

## Elections and reputations

### A comment on the papers of Coughlin and Ferejohn

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While we would all probably agree with the intuition that a political candidate's reputation and that candidate's positions on the issues are both important factors in any election, it is evident that we have a long way to go before theory captures intuition. Indeed Coughlin and Ferejohn have modeled "reputations" in diametrically opposite ways. Coughlin treats what he calls "redistributational reputation" as a prospective phenomenon. Voters listen to candidate positions and then predict the distributional implications. These predictions of the voters are then called reputations by Coughlin, although I suspect that most of us might have chosen another word. Ferejohn, on the other hand, treats reputations as purely retrospective phenomena. Voters completely ignore anything the candidate promises and base their voting decisions entirely on the candidate's performance in office. While there are merits in both theoretical models, neither completely captures what I think of as reputation. A more detailed examination of each paper may explain why.

#### COUGHLIN

In many respects, this paper consists mainly of new words to old

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music. Coughlin takes the specific issue space of income redistribution and then applies some existing election theory. We rediscover that if candidates are certain about voters' preferences over multidimensional issues, then no equilibrium exists (Theorem 1), but if voters vote probabilistically, then an equilibrium does exist (Theorem 2). Although this paper does provide us with new justifications for the probabilistic voting model which he developed with Nitzan (1981), based on the work of Luce (the independence axiom of choice) and of McFadden (the logit model based on random utility), virtually all of the results in this paper follow as a straightforward application of that early work. Among other things, this means that it is the candidates' beliefs about the voters and not the voters' beliefs about the candidates which are crucial.

To derive the current results from the earlier ones, remember Coughlin's original theorem:

Given the platforms  $x$  for candidate A, and  $y$  for candidate B, if the probability that  $i$  votes for A is  $p(x,y,i)$ , (the probability  $i$  votes for B is  $1 - p(x,y,i)$ ), and if

$$p(x,y,i)/[1 - p(x,y,i)] = [u(x,i)/u(y,i)], \quad (1)$$

for all  $x,y$ , where  $u(x,i)$  is  $i$ 's utility for  $x$ , then if candidates maximize the probability of winning (or their expected plurality) there is an equilibrium platform, call it  $z$ , and

$$z = \operatorname{argmax}_x \sum_{i=1}^n \ln u(x,i)g(i), \text{ subject to } x \text{ being feasible}, \quad (2)$$

where  $g(x,i)$  is the proportion of the population with  $i$ 's preferences. In Coughlin's paper for this volume, let  $u(x,i) = (x_i)^{\beta_i}$ , and then solve the implied maximization problem, subject to the constraints in his equation (19), to obtain the results in Theorems 3 to 9.

From a self-interested point of view, I cannot ignore the fact that those theorems also follow, given appropriate utility functions, from the election model in Ledyard (1984)--where the model included rational voter behavior with abstentions and rational candidates who maximize the probability of winning. There, the equilibrium was also characterized

as the solution to a maximization problem, albeit a different one:

$$z = \operatorname{argmax} \int u(x,i)g(i) di, \text{ subject to } x \text{ being feasible.} \quad (3)$$

To get Coughlin's results, let  $u(x,i) = \beta_i \ln(x_i)$ . Since both models yield identical results, for suitable utility functions, one can really only choose between them on empirical grounds. This can be done only if one can first estimate individuals' (von Neumann-Morgenstern) utility for income,  $u(x_i,i)$ , and then separately estimate equilibrium distributions of income as a function of the distribution of tastes,  $g(i)$ . Without such empirical evidence, I see no reason for choosing any particular class of utility functions and thus no reason for ruling out *a priori* any particular equilibrium distribution of income.

In conclusion, I find this paper to be a useful description of how one might apply some existing theories to elections over income distributions. There are, however, no new insights into the role of candidate reputations.

#### FEREJOHN

In his paper, Ferejohn really does try to come to grips with the role of a candidate's reputation in an election. He has generated a nice example incorporating some of the relevant considerations, but he has left us with a lot of research to do. Since I recognize the difficulty of the problem, and understand that this is the first tentative step in a new research direction for political science, the following remarks are intended to provide suggestions for further work.

Although he neatly sidesteps the issue by assuming that all candidates are alike and that candidates "have little incentive to do what they promise," it seems to me that no little part of a candidate's reputation is based on what voters think that candidate will do when faced with various decisions. If a candidate is, for example, an embodied platform or a preference over platforms, then the electorate's problem is the accurate identification of that platform or preference,

as well as the accurate evaluation of performance. (There is an adverse-selection and a moral-hazard problem.) If this is true, then the role of the challengers is significantly altered from the Ferejohn assumption, in which they are simply available. Of course this also complicates the structure of the model, but until this aspect of reputation is included, we will remain a long way from reality.

A concern that I have, which I have been able neither to confirm nor reject, is that Ferejohn has not considered all the possible strategies for the voter in his model. (See, e.g., Proposition 1.) This is important if the space of strategies is truly bigger, and the optimal strategy in that bigger space is not one that is now considered. An example of a different strategy would be one based on more of the details of history. To be specific, suppose that the voter chose constants  $\sigma_\tau$  and  $K$  for each period  $t$ , and then decided to vote against the incumbent if

$$\sum_{\tau=t_0}^t u_\tau \sigma_{\tau t} < K_t. \quad (4)$$

Note that the decision at  $t$  depends (as a weighted average) on the observed values of  $u$  back to the first period when the incumbent took office,  $t_0$ . Ferejohn  $K$  rules are a special case of these. A rule of the form in (4) could be more forgiving in the later periods of an incumbent's tenure if early performance were "above average." (It could also be stationary and subgame perfect.) This is clearly important if the incumbent cannot control all of the exogenous events, and might, under a single-period rule as used by Ferejohn, be voted out due to no fault of the incumbent. This could also explain an "incumbency advantage." (In the Ferejohn world, if  $\theta$  is 0, then, no matter what effort the incumbent is willing to put out, the incumbent will be replaced. Of course, this is a probability zero event, but the point remains valid for small  $\theta$ .) I have been unable to show that there are such rules which are "better" than the Ferejohn  $K$  rules and which are also subgame perfect. It may be that the ability of the incumbent to pick an effort after observing  $\theta$  causes the  $K$  rule to be optimal.

The last observation illustrates that timing is critical in these

models. If the incumbent must pick an effort,  $a$ , before observing  $\theta$ , then the voter faces an entirely different problem and will use entirely different decision rules. The voter must disentangle the quality of the decision rule from the quality of the outcome. In this case, a rejection rule based on averages of past performance may be more appropriate than a  $K$  rule. That this is not academic can be seen from consideration of the setting of economic policy. The actual implementation of a policy regime generally precedes the occurrence of unanticipated shocks (such as a rapid increase in oil prices). Although one wants politicians to "react" correctly to exogenous events, as in the Ferejohn model, one does not want to penalize them for the lower outcomes associated with unanticipated shocks. A deeper model which includes these factors is desirable but not yet available.

Finally, one should notice the similarity between the results of Ferejohn when the electorate is nonhomogeneous and the results due to Whitman (1977) when candidates have preferences in an income redistribution election. In both cases, the candidates can, by playing off the voters against one another, absorb all the rent. Contrast this with the results in the Coughlin paper, where the equilibrium distribution maximizes a social-welfare function. Three main factors seem to be at the heart of the different implications: (1) in Coughlin, candidates care only about winning, whereas in Ferejohn they care about winning at the least cost to themselves; (2) in Coughlin, each candidate is uncertain about the minimum amount needed by a voter to cause that voter to vote for that candidate, whereas in Ferejohn candidates are sure about voters' preferences; and (3) in Coughlin, the challenger represents a legitimate potential alternative to the incumbent, whereas in Ferejohn the challengers "are unable to make precommitments to the voters" and have the same preferences as the incumbents. The true world is undoubtedly a combination of all of these factors.

One must be careful to remember that Ferejohn's model is only an example and not a complete theory. We are just recently coming to appreciate how special the assumptions of risk-neutrality and the independence of values are when attacking game-theoretic problems under asymmetric information. Thus, although the comparative statics results in Propositions 4 and 5 are of some interest, it is not clear how much is due to the special utility and common knowledge assumptions. Until

we have a more general theory, the robustness of the results remains an issue. Nevertheless, this is an important beginning in a new area of research in Political Economy.

## REFERENCES

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