

On the Tradition of Intellectuals: Authority and Antinomianism According To Michael Polanyi

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Editor's Note: This essay was originally drafted by Edward Shils in preparation for his address at the celebration of the Polanyi centennial at Kent State, which he delivered in April 1991-- in a manner more extemporaneous and anecdotal than the written text, as was his custom on such occasions. At the time of his death, he had reviewed this draft in preparation for publication here; we have made only minor corrections to the text that follows and have included several passages at the end which had been removed from the final version. It is chiefly through the diligent efforts of Richard H. Schmitt and Christine C. Schnusenberg that *TAD* has been able to see to conclusion Professor Shils' desire to publish his reflections.

ABSTRACT Keywords: tradition, authority, intellectuals, tacit knowledge, scientific training, scientific community, history of science, sociology of science, politics and science, Michael Polanyi, Karl Popper.

Michael Polanyi made an original contribution in his reflections on tradition within the scientific community. Starting with his Riddell Lectures (Science, Faith and Society), he considered the role of authority and the transmission of tacit knowledge within the scientific community, an analysis that can be extended to other, often contrasting, realms of intellectual life.

I

I think that it is worthwhile on this occasion for the reassessment of Michael Polanyi's achievements to recall his reflections on the traditions of intellectuals. Tradition and traditions have had a hard time of it ever since the seventeenth century, but the traditions of intellectuals have, for better or for worse, thrived. Neither Bacon nor Descartes had much patience with knowledge which is acquired through tradition. The conviction that progress was a moral imperative made what was received from the past into a burden; the sooner it was discarded the better. More narrowly, the data obtained by direct observation of present events and the rational analysis of these observations meant that nothing was to be taken for granted except the primacy of direct observation (or observation through reliably uniform instruments), and rational analysis.

In the social sciences, economic theory had no place for traditions in its account of human action. Sociology, likewise, seeing societies moving irresistibly from *Gemeinschaft* to *Gesellschaft*, likewise provided little place for it. Political science, despite its motley composition, did not think that traditional belief or attachment to old practices had any role to play in the conflicts and compromises of "interest groups" in their struggle for power or in the calculus of "rational choice." Anthropology might have appeared to be the one branch of social science which would be compelled to deal with traditions and traditional practices, i.e., long recurrent and persisting beliefs and practices and a positive attachment to things inherited from the past and an appreciation of persons who lived and events which happened

in the past. But no, it has not been that way even in a branch of study devoting itself to what have been called traditional societies.

These academic attitudes towards tradition are only special manifestations of a more widely espoused refusal to acknowledge the existence, endurance and value of tradition.

Very recently, we have seen this war against tradition conducted through the rationalistic contention that “tradition” has been “invented.” It is part of the fashionable derogation, which attempts to discredit cultural and intellectual achievements of long standing and high merit. This, among historians who because they deal with persons and events more concretely and without the interposition of theories and doctrines could be expected to show a little more understanding of human life than social scientists who deal in abstractions or who deal with events under very general rubrics.

In the humanistic disciplines, where the objects of study have been works long carried and received as valued traditions, a passionate denial of the value of the canon of highly esteemed works has become widespread. The canon is viewed, as it is in the notion of the “invention of tradition” as a contrivance with the intention of domination and suppression. “Deconstruction”, the “new historicism”, the “sociological interpretation” are all intended to erode the preeminent standing of highly appreciated works which have long been accepted as the best of the Western intellectual tradition. In the humanistic disciplines, we see an active attack against tradition, not a mere indifference towards it.

Nor does “neo-conservatism,” the intellectual movement which sets its face against the political radicalism of recent years in the “humanities,” accord much more value to tradition or attribute more important functions to it. It too tends to be very rationalistic.

Michael Polanyi was one of the first writers of the twentieth century to call attention to the traditional element in the natural sciences. Polanyi put forward his views on tradition in scientific activity in the Riddell lectures which he delivered at the University of Durham in 1946.¹ A few years later than Polanyi, Karl Popper published his essay “Towards a Rational Theory of Tradition”.²

II

In Polanyi’s view even in the natural sciences with their powers of experimentation and control, precise and reliable observation and the possibilities of mathematical formulation and deduction, there remains a zone or margin of discretion, where intellectual judgments are made but without the guidance of precise rules and explicitly stipulated procedures. He said that scientific propositions could not be derived exclusively from observation by adherence to explicit rules. In 1946 he wrote: “There is a residue of personal judgment required in deciding -- as the scientist eventually must -- what weight to attach to any particular set of evidence in regard to the validity of a particular proposition.” (*SFS*, 31).

(Later in his life, Polanyi referred to this zone of discretion as the realm of tacit knowledge. Tacit knowledge he defined as knowledge which we possess but cannot articulate.)

Of course, Polanyi did not deny the need for meticulous accuracy. “Care and honest self-criticism” -- being

“conscientious” -- are among the most stringent requirements of scientific work. They are among “the first things that a student is taught on being apprenticed to science” (*SFS*, 39). But they are only the beginning. They are conditions for entry into the outer periphery of the scientific community. This kind of “routine conscientiousness” (*SFS*, 40) is indispensable to scientific work but if it is not complemented by “real scientific conscience,” it will come to nothing (*SFS*, 40). It is the latter kind of conscience which is pertinent to judging “how far other people’s data can be relied upon and avoiding at the same time the dangers of either too little or too much caution” (*SFS*, 40).

Polanyi in one place speaks of the mind of the scientist as the “theater of three modes of action.” “Unfettered intuitive speculation could lead to extravagant wishful conclusions; while rigorous fulfilment of any set of critical rules would completely paralyse discovery” (*SFS*, 41). It is the “scientific conscience” which transcends both “creative impulses” and “critical caution.” This is the “moral element in the foundation of science” (*SFS*, 41).

In his later writings about “tacit” knowledge, he went further and asserted that we cannot assert all that we know explicitly but that we know it nonetheless. This added to the penumbra of discretion lying between observation and rules on the one side and the theory on the other, another penumbra which lies outside theory but which is necessary to it.

These two penumbra of knowledge cannot be rigorously controlled. Yet, they are not arbitrary. Without intellectual discipline which is provided by the authority of outstanding scientists, by consensus within the scientific community and by the scientific conscience of the individual scientist, the results of scientific activity would be chaotic. They acquire the rudiments of that discipline through their submission as young students to the authority of those persons who embody the intellectual traditions of their subject or who express those traditions in their actions. Later, they become less submissive to the authority of individuals but they remain positively responsive to the scientific beliefs of other scientists conceived as a collectivity or community.

“A master’s daily labours will reveal these to the intelligent student and impart to him also some of the master’s personal intuitions by which his work is guided. The way he chooses problems, selects a technique, reacts to new clues and to unforeseen difficulties, discusses other scientists’ work and keeps speculating all the time about a hundred possibilities which are never to materialize, may transmit a reflection at least of his essential visions. . . .”³³ (*SFS*, 43-44). “Thus his mind will become assimilated to the premisses of science. The scientific intuition of reality henceforth shapes his perception” (*SFS*, 44). The consensus of the scientific community is the consensus of those who have learned to respect the authority first of their teachers and then of their traditions.

Intellectual traditions, general and particular in the reference, are both explicit and implicit. The explicit ones are the propositions which make up the bodies of scientific and scholarly knowledge at any point in time; they are the propositions ranging from specifically descriptive to general and explanatory which have, at best, been supported and have not been refuted by methodical research and analysis. There is widespread -- although not always complete -- agreement about them among those who work in the field and they are usually regarded as given in the work which goes on in the field. None of these substantive propositions is immune or exempt from being criticized or rejected but the criticism or rejection accepts the correctness -- at least for the time being -- of most of the other propositions specifically descriptive and generally explanatory which make up the existing body of knowledge in that field and which are the objects of the consensus of that particular sector or sub-community of the larger scientific community.

III

For any particular individual member of this sub-community, these propositions are the point of departure for subsequent work in the field. They are added to, revised, corrected and forgotten but none of these is done to all the propositions all the time. They are the tradition of the subject.

Popper in his essay on a rational theory of tradition also has much to say about scientific tradition. He makes an eloquent and forceful argument which is all the more striking because he himself for the early part of his intellectual life lived in an intellectual community which had little sympathy with any idea of the importance of tradition. Popper asserts about tradition that it is a precondition of the growth of scientific knowledge, which grows not by direct observation but by the juxtaposition of observations and received theories, i.e., tradition. Without such traditions, a science would have to begin afresh in every generation or indeed with every individual scientist.

Polanyi would surely have agreed with Popper on this particular point although as far as I know he never expressed himself about this. (Popper and Polanyi avoided any reference to each other. As far as I recall, whenever I mentioned the one to the other, I was answered with silence.)

Polanyi went further than Popper in the analysis of the tradition of science. With all due respect to Popper, Polanyi's analysis was deeper. Beyond his statements about the existing state of theoretical knowledge as the tradition to which every scientist has to respond, Popper touched only briefly on the scientific sensitivity to problems and possibilities in a short passage on the difficulties to establishing a scientific tradition in a society in which there has been none previously. Here Popper showed that he knew that there is more to the scientific tradition than the givenness of theories at any particular point in time. He did not however go any further. This Polanyi did.

He wrote: "...the process of learning must rely in the main on the acceptance of authority. Where necessary, this authority must be reinforced by discipline" (*SFS*, 46). But it is not authority as such which legitimates itself; it is rather the obvious devotion of the person in a position of authority to scientific truth. The claim to the right to exert authority and the readiness to acknowledge it rests on the fact that "masters and pupils...possess in general sufficiently sincere attachment to science and a sufficiently authentic vision of it to find therein a common ground for agreement" (*SFS*, 46).

There are occasions on each side where this condition is not met, when students are recalcitrant and where teachers are dogmatic and prejudiced. But these instances are relatively infrequent and they do not occur at the centers of scientific creation.

It is obvious that tradition is more than the simple factual existence of propositions which a newcomer must acquire as his own point of departure. Tradition has a normative content. There is a norm implicit in the exemplary action of persons in positions of authority; it must be heeded and observed on grounds other than prudence or inevitability. The persons to whom authoritative statements are addressed must accept them because they are rendered -- at least for the time being -- valid to him through the fact of their enunciation by the authoritative person and not solely on the basis of their rational or empirical persuasiveness.

Polanyi regarded this exercise of authority as a transient condition in the relationship between any particular

teacher and his particular pupils. In the course of time, the authority of the teacher yields to the evidenced validity of the proposition which he asserts. “The authority to which the student of science submits tends to eliminate its own functions by establishing direct contact between the student and the reality of nature. As he approaches maturity the student will rely for his beliefs less and less on authority and more and more on his own judgment. His own intuition and conscience will take over responsibility in the measure in which authority is eclipsed” (*SFS*, 45). His conscience is reinforced by the presence, proximate or remote, of other scientists whose own scientific attitudes, expressed in publications and conversation, heighten the scientific collective self-consciousness. The teacher might continue to enjoy a distinctive centrality because he enunciates more interesting and more plausible or more propositions with better evidence than other persons in the scientific community. His authority therefore becomes more specialized and less compelling than it was in his formally pedagogical role. His authority now resides in his indicating subject-matter, problems, hypotheses, techniques in a manner which, without being compulsory, is worthy of serious attention. The scientist who is regarded as outstanding remains an authority; he remains a center of a community which studies particular subjects. His authority is, however, more suggestive than it is imperative.

IV

Tradition has frequently been used to refer to the unformulated, the vague, the unspoken, the unwritten, which is accepted on grounds of the authority of those who declare it. This seems to be remote from rational thought. It seems to be antithetical to the rational grounds for the acceptance of a proposition or of a pattern of thought. Yet reasons might still be good reasons even if they are not clearly formulated. Vagueness might be a mist of unclarity around an image, or an insight, or a proposition which, when it is clarified by subsequent analysis, turns out to be an important image or an important insight. This would indicate that what is regarded as true was present first approximated through vague, unclear images, insights or propositions.

This general statement applies to the guiding rules or norms or procedures by which observations are turned into theories without the person who makes that operation knowing just how he has done it. But not to know a theory in a formulation which is explicit and precise does not mean that it is not known in a less explicit, rigorous and precise formulation. A person knows how to construct a theory from data but this does not mean that he knows the theory so well that he can make that procedure of construction clear to others.

These kinds of “propositional propositions” are part of what a newcomer to a given field of learning acquires from those who are already practitioners of that field (i.e., who are already members of the particular scientific community or scientific sub-community) which the scientist or any other intellectual creator “follows” when he is creating an intellectual work.

Polanyi has emphasized the role of these two kinds of penumbra knowledge in substance; i.e., in propositions which constitute the substance or content of a field or sub-field of science. I would like to extend the account of their function to the skill or art of the choice of problems for one’s own research, of the perception of the relative importance of particular or general problems for a whole field (or sub-field) to the assessment of individual scientists (or literary men or whatever other kind of intellectual).

The knacks of perceiving important problems, of perceiving clues which can lead to important interpretations, of imagining the potentialities and limitations of an existing theory are properties of individual minds; they are acquired

as penumbra traditions from the scientific traditions and bearing of teachers and eminent scientists with whom the younger scientist is in intensive and prolonged contact.

It is not possible to “score” intellectual works numerically with any pretense to validity. “Citation” indices are useful only *post facto*; they can only support assessments made in other terms or by other means but they cannot themselves constitute such assessments. The assessment of the intellectual importance of a work or of the intellectual merit of a particular individual creator of works requires skills much like the skills which are operative in constructing a theory from observations, or in the application of an inarticulated or “tacit” proposition to the assessment of a theory or of a body of data.

Polanyi has repeatedly emphasized that the basis of scientific work (or scholarly work of any sort) is the agreement that there is reality outside oneself and that it is the same reality for others as it is for oneself. A further basis is the belief that this reality can be known, that it is worth knowing and that there are rules of discipline for knowing it although these rules are not sufficient for knowing it. Yet what must be added to those rules are modes of judgment which are not arbitrary. These modes of judgment have constancy between individuals and within individuals. The latter rules must also be learned by combinations of the experience of investigation and the experience of close intellectual association with a person who has mastered them and who applies them in a way which can be apprehended, even though neither the person who performs them nor the person who sees them can state exactly and concretely -- or abstractly -- what he is doing or what he is seeing.

These phenomena of the penumbra might become traditions. They too can become part of what is given, just as the explicit theories can be traditions embodied in written or printed form; they are just beyond or prior to writing and print. They are nonetheless acquired just as written or printed propositions can be acquired.

V

The scientific community is a community because it is approximately consensual about the kinds of things which have been referred to in the foregoing pages. The consensus need not be -- in fact, it could not be -- perfect; not all scientists agree on everything in their field. Yet, there could be no body of scientific knowledge and there could be no intellectual growth of such a body of scientific knowledge, nor could there be any territorial expansion of that body of knowledge if there were no consensus and hence no community of the individuals participating in that consensus.

When we speak of a body of scientific knowledge, we speak of a body of scientific propositions. These are propositions which have been established and confirmed by investigation (research) and assessment. That confirmation of a single proposition and the acceptance as confirmed of a body of scientific knowledge is consensual means that a plurality of individuals are participants in the consensus. It is this consensus of pluralities of scientists, together with their interaction as scientists, that justifies our speaking of a scientific community or scientific communities.

The consensus must exist not only with respect to substantive propositions. It must also exist with respect to penumbra knowledge and to penumbra criteria of judgment. But the consensus is not required beyond the World 3⁴ of substantive propositions, penumbra knowledge and substantive and penumbra criteria of judgment.

Polanyi wrote in 1946: "The consensus prevailing in modern science is certainly remarkable. ... each scientist follows his own personal judgment for believing any particular claim of science and each is responsible for finding a problem and pursuing it in his own way; and ... each again verifies and propounds his own results according to his personal judgment. And yet ... we see scientists continuing to agree on most points of science" (*SFS*, 50).

The consensus of scientists is about substantive propositions in the first place. It is also about methods and theories. It extends beyond these rationally declarable things into two directions. One of these directions is towards and into the stratum of penumbra knowledge; the other is towards the institutions in which scientific work is performed and towards the scientific community in its wider reaches. (The scientific community embraces formally established institutions and informal patterns of relationships -- within the scientists' own (national) society and with scientists of other societies. The scientific community is a community because it has consensus about the interestingness of a set of common objects and objectives, because it has common rules governing not only the conduct of research -- "the scientific method" and the ethics of research -- but also governing the assessment of particular pieces of research and of whole fields of research and of the individuals who have done the research.) Polanyi wrote: "... no one can become a scientist unless he presumes that the scientific doctrine and method are fundamentally sound and that their ultimate premisses can be unquestioningly accepted." (*SFS*, 45).⁵

The membership of an individual in the scientific community is no formally inscribed or recorded condition. It exists when an individual who does scientific research has assimilated and is guided by its rules, affirms the values entailed in its objectives, and conducts himself towards his colleagues within the lines implied by those rules and objectives.

VI

The substantive propositions, ranging from the most explicit to the most general or abstract and from the most explicit to the most tacit, form at any particular moment a tradition for scientists working in a particular field. It is a tradition in the factual sense of an inevitable point of departure. The unarticulated or tacit "propositions" are not written down and by their nature are not explicitly stated. They are also traditions in the sense of being vague and unformalized. They must be transmitted largely orally and even then, in implicit rather than explicit form, although they can also be transmitted in the setting of the formation and exposition of more formalized propositions.

Authority plays a part in the transmission and acceptance of traditions. Does it also play a part in the traditions of the scientific community? It does but it is the authority, not of particular individuals in particular roles but of the scientific community as a whole and of the individuals who are regarded as capable of representing the scientific consensus. There are authorities whose judgments are regarded as worthy of acceptance with the understanding that they are subject to examination, revision and correction or outright rejection -- but never in their entirety at any moment of time. In any case, the existence of any confrontation of the tradition is ineluctable.

Of course, it is conceivable that each generation would or could develop penumbra kinds of knowledge from its own experience of seeking knowledge about reality. If it did so in any given generation, it is unlikely that it would fail to transmit it together with the scientific knowledge that had accumulated. Indeed penumbra knowledge is always transmitted in connection with the search for and the transmission of knowledge about reality. Just as the knowledge so gained becomes at least, for a time, tradition, so too the penumbra knowledge formed and practiced alongside it also becomes tradition.

It may be said that penumbra knowledge is more traditional, i.e., it lasts longer, as tradition, than does substantive knowledge. Penumbra knowledge acquires increments and some of this may pass into substantive knowledge and hence is rendered transient. Nevertheless, penumbra knowledge which is about the acquisition of substantive knowledge remains valid for a longer time than does substantive knowledge, which is revised, corrected and supplanted over shorter spans of time than are the arts or skills and “tacit propositions” of penumbra knowledge.

VII

Membership in the scientific community, accorded on the basis of the possession of a qualifying degree or of publications in accredited organs or of appointment as a teacher or research worker or high scientific administrator, constitutes grounds for trust. The belief that another person is a scientist, i.e., a member of the informally constituted scientific community, accredits him as trustworthy. The scientific community is constituted in part by the mutual confidence of scientists in the probity of their partners. There might be discrimination in the relative assessment of the scientific capacities of members but there is seldom doubt regarding their probity. This confidence is in part a product of the assurance that critical scrutiny is never, or at least very seldom, relaxed and that even if the moral disposition of those members were not sufficiently strong, they would be counselled by prudence to conform with the expectations of honesty. It is usually anticipated that detection of the failure to meet such expectations would result in severe sanctions. Even the suspicion of such failure to conform with the standard of probity damages the reputation of a scientist.

There are no officially and formally codified rules in the scientific community; scientists have no “code of ethics” like some other professions. The falsification of observations, claims to have made observations which were never made, and plagiarism of the results of other scientists are among the few activities which are unusually denounced but until recently prohibitions have not been formally promulgated. Yet their infringement leads to sanctions. The sanctions include dismissal from appointments, removal, permanently or for a specified period, from lists of persons eligible for the financial support of their research, etc. Most of the obligations entailed in membership of the scientific community are positive; but they too are not clearly promulgated.

The fundamental belief on which the consensus of the scientific community is focused is the obligation of the scientist to discover the truth about the stratum or sector of reality which falls within the boundaries of their discipline or subject. It is probable that most living scientists do not like to use the term “truth” or to speak of “the truth” except in the context of very particular propositions. They regard the term “the truth” as embarrassing or even repugnant. They object to the use of the term “the truth” except on ceremonial occasions or when discussion becomes rhetorical. It sounds too metaphysical or idealistic for them. Yet it is the postulate of their activity. By far the majority of scientists, both outstanding and obscure, act as if they have an obligation to seek the truth and to criticize error. They are indeed usually very scrupulous to adhere to this obligation.

VIII

The foregoing presents, with some reformulations, Polanyi’s account of the scientific community and the obligations of its members. It is perhaps a little too idealistic. It perhaps makes scientists out to be somewhat more virtuous than they are in fact. There are more strains in the scientific community than Polanyi indicates. Teachers and supervisors of research and research students do not always conform with the pattern which Polanyi set forth.

He overstates the degree of autonomy of scientists in the choice of their problems of research, given the financial dependence of scientific research on governmental and private institutions who grant support in anticipation of practically usable results. The great increase in the scale of research projects also entails a diminution of the autonomy of individual scientists, particularly younger ones and those who are not the most highly reputed. It has nothing to say about the probably still very slight but nevertheless increasing frequency of ease of fraud in research.

Even if Polanyi's account of the scientific community has to be corrected somewhat, it still remains an account of a section of the intellectual stratum -- nationally and internationally -- which is very devoted to the transcendental ideal of truth and orderly, peaceful and rational in the conduct of activities which presuppose an irreducible consensus among the disagreement and criticism.

What however about other features of the conduct of scientists? What about the other sectors of the intellectual stratum? What about the other sectors of the academic profession? Does Polanyi's analysis portray the situation of these other sectors? Does it even portray the activities of scientists outside of science?

In the academic parts of the intellectual stratum, outside the natural sciences, there are certain fields in which there is a some approximation to the situation obtaining in the field under Polanyi's scrutiny. In the past, the classical and philological disciplines were very rigorous. They were for the most part concentrated on observations. When the collation, emendation and annotation of ancient texts was the main task of classical philosophy, a very strict discipline was observed. Unlike the natural sciences, they did not seek to construct theories. Any generalization which sought to place the particular data in the setting of a wider or more fundamental proposition was not only scrupulously examined; it was, in fact, frequently treated contemptuously, as if it were illegitimate. In the social sciences, the situation of respect for the rules varied greatly; consensus within the disciplines was less comprehensive and less compelling.

There may be several causes or reasons adduced for such variations in the adherence to rules. The degree of direct observability of the phenomena studied might be one variable; the more directly or the more mechanically observable by the investigator, the more applicable are strict rules of observation. Social scientists do not see directly the objects of their investigation; they do not see political parties or national states or social classes; they have to construct their objects of study from fragments. The degree of personal contact between pupil and master in the conduct of observations might be another. Closely related to these two is the degree of precision which is attainable in the definitions of terms and in the formulation of concepts.

In the disciplines in which observations can be made more directly, in which the pupil stands closer to the master and in which terms, operations and propositions can be stated more precisely, the scientific conscience of the individual benefits from the imposition of the resulting discipline. The conscience of each individual scientist working in a given field is supported by his awareness that his colleagues are under the same discipline; he sees that attitude manifested in their conversation and in their work. All this strengthens the consensus of the communities of natural scientists.

The difference between the degree of consensus in the natural scientific stratum and the degrees of consensus in the other parts of the intellectual stratum may, in part, be accounted for by the relatively high degree of self-containment of the scientific community (and sub-communities) *vis-à-vis* the other spheres of society. Of course, it would be an exaggeration to suggest that the scientific stratum and community are entirely self-contained. After all,

scientists must be maintained as physiological organisms and insofar as they do not maintain themselves from the sale of the knowledge which they inherit and discover, they must be supported from revenues accruing to them from their own private property or from patronage by rulers, ecclesiastical authorities and persons or institutions with revenues coming to them through the inheritance of property or from their own economic activities. (Alternatively, they could maintain themselves from the sale of their services as teachers to the families of their pupils. This they have seldom done; higher educational institutions have only to a very small degree been supported by tuition fees alone.)

Michael Polanyi's portrayal of the scientific community refers primarily to the one and a half centuries between the Napoleonic regime in France and self-reforming Prussia at one end and the Second World War at the other end. These fifteen decades were not only the great decades of pure science; they were also the decades of the rise of academic science to dominance in the scientific community. This was a period of a marked increase in size and density of the population of academic institutions and of their expansion over a larger territory and a great multiplication of the institutions through which it was practiced. Scientific activities and scientists became more independent of ecclesiastical authority and religious doctrine; they also became more dependent of direct patronage by the earthly powers of state and economy.

Although there were numerous points at which the scientific community touched on the economy -- in seeking financial support from it and in the discovery of knowledge which was of practical interest to business enterprisers and their agents, the connections were not as frequent or as dense as they have since become. The same was true of the relations between scientific activities and the state in its civilian and military aspects. Despite these connections with state and economy, these connections could be reasonably regarded by scientists as marginal to the main objective of scientific activity which was the scientific discovery of scientific truth. Within those conditions at the margin of the scientific community, Polanyi's account is largely correct. Qualifications need to be introduced here and there, but as regards the scientific activities as such, the scientific community is intact and must continue to be so as long as scientific knowledge, in which truth is the desideratum, continues to be sought.

Nevertheless, qualifications must be introduced into Polanyi's account. For one thing -- and very important it is -- scientific activity has become much more dependent on separately budgeted financial support. Its main support is no longer provided from the budget of the university which covered both teaching and research without distinction. It has become more dependent on specialized institutions, governmental and private, which now supply the greatly increased funds for scientific research. This has been accompanied by a complicated distribution of the power of decision as to what research should be done. Although much of the power of decision as to what research should be done and how it should be done still rests with scientists, that power is now shared with non-scientists in governmental and private institutions, private foundations, business enterprises, etc.

Certain scientific developments which were less well advanced and less prominent at the time when Polanyi wrote about these things have brought scientific knowledge and practical economic activities closer together in content and in the lapse of time between discovery and application. This has made the commercial interest in scientific research more specific; it has also made academic scientists in these fields more interested in the pecuniary benefits to themselves of particular fields of research. Hence, the criteria of choice of subjects of research have become less exclusively scientific. It must be pointed out however that these observations refer only to very limited fields of research.

Finally, the autonomy and separation of the scientific community from its surrounding society has been

diminished from within the scientific community by the heightened interest of scientists in politics. This was not very prominent in Polanyi's mind in the 1930s. He was a severe critic of the movement in the small but influential circle of British scientists to further "the planning of science." Indeed, I think that it was his disapproval of the effort in the 1930s to introduce political and other external criteria into the allocation of funds and the choice of problems for scientific research that precipitated the elaboration of his ideas about the autonomy of the scientific community. (Before the Second World War, Polanyi was a severe critic of the claims made on behalf of economic planning in the Soviet Union.⁶ He must have been critical of communism since 1919 but he was not active politically.) His desire was to protect the scientific community from the intrusion of politics into its government. That is also the reason why he founded, with John Baker, The Society for the Protection of Freedom in Science. He took a serious interest in the suppressions of Mendelian genetics in the Soviet Union and it was this interest which led him to found the Committee on Science and Freedom in 1953 (with Alexander Weinberg and with the support of the Congress for Cultural Freedom).

Polanyi was never really interested in extending the political influence of scientists. The "atomic scientists' movement" in the United States and its even smaller British companion never aroused Polanyi's interest in the efforts of scientists to influence legislative decisions. In fact, he was utterly uninterested in it, even though some of his friends such as Leo Szilard and Eugene Wigner were much interested in it. As far as I know, he was not attracted by the "Pugwash movement." His political interests as a liberal did not go further in his thought than to set forth arguments for the freedom of scientific investigation and to elaborate the institutional arrangement with support of the freedom of science.

I think that he would have been very distressed by the strong political declarations made by American scientists; but, by the second half of the 1960s, his intellectual attention had moved far away from his earlier preoccupation with the freedom of science and with the traditions of the scientific community which sustains that freedom.

Had he taken that matter in hand again, it would have been incumbent on him to include the politicization of academic scientists. My own view is that he would not have had to change his account of the internal structure of the scientific community but he would certainly have had to broaden the range of his analysis to the external connections of the scientific community and consider the extent to which these were affecting the autonomy of the scientific community, the freedom of scientific work and the course of scientific development. He would also have had to consider the consequences for the internal structure of the scientific community of the much increased engagement of scientists in politics. In consequence of this interest in politics, the scientific community was renouncing one of the conditions which had kept it so concentrated on the pursuit of scientific truth. As long as scientists addressed themselves to an audience of other scientists and were contented to do so, they were kept under the discipline of their shared love of truth. They could also express themselves with confidence that they would understand each other.

IX

Thus far I have confined my account of Polanyi's writings on intellectuals to one particular sector of the intellectual stratum. Polanyi himself did not confine himself within those boundaries. From about the middle of the 1950s, he began to write about intellectuals of other kinds, literary intellectuals, publicistic intellectuals, intellectuals who wrote on philosophical matters and on society. In an essay in *Encounter*, in his Eddington lecture *Beyond Nihilism* and in chapters of *Personal Knowledge*, he dealt with intellectuals who had yielded to the totalitarian temptation. This new interest overlapped with his older interest in scientific intellectuals but he never entered into this new subject in

the fundamental way in which he analyzed the scientific sector of the intellectual stratum.

Polanyi was a cultivated man and in Manchester, London and Oxford, he moved in an intellectually mixed company of scientists, philosophers, literary critics, educated businessmen, leader writers and editors of the *Manchester Guardian*, *Encounter*, etc. Yet, until the mid-1950s, perhaps until the Hungarian uprising in the autumn of 1956, he was rather insensate to the political overtones of the intellectual society around him. He had no awareness of the political dispositions of the intellectuals of his own time. He did, it was true, oppose those who wished to bring political objectives into scientific activity but he did not seem to be aware that this movement of the Stalinist and fellow-travelling British scientists of the 1930s and 1940s had any wider roots or affinities.

In January 1947, I went to Manchester on his invitation to address the Manchester Literary and Philosophical Society. It was a small meeting, held in a private house of modest dimensions. I think that the quarters of the Society on John Dalton Street had been damaged by a German air war raid during the war; perhaps the Society was faltering towards its end. The meeting was attended by little more than ten persons. I spoke on "Intellectuals in Politics" and I stated what has become a commonplace since then, and which was perhaps not all that original, even then. This was before *L'Opium des intellectuels* which gave the imprimatur of a famous scholar to the ideas I expressed. It was before Lionel Trilling coined the term "adversary culture." What I said was centered mainly on that common feature of the culture of intellectuals in the West since romanticism and the Enlightenment, namely, their obsession with the deficiencies of their own societies, their vehement criticism of the rulers of their own society, their hatred of the bourgeoisie, etc. and since the middle of the nineteenth century, contempt for the liberal democratic order. All these things are perfectly obvious now.

They were not obvious to Michael Polanyi at that time. Polanyi disagreed completely with what I said. Perhaps he was ashamed of his rustic protégé? In his charming, soft-voiced way, he disrupted the meeting. He objected to everything I said. He gave no counter arguments. He repeated over and over again that I was wrong. Since I was his guest and he was my senior by several decades, I persisted by citing numerous individual instances from Rousseau and Voltaire to Sartre and his friend Koestler with many references to nineteenth century French and English novelists, and let it go at that. It was clear to me that Polanyi, with all his very intimate knowledge of scientists and his exceptional sensitivity and imagination about them, had none of these qualities when it came to thinking about intellectuals other than scientists. I think that he failed to see that scientists were intellectuals in a special situation. Nevertheless, he did towards the end of the ensuing decade begin to face the fact that there was a widespread outlook among non-scientific intellectuals which was not what he found among scientists within the scientific community. He became aware that many intellectuals did not participate in the consensus of natural scientist-intellectuals and in the moral trust which he thought was integral to scientific work. (He did not say that many of these intellectuals were also scientists, a significant omission.)

His Eddington lecture on "nihilism", delivered in Cambridge in the late 1950s, took up belatedly the view which he had unqualifiedly rejected when I set it forth in Manchester in January 1947. Of course, he tried to put it more philosophically than I had. The question is: was it adequate to account for the tidal wave of hostility to modern Western liberal democratic society? I think that it was not.

He described the outlook of a large block of these intellectuals as nihilistic. I think that this was inadequate. The intellectuals who became communists and fellow-travellers after the Russian Revolution of October 1917 were not

nihilists in the sense of having no beliefs or of believing that nothing was of any value. They were nihilistic in the sense that Bazarov was a nihilist; they wanted to make a clean sweep of all existing institutions and beliefs and wished to replace them by a rational society based on scientific knowledge.

After the Hungarian insurrection of 1956, which affected him deeply, he tried to understand what had led some of his contemporary intellectuals to espouse the Marxist-Leninist (and Stalinist) view of the world. I think that he did not make great progress in this effort. The mixture of scientism, the appreciation of violence, the tradition of hedonistic humanitarianism was not quite within his field of comprehension. He was unable to grasp that there were intellectuals who hated their own society.

The willing submission of scientists to the fundamental and unspoken values accepted throughout most of the scientific community about which he wrote with such deep insight was so different from the fundamental rejection of their own society by many intellectuals, including scientists, that he could not quite accommodate it.

But the tradition of scientific rationality is not the only tradition of intellectuals. Some of the traditions of intellectuals present and sustain the very antitheses of scientific rationality. For example, romanticism is no less important in modern societies and cultures than the tradition of scientific rationality. There are many variants of the romantic tradition; these run into other traditions. For example, the tradition of moral purity, the eschatological or chiliastic tradition, and the traditions of rejection of the world have all remained very much alive among intellectuals in the Western world.

The intellectual supporters of revolution were very far from nihilists. They believed a great deal. They believed that the kingdom of God on earth was possible -- except that they did not believe that God existed. They believed that human beings could and must be commanded and cajoled into conformity with a rationally conceived scheme for the ordering of society. They believed that an elite with a thoroughly scientific outlook could discover the right pattern for the ordering of society and that this right pattern would be ethically just, rational and efficient. They believed that there was nothing ethically problematic about coercion in the service of these ethically right ends.

Polanyi was right when he asserted that they had excessively high moral demands. This seems to me to be much closer to the truth than his statement about nihilism as a result of moral inversion.

There were other things in which they believed. They -- not all but many -- believed in the ethical value of violence; they saw not just that it was useful removing their enemies and intimidating them but because it had a transfiguring effect. They seemed to believe that human beings were ethically perverse and that violence could purge them of that perversity. They praised violence as an intrinsically purifying condition and they praised it as a means of overcoming the natural deficiencies of human beings. They thought that violent destruction was the prelude to a state of ethical perfection, a means of purification.

Polanyi glimpsed but did not follow-up his glimpse of the fundamental position of the anti-bourgeois attitude of many quite eminent literary intellectuals. He did see the important part played by bohemianism as the institutional counterpart of the scientific community. Although he interested himself in aesthetics in the last years of his life, he failed to see that the doctrine of "art for art's sake" was a counterpart of the scientist's love of truth for its own sake.

Although in his analysis of science he made much of the discipline imposed by an audience of the equally qualified, he did not see that the audiences of most other intellectuals were of the laity. The lay audience could always be accused of being insufficiently appreciative of literary or artistic achievement while such an accusation could scarcely be made in the scientific community. He also failed to see that literary and artistic intellectuals were enterprisers attempting to sell their products in a market as a means of obtaining a livelihood; this stood in contrast with scientists who did not sell their products to laymen and who did not depend on such sales for their livelihoods. These differences account for the existence of Grub Street and the bohemian mode of life and outlook among literary artistic and publicistic intellectuals in the eighteenth and nineteenth centuries, especially in the latter. It was from the culture of bohemia that the hostility of intellectuals to bourgeois emerged. There were many other factors such as an increased social sensitivity and the correspondingly increased publicity about the outermost peripheries of society.

X

I do not think that any other writer on the tradition of scientific knowledge and of scientific activity has ever penetrated so deeply and intimately into that subject as did Michael Polanyi. He was living in a society which did not understand much about tradition and his achievement is very noteworthy; it is indeed unique in his discussion of the various ways in which traditions operate in science. But there his penetration ended. He failed to extend his analysis of intellectual traditions, intellectual communities, intellectual disciplines beyond the practitioners of science.

He was steadfastly courageous in opposing the politicization of science, i.e., the bringing of political ends into the scientific community but he did not grasp the historical rootedness of the efforts to infringe on the autonomy of science.

It might sound odd that I should accuse Michael Polanyi of being oblivious to the dangers of scientism. He was unaware of the traditions of scientism. I do not think that Polanyi was guilty of scientistic arguments -- as Frank Knight very tenaciously insisted.⁷

I think rather that he was precluded from seeing the dangers of scientism because he thought that all factual or empirical statements about society or human actions were necessarily normative or moral statements. If one believes that empirical statements are also moral statements,⁸ then there is nothing formally wrong with the scientistic view that scientific knowledge contains moral imperatives. This is what the adherents of "scientism" believe. I think that Michael Polanyi was insufficiently aware of this affinity between his own views and those who thought that knowledge of the laws of history and society confers knowledge of the right order of society and the ethically justifiable path to the attainment of that order.

Michael Polanyi's discussion of the character of modern literary and publicistic intellectuals is of such limited value compared with his understanding of scientific intellectuals that it calls for an explanation. The simplest explanation is that he spent nearly his whole life until his last years among famous scientists and felt very much at home with them. He had very strong sympathy with them. He started from the other end in dealing with literary and publicistic intellectuals. He had no intimate knowledge of literary and publicistic intellectuals comparable to what he knew of scientists.

Perhaps when he was younger he was so habituated to association with persons who were of socialistic

inclinations that he thought that was what most intellectuals were. I doubt whether he was ever a socialist. He was I am sure never a communist or a sympathizer with the short-lived Communist dictatorship in Hungary. Yet, when he went to the Soviet Union in about 1928, he did not become an anti-Communist; he became that much later.

It is true that he was a liberal in economics. Throughout his entire career at Manchester and for much of the tenure of his fellowship at Merton College, Oxford, he was closely associated with Professor John Jewkes who was a stout-hearted proponent of the institution of the market as a fundamental and necessary feature of an economic system. He was certainly very critical of the pretensions of economic planning. But until the Hungarian insurrection of the autumn of 1956, he did not turn his mind to the task of understanding why collectivistic beliefs were so widespread among intellectuals.

XI

Let me conclude by saying that in his analysis of scientific traditions and the scientific community to the constitution of which they are so essential, Michael Polanyi made an enormous contribution to our understanding of the nature of the community of scientists and of how that community makes possible the growth of scientific knowledge. In doing so, he has also provided a pattern of analysis which can be extended with equal fruitfulness to the understanding of other intellectual traditions and other intellectual communities. The process of extension of his theoretical scheme to subject-matters different from that to which he himself applied will illustrate the process of the growth of knowledge which opens up to his successors.

Other Fragments

Editor's Note: The end of the existing typescript contains several passages which were largely replaced with text above that develops the theme in a different manner. Perhaps something of Professor Shils' method can be perceived in the comparison with these rejected passages.

A

Although, in the last decades of his life, Michael Polanyi stirred in the direction of the Christian understanding of divinity, it was in fact a variant of his earlier analysis as the scientist's striving to disclose a hitherto unknown reality in the existence of which he is confident. The traditions of the modern Western intellectual lie for the most part outside the traditions of scientific rationality and the tradition of the approximation to the unknown divinity. (There is of course an affinity between the tradition of scientific rationality and the tradition of scientism but Polanyi rejected out of hand the concrete political results of adherence to the tradition of scientism.)

Polanyi refused to have any connection with many of the traditions with which intellectuals outside the scientific community are sympathetic just as he would not accept the political version of scientism -- e.g., planning -- which some of the scientists accepted.

B

I have undertaken a very difficult task here. It is to account for the absence of certain ideas from the thought of a person who developed so richly closely related ideas. I do not wish to make a sociological or psychological

interpretation of Michael Polanyi's inadequate perception or understanding of the hostility in a great variety of forms of so many Western intellectuals to their own society. This is a very complex and differentiated matter and it would be wrong for me to suggest that I have adequately described and analyzed the phenomenon. I did however know Michael Polanyi, for a long time discussed many matters with him and have heard him speak freely among friends. I can say that this hostility came into the forefront of his attention only relatively late in his intellectual career.

The question why was it so marginal to him? One answer is that he was interested primarily in discerning and promulgating the constitutive and preconditions of creative scientific activity. An affirmative attitude towards the discovery of truth, the critical acceptance of tradition and of the institutions through which the traditions of science are maintained and improved interested him above all, once he ceased doing scientific research himself. Although towards the end of his life he interested himself in aesthetics, it was less out of an interest in understanding works of art than it was of extending his epistemological theories to artistic creativity.

The fact is that the hostility of modern Western intellectuals to their own society, although it is a very tenacious and widely accepted tradition is of secondary importance in intellectual creation. It is true that it enters into the substance of novels and poems and of works in social science but it is really irrelevant to their intellectual and aesthetic value. That hostility is a foible as far as serious intellectual activity, like the natural sciences; it is not of significance in the production or assessment of scientific works. It is of interest to a sociologist attempting to understand the attitudes and role of intellectuals in modern societies but it is of little significance in dealing with the topic which interested Polanyi above all others.

When the matter became urgent to him after 1956, he could not really master it. The strength of his writing about scientists is that he had spent most of his life among and had an intimate understanding of them. He did not have similar relationships to literary, publicistic and social scientific intellectuals, and he was not greatly interested in their works. He did not have enough familiarity with them to do what he had done for scientists and scientific works.

C

What Michael Polanyi did for the community of natural scientists is indeed susceptible to extension to other kinds of intellectual activity.

Note: There now appears a two-sided task: one is to delineate precisely the way in which the authority of a scientist works through the precipitation of penumbra tradition; two is to delineate the extension of the authority of a scientist beyond his strictly scientific role as a working scientists -- but as a center of institution counsel and governmental advice.

The scientific community and each sub-community within it is hierarchical or in other words attentive to a center. There are official institutions and whole institutions in the scientific community which are central not so much because of their power to allocate funds, to make appointments and to accept works for publication but because of the achievements of their incumbents. "A scientist is granted exceptional influence by the fact that his opinion is valued and asked for" (*SFS*, 48). This is the way in which the scientific community forms around its centers. The centers are exemplary in a very general way; they are consultative and advisory. Their imperative powers are generally very limited.

D

This order, a product of consensus about the value of scientific knowledge and of the ways of its attainment is only a part of the very diverse stratum of intellectual activities in any large society. Scientific activities are only a part of the stratum of intellectual activities and the natural scientific activities are only a part of the stratum of scientific activities.

Such consensus as exists within the vaguely effective boundaries of the natural sciences does not obtain in much of the social sciences. It does not obtain either in the sector of those intellectual activities having to do with the production and assessment of literary works of the various genres nor in the production and assessment of artistic works or of that field of intellectual activity which consists of observations and assessments of events in the political or public sphere -- overlapping with the social sciences.

In all these fields we find effective traditions -- there would be very little accomplishment in any of these fields without the availability and observance of traditions.

Endnotes

¹*Science, Faith and Society*. (Riddell Memorial Lectures. Eighteenth Series). University of Durham. (London: Oxford University Press, 1946). All citations use pagination in the more commonly found 1964 University of Chicago Press reprint.

²Delivered at a Conference on the Rationality of Press Associations in Oxford on 26 July 1948 and published in *The Rationalist Annual* (London: The Rationalist Press, 1949). Reprinted in *Conjectures and Refutations: The Growth of Scientific Knowledge* (New York: Basic Books, 1962), pp. 120-135.

³Polanyi interjects parenthetically: "Perhaps a modern version of the Apostolic Succession."

⁴Editor's Note: a reference to Karl Popper's world of statements in themselves in contrast to the world of physical objects (World 1) and the world of subjective experiences (World 2). See Karl Popper, *Unended Quest: An Intellectual Autobiography*, revised edition (LaSalle, IL: Open Court, 1982), pp. 180-87.

⁵Polanyi here quotes the Church Fathers: "*fides quaerens intellectum*" (SFS, 45) -- "to believe in order to know."

⁶ See *USSR Economics* (Manchester: Manchester University Press, 1935) and *The Contempt of Freedom* (London: Watts, 1940).

⁷ See *Ethics* about 1950. Editor's Note: Probably "Virtue and Knowledge: The View of Professor Polanyi?" *Ethics* LIX: 4 (July 1949), 274-284, a review of SFS (1946) and *The Foundations of Academic Freedom*, Occasional Pamphlets of the Society for Freedom in *Science*, No. 6 (Oxford, 1947).

⁸ Leo Strauss also seems to have believed this.