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Reflections on public choice *

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Abstract. My 2002 presidential address to the Public Choice Society consisted of three parts. The first had to do with the business of the Society's 2002 meeting and plans for the Nashville meeting, and has been omitted. This essay begins with the second section of that talk, in which I identify what I call *the* classic books of Public Choice, and then discuss what I view as common misconceptions about Public Choice. The next section of this essay surveys five of the most important empirical puzzles in Public Choice theory (three of them taken from the work of Anthony Downs), and my own attempts over the years to come to grips with these puzzles. I should note that, despite my courtesy appointment in UCI's Economics Department, in this essay I write primarily from the perspective of a political scientist – albeit, one with lots of friends who happen to be economists.¹

1. The five books of the Public Choice Pentateuch

In the eight year period between 1957 and 1965, five books were written that became the foundations of the Public Choice movement.¹ Each has sparked a vast literature. Singly and collectively these works have left political science indelibly changed.² The books that I regard as the Public Choice Pentateuch are (in alphabetical order):

Arrow, Kenneth. 1951, second edition 1962. *Social Choice and Individual Values*. New York: Wiley.³

Black, Duncan. 1958. *The Theory of Committees and Elections*. London and New York. Cambridge University Press.

Buchanan, James and Gordon Tullock. 1962. *The Calculus of Consent: The Logical Foundations of Constitutional Democracy*. University of Michigan Press.

Downs, Anthony. 1957. *An Economic Theory of Democracy*. New York. Harper.

Olson, Mancur. 1965. *The Logic of Collective Action*. New York. Schocken.

In addition, if we look through 1971, there were six other books that are almost equally worthy of mention, but which are not as fundamental to the

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shaping of the early Public Choice tradition as the five listed above. Three are written by economists; three are written by political scientists.⁴

Black, Duncan and R. A. Newing. 1951. *Committee Decisions with Complementary Valuation*. London: W. Hodge.⁵

Farquharson, Robin. 1969. *Theory of Voting*. New Haven, Connecticut. Yale University Press.

Hirschman, Albert. 1970. *Exit, Voice and Loyalty*. Cambridge, Mass. Harvard University Press.⁶

Niskanen, William. 1971. *Bureaucracy and Representative Government*. Chicago, Illinois. Aldine-Atherton.⁷

Rae, Douglas. 1967 (rev. ed. 1971). *The Political Consequences of Electoral Laws*. New Haven, Connecticut. Yale University Press.⁸

Riker, William. 1962. *The Theory of Political Coalitions*. New Haven. Connecticut. Yale University Press.⁹

Since each of the first five books and three of the next six listed above are authored (or co-authored)¹⁰ by an economist, many political scientists think of Public Choice (and rational choice approaches more generally) as a form of “economic imperialism,” but this is not fully accurate. It is certainly true, for example, that Black’s work was an explicit attempt to create a “science of politics,” and that Olson is concerned to demonstrate the inherent problematicity of a group basis to politics, including perhaps most notably Marxian ideas of class conflict. Still, any claim that the early works of Public Choice are examples of economic imperialism requires some important caveats.

Arrow’s work, for example, is at least as much a rebellion against the mainstream welfare economics of the 1930s and 1940s as an attempt to colonize political science. In particular, Arrow rejected one of the main tenets of the then dominant approach to social welfare functions in his discipline, the notion of interpersonal comparisons of utility based on an aggregation of cardinal utility functions. Similarly, even after receipt of a Nobel Prize, James Buchanan continues to think of himself as a rebel against mainstream economics, noting in his intellectual autobiography, *Better than Plowing*, (Buchanan, 1992: 764) that it was highly unlikely that he would have become a Nobel Laureate had the decision been left to the leading economists at major universities in the U.S., since his style of work, de-emphasizing mathematics, and refusing to draw a hard and fast line between normative and positive economics, is not held in high regard in these academic circles. Moreover, while due to the extensive bodies of work and high visibility of economists such as Gary Becker, we now, see economists as aggressive colonizers of other disciplines in the social sciences (extending even into biology), in the 1940s,

1950s and even throughout the 1960s, most economists were, in Jonathan Swift's metaphor from *Gulliver's Travels*, "little endians," with no interest in studying topics not found in Marshall's *Principles*.

In this context, it is important to note that Duncan Black, was a relatively marginal figure within economics who had great trouble getting the several now classic essays that became the heart of *The Theory of Committees and Elections* accepted by top economics journals. Indeed, Black's work across disciplinary lines was not understood, much less viewed as significant, by most of his economics colleagues.¹¹ In particular, another work of Black's which I've listed above as an important early work in Public Choice, *Committee Decisions with Complementary Valuation* (co-authored with R.A. Newing) was rejected by numerous publishers and ultimately privately published. It was so hard to find that it had very little influence on the subsequent evolution of spatial models of politics and, as noted earlier, is not even cited in Mueller (1989).¹² It is largely William Riker's discovery of Black's work (in Riker, 1961) that gave Black's scholarship visibility, and that led, first to a visiting appointment at Rochester in 1963, and then, after Black's retirement from the University College of North Wales, Bangor, to his visiting appointments in political science at Michigan State in 1971–1973, and 1975–1976.¹³

Similarly, as far as I can judge, for a long time, Downs remained far better known and more likely to be viewed as important by political scientists than by economists and, at least for his early career, I believe the same can be said for Mancur Olson. Also relevant to the notion that the economics profession was not that quick to recognize the early works of Public Choice as "real" economics, is the fact that Gordon Tullock, was denied promotion to Full Professor at the University of Virginia in 1967 (Brady, 2000: 164).¹⁴

Since viewing the world from the standpoint of individual goal maximization is as natural to classically trained economists as purring is to cats (even though not all cats purr, and no cat purrs all the time), conflating rational choice and Public Choice is another common mistake among critics.¹⁵ Indeed, even many political scientists engaged in formal modeling do not make any distinctions, thinking of Public Choice as just another name for what political scientists (*pace* Bill Riker) call positive political theory, and thinking of it as extending to virtually all applications of game theory, expected utility models, exchange models, etc. But viewing Public Choice and rational choice as synonymous is inconsistent with the views of some of the key figures in Public Choice.

Like many Public Choice scholars, I would distinguish between "rational choice," which I take to be the notion that individuals act consistently and logically in translating preferences (and beliefs) into choices, from "method-

ological individualism,” which I take to be the notion that any empirically valid theory of society must be founded on an understanding of the choices of individuals, without any reification of collectivities.¹⁶ In particular, James Buchanan is first and foremost a methodological individualist, in the sense of that term described immediately above, not a rational choice modeler. Moreover, in contrast to many contemporary economists of a more mathematical persuasion, Buchanan is careful to talk about economics as the study of exchange, not as the science of optimization.¹⁷

In fact, in many ways, the five founding classics of Public Choice have relatively little in common with one another! It is largely only thanks to the syncretic perspectives of Gordon Tullock, as expressed in the editorial choices about what to include in the early issues of the journal *Papers on Non-Market Decision-Making* (soon to be renamed *Public Choice*), and then the proselytizing on behalf of Public Choice within political science of a remarkably eclectic William Riker, that we see the ideas in these five works as indissolubly linked and part of a broader canvas. For example, James Buchanan, from the beginning, distinguished Public Choice (especially that portion of it that later became known as constitutional political economy) from social choice, and did not attach great importance to Arrow’s and related work, since in his view, societies were not like individuals and thus it was not particularly troubling that they did not behave as such.

Three other common mistakes about Public Choice made by many of its critics are to reduce it to a caricature (i.e., purely selfish behavior involving short-run optimization of some one-factor utility function under assumptions of complete information), or to make *a prioristic* claims that Public Choice approaches cannot account for some particular topic allegedly accountable for only “by incorporating “non-rational” considerations,¹⁸ or to attack Public Choice on the basis of claims that the predictions of its classic works are not born out by evidence. While the books I have identified above are classics, they are not classics in the way that some political theorists view classics, i.e., books in which the answers to the great questions are to be found. Rather these works are trampolines, jumping off points for further work, that allow us to get a lift (a head start) on some important issues. In my view, by and large these books are more important for (a) the style of their approach, (b) the new ideas they introduce, and (c) the importance of the questions they pose, than for any specific empirical predictions.¹⁹ Also, works such as Arrow, and Buchanan and Tullock, and even large portions of, say, Downs, Black, and Olson, are best viewed as contributions to normative political philosophy – as such, in my view, they among the best such normative work being done by anyone, especially with respect to issues of representation and democratic theory.

There is one last amusing point to which I would call attention: Despite very high levels of resistance to rational choice work – some but not all of which resistance is associated with an anti-science, anti-quantification bias, and some with professional jealousies – just as Thomas Jefferson said, “We are all Republicans, we are all Federalists” – now virtually all of empirical political science is inextricably tainted with rational choice ideas. Political scientists are all almost now “soft” rational choice theorists in the sense of sharing the simple notion that people (often) do things for reasons and that it’s important to try to figure out what those reasons are, and to understand how we may affect the choices people make by affecting the environment (e.g., the institutional structure) within which those choices are made ²⁰

2. Five usefully wrong predictions of public choice scholars

It would, on the fact of it, appear quite bothersome that what are commonly taken as some of the most important empirical implications of classic work by Public Choice scholars just don’t fit the evidence – a point acknowledged by Bill Niskanen (Niskanen, 1998) and others within the Public Choice community. Here let me focus on five important empirical claims usually attributed to Public Choice theory which have been topics of my own modeling efforts.

1. Majority rule cycles should be omnipresent
2. In two-party competition, political parties should converge.
3. Potential voters shouldn’t vote.
4. Potential voters should remain largely ignorant about the choices facing them.
5. Coalitions should be minimal winning.

We will briefly discuss each of these claims, first identifying what are usually taken to be the sources of the claim, then considering the empirical evidence for the inaccuracy of the prediction, then showing why, when we develop a more complex model or a more nuanced approach, the seeming empirical failure largely or entirely goes away. Because of space constraints, my discussion of each of these points will necessarily be somewhat elliptic; the reader is referred to the articles cited for further elaboration of the argumentation. However, the general thread is a simple one: there is no such as *the* rational choice model of any phenomenon, only *a* rational choice model (Wuffle, 1999). Moreover, even when rational choice models appear wrong, they are “usefully wrong.” I refer to models that generate empirical conundrums as “usefully wrong” when those models help us to think more clearly about how

we need to modify our theories (or rethink our stylized facts) in order to make sense of the world. In my view, as we will see, the models that generated each of the five predictions listed above readily qualify as models that are “usefully wrong.” That a model offered in one of the great books (or seminal articles) of Public Choice fails to predict well does not mean that we scrap the intuitions and insights that generated that model. Rather we should try to build on what has been done by other scholars.

Indeed, until certain issues were studied by Public Choice scholars it wasn’t clear what were the right questions to ask or the right puzzles to solve. Phenomena that we now see as highly problematic, e.g., voters bothering to vote, coalitions that are more than minimal winning, stability instead of cycles, long went unrecognized. Moreover, whatever may be the failings of existing Public Choice models (or of rational choice models, more generally), you can’t beat something with nothing. Thus, for any phenomenon that is being studied, one must compare the available model(s) with whatever alternatives scholars with other perspectives might be offering. If all somebody does is criticize existing work by Public Choice scholars without showing any ability to do better at making sense of the world, then, valuable as that criticism may be, the scholar should at least have the common courtesy to recognize just how difficult the problem being addressed really is.

2.1. *Majority rule cycles should be omnipresent*

There are three main sources for the claims about the inevitability of cycles: (a) analytic and simulation results for the impartial culture and related distributions (e.g., Black, 1958; Gerhlein and Fishburn, 1976 a, b); (b) theoremtic results apparently showing that cycles are inevitable unless highly restrictive and empirically implausible assumptions are met (Sen, 1970); and (c) work on generic cycling in majority rule voting in a multidimensional issue space (McKelvey, 1976, 1979; cf. Riker, 1982). Yet, cycles are so rare empirically that Tullock (1981) was moved to ask rhetorically “Why so much stability?” For example, my own empirical work, looking at well over a hundred elections (albeit mostly among a relatively limited number of candidates) essentially never finds cycles (see e.g., Feld and Grofman, 1988, 1990; Adams, Merrill and Grofman, forthcoming).

There are good reasons, however, why theory and evidence about the prevalence of cycles are in conflict. Each of the three main bases for the claims about the high likelihood of cycles needs to be reexamined.

Analytic and simulations results for finite sets of alternatives that show that cycles should be expected are almost always based on the *impartial culture* (the assumption that all (linear or weak) preference orderings are equally likely) or closely related distributions. Such distributions, while important

theoretically in setting bounds on what might happen, are of very limited empirical relevance. The impartial culture generates knife edge-results that give the *maximum* probability of generating a cycle (Tsetlin, Regenwetter, and Grofman, forthcoming). Similar results hold for other symmetric distributions often used for modeling purposes. In real world voter preference distributions, there is always some skewness.

A key result in Sen (1970) gives *value restrictedness* as a necessary condition to avoid cycles.²¹ This condition on triples of alternatives is highly restrictive and is essentially *never* satisfied in real world preference distributions.²² However, Sen (1970) uses the term “necessary” to refer to his value-restrictedness result in a peculiar fashion that has misled subsequent scholars. In the more conventional use of the terms ‘necessary’ and ‘sufficient,’ value restriction is a sufficient but not a necessary conditions for acyclicity (Regenwetter, Marley, and Grofman, forthcoming). We can, moreover, for linear orders state necessary conditions in terms of what Feld and Grofman (1986) call “net value restriction and net preference majorities.”²³ This condition is almost always satisfied in the data sets I have examined (see e.g., Feld and Grofman, 1988; Regenwetter, Adams, and Grofman, 2002).

Among a limited number of alternatives from some potentially infinite set embedded in a n -dimensional issue space, we can show that there is little chance of a cycle when the *yolk* is small (in two dimensions, the *yolk* is the smallest circle that intersects all median lines: McKelvey, 1986; see also Feld, Grofman and Miller, 1988; Feld and Grofman, 1990) and, empirically, the *yolk* has been found to be small (Feld, Grofman and Miller, 1988). Cycles are even less likely to be observed if cycles among essentially indistinguishable (nearly identical) alternatives are simply disregarded (Feld and Grofman, 1996).²⁴

2.2. *In two-party competition, political parties should converge*

Downs (1957) is often treated as arguing that, at least in two-party competition in a single dimension, parties will tend to converge to the location of the median voter. Yet, while there certainly is evidence for some (strong) centripetal pressures in two-party systems, parties don’t really converge, and the center can even “empty out,” as it arguably has in the present U.S. Congress. For example, numerous scholars have shown that, in the U.S., when a given constituency elects members of opposite parties (e.g., when a congressional seat changes hands to a member of the opposite parties, or in states which are simultaneously represented by senators of opposite parties), the difference in voting records (as judged, say, by ADA scores) between the office-holders of different parties can be huge (Fiorina, 1974; Poole and Rosenthal, 1984;

Bullock and Brady, 1983; Grofman, Griffin and Glazer, 1990). Similarly, when, as part of the European party manifestos project (Budge, Robertson and Hearl, 1987), Robertson generated a two-dimensional factor-analysis-generated issue space for the U.S., 1948–1980, based on party platforms, the Democratic and Republican parties in the United States remained in distinct areas of that issue space (Robertson, 1987: 69: Figure 3.1).

Downs' famous result about two-party convergence rests on numerous subsidiary assumptions in addition to unidimensionality and two-party competition (Grofman, 1993c). If one or more of these assumptions is violated/replaced with more realistic assumption or more complex institutional arrangements, then the (full) convergence result almost certainly fails to go through (Grofman, 2001).²⁵ We can illustrate this point with respect to the existence of multiple constituencies rather than a single election.

Consider a situation where each party can get a roughly 50% chance of winning each and every constituency if they pick identical platforms. Each party could suffer dramatic reversals in the parliament in some election if there were short term forces affecting each legislative constituency (related, say, to the personal attractiveness or policy positions of the presidential candidate of the party, or to national economic factors) that turned marginal seats into losses. For risk averse parties competing in marginal constituencies, tweedledum-tweedledee politics need no longer be optimal. In particular, if constituencies differ in the location of their median voter, and each party's candidates must take the same position as their national party, were the national party to stake out a policy position designed to make very likely wins for its candidates in some constituencies, while largely conceding a portion of the other constituencies to their opponents, parties could be assured that, no matter what happened, they would retain some hold on power.

It is also important to note that the candidates *elected from* each party might look different from one another even if the candidates *nominated by* each party present near identical positions within any given constituency. Because of association with national party images, in constituencies that are more liberal, the candidate associated with the more liberal of the two parties is likely to be advantaged, while in constituencies that are more conservative the candidate of the more conservative party is advantaged (Grofman, Koetzle, McDonald and Brunell, 2000). Thus, *ceteris paribus*, in the U.S., liberal constituencies and conservative constituencies are likely to elect candidates from different parties even if the candidates of each party within those constituencies *try* to compete for the allegiance of the median voter in the constituency – with liberal constituencies more likely to elect Democrats and conservative constituencies more likely to elect Republicans.

We should also note that there are conditions under which we may expect modal rather than median (or mean) outcomes to be chosen; i.e., we may have leaders chosen who are more extreme than their followers (Grofman, Koetzle, McGann, 2002; see also Merrill, Grofman, Brunell and Koetzle, 1999). One way this may happen is when we replace the basic Downsian assumption of plurality-based elections with sequential elimination runoffs (McGann, Koetzle and Grofman, 2002).

2.3. *Potential voters shouldn't vote*

The notion that rational choice models of turnout imply that (almost) no one should vote has been repeated so often that it has become a truism. Certainly, Downs (1957) offers a model in which purely instrumental calculations about the short term effects of one's vote in a single election (where the value of one's vote must be discounted by the likelihood that it will decide in affecting the outcome of the election) lead to expectations of few or no voters, unless we also build in factors such as citizen duty or non-instrumental benefits of voting (analogous, perhaps, to why sports fans might loudly cheer their team on to victory, while watching the game on TV at home, alone). Yet, in fact, while turnout varies both across countries, and within-nation, across types of elections, and across types of voters, just about everywhere a substantial proportion of eligible voters do vote – and at least some of those eligible to vote do so with substantial regularity. What they view as a “nobody should vote” prediction gives Green and Shapiro (1994) a lot of chuckles at the expense of rational choice theorists, supposedly caught like rats in a maze (of their own devising) and unable to find a way out. Even Morris Fiorina has pondered the question of whether “turnout is the paradox that ate rational choice theory.” (see Grofman, 1993b).

My view is that we have been “seduced” by Anthony Downs to believe that voters should only vote for instrumental reasons (and then “abandoned” by him as well when he lost interest in the topics of his doctoral dissertation). People rarely act *solely* for instrumental motives, and it is unnecessary to posit that that they do.²⁶ A better way to think about the Downsian model of turnout is in terms of “comparative statics,” i.e., by looking at the partial derivatives of turnout with respect to variables such as election importance, and perceived impact on outcome.²⁷ In doing so it is possible to develop non-trivial testable (and accurate) predictions related to turnout.²⁸

2.4. *Potential voters should remain largely ignorant about the nature of the electoral choices facing them, and should not seek out such information*

The notion of *rational ignorance* is correctly attributed to Downs (1957). However, while it is hard to overestimate voter ignorance (especially about factual matters); yet (some) voters sometimes follow political campaigns with much the same interest and intensity that others devote to, say, sports playoffs. Here, going back rereading Downs makes it clear that Downs' views about information and politics went well beyond the idea of rational ignorance. Indeed, Downs' details various ways in which voters will either come to be informed, or will be able to act "as if" they were informed in the sense of making a choice that would be appropriate if they were to know all that was to be known about the candidates/options available to them in an election. In particular, Downs introduces the role of parties as signaling devices and the by-product theory of knowledge. These and related ideas about information shortcuts have been further developed by later scholars (see e.g., Miller, 1986; Popkin, 1991; McKelvey and Ordeshook, 1985a, b, 1986, 1987; Grofman and Norrander, 1990; Lupia, 1994 a, b).

2.5. *Coalitions should be minimal winning*

While there is interesting (and now largely neglected) modeling of coalition processes by social psychologists even before Riker (1962), that book makes the important claim that we ought to expect minimal winning coalitions in a number of different contexts, including multiparty cabinet coalitions. When we turn to the evidence we find that, at least for cabinets, minimal winning coalitions, while common, are not modal. For example, Only about 40% of European cabinets in the post WWII period are minimal winning – albeit this is a far higher percentage than would be expected by chance (Laver and Schofield, 1990).

Why don't we get minimal winning coalitions? Well, one simple answer is that Riker's (1962) results rests on the assumption that politics is a zero-sum game, involving purely office-seeking; once, however, we admit of ideology/policy goals, than politics is no longer zero-sum (Grofman, 1984).²⁹ Yet Riker (1962) has played a remarkable role in inspiring a considerable amount of important work – on models of coalition formation and dissolution and of policy and portfolio bargaining game among coalition actors – work which usually rejects both the book's main assumptions and its main conclusion.³⁰ The work inspired by Riker (1962) demonstrates beautifully the importance of work that raises important issues and, in being usefully wrong, lays the groundwork for the future major advances in the social scientific

enterprise. And, if Bill had lived long enough to read these words, I think he'd agree with me.

Notes

1. The portion of that section dealing with how my own intellectual history has been intertwined with the great figures and great ideas in Public Choice has been relegated to an appendix.
2. Jim Buchanan (personal communication, 2002) has stated that the phrase "Public Choice" was coined by the political scientist, William Mitchell, in the late 1960s. It replaced the earlier label of "non-market decision-making" coined by Gordon Tullock, and used by him for the first several issues of the journal subsequently renamed *Public Choice*. The precursor of what was to become the Public Choice Society was a meeting organized by Buchanan and Tullock in Charlottesville in 1963.
3. Most have also had an important impact within economics as well, but, with the exception of Arrow, arguably not as great an impact as in political science.
4. It is really the second edition of Arrow that had the greatest impact. That edition is little changed from the first edition, with perhaps the most notable change the addition of reference to the earlier work of Duncan Black on single-peaked preferences and the paradox of cyclical majorities, giving rise to Arrow's "Possibility Theorem for Single-Peaked Preferences."
5. Robin Farquharson's last academic appointment was as a Fellow in Management Studies, University of Cambridge, but his initial training was in political science.
6. Black and Newing is the only work that I regard as a classic that is not cited in the second edition of Dennis Mueller's near definitive overview of Public Choice theory through the 1980s. (Mueller, 1989).
7. From a sociological point of view, Hirschman really cannot be labeled part of Public Choice, since he has never attended any meetings of the Society, not does he cite to the Public Choice literature. Yet, in my view, that is less important than the intellectual connections between his work and that of Public Choice scholars.
8. Both Anthony Downs (Downs, 1967) and Gordon Tullock (Tullock, 1965) also wrote about bureaucracy, even earlier than Niskanen, but their works did not then have and have not subsequently had anywhere near the visibility of Niskanen's book.
9. It is only relatively recently that Rae's book would be considered part of the Public Choice canon. Its importance has come to be recognized largely in the light of the recent development of work on game theoretic choice approaches to understanding the choice and impact of electoral laws, typified by Gary Cox (Cox, 1997).
10. We might also note that three of these five books are published by Yale University Press. Since then Yale University Press's only major addition to the Public Choice literature is the Green and Shapiro (1994) critique of same.
11. Gordon Tullock, trained as a lawyer, does not have a degree in economics. However, at the time *Calculus of Consent* was written, he had an appointment as a Post-Doctoral Fellow at the University of Virginia. Later he was an Associate Professor of Economics there. He has since held senior level appointments in economics at a number of universities, including VPI, George Mason, and the University of Arizona.
12. Jack Johnston, personal communication, 1983. Johnston was once a colleague of Black's.
13. Arrow is another exception to the failure of economists to recognize the importance of Black and Newing's work. See Arrow (1990).

14. However, James Buchanan also recognized the importance of Black's work and provided him a visiting appointment at VPI in 1962, and after his retirement from the University College of Wales, again in 1968, 1971, and 1972. Black also had been a Visiting Professor in Economics at the University of Toronto, 1951–52, and a Visiting Fellow in Law and Economics at the University of Chicago in 1965, 1969, 1972, 1973, and 1979. It was during the early part of this time period that I got to know him. In 1989, Black was finally elected to the British Academy.
15. This decision was an important factor in the decision of James Buchanan to leave Virginia and, not long thereafter, with the aid of a former student of his, to create a Center for the Study of Public Choice at VPI in 1969; Tullock then moved to VPI to join Buchanan in 1968.
16. For example, Green and Shapiro's *Pathologies of Rational Choice* (1994), despite its title, is actually almost entirely about work in political science inspired by the classic works in Public Choice.
17. Moreover, both concepts need to be distinguished from the more normative concept of "political individualism" (sometimes called "possessive individualism"), i.e., the (Lockean) notion that societies ought to be based upon a social contract that provides fundamental protections for individual rights.
18. In addition to homo economicus, Buchanan's work on constitutional economics emphasized the contractarian tradition. However, Buchanan, would not, I think, regard himself as social contract theorist in the Lockean tradition because of the importance he, following Wicksell, places on the need for unanimous agreement. Of course, as is clear from the *Calculus*, Buchanan does see his ideas as linked to those of perhaps the greatest of all American-born political scientists, James Madison. As Buchanan comments about *Calculus of Consent* in *Better than Plowing* (1992: 9): Tullock and I "more or less explicitly considered our exercise to be an implicit defense of the Madisonian structure embedded in the United States Constitution."
19. For example, some of the scholars who emphasize the explanatory power of norms, or social roles, or collectively embedded social identities, would insist that such ideas cannot be reconciled within a rational choice framework. Of course, that insistence mostly indicates an unfamiliarity with the potential scope of formal modeling. A prioristic criticisms of Public Choice models based on the "inevitable" empirical limitations of such models doesn't cut it; it's similar to debating how many angels can dance on the head of a pin. (To have an empirically meaningful debate about such an issue you would first have to find the angels, and then you would need to specify the size of the pin.).
20. I will elaborate on this last point later in the essay.
21. Unfortunately, absurd "lines in the sand" are drawn by scholars on both sides of the rational choice -anti rational choice divide in political science. For example, some of the most technically skilled rational choice modelers suffer from the hubris of believing that if it's not a theorem it's not a contribution to political science. That point of view is just as silly as the attempts to eliminate formalization or sophisticated quantitative analyses from the discipline. As I have noted in earlier work (Grofman, 1993a), game theory is to the social sciences what calculus is to the physical sciences, but just as not every application of calculus is a contribution to physics, not every application of game theory is a contribution to political science. For any model, for any explanation, the proof is in the pudding: does it help us make sense of the real world; is it (at least in principle) falsifiable; does it lead to (non-trivial) implications about other aspects of the world.

22. Value restrictedness occurs when for every triple of alternatives either the *NB* (*not best*) or the *NW* (*not worst*), or the *NM* (*not middle*) condition is satisfied. The *NW* condition corresponds to *single-peakedness*; the *NB* condition corresponds to *single-troughedness*.
23. The same empirical implausibility holds for other conditions given in Sen (1970).
24. This idea is further generalized in Regenwetter and Grofman (1998) and Regenwetter, Marley and Grofman (forthcoming).
25. McKelvey (1976, 1979) provides a “global cycling” theorem (made famous by Riker, 1982) that seems to imply that, when alternatives consist of points in some *n*-dimensional issue space, agenda manipulation should be easy. Yet, there is very little evidence of observed agenda manipulation except for contrived cases such as the Plott and Levine (1978) airplane purchase “conspiracy,” and even there the nature of the manipulation involves different agenda rules than the standard amendment procedure used for the McKelvey result. The problem in my view with applying the McKelvey result to the real world is that the agenda length needed to move outcomes far away from the Pareto set under standard amendment procedure will, for real-world preference distributions, tend to be very very long, indeed (Feld et al., 1988; Feld, Grofman and Miller, 1989; Miller, Grofman and Feld, 1989). Also, moves back to the Pareto set will be hard to resist, since such moves can have overwhelming support.
26. Among these assumptions are that elections take place within a single constituency; that there is a single round election for any office; that the election chooses a single candidate; that the election is decided by a plurality vote; that candidate policy positions are well defined; that candidate policy positions are accurately estimated by each voter; that voters care only about the next election; that voters care only about which candidate/party will enact policies closest to the preferences of the voter and vote for the candidate closest to their own location; that, if there are no policy differences among the candidates/parties, then voters will be equally likely to support each of the candidates/parties; that candidates/parties accurately estimate the policy preferences of voters, or at minimum, they are at least able to identify the location of the median voter overall and the median voter in each party; that candidates are part of a unified party team; that parties/candidates care only about the next election; and that eligible voters go to the polls if the expected benefits of their vote’s contribution to the election of the candidate for whom they would choose to vote exceed the “costs” of voting.
27. People usually have multiple motives; for example, very few people pick their food solely on the basis of its expected impact on their longevity; yet if information came out that a given food was dangerous to one’s health, at the margin, we would expect that information to matter for consumption choice.
28. I take a similar “comparative statics” perspective vis a vis related issues raised by Olson and others in terms of the provision of public goods.
29. This claim is elaborated in Grofman, 1993b; Grofman, 1996; Hanks and Grofman, 1998.
30. We will, however, note that, if we treat all European post WWII democracies as if their politics were unidimensional and consider the best-fitting array of parties on that single dimension, the simple prediction that the median party will be in the governing coalition works quite well, and if it is also the largest party it’s likelihood of being in the governing coalition is above 80%. See the excellent review in Laver and Schofield (1990).
31. We will not try to review that extensive literature here.
32. Many years later, at Gordon’s urging, I wrote a long intellectual homage to Black, (Grofman, 1981).
33. 1969 is also the first year I attended the annual meeting of the Public Choice Society.

34. Since that first article, I have published over a dozen articles and research notes in *Public Choice* (along with a couple of hundred other things).
35. The latter was the first appearance in print of work under the authorship of someone who was to become my long time research assistant and collaborator, A Wuffle – examining the importance for one’s vote totals of being a candidate with a middle initial (Wuffle, 1972). Wuffle and I have gone on to co-found the California (Drive-In) Church of the Incurrigibly Eclectic and to become the exponents of the “reasonable choice” alternative to rational choice modeling (Wuffle, 1999).
36. Many of you reading this are probably not old enough to know what a ditto master is; so think of it as inferior carbon paper – if you are old enough to know what carbon paper is.
37. In part to honor my debts, both professional and intellectual, to both Gordon and Jim, in 1988 I co-organized with Donald Wittman a Liberty Fund conference that looked at the lasting impact of *Calculus of Consent*. I am proud, too, that during my term as president of the Public Choice Society I could invite Jim and Gordon to give presentations commemorating the 40th anniversary of the publication of *Calculus of Consent* at the annual meeting in San Diego.
38. Many became my friends; at least one chose to become an enemy.
39. A third most important influence on my research agenda has been Douglas Rae (Rae, 1967, 1971). While Rae’s work is not counted in the usual Public Choice canon, it should be, and with this essay (see above) I have done my best to rectify that omission. Directly acknowledging Rae’s influence, in 1986 I co-edited a volume with Arend Lijphart entitled *Electoral Laws and Their Political Consequences* (Grofman and Lijphart, 1986). Since then, *i.e.*, I have been involved in organizing a series of conferences (and conference volumes) on each of the world’s most important electoral methods, including list PR, STV, the limited vote, and mixed member systems, with a fifth and final conference being planned for 2004 on plurality and runoff elections.
40. Papers from that conference were included in two volumes: one edited by me, *Information, Participation and Choice: ‘An Economic Theory of Democracy’ in Perspective* (Grofman, 1993a), and one edited by Professor Kristen Monroe, *The Limits of Rationality* (Monroe, 1991).
41. Indeed, in my talk I referred to Public Choice as having been both “seduced and abandoned” by Downs.
42. Here, Berlin is reformulating insights of the Greek philosopher, Archilochus. Archilochus divided thinkers into two types: *hedgehogs* and *foxes*. Foxes knew “many things;” hedgehogs had but “one or at most a few ideas,” but those were really big ones.
43. There are two types of people in the world: those who divide things into threes and those who do not. I am of the former persuasion. I divide thinkers into *popcorn machines*, *cuisinarts*, and *waffle irons*. The first type of scholar, the *popcorn machine*, generates lots of ideas, no single one of which amounts to very much, but which cumulatively might, if one is lucky, matter. *Waffle iron scholars* are ones who leave an indelible mark on the history of ideas; any intellectual endeavor they touch is radically transformed. The *cuisinart scholar*, on the other hand, turns anything he or she touches into intellectual mush; their work, having no distinguishable content, leaves no permanent trace.
44. Cf. “If wishes were horses, Californians would drive very powerful cars” (A Wuffle, April 1, 1976).
45. As an expert witness my slogan has been: “Speak slowly and carry a big vita.” (cf. Wuffle, 1984). One of my colleagues once claimed that I wrote articles faster than most other people could read them.

46. I've been to at least two-thirds of the Public Choice meetings. In particular, I've never missed any of the meetings held in New Orleans.

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Appendix: Public choice: Some intellectual debts

My connections to the Public Choice Society run long and deep, and my intellectual indebtednesses to a number of its founding figures even deeper.

Duncan Black, then a visiting Fellow in Law and Economics at the University of Chicago, was the external reader of my 1968 Master's thesis in political science. Perhaps more than any other single figure, I have taken Black as my source of inspiration – with much of my subsequent work in Public Choice dealing with questions and topics raised in the *Theory of Committees and Elections*, such as the empirical prevalence of single-peaked preferences, conditions for cycles, the Condorcet jury theorem, strategic voting, and, most generally, the impact of rules, especially electoral system rules, on outcomes.³¹

My first article, a version of that MA Thesis, was submitted to *Public Choice*. The first draft was still deeply flawed but, rather than rejecting the paper out of hand, knowing that I was still just a beginning graduate student, Gordon Tullock, then sole editor of the journal, strongly encouraged me to revise and resubmit. Indeed, he allowed the article to go through three rounds of revise and resubmit before running out of patience – and accepting it for appearance in *Public Choice* in 1969.³²

Gordon has said about altruism that it's not that the concept is meaningless, it's just that there isn't very much of it around. Well, Gordon's kindness to a graduate student whom he'd never met is not something that I've ever forgotten, even though there probably is some theorem of Gary Becker that can be used to explain it away as entirely self-interested behavior: the "rotten Gordon" theorem. Having an article published in *Public Choice* before I finished my degree helped me land my first political science job in 1970 (at SUNY Stony Brook).³³

Gordon's helpfulness to me did not end in 1969. In 1969 Gordon had been commissioned by the American Political Science Association to analyze the data on the Association's own elections – contested by members of a group of political scientists who were campaigning make political science more "relevant," and most of whose members were opposed to the war in Vietnam. Gordon recommended me as his successor to do the analyses of the 1970 APSA election. This gave me access to raw ballot data, and allowed me to do one of the first election studies doing computer analysis of actual individual ballots, including a study of the uniqueness of ballot patterns. One article and one research note in *PS* came of this.³⁴

I first met Bill Riker in 1970, when I was applying for a job at the University of Rochester. Bill took me out to lunch at a hamburger joint in Rochester that had very good French fries. I didn't get the job, but I did get the chance to get to know Bill. Ever since, I tell my own students that every job interview, even the unsuccessful ones, is a chance to make friends who can be friends for life. But even though Bill did not give me a job, not too long afterward, indirectly he got me an invitation to what turned out to be unquestionably the single most important conference of my intellectual life.

In 1971 Jim Coleman organized a week-long conference at Hilton Head, South Carolina, funded by NSF, that brought together most of the major Public Choice scholars of the time (including Gordon, Jim Buchanan, and Mancur Olson) as well a

number of people, most then still assistant professors, who would go on to become major figures in the field. This was an “invitation only” conference (about 30 people), and, at the suggestion of Bob Axelrod, whom I knew from my undergraduate days at the University of Chicago (where we had each majored in mathematics, with a minor in Student Government), I had applied to attend. Initially turned down, because of a cancellation, I got to go to the Coleman conference at the last moment – as a graduate student my travel plans could be quite flexible. The no-show, I later learned, was Bill Riker.

At Coleman’s conference we each presented a paper of our own (mine on iterated prisoner’s dilemma games, my Ph.D. thesis topic), and we each commented on another paper. As a graduate student I couldn’t afford good reproduction methods, so my own paper was dittoed – which made the mathematical formulas in it pretty much unreadable.³⁵ Thus, my discussant had little negative to say about it – indeed, little to say about it, period. By the luck of the draw (or perhaps, because I was replacing Bill Riker at the last moment) the paper I got to comment on was the one by Jim Buchanan. Undeterred by the fame of the paper giver – although still without a Ph.D., even then I was not known for my shyness – I proceeded to give what I vaguely remember as rather critical remarks. Jim, in turn, however, was very kind. His major complaint was not about the substance of my discussant remarks, but about the fact that my remarks were given while walking rapidly back and forth in front of the lectern. Jim said to me: “Don’t just say something, stand there.”³⁶

At Hilton Head I got to know a lot of people whom I have continued to interact with and learn from.³⁷ Because the Hilton Island conference was so important to my own professional development, in memory of my own good fortune at being invited, at any conference I run, I try to make it a point to make sure that there are at least a couple of very junior folks who get invited.

After Black’s *Theory of Committees and Elections*, it is *An Economic Theory of Democracy* that has had the greatest impact on my work.³⁸ However, while I met people like Duncan Black, Bill Riker, Jim Buchanan, Gordon Tullock, Mancur Olson and Jim Coleman while I was still a graduate student, and I met Ken Arrow in 1980, I didn’t get to meet Tony Downs until relatively late in my career – at a conference I co-organized with Kristen Monroe in his honor in 1990.³⁹ I had decided to run this conference because I thought that was my best chance to actually meet Downs, who had turned his intellectual energies on topics far away from his seminal dissertation on models of party competition and voter choice.⁴⁰

As a young academic I wanted to do the kind of work that would allow me to grow up to become president (of the Public Choice Society). But it wasn’t clear that that would ever happen. Most presidents of the Society have been figures who reshaped their discipline. In the typology made famous by Isaiah Berlin, they are hedgehogs, not foxes.⁴¹ Or to put it another way (my way), they are waffle irons, not popcorn machines, and certainly not cuisinarts.⁴² While I sometimes wish I were a waffle iron,⁴³ and I believe quite strongly that I am not a cuisinart, about one thing, I can’t kid myself: “Grofman, thy name is popcorn.”⁴⁴

Moreover, there is something quite mysterious about the Public Choice Society’s election processes for its president – which make it hard to see exactly how one

is supposed to grow up to become its president. Once upon a time the Society had a “show democracy” rather like what Mexico had throughout almost all the past century. I suspect most readers are familiar with how this worked in Mexico: the President *ante*, El Maximo Supremo, chooses El Maximo Supremo *post*, and his choice was rubberstamped by the ruling party, the PRI, and then the new El Maximo Supremo is elected without “serious opposition” – as determined by the PRI loyalists who were handling the ballot counting/ballot stuffing ritual.

But the Public Choice Society has done the PRI one better. For well over a decade we have eliminated even that vestigial appearance of democracy, the Society’s business meeting. The Society has obviously taken very seriously Ken Arrow’s argument that any reasonable procedure for generating social choice is flawed – unless we wish to accept a dictator. Confronted with the horns of that dilemma, the potential for intransitive social choice vs. dictatorship, we have seized one – we have opted for dictatorship.

A natural question, then, is “How did I become President of the Public Choice Society?” I attribute this event to three things: location, location and location. First, I was located in the right discipline: namely political science. I am an “affirmative action baby,” i.e., a beneficiary of the “rule” (sic!) that the Presidency of the Society, shall, roughly speaking, rotate between economists and political scientists – even though the latter are only a minority of the Society’s membership. Second, I was located at the right age – a stage of life I think of as “past one’s salad days,” but still young enough to look forward to dessert. (My first article was published in 1969, more than thirty years ago – of course, my present mental image of self is of someone barely old enough to have been *born* in 1969.) Third, while some other political scientists involved in the early days of the Society rarely ever attend these days, wherever Public Choice Society meeting have been held over the past thirty or so years, there generally you have found me.⁴⁵ Clearly, Jim and Gordon and other past presidents could see that, as far as the Society was concerned, I had, indeed, “paid my dues” – quite literally. Relatedly, if I were to become president it was very likely that I’d actually be at the next meeting. So, when Bill Niskanen, whom I’d never met, sent me a two-line e-mail asking me whether I would like to be president, I accepted.