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SCIENCE ON DEMAND

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Characterizing science as a public good, as Steve Fuller notes, is a part of an ideological construal of science, linked to a particular portrayal of science in the postwar era that was designed to provide a rationale for the funding of pure or basic science. The image of science depended on the idea of scientists as autonomous truth-seekers. But the funding system, and other hierarchies, effectively eliminated this autonomy, and bound scientists tightly to a competitive system in which the opportunity to pursue ideas in science depended on peer approval in advance. Funding agencies then turned to assessments of impact. John Ziman had already recognized the effects of these changes in the nature of science, and characterized it as "reliable knowledge" produced on demand from funders. As the competition for funds increased, there were further changes in the nature of science itself toward "reliable enough" knowledge. This made science into a "good". but a good in the sense of results produced for funders, a transformation that left the original epistemic aims of science behind.

***Keywords*:** science policy, liberal theory of science, John Ziman, Donald Stokes, impact assessment

НАУКА ПО ТРЕБОВАНИЮ

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Образ науки как общественного блага, как отмечает Стив Фуллер, является элементом послевоенного идеологического проекта, ориентированного на поиск обоснования для поддержки чистой, или фундаментальной, науки. Этот образ основан на идее ученого как автономного искателя истины. Однако система финансирования и прочие иерархии уничтожили эту автономию и намертво связали ученого, сделав его частью конкурентной системы, где сама возможность научного поиска заведомо зависит от одобрения коллег. Финансирующие организации впоследствии обратились к оценке результативности. Джон Зиман (Ziman) охарактеризовал эти изменения в естественных науках как переход к созданию «надежного знания» по требованию финансирующей стороны. По мере нарастания конкуренции за финансирование в самой науке началось движение к «достаточно надежному» знанию. Это превратило науку в «благо». Однако это благо признается таковым с точки зрения результатов, достигнутых для спонсоров. Такая трансформация привела к забвению исходных эпистемических целей науки.

***Ключевые слова*:** научная политика, либеральная теория науки, Джон Зиман, Дональд Стокс, оценка результативности

Steve Fuller's comment raises a large number of key issues about the way we are to understand science and its social and economic role today. One issue is this: we are still speaking about science in the language perfected by scientists – mostly physical chemists – who were drawing on their own experience in the interwar years. This account valorized "pure science". It was modified in various ways in the 1950s by notions like "public goods", which allowed pure science to be ascribed economic value. And this was justified by the Vannevar Bush linear model of the impact of science on technology.

Science as it is presently practiced bears little relation to that kind of science. It is vastly more expensive, competitive, error and fraud filled, and trivial. And it suffers not only from many ills and discontents voiced by its practitioners, but from a lack of coherent public discussion of what the ills are. The scientists, philosophers, and sociologists Fuller mentions, mostly from the 1940s and early 50s, were influential and respected figures who had an audience in science and beyond, in the public. Their problematization of science and their image of the "scientific community" has stuck, but the image has long outlasted the underlying reality of science. Fuller deals with one aspect of this. But the problem is larger. As I have said elsewhere, there is an "Owl of Minerva" quality to their thought, and perhaps this is inevitable: understanding, and sometimes even acknowledging, the transformations one is in the middle of is not possible. We are compelled to drive looking in the rear view mirror. But we are now barely looking at all. Fuller is right to think that one must dig deeply into the past, and to the theory behind it, to think about an alternative.

Donald Stokes added the category of "use-inspired basic research" to the categories of pure basic research and pure applied research. He called this category "Pasteur's Quadrant" (1997). Stokes is not important as a thinker. But he is important as a symptom. The book was published by the ultra-establishment Left-leaning Brookings institute. As with other writers, notably Philip Kitcher and John M. Ziman whom I will refer to in what follows, it is an attempt to justify what it takes to be the new order. Many discussions in science about these issues grapple with these same issues, using the language of Bush, the "linear model" of basic-applied-technology", while at the same time critiquing it [Smith, 1993; Dudley, 2013]. They often echo Fuller's question: "Why should we presume that 'basic research' of the truly fundamental sort is more likely to come from the agendas of self-appointed 'basic researchers' than external exigencies of the sort that provide the basis for Pasteur's Quadrant"? But they also try to defend basic science, from the point of view of insiders, who see it as a funding category that is shrinking relative to mission-oriented funding: "public good" is part of the salesman's language for this effort.

Stokes treats science as a simple thing – a results producing machine. And his discussion reflects the idea that one can have what I called "science on demand": scientific solutions for whatever problems one wishes to address. His argument is that this kind of science can contribute to what was formerly thought of as basic science, and that therefore the older picture of the relation of basic science to application is flawed. His concern is the salesman's concern. In making the public case for science funding, and he thinks a revision of its appeal, dropping the emphasis on basic science would generate a better deal for science.

There is a lot wrong with treating science as a public good. "Knowledge" is not a commodity, or a public park. Doing anything with it requires knowledge that isn't "public" in the sense of being universally accessible. One needs to know a lot already to make use of a published finding. That knowledge is often thought to be irreducibly personal and tacit. It is enough to say it is rare, difficult to obtain, and to communicate. So in practice it cannot be public. This means many things, but a key thing is this: we do not have a way of judging science without this knowledge; we are reduced to relying on those who do, meaning "peers". We can use indirect evidence of the judgements of peers, such as citations. But "peers" are an essential link in the process of affirming the validity of knowledge claims, their importance, and so on. But more important, they are a filter that controls what science is done, and by whom. There is, effectively, no science today that is not done without approval by others, often many others. Even to have free time to pursue an idea requires many hoops to be jumped through, and to have a reputation affirmed by peers. To run a lab costs money and time; to support at team requires a great deal of both. Individual effort is not enough in most areas of science. Equipment and access to platforms, such as satellites and telescope arrays, is critical as well, and controlled by the same peer processes.

Stokes recognizes the complaints about this system, or some of them, but dismisses them. In his view of science they don't really make a difference – for him, a scientific fact is a fact; the facts are going to be the same regardless, so our aim should be a mode of administration that gets more science, and makes science as appealing an object of subsidy as possible. He prefers the model of the US National Institutes of Health to the National Science Foundation model because it appears to do this: it does a better job of assessing impact, by using a more extensive form of peer review that uses experts who are not quite as closely bound to the research area being assessed. This is a very modest challenge to the current system of science evaluation.

The Tyranny of the Peers

The present system of science funding and evaluation, based on multiple and redundant peer evaluations, is thus unchallenged by Stokes. But is it inevitable? Or is it a pathology which we have merely come to accept – as Gloria Origgi (2017) calls it: a form of voluntary epistemic servitude? There are rebellions against it, for example by African scientists, who believe, correctly, that it devalues their work. But it is nevertheless a system rooted in an intrinsic problem for which it is a solution, and perhaps the only solution. The problem is this: we must rely on others to know what to take seriously, what to invest our precious time and lives in, and what to do to sustain careers that allow us to do scientific work at all. Our realm of competence is narrow. It is no longer possible to be a polymath or universal genius. And it is difficult to imagine a world in which there is not a hierarchical domain, or a center and periphery, to which we can orient ourselves. We cannot read and master everything. So we attend to what we think is important and likely to be valid and useful, and get clues from the hierarchical structure of science to determine what these things are. If we do not follow these clues, we are not going to be funded or given careers which allow us scientific work. The system is thus a consequence of the necessarily indirect character of these judgments. And to be clear, these are often highly problematic judgements of prospective merit and prospective "impact". Nevertheless these judgements, judgements to fund a particular direction in science, produce the science we actually get. But does it matter for the content of science itself, or is it just a matter, as Stokes assumes, of more science of one kind rather than another?

In a sense, the practice of ignoring this question is rooted in the older image of science. Classical writers on the nature of science, such as Michael Polanyi, admitted the problem, but also celebrated this system, because they thought it didn't matter: they appealed to "scientific opinion", in a way reminiscent of Rousseau's general will, as the sole guarantor of science and of the validity of the decisions made by committees and peers. Peer evaluation merely implemented scientific opinion. If the operation of scientific opinion was interfered with, that was bad. But living in accordance with scientific opinion was what made science the disciplined and successful activity it was. He acknowledged that opinion changed, sometimes was wrong, and sometimes constrained too much. But on balance, it and the system that implemented it were necessary and good. But scientific opinion is a myth – a political myth, with the same value as other myths. It serves to justify and obscure at the same time. What it obscured was the politics of decision-making, the snobbery, the ubiquity of error, the fact that prospective decisions about what scientific work was likely to be valuable are essentially impossible – the very argument made for funding "basic science", but forgotten when justifying peer review as a system – and the prevalence of groupthink as well as the ease with which scientists, trained to conform, *do* conform when faced with uncertainty.

Scientists chafe at this system, often, as do academics generally. It is a system that has the vices of its virtues. It is strongly inclined to exclusion and elitism: "peers" are elite peers or their progeny. Conant believed that one good scientist was worth ten mediocrities. But talent is probably normally distributed, and the "talent" end of the tail will thus have many people of talent close to the people at the top who have been excluded and will never reach their potential [Turner and Chubin, 1976]. It is more accurate to say that hierarchy is efficient: in science there are too many ideas, too many competitors, so there must be a way to choose. That the system strangles many of these good ideas, and marginalizes many of the competitors on dubious grounds, is the price of hierarchy. Miranda Fricker has made a point of talking about this in terms of "epistemic injustice" (2007), meaning the unfair ignoring of people who deserved to be heard. And this is a basic issue. The hierarchy validates itself circularly by measures that are in fact the product of hierarchy. Being unheard means one will not be cited, proving that one deserved to be ignored. Being in the wrong position assures that one is ignored. This is the meaning of what Merton called the Matthew Effect [Merton, 1995]. But Merton believed, and tried to prove, that the system of stratification was nevertheless fair, and justified by such things as citation counts.

But the concept "deserves to be heard" runs into a large problem. We have no external way of judging the importance, validity, truth, and so forth of basic science. Science is validated by scientific opinion – which is to say "peers". So are the decisions to fund scientific projects. The fact that these highly consequential judgements are made by "peers" is comforting. We can tell ourselves that the science we are getting is the "best" science. But we can never know that this is the case – we don't and can't know what would have happened if there were other funding decisions that led to different kinds of science or different directions in science, and in any case we can only judge science from the viewpoint of the winners. They are the ones who define the scientific knowledge we rely on to make these judgements. We cannot get out of this self-validating circle. The same point holds for the regime of science funding, journals, job competition, and the like which presently exists: we have the science that we have created under these organizational conditions; but we have no way of knowing whether a different kind of science would have emerged under other conditions. And this is a limitation for critics of science as well: we don't know what kind of knowledge we don't have, so we don't know what we missed. There are plenty of unsolved problems, but no guarantee that they could ever be solved.

Technologies work, fail to work, or work better or worse than other technologies. It is striking that some of the most vocal critics of the system have been scientists who established, despite the system obstructing their efforts, award winning technologies – including one cited by Stokes as an example of use-inspired basic research. Their complaints are directed at peer-review itself and the difficulty of getting innovative research approved, its conservatism, and the tendency to only fund sure things. This is an important clue. The peer system, operating on its own, doesn't have this check: it is entirely self-validating. So we do not know when it is wrong.

Is There an Alternative?

The term “basic” is already a relationship term – it implies that something stands on the base. Stokes is correct that at least from the point of view of discovery, “applied” science or technology may lead to the discovery of the most general and meaningful facts, such as the existence of DNA, viruses, background radiation, vitamins, and so forth. So it would be better to drop these terms. The term “curiosity driven” needs to be dropped also, for different, and more important reasons. The autonomy implied by the standard phrase “curiosity-driven research” is illusory: no matter what their motives, scientists are not free to follow their own best hunches, as Polanyi wished for them, which was the essence of autonomy. You have given up your freedom to the people who judge your grant. The problem is intrinsic to peer review. And because we are dependent on the judgement of peers, it is intrinsic to science and science policy as presently practiced. Nor is there an escape from this. Nor is autonomy a matter of who peer reviews your grant applications or what category they are reviewed under, basic or problem-oriented – if you are submitting grant applications you are not autonomous. Science is big business. It is about money.

The late John Ziman understood that the old image of scientists as making autonomous choices about what to study was simply false. And he also understood that there was something wrong with what science was reputed in the public eye to be – “the legend”, as he called it – and he tried to replace it with the idea of science as “reliable knowledge.” He understood the new role of science as service to society in exchange for vast sums of money. Science has long ago given up, albeit unconsciously, the model of pure scientific understanding that forms one of the quadrants identified by Stokes. For scientists, immured in this system and conditioned to its demands, the problems of science have to do with maintaining their research facilities, paying their staff, and getting grants to do so. For them there is always too little money, and too much competition for it, which produces fraud, conservatism, and stifles creativity. Abstract notions of scientific truth are far from their minds. Producing results to meet the demands of the system is at the center of what they do.

But the funding system is only part of the problem. Proposals have been made to set aside money for risky research, or to fund researchers without asking for proposals. But none of them have worked. The reason is simple: the controls exercised over scientists are redundant. To escape the grant system is not to escape the citation system, which is an indirect form of peer review. It is not to escape the need to place one’s students – another form of peer review, or to escape the struggle for positions that enable one to do research – positions one attains through one’s reputation with one’s peers, or through achievements, such as publications, that peers have evaluated, or to escape the struggle for attention. Oddly, the risks inherent in this system were well-known to the classical writers on the scientific community, such as Polanyi and James Bryant Conant. But they were simply ignored in later years, as Stokes ignores them. The success of science in getting funding was success enough to validate the system.

We need to step back from this mode of organizing science, and see it in a larger perspective.

There is no law that says social organization will automatically match up to, or adapt to, intellectual opportunities. The whole history of thought says otherwise – that social organization and its demands are more likely to restrict the development of thought. The blazing emergence and subsequent death of inquiry in the ancient world under Christianity, and then in the Islamic world, is a case in point. This reality has not been repealed in the present: what I have elsewhere called knowledge formations (2017) are governed by the twin and conflicting realities that intellectual work generally is not self-supporting, and that a certain amount of freedom is necessary. The present grant and peer review system, and the integration of multiple systems into a global science system, is just one example of a knowledge formation. Is it the best system? That is more difficult to say. Certainly it supports more scientists than ever before. But is it the best match for the intellectual opportunities of the present?

It is obvious that the big discoveries of the past which boosted the status and importance of science are without parallels today. There is nothing like the Darwinian revolution of the nineteenth century, or Mendel's discovery of genetics, or the twentieth century discovery of relativity, the quantum revolution, or the deciphering of DNA. Despite the massive investment in science and especially physics after the Second World War, and drug research after 1995 – estimated at two trillion dollars, divided more or less equally between public and private money – there have been no transformative breakthroughs, with the exception of DNA, which was deciphered by young researchers working under conditions that can no longer be found anywhere. For some, it is a scandal that major issues in physics have gone unresolved for seventy-five years. Cancer has not been cured, despite this goal being the justification for much of medical research funding since the thirties. The response to COVID-19 has been a fiasco, despite the huge sums devoted to the relevant fields over the decades.

Science and technology today suffers from diminishing returns, lower productivity, and hype. As Jeffrey Funk points out, *Nuclear fusion has received more than $30 billion (2017 dollars) in R&D funding from the US government and similar amounts from European countries. Nanotechnology has received more than $20 billion in government support, partly based on a market forecast made by the National Science Foundation in 2001 that pegged nanotechnology to reach a worth of $1 trillion by 2015. But no electricity has yet been generated by nuclear fusion, and* *the market for the most-hyped nanotechnologies – graphene and carbon nanotubes – is currently far less than $5 billion and barely growing* [Funk, 2019].

Thus a better question than "how can we get more money for science?" would be "what has gone wrong"?

It is obvious that the big discoveries of the past occurred with little money and under entirely different organizational circumstances. Darwin and Mendel had no peers approving their budgets and demanding reports. We know that people like Bohr and Fermi also had a degree of freedom to work and collaborate that is unknown today. We know that even within the world of recent science, organizational setting makes a big difference in creativity [Fox and Nikivincze, 2020]. We have reason to believe that the hyper-competitivity of science and especially of the grant system has negative effects on creativity [Sandström and Van den Besselaar, 2018]. But we cannot make the link between these large facts because we cannot say what would have been discovered under a different regime. We cannot refute the claim that all the big discoveries, including the "practical" ones, have already been made; that the "exploratory" period of science, as the finalization thinkers put it, is over. Nor can we refute the claim that all we can find now are small differences, but small differences that may be of sufficient practical value to justify the research expenditure.

But to speak in these terms is misleading: we remain, like Stokes, trapped in a rhetoric about science that no longer fits its content – content that has changed as a result of the global science system that has emerged. Ziman had a clue to these changes when he downgraded the claims of science from what he called "the legend", the idea that science had a method that could solve all problems, to the claim that it produced, in various ways, but nevertheless effectively, "reliable knowledge", and that the social system of science was a means of assuring that it in fact was reliable. This is what Fuller has in mind when he mentions the value of science as "a quality control check on knowledge produced for the public good".

We can go farther than this, however, in talking about how science has changed. Philosophers of science are a trailing indicator of change. But in their role as ideological defenders of science, they too grasped the changes. Some tried to justify the "service to society" model – notably Philip Kitcher [2000; 2011], and replaced ordinary criteria of the value of knowledge with a kind of network model in which knowledge was validated by its connections to other, social, values. The role of values in science became a new mantra. Others recognized, through a glass darkly, that the content of "science" had changed. Scientists no longer looked for "laws", as the Logical Positivists thought they did. They constructed models or representations that were justified by successful interventions. Hacking realized this early [Hacking, 1983], and later the entire discourse of philosophy of science shifted to talking about models [Morgan and

Morrison, 1999; Cartwright, 1983], mechanisms [Machamer et al., 2000], and simulations. The idea of science as providing a unified account of the world was relegated to the museum; the new ideologists of science pictured it as a collection of more or less unrelated discourses [Dupre, 1993].

This suggests a different justification for the present system that goes a step beyond Ziman. The best that science can do in the face of most of the new problems it is called upon to solve is to construct more or less reliable models that allow more or less effective interventions. The models we construct for the purposes of intervention do not add up to a coherent whole picture of the world. They make inconsistent assumptions, and simplify in inconsistent ways. They are thus immune to the kind of conceptual unification sought by thinkers like Darwin and Mendel, or by Einstein and Bohr. They are technically advanced, based on much better instrumentation and imaging, and they improve, to some extent, in predictive power and the success of the interventions they warrant. But they do not cumulate in the way that programs for science in the past, such as the unity of science movement, envisioned. Nor do they do what Stokes imagines use-inspired basic research will do. These models are not "basic" to anything. This new kind of science is no longer a public good in the sense originally intended – a body of stable knowledge. The new kind of science is, as Fuller rephrases it, "knowledge produced for the public good". This is what I would call "science on demand" [Turner and Chubin, 2020]. It is a kind of public service activity in which science produces good enough "knowledge" for its users, whether they be other scientists or public users, to act on.

We have turned science into something that produces results: this is what Stokes celebrates, and the term "public good" sanctifies. But every model for the social organization of science exhausts its possibilities eventually. There is more than enough disquiet with what the research world has become to think that the present system, rather than the opportunities for discovery in science itself, has reached the stage of diminishing returns.

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