

# The Republic of Science: Its Political and Economic Theory

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My title is intended to suggest that the community of scientists is organized in a way which resembles certain features of a body politic and works according to economic principles similar to those by which the production of material goods is regulated. Much of what I will have to say will be common knowledge among scientists, but I believe that it will recast the subject from a novel point of view which can both profit from and have a lesson for political and economic theory. For in the free cooperation of independent scientists we shall find a highly simplified model of a free society, which presents in isolation certain basic features of it that are more difficult to identify within the comprehensive functions of a national body.

The first thing to make clear is that scientists, freely making their own choice of problems and pursuing them in the light of their own personal judgment, are in fact co-operating as members of a closely knit organization. The point can be settled by considering the opposite case where individuals are engaged in a joint task without being in any way co-ordinated. A group of women shelling peas work at the same task, but their individual efforts are not co-ordinated. The same is true of a team of chess players. This is shown by the fact that the total amount of peas shelled and the total number of games won will not be affected if the members of the group are isolated from each other. Consider by contrast the effect which a complete isolation of scientists would have on the progress of science. Each scientist would go on for a while developing problems derived from the information initially available to all. But these problems would soon be exhausted, and in the absence of further information about the results achieved by others, new problems of any value would cease to arise, and scientific progress would come to a standstill.

This shows that the activities of scientists are in fact coordinated, and it also reveals the principle of their co-ordination. This consists in the adjustment of the efforts of each to the hitherto achieved results of the others. We may call this a coordination by mutual adjustment of independent initiatives--of initiatives which are co-ordinated because each takes into account all the other initiatives operating within the same system.

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When put in these abstract terms the principle of spontaneous coordination of independent initiatives may sound obscure. So let me illustrate it by a simple example. Imagine that we are given the pieces of a very large jigsaw puzzle, and suppose that for some reason it is important that our giant puzzle be put together in the shortest possible time. We would naturally try to speed this up by engaging a number of helpers; the question is in what manner these could be best employed. Suppose we share out the pieces of the jigsaw puzzle equally among the helpers and let each of them work on his lot separately. It is easy to see that this method, which would be quite appropriate to a number of women shelling peas, would be totally ineffectual in this case, since few of the pieces allocated to one particular assistant would be found to fit together. We could do a little better by providing duplicates of all the pieces to each helper separately, and eventually somehow bring together their several results. But even by this method the team would not much surpass the performance of a single individual at his best. The only way the assistants can effectively co-operate, and surpass by far what any single one of them could do, is to let them work on putting the puzzle together in sight of the others so that every time a piece of it is fitted in by one helper, all the others will immediately watch out for the next step that becomes possible in consequence. Under this system, each helper will act on his own initiative, by responding to the latest achievements the others, and the completion of their joint task will be great accelerated. We have here in a nutshell the way in which a series of independent initiatives are organized to a joint achievement by mutually adjusting themselves at every successive stage to the situation created by all the others who are acting likewise.

Such self-co-ordination of independent initiatives leads to a joint result which is unpremeditated by any of those who bring it about. Their co-ordination is guided as by 'an invisible hand' towards the joint discovery of a hidden system of things. Since its end-result is unknown, this kind of co-operation can only advance stepwise, and the total performance will be the best possible if each consecutive step is decided upon by the person most competent to do so. We may imagine this condition to be fulfilled for the fitting together of a jigsaw puzzle if each helper watches out for any new opportunities arising along a particular section of the hitherto completed patch of the puzzle, and also keeps an eye on a particular lot of pieces, so as to fit them in wherever a chance presents itself. The effectiveness of a group of helpers will then exceed that of any isolated member, to the extent to which some member of the group will always discover a new chance for adding a piece to the puzzle more quickly than any one isolated person could have done by himself.

Any attempt to organize the group of helpers under a single authority would eliminate their independent initiatives and thus reduce their joint effectiveness to that of the single person directing them from the centre. It would, in effect, paralyse their cooperation.

Essentially the same is true for the advancement of science by independent initiatives adjusting themselves consecutively to the results achieved by all the others. So long as each scientist keeps making the best contribution of which he is capable, and on which no one could improve (except by abandoning the problem of his own choice and thus causing an overall loss to the advancement of science), we may affirm that the pursuit of science by independent self-co-ordinated initiatives assures the most efficient possible organization of scientific progress. And we may add, again, that any authority which would undertake to direct the work of the scientist centrally would bring the progress of science virtually to a standstill.

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What I have said here about the highest possible co-ordination of individual scientific efforts by a process of self-co-ordination may recall the self-co-ordination achieved by producers and consumers operating in a market. It was, indeed, with this in mind that I spoke of 'the invisible hand' guiding the co-ordination of independent initiatives to a maximum advancement of science, just as Adam Smith invoked 'the invisible hand' to describe the achievement of greatest joint material satisfaction when independent producers and consumers are guided by the prices of goods in a market. I am suggesting, in fact, that the co-ordinating functions of the market are but a special case of co-ordination by mutual adjustment. In the case of science, adjustment takes place by taking note of the published results of other scientists; while in the case of the market, mutual adjustment is mediated by a system of prices broadcasting current exchange relations, which make supply meet demand.

But the system of prices ruling the market not only transmits information in the light of which economic agents can mutually adjust their actions, it also provides them with an incentive to exercise economy in terms of money. We shall see that, by contrast, the scientist responding directly to the intellectual situation created by the published results of other scientists is motivated by current professional standards.

Yet in a wider sense of the term, the decisions of a scientist choosing a problem and pursuing it to the exclusion of other possible avenues of inquiry may be said to have an economic character. For his decisions are designed to produce the highest possible result by the use of a limited stock of intellectual and material resources. The scientist fulfils this purpose by choosing a problem that is neither too hard nor too easy for him. For to apply himself to a problem that does not tax his faculties to the full is to waste some of his faculties; while to attack a problem that is too hard for him would waste his faculties altogether. The psychologist K. Lewin has observed that one's person never becomes fully involved either in a problem that is much too hard, nor in one that is much too easy. The line the scientist must choose turns out, therefore, to be that of greatest ego-involvement; it is the line of greatest excitement, sustaining the most intense attention and effort of thought. The choice will be conditioned to some extent by the resources available to the scientist in terms of materials and assistants, but he will be ill-advised to choose his problem with a view to guaranteeing that none of these resource be wasted. He should not hesitate to incur such a loss, if it leads him to deeper and more important problems.

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This is where professional standards enter into the scientist's motivation. He assesses the depth of a problem and the importance of its prospective solution primarily by the standards of scientific merit accepted by the scientific community--though his own work may demand these standards to be modified. Scientific merit depends on a number of criteria which I shall enumerate here under three headings. These criteria are not altogether independent of each other, but I cannot analyse here their mutual relationship.

(1) The first criterion that a contribution to science must fulfil in order to be accepted is a sufficient degree of plausibility. Scientific publications are continuously beset by cranks, frauds and bunglers whose contributions must be rejected if journals are not to be swamped by them. This censorship will not only eliminate obvious absurdities but must often refuse publication merely because the conclusions of a paper appear to be unsound in the light of current scientific knowledge. It is indeed difficult even to start an experimental inquiry if its problem is considered scientifically unsound. Few laboratories would accept today a student of extrasensory perception, and even a project for testing once more the hereditary transmission of acquired characters would be severely discouraged from the start. Besides, even when all these obstacles have been overcome, and a paper has come out signed by an author of high distinction in science, it may be totally disregarded, simply for the reason that its results conflict sharply with the current scientific opinion about the nature of things.

I shall illustrate this by an example which I have used elsewhere.[1] A series of simple experiments were published in June 1947 in the *Proceedings of the Royal Society* by Lord Rayleigh--a distinguished Fellow of the Society--purporting to show that hydrogen atoms striking a metal wire transmit to it energies up to a hundred electron volts. This, if true, would have been far more revolutionary than the discovery of atomic fission by Otto Hahn. Yet, when I asked physicists what they thought about it, they only shrugged their shoulders. They could not find fault with the experiment yet not one believed in its results, nor thought it worth while to repeat it. They just ignored it. A possible explanation of Lord Rayleigh's experiments is given in my *Personal Knowledge*. [2] It appears that the physicists missed nothing by disregarding these findings.

(2) The second criterion by which the merit of a contribution is assessed may be described as its scientific value, a value that is composed of the following three coefficients: (a) its accuracy (b) its systematic importance, (c) the intrinsic interest of its subject-matter. You can see these three gradings entering jointly into the value of a paper in physics compared with one in biology. The inanimate things studied by physics are much less interesting than the living beings which are the subject of biology. But physics makes up by its great accuracy and wide theoretical scope for the dullness of its subject, while biology compensates for its lack of accuracy and theoretical beauty by its exciting matter.

(3) A contribution of sufficient plausibility and of a given scientific value may yet vary in respect of its originality; this is the third criterion of scientific merit. The originality of technical inventions is assessed, for the purpose of claiming a patent, in terms of the degree of surprise which the invention would cause among those familiar with the art. Similarly, the originality of a discovery is assessed by the degree of surprise which its communication should arouse among scientists. The unexpectedness of a discovery will overlap with its systematic importance, yet the surprise caused by a discovery, which causes us to admire its daring and ingenuity, is something different from this. It pertains to the act of producing the discovery. There are discoveries of the highest daring and ingenuity, as for example the discovery of Neptune, which have no great systematic importance.

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Both the criteria of plausibility and of scientific value tend to enforce conformity, while the value attached to originality encourages dissent. This internal tension is essential in guiding and motivating scientific work. The professional standards of science must impose a framework of discipline and at the same time encourage rebellion against it. They must demand that, in order to be taken seriously, an investigation should largely conform to the currently predominant beliefs about the nature of things, while allowing that in order to be original it may to some extent go against these. Thus, the authority of scientific opinion enforces the teachings of science in general, for the very purpose of fostering their subversion in particular points.

This dual function of professional standards in science is but the logical outcome of the belief that scientific truth is an aspect of reality and that the orthodoxy of science is taught as a guide that should enable the novice eventually to make his own contacts with this reality. The authority of scientific standards is thus exercised for the very purpose of providing those guided by it with independent grounds for opposing it. The capacity to renew itself by evoking and assimilating opposition to itself appears to be logically inherent in the sources of the authority wielded by scientific orthodoxy.

But who is it, exactly, who exercises the authority of this orthodoxy? I have mentioned scientific opinion as its agent. But this raises a serious problem. No single scientist has a sound understanding of more than a tiny fraction of the total domain of science. How can an aggregate of such specialists possibly form a joint opinion? How can they possibly exercise jointly the delicate function of imposing a current scientific view about the nature of things, and the current scientific valuation of proposed contributions, even while encouraging an originality which would modify this orthodoxy? In seeking the answer to this question we shall discover yet another organizational principle that is essential for the control of a multitude of independent scientific initiatives. This principle is based on the fact that, while scientists can admittedly exercise competent judgment only over a small part of science, they can usually judge an area adjoining their own special studies that is broad enough to include some fields on which other scientists have specialized. We thus have a considerable degree of overlapping between the areas over which a scientist can exercise a sound critical judgment. And, of course, each scientist who is a member of a group of overlapping competences will also be a member of other groups of the same kind, so that the whole of science will be covered by chains and networks of overlapping neighbourhoods. Each link in these chains and networks will establish agreement between the valuations made by scientists overlooking the same overlapping fields, and so, from one overlapping neighbourhood to the other, agreement will be established on the valuation of scientific merit throughout all the domains of science. Indeed, through these overlapping neighbourhoods uniform standards of scientific merit will prevail over the entire range of science, all the way from astronomy to medicine. This network is the seat of scientific opinion. Scientific

opinion is an opinion not held by any single human mind, but one which, split into thousands of fragments, is held by a multitude of individuals, each of whom endorses the others' opinion at second hand, by relying on the consensual chains which link him to all the others through a sequence of overlapping neighbourhoods.

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Admittedly, scientific authority is not distributed evenly throughout the body of scientists; some distinguished members of the profession predominate over others of a more junior standing. But the authority of scientific opinion remains essentially mutual; it is established *between* scientists, not above them. Scientists exercise their authority over each other. Admittedly, the body of scientists, as a whole, does uphold the authority of science over the lay public. It controls thereby also the process by which young men are trained to become members of the scientific profession. But once the novice has reached the grade of an independent scientist, there is no longer any superior above him. His submission to scientific opinion is entailed now in his joining a chain of mutual appreciations, within which he is called upon to bear his equal share of responsibility for the authority to which he submits.

Let me make it clear, even without going into detail, how great and varied are the powers exercised by this authority. Appointments to positions in universities and elsewhere, which offer opportunity for independent research, are filled in accordance with the appreciation of candidates by scientific opinion. Referees reporting on papers submitted to journals are charged with keeping out contributions which current scientific opinion condemns as unsound, and scientific opinion is in control, once more, over the issue of textbooks, as it can make or mar their influence through reviews in scientific journals. Representatives of scientific opinion will pounce upon newspaper articles or other popular literature which would venture to spread views contrary to scientific opinion. The teaching of science in schools is controlled likewise. And, indeed, the whole outlook of man on the universe is conditioned by an implicit recognition of the authority of scientific opinion.

I have mentioned earlier that the uniformity of scientific standards throughout science makes possible the comparison between the value of discoveries in fields as different as astronomy and medicine. This possibility is of great value for the rational distribution of efforts and material resources throughout the various branches of science. If the minimum merit by which a contribution would be qualified for acceptance by journals were much lower in one branch of science than in another, this would clearly cause too much effort to be spent on the former branch as compared with the latter. Such is in fact the principle which underlies the rational distribution of grants for the pursuit of research. Subsidies should be curtailed in areas where their yields in terms of scientific merit tend to be low, and should be channelled instead to the growing points of science, where increased financial means may be expected to produce a work of higher scientific value. It does not matter for this purpose whether the money comes from a public authority or from private sources, nor whether it is disbursed by a few sources or a large number of benefactors. So long as each allocation follows the guidance of scientific opinion, by giving preference to the most promising scientists and subjects, the distribution of grants will automatically yield the maximum advantage for the advancement of science as a whole. It will do so, at any rate, to the extent to which scientific opinion offers the best possible appreciation of scientific merit and of the prospects for the further development of scientific talent.

For scientific opinion may, of course, sometimes be mistaken, and as a result unorthodox work of high originality and merit may be discouraged or altogether suppressed for a time. But these risks have to be taken. Only the discipline imposed by an effective scientific opinion can prevent the adulteration of science by cranks and dabblers. In parts of the world where no sound and authoritative scientific opinion is established, research stagnates for lack of stimulus, while unsound reputations grow up based on commonplace achievements or mere empty boasts. Politics and business play havoc with appointments and the granting of subsidies for research; journals are made unreadable by including much trash.

Moreover, only a strong and united scientific opinion imposing the intrinsic value of scientific progress on society at large can elicit the support of scientific inquiry by the general public. Only by securing popular respect for its own authority can scientific opinion safeguard the complete independence of mature scientists and the unhindered publicity of their results, which jointly assure the spontaneous co-ordination of scientific efforts throughout the world. These are the principles of organization under which the unprecedented advancement of science has been achieved in the twentieth century. Though it is easy to find flaws in their operation, they yet remain the only principles by which this vast domain of collective creativity can be effectively promoted and co-ordinated.

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During the last twenty to thirty years, there have been many suggestions and pressures towards guiding the progress of scientific inquiry in the direction of public welfare. I shall speak mainly of those I have witnessed in England. In

August 1938, the British Association for the Advancement of Science founded a new division for the social and international relations of science, which was largely motivated by the desire to offer deliberate social guidance to the progress of science. This programme was given more extreme expression by the Association of Scientific Workers in Britain. In January 1943, the Association filled a large hall in London with a meeting attended by many of the most distinguished scientists of the country, and it decided--in the words officially summing up the conference--that research would no longer be conducted for itself as an end in itself. Reports from Soviet Russia describing the successful conduct of scientific research according to plans laid down by the Academy of Science with a view to supporting the economic Five-Year Plans encouraged this resolution.

I appreciate the generous sentiments which actuate the aspiration of guiding the progress of science into socially beneficent channels, but I hold its aim to be impossible and indeed nonsensical.

An example will show what I mean by this impossibility. In January 1945, Lord Russell and I were together on the BBC Brains Trust. We were asked about the possible technical uses of Einstein's theory of relativity, and neither of us could think of any. This was forty-years after the publication of the theory and fifty years after the inception by Einstein of the work which led to its discovery. It was fifty-eight years after the MichelsonMorley experiment. But, actually, the technical application of relativity, which neither Russell nor I could think of, was to be revealed within a few months by the explosion of the first atomic bomb. For the energy of the explosion was released at the expense of mass in accordance with the relativistic equation  $e = mc^2$  an equation which was soon to be found splashed over the cover of *Time* magazine, as a token of its supreme practical importance.

Perhaps Russell and I should have done better in foreseeing these applications of relativity in January 1945, but it is obvious that Einstein could not possibly take these future consequences into account when he started on the problem which led to the discovery of relativity at the turn of the century. For one thing, another dozen or more major discoveries had yet to be made before relativity could be combined with them to yield the technical process which opened the atomic age.

Any attempt at guiding scientific research towards a purpose other than its own is an attempt to deflect it from the advancement of science. Emergencies may arise in which all scientists willingly apply their gifts to tasks of public interest. It is conceivable that we may come to abhor the progress of science and stop all scientific research, or at least whole branches of it, as the Soviets stopped research in genetics for twenty-five years. You can kill or mutilate the advance of science, you cannot shape it. For it can advance only by essentially unpredictable steps, pursuing problems of its own, and the practical benefits of these advances will be incidental and hence doubly unpredictable.

In saying this, I have not forgotten, but merely set aside, the vast amount of scientific work currently conducted in industrial and governmental laboratories.[3] In describing here the autonomous growth of science, I have taken the relation of science to technology fully into account.

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But even those who accept the autonomy of scientific progress may feel irked by allowing such an important process to go on without trying to control the co-ordination of its fragmentary initiatives. The period of high aspirations following the last war produced an event to illustrate the impracticability of this more limited task.

The incident originated in the University Grants Committee, which sent a memorandum to the Royal Society in the summer of 1945. The document, signed by Sir Charles Darwin, requested the aid of the Royal Society to secure 'The Balanced Development of Science in the United Kingdom'; this was its title.

The proposal excluded undergraduate studies and aimed at the higher subjects that are taught through the pursuit of research. Its main concern was with the lack of co-ordination between universities in taking up 'rare' subjects, 'which call for expert study at only a few places, or in some cases perhaps only one'. This was linked with the apprehension that appointments are filled according to the dictates of fashion, as a result of which some subjects of greater importance are being pursued with less vigour than others of lesser importance. It proposed that a co-ordinating machinery should be set up for levelling out these gaps and redundancies. The Royal Society was asked to compile, through its Sectional Committees covering the main divisions of science, lists of subjects deserving preference in order to fill gaps. Such surveys were to be renewed in the future to guide the University Grants Committee in maintaining balanced proportions of scientific effort throughout all fields of inquiry.

Sir Charles Darwin's proposal was circulated by the Secretaries of the Royal Society and the members of the Sectional Committees along with a report of previous discussions of proposals by the Council and other groups of Fellows. The report acknowledged that the co-ordination of the pursuit of higher studies in the universities was defective ('haphazard') and endorsed the project for periodic, most likely annual, surveys of gaps and redundancies by the Royal Society. The members of the Sectional Committees were asked to prepare, for consideration by a forthcoming meeting of the Council, lists of subjects suffering from neglect.

Faced with this request, which I considered at the best pointless, I wrote to the Physical Secretary (the late Sir Alfred Egerton) to express my doubts. I argued that the present practice of filling vacant chairs by the most eminent candidate that the university can attract was the best safeguard for rational distribution of efforts over rival lines of scientific research. As an example (which should appeal to Sir Charles Darwin as a physicist) I recalled the successive appointments to the chair of physics in Manchester during the past thirty years. Manchester had elected to this chair Schuster, Rutherford, W. L. Bragg and Blackett, in this sequence, each of whom represented at the time a 'rare' section of physics: spectroscopy, radioactivity, X-ray crystallography, and cosmic rays, respectively. I affirmed that Manchester had acted rightly and that they would have been ill-advised to pay attention to the claims of subjects which had not produced at the time men of comparable ability. For the principal criterion for offering increased opportunities to a new subject was the rise of a growing number of distinguished scientists in that subject and the falling off of creative initiative in other subjects, indicating that resources should be withdrawn from them. While admitting that on certain occasions it may be necessary to depart from this policy, I urged that it should be recognized as the essential agency for maintaining a balanced development of scientific research.

Sir Alfred Egerton's response was sympathetic, and, through him, my views were brought to the notice of the members of Sectional Committees. Yet the Committees met, and I duly took part in compiling a list of 'neglected subjects' in chemistry. The result, however, appeared so vague and trivial (as I will illustrate by an example in a moment) that I wrote to the Chairman of the Chemistry Committee that I would not support the Committee's recommendations if they should be submitted to the Senate of my university.

However, my worries were to prove unnecessary. Already the view was spreading among the Chairmen of the Sectional Committees 'that a satisfactory condition in each science would come about naturally, provided that each university always chose the most distinguished leaders for its post, irrespective of his specialization'. While others still expressed the fear that this would make for an excessive pursuit of fashionable subjects, the upshot was, at the best, inconclusive. Darwin himself had, in fact, already declared the reports of the Sectional Committees 'rather disappointing'.

The whole action was brought to a close, one year after it had started, with a circular letter to the Vice-Chancellors of the British universities signed by Sir Alfred Egerton, as secretary, on behalf of the Council of the Royal Society, a copy being sent to the University Grants Committee. The circular included copies of the reports received from the Sectional Committees and endorsed these in general. But in the body of the letter only a small number of these recommendations were specified as being of special importance. This list contained seven recommendations for the establishment of new schools of research, but said nothing about the way these new schools should be co-ordinated with existing activities all over the United Kingdom. The impact of this document on the universities seems to have been negligible. The Chemistry Committee's recommendation for the establishment of 'a strong school of analytic chemistry', which should have concerned me as Professor of Physical Chemistry, was never even brought to my notice in Manchester.

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I have not recorded this incident in order to expose its error. It is an important historical event. Most major principles of physics are founded on the recognition of an impossibility, and no body of scientists was better qualified than the Royal Society to demonstrate that a central authority cannot effectively improve on the spontaneous emergence of growing points in science. It has proved that little more can, or need, be done towards the advancement of science than to assist spontaneous movements towards new fields of distinguished discovery, at the expense of fields that have become exhausted. Though special considerations may deviate from it, this procedure must be acknowledged as the major principle for maintaining a balanced development of scientific research. [4]

Let me recall yet another striking incident of the post-war period which bears on these principles. I have said that the distribution of subsidies to pure science should not depend on the sources of money, whether they are public or private. This will hold to a considerable extent also for subsidies given to universities as a whole. But after the war, when in England the cost of expanding universities was largely taken over by the state, it was felt that this must be repaid by a more direct support for the national interest. This thought was expressed in July 1946 by the Committee of Vice-Chancellors in a memorandum sent out to all universities, which Sir Ernest Simon (as he then was), as Chairman of the Council of Manchester University, declared to be of 'almost revolutionary' importance. I shall quote a few extracts:

The universities entirely accept the view that the Government has not only the right, but the duty, to satisfy itself that every field of study which in the national interest ought to be cultivated in Great Britain, is in fact being adequately cultivated in the universities....

In the view of the Vice-Chancellors, therefore, the universities may properly be expected not only individually to make proper use of the resources entrusted to them, but collectively to devise and execute

policies calculated to serve the national interest. And in that task, both individually and collectively, they will be glad to have a greater measure of guidance from the Government than, until quite recent days, they have been accustomed to receive....

Hence the Vice-Chancellors would be glad if the University Grants Committee were formally authorised and equipped to undertake surveys of all main fields of university activity designed to secure that as a whole universities are meeting the whole range of national need for higher teaching and research....

We meet here again with a passionate desire for accepting collective organization for cultural activities, though these actually depend for their vigorous development on the initiative of individuals adjusting themselves to the advances of their rivals and guided by a cultural opinion in seeking support, be it public or private. It is true that competition between universities was getting increasingly concentrated on gaining the approval of the Treasury, and that its outcome came to determine to a considerable extent the framework within which the several universities could operate. But the most important administrative decisions, which determine the work of universities, as for example the selection of candidates for new vacancies, remained free and not arranged collectively by universities, but by competition between them. For they cannot be made otherwise. The Vice-Chancellors' memorandum has, in consequence, made no impression on the life of the universities and is, by this time, pretty well forgotten by the few who had ever seen it.[5]

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We may sum up by saying that the movements for guiding science towards a more direct service of the public interest, as well as for co-ordinating the pursuit of science more effectively from a centre, have all petered out. Science continues to be conducted in British universities as was done before the movement for the social guidance of science ever started. And I believe that all scientific progress achieved in the Soviet Union was also due—as everywhere else—to the initiative of original minds, choosing their own problems and carrying out their investigations, according to their own lights. This does not mean that society is asked to subsidize the private intellectual pleasure of scientists. It is true that the beauty of a particular discovery can be fully enjoyed only by the expert. But wide responses can be evoked by the purely scientific interest of discovery. Popular response, overflowing into the daily press, was aroused in recent years in England and elsewhere by the astronomical observations and theories of Hoyle and Lovell, and more recently by Ry1c, and the popular interest was not essentially different from that which these advances had for scientists themselves.

And this is hardly surprising, since for the last three hundred years the progress of science has increasingly controlled the outlook of man on the universe, and has profoundly modified (for better and for worse) the accepted meaning of human existence. Its theoretic and philosophic influence was pervasive.

Those who think that the public is interested in science only as a source of wealth and power are gravely misjudging the situation. There is no reason to suppose that an electorate would be less inclined to support science for the purpose of exploring the nature of things than were the private benefactors who previously supported the universities.

Universities should have the courage to appeal to the electorate, and to the public in general, on their own genuine grounds. Honesty should demand this at least. For the only justification for the pursuit of scientific research in universities lies in the fact that the universities provide an intimate communion for the formation of scientific opinion, free from corrupting intrusions and distractions. For though scientific discoveries eventually diffuse into all people's thinking, the general public cannot participate in the intellectual milieu in which discoveries are made. Discovery comes only to a mind immersed in its pursuit. For such work the scientist needs a secluded place among like-minded colleagues who keenly share his aims and sharply control his performances. The soil of academic science must be exterritorial in order to secure its rule by scientific opinion.

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The existence of this paramount authority, fostering, controlling and protecting the pursuit of a free scientific inquiry, contradicts the generally accepted opinion that modern science is founded on a total rejection of authority. This view is rooted in a sequence of important historical antecedents which we must acknowledge here. It is a fact that the Copernicans had to struggle with the authority of Aristotle upheld by the Roman Church, and by the Lutherans invoking the Bible; that Vesalius founded the modern study of human anatomy by breaking the authority of Galen. Throughout the formative centuries of modern science, the rejection of authority was its battle-cry; it was sounded by Bacon, by Descartes and collectively by the founders of the Royal Society of London. These great men were clearly saying something that was profoundly true and important, but we should take into account today the sense in which they have meant their rejection of authority. They aimed at adversaries who have since been defeated. And although

other adversaries may have arisen in their places, it is misleading to assert that science is still based on the rejection of any kind of authority. The more widely the republic of science extends over the globe, the more numerous become its members in each country, and the greater the material resources at its command, the more there clearly emerges the need for a strong and effective scientific authority to reign over this republic. When we reject today the interference of political or religious authorities with the pursuit of science, we must do this in the name of the established scientific authority which safeguards the pursuit of science.

Let it also be quite clear that what we have described as the function of scientific authority go far beyond a mere confirmation of facts asserted by science. For one thing, there are no mere facts in science. A scientific fact is one that has been accepted as such by scientific opinion, both on the grounds of the evidence in favour of it and because it appears sufficiently plausible in view of the current scientific conception of the nature of things. Besides, science is not a mere collection of facts, but a system of facts based on their scientific interpretation. It is this system that is endorsed by a scientific interest intrinsic to the system; a distribution of interest established by the delicate value-judgments exercised by scientific opinion in sifting and rewarding current contributions to science. Science *is what it is*, in virtue of the way in which scientific authority constantly eliminates, or else recognizes at various levels of merit, contributions offered to science. In accepting the authority of science we accept the totality of all these value-judgments.

Consider, also, the fact that these scientific evaluations exercised by a multitude of scientists, each of whom is competent to assess only a tiny fragment of current scientific work, so that no single person is responsible at first hand for the announcements made by science at any time. And remember that each scientist originally established himself as such by joining at some point a network of mutual appreciation extending far beyond his own horizon. Each such acceptance appears then as a submission to a vast range of value-judgments exercised over all the domains of science, which the newly accepted citizen of science henceforth endorses, although he knows hardly anything about their subject-matter. Thus, the standards of scientific merit are seen to be transmitted from generation to generation by the affiliation of individuals at a great variety of widely disparate points, in the same way as artistic, moral or legal traditions are transmitted. We may conclude, therefore, that the appreciation of scientific merit too is based on a tradition which succeeding generations accept and develop as their own scientific opinion. This conclusion gains important support from the fact that the methods of scientific inquiry cannot be explicitly formulated and hence can be transmitted only in the same ways as an art, by the affiliation of apprentices to a master. The authority of science is essentially traditional.

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But this tradition upholds an authority which cultivates originality. Scientific opinion imposes an immense range of authoritative pronouncements on the student of science, but at the same time it grants the highest encouragement to dissent from them in some particular. While the whole machinery of scientific institutions is engaged in suppressing apparent evidence as unsound, on the ground that it contradicts the currently accepted view about the nature of things, the same scientific authorities pay their highest homage to discoveries which deeply modify the accepted view about the nature of things. It took eleven years for the quantum theory, discovered by Planck in 1900, to gain final acceptance. Yet by the time another thirty years had passed, Planck's position in science was approaching that hitherto accorded only to Newton. Scientific tradition enforces its teachings in general, for the very purpose of cultivating their subversion in the particular.

I have said this here at the cost of some repetition, for it opens a vista of analogies in other intellectual pursuits. The relation of originality to tradition in science has its counterpart in modern literary culture, 'Seldom does the word [tradition] appear except in a phrase of censure,' writes T.S. Eliot.[6] And he then tells how our exclusive appreciation of originality conflicts with the true sources of literary merit actually recognized by us:

We dwell with satisfaction upon the poet's difference from his predecessors, especially his immediate predecessors; we endeavour to find something that can be isolated in order to be enjoyed. Whereas if we approach a poet without this prejudice, we shall often find that not only the best, but the most individual parts of his work may be those in which the dead poets, his ancestors, assert their immortality most vigorously.[7]

Eliot has also said, in *Little Gidding*, that ancestral ideas reveal their full scope only much later, to their successors:

And what the dead had no speech for, when living,  
They can tell you, being dead: the communication

Of the dead is tongued with fire beyond the language of the living.

And this is so in science: Copernicus and Kepler told Newton where to find discoveries unthinkable to themselves.

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At this point we meet a major problem of political theory: the question whether a modern society can be bound by tradition. Faced with the outbreak of the French Revolution, Edmund Burke denounced its attempt to refashion at one stroke all the institutions of a great nation and predicted that this total break with tradition must lead to a descent into despotism. In reply to this, Tom Paine passionately proclaimed the right of absolute self-determination for every generation. The controversy has continued ever since. It has been revived in America in recent years by a new defence of Burke against Tom Paine, whose teachings had hitherto been predominant. I do not wish to intervene in the American discussion, but I think I can sum up briefly the situation in England during the past 170 years. To the most influential political writers of England, from Bentham to John Stuart Mill, and recently to Isaiah Berlin, liberty consists in doing what one likes, provided one leaves other people free to do likewise. In this view there is nothing to restrict the English nation as a whole in doing with itself at any moment whatever it likes. On Burke's vision of 'a partnership of those who are living, those who are dead and those who are to be born', these leading British theorists turn a blind eye. But practice is different. In actual practice it is Burke's vision that controls the British nation; the voice is Esau's, but the hand is Jacob's.

The situation is strange. But there must be some deep reason for it, since it is much the same as that which we have described in the organization of science. This analogy seems indeed to reveal the reason for this curious situation. Modern man claims that he will believe nothing unless it is unassailable by doubt; Descartes, Kant, John Stuart Mill and Bertrand Russell have unanimously taught him this. They leave us no grounds for accepting any tradition. But we see now that science itself can be pursued and transmitted to succeeding generations only within an elaborate system of traditional beliefs and values, just as traditional beliefs have proved indispensable throughout the life of society. What can one do then? The dilemma is disposed of by continuing to profess the right of absolute self-determination in political theory and relying on the guidance of tradition in political practice.

But this dubious solution is unstable. A modern dynamic society, born of the French Revolution, will not remain satisfied indefinitely with accepting, be it only de facto a traditional framework as its guide and master. The French Revolution, which, for the first time in history, had set up a government resolved on the indefinite improvement of human society, is still present in us. Its most far-reaching aspirations were embodied in the ideas of socialism, which rebelled against the whole structure of society and demanded its total renewal. In the twentieth century this demand went into action in Russia in an upheaval exceeding by far the range of the French Revolution. The boundless claims of the Russian Revolution have evoked passionate responses throughout the world. Whether accepted as a fervent conviction or repudiated as a menace, the ideas of the Russian Revolution have challenged everywhere the traditional framework which modern society had kept observing in practice, even though claiming absolute self-determination in theory.

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I have described how this movement evoked among many British scientists a desire to give deliberate social purpose to the pursuit of science. It offended their social conscience that the advancement of science, which affects the interests of society as a whole, should be carried on by individual scientists pursuing their own personal interests. They argued that all public welfare must be safeguarded by public authorities and that scientific activities should therefore be directed by the government in the interest of the public. This reform should replace by deliberate action towards a declared aim the present growth of scientific knowledge intended as a whole by no one, and in fact not even known in its totality, except quite dimly, to any single person. To demand the right of scientists to choose their own problems appeared to them petty and unsocial, as against the right of society deliberately to determine its own fate.

But have I not said that this movement has virtually petered out by this time? Have not even the socialist parties throughout Europe endorsed by now the usefulness of the market? Do we not hear the freedom and the independence of scientific inquiry openly demanded today even in important centres within the Soviet domain? Why renew this discussion when it seems about to lose its point?

My answer is that you cannot base social wisdom on political disillusion. The more sober mood of public life today can be consolidated only if it is used as an opportunity for establishing the principles of a free society on firmer grounds.

What does our political and economic analysis of the Republic of Science tell us for this purpose?

It appears, at first sight, that I have assimilated the pursuit of science to the market. But the emphasis should be in the

opposite direction. The self-co-ordination of independent scientists embodies a higher principle, a principle which is reduced to the mechanism of the market when applied to the production and distribution of material goods.

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Let me sketch out briefly this higher principle in more general terms. The Republic of Science shows us an association of independent initiatives, combined towards an indeterminate achievement. It is disciplined and motivated by serving a traditional authority, but this authority is dynamic; its continued existence depends on its constant self-renewal through the originality of its followers.

The Republic of Science is a Society of Explorers. Such a society strives towards an unknown future, which it believes to be accessible and worth achieving. In the case of scientists, the explorers strive towards a hidden reality, for the sake of intellectual satisfaction. And as they satisfy themselves, they enlighten all men and are thus helping society to fulfil its obligation towards intellectual self-improvement.

A free society may be seen to be bent in its entirety on exploring self-improvement--every kind of self-improvement. This suggests a generalization of the principles governing the Republic of Science. It appears that a society bent on discovery must advance by supporting independent initiatives, co-ordinating themselves mutually to each other. Such adjustment may include rivalries and opposing responses which, in society as a whole, will be far more frequent than they are within science. Even so, all these independent initiatives must accept for their guidance a traditional authority, enforcing its own self-renewal by cultivating originality among its followers.

Since a dynamic orthodoxy claims to be a guide in search of truth, it implicitly grants the right to opposition in the name of truth--truth being taken to comprise here, for brevity, all manner of excellence that we recognize as the ideal of self-improvement. The freedom of the individual safeguarded by such a society is therefore--to use the term of Hegel--of a positive kind. It has no bearing on the right of men to do as they please; but assures them the right to speak the truth as they know it. Such a society does not offer particularly wide private freedoms. It is the cultivation of public liberties that distinguishes a free society, as defined here.

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In this view of a free society, both its liberties and its servitudes are determined by its striving for self-improvement, which in its turn is determined by the intimations of truths yet to be revealed, calling on men to reveal them.

This view transcends the conflict between Edmund Burke and Tom Paine. It rejects Paine's demand for the absolute self-determination of each generation, but does so for the sake of its own ideal of unlimited human and social improvement. It accepts Burke's thesis that freedom must be rooted in tradition, but transposes it into a system cultivating radical progress. It rejects the dream of a society in which all will labour for a common purpose, determined by the will of the people. For in the pursuit of excellence it offers no part to the popular will and accepts instead a condition of society in which the public interest is known only fragmentarily and is left to be achieved as the outcome of individual initiatives aiming at fragmentary problems. Viewed through the eyes of socialism, this ideal of a free society is conservative and fragmented, and hence adrift, irresponsible, selfish, apparently chaotic. A free society conceived as a society of explorers is open to these charges, in the sense that they do refer to characteristic features of it. But if we recognize that these features are indispensable to the pursuit of social self-improvement, we may be prepared to accept them as perhaps less attractive aspects of a noble enterprise.

These features are certainly characteristic of the proper cultivation of science and are present throughout society as it pursues other kinds of truth. They are, indeed, likely to become ever more marked, as the intellectual and moral endeavours to which society is dedicated enlarge in range and branch out into ever new specialized directions. For this must lead to further fragmentation. of initiatives and thus increase resistance to any deliberate total renewal of society.

1. M. POLANYI, *The Logic of Liberty*, London: Routledge & Kegan Paul, and Chicago: University of Chicago Press, 1951, p. 12.

2. M. POLANYI, *Personal Knowledge*, London: Routledge & Kegan Paul, and Chicago: University of Chicago Press, 1958, p. 276.

3. I have analysed the relation between academic and industrial science, elsewhere in some detail, see *Journal of the Institute of Metallurgy*, 89 (1961), pp. 401 ff. Cf. *Personal Knowledge*, pp. 174-84.

4. Here is the point at which this analysis of the principles by which funds are to be distributed between different branches of science may have a lesson for economic theory. It suggests a way in which resources can be rationally distributed between any rival purposes that cannot be valued in terms of money. All cases of public expenditure serving

purely collective interests are of this kind. A comparison of such values by a network of overlapping competences may offer a possibility for a true collective assessment of the relative claims of thousands of government departments of which no single person can know well more than a tiny fraction.

5. I have never heard the memorandum mentioned in the University of Manchester. I knew about it only from Sir Ernest Simon's article entitled 'A Historical University Document', *Universities Quarterly*, I-2 (1946-48), pp. 189-92. My quotations referring to the memorandum are taken from this article.

6. T.S. Eliot, *Selected Essays*, London: Faber, 1941, p. 13.

7. *Ibid.*, p. 14.