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ORIGINAL PAPER

Tullock on the organization of scientific inquiry

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Abstract In (Tullock, The organization of inquiry, Duke University Press, Durham, NC, 1966), Gordon Tullock sets out to establish how knowledge is developed and dispersed in science, to such a high degree of reliability, despite the lack of formal organizational structure. He contrasts this against the unreliability of the "non-sciences". In this essay, I review his perspective and consider the validity of his proposed reforms.

Keywords Methodology · Economics · Science · Knowledge

JEL Classification A1 · B2

1 Introduction

Gordon Tullock produced two seminal pieces in the field of Public Choice on vastly different topics a mere five years apart; the landmark volume *Calculus of Consent* (Buchanan and Tullock 1962) which established a foundation on which much of constitutional economics was subsequently built, and his path-breaking article presenting the first formal model of rent-seeking (Tullock 1967). In between, he published several other articles and two additional books: *The Politics of Bureaucracy* (Tullock 1965) and *The Organization of Inquiry* (Tullock 1966). Unlike almost all his other works during this time period which helped develop parts

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of the public choice paradigm, *Organization* finds Tullock outlining his vision of how the scientific method proceeds, and why social science remains (at least at that time) a "backwater" relative to the natural sciences. Halfway through the book Tullock declares "The subject of this book is the organizational system which takes these rather normal human beings [scientists] and uses them to produce knowledge of a very high degree of reliability" (p. 131). He is interested in how knowledge is developed and dispersed in science, despite the lack of formal organizational structure. In this sense, the subject still falls within Tullock's general interest in methodological individualism. Throughout the book, Tullock paints a picture of a relatively efficient Smithian guiding hand at work in the sciences that is largely absent from the "non-sciences".

I cannot address the state of science and social science as it existed in the 1960s to which Tullock was reacting. Instead I will focus on how his critique applies to economics today, and how useful would be his proposed reforms for our profession. Some of his conclusions would fit within the public choice paradigm of interest groups and incentives, although not consistently. It will therefore be useful to compare Tullock's vision of scientific progress and social scientific stagnation against Mancur Olson's views on the nature of knowledge accumulation within the social sciences. Throughout his career, Tullock provided new insights into several areas of inquiry, including rent-seeking, bureaucracy, dictatorships, constitutions, voting, courts, etc. yet most of his separate contributions were independent of the others. Olson, in contrast, also wrote on several areas including rent-seeking, interest groups, dictatorships, institutions, macroeconomic policy, etc. but almost always developed them around the central theme of collective action. Their contrasting styles are reflected in different visions of the potential for social science.

As an outsider to the history of economics field, and not having a direct association to the Virginia School as faculty or student, I am perhaps not as well qualified as others to interpret this book from Tullock's perspective.[1] Yet my undertaking of this task would be consistent with Tullock's declaration (p. 103–104) that outsiders may generate insights missed by experts by virtue of pursuing a different methodological approach.[2] Certainly, many of Tullock's own insights within economics could be characterized this way.

The next section presents a brief summary of Tullock's perspective on the scientific process and why the social sciences lag behind the sciences. This is followed by the classification of economics within Tullock's taxonomy, and then a comparison to Olson's view of social science. The penultimate section considers several of Tullock's proposed reforms. I then close this essay by speculating on which ways Tullock's critique does or does not apply to his own approach and experiences, and to what extent he implemented his suggested reforms to the editorial process when he was editor of *Public Choice*.

[1] For recent insights from insiders, see Caldwell (2008) and Levy and Peart (2012) who confront some of the issues discussed in the present essay but generally focus on other aspects of intellectual thought related to *Organization*.

[2] On the other hand, perhaps not. Gordon once stopped me outside of another scholar's office and told me not to go in because "it would just make the both of you dumber".

2 Tullock's view of science

In the opening chapter, Tullock presents three overview questions he seeks to address. The first is why do individuals contribute to a cooperative activity of science when there is no central authority affecting their decisions. Tullock references Adam Smith's infamous analogy of an "invisible hand" directing their activities toward the common good. His second question is how does does the society of scientists function. Finally, he wishes to determine how it ensures accuracy and reliability. Tullock concludes there exists a social organization of science and sets out in the remaining chapters to describe its workings.

Tullock suggests scientists are driven by one of three paths: "pure" research is connected to curiosity, "applied" research relates to a need for practicality, and an unnamed third category entails induced curiosity where research is driven only by the desire to secure salary. While Tullock finds the the first two laudable, he expresses concern over the latter. Tullock presents, and then refutes, the conventional wisdom that pure science is more important than applied science because the former leads the other. Rather, he details several examples of how both build off each other which helps to reinforce the validity of both of these types of science. Such reinforcement, however, is missing from induced curiosity research.

Tullock criticizes science driven by induced curiosity because this leads to over-complexity to signal an appearance of "importance" for which scientists will be rewarded with higher salaries. In contrast, those engaging in pure science are supposedly driven only by the desire to obtain the truth. (Tullock does not explicitly state in which camp applied scientists fall.) According to Tullock, pure scientists have a higher IQ than those who have to have their curiosity induced and are in the minority so they cannot set the tone for research. This might seem counter-intuitive because the gatekeepers of research agendas ultimately are the journal editors who presumably represent those who have ridden to the top of their profession and would possess the strongest intellect. However, Tullock also offers a scathing critique of the journal editorial structure which runs counter to this ideal (discussed below).

In Tullock's mind, scientists form a community whereas non-scientists do not. The theme of the book is primarily devoted to characterizing this society. Voluntary membership in the scientific community represents a guiding hand to produce the common good of knowledge. Throughout, efficiency of science stands in stark contrast to the non-science fields.

Tullock explains why experts are better able to deduce new theories compared to novices; experts have more familiarity and better retention of "bits" of information in their proper "clusters". Yet Tullock also notes that when these clusters interfere with viewing a new problem, the novice might excel instead. Tullock argues science cannot be 'planned' because it is impossible to know the future discoveries or what methods will be used to discover them. He recognizes that "many scientific discoveries are accidental", applying to a different problem than the one originally under study. Thus, the notion of a welfare-enhancing social planner devoting resources to a particular problem would be an inefficient way to solve the problem.

Tullock concludes that reliability in science is not due to universal honesty but rather the lack of temptation and strong auditing. Tullock believes the pure scientist is only interested in obtaining truth. Applied scientists are interested in practical applications, and false ideas will not work for them. Only the induced scientists may be unscrupulous but sufficient auditing roots this out. Sufficient auditing is lacking in the non-science fields.

The social sciences are viewed as deficient compared to the natural sciences because the social organization of social science differs from his description of natural science. The field of sociology in particular is denigrated for engaging in a lot of repetitive research that is neither creative nor represents "real repetition".[3] In contrast, Feigenbaum and Levy (1993, p. 217) take the position that replication in the natural sciences is distinct in that "even imperceptible differences in 'controlled' experimental conditions can lead to new data from which inferences are drawn. Thus, replications in the natural sciences can more appropriately be viewed as experiments that increase sample size (data points)...[as opposed to the attempt] simply to reproduce results with the same, fixed data set". It is these sorts of "reproductive replications" that Tullock finds lacking in the social sciences.

Tullock attempts to dispel the myth that it is the reliance on historical data in the social sciences, which is not part of the natural sciences, that limits the social scientist.[4] Dispelling the myth is important because that would suggest the backwardness of social science is due to exogenous factors. Instead, Tullock believes the problems are self-induced. He describes eight problems.

1. unwillingness or inability to publish controversial findings orthogonal to politically correct held beliefs
2. easier to convince majority in natural science as minority adoption of practical tools will prove itself but such tools do not exist in the social sciences
3. "radical ideas" in natural science are less of a concern to the public who have no independently held views
4. lack of patent ability in social science; applied research is a check on pure theory which is largely absent in social sciences
5. curiosity in natural sciences is general whereas in social sciences is more narrow and cannot be easily extrapolated; social science is dominated by induced curiosity with very little practical application
6. natural science ideas are begun in science and then distributed to the public whereas research in social sciences is driven to confirm beliefs of the public
7. errors are more obvious in natural science whereas social policy proscribers are unwilling to admit their theories are wrong

[3] "Sociology, oddly enough, involves a lot of repetitive research without real repetition. The conundrum results from the fact that sociologists apparently do not have very original minds and tend to partially copy each other's research. They almost never, however, copy the previous research completely, with the consequence that their work never constitutes a real repetition". (p. 122).

[4] While working on my dissertation, I expressed concern to an economic history professor over the quality of the historical data I was reconstructing. I was then informed by him that "all data are crap; its just that economic historians *know* their data are crap".

8. need for grant foundation support leads to ideologically driven results in the social sciences

Tullock became an economist by trade (if not by formal training). The subject of economics is that of a social science, but the methods borrow heavily from the natural sciences. In the next section, I attempt to determine whether economics as a field follows the organization of a science or non-science as laid out by Tullock.

3 Is economics a science?

As a reader it is somewhat unclear whether economics is to be treated as a science or not. Indeed, there have been contrasting interpretations of Tullock's view by Caldwell (2008) and Levy and Peart (2012). Part of the difficulty stems from that fact that Tullock's classification scheme does not strictly conform to the natural science / social science dichotomy. For example, anthropology, a social science, is specifically presented as a scientific field (p. 81) whereas biology, a natural science, is specified to not be a science (p. 49). Caldwell (2008) notes the "demarcation problem" of separating science from non-science but does not identify Tullock's own inconsistency on the placement of economics.

Early on Tullock states "Left to myself, I should like to define science in such a way that only fields in which fairly elaborate theoretical structures have been developed, like physics or economics, would be included. Other areas, like biology...would not be considered sciences because they have not yet attained the theoretical stage" (p. 49). He also uses *Journal of Political Economy* as an example of a science journal (p. 28). These statements suggest Tullock treats economics as science, consistent with Caldwell's (2008) perspective. Yet, as claimed by Levy and Peart (2012), Tullock presents economists as if their curiosity is "induced" rather than "pure". Indeed, economics shares many of the characteristics presented as the limitations on the social sciences. For example, toward the end of the book, the discussion of economic arguments over the usage of tariffs as exemplifying the social science problem of special interests and ideology, suggests that economics cannot be viewed as science. Specifically, Tullock explains why tariffs are still supported in some circles despite his claim that the "dispute was intellectually settled over a century ago" (p. 158).

> While almost everyone would, in the long run, benefit from the removal of tariffs, and the raising of tariffs is a blow to the welfare of almost everyone, there are, at any given time, minorities which can be hurt by the reduction of specific tariffs and helped by the increase of others. Now the benefits of the repeal of a given tariff are likely to be dispersed over the whole population, while the injury will be concentrated in a small group. Although the benefit will be much greater in total than the injury, it is slight for any individual. The group which suffers concentrated injury, however, is likely to convince the majority that they really gain nothing and to hire economists for this purpose.

> Since there are always such groups, there will always be economists who are hired for this purpose.
>
> Not all of the advocates of tariffs, of course, are hired by "the interests". But the existence of people whose living does depend on finding arguments for tariffs and the further existence of another group who think that maybe, sometime in the future, they might need the assistance of either someone who believes in tariffs or an economist who is in this racket makes it possible for them to continue to publish, even in quite respectable journals. (p. 158)

Economics would therefore seem to suffer from several of the social science problems identified by Tullock and listed above. In particular, economics, as the tariff discussion exemplifies, relates to many issues in which the public already holds strong views. Relatedly, foundation grants can often be driven by ideological concerns. In addition, most of the economic advances are unpatentable. The lack of applied economists providing reliability of the pure scientists, and the larger relative percentage of induced economists suggest the weed-out problem in economics is not as easily dismissed as it is in the other (patentable) sciences. Based on these characteristics we are left with the unappealing conclusion that economics is driven more by inducement than by the pursuit of "truth".

Yet others disagree. According to Ronald Coase (and George Stigler)

> One might have expected, given the stakes involved, that the various groups active in the political arena could have procured economists to voice opinions which served their interests... No doubt some economists have been corrupted. Yet my experience is that corruption of this sort, at any rate among economists of quality, is very uncommon or even non-existent. As Stigler says "I have seen silly people—public officials as well as private, by the way—try to buy opinions but I have not seen or even suspected any cases in which any important economist sold his professional convictions". (Coase 1981/1994, pp. 30–31)

One interpretation is that Coase and Stigler are acknowledging the economics profession may be vulnerable to Tullock's critique of special interest domination, but that in practice it has largely escaped unscathed. Alternatively, the "quality" or "important" economists which are their focus could represent Tullock's pure scientists only and the plethora of induced economists could still be endemic to the profession. Yet the corruptible, induced economists would seem to be in the minority if only "some" have been corrupted. Indeed, Samuelson (1962) believed economists are induced not by salary, but by "our own applause" which is "the only coin worth having". Conceivably, to attain such applause and not subsequently have that applause turn to jeers would require both correctness and integrity.

The discussion surrounding the reliability of pure science being driven by a desire for truth and applied science for practicality, whereas induced curiosity results in dishonesty, forces us to look inward at our profession. "Data mining" to find a pre-conceived result is certainly a noted plague within the empirical world

(Mayer 1980; Ferson et al. 2003), and presumably would not be found among the pure scientists seeking only the truth. Yet reliability depends not only on honesty (indeed the data-mined findings might well be true, just not representative) but also on competence. Honest mistakes can occur and neither the pure nor applied scientist can be automatically absolved of such. Finding honest mistakes in theory requires redoing proofs line-by-line, an arduous task for any reviewer and the more prestigious the journal and reviewer, the greater the opportunity cost of time to do so. This is something that it is rarely done and Tullock is right to trumpet its importance. In fact, proofs may make use of previously developed theories and lemmas, and if those have yet to have been properly falsified, the unreliability of the current proof will be missed despite thorough inspection. Similar problems apply to empirical studies where catching honest mistakes requires access to the data and coding which is also rarely done during the review process. To rely so heavily on the reinforcement mechanism between pure and applied scientists to capture all errors (intentional or not) may be asking too much.

Herein lies the promise of the replication studies Tullock advocates. Tullock would be supportive of those journals which have recently announced an explicit policy of encouraging replication studies for submission. Yet these journals are not in the top-tier, where correction of errors to their own publications are limited to "Comments" at the back end of the journal. Prestige of conducting replication remains low. Replication studies are rarely cited. If the original study is confirmed, future citations are usually to the original with at best a footnote to the replication study (but very rare). If the replication upends the original, the usual response is to simply stop citing the original because now it is known to be false, but rare would be the case of citing both the original and the replication study which would in essence be informing the reader to ignore the citation just given. In terms of publication itself, it is also much riskier for the scientist to replicate theory than empirics. Finding an error in theory is certainly worthy of publication, but I am not aware of a single publication which merely confirmed the correctness of a mathematical proof. Replication confirming empirical results have been published, although again, they tend to be relegated, at least until very recently, to the lesser journals. Tullock would most likely have been discouraged by Hubbard and Vetter's (1996) analysis which showed that (some 30 years after Tullock's call for reform) replication studies comprised less than 10 % of the published empirical work.[5]

Yet Tullock would surely be happy that even journals which tend not to publish replication studies themselves are now making it easier for such studies to be conducted. It has become routine to require the publication of data sets and coding for accepted papers; publication of these ancillary materials is often housed at the journal website or other publicly-available locations.[6]

Still, Tullock may place too much faith in replication. Although beneficial, this is certainly not a fool-proof plan to catch all errors. And even if found, and published,

[5] Even more disheartening, this is about double the rates they report for the fields of management and marketing.

[6] Somewhat surprisingly, McCullough (2009) notes that the recently created "open-access" journals typically do not have policies requiring archiving data and code on their websites.

readers are more likely to be aware of the original celebrated study than they are of the replication study undermining it. Conclusions drawn from false studies may well persist.

Far more resources are spent on "robustness studies" (my terminology, not Tullock's) that determine robustness of the original, rather than correctness per se. Samples or methodologies are tweaked, assumptions are altered or proofs are simplified. These are not what Tullock has in mind as replication because "they almost never, however, copy the previous research completely, with the consequence that their work never constitutes a real repetition" (p. 122). He attacks the non-sciences for following this route, holding up sociology in particular as the poster-child of abuse.

Thus, there appears to be some movement of the economics profession in the direction Tullock would like, but replication within economics does not fit the social organization he claims to be representative of the sciences. Tullock's identified problems with the non-scientific methods of the social sciences would seem to suggest that economics, although making strides, still has plenty of room for improvement in the pursuit of new discoveries. However, as determined by Levy and Peart (2012, p. 170), Tullock's "characterization of economics as something other than a science has had, as far as we can tell, no impact in the literature in the economics of science".

4 Comparison to Olson

During the same time Tullock was developing his manuscript, Mancur Olson had just published his landmark treatise on *The Logic of Collective Action* (1965) explaining the incentives and disincentives involved for contributing to an inexcludable good. Olson's notion of "selective incentives" can go a long way to answering some of Tullock's questions.

In general, Olson expects free-riding to dominate latent group behavior for creating a common or public good. The common good in this case would be scientific advances. Tullock believes pure science occurs because of a personal quest for "truth" independent of, but consistent with, the social interest; applied science is spurred by a desire to create patentable practical applications; and induced curiosity is problematic due to the desire only for pecuniary benefits. According to Olson, large groups producing an inexcludable good would need to offer additional private benefits to elicit contributions. One such private benefit would include promotion and salary.

A distinction between Tullock and Olson centers on the underlying motivation stimulating cooperation. For Tullock, motivation is central to the quality of the contribution ("honest" versus "dishonest" science) whereas for Olson all marginal contributions are of equal quality.[7] Thus, to Olson, ideas in general would be the public good under consideration but Tullock is concerned with both the proliferation of good ideas and dismissal of bad ideas.

[7] Sandler (2015) discusses extensions to Olson which include heterogeneous contributions.

Olson relies strictly on the economic model of incentives. Tullock relies instead on the sociological explanation of inherent curiosity for the occurrence of pure science. Only applied science and induced curiosity are subject to economic incentives. Applied science is driven by the private rewards of a patent and curiosity can be induced by salary. Any patent may increase prestige, but only successful patents will generate income. Salaries can create an incentive for negative contributions (which are outside of Olson's model) by generating false science. Although bad ideas are generally weeded out by experts, when salaries and merit raises are determined by administrators outside of the field, they will tend to reward any new ideas developed regardless of their accuracy. Outsiders will be unfamiliar with journal prestige and more likely to count the number of publications rather than properly assess the quality of the publication.[8] They may also be fooled by studies which appear to be important simply due to the technical nature of the publication, leading to unnecessarily complicated theories. Long papers filled with obscure notation and lots of separate specialized theorems may impress outsiders more than similar concepts developed more succinctly by a single generalized theorem created by less restrictive assumptions and fewer lemmas. Experts would recognize the superiority of the latter approach but outsiders might figure the more unreadable it is, the more important it must be. While there is much potential truth to Tullock's critique, his argument would benefit from explaining why the total number of patents, representing additional lines on a c.v., as distinct from the success of such patents, would not lead to similar confusion between quantity and quality by the same outsider evaluators.

More directly connected to Olsonian "by-product theory" would be when the scientists themselves offer rewards for (positive) contributions. Tullock's suggestion of prizes for the best new research would fit this category. Tullock's description of scientific organization could also be extended to include the opposite type of selective incentive: private costs imposed on non-contributors who engage in false science. These methods could include ostracism or pointed reputational attacks in print (cf. recent examples include episodes related to Bruno Frey and John Lott in economics, or Donald Green and Michael LaCour in political science).

Tullock believes that bad ideas developed from induced curiosity are dominant in the social sciences but largely weeded out in pure science which produces "truth" and applications based on truth. Olson (1983) takes a different view on scientific progress. What are believed to be good ideas at the time of introduction may later turn out to be false. Similarly, what is not currently *entirely* correct may later be improved upon but that is only possible if the incorrect ideas are given attention rather than simply dismissed and forgotten. Olson strongly believes that science is progressive, even the social sciences. Successful new ideas are mostly corrections and extension of existing ideas. This is a constant theme for Olson, but stated most succinctly in Olson (1983, p. 29–30):

> How do we explain why some researchers assume that the truth of today is the error of tomorrow, whereas others suppose that the truth of today is, probably,

[8] As explained below, Tullock himself falls prey to a similar error.

> a special case of the truth of tomorrow? Part of the explanation, surely, is that in some areas the prevailing theories are so useful and persuasive, and are so clearly extensions or generalizations of their predecessors, that it is natural to suppose that the theory of the next generation will in turn be an extended or amended version of the present theory; in other areas, by contrast, the achievement and appeal of even the most fashionable approaches are so limited, and the life expectancy of paradigms so short, that it is natural to suppose that the first thing a researcher ought to do is clear away the rubbish.

> Another reason for the differences in attitudes towards prior work is presumably differences in temperament. Some like to find fault. Others would prefer, if possible, to be constructive, and are always on the lookout for new ideas that will expand their own and their professions' explanatory powers.

Olson was always keen to show not only how his theories developed new insights for some cases, but also how they were also consistent with conventional wisdom for others. In the preface to *Rise and Decline of Nations* (1982), Olson gave credit to those who influenced his ideas and mimicked Sir Isaac Newton by stating he "stood on the shoulders of giants". He viewed progress as a serious of building blocks, and would often stand upon even his own shoulders. Most of Olson's work post-*Logic* tended to build on his own prior work whereas Tullock, although sometimes doing the same (especially in refinements to his model of rent-seeking), often forged into whole new areas of inquiry starting from scratch. My interpretation is that Olson seems to best fit his second characterization of a scientist (constructive) and Tullock perhaps the first (likes to find fault, as anyone who ever engaged him in conversation might attest). But not entirely. In *Logic*, Olson first tore down the Marxist and Trumanite views of collective action before building his own, and Tullock as editor of *Public Choice* once published a paper (Laband and Sophocleus 1988) he knew to be wrong (with a disclaimer to readers alerting them to his viewpoint) just to stimulate new ideas on the subject.[9]

Interestingly, Tullock's description of the persistence for tariff advocacy described above is strikingly similar to the much later development of Olson's (1982) theory of "institutional sclerosis" on the formation of privileged groups who advocate for policies of concentrated benefits limited to themselves, financed by diffuse costs over the general public, overwhelming latent groups whose members have limited personal incentives to join the fight against such inefficient policies. To Olson, these policies are pushed through by the minority and largely ignored by the majority. For Tullock, inefficient policy adoption occurs when the special interests dishonestly convince the majority the special interests are not gaining at the

[9] Tullock added an opening footnote, the likes of which I have never seen before or since, to the published paper stating: "The editor of the journal has accepted and published this article because he feels it is important to get research started in the area. The weaknesses of Laband's approach, which are fully recognized by Laband, are obvious, but the editor at the moment can think of no way of doing better. Can the readers do better than both Laband and the editor?" In fact, this statement remains the only publicly-viewable part of the paper on the journal website.

majority's expense. Tullock's view would seem to be even more cynical than Olson's view as it involves intentional duplicity on the part of the special interests. Yet these contrasting views can be rectified if the reason the majority is so easily duped is because they recognize the lack of individual incentive in becoming better informed. Perhaps if *Organization* had received more attention and his fable became popularized, Tullock might have been mentioned as one of the giants on whose shoulders Olson stood as he (Olson) developed his theory of institutional sclerosis.

5 Tullock's suggested reforms

Tullock concludes with several practical proposals of his own. His suggested changes include reforming both the method of research, and the way in which research becomes known, primarily through publication in journals. Tullock offers positive reinforcement to scientists and negative reinforcement to editors. Additional light can be shed on understanding these reforms by applying his theory of rent-seeking, and reviewing his experiences in trying to publish his original rent-seeking paper (Tullock 1967).

5.1 Incentives for scientists

Tullock expresses concern over time wasted on preparing proposals. Here we see a slightly earlier view of Tullock's vision of the social waste from rent-seeking costs, before it was formally modeled in Tullock (1967). Time and resources devoted to making proposals in pursuit of a grant can be viewed as a form of rent-seeking. In the aggregate, under conditions of free entry and risk neutrality, social losses from rent-seeking may total the value of the grant entirely (Hillman and Katz 1984). Tullock's solution is that grant support should depend on past success of the applicant rather than the particular project. While he acknowledges this institutional design puts junior researchers at a distinct disadvantage, he believes that is already the case informally. Tullock's reform would see fewer resources spent to obtain the grant which can now instead be spent on making new discoveries. It also puts the premium on successful research to generate new grants rather than rewarding clever proposals that may end up going nowhere. If Tullock is correct that the current process is already biased against junior researchers, then the social losses from them not yet qualifying for a grant would be minimal. Net social losses from resources spent on trying to devise the most eye-catching proposal, as well as on all the proposals turned down, would be reduced.

Tullock also suggests offering rewards and prizes for general and specific research. Specific research awards would be offered to address specific problems, whereas general prizes would allow for creative research to uncover new solutions to problems not being otherwise addressed. This approach has been adopted to a certain extent. Some general interest and field journals award prizes, such as Georgescu-Roegen Prize for best article in *Southern Economic Journal* or Duncan Black prize for best article in *Public Choice*. Independent organizations tend to offer awards for specific research such as the Paul A. Samuelson award for Outstanding

Scholarly Writing on Lifelong Financial Security given out by TIAA-CREF. The American Political Science Association offers several book awards, each geared toward a different broad category (e.g. U.S. national policy or women and politics), but no general prize for best book in all of political science. The association does, however, offer a general prize for best article in the flagship general interest journal *American Political Science Review*.

From a purely economic perspective, this reform offers a greater potential private benefit from research which should clearly increase the level of activity. Yet as on grants, there is potential for social misallocation of resources through rent-seeking in a race for the prize. Consider journal awards. The *Southern Economic Journal* offers a prize whereas *Economic Inquiry* does not. The latter is typically ranked higher and considered more prestigious among the general interest journals. In contrast, *Public Choice* remains the primary journal of its field yet is the only political economy journal which offers a prize (two, in fact, with one specifically named in Tullock's honor). In either case, if the prizes serve only to redistribute submissions toward that journal rather than expand the market for research in total (or in quality), then social losses due to rent-seeking for the prize will occur. One benefit to prizes, however, is the public announcement of the winner, which calls attention to the most worthy research which might otherwise get overlooked by other scientists.

Tullock has suggested the "pure" researcher is not motivated through an economic benefit-cost perspective but instead by an intrinsic desire to learn the truth. Rather, it is the curiosity which is induced that Tullock claims will lead instead to unreliability. The pure scientists are said to be driven solely by pursuit of knowledge and therefore are to be trusted. The induced simply follow the money and are willing to falsify results if there are monetary rewards for doing so. (Tullock rules this out for the applied because it will be discovered that falsified applied tools will not actually work as intended.)

Tullock's proposed system of prizes and rewards will only work as an incentive for those who can be induced, on which Tullock lays all the problems of science. Tullock believes prizes are a socially beneficial inducement, whereas salary is not. This is because the hiring process can be flawed whereas an open call for prizes is judged not to be. Yet this open call represents an opening for unproductive rent-seeking as well. The attack on the university system for inducement problems seems to conflate other issues, such as administrators relying simply on journal reputation or journal editors making poor decisions. The problem of outsiders evaluating scientific progress has already been discussed. Tullock's critique of the editorial process is considered next.

5.2 Incentives for editors

Data collection represents the demand side whereas data publication represents the supply side. Tullock believes the quality of publication can be improved by ensuring only leaders of their field are employed as editors, but to limit the opportunity cost of their time, they should be in the "non-creative" phase of their career. To recruit the best editors would require increasing the salary and prestige of these positions.

Tullock also believes anonymous reviewers reduce the efficiency of submission and publication. Instead, decisions should be made only by the editor, or members of the editorial board in cases of submissions which fall outside the editor's area of expertise. Board editors would then make decisions independent from the chief editor. Tullock is concerned that anonymous reviewers may be junior scientists, selected because they are less likely to cause delay.

The internet has mitigated, but not completely solved, such delays and many of the website submissions allow for tracking so it is known when delay is caused by the editor (because the paper has never been sent out or is "waiting on editorial decision") or if instead the editor is waiting along with the author. Yet despite the time saving from electronic posting and retrieval of submission, it remains true that reviewers are often derelict in returning reports in a timely manner. Some journals have tried to play the incentive game by offering payment for returning reviews quickly. The amounts, however, are relatively meager, and I expect that if a reviewer is not already motivated to provide the collective good of a quality and timely review,[10] the private benefit of a few extra dollars will be unlikely to have much of an impact. If anything, it would more likely induce those with lower salaries, who would tend to be either junior or weaker scholars. Related ideas have been modeled and empirically tested (e.g. Hamermesh 1994; Chang and Lai 2001). Because the authors of such studies may have a vested interest in the outcome (if they are one of the induced), whether such studies of reviewer responses to monetary incentives would qualify as pure or applied research, or instead induced curiosity, I will not speculate.

Tullock also worries that editors are overly conservative and may reject worthy submissions. Editor prestige is determined by journal prestige, which is in turn determined only by publications and not by rejections. A journal gains prestige from publishing a success but is not penalized by rejecting submissions that eventually become successful after publication elsewhere. Therefore, editors will be more likely to make type II errors in rejecting important articles that may be more controversial. Progress is delayed by unnecessary rejection. To counter this, Tullock proposes two reforms. First, published articles should contain a list of where they were previously rejected. Second, there should be future investigation of the publication path for what are determined to be the best of the pure science articles. His solution is to shame editors by publishing the list of journals where a published paper had previously been rejected.

In my view, publication of past rejections has multiple flaws. First, both the author and publishing editor may be harmed. A list of rejections attached to a publication can serve as a scarlet letter worn by the author and publishing journal. It may deter readers away from that paper if rejections signal a lack of quality. Progress would then be further delayed. Furthermore, the public announcement that the journal is publishing a paper rejected elsewhere might also signal (or be misinterpreted as) lower standards of the accepting journal and potentially hurt its

[10] In an advice piece for reviewers, Choi (1998/2002) suggested to be timely, but not *too timely*, because a reputation for timeliness will impose upon the reviewer the private cost of receiving additional requests to review.

reputation. The wrong parties are being targeted. Second, shaming the rejecting editors may be inappropriate in many cases. Rejected papers may be improved before submission to a new journal where it subsequently gets accepted, in which case there would be a false implication of mistakes by rejecting editors if that paper winds up successful. It may have been the correct decision at the time to reject the original version of the paper. Thus the innocent may be punished along with the guilty.

Tullock also suggests that the submission path of specific papers be traced once they become prominent. While this counters my concern of limiting readership of the original paper when it is accompanied by a list of rejections, it still suffers from punishing innocent editors who may have rejected earlier, inferior, versions of the paper. The problem is compounded by scholarly prominence taking several years to develop, during which time editorship of various journals could have changed. Then, current editors can be punished for the sins of their fathers (or now, mothers).

Tullock identifies some important flaws in the editorial process, but his solutions may exacerbate rather than reduce the problems.

6 Wherefore art Tullock?

In this concluding section, I consider to what extent Tullock's descriptions can be profitably applied to Tullock's own scholarship and editorial experiences.

Both aspects of the expert-novice trade-off in making discoveries would seem applicable to Tullock himself. Tullock suggests that formal education is more important for developing useful "habits and contacts" rather than for subject knowledge. Tullock also argues self-education is more important for making new discoveries. This position naturally follows for someone who contributed so much to economics with so little formal training. It is what led Buchanan (1987) to refer to Tullock as a "natural economist". As an economics outsider (at least early in his career), self-taught without a graduate degree in economics, Tullock was not limited by the traditional usage of "resource cost" when developing his concept of rent-seeking (Tullock 1967) which may help explain why his original paper had such trouble being accepted by the top economics journals. Yet, being the foremost expert on rent-seeking may have later prevented him from being able to properly classify various types of rent-seeking due to his limited vision on formulating a precise definition of the term he could never articulate beyond what he refers to only as a "crude rubric" (Tullock 1988).

Tullock promotes detailing the history of important papers for the purpose of exposing the editors who had been unwilling to publish. Elsewhere (e.g. Tullock 1993, 2003), he has elaborated on the problems encountered trying to publish what became his own most famous article (Tullock 1967), naming the rejecting journals and in some cases the editors themselves. When I used to read these tales of woe I took the message to be one of persistance. Yet after reading *Organization*, I now wonder if the purpose of retelling the same narrative in various places was part of the "shaming" process to out the journals and editors who passed on such a significant insight. If so, continuing to shame George Stigler more than a decade after his passing (as in Tullock 2003) seems a bit churlish. Despite all the subsequent accolades for this pioneering study, Tullock appears to have remained resentful. Certainly, had Stigler accepted the paper at *Journal of Political Economy*, Anne Krueger would have been much more likely to have encountered it prior to publishing her *American Economic Review* article several years later (Krueger 1974), where the phrase rent-seeking was coined without any acknowledgement of Tullock's paper which introduced the subject of rent-seeking, if not the name. Tullock (2003) writes of the history as if he blames Stigler more for his arrogance than he does Krueger for her ignorance.

Tullock argues that tenure does not protect radicals, and is not needed for older faculty because radicalism dies out over a person's lifetime. By the time of *Organization*, the "young" radical Tullock had already received tenure (in the Dept of International Studies at University of South Carolina). Yet he was denied promotion to full professor three times after returning to the University of Virginia, for what appears to be political reasons (Breit 1987). Certainly, Tullock's own radicalism did not diminish with age.

In *Organization*, Tullock places emphasis on the spreading of ideas. The same year his book was published, Tullock founded the journal *Papers in Non-market Decision Making* (to become the less cumbersome, but possibly less descriptive, *Public Choice*) in order to give a home to papers using methods and addressing questions being shut out of the mainstream journals. These might well qualify as the type of "controversial" ideas (at least to economists if not the uninformed general public) being rejected by the type of overly conservative editors Tullock castigated. Surely, however, he was not yet in his own "non-creative" career phase suggesting Tullock did not follow his own advice for selecting an editor. Although, to be fair, given the infancy of the field it might have been difficult to find anyone who had established a strong enough reputation as a successful pure scientist to already be considered a leader of the public choice field and have so quickly moved into the non-creative twilight years in such a short span of time.

According to his editorial memoirs (Tullock 1991), Tullock made his own independent decisions on over 90 % of the submissions. He solicited the advice of other anonymous reviewers only in those rare cases when he felt he lacked enough personal expertise on that particular topic (or methodology?), consistent with his proposed editorial reform. The only manner in which the journal was not run in accordance to the ideas laid out in *Organization* stems from the lack of any "full repetition" studies being published in *Public Choice* nor did he ever (as far as I am aware) make a specific call for such papers to be submitted to the journal. In all other respects, Tullock successfully put his ideas into practice.

Finally, one might wish to categorize Tullock himself. One example, of many possible, might suffice to classify Tullock within the taxonomy developed in *Organization*. When Tullock was editor of *Public Choice* he published a paper by Meltzer and Richard (1983a) despite expressing "doubt" about the theory in a comment of his own (Tullock 1983). Still, he allowed the authors to have the last word in a rejoinder even though they claimed Tullock's comments on their paper to be "both wrong and irrelevant" (Meltzer and Richard 1983b). Clearly, Tullock was more interested in getting to the truth than merely trumpeting his own views.

Although Tullock has been referred to as a “natural economist” (Buchanan 1987), he also fit the characteristics of a “pure” scientist.

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