Review Article:

Efficiency in Politics: Competing Economic Approaches

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The article reviews four recent economic books on efficiency in democracy. Special emphasis is given to two ambitious competing approaches: Mancur Olson’s theory of encompassing interests and Donald Wittman’s Myth of Democratic Failure. It is argued that the thesis that democracy should generally be presumed to be efficient is problematic in several respects. Sweeping conclusions are reached by virtue of a superficial application of economic analogies, which abstracts away many crucial characteristics of democratic politics, such as information imperfections, weak incentive structures, and collective action failures. The basic explanation for efficient outcomes may have more to do with the size of rulers’ stakes in the economy than with voluntary Coasean bargaining.

Political scientists’ thinking about democratic politics has been fundamentally influenced by economists over the past few decades. Early individual contributions by Ronald Coase, Mancur Olson and others defined the agenda. The influential Chicago school claimed, roughly, that the economic sphere of life is essentially efficient, but it held the opposite view on the political sphere, characterized as it is by monopolies, rent seeking and voter ignorance. The school of institutional economics has qualified this lopsided assessment. It demonstrated that because of various transaction costs, political institutions, and hierarchical organizations more generally, often perform better than markets in the activities for which they are set up.

This article reviews some divergent yet interrelated economic approaches that have been developed in recent years. While building on the earlier work, they claim to propose new arguments on the old issue of efficiency in democracy. In The Making of Economic Policy (MEP), Avinash Dixit (1996) takes up the second school’s insights to develop a framework for understanding more explicitly how transaction costs operate in politics. In his APSA-award winning The Myth of Democratic Failure (MDF) Donald Wittman (1995) synthetically sweeps together both Coase’s insights and those of the two schools, to reach radical and counterintuitive conclusions. In Power and Prosperity (PP), the late Mancur Olson (2000) proposes an alternative theory based on coercive power and collective action. A Not-So-Dismal Science (NSDS), edited by Olson and Satu Käkhönen (2000), collects various contributions that are much influenced by Olson’s work.
Big Stakes: Olson’s (Partly) New Theory

Why are so few countries in the world prosperous, despite the pervasiveness of markets even in poor countries? Both PP and Olson’s two related chapters in NSDS set out to answer this question. Olson argues that coercive authority by government, that is, power, is an indispensable part of any such answer. Compare a state under the anarchy of many roving bandits with one ruled by a single stationary bandit. The latter state will be more prosperous, because a stationary autocrat, even if utterly selfish, has strong incentives to be ‘a benefactor to those he robs,’ by promoting the total social product of the society under his rule. Given disincentive effects, reducing taxes enhances the autocrat’s base for tax theft. So does providing domestic order and other output-enhancing public goods.

Democratic majorities are unlike autocrats in that they also earn a substantial share of society’s market income, in addition to controlling tax collection. Since this gives them an even stronger interest in society’s productivity, they will impose fewer taxes and spend more on public goods than autocrats do. Unless they are captured by powerful small interests, democracies will therefore be more conducive to prosperity than autocracies. Olson’s Laffer-curve logic basically follows a simple reciprocal rule. Suppose the ruler’s share of any increase in total output is \( S \). At the margin, he will then impose an extra tax pound until output falls by \( 1/S \). Similarly, he will spend an extra pound on public goods up to the point where output increases by \( 1/S \).

For instance, imagine an autocrat who taxes away one-fourth of society’s output. He will then continue imposing an extra tax pound until output goes down by four pounds. Now suppose this autocrat were replaced by a homogenous democratic majority that earns, in addition, half of total output. Then it will impose an extra tax pound only until output goes down by four-thirds of a pound. But if instead that majority were a motley crew composed of ten different interest groups each earning, say, one-twentieth of output, then none of those groups would have a serious incentive, individually, to curb taxation. Hence Olson’s first main thesis: the key to prosperity lies in whether those who hold coercive power have an encompassing rather than a narrow stake in the economy.

Yet, on the prospects for ‘efficient’ transitions from autocracy to democracy, there is a sobering caveat. Overthrowing a dictator is a public good. While enhancing aggregate welfare, it also imposes prohibitively high costs on individual revolutionaries. Moreover, if the latter nevertheless organize successfully, they will want to establish an autocrat-like stake themselves. Hence, transitions depend on accidents of history such as a contingent balance of power between revolutionaries. J. Bradford De Long (NSDS) lists a number of such fortuitous accidents. Because princes care for glory and merchants for commerce, the former impose higher taxes. This explains why the Flemish and Italian city states of medieval Europe were rich – but only thanks to the temporary military ineptness of their neighbouring princes. Later on, the Spanish monarchy succumbed to ‘imperial overstretch’: by imposing ever more distortionary taxes to finance its military conquests, it caused its own economic decline. Only the British empire escaped this fate –
again thanks to a lucky constellation of factors, such as national unity in war against the French and an unprecedented demographic boom.

Olson’s second main thesis regards the specific contribution of government in promoting prosperity. Law and order provided by 'market-augmenting' governments and independent legal systems are the key here. Secure and independently enforced individual rights make credible commitments possible. Hence, they allow gains from trade to be made also in transactions that are more complex than the simple, self-enforcing ones typically found in poorer countries. Pranab Bardhan (NSDS) specifies this by focusing on the degree of homogeneity in society and on the quality and credibility, rather than the extent, of state intervention. For instance, because society in poverty-struck India is more conflict-ridden than in many East Asian tiger economies, the Indian governmental system is more decentralized. The high number of institutional veto points, in turn, induces decentralized bribe-taking, which is particularly harmful because at each point officials have only a small stake in the economy.

Governments that do not augment market exchange, however, will over time become dominated by special interest groups. In the early years of Stalin’s rule, Soviet-type economies were characterized by high growth rates. Olson suggests that this was due to sophisticated allocation and taxation mechanisms of a kind that only dictators can afford, such as progressive piece rates that provide strong incentives to work very long hours. Somewhat less innovatively, he goes on to explain why over time these societies slumped into sclerosis. The small number of large state enterprises became increasingly able to collude and obtain rents, rather than to produce efficiently. During post-communist transition, moreover, precisely these enterprises were best organized for the pursuit of particularistic benefits, again impeding aggregate welfare.

As it happens, the gist of these arguments on Soviet systems and property rights was already prevalent in work done two decades ago, most notably János Kornai’s (1980) theory of the soft budget constraint and Douglass North’s (1981) neoclassical theory of the state. The latter, for instance, was centered around the very paradox (p. 20) that ‘The existence of the state is essential for economic growth; the state, however, is the source of man-made economic decline.’ North subsequently went on (1990, p. 16) to argue explicitly that institutions are not usually created to be socially efficient, but rather to serve the interests of the most powerful. The link with Olson’s own arguments should be obvious, yet Olson’s references to North’s (or Kornai’s) work are scarce and sketchy at best.

In contrast, the ‘big stakes’ idea is vintage Olson. Here lies the author’s new claim to fame: a simple yet powerful hypothesis, extending his original (1981, p. 43) argument about distributional coalitions, with potentially important implications. These range from the optimal level of union bargaining to the optimal design of federal, electoral, and taxation systems. The stakes idea also qualifies the traditional public choice conception of rulers as Leviathan-type tax predators, without even slightly watering down its assumption that rulers are self-interested (for more extreme positions either way, see Buchanan and Musgrave, 2000). Lastly, Olson’s theory throws an interesting new light on a competing strand of theses on political efficiency. To these I now turn.
Coasean (or Panglossian) Approaches to Efficiency

In what was to become one of the most cited economics articles ever, Ronald Coase ([1960] 1988) argued that in the presence of an externality, voluntary bargaining between the parties involved will lead to an efficient allocation of resources, regardless of the allocation of property rights. Only high transaction costs could prevent an efficient outcome. The Coase theorem lies at the basis of both the Chicago and the transaction costs approaches to political economy. In MDF, Donald Wittman presents himself as the standard-bearer of a bold new thesis that goes beyond these approaches. Wittman agrees with the first school that economic markets work well. But in addition he claims vigorously that ‘political markets’ work well too (see also Wittman, 1989a, b, 1999). MDF sets out

... to (1) cure the schizophrenia facing most economists, who believe that economic markets work well, but political markets work poorly, (2) overcome the blindness of many sociologists to the full implications of rational behavior and competition, (3) help political science (a field lacking in a coherent understanding of democratic phenomena [sic]) to develop a consistent theoretical approach (p. 2).

Instead, Wittman puts forward his own benchmark theory: ‘this book develops an invisible-hand theory of efficient democratic markets and provides an analytic framework for finding errors in models of political-market failure’ (p. 3). Unequivocal about how the implications of this theory are to be understood, the author advocates a methodological redirection for the study of politics, centered around a new research agenda: ‘If my thesis is accepted, the nature of economic analysis of political behavior is altered’ (p. 5). And: ‘Henceforth, the burden of proof will be on those who argue that democratic political markets are inefficient’ (p. 2).

By any yardstick, these are sweeping claims. Broken down to their very core, they amount to a mechanical application of the Coase theorem to the domain of politics. The straightforward analogy between economic and political markets forms the heart of Wittman’s theory: ‘The only difference between this version and Coase’s is where the deals are made. In his version, they are made in the private sector; in mine, they are made in the public sector. But in both versions the outcome is wealth maximizing.’ (pp. 31–2). Time and again, Wittman’s arguments are built around phrases like ‘We start with the Coase theorem’ (p. 160), ‘This analysis is a variant on Coase (1960)’ (p. 167, fn.), and ‘The Coase theorem again applies’ (p. 176).

The reasoning goes as follows. Just like economic markets, democratic politics is free and competitive, which will induce political agents to maximize the wealth of their principals. Moreover, many political institutions are set up to reduce transaction costs. So as in private exchange, democracies will lead to efficient outcomes. Suppose that some outcome B would entail higher aggregate welfare than the prevailing outcome A. Then there exists some distribution of the gains of moving from A to B that is Pareto-improving: it would leave nobody worse off. Citizens, or political entrepreneurs eager to get their votes, would therefore have strong incentives to bring about this efficient move, unless the transaction costs involved are too high – in which case A is not ‘really’ less efficient.
The consistent pursuit of this logic thus inevitably leads to the very counterintuitive conclusion that whatever exists in democracy – and especially, whatever persists – has to be efficient. In Olson’s paraphrasing of this reasoning: there cannot be any ‘big bills’ left on democracy’s sidewalks to pick up, since if there were, someone would have picked them up already. This view, which forms the core of the standard economic approach to trade and finance, has acquired forceful proponents outside these areas as well. For instance, George Stigler (1992, p. 459) builds on the Coase theorem to conclude that ‘all durable institutions, including common and statute laws, must be efficient.’ Acknowledging that the US government’s subsidies to sugar producers involve a 75 percent dead weight loss, he nevertheless proposes (p. 459) that ‘Lacking a cheaper way of achieving this domestic subsidy, our sugar program is efficient. This program is more than fifty years old – it has met the test of time.’ Others similarly argue in favour of a presumption of wealth-maximizing formats for common laws (Posner, 1992) and governmental structures (Breton, 1993, 1999). In the magic black hat of Wittman and colleagues, Prof. Coase turns into Voltaire’s Dr. Pangloss: tout est pour le mieux dans le meilleur des mondes.

The claim here is much stronger than earlier arguments by political scientists and economists that competitive politics reveals tendencies towards efficient outcomes. Rather, Wittman makes a Panglossian ‘end-state’ case for widespread democratic efficiency. The subtitle of MDF, incidentally, is not ‘Which political institutions will tend to increase efficiency (question mark),’ but ‘Why political institutions are efficient (full stop).’ Yet such reasoning by Coasean analogy ultimately fails to convince. As Olson (NSDS) argues, even the standard economic efficiency predictions do not square with a wide range of empirical observations. The large differences in per capita income across countries cannot be sufficiently explained by the differences in productive resources (capital, land and labour) that economic models typically resort to. For instance, one does not observe massive inflows of capital into poor countries, where it would command much higher returns. Very often, it appears, big bills are left on the world’s sidewalks.

Moving towards the political sphere of life, there can be little doubt that democracies do not usually come close to approximating the ideal environment in which Coasean efficiency gains are possible, because of pervasive information problems (more on this below). Another crucial difference is that much of politics deals with public goods, not the private goods exchanged in economic settings. PP indicates that in such cases even the most basic Coasean insight – that only transaction costs can prevent voluntary bargaining from attaining efficient outcomes – is unlikely to hold.

**Voluntary Bargaining Reconsidered**

Suppose that the provision of some discrete public good would constitute a Pareto-improvement. The good is public in the sense of being nonexcludable. Once provided (at a total production cost of $C$), it would offer a benefit $V$ to *every* member of a population of $N$; nobody could be excluded from consuming it. In many cases, the contributions of only a subgroup $M < N$ will be needed to cover the produc-
tion cost of this good. Specifically, define \( M \) such that \( MV > C > (M - 1)V \). It then follows, says Olson, that purely voluntary bargaining will not lead to the provision of the public good.\(^1\)

Once a certain number \( n \geq M \) of individuals meet in order to bargain and bring about the Pareto-improvement, they will in the end succeed, if only transaction costs are low enough. This is exactly what the Coase theorem posits. But voluntary provision of the good logically requires something prior to this meeting: that it is actually convened freely, without coercion by third parties such as governments or courts. Now, the probability of any one individual being pivotal in bringing about the meeting, that is, of being the \((M - 1)\)th individual in turning up for it, quickly approaches zero as numbers rise.\(^2\)

In other words, the more one approaches the large-number context characteristic of democratic politics, the less likely it is that the Pareto-improving good will be provided by voluntary action. Given the dispensable likelihood of being pivotal (hence of benefiting either at least \( V - C/M, \) or 0), the choice is quickly made between either free-riding on others and benefiting \( V \), or turning up oneself and benefiting \( V - C/n \). So as in Olson’s earlier work (see below), individually rational free-riding is the key to socially suboptimal outcomes: the best strategy for everyone is to let others contribute towards the good’s provision, while still enjoying its benefits. Note that this inefficiency result does not arise because of high transaction costs in the bargaining stage, as Coaseans might argue. It arises, even in repeated games, because free-riding is the rational strategy in the prior decision of whether to start bargaining or not.\(^3\)

The upshot is that voluntary Coasean bargaining, on its own, is insufficient for an explanation of efficient outcomes in democracies. Olson’s micro-theory argues that, strictly speaking, end-state political efficiency arguments such as Wittman’s are flawed in their Coasean core. Yet one might still argue that many such claims can be saved by treating them as ‘directional,’ indicating a strong tendency towards efficiency. Might not the very possibility for Pareto-improving gains lead one to expect transaction-cost reducing institutions in politics too, as we have sometimes seen in economic history (North, 1981) and industrial organization (Williamson, 1985)? Might not political entrepreneurs, competing to win the favours of voters, take us a long way towards efficiency?

**Shadow-boxing around Olson’s Old Logic**

At different stages, Wittman invokes the low number of Congressional representatives, exclusive committees, and monopoly positions as being transaction-cost-reducing institutions. An entirely new research agenda is advocated, based on seeing pressure groups as ‘part of the solution’, since they ‘perform a valuable function by reducing the costs of transmitting information from individuals to politicians and vice versa’ (pp. 81–2). Many well-established theories in political science die a peculiar death in the process. For instance, rent seeking or the distribution of economic wealth and political power allegedly do not matter nearly as much as political scientists have generally believed: ‘... the extent of power held by bureau-
cracies, corporations, congressional committee chairmen, and pressure groups, such as the National Rifle Association, has been greatly exaggerated’ (p. 2). Presented with the standard story of distributional logrolling in a three-person community, we are told that ‘... one must discount most of the extensive political science literature on the distribution of political power and remain agnostic’ (p. 162). Wittman asks: ‘Why doesn’t the majority vote to confiscate the wealth of the wealthiest 10 percent of the population?’ (p. 163).

At the most basic level, the answer to such objections is as least as old as Olson’s seminal 1965 theory – to which, curiously, there is not a single reference in MDF. Individuals often share with others an interest in having some public good provided. Olson’s original contribution was to argue that there can nonetheless be no general presumption that all such ‘interest groups’ will therefore organize, as if naturally, to pursue their joint goal. Olson asserted that in groups of large size, rational individuals would not contribute towards providing the good. This is not uncontroversial. As Marwell and Oliver (1993) show, group size negatively affects collective action if and only if the public good displays ‘zero jointness of supply.’ The benefit of a given level of the good then decreases proportionately with the number of people sharing in it. Now, this may sometimes be the case, but not often. My benefit of living in a democracy does not decline proportionally with the number of people I share it with. As we have seen, in his recent work Olson seems to recognize this: he now assumes a fixed individual benefit \( V \) (independent of group size), and a fixed total cost \( C \) (hence an individual cost decreasing, not increasing, with group size). Yet here too, free-riding impedes collective action.

Individuals making an independent private cost/benefit calculus without taking into account the expected effect of their actions on others may still prefer to spend their limited resources on an (excludable) private good while hoping to benefit from the (non-excludable) public good financed by others. If I choose to spend my £100 on a bike and others spend theirs on getting rid of dictatorship, I may both have my bike and enjoy democracy, even if I value the latter higher than the former. Such reasoning is more likely to occur in large groups than in small groups. Since large groups contain many more ‘others,’ free-riding is less easy to identify, let alone to sanction, and it seems to make more subjective sense. Yet if everybody reasons likewise, the good will not be provided. In this sense, Olson’s free-riding intuition rings true beyond his own original group-size argument. And it may often ring true more, the larger the group.

Wittman seems to misunderstand the notion of differential organizational ability in both its positive and its normative implications. In fact, his theory of distribution and rent seeking mainly amounts to a few rounds of shadow boxing around collective action failures. He argues that the small number of members in Congress ‘reduces negotiation costs, thereby creating the conditions for efficient logrolling (exchange)’ (p. 32). Specialized Congressional committees ‘allow those who are most affected by the relevant policy to trade among themselves rather than involving many actors whose interests are only peripheral’ (p. 33).

To be sure, these and other low-number contexts may reduce the transaction costs of making deals, thereby promoting the interests of the insider parties involved.
But such particularistic gains cannot simply be equated with social efficiency (wealth maximization). They are, at best, of secondary importance as a normative yardstick for democratic politics. More generally, such contexts seem an open invitation to capture by special interests, notably in the form of lobbying and campaign financing for sympathetic political candidates or parties. Wittman is actually surprised to see that rent seeking ...

... clearly has a negative connotation ... However, similar activity in other markets is viewed as value enhancing. Pet stores try to sell you bird feeders (this redistributes income from humans to bird), churches try to convince you to donate food to the poor, gambling casinos provide brochures, and workers search for better-paying jobs (p. 36).

The analogy is superficial. All ‘similar’ activities listed involve voluntary decisions of individual agents about their own money. Rent seeking, in this context, involves small groups exploiting their greater organizational ability in order to obtain high per capita benefits at the expense of low per capita taxpayer interests. MDF aims several rounds of punches at a mere shadow version of the Olsonian argument. It argues (p. 78) that even though each taxpayer loses little, the large number of such small-time losers will still render programmes that go against taxpayer interests politically unfeasible. But this skips over the core insight that since taxpayers lose little per capita, they have no incentives in the first place to get informed about the programme, to organize towards letting their interests be heard, and to go and vote against it. The costs per capita of information and organization are likely to exceed the small loss they might thereby avoid. In contrast, small groups, if they are after the same pie of public money, will always obtain much higher per capita benefits. Hence their members will always have greater incentives to organize – unless, implausibly, they face a proportionally higher per capita informational and organizational cost.

Likewise, one cannot debunk traditional theories of power merely by observing that ‘capitalists’ compete: ‘Capitalists don’t control, markets do. The capitalists, like the workers, face supply and demand curves’ (p. 176). It is one thing to deny the relevance of Marxist class concepts. It is quite another to go on by entirely discarding the structural advantage of some groups of actors over others. There is nothing in Olson’s logic that implies that capitalists, as a ‘class,’ would enjoy special advantages. However, within each industry, capitalists do have an organizational advantage over larger groups of actors in that industry, such as workers or consumers. As Olson (1965, p. 145) literally observed: ‘... the business community as a whole is not well organized in the sense that particular industries are.’

At the very least, this indicates that it cannot be automatically presumed that majoritarian politics – be it through individual representatives, ‘affected’ members or agenda-setters in committees, or leaders worried about their party’s overall standing – will generally favour the interests of a majority of taxpayers. Olson (1965) had the first word on this issue; 35 years later, he restates it as follows: ‘No society can ever have a comprehensive and symmetrical organization of all groups. Accordingly, such groups as the unemployed, the poor, the consumers, and the taxpayers are not organized’ (p. 88). Arguably, as a general broad-brush prediction this is still not altogether inaccurate.
Of course, there are many ways to modify or specify the collective action problem. For instance, self-interested entrepreneurs (e.g., in trade unions) may get big groups organized by offering selective incentives (private benefits) to members. But such ‘solutions’ are less general than the original problem. And as Elster (1989) argued, they need to solve second-order collective action problems (e.g., how do trade unions get organized in the first place?). Importantly, to take up two Olsonian propositions discussed above, the voluntary convening of bargaining meetings and the incidence of democratic transitions can be much better understood through critical mass effects arising with individuals who act interdependently and are heterogeneous with respect to their resources and their preferences for public goods. These would be compelling extensions of Olson’s original logic (for examples, see Kuran, 1995, and especially Marwell and Oliver, 1993). Wittman, in contrast, does not even acknowledge the basic problem as such, although it is central to most political theories he claims to refute.

**Voter Information: (n)ever problematic?**

The modern economics of information has by now moved far beyond the standard neoclassical model of efficient markets. Akerlof (1970), and many after him, demonstrated that in the presence of asymmetric information, Pareto-improving bargains will not be struck. Stiglitz (1994, p. 13) summarizes the state of the art as follows: ‘... once information imperfections (and the fact that markets are incomplete) are brought into the analysis, as surely they must be, there is no presumption that [economic] markets are efficient.’ In Wittman’s theory of politics, however, all information imperfections are brushed aside, ‘most of the time’ (p. 5). Voters are not even assumed to be rationally ignorant, as most theorists have accepted since Downs (1957, pp. 240–7). Wittman argues that the amount of voter information has been underestimated, and the costs of obtaining it overestimated, since ‘voters receive a lot of ‘free’ information – in the news, in the mail, and in ordinary conversations’ (p. 10). We are told, furthermore, that ‘voters will sufficiently discount’ any biased information they receive (p. 15). Political advertising, in turn, should be considered ‘informative’ (p. 79) – exclusively and unambiguously.

Despite all the above, ‘at other times’, Wittman changes tack and argues that the existing information failures will be structurally mitigated in democracies, maximizing citizens’ wealth in the process. Admitting that occasionally voters’ information may nonetheless be biased, the author now suggests that these individual errors will be cancelled out on aggregate (pp. 16, 17). Yet there is by now an established body of evidence demonstrating that voters’ choices vary systematically according to the way these choices are presented – ‘framed’ – to them (Quattrone and Tversky, 1988). In evaluating incumbents, the weights that voters assign to different policy domains vary according to which domains are made accessible to them, or suggested – ‘primed’ – as important (Iyengar and Kinder, 1987).

This indicates that in complex and multidimensional settings, the provision of deliberately biased information may under some conditions change preferences in a direction favourable to the provider. Now, if one could be sure that all actors competing to obtain their political goals can provide such information on an equal footing, there would still be no cause for concern. But to the extent that some
actors enjoy systematic advantages over others, there would be. For instance, the theory of collective action provides firm grounds to expect pressure groups to have an advantage over voters in making their views heard. There are plausible reasons, too, for expecting incumbents to enjoy some advantage over the opposition in shaping the distribution of political preferences (Dunleavy, 1991).

Context determines the extent to which such strategies are possible. On a certain number of (local) topics, any voter’s knowledge may be ‘hard.’ On many other topics, however, it is likely to be ‘soft,’ and liable for manipulation by outsiders as well as bandwagon effects that may lock in inefficient policies (Kuran, 1995). The nature of the policy domain, too, circumscribes the scope for shaping preferences. For instance, Margaret Thatcher successfully reduced the traditionally Labour-oriented public sector by privatizing state firms and public housing (Dunleavy, 1991). But she did not manage to dismantle the welfare state (Pierson, 1994). Again, this may be due to Olson’s logic: welfare retrenchment involves imposing concentrated costs on some, in exchange for diffuse benefits for all.

The general point is that the potential malleability of preferences ought to be carefully addressed, not swept under the carpet, by any theoretical argument claiming that information failures are really not all that problematic in real-world politics. Wittman, for one, simply brushes aside the whole body of evidence on the framing effect as being completely irrelevant to politics. This blanket dismissal is not grounded in ‘better’ supporting evidence, but in the argument that voters can always discuss the issue together (p. 43), and in the a priori assumption that ‘advertising, political speeches, and so on do not affect voters’ preferences; rather, these preferences are embedded much deeper within the culture (e.g., parental values and early religious upbringing) and possibly in the voters’ genetic makeups’ (p. 2, fn.).

**Efficiency in Democracy: a matter of degree**

Wittman argues that democratic failure is a myth because political institutions reduce information failures, transaction costs and other problems that could impede efficient outcomes. That is true enough, to some degree. Political competitors can gain some advantage by catering for voter ignorance. Coalitions that imply much dead weight loss will, ceteris paribus, tend to be outcompeted by more efficient ones that have more pie to divide. Candidates and parties, by building up reputations, can to some extent solve problems of time inconsistency and agent shirking. After all, there is competition both within and between parties.

However, any argument that democratic systems are to some degree competitive is trivially true – otherwise one could scarcely call them democratic. The relevant question is by how much democratic institutions will tend towards efficient outcomes. By most standards, Wittman does not provide an adequate theory of constrained political efficiency to start answering this question. By and large, he contents himself with presenting mere judgments and assertions. Thus, we are told (pp. 73, 108, 186, 174) that if academic analysts can detect asymmetric information and principal-agent problems, then it must follow that political principals can, too – hence those problems are ‘exaggerated.’ Elections, in turn, are deemed
to be transaction cost-reducing ways to exercise political takeovers since ‘the time period between elections is relatively short (legislators are not elected for life), there are no super-majority requirements for being elected, and the opposition participates in the legislature’ (p. 23).

Such assertions are especially problematic for a theory that adamantly dismisses existing approaches in political science in favour of the view that ‘democratic markets do indeed have the qualities typically associated with efficient markets’ (p. 3). Regarding the question of degree, there is little doubt that the information failures and the costs of transacting are particularly pervasive in politics. Avinash Dixit’s MEP helps to clarify this in a number of ways. For instance, it is not inconsequential that politicians often allocate income on the basis of explicitly political considerations, such as prospective votes.

Take an industry that is affected by a negative shock. Politicians may want to compensate those workers who are affected. The economically efficient way to do so would be a lump-sum grant equalling the capitalized value of the workers’ income losses. Rather than distorting subsequent decisions by workers, such a grant would allow them to move at once to their most productive alternative jobs. Lack of credibility complicates matters, however. Workers cannot credibly promise to reward the politicians with votes over all elections that span their prospective loss. So the latter will offer compensation as partial payments over many periods, rather than up front. But politicians themselves cannot credibly promise to keep on compensating far into the future. Workers know that they will keep on receiving compensation only if they retain enough political clout. This may lock them in the uncompetitive industry, and prevent them from moving to more scattered, albeit more productive, occupations. In equilibrium, time inconsistency results in inherently inefficient political redistribution.

Building on economic agency theory, Dixit systematically analyses the incentive structure in political institutions. Fairly consistently, the incentives for political agents to perform according to their principals’ objectives turn out to be substantially less powerful than in equivalent market contexts. This is due to many structural characteristics of political agencies, including the lack of transparency and of yardstick competition. The political agency contract itself is ill-defined. It differs from economic contracts ‘in several ways, all of which make them more complex and harder to define’ (p. 48). Electoral campaign promises are vague, multidimensional, and not legally enforceable. One might add that voters do not make the repeated, separate choices over many different issues that actors make in those economic markets that approximate efficient competition. Typically, their preferences over many different issues have to be bundled together, every so many years, in one single vote, for one out of only a few credibly competing parties. Assuming that voter control (not mere political takeover) matters in democracy, elections come out as a less than powerful weapon for voters to sanction their agents (Przeworski et al., 1999).

Political agencies often perform ‘multiple tasks,’ sometimes along many dimensions of input (effort) and output (performance), which are difficult for principals to observe. Moreover, they may have to deal with ‘multiple principals’ with partly conflicting interests. For instance, a bureaucratic agency may be simultaneously
answerable to the executive and the legislative branch of government. Dixit (pp. 157–71) models such an incentive scheme with \( n \) principals. In equilibrium, it has only about \( \frac{1}{n} \)th of the power of a second-best scheme with a unified principal.

MEP adds a more complete conceptual framework to earlier transaction-cost analyses by Douglass North, James Q. Wilson, Oliver Williamson and others. But it reaches virtually the same conclusions. They are twofold. First, like Williamson before him (and in NSDS), Dixit notes that in political or bureaucratic institutions where property rights are poorly defined or costly to enforce, apparently inefficient arrangements may not actually be ‘remediable.’ That is, there may be no feasible (as opposed to hypothetical) alternative that is superior in all respects. On its own, this argument might be seen as compatible with the Panglossian efficiency conclusions. But second, these political arrangements also incorporate incentive schemes that typically are significantly less powerful than those found in economic markets. Hence, far from being exaggerated, the information problems inherent in democracies constitute serious theoretical grounds for doubting Wittman’s assertion that political markets approximate the degree of competition in economic markets. Accordingly, the presumption of constrained efficiency in politics should be so much weaker.

Conclusion

The thesis that democratic failure is a myth reaches its sweeping conclusions only by virtue of a superficial application of economic analogies which asserts away many defining characteristics of politics. As an end-state approach, the thesis seems misguided in its Coasean core. In large-number contexts, Pareto-improving public goods will not be provided purely through voluntary bargaining, due to free-riding at the stage of initiating cooperation. Efficient outcomes, and the lack thereof, may be better understood through the size of powerholders’ stakes in the economy, and the market-augmenting quality of their interventions. As a directional approach, the political efficiency thesis lacks in rigour and precision. It conveniently shadow boxes around collective action failures and information problems. More systematic transaction-cost analysis indicates that the incentive structures in politics are significantly weaker than those in economic markets. Democracy may be the worst form of government except for all other forms, but an invisible-hand theory does not help us much in understanding why.

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Notes

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What follows is based on Dixit and Olson (2000). Due, undoubtedly, to Olson’s untimely death, PP is unfinished. Thus, the book presents only an intuitive verbal discussion of the bargaining and stakes ideas, referring the reader much too readily to their formal foundations (Dixit and Olson, 2000; McGuire and Olson, 1996, respectively) for proofs.

Dixit and Olson (2000, p. 215) report that with values for M and N as small as 10 and 30, the likelihood of provision of the public good is less than one in a million.

Repeated versions of this two-stage game produce two equilibria. In the first, repetition actually reduces the incentives towards efficiency. The second equilibrium has higher payoffs than the first, but is not robust to the introduction of even small costs in attending the meeting (Dixit and Olson, 2000, pp. 315–6).

In fact, Olson remains ambiguous about this: in PP (pp. 71–9), unlike in his work with Dixit (2000), he still frames his argument in terms of zero jointness.

The first argument also skips over the fact that whilst the transaction costs per deal may well be reduced by logrolling, the number of transactions, hence total transaction costs, will be higher (North, 1990, p. 51). Hence it is problematic to call logrolling efficient without further qualification.

References


