

## Comment

# An insider's view on science and society. Re-reading John Ziman

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A physicist. That is what John Ziman was in the beginning. As he tells us in “On being a physicist,” this implies a kind of nationality, that is, a laboriously learned identity that, at the end of the day, becomes natural. Physics was for him a way of seeing and a way of thinking, inextricably embedded in his own being: “a deeply rooted mode of personal existence.”<sup>1</sup> And what is physics? A discipline that strongly connects mathematics and the world—“The essence of physics is that it is a science in which every endeavor is made to represent nature in mathematical terms”—, through a kind of relationship on which its practitioners have a broad and deep agreement. A perfectly “normal” science, in Kuhnian terms: “Physics is the archetype of a ‘finalized’ science—that is, one where there is a well-established ‘paradigm’ around which most of what is known can be rationally ordered.”<sup>2</sup>

We should never forget the obvious. Physics, this rationality/nationality, was the starting point for Ziman’s reflection on the complex relation between science and society. This is the evocative tone he uses when talking about past times in physics, regarding what may be seen as his life-long meditation on the social character of science, and his increasing concern about the process of the “collectivization of science,”

“There was a time—how long ago it now seems—when physics was an individual vocation. (...) [E]ach project could still be the work of a single physicist, reporting the results as the single author of a paper, or reporting on the development of an invention to the stage where a properly engineered project might be called for. Even when large groups of researchers gathered around a particular effective leader of research, such as Enrico Fermi or Ernest Rutherford, they would undertake their research projects and gain personal credit from them as independent individuals.”<sup>3</sup>

In some way, his denunciation of this new situation was a very personal enterprise. The Preface to *Of One Mind: the Collectivization of Science* offers more clues on how Ziman viewed this process in his own career. He lists all the commitments he had as a professor of theoretical physics in Bristol. By that time, he tells us,

“I was directly involved in the whole business of scientific communication at all stages, including a primary physics journal, the international network of abstract journals and databases, a major physics review journal, a monograph series, and a general review journal. Organizing study programs at the International Center for Theoretical Physics at Trieste gave me unique opportunities to observe the growth and spread of science outside its traditional centers in Europe and North America. Above all, I had become one of the highly cosmopolitan inner group of the ‘Invisible College’ of Condensed Matter Physics, and was traveling here and there around the world to plan and take part in scientific conferences, summer schools, workshops, colloquia and other meetings.”<sup>4</sup>

Although he acknowledges that was “an exciting whirl of activity, facilitating the general advancement of knowledge,” he regrets these disparate commitments were “taking its tool” on his own research. He finally retired from physics in 1982. By that time he had already written a lot trying to understand and solve a paradox, that of the centripetal-centrifugal forces that make creativity and collaboration possible in science. As he once put it, he incessantly wondered: “how can it be that a wild mob of rabid individualists can combine voluntarily to create the most elaborate and exquisite artifacts, material and abstract, ever known to humankind?” And from that starting point and his emphasis on consensus as a key aspect of science, he had conducted a subtle and suggestive line of reasoning on the changes science

was (and still is) going trough. Again it is revealing the way he reflected on his own career, and particularly on his early models. Ziman evocates the talk he had with Nevill Mott in 1954, in the “dark, paneled office” he occupied as head of the Physics Department in the H.H. Wills Physics Laboratory. Mott’s situation is compared by Ziman to that of “an 18<sup>th</sup> Century nobleman.” As he pictures it,

“In return for material security, social esteem, freedom from mundane tasks, and complete independence of thought, word and deed, he was expected to be fully committed to larger causes and to devote himself to the public welfare *as he saw it*. As a member of an elite stratum of society he was under little pressure to perform or conform, so he could easily have been idle or irresponsible. But Nevill Mott obviously had a very clear conscience. *He more than paid for his privileges by his contributions to knowledge and to national life.*”<sup>5</sup>

He then considers this setting had not substantially changed when he eventually took his turn as head of that “outstanding Department” in Bristol in 1975. But he saw important changes twenty years later—and he was well prepared to notice them, because his reflection on these matters was already mature. He observed that his successor, John Enderby—who occupied “the same old dark, paneled office” Mott and himself had in turn occupied—although as committed “to serving the public through the advancement of science” was nevertheless not free to perform this task “in his own particular way.” As Ziman describes the new situation, impersonated in Enderby,

“He is expected to behave as a middle manager in an industrial firm, reporting regularly to the chief executive officer of the university, and competing with other senior professors to maximize the grant income and performance rating of their departments and research groups. He too has been ‘collectivized,’ along with all his professional colleagues.”<sup>6</sup>

That is the collectivization of science in one sub-discipline, in one lab, in one line of succession. And this is how Ziman describes it in more general terms in the Preface of *Prometheus Bound. Science in a Dynamic Steady State*, when his views had already reached a definite articulation,

“Science is reaching its ‘limits to growth’. It is expected to contribute increasingly to national prosperity, yet national budgets can no longer support further expansion to explore tempting new research opportunities, by larger research teams, equipped with increasingly sophisticated apparatus. As a result, science is going through a radical structural transition to a much one tightly organized, rationalized and managed social institution. Knowledge-creation, the acme of individual enterprise, is being collectivized.”<sup>7</sup>

The origin of the idea of a “major structural change” in science is traced back by Ziman in three crucial books: Derek de Solla Price’s *Little Science, Big Science* published in 1963; Jean Jacques Salomon’s *Science and Politics*, published in 1970; and John Ravetz’s *Scientific Knowledge and its Social Problems*, published in 1971. He comments that since those times, scientists “have become uncomfortable accustomed to level funding, and complain loudly about its various effects on the research enterprise.”<sup>8</sup>

### **Preliminary observations**

It may be illuminating to explore how Ziman developed his own conception on the changes in science; the observations and reflections he made during his career, the kind of arguments he presented, the lines of reasoning he followed in four decades. He wrote a lot. We will track his steps in his books, each of which represents an important juncture in this journey. Ziman’s first venture out of the realm of physics is *Public Knowledge. An Essay Concerning the Social Dimension of Science*. Anticipated in a 1960 brief essay, planned in 1964 as a joint project with colleague Jasper Rose, completed in 1966 as a one man’s work, and finally published in 1968, it represents Ziman’s first reflection on what science is as a collective endeavor.<sup>9</sup> With witty humor, he alludes at previous efforts in trying to locate the source of creativity in science, and for the first time points at the importance of understanding the scientific community as an innovative group of people out of which new science comes up, a theme that will accompany him from then on,

“Cleopatra’s Nose, in the guise of Einstein’s Cerebral Cortex, is now somewhat discredited as the causative agents for events of intellectual history. Explanations in terms of movements, climates of opinion, the *Zeitgeist*, are now prescribed.

“Natural Science, whose internal development for three centuries is so uniform, well-documented and relatively self-generated, is an obvious candidate for such treatment. And having noticed the *intellectual* connections between the ideas of various scholars, we must surely pass on to the *social* relations through which those connections are established. How do scientists teach, communicate with, promote, criticize, honour, give ear to, give patronage to, one another? What is the nature of the community to which they adhere?”<sup>10</sup>

In addition to Ziman’s very early reflection on the social character of science, this book also reveals his concern about autonomy—as the claim that science is “relatively self generated” betrays. As contemporary reviewers mentioned, in times when both a novel sociology of science and the demands of the public sector—the source of most academic funding by that time—were exerting converging pressures on the scientific community, Ziman’s book represented an attempt at challenging those forces, proving that scientists know better. As A. E. Musgrave put it, by that time scientists “thought it necessary to show that science plans very well from the inside, and that the self-government of science by scientists is actually essential to its progress.”<sup>11</sup>

The complicated situation in which Ziman will encounter himself later on in terms of his positioning himself as just a scientist is also anticipated here. It is not surprising that, in another review, Harriet Zuckerman pointed at the many “stereotypes the author shares with his fellow-scientists about what they do and why.”<sup>12</sup> Certainly, in order to legitimize his stance, Ziman has both to acknowledge he is entering the realm of disciplines he does not master, and to argue in favor of the privileged position a scientist has in terms of explaining science. Regarding the former problem, he simply and humbly describes the book as an exercise of “amateur philosophy.” But to address the latter, he develops a more elaborate argument, first claiming that the literature of the sociology of science “often lacks the authenticity of personal witness.” He then compares the scientific community with other groups investigated by sociologists—and says that it may be as strange to sociologists as “the psychological world of a Chinese family” to a European. But that does not mean he considers acceptable any kind of ethnographic approach: sociologists must ask scientists, and listen very carefully to what they say, because they have already thought about themselves—they are not as those primitive, instinctive societies. As he puts it,

“Scientific research is an intensely self-conscious, deliberate and rational activity, the antithesis of those traditional, semi-rationalized, almost unconscious patterns of behaviour which are the conventional objects of anthropological and sociological scrutiny.”<sup>13</sup>

If *Public Knowledge* represented an attempt at defending autonomy by arguing in favor of scientists’ right to speak for themselves, Ziman’s following book is a message mostly addressed at insiders—particularly, at those entering the scientific community. *The Force of Knowledge. The Scientific Dimension of Society*, finished in 1973 and published in 1976, is an original, entertaining text whose ideal reader is “the average second year Honours student.”<sup>14</sup> It was the result of Ziman’s incursion in public matters, notably his involvement in the discussions around the Society for Social Responsibility in Science. The *raison d’être* of the book lies on the moral responsibility of professors: “Our own conscience, and our professional pride as university teachers, demanded that we introduce the theme of the social relations of science and technology to our students.”<sup>15</sup>

In fact, the book is the systematization of a course based on ten lectures he began teaching at Bristol University in 1971. With a number of diverse, very telling pictures and diagrams—which range from medieval drawings on how to treat spine dislocation, to a photo of Winston Churchill visiting a lab in 1946, to contemporary ads for scientific equipments, to a chart that estimates the costs and benefits of developing corn hybrids—it represents a remarkable effort at introducing students in what would soon be named science and society approaches.

As the title suggests, *The Force of Knowledge* is structured around a quite linear, quite Wiggish account of the history of science. Although it is visible the effort to acknowledge the existence of non-Western scientific traditions—such as that of India, for example—the books is obviously constructed around trying to understand how Western science became to be what it is, and to face the problems it was already facing. Again, his views are a bit stereotypical; for example, when he laments the general

public's lack of scientific education, and attacks the popular press because of its sensationalism, he is simply echoing some notes of the "dominant view of popularization" Hilgartner characterizes.<sup>16</sup> He is still far away from the kind of reflections he will do after being part of the Bodmer Committee. More importantly, he enters political arenas with no compass at all, as critics pointed out.<sup>17</sup>

However, *The Force of Knowledge* also anticipates Ziman's most important contribution to the ongoing discussion on science issues, and in that sense it provides students with incisive observations on the challenges they would face. This is clear in the chapter devoted to big science, in which the increasing complexity of the scientific equipment is presented both as an occasion for scientists to amplify the powers to understand nature, and a constraint in terms of their independence to find his or her own way in this enterprise—his observations certainly evoke those of A. M. Weinberg in *Reflection on Big Science*. Ziman is concerned particularly about academic science. As he explains at the end of the chapter, already warning on the new collectivization of science,

"What I have been trying to show here is that science itself, as a way of life, has changed enormously in the past half century, and is much less readily distinguished from ordinary industrial, commercial, managerial or bureaucratic existence than it once was. Paradoxically, the scientist who multiplies his observational or computational power manifold with new and complex apparatus has become a slave to his own machines and to those who cooperate with him in their use. What he has gained in technique, he may well have lost in intellectual grasp and in the joys of the hunt."<sup>18</sup>

A new series of lectures which evolved to become "four long seminars" funded by the Van Leer Jerusalem Foundation in 1975 provided Ziman with an opportunity to come back to and revise some themes he had touched upon in *Public Knowledge*. In the Preface to *Reliable Knowledge. An Exploration of the Grounds for Belief in Science*, he still describes himself as "[h]aving no academic pretensions or professional affiliations outside physics," and acknowledges he made his "own way into the diverse literature." This time, Ziman focuses more on the cognitive aspects of science. The central question, as he explicitly formulates it, is: "how much ought we to believe of what science might tell us about man as a conscious social being, subject to unreasonable emotions and irrational institutions?"<sup>19</sup> It is certainly a strange question to be addressed by an active physicist. Of course, unless you are convinced science is the only "reliable knowledge," and physics the supreme model. Again, there is something of a defensive approach to the matter. Ziman considers science is under attack from different quarters "not well concerted." The new concern for the environment—introduced by authors such as Rachel Carson, and which by that time had already motivated the birth of important non governmental organizations such as Greenpeace and Friends of the Earth—plays an important role here, but is not unique. This is how he describes the diverse criticism,

"The conservative fears that science will destroy the only world that they know; the progressive imagines that it will poison the paradise to come; the democrat is cautious of the tyrannous capabilities of technique; the aristocrat fears the leveling tendency of the machine."<sup>20</sup>

Essentially, in *Reliable Knowledge* Ziman wants to show that science—that is, the natural sciences—provides useful truths. He revises some characteristics of the natural sciences, which he considers are at the root of their reliability: the rigorous communication of results; the underlying rational language of mathematics; the accurate although simplified modeling of reality; the demanding codes of the scientific community. Above all, consensus is the key. But his strategy to defend the natural sciences does not limit itself to pointing at their strengths: he ends the book overtly attacking the social sciences in quite aggressive tones, arguing those lack "consensual status" and "do not constitute a working theoretical basis for social action"—Kuhn's notions of "normal science" and "paradigm" clearly resonate here. He warns,

"[I]nterpretive schemes that purport to go deep beneath the surface of social life, to uncover quite unsuspected forces, or to assert necessities that are by no means evident, must be treated with extreme skepticism. The scientific expertise of the aircraft designer or the heart surgeon is to be respected; claims to be able to 'engineer' a social system or to 'doctor' a faltering economy have no such justification in practice or in principle."<sup>21</sup>

He goes on, and boldly states that “[t]he challenge to the behavioural sciences does not come from physics but from the *humanities*,” and recommends novels such as *Anna Karenina*, *Madame Bovary* or *Pride and Prejudice* as a source of knowledge on the “psychology of social relations.”<sup>22</sup> Not surprisingly given the extreme point of view, in *Science* Robert Lindsay found Ziman’s arguments not very persuasive.<sup>23</sup> And in *Isis*, Robert Palter pointed at a serious inconsistency regarding his treatment of the history of science. Given Ziman’s emphasis on consensus, Palter finds “most disheartening” that,

“as most scientists, he has apparently given no serious thought to the possibility that history might, like science, possess its own special ‘paradigms of verification and proof’. One might even venture to suggest that history could provide some part of that sensible and consensual basis for the social sciences that Ziman is so dubious about.”<sup>24</sup>

Provocative, some times controversial, always open to discussion, Ziman never underestimated the public sphere. This is very well illustrated in his first collection of popular pieces, published in 1981. *Puzzles, Problems and Enigmas. Occasional Pieces on the Human Aspects of Science* compiles more than forty short articles and radio broadcasts issued in the previous twenty years. Among pieces on the many different concerns of a concerned scientist—Is war good for physics? Should developing countries do basic science? Why be a scientist?—, there is the reprint of the seminal 1960 article that was the basis of *Public Knowledge*—and of *Reliable Knowledge* as well. In its final paragraph, Ziman presents an analogy between the scientific community and the stock exchange that stresses the importance of the community over the individual in science, since the value of a theory is not judged by the “genius of its discoverer but depends on the price which other scientists will put upon it.” In such a system, honesty is understood in terms of long run market price, and independence of judgement and cooperation are the clues. As he concludes, “I doubt is there is any better instrument for achieving reliable knowledge of the world than a freely cooperating community of scientists.”<sup>25</sup>

### Becoming a classic

Ziman’s two following books mark a turning point in his career. Focused primarily on education, *Teaching and Learning about Science and Society*, published in 1980; and more importantly, *An Introduction to Science Studies. The Philosophical and Social Aspects of Science and Technology*, published in 1984, cannot be seen simply as an upgrade of *The Force of Knowledge*. In 1982, as already commented, he left Bristol and physics. He was appointed at the Department of Social and Economic Studies, at the London Imperial College of Science and Technology. The writing of *An Introduction to Science Studies* reflects this change, and makes him a classic author; with all its shortcomings, it is a must-read, a book any course on science studies should at least mention. Some authors celebrated its publication in quite enthusiastic tones. This is how Michael Ruse describes the core chapters of the book, which deal with the social aspects of science and science policy: “Simply stated, this is a brilliant synthesis, and should be compulsory reading for those who are concerned about or affected by science—namely everyone.” Ruse then goes on explaining his own view on how science should be taught to undergraduate students, and considers *An Introduction to Science Studies* presents the kind of contents science syllabus should include:

“I have frequently complained of the irrelevance of much supposed science undergraduate teaching. Most students will not be researchers, but teachers, sales persons, civil servants and so forth. What they need is not yet another course on biochemistry, but an introduction to the ideas sketched by Ziman. Read this book and you will agree with me.”<sup>26</sup>

In *Science*, Paul T. Durbin praised *An Introduction to Science Studies* “as a masterly summary of the whole range of the science studies literature, from philosophy of science to sociology of science to science and technology policy studies.” And pointed at some continuity with Ziman’s previous work, particularly with *Public Knowledge* and *Reliable Knowledge*. He also noticed that, compared to two previous collections on science and society issues—*Science, Technology and Society*, edited by Ina Spiegel-Rösing and Derek de Solla Price, and *A Guide to the Culture of Science, Technology and*

*Medicine*, edited by Durbin himself—, Ziman’s book represented “the perspective of the spokesperson for science,” and welcomed it.<sup>27</sup>

Even critics of the book treated it as a landmark. In *Isis*, Brian Wynne criticized what he considered its biased views on the social aspects of science, and noticed it does not mention the abundant bibliography on technical controversies, which showed “the fundamentally similar problems that exist within or beyond the social frontiers of a scientific or technological cadre.” He also considered the suggested bibliography—which ranges from J. D. Bernal and M. Polanyi to Bruno Latour, from Thomas Kuhn to Karl Popper and Paul Feyerabend, from Robert K. Merton to Roy McLeod and Dorothy Nelkin—was not the best available at the time. However, he acknowledged that students planning to pursue a scientific career could find the book “a useful starting point,” whose flaws are compensated by “the breadth of knowledge and the determinedly liberal spirit of discussion.”<sup>28</sup>

Revealingly, *An Introduction to Science Studies* has a section on “Academic scientism.” Ziman’s view on the social sciences is completely different here than it was in *Public Knowledge*. He reviews the two uses the word “science” has in English: one historical, broader, which encompasses “any orderly body knowledge or recognized branch of learning”; and another contemporary, which refers to “subjects such as physics, chemistry, biology and geology, and their associated technologies such as engineering, medicine and agriculture.” He then mentions the discussions surrounding the “semantic” confusion between the two. And he comments,

“But underneath the rhetoric there lies the very important question of the influence of ‘science’ (in the narrower sense) within academia. In other words, we are led to an investigation of the extent to which other academic disciplines are—or ought to be—regarded as equivalent to, or comparable to, the established natural sciences. The object of such an investigation should not be to prove, say, that sociology is ‘so’ like physics that it could be just as ‘true’, but to discover what such disciplines have in common, and what they may learn from one another.”<sup>29</sup>

He certainly had already begun to make peace with the social sciences. But not with the public at large: his ideas about the “public understanding of science” had not yet changed. In the one-page section he devotes to the topic in *An Introduction to Science Studies*, he still insists on and deplores the lack of factual information of the general public, and considers their views of science are “overwhelmingly instrumental.”<sup>30</sup> But that simplified view would also change soon, after his participation in the Bodmer Report and his examination of the results of the research program conducted immediately thereafter by the Science Policy Support Group he directed. Paraphrasing Brian Wynne and Roger Silverstone, he summarizes the “final message” of the research program,

“...scientific knowledge is not received impersonally, as the product of disembodied expertise but comes as part of life, among real people, with real interests, in a real world.”<sup>31</sup>

Ziman published two more very interesting books in the 1980s: *The World of Science and the Rule of Law. A Study of the Observance and Violations of the Human Rights of Scientists in the Participating States of the Helsinki Accords*, with Paul Sieghardt and John Humphrey, in 1984; and *Knowing Everything about Nothing. Specialization and Change in Scientific Careers*, in 1987. Although it shows an important aspect of Ziman’s personality and public engagement, we will say about *The World of Science and the Rule of Law* more than what is already well known: the importance of Ziman involvement in human rights’ causes. As John Ravetz would remind us in Ziman’s obituary in *The Guardian*,

“This [the book] made some eminent scientists aware for the first time of the realities, sometimes brutal, of science in its political context. During this period he was also engaged in ingenious and courageous work on behalf of refusenik scientists in the Soviet Union.”<sup>32</sup>

In turn, *Knowing Everything about Nothing* is an articulate effort to describe how the scientific system works, from the scientists’ point of view.<sup>33</sup> It is not an individual work, as one may imagine. It is not a finished, conclusive work either. It came up from three different rounds of talks Ziman and Ray Beverton had, after receiving a grant by the UK Research Council to examine the problems scientists were facing in planning their careers within a changing milieu, mostly marked by downsizing. The first

round of talks, with social scientists like Terry Shinn, Daryl Chubin, Dorothy Griffith, Gerard Lemaine, among others, confirmed an idea previously held by Ziman and Beverton: that a systematic reflection on the role of “scientists as specialists”—as they described their aim—had not been performed yet. That put them on another track, that of “social practice.” In the summer of 1981, they interviewed heads of research councils and of research laboratories, among other first rate scientists with managerial responsibilities. And to understand what was happening a bit down below—actually, the people who could be suffering the most because of the downsizing—, they also had informal meetings with more than one hundred ordinary mid-career researchers from 15 different institutions, ranging from the public sector to the industry and including academic institutions. This was certainly the most revealing round of talks. The records of these talks took 50 hours of tape and were eventually transcribed to be the basis and starting point of Ziman’s systematization—by that time, Beverton was already engaged in other commitments. However, writing the book revealed to be a “surprisingly laborious” task.

The whole project had been planned to explore “the general feeling that British science was going through a grave crisis of confidence.” Or, as Thomas Schott put it more directly, the “theme of the book” was “the inertia in a typical research career and the managerial promotion of career change (which is actually somewhat of a class struggle but not conceived as such).”<sup>34</sup> Its results were published when things had already got worse, since more cuts had been implemented, and a general reorganization of science was underway. In the end, Ziman found out that the scientists interviewed revealed to be “more versatile than they tend to believe.” Although the book disregards bibliography which might have been illuminating, particularly regarding autonomy—“a useful concept in studies of other kinds of occupations” and “a strong norm of value in academic or pure science,” as Schott pointed out—,<sup>35</sup> it certainly constitutes a remarkable effort to try to describe a scientific community as it evolves into collectivization, taking into account the insiders’ views of this change. A perspective that Ziman always considered necessary and sometimes privileged, as he argued in *Public Knowledge*.

### **Prometheus bound, unbound**

Regarding Ziman’s theoretical contributions to the ongoing reflection on the changes in science, in his 1995 account he first points at a 1978 article in *Minerva*, “actually a review of Bruce Smith and Joseph Karlesly’s disturbing account of the then state of academic science in the U.S.A.,” whose title he would then go back to. Then, he mentions another article in *Minerva* in 1981, when he noticed “various structural effects, such as increasing societal influence on problem choice in fundamental research.” A certainly more mature version appeared in his remarkable Bernal Memorial Lecture in 1983, when he pointed at “the shift away from the traditional individualism of scientific activity.” This is the “collectivization of science.” The first two paragraphs of this lecture suitably touch upon the most important issues involved in this process:

“The modern ‘R&D’ system is undoubtedly beneficial to society, and to the advancement of knowledge. But collectivization has not only changed the societal function; it has also changed its internal sociology. Personal discretion in the choice of research projects is now severely limited, even in the university sector, because most projects are not funded by outside agencies. Tension between the individualists norms of the academic tradition and managerial principles derived of the industrial tradition has made research an ambivalent profession. Should scientists be regarded as members of a transnational community devoted to the ‘search of truth’, or are they simply typical employees of governmental and commercial organizations with very wordly aims? This ambivalence is evident in controversies over scientific freedom and responsibility, and in the ethical problems of military research.

“Collectivization generates pressure for efficiency and public accountability. The R&D system is then driven from the top toward more utilitarian programmes. Is highly innovative research fostered adequately by the method of awarding project grants on the basis of peer review, which is often accused of being ponderous and unadventurous?”<sup>36</sup>

Each sentence summarizes a world—and a different aspect of collectivization. First, Ziman refers to the two functions science has, predominantly in relation to the external world—that is, society at large—; and to the internal world—that is, the scientific community. The changing balance between these two

functions is precisely at the root of the conflict collectivization introduces. That is why it is not only the relationship between science and society that is changing, but also science itself: scientists' freedom to choose their research topic has become more limited. There is "tension" between the traditional Mertonian norms, and those of the industry—Ziman mentions specifically the problem of "universalism" in science (we will analyze this later on). And he closes the first paragraph addressing a problem that would increasingly preoccupy him: how can scientists be held responsible if they are not free to take decisions? What kind of ethics is to be developed to deal with the problems collectivization brings in or exacerbates?<sup>37</sup> And the second paragraph introduces in a nutshell the problem of science policy, pointing at the criteria to orient the course of the scientific endeavor.

Apart from *Of One Mind: The Collectivization of Science*, which is a collection of articles and popular pieces that takes its title precisely from the Bernal Lecture we have just quoted, the two key books that thoroughly discuss the collectivization of science are *Prometheus Bound. Science in a Dynamic Steady State*, published in 1994; and *Real Science. What It is and What it Means*, published in 2000. In these two books Ziman sums up and organizes within a theoretical framework most of the topics he had been reflecting on for much of his career.

At the core of Ziman's analysis in *Prometheus Bound* there is the dispute between the traditional Mertonian norms usually conceived as ideals for scientists, and the industrial norms newly introduced in academic science. Ziman uses the acronym CUDOS to summarize Merton "scientific ethos": communalism, universalism, disinterestedness, originality and skepticism. And he coins the acronym PLACE to list the characteristics of industrial science: proprietary, local, authoritarian, commissioned and expert. These two sets of norms began to compete to guide academic research because of budgetary restrictions and, more generally, because science could not continue the expansion De Solla Price described—science entered "the steady state," Prometheus is now "bound." These changes are both "radical" and "irreversible," and imply a more porous boundary between science and technology. In this new situation, "collectivization" refers specifically to the increasing need to share costlier equipments and facilities, and to the kind of transdisciplinary work encouraged. Concomitantly, a more articulate vision of science policy is now required, with novel, more stringent criteria of "scrutiny" and "accountability." In turn, "bureaucratization" is an inevitable consequence of a novel managerial approach. Scarcity of funds may well improve research quality—as Ziman a bit too willing concedes—but certainly it takes its toll on scientists, since it introduces "anxiety" and impacts heavily on career planning. And the proprietary character of the new knowledge may have an effect on collaboration, and on diffusion of science.<sup>38</sup>

*Prometheus Bound* made history; it was both intensely contested and praised. Some critics saw the book not as a description, even less as an admonition, but as a justification for the current state of affairs—maybe due to Ziman's previous involvement in science policy, in particular the times when he was close to Margaret Thatcher's administration as director of the Science Policy Support group.<sup>39</sup> In the UK, James H. Sang wrote about the book very straightforwardly, suggesting this connection. Regarding the decline in public funding, he observed Ziman says little about its origin. As he put it,

"There is no analysis as to how this state of affairs came about, except that the slump of the 1970s-1980s may have acted as a trigger for the change of structure and organization of science currently imposed by 'society'. Surprisingly, there is no recognition that today everything is organized to maximize the free market, initially Thatcher's market but now that of most Western governments."<sup>40</sup>

Sang sounded very angry—perhaps his views were part of the kind of "class struggle" Schott mentioned in his review of *Knowing Everything about Nothing?* He even used ad hominem arguments: he described Ziman as "a retired British solid state physicist who is devoting his spare time to the present state of Western science, mostly in terms of his own experience."<sup>41</sup>

But praise of the book was also powerful. In *Science*, Rodney Nichols considered it a crucial input to the ongoing discussion on science policy:

"Across the industrialized world, strategic planning for basic science is the rage. From Tokyo to Brussels and Washington, the common purpose is to use scarcer funds more productively, in order to serve national economic goals. But there is a catch: no one knows confidently how to plan science more reliably now than in the past. ...

"*Prometheus Bound* is a masterly contribution to our thinking on these issues."<sup>42</sup>



Contrary to the kind of criticism Sang's review epitomizes, Nichols took *Prometheus Bound* as an alerting message on the perils science faces. After a detailed account of the book contents, and of the many arguments and suggestions presented by its author—"a distinguished British scientist"—, he recommended:

"Let every minister and legislator, every journalist and citizen, and, yet, scientists and engineers too, go to Ziman's inspiring seminar. An antidote to facile judgements, his essay is also a wonderful read. With wit, grace, and light touches of erudition, Ziman clarifies the challenges of managing the enterprise of discovery. The horizonless enterprise may be in danger, says Ziman, because of new requirements and practices 'so ill judged that they could do lasting damage to the health of science and its efficacy as a social institution'."<sup>43</sup>

*Prometheus Bound* was also read in close relation to another book published that same year: *The New Production of Knowledge. The Dynamics of Science and Research in Contemporary Societies*, by Michael Gibbons, Camille Limoges, Helga Nowotny, Simon Schwartzman, Peter Scott and Martin Trow. It talks of a similar transition from a Mode 1—identified as "traditional knowledge"—to a new Mode 2—which describes knowledge generated in broader, transdisciplinary, transinstitutionally contexts. One of the many interesting aspects regarding the joint influence of these two books is that they promoted new lines of research, particularly regarding the interpretation of hard data on science, and on science policy. One early example was conducted by Diana M. Hicks and J. Sylvan Katz.<sup>44</sup>

*Real Science*, in turn, essentially reworks and better systematizes the model presented in *Prometheus Bound*. It also introduces chapters on the philosophy of science—a discipline most cherished by Ziman—and analyzes the new science through this lens, looking for its epistemological roots. Another key contribution is the notion of "post-academic science" to name the new situation described, instead of the grim metaphors of "Prometheus bound" and the "steady state," which evoke either restraint or stagnation. Is this science as "reliable" as the one we once have? Ziman answers:

"To put it simply: post-academic scientists still formulate and try to solve practical and conceptual problems on the basis of their shared belief in an intelligibly regular, not disjoint, world outside themselves. They still go on theorizing, and testing their theories by observation and experiment. They still try as best they can to eliminate personal bias from their own findings and are extremely canny on the acceptance of the claims of others. To that extent at least, we, the public at large, have just as good grounds as we ever did for believing (or doubting) the amazing things that 'science' tells us about the world in which we live."<sup>45</sup>

Ziman's last book is a balanced, thorough, confident account of contemporary science. The magnificent legacy of a fifty-year scientific career.

## Notes and references

<sup>1</sup> J. Ziman, *On being a physicist*, in *On One Mind: The Collectivization of Science*, Woodbury, NY, American Institute of Physics, (1995) pp. 379-394.

<sup>2</sup> J. Ziman, (1995)pp. 386 and 390.

<sup>3</sup> J. Ziman, (1995) p. 391

<sup>4</sup> J. Ziman, (1995) pp. ix-xii)

<sup>5</sup> J. Ziman, (1995), p. xv. Emphasis is mine.

<sup>6</sup> J. Ziman, (1995), p. xvi.

<sup>7</sup> J. Ziman, (1994), *Prometheus Bound. Science in a Dynamic Steady State*, Cambridge, Cambridge University Press, p. vii.

<sup>8</sup> J. Ziman (1994), p. viii.

<sup>9</sup> Many ideas presented in *Public Knowledge* were anticipated in a brief essay revealingly titled "*Science is social*", *The Listener*, 18 August 1960. We will touch upon this article later on.

<sup>10</sup> J. Ziman, *Public Knowledge. An Essay Concerning the Social Dimension of Science*, Cambridge, Cambridge University Press, (1968) p. ix. Emphasis in original.

<sup>11</sup> A.E. Musgrave, "*Public Knowledge. An Essay Concerning the Social Dimension of Science*", book review, *The British Journal for the Philosophy of Science*, 20 n.1 (1969) pp. 92-94, on p. 92.

<sup>12</sup> H. Zuckerman, "*Public Knowledge. An Essay Concerning the Social Dimension of Science*," *Political Science Quarterly*, 85 n.1 (1970) pp, 116-117, on 116.

- <sup>13</sup> J. Ziman (1968), pp. x-xi.
- <sup>14</sup> J. Ziman, (1976), *The Force of Knowledge. The Scientific Dimension of Society*, Cambridge, Cambridge University Press,
- <sup>15</sup> J. Ziman (1976), p. vii.
- <sup>16</sup> See, for example: “*The language in which most modern scientific ideas are expressed and grasp takes years to learn, and cannot be paraphrased for easier comprehension*”, J. Ziman (1976), p. 119. On the dominant view of popularization, see: S. Hilgartner, “*The dominant view of popularization: Conceptual Problems, political uses*”, *Social Studies of Science* **20** (1990) pp. 519-539.
- <sup>17</sup> R. Olson, “*The Force of Knowledge. The Scientific Dimension of Society*,” *Technology and Culture* **18** n.2 (1977) pp. 264-276, on p. 65.
- <sup>18</sup> J. Ziman (1976), p. 239.
- <sup>19</sup> J. Ziman (1976), p. 10.
- <sup>20</sup> J. Ziman, (1978), *Reliable Knowledge. An Exploration of the Grounds for Belief in Science*, Cambridge, Cambridge University Press, p. 1.
- <sup>21</sup> J. Ziman (1978), p. 184. Emphasis in original.
- <sup>22</sup> J. Ziman (1978), p. 185.
- <sup>23</sup> R. Lindsay, “*The claims of science*”, *Science* **205** n.4406 (1979) pp. 576-577.
- <sup>24</sup> R. Palter, “*The reception of unconventional science; Reliable Knowledge. An Exploration of the Grounds for Belief in Science, Isis*, **71** n.4 (1980) pp. 660-661.
- <sup>25</sup> J. Ziman, *Puzzles, Problems and Enigmas. Occasional Pieces on the Human Aspects of Science*. Cambridge, Cambridge University Press (1981).
- <sup>26</sup> M. Ruse, “*An Introduction to Science Studies. The Philosophical and Social Aspects of Science and Technology*,” *The Quarterly Review of Biology*, **61** n.3 (1986) p. 391.
- <sup>27</sup> P.T. Durbin, “*Science surveyed*”, *Science* **229** (1985) pp. 459-459.
- <sup>28</sup> B. Wynne, “*An Introduction to Science Studies. The Philosophical and Social Aspects of Science and Technology*,” *Isis*, **79** n.1 (1988) p. 129.
- <sup>29</sup> J. Ziman (1984), pp. 187-188.
- <sup>30</sup> J. Ziman (1984), pp. 184-185.
- <sup>31</sup> J. Ziman, “*Public Understanding of Science*”, *Science, Technology, and Human Values* **16** n.1 (1991) pp. 99-105.
- <sup>32</sup> J. Ravetz, “*John Ziman*”, *The Guardian*, 2 February 2005. Available at: <http://www.guardian.co.uk/obituaries/story/0,3604,1403544,00.html>.
- <sup>33</sup> J. Ziman, (1987), *Knowing Everything about Nothing. Specialization and Change in Scientific Careers*, Cambridge, Cambridge University Press.
- <sup>34</sup> T. Schott, “*Knowing Everything about Nothing. Specialization and Change in Scientific Careers*” book review, *Contemporary Sociology* **18** n.2 (1989) pp. 264-265.
- <sup>35</sup> T. Schott (1989) p. 265.
- <sup>36</sup> J. Ziman, “*Bernal Lecture 1983. The collectivization of science*,” *Proceedings of the Royal Society of London*, **B 219**, (1983) pp. 1-19. It was reprinted in Ziman (1995), pp. 337-359.
- <sup>37</sup> A well remembered opinion piece on ethics would be: J. Ziman, “*Why must scientists be more ethically sensitive than they used to be*”, *Science* **282** n.5395 (1998) pp. 1813-1814.
- <sup>38</sup> J. Ziman (1994).
- <sup>39</sup> J. Ravetz (2005).
- <sup>40</sup> J.H. Sang, “*Prometheus Bound. Science in a Dynamic Steady State*,” *The Quarterly Review of Biology*, **71** n.2 (1996) pp. 267.
- <sup>41</sup> J.H.Sang (1996), p. 267.
- <sup>42</sup> R. Nichols, “*Pipers and tunes in science*”, *Science* **264** n.5161 (1994) pp. 983-984, on p. 983.
- <sup>43</sup> Rodney (1994), p. 984.
- <sup>44</sup> D.M. Hicks and J. Sylvan Katz, “*Where is science going?*”, *Science, Technology and Human Values* **21** n.4 (1996) pp. 379-406.
- <sup>45</sup> J. Ziman, *Real Science. What it is and What it Means*, Cambridge, Cambridge University Press, (2000) on pp. 330.

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