

Maurice A. East:

Commentary on "Småstaten - et brugbart komparativt begreb?"

Niels Amstrup has contributed significantly to the discussions concerning the renaissance of the term "small state" in the literature of international politics and foreign policy. In particular, his article in Cooperation and Conflict, Vol. 11 (1976) made me aware of a wide variety of hitherto unknown (to me) literature. In the present article, he makes some comments and arguments regarding several of my articles in particular and the CREON Project specifically. I would like to respond to some of these points.

First, it is clear that Amstrup and I do not share the same views about ways in which one can further the study of foreign policy. I clearly feel that attempts to generalize about classes of phenomena (e.g. small states) and to seek patterns of foreign policy behavior are valid and desirable ways to proceed (however, not the only way!). The question then becomes how valid and useful are the results from such attempts.

Let me now, with these opening remarks, turn directly to some points made by Amstrup. He begins by asserting that previous scholars have concluded that they could not decide on what smallness meant and therefore, the concept should be dropped. The fact that a group at the Institute of Commonwealth Studies was unaware of the differences in levels of measurement, and thus concludes "it is difficult to pick out ... where smallness begins or ends", does not end the question whatsoever. Just one observation here: I consider size to be a continuous variable, and not a dichotomy between large and small. My earliest empirical piece in fact carried out its analysis with a dichotomous categorization of size. But it is entirely possible to avoid the question of where smallness begins and ends by using a continuous variable. And in our recent research (forthcoming in numerous CREON articles) I do re-examine some of my research using size as an interval level measure. Therefore, it does not seem to me that one can argue convincingly that it will never be possible to define or measure the concept of size or smallness in an acceptable way. Perhaps I

have not done it here, but the effort should not be abandoned for the sort of reasons offered here.

A second major claim made by Amstrup at the outset is that the foreign policy behavior of small states is so varied that size cannot possibly be the most critical variable. My response here is that variation in foreign policy behavior is determined by the dimensions of behavior that you choose to examine. It is not at all clear that the foreign policy behavior patterns of small states as a group vary greatly in terms of all dimensions of foreign policy. There may be differences in the amount of war between Israel and some other small states. But there may well be similarities in terms of overall hostility or friendship patterns, participation in IGOs, activity in economic vs. military-security areas, etc. So it cannot be said that the foreign policy behavior of small states varies greatly on all dimensions of foreign policy behavior.

Let us turn now to Amstrup's comments on the CREON Project and my own writings about size and foreign policy. First, I must say that my own further analysis of the CREON data has indicated that at least one part of my earlier analysis is in need of revision. This has to do with the relationship between size and conflict-cooperative behavior. I hope to have these analyses published in the near future. On the other hand, the reasons that my earlier work is in need of revision do not seem to be the ones Amstrup discusses.

To begin with, it is a bit misleading to present my thoughts on the effects of national attributes (see figure 1 in his article) as a single factor analysis explaining foreign policy behavior. First, size is just one of the factors comprising a much broader category of factors called national attributes. It is my attempt to conceptualize the variety of factors making up national attributes and consolidate them into a theoretically useful concept (capacity to act) that is most important in that article. Size is only a part of it. Second, the entire book in which that piece appears¹ is an attempt to sketch out the theoretical basis for a multifactor explanation for foreign policy behavior. Our strategy

1) M. East, S. Salmore, and C. Hermann (eds.), Why Nations Act, Beverly Hills: Sage, 1978.

in those chapters was to see how far we could go in articulating the causal connections between a single cluster of variables (e.g. national attributes, personal leader characteristics, etc.) and aspects of foreign policy behavior. It was not to put forth a monocausal "theory" based on size alone, or even on national attributes.

Amstrup then argues that I do not take into consideration the ideas of my mentor from Princeton, Harold Sprout and his wife Margaret, in my discussion of modernization (which is a major aspect of what I refer to as social organization). The Sprouts clearly and convincingly argue that increased modernization increases the vulnerability of nations. But they do not deny anywhere that I am aware of my major assumption - that other things being equal a nation with a higher degree of social organization has a greater capacity to act in foreign affairs. The costs of pursuing certain courses of action may increase, but they have the capacity to act if they so desire. Furthermore, the quotation used from the Sprouts argues that military expenditures have not commanded as high a priority in Britain and some other lands as social expenditures. They specifically do not argue that resources available to act internationally in other ways - IGO participation, diplomatic activity, international trade, international conferences, etc. - have decreased. In fact, such a finding would be very strange in light of all that has been written about increasing interdependence and the internationalization of politics.

A short comment must also be directed toward Amstrup's argument that my work is only a continuation of the "power politics" approach which has seemingly been put to rest long ago. I am one of those who feels strongly that earlier attempts to deal with the concept of power in international politics, although extremely important for the development of the field of international politics, were essentially misguided. However, this does not mean that subsequent scholars should avoid using the concept of power (or something similar) in future analysis. To condemn Morgenthau certainly should not condemn all others who try to deal with power in international politics in other ways.

Another short comment, regarding comparing Israel's capacity to act to that of Sweden. Amstrup's article leads one to believe

that both Israel and Sweden are included in the CREON set (Sweden is not included). But his point is that Israel's rather high level of international activity (relative to some other small developed states) means that it must have a higher capacity to act than Sweden, for example. That is not the correct conclusion to draw. Capacity to act means just that - it does not necessarily mean that the nation does in fact use that capacity, nor does it mean that the nation is faced with the opportunity to act. And this certainly admits the need for additional factors to give a more complete explanation of foreign policy behavior - something that is never denied by me or the CREON Project group in general.

Another question is raised by Amstrup when he claims that my work is not theoretically generated but is rather the result of empirical analysis. While I will never deny the value of the interaction between theory and data analysis in the scientific enterprise, nevertheless I submit that my own writings demonstrate a considerable degree of theoretical elaboration of the causal mechanisms underlying my findings. The theoretical elaboration that was eventually published in Why Nations Act was the result of a combination of empirical analysis and good old fashioned arm-chair "theorizing". But the reader must ultimately decide this point.

The final point I want to make before turning to the general question of the validity of the CREON data has to do with Bjøl's argument that small states have less need for monitoring international events than large states because their interests are regional and more limited in general. I can fully accept this argument as far as it goes. But what is of particular concern to me is whether small states are able to determine what information is most relevant to their concerns if they do not have a good overview of international activities. Taking the example used by Amstrup, he claims that Finland is probably better informed than the USA on those aspects of Soviet foreign policy that concern Finland. Without for a moment wanting to defend the USA's perspective on the USSR, I would say that it is at least as plausible to argue that Finland's perspective may be so narrowly focused on their own concerns vis a vis the Soviets that they fail to see where their relations with the Soviets fit into a broader global view of

changing Soviet strategy. It is not at all obvious to me that the Finns have a better grasp of this than do the Americans or the West Germans, for example.

Validity of Events Data and the CREON Project

Let me say that the question of the validity of events data is one that has been much discussed in the literature. I would refer you to the volume by Don Munton² that is perhaps the most comprehensive to date. It is not doing too much injustice to this literature to assert that the conclusions are mixed. Events data seem better suited to answering some sorts of research questions better than others. But this is true with all data - no data set is either "Good" or "Bad" in the abstract. We can only talk about the appropriateness of the data for answering this or that research question. And this is certainly true of the CREON data set also. It is clearly not designed to allow one to do an in-depth study of the foreign policy behavior of a single state. It is designed to be used to explore patterns of foreign policy behavior. The degree to which we can generalize from these findings to the "real world", of course, is the big question. But in a very real sense, is this not exactly the same problem faced regardless of what sort of data you have - whenever you want to generalize, the problem arise and must be confronted.

Amstrup begins by pointing to a table that he says can be considered characteristic for my thinking in an entire article.³ I would not agree that there is a single "characteristic table" for the entire article. Rather the article is focused on differences between large and small states - and it is of more marginal interest what the differences between developed and less developed nations are. But Amstrup interprets this table (along with another table in the original) correctly when he says that differences between large and small states are greater than those between developed and less developed.

But he goes on from this table to make what is in fact the most

2) Don Munton (ed.), Measuring International Behavior: Public Sources, Events, and Validity, Halifax, Nova Scotia: Dalhousie University Press.

3) Maurice A. East, "Size and Foreign Policy Behavior. A Test of two Models", World Politics, vol. 25, 1972/73, pp. 556-576.

frequently heard argument about the source bias in events - data that the source will over-report foreign policy behavior for small states "... når de har en mere kategorisk eller dramatisk karakter". The table below addresses this question directly. In the CREON data set we have a variable that categorizes events according to what sort of situation generated the event. We use Hermann's 8 categories from his "crisis cube", ranging from crises to routine behavior. When dividing up the percentage of events between types of states, we get the following results:

Type of Situation By Groupings of Nations (Per cent)

	Norway	USA	USSR	Large Powers	Middle Powers	Small Developed	Small Developing
Crisis	8	15	10	6	8	8	10
Routine	65	46	49	72	62	65	67
N	107	704	327	1140	1293	513	1009

CREON coding from Deadline Data does not indicate that this sort of source bias is present in the data set. Crisis events are, not over-reported for smaller states in CREON. Which leads us to a major point regarding the differences between using newspapers and using a compilation like Deadline Data. The contents of a single newspaper is dependent upon judgements of the "newsworthiness" of events. Thus one can end up with Denmark having only 5 events in the WEIS analysis, based on The New York Times as Amstrup points out. But in the compilations, the coders are instructed to regularly and systematically seek out events for countries. They are not just guided by what appears in one newspaper. Deadline Data includes information from a number of newspapers from all over the world. It is certainly not as complete in its coverage of the Middle East as Cahiers de l'Orient (Rest In Peace!). But our working assumption is that it gives us a reasonable approximation of the patterns of events that actually did occur if we had all information available (which is never the case for any research project I would claim). Amstrup goes on to attack the face validity of the data by arguing that it is implausible that the USSR has scarcely one half the activity of the USA. He argues that it is

hardly the case that the USA's capacity to act was twice that of the Soviet Union during the period of the CREON data (1959-1968). I would argue this is not so implausible at all if you consider for example, number of embassies in each land (117 in the USA vs. 80 in the USSR in 1965), the number of memberships in IGOs, amount of international trade, etc. And on a factor analysis to determine size scores for nations, for 1965 the factor loadings give a rough order of magnitude here (USA = 7.16, USSR = 3.65). Again, my claim is that I do not find these figures implausible whatsoever, especially in light of the way CREON has set out to conceptualize and operationalize these concepts.

An excellent point made by Amstrup is that one should carry out the analysis of small developed states after pulling out Israel, for example. This can easily be done, and because of his arguments I shall undertake this in the near future. The question is what can one say afterwards? That Israel is a "deviant case"? That it does not belong to the group of small developed states? How do we use this information to build on? An entire theoretical framework should not be abandoned because of a single deviant case. At least this is certainly not the view of science that I adhere to.

To close, let me say that I feel that Amstrup's discussions of WEIS and Burrows' article on the Middle East and its coverage in various sources only demonstrates some of the reasons why we chose Deadline Data as the source for our project. Obviously, we would have liked to have been able to use multiple sources (particularly a major regional newspaper from each geographic region to supplement Deadline Data), but financial resources were lacking for a projekt of such scope. But we did try to avoid the major problems of using newspapers only, with their very obvious criterion of "newsworthiness". And at the same time, we were not trying to get the most complete coverage for a given region. Rather we wanted a single source that would give an acceptable level of coverage of the patterns of foreign policy behavior across all regions. It is certainly an open question whether we have attained this goal. But it does not seem that some of the arguments made by Amstrup on source bias are entirely relevant for the CREON Project.

It seems to soon for the international relations community to give up entirely on the concept of size and its relation to foreign

policy. We may need to refine it and to work with it further.
And it is exchanges like this that allows this process to proceed.