

CHALLENGING THE PLURALISTIC INTOLERANCE THESIS

A Reply

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In their article in this issue, Sullivan et al. address the important question of the implications of comparative research for theories of political tolerance. Specifically, the article is concerned with the discrepancy between attitudinal intolerance and regime tolerance in the United States, and attempts to test the thesis of "pluralistic intolerance" proposed by Sullivan et al. (1979) and reiterated by Sullivan et al. (1982). That thesis argues that the United States retains a relatively tolerant regime in spite of high levels of mass intolerance because the targets of intolerance are many and varied. The U.S. public, according to this view, does not focus enough on any one group to enable intolerant actions. Sullivan et al. seek to test this thesis by looking at comparative data from Israel, New Zealand, and the United States.

In our article, "Toward an Empirical Theory of Tolerance: Radical Groups in Israel and Costa Rica" (Caspi and Seligson, 1983), we test the same thesis and take the same basic approach, examining data from Israel and Costa Rica. Our findings have led us to reject the thesis of "pluralistic intolerance" because we have found that in both nations the targets of intolerance were highly concentrated yet the norm of regime tolerance was strongly upheld. If tolerant regimes can survive even when

mass intolerance is focused on a single group or on a set of closely related groups, then the survival of regime tolerance in the United States cannot be explained by the dispersion of targets in the U.S. political system.

Sullivan et al. dispute our refutation of the thesis of pluralistic intolerance by arguing that we "claim to show that in both nations the masses are relatively tolerant but their targets are heavily concentrated, and thus the pluralistic intolerance thesis does not fit." They then go on to state, "We will reexamine the level of tolerance in Israel, and then examine the pluralistic intolerance thesis by comparing three countries—Israel, New Zealand, and the United States."

What we find odd about their critique of our article is that their findings strongly suggest that the pluralistic intolerance thesis is indeed refuted by the Israeli case: "Israel has four strikes against it—intolerance is high, target groups are agreed upon, intense feelings are more on the side of intolerance than tolerance, and most Israelis are more intense than most Americans or New Zealanders on all issues." In Israel, Sullivan et al. find intolerant majorities (in one case as high as 87%) on five out of six of the items they used to measure tolerance, and find only a slim tolerant majority (53%) on the final item. We read these findings as consistent with our own; using a somewhat different sample design and different questionnaire wording, we nonetheless found that intolerance was highly concentrated, focusing on groups that have expressed support for the PLO (Caspi and Seligson, 1983: Table 1, 391). We also found that on every item a majority of our respondents expressed intolerant attitudes toward their least-liked group (Table 2, 393). Finally, we found that over one-third of the Israeli responses were concentrated at the extremely intolerant end of the continuum. Hence our findings and those of Sullivan et al. are not at odds, given that both provide strong disconfirmation of the "pluralistic intolerance thesis." We concluded our article by disputing that thesis. Sullivan et al., however, challenge our contention, even though they present evidence consistent with our own; they do not resolve the question of the validity of the pluralistic intolerance thesis in their discussion of these findings. It appears to us that the only conclusion that could be drawn, given the consistent evidence in the Israeli case, is that the validity of the thesis of pluralistic intolerance needs to be questioned seriously. Rather than do so, however, Sullivan et al. allow their critique of our article to stand, and instead state that the pluralistic intolerance thesis "was presented as a *partial* explanation of why such obviously intolerant attitudes were not automatically translated into intolerant actions" (emphasis added).

They now suggest, based on the comparative data, that there is a fourfold typology of tolerance: New Zealand is a case of "pluralistic tolerance"; the United States an illustration of "pluralistic intolerance"; and Israel an example of "focused intolerance." No data are presented for a case of "focused tolerance," the fourth category of the typology, although we find this a rather odd category, with questionable empirical or theoretical utility. We find this new typology and the emphasis on the "partial" nature of the explanation we sought to challenge a significant retreat from Sullivan et al.'s earlier position, which was strongly dependent upon the explanatory power of the pluralistic intolerance thesis. Indeed, if we understand the conclusion to their present article, Sullivan and his colleagues are suggesting that intolerant attitudes are an intervening variable that becomes important only when activated by "catalytic events" or "leadership cues."

The failure to allow these Israeli data to challenge the pluralistic intolerance thesis is not unique to this article alone, and that is why we are particularly concerned. In a study based on the same data set and authored by John Sullivan, the principal architect of the pluralistic intolerance thesis, and Michal Shamir, it is found that Israeli intolerance is concentrated and intense. When Sullivan is confronted with his own evidence, however, no reference whatsoever is made to his theory. Instead, we are told the following: "This focused, intense intolerance has quite different political implications from the more diffuse intolerance found in the United States, *but pursuing these implications would take us far afield from our main purposes here*" (Shamir and Sullivan, 1983: 918, emphasis added). Shamir and Sullivan then refer the reader to an unpublished paper from Tel-Aviv University for further reference. If that is the paper we have seen, which appears to be an earlier version of the published version, the reader ambitious enough to attempt to obtain a copy of it will not find any enlightenment on this crucial point.

There is a larger issue that grows out of this discussion, one related to a lesson that comparative politics has repeatedly attempted to teach its practitioners, but apparently with limited success. We all ought to be aware of the hazards of cross-national comparisons of marginal data, but there remains a great temptation to make such comparisons nonetheless. When we began to discuss the results of our Israeli study with academic audiences, we repeatedly found our listeners saying, "Yes, the correlates are interesting, but in light of the historical circumstances, continued terrorism, and regional conflict, Israelis must be very intolerant." We then had to enter into a discussion of the marginal percentages, an exercise that often made us feel that we were

replaying discussions of the *Civic Culture*. When we found, for example, that 48% of Israelis in our sample would allow members of their least-liked group to hold a public rally, whereas in the United States, as Sullivan et al. (1979: 789) reported, 34% of Americans were similarly tolerant, we were led to conclude, as we did in the article being critiqued, that levels of tolerance were "surprisingly high." These findings were "surprising" to our audiences. We concluded by calling the Israeli responses "comparatively" (not absolutely) tolerant.

Sullivan et al., on the other hand, find 36% of Israelis giving a tolerant response to this item in their survey. Since our sample size would lead us to a sampling error of about 4.5%, and the Sullivan et al. sample probably produces an error of something over 3%, perhaps as few as 4 or 5 percentage points separate the confidence intervals of the results. Even that difference might be an artifact of the sample designs, which, although very similar insofar as they both used the identical sample frame and survey research organization to conduct the fieldwork, differed in that our sample did not include rural areas, a point Sullivan et al. make (see their note 2). Finally, of course, public opinion is noted for its volatility, and our two surveys are separated by approximately two years, an especially long interval within the context of Middle Eastern politics.

It should be rather obvious from these calculations that categorizing a nation as "tolerant" or "intolerant" on the basis of small percentage differences is not a very productive enterprise. Hence we agree with the Sullivan et al. statement that "whether one characterizes a country as tolerant or as intolerant on the basis of such results is difficult and to some extent arbitrary" (note 2). The far more important task is to subject interesting theory to comparative examination. As Verba et al. (1978: 41) warn us at the beginning of their exemplary seven-nation comparative study of political participation:

Comparisons of the proportions reporting that they contacted an official or of the proportions expressing one or another political attitude are most susceptible to the challenge that they are invalid. The marginal responses to a question are sensitive to question wording, question order, and the way in which the interview is administered. This creates problems of comparability cross-nationally, since nuances of question wording, the structure of the questionnaire, the relationship of the interviewer to the respondent, and so forth, will all differ systematically across national boundaries. The comparability of marginals depends upon the equivalence of the stimulus; and such equivalence is hard to achieve in cross-cultural research.

In sum, we remain convinced that the Israeli data undermine the validity of the pluralistic intolerance thesis. We agree with Shamir and Sullivan (1983: 913) that the Israeli data enable the use of a "most-different-systems" design (Przeworski and Teune, 1970). Those data have shown that a regime can uphold the norm of political tolerance even when many of its citizens express focused intolerance. We are still left, therefore, with the puzzle of linking the attitudes of the public with the nature of the political system in which they live. This is the puzzle that has perplexed empirical democratic theory for decades, and is the very puzzle that Sullivan and his several colleagues set out to solve. In light of the evidence presented by us and by Sullivan et al., we think it is time to go back to the drawing boards on this important question.

In our view, after considering much of the recent research on tolerance, far too direct a link is assumed to exist between citizen attitudes and system characteristics. In their article, Sullivan et al. state this presumed linkage in a rather extreme form: "It should be clear that, inasmuch as mass support for repression is a *prerequisite of massive intolerance* in a democratic society, these three nations do in fact differ in the potential for such actions" (emphasis added). On the contrary, we think that this issue is far from clear. Moreover, the entire issue is further beclouded by Sullivan et al.'s suggestion that certain attitudes create a "potential" for intolerant actions. Views such as this take us back to the days when we looked at every German as a "potential Nazi" and every member of the working class as a potential supporter of authoritarian causes.

In a recent paper Booth and Seligson (1984) have attempted to show that to account for the authoritarian nature of the political system in Mexico by the attitudes of its citizens is a dubious proposition, despite scores of published attempts to do so. That paper suggests some factors that may help provide an explanation. The repeated attempts by theorists of tolerance to bridge the micro-macro gap have thus far not borne fruit, the theory of pluralistic intolerance notwithstanding. It is time for a fresh approach. Let us continue the debate over the pluralistic intolerance thesis in a tolerant mood.

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