Chapter 5
Science in Social Contexts

Abstract Gradually since 1990 a growing number of critical analyses from within science have been published of how science was organized as a system and discussing its problems, despite, or paradoxically because the growing size of its endeavour and its growing yearly output. Because of lack of openness with regards to sharing results of research, such as publications and data but in fact of all sorts of other products, science is felt by many to be disappointing with respect to its societal impact, its contribution to the major problems humanity is facing in the current times. With the financial crisis, in analogy, also the crisis of the academic system as described in Chap. 3 was exposed and it seemed that similar systemic neoliberal economic mechanisms operated in these at first sight seemingly different industries. Most of these critiques appeared with increasing frequency since 2014 in formal scientific magazines, social media and with impact reached the leadership of universities, government and funders. This raised awareness and support for the development of new ways of doing science, mostly intuitively and implicitly, but sometimes explicitly motivated by pragmatism aiming for societal progress and contribution to the good life.

To get to this next level we need the critical reflection on the practice of science as done in previous chapters in order to make systemic changes to several critical parts of the knowledge production chain. I will discuss the different analyses of interactions between science and society, in the social and political contexts with publics and politics that show where and how we could improve. The opening up of science and academia in matters of problem choice, data sharing and evaluation of research together with stakeholders from outside academia will help to increase the impact of science on society. It ideally should promote equality, inclusion and diversity of the research agendas. This, I will argue requires an Open Society with Deweyan democracy and safe spaces for deliberations where a diversity of publics and their problems can be heard. In this transition we have to pay close and continuous attention to the many effects of power executed by agents in society and science that we know can distort these 'ideal deliberations' and undermine the ethics of these communications and possibly threaten the autonomy and freedom of research.

F. Miedema, *Open Science: the Very Idea*,
https://doi.org/10.1007/978-94-024-2115-6_5

In Chaps. 2 and 3, I discussed the current state of science and the underlying assumptions and images of science. I have shown how this has determined the mainstream 'idea of science and scholarship' and how this has distorted the practice of scientific inquiry and academic culture. It was discussed in Chap. 3 how this still has major impact on science and on the community of scientists. In this Chapter, I will focus on how it disturbed the external relationship between science and society. In the previous chapter I have argued for a more realistic vision of scientific inquiry, beyond positivism and empiricism, as found in pragmatism. Pragmatism, I argue, may help to reshape science and the practice of inquiry to restore the practice of science and importantly its relationship with society and increase in a meaningful way its impact on our social life. The idea of inquiry in pragmatism's theory of inquiry is 'outside in'. Research starts with a problem in social life or something the scientists assume lacks proper understanding and is a cause of uncertainty. It is concluded that the problem relevant for science and or social action based on new knowledge. As a result, knowledge claims are produced that are tested in the contexts where the problem of inquiry surfaced. In this chapter I will from this perspective discuss the current ideas about the relationship between the inside and the outside; science and experts and the relevant publics in societal contexts. I will describe some very recent initiatives aimed at novel, or sometimes rediscovered methods to organize science in academia to improve impact. First let's look at critical thinkers and social experiments in the field of Science and Society that have walked these roads before.

I have, in Chap. 1, discussed the critical reassessment of science mainly by politics in the late 1960s, that one may assume, has resulted in the first serious wave of Science and Society after WW 2, that lasted some twenty years between 1960 and 1980. Inspired by the critical social science theorists of the Frankfurter Schule, Marcuse and Horkheimer, our thinking about the interactions between science and society went through a next phase of 'critical theory' in Europe. Dominant thinkers were Habermas, Foucault and Bourdieu and later Giddens, Beck, Lash, Barnes, Edge. They were highly critical about the role of science in society for different theoretical or socio-political reasons. Some argued against the technocratic dominance with its alienating and distorting social effects (Marcuse, Foucault, Habermas, Toulmin, Illich, Beck, Giddens). Some, from a neo-Marxist but also social-democratic perspective, pointed out that not only government with its military interests, but increasingly multinationals had taken over science and that science should be regained and redirected to be an emancipatory force in society (Marcuse, Habermas, Rose and Rose). This movement of 'humanizing modernity' as Toulmin did describe it in 1990 (Toulmin, 1990), questioned the practice of science, its self-image and with it the ideological dichotomy between the 'hard' rational sciences and the 'soft' social sciences and humanities which also juxtaposed 'timeless, abstract, universal, context free' against 'practical, local, transitory and context bound':

> *…the issues at stake were broached during the 1960s and 70s, in a public debate about the aims of higher education and academic research. The debate was dominated by two vogue words: on one side "excellence", on the other side, "relevance". The spokesman for*

> *"excellence" saw institutions for higher learning as conserving the traditional wisdom and techniques of our forefathers, while adding to the corpus of knowledge. The focus was on the values of established disciplines….: the subjects should keep their intellectual instruments polished and sharpened….at all cost preserving their existing merits. The spokesman for "relevance" saw matters differently. In their view it was not valuable to keep our knowledge oiled, clean and sharpened, but stored away: it was more important to find ways of putting it to work for human good. From this standpoint, the universities should attack the practical problems of humanity: if established disciplines served as obstacles in this enterprise, new interdisciplinary styles of work were needed… The inherited corpus of knowledge was no doubt excellent in its way, but academics in the 1970s could no longer afford to behave like Mandarins* (Toulmin, 1990)(p184,185).

In these days the call for societal relevance of academic research was strong and many university academics were visibly active in public and political debates. This shift was indeed also seen in the research agenda of academia as described very insightful by Toulmin (Toulmin, 1977). He wrote: from *'the focus on disciplinary autonomy and excellence and the pursuit of pure knowledge and technical refinement', From 'Leave us alone to do our own academic thing. Take away your concrete interdisciplinary problems, to knowledge focused on problems and issues that are relevant for human applications'*.

Free Chemistry

After the twenty years of economic growth and prosperity after WW2, in the sixties a widely felt threat and danger of the Cold War, of global nuclear war and the war in Vietnam was felt. The war in Vietnam, which from at least 1967 determined daily prime-time radio and TV evening news in the US and Europe was a dominant divisive political issue, also at the dinner table in my family home. Footage from the battle fields on a daily basis are considered catalysers of inducing a broader disappointment and distrust in the younger generation of the role of science and technology in society. The new generations had not experienced the effects of the war or the poverty of the Great Depression and experienced liberty and freedom to make up their own mind, less dependent on the 'old politics' and sociocultural ideas dominated by religion. It was a mix of worries about pollution and environmental threats expressed in the works of Rachel Carson and The Club of Rome, and in the seventies of recession and gloomy socioeconomics.

For this change of our appreciation of science therefore, historians and sociologists of science point to the cultural and political developments in the 1960s. The historic anti-establishment movements of the summer of 1968, mainly from students in the US, France, Germany and also in some other countries in Europe were however short-lived (Miller, 1994). Still, for twenty years they have had a significant effect on science and its relationship with society. Science was seen as the main power in society that could do harm but when tuned to the needs of society could do a lot of good. Famous initiatives

(continued)

were the ‘science shops’ in The Netherlands and a unique nationwide public debate in our country in the late seventies on nuclear energy in which many of my friends actively participated. When I entered university in 1971, vigorous debates went on about the role of science in society, social responsibility of scientists and who controlled the curriculum. Friends of mine after their B.Sc. in chemistry went on doing a M.Sc in Free Chemistry in Groningen, a mix of chemistry, science studies, social theory and sociology and easily found interesting jobs in these fields after graduation.

Several mostly local and national movements in the 1960s and 70s, both in academia as in the political domain, have responded to the disconnect between science and society. Science and Society and later Science and Technology Studies (STS) became an academic trans-discipline in the late 1970s with its critical stance and appeal for responsible science and societal relevance. The movement inspired the idea of legendary Science Shops, and many other quite different forms of public participation, public hearings, problem-driven bottom-up movements where citizens and lay publics could meet with academic experts for help, advice but also influencing and building joint research agendas. In many countries, academics became organized to become politically and socially active. Conceptually, this in some respect developed in parallel with the development and critique of the popular image of science described in Chap. 2. Studies from sociologists, political theorists, and from the then newly established field of STS about positive and negative interactions between science and society provided insights for these actions (Ravetz, Blume, Rose and Rose, Habermas, Sarewitz, Guston, Bijker, Rip, Meulen). These analyses, and actions, have led to many small-scale local actions and interventions to engage and increase societal relevance in the practice of research. Despite all this, these movements from outside and inside universities have not changed the practice of mainstream academic science in the longer run.

COVID-19: the public looks on and talks back.

As I am writing this, March 30, 2020, we are in the first surge of the Corona Crisis, the COVID-19 pandemic. In times of war and crises like the corona crisis the dangers and pressures are such that the response from government goes beyond partisan lines, one would think. Not always. Anthony Fauci and Deborah Birx have just yesterday convinced Donald Trump that the virus is not a hoax of the Democrats. That it really is a very serious health problem, with a high death toll to be expected even when the US government in close collaboration with experts in the public health system responds adequately. Experts these days are talking with the responsible politicians, are on news

(continued)

and talks shows everywhere you look. The people after some time in large majority accept their advice, no matter how disruptive to social life and economy. When this crisis develops, it is being asked why ‘we’ have not invested more in health care, public health intelligence and research and why there are no facilities and institutions who can ramp up to meet the scale of this pandemic. Indeed, we stare at the screens and start to reflect on who and on which grounds we are making the choices for all kind of things of science and technology and how that shapes our social life. This COVID-19 problem is a threat so immense and as its effects are highly visible on the evening news, that there is unanimity regarding expert opinion. In response politics and publics ask from science: ‘Screening, testing, treatment, therapy and a vaccine, now’. Dealing with uncertainty and its resulting insecurity about the course of the pandemic is unbearable. It happens that in some daily discussions in the media non-experts denounce the experts for lack of certainty and adjustments they make in their science analysis and advice. Risk of disease and death, in our times are unacceptable. Experts in the field of infectious diseases, however, know uncertainty from experience and from the recent history of pandemics, despite their excellent modelling based on high quality mathematics and sophisticated biology. They are openly and honestly declaring that there are many critical unknows and new data keep coming also to them, The public, in parallel, via the media see on a daily basis new data coming that is immediately before their eyes and used by scientists to update the models which changes the predictions and informs policy. This is hypermodernity. The scientists study the virology and public health but also social and economic disruption and are weighing the evidence. The publics in the meantime, with a feeling to be subjects of the study, are aware and asked to adapt their behaviour to influence reality. Researchers, in fact are mediators between science and politics, like Fauci, and politicians are deliberating every day to come to the best policies to deal with the pandemic. Weighing health risks, economical risks and social disruption, the politicians in the end have to decide. This functions best in democracies when free flow of information, communication and undistorted discourse is possible, which even in modern democracies is not obvious, as we have seen not only in the White House Corona briefings over the past months.

5.1 The ‘Pragmatic Model’ in Frankfurt

Most scholars in the philosophy of science and political theory recognize Habermas to be the most important link between American Pragmatism and European Continental philosophy. As discussed in Chap. 3, Habermas in his *Knowledge and Human Interests* discussed at length the work of Peirce on the logic of inquiry (Habermas, 1971). This is emphasized by Habermas in the Appendix which is his

inaugural lecture of June 1965. He explicitly endorsed the essence of pragmatism in the relation with real world problems and the values that in inquiry do come with them and the role of the community of inquirers in the process of defining acceptable knowledge claims. In *Technik und Wissenshaft als Ideology* (Habermas, 1968, 1970) that I bought in June 1976, he describes the penetration or in his words, the rationalization of the social sphere by science and technology in our modern late capitalist Western societies. Science refers in this context to the natural sciences with their positivistic philosophy. The classical separation, he argues, between science and its knowledge and the social life in society does not exist anymore and this has two results. The sciences are coupled to and are drivers of economic and technologic innovation shaping and dominating our social life. At the same time, they became uncoupled from the humanities, 'from the humanistic culture', with which reflection on its practice is lost (p55) (Habermas, 1970). Habermas argues that the capacity to control nature and social life are assets of science, which has allowed for at least the most of us, to live a better life and in comfort, but that capacity has become the problem. The institutionalized rationalization that comes with science and technology, is largely uncoupled from the needs and problems of the diverse publics and has become dominant and repressive. The logic of science and technology as a power, he argues, penetrates society and politics, has its own intrinsic dynamics, which brings the problems in social life as we see them unfold. Habermas proposes that science and the publics should work on their 'self-understanding' in order, citing Dewey, to be able to come to a *'pragmatistic model'* that is associated with democracy in which *'the strict separation between the function of the expert and the politician is replaced by a critical interaction. This interaction not only strips the ideological supported exercise of power of an unreliable bias of legitimation but makes it accessible as a whole to scientifically informed discussion, thereby usually changing it'* (p66, 67) (Habermas, 1970). This model allows for social interests and their value systems to be played out in the deliberations and allows for true legitimation of policies before the public. He continues discussing Dewey:

> *For Dewey it seemed self-evident that the relation of reciprocal guidance and enlightenment between the production of techniques and strategies on the one hand and the value-orientations of interested groups on the other could be realized within the unquestionable horizon of common sense and an uncomplicated public realm. But the structural change in the bourgeois public realm would have demonstrated the naïveté of this view even if it were not already invalidated by the internal developments of the sciences* p69.
>
> He refers specifically to the confusion of *'the actual difficulty of effecting permanent communication between science and public opinion with the violation of logical and methodological rules. True, as it stands the pragmatic model cannot be applied to political decision making in modern mass democracy. The reason is….the model neglects the specific logical characteristics and the social preconditions for reliable translation of scientific information into the ordinary language of practice and inversely for translation from context of practical questions back into specialized language of technical and strategic recommendations*.' (p70).

He argues that in USA politics since the war this is being practiced and describes the necessary sequences of actions in such a case. He argues for a long-term science policy which *'attempts to bring under control the traditional, fortuitous unplanned*

relations between technical progress and the social life-word.' (p72*).* He, with Dewey, is thus well aware that the ideal conditions for this pragmatic model are generally not present. Habermas, as Dewey and the pragmatists, is neither a nihilist nor hard-boiled sceptic paralysed by the idea that everything is determined and defined by power games and by unchangeable practices of repression and domination. Importantly, Habermas, despite coming from a Marxist tradition of political theory, and being the successor of Marcuse and Horkheimer at the Frankfurter Schule, does not see human values and interests per definition as distortive forces in the interaction between science and politics. It is the belief in human agency and the trust that communication and ethical discourse is possible and wanted by most, but they have to be consciously, monitored, regulated and managed in well-designed and carefully executed open democratic processes. As in most of his later works of political theory, communication, mediation and discourse ethics, freedom from domination and repression, is essential in all phases of societal development and social action where these deliberations need to take place. In his emphasis on communication Habermas builds heavily on the work of the sociologist George Herbert Mead a prominent pragmatist in the early decade of the previous century.

Finally, citing D.J. de Solla Price's now famous studies published a few years before, (Price, 1963) he mentions the problems of specialization and barriers between disciplines and scientific communication with the overwhelming numbers of articles and journals and issues of military research and secrecy. He describes the requirements of political and institutional advisory bodies, societal organization and the organization of the research process, that will facilitate the model. Remember, these were the days when environmental issues, nuclear energy, radioactive waste and the nuclear arms race, the first signs of the energy crisis and a war in Vietnam for which the motives and logic had long evaporated, were the topics of major public concern, debate and protests. In this technocracy the publics felt alienated in all kind of respects as they felt that their issues and concerns were not being dealt with. These were seen as the consequences of blind belief in and application of the natural sciences not only to the war in Vietnam which aroused massive political movements and the student protests of 1968. In this stage of capitalist society, the old Marxist materialistic dialectics of 'capital and proletariat' had lost bite. Because of the atrocities of the Stalin regime that were generally acknowledged and condemned and because synergy of science and capitalism has brought enormous economic welfare, at least in the West. The discussion was whether science and technology are in the true humanistic meaning of the word being used to relieve hardship and inequality and promote 'the good life'. It seemed that science and technology that claimed to be neutral, was exploited by commercial and military interests that were not effectively being controlled by public deliberation in our democracies. It has been argued that al lot of research can be initiated without the involvement and mediation of governmental agencies when a public focussed around a well-articulated problem talks with researchers. This can be at the national level but is increasingly happening at regional levels when networks have been established of citizens and representations of science and research around major themes like public health, welfare or environmental policy. This problem of the relation between

experts, the public and politics thus concerns the democratic way to set the research agenda and how expert scientific knowledge is taking into account in the formation of national and local governmental policies. Leaving out the (neo-Marxist) jargon and undertones, this piece could after more than 50 years easily be mistaken to be part of a paper on Open Science in our time and age pointing to the Sustainable Development Goals of the United Nations, a list of societal issues that is in number and calamitous impacts not less compared to the list of 1968.

A Fellow Traveller in Science in Transition

The initiators of Science in Transition, as described in Chap. 3, had very different experiences and diverse perspectives on science and academia. Introducing one of them, Huub Dijstelbloem, in this particular chapter is appropriate and of relevance. Dijstelbloem, is since 2009 a Senior Researcher at the Dutch Scientific Council for Government Policy (WRR) and since 2015 also appointed professor of philosophy at University of Amsterdam. Before that he was a Program Coordinator at the Rathenau Institute, the institute of the Royal Academy in The Hague which advises government policy on science, technology and innovation. I didn't know Huub, but his straightforward contribution to the Spui25 debate of October 2012 was in agreement with my own views about science. We briefly met that evening after the debate and decided that we should keep in touch in order to jointly prepare for a public action. The end of November 2012, over a simple dinner in 'The Ysbreeker' overlooking the Amstel, we exchanged our views and ideas and started to talk about a plan. Our professional backgrounds were quite different, but we had a very important common interest, namely the problematic interaction between science and society. We had both come to the conclusion that there were two problems there: how science was organized and how its communication with politics and publics was thought of and organized. Just the year before he had edited (Dijstelbloem, 2011) with Rob Hagendijk a very nice book about trust in science with modern philosophical and sociological perspectives. When we started, I discovered his work on research evaluation done with Jack Spaapen (KNAW) which obviously was most relevant to our later discussions on Incentives and Rewards (Spaapen et al., 2007). The current chapter deals with one of the major topics that Science in Transition believed should be addressed to improve the practice and impact of science: science in the societal context. This happens to be the topic of Huub Dijstelbloem's appointment with the Dutch Scientific Council for Government Policy (WRR) where he contributed his views to many advices and reports. His PhD thesis published in 2007 and much of his work since 2007 is about the science and society interface, mainly about policy advice. In his thesis he discusses the problems of the interaction and deliberation between scientific experts, representatives from the public and politics. He takes pragmatism as his main conceptual perspective, starting from Dewey's *The Public and its Problems*, (Dewey & Rogers, 2012),

(continued)

Latour's work on Pasteur (Latour, 1988) and the work of Habermas discussed in this chapter. In his thesis he analysed in great detail the fascinating case of the response of the government and public institutes to the HIV epidemic in 1982 in the Netherlands (Dijstelbloem, 2014a, b). He described the case in which the blood supply foundation asked the gay community to voluntarily refrain from donating blood, with major roles for Vincent Eijsvoogel and Pim van Aken, directors of CLB Blood Transfusion Service at Amsterdam (now Sanquin), Roel Coutinho the director of the Municipal Health Service of Amsterdam and representatives of the community of gay men. This for me is special, as I was in these days doing research at CLB and knew the issue and nearly all the actors in this significant example of boundary work between science and the public.

5.2 The Problem of Power

The ideas of Dewey and also Habermas about deliberations between experts and representatives from the public and politics have by many scholars been seriously criticized for being naïve with respect to the distorting effects of all kinds of power and on too much reliance on the proper functioning of formal institutions. Although both men were aware of these distorting forces, they believed that in principle people are to be considered moral beings that want to achieve the conditions that allow for the betterment of social life, 'the good life'. Habermas has his whole life, literally until this day, worked to develop this concept of 'discourse ethics' and 'communicative reason' which provides the intersubjective foundation for actions of people and institutions. Dewey emphasized social and personal education to emancipate the public and to provide them with the means to engage in civil society. This was not as much a believe, but for Habermas a moral principle on which most people build, when they engage in social and political life. This communication, in contrast to dialectics and conflict was also the main theme in the work of George Herbert Mead (1863–1931) another major American pragmatist who was an inspiration to Habermas. In this perspective communication and language are powerful instruments to deal with subjectivity, to achieve intersubjectivity as a form of objectivity and importantly to expose the misuse of power in pursuance of particular interests. Here we see the concept of 'objectivity' making the same turn in social theory that in pragmatist epistemology turned from individual to 'intersubjective'. Habermas disagreed with the philosophy and writings of Foucault, Nietzsche and others, in which the corruption and domination by language and communication is central in how power is executed and totally penetrates social life. Foucault and others analysed and exposed the distortions of inequalities of power and the adverse and perverse effects of misuse of power in many major sectors and institutions of society like medicine, sexuality, education, law in which discipline and punishment are practiced from without positions of power using language and communication to achieve to discipline. With Nietzsche in the background, these analyses imply

that it is hard to image how to curb these perverse effects of power and thus hard to avoid scepticism and nihilism.

Bernt Flyvbjerg in *Making Social Science Matter* (Flyvbjerg, 2001) criticizes the social sciences from within and starts from the perspective that power is pervasive in social life and politics and has to be dealt with when we are thinking about communication as driver of social action in the public sphere. Flyvbjerg, inspired by Bourdieu and Latour, writes about social theory and he also empirically studied social actions at local levels of citizenship and governments. In these contexts, he sees values and power as prominent factors in debates. He concludes, correctly, that Foucault and Habermas are both very aware of the problem of power but approach them differently and complementary. Habermas indeed argues for engaging with publics focused around problems but believes this must be taken up by the institutions and change the relevant institutions and agencies. Foucault sees institutions often as part of the problem, because they inevitably will define their own goals and agenda. He believes that issues of power will have to analysed, understood and dealt with in the specific contexts where they occur. Habermas, with Dewey, clearly tries to avoid and anticipate situations of conflict through proper communication and understanding of all sides, but Foucault of course interprets the evasion of conflict as suppression and restriction of freedom. In agreement with other scholars, Flyvbjerg sees conflict not as a danger per se, but believes that it can result in new opportunities and change. Hence if one engages about controversial issues of social action with parties with different interests, one has to choose, dependent on the context of the issues, which of these approaches to employ. Obviously, this also depends on the level of democracy of governance at the regional or national level where you are. It is of interest to read in exactly this context Diggins account how the American framers of the Constitution were not naïve romantics either, regarding the problem of power, private interests and the abuse of language. They 'followed Locke and Hume but not Descartes and Kant' and anticipated conflicts and saw to it that in the words of their criticizers *'they* (had been) *burdening the young Republic with excessive reliance on controlling mechanisms, such as the separation of power, instead of centralizing all authority in a single national assembly that would represent a virtuous citizenry'* (p428–434) (Diggins, 1994).

5.3 Well-Ordered Science

The problem of power and distortion in philosophy and theory about 'ideal deliberations' in a 'well-ordered society' are well known (Rawls, 1999). These theories about justice in society are to be read and used as aspiration and guidance for our thinking about how to act in social life. Philip Kitcher (Kitcher, 2001, 2011) has in that vein, proposed a theory for 'well-ordered science' where in a democratic fashion the agenda for scientific research is being set in order that society will have optimal benefit of the research. This idea has been developed and discussed in detail in two books published ten years apart (Kitcher, 2001, 2011). He discussed all the

issues that are related to allowing external voices and opinions in the deliberations about science and the science agenda. Although Kitcher expresses his doubt and anticipates the critical opinions of the majority of scientists, he came to the conclusion that we must somehow engage in this. He concluded that science based on the Legend was misguided and that we should aim for 'significant knowledge'. Significant knowledge for Kitcher being knowledge that has impact directly or indirectly on real world problems. He is clear about the purpose of science: *'Even the slightest sympathy with pragmatism (in either the philosophical or the everyday sense) will recognize circumstances in which esoteric interests of scientific specialists ought to give way to urgent needs of people who live in poverty and squalor'*(p110).*(Kitcher*, 2011).

Kitcher uses the term 'significant knowledge' for the type of results of inquiry which contrast with the 'esoteric' form of knowledge production. It is obvious that here problem choice in inquiry and values next to cognitive criteria to steer that process are central. This determines the quality of inquiry in terms of its potential contribution to the body of knowledge and to decide and structure social or political action in the context of problems and needs. Kitcher does analyse the situation of the research agenda in the institutions. He concludes that in the past one hundred years, research has shifted from private to public but there is no oversight of the research agenda, either at the institutional nor at the national level. It simply is a list of a series of actions 'any institution of public knowledge' has to do. This poses the question which problems to study based on their estimated significance (p101) (Kitcher, 2011) It is unclear at exactly what level the institution here referred to, should act. Kitcher as many others seems to consider this as a 'black box' or a product of the legendary 'invisible hand' and states *'Science has evolved by happenstance'*. He does however mention with admiration the intervention by Vannevar Bush' who with very visible hand '*brilliantly developed a utilitarian case for public support of science' 'but preserves the idea of scientific autonomy; the public is to provide, but the community of scientists is to decide…*'. *'Optimistic visions like those…contrast with others that view any system of public knowledge as potentially oppressive'* (p101). Here he refers to Foucault who he says, despite his rhetoric, produced real insight in this problem, not citing many other influential scholars of Critical Theory as for instance Habermas. Fortunately, Kitcher explicitly discusses issues of power and interests and believes that *'they should not be used to scoff at philosophical ideals on the grounds that they require a lot of changes.*' He mentions major aspects of the current practice of science that are obstructive or even do run counter to the ideal of well-ordered science which require several changes including: competition in academia; flaws of vulgar democracy infested in public engagement; privatization of university research; neglect of many publics and their problems in the less affluent parts of the world; myopia on the part of academics in problem choice options.

The problem of majority vote on issues where expert opinion is of great importance and the majority may not be well informed or able to come to a justified opinion has been bothering Kitcher. He coined the very ominous terms '*vulgar democracy'* and *'tyranny of the ignorant'* describing all the fears and nightmares of

not only elitist scientists when they have to consider the idea that even well-informed and educated citizens get involved in decision making about the different agendas for inquiry or science driven policy making. Kitcher also has these fears and proposed a form of '*enlightened democracy*' in his earlier works to mitigate these threats (Kitcher, 1993). He considered the problem of elitism, but came to the conclusion that experts, who are to be trusted to be able to properly understand and make judgement, should inform groups of citizens and the experts then should decide based on the various perspectives (p133–135).(Kitcher, 2011).

Kitcher does not discuss, or only very indirectly discussed, one very obvious 'visible hand' that already for ages is steering the national research agenda and the agenda of institutions, namely how economy shapes science (Stephan, 2012), funding policies and money from whatever source available to institutions, individuals or groups of researchers determine to a major degree the agenda. We have seen in Chap. 3 how the incentive and reward system, has developed into a distorted system and how it determines our problem choice and the research agenda and the more strategic choices made on daily basis by committees of researchers all around the globe. The research agenda of funders has in most cases until recently been determined by internal scientific arguments based on quality measures of science, as its scientific committees are preferably populated by elite scientists.

In the final pages on Well-Ordered Science (p131–137) (Kitcher, 2011) he discusses moral issues, rephrased here by me: Are scientists obliged to work on the research that will yield the most significant knowledge? Should this be organized by procedures, so they are made to do the research that is ethically required of them? This is, he says, of course not what we should want. When scientists with their goals and preferences take part in ideal deliberations, they express their motivations and will be heard. There are, however, situations of emergency, Kitcher correctly points out, when there will be overriding public demands and researchers must drop their work and join to work on major problems. Most famous, is the Los Alamos Project and other major research projects during WW2, but think of pandemics of Flu, HIV, microbial warfare, the financial crisis of 2008. At the time I am literally writing these lines, the COVID-19 pandemic is such a global emergency and we see that scientist, in international networks from many disciplines have started to work together sharing data, materials and concepts in order to limit the damage to individual and public health and subsequently to try to avoid as much as possible the ensuing economic depression and its dramatic social effects.

Public representation is a problem for most writers on deliberation and the interaction between experts and the public it involves. This is most prominent in issues of political choice when they have complex technical or scientific components and the choice between policy options involves scientific advice and expertise next to social-economic and political arguments and values. Kitcher is very honest about the limitations of his philosophical position: *'My original thoughts about well-ordered science and the potential of groups of citizens to participate in deliberations that are simultaneously broadly representative and well-informed were advanced in ignorance of the actual experiments that have been carried out. '(*p223*) (Kitcher,* 2011*)* Based on the two cases he discussed, he concludes rather gloomy

that one of the greatest stumbling blocks is loss of authority and trust of experts. He writes: 'the *situation of our democratic discussion is currently so dire that no redress along the lines proposed is possible: there will always be loud voices decrying any efforts to rebuild trust in expertise'*. He concludes with the observation that only if the majority becomes aware and we start to address this problem, deliberative democracy will have a chance.(p226) (Kitcher, 2011).

5.4 The Legend Meets Reality and Pragmatism

Kitcher, influenced by Dewey and the new pragmatists, has converted since the late 1990s in his thinking about science and inquiry from analytical philosophy to pragmatism. This may explain why he writes about well-ordered science and the interaction between science and society as he does in these two important books cited above. These books are important in my mind exactly because of his philosophical history. He was well aware of the intellectual and emotional struggle of what it was that he had to leave behind and to face the accusations of engaging in relativism, post-modernism and being anti-science. Because of his background it seems he does yet not fully engage with pragmatism on two accounts: the ideas of Dewey, who he otherwise cites as an important inspiration, about the essential engagement with the public and their problems in inquiry and the more theoretical work of Peirce on that same issue. The pragmatic idea that the results of inquiry are really tested for value and acceptability by the community when translated into action, within or outside science depending on the problem they started with, is of great relevance to his idea of significant knowledge and well-ordered science. Mark Brown and Huub Dijstelbloem, and several others have in their essayistic reviews commented on these issues (Brown, 2004, 2013; Dijstelbloem, 2014a, b). A major criticism was that Kitcher did not mention the wealth of studies that were published between 1990 and 2010 on the many cases of public and citizen engagement and about prominent public debates with problematic interactions of scientist/experts and the public. To understand the major aspects of this interaction, Mark Brown in his own work goes from philosophy, sociology, STS to political and social theory (Brown, 2009). He explicitly observes science and society from an integral pragmatist perspective. Dijstelbloem, like Brown, suggest that the idea of 'the public', and more general groups of selected citizens, does not hold, as in many issues of policy making and expert advice, the debates and interactions are between designated groups of citizens who are concerned and directly affected by the respective political, public or private actions in their community. This indeed agrees with Dewey's idea of publics that are focussed and get organized in time and place around well-defined problems. Compared to the established governmental institutions, these publics are dynamic and fluid like their problems are. Dijstelbloem, as an example of such a public, performed a detailed analysis of the history of the initial institutional response in 1982 to the HIV epidemic in The Netherlands (Dijstelbloem, 2014a, b). The most interesting thing was that around that time in the AIDS

epidemic, HIV was to yet be discovered. Yet, several affected and concerned parties from society convened through an initiative of CLB, a national not-for profit foundation doing blood research, diagnostic services and a major national producer of blood products, clotting factors and other blood plasma derived medicines. The board of CLB was worried about the unknown pathogen that they anticipated caused AIDS and apparently was transmitted by blood products produced from blood donations from infected donors. To protect patients receiving blood transfusions and other blood products and specifically haemophiliacs who regularly need blood products produced from blood plasma batches involving pooled donations of thousands of donors, the idea was to ask gay men to refrain from donating blood. This directive was discussed before action was taken, with respect for feelings of discrimination on part of the gay community. At the table were the representatives and experts of CLB, representatives from physicians who treated haemophiliacs, representatives from the gay community (Men who have Sex with Men, MSM). The chairman was Roel Coutinho, director of the Municipal Health Service of Amsterdam and involved in a Hepatitis B vaccine study in gay men. The gay men objected as expected, casted doubt on the relevance for the local situation of the scientific arguments mainly based on data from the USA. It took four months to agree on a directive in which not homosexuality, not promiscuity, but having 'multiple sexual relations', which was quantitated to more than five in the previous six months, was the consideration on which one was asked to refrain from donating blood. Interestingly, no representatives of haemophilia patients, government, governmental agencies nor intravenous-drug users had been involved. Apparently, not all relevant concerned publics were aware and organised yet. The process showed that formal institutions need not to be at the centre. The latter contradicts the believe that Dewey's social approach via publics, in contrast to more formal institution approaches, would not have the power required to change and impact policies. This successful activist type of actions by the gay community in the history of AIDS and HIV have been quite common as has been shown by Steven Epstein's *Impure Science* (Epstein, 1996) and are badly needed in many fields of biomedicine, as he showed in his *Inclusion*, *The Politics of Difference in Biomedical research(Epstein,* 2007*).*

Dijstelbloem's study and its theoretical interpretation is a nice example of Science and Technology Studies (STS) done since the 1980s. Major researchers in early STS are amongst others Brian Wynne, Wiebe Bijker and Trevor Pinch (see Oudshoorn and Pinch eds, *How Users Matter; The co-construction of users* (Oudshoorn & Pinch, 2003). Sheila Jasanoff has since the 1980s produced an impressive body of in-depth scholarly work on the interaction and relationship between science, science advice and policy making which has guided many researchers since 1990. She is most interested in the dynamics of the policy making process, and I refer to (Jasanoff, 2012), a collection of her papers and especially Chap. 6. In that paper published in 1987 she very concisely discusses how philosophy and sociology of science have revealed the real practice of science and the flaws of the classical image and limited self-understanding of science, which has a major

effect on science advice at the boundary when scientists are meeting with representatives and opinions of publics and politics (p103).

Irwin and Wynne's *Misunderstanding Science? The public reconstruction of science and technology* is an excellent series of papers on case studies in which various fields of expertise and its experts are involved (Irwin & Wynne, 1996). Among them is Wynne's famous study on the Sellafield sheep farmers and nuclear fallout in which scientist discovered that the sheep farmers had expertise and ideas of their own which were highly relevant to the problem. The authors thoroughly analysed the cases to understand the problems in the interaction of experts and the lay public. I will not go in detail, but many common issues concerning science and experts become evident when their scientific claims have to face up to public scrutiny, which Irwin and Wynne in quite clear language summarized in their Conclusions (p213–221). They start with their own definition of what Kitcher later termed 'significant knowledge'. They more prosaically call it 'useful knowledge', meaning 'valid and socially legitimate as well as being of immediate practical relevance and use'. Social groups often ignore expert (scientific) knowledge because *'it is not tailored to the needs, constraints and opportunity structures of the social situation into which it has been interjected as authoritative knowledge'*. Experts must be sensitive to *'local contexts and need to listen to and to try to understand user situations and knowledges*.' For social legitimation of expertise it is required to *'reopen ..expert knowledge and its validation all over again - but in more complex, less reductionist circumstances. Often,…the prior context of scientific validation has been shaped by social assumptions and these have been 'black boxed..'*. They latter have not been made explicit and in addition the classical idea is *that validation of expert knowledge is completed before (and insulated from) its social deployment or use'.(p214)* This is as discussed in Chap. 2 in agreement with the scholars who have concluded that in the practice of science 'universal validity' was limited by strict experimental conditions which are hardly ever met in the world outside the laboratory or ideal research setting, which is the reason why despite positive clinical trials in a less ideally selected patient group many medicines fail (Cartwright, 1999).

This is, Irwin and Wynne say: *'the public understanding of science problematique',* the projection on to *'the public* of *the internal problems and insecurities about legitimation, public identification, and negotiation of science's own identity.* They conclude that this is the heart of the problem: *'all of the troubling experiences of apathy, resistance, plain distortion, and exaggeration which disfigure the public life of science in modern scientific democracy have led to little or no consideration of whether they imply anything might be wrong with the organisation, control, and conduct of 'science' (in addition to its communication).'(p214*). They conclude that the expert's idea of the public is wrong seeing them as a socially amorphous aggregate of individuals with erroneous unchecked assumptions about what the public wants and needs. This is a major point, as argued above and follows Dewey's proposition engaging the relevant citizens, although his work is not cited. Policy issues get a very different, more practical context if representatives of the public that is concerned and affected are involved (Marres, 2007). Several issues relate to the lack of understanding of the specifics of local contexts and their publics. Irwin and

Wynne anticipate that these will be *'uncomfortable conclusions for the scientific community since it suggests a pressing need for debate over the limitations of science as well as its putative benefits. However, in a situation where public groups more often see science as obstacle to development rather than a facilitator, there is little choice.'* (p219). To come to 'more progressive relationships between knowledge and citizenship' they propose 'new institutionalized forms which attempt to deal with these issues' and are sympathetic to small scale experiments and projects from which a lot is to be learned. They realized however that *'such localized and specific initiatives struggle to gain credibility within scientific institutions…*not being seen as belonging *to the preferred and more cloistered world of science..'* They emphasize that it all comes down to the institutions and the organization of science to achieve the required change in the attitude and practice in order to have more impact and with it more social legitimation. We have seen other scholars who analyse the problems of science and come to the same conclusion, although such a strong call for organizational change for instance in the incentive and reward system are rare. In terms of the analysis in Chap. 2 and 4, this sounds like The Legend meets Reality and Pragmatism. Flyvbjerg, (Flyvbjerg, 2001) with his advocacy for *phronesis*, the method of understanding and interpreting in inquiry, accompanies his plea to leave the idea of the Legend behind, as its method that may work for some of the natural sciences, is inadequate for other natural and biomedical sciences and the social sciences. We have seen in the previous chapters how difficult it is to achieve this systemic organizational change, as also in science and academia knowledge, power and interests are entangled (Bourdieu, 1988, 2004; Rouse, 1987, 1996).

ACT UP at the National Institutes of Health, 1990

My blog on interactions with the participants from the Amsterdam Cohort Studies: To confront 21st century challenges, science must rethink its reward system: **One of Science in Transition's founders describes how his experience as a young HIV/AIDS researcher convinced him that science needs to change.** The Guardian, 12 May 2016. https://www.theguardian.com/science/political-science/2016/may/12/to-confront-21st-century-challenges-science-needs-to-rethink-its-reward-system

'HIV/AIDS research in the early 1980s was a new and exciting field of science. I had started working as a biomedical researcher in Amsterdam, a city with a large and visible gay community. The new disease was a threat to public health and was highly contagious. It was transmitted by sexual contact and in the developed world affected young healthy gay men and recipients of blood and blood products. It took some time to realise that a truly immense and devastating epidemic was going on in sub-Saharan Africa affecting men, women and children. This disease attracted bright scientific minds all over the world, working feverishly to understand the origin and biology of the virus. We wanted to know how the virus moved through the population, entered and killed immune cells and how to counteract it. AIDS patients were dying in the hospitals and we were working as fast as we could towards better therapies for HIV-positive patients. Or were we?

I was very proud when results of experiments from my laboratory were published in prestigious academic journals like *Nature, Science* and *The Lancet*. I felt I had made a significant contribution to understanding and battling HIV. As well as at scientific conferences, we presented our results to participants of the Amsterdam Cohort Studies which started in the late 1980s. These were mainly gay men who helped our research by donating blood samples and filling in lifestyle questionnaires. One evening I presented with my usual enthusiasm new results on how HIV destroyed white blood cells of the immune system. Then a man came to the microphone. "Doctor Miedema, thank you for your interesting talk, but to be honest, it was a bit over my head, with apoptosis, virus particles and what have you. However, what I would like to know from you is whether we should practice safe sex even when my partner and I both are already HIV-positive." I was of course flabbergasted. Here I was with my clever immunological experiments and detailed molecular understanding of the virus, but I couldn't answer this real-world question. And the question made sense. Rephrased in viro-immunological research questions: is it possible and, if so, is it bad to become co-infected with a different virus strain? Can mosaic viruses with increased pathogenicity emerge? We, the smart boys and girls in the lab, hadn't thought of that question. Why not? Because it hadn't come up in the lab. We had informed the patients but forgotten to talk to them, the people we were supposedly working so hard for'. In my 'academic reflex' I translated his question into a research question

(continued)

which could give my team a nice publishable result and a paper. Indeed, not only didn't we talk to the patients to hear their needs, but it was felt that paying too much attention to those needs might be bad for our academic career, unless it would yield top publications. At that moment I realized how detrimental for societal impact the reward system and the corresponding research strategy is when the journal paper is the goal instead of a means to and end of having true societal or clinical impact.'

Public participation is a complex two-way process in which the scientist and experts must reflect on their practice and on needs and motivations of the lay participants. Of course, we must not be naïve and overly optimistic. Complexity reaches the next level as soon as policy making is discussed and the debate is organized tripartite with politician, local or national. There is a host of critical research on how the dynamics of these processes can be gamed by politicians who may well have their own motives and plans which do not a line with that of the public it concerns. It has been shown that in these cases participation is sham democracy and not real deliberation but simple a means to win the public over. (Felt & Fochler, 2010; Wilsdon et al., 2005; Wilsdon & Willis, 2004)

5.5 Rethinking Science

Most authors writing about science, for obvious reasons have evaded the issue, but it is clear we need to re-think the system of inquiry and academia, no matter how hard that may seem to be. A collective of authors lead by Helga Nowotny and Michael Gibbons, however, have just done that and published two remarkable books in 1994 and 2001 (Gibbons et al., 1994; Nowotny et al., 2001). Both books communicate a strong, nearly tangible sense of urgency for change. In *'Re-Thinking Science'*, they present a thorough comprehensive and dazzling analysis of ongoing parallel developments in society and science driven by socio-economic, scientific and technical, digital innovations that disrupt the way we live with due references to the seminal work of Giddens, Lash and Beck. These developments are the cause of persistent but rapid change that comes with disruption and fundamental uncertainty in society. It affects and changes our basic ideas and concepts about the good life: human interactions, community, communication, identity and belonging, ethics and responsibility, commitment and freedom in the personal, the national and the global public sphere. They point to the blurring of boundaries between science and society, they refer to the 'agora', the many physical but increasingly also virtual regional and national marketplaces where science and society meet and become entangled. Science is invading society, society is 'talking back to science', with at the same

time science and research closing the gaps between investigation, action and application.

Giddens, Beck and Lash have argued that it is a logical consequence of post- or hyper-modernity for science to have to be reflexive regarding its own functioning, concepts, results and instruments. Doing sociological research on problems and issues in society, subjects and publics of these studies will immediately be able to know the results and want to become or are engaged. This will affect the social practices and behaviours of the subjects who have an interest in that research. Researchers must deal with this public reflexivity by reflexivity on their part in the practice of their research. This is one of the consequences of modernity, where initially clear boundaries between science and society (church, state, politics) were needed to provide freedom for investigation, in our time of hypermodernity science and social practice are developing and organized in parallel and in continuous interaction. This happens in a common public sphere where their relationship is based on communication, ideally the ethical discourse that Habermas believes is required (Beck et al., 1994; Giddens, 1990; Habermas, 1971).

As a consequence, science also has entered a much more uncertain time and age in which is asked to be agile and to be able to rapidly adept to changes in the real world. Nowotny et al. (2001) are politically not naïve, open to all kinds of interactions between science and society for all kinds of aims and goals, be it public, government and private. They conclude presenting a set of seventeen cultural, ethical, political and socioeconomic issues to be discussed in the agora where science and society meet. Nowotny et al., clearly go beyond the Legend, when they state that the 'epistemological core is empty', or 'there is no foundation' as it has been concluded by post-positivism (Chap. 12). Here they refer to the Legend and the lack of contribution analytical philosophy of science has had to the actual practice and methodologies of science in agreement with the discussion in Chap. 2. The value of research in the new way of doing research has, they argue in an outright normative stance, thus become dependent not on its 'eternal' abstract epistemic value, but on its reliability. Reliability and value in the epistemological meaning, but very much also when applied and tried out in action and set to work in the real. Here they refer specifically to Ziman's *Reliable Knowledge* (p157) (Ziman, 1978).

The thinking in Mode-2 differs very much from the 'solution' of Collins and Evans, two major scholars on the topic, who in 2017 wrote: *'In contemporary science and technology studies the predominant motif is to eliminate the division of powers between science and politics in order that science and technology become socially responsible. In contrast, our motif is to safeguard the division of powers so that science and technology can act independently of society'* (p7,8) (Collins & Evans, 2017) In a 'last defence', they argue that despite that we accept that no absolute value-free truth is produced by science, to regain its consequential loss of self-evident authority, science must be rescued by explaining science better and more honestly to society by those academics ('owls') that have that oversight of science and meta-science. Not by reflecting on the deficiencies of science, but through the moderation of these 'owls', they believe that society can be brought round to accepts the values of science. Then scientific experts can effectively play their role in the

deliberations again. They discuss the work of relevant scholars, but the Mode-2 books discussed here are not mentioned. They do not trust the discourse in open debates with the public or within the scientific community, because of the interests and powers that are at play in non-ideal situations. They therefore do not take the, in my eyes, necessary reflexive next step to opening up science (p124–127).

Most of the critical scholars who's work I have discussed thus far, did blame the organization of science, its poor self-understanding, its flawed self-perception and some even courageously pointed at the reward system. They have, however, been much less bold with regards to proposing explicit ideas of how system change is to be made to the incentive and reward system to mitigate the observed problems. From the analyses presented in Chap. 3, it is clear why they did not discuss this highly sensitive issue. In contrast to most other scholars, Nowotny et al., do not at all duck at the difficult questions which relate to ideology and the self-understanding of science. In fifteen wonderful very confrontational pages which will still be disturbing for many scientists to read, they discuss the problems of The Legend (p50–65) (Nowotny et al., 2001). They have high hopes of Mode-2 research in which '*society speaks back to science*'. They at the same time realized that those who are still in the classical mode of science consider Mode-2 not 'real science', since they fear it obstructs 'real science' to be done. These critics will say it is not 'objective', disinterested and value free as science should be (Rouse, 1996; Douglas, 2009; Longino, 1990). Contextualization of science, in this classic vision, as we saw, is incompatible with the ideal and dream of 'objectivity' in the sense of the Legend. In line with post-positivist philosophy, Nowotny et al., argue that Mode-2 type knowledge production is done in a community of inquiry. Its claims are accepted by, and validated in social life and thus are intersubjective, reliable and socially robust. This is much more meaningful idea of 'objectivity'. Contextualization, that is starting with a problem and do the inquiry in that context or with that context in mind, is doing science the way of pragmatism. Despite that active scientists know that this is how science as a mature modern professional institution is being done, we saw that they are afraid to openly confess to their fallibility and limitations and are anxious of external influences and criticism. This, they believe, may hurt the image of purity and trustworthiness of their research. As Latour (1993) has concluded, this classical inward attitude and knee-reflex shows that science has not reflected and not made the full transformation, past positivism to real modernity. In our times in society it is recognized as 'scientism' which Habermas called 'halbierten Rationalismus', partial rationality, a science that operates from a positivistic frame in which it is insulated from social and cultural values.

Nowotny et al., are convinced of the opening up of science as the way forward to improve the impact of science. Science, the authors say, in our modern times should be reflexive and truly modern and thus have other worries: *'Today's scientists have to confront different, but analogous fears- their fear of the social world, with its imputed interests and ideological distortions, of cultural influences and of their own, subtle and not-so-subtle, accommodations to political and economic pressures….As public controversies proliferate, the trust of the public..has to be carefully nourished. If scientists would openly acknowledge these perceived threats, it*

might be possible to develop another model of knowledge production, in which knowledge becomes socially robust.'

They argue that '*contextualisation has surreptitiously crept into what was once held the be the inner core of science whereas it has been embraced by more outward-oriented parts of science'* and argue that *…the actual practice of science …might be set free to explore different contexts and perhaps to evolve in different directions…more as a comprehensive, socially embedded process' (p64, p65).*

Given the different ideologies, interests, fears and powers that are at play in the field of science, it is clear that this involves many actors and for some their most existential professional feelings. In short, this reorientation is not a small thing. Nowotny et al. describe the institutional changes toward the practices of Mode-2, for instance the alternative movements in the EU research area, where in Framework Programma (FP) 4 that ran from 1994–1998 a contextualized research programme was successfully launched which aims at targeted and problem-driven research programmes (EU, 2017). They use the term 'core' for the classical core of academia that is in Mode-1 and 'periphery' for the research and researchers that engage with problems and stakeholders from outside. They describe the tensions between them when governmental funding agencies are programming for problem-oriented research like in the EU FP 4 and 5. This is conceived by the core as 'undermining the peer review system' and the role of experts. Nowotny et al., correctly state that these directed programmes even are only 'Weakly Contextualized' as 'they were designed to solve yesterday's problems' and still operate in the classical linear mode of innovation. They discuss in depth the practice of Strong Contextualization, which starts with a policy agenda for research, prioritizing actual problems against each other as Kitcher has been describing in the ideal philosophical setting of well-ordered science.

The idea that Mode-2 was winning ground was not uncontested. In the boards and advisory committees of national and European Research Councils, they say, discussions, power struggles, between 'traditionalists' and 'modernists' in the complex field of science was pushing Mode-2 modernists out of the mainstream (high church) 'core', back into the 'periphery'. As the Mode-2 practice aims for robust reliable results and application, research, applied science and technology is being performed close to and many times with agents in the relevant societal contexts where urgent problems are being experienced and where it will present themselves to researchers. This type of research will have many different products, needs many forms of competences, skills and attitudes, demands different measures of quality control. Nowotny et al. wrote these observations on the practice of science in the present tense in the years just before the year 2000. Flyvberg, (2001) writing in the same years about 'how to make social science matter', points to this dynamics and to 'physics envy' as the wrong road for the social sciences. He makes for the social sciences a similar strong case for Mode-2 by revoking Aristotle's concept of *phronesis* which is a *'true state, reasoned, and capable of action with regards to things that are good or bad for man'* and *goes beyond techne and episteme since it is involved in social practice'* and he argues that '*attempts to reduce social science*

and theory to episteme (analytic) and or techne (technical knowledge) or to comprehend them in those terms, are misguided' (p2). These authors in the year 2000 cautiously, but optimistically, concluded that the practice of Mode-2 science and research -which is problem-driven, cross-disciplinary and pluriform in methods and approaches using modern and pre-modern humanistic ways to human understanding- had already reshaped part of science and research. However, we have seen in Chap. 3 that Mode-2 research in 2020 is still struggling with its image and standing in academia, despite the obvious problems of 'traditionalist' Mode-1 research.

5.6 Mode-2: Not the Highway of Academic Science

There thus have been strong local and even larger movements in academia and society in the 1960s and 1970s driving the case for social relevance. In addition, and derived from that until 2000, powerful and convincing academic analyses were produced to show the urgency to optimally connect science with society and to remodel research and academia for that reason. As Nowotny (Nowotny et al., 2001) and others (Rip & van der Meulen, 1996) (Rip, 1994; Sarewitz, 2016) observed, in parallel and in reaction to the inward looking well-organized academic community, in many countries a system of intermediary institutions and (semi-)government agencies had been established and was going to be even more firmly established to program science in Mode-2 style.(Whitley, 2000) Top down programming and research management '*seek to reconcile the upholding of standards of scientific quality with new demands that transcend them and need to be incorporated. The difficulty of setting priorities in funding in basic research highlights how the system is struggling to embrace a kind of social reflexivity- to which there is no alternative' (p47).* Nowotny et al., did realize that Mode-1 disciplinary science and scholarship, science for its own curiosity sake, aiming to add objective 'eternal' truths to the body of knowledge would not cease to exist. In their vision it is one of the consequences of the self-organizing capacity of science to manage *'the failure of scientific elites (Mode-1 elites) to accommodate to demands for accountability and priority setting and to accept additional criteria of judging the quality and relevance of scientific work'* (p47). They express their good hope for change in the very last lines: '*Just as 'publish or perish' is underpinned by certain rules of the game, to which scientists and their peers have agreed to adhere, so the opening up of science towards the agora presupposes and necessitates 'rules' of a game that partly still wait to be established.'…Not everyone will be able or willing to participate, and not anything goes-but the often feared 'contamination' of science through the social world should be turned around. Science can and will be enriched by taking in the social knowledge it needs in order to continue its stupendous efficiency in enlarging our understanding of the world and of changing it. This time, the world is no longer mainly defined in terms of its 'natural' reality but includes the social realities that shape and are being shaped by science' (p262).*

Helga Nowotny: Reflecting on the Modes of Science

It was 7 November 2017 that I met Helga Nowotny at a half-day invitational workshop at the Robert Bosch Stiftung in Berlin. The debate was on '*Science and Science Policy: Is Knowledge Losing Power? Towards a More Resilient Science System for the 21st Century'*. Among the select group of participants were also Sir Mark Walport, Prof. Chief Executive Designate, UK Research and Innovation, United Kingdom, Sir Philip Campbell (Editor-in-Chief, Nature, London, United Kingdom), J-P Bourguignon (Director of the ERC), Dianne Hicks, Jack Stilgoe (UCL) and Tracey Brown (Sense about Science, UK). I was invited and explicitly instructed by the organisers to give the short opening statement of the workshop about Science in Transition in the 'provocative style'. I believe, judging from the report of the meeting*, that I managed to live up to these expectations. The reactions from the participants, given their positions in the field, were totally predictable. Walport and Bourguignon acted as if professionally offended, did not recognise the problem analysis at all. Campbell argued, as other publishers, publishers were not to blame. Stilgoe, Hicks and Brown joined me in adding their own critiques of science. Helga Nowotny, sitting at a corner of the table, did not appear to be shocked at all, but seemed rather slightly amused by the discussion. She quietly looked on. After the lunchbreak, Nowotny presented her reflections on the position and responsibility of science in society as to be found in here recent book *The Cunning of Uncertainty (Nowotny,* 2016*).*

I was familiar with Nowotny's work. We had discussed science as she gave a seminar at UMC Utrecht in our PhD course *This Thing Called Science* a year before, which was concluded with a small group dinner with Frank Huisman at the Faculty Club. Given the *Re-Thinking Science* book it puzzled me that she was also a founder of the ERC. Why in 2005 did she think we needed the ERC, a Mode-1 'high church' science build on the myth of Legend? That day in Berlin, I again wondered how to properly understand and interpret Helga Nowotny's work. According to her CV, on her website and interviews (Nowotny & Leroy, 2009) she was born in 1937, studied law in Vienna, and after that moved with her partner to New York and took to study sociology of science at Columbia in New York, studying with Robert Merton in the 1960s. Back in Austria in the 1970s, she researched political issues which involved scientific expertise, such as the debate about nuclear energy and got deep into STS ever since. She was affiliated with the Institute of Advanced Studies in Vienna, Faculty of Sociology, University of Bielefeld, École des hautes études en sciences sociales, Paris; Institute for Theory and Social Studies of Science, University of Vienna, professor at ETH Zurich. She was vice-president and until 2013 president of the European Research Council (ERC) of which she was one of its main founders. Reading her work, the books discussed in this chapter, but also her latest book *The Cunning of Uncertainty,* it is clear that Nowotny is a fine scholar and an exceptional

(continued)

expert on science, research and its interaction with society. She personally knows or has known the main scholars of her time, most of them discussed in these pages. She is completely aware of the conflicting interests, tensions, factions, politics and power struggles in academia. She was raised on the normative functionalism of Merton, knows the works of Habermas, Bourdieu, Foucault and Latour, but also has a broad overview of post-Merton, post-Popper philosophy and sociology of science. With this in mind, reading her work it is obvious that Nowotny at least in her writing evades the 'raw' politics of science. She does however not evade the problems of the Legend, she is leaving the myth of positivism behind and without explicitly mentioning Dewey, promotes the practice of American pragmatism. In my terminology, she moved from Mode-1 Legend to Mode-2 Pragmatism. Commentators on *The New Production of Knowledge* and *Re-Thinking Science* have correctly concluded that she and her co-authors seem scientifically and politically neutral with regards to Mode-1 and Mode-2 (Pestre, 2003). Mode-2, as the commentators argue, in their vision is thus open and vulnerable to the penetration by economic goals and the powers of the private sector. Nowotny et al., responded (H. Nowotny et al., 2003) adequately to these comments, taking all interests in scientific inquiry, economic and social into account, but staying out of issues of power and politics. The remark on the poor position they see for fundamental 'blue skies' research, explains the link with the ERC. Did they really believe that in academia Mode-2 would drastically displace Mode-1? I was inclined to read *Re-Thinking Science* as being a mix of descriptive and normative, that Mode-2 is a necessary complementation given the limitations of Mode-1. Nowotny even at the November 2016 small group dinner table in the Utrecht University Faculty Club could not be tempted to engage into too informal exchanges about the 'raw politics of science'. She reflects and presents the different options to us. To whom? I guess to the Deans and Vice Chancellors, boards of funding agencies who have to act in the real world once these academic insights have been presented.

*https://www.bosch-stiftung.de/en/project/berlin-science-debate/berlin-debate-2017

Despite these calls to contribute to the needs and urgent problems of society main-stream academia, science and scholarship in universities, Learned Societies, Royal Academies remained largely in Mode-1. Moreover, in academia the credit system with the typical metrics giving Mode-1 the highest esteem and the dominant academic career path was further embraced (Hicks et al., 2015; Wilsdon, 2016). This was not what the Mode-2 authors had hoped for. It was caused by a general development of government policies based on the idea that organized and programmed knowledge production is driving national economic and military competitiveness. This idea of 'the knowledge economy' started to fully play out in the late

1980s in the global economy for which the size and performance of national or regional (EU) science and technology systems were absolutely crucial. These national and in the EU international and regional science and innovations systems are however not to be thought of as 'an institutional set up somehow geared towards innovation. There is no inherent purpose of the overall system to work to some goal'(Rip & van der Meulen, 1996).

A multitude of non-synchronous interactions between the various bureaucratic organizations that acted as intermediate -sociologically designated as boundary-organizations on behalf of the government in relation to research organizations, universities and other public knowledge institutes (Whitley, 2000; Ziman, 1994). This was and is not a level playing field for researchers from the natural sciences, social sciences and the humanities and their subdisciplines. Some field thrived, others barely survived, waned or completely disappeared depending on internal academic ideas about autonomy, academic esteem and reputation; levels of proactive organization; external socioeconomic and all kinds of political developments. Probably the most significant of these 'fluctuations' after 1980 is the major decrease of investments mainly in physics and some natural sciences and the simultaneous enormous increase in biomedical and health research. This was directly related to the overall increasing expenditure on science and research, the end of the Cold War and amongst others the fight against cancer, increasing awareness of the effects of aging causing rapidly increasing health care expenditures (Stephan, 2012). Of note, for the same reasons, even within the field of biomedicine and health, but in fact in all fields, some researchers did benefit enormously compared to others from this increased public spending which depended mainly of what scientific advisory boards thought was excellent, held promise and should be funded. For instance, in biology and biomedicine from about 1970 on, due to major rapid breakthroughs in molecular biology, molecular genetics and protein chemistry, with its various more physical and chemical methods, this type of research in scientific advisory boards from many funding agencies became the norm for excellence. The molecular reductionist approach (*the molecular turn*) has ruled, from research on cancer, cardiovascular disease, paediatrics, infectious diseases, neurology to mental health and psychiatry. The role of the scientific committees at NIH, INSERM, CNRS, DFG, NWO, MRC, ERC, but also at Welcome Trust and other institutes and charities around the world is in this respect of interest (Miedema, 2012; Sarewitz & Guston, 2006). These institutes are at the boundaries were governments meets with universities and other public research institutes. It is their task to advance the societal missions and goals (the higher purpose) the government funder or charity has in view. On the other hand, to get the research properly done, it has to deal with the ideas and fashions and taste, about excellence, and internal politics of the different scientific fields (Lamont, 2010). This complex so-called 'principal- agent' problem has to be managed by esteemed scientists that are active in their fields and are appointed to the scientific committees. Complex it is, because the goals of funders and researchers in academia and public institutes are at best partially aligned. President Johnson, as

mentioned above, in 1966 already expressed his doubts about the NIH. The MRC (Miedema, 2012) and most charities and also, as a national example the Dutch Cancer Society (KWF) for many years were 'hijacked' by the basic science approach of mainly molecular geneticists, disconnected from clinical care or public health. Writing in 2015 therefore about a new approach to funding, KWF courageously declared:

> *Our present approach to assessing grant requests is to focus on the scientific quality of the project or programme in question. From now on, we will place greater emphasis on the potential of the research in question to make a genuine contribution to our mission. The only way to obtain a clear picture of this is to examine each study in terms of its strategy for developing the results obtained into new treatments for patients. This calls for flexibility in the types of funding used: every effort must be made to ensure that the results genuinely reach patients. Throughout the entire research chain, from the laboratory to the patient, those working in the research field put forward research proposals, and we facilitate the flow of results. There is still a substantial focus on basic cancer research, as this is the source of new insights. Yet there is also scope for promising initiatives in the field of infrastructure, for example. https://www.kwf.nl/sites/default/files/2019-10/dcs-policy--vision-2015-2019.pdf*

The Bill and Melissa Gates Foundation must have realized this problem fully when the multi-billion Grand Challenges which started in the beginning of this century was evaluated. My laboratory was involved in a Mode-2 research project in which a large consortium, including researchers from Sub-Saharan Africa was addressing major problems on protective immunity to HIV/AIDS, malaria and TB with the goal of developing vaccines. Investments over ten years yielded many academic publications. The application, implementation and evaluation of the new knowledge in practice was a different game and appeared to be hard.

5.7 Re-visioning Science is Opening Up Science

Nowotny et al., in a chapter with the title Re-Visioning Science (Nowotny et al., 2001) briefly summarize the major issues with the following buzz words: realistic, reflexive, autonomy in localized forms, reliable knowledge, no universal objectivity, anticipate contexts. *'Co-evolution with society, demands a historically unprecedented openness on the part of science. Merely to add to the supposedly hard scientific core an additional outer layer consisting of 'softer' institutions, 'softer' norms and 'softer' behaviour on the part of scientists, all of which are designed to give greater weight to economic and social issues… cannot work. Science should attempt to reconstruct its image and authority ….: science is more heterogeneous, diverse, local and disunited* than the public and science itself realizes (p233). As I will discuss in Chap. 7, it took more than fifteen years to get broad institutional and international follow up of this call to the opening up of science.

References

Beck, U., Giddens, A., & Lash, S. (1994). *Reflexive modernization: Politics, tradition and aesthetics in the modern social order*. Polity Press.

Bourdieu, P. (1988). *Homo academicus*. Stanford University Press.

Bourdieu, P. (2004). *Science of science and reflexivity*. Polity.

Brown, M. B. (2004). The political philosophy of science policy. *Minerva, 42*(1), 77–95. https://doi.org/10.1023/B:MINE.0000017701.73799.42

Brown, M. B. (2009). *Science in democracy: expertise, institutions, and representation* (pp. 1 online resource (xvi, 354 pages)).

Brown, M. B. (2013). Philip Kitcher, science in a democratic society. *Minerva, 51*(3), 389–397. https://doi.org/10.1007/s11024-013-9233-y

Cartwright, N. (1999). *The dappled world: A study of the boundaries of science*. Cambridge University Press.

Collins, H., & Evans, R. (2017). *Why democracies need science*. Polity Press.

Dewey, J., & Rogers, M. L. (2012). *The public and its problems: An essay in political inquiry*. Pennsylvania State University Press.

Diggins, J. P. (1994). *The promise of pragmatism: Modernism and the crisis of knowledge and authority*. University of Chicago Press.

Dijstelbloem, H. (2014a). Missing in action: Inclusion and exclusion in the first days of AIDS in the Netherlands. *Sociology of Health & Illness, 36*(8), 1156–1170. https://doi.org/10.1111/1467-9566.12159

Dijstelbloem, H. (2014b). Science in a not so well-ordered society. A pragmatic critique of procedural political theories of science and democracy. *Krisis, 1*, 39.

Dijstelbloem, H., & Hagendijk, R. (Eds.). (2011). *Onzekerheid troef: het betwiste gezag van de wetenschap, Amsterdam: Van Gennep*. Van Gennep.

Douglas, H. E. (2009). *Science, policy, and the value-free ideal*. University of Pittsburgh Press.

Epstein, S. (1996). *Impure science: AIDS, activism, and the politics of knowledge*. University of California Press.

Epstein, S. (2007). *Inclusion: The politics of difference in medical research*. University of Chicago Press.

EU. (2017). EU frame work programmes for research and innovation evolution and key data from FP1 to Horizon 2020 in view of FP9. Retrieved from.

Felt, U., & Fochler, M. (2010). Machineries for making publics: Inscribing and de-scribing publics in public engagement. *Minerva, 48*(3), 219–238. https://doi.org/10.1007/s11024-010-9155-x

Flyvbjerg, B. (2001). *Making social science matter: Why social inquiry fails and how it can count again*. Cambridge University Press.

Gibbons, M., Limoges, C., Nowotny, H., Schwartzman, S., Scott, P., & Trow, M. (1994). *The new production of knowledge: The dynamics of science and research in contemporary societies*. SAGE Publications.

Giddens, A. (1990). *The consequences of modernity*. Polity in association with Blackwell.

Habermas, J. (1968). *Technik und Wissenshaft als 'ideologie'*. Suhrkamp verlag.

Habermas, J. (1970). *Toward a rational society*. Heinemann Educational Books.

Habermas, J. (1971). *Knowledge and human interests*. Beacon Press.

Hicks, D., Wouters, P., Waltman, L., de Rijcke, S., & Rafols, I. (2015). Bibliometrics: The Leiden Manifesto for research metrics. *Nature, 520*(7548), 429–431. https://doi.org/10.1038/520429a

Irwin, A., & Wynne, B. (1996). *Misunderstanding science?: The public reconstruction of science and technology*. Cambridge University Press.

Jasanoff, S. (2012). *Science and public reason*. Routledge.

Kitcher, P. (1993). *The advancement of science: Science without legend, objectivity without illusions*. Oxford University Press.

Kitcher, P. (2001). *Science, truth, and democracy*. Oxford University Press.

Kitcher, P. (2011). *Science in a democratic society*. Prometheus Books.

Lamont, M. (2010). *How professors think: Inside the curious world of academic judgment*.
Latour, B. (1988). *The pasteurization of France*. Harvard University Press.
Latour, B. (1993). *We have never been modern*. Harvard University Press.
Longino, H. E. (1990). *Science as social knowledge: Values and objectivity in scientific inquiry*. Princeton University Press.
Marres, N. (2007). The issues deserve more credit: Pragmatist contributions to the study of public involvement in controversy. *Social Studies of Science, 37*. https://doi.org/10.1177/0306312706077367
Miedema, F. (2012). *Science 3.0. Real science real knowledge*. Amsterdam University Press.
Miller, J. (1994). *"Democracy is in the streets": From Port Huron to the siege of Chicago: With a new preface by the author*. Harvard University Press.
Nowotny, H. (2016). *The cunning of uncertainty*. Polity.
Nowotny, H., & Leroy, P. (2009). Helga Nowotny: An itinerary between sociology of knowledge and public debate. *Natures Sciences Sociétés, 17*(1), 57–64. Retrieved from https://doi.org/10.1051/nss/2009010.
Nowotny, H., Scott, P., & Gibbons, M. (2003). Introduction: 'Mode 2' revisited: The new production of knowledge. *Minerva, 41*(3), 179–194. https://doi.org/10.1023/A:1025505528250
Nowotny, H., Scott, P., & Gibbons, M. (2001). *Re-thinking science: Knowledge and the public in an age of uncertainty*. Polity/Published in the USA by Blackwell.
Oudshoorn, N., & Pinch, T. (2003). *How users matter: The co-construction of users and technologies*. MIT Press.
Pestre, D. (2003). Regimes of knowledge production in society: Towards a more political and social Reading. *Minerva, 41*(3), 245–261. https://doi.org/10.1023/A:1025553311412
Price, D. J. d. S. (1963). *Little science, big science*. Columbia University Press.
Rawls, J. (1999). *A theory of justice* (Rev ed.). Oxford University Press.
Rip, A. (1994). The republic of science in the 1990s. *Higher Education, 28*(1), 3–23. https://doi.org/10.1007/BF01383569
Rip, A., & van der Meulen, B. J. R. (1996). The post-modern research system. *Science and Public Policy, 23*(6), 343–352. https://doi.org/10.1093/spp/23.6.343
Rouse, J. (1987). *Knowledge and power: Toward a political philosophy of science*. Cornell University Press.
Rouse, J. (1996). *Engaging science: how to understand its practices philosophically* (pp. 1 online resource (ix, 282 pages)). Retrieved from http://purl.oclc.org/DLF/benchrepro0212
JSTOR. Restricted to UCSD IP addresses http://www.jstor.org/stable/10.7591/j.ctv75d2nk.
Sarewitz, D. R. (2016). Saving science. *The New Atlantis, 49*, 4–40.
Sarewitz, D. R., & Guston, D. H. (2006). *Shaping science and technology policy the next generation of research*. University of Wisconsin Press.
Spaapen, J., H.Dijstelbloem F. Wamelink. (2007). *Evaluating research in context. A method for comprehensive research assessment*.
Stephan, P. E. (2012). *How economics shapes science*. Harvard University Press.
Toulmin, S. (1977). From form to function: Philosophy and history of science in the 1950s and now. *Daedalus, 106*(3), 143–162.
Toulmin, S. (1990). *Cosmopolis: The hidden agenda of modernity*. Free Press.
Whitley, R. (2000). *The Intellectual and Social Organization of the Sciences,* vol 11.
Wilsdon, J. (2016). *The metric tide: The independent review of the role of metrics in research assessment & management*. SAGE.
Wilsdon, J., Stilgoe, J., & Wynne, B. (2005). *The public value of science: Or how to ensure that science really matters*. Demos.
Wilsdon, J., & Willis, R. (2004). *See-through science: Why public engagement needs to move upstream*. Demos.
Ziman, J. M. (1978). *Reliable knowledge: An exploration of the grounds for belief in science*. Cambridge University Press.
Ziman, J. M. (1994). *Prometheus bound: Science in a dynamic steady state*. Cambridge University Press.