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ORIGINS OF THE UNCOVERED SET

I had the first inkling of the uncovered set idea during the winter of 1970 as graduate student at Berkeley. I was revising and extending a seminar paper (and prospective dissertation chapter) on "Voting in Committees" that I had written the previous year in a course on Formal Models in Politics taught by Michael Leiserson and Robert Axelrod (probably one of the first such political science courses taught anywhere outside of Rochester). We had read a book by Otomar Bartos on *Simple Models of Group Behavior* (1967) that included a chapter on "dominance structures" in societies and employed adjacency matrices and directed graphs — and tournaments in particular — to represent and analyze such structures. This in turn led me to *Structural Models* by Harary, Norman, and Cartwright (1965). It occurred to me that, given an odd number of voters with strong preferences, the same analytical device could be used to represent the majority preference relation and that some of the deductions Bartos presented concerning social dominance structures (many of which were standard graph-theoretic theorems found in Hararay et al.) had analogs for majority voting. In my seminar paper, I set out to use the tournament technology to systematize and extend Duncan Black's work (1948, 1958) on committee voting. Tournament diagrams drove home a point that I had not adequately appreciated: if you arrange four or more points around a circle and draw arrows around the perimeter so as to create a cycle encompassing all the points, this cycle also has an "internal structure" — that is, arrows must also extend across the interior of the circle. Moreover, given five points or more, cycles of given length may have different internal structures.

By the time I had finished the paper, I had discovered with some disappointment that Michael Taylor (1968) had already had much the same idea by applying graph theory to social choice problems. Nevertheless, the tournament technology allowed me to make some modest advances in the "Black program." For example, Black (1958: 40-41) had provided an example with three voters and four alternatives in a cycle of majority preference showing that, for each alternative, there was some voting order under "ordinary committee procedure" (what we now call *amendment procedure*) that would lead to its winning under sincere voting. Using the tournament technology, I was able to extend this result to any odd number of voters (indeed, the tournament device made it unnecessary to specify a particular number of voters) and to any number of alternatives in a cycle. (This became Proposition 4 in Miller, 1977.)

In a last-minute addendum to the seminar paper, I made a first stab at deriving a similar kind of result under strategic voting. Using a three-dimensional strategic form for a voting game, I was able to show that the same result held under strategic voting with three voters and three alternatives. I could also show that the voting orders that led to victory by a given alternative were different under sincere and strategic voting and, in particular, that under sincere voting the last alternative in a three-alternative cycle to enter the voting wins, but under strategic voting the first such alternative wins. But I had no good idea as to how to proceed beyond three voters and three alternatives in the strategic voting case.

By the winter of 1970, Robin Farquharson's *Theory of Voting* (1969) had been published and I studied it eagerly. His "Table of Results" in Appendix I confirmed my results for both sincere and strategic (or "sophisticated") voting in the three-voter three-alternative case. But I was completely mystified by his statement in the Preface that these "results . . . can be readily extended to cover any desired number" of voters and/or alternatives, since I had been using essentially the same kind of strategic form analysis that Farquharson was manifestly using in the body of his text, and I had found it to be impossibly burdensome to extend to a larger number of voters and/or alternatives. But Farquharson also introduced the "voting tree" device (a highly compressed extensive form) to concisely represent binary voting games. At some point that winter, while staring at such a voting tree, I had an epiphany and discovered the kind of backwards induction that readily solves binary voting games and that was later definitively characterized by McKelvey and Niemi (1978). With this tool, I could easily determine the strategic voting outcome for any number of voters and relatively many alternatives, and I enthusiastically began using it to derive Black-style propositions for strategic voting. But an apparent anomaly quickly turned up: when I extended the number of alternatives in a cycle from three to four, I discovered that there was always one alternative in the cycle that could not win under any voting order. This was surprising and somewhat disconcerting, since I expected sincere and strategic voting results to run in parallel. Something was preventing one alternative from winning, regardless of the voting order. Evidently it had to do with the fact that, when internal structure is considered, the alternatives in a four-element cycle (unlike those in a three-element cycle) occupy distinct positions in the tournament structure. But this structural asymmetry did not preclude a degree of symmetry with respect to sincere voting outcomes, so the discrepancy remained puzzling. I further discovered that, given a cycle of five (or more) alternatives, different internal structures are possible, some of which made it impossible for certain alternatives to win while others did not. In my subsequent dissertation chapter, I simply presented the following proposition: "Under ordinary [amendment] procedure, there may be a motion [alternative] in the Condorcet [top cycle] set that is not the sophisticated voting decision under any voting order."

In 1976, I had a revise and resubmit decision on a paper largely derived from this dissertation chapter. My main task was to revise the presentation of the material (on the basis of wise editorial guidance provided by *AJPS* editor Phillips Shively) rather than its substance. But in revisiting the analytical issues, I noticed two additional points concerning the strategic voting anomaly. First, each alternative y that could not win under any voting order (in the relatively small cyclic tournaments I was examining) was "dominated" in a particularly strong way by some other alternative x — not only did x beat y but x also beat everything that y beat. Given an arrangement of x, y, and the alternatives beaten by y into a three-level structure with x at the top and with downward-pointing arrows representing majority preference, in my own mind it seemed natural to say that x "covers" y — and, for better or worse, the terminology stuck. The second point I noticed was that if x is unanimously preferred to y, then x is majority preferred to all alternatives to which y is majority preferred — that is, x covers y. I incorporated the latter point into the revised paper (Miller, 1977, Proposition 10), and I then turned quickly to explore the covering relation more thoroughly and discovered that it had many interesting properties. That exploration led to a paper that I presented at the 1978 APSA meeting, which drew almost no attention (perhaps because it was one of six papers squeezed into a two-hour panel, one of which was Kenneth Shepsle's early statement of the "structure-induced equilibrium" idea) but which did attract considerable attention once it was published (Miller, 1980).[1]

In Miller (1980), I focused primarily on discrete alternatives and unrestricted preferences, but I also ventured some observations and conjectures concerning covering and the uncovered set in the spatial context, especially in light of McKelvey's (1976 and 1979) "global cycling theorem" and his famous concluding comments (in the earlier article) about its implications for agenda control. Since at the time I did not have the analytical tools at hand to pursue this aspect of covering effectively, I sent my uncovered set paper to Richard McKelvey and invited him to apply his expertise to the problem. I like to think that this helped lead to McKelvey (1986).

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[1] I have noticed that authors sometimes give Miller (1977) as the original citation for "Miller's uncovered set." While these recollections indicate that such a citation is not entirely off-the-mark, I did not use the term, identify the concept, or explore its general ramifications in print until Miller (1980). These recollections also tend to contradict the statement in Bianco et. al., (2004, p. 259) that the uncovered set originated as a cooperative game-theoretic concept. In any event, my original motivation was to understand strategic voting in an explicitly non-cooperative setting.

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