significant because Keeler apparently agrees that corporatism requires the presence of institutionalized group-state contacts. Institutionalization, it seems to me, must at a minimum mean that the relationship would survive changes in government or group leadership. I contend that the easy and near-complete dissolution of the corporatist features of the relationship between the agriculture community and the new Socialist government proves the French government’s mastery of such contacts. When it chooses to work with the farmers or other groups because it shares their viewpoint or wants their electoral support, it involves those groups in the policymaking process; when it does not so choose, it can and does exclude them. Thus corporatist bodies are not the signs of an entirely new system of interest group-government interaction. These and other presumably corporatist forms of interest group-government interaction are simply some of a panoply of avenues of influence that French groups may resort to within what remains a complex pattern of interest groups politics.

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Reply to Calvert and Wilson (Vol. 78, June 1984, pp. 496-497)

We are responding to Calvert and Wilson’s (1984) critique of our article (van de Kragt, Orbell, & Dawes, 1983) which first came to our attention when it appeared in the Review.

Calvert and Wilson take issue, first, with our use of the term “public good” to describe the bonus we offered our subjects. We don’t wish a terminological quibble, but our bonus was characterized by both joint supply (one person getting $10 did not prevent another from getting it) and by impossibility of exclusion. Of course, it was a “step level” good with some specified number of contributions being needed before provision, not an n-prisoners’ dilemma such as the one offered in Calvert and Wilson’s tangential table. But the logic of the minimal contributing set we offered depended on this step level characteristic. We attempted to be quite explicit that our model was not intended as a solution to linear dilemmas. If Calvert and Wilson wish to define a public good by the existence of a dominant strategy to defect, then that is their choice. But, we believe, their preferences hardly detract from the interest attached to the problem of providing a step level good that is jointly supplied and non-exclusionary.

Second, they argue that the minimal contributing set is not a “solution” in the sense “of being sufficient, merely by its existence, to drive participants to provide public goods optimally.” Of course not. As Calvert and Wilson correctly saw, there are two equilibria in this game, one the Pareto-efficient “all contribute” and another the deficient one of “nobody contribute.” We were at some pains to point out that a belief among set members that other set members would not contribute (for whatever reason) would make withholding contribution to their personal benefit. Contributing is not a dominant strategy; there are two equilibria; hence, neither is a necessary “solution.” (In contrast to the Pareto-optimal equilibrium, the inferior one is the maximin solution—agreement or no agreement.) But the empirical result is that our subjects choose the Pareto-optimal one.

Calvert and Wilson also raise an “important epistemological issue.” They point out that much work has been undertaken by economists and political scientists using the utility-maximizing model of participants and, it seems, they dislike our intrusion of “extra features” into explanations of behavior. Our model, that members of an MCS will contribute because their contributions will be necessary (i.e., critical) for their own enjoyment of the public good, is surely within the utility-maximizing tradition. The point is that free riding is impossible. What is not impossible is wasting one’s contribution (if for whatever reason someone else does not contribute). The only “extra feature” beyond the standard utility maximization principle introduced by us is the hypothesis that this possibility of loss has no effect on behavior. That was our interpretation of the finding that the minimal contributing set mechanism (which might better be labelled the designated minimal set mechanism) works. That’s standard utility analysis. When outcomes that have nonzero probability have no effect on behavior (and surely the probability that someone doesn’t contribute is nonzero), their utility is considered trivial. Our finding demonstrated that within our context the possibility of free riding influences behavior, but the possibility of loss doesn’t. The findings of these studies alone may be interpreted by assigning trivial utility to the possibility of loss.

In claiming that the MCS is not sufficient “merely by its existence” to ensure optimal provision, Calvert and Wilson make two elementary misreadings of our article. First, they argue that “the structure of the experiment . . . facilitates the enforcement (their italics) of such an agreement.” Most decidedly it did not. Subjects made their decisions in strict anonymity, knowing they were anonymous; they were paid individually in a separate room from the room in which the experiment was carried on; prior subjects were well clear
A further condition offered individual members of the MCS the bonus (regardless of what they personally did), if the other four contributed, with the result that not contributing became a dominant strategy. With the MCS again specified by a lottery, we observed a mean of 40.0% contributing—which does not reach the 96% contributing when discussion was permitted (five groups in each case)—but which compares favorably to the 48.8% when no contributing set was designated (van de Kragt et al., in press). Evidently eliminating the opportunity to free ride does contribute substantially toward group success even when the contributing set is not designated via discussion, but group success is short of the 100% observed in the experiments when it was specified via discussion. What role is discussion playing, therefore?

Second, Calvert and Wilson argue, communication is crucial to optimal provision. Because communicating groups were never compared to noncommunicating ones, that inference cannot be drawn from the article. It turns out that Calvert and Wilson made a good guess. We have run 11 groups (nine players each) in which no communication was permitted and in which a minimal contributing set of five players was designated via a random drawing. The mean incidence of contributing was 83.6%, which is not 100% but compares favorably to the 48.8% when no contributing set was designated (van de Kragt et al., in press). Evidently eliminating the opportunity to free ride does contribute substantially toward group success even when the contributing set is not designated via discussion, but group success is short of the 100% observed in the experiments when it was specified via discussion. What role is discussion playing, therefore?

We interpret our findings so far as strong evidence that decision rules harnessing self-interest to group welfare improves things greatly. But we also interpret them as saying that "extra features" are necessary for a full explanation of what happens. We know, for example, that an enforcement procedure is not a necessary "extra feature," contrary to what Calvert and Wilson assert.

Any disinclination to think about "extra features" seems to us more evidence of a religious disposition than of a scientific one.

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References


Comment on Carson and Oppenheimer
(Vol. 78, March 1984, pp. 163-178)

I doubt that many of us who use the ADA or similar voting index to measure the personal ideology of congressmen are entirely happy with those measures, but the discipline is clearly better off if researchers continue using that type of measure rather than the one proposed by Carson and Oppenheimer in the March Review.

Carson and Oppenheimer point to two types of problems in using the voting-based indices of ideology: 1. overestimating the impact of ideology on voting—because voting-based indices reflect not only personal ideology, but also other variables, such as party, and 2. confusion of public and personal ideology—because the representative, in order to get reelected, may hide his true feelings.

Neither of these "problems" justifies a shift to the suggested measure. The first, overestimation, should never be a problem using voting-based indices. The second, confusion of public and personal ideology, cannot be resolved by using the Carson and Oppenheimer measure, and a shift to that measure will create linguistic confusion in the discipline.

Overestimation of the impact of ideology is never a problem if the researcher uses control techniques that allow other variables to account for as much variance as possible before correlating the voting index with the voting behavior.