

Rejoinder to Gillett

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A number of misleading statements and errors in Gillett's paper are identified, and the points at issue are briefly discussed. A computational error in Colman's earlier paper is acknowledged.

(1) Readers who lack the time, energy and detailed knowledge required to master a specialized body of literature can easily be misled by a strongly worded 'knock-down' critique. Even I was temporarily bemused by some of Gillett's arguments until I examined them with a magnifying glass. Then I discovered a number of misleading statements and errors in the arguments presented in his paper.

His first paper criticized me for proposing a 'flawed and misleading' index of plurality-majority disagreement, namely the weak Borda effect. I replied that I had proposed it not as an index of plurality-majority disagreement, but rather as an index of the propensity of the plurality voting procedure to select a unique winner when 'a majority of a committee or an electorate . . . prefer one of the defeated alternatives to the plurality winner'. I presented formalizations of a criterion of fairness and reasonableness (previously discussed only informally) that is violated by the weak Borda effect, and of the weak Borda effect itself, and I appended a method of calculating probabilities. I offered no 'new justification for studying the weak Borda effect'.

(2) In his latest paper, Gillett no longer claims that I committed the error that he devoted his first paper to accusing me of. But he now claims that I was none the less guilty of 'confusion of the weak Borda effect and plurality-majority disagreement' (p. 81). To justify this revised criticism, he refers not to the article that was the target of his first criticism, nor to my reply, but to two *earlier* writings of mine on the weak Borda effect in which I cited articles by Fishburn and Gehrlein on plurality-majority disagreement. He comments that 'at no point in these articles is the weak Borda effect discussed' (p. 81). Readers will no doubt assume that these famous authorities, at least, agree with him that the weak Borda effect is not closely related to plurality-majority disagreement. But hang on a minute: the articles by Fishburn and Gehrlein were published in 1974, 1976, and 1977. What Gillett omits to mention is that the weak Borda effect was first identified and named by me in 1978! His revised criticism is therefore misleading and also unfair.

(3) Gillett notes that in those earlier writings I treated the weak Borda effect as closely related to plurality-majority disagreement, but adds: 'However, as Gillett (1984) has demonstrated, such an approach is unsound' (p. 81). Readers will assume that he did, indeed, demonstrate this in his previous paper. But what he actually demonstrated was that the two *are* closely related—that the weak Borda effect occurs if and only if plurality-majority disagreement or a cyclic majority occurs. Since cyclic majorities are very rare, this clearly vindicates my judgement that the two phenomena are very closely related. (The probability of a cyclic majority is 0.075 in the equiprobable culture with $n=7$ voters and $m=3$ alternatives that I dealt with; it is much rarer in naturally occurring committees and elections.)

(4) A large portion of Gillett's latest paper is devoted to showing that 'the criterion underlying the weak Borda effect is not weak dominance'. I defined the criterion as follows:

If $x \in F(X, D)$ and $y \in X - F(X, D)$ then not yMx .

I agree that I should not have called it weak dominance, since Schwartz's consequent clause is different, although Richelson, Fishburn, Nurmi, and others all interpret Schwartz's consequent clause the way I did. But this is really a quibble about names because, as Gillett admits on p. 84, my criterion and weak dominance amount to exactly the same thing in the plurality procedure. My consequent clause 'not yMx ' has the same effect as Schwartz's ' $\text{not } F(\{x, y\}, D) = y$ ' because there are only two elements in the option set and F is the plurality procedure in the case under discussion.

(5) Gillett claims that the criterion defined above is inconsistent with my definition of the weak Borda effect because the criterion encompasses cases in which there is a plurality winners' tie, which the weak Borda effect does not. There is no inconsistency: the *general criterion* is violated by the *particular phenomenon* of the weak Borda effect (which must have a unique plurality winner). The assertion that 'the two definitions are inconsistent as they stand' (p. 82) is very misleading: it is explicit in my paper that they are definitions of two different things. Presumably no one would assert that the definition of primality is inconsistent with the definition of the number 3.

(3) The misunderstanding arises, perhaps, from a conceptual confusion in Gillett's paper between a criterion and a social choice function. In Section 2 of Gillett's paper, the Condorcet criterion is wrongly cited as an example of a social choice function, and is labelled F_m . It is not a social choice function, however, because it does not assign a non-empty subset of the option set X to every point (X, D) in the domain. Specifically, when a cycle exists in the configuration of voters' preferences, the value of F_m is undefined. There is an elementary error here, because as Gillett defines it, F_m is not even a function. If the implication is that $F_m = \phi$ when a cyclic majority exists (though Gillett does not say this) then F_m is a function but not a social choice function. By definition, a social choice function does not include the empty set in its range.

(7) Gillett's long discussion of the unreasonableness of my criterion is mostly irrelevant because it focuses on general properties of external stability (weak dominance) rather than the criterion that I defined. For example, his demonstration on p. 84 that the 'quintessentially undemocratic' dictatorship function and the perverse function pass the external stability (weak dominance) criterion is beside the point: both of these functions fail the criterion I actually defined. Gillett quotes Richelson's judgement that the external stability criterion is of moderate, but not major importance. He does not mention that in the same publication Richelson judges Schwartz's GOCHA function, which passes my criterion (it selects the entire top cycle whenever there is a cyclic majority) as among the 'most attractive' procedures according to the numerous criteria he looked at, in spite of its relative indecisiveness.

(8) Of course, my criterion requires a voting procedure to be indecisive when there is a cyclic majority. It is my judgement that if, for example, A is preferred to B , B to C , and C to A , by a simple majority in each case, then the only outcome that gives a fair reflection of the group's opinion is a three-way tie. Gillett seems to think that the cardinal virtue of a voting procedure is that 'the outcome should be as

unequivocal as possible'. I happen to believe that fairness, reasonableness, and rationality are more important than decisiveness. I thought I had adequately exposed the absurdity of insisting on decisiveness in my previous paper.

(9) Gillett has found a computational error in my illustrative calculation. I am pleased that it has been pointed out, and that a correction is being published. The *method* of calculating the probability of plurality–majority disagreement, by subtracting the probability of a cyclic majority from the probability of a weak Borda effect, is not at fault. Gillett considers it unnecessarily circuitous, but it was intended mainly to clarify the distinction between plurality–majority disagreement and the weak Borda effect, which Gillett had accused me of ignoring. In fact the method could be useful in cases where the probabilities of a cyclic majority and the weak Borda effect are known and an evaluation of the probability of plurality–majority disagreement (without plurality ties) is required.

(10) I must comment lastly on the following seemingly devastating criticism: 'when n is even, Colman's approach leads to incorrect results' (p. 85) because of ties. How could a reader who did not have my previous article at hand guess that I did not, in fact, suggest an approach that gives the wrong answers? I stated very clearly that my method of calculation (with no correction for ties) applies only to $n=7$ voters and $m=3$ alternatives; there is absolutely no ambiguity about this in my paper.

Received 18 February 1986

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