COMMUNICATIONS

To the Editor:

Professor Kaplan's amusing conclusion of his review of my World Politics: The Global System (this Review, June, 1968, p. 694) moves me to exonerate the Dorsey Press from his charge "that they could not afford proper critical help to the author . . ." They preferred more critical help—all of it relevant and from highly reputable scholars—than I was willing to accept.

Mr. Kaplan and I could argue about who misunderstands, and how grossly, the literature on strategic doctrine. Some highly placed practicing strategists have assured me that my understanding and interpretation of it in the book are sound. In any case, the absurdum to which some strategists have reduced their doctrines in the device of the doomsday machine was not presented as a preferred strategy in my book. As for Mr. Kaplan's magistral assertion that "it is not true that nuclear weapons cannot be used to acquire some future good," would it not serve the purposes of the Review—which is one of our profession's "present means to obtain some apparent future good"—if he were to elaborate, corroborate, and engage in a little dialectic?

HERBERT J. SPIRO
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(Editor's Note: The Managing Editor regrets that inadequate copy-editing and proof-reading allowed this unwarranted statement to appear in the Review, and apologizes to Professor Spiro and the Dorsey Press for any embarrassment it may have caused them.)

To the Editor:

In his article, "On the Neo-Elitist Critique of Community Power," this Review (1968), pp. 451-60, Richard Merelman is severely critical of our analysis of the nature and process of nondecision-making. Within the limited space permitted us here, we can do no more than point out his major errors.

In Merelman's view, we make the following interrelated points: (a) "non-elites are encased in values foisted on them by the elite"; (b) "non-elites are not even conscious of having major differences with the elite"; therefore (c) conflict in the community is limited to "unimportant matters which do not threaten the elite." (p. 452.) Given this perception of things, says Merelman, we must be "neo-elitists." Worse, our formulation is riddled with logical and empirical problems.

The chief objection to his critique is that it is predicated on a careful misreading of our discussion of nondecision-making. As we see it, the process is at work when the mobilization of bias, created or sustained by its beneficiaries (who may be either elites or non-elites), is sufficiently dominant—not necessarily or even usually omnipotent and all-encompassing, as Merelman contends—to prevent a political issue from reaching the decision-making arena. Obviously, if a particular set of political beliefs was universally embraced in a given community, no one could tell whether the consensus was "false" or genuine. Merelman labors this point; but in concentrating on the limiting case, he ignores all those other, more common situations in which consensus on values is lacking, yet persons and groups shaping and supporting currently dominant beliefs are able to utilize these beliefs as a means to silence the opposition or render it ineffectual.

We deny the contention of our pluralist critics that so-called non-events are beyond the reach of empirical investigation. It is possible to determine, on the one hand, the nature and degree of covert support for critics of the dominant set of values and, on the other, the extent to which those who stand to lose from public airing of an issue use power or its correlates to sustain the beliefs that will keep the issue from being seriously considered. It seems well established, for example, that Senator Joe McCarthy and his supporters engaged in nondecision-making in the early 1950's, when their exploitation of anti-communist sentiments effectively blocked access to the decision-making arena of demands for social reform. Even more interesting is the evidence we have found in Baltimore, where the value of equal opportunity for blacks has lately gained wide acceptance—only to be used by prominent whites to deny Negroes a greater share in the actual making of community decisions. It is immaterial for present purposes whether white leaders consciously adopted a pro-civil-rights posture in order to isolate the advocates of "black power." What is significant is that they exercised authority and influence on the nondecision-making level and did so without creating a community-wide consensus. Need we add that had we relied exclusively on analysis of "concrete" decisions, in the fashion of Merelman and his
fellow pluralists, we would have missed this
critical aspect of contemporary politics in
Baltimore?
From what we have said here and elsewhere,
it should be clear that we are not "neo-elitists."
We neither presuppose that every community
is run by an elite nor that an elite consensus
must exist in order for nondecision-making to
be operative and effective. We simply reiterate
that nondecision-making may be and often is a
critical element in the political process and, as
such, deserves further theoretical analysis and
empirical study. We plan shortly to produce
another paper along these lines.

Peter Bachrach
Temple University

Morton S. Baratz
Bryn Mawr College

To THE EDITOR:

Mercifully, the Bachrach-Baratz letter per-
mits me brevity:

1. Parts III and IV of my article, which deal
with cases of nondecision-making involving
power and force, are distinguished from Part II
by the fact that while the latter presupposes
false consensus the former two do not. There-
fore, as Bachrach and Baratz can no doubt see,
the power and force sections deal with the
class of situations they accuse me of ignoring.
In fact, I intended Parts III and IV to absolve
me of any suspicion that I was only treating a
"limiting case."

2. Non-events are, by definition, non-em-
pirical. You cannot observe what has not hap-
pened. Therefore, interpretations made
about nondecisions are subject to the difficult
and peculiar problems of inference I discussed
in my article. The fact that you can find evi-
dence supporting your version of why some-
thing did not happen in Baltimore, say, does
not circumvent the inferential problems I dis-
cuss. The situation is analogous to a jury
forced to convict on circumstantial evidence.
It is always nice to have a corpus delicti around.
We must, otherwise, make leaps of faith that
are usually too great for most political scien-
tists, as scientists and not as citizens, to at-
tempt.

3. Bachrach and Baratz deny that they are
neo-elitists, or that they believe that every
community is run by an elite, or that elite
consensus is necessary for nondecision-making
to operate effectively. Of course, only Part II
of my article makes the latter two assump-
tions are not reasonable given other
literature on representation; and (3) that we
state one of our assumptions as a conclusion.
The hypothesis that different models are
appropriate for competitive and non-com-
petitive constituencies is an attractive one and
worth investigating properly. If the hypothesis
were supported by appropriate testing,
would be an important and welcome addition
to the literature on representation. Unfortu-
nately, Forbes and Tufte utilize an unsound
and misleading methodology in their test.
The magnitude of correlation coefficients
depends in part upon the variance which is
measured in a variable. When we divide a body
of data into subgroups, say into competitive
and non-competitive districts, we may differ-
entially affect the variance in variables and,
therefore, the correlation coefficients. Since the
values of those correlation coefficients depend
in part on how we divided the data, they are
descriptive of the divided data only, but we
should not be used as comparisons with those from the
originally undivided set or from the set divided
in other ways. It is not surprising, then, that
these writers find a model other than ours in
one of their subgroups since their analysis
utilized correlation coefficients. (Note that our
analysis also uses correlation coefficients and
therefore describes our data set only, but we
do not attempt comparisons with other sets or
subsets.) We suggest that if Forbes and Tufte
are serious in the testing of their hypothesis,
they should use unstandardized regression
coefficients in the future. The procedure they
utilize was specifically warned against and
reliance on unstandardized coefficients was
suggested nearly two years ago in this REVIEW
(March, 1967) by Blalock. Ironically, Blalock
used the Miller-Stokes data as his case study
in warning against reliance on correlation
coefficients for this type of model analysis.

We should also note, however, that for
Forbes and Tufte to utilize unstandardized
regression coefficients for subgroup analysis,