Gordon Tullock and Karl Popper: Their Correspondence

David M. Levy
Center for Study of Public Choice
George Mason University

Sandra J. Peart
Jepson School of Leadership Studies
University of Richmond

26 May 2015

An earlier version was presented at the 2015 meeting of the Public Choice Society. We thank Mr. Ron Basich for helping with the manuscript collection. Alex Tabarrok has been a source of much enthusiasm and encouragement. Mr. R. August Hardy helped check the paper. We are grateful for access to the Hoover Institute archives.

1 Introduction

Our recent essay on Gordon Tullock's 1966 *Organization of Inquiry* (Tullock [1966] 2005) made two points. First, this work even after nearly 50 years since publication has important things to say about what economists choose; lessons that seemed missed by the first generation of readers. The most pugnacious claim that Tullock advanced was that economics is more of a racket than a science. Second, Tullock seemed to be relying on an unformalized notion of necessary truth in which purposive behavior is a concept we apply to the world, instead of discovering it in the world. We made these claims on the basis of Tullock's book and what we know of the published philosophy of science literature that engages these topics. We'll consider these two issues in separate sections.

2 The Tullock Popper Correspondence

Now that we have begun our study of the long correspondence between Karl Popper and Gordon Tullock, we can add some to our previous conclusions.[1] The first thing we learn is that we need to be very careful locating *Organization of Inquiry* on the basis of what Tullock wrote about it. From the correspondence, we learn from a letter from Tullock to Joseph [Agassi][2] and Karl [Popper] of July 9, 1958 about Tullock's forthcoming fellowship at the economics department of the University of Virginia which he describes a "practically a colony of the University of Chicago." We quote from an important paragraph:

> I have been giving some thought coming over to London. My program would call for writing a book essentially based on the *Logic* [*of Scientific Discovery*?] I think maybe I have discovered a third system of Positional Logic the subject matter of which may be indicated by my provisional title: *The Organization of Inquiry*. The problems are two, in the first place I am not certain my theory of right, and secondly, it may be too trivial to bother with. The positional logic of *Inside Bureaucracy* is much less elaborate than that of economics, and my latest theory is even less so. At

[1] We are grateful to the Hoover Institution for access to the Karl Raimund Popper papers in which all the correspondence is located. The Gordon Tullock Papers were being processed in our December 2014 visit so these will be the subject of a later visit. However, in March 2015 Mr. Ron Basich carefully photographed the contents of a list of folders in the Tullock papers that we sent him.

[2] Joseph Agassi (2013, p. 131) cites Tullock in *Organization of Inquiry* as asking the right question about the origin of scientific associations.

> any event, I would like to get the *Logic* as soon as possible, and after further thought in Virginia I might be able to decide definitely.

Our reading of "*Logic*" as Popper's English version of his 1935 *Logik der Forschung* is consistent with Tullock's concern in a March 5, 1958 letter:

> I am sorry to hear that *Logic of Scientific Inquiry* [sic] is still incomplete, partly because I am, as you know, enthusiastic about the book, and partly because I hope to get your opinion of my project after you finish it.

In this context, let us reread Tullock's first paragraph in *The Organization of Inquiry* :

> The genesis of this book was a period of about six months spent working with Karl Popper. At the time I had no intention of writing a book on science and my studies were devoted to an entirely different problem [the note cites *Politics of Bureaucracy*]; nevertheless, Popper's approach necessarily rubbed off on me, and I became interested in the problems of science. Since I felt I had little chance of making any significant addition to Popper's work on the philosophy were directed toward the problem of a science as a social system [1966, p. 1; [1966] 2005, p. xix.)

Clearly, Tullock was thinking of a visit with Popper before he came to Virginia. However, the oddity of Tullock denying an interest in science studies before associating with Popper need to be remarked. The claim seems to have passed without comment.[3] But the oddity expands when read in the context of Tullock's letter in which he already has the actual title, *Organization of Inquiry.* Tullock's decision to forego direct citations to Popper's work closes off one line of inquiry because we know some of the offprints Popper was sending Tullock before Virginia.

We pointed out (Levy and Peart 2012) Tullock's argument that economics is a kind of racket because economists do not pay very much, if at all, for denying professional consensus in their service to some political popular cause. In Popper's letter of March 6, 1967 acknowledging his delight at receipt of *Organization of Inquiry* he lets on that he knows all about the sort of factionalized science that would feature so prominently in the variations on the Duhem-Quine principle that would frequently quoted against Popper's falsification principle.

---

[3] It is not questioned in Charles Rowley's notes to the Liberty Fund edition. The rather obvious error in the Duke edition (Tullock 1966) that Popper pointed out in his letter of March 6, 1967 remain in Rowley's edition. Popper's book is not *Logic of Scientific Inquiry* (Tullock [1966] 2006, p. 53; 1966, p. 65).

In a letter to Popper of January 23, 1991, Tullock extends this self-interested account to explain the silence of the economists on political sensitive issues: .[4]

> The main point of this letter to you, however, is to enclose a rather long paper on methodology. This is very rough draft and inspired essentially by a general annoyance with some of the things that are going on in economics at the moment. To give a little bit of Freudian psychology (even that may be true in some cases) I suspect that the present turn to extremely abstract economics is simple escapism. Many of the conclusions drawn by economics about actual policy are very unpopular in the academic circles outside of economics. The young man who wants to get along well at faculty cocktail parties is better advised if he can say he's doing mathematical work in economics than if he says that the minimum wage act is hard on the poor. But this may be pure bias on my part. In any event, if you take the time to read this rather long paper, I'd appreciate any comments.

3 Necessary Truth

We begin with a conversation with Tullock (August 31, 2006) after his discussion with Buchanan at the Summer Institute about the *Calculus of Consent*. Here it was obvious that his memory was fading.[5]

> *David:* You surprised me, I think you surprise lots of people when you said that von Mises's *Human Action* had a big impact on you.
>
> *Gordon:* Yes. In the first place, let's begin with the fact that at the time I had one course in economics, which lasted 12 weeks, it was supposed to last 13 weeks but I was drafted, and that had got me to reading economics journals. I saw at the Yale Co-Op, when I was studying Chinese at Yale, I saw a pile of books bound in red that said *Human Action* and I picked one up. The thing which made a big impact on me was the early part where he talked about that you can use the same kind attack on things others than economics, I'd never heard anyone say that before. I read the book actually three times and during that time I came to the conclusion that I was going to write a book about bureaucracy on the same kind of self-interested motives on the part of the participants as economics. He did not maintain that it also led to good results even though it did in economics.

---

[4] The silence of the economists on minimum wage laws comes up in another context, in letter from Leo Rosten to Milton Friedman August 25, 1965 in which Rosten reports a conversation with an unnamed MIT economist (Friedman is told that he is not Samuelson) who explains why mainstream economists maintain a silence on minimum wage laws. They do not want to be seen agreeing with Friedman. The episode is noted in Friedman and Friedman (1998, p. 218). Friedman and Rosten were very close; Rosten's 1970 pen portrait of Friedman—"An infuriating man"— cites his opposition to minimum wage laws. The letter is found in the William Baroody Papers at the Library of Congress. We discovered it accidently when doing manuscript work on the failed grant proposal to the Ford Foundation by the Thomas Jefferson Center at which Tullock was the first fellowship holder, Levy and Peart (2014).

[5] Alex Tabarrok tells us he had similar conservations.

The question Tullock raised is the critical one: what is the institutional setting in which the self-interest of economists is directed toward good results? There is no reason to believe that the one we have now does this.

Since the von Mises Tullock connection would disorient scholarship on both the Austrian and Virginia Schools, perhaps we ought not to rely on memory in a single conversation. Fortunately, we can control memory by manuscript. In Tullock's 1971 contribution to *Toward Liberty*, the multilanguage tribute to von Mises on his 90th birthday, we read how Tullock preface's his contribution:

> (It may seem odd to place an article originally designed for publication in a biological journal in a collection of articles to Ludwig von Mises. Among his other distinctions, Professor von Mises was among the first to point out that economics can be expanded to deal with many areas outside of its traditional scope. In my own case, my work in expanding economics into new areas was, in a real sense, begun by my reading of *Human Action.* The article below, then, represents my most extreme application of economics outside its pre-von Mises boundaries.) (Tullock 1971, 2:375).

This article is not included in the Rowley edition of Tullock's works.[6]

4 Concluding Puzzle

Did Tullock have an impact on Popper? Popper surely did not need Tullock to tell him that scientists are reluctant to allow their models to be falsified. And why this reluctance? Presumably because falsification would not serve their purpose. Could that appeal be von Mises via Tullock? Perhaps it is important that Popper did, much to the discomfort of some admirers, take purposive behavior as a necessary truth (Levy and Peart 2012).

Popper as his correspondence with A. N. Prior makes clear was completely familiar with the Lewis systems of strict implication. We have not begun a study of the Popper Tarsksi correspondence which might be illuminating. Tarski co-formalized Gödel's intuition that one can move between "necessary" and

[6] Tullock's references to von Mises's works are found in the cumulative index, [Burgess], p. 554. In violation of Library of Congress conventions, the entry is "Mises, Ludwig von" not "Von Mises, Ludwig."

"demonstrated." The term von Mises uses for necessary truth is apodetic, which is a transliteration of the Greek for demonstrated.

Documentary Appendix

1. Earliest [?] Tullock to Popper [GT Papers]
2. Response to #1 Popper to Tullock [GT Papers
3. August 7, 1957 Tullock to Popper [KRP Papers; GT Papers
4. August 14, 1957 Popper to Tullock [GT Papers
5. September 10, 1957 Tullock to Popper [KRP Papers; GT Papers
6. January 29, 1958 Popper to Tullock [GT Papers
7. March 5, 1958 Tullock to Popper [KRP Papers; GT Papers
8. July 2, 1958 Agassi to Tullock [GT Papers
9. July 9, 1958 Tullock to Popper and Agassi [KRP Papers
10. February 14, 1959 Tullock to Popper [GT Papers
11. April 14, 1959 Popper to Tullock [GT Papers
12. April 21, 1959 Tullock to Popper [GT Papers
13. March 6, 1967 Popper to Tullock [GT Papers
14. March 13, 1967 Tullock to Popper [GT Papers
15. July 12, 1967 Popper to Tullock [GT Papers
16. July 21, 1967 Tullock to Popper [GT Papers
17. July 24, 1967 Popper to Tullock [GT Papers
18. March 31, 1970 Tullock to Popper [GT Papers
19. April 4, 1970 Popper to Tullock [GT Papers
20. January 23, 1991 Tullock to Popper [KRP Papers
21. March 19, 1991 Tullock to Popper [KRP Papers
22. June 3, 1991 Popper to Tullock [KRP Papers

23. September 23, 1991 Tullock to Popper [KRP Papers

24. October 22, 1992 Tullock to Popper [KRP Papers

25. [Post October 22, 1992] Popper to Tullock [KRP Papers

26. December 7, 1992 Tullock to Popper [KRP Papers

27. December 19, 1992 Popper to Tullock [KRP Papers

28. January 11, 199[3] Tullock to Popper [KRP Papers

Dr Popper:

I should like to thank you again for your encouragement and assistance. Ifeel particularly strongly about your assistance because I now have a ~~fairlyzatzgxnkzgood~~ much improved formulation for that part of my theory to which you objected ~~to~~ so strenously. The new formulation doesn't directly derive from anything you said, but the fact that ~~I xxk worked it~~ it came to me out within ~~two weeks~~ 10 days of your objections is fairly good evidence that they played a major role.

~~Thexrestzrexsonxforxthiszletter,~~

I was not functioning at full efficiency on the last day of the seminar and I ~~thinkgxthax~~ failed to tell you my decision~~x~~ to take advantage of your kind offer.~~toxworkxwithxyoux~~ I have a pplied to theVolker Fund for a grant to go to Stanford to work on ~~a~~ my book. I think (please don't tell them this) that I will probably join you there even if they don't pay my way. For personal reasons I will probably ~~haxexto xspendxthexearly~~ not be able to go to the West Coast until late in the fall, but I can do a good deal of preliminary work before I actually move.

I look forward to seeing you in the not to distant future.

Manor Road
PENN, Bucks,
England.

Dear Mr Tullock,

It was nice to hear from you, and the contents of your letter added to my pleasure: (a) that you have succeeded in improving that part of your theory to which – as you say – I "objected so strenuously". (I did not feel that I was labouring so hard at the time.)

(b) that you intend to work in Stanford.

I intend to arrive in Palo Alto on October 15th; and perhaps I shall find you there. As to the Volker Fund: Mr Cornuelle knows my opinion of you, and since you do not say that I should write to them about you, I shall not do so, except you or they ask me to do so. Of course I shall write if it may help~~s~~.

Please let me know about your movements, especially if you go to Palo Alto before I come there.

I am looking forward to seeing you again.

Yours sincerely

K. R. Popper.

August 7,1957

Karl:

I was very dissapointed at not being able to see you in New York, but it was unfortunately impossible. My mother is now feeling fine, although a little weak, but a few weeks of rest will fix that up. The whole thing put me a little behind on my schedule, but I still hope to have a second draft manuscript ready by the end of the month. I can send it to you at your convenience, but I am uncertain whether this would be a good time from your standpoint. As you know, I am planning to send it to a number of people (partly to get comments, and partly as a sort of indirect way of sneaking up on a publisher) and I don't want to waste time by letting it sit on somebody's desk who is temporarily too busy to read it. Thus, if you are going to be busy in September, possible I could send it first to Von Mises or Wittfogel and then send it to you in October.

At Atlanta you told the assembled scholars that they should avoid becoming too specialized. As an indication that your advice was followed by at least one member of the audience, I attach a short essay on nucleur physics. This is sent partly as proof that I am following your advice, for what could be farther from my field, and partly because it is just possible that I am right.

Sincerely,

THE LONDON SCHOOL OF ECONOMICS AND POLITICAL SCIENCE.
(UNIVERSITY OF LONDON)

HOUGHTON STREET,
ALDWYCH,
LONDON W.C.2.

Telephone: Holborn 7686 (7 lines).
Telegrams: "Poleconics, Estrand," London.

August 14th, 1957.

Dear Gordon,

Thank you for your letter of August 7th. I am glad to hear that your mother is well again and that, in spite of the upset, you are almost ready with your second draft. I think it would be a good idea to send it to von Mises etc. first because I am still extremely busy with proofs which I should have returned to the printers in February.

I am very glad that you have given a try to my suggestions, in Atlanta, not to specialise, and I liked your paper both as an attempt at theory construction in general and as one which may have some use in nuclear theory. You will not be surprised, however, if I tell you that it seems to me that your first suggestion does not seem to be adequate numerically (as you yourself anticipate). The forces needed are of a different order, I think.

Your second suggestion seems to me free from this objection, and even if it should be unacceptable in the form you give it, there is something new and worth considering in the idea of a force which is directed like a searchlight.

This idea could be used, of course, quite independently of your suggestion to identify nuclear attractive forces and gravitation (an idea which does not exert any sufficiently great attraction upon me). I shall think more about this idea.

Yours,

Karl.

49 Morgan Ave.
East Haven, Conn.
Sept 10, 1957

Karl:

I suppose I should start by explaining the above address. Shortly after I received your last letter Claude Robinson, the one indispensable and irreplaceable man at the Princeton Panel had a heart attack. As a result the Panel folded like a tent, and I fled to New Haven (East Haven is a suburb). I am now engaged in finishing up my book, which is almost done, working on some articles,, refurbishing my Chinese, producing a pot-boiler history of Korea from 1945 to the present forwhich I already have a publisher, and job-hunting. The job-hunting is temporarily being deferred, since I should know a good deal more about the propsects for my book in another two months, and that seems a good time to begin a serious search for employment.

Turning to my efforts to get a Nobel prize in physics, your comments on my idea suggest two thoughts to me. In the first place, I was rather unclear in stating the problem of "strengthening" gravity. The question is how far apart the elementary particles are; if they are close enough together, then my idea would permit the weak gravitational forces to overcome practically any electrical repulsive forces. If the distance between the "surface" of two particles was only 1/1,000,000th of the distance between their centers, then the inverse squares law would permit even the weakest attractive force located at the surface to overcome a tremendously stronger force at the centers. I presume, however, that you realized this even if I didn't express it clearly. The other thought which occured to me, and probably also to you, is that, if each elementary particle radiates a force out "like a searchlight," then, assuming the points and directions of the radiation are fixed, we could build up a picture of the nucleus rather like the chemists model of an organic compound, with the various particles sticking to each other only at certain points.

I shall shortly send you another draft of my article on Fisheries.

Sincerely,

Gordon

Fallowfield, Manor Road, Penn, Buckinghamshire

THE LONDON SCHOOL OF ECONOMICS AND POLITICAL SCIENCE.
(UNIVERSITY OF LONDON)

HOUGHTON STREET,
ALDWYCH,
LONDON W.C.2.

Telephone: Holborn 7686 (7 lines).
Telegrams: "Poleconics, Estrand," London

January 29th, 1958.

Dear Gordon,

When I wanted to send you a copy of The Poverty of Historicism I found that I had mislaid your last letter. You wrote in this letter that you had left the Princeton Panel - or that it had left you - and, I believe, you wrote that you are now somewhere near Yale. Unfortunately, the card in our address-index has only two Princeton addresses and now what is obviously your mother's address in Florida. However, I do not know whether I have deciphered it successfully. I am trying my luck, anyway. Many thanks for your Christmas wishes.

I have still not finished the galley proofs of my Logic of Scientific Discovery.

With the best wishes for 1958

Yours sincerely,

Karl (Popper).

P.S. I am also sending a copy of this letter to the Princeton Panel address in the hope that one of the two will reach you.

49 Morgan Ave.
East Haven, Conn.
March 5, 1958

Karl:

I am sorry about the delay in answering your letters of January 29, but they happened to arrive at a time when my address is most uncertain. The Princeton Panel is in process of being partly revived and they have been negotiating with me to come back. So far we have not reached agreement on the highly important item of salary, so I don't know whether I will or not. Meanwhile I can only add the address from which I write to the list you have and say that I don't quite know where I will be living next month. Presumably any one of the three addresses will eventually reach me.

In any event, I already have The Poverty of Historicism. I saw it on sale in the Yale Co-op and picked up a copy. An autographed copy would be even better, of course, but I hope to get you to autograph it sometime. The reviews have been good enough to surprise even me. I was particularly impressed by your getting lead position in the Times. The only criticisms from the English periodicals come from those who have not read the Historical Note, and who accuse you of beating a dead horse. This was to be expected; if reviewers don't read the book, you can hardly expect them to read a mere note. I haven't seen any American reviews, but I imagine they will be rather more interesting since historicism is somewhat more alive as a doctrine here than in England. The horse is not really dead, of course, even in England, and on the continent, where your book is most needed, it is very lively.

I am sorry to hear that Logic of Scientific Inquiry is still incomplete, partly because I am, as you know, enthusiastic about the book, and partly because I hope to get your opinion on my project after you finish it.

Meanwhile I have been scientizing. You may recall that I was very much surprised by the "perpetual motion machine of the second order", mostly because I had not realized that the second law of thermodynamics was taken seriously by physicists. It was obviously true as an empirical observation, but there seemed no theoretical reason for accepting it beyond the range of present experience. I have been thinking about the problem ever since and I think I have discovered an explanation for the "law", and, if my explanation is correct, then the law is true only under certain conditions, although every machine so far built by man would fall within these conditions.

Firstly, let me invent a machine of my own. In a heat bath we place an endothermic chemical reaction. Such reactions are at least as numerous as exothermic reactions, and some of them are capable of absorbing really surprising amounts of heat. We now have a temperature gradie nt, and can use it to power xxxx almost any kindof heat engine. Suppose we use a steam engine, with the condenser attached to the endothermic chemical reaction and the boiler and turbine simply sitting in the heat bath. The apparatus is perfectly normal and works just the way any steam engine works. The only difference is in its relations with its environment. While the normal steam engine obtains heat from an exothermic chemical reaction, and then discharges it into its general environment, this device obtains heat from its general environment. The heat is then, partly converted into mass in the the endothermic chemical reaction, partly converted into useful work, say lifting a weight, and partly re-dissapated back to its environment by way of friction in the machinery. The "power source" is located at the condenser instead of at the boiler, but the machine itself is identical to a normal heat engine.

No such machine has ever been built because the earth is too cold. Given the range of possible chemical reactions, the relationship between speed of chemical reaction and temperature, and the various problems of designing heat engines, I doubt if such an engine will ever be possible useing the earths crustal area as a heat bath. The earth's crustal area, on the other hand, is an admirable heat sink, and thus all of our existing heat engines operate according to the second law. The law, however, only applies because of the general temperature level. At higher environemental temperatures, machines violating the law could be built and might well be economically more desirable than machines obeying the law. Confining ourselves to chemically powered machines, if we raise temperatures high enough, to the range of 4,000-5,000 degrees where molecules tend to dissasociate, only machinese which violate the law will work. Under these circumstances it would be as hard to run a steame ngine on an exothermic chemical reaction as it would be to power one with an endothermic reaction on the surface of the earth, At these temperatures, however, an endothermic machine would reach its maximum effidency.

So far I have discussed only engines using chemical power sources, but the argument can be easily generalized to cover any heat engines. Such engines require a heat gradient to operate, and all existing examples obtain this gradient by some method of raising temperatures above the environmental level. The second law then applies, but it is possible to take the opposite course, and obtain your heat gradient by reducing temperatures, under these circumstances the converse of the law would apply. In the situation where there is no perceptible heat gradient, as you show in your book, machinesx can be built which obey neither the law nor its converse, but these machinese would not be economically feasible.

There is, however, another set of conditions which make many machinese conform to the second law, briefly, we aren't as good engineers as might be desired. There is always a lot of friction in any of our machines, although the amount goes down as we improve our techniques. If we take a machine, such as a windmill, which is not directly a heat engine, it may still conform to the second law. Engineers, of course, will normally tell you that windmills really are heat engines because the winds are the result of heat differentials, but this is irelevant to the application of the second law because the equations, on examination, will turn out to apply only to the immediate environment of the windmill. A functioning windmill causes fourc hanges: the speed of the air passing over it is reduced, some work is accomplished, turbulence is created or increased in the air passing over the mill, and friction releases considerable heat. The second law is taken as indicating that the sum of the two later effects is g reater than the summ of the first two. Since the law itself cannot b e used to prove this proposition in a d iscussion of the validity of the law, there is only one bit of evidence for its truth, the fact that no windmill has been built which violates it. Since we are only at the beginning of the process of discovering the real nature of the universe, and since our present methods are presumabl y far from ideal, our failure up tothe present to perform a certain task can certainly not be taken as proof that the task is impossible. Further, in this particular case, our failure to build machinesx efficient enough to violate the second law in part arises out of the widespread beleif in the law itself. No engineer who is convinced of the truth of the law is likely to waste resources on devices to violate it. Thus, even ifour knowledge of airfoils and bearings was, in fact, great enough for us to build a windmill which would violate the rule, it would not be built.

As you can s ee, I am taking advantage of your beliief in science as a process of discussion by "discussing" with you. I don't see how you can complain without tearing up <u>The Logic of Scientific Inquiry</u>.

Sincerely,

July 2nd, 1958

Dear Gordon,

Karl showed me your last letter to him (of 5 March) when it arrived; we read it together and enjoyed it. Since then and till yesterday, when I took it from him, it lied heavily on his conscience. He wanted to reply ~~to~~ in detail but has no time as yet. The first volume of his *Logic of Scientific Discovery* is now being prepared in page proofs, and meanwhile he is reading the galley proofs of the second, the *Postscript: After Twenty Years*, a sequel to *Logic* etc., apart from writing an occasional article and giving an occasional lecture and even attending to pressing letter writing from time to time (testimonials, recommendations, official material) and so on. He is as busy as ever, though he feels much better now that he went to Switzerland for a few days – he gave a lecture a week ago and came back only yesterday. Of course he goes on improving his work untill it is taken to the book-binder. He was very unwell in the winter and ~~tay~~ tried

2

to dodge an operation which he may still have to undergo after the books appear, though I hope not. Besides, he has a new department of philosophy – up till now it was merely an optional course in a dpt of logic and scientific method – in the new faculty or degree course or whathave you of philosophy and economics. This raised quite a number of administrative problems. Here I come in too, for there is a project of having one (or two) lectureship(s) in the department, which I hoped to apply for, as well as a readership. ~~which another~~ As usual the project was nearly postponed again – lack of money – and ultimately the readership was established but the lectureship postponed for another year. I tried my luck elsewhere with no success. Ultimately Karl arranged for a temporary arrangement (for me) for another year even though the person whom I am replacing now is back from his leave.

This gives you some idea about how busy Karl is and about my present plans. The year went by quickly; I did much of Karl's teaching and some of my own, published two reviews offprints of which I enclose and reviewed L von Mises' ~~History of The~~ Theory & History in Times Literary Supplement of May 16th (no offprints) which you may have seen, and spend much time on an index to Karl's book. This is practically all apart from two more unpublished reviews and two rejected papers (both ~~very~~ good but very unconventional). Judith has done much better: she had a son eight weeks ago – his name is Aaron (the Golden Calf and all that, let us hope) – and handed in most of her PhD thesis which she hopes to complete soon. Our daughter Tirza has acclimatized back to the English standards, very reluctantly I admit. This is about all, apart from my adventures in other departments here as Karl's unofficial representative. I read a paper in the statistics

Sem
seminar on Karl's work on probability and one in the sociology seminar on methodological individualism – its head, (Ernest Gellner) had declared a few times that Karl's methodological individualism is letting in psychologism through the back door. and I succeeded to break his opposition. But the main methodological interest is among the young economists and I participate in a staff seminar on the testing of economic theories. I would have enjoyed a conversation with you on the topic I am sure, and would like to know of your reaction to the recession or however you come to call the present American economic setup.

As to your comment on the second law of thermodynamics, I find it most imaginative and interesting. You should publish it. However, here are some comments which might interest you. Insofar as your cold engine is viewed as utilizing the heat gradient between

5

two *given* reservoirs, your machine is explained as well, or rather as unsatisfactorily, as ordinary steam engine. ~~As~~ Insofar as the cause of the reservoir which differs from the environment is concerned, not only your cold reservoir, but also the coal heat reservoir, is not adequately explained – if at all. The point is really the loss of sight of the problems, due I think to inability to admire thinkers in spite – or rather because – of their ingenious errors. Rumford, you may remember, declared that heat is no substance for it does not follow the law of conservation of matter being capable of creat~~ed~~ion (by friction) in unlimited quantities~~.~~. The obvious answer to this is that in boring cannons we transfer caloric or heat matter. This is, for

6

instance Dalton's reply. To mitigate this Davy – Rumford's associate – rubbed two pieces of ice in vacuo. Hare of Philadelphia – Franklin's pupil – tried to refute Davy's energetic calculations. Fourier tried to find the law of heat-transfer acceptable to both parties to be used in the argument. Carnot tried to argue that there can be no work without heat transference thus trying to come closest to answering Rumford's contention. Very reasonably he substituted for Rumford's chemical machine (that man *is* a chemical machine is a very old idea, see for instance Franklin's *Autobiog.*) by heat reservoir, claiming that however chemical machines produce work, if they produce heat they *may* be heat reservoirs, so we should see if the simpler case is explicable by a caloric theory.

7

Since Carnot was wrong and since this 'has' to be ignored, confusion abounds till today. People still say that heat is energy though of course they mean to say that it is the concentration of energy, which is quite a different matter. The argument was considered closed - which it is evidently not the case - and the only successful theory of heat - the theory of gases - is hailed as a complete success while it is a small region, as you point out. People come independently again to the problem in a wider context - chemical (and biological) heat engines and non-heat engines converting chemical potential straight into ~~heat~~ work. The first problem is admirably well summarised in Prigogin's Thermodynamics of Irreversible Processes (Published in US) which emphasises that the law as it stands excludes chemical

(both exo-and endo-thermic)
heat processes). The second was worked in Israel by the Katchalsky brothers. I heard a paper on it but have no reference.

But I am too prolix. My point is that Karl hit the theory at its stronghold – theory of gases – claiming that the Brownian motion is a refutation to it. Your criticism shows that even if successful it covers a very narrow ground. Two very different criticisms, each having its own merit. However, I hope you see that I am with you only claiming that you can still widen your claim.

I cannot judge how successful are recent attempts to generalize the law, but they clearly show its need for generalization. But I am afraid that professional physicists are impatient with your kind of criticism (claiming that it is too much ahead of its time).

Yours sincerely,
Joseph Agassi

Apt. E-1
120 Prospect St.
Princeton, N. J.
July 9, 1958

Joseph: (Also Karl)

Thank you for your letter, and thank you also for rescueing mine. I was just about to write again giving my new address and bringing you up to date on various developments. As you can perhaps geuss from the address given above, Robbinson and I reached agreem nt on money and I am now working again for the Princeton Panel. This turns out to be merely temporary, however. This fall I will go to the University of Virginia, the econmic department of which is prrctically a colony of the University of Chicago. I will hold for one year the Thomas Jefferson Post-Doctoral Fellowship. I got thisfellowship without really applying; I just sent them a copy of the manuscript of my book, and thefellowship followed. This means I have already made more money out of my book (there are no duties connected with the fellowship except adding an element of intellectual tone to the university atmosphere) than I could normally expect from royalties. From the 15th of September my address will be: Dept of Economics, U. of Va. Charlotsville, Va.

I am sorry to hear about Karl's health. The new child, however, is good news. With that name you should be careful not to return to Isreal since the widespread use of alphabetical ordering in our society gives him a major advantage in areas where the latin alphabet is used. I am also happy to hear that you are still around London. I may come to London myself next year, and look forward to seeing you.

Thanks for the reprints. I had seen the Duhem one but not Hegel. I will look up Von Mises since I am curious about your views on his position. Too bad you had your papers rejected, but this happens sometimes. It doesn't necessaily mean much about the text since editors frequently don't know much about the subjects.

The present status of my book is confused. A surprsing number of people were willing to read and criticize it, but so far I have received no significant negative substantinve criticism. Although I would still like your (plural) opinions on it, I think I can proceed on the assumption that at least I am not crazy. Almost all of the readers made negative comments ont he style, however. Since I am very far from a distinguished writter, these comments are justified. I don't think this is very important, however, because almost all social scientists, including my critics, write badly. While I prefer good writting to bad, bad is demonstrrtively publishable. To check my opinion I have sent one copy of the manuscript off to Knopf. If they accept it I will just make some minor substantive changes. If they reject it I will use the first part of my stay in Virginia to re-write it, although I fear that my style is iremediable. If Knopf rejects it I plan to have about 100 copies run off and will send you copies.

My later plans are neccessarily vague. I don't intend to"firm them up" until I know more about the reception my book will receive. I have been giving some thought to coming over to London. My program would call for writting a book essentially based on the <u>Logic</u>. I think maybe I have discovered a third system of Positional Logic the subject matter of which may be indicated by my provisional title: <u>The Organization of Inquiry</u>. The problems are two, in the first place I am not certain my theory is right, and, secondly, it may be too trivial to bother with. The positional logic of <u>Inside Bureaucracy</u> is much less elaborate than that ofeconomics, and my latest theory is even less so. At any event, I would like to get the <u>Logic</u> as soon as possible, and after further thought at Virginia I may be able to decide definitely.

Naturally I am pleased to have my ideas on thermodynamics taken seriously by two such distinguished critics. The idea of getting something on physics published and thus making myself permanently one-up on all other "social scientists" is also most attractive. I have, however, considerable doubts which I hope you will help me with. In the first place, do you really think anything as minor as this would be publishable? If so, where, and what would be the most acceptable form? Please do not let your natural good manners overcome your love of truth in answering the first question.

The second problem arises from your letter. I just don't understand some of your comments. This probably results from your flattering but untrue assumption that I know as much about physics and the history of science as you do. My idea is really very simple: Any heat engine requires a heat gradient to perform. in the environment of the surface of the earthx the only practical heat gradients result from contrasting a heat source with the general envdronment. In a hotter environment this would not be so, a"cold"source such as an endothermic chemical reaction might be used to produce the gradient. This idea is independent of theories of heat and really is not in the same universe of discourse as Karl's position. My theory, for example, could be true even if there were no such thing as heat and if cold was a real quantity.

Your remarks about recent theories indicating that chemical reactions are not subject to the "law" seem to me a particularly clear case of fitting facts to pre-conceived notions. Although it has little to do with my basic position, I think it is clear that an exothermic chemical reaction, which converts mass into energy is an illustration of the law while an endothermic reaction converting energy into mass is a contradiction. The effort to take all chemical reactions out of the law obviously arises from the fact that accepting exothermic reactions as illustrative would require recognition of endothermic reactions as exceptions. Exothermic and endothermic nucleaur reactions would also fit in the same way.

Whether I am right or wrong, I am at least discussing, which is likely to lead to truth in the long run.

Sincerly

PS, With regard to your seminar on confirmation of Economic theory, Are you familiar with Oskar Morgenstern's work on the accuracy of economic statistics? He has done three studies of which the one on international gold movements is, I think, the best. The present depression here is an obvious illustration of the difficulty. If one takes the statistical series seriously, then it has disproved all existing theories of depression except the very old fashioned (I learned it in high school) "business confidence" one. Personally I tend to the view that some of the series are wrong in the sense that they do not measure the relevant magnitude, and consequently stick to the "Chicago" theory, but this is more an act of faith than anything else. Certainly present developments seem to present a case book example of a "confidence" depression.

sinc,

Feb 14, 1959

Karl:

I have sent to you by boat a multilithed copy of my book. The title has been temporarily changed, largely because the one you suggested seemed to have an unfortunate effect on many of my readers. They tended to consider bureaucracy even more important to my book than it is, and to completely ignore the parts of it which refer to other subjects.

Since I have not heard from you since Agassis letter, and he has not replied to my letter, I presume you are very busy. Nevertheless, I hope you can find time to read and comment on my book. I have sent ten copies to ten publishers and <u>hope</u> that at least one of them will accept. This puts some thing of a time limit on the usefulness of criticisms.

A friend here has received a copy of <u>Logic</u> and I have re-read it again. I look forward to the <u>Postscript</u> on which you are probably now engaged.

In my letter to Agassi I mentioned the possibility of my coming to London to do some work on a project connected with the social organization of science. In a sense it would be another postscript to the <u>Logic</u>. It is not yet certain that this will be possible, but I would like to have your reactions in order to simplify my planning.

Give my regards to your wife and the Agassis.

Sincerely,

April 14th
1959

My dear Gordon,

I am sorry to have to let you wait so long for a reply to your letter, and an acknowledgement of your book. I have read quite a bit in it, and I find it extremely well written, and very interesting; but there are lots of points with which I disagree – mostly points which I treated at some length in my Open Society. Also, I have not finished the book – far from it: my eyesight is very bad, and I am still reading the proofs of my second volume ('Postscript'). I asked the publisher to send you a copy of the first, and I hope you have got it. I realize that this is a most unsatisfactory letter to receive, but I cannot do better just now: it was in the hope

of finding time to finish the book and
to write a better letter that I have let
you without reply so long.

With kind regards,

yours sincerely

K. R. Popper

April 21, 1959

Dr. Karl R. Popper
Fallowfield
Manor Road Penn
Buckinghamshire, England

Dear Karl:

I have received your book and took the opportunity to re-read it. From your letter I gather I will also have to re-read The Open Society. This will be the fourth time through, but its worth going over from time to time. I wish I knew what your objections were in more detail, but I understand your difficulties. I am having some difficulty finding a publisher, and maybe you will be able to give me some more specific comments before I actually do get it published. This gives me a further reason for hoping you get Postscript out shortly.

I am sorry to hear that your eyes are bothering you. Opthamology is a particularly backward field of medicine.

Cordially,

Gordon Tullock

GT/gb

Fallowfield, Manor Road,
PENN, Buckinghamshire,
England
March 6th 1967.

My dear Gordon,

What a marvellous surprize! Your book is really a charming, sane, and very excellent piece of work. I am happy that I am involved in it.

As to p VII, The Duke University is clearly solely responsible for calling my book on p. 48 *The Logic of Scientific Discovery* (right) but on p. 65 .... of *Scientific Inquiry* (wrong). What a Press! Incidentally, you praise Jerold J. Katz; but in my *L. Sc. D.* there is, so far as I know, ~~a~~ still more "strict" proof (in fact, several proofs). Incidentally, do you not know my *Conjectures and Refutations*? (Basic Books*)) Tell me if you have not got them, and I shall ask Basic Books to send you a copy. It is, I think, the most readable of my books.

But as to your chapters VI, and VIII. Do you know that I know a very good theoretical physicist

*) BB have now also published *The Poverty of Historicism* which, if I remember well, you read in proofs; or did you only read the French Edn?

who has published many papers in highly reputed Journals but cannot get the official quantum theorists to listen to him? He has developed a new non-linear relativistic field theory of particle interaction, and he has written a book on it, but cannot get the book published.

Do you ever come to England? I have been quite often in the U.S., but mainly in California; also in the Middle West and North-East.

Let me thank you again for the very enjoyable book.

Yours, as ever,

Karl (Popper)

13 March 1967

Sir Karl R. Popper
Fallowfield, Manor Road
Penn, Buckinghamshire
England

Dear Sir Karl:

Thanks for the encourgaing letter. I am in the painful process of waiting for my reviews, and this is an unusually trying business for The Organization of Inquiry. There aren't very many journals which would feel obligated to review it, and the book will make at least some reviewers unhappy. Thus I stand a not insignificant statistical chance of getting nothing but negative reviews. Your kind words are, therefore, particularly appreciated.

The footnote reference to Katz is the result of very hasty work. I had a great deal of trouble getting the book published, and when it was finally accepted the publisher demanded the addition of "20 to 40 pages" of footnotes. His argument was that it was controversial, and that a lot of footnotes might insure more respectful treatment. He was probably right about this and I was in no position to argue so I put in 123 footnotes. I was working to a close deadline, and had no time to give them any real thought. Hopefully, there are no more serious errors.

Your friend's difficulty in getting his book published isn't surprising. You gave me a thorough introduction to the gospel according to St. Bohr at Stanford and there doesn't seem to be any improvement. Did you see the review of Jammer's book in the Scientific American? It was criticized for using the term "Copenhagen Interpretation" and for merely mentioning the possibility of other interpretations. This quasi-religious approach is really quite astounding and we can only hope that such work as your friend's will eventually wear it down. C n he, by the way, deduce testable differences between his theory and the orthodoxy?

The situation in biology is even worse than in physics. Some of the American journals actually have announced editorial policies against publishing critical articles. I wrote a book on the social organization of ants, termites, etc., and after letting a couple of biologists read it decided it wasn't even worth sending to a publisher. Their approach was completely atheoretical and extraordinarily specialized. The termitologists would not discuss ants and the ant experts would not talk about termites.

Sir Karl R. Popper
13 March 1967
Page two

Still, granting all of this, it does seem to me that the situation in the social studies is worse. My Chapter VII was an effort to explain why. I do not think it was completely successful, as I do not think the book as a whole was completely successful. I started out intending to produce a formal model of the system of science, and found that I couldn't make it. Thus the book is a "second best" solution.

I hope to get to England in August for a few days, and will certainly let you know when I have more definite plans. My colleague and co-author James Buchanan will be in England most of the summer and I think you would both enjoy meeting. With your permission I will suggest that he get in touch with you. Meanwhile, if you are going to be in this part of the world, please let me know.

I have a copy of Conjectures and Refutations, but I thank you anyway for your offer. Both Buchanan and I enjoyed Of Clouds and Clocks.

Sincerely yours,

Gordon Tullock
Associate Professor

/bt

Fallowfield
Manor Road
Penn, Buckinghamshire
England

July 12th, 1967.

Dear Gordon,

Thank you for your note. Unfortunately I won't be in London between August 23rd and 27th: There is a Congress in Amsterdam where I have to perform. I am to arrive in Amsterdam on August 24th, and I shall stay there till September 2nd or perhaps even till September 5th.

Is there any chance that you can catch up with me in Amsterdam? This would be most welcome.

Yours sincerely,

Karl (Popper)

July 21, 1967

Sir Karl Popper
Fallowfield
Manor Road
Penn, Buckinghamshire
England

Dear Karl:

My plans are still a little uncertain, but I doubt that I'll be able to get to Amsterdam between the 23rd of August and even September 5th. There is a possibility, however, and I would appreciate knowing where I could get in touch with you if it does turn out to be possible. Later in September I'm going to the Mont Pelerin Society meeting. Since you were one of the charter members, why don't you come also.

Sincerely,

Gordon Tullock
Associate Professor

GT:ls

The London School of Economics and Political Science
(University of London)

Houghton Street, Aldwych
London, W.C.2
Telephone HOLBORN 7686

July 24th, 1967.

Dear Gordon,

Many thanks for your letter of July 21st.

I cannot possibly come to the meeting of the Mont Pelerin Society, there just is no time.* I hope you will enjoy yourself

Perhaps you can come to Amsterdam, after all.

Yours sincerely,

Karl

K. R. Popper

My address (August 24th to September 2nd or 5th) will be:

c/o 3rd International Congress for Logic, Methodology and Philosophy of Science
Congrescentrum Grandhotel Krasnapolsky, Dam,
Amsterdam
The Netherlands

* Also, the Society which in the beginning was interested in philosophy and history has more and more become almost exclusively interested in economics.

31 March 1970

Sir Karl R. Popper
Fallowfield, Manor Road
Penn, Buckinghamshire
England

Dear Karl:

I have been watching with amusement the reports of the "positivism" debate in Germany in The Times Literary Supplement. Do you happen to have an English copy of your twenty-seven theses which you could send me? The whole performance, your paper naturally exempted, is another example of the revolt against reason which seems to be such an important part of the current intellectual climate.

I hope to be in London in mid-August and would like to see you then if you also will be there. I will write and give exact dates later.

Sincerely yours,

Gordon Tullock

/bt

Fallowfield
Manor Rd., PENN, Buckinghamshire
England.

April 4th, 1970

My dear Gordon,

I was very glad to have your letter. Unfortunately, the 27 theses are not yet translated.

In all modesty I think that a little propaganda for my ideas and especially _The Open Society_ and _The Poverty_ may perhaps be some help to combat the present Revolt Against Reason.

You may be interested to hear that during the L.S.E. Revolution, not one single philosophy student (or staff member) was among the militants.

Since elsewhere philosophy was almost as bad as sociology, this can hardly be an accident.

I am looking forward to seeing you.

yours sincerely

Karl.

THE UNIVERSITY OF
ARIZONA
TUCSON ARIZONA

Department of Economics
College of Business &
Public Administration

Building #23
Tucson, Arizona 85721
(602) 621-6224
FAX (602) 621-8450

January 23, 1991

Sir Karl Popper
Department of Economics
University of London
London, ENGLAND

Dear Karl:

You may not think of yourself as a "German historian of science" but I have it on excellent authority - Scientific American February 1991, page 122 - that that's what you are. I have in fact written them about it.

The main point of this letter to you, however, is to enclose a rather long paper on methodology. This is very rough draft and inspired essentially by a general annoyance with some of the things that are going on in economics at the moment. To give a little bit of Freudian psychology (even that may be true in somecases) I suspect that the present turn to extremely abstract economics is simple escapism. Many of the conclusions drawn by economics about actual policy are very unpopular in the academic circles outside of economics. The young man who wants to get along well at faculty cocktail parties is better advised if he can say he's doing mathematical work in economics than if he says that the minimum wage act is hard on the poor. But this may be pure bias on my part. In any event, if you take the time to read this rather long paper, I'd appreciate any comments.

Sincerely,

Gordon Tullock
Karl Eller Professor of
Economics and Political Science

GT/mc
Enclosure

Department of Economics
College of Business &
Public Administration

THE UNIVERSITY OF
ARIZONA
TUCSON ARIZONA

Building #23
Tucson, Arizona 85721
(602) 621-6224
FAX (602) 621-8450

March 19, 1991

Sir Karl Popper, CH, FRS
136 Welcomes Road
Kenley, Surrey
CR8 5HH
ENGLAND

Dear Karl:

I'm unhappy to hear about the pneumonia but happy to hear about the antibiotics. Science does progress.

The point of this letter, however, is to warn you briefly that I am about to send you an attack on your work. At the Public Choice meeting in New Orleans one of the professors devoted a whole paper to an attack on your long-ago "Poverty of Historicism". It's been a long time since I've read that article but I think that his attack was not totally misplaced. A good deal has been discovered since then and all of us have to change our minds from time to time. Basically, however, the article still stands.

The reason I'm warning you instead of sending it on to you is that the paper wasn't finished but he will send me a copy when it is. Anyway, it proves the things you wrote that far in the past are now still on the agenda.

Sincerely,

Gordon Tullock
Karl Eller Professor of
Economics and Political Science

GT/mc

Sir Karl Popper, CH, FRS

136 Welcomes Road,
Kenley, Surrey
CR8 5HH

6-3-91

Gordon Tullock
Karl Eller Professor of Economics and
Political Science
The University of Arizona, Tucson, Arizona.

My dear Gordon,

I was so happy to get your letter of January 23, 1991: it reached me only today (because my Secretary was absent from the LSE). I have often thought about you, for many years; but I was always very deeply involved in urgent work — and I did not know your address.

I am nearing my 89th birthday, and I just got over three consecutive bouts of pneumonia, thanks to lots and lots of antibiotics. As a consequence my commitments have piled up. So I cannot promise to read your work — but I hope I shall get round to it. I am happy about U.S. policy, and I hope it will be pursued with vigour.

All the best and kindest regards.

Yours, as ever

Karl.

THE UNIVERSITY OF
ARIZONA
TUCSON ARIZONA

Department of Economics
College of Business &
Public Administration

Building #23
Tucson, Arizona 85721
(602) 621-6224
FAX (602) 621-8450

September 23, 1991

Sir Karl Popper, CH, FRS
136 Welcomes Road
Kenley, Surrey
CR8 5HH
ENGLAND

Dear Karl:

I see that you are on the program of the American Economic Association meeting in New Orleans. I don't know whether you are actually coming or just sending a paper, but if you are coming, I would appreciate an opportunity to introduce you to the New Orleans restaurants. Due to the fact that New Orleans is a favorite convention city in the United States, I feel that I am quite familiar with them and can do you well.

In addition to that, if you are coming to New Orleans you might be ameanble to going further west. I am sure I could arrange something for you here in Tucson with the Philosophy Department and a lot of people from other departments as well.

Lastly, I enclose a paper which may amuse you. This paper was not written to be published but in an effort to start discussion with the new mathematical types who are beginning to dominate the department here and many other places. So far this effort to start a discussion has been totally unsuccessful which rather confirms my view that mathematical economics in its present form is actually motivated by escapism.

Sincerely,

Gordon Tullock
Karl Eller Professor of
Economics and Political Science

GT/mc
Enclosure: Reflections on Mathematics

Department of Economics
Karl Eller Graduate School of Management
College of Business and Public Administration

THE UNIVERSITY OF
ARIZONA
TUCSON ARIZONA

McClelland Hall
Tucson, Arizona 85721
(602) 621-6224
FAX (602) 621-8450

October 22, 1992

Sir Karl R. Popper
Professor Emeritus
Economics and Political Science
136 Welcomes Road
Kenley
Surrey CR 25 HH - UNITED KINGDOM

Dear Karl:

Congratulations on your birthday and on the interview in Scientific American. You may not like everything that they said about you, but they certainly gave you a suitable amount of importance.

Although, I am, of course, primarily concerned with the social sciences I occasionally get involved in natural sciences. I enclose a recent paper of mine on biology. Assuming that I am right it's actually quite important since the debate between individual selection and group selection has never really been solved. In this case I think I do have some cases of group selection that can hardly be explained in any other way. This does not of course prove that individual selection is not dominant in most cases, as I believe it is. You might be interested to hear that I have recently carried purely biological work "The Hawk, Dove Equilibrium" over into an economic article.

I am even detouring into physics. It has occurred to me that the red shift might be explained by the slowing down of light over very, very, very long distances. I think this is very unlikely hypothesis but not certainly untrue, and if I can only convince some professional or amateur astronomer to run a rather simple test that I have devised we could find out whether it is correct. You don't like the big bang theory and neither do I. In one of my social science papers I used it as an example of modern myth.

This is enough of this letter. Once again I congratulate you and wish you another equally successful ninety.

Cordially yours,

Gordon Tullock
Karl Eller Professor of
Economics and Political Science

GT:vf

136 Welcomes Road,
Kenley, Surrey
CR8 5HH

Sir Karl Popper, CH, FRS

92

Professor Gordon Tullock,
Karl Eller Professor of Economics & Political Science,
Mc Clelland Hall, Tucson, Arizona 85721

My dear Gordon,

I was thrilled to get your letter. Thanks for your Congratulations. I have still not seen the Interview in Scientific American!

Your detouring into Physics is interesting: similar ideas were published some time ago by my old friend J-P. Vigier, one of Louis de Broglie's pupils and collaborators. I have proposed an energy loss owing to hitting particles: we know this reddens the raisin and setting sun:  a<b; an extremely simple explanation, and one that can explain that in some cases, parts of ONE system of gallaxies have not the same redshifts.

I too am interested in evolution theory, of course. (I do not know whether the Interview mentions this and my interest in the origin of life.)

I found your kind letter on my return from an extremely strenuous trip to Japan. So I have not yet been able to read your paper on individual versus group selection; but I hope to be able to read it soon.

Yours, as ever,
Karl.

Department of Economics
Karl Eller Graduate School of Management
College of Business and Public Administration

THE UNIVERSITY OF
ARIZONA
TUCSON ARIZONA

McClelland Hall
Tucson, Arizona 85721
(602) 621-6224
FAX (602) 621-8450

December 7, 1992

Sir Karl Popper
136 Welcomes Road,
Kenley, Surrey
CR8 5HH

Dear Karl:

I assume by now you have read the interview in Scientific American.

Actually on one occasion I wrote something in Physics that was published. Unfortunately what I wrote was pretty trivial and the journal that published it was perhaps the worst of the journals for such a thing. Nevertheless, I enclose it.

Your theory about slowing down of light is rather more complicated than mine. I was assuming that it just slowed down without any explanation. Nevertheless, the test that I have devised would work for yours as well as mine.

It must be occasionally be true that the outer planets, Jupiter for example, occlude some distant galaxies. If the light from the galaxies is travelling slower than the sunlight within the solar system then the oclusion of the galaxy would occur at a different point in the orbit of the planet than the apparent position. Thus, for example, if the planet went in front of the galaxy as seen from earth we would anticipate that the interruption of the light from the galaxy would reach us later than the light from the planet.

Observing this would be a very complicated problem which requires not only a telescope, but a computer analysis of celestial mechanics which is beyond me. Both the earth and Jupiter are in motion. and the whole solar system is also in motion. I think the test should be run even if as I rather suspect it simply will confirm the conventional wisdom. It is always sensible to test predictions of existing theory even when you are reasonably confident of the truth of the theory. So far I have not been able to sell it to any astronomer.

While we are on the subject of physics I have another calculation problem which I can't do myself. The solar system is moving rapidly and planetary orbits are not circles but ovals, which means that the planets must be at different distances from the sun at different times. Granted the fact that light radiates only at the speed of light this would mean that the apparent progress of the sun would not be quite stable. In other words we would see it as it was, shall we say, 100 seconds ago when we are close and 103 seconds ago when we are far away.

The reason I think this is important is it would permit a way of determining whether gravity is disseminated instantaneously or only at the speed of light. If it disseminates only at the speed of light, then the focus of the oval would also be in different relative positions depending upon how far the planet was from the sun at that particular time. The earth has a near circular oval orbit, but some of the outer planets don't and the effect would be much larger for them. As far as I know nobody has made any effort to calculate this.

I still follow you on the Copenhagen interpretation. Since the Bell inquality and its experimental test, we may be the only two people who do. Incidentally, I thought your comments on it were very helpful.

As evidence that you are really famous, I enclose a book add which accepts you as the orthodoxy against which the author argues.

I hope you enjoyed your visit in Japan. You were in Japan at the right time even if it was very strenuous. Japan is hot and muggy in the summer, delightful in the fall.

Cordially yours,

Gordon Tullock
Karl Eller Professor of
Economics and Political Science

GT:vf

Enclosures: Rhigodynamics

Kenley, 19-12-92
December 19

Professor Gordon Tullock,
Department of Economics
Mc Clelland Hall
Tuscon, Arizona 85721, USA.

My dear Gordon,

Thanks for your letter of December 7.

My theory assumes that the velocity of light (in empty space) is constant, c. According to my theory, light loses energy not by slowing down, but by (well known) interactions, which, ~~were~~ if space were totally empty, would not occur, and which (I assume) are extremely rare in the actual empty space. This theory is tested when the sun is reddened, "morn & night".

Your theory of "light getting tired" was first proposed (under this name, "light-getting-tired") and discussed, when Hubble drew attention to the red shift (1921? 1922?). It has been often proposed since – among other proposals there was a very elaborate one by my friend, Professor Vigier of the Univ. of Paris. It could in principle be tested much more easily than by the test you propose (which seems to me not possible – but I may have not grasped your point), simply as follows. There are two points on Earth, A and B, visible to each other, with well-known time interval T for light to travel A→B→A or B→A→B. We then put a big telescope up at A, direct it at night at a distant nebula and reflect it towards B (who have a telescope directed at A). A and B have (of course) synchronized their clocks. If a light signal, made with the light from the nebula received by A, takes longer than T/2, then your theory has been tested with a favourable outcome. (If it takes T/2 (like ordinary light), your theory is refuted.) This test would be possible; and there are other (local) ways to test (locally, at A, and with little expenditure – not more, say, than 500 dollars!) the velocity of incoming light, without all this trouble. I simply do not know whether these tests have been done; but I suppose they have – and with negative results; for the mention of the

136 Welcomes Road,
Kenley, Surrey
CR8 5HH

Sir Karl Popper, CH, FRS

Addendum to my letter to Gordon Tullock of 19 December 92.

I just see that I failed to comment on your problem starting with the last paragraph of your p. 1 (7. December 92). Since the movement of our sun is almost precisely an inertial movement (no noticeable deviation from the straight line over thousands of years (even though it follows something like a huge ellipse — with thousands of light years diameter) Newtonian relativity applies to the solar system; all the things you mention are allowed for as a matter of course in Newtonian theory and are dealt with most successfully by Newton's theory ~~of~~ (by his method of perturbation). There is no instability here.

K.

Department of Economics
Karl Eller Graduate School of Management
College of Business and Public Administration

THE UNIVERSITY OF
ARIZONA
TUCSON ARIZONA

McClelland Hall
Tucson, Arizona 85721
(602) 621-6224
FAX (602) 621-8450

January 11, 1992

Sir Karl Popper, CH, FRS
136 Welcomes Road
Kenley, Surrey
CR8 5HH

Dear Karl:

Thank you for your letter of December 19, 1992. You may be interested to know that the red shift was actually discovered here at Arizona. The combination of moderately high mountains and dessert air makes this a center for visual astronomy. For the last couple of years one of the major local preoccupations has been whether a proposed new telescope will or will not wipe out a sub-sub-sub species of red squirrel.

I didn't realize that this "tiring" of light had been considered when red shift was first proposed, but, of course, retrospectively I should have figured it out.

I am not at all convinced that any tests have been run on the subject. In the first place, physicist in general are not willing to talk to me about possible drastic chances in the basic view of the world. I have succeeded in starting conversations with several prominent physicists on the subject, and none of them have told me that any tests have been run. It's obvious that your proposed way of testing is better than mine, although it would require a good deal more elaborate equipment. Mine was intended to be something that an amateur astronomer could run because I had no hopes of getting a professional in.

Your remarks about the possibility of applying my rhigodynamics experiment here was something I had not thought of. I suppose I should expect it out of you, granted your proposal long ago for "perpetual motion" machine would actually be drawing power from the difference between two sub-areas of the environment.

I think I will leave the discussion of my gravity problem off to be discussed in a future letter, but for the time being let me enclose a paper of my which does at least leave some chance of getting me a Nobel Prize. When you read it you will realize that it is a very simple, almost simple-minded.

January 11, 1992
Sir Karl Popper
Page 2

It was turned down by three major economics journals and was eventually published in an obscure journal with a result that it had no impact for almost eight years after it was published. Interestingly during this period it was published in the supplementary reader for elementary students. I think you will agree, it would not have strained the students minds. It is a case of a very, very simple idea which nobody had thought up before. Since I am one of the people who hadn't thought of it, I can't blame the other people. At the moment it's all the rage under the name "rent-seeking" in not only economics, but political science, etc.

Cordially yours,

Gordon Tullock
Karl Eller Professor of
Economics and Political Science

GT:vf

Enclosure - Welfare Costs of Monopolies, Tariffs and Theft

Archives

Sir Karl Raimund Popper Papers, 1928-1995. Hoover Institution.

http://pdf.oac.cdlib.org/pdf/hoover/reg_189.pdf

William Baroody Papers. Library of Congress.

http://memory.loc.gov/service/mss/eadxmlmss/eadpdfmss/2008/ms008097.pdf

Gordon Tullock Papers, Hoover Institution

http://www.oac.cdlib.org/findaid/ark:/13030/kt787034zq/entire_text/

Published Work

Agassi, Joseph. 2013. *The Very Idea of Modern Science: Francis Bacon and Robert Boyle.* New York: Springer.

[Burgess, Jo Ann] 2006. "Cumulative index." In Gordon Tullock, *Economics without Frontiers,* in Volume 10 of *The Selected Works of Gordon Tullock (ed.)* Charles K. Rowley. Indianapolis: Liberty Fund, pp. 467-619.

Friedman, Milton and Rose Director Friedman. 1998. *Two Lucky People.* Chicago: University of Chicago Press.

Levy, David M. and Sandra J. Peart. 2012. "Tullock on Motivated Inquiry: Expert-Induced Uncertainty Disguised as Risk." *Public Choice* 153: 163-180.

Levy, David M. and Sandra J. Peart. 2014a. "'Almost Wholly Negative': The Ford Foundation's Appraisal of the Virginia School." http://papers.ssrn.com/sol3/papers.cfm?abstract_id=2485695

Popper, Karl. 1935. *Logik der Forschung.* Vienna: Springer.

Popper, Karl R. 1959. *Logic of Scientific Discovery.* New York: Basic Books.

Rosten, Leo C. 1970. "An Infuriating Man." In *People I have Loved, Known or Admired*

http://www.freetochoosemedia.org/broadcasts/freetochoose/detail_samples.php?page=article1&

type=1

Tullock, Gordon. 1966. *The Organization of Inquiry.* Durham: Duke University Press.

Tullock, Gordon. 1971. "An Application of Economics in Biology." In F. A. von Hayek, Henry Hazlitt, Leonard E. Read, Gustavo R. Velasco, F. A. Harper (eds.) *Toward Liberty: Essays in Honour of Ludwig von Mises on the occasion of his 90th birthday, September 29, 1971.* Menlo Park: Institute for Humane Studies 2:375–91.

Tullock, Gordon. [1966] 2005. *The Organization of Inquiry.* Volume 3 of *Selected Works of Gordon Tullock.* Edited by Charles K. Rowley. Indianapolis: Liberty Fund.