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From Duverger to Cox, and beyond: The State-of-the-Art in Electoral Law Studies

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Prior to its publication, Gary Cox's *Making Votes Count* was widely and eagerly anticipated. (Indeed, some years ago, I received a referee report dismissing my submission as unnecessary because superior analysis would eventually appear in Cox's then forthcoming manuscript.) Upon its release in 1998, the book was instantly lauded: it collected multiple awards, including the prestigious Woodrow Wilson prize for best book published on government, politics, or international affairs. This acclaim was scarcely surprising – Cox has been one of the foremost scholars of elections and legislatures for the whole of his professional career. He is responsible for an impressive body of work spanning multiple research topics, nations, and methods of analysis. This book is testimony to his breadth, as it catalogs the key electoral features of virtually the whole set of modern democracies (more than 70), and makes frequent forays into diverse nations in search of empirical support for novel theoretical findings. In brief, Cox's project is to bring together a large formal, deductive literature on voting rules and social choice with an equally voluminous empirical, inductive literature on elections and party systems in the world's democracies. The prizes it garnered are one measure of the book's success at this merger; a large boost in Cox's swelling citation count will doubtless follow, reiterating the judgement that this is the major work on electoral law and voting to date.

And yet, a few years after the book's release, a second look reinforces the sense that political scientists still know surprisingly little about how electoral institutions constrain the behaviour of parties, candidates, and voters. Cox's work marks a frontier, but there is a vast amount of uncharted territory just beyond. From a purely descriptive vantage point, consider that Cox (with few exceptions) examines only lower houses of national legislatures in the very recent past. He thus omits many interesting cases and vast amounts of data. And, importantly, he defines out of his scope the neglected question of how multiple electoral environments – as produced by federalism, bicameralism, supranationalism, subsidiarity, and so on –

interact. Of course, this book is not principally a catalogue of descriptive national case studies, so it is limitations in regard to its theoretical arguments that are most serious. Mel Hinich, reviewing the book quite favourably in the *American Political Science Review*, has made one case that Cox's definition of rationality (central to the project represented by this book and a large number of his papers on which it builds, and from which it sometimes draws) is merely one of many possible conceptions, and is an unfortunately narrow one at that. I shall expand below on why the rationality Cox employs is, in some ways, unsatisfactory. I would also suggest that one weakness of the book, the sometimes thin links between theory and empirics, is a weakness characteristic of political research in general, and should be a priority for future work. Cox has been a pioneer in bridging positive, formal work, and statistical analysis, but, perhaps because he is one of the few who consistently strives to do so, he does not always succeed in explaining what principles guide his transition for the stark world of equilibria to the messy world of data.

The centrepiece of Cox's work is the $M+1$ rule, an extension of Duverger's famous contention (one of the few claims in political science accorded 'law' status) that plurality elections breed two-party systems whereas proportional formulae produce multi-party systems. Cox has generalized this finding by developing an impressively thorough and careful scheme for categorizing electoral systems. His rule, in brief, is that no more than $M+1$ candidates can be viable (i.e. receive non-negligible numbers of votes) in an M-seat district. Cox's rule thus shifts attention from the formula for victory (plurality or proportionality) to the magnitude of the district (how many seats are being contested), and, in that sense, represents quite a departure from Duverger. This book builds the case for the rule in steps. Cox presents abbreviated versions of formal models he has developed elsewhere to establish equilibrium results for various different electoral systems, considering strategic behaviour by both voters and candidates. He discusses aggregation from individual districts (the level at which the rule applies in the models) to nations, contending that it is the method of choosing an executive that is critical for whether district-level party systems are reproduced nationwide. He presents empirical support for the model, and he even discusses how and why violations of assumptions required to support the rule might occur.

A quick review of the formal model of voting excerpted in chapter 4 and presented more rigorously in Cox (1994) and Cox and Shugart (1997) clarifies what might be regarded as problematic in the underlying notion of rationality, and also why it is difficult to interpret the book's data analysis as a test of the model. The voters in these models solve a decision-theory problem involving knowledge of the distribution of types of voters, expected utilities for victory by all possible candidates, and a universally known set of expectations about support for all parties in competition. What about the famous conundrum that it is not rational to vote at all in cost–benefit terms, if the electorate is not very small and the act of voting not extremely easy? Following ample but unsatisfactory precedent, Cox assumes this

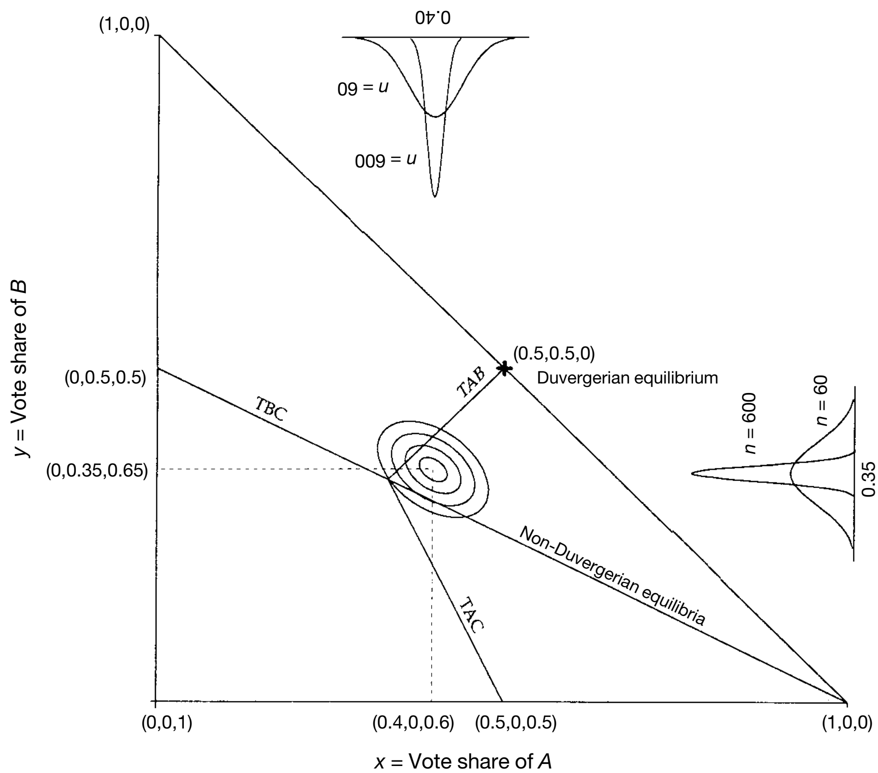

Figure 1

problem away by stipulating full turnout. It then turns out that a critical quantity for the analysis is the probability of tie outcomes.

Consider a very simple instance. Figure 1 depicts vote shares for three candidates competing under plurality rule in a single-member district. The x-axis maps vote share to candidate A, the y-axis corresponds to B's vote share, and C's share is simply $x-y$: hence, the origin, (0, 0, 1), represents the outcome in which candidate C wins all the votes, the points (0, 0.5, 0.5), (0.5, 0, 0.5), and (0.5, 0.5, 0), are the three possible results in which two candidates tie and one receives no votes, etc. The ellipses are probability contours connecting sets of equally likely outcomes, given that each of 60 voters vote for A with a probability of 0.4 for B with a probability of 0.35, and for C with a probability of 0.25. Under these conditions, the vote-shares variable follows a multinomial distribution. There are 1,830 possible outcomes, the single most likely of which is 24 votes to A, 21 votes to B, and 15 votes to C, which occurs with probability 0.014. This is the mode of the distribution, the centre of the contour ellipses. One can compute the probability of all possible outcomes directly from the multinomial distribution function, though it becomes simpler to use continuous-distribution approximations as n gets large. For instance, the marginal distributions for $x =$ vote share of A and $y =$ vote share of B are closely approximated by univariate normals, as shown on the top and right of the figure for both the case of $n = 60$ and the case of $n = 600$.

How likely are various possible ties for first place when there are 60 voters? The sum of all outcomes on the line labeled *TAB* is the probability that A and B tie for first place, i.e. $\Pr((A = B) \cap (B \geq C)) = 0.051$. Just as the expected vote shares suggest, the least likely first-place tie in this example is between B and C, as $\Pr((B = C) \cap (C \geq A)) = 0.007$ while $\Pr((A = C) \cap (C \geq B)) = 0.010$. Perhaps surprisingly, it is precisely these sorts of miniscule probabilities of ties that are the key to Cox's model, since his voters compare utilities from all possible votes and care only, ultimately, about their chances of making or breaking a tie for victory. Moreover, the assumption of a common set of expectations across all voters is also critical, since every voter in his analysis considers the same multinomial distribution when evaluating possible ties. This figure shows a distribution of possible outcomes for very few (e.g. 60) voters, but Cox's results rely on asymptotics as n increases. With 60,000 rather than 60 voters, the ellipses shrink drastically. (Compare the marginal distributions of x and y when n rises from 60 to 600. With large enough n's, there is very little chance of obtaining any outcome outside of a tiny region in which there are no first-place tie results. As n grows, all first-place ties become more and more remote possibilities.

Cox notes that the probability of a tie between the leading and second candidate (in expectation) is (asymptotically) orders of magnitude more likely than any other first place tie, and so his equilibria turn on only the *TAB* outcomes. This feature of the analysis may offend intuition: voters base their choices on very, very tiny probabilities, disregarding other very, very, very tiny probabilities as negligible. How, moreover, does one justify the appeal to asymptotics? Electorates are generally large, in the hundreds of thousands for US House seats, for instance. But, why, one might wonder, is the size of the electorate relevant? Only if voters are certain about their expectations concerning the parties' support levels should they perform the calculations that drive Cox's analysis. If these expectations are, instead, generated by polls of typical size (e.g. 1,500 voters), the appropriate n is not too large, the justification for asymptotic arguments vanishes, the relevant figure again features small ellipses rather than miniscule spikes, and the voters' computations become much more complicated, since they include multiple terms for different possible ties. A voter confronted with multiple polls that differ ought to consider a mixture of outcome distributions, making the problem more complicated still.

In one sense, this criticism is a bit of a cheap-shot, a complaint that Cox developed a tractable model rather than a more realistic but intractable alternative. But his $M+1$ rule does follow from precisely this curious feature of his analysis. Indeed, Cox finds two classes of equilibria, the 'most natural' of which he posits to be the Duvergerian, i.e. exactly $M+1$ vote-getting candidates (1994: 608). In the figure, note, the Duvergerian equilibrium is the single point (0.5, 0.5, 0). No matter where the expectations distribution is centred in the lower-right quadrilateral (i.e., the region in which ($A \geq B \geq C$), the predicted outcome is an exact tie between A and B, with C receiving no votes. It does not matter if A leads B by a lot or by a little. The line extending *TBC* into this quadrilateral, meanwhile, maps out all 'non-Duvergian' equilibria, in which 'there are two or more runners-up, whose nearly identical expected vote totals prevent any being winnowed out from the field of viable candidates' (1994: 612). Whether these are somehow less 'natural' a prediction than (0.5, 0.5, 0), all of these equilibria are exact and are virtually never observed in real-world elections.

Cox is both a modeler and an empiricist, so he is aware of this gap. He devotes time in his articles and his book to discussing which assumptions in the model might be poor fits to real-world voters (e.g. their short-term perspectives). But he is at his sketchiest when elaborating on how, exactly, the precise but unbelievable equilibria change as one drops or modifies various underlying assumptions. Indeed, following a short and informal discussion, he proceeds to empirical analysis showing only a very rough correspondence to the pattern suggested by the model's equilibria in selected election data. I would be quite surprised if many readers find that this analysis renders the model any more clear or persuasive.

In other applications, Cox is devilishly clever in concocting empirical tests. But, to take just one example, consider that his exploration of vote-splitting in Germany (pp. 81–83) is a search for closeness effects (i.e. sensitivity to how close the top two finishers in a plurality race are expected to be) that are totally absent in the formal model described above. Moreover, that analysis is prone to an ecological fallacy, since (for example) his proxy for the *actual* number of ballots featuring CDU votes in the plurality race and FDP votes in the PR race is the difference in the *total* of FDP votes in the plurality race and FDP votes in the PR race. In short, Cox is inventive, but not always cautious enough in qualifying the ambiguities, compromises, or disanalogies to models in his data analysis. Rarely is it clear that his empirics are tight matches to his models. It is not that the work is careless, but Cox seems at times to over-reach because he is intent on addressing questions both formally and empirically, whenever possible. And to his credit, Cox is scrupulous about documenting actual mistakes; he maintains an ongoing list of errata in the book at http://weber/ucsd.edu/~gcox/errata.htm.

Do those designing election law consult political scientists? Were the mixed systems recently adopted in much of Eastern Europe, Japan, Italy, and New Zealand an outgrowth of theoretical work? Alas, no. In fact, political scientists have followed rather than led on the issue of what happens when plurality and proportionality are mixed. If Israel's attempt to lessen the influence of small parties by altering its electoral rules was guided by advice from political theorists, those advisors did not earn their consulting fees! Professional economists offering new democracies advice on markets, regulation, property rights, and so on may not have all the answers, but political scientists are in a more embarrassing situation of having startlingly little guidance to offer constitutional engineers.

This book belongs in the library of everyone who takes an interest in elections – it belongs front and centre in the library. Cox's division of the fundamental issue of how electoral institutions shape party systems is ingenious, even if he has not solved all the problems he has identified. And the book, far from being dryly analytical, is rich in detail. A short analysis of what David Lloyd George was up to as he oversaw the British Liberal party's disintegration in the 1920s is entertaining if not thorough or *entirely* compelling. In the end, *Making Votes Count* is best regarded less as a canon of solved problems and theorems than as a treasure trove of intriguing questions and clearly identified puzzles about connections between mass and elite strategy in campaigns under diverse institutional settings. To Cox's credit, when you put this book down, you will want to roll up your sleeves and get to work on the many theoretical problems it illuminates!

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