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Public Law and Public Choice: Critique and Rapprochement

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Introduction

Public choice theory is a theory of how government works and therefore of how public law is made and applied. But it is not a theory like the general theory of the second best, that is, a single proposition that articulates a general truth. It is better understood as a field whose practitioners share some general commitments and whose boundaries remain fuzzy. Public choice is a part of the more general fields of law and economics or of political science. It has affinities and overlaps with fields that use different labels, such as, “social choice” and it is difficult to distinguish from the work of those who style themselves as contributors to positive political theory, but who would not necessarily identify themselves as public choice theorists.

For purposes of this chapter I will not attempt to distinguish carefully among Public Choice Theory (PCT), Positive Political Theory (PPT) and Social (or Collective) Choice Theory (SCT). For, they all share a basic assumption: that political actors – the individuals, groups, and politico-legal institutions that make public law – act on the basis of rational self-interest. And they all seek to understand what types of decisions and legal arrangements will emerge from the interactions of individual or organizational preferences and particular institutional arrangements for decision making. This is a micro-level form of analysis that does not rely on descriptions or predictive hypotheses employing broader sociological constructs such as class interest or ideology.

Social choice theorists (SCT) tend to focus on an important, but relatively isolated, aspect of the overall governance problem – what type of voting arrangements, if any, can make collective decisions accurately reflect the preferences of individuals, whether those individuals are voting in general elections, committees, legislatures, clubs or whatever. PCT and PPT

scholars address a broader set of topics. They seek to explain the behavior of voters, legislators, administrators and courts, not just the means by which individual preferences can be aggregated effectively within various institutions for collective choice. This interest in multiple lawmakers and their interactions gives a strategic or game-theoretic cast to PCT and PPT accounts.

Public choice theory and positive political theory are distinguished from each other largely by their differing normative commitments. PCT practitioners tend to be committed to norms of strong efficiency (the Paeto principle) and individual liberty. Because collective choice often fails to honor either or both of these values, PCP analyses, certainly as explicated in the foundational works in the field, have a strongly critical tone. Much collective action is described as “rent seeking”, meaning the use of collective means to make inefficient transfers from more politically powerful to less politically powerful groups. By contrast PPT tends to exalt democratic choice. PPT scholars focus on the means by which the decisions of majoritarian institutions – particularly legislatures – can be made and implemented effectively given the necessity for division of labor, both within the legislature and between the legislature and implementing institutions – agencies and courts. (McNollgast, 2007).

These differences in institutional focus and normative commitments are hardly trivial, particularly from the internal perspective of practitioners within these various fields. But in surveying their contributions to the understanding of public law, and the criticisms of those contributions, we will only occasionally need to distinguish one school from the other. Hence, for convenience, I will generally lump them all together as Public Choice Theory (PCT).

The unruliness of Public Choice Theory (PCT) is mirrored in the critiques of its propositions. Some critiques are from within the core of the field, and tend to challenge particular findings or propositions on evidentiary or methodological grounds. Other critics, often

from mainstream political science or the legal academy, present more fundamental attacks, claims that PCT's basic assumptions are false or that its empirical hypotheses are inherently untestable.

Surveying and critically examining the literature on PCT must somehow bring some order to both the content of the field under discussion and the critical perspectives on its achievements – or failures. This chapter addresses that challenging task in a stepwise fashion. First, it will provide a basic overview of some of the foundational public choice literature and a brief sampling of critical commentary on the PCT claims and approaches. The discussion will then turn to a more detailed view of the critical literature as it relates to various sub-fields of public choice inquiry. Much of PCT scholarship is fragmented by particularistic focus on a single institution – elections, legislators, administrative agencies or courts – or on the interactions between diads of institutional actors (voters and legislatures, legislatures and agencies, etc.) The critical commentary often mirrors that fragmentation and this discussion will track that (dis) organization. Finally the chapter concludes with an attempt to put PCT's claims and its critics' complaints into a more balanced perspective than a reading of the PCT literature or its critics alone might suggest.

I. Public Choice: A Brief Overview

As has been noted, the crucial unifying thread in public choice theory is the assumption that all actors in political life – voters, interest groups, representatives, legislative committees, bureaucrats or courts – behave rationally to maximize or optimize some objective function (wealth, status, power). Governmental institutions and activities are thus to be explained as the outcome of the rational pursuit of individual or group ends. Because this approach emphasizes the self-interested motivations of political actors, it shares fundamental behavioral premises with

microeconomics, and it is sometimes described as the application of economic analysis to politics. (Mashaw, 1997).

This body of theory is “positive” in the sense that it aspires to make predictions about the form that institutions or collective action will take or the expected outputs of collective action given particular organizational forms. Public choice analysts do not explicitly assert that rational persons would value only wealth, or anything else. The underlying presupposition of rational actors pursuing their own “self interest,” however, may easily be interpreted as suggesting that the normative rhetoric of much political action – and of conventional legal discourse – is only so much window-dressing obscuring the underlying pursuit of private satisfaction. Positive political theory thus has a “realist” flavor similar to the explanations of various critical realists, but without engaging questions of ideology or the dominance of particular social groups.

To see the “unmasking” side of PCT in stark relief we might contrast it with an idealized version of the democratic-bureaucratic process. A naive idealistic vision of lawmaking in the U.S. democratic system might say something like this: Under conditions of majority rule statutes express the desires of a majority of citizens. Bureaucratic organizations are designed to assure that administrators carry out the citizens' desires as expressed in statutes. Administrative law, including judicial review of administrative action, is that body of law that protects the citizenry against the failure of administrators to carry out the statutory policies put in their charge and against unfairness in the treatment of any person or group in further specifying or applying those policies. Thus are elections, legislatures, agencies and courts harnessed to the task of majoritarian democratic governance.

Now, almost everyone would immediately object that U.S. democracy is “representative democracy” and that therefore the laws passed by representative assemblies should not be

imagined to reflect perfectly the desires of the citizenry. Nevertheless, in the idealist account this “defect” is assumed to be ameliorated by the dynamics of the system. Over time the voters can replace legislators whose preferences do not reflect those of their constituents. Similarly, everyone recognizes some slack in the system of statutory instructions for administrators. But, here again, congressional oversight, congressional budgetary authority, and the opportunity for statutory amendment, together with judicial policing of particular actions for irrationality and lack of authority, combine in the idealist account to keep bureaucratic organizations within the pathways marked out by the people's representatives.

The basic implication of much of early PCT scholarship is that the benign governmental process just described does not exist and is impossible to construct. The notion that voters’ preferences control legislative action or that legislative action controls bureaucratic decision making is, in this view, a fundamental misdescription of U.S. government. In fact, PCT seems to say, there is no demonstrable relationship between voter or even representative preferences and the outcomes of the legislative process. Rather, the most plausible view of both representative assemblies and bureaucracies is that they satisfy the demands of certain special interest groups, including the interests of legislators and bureaucrats themselves.

In the PCT account the initial and fundamental difficulty with the story of majority rule is the presupposition that citizens pay attention to governance. In one of the foundational texts of PCT, *THE CALCULUS OF CONSENT, LOGICAL FOUNDATIONS OF CONSTITUTIONAL DEMOCRACY* (1962), James Buchanan and Gordon Tullock present a cogent argument instead for rational voter apathy. Any citizen who calculated the likelihood that his or her vote would be determinative in the election of a legislative representative, discounted by the probability that the representative would in fact correctly express the citizen's preferences with respect to legislation

that came before the assembly and that that representative's position would be determinative, would conclude that voting is useless. The *de minimus* expected value of casting a vote is dwarfed by the combined costs of obtaining sufficient information to determine how to vote and of voting itself. Moreover, even if we were to assume that voters were conscientious and well informed about the actions of their representatives – or could exclude representatives entirely by choosing policy directly – we could have no confidence that the policies chosen actually represented the preferences of the electorate. That at least is the implication of another foundational text, Kenneth Arrow's SOCIAL CHOICE AND INDIVIDUAL VALUES (1963).

The so-called “Arrow Impossibility Theorem” will be elaborated further below. The basic result is simple enough: so long as there are no constraints on what preferences can be expressed or the order of voting, and voters vote rationally and sincerely, collective action is likely to cycle endlessly among alternatives. And, any set of voting rules that makes collective action decisive must constrain the collective decision process in ways that make it impossible to conclude that collective action accurately reflects underlying individual preferences. Moreover, since Arrow's initial article on the subject in 1950, he and others have demonstrated that this “paradox of voting” (discovered initially by the Marquis de Condorcet in the late 18th century) may apply to a large number of the run-of-the-mill issues that come before electorates or assemblies. (Mueller 1979; Riker 1982 Sen 1970, 1977)

Of course, however useless it may be, many voters do vote. And, however unknowable the relationship between majority rule outcomes and the underlying preferences of the electorate, people do engage in political action *as though* their demands might influence what public policies are chosen. How are we to understand the political behavior that we observe?

This strand of positive political theory owes much to another foundational text: Mancur Olson's *THE LOGIC OF COLLECTIVE ACTION* (1965). Observing that people do engage in politics, particularly as members of pressure groups demanding particular policies or statutes, Olson asked what the determinants of political mobilization would be for rational political actors. The unsurprising answer is that people will engage in politics when the expected returns from that effort exceed its cost. And, while this might rarely be true for any single act of voting, where politics permit individuals or groups to demand specific policies, they may find that the benefits of convincing legislators to take favorable action exceed the costs of political mobilization.

Olson, however, recognized another problem with political mobilization. If A and B both want and will benefit from the same policy, each has a strong incentive to shift the costs of political mobilization to the other. Since most policies that favor a group do so in a way that makes it impossible to prevent the benefits from accruing to all members, including those who remained on the sidelines during the (sometimes expensive) campaign for their enactment, all potential political action groups are beset by a “free rider” problem. The crucially important question then is, “Which groups will form and engage in political action?” Or to put it in other terms, “How can the free rider problem be solved for groups who would be better off if they could all act together to pursue policies that benefit the group?”

Olson predicts that solutions will emerge in two basic situations. In one sort of case groups need not actually form to lobby for a policy because benefits to a single member more than offset the full costs of the political action necessary to secure its adoption. This “group member” might be a quasi-monopolistic producer or even a politician who hopes to make a career out of a particular issue. In the other prototypical situation, groups solve their organizational difficulties through some device for penalizing members who fail to pay their fair

share of the costs of political action. The most straightforward situation is one in which the group already provides selective benefits in forms such as technical information, professional prestige, group health benefits, or the like. Receipt of these goods and services can often be “tied” to support of lobbying activity in ways, such as dues, that prevent members from receiving the first without contributing to the second.

On Olson’s account group formation, and hence political activity, will be dominated by interest groups which either need not organize or which can solve the “free rider” problem by the selective provision of benefits. Large groups, those whose members will normally have individually small stakes in a particular issue, will find it difficult to overcome the impediments to effective political action. By contrast, monopolistic or quasi-monopolistic enterprises that can capture most of the benefits from particular public actions, or producer and professional groups that have already formed for other purposes, have a major advantage in pluralist politics.

Olson’s analysis describes only the “demand” side of the legislative equation. Why should legislators respond to attempts by special interests to feather their nests at the expense of the general public? To answer this question public choice theory must identify an “objective function” for legislators. According to PCT, the legislator’s objective function is reelection (Mayhew 1974, Fiorina 1977). He or she responds to interest group demands based on the expected benefits of that response in retaining office. Legislators will supply legislation to interest groups which demand it when doing so helps the legislator – or a majority of them – gain or stay in office.

The trick in predicting what legislation will emerge is to be able to describe how interest group demand matches up with legislative supply. Michael Hayes, in *LOBBYISTS AND LEGISLATORS: A THEORY OF POLITICAL MARKETS* (1981), develops a general “transactional

model” of the legislative process. In doing so he draws on a taxonomy of legislative politics first developed by James Q. Wilson in his work, *POLITICAL ORGANIZATIONS* (1973).

The results of Hayes’ transactional analysis are not very encouraging. It tells us to expect that policies having widely distributed benefits and costs will not be adopted even if they would substantially improve general welfare. No one will organize effectively to demand or supply them. (Climate change legislation, perhaps?) By contrast, where policies offer concentrated benefits whose costs can be distributed to an unorganized and apathetic public (e.g., pork barrel projects), the political exchange markets should generate a large number of transactions. Perhaps even more depressing, where broad public benefits are achievable through the imposition of costs on an identifiable number of enterprises or individuals we should expect that the legislature will either fail to act or will delegate the imposition of these costs to an administrative agency. And by later denying the agency the necessary resources to get the job done, legislators may protect well-organized interests from most of the costs and simultaneously blame the bureaucrats for failing to carry out the ringing mandate embodied in the statute (e.g., environmental regulation). Similar behavior should be expected where benefits and costs are both concentrated (e.g., labor legislation).

It is at this point in the positive account of lawmaking that administrators make an appearance. But, they have not been overlooked elsewhere in positive political theory. In particular, William Niskanen developed a theory of bureaucratic action that does for agencies what Mayhew and Fiorina did for legislators, that is, described an objective function for bureaus and explained how that objective function might be used to predict administrative behavior. (Niskanen, 1971).

In Niskanen's models, agencies have a straightforward goal: to maximize the size of their budgets. These budgets are obtained from a Congress which has sole possession of the keys to the federal treasury. Nevertheless, agencies are not without weapons. In particular, they may have a virtual monopoly over information about the effects of particular policies or over the relationship between bureaucratic inputs and policy outcomes. The result, according to Niskanen, is a process of monopolistic bargaining between Congress (or the relevant committees) and the agencies concerning the size of the bureaucratic budget. The outcome of this bargaining is predictable – the steady growth of agency budgets over time. Not only are agencies themselves well-organized interest groups that can provide favors to legislators in return for increases in appropriations, legislators are not by inclination budget cutters. Because the existence of administrative agencies is explicable, in PCT, in terms of their blame-shifting electoral advantages for legislators, and because appropriations subcommittees can be expected to be made up of members most benefited by the activities of agencies whose budgets they oversee, administrators will be bargaining with highly sympathetic legislators.

Later PCT Scholarship acknowledges a number of difficulties with this depiction of compliant legislators and accommodating agencies supported, of course, by demanding interest groups – the so-called “iron triangle.” The agencies' monopoly over information may mean that it is difficult for legislators to determine whether the agencies' activities are actually beneficial to their reelection. Monitoring agency activities and penalizing them for poor performance is costly for legislators, and it gives rise to a problem of “agency costs,” an issue that has been extensively developed in the literature on private firms. To put the matter concretely, the problem is to discover how principals can, at least cost, insure that their agents are serving the principal's interests rather than pursuing interests of their own.

Reaching beyond the Niskanen tradition, one group of positive political theorists speaks directly to this question and provides an answer that purports to explain the general structure of administrative law. According to Matthew McCubbins, Roger Noll and Barry Weingast (who often write under the pseudonym “McNollgast”), legislators attempt to solve their agency cost problems by the construction of administrative procedures, or more generally, administrative law. (McNollgast 1987, 1989). Although complexly argued, the “McNollGast” thesis is straightforward. Legislators who vote to create programs, at least hypothetically, want administrators to carry out their statutory instructions. Because monitoring of administrators is costly, legislators may empower others (citizens, firms and courts) to monitor administrators for them. Their desire to empower others to monitor administrators to protect original legislative bargains, according to this view, explains many of administrative law’s demands for transparency, open and regular procedures, and judicial review.

Legislatures define administrative procedure, that is they define specific beneficiary rights, prescribe processes through which those rights may be defended, and set up external institutions – particularly judicial review – to protect beneficiary-monitors’ interests. In so doing legislators protect their own interests in maintaining the bargains they have struck and maintain the political support of those with whom they have transacted. From this perspective, the normative emphasis in conventional legal discourse on the protection of individual rights or the pluralistic values of participation in administrative processes is translated into the positive political vocabulary of “monitoring”, “agency costs”, and “sanctioning” techniques. Rather than the elaboration of some fundamental commitment to liberal or pluralist democracy, as in the idealists’ story, the hallmark features of U.S. administrative process are explicable as the creations of legislators bent on controlling their agents at the least cost to themselves.

Note, however, that in the McNollgast story pessimistic predictions about democratic politics are not necessarily implied. Legislative control supports majoritarian outcomes, even if legislators are motivated exclusively by crass concerns for re-election.

As this brief overview suggests, PCT is composed of a nested set of interrelated hypotheses. While no theorist claims to offer a complete, positive view of governance in all its many guises, the insights of different theorists can be fit together to provide an overall picture. Yet, the resulting portrait is one that many find unpersuasive. Indeed, positive political theory has been attacked root and branch, perhaps stem, leaf, and tendril. (E.g., Farber and Frickey 1987; Kelmon 1988 Panning 1985; Alexander 2002).

At the most fundamental level critics object that the positivists have completely misunderstood what politics and political action are about. Rather than attempts by individuals to maximize previously determined private values, neo-republican theorists argue that politics is primarily about the collective definition of public values. (Maas 1983; Sunstein 1985, 1986) From this perspective the activities that positive political theorists classify as costs – becoming informed about public issues, organizing groups to act on the basis of collective beliefs and the like – are really benefits that people seek by engaging in political action. (Dorf and Sabel 1998).

Other critics argue that the predictions of positive political theory are falsified more often than proved, and that PCT empiricism is addicted to *post hoc ergo propter hoc* explanation. (Green and Shapiro, 1994; Kelman, 1988). Categories of statutes that are not supposed to exist, e.g., health and safety regulatory regimes providing diffuse benefits and imposing concentrated costs, proliferate. And, empirical investigations of the voting behavior of legislators find that ideology – a legislator's value commitments – explains as much as, or more than, the material interests of those whose support is needed for reelection. (Kalt and Zupan 1990).

Predictive incompetence is not a happy feature for a “positive” theory. Worse yet, these modern realists are found to be naive about electioneering and the political organization of the Congress. They appear to have missed the fact that legislators may solidify electoral support through false favors that have no effect on legislative output. (Fiorina & Noll 1978). And, the presumption that committee membership will be unrepresentative of the Congress is argued not only to be false, but a violation of what a more sophisticated rational choice perspective on congressional organization should predict. (Krehbiel 1991). Some public choice scholars even propose that the public choice perspective supports the current structure of the administrative state as a plausible device for pursuing something like the “public interest”. (Spence 2000, Spence & Cross 2002).

Indeed, the circumstance that public choice theory has been developed in fits and starts by many minds with many interests provides fertile opportunity for pointing out that positive theorist B has forgotten about the findings of positive theorist A. Interest group theorists, for example, often seem to forget the serious monitoring cost problems that are the principal preoccupations of agency theorists. (Mashaw 1985). Meanwhile the latter may be so preoccupied with ferreting out how politicians solve the “agency” problem that they forget to ask whether they would want it solved in any event. (Mashaw 1989).

II Critiquing Public Choice: A More Detailed Account

A. Voters, Elections and Market for Legislation

Democratic governance presumes that voters preferences for candidates determine the composition of law making institutions and that the output of those institutions reflects the underlying policy preferences of the electorate. As noted above, no one seriously believes that

there is no slack in the system – that is, that legislation perfectly reflects voters’ preferences. But, public choice theory can be interpreted to question whether the legal output of legislative institutions has even a reasonable chance of reflecting voters' real preferences. The efficiency of democratic institutions is subject to two general lines of PCT attack: one from what I will term “voting theory”; the other from “interest group theory”.

Voting Theory. Much of what voting theorists do is wonderfully abstract and abstruse. Nevertheless, these axiomatically derived propositions challenge some of our most cherished beliefs. The central proof of axiomatic social choice theory, what is generally called the “Arrow Impossibility Theorem,” suggests that majority-rule determination of social preferences must be either chaotic or illusory.

The Arrow Theorem generalizes an eighteenth-century proof called Condorcet’s Paradox (Condorcet 1785). Imagine for simplicity that three voters A, B and C are asked to vote on their policies X, Y, and Z. A prefers X to Y to Z; B prefers Y to Z to X; and C prefers Z to X to Y. These preference orderings can be depicted tabularly as follows:

Table I.I. Condorcet’s Voting Paradox

Voter	Preference Ordering (N)		
A	X	Y	Z
B	Y	Z	X
C	Z	X	Y

Obviously, no policy has a majority of first-place votes. Moreover, these preferences result in a “vicious” cycle when voted on in pairs. When X is paired against Y, X wins (A and C versus B). When X is paired against Z, Z wins (B and C versus A). When Z is paired against Y, Y wins (A and B versus C). that is, X beats Y, which beats Z, which beats X.

Ordinary voting routines simply will not choose a most preferred alternative. Indeed, the startling generality of Arrow's proof is that no voting rule which allows voters to express their true preference and which treats each preference as equally decisive can assure us that it will produce a single preferred choice for three or more voters who have at least three alternatives. In short, majority-rule systems may produce indeterminate, shifting, and therefore chaotic outcomes even if applied to small groups like committees, not to mention legislatures or the whole electorate.

As casual empiricists, we do not often observe the cycling phenomenon in operation. Elections routinely produce winners and representative assemblies make decisions that are not immediately undone by subsequent votes. Does this mean that the Arrow Theorem is either incorrect or irrelevant? Not at all. In fact, the absence of cycling gives the theory much of its normative interest. Stability can be achieved by relaxing any of the "conditions" that underlie the proof. We can give someone the power to constrain the alternatives or to determine the order of expressing preferences; we can give someone an extra vote; or we can allow false or strategic voting. But, of course, none of these choices is particularly attractive. Thus, the Arrow Theorem might be recharacterized as telling us that whenever we find stable choices, we cannot know that stability has not been bought at the price of unfairness in the underlying voting process. Our choice seems to be between incoherence (cycling) and some form of unfairness.

Most voting systems, whether governing elections or other types of choice, use some set of institutional devices to constrain choice, or else they explicitly abandon majority rule. In most elections, for example, we vote for one of two candidates and the winner is the candidate who obtains more than 50 percent of the votes cast. In multiple-candidate elections, various techniques may be used to produce a final majority winner. Run-off elections between the top

two candidates are common. So is the Electoral College winner-take-all system for American presidential elections – a system that generally produces a clear majority winner by marginalizing serious third-party candidates. It, of course, also sometimes permits a candidate to prevail without garnering even a plurality of the popular vote.

But note what these devices say about majority-rule elections. Majoritarian elections depend on getting the choices down to two. And who is elected depends critically on how the final list of two candidates is generated. It is, thus, intuitively obvious that if anyone had the authority to specify who the two candidates would be, they would thereby gain the power to determine who would be elected. It is just an extension of this obvious point to realize that it is the institutional machinery for setting the agenda, here a slate of candidates chosen by committees, conventions, caucuses, or primaries, that ultimately determines the outcome of the election. Or, to put the matter back in terms of our formal example, should X beat Y, we have no assurances that he or she would not have lost to Z, who would have been beaten by Y. The outcome may be as much a product of the order of voting, or some other aspect of agenda setting, as it is a product of the underlying preferences of the voters.

In some sense this result is quite familiar. We all have had the experience of seeing our favorite candidate disappear from the available choices before the general election, just as we have seen our favorite proposals disappear in committee or family discussions. We have also sometimes harbored the belief that our candidates or ideas could have beaten the ultimately victorious ones if they somehow could have been resuscitated before the final vote. (How did we end up at Disney World again when the original proposals had been Paris or the Bahamas?) But, of course, the election or committee rules (and often the unwritten rules of family discussion) prevented going back to the beginning and starting the whole process over. Recognition that the

agenda matters – indeed, is crucial in majoritarian processes – also undergirds our common understanding that a legislative committee chair is a powerful position. And it fuels the fires of democratic unrest when we believe that the chair is “biased” or unrepresentative.

To be sure, agenda influence can be avoided, but usually at significant cost. Majority rule without a constrained agenda – that is, a binary choice – usually will produce no decision at all. So says the Arrow Theorem, and so says our ordinary experience as well. Unless the choices can be narrowed in some way, discussion will be endless. Possible candidates and possible policies are a dime a dozen, and there is an old rule of thumb in faculty politics that says that if three proposals can be got on the table, nothing will ever be done.

Majority rule itself can be abandoned in favor of plurality voting or proportional representation. In such systems decisions will be made no matter how many alternatives are presented. And they have the “democratic” advantage that everyone's candidate or proposal can be put before the voters. But proportional voting is useful only in elections, not in policy choices; and in elections it holds out the ever-present prospect of splintered and ineffectual governance. Plurality voting, by contrast, permits governance by a minority candidate or party, or selection of a minority-backed program; pluralities can pick “winners” who would have lost to one or many others in a head-to-head majority-rule vote. Hence democrats cling to majority rule notwithstanding the sense that the majority is not quite ruling.

Arrow's general results have proven very sturdy in the face of attempts to disprove them axiomatically, that is, by attacking the internal logic of the theory. Alternative voting theories, such as those that find that election outcomes will accurately reflect the preferences of the “median voter”, (Downs 1957, Black 1958) must resort to the heroic and unrealistic assumptions that candidates and voters are arrayed along a one dimensional policy space and that all voters

preferences are “single-peaked.” (The latter assumption means that each voter has a most-preferred spot on the policy continuum and that his/her preferences for other positions are inversely proportional to their distance from this most preferred point.)

But, of course, legislators will be faced with many policy choices which can be described in terms of multiple dimensions (moral, economic, partisan- political, etc.). And voters' preferences need not be single-peaked. I may prefer either a free market in medical insurance or a governmentally-sponsored single-payer system to the current mixed public-private arrangements, although along the issue dimension of “government involvement in insurance markets” the status quo is closer to both the “free market” and the “national-health insurance” results than they are to each other. Actual voting situations may better reflect Arrow’s assumptions than those that produce more cheerful median voter results. The question is what to make of this situation. The sturdiness of the Arrow results has been taken by some PCT scholars as a persuasive argument against strongly democratic institutions. In *Liberalism Against Populism: A Confrontation Between the Theory of Democracy and the Theory of Social Choice*, William Riker argues that voting theory demonstrates the superiority of what he calls liberalism – essentially, representative democracy with substantial guarantees of particular individual liberties – over populism or direct democracy (Riker). His logic is straight-forward: Populism is justified by an appeal to the fairness of majority-rule procedures that give each citizen an equal opportunity to express preferences and, therefore, presumably to determine collective decisions. However, because voting theory demonstrates that there is no necessary connection between the outcomes of majority-rule procedures and the underlying preferences of voters, the democratic control promised by populism is illusory. Procedural fairness leads to meaninglessness, not to the expression of the will of the majority.

In Riker's view, liberalism is not subject to the same objection. The basic value of liberalism is the protection of individual interests. This can be accomplished through a system of specific guarantees combined with periodic opportunities to dismiss public decisionmakers. For even if dismissal or retention of public officeholders has no necessary relationship to the underlying preferences of the members of society, it will nevertheless prevent the maintenance of stable tyranny. Thus, in Riker's analysis, social choice theory demonstrates that while "expressive" democracy is impossible, "defensive" democracy can nevertheless be justified.

But has Riker made out his basic claim? If liberalism can be justified as instrumental to the preservation of individual rights, then why can populism not be justified as instrumental to the citizens' sense of self-worth or collective identification, or some other value? Riker seems to assume that the unavailability of a proceduralist justification for either liberalism or populism is a decisive criticism only against the latter. But Riker gives no rationale for his failure to explore the possible instrumental justifications for populism. Nor is it clear why the arbitrariness of outcomes in populist schemes is so troubling. After all, Riker views with equanimity a voting system for displacing officeholders that may operate about as rationally as a plague or a terrorist attack – or that may be under the control of some tyrannical, but clever, agenda manipulator. And he fails to consider the possibilities for combining populism with constitutional restraints on plebiscitary power or the invasion of individual liberties.

Moreover, the chaotic or arbitrary features of voting systems with no institutional mechanisms beyond majority rule are ameliorated by adding some institutional structure. The outcomes of a democratic political process are not meaningless. They just must be interpreted as a function *both* of the underlying voter preferences and of the institutional mechanisms for preference expression. Now to be sure, the idea that outcomes are *interpretable* in terms of some

functional relationship between underlying preferences and existing institutional structures has little normative significance. We should still like to know what relationship the outcome has to underlying preferences or judgments and how the institutional structure shapes the outcome. For unless outcomes have some meaningful relationship to representatives' choices, or to an institutional structure for decisionmaking which can be justified on other grounds, we still seem to confront statute books that are full of nonrandom, but nevertheless arbitrary, statements.

The way out of this mess may be through more voting theory. For while voting theory says that anything is possible, it does not say that all things are equally possible. The likelihood that voting processes will produce chaotic results is directly related to the number of voters and the number of alternatives put before them. Institutional arrangements which constrain the number of decisionmakers and which reduce alternatives to a manageable number can produce coherent choice a very large proportion of the time. If we imagine, therefore, that the institutional structure of American government is one which delegates choice routinely to institutions such as congressional subcommittees or administrative agencies where small numbers and deliberations can produce coherence, there is no reason to imagine that the output of the public law process is chaotic. Meaning is to be found in the actions of those who exercise delegated power.

Hence, we might better describe American government not as representative democracy, but as a system of transitory institutional dictators. This may threaten the notion that statutes express the general will, but it does not challenge the meaningfulness of statutory language. Nor does "dictator" here have a particularly pejorative connotation. Our agenda setters and quasi-independent policy prescribers – presidents, congressional leaders and administrative agencies—are subject to multitudinous forms of public, political, and legal oversight. On this

interpretation the Arrovian voting theory tradition simply sets the stage for serious thought about the acceptable design of democratic institutions given their ever present possibilities for failure.

Moreover, a related line of research in voting theory explores the reasons for the observed coalescence of choice around a small number of alternatives. (Davis, Hinich & Ordeshook 1970) The basic logic of this position is straightforward: while the “anything is possible” proofs demonstrate that there are decision paths to any result, it is also demonstrable that widely preferred alternatives have many paths leading to them, while relatively nonpreferred alternatives have only a few. This suggests that repeated expressions of preference should demonstrate patterns coalescing on relatively preferred alternatives. There may still be cycles that are stopped only by some “arbitrary” feature of the institutional landscape, but those cycles are likely to be among alternatives all of which have significant support. (Faber & Frickey, 1991.) Hence, the claim of recent PPT scholars that voting theory establishes at least a weak form of legitimacy for majoritarian democratic processes. (McNollgast, 2007).

Interest Group Theory. Remember for a moment what the economic theory of political behavior predicts. First, voters have no rational basis for engaging in political activity. They should be ignorant about politics, apathetic about political issues, and nonparticipants in political life. Second, because groups are difficult to form and hold together, we should expect that few groups will form to pursue the “public interest.” Group cohesion is a function of small size and members’ individual economic benefits from joining and supporting the group. It should be easy for the automobile manufacturers to act in concert to pursue their political interests, but hard for automobile consumers to do so.

Third, legislators are thought to respond essentially to group demands. The groups are the ones capable of supplying both the money and the votes needed to maintain legislators in

office. Hence, we should expect to see the output of legislative assemblies sharply skewed in favor of those economic interests who form themselves effectively into political forces.

All of this, alas, seems vaguely familiar. Voter turnout is low, at least in the United States, and there are constant complaints about the impact of one or another lobby on the shape of national legislation. How narrow interest groups got what they wanted in the legislative arena is the staple of investigative press reporting. And because in our checked and balanced government an interest group needs to control only one institution (the House, the Senate, or the presidency) to block legislation or to derail implementation in the bureaucracy or the courts, it seems quiet reasonable to predict that even more public interest legislation may be blocked than private interest legislation is passed. We might further predict that, if passed, public interest legislation will be long on aspiration and short on effects.

Yet, this is not the whole story (Croley 2008). A more balanced view requires two connected inquiries: The first explores the conceptual ambiguities and predictive anomalies that should temper our enthusiasm for drawing very general conclusions from public choice premises. The second reevaluates the utility of public choice ideas when deployed in a more tentative and discriminating fashion.

There are in fact a number of good reasons to reject some of the more general implications of the interest group theory of legislation. The first is that the theory makes obvious predictive errors. Interest group theories of regulation have had great difficulty, for example, accommodating the undeniable political movement toward deregulation. The existence and later development of a regulatory regime like that administered by the Civil Aeronautics Board fit happily within the basic capture theory of regulation. A small group of companies dominated a regulated industry, competed hardly at all on price, and prevented the certification of all new

carriers on national routes for over thirty years. The result was high wages, high profits, and an easy life for the industry; high prices and limited choices for consumers. But then, strangely, the CAB and the Congress, who were supposed to be in the pockets of the special interests, shut the whole cozy system down. The result has been fierce (even ruinous) competition, expanded service, new entrants, and dramatically lower airfares. Unorganized consumers have benefited while airline companies and air carriers labor unions have lost. How could this happen? (Levine, 1981).

Other familiar moves toward easing regulatory restrictions and opening up markets in transportation, banking, and elsewhere also provide counterexamples to interest group theory's predictions. Moreover, case studies of the processes by which various regulatory statutes were enacted during the 1970s have given little support to the interest group interpretation. The enactment of the Clean Air Act, for example, surely had interest group elements, but it seemed more fundamentally to be an aspect of what has been termed "entrepreneurial politics." Rather than waiting for some demand to emerge from organized interests, entrepreneurial politicians pursued issues of interest to a broader electorate and used the voters' positive responses to political advantage in seeking national office. (Elliott, Ackerman & Millian, 1985). The interest group theorists seemed to have forgotten that political parties and aspiring presidents might simply bypass entrenched interests and go directly to the electorate.

Indeed, as one begins to look for them, more and more anomalies appear. As we just noted, the development of environmental legislation had interest group elements, but often the groups were "public interest" organizations. According to interest group theory, groups representing such diffuse interests as those "concerned about the environment" should never form, much less be effective. But for decades the Sierra Club, the Environmental Defense Fund,

the National Resources Defense Council, the National Wildlife Federation, and a host of others have grown, prospered, and had influence. (Croley, 2008).

Nor is environmentalism the only cause that has attracted effective champions for something other than the narrow self-interest of well-organized pressure groups. One could hardly accuse Ralph Nader of pursuing the interests of the automobile industry, as he and a band of epidemiologists and safety engineers, aided by a few entrepreneurial politicians, promoted the Motor Vehicle Safety Act. (Mashaw & Harfst 1990). And in some cases the unorganized and underrepresented interests seem to be protected without any visible group formation activities at all. And though often criticized as inadequate, American social programs, TANF, Medicaid, the EITC, section 8 housing vouchers, food stamps, and many others make substantial transfers to low income Americans. But as Robert Dole once put it, “There is no poor-PAC.” If statutes are the outcome of interest group pressure and bargaining with representatives, how are these results to be explained?

To some degree, deficiencies in the interest group theory’s record of predictions might be explained away by broadening the theory’s scope. The relevant “interest group” need not be an industry capturing rents. Wealth transfers created by statutes may run in several directions at once and provide benefits to a number of groups. If we consider the power and prestige of bureaucrats, the opportunities created for legislators to do special favors for regulated groups, the secondary benefit of changes in increased safety to insurance companies, the possible redistribution of resources regionally within the United States, and a host of other factors, we can find interest groups (or “latent” interest groups) who are being benefited by all statutory change.

But this approach threatens to make public choice uninteresting as a predictive theory. Statutes always shift rights or expectations. After the fact, a benefited group can always be

identified. And, if we let bureaucrats and politicians serve as proxies for not-yet-formed groups, groups for whom they can later do favors, or with whom they can form coalitions, then the theory becomes both complete and tautological. Given the right hypotheses, everything can always be explained ex post. But interest group theory itself may give us very modest assistance in choosing the correct hypothesis concerning the political dynamics of particular issues ex ante. We have entered the realm of faith, leaving public choice theory with no claim to “scientific” explanation.

Nor is this the only difficulty. The components of the theory, which together define its casual dynamics, are often ambiguous. Consider the following questions:

When investigating the influences of interest groups on legislation, should we be looking for large or small groups? The theory really cannot say, because the currency of exchange is uncertain. Where representatives are being rewarded with money, most interest group theorists seem to believe that small size and large stakes in the outcome would be an advantage in forming, maintaining, and exerting interest group power. On the other hand, if votes are the medium of exchange, then groups of large size obviously have a significant advantage, even if their interests are not so intense. Should we imagine then that politicians will respond to large or to small groups. The answer seems to be to both. But if it is both, then the basic scenario of intense factions producing legislation that suits their interests as the expense of the general public begins to unravel. At best it must be significantly qualified by attention to a host of contingencies that bear on interest group power in particular contexts.

What about the technique of drafting legislation to effectuate interest group desires? Should we anticipate that statues conferring benefits on private groups would be drafted in specific or in vague terms? Interest group theorists tell two different stories. Some imagine that

interest group bargains will be set out in quite specific terms in order to assure that value is actually received by the interest group for its contribution of either monies or votes.

(McChesney 1987). Yet others predict that interest group legislation will be vague in form and contain broad delegations of authority to administrative personnel. (Fiorina, 1982). Presumably, in this later scenario future cooperation between regulated groups and administrative agencies will assure the returns from capture, while protecting legislative politicians from the assignment of responsibility for promoting a particular interest group's private ends.

Indeed, there is not even agreement on whether interest group competition in the political space ought to be viewed primarily as producing private goods at public expense or public goods that must of necessity be produced through collective action. At base this seems to be due to another ambiguity, differences in the models' specifications of who the bargainers are when interest groups seek or oppose legislations. Most interest group theorists seem to imagine a bargain between interest groups and legislators in which the legislators are induced to provide private goods at public expense in return for electoral support. (*e.g.*, Stigler 1971) But in other models representatives are mediators between or among interest groups in a market that demands and supplies an equilibrium level of public goods. (Becker 1983) In these latter explanations, cheerful visions of pluralist compromise and the improvement of public welfare through politics reemerge as real possibilities.

Finally, the more depressing general predictions of interest group theory must be viewed with skepticism because of the implausibility of its account of both the electoral and the legislative processes. These two problems are rather different, but both are fundamental to a coherent story. You will remember that the basic interest group scenario is one in which intensely interested coalitions of voters procure legislation from representatives at the expense of

the general population. The underlying mechanism depends upon the “rational apathy” of voters in general combined with close monitoring of representatives by interest groups.

Now, the shocking thing about voters is that they refuse to behave rationally. It is demonstrably not worth it for voters to inform themselves on the issues, much less go to the polls and vote. Yet in election after election, millions turn out to engage in an activity that produces individually negative returns. This perverse behavior strongly suggests that calculations of personal self-interest, defined wholly in instrumental or economic terms, are not a dominant, perhaps not even an important, element in civic participation. (Kelman 1987). But if this is true, then politicians cannot count on voters to be either uninformed or apathetic. Honoring the demands of “special interests” will not be costless.

It is also unclear what the mechanism is by which interest groups police the representatives that they support for failure to deliver on their promises. There are no transferable property rights in a representative's vote. Moreover, the ability of an interest group to punish a recalcitrant representative is sharply limited by the interest group's alternatives with respect to future trading partners. Collusion among representatives, not to mention their nonfungibility, makes the “market” for interest group influence highly imperfect.

Representatives have significant opportunities to welch on deals and to rationalize that welching in terms of institutional obstacles to performance, or considerations of long-term interest in coalition building. And if the chair of the House Armed Services Committee welches on a deal, there is no alternative representative with whom to make a comparable bargain. Interest groups constantly have to deal with people who – at any one moment – have near monopoly power.

In short, there is an enormous amount of organizational slack in the linkage between interest groups and representatives. For all we know this slack may be allowing representatives

to engage in consumption expenditures, including pursuit of the “public interest” as the representative understands it. After all, that is the perquisite uniquely available to public office holders. Indeed, one wonders why they have chosen to pursue the “public service,” given its pattern of potential returns, unless they plan to obtain much of their satisfaction from seeking the good as they understand it. If interest groups are principals in the political process and representatives their agents, there seems to be a vast domain for “corruption” in the form of “voting one's conscience.” And the foundational empirical investigation of representatives’ preferences finds that reelection is important, but so is the desire for reputation within Congress, the pursuit of higher office, and the pursuit of the representative's ideas of “good public policy.” (Fenno 1973).

The work that has been done on the principal-agent issue seems to validate what these speculations would predict. The narrow self-interest of the interest groups active in representatives' constituencies is not a very good predictor of their voting patterns. Prediction is much improved by adding in ideological variables which represent either legislator beliefs about what is good for the country or certain broad preferences based on general interests of constituents that are unrepresented by the activities of existing interest groups. (Kalt & Zupan 1984, Poole & Rosenthal 1991).

None of this, of course, is to deny that interest group activity is massive and that the potential impact of faction on politics should be a cause for concern. The point is more modest. The interest group theory of legislation is often ambiguous and its predictions are beset by counterexamples. In some forms it is tautological and empty; in others it seems honored mostly in the breach. At least at the level of national politics, Madison’s vision of the efficacy of the Constitution's institutional structure to transform the raw material of the electorate's passions and

interests into something approximating the public interest remains at least plausible. Perhaps civic virtue, the public spirit, and pursuit of the public interest are not dead.

The defects of interest group theory should not, however, be oversold. It is true that one observes outcomes that do not seem to fit well with the theory. Voters vote, legislation pursues things that seem to be in the general public interest, and politicians are even sometimes found to have character and conviction. But this does not make the interest group story untrue unless one imagines that it was meant to be *universally* true.

First, think about the notion that we do sometimes see legislation that pursues broad public interests rather than narrow special ones. Environmental legislation is often trotted out as a case in point. It surely is the case that environmental legislation, broadly considered, seems to pursue such a general interest that you would never imagine an effective group forming to promote protection of the environment. Indeed, since much environmental protection may be oriented at preserving the Earth for future generations, the beneficiaries of this legislation may be an interest group that could not possibly be organized to pursue its own self-interest.

The problem with taking too much comfort from this story is its incompleteness. First, we should probably turn a jaundiced eye on any claim that we can learn whether the public interest is alive and well or dead as a doornail from looking at the output of politics. That legislation has a particularly beneficial effect on a particular group or seems designed to benefit the general public tells us little about *why* the legislation was enacted. That we can provide a number of public interest stories as counterexamples to private interest predictions hardly means that legislative activity is generally public-interested. Indeed, a close look at virtually any individual piece of legislation will almost always generate both a plausible private-interest and a plausible public-interest explanation.

Is occupational licensing a means of protecting consumers from fraud and incompetence, or a device for restricting competition and raising the earnings of licensees? Does Food and Drug Administration regulation of prescription pharmaceuticals protect the public from dangerous drugs? Or is it a handy means for ensuring that only large producers will have the capacity to do drug development, thus limiting the supply and raising the prices of useful medications? Does Environmental Protection Agency regulation of sulfur dioxide in coal-fired electrical generating plants protect the public health? Or is it designed instead to curry favor with western real estate interests and protect the market for eastern high-sulfur coal? In most cases the answer is likely to be "both" or "all of the above."

Second, we should be wary of claims that anyone knows why legislators vote for legislation. There have now been a host of attempts to sort out the voting records of legislators, usually congressional representatives. The question generally asked is whether legislators vote the interests of the groups and constituents whose support they need for reelection, or instead vote their own views of good public policy. Analysts, indeed, have been extremely clever in attempts to determine and assess the mix of legislative motives that one finds in particular cases. While some of these studies are triumphs of scholarly energy and social science methodology, no one has yet been able to tell us more than that legislators seem to act out of some mix of personal ideology and reelection-oriented self-interest. This is not very big news. The problem is, of course, that it is very difficult to control for all the relevant variables—party membership, constituency characteristics, legislative position, campaign contributions, and personal ideology. These things are not only highly correlated one with another, researchers have no way of observing some of them directly.

Take, for example, studies that claim legislators vote their ideologies more than they vote the economic interests of their contributors or constituents. When such an empirical test is conducted, a legislator's "ideology" (view of the public interest) is constructed by looking at his or her voting records. Those records are then interpreted to place the legislator's views at some point on an issue-ideology scale—say, from liberal to conservative. The analysts then look at whether this "liberal" or "conservative" votes the liberal or conservative line on specific issues that come before the Congress. If the ideological variable explains more votes than, say, the legislator's list of campaign contributors or the interest group complexion of the legislator's constituency, then it is concluded that this is a legislator voting his or her conscience.

The problem with this methodology is that the legislators' general patterns of voting, the ones that were used to locate them on the ideological scale, are being used to explain votes on particular issues. But if something that is not being or cannot be measured explains legislators' general voting patterns, then these things also explain the "ideology" that is being assigned to them by the ideological scale. When the legislators' positions on the ideological scale are then entered into the researchers' predictive model, legislators' "ideology" may be being counted twice (or three or four times). About all that can be said is that models that contain both ideological and economic self-interest factors seem to outperform purely economic models in predicting the behavior of legislators.

The crucial point is this: Much of the social science research on voting behavior or interest group activity may be asking questions and giving answers at much too gross a scale. (Kramer 1983). That there are public-interest-appearing outcomes in the world of practical politics and that legislators may vote their convictions or ideologies is cheerful news. But if we are asking that question simply about whole statutes, we may not be asking the relevant question.

It is quite easy to concoct environmental legislation, for example, that is special interest in nature. But to see that it is, one must inspect the specifics of the legislation rather than focusing simply on the fact that a clean air act or a clean water bill got passed. God may be in the details, but special interest pleading may be there too. And it is devilishly hard to calculate whether the public interest benefits of doing something about environmental protection are overwhelmed by the costs of private-interest deals that are in the interstices of the legislation.

The interest group story of the market for legislation has many failings. Neither the demand nor the supply side of the "market" seems to be structured consistently in the way that interest group theorists have imagined. In addition critical actors are often missing from the discussion. Political parties have broad "brand" or reputational interests at stake in legislative action and exercise significant power over agendas and representatives behavior. (Cox and McCubbins 2004). Presidents are also legislative actors via vetoes and veto threats, and they have a much broader and more diverse constituency than representatives or senators. (Mashaw 1985; Moe 1987, 1989; Moe & Howell 1999a, 1999b). Indeed, looking primarily at the PCT approach to legislation some critics have concluded that PCT generally fails to present empirically testable hypothesis or to test the hypotheses available in a scientifically sound fashion. The solid empirical results achieved are said either to disconfirm PCT hypotheses or to confirm the obvious. Thus virtually nothing has been learned. (Green & Shapiro, 1994).

We will return to these more general critiques below. But first we should look at how PCT looks at other law making and law applying institutions and the critical response to those efforts.

B. The Interaction of Legislatures and Administrative Agencies

Much of the critique of the PCT view of legislative—bureaucratic behavior has come from within the PCT-PPT fraternity, but outsiders have made substantial contributions as well. The central question in these debates might be characterized as, "Who controls policy, legislatures or bureaucrats?" "Through what techniques?" and "To what ends?"

Early work focused on Niskanen's claim of bureaucratic control and bureau budget maximization. These claims were disputed from many directions: that bureaucracies valued discretion and discretionary budgets, not total budgets (Wilson 1989, Carpenter 2001); that Niskanen was unclear whether bureaus relied on information advantages or on agenda control (Bendor 1985); and that Niskanen had misdescribed the real bargaining situation because he unrealistically assumed that the legislature's options were the bureau's proposals or nothing (Romer & Rosenthal 1979). Indeed simple budget maximization seems an odd goal because agency size is not well correlated with bureaucratic salaries or agency status and power (Breton and Wintrobe 1975).

Perhaps the most productive critique challenged Niskanen's view of the legislature as a passive participant with virtually no ability to monitor administrators or generate independent information. (Miller and Moe 1983). Much of the later work in the PCT tradition has reversed Niskanen's claim and focused instead on how Congress (or the president) controls the bureaucracy (Moe, 2007). We will return shortly to a particularly salient branch of this approach that attempts to explain much of administrative law as a function of legislative political control of the bureaucracy.

Other early PCT work on bureaucracy has also been mostly superseded. Simple models of regulation as an exchange between the regulators and the regulated (Stigler, 1971) or "iron triangles" of interest groups, regulators and congressional committees (Schatterschneider, 1960;

Lowi, 1969) did not hold up on closer examination. (Pelzman, 1976; Becker, 1983; Croley, 2008; Kelman, 1987). Too many anomalies appeared (e.g. economic deregulation, new social and environmental regulation). And work tending to show that congressional committees were majoritarian and that bureaus and bureaucrats had complex preferences undermined the idea of simple bargains and "captured" regulators. Ignoring the president and the courts was also recognized as a serious problem (Moe 2007).

A number of PCT scholars shifted rather radically from Niskanen's bureau control model to one featuring congressional control of the bureaucracy. Although Congress might not be observed to be monitoring agency behavior in a systematic fashion, these scholars offered reasons why it need not do so. Agencies can anticipate congressional preferences by observing the make-up of its committees (Weingast and Moran, 1983), and Congress can intervene by focused oversight and sanctions when interested parties inform the relevant committee of agency deviation from congressional goals (McCubbins and Schwartz, 1984). Indeed, the agency dominance idea seemed to presume that Congress behaved irrationally by creating a bureau that it could not control but for whose actions the electorate, or relevant interest groups, could hold it responsible.

Yet, surely, congressional dominance was too simple a story as well. While Niskanen had ignored the legislature's capacities, congressional dominance theories ignored the bureaucracy's resources. Niskanen's story may have been one-sided, but it was not necessarily totally false. (Moe, 1987). Moreover, basic agency theory and mainstream empirical work by political scientists (Wilson, 1989) make clear that monitoring and sanctioning were costly and that slack in the control system could produce almost complete bureaucratic policy autonomy (Carpenter, 2003).

Because *ex post* political control is costly, one might imagine that a rational Congress would seek to design administrative systems *ex ante* to make control effective. And, if that were true, then perhaps much of the structure and processes of administrative governance might be explained by thinking through how they foster political control. It is this premise that has set the agenda for much of the PCT-PPT scholarship on administrative structure and administrative law for more than two decades. (For a more detailed account see Moe, 2007).

Many of the questions raised by this literature, e.g., when, how and why do legislature's delegate policy authority to administrators; or what explains the structure of agencies, the requirements of administrative procedures and the contours of judicial review—are of significant interest for public law and public lawyers. One might even conclude that the interest in the PCT account of these matters, for example, has been excessive. There has been considerable over-enthusiastic use of early PCT "findings" by reform-minded legal scholars interested in reviving the moribund non-delegation doctrine (Aranson, Robinson and Gulhorn, 1982; Schoenbrod, 1993). These attempts have been duly critiqued in the legal literature (Mashaw, 1990, 1997; Schuck, 1999). PCT scholars have also made extravagant claims to explain administrative structure and process (McNollgast 1987, 1989), and have once again been taken to task by legal scholars, both for conceptual inconsistencies and failure to explain widespread anomalies in the available evidence. (Robinson, 1989; Mashaw, 1990, 1997).

But in many ways the political control scholarship has settled into a "normal science" mode. Scholars within the PCT-PPT tradition, while often ignoring external critiques, criticize each other's work for failure to take account of the fact of multiple political principals, the ubiquity of political uncertainty, the political desire for administrative efficiency and the strategic behavior of agents. (Moe, 2007). Of course, as PCT analysts attempt to account for

these complexities, models become less tractable, predictions less determinate and the distinctive features of PCT, as against mainstream political science, less obvious. The general thrust of the Green and Shapiro critique emerges once again: what exactly is positive theory telling us that is demonstrably true and not already well understood?

While the political control literature has focused on one of the abiding concerns of public law, the legitimacy of the administration state from the perspective of majoritarian democratic politics, it has been almost exclusively preoccupied with the Congress-administration linkage. The president appears largely as a strategic legislative actor, whose influence over the bureaucracy pales beside that of Congress. (McNollgast, 2007). This "congressional dominance" flavor to the PCT-PPT literature has not received sustained critique, but it surely appears odd to administrative lawyers. One of administrative law's chief scholarly projects for the past three decades might be described as the study of the effects of the aggressive use of presidential powers of oversight and direction on the policy output of the administrative state.

C. PCT and the Courts.

Administrative lawyers' recent emphasis on the role of presidential control of administrative action has not, of course, displaced public law's central concern—some might say obsession—with the role of judicial review in shaping the processes and context of public law. Because many PCT models ignore the judiciary, they are easily criticized as seriously incomplete, if not misleading. For example, critiques of the McNollgast thesis that administrative procedures are to be explained in terms of *ex ante* congressional controls on administration, have emphasized that much of the content of these requirements is a judicial gloss on a text that actually requires very little and leaves agencies with vast procedural discretion. (Mashaw 1990, 1997; Robinson 1989, 1991).

These problems disappear, of course, if courts are modeled as faithful enforcers of legislature bargains. (Landis and Posner, 1975). On this view courts are a part of the legislative control apparatus. But, the "neutral enforcer" model is hardly persuasive. It may be true that the legislature needs to construct an independent judiciary as a commitment device to reassure interest groups or the electorate as a whole that legislative promises will be kept. But that does not explain why an independent judiciary would then function as a faithful agent of the legislature. Judicial behavior itself must be addressed.

Public choice has tended to focus on two major 'schools' of judicial behavior: the attitudinal model (e.g. Segal and Spaeth 1996) and the strategic model (e.g., Ferejohn & Weingast, 1992). The attitudinal model predicts that ideology determines judicial behavior. The strategic model predicts that courts account for the preferences of Congress and the president when voting and when accepting cases to avoid being overturned or facing sanctions such as jurisdiction stripping or budget cuts (e.g. Epstein et al, 1998, 1999, 2001).

Neither account supports the public choice models that either omit the courts or simply assume that courts both know and sincerely enforce congressional intent. If the attitudinal model is correct, and judges vote to maximize their policy preferences, then the assumption in many public choice models (e.g., Huber and Shipan 2002) that courts enforce the law in accordance with congressional preferences cannot be correct. Similarly, public choice models generally fail to account for the strategic model's prediction that the courts tailor decisions in anticipation of reaction by Congress and the president. Courts that either feel at liberty to move policy freely when policy is in the 'gridlock interval' or are constrained by the preferences of a united Congress and president are not simply sincere interpreters of the law.

Neither the attitudinal model nor the strategic model has received overwhelming empirical support, but evidence for the attitudinal model is stronger (Revesz 2001). Recent studies of appeals courts have found a significant ideological effect in panel voting (e.g., Sunstein, et al 2006). However, these studies use a relatively crude measure of ideology at the appeals court level, such as the partisanship of the appointing president and confirming Congress. Ideology measures are more advanced at the Supreme Court level (e.g., Martin and Quinn 2002), and ideology is a strong but imperfect predictor of voting on the Court (for descriptive statistics, see Eskridge and Baer, 2008). These claims have been challenged by judges who argue from personal experience that the assumptions of the ideological models are simply false. (Edwards 2002, 2003) They are also challenged by empirical work that emphasizes the way that factual context shapes judicial outcomes. For instance, Segal (1984) examined the influence of facts in search and seizure cases on outcomes, and found facts to be a significant predictor of outcomes. Other studies have drawn similar conclusions about the importance of case facts (e.g. Ignagni (1994) on establishment of religion; Segal and Reedy (1988) on gender discrimination).

The strategic voters model is also contradicted by recalcitrant facts. Under a literal reading of the strategic model, judicial interpretations should never be overridden by new legislation because they place policy exactly at the legislature's indifference point. Even if the model acknowledges that a court could miscalculate and includes an error term, the theory predicts narrow overrides. But, empirically Congress overrides judicial decisions in a substantial number of cases (Eskridge 1991) and sometimes overrides the Court by a wide margin (Elhauge, 2002). Moreover, Spiller and Tiller (1999) find that the probability that Congress overrides the Court does not increase monotonically with the distance between the House and Senate medians. Instead, the relationship resembles an inverse U. This relationship is subject to various

interpretations, but it does not fit the predictions of the strategic voting model. Finally, a reading of opinions indicates that the Court often appears to invite legislative override of its decisions by using canons that trigger override or even include explicit language inviting override (Hausegger and Baum 1999).

Legal scholars have attempted to draw normative lessons from public choice findings to instruct courts about how they *should* approach the task of statutory interpretation. Their suggestions range from narrow textualism (Easterbrook 1983, 1984), to broad purposivism (Macey, 1986), to situational responsiveness based on the distributional characteristics of particular legislative enactments (Eskridge, 1988). None of these attempts have provided a methodology that is both normatively attractive and operationally available to judges who lack perfect information about the legislative process that produced a particular statutory provision (Mashaw, 1997).

Within PCT scholars have claimed that legislative intent is an oxymoron because the relationship between the individual preferences of legislators and the collective product of the legislative body is not discoverable. (Shepsle, 1992) If the Arrow Impossibility Theorem holds, individual preferences simply cannot be mapped onto collective choices. Others deny precisely that the Arrow Theorem holds, (McNollgast, 1994). Legislatures have relatively stable institutions and procedures for conducting business. This "structure-induced equilibrium," (Shepsle, 1979) may make legislation less than fully democratic, but it does not make it uninterpretable. Indeed, by attending carefully to the strategic interaction of responsible players in the legislative game, these scholars believe that courts can mine legislative history for legislative intent and separate sincere expressions of purpose from "cheap talk" that should be ignored.

But once again, this would be an attractive approach only if we could believe that courts could actually distinguish sincere expressions from strategic posturing and that the parties who have been delegated authority within the legislature to manage or oppose particular bills were representative of the Congress as a whole. (Mashaw, 1997). And on this latter point the PCT literature seems to be rather hopelessly divided. (Compare Weingast and Marshall, 1988, with Krehbiel, 1991).

Summary and Conclusions

Public choice theory has been criticized from the number of directions. Some of these might be characterized as the standard critiques of normal social science. Critics complain that the hypotheses and models of public choice theory are not empirically verified. One such complaint notes that models are developed and then applied to explain actions or institutions which seem to fit the model. But this is a form of anecdotal storytelling that tends to ignore counterexamples and discrepancies between the model and other facts to which it might be fitted. Thus, for example, the McNollgast explanation for the Administrative Procedure Act ignores alternative structures for that Act that would have made it much more useful as a control device for Congress. And, it fails to account for the fact that Congress delegates much authority to administrators who act outside of the Administrative Procedure Act's requirements. Fixated on legislative control, the McNollgast theory similarly ignores that much of the procedure that is required for administrative rulemaking was developed by courts, not by Congress, and that agencies have a multitude of devices for both shaping and ignoring the procedures that the Act does prescribe.

Critics have also complained that certain public choice findings do not fit well with other public choice assumptions. To continue with the same example, the McNollgast hypothesis

posits that administrative procedures stack the deck in favor of an original enacting coalition. But if legislators are interested in reelection, they should be interested in responding to voters' current preferences, not to preserving the gains of some previous majority. The theory solves this problem by suggesting that administrative procedures also put policies on autopilot. By acting within the administrative process current majority coalitions can change policy without resorting to Congress. But, if this is true of administrative procedures, then the theory is untestable. The theory would be proved whether we find policy stability or policy change.

Critiques often also point out that particular models have important left out variables. And again, like many other models, the legislative control explanation of administrative procedures ignores the president. Given the ubiquitous competition over the course of American history between presidents and congresses for influence over the bureaucracy, one should at least ask why a president would sign a bill creating administrative procedures that gave significant control to Congress rather than the president. In addition, if administrative procedures provide some insulation against presidential political control of administration, then we need some explanation of why the procedures do not insulate against legislative control as well.

This is not, I should hasten to add, a suggestion that the political control hypothesis with respect to the structure of administrative procedure is wholly implausible. Many examples could be given that would tend to support that theory. And, the shortcomings of particular explanations are not necessarily an indictment of the whole project of public choice theory. Social science explanation advances incrementally through critique and attempts at replication. To be sure, some critics have claimed that the poor predictive performance of public choice theory suggests that its contributions to our understanding of public institutions will be marginal. But, that in

itself is merely a prediction which, to be persuasive, would need to be premised on some more fundamental failing in the public choice approach.

These more fundamental critiques have also been an evidence as we have pursued the claims and criticisms of public choice theory. The theory operates on the basis of two critical assumptions: (1) all political actors are assumed to act out of their own self interest, and (2), their behavior is assumed to be both rational and strategic. Many critics of public choice theory believe that neither of these assumptions is a plausible basis for explaining action in the public sphere.

Counterexamples to self-interested behavior abound. Voters vote even though rational self-interest seems to tell us that the costs of voting exceed the benefits. Highly paid executives in private firms take public positions that radically reduce their earnings. Taxpayers support redistributive programs even though these programs increase their taxes and lower their net incomes.

Similarly rational strategic action requires that actors understand the current state of the world and how their actions are likely to influence subsequent events. But, polling data tells us that voters are stunningly badly informed concerning both the positions of potential representatives and the likely effects of particular policies on their own circumstances. And how is it that the same legislators who delegate policy authority to agencies because of their inability to predict what will need to be done can yet devise structures and procedures that ex ante determine policy outcomes? Moreover, several decades of work in behavioral economics and cognitive psychology have demonstrated both that people have tastes for altruism and that we humans are beset by cognitive biases that limited our ability to behave rationally.

The existence of altruism and the limits on human nationality are, of course, common complaints against microeconomics as well. (Sen, 1982) But lack of realism is not a good complaint against a predictive theory so long as the assumptions allow the theory to predict accurately. The problem for public choice theory is that its predictive accuracy has, to date, been relatively weak. Moreover, the analogy to microeconomics is not perfect. Although economists fully understand that their assumptions concerning self-interest and rationality do not reflect universally true states of the world, they have additional reasons for believing that their assumptions will nevertheless allow their theories to behave acceptably.

The most important is that they presume that firms or individuals in markets will be acting under conditions of reasonably vigorous competition. And, under those conditions, individuals or firms that do not behave in their own rational self-interest are unlikely to survive. Hence, economic actors do not have to be self-interested and rational from an internal perspective for the theory to have predictive power. The markets are expected to select for those traits and to reward parties who display them, and punish those who do not. This will doubtless occur with some slack, because markets are not perfectly competitive. Economic actors are not always acting at the margins of survival. Hence, altruism and error are not always fatal. Economics merely claims that under competitive conditions one should expect that economic actors will be forced to behave in a reasonably rational and self-interested fashion. If most markets are reasonably competitive, then economic theory will get its predictions about how economic actors will behave right most of the time.

The problem for public choice theory is that political markets may not be competitive to anything like the same extent as economic markets. How well we perform in the market place bears directly on our economic well-being. But, as PCT itself insists, how individual voters vote

is only remotely related to how well off they will be. Electoral competition looks fierce, but most legislative seats are safe seats. Bargaining between legislatures and administrative agencies is clearly monopsonistic bargaining, not competitive bargaining. And, judges, federal ones at least, have life tenure and a monopoly over the authoritative resolution of legal disputes. If rational self-interest is not true from the internal perspective of political actors, then the external environment of the political marketplace may not force them to behave as if it were. The lack of realism of public choice theory's assumptions looks like a bigger problem for it, than that same lack of realism would be for economic theory.

Yet, once again, these objections are not necessarily sufficient to convince us to throw public choice theory into the dustbin of failed social science projects. First of all, the dangers of self-interest in political life are a staple of public interested reform efforts. Those concerned with the very low turnouts in American elections constantly strive to reform voter registration and voting laws to bring the costs of voting to voters near zero. Those concerned with interest group pressures on representatives press relentlessly for electoral reforms that will get the money out of politics and for legislative and administrative reforms that will make legislative and administrative action more transparent to the general populace. Those concerned with the potential for ideological voting by judges emphasize the need for checks and balances in the appointments process to protect against extreme political preferences in the judiciary. The assumption of self-interested political behavior is common currency for both public-interest performers and public choice analysts. Rational self-interest may not be the whole story of political life, but it is almost certainly a part of it.

Second, as the references to reform efforts suggest, public law may be interested in public choice theories and findings because of an interest in institutional design rather than because

public lawyers believe that political life is exactly as public choice theory depicts it to be. Indeed one of the fathers of public choice theory, the Nobel Laureate James Buchanan, explicitly denied that public choice was valuable because of its predictive power. (Brennan and Buchanan, 1988.) In Buchanan's words, "We model man as a wealth maximizer, not because this model is necessarily the most descriptive empirically, but because we seek a set of rules that will work well independently of the behavioral postulates introduced." From the perspective of institutional design the public choice approach to governance is reminiscent of James Madison's comments on constitutional design in Federalist 10. The Constitution was not being designed as though men were good and would act in a public spirited fashion, but in Madison's famous phrase so that "ambition would counteract ambition" in a system of checks and balances. The constitution hoped to distill public interest outcomes from self-seeking behaviors. The value of public choice theory might thus be better judged, not by its predictive prowess, but by its capacity to lead us toward better institutional arrangements.

There may be much wisdom in Buchanan's approach, as there surely was in Madison's. But Buchanan's answer will not necessarily be comforting to public choice practitioners or to some external critics. The former treat their activities as a form of standard social science. To reject the value of public choice as a predictive discipline, or at least a descriptive one, is to reject their central project. And, from an external point of view, Buchanan's starting point for institutional design may have its own pernicious effects. Civic republicans have long maintained that people's attitudes and values about political action are not independent of the institutional settings within which they act. As Stephen Kelman has put it,

"cynical descriptive conclusions about behavior in government threaten to undermine the norm prescribing the public spirit. The cynicism of journalists -- and even the writings of professors -- can decrease public spirit simply by describing what they claim to be its absence. Cynics are therefore in the business

of making promises that threaten to become self-fulfilling. If the norm of public spirit dies, our society would look bleaker and our lives as individuals would be more impoverished. That is the tragedy of public choice.” (Kelman, 1987).

Properly understood, Kelman's argument is that his and Buchanan's worlds are incommensurate. The people who learn their politics from the design and operation of Buchanan's defensive democratic institutions will become different people than the ones Kelman envisages. They will have different preferences and values. And they will design different institutions in pursuit of those values. Viewed in this way, not only are the prophecies of public choice theory self-fulfilling; the complaints of neo-Republican critics like Kelman are unanswerable. The continuous emphasis on designing public institutions and limiting public interventions to avoid the perils of self-interested behavior construct a world in which the possibilities for nurturing the public spirit and extending its reach are sharply constrained. Civic republicanism will never have been given a trial.

Kelman's point is a powerful one, but it may prove too much. For one thing, as public choice scholarship has proceeded from its early pessimistic prognostications, much of the field has begun to attempt to explain how public institutions support majoritarian democracy, not how democratic government fails. More importantly, the civic republican view that political actor's preferences and values are shaped by public institutions provides a necessarily incomplete picture of human nature. Political socialization into virtuous or public spirited habits and commitments cannot be the whole story of human preference formation. If it were, political conflict would disappear. Socialized by our joint participation in public affairs, we would ultimately all have the same values and preferences, all public choice would become consensual. The neo-republican vision of politics is but the flip side of the public choice of vision. Where the latter sees atomized individuals relentlessly pursuing their own exogenously determined

individual preferences, the former sees fully socialized altruists exercising civic virtue in the pursuit of community welfare.

A survey of public choice theory and its critics this leaves us in something of a muddle. Public choice practitioners have overclaimed and underperformed as positive social scientists. But there is much evidence that the field has its own capacity for self correction. Constant emphasis on selfish strategic behavior in politics does pose dangers for the public spirit. But designing institutions as if people always attempted to act in the public interest is almost certainly a formula for disaster. As I have argued elsewhere, (Mashaw, 1997), it would be nice if we could simply accept the public choice approach to either understanding or designing public institutions or reject it. But that seems not to be our fate. We must instead mine the field for insights and usable knowledge, while avoiding either overly enthusiastic acceptance or derisive dismissal of PCT analyses. If public law is to pursue the public interest, it must do so in part by attempting to learn from those who here sometimes seemed to suggest that the public interest could not possibly exist.

Indeed, PCT approaches may sometimes reorient our thinking in ways that support widely acknowledged public law norms. For example, a recent article (Stephenson 1987) addresses the perennial problem of justifying bureaucratic policymaking in an electoral democracy. Like courts, staffed by lifetime appointees and exercising the power to declare statutes unconstitutional, appointed administrators are widely believed to be “undemocratic”. While at the federal level in the United States these administrative policymakers operate under congressional statutory mandates and are appointed by the President, subject to Senate confirmation, no sensible person believes that the value-laden choices made by administrative policymakers are fully within the control of elected politicians. There is a constant lament that

Congress delegates broad authority to these unelected policymakers and a significant current in the legal literature seeks to reinforce political accountability by emphasizing presidential control of administrative decisionmaking. Presidential control gains normative force from the intuitive notion that presidents will better represent majoritarian interests than appointed bureaucrats. Hence, to the extent that these bureaucrats can be made responsive to the President they are made more responsive to electoral majorities.

In a complexly argued essay employing formal modeling and standard PCT assumptions about preference aggregation, Matthew Stephenson argues persuasively that this intuition is false. Stephenson's argument is relatively straightforward. In our two-party system we oscillate between Republican presidents who are somewhat more conservative than the majority of Americans and Democratic presidents who are somewhat more liberal. Over time, the average policy position of presidents may track the preferences of the median voter fairly well. But, each particular president diverges from the preferences of the median voter to some degree. In Stephenson's account, a degree of insulation of administrators from presidential control creates a "compensatory inertia" that dampens the significance of a particular president's policy preferences. This dampening effect reduces the variance between the policies actually implemented by particular presidents and the preferences of the median voter in the electorate. Thus, some degree of insulation of the bureaucracy from political control is not "undemocratic". It, instead, helps to align policy with median voter preferences.

This is not to claim that Stephenson's results are unnecessarily correct, that the assumptions built into his models are without difficulty or that his argument tells us in any very specific way how much insulation to build into bureaucratic institutions. The point rather is that PCT approaches, building up arguments from the basic materials of voters' preferences,

politicians' preferences, and their interaction in the context of governmental institutions, can provide quite new perspectives on old arguments. While the defenders of administrative insulation or independence have largely focused on welfarist arguments sounding in rationality or expertise, or have exalted deliberative democracy as the normative underpinning for bureaucratic autonomy, Stephenson's argument reveals that this defense can sound in electoral democracy as well. And, if that is true, then every advance toward political control of bureaucratic decisionmaking by presidents is not a gain in democratic legitimacy to be weighed against potential losses in either expert or deliberative rationality, but may instead be a loss of legitimacy from the perspective of electoral democracy as well. This is a fundamental shift in the cost-benefit analysis of institutional design and one that legal architects concerned with the democratic pedigree of administrative institutions ignore at the risk of misconceiving their task.

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