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Public Choice: A Critical Reassessment

Abstract

Over the last several decades, public choice has become one of the most influential schools of academic thought that seeks to analyze the relationship between private interest groups and economic policy-making, spawning an abundance of both theoretical and empirical work. Despite its apparent dominance, however, public choice suffers from several serious flaws in both the way it theorizes the policy-making process and the way in which it marshals empirical evidence to support its analytical claims. Accordingly, developing rigorous theories of the policymaking process and testing them adequately remains a central unfinished research agenda for social scientists.

Introduction

The school of thought broadly known as public choice, emphasizing the pervasiveness of self-interested behavior in government, the capture of the state by private interests, and thus the pernicious influence of the government's role in the economy, has now been ascendant in academia for several decades. Arguments emphasizing the failures of government dominate the majority of both theoretical and empirical work undertaken in a variety of fields. On the other hand, studies that analyze or even define cases of policymaking success, or offer a theoretical rationale for government action, particularly in the economic realm, are rare and receive relatively little attention.

Accordingly, the dominant impression gleaned by the layman and the specialist alike is that a fairly wide consensus has been reached and the debate concluded: scholars have a clear and accurate understanding of the fundamental mechanisms of the operation of government and its intervention in the economy, as well as the consequences of that intervention. It is the contention of this paper that, on the contrary, the dominant literature has ignored important critiques of the public choice theory of policymaking that highlight major weaknesses of the theory. Taken together, these critiques deserve far more attention than they have as yet received.

Far from being a minor academic controversy, the validity or lack thereof of public choice is centrally important, shaping our views about the most fundamental of questions: not what the government should do to correct a perceived deficiency in the polity or economy, but whether the government has a role in resolving collective dilemmas at all. Thus rigorously theorizing and testing models of policymaking and the economic role of the state remains a crucial unfinished research agenda.

Origins of Public Choice

The debate over the economic potential of government began as a debate over the failings of the market, as a significant body of research into its flaws emerged for the first time in late-nineteenth century continental Europe. The first major conceptual innovation was the idea of “public goods” – a type of good for which the government must necessarily be the supplier. The economists Ugo Mazzola, Knut Wicksell and Eric Lindahl all worked out the basic theory of public goods, paving the way for the landmark article by Paul Samuelson on public goods decades later (Samuelson 1954). Samuelson argued that in the case of goods—such as clean air or national defense—that are jointly consumed by all without exclusion, the market level of provision will be inefficiently low. This provided the conceptual basis to defend a substantial expansion in public expenditure.

In the 1920s, A.C. Pigou launched a related strand of analysis, defining the economic importance of externalities and outlining a potential government role in addressing them. Defining externalities as a divergence between the private rate of return and the collective rate of return, Pigou argued that government taxes or subsidies could be used to correct such differences and thus generate an efficient outcome. Subsequently, scholars analyzing the sources of potential market failures increasingly turned their attention to the role of government in regulating natural monopolies and the implications of informational imperfections for the operation of markets. Broadly speaking, these theories identified various circumstances in which the market might prove inadequate to achieve collective goals—the provision of public goods, the existence of externalities, the presence of natural monopolies and an absence of adequate information—and thus argued that the government could effectively play a role in

correcting these failures and supplementing market operations. In the words of one analyst, “The affirmative view on governments’ and other public agencies’ ability to ameliorate identified market failures at low cost, or adjust inequitable market practices by means of regulatory techniques, has been coined the *public interest theory*” (Hägg 1997, 339).

These various strands of analysis never constituted a coherent or self-identified school of thought or intellectual movement. As Michael Hantke-Domas points out, “no author has claimed intellectual ascendancy over the public interest theory, nor have they mentioned any author or supporter of it. . .the Public Interest Theory does not have any known origin” (Hantke-Domas 2003, 166) “Public interest” was a label that later public choice theorists retrospectively, and critically, bestowed on this collection of approaches to market failure and government intervention, largely in the four areas outlined above: public goods, externalities, natural monopoly and informational imperfections. It became an epithet to hurl at those who asserted, with what these analysts considered to be great naïveté, that the modern government had a substantial role in economic policymaking.

This is not to suggest that theories of regulation as a mechanism to correct market failure did not exist. In the aftermath of the New Deal and World War II, such theories were influential and arguably dominant, both in economics, largely under the leadership of Paul Samuelson, and in political science, sociology and law. However, public interest as we now refer to it only became a significant and recognizable school within the social sciences after a new wave of academic analysis took the initiative to name their conceptual forerunners (and rivals).

Public choice, by contrast, emerged as a coherent, self-conscious intellectual movement, nurtured by conservative foundations and academic centers, and so has grown into maturity with

a significant complement of sympathetic intellectual historians who chronicle its origins and growth. Such historians generally highlight the founding theoretical contributions as those of Duncan Black (1948), Anthony Downs (1957), and James Buchanan and George Tullock (1962), who argued that political actors behave like economic actors and then expounded on the implications of this parallel for the political sphere. Equally important, however, were a series of empirical and historical works analyzing the impact of existing regulatory policies—particularly the operation of the oldest regulatory institution in the U.S., the Interstate Commerce Commission—that found them to be inefficient, competition-reducing, oblivious to the impact of ongoing technological change, and damaging to the public welfare. Most prominent among these studies were Meyer et al. (1959) on railroads, buses and trucking, Caves (1962) on airlines, MacAvoy (1965) and Kolko (1965) on railroads, and MacAvoy and Sloss (1967), Friedlaender (1969) and Harbeson (1969) again on various modes of transporting freight. These studies marshaled substantial evidence that the existing forms of regulation, at least within the realm of transportation, had not been effective. On the contrary, they had served only to reinforce the power of existing cartels or oligopolies, contradicting the prevailing assumptions about the political economy of regulation. These research findings presented researchers with an obvious question: if regulation was not serving the public interest, whose interest was it serving, and why? This question became a central preoccupation of public choice theory.

In essence, the theoretical innovation of public choice was that of analyzing political actors as essentially identical to economic actors, and the operation of a policymaking process as analogous to a market. Legislators or regulators have economic policies to “sell”; other actors, individuals or firms, may wish to “purchase” policies they find to be beneficial; all actors on the political and governmental stage seek to maximize their utility. The resulting political analysis

proved to be disarmingly simple: interest groups purchase policies from legislators and/or bureaucrats, while voters are generally rationally ignorant—due to the fact that the cost of acquiring information about regulation is high and the marginal benefit from voting rationally virtually zero—and thus irrelevant. Despite the analogy between the political process and the economic market, public choice scholars see the former as beset by a number of pathologies that make it relatively inefficient (Mitchell 1989). As the economist Gary Becker puts it, “monopoly and other imperfections are at least as important, and perhaps substantially more so, in the political sector as in the market place” (Becker 1958, 109).

The two key early lodges of public choice work were the Center for Study of Public Choice, now at George Mason University (previously at Virginia Polytechnic University and the University of Virginia) and the University of Chicago economics department, with notable outposts also to be found at the University of Rochester and Indiana University – Bloomington (Mitchell 1988). Chicago and Virginia continue to be principal centers of public choice work and the intellectual homes of its most distinguished proponents, though the work of scholars at the two universities reflects different emphases. Primarily trained in political science, with the notable exception of Buchanan, public choice scholars at Virginia have developed a detailed theory of mechanisms of collective choice, their potential flaws, and the relationship between these mechanisms and private decision-making as it occurs in the political market, while paying scant attention to particular policies that the political process might generate. Romer provides a summary of the work of the Virginia school in his tribute to its founder and leading light, Nobel laureate James Buchanan. He argues that Buchanan’s key contribution, and by extension the contribution of the many theorists who have taken Buchanan as their inspiration, is to integrate theories of collective decision-making into the study of public economics: i.e., to argue that

government policy cannot be considered as an exogenous variable in the analysis of economic outcomes. This conclusion is intimately linked to the Virginia school's view of government as a locus of exchange, a conceptual legacy of Knut Wicksell (Romer 1988.)

By contrast, the Chicago school is composed principally of economists, focused on identifying socioeconomic inputs of interests or demands and evaluating the form and impact of government intervention in the economy (outputs), without emphasizing the conversion process by which interest groups obtain the policies they seek (Mitchell 1989). As a rule, the Chicago scholars do not emphasize their intellectual ties to earlier public choice theorists and the Virginia school. However, their account of politics as an economic marketplace, essential to their theories of regulation, reflects their connections to the work of the Virginians. Moreover, the two groups have been united by a certain self-conscious spirit of rebellion against the perceived dominance, both in the academy and in government, of what they contemptuously deemed public interest analysis. In 1965, Stigler declared that "Pigou's views of the competence of the state were, like his predecessors' views, a tolerably random selection of the immediately previous views, warmed by hope" (Stigler 1965). James Buchanan was no less sweeping in his characterizations, arguing that the prevailing mindset in the 1950s "was socialist in its underlying presupposition that the government offered the solution to social problems" and that inevitably led to the overreaching and failure of governments in the 1960s and '70s (Buchanan 2003).

In the following analysis I will note at some points a divergence in the analytical approaches adopted by what are frequently considered to be the "classic" public choice theorists, and the Chicago scholars and their successors, many of whom are more frequently associated with the economic theory of regulation than public choice per se. Despite these differences,

considering them together is appropriate for two reasons: first, public choice as a school clearly considers the work of Stigler and his successors to be part of their canon, even if Stigler et al. do not explicitly identify themselves in this manner;¹ and second, these authors share many of the same fundamental assumptions about politics, market failure and government failure and that I intend to revisit here.

The Dominance of Public Choice

One can see the profoundly deep imprint of public choice as a framework for analysis of the modern state in three areas that public choice has devoted itself assiduously to exploring: mechanisms of collective choice; the behavior of voters; and the causes and consequences of government intervention in the market. In each case, the direction of current academic writing embraces the key assumption that government frequently acts as a perverse mechanism for wealth transfers, yielding policies that benefit select interest groups but are often inefficient or otherwise detrimental to the public welfare. The following three sections will outline the current state of research in each of these areas. Subsequently, I will consider criticisms of public choice. A final section evaluates the state of the field as a whole, offering an initial hypothesis as to why public choice as remained so clearly dominant, despite the profusion of (largely unanswered) criticisms.

¹ Robert Tollison, a leading public choice researcher based at the center at George Mason, characterizes the work of the Chicago political economists as “the Chicago version of the modern development of public choice theory”. He argues that the principal theory of economic regulation at the time of Stigler’s first paper, the Pigovian or public interest theory, was “already under heavy assault from earlier contributions to public choice theory by Buchanan, Tullock and others” and now has been virtually discredited, while the interest-group theory “has accumulated widespread recognition” (Tollison 1989, 295).

Mechanisms of Collective Choice

Anthony Downs launched public choice as a coherent school with his *An Economic Theory of Democracy*, published in 1957, thereby becoming the first scholar to articulate three postulates that later became mainstays of public choice political analysis. First, voters making political decisions seek to maximize the utility they glean from the political process. Second, lawmakers seek to maximize the votes they obtain, and thus carry out “those acts of spending which gain the most votes by means of those acts of financing which lose the fewest votes” (Downs 1957, 52). Third, given that voting and informing oneself about policy are both costly, and the benefit derived from an individual vote one way or another is virtually zero, it may be rational for voters to remain ignorant about many policies that affect their well-being. All of these postulates were, at the time, relatively new in political analysis. They were at least partially inspired by Kenneth Arrow’s then-recently established “impossibility theorem”—Downs was a student of Arrow—demonstrating that it was impossible to devise a rule for reconciling individual preferences that met a certain set of seemingly basic criteria. Though many critics, including Tullock himself (1967), would later argue that the conditions Arrow imposed did not approximate the desirable characteristics of majority rule in a democracy, the impossibility theorem served to direct scholarly interest toward the question of how society could combine a given set of individual preferences in a process of collective decision-making, e.g. in a democracy. These two works served to launch a research agenda that reexamined the operation of mechanisms of collective choice, applying techniques for analyzing the behavior of individuals in markets to the study of politics.

Also important in this period was the pioneering work done by Ronald Coase re-analyzing the problem of externalities that had originally been posed by Pigou. Coase argued that endowing private actors with property rights and creating a market for such rights would more efficiently address externalities than direct intervention by the government (Coase 1959; Coase 1960). He suggested that “analysis in terms of divergences between private and social products concentrates attention on particular deficiencies in the system and tends to nourish the belief that any measure which will remove the deficiency is necessarily desirable. It diverts attention from those other changes in the system. . . changes which may well produce more harm than the original deficiency” (Coase 1960, 42-3). In other words, the cure may be worse than the disease, as governmental efforts to correct existing distortions in the market can simply generate new ones. At the same time, Coase noted that transaction costs, the costs of making private agreements to address market imperfections, could at times be so high as to prohibit the effective private adjudication of externalities, in which case regulation would be required. His work served to introduce the question, much examined in the years ahead, of whether and in what circumstances the unanticipated costs associated with regulatory action might be worse than the socioeconomic problem it sought to correct.

The true bible of public choice, James Buchanan and Gordon Tullock’s *The Calculus of Consent* was published later in the decade. Buchanan and Tullock highlighted the congruity between the individual as “economic actor,” a trope that has long been the preoccupation of economic theory, and the individual as a political actor participating in collective decision-making processes. They insisted that the separation between the two, characteristic of previous political science analysis, had no logical foundation. “No one seems to have explored carefully,” they pointed out,

. . .the implicit assumption that the individual must somehow shift his psychological and moral gears when he moves between the private and social aspects of life. We are, therefore, placed in the somewhat singular position of having to defend the simple assumption that the same individual participates in both processes against the almost certain onslaught of the moralists. (Buchanan and Tullock 1962, 20).

The most important collective decision societies make, they continued, was between the sphere of collective choice and that of individual choice. For such constitutional questions, they argued that unanimity is the appropriate decision rule.

Though this emphasis on unanimity in constitutional crafting was arguably the central conclusion of Buchanan and Tullock's work, their call to link conceptions of economic and political man proved to be far more influential. Buchanan and Tullock did note, however, that it was not necessary to conclude that the same imperatives governed individuals in social choice as in individual choice, emphasizing that "The analysis does not depend for its elementary logical validity upon any narrowly hedonistic or self-interest motivation of individuals in their behavior in social-choice processes" (Buchanan and Tullock 1962, paragraph 3.1.4). A similar point had been made by Buchanan even earlier (Buchanan, 1954), when he outlined a series of differences between economic and political behavior, most notably that the individual in the economic sphere assumes that his decisions will have no impact on "social variables," while the individual as voter recognizes his vote to be a form of participation in collective decision-making. Downs likewise noted that the utility a voter receives from government actors may stem from sources unrelated to his material income (though he maintained the exclusive focus on vote-maximization by governments). Nonetheless, subsequent scholarship frequently ignored these qualifications.

Following Buchanan and Tullock, George Stigler furnished the next notable advance in public choice (and the first volley in the Chicago school's contribution to it), expanding the theory's focus to consider not only the manner in which a polity makes collective decisions, but also the implications of this process for government intervention in the economy. In his foundational article on the theory of economic regulation in 1971, Stigler implicitly adopted the political economy framework of Buchanan and Tullock. He then applied it to the analysis of the formation of regulatory policy in a setting of universal utility-maximization by those groups that desire (and pay for) regulatory policy and the legislators who supply them. Stigler contended that "the problem of regulation is the problem of discovering when and why an industry (or other group of like-minded people) is able to use the state for its purposes," and defined the terms of the exchange to be such that an industry receives one of four things—direct subsidies, control over entry, control over substitutes and complements, or price-fixing—in return for providing a political party what it wants, namely money and votes (Stigler 1971, 4).

This basic model was then further elaborated, refined and formalized by Peltzman (1976) and Becker (1983). Peltzman focused on the relationship between the size of an interest group, its informational and organizational costs, and the gains it obtained from the political process, arguing that smaller groups would be more likely to win rents from the government since they face a less severe collective action problem in organizing their members. This basic insight between the size and other characteristics of interest groups and their ability to organize had previously been articulated, in a principally qualitative framework, by Mancur Olson in his landmark 1965 book.

Subsequently, Becker's analysis modeled the policy-making process as one of competition for policies between interest groups with opposing policy goals and differential abilities to capture policy-makers, rather than hypothesizing one monolithic "interest group". He concluded that interest groups that are more efficient in producing political pressure will win greater regulatory benefits in equilibrium. For example, small pressure groups can generally monitor their members more cheaply to ensure that everyone is participating, thus limiting free-riding and producing more effective political pressure for the same cost; they are likely to be more successful in manipulating the political process. On the other hand, groups that advocate policies that are welfare-increasing, or policies that have small costs in terms of distortions imposed on the economy—in the language of welfare analysis, minimal deadweight loss—are also at an advantage in the competition for political influence. The "winning" interest group will be the one for which the benefit to the policymaker of implementing the policy desired, measured in money and votes, is equal to the cost in terms of loss of electoral support and contributions from the victims of the policy change.

Interest Groups, Legislators and Regulators

Drawing on the conceptual framework of these seminal scholars, a number of other authors developed somewhat more specific or detailed accounts of the intervention of interest groups in economic policymaking. McKenzie and Macaulay (1980) contended that regulation is used to enhance growth of the public sector: since regulation increased the price of private goods and services, public goods and services became more competitive. Barke and Riker (1982) developed an "incrementalist" theory of the design of regulation, in which different interest groups obtain (i.e., purchase) small adjustments to the existing policy over time that serve their

interests. In the long-run, however, each regulatory adjustment produces greater market distortions and damages the collective welfare. They argue that when the cumulative deadweight loss exceeds the small benefit gleaned by any one group, incentives for deregulation emerge. In subsequent work, Becker (1985) and Ellig (1991) elaborated this perspective, arguing that over time, the deadweight loss imposed by even a constant regulatory policy increases as interest groups adapt to win more private rents from the government. The increased welfare loss creates a constituency for deregulation—an argument employed to explain the wave of deregulation visible in the U.S. economy in the late 1970s and 1980s.

Other scholars probed the role of legislators as the nexus of the political marketplace. Kalt and Zupan (1984) relaxed the assumption that legislators' votes solely reflect a process of interest group purchase, arguing there is space for "ideological shirking" in which legislators' own preferences are sometimes decisive. Robert Tollison, one of the leaders of the Virginia school, analyzed the interaction of the demand for legislation from interest groups and the supply from legislators, considering how various characteristics of the legislator-brokers and the political process in which they operate—including both ideology and seniority—influence the formation of regulatory policy (Tollison 1988). Similarly, his joint work with a number of others examined features of the political process that effectively facilitate its manipulation by interest groups. His earlier papers argued that the political market typically generates forms of regulation that impose costs on "foreigners" outside of the constituency, district or local polity (Maloney, McCormick and Tollison 1984), and that campaign finance regulation has an impact on the influence that interest groups exert on the political process (Crain, Tollison and Leavens 1988). Subsequent work on executive vetoes and state constitutional amendments demonstrates that both of these institutions serve to provide more durable forms of protection to interest

groups, for which they are willing to pay a higher price (Crain and Tollison 1979a, 1979b).

Landes and Posner had made a similar argument regarding an independent judiciary as a guarantee of more enduring protection for interest groups, suggesting that it too leads to legislation commanding a higher effective price (Landes and Posner 1975).

The preoccupation with assessing the legislative supply and demand process suggested the importance of examining more carefully the determinants and impact of campaign contributions, given that they are the analogue to the price paid for the policy desired. A first set of models analyzed equilibria in which candidates equate a marginal vote return from a given policy to be adopted with the marginal return to a dollar of contributions, thereby shifting policy toward preferences of the contributor (Ben-Zion and Eytan 1974, Welch 1974 and 1980; Austen-Smith 1987; Lohmann 1995). A second set of studies analyzed contributions as a sort of contract: without affecting the likelihood of any candidate being elected, interest groups make contributions as part of an agreement that the candidate will deliver services if elected (Snyder 1990; Baron 1989, both as cited in Austen-Smith 1997). Yet another literature has examined lobbying as a potential mechanism by which rents are transferred to lawmakers by interest groups. The analysis of lobbying also addressed the ability of interest groups to provide highly specialized, and potentially biased, information to legislators in their area of regulatory interest and thus shape the legislative agenda. Though this agenda-setting channel of influence has received less attention in economics, which focuses principally on financial blandishments offered by interest groups to legislators, its importance is more widely noted in the political science literature (Besley and Coate 2001).

Scholars remained divided, however, as to whether it was legislators, bureaucrats or some other agent who were the target of interest group blandishments and thus the pivotal actors in shaping the state's intervention in the economy. Buchanan and Tullock, with their primary focus on mechanisms of collective decision-making, remained silent on this question, while Stigler suggested that industries seeking regulatory action of some kind must pay tribute to the political party in power. Peltzman noted the problem of separation of agency between elected officials and bureaucrats but concluded that the two could reasonably be considered to be equivalent, given the political dynamics of appointments. McCubbins, Noll and Weingast (1987, 1989), on the other hand, contended that there exists a principal-agent problem between legislators and bureaucrats, and the former does not have a perfect ability to control the latter. As well-summarized by Croley (1988), "agencies, like legislators, tend to stick around a long time, during which their budgets tend to grow in spite of legislative preferences to the contrary. As a general matter, it is just not clear that legislators are able to monitor their administrative agents very successfully" (Croley 1988, 4).

Theorists reacted to this observation in different ways. Weingast and Moran (1983) and Weingast (1984) analyzed the Federal Trade Commission and the Securities and Exchange Commission respectively and concluded that there is systematic congressional influence over their decisions, but it remained unclear whether this result was generalizable. Subsequent empirical work seeking to validate the central public choice contention regarding regulation—that intervention in the economy reflects the undue clout of interest groups—divided between efforts to demonstrate influence over legislators and attempts to document influence over bureaucrats. Becker (1983) cited as evidence for the contention that interest groups purchase policies two studies (Palda 1975 and Jacobson 1979) analyzing the funding of political

campaigns, which he presumably views as a mechanism for establishing control over legislators. In a subsequent study, Becker (1986) likewise applied the analysis of the influence of competing interest groups to legislators, and the theme of legislative self-interest was echoed by Abrams and Settle in their studies of campaign finance reform (1978, 2004). At the same time, there is a strain of literature, including Hilton (1972), Eckert (1973), Russell and Shelton (1974), Toma (1983), Kaserman et al. (1984), Hazlett (1990), Caudill et al. (1993), and Olson (1995), that instead attempted to show how largely unelected regulators make decisions according to material or tenure incentives or otherwise yield inefficient outcomes, suggesting that the object of analytical focus should be bureaucrats rather than—or in addition to—elected officials. Baron (1988), Spiller (1990), Laffont and Tirole (1991) and Martimort (1999) directly modeled the interaction between interest groups, a regulatory agency and a political principal, examining the possibility that interest groups can bribe the agency to retain information relevant to the regulatory process rather than passing it onto legislators, thus forcing the latter to provide incentives for honesty strong enough to counterbalance interest group pressures. Subsequently, Besley and Coate (2003) analyzed the difference in regulatory policies enacted by elected versus appointed regulators.

While some social scientists within public choice focused especially on the policymaking process, others assessed patterns in the output of economic policymaking. McChesney (1987) suggested that scholars had to widen their analytical lens beyond those regulatory policies actually enacted, developing a model in which bureaucrats extract rents simply by threatening to regulate. Maxwell et al. (2000) presents an inverse argument, contending that industrial self-regulation is a strategic move intended to defer political regulation and becomes more stringent as the threat of regulation increases. Fiorina (1982) and McCubbins (1985) analyze how and

why legislatures delegate regulatory tasks to commissions, presenting a taxonomy of reasons that include decision-making costs, a desire to shift responsibility, and rent-seeking. Finally, Levine and Forrence (1990) develop a taxonomic analysis of diverse regulatory motives and how they will be linked to various policy instruments.²

Most recently, there has been a new wave of scholarly interest in the formal modeling of capture, often using mathematical frameworks far more sophisticated than those employed in the earlier scholarship on the subject. Laffont and Tirole (1991) analyze regulation as an agent-theoretic problem and consider its implications for the incentive scheme a natural monopolist will face. Grossman and Helpman (1994) develop a model of the formation of trade policy in which, analogous to Becker, interest groups use their campaign contributions to bid for protection. Martimort (1996), Martimort (1999), Laffont and Martimort (1999), and Willenborg (2000) analyze the separation of regulatory powers between agencies as a mechanism to reduce the risk of capture by introducing an information gap between the rival regulators. And Bardhan and Mookherjee (2000) model various factors that make local governments more or less vulnerable to capture.

In all of this large and diverse set of works analyzing policymaking processes and their implications for government economic interventions, the focus remains interest groups and their interactions with legislators; public choice scholars generally, though not uniformly, exclude voters from the frame of analysis. They do so because the dominant strand of theory of voter behavior has hypothesized that voters will rationally remove themselves from an influential role

² Similarly, Rasmusen and Zupan (1991) attempt to explain why U.S. regulatory history shows the extensive use, or overuse, of entry barriers, developing a model that shows both monopolists and policymakers will prefer this regulatory intervention.

in this process, thus making possible the brazen operation of interest groups. Accordingly, a second strand of public choice analysis has sought to elaborate and test this theory.

Voters and Rational Ignorance

One of the pillars of the political analysis of public choice is the rationally ignorant voter: interest groups can exert an outsized influence on the operation of government because voters know nothing about their depredations and have no incentives to attempt to stop them given the high cost of informed participation. Without this assumption, the conceptual architecture of the public choice account of the policymaking process would be seriously weakened. Accordingly, the motif of the rationally ignorant or rationally abstaining voter, which scholars often refer to as the rational choice model of voter behavior, frequently recurs in public choice research.

The basic principle of the voter as a rational, strategic decision-maker was first advanced by Downs, who argued that given the high cost of informing oneself and the low expected return to resources invested in becoming better-informed, the rational decision for voters is to remain uninformed. The problem, of course, is that a voter's level of relevant information is virtually impossible to observe. Analysts soon realized, however, that a virtually identical collective action argument could be applied to the decision to vote itself: high costs and a low expected return given the minimal incidence of an individual voter's decision in the overall voting process. Riker and Ordeshook (1968) formalized this insight into a simple model. Voters will vote if the gain is greater than the cost, where the gain is defined as a postulated consumption benefit from voting C, plus the expected benefit from casting the decisive vote for a winning candidate. However, other scholars pointed out that the probability of casting the decisive vote in most elections is tiny (Beck 1975; Chamberlain and Rothschild 1981; Margolis 1977), and the

potential costs diverse, including the opportunity cost of time required, transportation expenses, the transaction costs of registering, and a number of other potential inconveniences (Knack 1993; Rosenstone and Wolfinger 1978; Tollison and Willett 1973). Given these assumptions, a low-turnout voter equilibrium appears to be the only reasonable prediction, though it clearly contradicted available evidence. Accordingly, public choice scholars acknowledged the low-turnout equilibrium predicted by their models of voter decision to be an empirical failure (Brennan and Buchanan 1984; Hardin 1982; and Tullock 1967).

This problem of explaining the behavior of the voter—particularly the stubborn fact of reasonably high turnout—generated a vast literature from scholars of many persuasions across several disciplines, principally seeking to tweak the Rider-Ordeshook model by redefining the various terms, and to evaluate empirically what factors are the most accurate predictors of turnout.³ The rather dispiriting mismatch between the Downsian-derived theory of voter behavior and the everyday fact of voting in elections served to generate an entire subfield within political science devoted to attempting to develop a cogent explanation of turnout, a debate that still continues. Yet at the same time as the broader field has pursued a variety of different analytical and empirical strategies to explain turnover, public choice scholars continue to use the

³ Downs and Stigler (1972) argued that the probability that a vote was influential was higher than infinitesimal. Niemi (1976), Uhlaner (1989), Hinich (1981), Brunk (1980), Blais et al. (2000), and Feddersen and Sandroni (2001) focused on redefining the consumption term. Ledyard (1981, 1984), Owen and Grofman (1984), Palfrey and Rosenthal (1983, 1985) and Myerson (1988) sought to endogenize the probability a vote was decisive using game-theoretic models. Morton (1981, 1991), Shachar and Nalebuff (1999) and Feddersen (2004) developed group-based theories of turnout. A vast array of studies empirically test for a relationship between the perceived closeness of an election and turnout, or between cost and benefit and turnout: see for example Barzel and Silberberg (1973), Ashenfelter and Kelley (1975), Silberman and Durden (1975), Smith (1975), Tollison et al. (1975), Settle and Abrams (1976), Frohlich et. al. (1978), Rosenstone and Wolfinger (1978), Powell (1980), Filer and Kenny (1980), Rubinfeld and Thomas (1980), Sanders (1980), Crewe (1981), Thompson (1982), Bennett and Orzechowski (1983), Foster (1984), Darvish and Rosenberg (1988), Colomer (1991), Knack (1993, 1994), Heckelman (1995), Matsumaka (1995), and Blais (2000).

theory of rationally ignorant voters to buttress conclusions about the operation of the policymaking process.

The State and the Market

A third substantial body of literature in the broader public choice canon seeks to analyze the causes and consequences of government intervention in the economy. This work rounds out the earlier, more theoretical contributions by providing an overview of the potential consequences of the perversions of policymaking by interest groups, and has yielded an abundance of empirical studies analyzing the efficiency or welfare implications of regulatory policy. Within this family of empirical articles on regulation, the first wave focused largely on linking public choice theory to specific case studies, providing a (generally qualitative) outline of the theory's predictions and then a descriptive account of existing regulatory regimes that, as predicted by the theory, largely benefited the industries they were designed to control. They thus concluded on the basis of these examples that capture by interest groups explains the enactment of regulatory policy.

The most commonly cited case studies assessed regulatory policy in trucking, airlines, railroads, taxis and telecommunications, as in Huntington (1952), Kitch, Isaacson and Kasper (1971), Hilton (1972) Adams (1982), Hammond and Knott (1988), Hazlett (1990), Ellig (1991), and Benson (2002). These articles generally lacked formal empirical testing, and failed to engage in any systematic search for disconfirming evidence. At times, the historical analysis seemed strongly guided by, if not dictated by, the expectations of the theory, as demonstrated in one recent analysis that criticizes Hazlett's argument regarding the determinants of the decisions of the Federal Radio Commission and presents a range of disconfirming evidence (Moss and Lackow 2007). This strain of analysis was also challenged by several additional case studies that

detailed instances in which commissions were not dominated by the industries they regulated.

Berry (1984) cites numerous studies in this regard, including work by Meier and Plumlee (1978), Welborn and Brown (1979), Anderson (1981), Katzman (1980), Wilson (1980), Culhane (1981), and Sanders (1981).

A second set of studies analyzed the operation of regulation in given industries in order to demonstrate prejudicial impact to consumers or the public welfare, usually in the form of higher prices, higher profits, or more limited competition, again largely focusing on the same handful of industries. Joskow and Rose (1989) furnish an excellent overview of these studies, including their methodological approaches and the various questions they addressed. As they note, Bernstein (1952) was the first to publish a general study of regulatory commissions, arguing that they were subservient to the regulated industries. Gerwig (1962), MacAvoy (1971), and MacAvoy and Pindyck (1973) found that the regulation of natural gas induced shortages and increased cost. Peltzman (1965) contended that bank regulation decreased entry, though this conclusion was contested by Edwards and Edwards (1974), while Black and Strahan (2001) found that regulated banks shared rents they earned with male workers only. Demsetz (1968) published an influential article criticizing the rationale for regulating utilities as a supposed natural monopoly. Sloss (1970) and Kim (1984) found that the cost of trucking freight services was higher in regulated provinces in Canada, while Annable (1973), Moore (1978), Frew (1981) Rose (1985, 1987), and Daniel and Kleit (1995) showed that trucking regulation increased rates, generating rents that accrued to trucking companies and their unions. Grabowski, Vernon and Thomas (1978) and Danzon and Chao (2000) argued that pharmaceutical regulation suppressed innovation and limited competition. Farris (1969), Gellman (1971), Noll (1971), Jordan (1972) and Prager (1989) presented evidence regarding the inefficiency of regulation in surface freight

transportation industries. Keeler (1972), Hendricks et. al. (1980), Olson and Trapani (1981) and Graham et. al. (1983) argued that airline regulation had negative welfare effects and increased union power. A long series of works analyzed inefficiency in railroad regulation, differing as to its magnitude but generally agreeing that regulatory intervention was on the whole undesirable (Spann and Erickson 1970; Zerbe 1980; Golbe 1983; Keeler 1983; Braeutigam and Noll 1984; Boyer 1987). And this lengthy list is merely a summary of the better-known work on major industries.⁴

In this substantial body of research, the vast majority of studies confirmed the Stiglerian-public choice hypothesis of regulation as a mechanism to distribute rents to powerful interest groups, with only a handful of dissenters.⁵ The conclusion was similar to that drawn above: the negative welfare consequences of regulation, whatever their manifestation, are presented as evidence of the flaws inherent in, and particularly the interest-group dominance of, the policymaking process.

A third area that attracted considerable attention from public choice scholars interested in regulatory outcomes involved regulation at the state (or provincial) level. Here, researchers sought to take advantage of the natural experiment that states can provide: given that they frequently face similar regulatory dilemmas, comparing fifty responses in fifty different

⁴ Other work examined a variety of less prominent industries, including milk (Brinegar 1957; Carter 1957; Kessel 1967; Ippolito and Masson 1978; Masson and DeBrock 1980), automobile safety regulation (Peltzman 1975), electricity (Sampson 1958; Jarrell 1978; Joskow et. al. 1989; White et al. 1996), moter carriers (Carter 1958; Maxwell 1960), automobile distribution (Smith 1982), insurance (Ippolito 1979; Frech and Samprone 1980), dry-cleaning (Plott 1965), hospitals (Sloan and Steinwald 1980), television (Comanor and Mitchell 1972; Crandall 1972; Webbink 1973; Fournier and Martin 1983; Krattenmaker and Powe 1994; Crawford 2000), mining (Kalt 1983), and medicine and its various subspecialties (Shepard 1978; DeVany, Gramm, Saving and Smithson 1982; Paul 1982; Haas-Wilson 1987; Kleiner and Kudrle 2000). Similar analyses were extended to the realm of non-industry specific “social regulation”: Ackerman and Hassler 1981, Magat et. al. 1986, and Crandall 1983 analyzed environmental regulation, Bartel and Thomas (1987) examined occupational safety and environmental regulations, and Morrison et al. (1999) evaluated airplane noise controls.

⁵ These are Edwards and Edwards (1974) on banking, Schwert (1977) on securities regulation, Leffler (1978) on medicine, Levin (1978) on surface freight regulation, Ippolito (1979) on insurance, Zerbe (1980) on the regulation of short-haul rail freight, Mayo and Otsuka (1991) on cable television, and Kridel et. al. (1996) on telecommunications.

institutional contexts offers considerable scope for analytical advances. In contrast to the previously cited studies that generally inferred conclusions about the nature of the policy-making process from an observation of the policies it generates and their economic impact, these articles attempted to examine directly those factors relevant to the formation of regulatory policy, particularly the role of interest groups.

Cross-state analyses performed along these lines have yielded mixed results as to the determinants of regulatory policy. In his seminal 1971 article, Stigler found the relative political influence of different professions to be positively correlated to the stringency of licensing laws, while Becker (1986) noted that dentists were less successful in winning government aid in the form of more restrictive licensing requirements when the overall rate of citizen participation, as proxied by voting rates, was higher. Williams and Matheny (1984), Caudill et al. (1993), Kaserman et al. (1993), Teske et al. (1994), Howard (1998), and Kroszner and Strahan (1999) likewise concluded in their data analysis that the strength of interest groups predicts regulatory outcomes. On the other hand, Berry (1979), Welborn and Brown (1979), Anderson (1981), Berry (1984), Moe (1985) and Teske (1991), found that institutional variables (such as government autonomy, resources and ideology) as well as the power of interest groups determine regulatory outcomes in electricity, labor disputes and telecommunications, respectively.

Most usefully, Gerber and Teske (2000) and Teske (2003) provided exhaustive overviews of a massive literature on regulatory outcomes in states that categorize a wide variety of studies (48 in the first article and 10 in the second) according to the dependent variable they study (regulation in which industry or ambit) and the various explanatory variables they consider, which include the strength of interest groups, the characteristics of the elected legislators, bureaucratic or institutional characteristics and economic and demographic variables. Their

findings demonstrate that a wide variety of factors have been identified as significant in explaining regulatory outcomes, including variables in each of the broad categories cited above. Thus the picture they paint is one of policymakers at times influenced by interest groups, but also driven by ideological preferences and faced with a wide variety of other constraints, from a concern for their reputation in the eyes of elite opinion to pressure from colleagues to dictates of the party.

In a sense, such a conclusion may be so general as to seem not particularly illuminating. It does, however, suggest that a theory of some complexity may be needed in order to accurately match the richness of the empirical data, and raises some questions as to whether one that hypothesizes interest groups as the sole (or dominant) determinant of political decision-making has sufficient predictive capacity, given the abundance of studies that identify other variables as significant in explaining political outcomes.

Criticisms of Public Choice

As public choice theory has matured and increased in influence, it has attracted a number of important criticisms, both in terms of its theoretical conceptualization and its empirical support. These objections center on three central points. First, the claim that utility maximization is the central motivation of political actors lacks coherence. Second, the meaning of capture, clearly a crucial analytical element of the public choice account of collective decision-making, remains unclear. Third, the theory has weak empirical foundations and little predictive power.

Maximizing What?

The public choice theory of politics manifests a fundamental ambiguity about the definition of utility and the utility-maximizing impulse that supposedly underlies all political motivations, a flaw that Green and Shapiro analyze as the competition between “thin” and “thick” accounts of individual rationality. Thin rational accounts of political actors suggest only that they should “efficiently employ the means available to achieve their ends,” or, in the parsing more popular with economists, maximize their utility. Thick-rational accounts make more specific assumptions about the content of that utility, and, in the context of models of political behavior, typically emphasize money, re-election and the holding of political office; they thereby discount the potential significance of any other motivation for political behavior, and reject any suggestion that such motivations could evolve over time. Green and Shapiro note that rational choice theorists vary in the type of rationality they assume, and there is frequently ambiguity about the type of rationality that is required to support a given conclusion (Green and Shapiro 1994).

I would suggest, however, that this critique is only part of the story. The analytical flaw, and rhetorical advantage, of public choice theory is that it claims to require only thin rationality or utility maximization—a definition of individual behavior so flexible and capacious that it remains impervious to contravening evidence—but invokes various forms of thick rationality in practice. As Kelman puts it, “the general public choice notion that public leaders (whether elected or appointed) seek to maximize anything resembling a narrowly specified wealth function is likewise hard to defend unless put in nonfalsifiable form (i.e., incorporating all ideological and desire-for-service variables within the selfish utility function)” (Kelman 1988, 217-8). In fact, when outlining their theory of political motivation, public choice theorists do employ precisely such “nonfalsifiable” formulations, and they defend them easily. In their

empirical analysis, however, they routinely equate utility with votes, reelection or money, an elision that receives no defense and frequently, not even any explicit acknowledgement. The ambiguity on this point is not incidental, but central to the theory's appeal: it serves to obscure a major, and potentially unresolvable, contradiction.

In their original works, Downs and Buchanan-Tullock appear to advocate a thin-rational account of human behavior, arguing that a model of individuals interacting in accordance with their specific preferences is useful to analyze exchange in both the political and economic realms and “we might assume that the representative or the average individual acts on the basis of the same over-all value scale when he participates in market activity and in political activity” (Buchanan and Tullock 1962, paragraph 3.3.8). They specifically note, however, that they do not assume strictly identical preferences in economic and political decision-making or the dominance of what they call “hedonistic” conceptions of utility in the public sphere. These various carefully parsed statements offer considerable room for analytical flexibility, and suggest that the preferences operating in political decision-making can be (and according to Buchanan 1954, should be) characterized as distinct from whatever utility-maximizing logic may be dominant in the market. However, the implicit assumptions used to sustain the central conclusions of public choice theory are in fact far stronger than these statements would suggest, and more difficult to defend, in analyses of both political actors and voters. Here, I will outline the operation of differing concepts of utility in each separately.

First, consider the analysis of political actors within the government, whether legislators or regulators. Stigler does not even address the conundrum of defining their utility, simply asserting that “the industry which seeks regulation must be prepared to pay with the two things a party needs: votes and resources” (Stigler 1971, 12). Becker, on the other hand, employs the

same general formulation favored by Buchanan and Tullock, stating that, “The economic approach to political behavior assumes that actual political choices are determined by the efforts of individuals and groups to further their own interests” (Becker 1983, 371). Such a statement seems unimpeachably plausible, yet it is also vague. Clearly, a more specific account of the motivations of the regulators underpins the analysis that follows: interest groups undertake expenditures on political influence, including “maintaining a lobby, attracting favorable votes, issuing pamphlets, contributing to campaign expenditures, cultivating bureaucrats and politicians.” For Becker, then, all of this activity somehow increases the utility of the policymaker, inducing him or her to cooperate with the interest group and, thereby leading to the implementation of perverse public policy (Becker 1983, 377). There is no obvious reason to rule out the presumption that at least some interest groups or legislators view emotional commitments, or ideology, or the considered recommendations of policy experts, as one element of their motivational calculation. However, public choice scholars routinely ignore this possibility.

Similarly, Tollison states in his overview of public choice and legislation that “Politicians are like the rest of us: their personal, private interests are their fundamental concern” (Tollison 1988, 362). Again, a narrow conception of the utility “wants” of political actors generates the conclusion that capture is a dominant, or even inevitable, political phenomenon; if legislators were at least partly concerned with some broader interest, or some general principle, the conclusions would be quite different. As Croley puts it, “the legislator motivation claim is similarly indispensable to the interest group theory’s capture vision. Were legislators by and large immune to interest groups’ regulatory preferences, it would not matter much how lopsided interest-group activity was” (Croley 1988, 16).

There are, of course, works that do consistently apply a more sophisticated theory of political motivation. Niskanen in his classic work on bureaucracy argues that bureaucrats' core objective is the maximization of their budget, given that any conception they have of serving the public interest will inevitably be narrowed to the fulfillment of a particular function that requires budgetary power (Niskanen 1971); this approach paralleled a similar assumption popular in the literature at the time that managers inevitably sought to increase the size of their firms. The budgetary-maximizing objective has subsequently been employed by, among others, Laffont and Tirole (1991). De Alessi (1974) and Russell and Shelton (1974) hypothesize a utility function encompassing both pecuniary and non-pecuniary sources, while Evans and Gerber (1988) likewise employ a more complex objective function that incorporates pressure from many pressure groups placed on the regulator, and Grossman and Helpman (1996) balance consumer interests with income in their regulator's objective function.

However, most public choice analyses follow the example of Stigler, Becker, Tollison and other "founders" of public choice, employing a certain degree of analytical subterfuge. The most common maneuver is to assert the broadest and most unobjectionable theory of political behavior -- utility maximization -- while implicitly assuming a much more specific, and questionable, account without any explicit justification. Eckert, for example, argues that the regulator is a utility-maximizer, which he defines as "a function of numerous items such as his personal prestige, wealth, convenience, and working conditions, and his desire to please other officials and voters, expand his agency's budget, and implement his conception of the public interests" (Eckert 1973, 83-4). Such a nuanced definition will attract little criticism. However, he then tests the validity of his theory by analyzing how regulators respond to different incentives to increase their salaries, implicitly conflating utility with money—an assumption

likewise employed without any justification by McChesney (1987). This assertion appears problematic: individuals seeking solely to maximize their lifetime earnings might reasonably be expected to select a field other than politics initially (Kelman 1988).

Brock and Magee (1978), Barke and Riker (1982) and Abrams and Settle (1986, 2004), on the other hand, equate utility with reelection in their analysis without justifying their assumption or even noting that is potentially implausible. First, this presumption is contradicted by the evidence of voluntary retirement, and second, it would seem to beg the question of some more fundamental motivation. It is difficult to assert that politicians seek political office only to continually attempt to maximize their utility by remaining in office, without specifying what exactly is to be gained from this status in the first place—and if one asserts that this gain is monetary, the conceptual thicket outlined above must be confronted (Croley 1988, 42). Thus combining the two motivations does not seem like a particularly promising strategy either.⁶

The problem is neatly summarized by Wilson, who notes that theorizing that politicians' or regulators' behavior serves "personal interests" represents a trivial and "nearly useless" interpretation of the theory (Wilson 1980, 361). In this sense, everyone's behavior serves his or her perceived personal interests, somehow defined, all the time. The theory may be true, but it is of little interest and has no predictive power. However, the various attempts by public choice scholars to narrow their analytical focus to money or reelection have serious limitations. A more realistic account of political behavior, more specific than utility maximization, must inevitably concede that political actors are driven by a spectrum of often changing motivations, personal, ideological and pecuniary.

⁶ As already noted, Stigler simply defines parties to be seeking money and votes, an assumption echoed by Peltzman (1976) and applied to all "regulators," not merely parties. While implicitly suggesting that political actors are motivated by money and reelection, the two authors seem to be open to both sets of criticisms, in that neither half of their utility formulation is particularly cogent.

Even public choice theorists themselves appear at times not to find this account of political motivation particularly convincing, or encounter contradictions when attempting to deploy it. The most frequent stumbling-block, somewhat ironically, is their own policy success. Although most historians would identify public choice theorists as a central force behind the deregulation movement, their own theory offers little room for intellectual influence over policymakers, given that their presumed sources of utility admit of no mechanism by which theory might shape policy. As Tollison stated bluntly in his analysis of Chicago political economy, “another strand of argument. . .mostly associated with Stigler is that ideas do not matter” (Tollison 1989, 295). In 1978, Buchanan had rather dourly suggested that “government failure against standard efficiency norms may be demonstrated, analytically and empirically, but I see no basis for the faith that such demonstrations will magically produce institutional reform” (Buchanan 1978, 368).

Yet the triumph of deregulation, proudly claimed by Tullock, among others, as a public choice achievement and acknowledged as pivotal by a number of analysts of the deregulation movement, demonstrates that ideas can indeed make a difference (Derthick and Quirk 1985; Joskow and Noll 1981; Pasour 1992; Tullock 1988). In fact, near the end of the 1980s, Crew and Rowley (1988) continued to champion the role of economic education in promoting public choice theories and thus putting “many a special interest group to flight,” though their own logic presumes that only deployed caches of money can put special interest groups to flight. The most prominent theorists to acknowledge that deregulation was a heavy blow to public choice theory were Michael Levine, Roger Noll and Peltzman himself (Levine 1981; Peltzman et al. 1989), while Becker, Barke and Riker and Ellig sought to develop theories that could encompass

deregulation. Yet many of their colleagues seemed to remain unaware of their conceptual predicament.

An example of the analytical thicket that these potential contradictions can create can be found in the work of Abrams and Settle, who published two articles (1978 and 2004) arguing that two separate waves of campaign finance reform reflected nothing more than the self-interest of the Democratic party in cutting off higher financial flows to the Republicans and thus securing their own reelection. They concluded with a generalized criticism of such reforms, noting that money “isn’t really so bad” in American politics and that “any politician who compromises himself or herself by adopting a special-interest policy contrary to the public interest merely to achieve financial support runs the risk of exposure and humiliation. The ‘test of publicity’ acts to curb any abuses of money” (Abrams and Settle 2004, 395-6). This is a surprising statement when juxtaposed with a school of thought that considers the power of contributions to be near-absolute and legislators available for sale to the highest bidder.

Such examples effectively encapsulate the central criticism of the public choice theory of political motivation: politicians may gain utility from monetary advantage or reelection, to be sure, but any assertion that these goals exhaust their objectives immediately runs up against major conceptual challenges. If we wish to follow the summons of the public choice or rational choice theorists and treat political behavior like economic behavior and political actors like economic actors, we should acknowledge the obvious: individuals operating in the market gain utility from a variety of different sources beyond the purely pecuniary. Consumers do not uniformly buy the cheapest good; they leave money to their children; they donate to the Sierra Club or to Unicef. It is perfectly reasonable to conclude that individuals as politicians will be driven by a similarly diverse range of motivations.

As one might now expect, the public choice account of the voter's utility-maximization calculus similarly manifests a divergence between the capaciously broad models of the voter's rationality, and the very specific (and again, often acknowledged) assumptions about voter behavior that sustain public choice accounts of policymaking. As has already been outlined, theorists' responses to the empirical failings of the Rider-Ordeshook cost-benefit account of the voting decision have been to stretch the definitions of the costs and benefits in every possible direction so as to match the desired conclusion and the available evidence that many people vote, but not all. As Aldrich put it, "there is a genuine danger that interpreting cost and benefit terms broadly will make the rational choice explanation tautological. If everything is a cost or benefit, then the theory predicts everything, which is the same as predicting nothing" (Aldrich 1993, 275). Green and Shapiro likewise argue that public choice scholars stretch the theory of strategic voting to such an extent that it becomes impossible to falsify, noting that any observation could be consistent with "a theoretical framework that allows for post hoc insertion of idiosyncratic tastes, beliefs and probability assessments as explanatory devices" (Green and Shapiro 1994, 69). Why do some people vote while others do not? Rational choice voting theory replies that those who perceive the benefits to be greater than the costs vote—not a particularly illuminating insight.

For all the power of these criticisms, yet another important dimension of public choice's conceptual fuzziness has not been sufficiently acknowledged. The literature on voter behavior has continued to alter, and at times painfully twist, its basic model to fit the empirical evidence. However, the account that underlies the enormously influential public choice analyses of the policymaking process remains the original Downsian collective action-theory that predicts rational ignorance on the basis of a simple cost-benefit analysis. This analytical framework,

applied to the decision of whether or not to vote, has already been deemed inadequate to explain the phenomenon of turnout at any level above an extremely low one; indeed, this predictive failure is precisely what motivated the subsequent research. Voters, at least some of them, do vote, and thus disprove the collective action dilemma account of turnout. Accordingly, there seems to be no reason to conclude on the basis of the hypothesized existence of a similar dilemma that they must necessarily be rationally ignorant.

Scholars have shown, of course, that voters frequently lack good information about issues relevant to their welfare, though not uniformly so, and the reasons for this uneven and potentially inadequate level of information are no doubt of considerable analytical interest. However, the rational costs and benefits model of voting does not appear to be a particularly viable candidate for a concept to explain either the causes or the implications of voters' degree of ignorance. Incorrect about turnout, the Downsian model may offer no more reliable predictions about rational ignorance.

Nonetheless, public choice continues to rely on the rational ignorance assumption either explicitly (recall Becker's assertion of the irrelevance of voters for his analysis) or implicitly, when an analysis of voters is frequently excluded from an account of the interaction of interest groups and legislators that yields policy outcomes. This assumption is visible in much of the theoretical work already cited, including Stigler (1971), Landes and Posner (1975), Peltzman (1976), Barke and Riker (1982), Tollison (1988), and Crain and Tollison (1979a, 1979b), as well as an enormous empirical literature that seeks to evaluate interest group influence over legislators' votes. As Shughart et al. state, "the discussion of the political process tends to be carried out in terms of the interests of groups within the polity that either demand or supply wealth, and those of the legislators who broker the transfers" (Shughart et al. 1986, 962). In a

sense, then, the body of public choice literature on rational voting is not only flawed in itself. It also appears to serve as a screen of sorts for the fact that the essential theory of voter behavior relied on by the field has not changed since Downs—and that theory rests on extremely shaky empirical foundations.

Deconstructing Capture

“Capture” is a pervasive, if sometimes implicit, concept in public choice theories of regulation, a conceptual summary of all things perverse in economic policymaking. Why is regulation bad? In a word, capture. There is a certain difference in emphasis between the Virginia and Chicago theorists on this point, with the former generally less categorical in their accounts of the relationship between the state and interest groups. In their analysis of constitutional amendments, Crain and Tollison state simply that “in the interest-group theory of government, legislation is sold to groups who bid for the protective shelter of the state” (Crain and Tollison 1979a, 166). Tollison subsequently described the process in somewhat more neutral terms, postulating a demand for legislation by organized interest groups who can organize efficiently to gain benefits that are more than their costs, and a supply of legislation “grounded in the unorganized or relatively less-organized members of society” (Tollison 1988, 343). Legislators then act as brokers, pairing demand and supply, though as already noted he emphasizes that they pursue their narrow private interests, creating the opening for interest groups to fulfill those interests and so win the regulatory policy they seek.

The Chicago theorists, on the other hand, offer far more robust declarations of the analytical importance of capture. Stigler defines capture in a broad and straightforward manner, as the ability of “an industry (or other group of likeminded people) ... to use the state for its purposes,” or a process whereby an industry “is singled out by the state to be used for alien

purposes” (Stigler 1971, 4). He then cites various examples of subsidies, control over entry and other measures that he regards as an industry using the state for its purposes. However, Stigler shies away from articulating the mechanism that links the input, the industry that seeks to use the state for its purposes, and the output, the policy enacted according to the industry’s specifications. Peltzman and Becker, on the other hand, avoid the word capture and model a more sophisticated process of competition among different interest groups with distinct characteristics seeking benefits from the state. Peltzman credits Stigler as a joint contributor to the development of this model of multiple interest group competition, noting that “we simultaneously reached one of the conclusions elaborated here—that regulatory agencies will not exclusively serve a single economic interest” (Peltzman 1976, 211). Elaborating on a similar point, Becker maintains that “competition among these pressure groups for political influence determines the equilibrium structure of taxes, subsidies, and other political favors” (Becker 1983, 372), noting that “in effect, the analysis in this paper assumes that pressure groups can ‘purchase’ favorable votes with lobbying and other political activities” (Becker 1983, 392).

The more recent literature has refined these original models still further. Grossman and Helpman (1996) analyze a regulator whose objective function includes both campaign contributions and the welfare of the average voter, relaxing the self-referential utility assumption. Other works consider the implications of separation between regulatory agencies and a principal-agent relationship between legislators and regulators, dropping the unitary government assumption (Laffont and Martimort 1999; Laffont and Tirole 1991; Martimort 1996; Willenborg 2000). Given these considerable advances in theoretical and mathematical sophistication vis-à-vis Stigler’s original work, it is reasonable to ask: what remains of capture, and what, if any criticisms, can be leveled against it?

The economic theory regarding the influence of interest groups over the formation of government policy—a school of thought variously called capture theory, primarily with reference to Stigler, or the interest group theory of government, primarily with reference to Peltzman and Becker, or the economic theory of regulation—has two principal flaws. First, despite the development of models of interest group operation broad enough to account for an abundance of influences on government decisions and a wide range of regulatory outcomes, the assumptions underlying the bulk of both theoretical and empirical work on this topic clearly, and often explicitly, reflect Stigler’s original 1971 conclusion: the dominance of a single interest group of producers, with negative welfare implications for government intervention in the economy.⁷ This is an assumption that is somewhat outmoded, if not discredited, and has been so virtually since Peltzman disowned it in 1976. Yet there is a disjuncture between the increasing sophistication of the modeling framework, broad enough so as to be difficult to criticize, and the persistent assumptions—highly specific, and certainly open to criticism—that nonetheless remain widely in evidence in the broader literature. Even as our capacity to predict has grown, there persists a strong tendency to presume the outcome of regulatory analyses based on widely shared, but not explicitly defended, assumptions.

Second, the fundamental concept that there is a mechanism by which interest groups establish influence over legislators or regulators rests on empirical foundations that, at this point, are still relatively weak. Every model in this field, from Stigler forward, relies on the postulation of some sort of relationship between an interest group and a regulator that impacts policy decisions in some specific way. However, a strong and comprehensive case outlining the type of

⁷ See for example Abrams and Settle 2004; Bardhan and Mookherjee 2000; Barke and Riker 1982; Benson 2002; Coates 1995; Crew and Rowley 1988; Faure-Grimaud and Martimort 2003; Howard 1998; Jordan 1972; Krosner and Strahan 1999; Laffont and Martimort 1999; Martimort 1996; Martimort 1999; Teske 1991; Teske 2003; Williams and Matheny 1984.

evidence that would serve to confirm or disconfirm this hypothesis has not yet to emerge, and the existing literature addressing these questions suffers from some serious weaknesses. This point will be considered in the subsequent section on empirical weaknesses.

To begin with, consider the evolution of the concept of “capture.”⁸ Stigler provided a clear starting point in his analysis, with an unmistakable conclusion about the presence and negative implications of capture; despite his endorsement elsewhere of the view that all observed economic phenomena are the outcome of the behavior of utility-maximizing individuals and therefore must be efficient (Stigler 1976), in this case he unequivocally maintains that regulation is welfare-decreasing. This is not an anomaly but a reflection of the fundamental nature of the regulatory process and the incentives embodied within it. “The idealistic view of public regulation,” Stigler argues,

is deeply imbedded in professional economic thought. So many economists, for example, have denounced the ICC for its pro-railroad policies that this has become a cliché of the literature. This criticism seems to me exactly as appropriate as a criticism of the Great Atlantic and Pacific Tea Company for selling groceries, or as a criticism of a politician for currying popular support. . . Until the basic logic of political life is developed, reformers will be ill-equipped to use the state for their reforms, and victims of the pervasive use of the state’s support of special groups will be helpless to protect themselves. (Stigler 1971, 17-18)

Peltzman and Becker’s theories of multiple interest group interaction, on the other hand, do not suggest any particularly obvious conclusion regarding the nature of regulatory policy generated by this process, the identity of its beneficiaries or its welfare implications. Thus Becker observed that interest group competition could be efficient or nearly so, arguing that “policies that raise efficiency are likely to win out in the competition for influence because they

⁸ I will employ the term “capture theory” throughout the subsequent analysis to refer to Stigler, Peltzman, Becker and the later analyses of the operation of regulation cited here. Clearly, I recognize that some of these authors do not employ the concept or the term capture in the same way that Stigler does. However, I hope to make the case that this cluster of work has shared conceptual underpinnings justifying a common rubric.

produce gains rather than deadweight costs, so that groups benefited have the intrinsic advantage compared to groups harmed” (Becker 1983, 396). The tenor of this work largely accords with Macey’s assertion that “the economic theory of legislation focuses narrowly on the mechanics of the legislative process, not on whether the outcomes generated by this process offer a net benefit to society” (Macey 1988, 476). In fact, this agnosticism about the efficiency implications of regulation has attracted criticism from scholars disposed toward a more robust condemnation of government interference (Benson 2002, Crew and Rowley 1988). Moreover, with the caveat that larger groups, such as various classes of consumers, may find it more difficult and expensive to organize, this analytical framework suggests a genuine indeterminacy regarding regulatory outcomes: influence will be exerted by a variety of interest groups, including a range of different consumer and producer groups differentially affected by various proposals (including, for example, other sectors that supply or consume the inputs or outputs of the industry in question), and any one of them could potentially “win” depending on a host of organizational and political variables that scholars must specify and then measure. Such an account is broad enough to encompass many accounts of policymaking, and virtually any outcome: hence, perhaps, its appeal.

Yet the majority of subsequent work in this area, both theoretical and empirical, ignores this indeterminacy and take as preordained Stigler’s conclusion about business “using” the state. Through thirty years of analytical work on this topic, the basic assumption that regulation will inevitably serve as an instrument for the most powerful interest group in the sector it administers remains pervasive, permeating both the empirical and the theoretical literature. Consider the following examples. Williams and Matheny argue that, “The work of Stigler (1975), Bernstein (1955), Huntington (1952), Wilson (1980), and others has produced a consensus that, when

regulation bestows concentrated benefits on a group and its costs are dispersed, the regulated will come to dominate the regulatory process: organized representatives of the regulated ‘capture’ regulatory agencies” (Williams and Matheny 1984, 430). Likewise, Barke and Riker note, “It is true that in recent years one truly scientific theory about regulation has been propounded and investigated. This is the so-called capture theory in which the content of regulation is said to derive from the interests of the regulated. . .initially propounded, at least in the American context, by George Stigler. . .” (Barke and Riker 1988, 74-5). In the same year, Crew and Rowley state, “The rent-seeking insight, now widely disseminated through the literature of economics and public choice, is essentially destructive of contemporary ‘Chicago school’ complacency on the relationship between economic efficiency and institutional form” (Crew and Rowley 1988, 56).

The implicit invocation of capture as “truly scientific” continued in the following decade. Teske notes, “Economists went even further in the ‘theory of economic regulation’ (Stigler 1971; Peltzman 1976), arguing that regulation was established from the start to favor the regulated firms” (Teske 1991, 139). Martimort in his analysis of regulatory agencies jointly regulating a single firm argues that “separation is good since it reduces the regulators’ discretion and the scope for their non-benevolent behavior” (Martimort 1996, 674), while in a later work he states bluntly in the first sentence that “following Stigler (1971), it is now accepted that regulatory capture shapes policy outcomes” (Martimort 1999, 929). Citing Stigler, Peltzman, Becker, Winston, Tullock (all as cited in this paper) and Anne Krueger, Howard similarly contends that “[al]though some scholars argue that regulation benefits the public by addressing market failure and increasing efficiency, many others have demonstrated that regulation costs the public because it is often sought by regulated businesses to create entry barriers and, hence, to increase

their income,” (Howard 1998, 21). Kroszner and Strahan concur: “The private-interest theory of regulation, also called the economic theory, characterizes the regulatory process as one in which well-organized groups use the coercive power of the state to capture rents at the expense of more dispersed groups” (Kroszner and Strahan 1999, 1437.)

Even as Stigler’s landmark article approached its thirtieth anniversary, it appeared to remain as dominant as ever. Analyses focusing on the relationships between Congress and regulatory authorities note that “nonbenevolent regulators may use their power to pursue personal agendas, for example by colluding with the regulated firm” (Laffont and Martimort 1999, 233), and “independence increases the scope for capture by the interest group that he [the regulator] is supposed to regulate” (Faure-Grimaud and Martimort 2003, 413). Bardhan and Mookherjee, in their work on the capture of local government, evince surprise that no analyst has compared relative levels of capture at the central and local governments, but seek to remedy this lack with a model in which a single lobby of the rich makes campaign contributions to a party that maximizes reelection probability, essentially a reformulation of Stigler (Bardhan and Mookherjee 2000). Teske (2003) perhaps sums it up best, and most concisely: “The dominant view about much economic regulation over the past 25 years is that it was either a bad idea from the start or a practice that failed over time.” He cites as evidence for this observation Stigler (1971) and unnamed “subsequent Chicago-school arguments about regulation.”

This vast and sprawling literature, then, crucially rests on an inconsistency that parallels the difference between the “thick” rationality that many public choice theorists concede and the “thin” rationality that they actually rely on to generate their most important conclusions. Stigler’s simple model of capture has since been broadened by many theorists into a far more

flexible model that accommodates a complex interaction of interest groups and can explain a variety of outcomes. As such, the model is relatively easy to defend. Yet the majority of the time, the reigning presumption seems to be the same: regulatory capture by a small group of producers, and the search for its presence, reigns supreme. In some cases, moreover, belief in the theory of capture powerfully shapes interpretation of evidence (Moss and Lackow 2007).

Despite having largely moved beyond a model that considered the operation of only a single interest group and its dominance over regulatory decision making, public choice does not appear to have moved far beyond the conclusions of that model. If the field wishes to defend its theoretical sophistication by pointing out the many more complex models it has generated, its theorists should take seriously the indeterminacy inherent in those models, use them to generate predictions, and rigorously test those predictions. If they wish simply to employ the conclusions reached by the earliest, and simplest, models in the field, their reliance on those conclusions should be explicit, and explicitly defended. An ambiguous middle ground is not a viable option.

Empirical Weaknesses

Writings in public choice have long cited an abundance of regulatory studies that justify their theories and the policy conclusions that grow out of them. According to intellectual historians of the discipline, Stigler's 1971 article was itself inspired by earlier, empirical work published in the *Journal of Law and Economics* analyzing the operation of New Deal-era utilities regulation, particularly Stigler's joint work with Claire Friedland and the work of Harold Demsetz (Priest 1993.) The wide range of the literature in this area has contributed to the impression that the weight of evidence overwhelmingly supports the theory.

In fact, a substantial number of the empirical studies in this area either suffer from methodological weaknesses or are relatively limited in scope, insufficient to support the weight of their conclusions. As Williams and Matheny noted, “scholars have asserted rather than empirically tested their beliefs about several critical political and economic processes underlying the politics of social regulation” (Williams and Matheny 1984, 429). More bluntly, Barke and Riker contended that “most of the discussion consists of biased, ad hoc explanations of particular cases, with little effort to formulate and test alternative generalizations. We have, therefore, nothing but unresolved and unresolvable disputes about a more or less random collection of stories and some contradictory recipes about cause and effect” (Barke and Riker 1982, 73).

The majority of case studies cited in this paper fall into this category. Only rarely do these studies consider examples beyond the industries mentioned, or compare more than one possible explanation for the regulatory result observed. The focus on individual, industry-level studies allows for the invocation of idiosyncratic explanations for features of the regulatory process that prove difficult to explain. In the words of Richard Posner, a judge and law scholar highly sympathetic to public choice, “[i]t is, of course, a weakness rather than a strength in a theory that it is so elastic as to fit any body of data with which it is likely to be confronted. . . . Exceptions to the general rule that regulatory agencies are captured by the regulated firms are explained away by facile references to the personality of the legislators, public opinion, ignorance, folk wisdom, etc” (Posner 1974, 343-4). Although this critique, along with those of Williams and Matheny and Barke and Riker, emerged quite early in the evolution of the field, their core criticism remains valid: public choice is characterized by an excessive reliance on case studies tailored to meet the requirements of the theory, and a paucity of careful and systematic testing of a meaningfully formulated, and clearly falsifiable, null hypothesis.

The more formal empirical studies that public choice has generated also suffer from analogous flaws. Despite the persistent analytical importance of the concept of capture, a compelling account of how to identify it or its milder variants, such as interest group influence, remains elusive. The answers provided by the enormous literature to the three most important questions—how does capture occur, how do we know it has occurred, and what type of evidence would show that capture has *not* occurred—are varied, at times contradictory, and subject to serious challenge.

In general, the most common answers in the literature are that capture occurs via campaign contributions (Becker 1983; Jacobson 1980; Palda 1975) and proof that it occurs is found in the evidence that legislators vote for initiatives favored by interest groups that are strong in their constituencies, long a popular topic for study.⁹ A second strand of analysis directly addresses the role of contributions, with Hall and Wayman (1990), Langbein and Lotwis (1990), Steagall and Jennings (1996), Stratmann (1991, 1995) and Wilhite and Theilman (1987) finding a statistically significant relationship between contributions received from an interest group and votes cast in accordance with the group's policy preferences, while Wright (1990) contends lobbying contacts, rather than contributions, explain interest group influence.

However, no studies have firmly established that interest groups are the only or even the principal determinant of legislators' decisions. Kau and Rubin state that at the beginning of their research they were convinced that Stigler's hypothesis that "voting could be explained entirely on the basis of economic interests, and ideology would not be significant if all relevant economic variables were controlled" was correct (Kau and Rubin 1979, 381-2). Their empirical results,

⁹ See for example Danielsen and Rubin 1977; Kau and Rubin 1979; Jackson 1974; Kingdon 1973; Silberman and Durden 1976; Jarrell 1978; Oster 1980; Leone and Jackson 1981; Kalt 1981, 1982; Riddlesperger and King 1982; Crandall 1983; Peltzman 1984; Bailey 1985; Pashigian 1985; Peltzman 1985; Gilligan, Marshall and Weingast 1987; Coates 1995

however, indicate that ideology (or potentially some unobserved economic variable) is significant in legislative decision-making, and a number of other studies also find that ideology is significant and interest group influence limited.¹⁰ Bernstein and Anthony (1974), Bernstein and Horn (1981), McCormick and Black (1983), Fleisher (1985), Lindsay (1990) argue that voting on specific defense and energy bills reflects legislators' ideology rather than constituents' economic views; and Levitt (1996) estimates senators' utility function and concludes that "the senator's own ideology is the primary determinant of roll-call voting patterns" (Levitt 1996, 425). The already cited articles comparing regulatory policies enacted across states—rather than different votes across legislators—also frequently identify significant ideological variables.¹¹ This body of literature together appears to pose a significant challenge to the assumption that that legislators or regulators are exclusively self-interested rent-seekers, driven in their decision-making by purely pecuniary incentives within the constraints of the offers provided by interest groups.

As previously noted, there is also a persistent ambiguity regarding the question of whether interest groups capture legislators or bureaucratic regulators. Though the dominant strand of empirical work analyzes the role of interest groups in capturing legislators, another set of articles instead attempts to demonstrate the way in which largely unelected regulators make their decisions according to material or tenure incentives, suggesting that the object of analytical focus should be bureaucrats (Caudill et al. 1993, Eckert 1973, Hazlett 1990, Kaserman et al. 1984). In light of these two different sets of analyses, one might reasonably wonder if both

¹⁰ These include Chappell 1982; Davis and Porter 1989; Kau, Keenan and Rubin 1982; Miller and Stokes 1963; Nelson and Silberberg 1987; Poole and Daniels 1985; Poole and Rosenthal 1985; Wright 1985.

¹¹ In these studies, ideology is generally measured by the ratings given to legislators on the basis of their voting record by non-governmental organizations that advocate and lobby for a particular broad policy agenda, such as the American Civil Liberties Union, the National Rifle Association, etc. Thus "ideology" does not refer to an overarching theory of politics, as the term is generally used in more theoretical work, but rather the endorsement of various sets of policy goals, clearly identified with the left or the right in American politics.

legislators and regulators are available for interest group purchase, and if so, what happens if one interest group purchases the legislators and another, the bureaucrats. Imposing an identity of interest, one potential solution, seems no better; in the words of Barke and Riker, “we are uneasy about the facile assumption of an identity of interest at all levels of government” (Barke and Riker 1982, 76). The ambiguity opens the door to a certain empirical opportunism: by not specifying the identity of the appropriate target of capture analysis, scholars can cite a diverse range of evidence as proof that regulation serves the ends of interest groups, without ever making clear what theoretical account of the political process underlies these conclusions.

Perhaps most important, studies that seek to establish the relationship between interest groups and legislative decision-making by analyzing roll-call votes—or the relationship between interest groups and bureaucratic processes by analyzing commission rulings or the like—invite two fundamental methodological criticisms. First, a pattern of correlations between interest groups and votes does not, in and of itself, demonstrate a causal relationship between interest group influence and political decision-making; social science has long attempted to guard against such fallacious conflation of correlation and causation. Legislators cast many votes, they have many constituents, they receive many contributions, and they presumably have some opinions of their own. Inferring capture on a wide scale from voting patterns alone requires a risky, and arguably indefensible, analytical leap. In any given policy dispute, someone has to win and someone has to vote for the winner. Given that most legislators, if not all, have conflicting interest groups represented in their constituency and among their contributors, does voting in accordance with the preferences of any one of them constitute evidence of capture? Is it plausible to suppose that a legislator could last a term without casting some votes that line up with the interests or opinions of some subgroup of his constituents?

This weakness suggests that Peltzman and Becker's key insight—that there may be a relationship between the characteristics of interest groups, their size and organizational costs, and their political influence—is rarely tested adequately. In order to empirically validate this theory, it is not sufficient to show that legislators' votes are correlated with the policy preferences of one or two or more interest groups. Any model that took such a correlation as a confirmation of its predictions would be virtually non-falsifiable, and thus of little interest. A more realistic test of Peltzman-Becker requires developing an empirical framework that identifies how interest group characteristics affect their quotient of political influence, measures groups along those dimensions, generates predictions regarding which interest groups should be more influential and tests those predictions against the data regarding the relationship between different groups' policy preferences and the outcomes. Yet attempts to implement such a strategy are rare. Jordan (1972) proposed a test based on one element of industry structure (an oligopoly versus a natural monopoly) but did not implement it systematically, instead examining case-by-case evidence from four industries (electricity, natural gas, airlines and trucking). Caudill and his colleagues, after noting the inadequacy of most empirical studies seeking to show the influence of interest groups in regulatory outcomes involving rate-setting, nonetheless test a simple model that incorporates as characteristics of the industry only the rate of return requested and expenditures on lobbying the commission (Caudill et al. 1993). Such basic tests may not be sufficient.

A second key methodological flaw in the existing literature on the empirical foundations of capture concerns the fact that many analyses of roll-call votes for evidence of capture utilize a set of observations—individual votes cast by legislators—that are quite clearly endogenous. Tullock himself highlighted the importance of log-rolling or vote-trading in one of his earliest articles (Tullock 1959). A single legislator's different votes may be related to each other because

they represent elements in some broader trade such as those that Tullock hypothesizes, because they are cast on similar issues, or because (very plausibly) the legislator's opinions on a diverse set of topics are interrelated. Votes cast by different legislators could also be correlated if they are elements of a vote trade, or if they are jointly determined (or partially determined) by a common external force. A typical analysis of roll-call votes considers a certain number of votes cast over a given period or on a given topic, and thus both "within legislator" and "between legislator" correlations could be operative. If any or all of these premises are correct, then a standard regression of roll-call votes on measures of interest-group strength and ideological orientation will be flawed unless the analyst adequately addresses the presence of an abundance of unobserved factors that potentially confound the relationship of interest. A critique of such studies of the relationship between roll call voting and various explanatory factors notes that "both the independent and dependent variables represent the same dimensions or highly related dimensions in the underlying structure of legislative roll call voting" (Jackson and Kingdon 1992, 809). Scholars have yet to fully resolve the challenges posed by the potential presence of this form of endogeneity.

To sum up: the evidence that interest groups are a primary determinant of government regulatory decisions is mixed at best, and open to some potentially serious methodological criticisms. Moreover, the empirical strategy implemented by the proponents of capture theory is, in general, characterized by the troubling absence of a search for disconfirming evidence. Public choice theorists point to correlations between various measures of interest group activity and decisions of government actors of various types as evidence of capture, often without a systematic attempt to examine and test other hypotheses. In some cases, the definition of

“interest group influence” has been made so broad that it is not clear that the theory is even falsifiable.

Given these weaknesses, a more promising avenue of exploration in the field may be the studies already cited that compare regulatory outcomes and causal factors across states and enable the formulation of systematic theories of the determinants of regulatory policy that can be tested. There are still a number of potential methodological quibbles with this type of comparative study of state regulatory policymaking. The validity of the variables chosen to represent various institutional or ideological features or the strength of interest groups is not always clear. The models are not necessarily capable of capturing the nuances distinguishing one regulatory policy from another. And again, there is the risk of inappropriately inferring causality, a risk that appears particularly acute given that the theory of capture such empirical studies could serve to test remains weak.

Yet it is worth recalling that surveys of work in this area provided an extensive taxonomy of ideological, demographic, and structural variables that have been deemed significant, along with measures of interest group strength, in explaining regulatory outcomes in various contexts. In order to be empirically validated, purist capture and interest-group theorists must be able to show in some way that a more precise formulation would demonstrate all of these variables to be insignificant or less significant than interest group variables—a formidable hurdle indeed.

A Response to Skeptics

Before concluding, it seems appropriate to pre-emptively address two of the most prevalent and far-reaching critiques that can potentially be made of the argument I have presented here: first, that public choice is no longer a relevant or important school of thought in

social science, having since been superseded, and second, that I inappropriately characterize its membership, subsuming under the public choice label scholars who would not define themselves as such.

First, some will argue that public choice is, for all intents and purposes, outmoded and discredited, and that the most current work in academia has moved on. No part of my argument can or should be construed as an assertion that public choice is the only school of thought in economics, political science, or law that analyzes the formation and impact of economic policy, or that these disciplines have failed to evolve since the genesis of public choice. On the contrary, there has been a profusion of new work in a variety of different fields over the last decades analyzing regulatory policy, the political determinants of economic policy, and the relationship between voters, interest groups and political agents of various times. Some of this work has been grouped under political economy, some under the new institutional economics, and some under positive political theory, while other research manifests similar scholarly preoccupations without an explicit affiliation to a particular “school.” The intellectual diversity and richness of this work should, of course, be acknowledged. My intention is not to minimize its significance—nor do I wish to criticize the general methodological framework of applying techniques of formal analysis traditionally associated with economics to the consideration of political processes. This cross-disciplinary fertilization has already proven to be enormously useful, and my criticisms address a specific set of ancillary assumptions that have been imposed within this methodological framework, not that selection of analytical tools itself.

Nonetheless, any serious consideration of the state of social science and particularly economics today must concede that the public choice view of regulation remains an enormously

influential one. The influential textbook on regulation by Jean-Jacques Laffont and Jean Tirole identifies public choice as one of two major extent theories explaining the pattern of government intervention in industry, stating that “the 'public interest' theory emphasizes the government's role in correcting market imperfections such as monopoly pricing and environmental externalities. . .[t]he 'capture' or 'interest group' theory emphasizes the role of interest groups in the formation of public policy” (Laffont and Tirole 1993, 475). Djankov, La Porta, Lopez-de-Silanes and Shleifer (2002) likewise declare that there are two theories of regulation, public interest and public choice, and argue that a comparative study of regulations governing business start-ups favors a public choice theory. Moreover, they perhaps unintentionally highlight the recent dominance of public choice by citing only a single reference for the public interest theory of regulation, Pigou’s seventy-year-old work from 1938, seemingly a relatively clear indication of which scholarly area has been most dominant over the last decades.

It is clear that public choice is not the only relevant school analyzing regulation. Yet it seems equally clear that it remains important and influential, and **is widely acknowledged to be so among practitioners of the discipline, surveying their own field**. This alone seems sufficient to indicate that it is a worthy object of analytical consideration and, if necessary, criticism.

A second potential objection holds that I inappropriately characterize scholars as “public choice” when they are in fact no such thing. Clearly, there are no public choice membership cards; academic schools are flexible constructions, and there is disagreement about their boundaries. Some of the work cited here may not have been explicitly identified as public choice by its authors, and they may object to that identification. My preoccupation is not with

particular labels, but with a certain set of analytical assumptions and practices, generating a body of work that has been widely characterized as “public choice”, and I will make a case for how the various works addressed here share, in whole or in part, those assumptions.

The evolution of public choice at Virginia and Chicago and its emphasis on “public interest” as a united school of thought—a construction that many would argue was somewhat artificial—serves itself to indicate that a certain degree of generalization is inevitable in academic debate, even if it is at times contestable. It is precisely that type of identifying generalization I am employing here, in a way that I will outline and defend in the remainder of this paper. While conceding that some of these assertions may be controversial, I would contend that the basic task of identifying, describing and critiquing a certain school of analysis is not only unobjectionable, but highly necessary.

Conclusion

If one traces public choice back to the work of Duncan Black and Anthony Downs, it has now existed as a school of thought for more than fifty years. If one likewise considers Posner’s 1974 article to be the first articulation of the major flaws of public choice theory—perhaps somewhat surprisingly, given that his general intellectual and ideological sympathies certainly lay with the project—it has now been more than thirty years since the first critiques. A number of other works subsequently expanded this analysis of the weaknesses of public choice, implicitly or explicitly suggesting a research agenda to address its failings. Yet despite this significant body of criticism, at a variety of different levels, the public choice account of policymaking still remains enormously influential both inside and outside the academy. The

defeat of public interest ideas hailed by writers such as Tollison is by no means an exaggeration, and it has profoundly shaped the dominant perception of the nature of government.

In the words of one analyst, “Governmental efforts are viewed as inevitably flawed. . .all things ‘public’ have become suspect. For some, the only public purpose worthy of respect seems to be the elimination of the public sector” (Mashaw 1997, 23). This view, dominant in both politics and the academy, has been crucial in sustaining the wave of privatization and deregulation evident in the American economy over the last two decades. Its emergence in the late ’70s and early ’80s coincided with a broader wave of governmental disillusionment in the aftermath of the Watergate scandal, the debacle of the Vietnam war, and the failure of many Great Society programs, paving the way for a broad rollback of governmental functions under the Reagan administration. In the initial years, deregulation was explicitly linked to the work of public choice analysts, as already noted. More recently, the connection between intellectual and political history has been rarely noted, even as it has become even more powerful. Assumptions about the near universal pursuit of narrow self-interest, about the uninformed voter, about the inevitability of capture, have become woven into the basic fabric of political common sense.

This panorama raises further questions: why does public choice continue to be so successful despite its many weaknesses? Why have its critics been so ineffective? Why are those critics so few in number? Public choice theorists themselves might be sympathetic to the argument that their would-be academic critics face a collective action problem. While theorists of public choice have clustered at recognizable academic homes, particularly at George Mason and Chicago, rely on *Public Choice* and *The Journal of Law and Economics* to provide a receptive outlet for their work, credit the inspiration provided by a few respected founding scholars, and steadily add to their ever-expanding corpus of work, public choice skeptics appear

to be scattered and without any institutional focal point. The dispersion of authors across disciplines—economics, political science, history, sociology and law—appears to have exacerbated the lack of coordination, in marked contrast to public choice, where disciplinary diversity appears to have largely been a strength, enabling its rapid ascent in not one but several fields. Most of the authors who have addressed the flaws of public choice have published on the subject only once or twice, and their critiques largely exist in isolation from each other, without extensive cross-citations. There have been no attempts to unify or synthesize the existing critiques and use them to generate a coherent research agenda that could begin to generate an alternative to public choice—likely the most important reason for its continued dominance. Equally important, few scholars have attempted to illuminate this debate by engaging in cross-country comparisons and testing the ability of public choice as well as competing theories to explain the vast differences in regulatory regimes between polities. Such structural flaws have left the critics of public choice at a substantial disadvantage vis-à-vis their better-established and better-organized colleagues.

Beyond such organizational problems, however, one can speculate that public choice theory's tenacity is due partly to the fact that it resonates with a widespread sense that American legislative and regulatory processes lack adequate transparency and equality of access. Frequent cases of legislator misconduct, the influence of lobbyists, and the often incestuous relationships among such lobbyists, legislators and bureaucrats all appear to lend weight to the argument that some narrowly defined groups, whose interests do not coincide with those of the majority of voters, wield excessive control over the operations of government and thus generate outcomes detrimental to the public welfare. These apprehensions are legitimate, and in many cases rooted in abuses that may require a remedy. However, this alone is not adequate support for a *general*

theory of the political process and the economic role of the state. The sense that public choice addresses aspects of the operation of the political system that we find troubling may have served as a shield for the many flaws in the way in which it addresses those challenges.

Regardless of the explanation for public choice's continuing appeal, our understanding of legislative and regulatory decision-making (and of democracy itself) remains far from complete. The existing theories lack both internal coherence and strong empirical foundations. Further work in this area—particularly coordinated and sustained work across the social sciences—is urgently needed if we are to move beyond the current flawed consensus.

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