Gordon Tullock Corresponds with Karl Popper and Visits Flatland

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DRAFT

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I Introduction

Our recent essay on Gordon Tullock's 1966 *Organization of Inquiry* (Tullock [1966] 2005) made two points (Levy and Peart 2012). 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 because economics allows concealment in service to a cause. 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 the context of two thinkers who are important for Tullock's work.

2 The Tullock Popper Correspondence

From the long correspondence between Karl Popper and Gordon Tullock, we can add to what Tullock himself published.[1] We know from Jeremy Shearmur's reconstruction of Popper's lecture at Emory University (25 June–6 July 1956), at which Tullock was in attendance, that the Tullock–Popper connection is much earlier than Tullock's association with the economists of the Thomas Jefferson Center. Indeed, 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 that speaks of the origin of *Organization of Inquiry:*

> 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

[1] We are grateful to the Hoover Institution for access to the Karl Raimund Popper papers and the Gordon Tullock Papers in which all the correspondence is located.

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

> 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 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.[3]

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 his visit with Popper needs to be remarked. 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. Perhaps the key is the phrase "third system of Positional Logic" and perhaps there are aspects of what Tullock originally planned for *Organization of Inquiry* that we might recover.

We pointed out (Levy and Peart 2012) Tullock argues 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

[3] Tullock's confusion about the title of Popper's book persisted through the *printed Organization of Inquiry* as Popper points out in his letter of March 6, 1967. The Rowley edition, perhaps in homage, preserves all Tullock's errors that we spotted in the original edition.

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.[4]

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: [5]

> 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 Ludwig von Mises

Several of Tullock's colleagues had conversations with him in which he stressed the importance of the methodological aspects of von Mises's *Human Action* to his work.[6] Since the von Mises Tullock

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[4] "But as to your chapters vi, and viii. Do you know that I know a very good theoretical physicist 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." Popper to Tullock, March 6, 1967,

[5] 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. A copy of 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).

[6] We asked him (August 31, 2006) to explain his statement about von Mises's importance. "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 other 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

connection would disorient scholarship on both the Austrian and Virginia Schools, perhaps we ought not to rely on memory and conversation. Fortunately, we can control memory by manuscript. In Tullock's 1971 contribution to *Toward Liberty*, the multi-language 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).

4 "Flatland Revisited"

One of the two unpublished appendices to *Organization of Inquiry* – "Flatland Revisited" [7]– speaks to both Popperian themes and those laid out by von Mises so we turn to that now. (The full text is printed in the documentary appendix.)

An overview. "Flatland Revisited" is a seemingly simply addendum to Edwin Abbott's famous *Flatland* in which Tullock supposes that "Flatland" isn't really flat but that the minds of the inhabitants have evolved so that all their perceptions are filtered through the supposition that their world is flat. A crisis occurs when one of their scientists compares the implication of their axioms with that which can be measured. As the axioms hold with flat but the world isn't flat, there is, not surprisingly, a mismatch. The scientists struggle to find theoretical accounts that predict without ever challenging the flatness axioms. Tullock is optimistic that the theories will continue to improve. Admirers of Tullock's published work know that his simple presentations often cloaked very deep issues. To these we now turn.

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did not maintain that it also led to good results even though it did in economics. Alex Tabarrok tells us he had similar conservations.

[7] This is found in the Hoover Institution's Gordon Tullock Papers (Boxes 42, 108).

Von Mises. His lifelong defense of the claim that the theorems of praxeology are matters of apodictic certainty is what students of economic methodology find unique to von Mises's labors. "Praxeology" is simply the name given to the study of the connection between ends and means, so that in and of itself ought not to be a matter of controversy (Gasparski 1996). Von Mises restricts "economics" to katallactics, the Richard Whately coinage that carries the connotation of reciprocity (Whately [1831] 1832; von Mises 1949, p. 4; Levy and Peart 2010).[8] Apodictic is a transliteration of the Greek word for "demonstrated" so when von Mises uses the phrase "apodictic certainty" he is making a strong claim that there is no doubt about praxeological theorems because they are demonstrated from axioms that cannot be denied without falling to racial polylogism (von Mises 1949, p. 5; Peart and Levy 2005). To use traditional terms, for von Mises praxelogical theorems are necessary truths. It is fair to report that this claim separates von Mises and his disciples into a school at odds with the vast majority of the economics community. To give the only needed instance, it is the issue of apodictic certainty that Milton Friedman asserts is what ultimately separates him from von Mises (Friedman 1991).

Tullock asks in his "Flatland revisited" what follows from a necessary truth. We use notation that Tullock doesn't, simply to insure that this question is taken with sufficient seriousness. The traditional approach to modal logic takes necessary *(alternatively* possible, strict implication) as primitive and then defines by means of it the other terms. To mark that a proposition (sentence) α is necessarily true, we write $\square\, \alpha$. From antiquity through the 1940s it seems to have been taken for granted that $\square\; \alpha \rightarrow \alpha$.[9] What is necessarily true is true (actual). In retrospect the change came in when Kurt Gödel proposed to think about the necessary in terms of the demonstrated; thus using the assertion mark $\vdash$ for demonstrated;

[8] Tullock's life work might be seen as developing the nonkatallactic aspect of praxeology, the connection between means and ends unconstrained by reciprocity.

[9] The traditional view is discussed in Lemmon ([1966] 1979, pp. 1–11). All of the systems C. I. Lewis proposed allow this inference. Prior ([1955], 1962, p. 311) gives the axioms for the original Lewis systems and (pp. 312–13) for Lemmon's Gödelized axiomation. In Lewis's axiomization taking "strict implication" as primitive, the actual strictly implies the possible; the Gödelized version has the necessary implying the actual.

thus, $\vdash \alpha \rightarrow \Box \alpha$ (Gödel [1933] 1986). This, of course, ratifies the intuition we find in von Mises that what is necessary is that which is demonstrated. While Gödel's immediate purposes were very limited, it might have been one of the great moments in modern modal logic because his technical step began developments in which it was made it clear that $\vdash \alpha \rightarrow \Box \alpha$ and $\Box \alpha \rightarrow \alpha$ are independent issues.[10] In the years that followed it was made clear that there are systems in which the necessary only entails the possible, not the actual; thus: $\Box \alpha \rightarrow \Diamond \alpha$.[11]

This is where Tullock's Flatland comes in. Tullock imagines a world in what is necessarily true – a flatness axiom – is nonetheless false. This is clear to us, but not to the Flatlanders, because we can view their world and their minds from the outside. Apodictic certainty is only certainty about our deductions, not about the world. In Tullock's "Flatland" – which he is at pains to distinguish from Abbott's – the flatness axiom comes from something akin to von Mises monologism. There is only one logic in Tullock's Flatland because that's how everyone's mind evolved.

Popper. Karl Popper comes into the picture because of the concern over propositions that could not be falsified. Falsification is of course Popper's distinction between the scientific and the metaphysical ([1959] 1974, pp. 34–5.) Long before Popper's *Logic of Scientific Discovery,* Pierre Duhem made the case that there are no critical experiments in physics; one can always find (to use Popper's terminology) an "ad hoc" premise to blame the failure on (Popper [1959] 1974, p. 81). We save what is important to us; discard what isn't. Popper, when writing *Logic of Scientific Discovery,* was optimistic, at least in some

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[10] G. H. von Wright describes his contribution: " … the conception of modal logic as a superstructure, or 'second story', to be erected – like quantification theory – on the basis of the logic of propositions … (I later learnt that the idea was not entirely novel. It can be traced back to a short paper by Gödel from the early 1930s and to a paper by Feys from 1937.) Von Wright (1989, p. 29).

[11] Lemmon ([1966] 1979, p. 50) credits the weakening from $\Box \alpha \rightarrow \alpha$ to $\Box \alpha \rightarrow \Diamond \alpha$ to von Wright's deontic logic in which "necessary" is taken as "obligatory." In this context it is completely implausible to suppose that the actual follows from the obligatory (von Wright 1951, p. 41). In Robert Feys' comprehensive account, "System I" [Lewis SI] is constructed from a modal grammar developed in "System I^0" plus the theorem that the actual strictly implies the possible (Feys 1965, p. 64). Tullock's contribution might be seen as proposing a nonnormative interpretation as an alternative to von Wright's although as we suggest in our concluding sentence, Tullock's point might be von Wright's.

passages, that Duhem's claim could be avoided by his falsification approach.[12] By the time of the *Postscript,* that confidence is replaced by an almost holistic Quinean focus on context in which elimination of the reasons for the falsification is seen as a major undertaking. In the *Postscript* Popper introduces the term "metaphysical programmes for science" to describe the possibilities of theoretical systems with nonfalsifiable elements (Popper 1983, pp. 189–93). The Flatlander's flatness axiom is in Popper's terms metaphysical since it cannot be falsified.

Science, in Tullock's account of Flatland, functions much as Popper and other philosophers of science imagine. All claims are replicated; nothing is concealed. This is not how Tullock views economics in the world in which he lives! (Tullock 1966; Levy and Peart 2016). A crisis occurs in Flatland when a scientist of stature applies the flatness axiom to his rather nonflat world. The measurements do not match the implications. From the crisis follows an intense period of discussion in which many revisions are proposed. There is one result upon which all the revisions agree, one that allows the flatness axiom to be maintained. Tullock captures the Duhem-moment perfectly:

> Making careful measurements of various figures on the surface which is thought to be flat, and then trying to develop theories fitting these measurements is a major scientific activity. Probably the most important and certainly the only generally applicable of these theories is the theory which "proves" the existence of inherent limitations on the accuracy of measuring instruments. Needless to say, this is a great help in fitting other theories to the measured data.

Tullock describes a process by which scientific progress is real:

> As far as accuracy goes, some few of the Flatlanders' theories use equations which are exactly those we would use ourselves, although they have derived them differently. In a few more cases, they use equations which lead to the same results as ours but which are more complex. In most cases,

[12] Popper ([1959] 1974, p. 78): "Duhem denies (Engl. Transl. p. 188) the possibility of crucial experiments, because he thinks of them as verifications, while I assert the possibility of crucial *falsifying* experiments." In the *Postscript* Popper offers an holistic approach in which theoretic systems are tested as wholes (Popper 1983, p. 178). It isn't clear that there is difference between a later Popperian approach and that of W. V. O. Quine (Quine 1960).

> however, the theories developed by the Flatlander scientists are mere approximations of reality and many of them are not even close approximations.

Tullock reports that the Flatlanders are hard at work improving their approximations.

5 Questions

Instead of a conclusion we have questions. **i.** Why didn't Tullock publish this? **ii.** Did he discuss this with anyone? **iii.** Tullock regarded himself as a disciple of von Mises, inspired as he was by *Human Action*. Did any other disciple take Tullock's modal path? iv. If racial polylogism is a viable alternative to von Mises's monologism, isn't Tullock's path the right one? v. If monologism is normative have we not returned to von Wright's insight (von Wright 1951, p. 41).

Documentary Appendix

We print "Flatland Revisited" first and then the Tullock–Popper correspondence in chronological order.

0. "Flatland Revisited" an unpublished appendix to *Organization of Inquiry* [GT papers

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

[Gordon Tullock]

APPENDIX II

Flatland Revisited

Practically every mathematics student at one time or another has read FLATLAND,* Abbott's instructive tale of an inhabitant of

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*FLATLAND, A ROMANCE OF MANY DIMENSIONS, A. Square, (Edwin A, Abbott). The work has gone through numerous editions. I refreshed my memory with the Basil Blackwell Oxford edition of 1926 and all page citations are to this version.

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a two dimensional world and of how he had the existence of a third dimension proved to him by a being who removed from his two dimensional world, "Flatland," and showed him a three dimensional continuum. The book, as written, gives a false impression, particularly through its title. The land in which A. Square lived was not flat. If we were to view his two dimensional world from the outside, we would quickly recognize that it was as irregular in shape as the surface of any other world. The failure of Mr. Square to notice this fact during the period when he was outside the two dimensional world may be put down partially to the limitations on his opportunities for observation and partly to the hereditary constitution of the mind of an inhabitant of this universe which might better be called "Bentland."

Mr. Square was only outside his two dimensional world for a short time, and his state of emotional and intellectual shock during that period was such as to make it unlikely that he would make any very careful observations of the environment in which he found himself. Further, he seems mostly to have been interested in observing the inhabitants and structures of his native land rather than the physical structure of the land itself. In addition, when he first left his two dimensional world, he was quite incapable of appreciating the nature of any surface other than a flat one. It was only after his guide, Mr. Sphere, had carefully explained this idea to him with the help of a cube that he began to perceive the possibility of non-flat surfaces. In the short and exciting period remaining he can be excused for not noticing the irregular nature of his native world.

The question remains of why his instructor, the sphere, did not acquaint him with this feature of his world. As a being fully conversant with the three dimensional world within which the two dimensional "Flatland" lay, he can hardly have been unaware of its irregular nature. Indeed, he refers to "the plains of Flatland"* and plains are not

*Page 79.

absolutely level areas, but gently rolling nearly flat areas. Further, "plains" naturally is put in opposition to other terms like mountains, canyons, and hills, and Mr. Sphere, therefore, must be taken to have known that, while the bulk of the inhabitants of Flatland lived in a relatively level area, there were numerous pronounced irregularities in their two dimensional world particularly in its less settled parts.

Shortage of time, as we have said, may have led the sphere to avoid this subject, but it may also have seemed useless to him in view of his great knowledge of the inhabitants of "Flatland." For it is a fact that the minds of these dwellers is so constituted that they cannot conceive of their land as anything except flat. It is possible that the sphere might have succeeded in convincing Mr. Square that deviation from flatness was theoretically possible, but he could never have given him a real appreciation of what a two dimensional continuum which was irregular rather than flat when viewed from a three dimensional space was like. This peculiarity of the minds of Flatlanders has occasioned much interest among the inhabitants of "Spaceland" and the savants of the area have devoted much time to speculating on its origin. To an account of the results of this discussion, I shall shortly turn. After briefly indicating the principle points of view expressed in this debate, I shall then describe the effect of the concurrence of irregularities and minds inherently unable to think of such things on science in "Flatland." Finally, I shall explain what may not be obvious to some of my readers, what all of this has to do with us.

Among the scholars of spaceland there are quite a number of views on how the "Flatlanders" came to have minds which are incapable of thinking of their world as anything but flat. One thread unites all of these theories, however; all the savants are agreed that the Flatlanders evolved from lower forms and that the present constitution of their minds must be the product of that evolution. The exact evolutionary process is the only matter which divides them although there are sufficient grounds for division within this sphere to permit the development of a large number of warring schools of thought.

The first and, in some ways, most influential of these schools of thought holds that evolution necessarily proceeds from the simple to the complex. One-celled species necessarily preceded multi-celled and the Amphibia preceed the lizards. It seems likely, therefore, that in the course of evolution the first brain which could really think would be the simplest type. Clearly, it is easier and simpler to think in terms of a flat two dimensional surface than in terms of an irregular one. It is, therefore, easy to see why the Flatlanders all have such simplified brains. Whether, in time, further evolution will lead to further development is, of course, a mere matter of opinion.*

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*See "Explanation and Prediction in Evolutionary Theory" by H. Scriven, SCIENCE, August 28, 1959, p. 477.

A second school of thought, in part allied with the first, holds simply that a brain which could think in terms of a wavy two dimensional continuum would have had little evolutionary value at the time the race originally was formed. It is an undoubted historical fact that the race of Flatlanders first developed in the relatively level part of their world, and in this area an appreciation of the minor irregularities in the landscape would have been of little help to primitive tribesmen trying to catch wild animals while at the same time avoiding being caught themselves. While such a set of mental equipment would have had little or no positive evolutionary value, this school points out that it would most certainly have had a negative value. In the first place, the mind which was capable of considering that its two dimensional world varied in an almost inconceivable third dimension would necessarily be larger than one which could not, and this would be an additional weight for the organism to carry around. Further, most genes have multiple effects. The genes which gave the mind this power, then would probably have other effects on the organism, and, if these were negative, even if only mildly so, the whole effect would be to secure the elimination of individuals with such equipment from the race in its earliest stages of evolutionary development.

Once the race had developed with this type of mind, any mutation to another type with an ability to think in other terms than a completely flat universe would have been of negative evolutionary value due to the fact that the non-mutated members of the race would undoubtedly consider the mutant insane. Further, the advantage which such a mutation would give would be very slight to non-existent since only a very small part of the race would, at any given time, be doing things which required the new type of mind. The mutant, being different from his fellows in precisely such a field would probably find that, in those areas where he had a superiority, he would be distrusted by his colleagues, and, consequently, would not be permitted to work, or if he did, his results would not be accepted. Altogether, the "civilized" environment is most unfavorable to the survival of genetic mutations radically different from the prevailing type of mentality, and once a race of one basic mind type has become established, it is unlikely to be replaced by another.

The two remaining schools of thought are less influential than the two we have discussed so far. One holds that there are quite a number of mind types possible for such a race as the Flatlanders, and that it is largely a question of chance and the detailed historical development of the evolutionary process which determines which one any race will have. Once a mind of any type is achieved, however, it immediately gives the species holding it a major competitive advantage over the other, less intelligent, species. This species is then likely to establish its dominance over its environment and, for reasons similar to those given by our previous group of scholars, it forms an unfavorable environment for any mutation which might lead to a different way of thinking.

The last group of savants, in radical opposition to all of the others, holds that the limitation on the Flatlanders' minds which makes it impossible for them to think of their world as other than flat arises essentially from chemical rather than biological factors. They point out that a brain is essentially a carefully arranged collection of chemicals, and they point out that only some chemicals can exist in Flatland, those which have molecules in which the atoms are arranged in three dimensional lattices being, ex definitions, ruled out. This means that there are natural limits on the types of mind which can be constructed, and these savants hold that these limits happen to forbid the construction of a mind which can think of its environment in other than flat terms,

Clearly, our present knowledge of the nature of biological organisms is not great enough to permit us to determine which of these schools of thought is correct. Perhaps none of them are or perhaps the truth involves some sort of compromise between them. Nevertheless it would seem clear that the development of such a limited mind as the Flatlanders have would be evolutionarily possible. Certainly, the Flatlanders have these limits built into their minds, and never succeed in thinking of their world as anything but flat.

The effect of this limitation on the minds of the Flatlanders has been most peculiar. In the early days of their civilization, it had almost no influence. They learned to make various things and used simple geometric forms in their construction, but surveying did not develop as a science due to the fact, of course, that forms of any size would have widely varying characteristics, depending on where it happened to be located. Eventually, formal geometry was invented (although it was not called "earth measuring") and carried to quite a high level of development. This development, however, eventually led to a crisis which destroyed the simple symmetry of the geometric view of nature. A leading geometrician decided to apply his learning on a large field and attempted to determine the distance between two points by triangulation. The irregularity of the surface at this point was such that his computed results were greatly different from directly measured distance. The experiment was repeated by a number of other scholars at other points and the uniformly disappointing results may be said to have constituted the most important revolution in scientific thought in the entire history of Flatland. The eventual outcome was the conclusion by most scientists that simple geometry was only an approximation of reality. Although normally a close approximation for small figures, even that was not exact and for larger figures it was almost useless.

The result of this revolution in science was the development as the largest, most important, and most difficult area of scientific investigation of the field of surveying. Mr. Square does not mention this in his brief summary of the characteristics of his land for much the same reasons which would lead an average inhabitant of our country to omit the Einstein theory from a brief account of its nature. Among the scientists, however, the various problems of surveying are a continuous preoccupation. Making careful measurements of various figures on the surface which is thought to be flat, and then trying to develop theories fitting these measurements is a major scientific activity. Probably the most important and certainly the only generally applicable of these theories is the theory which "proves" the existence of inherent limitations on the accuracy of measuring instruments. Needless to say, this is a great help in fitting other theories to the measured data.

All the other theories are regional in nature. That is the theory [which] will attempt to explain the variations in some particular locality. As of today, there are such theories for only a small part of the total area of the country, but the scientists of Flatland are most optimistic about the possibilities of further development. They point out that the history of surveying has been one of steadily accelerating progress. In the last fifty years, in particular, many new areas have been "explained," and many older, rather inaccurate, theories explaining areas have been replaced by new and better explanations, They look forward to an accelerating process of expansion of the area covered by their theories and hope eventually to find a "general surveying theory" which will provide a single equation which covers the whole country. To the outside observer, the problem appears more difficult. Since he knows that the present theories are, in fact, all wrong, he may be dubious about the possibility of extending them to the whole area. On the other hand, the scientists of Flatland have so far shown undoubted ingenuity in applying their incorrect theories to reality and the possibility that they will eventually solve their problems cannot be disregarded. If they do find their "general surveying theory," it will be an interesting example of a theory which is completely incorrect, yet which explains all of the observed data in terms of its own, improper, assumptions.

The presently existing local theories may be divided among three basic categories. In the first place, there are a few in which the theory simply consists of an equation with no explanation of why it should work. Those theories which are explanatory, and they make up the vast bulk of the total, normally depend either on an assumption that measures of length vary from place to place or that straight lines are actually bent is various ways.* Some combine elements of

*Bent within the plane in which the Flatlanders imagine themselves living, of course. Many of the lines are bent, as we third dimension dwellers can see, but they are bent quite differently than the Flatlanders believe.

both these explanations or, in some cases, also combine unexplained elements with one or the other of these basic explanations. As far as accuracy goes, some few of the Flatlanders' theories use equations which are exactly those we would use ourselves, although they have derived them differently. In a few more cases, they use equations which lead to the same results as ours but which are more complex. In most cases, however, the theories developed by the Flatlander scientists are mere approximations of reality and many of them are not even close approximations.

But, what the reader may ask, has all of this to do with us? I am coming to that and as an introduction may I ask that you consider the possibility that some Flatlander might begin to doubt the flatness of his universe. While he could doubt its flatness, he could not, given his mental constitution, think at all in non-flat terms. He could only feel that possibly the universe was non-flat, but he would have no idea what that meant in positive terms. In support of this view that the world was non-flat, he could offer only two, rather feeble arguments. Firstly, it would seem unlikely that the type of brain which would evolve under primitive conditions would be particularly suited to scientific efforts to penetrate the real nature of the universe. Secondly, he could point out that most scientific theories, efforts to explain the universe in terms of this built-in flatness axiom, were mere approximations of the data obtained by measurement and that vast areas were completely unexplained.

Weak as these arguments are, those on the other side are even weaker. There is first the argument from hope—someday our theories may fit the measurements exactly. Secondly, there is the argument of non-comprehension. A great many of the scholars of Flatland could be depended upon to simply point out that the results of reasoning based on the flatness axiom which was part of their biological brains seemed perfectly logical and that no other line of reasoning was so logical. This would, of course, he quite true, but also beside the point. The contention would be quite simply that the minds of the Flatlanders were so constructed that what seemed logical to them was nevertheless not in exact accord with the reality of nature. The fact that Flatlander logical reasoning appeared logical to Flatlanders would be irrelevant.

Obviously, with such weak arguments on either side, it would be impossible for the Flatlanders to determine who was right; the problem would have to remain an open question. Possibly after a few hundred thousands of years, some conclusion might be drawn by considering whether the whole of Flatland were covered by a coherent explanation, but surely nothing can be decided now.

Nevertheless, even a Flatlander who became convinced that the world was, in fact, non-flat would have to continue investigations using the flatness axiom. As we have pointed out, their minds are so constituted that they can think in no other terms. It would be a question of thinking in terms of this axiom or not thinking at all, and as long as any progress at all was possible with the use of the false axiom, it should be used. Our Flatlander would be in much the same situation as a modern Indian peasant. He knows that it would be much easier to break ground with a tractor and plow than with a hoe, but he doesn't have the tractor and plow so he makes do with what he has.

The application of all of this to ourselves is, I suppose, obvious by now. We are biologically equipped with brains of a certain pattern. These brains permit us to think in certain ways, which are as such part of the biological equipment of the species as are arms and legs. Clearly, this thinking ability has positive evolutionary value and has given the human species a major competitive advantage over other species, but this does not prove that human logic and the real interrelations of things in this world are in a one-to-one relationship. Nevertheless, we have no choice but to continue thinking in our natural way. It may or may not be the best key to the universe, but it is the only one we have.

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 ~~fairlyzatagxnkzgood~~ much improved formulation for that part of my theory to which you objected ~~to~~ so strenuously. The new formulation doesn't directly derive from anything you said, but the fact that ~~I xxx 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.

~~Thexrsatzreasonxforxthisxletter,~~

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 ~~havextoxspendxthexearly~~ 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.

Fallowfield,
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 adaquate 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)

RERUM COGNOSCERE CAUSAS

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 beliaf in science as a process of discussion by "discussing" with you. I don't see how you can complain without tearing up The Logic of Scientific Inquiry.

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 <u>Logic of Scientific Discovery</u> is now being prepared in page proofs, and meanwhile he is reading the galley proofs of the second, the <u>Postscript: After Twenty Years</u>, a sequel to <u>Logic</u> 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 tried 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 (for me) arrangement 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 & Th~~ 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 came 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

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two <u>given</u> reservoirs, your machine is explained as well, or rather as unsatis-factorily, 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 in unlimited quantities (by friction). The obvious answer to this is that in boring cannons we transfer caloric or heat matter. This is, for

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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.

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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 heat processes (both exo- and endo-thermic). 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 Logic. 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 is right, and, secondly, it may be too trivial to bother with. The positional logic of Inside Bureaucracy is much less elaborate than that ofeconomics, and my latest theory is even less so. At any event, I would like to get the Logic 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 earth the only practical heat gradients result from contrasting a heat source with the general environment. 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

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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

Gordon

PS, With regard to your seminar on confirmation of Economic theory, Are you familiar with Oskar Morgensterns 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,

G

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 something 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.

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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

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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.

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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

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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 <u>Scientific American</u>!

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.

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

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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 <u>constant</u>, c. According to my theory, light loses energy <u>not</u> 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 (by his method of perturbation). There is no instability here.

K.

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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.

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

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