Rejoinder

by H. W. Houweling and J. G. Siccama

The main objection of Smies and Riemersma to our analysis and conclusion can be summarized in the following statement:

> ' "While specific (. . .) assumptions imply a unique statistical distribution, the converse is not true; a variety of different circumstances can imply the same distribution". Therefore, contrary to what Houweling and Siccama's paper purports, war proneness analysis does not effectively restrict the scope of research into war causation'.

The statement that specific assumptions imply a unique distribution, but that the converse is not true, is undisputable. This is the reason why we stated on p. 81 of the article that 'although a good fit (. . .) is a necessary condition for the assumptions of the model to apply in fact, it is not a sufficient condition'. Consequently, we came to the conclusion that

> 'The Negative Binomial Distribution will necessarily arise if the nations form subgroups which are internally homogeneous in their propensity to fight, but differ amongst themselves in the degree of war proneness. It is well known, however, that the converse is no longer true in fact. Other assumptions have been developed which also result in a Negative Binominal Distribution. Even if these assumptions look somewhat implausible, the fact that they have been found underlines the hazards of rushing from facts to theory'. (p. 81).

Consequently, we did not 'fail' to observe the warning to this effect by Pielou. On the contrary. We referred to the specialized literature for the case at hand.

This state of affairs is not unique to the application of probability models to facts of nature. We live in a multiple hypotheses world, and we did not intend to overcome this fact. Some of these hypotheses, however, have to be rejected because they fit the data so badly. It is a distinct advantage of probability theory to have set up explicit decision rules for the rejection of hypotheses. This seems to be the case with regard to the Poisson Law of distribution. *This (negative) finding implies a redirection of the analysis of the causes of war*

towards national and (or) dyadic attributes, away from the international system as a whole.

Of course we do not maintain that further research in this direction is limited to data fitting with statistical distributions, as Smies and Riemersma seem to think. Straightforward testing of hypotheses announces itself as the most promising way to go. There are no decision rules for the acceptance of hypotheses as 'proven'. To accept a hypothesis as 'proven' would imply the claim that alternative generating mechanisms can never be found in the future. Such a claim cannot be warranted empirically. In any way, we did not fall in this inductivist trap.

We are utterly disappointed by the statement of the authors in note 10 of their contribution:

> 'We observe that Houweling and Siccama (. . .) in their derivation of the nbd use the exact wording by Pielou, *although no note to this effect is given*' (emphasis by Houweling and Siccama).

This effort towards delegitimization also breaks down completely. On p. 76, note 21, of the article, we refer to Pielou, p. 81 ff., for both Poisson and its compound variety. Instead of using Pielou's notation, should we have used a different one, or should we have invented a new way to derive these distributions in order to use them legitimately?