

**On Some Neglected,  
But Profound, Contributions of Gordon Tullock**

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**Abstract:** This chapter suggests that Tullock has made more profound contributions to constitutional political economy and other related fields than he is unrecognized for, in part because he himself has failed to recognize them. Gordon is not self-consciously pursuing the profound, but simply pushing out the frontiers of knowledge in as many directions as occur to him, more or less as rapidly as possible for a very insightful active man. Carefully integrating his research and plumbing its depths has largely been left to others.

**I. Introduction: Gordon and the *Calculus of Consent***

The thesis of this paper is that Gordon Tullock's contributions are often more profound than (even) he recognizes. This series of profound contributions may be said to have begun in 1959, when Gordon Tullock prepared and circulated a mimeographed research paper while a fellow at the Thomas Jefferson Center for Studies in Political Economy at the University of Virginia. The paper was entitled "A Preliminary Investigation of the Theory of Constitutions." That working paper served as the foundation of chapter 6 of the *Calculus of Consent*: "A Generalized Economic Theory of Constitutions." Probably the

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most often referred to part of the *Calculus*, the chapter has played an important role in subsequent theoretical literature on voting rules that emerged after the *Calculus*. Indeed, that working paper can be given credit for launching the entire field of constitutional political economy, as it was the first systematic effort to use rational choice models to evaluate the relative merits of alternative voting rules.

However, even if one accepts this argument, it does not in any way diminish Buchanan's contribution to the *Calculus*. Buchanan is, of course, a very talented and creative man in his own right—and one of his rare gifts is the ability to recognize good ideas when he hears them. Most scholars are so fascinated with and focused on their own ideas that they have a hard time giving the ideas of others serious attention. Jim differs from most scholars in that respect. Indeed, he often appreciates more about such good ideas than the originator him- or herself.

My hunch is that it was Jim, rather than Gordon, who first appreciated how profound a contribution Tullock's working paper actually was. If true, it was Jim's appreciation of the profound, given Gordon's working paper, that caused two very bright and busy men to write a path-breaking book that created an entirely new methodology for constitutional analysis. Both men were prominent scholars before the book was written, but it was the *Calculus* that made both men famous.

Under-appreciation of one's own ideas seems often to be the case with Gordon, although not because of an excess of modesty.<sup>2</sup> Rather, it seems more because he is so busy

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<sup>2</sup> This conclusion is based on several decades of conversations and arguments with Gordon Tullock. I took Gordon's graduate public choice course at Virginia Polytechnic University in

showing how existing arguments can be rebutted and/or improved that he does not take the time to think very long or deeply about his own contributions. Gordon is so busy creating new ideas that he doesn't have time to evaluate them fully before moving on to the next. Indeed, that is partly what makes Gordon's work so interesting to read. Tantalizing nuggets and half-baked thoughts interweave with profound ideas every few pages. (Of course, readers will often disagree about which is which, but most of his readers recognize both Tullock's creativity and his ability to deliver penetrating insights.)

In this short paper, let me point out two of many instances in which his analysis appears to be more profound than Gordon appreciates.

## **II. Missing Generalizations of the Rent-Seeking Contest Success Function**

The first example is another paper that demonstrates the importance of the rules of the game, namely Tullock's 1980 paper on "Efficient Rent Seeking." In 1980 Robert Tollison (with Buchanan and Tullock) assembled a collection of papers on rent seeking that included both "classics" papers and new research. In that volume, Tullock's second paper on rent seeking criticizes his own classic 1967 "full dissipation" model, arguing that it is

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1976, where Gordon was, in his own way, very generous with students. He was my colleague at George Mason University many years later for more than a decade.

I was the director of the Center for Study of Public Choice when Gordon returned to George Mason University in 1999. Upon his arrival, I invited Gordon to join my Friday "visitor's lunch," which he did nearly every week until he retired. As a consequence, the visitor's lunch rapidly became Tullock's lunch, as the Center for Study of Public Choice's visiting scholars and I were challenged and entertained by Gordon's wry humor, insights, and argumentative style of discourse.

really a special case, rather than the general one suggested in his 1967 paper.<sup>3</sup> The extent of dissipation, it turns out, is not always 100 percent, but is shown to vary with the number of participants and the technologies applied.

His characterization of rent-seeking contests as lottery games has become known as the Tullock Contest Success Function (or simply as the Tullock payoff function). The lottery representation provides a powerful and simple mathematical representation of the contest among rent seekers. It has an equally simple (although far from obvious) Nash equilibrium that can be used to characterize the extent to which participants will invest in a given rent-seeking contest. It rapidly became one of the most widely used models for analyzing rent seeking and other contests. Together the game and equilibrium allow theorists, including Tullock, to say a good deal about the extent to which resources tend to be invested in winner-take-all contests by rational participants in settings where the winner is not necessarily the high bidder.

Given prize  $\Pi$  and personal effort  $R_i$  costing  $C$  per unit, the expected net rents,  $R_i^e$ , are:

$$R_i^e = [(R_i)/\Sigma R_j] \Pi - R_i C$$

At the Nash Equilibrium, player  $i$ 's competitive effort (and that of  $i$ 's rivals) is:

$$R_i^{**} = [(N-1)/N^2] [\Pi/C]$$

And the total resources invested are:

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<sup>3</sup> It bears noting that Hillman and Samat (1987) and Hillman and Katz (1987) show that the original Tullock (1967) complete dissipation case is more general than it appears if contest prizes are always awarded to the high bidder.

$$(NC) R_i^{**} = [(N-1)/N] \Pi$$

Both the model and the equilibria of such contests have a variety of broad implications. Among the properties developed by Tullock (1980) are that total investments and deadweight loss increase with the number of contestants and with economies of scale. In the simple case developed above, the full dissipation case occurs as  $N$  approaches infinity. The theoretical literature inspired by Tullock's paper shows that similar conclusions can often be reached for somewhat more complex contests. The applied literature suggests that the contest and rent-seeking concepts can be used to analyze a wide variety of social settings. (Congleton, Hillman, and Konrad provide a thorough overview of those conclusions and others linked to his first paper in *40 Years of Research on Rent Seeking* published in 2008.)

However, several profound conceptual features of the game and equilibrium seem to have passed by largely unnoticed. For example, the Austrian critique of the neoclassical economic paradigm suggests that the usual model of competition does not really characterize competition per se. In the usual model, there are no resources devoted to competition and no competitive activity that persons engage in. Rather, competition is normally characterized by mere numbers of persons engaged in the activity of interest (usually buying or selling).

Clearly, mere numbers are not sufficient to characterize competition in any meaningful sense. For example, a Greyhound bus depot with thousands of passengers and dozens of sales agents is not a competitive market in any normal sense of the word, although there are lots of participants buying tickets and passing through the depot's hallways and doors, and lots of buses leaving for many towns. With the exception of an

occasional mishap in seat assignment or crowded passageway, there is no conflict, no rivalry. Similarly, the number of persons standing on a tennis court is not a very good indicator of the competitiveness of a particular set of tennis. Indeed in that case when there are more than 2 or 4 persons on court, the competitiveness of the tennis match normally diminishes.

The Tullock contest has two indices of competition: the number of players and the level of investments made in the contest. The latter is an excellent indicator of the intensity of the competitive process. The commitment made to the contest ( $R_i^{**}$ ) captures far more of the ordinary meaning of the term competition than the number of players. It also bears noting that the outcomes of Gordon's competitive contests are stochastic; as a consequence, "the best man or organization does not necessarily win," regardless of his or her effort level.

Because of its clear and tractable structure and equilibrium, the Tullock contest success function (and the rent-seeking contest itself) when properly generalized provides a natural vehicle for thinking about the intensity and net benefits of competition under alternative rules.

The equilibrium outcomes of Gordon's game challenge a number of intuitions and conclusions used by economists, social Darwinians, and biologists: **Competition is not always good.** In the contests that Gordon focuses on, competition reduces social welfare (increases social deadweight loss). His examples include two interest group activities: efforts to obtain monopoly privileges and protective tariffs. It also includes one private example: efforts to steal private property. In all three cases, the greater the efforts, the lower are social net benefits. The private case implies that rent-seeking losses are not products of government policies alone.

Investments in contests in which resources are merely transferred from one person to another, or in which ordinary (Harberger) deadweight losses are produced, tend to reduce social welfare. Resources are consumed, but little or nothing of value is produced. The more resources invested in such contests, the smaller social welfare (in the social net benefit sense) tends to be. Competition in such cases is a “bad,” rather than a good. Such results undercut naive libertarian, liberal, economic, and biological arguments that “openness is always good” and “competition is always good.”

Whether competition is good or bad depends on the rules of the game. **The rules of the game matter.** Excluding entry to contests that produce net losses can be socially advantageous, as can limiting the use of resources in such contests. Blocking entry in unproductive contests and/or limiting competition tends to increase social welfare, rather than reduce it.

The usual conclusions about openness and competition rely on implicit assumptions about the types of rules in place for the game of interest. Some forms of competition are good because they are framed by what might be called “efficiency-enhancing” rules. That many, perhaps most, forms of market competition are value increasing is strongly indicated by a variety of economic models. However, the benefits of price and quality competition cannot be taken for granted; rather they vary with the rules of the contest. Both fraud and theft must be curtailed to reach the conclusion that consumers benefit more than firms lose from competition for their favor. Similarly, some, but not all, forms of political competition are also good, because public policies are improved by them, rather than worsened.

These points are not directly discussed or emphasized in Tullock’s 1980 paper or in much of the rent-seeking literature, but Tullock’s analysis of the lottery model makes it clear

that the rules and technologies for seeking a prize (rents) clearly matter: they affect the level of resources committed, the types of resources committed, and how they are used by competitors. Essentially all competitive contests consume resources, but not all of them are wasteful.

The losses from rent-seeking activities arise for three reasons: The process used to influence one's probability of winning a prize (or share of a prize) is costly. Equilibrium efforts (investments) are greater than zero. Those efforts produce zero or negative net value (negative externalities) for those outside the contest. Other contests neglected in Gordon's papers and most of the rent-seeking literature generate external benefits, as with contests for patents, market share, scientific breakthroughs, and major sporting events. In such contests, the process used to increase one's probability of success generates value directly or indirectly for others outside the contests of interests (for other producers, for consumers, for future generations, etc.).

Much of the rent-seeking literature stresses the redistributive consequences of such political games, but not all contests are redistributive in the narrow zero-sum sense of the word. Nonetheless, in such cases, both for those in and outside the game of interest may benefit when investments in rent seeking are reduced. In the standard Tullock contest, a uniform reduction in competitive effort frees resources for other uses without reducing any player's probability of winning the prize.

### **III. Revolutionary Pessimism**

Tullock's interest in conflict, rather than cooperation, led him to focus on a wide range of topics normally outside the range of economics: criminal law, anarchy, revolution,



bureaucracy, and science itself. In nearly every case, Tullock's work is original, insightful, and penetrating. And, it can be argued again that his insights are often more profound than appreciated by Gordon himself. His somewhat depressing work on popular revolution (1971, 1974) is used below as a second illustration.

There is a long tradition in America and elsewhere that popular revolutions can and do happen with some regularity. The masses rise up and throw out the tyrants. Many movies and historical narratives rely on such events to motivate great historical changes. Here, one may note that the various wars of succession waged in North and South America are often termed revolutions. In such cases, it is often argued that political revolutions have occurred, because a popular revolt has caused one system of government to be replaced by another.

The term revolution is also applied to science and to technological advance, although with a somewhat different meaning. A revolution may also be said to occur when one theory is replaced by a new one or some new production technique radically reduces the cost of production and so makes natural resources more useful and outputs more widely available. Indeed, a series of such improvements may also be termed a revolution, as with the Industrial Revolution of the nineteenth century. Note that this use of the term revolution may involve rather modest changes.

However, the use of the term revolution, as applied to politics, tends to imply radical change. For example, the *Merriam-Webster Dictionary* defines political revolutions as “a fundamental change in political organization, especially, the overthrow or renunciation of one government or ruler and the substitution of another by the governed.” Note that there are several important parts to this definition: “fundamental” meaning large or major

changes, “overthrow” suggesting violence or at least the threat of violence, and “by the governed” suggesting that a broad range of citizens participate in the revolution.

Tullock’s paper on revolution (1971) and his short book on the *Social Dilemma* (1974) argue that popular revolutions are essentially impossible and therefore extremely rare. Collective action problems and the efforts of those already in power to resist being overthrown imply that few if any popular revolutions will get off the ground. Tullock uses his theory to explain why dictatorships, rather than democracies, are historically the most common form of government.

As commonplace in Gordon’s work, a breathtaking (ingenious) series of simplifying assumptions allows him to cut to the core of a variety of fundamental issues. There are two key assumptions: the first is simply an application of Olson’s logic of collective action. A revolution that broadly supports the interests of ordinary persons (e.g., those not already in government) is a public good, because the activity of producing a revolution is privately costly, and the benefits are diffuse and realized both by those who do and do not participate in the revolt. Potential revolutionaries will free ride rather than participate. As a consequence, revolutions will not emerge spontaneously.

This is not to say that popular revolts are impossible, only that they will not occur spontaneously. They require organizations of some kind. It bears noting that the facts are largely, although not entirely, consistent with this conclusion. In most revolutions, as in the case of the recent Egyptian revolts, there is a significant organization (the Muslim Brotherhood) that encourages revolutionaries to turn up at certain times in certain places and behave in certain ways (armed or not, signs, etc.).

Gordon's more profound insight is that revolutions are not simply one-sided events organized by revolutionary cadres. A well-functioning state can head off revolutions in a variety of ways. Many proponents of revolutionary theories suggest that an authoritarian state may do so by being a productive state, that is, a state that produces many desired public services. Tullock, following Machiavelli, suggests that authoritarian states can often avoid being overthrown by being repressive, rather than productive.

Taking Olson's argument in a new direction, Tullock suggests that authoritarians may avoid revolution by **increasing collective action problems** for revolutionaries. That is, collective action problems are not entirely determined by exogenous organizational costs, as assumed by Olson. Many of those costs are endogenous, and so can be manipulated by government to make collective action problems more difficult for their opponents and less so for their supporters.

Authoritarian governments reduce the likelihood of popular revolts in a number of ways, including the use of spies and secret police; imposing severe punishments on revolutionary publications, participants, and organizers; and organizing demonstrations that indicate "broad" support for their regimes. All these steps make revolutions less likely by increasing collective action problems. Tullock does not point out the connection to Olson explicitly, but notes that revolutionary activity can be suppressed by increasing the costs of revolting.

The latter helps explain why tyrannical regimes tend to be more robust than popular revolt theories suggest. He also suggests that "palace coups" are more difficult to discourage and so far more common than "revolutions." This, in turn, implies that authoritarian

institutions are more stable than the persons occupying seats in the upper echelons of government.

In this case, it is clear that Gordon understands that importance of his contribution to constitutional political economy, if not to the broader issues that concern collective action in general. And his arguments should be, but have not been, taken into account by the revolution-driven literature on constitutional reform that has emerged in the past decade or so (Acemoglu and Robinson 2005), which ignores collective action problems. So, his theory of counter-revolutionary policies remains relevant and provides a persuasive explanation for an important historical phenomenon (the prevalence of dictatorship or at least nondemocratic states) neglected by much revolution-based research.

His theory can also be criticized in various ways. For example, dictators may differ in important ways neglected in his analysis (Wintrobe 1990, 1998), or they may have more complex internal structures than assumed by Tullock (Bueno de Mesquita et. al. 2004; Congleton 2011). But these can be taken into account without significantly affecting Tullock's main conclusions.

His broader point about the role of government decisions for organizational theory and related issues concerning economic and political freedom has also received less attention than it deserves from others (and by Tullock, himself). That collective action problems are partially endogenous has broad implications that are not articulated in his work on revolution or elsewhere (as far as I know). Olson also mentions such endogeneity in passing but fails to focus much attention on it or on the incentives for government officials to actively encourage and discourage various forms of collective action.

Yet, it seems clear that both democratic and authoritarian governments have good reasons to encourage anti-revolutionary and other status quo– preserving collective activities (e.g., demonstrations in support of national holidays, leaders, and policies) that encourage law-abiding behavior—what Wintrobe terms loyalty-building activities. Moreover, the endogenous component of organizational costs has broad implications for both market and governmental stability and evolution. Civil and constitutional law both affect the extent and kind of collective action that we observe in a wide variety of economic, political, and religious areas of life. They do so by affecting the costs (and benefits) realized by all sorts of organizations, not simply revolutionary and politically active interest groups.

Indeed, the manipulation of organizational costs may be said to determine the extent to which individuals are “free to choose” as well as “free to organize.” Again, as indicated by the rent-seeking case, complete freedom to organize may cause problems. Rent-seeking organizations should be discouraged to avoid social losses. However, the formation of organizations that produce net benefits should be encouraged by reducing their collective action costs. Groups lobbying for peaceful reforms may improve public policies, but it is not obvious or easy how to encourage the latter but discourage rent-seeking groups, or whether this should be done in every case. It bears noting that corporate law attempts to both encourage the formation of productive enterprises and constrain them in a manner that reduces the ability of corporate governments to extract rents from their stockholders (through rules on fiduciary interests).

That liberty itself is partly endogenous is acknowledged by all students of government; that liberty in the long run is substantially the result of manipulating the costs of collective action is not nearly as widely understood.

#### **IV. Is Underappreciation of the Profound a Problem?**

Buchanan in his Nobel speech (1986) spoke eloquently about how equality before the law tends to increase both economic and political efficiency. By doing so, with quotes from Wicksell, he was both honoring his hosts and connecting modern constitutional political economy with the liberalism of the nineteenth century. This ability to recognize the profound, the essential, and the enduring is one of Buchanan's great strengths as a political economist.

Gordon, on the other hand, somehow seems to lack this perspective and so many of his truly profound observations pass unnoticed by his readers (and evidently himself), because he does not emphasize them. Yet, his creative, contrarian approach to social science has generated an impressive body of work that includes numerous original insights and much that is profound. That work can be argued to have begun with the *Calculus of Consent*. It clearly continues through his work on rent seeking, anarchy, and the law.

In this short piece, I have suggested that Tullock has made more profound contributions to constitutional political economy and other related fields than he is unrecognized for, in part because he himself has failed to recognize them. Gordon is not self-consciously pursuing the profound, but simply pushing out the frontiers of knowledge in as many directions as occur to him, more or less as rapidly as possible for a very insightful active man. Carefully integrating his research and plumbing its depths has largely been left to others.

This is not entirely a loss for his readers or for the fields of research in which he has engaged. It can be argued that by pressing on with his research, rather than carefully working out the implications of a few key ideas, he has produced a far larger body of

interesting research than would have been produced had he self-consciously attempted to plumb the profound. Although Tullock occasionally waxes philosophically, he normally simply attempts to amaze and confound those engaged in more conventional work. He does so by showing how many implicit assumptions (and blinders) conventional work has relied on.

Now that Gordon's work has itself become conventional, perhaps it is time to see what blinders and implicit assumptions have grounded his work. In some cases, this will lead us to honor his work for being more penetrating—more profound—than he seems to have understood. This paper has provided a few examples in which this may prove to be the case. In others, his work may prove to be wrong in significant ways, and research will head off in entirely new directions as his assumptions are replaced with different or less restrictive ones. However, much in his work is likely to prove both correct and very durable, because it is more deeply grounded than it may appear on first impression.

Fortunately for the rest of us, even after Gordon's and Jim's extensive research on constitutional issues, there are many questions that remain, many of which are latent in their great bodies of research. The institutions examined in the *Calculus* were mid-20th century American ones, which had long functioned tolerably well. Their work attempted to show why that was the case. On the other hand, a few years later, Tullock's work on rent seeking suggests that not all is well with American governance.

Clearly, both cannot be entirely true. If rent seeking (interest group politics) is omnipresent and very costly, how did we get such good institutions and so many reasonably good policies, rather than the efficient rent-extracting machines that one might have anticipated?

Even though Jim and Gordon provided us with much that may prove essentially final upon further reflection and investigation, much of their work needs extension, review, and correction. As Jim has often been heard to say, “onward and upward.”

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