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SCIENCE AND LIBERALISM

(Michael Polanyi on the freedom of science)

By the end of the '30s and in the early '40s Michael Polanyi entered into a debate with J.D. Bernal on the relation between science and society [1]. The immediate motivation for starting the polemic against Bernal and his fellow Marxists (like Crowther and Hogben [2] was the publication of Bernal's *The Social Function of Science* (1939). There were, however, other imminent motives and reasons prompting Polanyi to raise and discuss the questions concerning the freedom of science in society, as well as the problems and consequences of planning the development of science. Among these motives should be mentioned the strong impact his 1935 journey into the Soviet-Union made on Polanyi. He, in contradistinction to many other Western visitors (like, say, H.G. Wells, G.B Shaw, Romain Rolland or André Gide), was very negatively impressed by his travel experiences, like the abolition of the free market economy or the violation of human rights, and especially by the Soviet political attempts at planning and directing scientific research. He was particularly horrified by the notorious Lysenko affair, which he brings up (in his `The Autonomy of Science', 1942 [3] as an example to illustrate how far the corruption of a branch of science (genetics) can go when its pursuit is placed under the direction of the state.

He gave vent to his scorn and revolt over all these developments in his *The Contempt of Freedom, the Russian Experiment and After* (1940) and in many other papers and addressses collected and republished in his *The Logic of Liberty* (1951).

Among other factors [4], these experiences might have been the ones which made M.Polanyi alert and sensitive towards enthusiastic suggestions and attempts in Great Britain at steering scientific research and in general at subordinating it to social aims and purposes.Such proposals had been put forward by e. g. the British Association for the Advancement of Science in 1938, and more emphatically by the Association of Scientific Workers in 1943.[5] The rhetoric backing up these suggestions to put science in the service of society (more precisely: of the state) was based partly on general, abstract humanitarian arguments and partly on (mostly ignorance-induced) illusions concerning the successfulness of planned science in the Soviet-Union.

> I appreciate the generous sentiments which actuate the aspiration of guiding the progress of science into socially beneficient channels, but I hold this aim to be impossible and nonsensical. (*RS*, p. 62)

Polanyi---as the above quotation and all the numerous papers bestowed by him on this subject testify---was strictly and unconditionally against

> Any attempt at guiding scientific research towards a purpose other than its own because it is an attempt to deflect it from the advancement of science. (...) 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.* (*RS,* p. 62, italics mine - M.F.)

In the followings I intend to reconstruct Polanyi's reasons and argumentation for rejecting the very possibility and legitimacy of planning or directing the advancement of science. I also try to show why he thinks that science cannot and should not be interfered with lest serious harm be done to it. Reconstructing his arguments, I hope, may prove insightful concerning Polanyi's conception of science and also his views on liberalism.

The image of science with Polanyi

1. The pure/applied distinction

As a starting point in reconstructing Polanyi's vision of science we have to refer first to the (intendedly) sharp distinction he makes between *pure* and *applied* science. This he often prefers to call the "liberal" distinction between the two kinds of science in order to emphasize the difference to the Marxist position, in which, as he writes [6], any such distinction is denied.

> In Marxism a distinction between *pure science,* which seeks to find truth for its own sake, and the *application of science* to practical purposes is not admitted, because *all* intellectual processes are assumed to be equally determined by the mode of production of the material means of life. (*CF,* p. 3)

In contrast to this, Polanyi assumes that there is a substantial (not only a factual) difference between the two. The distinction, according to him, rests on a twofold basis: there are sharply defined economic criteria separating the two, insofar as pure science needs (possibly huge) financial subsidies and may not bring profits at all, not even in the long run, while applied science is and can be subjected to the criteria of profitability and rentability. The contrast between pure and applied science, at the same time, presupposes, as well as involves, a profound difference in the *logical* structure of the two fields. In his view *pure* science is aimed (exclusively) at finding the Truth (with capital T), as an intellectual value in itself, i.e. it is aimed at finding true knowledge for its own sake. It is a completely free rational enterprise, free from any other than *cognitive* considerations, i.e unconcerned with social (moral, political, economic or other) motives or consequences. And, though it is in need of economic (financial) support on the part of society, its value cannot and must not be measured by economic criteria (like rentability, profitability and other such measures.)

On the other hand, *applied* science (which Polanyi at some places equates with technology [7] is aimed at practical purposes; its goal is useful knowledge in the service of human beings, i.e. its aim is the improvement of human life. In the pursuit of this goal,applied science relies heavily on the results

attained by pure science; its activities consist essentially in finding the practical consequences and applications of pure scientific knowledge.

Applied science must therefore be concerned with social (esp. with economic) demands and consequences. It can be and, in fact, is subordinated to social (including even industrial, short run) economic considerations. It can be and is evaluated by economic measures.

As far as the *logical* differences are concerned, Polanyi's view is that pure science can progress only if guided by systematic principles, since it is a compact body of basic assumptions shared by all scientists working in the respective discipline. As he writes:

> The progress of mechanics, [which he takes to be the paragon of pure science - M.F.]...through four centuries can be seen to go on continuing on the lines of the same basic ideas. Each new phase re-states that which was known before and reveals that its predecessor was the embryo of a truth wider and deeper than itself. We are faced with a persistent unfolding of thought by logical stages. Technology progresses differently. Lighting [the development of which is his example of the achievements of applied science - M.F.] is constantly made cheaper and pleasanter. To that extent the development is also continuous. But logically each forward step represents a new departure.There are no principles, unless the most trivial ones, which are common to the candle, the gas-burner and the incandescent lamp. (...) Each new improved form of illumination simply displaces its predecessor.Instead of a single principle, we see a series of logically dis- connected attempts to serve a steady purpose. (*LL*, pp. 74-75)

Thus, it seems, that both pure and applied science are governed by a single aim or purpose, but in the former it is Truth, while in the latter it is utility in general (prevalent in all the particular cases like in the above example: the improvement of lighting). The logical differences, as assumed by Polanyi, are however large. Pure science advances on an *unchanging* basis by a cumulative process, enfolding and developing the logical consequences of the basic principles, expanding thus the network of ideas on new unknown fields. The direction of this expansion is, however, *unpredictable* and even the future practical use of the results is *unforseeable*. This way of progressing has its own (unknown) "logic", it is a self-governed or intrinsically-steered process, which must not be extrinsically meddled with lest serious disturbances ensue. This is an important reason why social interference in pure science is not only unwanted and unnecessary, in Polanyi's view, but it is dangerous and potentially harmful [8]. In applied science, where there is no coherent body of basic ideas, where progress is based on ad hoc principles and the tasks to solve are extrinsically given, social direction causes no harm, nay, it may even be needed.

The pure/applied distinction is thus utterly relevant for Polanyi's further argumentation for the freedom of (pure) science (i.e. freedom from all intentional social influence). And though the (above) distinction with which he operates is rather vague from an epistemological point of view, it seems to be apt for him to be used in a debate in which his opponents' arguments are, in this respect, not too sophisticated either. Polanyi mentions e.g. his talk with Bucharin in 1935, who explained to him that the distinction between pure and applied science was due only to the inner conflict of the capitalist society, which deprived

scientists of the consciousness of their social function. And he also quotes Bernal's words, according to whom the ideal of pure science is a form of snobbery, which testifies of an elitistic mentality, and by insisting on science for its own sake, the pure scientist tries to repudiate the sordid material foundation on which his work is based [9]. As it seems, Bucharin and Bernal eliminate any pure/applied distinction by declaring it illusory. Polanyi, however needs it, because he wants to argue for the thesis that social intervention in (pure) science is not only unnecessary, but, more than that: harmful. Thus, if correct, his arguments would cut not merely against such Marxists as Bucharin or Bernal and Co., but against such non-Marxists as, say, Merton as well. In his famous case study, namely, Merton has shown how important and beneficial consequences the emergence of military and commercial problems to solve had on the development of even Newton's theory of gravitation (a paragon of "pure" science) in the 17th century [10]. It is important to emphasize, however, that Polanyi seems to argue explicitly only against intended and centralized, deliberate social or more precisely: state intervention, like planning, and not against the possibility and consequentiality of non-intentional, diffuse social influence upon the development of science (like the appearance of strong economic, military or other needs and interests). Nor does he seem to argue against the assumption of the (possible, partial) social determination of scientific knowledge, (i.e. a sociology-of-knowledge type of assumption concerning science). Implicitly, however, his conception of pure science excludes already such a possibility. As we will see in the next section, Polanyi would be unwilling to admit that scientists' prestige-interests and the whole scientific reward-system is a form of vested but very effective social influence on science and an important motive of scientific progress. His main target in his argumentation against the planning of science is, however, not the diffuse, indirect way of socially influencing science, but the direct, centralized attempt of the state at directing the development of science.

The assumed *logical* distinction (quoted above) between pure and applied science is simply taken for granted by Polanyi, though it is the most important, the strongest premiss in his whole argumentation. But he does not prove this premiss true; he merely brings up examples of it and relies upon its plausibility. In general, in my view, during the whole debate with Bernal and the "planners", Polanyi seems to uncritically accept the then "received view" of science, i.e. the logical positivist view of science as a logically coherent body of knowledge, which is historically invariant and whose development is a kind of cumulative growth. This oversimplified view of science was, however, somewhat obsolete already at the time of the Polanyi - Bernal debate.To this effect, it is instructive to quote only Mannheim's words (from his *Ideology and Utopia*) concerning the cumulative model of scientific growth:

> If this were the case any two periods in the history of human knowledge would only be distinguished from one another by the fact that in the earlier period certain things were still unknown and certain errors still existed which, through later knowledge were completely corrected. This simple relationship between an earlier incomplete and a later complete period of knowledge may, to a large extent be appropriate for the exact sciences *(although indeed today the notion of the stability of the categorical structure of the exact sciences is, compared with the logic of classical physics, considerably shaken*). (Mannheim 1936, p. 243, italics mine - M. F.)

As is to be seen, the compactness and invariance of the logical-categorial framework of the exact sciences appeared to be doubtful at that time to even such "outsiders", i.e. non-scientists as the philosopher-sociologist Mannheim. Polanyi, however, though he is an "insider" (or just because he is a natural scientist) sticks to the idea of the logical coherence and historical invariance of the basic principles of pure science.

> It is true that many of Kepler's, or even Galileo's or Newton's arguments may appear irrelevant today. And again, Galileo and Newton would probably be profoundly unsatisfied with the kind of explanation quantum mechanics gives us of atomic processes. But Galileo and Newton remain nevertheless classics of modern science. (*LL* pp. 37-38)

2. Polanyi's conception of (pure) science and its development

As quoted above, the pursuit of pure science (or science simpliciter) is, according to Polanyi, the search for the Truth and nothing else but the Truth. In his view the scientist is not and must not be concerned in his search for knowledge with any other non-cognitive goals like money, glory or power (attainable through knowledge). Truth is an ultimate and fundamental value needing no further justification. It stands above each and every individual or collective and belongs to a "spiritual reality", the realm of the highest values.

> The coherence of science must be regarded as an expression of the common rootedness of scientists in the same spiritual reality. (*LL* p. 39)

He goes even as far as to presume that

> The totalitarian form of the State arises logically from the denial of reality to this realm of transcendent ideas. (...) For if truth is not real and absolute, then it may seem proper that the public authorities should decide what should be called the truth. (*LL* p. 47)

Pure science, then is, according to Polanyi an essentially *individual* but at the same time *cooperative* enterprise. And just because it is essentially individualistic, it must be governed by a shared vision, a common tradition (*LL* p. 42) and it must contain also a *fiduciary* element (*LL* p. 30). Since the individual scientist must believe that the tradition to which s/he belongs is not merely a series of accidents, not one historically given tradition among others, but on the contrary: as it represents the consistent expansion of Truth, it is a unique and privileged tradition.

Polanyi lays strong emphasis on the cooperative character of science. The pursuit of science consists in the "coordination of individual activities without the intervention of any coordinating authoritiy" (*LL* p. 35), not even scientific collectives can direct it. [11] The example he brings up in this context is that of the putting together a very large jigsaw puzzle, by the coordinated efforts of many participants. Unlike a party of women shelling peas together, whose work represent no coordinated effort, the jigsaw puzzle needs real cooperation and contribution to the solution of the same task.

It is interesting to note that the jigsaw-puzzle metaphor (which turns up later on so essentially in Th. Kuhn's conception of "normal science") is brought up by Polanyi at several places in his writings [12], but is not utilized "in depth" by him (or: in the same way as it is later on by Kuhn). I mean, that he nowhere seems to be sensitive towards the "visual" suggestion of this metaphor, i.e he never hints at the possibility that the picture represented by the completed jigsaw-puzzle may be, metaphorically, identified with that "shared vision" (the Kuhnian "paradigm") which, Polanyi claims, coordinates the individual scientists' efforts. He seems to utilize the jigsaw metaphor in only a quantitative sense: namely, that though the puzzle could be solved by one single scientist, the solution would take a much longer time than if the task was completed by many scientists in concert. (*LL* p.35, *RS* p. 55). The reason why he thinks that the jigsaw-puzzle metaphor is a better analogy of the pursuit of science than that of the collective shelling of peas is that in the putting together of a jigsaw puzzle the task itself needs and generates a spontaneous coordination of the independent, individual activities, and thus, there is no need for a central direction or supervision of them. The following illustration is brought up by him in *RS*:

> 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. (*RS* p. 55)

It is, namely, the *fitting together* of the (differently shaped) pieces which directs the activity of the individuals as well as their cooperation. And this is the reason why no further, extraneous directing is needed. The basic analogy then, subsists between the fitting together of the pieces of the jigsaw puzzle and the (logical) fitting together of the principles and theorems of science. The assumption of the logical coherence of science is thus necessary for Polanyi's argument by analogy (of the jigsaw puzzle) to work. But even if it works, it establishes only that external direction of science is *unnecessary*, not that it is harmful. He brings up, however, a bunch of other suggestive metaphors in connection with science.

Thus, he likens the pursuit of science, following Milton, to the putting together of a fragmented statue (*LL* p. 88), at other places, to the workings of the market coordinated by an "invisible hand" (*RS* p. 56), or again, to the functions of a living organism (*CF* p. 8, *LL* p. 88).

Based on the organism-analogy he then envisages the development of science as organic, spontaneous, unforseeable growth, like that of a living being. A process which enfolds by itself and could easily be destroyed by any external interference. This way he arrives at establishing his claim concerning the harmfulness of external interference with science.

Polanyi even elaborates a further, organisational argument to back up a further claim of his concerning the structure of scientific research organizations. For this he makes use of the fundamental distinction

made between (what he calls [13] the *corporate order* and the *spontaneous order* of members of an institution engaged in performing a common task. People organized in the corporate order work in a strictly hierarchical (pyramidal) structure, where the workers at each level are subordinated to those higher above, and superordinated to those at lower levels. Everybody, except the chief, the topmost person, is performing a fragmentary task, under the direction of the immediate superiors. The actions carried out by persons organized in such a corporate order may therefore be called centrally directed or centrally planned. Spontaneous order comes about (as its name indicates) in a spontaneous, non-intended way; it is not hierarchical and rests on mutual, (essentially) symmetrical relationships ("mutual adjustment") between and among the participants in the structure. The actual pattern of the spontaneous order is not previously *(a priori)* given but generated by the actual task to solve (through mutual adjustment during the performance). In `The Span of Central Direction (1948) Polanyi demonstrates "the immense quantitative superiority" of a system of spontaneous order over one of the corporate order in solving tasks. He takes this thesis to be true, among others, for scientific research organizations as well. Thus, it supplies a further argument for him against (at least) the setting up of corporate order and the efficiency of central planning in and of science.

> For a scientific community, comprising great numbers, to function, there must exist a large area of hidden truths, far exceeding the capacity of one man to fathom; there must be work for thousands. Each scientist starts then by sensing a point of deepening coherence, and continues by feeling his towards such coherence. (...) This is why the initiative to scientific inquiry and its pursuit must be left to the free decision of the individual scientist; the scientist must be granted independence because only his personal vision can achieve essential progress in science. Inquiries can be conducted as surveys according to plan, but these will never add up to new ideas. (`The Growth of Science in Society' In: *KB* p. 82)

As it seems, then, Polanyi is not only against the planning of (pure) science by some external authority, like the state, but he is definitely also against trying to erect a hierarchical organization or central planning *within* science. This is completely consistent with his view (quoted above in section 1), according to which the progress of science is unforseeable. (It is unpredictable not merely by the outsider, like the politician but even by the insider, the scientist.) .

> No committee of scientists, however distinguished, could forecast the further progress of science except for the routine extension of the existing system. No important scientific advance could ever be foretold by such a committee. The problems allocated by it would, therefore, be of no real scientific value. (...) The pursuit of science can be organized, therefore,in no other manner than by granting complete independence to all mature scientists. They will then distribute themselves over the whole field of possible discoveries, each applying his own special ability to the task that appears most profitable to him. (*LL* p. 89)

This is how the pattern of spontaneous order (a system of mutual adjustments) within scientific research work emerges if left alone, without external interference, under ideal conditions.

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Notes

1. In his paper on `The Rights and Duties of Science' published as part of Polanyi's *The Contempt of Freedom /CF/* (1940).

2. "To-day boys and girls who are interested in science... read books which profess that the primary function of science is to promote human welfare. The best-seller in the field has been for the last seven years Hogben's *Science for the Citizen,* which was closely rivalled in its success by J.G. Crowther's books, particularly the *Social Relations of Science* and the famous Social Functions of Science by J.D. Bernal", writes Polanyi in his `Science and Welfare' (1945, In: *The Political Quarterly,* republished in *The Logic of Liberty /LL/,* 1951. p. 68). Hogben published his *Science for the Citizen* in 1938 and J.G. Crowther's above mentioned book appeared in 1941.

3. Originally an address to the Manchester Literary and Philosophical Society, given in February 1942, published in *LL*. In a footnote to p. 59, where he starts describing how the intervention of the Soviet state began in the fields of genetics and plant-breeding about the year 1930, and was definitely established by 1932, Polanyi writes the following: "Note, that the date of writing is December /sic!/ 1942. I have left the account unchanged for its historical interest in showing the position of the Genetics Controversy, as it appeared at the time. This, I believe, was the first paper to draw attention to the danger involved in it to science in general." Which seems to be true, because the most horrible consequences of the genetics controversy: imprisonments and the destruction of the research field came only in the late 40s.

4. Like the similar attempts at steering science in Nazi Germany, another totalitarian dictatorship, where state interference with science resulted in crude falsifications and in a complete and general contempt of scientific truth. Polanyi refers to these developments in his `The Autonomy of Science' (Chap. 4 of *LL*).

5. Cf. Polanyi's accounts about these suggestions in his *LL* (Part I, Chap. 6) and in *The Republic of Science /RS/* (1962).

The Association of Scientific Workers, founded in 1927, was a successor organization of the National Union of Scientific Workers, formed already in 1917. J.D.Bernal was a prominent figure in the AScW. He published the manifesto of the Association as Appendix X of his *The Social Function of Science.* In this document, among the items listed as the aims the AScW seeks to ensure, we find "That scientific research should be directed primarily to the improvement of the conditions of life." (Bernal, *op. cit.* p. 461)

6. See `The Rights and Duties of Science', In: *The Manchester School of Economic and Social Studies,*

10, Oct. 1939, pp. 175-193, republished in *CF*.

7. E. g. in `Science and Welfare' (1945), republished in *LL*, Chap. 5.

8. As he writes in `Science and Welfare' (*LL*, p. 77): "...we note that an extraneous direction of science is mischievous preciesely to the extent to which it is effective."

9. See `The Rights and Duties of Science' in *CF* p. 3 resp. p. 13.

10. See: `Science and Economy of 17th century England' (1939) In: Merton 1964. It is interesting to note, that in this paper Merton argues against the Marxist Boris Hessen's controversial views put forward in his `The Social and Economic Roots of Newton's *Principia'* (In: *Science at the Cross Roads*, London, 1931). Merton shows that social (economic and other) needs can motivate the (pure) scientist and thus exert a positive influence on the progress of science even if the scientist is not conscious of these needs which his achievements serve to satisfy.

11. See his `The Autonomy of Science' (1942) in *LL* Chap. 4.

12. Cf. especially `The Foundations of Academic Freedom'. The Lancet, 3 May 1947, pp. 583-586. Republished in *RS*.

13. Cf. `The Span of Central Direction' (1948), republished in *LL*.

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