Chapter 13
The Tragedy of the Liberal Theory of Science

Stephen Turner

Abstract The Liberal Theory of Science, best articulated by Michael Polanyi, held that science advanced when autonomous scientists followed their best hunches and spontaneously coordinated their efforts as a result of their mutual dependence, in a setting devoted to scientific truth with a tradition supporting it, in a quest for a comprehensive understanding of reality. Pure science was for him an international community with the characteristics of the Republic of Letters of the past. This image of science was an idealization at the time he wrote, and became less applicable as science grew and became more dependent on funding decisions, and therefore also on peer review, which compromised autonomy. From the painful attempts to make sense of these changes, particularly in the writings of John Ziman, we can reconstruct the effects of these changes: initially from a focus on discovery to the production of "reliable knowledge" for sponsors. This change made science more useful, but at a price, and made the Liberal Theory of Science increasingly irrelevant to actual science, but still useful as a basis for critique.

Keywords Liberal theory of science · Michael Polanyi · John Ziman · Impact statements · Finalization · Autonomy of science · Peer review

13.1 Introduction

Michael Polanyi was the primary and most lucid articulator of what can be called "the Liberal Theory of Science" (Beddeleem, 2017, 2019, 2020, unpublished ms.[1]).

[1] Michael Polanyi: from academic to cultural freedom. Unpublished manuscript. https://www.academia.edu/25225200/Michael_Polanyi_From_Academic_to_Cultural_Freedom

S. Turner (✉)
Department of Philosophy, University of South Florida, Tampa, FL, USA
e-mail: turner@usf.edu

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What made this theory "Liberal" was the idea of autonomy: that science depended on individual free assent to the scientific claims of others, and on the ability of scientists to debate freely. Like Liberalism generally, it did not deny the need for some constraints, but preferred that these take the form of informal norms, mutual adjustment, and self-discipline, and also preferred indirect to direct means of governance. It distinguished "pure science," and the idea that science set its own goals, from applied science and technology, where the aims were determined by practical needs.

The explicit formulation of the central ideas of this theory was the result of an immediate conflict, and a particular moment in the history of science. There were variant formulations of these central ideas. Karl Popper (cf. Jarvie, 2001) and James Bryant Conant were also prominent contributors to the Liberal Theory.[2] They were motivated to speak on the nature of science in part in response to the Marxist view of science, an amalgam of ideas that coalesced in the 1930s. Nikolai Bukharin (1931), the official Marxist source, treated all thought as a superstructural element of the class struggle, and had long regarded the idea of pure science as bourgeois ideology (Bukharin, [1925] 1969). The Marxists regarded technical practice and needs as the motor for scientific development. A large group of scientists and science commentators promoted this interpretation and, like the Soviets (Hessen, [1931] 1971, pp. 145–208), used it to reinterpret the history of science. Historical materialism was "science" for Bukharin and the followers of this view of science, and its methodological precepts were theirs as well: science was something that could be codified, made authoritative, and guide society.

The view on the Left evolved and produced variants that emphasized different elements, particularly after the Lysenko affair. In the 1930s it was nominally about planning, the popular idea of the time. But its most enduring legacy was a conflict over the nature of science itself. The doctrine that eventually developed based on this, associated with what came to be known as the Social Relations of Science movement, was elaborated by scientists such as J.D. Bernal (1939), Lancelot Hogben (1938), and the science journalist J.G. Crowther ([1941] 1967). The key to their thinking was the idea that capitalism was at the late stages of its existence. Science, as exemplary of the means of production, was being restrained—frustrated, as they put it—by capitalism's failure to utilize it, resulting from the subordination of science to finance. This restraint would be released in a communist economy. This was a standard application of the idea that the forces of production in the later stages of class domination under capitalism were in conflict with the relations of production, holding back the beneficial realization of the forces of production. Technology was the domain of the modern proletariat and the subordinate classes of the past; their practical knowledge was the true foundation of science. Where

[2] Robert K. Merton's variously titled paper on the norms of science played a more ambiguous role, but to the extent that it also promoted the idea of the informal self-governance of science, it was liberal. But it avoided issues of the content of science and the nature of scientific progress, and took the view of the left at the time that science would only be fully utilized under communism. (cf. Turner, 2007).

scientists listened to workmen, as when Galileo discussed pumps with one of them, progress occurred (Hogben, 1938, p. 368). More generally, science advanced as technological advances responded to human needs. Moreover, in the modern world, as Bernal and the large Left wing scientists movement insisted, scientists were workers not autonomous agents: “The scientist is not, if he ever was, a free agent. Almost universally he is now a salaried employee of the State, of an industrial firm, or of some semi-independent institution such as a university which itself depends directly or indirectly on the State or industry” (Bernal, 1939, p. 387).

Their version of socialism, however, was not a mere matter of public ownership of the means of production—which they characterized as the false theory of German Social Democracy that was based on a static view of the economy and technology. Instead “The primary task of constructive democratic statesmanship is not to transfer property rights in existing industry from private to public. It is to devise the machinery of social ownership to exploit new technical resources made available by State subsidized research” (Hogben, 1938, p. 1083). Hogben claimed that “from about 1880 onwards, technical improvements of industry have been largely due to *discoveries made in laboratories supported from public funds* by such men as Faraday and Henry (see: pp. 713–14, Hogben, 1938, p. 1084; emphasis original). The names signaled the fact that “pure and applied science are not independent social phenomena. They are inextricably linked as shoot and root in the process of healthy growth. Growing science is the unity of theory and practice. Without its roots firmly planted in the moist soil of social practice the green shoot of pure science withers and becomes the dead trunk of metaphysics. But the green shoot of pure science is needed too, because without it “the root of applied science degenerates into the dry wood of empirical repetition” (Hogben, 1938, p. 1078).

Contempt for liberal democracy was central to their vision: “Democracy will not be salvaged by men who can talk fluently, debate forcefully, and quote aptly,” because “[i]t is the age of the engineers” (Hogben, 1938, p. 1089). But there were many ambiguities in their view of the democracy they were salvaging. What role the “engineers” were to have in the state was a long-running puzzle. Some of what they said resembled guild socialism, and the idea of governance through the co-operation of unions acting collectively in developing a plan, that scientists, also acting collectively and in co-operation with other guild-like unions would co-ordinate. The problem of the relation of science to politics was reduced to slogans like this: “modern science offers us a New Social Contract . . . that the sufficient basis for rational co-operation between citizens is scientific investigation of the common needs of mankind, a scientific inventory of the resources available for satisfying them, and a realistic survey of how modern social institutions contribute to or militate against the use of such resources for the satisfaction of fundamental human needs” (Hogben, 1938, p. 1089). But the model of the Soviet Union and its form of rational co-operation loomed large, and these writers made little effort to critique it: Hogben called the authors of this new social contract “the founders of a new social culture” (Hogben, 1938, p. 1090), underlining this affinity to the new Socialist Man of Soviet society. This proved to be a rhetorical weakness once Lysenkoism, which appeared to exemplify the model of theoretical science rooted in and growing out of practical discoveries, was discredited, and held out as an example of what happens when the process of criticism is suppressed, and pure science vilified. But it also made it clear that "culture" was the issue.

13.2 The Liberal Theory and the Messy Reality

Michael Polanyi was the architect of the most complete and most polarizing account of the liberal alternative. In what follows I will concentrate on his texts collected as *The Logic of Liberty* (1951). The first item, "The Social Message of Pure Science" (1946), was given at the 1945 conference of the Society for Freedom in Science in anticipation of the return to the idea of planning. When he gave it, however, he discovered to his surprise that this battle was won: the crusade for planning ended with the war. The next battle, which will be my concern in this paper, was more difficult, and it is fair to say it was one that he lost, though the character of the loss was not fully apparent at the end of his life, nor even today.

In 1933, J. W. N. Sullivan could write that "the matters with which science has hitherto been concerned are comparatively indifferent to us. For that reason, science has been so successful. … The splendid moral integrity manifested in scientific work, therefore, is due very largely to the nature of scientific material. … Science is truthful because it has practically no temptation to be anything else" (Sullivan, 1933, pp. 174–175). In the wake of Nazi physics, the atomic bomb, and the example of Lysenko, this was no longer a tenable view. Science was no longer a matter of indifference, and there were evidently temptations to be "something else" than the pure pursuit of truth. Polanyi's response to this new situation was to focus on the morality of science as a community: he appealed to the need for goals that were higher than mere comfort, that, as he quoted Clement Attlee, was a need for spiritual goals, and held out science as a model, "an example of the good life." Polanyi regarded science as a worldwide community that embodied higher ideals: "Spread out over the planet scientists form … the body of a great and good society" (Polanyi, 1951, p. 6).

The ideal of purity nevertheless co-existed with the idea of the social utility of science—the idea that by doing science one was doing something generally beneficial. From the start the atomic bomb caused a great deal of soul searching precisely because of this idea. The fact that this difference in ideals was maintained for so many years, and didn't appear to conflict, is remarkable. To be sure, the Left view of science prioritized service to society and technology over pure science. But the fact that Polanyi could articulate the ideals of pure science convincingly in 1945 indicates that the ideal was still alive in the hearts of scientists. But from there on in the story gets murkier.

The famous book by Vannevar Bush, *Science, The Endless Frontier* (Bush, 1945), managed to reconcile the two images of science, and set the tone for the way in which science and science policy was conceived for the next 30 years. But it did so by turning Hogben's image of pure science as a green shoot and social practice as the root upside down. The central idea of the book was that basic science, which Bush understood as unfettered free inquiry, as Polanyi had, was crucial for producing the practical benefits of science. Applied science turned the discoveries of basic science to practical ends, and led to technology that incorporated the results of applied science. On the surface, this was a strong argument for basic science. But it reduced the justification of basic science to its ultimate outputs in technology. As one commentator put it: “Perhaps deep in his psyche, basic research was important to Bush for its beauty and transcendent qualities, but in reading his three arguments for federal science, he believed it was crucial for American survival” (Thorpe, 2020, p. 227). Bush affirmed free inquiry on the same grounds as the Marxists gave for planning: that science’s utility needed to be unleashed, but he argued that this required basic science to prosper. Basic science was understood in “Liberal” terms. The rest of science was not. Polanyi made the same distinctions. Basic science was not necessarily university science: results in physics were arrived at in corporate labs, incidental to the pursuit of practical results, and Polanyi’s own lab in Germany had been non-academic, and “basic” physicists were often impressive gadgeteers.

What was “Liberal” about this account of science? In the first place, in multiple ways it was individualistic. The account of scientific discovery Polanyi spent his life constructing was about knowledge that was in the first instance “personal,” meaning that discovery was a matter of the creation of a personal construction, of finding a pattern where others had not found one, and building this into something that could be communicated to others. It was social in the sense that the community of other individual scientists exercised constraints on the scientist, but in the end, acceptance of a fact was personal (Turner, 2023a, pp. 188–189). “Discovery” played a large role in this theory: it was a kind of autonomous standard that did not depend on the acceptance of peers, though acknowledgement was an important goal of science. Both Polanyi and Conant, for slightly different reasons, emphasized the fact that scientific advances could be born out of time and lie unrecognized by the community for a long time. This underscored two points: the community was not the ultimate standard of science or the goal of science. Truth was something higher than inevitably temporary consensuses. And science depended on the community’s recognition of the higher goal of truth-seeking, and on its honoring this goal in its communal practices.

13.3 Autonomy and Its Rise and Fall

Universities were in low repute in the period of the Republic of Letters, but when they revived, led by the German Universities and by Wilhelm von Humboldt, Friedrich von Savigny, and Justus von Liebig, they incorporated the ethic of truth-seeking and discovery. With the development of professorships, based on reputation, with a high degree of autonomy, legally protected academic freedom, a hierarchy of students and assistants, and typically a publication series, there was now a powerful material base for this system. But—there was a price to be paid for integration with the state, including various exclusions of scholars of the wrong ethnicity or religion. Nevertheless, this price was mitigated by the many constraints placed on the state by the market-like competition for prestige that developed. This system spread, slowly and with many local adaptations, throughout the world of higher education in the nineteenth century, leading to the system of "research universities" in the United States that solidified around the time of the First World War.

The academic ethic that emerged internationally followed the basic outlines of the Republic of Letters, with some specific aspects that were especially prominent in science. Talent was individual; high talent was rare and to be honored and respected. One had an obligation to talent that included fostering it and making it possible for those with talent to achieve and fulfill it. There were also some assumptions. Discoveries were more or less autonomous achievements, not a matter of opinion, and were recognized, very often, by prizes. Ideas could be born out of time, and not recognized as true for decades, but eventually science caught up and recognition occurred, and there was an obligation to honor discovery, as the statues erected to scientists testified. Patience was essential not only for achievement, but for recognition and rewards. This applied to the people funding science as well: flourishing free science was the best; demanding that science produce immediate practical results backfired.

Freedom in science, for Polanyi, meant autonomy: "scientists, freely making their own choice of problems and pursuing them in the light of their own personal judgment." But he added to that, and made it his task to explain, how they "are in fact cooperating as members of a closely knit organization." This was close to the language of the Left: co-operation was central to science. But Polanyi's point was that this was not directed or planned. The basis of science as a model case of the "spontaneous order of a closely knit organization" (Polanyi, 1962, p. 54) was that scientists constantly adjusted to one another's results. This was thus far from anarchy. His picture of what is allowed in the name of science was quite conservative. Moreover, the idea that scientists were to have the power to decide what to study and how was problematic: academic freedom was one thing: research that required funding depended on the judgements of others.

The idea of "scientific opinion" as a controlling force in appointments and other decisions was critical to this account. But there was an "internal tension" between the standards scientists used to judge of scientific value, especially between plausibility, and originality:

> Both the criteria of plausibility and of scientific value tend to enforce conformity, while the value attached to originality encourages dissent. This internal tension is essential in guiding and motivating scientific work. The professional standards of science must impose a framework of discipline and at the same time encourage rebellion against it. They must demand that, in order to be taken seriously, an investigation should largely conform to the currently predominant beliefs about the nature of things, while allowing that in order to be original it may to some extent go against these. (Polanyi, 1962, p. 58)

These were limits on autonomy not found in the Republic of Letters. From the start he acknowledged what we might call political or administrative restrictions and limits on autonomy.

First, it was restricted to qualified scientists. Second, he acknowledged that decisions to support science and scientists had to be made, and usually were made by senior scientists. In the post-war era he likened them to Plato's guardians—who were free from political interests and family connections. But he acknowledged the possibility of the corruption of science as well.

Funding was the worm in this apple. In 1953 Edward Shils, who had been involved in the postwar efforts of the atomic scientists to control weaponry, and who would later edit the science policy journal *Minerva,* reported from the Hamburg Conference of Scientists and Intellectuals that the problem of government funding of science was a great conundrum (Shils, 1954). The favored solution by Polanyi and Conant, of funding the person and granting complete freedom, was an established model. As Melinda Baldwin notes, "The German Research Foundation, created in 1920 and initially called the Emergency Association for German Science, deliberately chose to rely on a small number of elite scientists for opinions on grant proposals, and much of the evaluation focused on the personal qualities of the applicants" (Baldwin, 2018, p. 544). This was more or less the system adopted in Britain by the Medical Research Council and later by the US National Institutes of Health.

The National Science Foundation of the US was created in the course of a political conflict with Senators who proposed funding science on the established model of funding agricultural research—distributed to the various states and highly responsive to local needs. Scientists resisted the idea of non-scientists governing the foundation, which initially led to the legislation being vetoed as undemocratic. But the concern for accountability was also more acute for pure science, where the practical value was less obvious, so during this early Cold War period, "various stakeholders sought to navigate a growing tension between desires for scientific autonomy and public accountability in controversies over government science funding (Baldwin, 2018, p. 539). Peer review was a solution to this problem: it relieved administrators of personal responsibility for decisions to fund grants, and bureaucratized the process in such a way that the issues with decisions could be turned into impersonal procedural questions.

The system that emerged was consistent with Polanyi's image of science, and he also thought it was optimal for science, "So long as each allocation follows the guidance of scientific opinion, by giving preference to the most promising scientists and subjects, the distribution of grants will automatically yield the maximum advantage for the advancement of science as a whole. It will do so, at any rate, to the extent to which scientific opinion offers the best possible appreciation of scientific merit and of the prospects for the further development of scientific talent" (Polanyi, 1962, p. 61). Scientific opinion was a political myth, in the sense that it justified the procedures of peer review, but could not be ascertained independently. Moreover, it was a concept better suited to the question of scientific truth than to the very different kinds of questions raised in the task of making prospective judgements on grant proposals. By the 1980s Shils was able to say that the peer review system had proven itself and was not a problem (Shils, [1982] 1997).

Like any system, it created winners and losers, but was also run by the winners. The balance between discipline and originality that was supposed to be arrived at unproblematically, had devolved, under the pressure of competition, into a situation in which risk taking was minimized and grant proposals essentially had to guarantee results. The standing joke at the time was the scientist who tapped the already finished paper on his desk and said "I got the grant, now I can publish." Eventually, people began to speak out against the system for stifling innovation. Richard Muller, who had won the Waterman Prize at the National Science Foundation and the Texas Instruments Foundation's Founder's Prize, used the occasion of his award to explain how the system was preventing work like his from being funded. After describing a large number of burdens that had come to be imposed on grantees in the form of paperwork, restrictions, and counter-productive rules designed to avoid waste, Muller adverted to the Liberal Theory of Science when he said "We cannot obtain all the benefits of a 'free enterprise' system in science while maintaining public funding" (Muller, 1980, p. 883), meaning that the risk taking and openness of science was undermined by the bureaucratic system he complained about. But he thought some ways of encouraging risk taking could be built into the system, and that an ethos supporting risks, and therefore failure, could be promoted. He appealed again to the idea of supporting people, not projects. But he also acknowledged that the government had the right to channel research and demand results. He only hoped for some acknowledgement and support of the kind of risky basic research that could not be fit into the mold of the system as it had developed.

These were, so to speak, internal complaints about science from the point of view of the frustrated practitioner. But gradually it dawned on commentators on science that something fundamental had changed. In Britain, physicist John Ziman was far more brutal. Bernal, he argued, had won. Science was now collectivized.

> Within 40 years, J. D. Bernal's radical commentary on "the social function of science" [1939] has become the conventional wisdom of public science policy. It is now commonplace that science should be organized and financed on a large scale, and directed towards societal goals. This policy also includes basic research, which needs financial support for expensive apparatus. Science is thus being transformed from an individualistic community into a homogeneous collective enterprise, which now covers all types of research from the academic to the technological. (Ziman, 1983, p. 213)

Ziman recorded the same issues as Muller. But he posed the problem in terms familiar from Polanyi's debates with Bernal:

> Tension between the individualist norms of the academic tradition and managerial principles derived from the industrial tradition has made research an ambivalent profession. Should scientists be regarded as members of a transnational community devoted to the search for truth, or are they simply typical employees of governmental and commercial organizations with very worldly aims? (Ziman, 1983, p. 213)

Muller and Ziman were the products of what remained of the *ancien regime*. Muller was a student of Luis Alvarez, himself something of a rule-breaker and independent spirit, who supported him and encouraged his innovative streak. Ziman had been a Communist party member during the war. They were transitional figures, and clearly understood that they were living through a major change in science, whose dimensions were difficult to grasp.

13.4 The Great Change

What had changed? Muller's and Ziman's comments deal with various aspects of the problems and constraints faced by scientists. Muller outlined the bureaucratic challenges: the fact that scientists had lost much of the freedom they had when money was more generous and time less accountable, as well as their ability to choose strategies, for example, to teach a course to get up to speed on a topic rather than service a grant, or to spend time wool gathering and being able to accept the risk of time spent coming up with novel ideas of which few would survive scrutiny. Ziman responded to the idea of Mode II research; the notion that interdisciplinary teams focused on specific problems were going to replace much of traditional academic science. These organizational changes required different skills, and were episodic rather than cumulative in the way a traditional academic science career was. One moved from the category of autonomous researcher to the category of expert contributor. He wondered about the effect of sponsorship, and the dependence on agencies and corporations with their own agendas.

What is striking about these comments, and the many complaints about the development of science that followed in the next 40 years, is how diffuse these changes are, and how difficult it is to get a grip on them. Each criticism and concern focuses on a valid issue, yet they all seem to be very partial—reforming the peer review system to make it easier for innovative research by giving program directors more discretion, for example. And there is a pervasive ambiguity about whether the way science developed may merely have been inevitable, and for the most part a good thing. Ziman in particular was careful to admit that Mode II knowledge was an expansion of the value of science.

What haunted this discussion was a sense that this might be all there is to science—that we had reached a point where the knowledge being produced by science under these new arrangements was the only knowledge that science could contribute. Their concern was that science was over, other than the application of science and problems solving related to technology, drugs, and integrated approaches to interdisciplinary problems. This thought was articulated at the time in Germany in the "finalization debate," which argued that science had exhausted its "exploratory" tasks, had "finalized" basic science, and should now be understood as a project of application, and funded accordingly—for social ends (Böhme et al., 1983).

Ziman worried about science losing its objectivity, given the changes, and this raised interesting questions. The results of an interdisciplinary team solving a problem may be "science," but this is not science in the sense of basic science. And in the 40 years since Ziman wrote, more and more science has taken the form of constructing usable models to approximate poorly understood complex phenomena for the purpose of intervening rather than establishing fundamental principles that adjacent fields of science can rely on. The team aspect, even in the case of basic science, also changes the character of science. A CERN experiment with a thousand collaborators involved as "authors" of a paper announcing a discovery or the result of a complex microphysics experiment is a radical departure from even the team physics described by Muller.

But these changes in science did not take the form of a revolution, despite attempts to reconstruct them in this way. They were incremental changes: changes in what science became important, changes in funding sources and the contingencies of organization produced by these changes, and, as Muller pointed out, changes in paperwork justifying what the scientist was doing. Changes affected each field, but each field changed in different ways, and for different reasons.[3] The Liberal Theory of Science, in retrospect, was a theory of what Ziman called academic science. Academic science itself, however, became something different. While institutions like the textile lab that Polanyi ran in Dahlem were not strictly speaking academic, they were adjuncts to a larger academic field that validated and provided a tradition for their work. In the last 40 years, this relation has gradually reversed. In the fields in which there is a great deal of private money, notably pharmaceuticals, academic institutions have become the poor relation, the adjunct to corporatized science, and have themselves corporatized. They produce for a market of funders and investors, produce graduates for this market, and evaluate themselves in terms of success with these markets.

Separating the elements of change and finding a single "cause" is not profitable, but it is possible to identify re-enforcing trends. The largest change involves the systematic replacement of informal and personal mechanisms of judgement, the relic of the Republic of Letters, with bureaucratic procedures. Peer review is an amalgam of both personal judgement and procedure. But increasingly the work of evaluation is based on quantitative indicators, such as citation counts, that have come to be pursued as ends in themselves. One suspects that the new prevalence of questionable science, retractions, high profile ethics problems, and the like are in some way a result of the dissociation between the everyday practice of science—the achievements and discoveries on the lab bench, and the external demands on scientists (Schott et al., 2010; Edwards & Roy, 2017; Heesen, 2018; Tiokhin & Derex, 2019; Haven et al., 2020; Lakens, 2020). Sullivan's point quoted earlier was that there was no incentive to cheat in science. This was never entirely true. But the creation of quantitative measures provides new opportunities and new incentives to do so.

Polanyi described a world in which the two matched up; personal esteem, personal relations of mentoring, conviviality, and a kind of bonding between researchers and also their rivals, were the characteristic features of science. Muller's descriptions of his relations with Luis Alvarez (Muller, 1980, p. 882), his protector and mentor, have this sense of seamless relatedness between the thinking part of science and the world of the scientists' personal relations. This, for Muller, was the meaningful community—not the pursuit of the awards he received. But Muller

[3] It is noteworthy that the changes in the National Science Foundation review process, which gradually put more emphasis on impact, were routinely claimed by the administrators to not be changes at all, but merely specifications of what had always been the case (Rothenberg, 2010).

well recognized how unusual this community was, and how difficult it would be to produce for younger scientists in the future. And his outline of what conditions were needed to innovate, and what reforms were needed, is indicative of what was already, by the time he wrote, no longer the case in science.

Muller's point was that the scientist needed time to come up with new ideas, most of which would not work out, and allow them to be tried out, at least in thought. As Bauer notes, "Charles Townes, who developed masers and lasers, gave a similar talk at the same time on the occasion of an award, [and] testified to his difficulty in getting research support for his ground-breaking work, or even encouragement from some of his distinguished older colleagues" (Muller, 2018, n4). Both Muller and Townes noted the conservative effect of peer review—that one bad review would kill a proposal, and that innovative ideas needed to be developed under the radar—something Alvarez, who had done this himself, allowed, but was increasingly difficult under the tighter financial controls over grants, and the decreased slack given by them. Wisdom, in the new mode of science, was to stick with work that guaranteed results.

This is an important clue to what changed. There is a difference between producing innovative science on basic topics and producing technicians—experts, as Ziman styled them—who have adapted to the demands of the grant system or the pharmaceutical research market and can compete with others to produce what these markets require. There is a traditional distinction between "contract research" and research based on the initiative of the researcher. Freedom, under the grant system, had been redefined as freedom to propose research for funding in a competitive system with peer review. But the pressures of competition, and the changed character of the grants and corporate funding on offer, have brought the two closer together. The "initiative" of the researcher is, to an increasing extent, initiative in anticipating the vagaries of the review process, the preferences of the funders, and so on. And this no longer is a separate personal quality of grantsmanship, but an ingrained part of scientific thinking. Nor could it be otherwise: competitive science requires competitive technology, competitive post-docs and technicians, and division of lab labor. All of this takes money and getting this money costs not only time and effort, but total commitment to the task of creating a project that can satisfy all these demands. Team science is expensive science. And teams themselves produce a kind of conservatism that the individual or very small group developing ideas is free from. The woolgathering Muller describes as essential to risky, innovative science is too much baggage for the new model scientist. Townes described his work as an uphill battle—and this was work done in the more relaxed era before 1980.

13.5 Post-autonomous Science

Polanyi's critics, once the "planning" movement died out, were quick to point out the ways that the internal politics of science intruded in decision-making supposedly governed by considerations of merit (Bernal, [1965] 1979, pp. 1296–1297). If these had receded during the flush years, they did not vanish. Grants committees said yes and no to proposals. Science was no longer largely funded out of university funds, but, in most cases, required equipment and staff that had to be paid for from funds that came from grants. The locus of administrative power in science had decisively shifted. Even editorial decisions did not have the life and death power that peer review committees of the major national funding agency had. There were many alternative journals in the biological sciences; in the physical sciences the journals had simply expanded to publish virtually everything, and without much risk: the research had been pre-screened by the funding process, and the specialized series that published a given paper mirrored the funding pool for that field.

Polanyi's ally and friend Edward Shils was focused on a related problem: appointments made to the University of Chicago on the basis of grants or the expectation of grants. He insisted, in a report dated 1970/1972, that "No appointments should ever be made in which the chief or major argument is that "outside" funds would accompany the appointment sufficient to relieve the regular budget of the cost of the appointment" (Shils, 1970/1972, p. 6). Of course that horse had left the barn: no appointment in science could be reasonably made unless the scientist was going to pay his way, however much this was obscured by the system of accounting. Science, especially grant funded natural science, was a money loser for the university (Holbrook & Sanberg, 2013). And increasingly the concept of "research" was equated with externally awarded research funding. University research offices publicized their successes, rankings were crafted, and grant-getting became an integral part of promotion, tenure. "Startup money" for new junior appointments, often in the hundreds of thousands of dollars, was typically financed out of overhead money in the expectation that it would be earned back in the form of more overhead money. Eventually people with tenure began to be fired, humiliated, or have their salary reduced for insufficient grant getting.[4] In some fields, appointments were designed so that the pressure to get grants would be increased; in others, the appointment was fractional, for a month or a year, so that a full salary was paid only if the incumbent funded it from grants.

Students and aspiring scientists grasped this system implicitly. To be an apprentice scientist meant that one was supported on a grant, and was working on one. Labs had hierarchies and a complex division of labor, and the bigger the lab, the more complex and hierarchical. One's work as an entering student was likely to be on a mundane and onerous task: preparing samples, running repetitive experiments, cleaning data sets, and so forth. One graduated to tasks with more intellectual content, but within the structure of the grant and its aims. And one looked forward

[4] https://www.detroitnews.com/story/news/local/detroit-city/2017/03/29/wsu-moves-revoke-tenure-professors/99758592/
https://www.timeshighereducation.com/news/imperial-college-professor-stefan-grimm-was-given-grant-income-target/2017369.article
https://www.columbiamissourian.com/news/mu-professors-overwhelmingly-support-reversal-of-tenured-faculty-pay-cuts/article_a343561a-8f53-11ec-8b9c-87a134970fef.html

to the next grant, to being written into that grant as a junior participant, something encouraged by the grant system as a baby step into the grant world, and eventually to the prospect of getting grants on one's own. The more important the scientist, the larger the lab. The scientist needed, both for prestige purposes and as the central part of the job, to keep a lab team together, to keep the technicians employed, and to support students, post-docs, and junior collaborators. The kind of output expected of the grant-holder required that they produce from the grant: that was something future grant applications traded on.

Ziman realized that this dependence was the death of the autonomy required by the Liberal Theory of Science: "Personal discretion in the choice of research problems is now severely limited, even in the university sector, because most projects are now funded by outside agencies" (Ziman, 1983, p. 1). The tables had turned. Academic science, the institutional basis for the Liberal Theory of Science, was now dead; the grant system had turned it into a form of dependence, out of financial necessity. And this meant a changed ethic.

> Ziman proposed for the norms that obtain in a corporate scientific enterprise, be it government or private, the acronym PLACE: Researchers nowadays get their rewards not by adhering to the Mertonian norms but by producing Proprietary findings whose significance may be purely Local rather than universal, the subject of research having been chosen under the Authority of an employer or patron and not by the individual researcher, who is Commissioned to do the work as an Expert employee. (Bauer, 2018, np)

However, Ziman himself struggled to characterize what science had become and settled on the idea that it produced "reliable knowledge" (Ziman, 1978). What he grasped was that science could no longer be understood in the traditional terms of "realism," generality, discovery, and so forth. Its achievements were valuable, but not the same as the achievements of the past. The kind of knowledge being sought had changed. But characterizing this change was difficult. It was still science, and successful science.

What was sought under the heading reliable knowledge was increasingly a matter of models and simulations, which focused on determining the effects of interventions in complex systems whose complexities were of no interest in themselves. These models were empirical and rigorous, but in a different sense. There was a saying in physics that if you needed statistics, you needed a better experiment. This was relevant if the experiments were about discoveries which raised new questions and pointed to new discoveries. The results that were now being produced were discoveries in a weaker sense. It was enough to show the effect—enough effect—of an intervention such as a drug or therapy to pass a bureaucratic test (Gabriel & Holman, 2020).[5] It was accepted that the systems being intervened in were too complex to be understood, with too many mechanisms, whose fitting together was impossible to fully understand, but that this didn't matter.

[5] I have discussed the limitations of statistical models of this kind in the social sciences elsewhere, and made the point that the use of these kinds of models in the natural sciences has similar limitations (Turner, 2023b).

Drug research was an example of this. It was enough for regulatory purposes to demonstrate effectiveness, statistically, and to show that side-effects, which were often numerous and not understood, were infrequent enough to pass a regulatory committee.

13.6 Instrumentalization and Externalization: The New Order

Ziman was concerned with the epistemic changes that resulted from the new world of sponsorship. But we can place his concerns in a large framework. The Liberal Theory of Science assumed that scientists set their own goals, and were disciplined by the informal mutual relations of acceptance and recognition that they bestowed on one another as individuals. Prizes were one of the formal mechanisms, but they were expressions of the same esteem. Formal recognition followed from informal recognition. The values of science, when they were expressed, were articulations of informal standards, developed in the same processes of mutual adjustment, acknowledgement, and recognition. What we have seen in the postwar period is a gradual but inexorable process of instrumentalization and externalization of the processes of science: of goal-setting, funding, evaluation, and decision-making, which inevitably affects epistemic standards and the purposes of scientists. There were, of course, always instrumental and external elements of science. The Liberal Theory of Science minimized them and subordinated them.

The goal of reliability that Ziman saw much of the newly dominant kind of science as seeking was also an external and externally generated goal. To be sure the specific way reliability was operationalized in different scientific contexts would vary and result from a kind of consensus, sometimes informal, sometimes, as in the case of drug approvals, procedural and legal. But in either case it was a user's goal. The larger processes of externalization of which this is a part become apparent as science replaces informal and indirect forms of mutual discipline with formal rules and procedures.

A large literature now attests to these changes (cf. Brint, 2005 for an overview) and scientists have found ways not only to accommodate them but to justify them. At the same time, the rhetoric of funders has changed. It is common in the US to cite the 1980 Bayh-Dole and Stevenson-Wydler Acts, which encouraged university entrepreneurship, as a turning point in the corporatization of universities. But the emphasis on impact in the sense of serving societal purposes goes far beyond this, and has steadily increased, both from the point of view of administrative processes and the financial basis of science and in the rhetoric of science organizations, such as the American Association for the Advancement of Science.

Grants from the US National Science Foundation have long required "impact statements,"[6] but the expectations for these were vague and they were largely ignored in the decision-process. The requirements were clarified in 2010, and emphasized in the authorizing legislation of that year. The changes represent not only an intensification of accountability and demand for societal impact, but a proliferation of externally generated instrumental goals. The suggested

> outcomes include (but are not limited to) increased participation of women, persons with disabilities, and underrepresented minorities in science, technology, engineering, and mathematics (STEM); improved STEM education at all levels; increased public scientific literacy and public engagement with science and technology; improved well-being of individuals in society; development of a globally competitive STEM workforce; increased partnerships between academia, industry, and others; increased national security; increased economic competitiveness of the United States; and enhanced infrastructure for research and education. (National Science Foundation, 2011)

The list is constantly expanding. Among the first actions taken by Biden as President was to place special emphasis on the use of science for the purpose of equity (Office of Science and Technology, 2022). Similar themes are echoed by multiple organizations devoted to advocacy, most of which is directed at increasing science funding, which equates scientific progress with the public benefits of science.[7]

The items on this list, especially the goals of economic competitiveness and training a technical workforce, are mirrored in the policies of other countries. They reflect a brute fact: the kind of science that has developed and adapted to the needs of the public has been successful to an extent unimaginable on the model of what Ziman called "academic science." There is more science, and there are more scientists, than the old system could have supported, and science is more central to human life than ever. Moreover, the new form of science is arguably better than the older informally governed forms it replaced, and the alternatives to university-based science, even in its corporatized form, provide a new and attractive range of opportunities.

The form of science Ziman described was transitional: it was part what Ziman called "academic science" and part something else. Although the new form of science is difficult to grasp conceptually, and difficult to describe, its advantages can be articulated by comparing it to Ziman's "academic science." The first, of course, is money: the amount of funding available today from public and private sources dwarves the funding available in Polanyi's time, and is ever increasing. This in itself is a source of "freedom," especially for scientists whose goal is not academic prestige, but contributing to human betterment. It is also less exclusive: good science gets funded. But there are many other advantages. The system is more bureaucratic, but even this has its advantages. The metricized standards about which

[6] https://beta.nsf.gov/science-matters/nsf-101-five-tips-your-broader-impacts-statement

[7] Including: Research! America. https://www.researchamerica.org/take-action/
Act for NIH. https://www.actfornih.org/about/
Ad Hoc Group for Medical Research, The Good Science Project. https://www.aamc.org/research/adhocgp/start.htm

writers like Jerry Z. Muller (2018) and Gloria Origgi (2017) complain are more transparent and fairer than the informal judgements of the past. They are more open to talent. And because they are objective and standardized, they are more persuasive to funders and administrators who are themselves judged according to similar standards—often the aggregate form of the standard question, such as grant funding. The use of “initiatives” by funding agencies specifying the type of science they want pursued, usually accompanied by seminars to invited potential grant recipients to explain what is wanted, which most closely resembles the “planning” which Polanyi rejected, has its own advantages: it reduces the mystery element of the process. So does the explicit use of “impact” criteria. By making these goals explicit, some of the burdensome guesswork of dealing with “peer” committees and open disciplinary competition is removed. So is the need to anticipate the preferences of such committees for particular scientific fashions: the initiatives make clear what is expected.

The Liberal Theory of Science was based on a strong presumption that advances in basic science could not be “planned.” The ground for this was history: the big discoveries and changes in scientific theory were made by scientists free from practical constraints. And it assumed that what followed from this was that science directed at particular goals would not contribute, or not contribute appreciably, to basic science. This reasoning is increasingly challenged by a new rationale. This too has a historical basis: in the successes of use-inspired basic research, research, which is problem oriented, but also makes basic science discoveries. Stokes calls this category of research Pasteur’s Quadrant (Stokes, 1997), but is given the new name of Highly Integrative Basic and Responsive (HIBAR) research by one of the organizations promoting it.[8]

This takes a key argument away from the Liberal Theory. Discoveries in basic science do not require science funding on the basis of the freedom of scientists to choose their own goals. To be sure, the science that is produced, and the discoveries that are made, are the result of decisions made by people—funders, administrators, and the scientists themselves. These decisions will be different, and the outcomes will be different, under the emerging order of science. But “academic science” was a system too: it also directed decisions about what to study and how to study it, albeit in a different way, with different outcomes.

The case for the new order does not need to be made on the basis that it is without flaws, or that it produces everything that science can produce. Every order has its limitations as a social structure, as well as its own epistemic blind spots that result from its structure. We can accept that “basic science” in the traditional sense is still important, but that its importance to the rest of science is overstated, and that basic science discoveries may arise from user-oriented research, and always has. If we accept the fact that science can do much more than generate scientific laws and near-certainties—that it can produce all sorts of useful, reliable knowledge without the

[8] https://hibar-research.org/

kind of finality scientists of the past strived for—we open new topics for research, new possibilities of service, and new sources of funding.

13.7 Historicizing Theories of Science

Where is the tragedy here? For the adherents of the Liberal Theory of Science, the tragedy is this: its nemesis of planning, in the new guise of a complex system of external constraints, demands, and systems of evaluation, has become the condition of science. Autonomy, the core value of the Liberal Theory of Science, is necessarily compromised. This system has been partly unconsciously adapted to and partly consciously embraced by scientists, but it is nevertheless constraining in its own ways. Conformism pays.[9] The tragedy is that this world of relative autonomy is lost. But a realistic assessment of the present state of science would concede that there is no going back to the world of interwar academic science, with its small numbers and small budgets. The sheer cost of contemporary science means that much of it must pay for itself through tangible benefits to its financial supporters. We can mourn the lost world, but we can no longer inhabit it.

What would Polanyi say to this? Does the Liberal Theory of Science have a response? One response might be this: to express sadness that fundamental issues have gone unanswered, and that free inquiry by autonomous individuals into them is no longer a priority; to admit that the heroic model of science is no longer relevant and that discovery is no longer the modal form of science, nor the most relevant to the way science is done. But he might also take credit for the historical role of the Liberal Theory: the development of the science that made this new mode of science possible, by protecting the informal mechanisms of criticism that produced public trust in science, and by preventing the closure of paths of discovery that would have resulted from premature efforts at planning for the public good. We can acknowledge that what is sometimes called "transformational science" is no longer happening, and that basic discovery has slowed down, and, in effect, concede the "finalization" case: that the exploratory age is over, and the new age requires organizing science not for its own sake but science for service.

To do this would vindicate the Liberal Theory of Science, but not on its own terms. It would credit it as the practical ideology of a period of successful basic science that was a mid-wife to the present vastly expanded role of science, but one that is no longer suited to it. This would, in effect, historicize the theory. The true tragedy of the Liberal Theory would not be that the world of science turned out differently than it wished, but that the theory failed to recognize the fact that it was

[9] Nicholson and Ioannidis (2012) made this point in an article in *Nature,* and met with a ferocious response by the defenders of the system. Ioanidis the extended his criticisms to an analysis of the science system itself and ways it might be reformed, reflecting the new reality of multiple stakeholders and motives (Ioannidis, 2014).

an articulation of an ideology of science limited to a particular set of circumstances and a particular state of science. In short, it absolutized itself as a general account of science, when it should have recognized that any such account of science was bound to a historical moment.

We can go farther, and concede that the "planner" critics of the Liberal Theory of Science had a point—there may be surprises in science, but there is a sense in which science is always planned, both by individual decisions and funders. They can plan badly, but they do so under the limitations of the organizational structure under which they decide,[10] and on the basis of their limited knowledge, perspectives, and so on. How these prospective decisions are made is conditioned by the structures under which they work. The new structures under which scientist operate requires a new account of science consistent with these structures.

If we acknowledge this, we can recognize that this rethinking is already well-underway. Scientists have unconsciously and consciously reoriented themselves, as has the rhetoric supporting science. There is no longer a need to defend investment in pure science with an elaborate argument about its potential ultimate value for technology. Science can justify itself directly by reference to societal values. Philosophers of science are eagerly rationalizing and providing theoretical justifications for the change (Kitcher, 2001; Fuller, 2020; Douglas, 2009). Science, according to the developing consensus, is intrinsically valuative, and this means that science must conform to or serve the correct values. For the practicing scientist, this is planning without the plan: one simply aligns one's research to the correct values, in order to survive in the grant system. Scientists still compete, but the goals are now such things as science as a public good.

Is this the end of the story? Perhaps not. The new world of science still employs the rhetoric of the Liberal Theory of Science when it claims authority over truth, the overwhelming value of scientific opinion, now presented as "consensus," and the need to respect the special moral quality of science as a truth-seeking pursuit. Elizabeth Anderson presents the orthodox view of this when she says "Science needs a balance of diverse inquirers to formulate and investigate a wide range of hypotheses, uncover a wide range of relevant evidence, and check one another's biases" (Anderson, 2011, p. 149). But is this claim still justified? The Liberal Theory relied on indirect means, the nature of the scientific community, its evolving traditions, and its informal processes to assure not that scientific opinion was true, but that it gradually corrected itself through mutual exchange, especially with cognate fields. The informal and indirect means that the Liberal Theory relied on depended on autonomy, a "market," no coercion, long time frames, incentives to contest results, relatively modest financial rewards, a low bar to communication in the form of generous peer review for publication, informal controls, and trust. It also depended on scale, the scale that allowed for personal relations. These conditions

[10] I have elsewhere discussed these forms under the heading "Knowledge Formations" (Turner, 2017).

have all changed radically. And it is far from clear that anything has taken their place.

When critics like Ioannidis propose reforms, such as transparency, registering protocols in advance, and changing the incentive system to reward replications and refutations (Ioannidis, 2014), they are recognizing the gaps in the system of assuring the validity of science that have opened up as a result of the changes in the relations and incentives that characterize current science. They recognize, implicitly, that the old informal controls no longer work. The Liberal Theory of Science can no longer guide us. But it can still serve as a means of critique. We can use the autonomy that the theory assumed, and which claims about scientific consensus today still assume, to raise questions about sources of bias in the grant system and peer review, in relationships with corporate funders, and in the system of incentives for scientists. These structures compromise what autonomy is left. We can ask what has replaced the informal mechanisms of the past, and whether they are effective under present circumstances. We can ask what constrains scientists. This may not seem like much, but in a world increasingly dependent on and ruled by science, and in which we are asked to trust science to provide expert answers to questions of policy that have massive and inescapable consequences for everyone, we need more than ever to examine the way the consensus underlying this advice is produced. The power of science over public questions means that it requires critique. There is no account of science that provides for the present an account of science that does what the Liberal Theory of Science did for past science. But if this theory no longer has the right answers, it still has the right questions.

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Péter Hartl
Editor

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