Polanyi and the concept of well-ordered science

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Polanyi had an abiding interest in the social aspects of science. In 1941, in the midst of World War II he wrote a paper entitled “The Growth of Thought in Society”, in opposition to the views of British Marxists, notably J. D. Bernal, a well-known scientist (Polanyi 1941). He argued for the freedom of science to pursue its own ends. At the same time, he understood that the relationship of science to the larger society depends in large measure on society's acceptance of science:

> "Professional scientists form a very small minority in the community, perhaps one in ten thousand. The ideas and opinions of so small a group can be of importance only by virtue of the response which they evoke from the general public. This response is indispensable to science, which depends on it for money to pay the costs of research and for recruits to replenish the ranks of the profession. Clearly, science can continue to exist on the modern scale only so long as the authority that it claims is accepted by large groups of the public. "
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> "I have shown that the forces contributing to the growth and dissemination of science operate in three stages. The individual scientists take the initiative in choosing their problems and conducting their investigations; the body of scientists controls each of its members by imposing the standards of science, and finally, the people decide in public discussion whether or not to accept science as a true explanation of nature. …any attempt to direct these actions from outside must inevitably distort or destroy their proper meaning (Polanyi 1945)."

This theme is elaborated upon in his "The Republic of Science" (RS) paper in 1962. Polanyi asserts that science advances by the spontaneous coordination of individual initiatives,

> "… 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-coordinated 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 (Polanyi 1962, 56)."

These quotes, and many others that could be cited, show that for Polanyi, science should set its own goals, methods and organization without reference to the dictates of the larger society, because the nature of science as a community of scholars operating in pursuit of truth about the natural world demands a high degree of autonomy. In RS he describes attempts made to place constraints on the directions of science in the Universities in England following WW II, and their eventual abandonment.

In the post-WWII environment in the US, the establishment of the National Science Foundation set the stage for a period of strong support for basic science, in which science operated with a great deal of autonomy, pretty much as Polanyi would have liked it. The underlying motivations for those policies, however, were that a strong science cadre was essential for national security. Later, the large investments in biomedical research began and led up to the present huge investment in NIH. Much of this research is targeted, but individual scientists have retained a great deal of autonomy in their pursuit of research goals, within broad areas. But science is today a much different entity from what it was in Polanyi's time. It is in fact much too big to operate in the sort of communal mode that P envisions. More research is targeted toward areas that promise to enhance formation of new technologies, in furtherance of economic growth goals. Furthermore, it is more deeply intertwined with many more issues of ethics, morality and public policy than in the past: stem cell research; nanotechnology safety issues; genetic modification of food products; product safety issues of all kinds; climate change - the list is long.

For these reasons, Polanyi's ideal of a Republic of Science immune from the quotidian concerns of society is no longer viable. It fails to take into account (a) the intimate coupling between scientific results and their rapid applications in the world; (b) the sheer economic influence of science; (c) the sometimes overriding influences of political forces; for example, with respect to environmental research (d) the degree to which scientific results have occasioned moral and ethical challenges.

Well-ordered Science

In his book, *Science ,Truth, and Democracy*, 2001, Philip Kitcher has responded to the current challenges of science's place in society with his idea of

"well-ordered" science (Kittcher 2001, Chapter 10). His primary points of departure in formulating his plan are:

1. Science cannot be considered in isolation from technology. There is no such thing as "pure" science.
2. Context-independent goals for scientific inquiry are not generally identifiable.
3. Science should be directed toward finding *significant* truths. The goals of simply epistemic significance (which would be primary with Polanyi) are not adequate or even coherent. What *are* the epistemic aims? (Kitcher, 66)

Kitcher proposes the concept of *Well-Ordered* Science, in which the activities of science are to be determined in three steps:

1. Commitments are made to particular projects in particular amounts.
2. The most efficient pursuit of the agreed-upon projects is determined, subject to moral constraints.
3. The ways in which the results of the studies are to be applied are determined.

Step 1. is carried out as deliberators who possess varying initial personal preferences confer, trade information and insights, assess the significances of various possible courses of action. This process results in the deliberators having *tutored preferences.* The result is a list of items the deliberators want advanced, informed by the deliberators' clear understanding of the sources of significance (Kitcher, 119) However, Kitcher does not indicate that these deliberators are to have scientific training. In fact, quite the opposite seems to be indicated.

Step 2 consists in identifying the ventures that can deliver what the deliberators want. (Only at this point does Kitcher make a role for "experts".)

Step 3 consists in determining budgetary levels and research agendas. Here Kitcher envisions a role for minority views in determining lists and distributions to various endeavors. (For example, a minority that might object to embryonic stem cell research would somehow have its voice heard at this juncture.)

Critique of Kitcher's proposals

It is possible to raise several objections to Kitcher's proposals:

1. Only scientists can judge which problems, among the countless available for study, are most likely to yield to currently available approaches. Putting together a list of desirable outcomes of scientific work is relatively easy; relating those to reasonable pathways for directing scientific resources is much more difficult.
2. Kitcher argues that there is no overriding right to pursue knowledge for its own sake, quite independently of the consequences. But whether there is a right, not necessarily inalienable, to do so, it should be asked whether it is not in society's *best interests* to foster disinterested pursuit of basic discovery. It is undeniable

that many of the most important applications of science to society have had their origins in serendipitous discovery, the products of a free and unfettered study of nature.

3. It is impossible in advance to choose projects that will yield important new insights, that will open up new frontiers.

4. How can science be well-ordered in the ways that Kitcher envisions, in a society that is not itself well-ordered in so many other ways?

5. There is no recognition on Kitcher's part that the vast majority of citizens in society lack sufficient understanding of science as a working enterprise: how science gets done, the limitations of science, the ways in which the scientific community operates to produce something that can be called "scientific opinion", after Polanyi.

6. Kitcher does not recognize the various ways in which society already imposes a host of organizational, funding and policy constraints on science. Many special interests, from many sectors of society, have inputs into government decisions about which areas of science are to be emphasized, which are to be funded, and at what level. In addition, policy moves that place constraints on the performance of some kinds of research are arrived at through processes that are responsive to demands from one or more interest groups. Kitcher needs to critique the present system as it actually operates before attempting to put forward an elaborately organized plan that might address only some small part of the total picture as far as ordering science is concerned.

Cartwright and "warrant for use"

In her paper, "Well-ordered Science: Evidence for Use", Nancy Cartwright begins by subscribing to the arguments of Philip Kitcher: " ..the most important demand that we should make of science is not that it be accurate, progressive, or problem solving (or whatever are your favorites from the traditional list of scientific virtues). But rather that it be well-ordered and that it answer the *right* questions in the *right* ways, where value judgements and methodological issues are inextricably intertwined in determining what is *right* (Cartwright 2006)."

Cartwright's focus in her paper is on issues of methodology, " …specific questions about particular methods, their range of validity, their strengths and weaknesses and their costs and benefits."

By evidence for use, Cartwright means something like the following: "Philosophers of science tend to buy into the Positivist/Popperian picture of exact science: into the view that science can and does establish *stable, unambiguous* results, "off the shelf" results that are warranted and once

warranted can be put on the shelf to make them generally accessible, from whence they can then be taken down and put to various uses in various different circumstances. (Cartwright 2006, 983)"
She thinks that what a claim means in the context in which it is first justified may be very different from what it means in the different contexts in which it will be put to use. So, "What justifies a claim depends on what we are going to do with that claim, and evidence for one use may provide no support for others."

She argues, quite unexceptionally in my view, that natural science results, while warranted by natural science methods, cannot be implemented in the larger world except through social processes of one sort or another. And in those processes the pure scientific warrants may become irrelevant or at odds with the goals of the implementation, lacking some interpretations and modifications.
She talks a bit about the difficulties in coming to a full and satisfying solution to some problems, which on the other hand may yield to intuitive solutions. She references Gigerenzer's idea of "cheap heuristics", which looks a lot like Polanyi's tacit knowledge (Raab and Gigerenzer 2005)
Cartwright does not attempt to justify or even directly support Kitcher's ideas for well-ordered science, but she does seem to emphasize the idea that it "answer the *right* questions in the *right* way" without indicating who is to decide what those questions are, what right ways are to be employed. But there is in her overall message a challenge to Polanyi's assertion of the autonomy of science.

Some Observations

The foregoing discussion leads me to the following observations:
1. As an accomplished scientist, Polanyi had a deep sense of how science is actually performed. He nourished in his breast the conviction that scientists comprise a special, elitist and powerfully effective community of scholars committed to the unfettered pursuit of knowledge:

"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 (Polanyi 1962, 67)."

By contrast, Kitcher's plan does not reflect a deep sense of how science is actually done. It would have the effect of virtually foreclosing much adventitious discovery and subsequent development.

2. Kitcher's plan leaves little room for the formation of 'scientific opinion', a cornerstone of Polanyi's model of the scientific community as a source of authority:

"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 functions 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 (Polanyi 1962, 68)."

3. Polanyi's "Republic of Science" has more porous boundaries than in the past, for both for good and ill. Nevertheless, his ideas about how scientific truth is made, his recognition of the fundamentally social nature of the scientific enterprise, and the necessary grounds for the establishment and maintenance of a scientific authority, remain as solid as ever. What cannot remain is the sense of a community isolated from the larger society in terms of justifying its aims and practices, and closed to inputs from the larger society as to what science must do to fulfill its communitarian obligations. We are left to deal with a continuing source of tension between science and society.

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