripped to its essentials, the underlying model formalized by Richardson supposes that some level of war-readiness, usually latent, exists in a population; that under certain circumstances this level becomes manifest, rising rapidly by interpersonal "contagion" through a population; and that a sufficient increase in war fever can make some causal contribution to the entrance of the nation into war. Using scattered data from public opinion polls, Campbell and Cain attempt to relate parameters that they estimate for a central Richardson equation to an earlier estimate he had made. One or two fair "fits" are found, along with several misfits. The issues underscored by the paper have to do with Richardson's conception of the problem, the empirical referents which are presumed, and the nature of the data that may be brought to bear in any effort at confirmation.

Taking the relatively technical problems first, one can commiserate with the authors for the inadequacy of the data available for even rough parameter estimation within the terms of the primary Richardson equation used. Part of the problem, as the authors note, has to do with the dearth of identically-worded questions, particularly in the earlier period. The estimation of the Richardson constant might vary widely according to whether the "war fever level" was pegged at 5 percent or 20 percent in the initial reading; yet it is not difficult for the survey researcher to move a response distribution this "distance" by changes in