Chapter 11
Public Participation in the Setting of Research and Innovation Agenda: Virtues and Challenges from a Philosophical Perspective

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Abstract Inclusiveness in scientific research and innovation is more and more valued by many scientific institutions, as attested by the increasing visibility and displayed institutional support in favour of "citizen science", "participatory science" and other forms of science involving in one way or another lay people. Could science benefit from being more inclusive and, in turn, could society benefit from a more inclusive science? The general aim of this chapter is to investigate how public participation may challenge and renew traditional epistemological and organisational features of scientific research, thereby providing a basis to assess the merits of public participation in this sphere. It will in particular offer epistemological arguments disqualifying common sources of resistance to public participation and discuss pending issues that need to be addressed if one wants to make a strong case in favour of public participation in science. In doing so, the chapter will (hopefully) contribute to going beyond an isolationist, decontextualised view of scientific developments and redefine the role that society is expected to play in new models of scientific research and innovation aiming at a better alignment of its outputs with society needs and interests.

11.1 Introduction

In a recent editorial entitled "Beyond the science bubble", the influential scientific journal *Nature* (2017) calls for a better alignment between the outputs of scientific research and innovation and the needs and expectations of society. The charge is rather virulent: "the needs of millions of people in the United States are not well enough served by the agendas and interests that drive much of modern science. (…) Research leaders in the United States and elsewhere should address the needs and employment prospects of taxpayers who have seen little benefit from scientific advances". This editorial echoes a seemingly growing dissatisfaction with scientific research and innovation: global contribution to economic growth is still of course

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centrally on the agenda but it does not exhaust today society expectations: a *socially relevant* and *desirable* research and innovation is also expected. On the institutional side, these additional expectations are displayed for instance as lying at the core of the concept of "Responsible Research and Innovation" (RRI) put forward by the European Commission in its Horizon 2020 programme. Aiming at fostering "the design of inclusive and sustainable research and innovation", RRI implies that "societal actors (researchers, citizens, policy makers, business, third sector organisations, etc.) work together during the whole research and innovation process in order to better align both the process and its outcomes with the values, needs and expectations of society."[1] Public participation in research and innovation is thus seen and advertised as a mean to foster and achieve responsible research and innovation. More generally, inclusiveness in scientific research and innovation is more and more valued by many scientific institutions, as attested by the increasing visibility and displayed institutional support in favour of "citizen science", "participatory science" and other forms of science involving in one way or another lay people.[2] In that perspective, scientific research is no exception to a broader societal demand for more direct participation of the citizens in various areas of public and political life. Political valorisation of direct participation of citizens has become ubiquitous and leads to a variety of concrete participative forms of democracy at various levels (participatory budgets at municipal levels, crowd-sourcing in electoral campaigns, citizen consultations, etc.). From the perspective of democracy theorists, participative forms of democracy are often seen as a way to renew and enrich representative democracy, in response to its diminishing legitimacy and appreciation in the eyes of the citizens of our contemporary democratic societies.

Similarly, could science benefit from being more inclusive and, in turn, could society benefit from a more inclusive science? On the face of it, opening the scientific sphere to non scientists appears quite challenging in many respects, and especially from an epistemological point of view. After all, as much of historical, philosophical and sociological thinking about science has taught us, science is characterized, as a social field, by a very high level of closure ("among peers" is the rule in science). The general aim of this chapter is to investigate how public participation may challenge and renew traditional epistemological and organisational features of scientific research, thereby providing a basis to assess the merits of public participation in this sphere. It will in particular offer epistemological arguments disqualifying common sources of resistance to public participation and discuss pending issues that need to be addressed if one wants to make a strong case in favour of public participation in science. In doing so, the chapter will (hopefully) contribute to going beyond an isolationist, decontextualised view of scientific developments and redefine the role that society is expected to play in new models of scientific research and innovation aiming at a better alignment of its outputs with society needs and interests.

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[1] https://ec.europa.eu/programmes/horizon2020/en/h2020-section/responsible-research-innovation. Accessed December 2017.

[2] For a typology see for instance Bucchini and Neresini (2008).

More precisely, I will proceed as follows. Starting with some preliminary remarks, I will first recall recent contributions from science and technology studies (STS), as well as from philosophy of science, emphasizing an evolution of the very aims assigned to scientific research in our societies, which can be broadly captured by the notion of contextualisation of these aims. As I shall explain, this background evolution is what makes room for the very idea of public participation in scientific research, leading also to a principled limitation of the autonomy of scientific communities. Distinguishing (classically) between two phases of the scientific enterprise (the choice of the problems to be addressed and their resolution), I will then discuss a first form of limitation of scientific autonomy, namely, a limitation of freedom of scientific communities when it comes to the setting of their research priorities. My main contention will be that resistance to any form of "external" piloting of research priorities on the grounds that it would hamper the fecundity of science (a widespread stand in public debates about scientific freedom) turns out to rest on misplaced epistemological views on the very nature of the dynamics of science and to remain in the grip of a linear model of innovation. Having established the epistemological acceptability of an externalisation of the setting of research agenda, I will then address the issue of which form of such "external" piloting (more on this notion later) is preferable when a better alignment between the outputs of scientific research and the needs and expectations of society is sought for. This part of the chapter will be primarily exploratory (rather than conclusive), discussing pro's and con's of various options, especially in comparison with the option of direct involvement of lay citizens.

11.2 Preliminary Remarks on the Evolution of the Aims of Science

It is now commonly acknowledged that science has gone through significant changes in the past few decades and especially in its relationship with other components of the society. Influential works in science and technology studies have proposed various conceptual tools to grasp these changes that affect in particular modes of research funding and the setting of research agendas.[3] For instance, the concept of triple helix of entrepreneurial science, developed by Etzkowitz (2003), puts forward the high level of intertwining between government, industry, and academia. The widely discussed 'mode-2' of knowledge production proposed by Gibbons et al. (1994) emphasizes a new social contract between science and society characterized, amongst others, by a research agenda much more open to "external" problems, that is, to problems defined in response to some identified needs of the society, by contrast with 'mode-1' of knowledge production, in which problems addressed by science are mainly defined according to interests and needs internal to a scientific discipline.

[3]For a useful historical perspective on these STS contributions, see Pestre (2003).

11.2.1 *Decontextualized Versus Contextualized Views on the Aims of Science*

As regards more specifically the aims of science, these contributions from science and technology studies coincide, on the philosophical side, with "contextualized" views on what makes science valuable, by contrast with "decontextualized" ones. Broadly speaking, decontextualized views conceive the ends of science in terms of gaining knowledge about how the world is, its structures, its constituent parts, independently of what could be the specific needs of a society at a given time of its history, be they epistemic (e.g. expertise) or practical. Within this decontextualized perspective, philosophers may differ about what exactly scientific inquiry is after, but they at least agree on the fact that these goals do not depend on contingent, socio-economical or cultural expectations. Various lists of goals have been proposed, including general items such as identifying the laws of nature, providing objective explanations, providing reliable predictions, formulating unitary principles, or more specific ones such as depicting and making use of causal patterns by using idealizations (Potochnik 2017).

Contextualized views on the ends of science, on the other hand, do not reject these purely epistemic goals, but acknowledge that other, non-epistemic considerations must be taken into account as well. After all, there are many, if not an infinity of questions that can be asked about the world, and many, if not an infinity of phenomena that could be the object of predictions and explanations. But the fact is that we do not deem them all equally worth being the objects of scientific inquiry. Depending on our needs and interests, we make choices: for instance modelling climate evolution is today given high priority. Kitcher (2001, Chap. 6) proposes a notion of "scientific significance" accounting for this dependency, by combining curiosity-driven, context-independent considerations with context-dependant ones. He gives the example of a research program aiming at cloning mammals and asks what makes such programs valuable and worth being pursued. The answer combines purely epistemic reasons (gaining, for instance, a better understanding in developmental biology of the first stages of development and the migration of DNA) and interest-driven (hence context-dependant) ones such as improvement of livestock or improvement of drug production processes using animals. Kitcher's mixed conception of scientific significance thus offers an integrative articulation of curiosity-driven, context-independent ends and interest-driven, context-dependant ones. From Kitcher's perspective, views of the aims of science must thus overcome the traditional contrast, if not opposition, between what is also often described as "disinterested" aims and "utilitarian" aims, corresponding to two broad types of expectations toward science. On the one hand, one can expect from science that it provides us with reliable knowledge about the world, and this knowledge is valuable in itself, independently of any practical use that can be made of it. On the other hand, one can adopt a more utilitarian stance toward science and expect primarily some practical usefulness of the outputs of scientific inquiries.

11.2.2 Scientific Autonomy and Utilitarian Expectations

Disinterested and utilitarian expectations towards science coexist today, but not always as harmoniously as suggested by normative philosophical views such as Kitcher's. They can be on the contrary experienced as being in tension, or even incompatible, especially by some practicing scientists. Just as an example among many, here is a recent public statement made by an eminent British chemist, Sir J. Cadogan, also endorsed by forty-one of his fellows from the Royal Society:

> The nature of all politics and politicians means it is easier for our pay-masters to feel comfortable about the proclaiming of programmes relating to Energy, Health, Materials, Climate Change, the Hydrogen Economy and so on, rather than to announce, let alone trumpet, that money is available for scientists to follow their curiosity in their own disciplines. (Cadogan 2014)

Grounds for resistance to a driving of scientific inquiry by utilitarian considerations are easy to identify. Such driving may first be perceived as running counter to values that are taken as central to the scientific enterprise such as disinterestedness and autonomy from other components of the society, especially the political sphere. Note, though, that utilitarian views of science are not necessarily incompatible with a defense of its autonomy, on the contrary. A historically central and well-known example of this compatibility is Vannevar Bush's claim that autonomy is not only compatible with utilitarian expectations, but even a necessary condition for science to be able to deliver benefits to society. In his influential science policy report, *Science, The Endless Frontier* (1945), Bush, who was at the time Roosevelt's scientific counselor, formulates these utilitarian expectations in the following broad terms: "scientific progress is one essential key to our security as a nation, to our better health, to more jobs, to a higher standard of living, and to our cultural progress" (1945: 2), adding immediately that "scientific progress on a broad front results from the free interplay of free intellects, working on subjects of their own choice, in the manner dictated by their curiosity for exploration of the unknown. Freedom of inquiry must be preserved under any plan for government support of science" (1945: 12). This kind of utilitarian justification of scientific autonomy goes hand in hand with what can be described as a "cascade model" of the social contract between science and society (e.g. Guston 2000), according to which policies of research oversight and funding should limit themselves to inject money in scientific communities, and let them self-organize and self-regulate.[4] Society will then receive in return all kinds of benefits (technological innovation fueling economic growth, expertise and knowledge improving living conditions, etc.). But the fact is that this classical cascade model has proved unsatisfactory on several grounds,[5] as acknowledged quite vividly in the *Nature* editorial evoked at the beginning of this chapter. When complaining

[4] See also Wilholt and Glimell (2011) for an analysis of this kind of mode of research oversight that they call "blind delegation".

[5] It has been for instance challenged on the grounds that its underlying linear model of innovation linking fundamental science to technological innovations neglects some degree of independence of the latter from the former (e.g. Rosenberg 1992; Edgerton 2004).

that science response to the needs of society is insufficient, the authors immediately warn that "just telling the same old stories won't cut it. The most seductive of these stories—and certainly the one that scientists like to tell themselves and each other—is the simple narrative that investment in research feeds innovation and promotes economic growth" (Nature 2017). I want to draw attention here to a lecture of the decline of this model in terms of evolutions of the expectations toward science.

11.2.3 Shift Towards More Targeted Expectations

It is commonplace to emphasize that science and innovation are considered as playing a central role in development projects of our societies. But what distinguishes our 'knowledge societies' from Bush's time (after World War II), is that our expectations towards science have become, I suggest, both more pressing and more targeted. This can be seen as the other side of the coin of the very success of science and innovation as a key element of so many aspects of the development of our societies. Given this central and ubiquitous role, it shouldn't come as a surprise that public science funders do not expect from science more knowledge and more technological innovation *tout court* (as Bush did), but more knowledge and more innovation in specific domains, considered as having priority because they correspond to currently pressing needs and expectations of society. This evolution can be formulated in terms of a shift away from what I would describe here as an "offer mode" and towards a "demand mode". In the first mode, scientific communities are expected to produce, following their curiosity, new reliable knowledge, which is then made available to society and, in turn, may lead to very useful developments (the laser is a case at hand, being a remote bonus from very theoretical, curiosity-driven developments of quantum mechanics at the beginning of the XXe century). In the second mode, some particular problems and needs of society are identified and deemed as having priority, and addressing them will then constraint and direct research programs towards specific topics, given, again, the central role ascribed to science in our societies.

11.2.4 Is the Shift Legitimate?

A crucial normative issue is then whether this shift towards more targeted, hence contextualized, expectations is legitimate and desirable. Two types of considerations, epistemological and political, need to be distinguished to address this normative issue. From an epistemological point of view, what are the consequences, in terms of the epistemic productivity of scientific research, of a shift towards a more interest-driven science?[6] In other words, how does a limitation of the autonomy of science with regards to the setting of research agenda impact the fecundity of sci-

[6]Note that utilitarian expectations towards science are by no means new.

ence? Independently of this epistemological dimension (which will be addressed in the next section of the chapter), the normative issue of the legitimacy and desirability of this shift has also a political dimension, which is two-fold. First, whether or not a contextualized view on the ends of science is preferable to a decontextualized one is a political issue, and should be considered and treated as such, which means (minimally) that it is not up to scientists, of for that matter to philosophers to decide, what the ends of science and innovation are. Much more could be said here to make a case in favor of a contextualized view on the ends of science. I will take for granted in the rest of the chapter that such a view is both descriptively and normatively more adequate, if only because it is a preliminary necessary condition for raising the very issue of the virtue and challenge of public participation in scientific research. Let me briefly spell out why. If one sticks to a decontextualized view on the ends of science, choices of research priorities remain internal to scientific communities, and rightly so: which problems should be addressed, when aiming at discovering the laws of nature or explaining natural phenomena, regardless of society specific needs and interests, is certainly a matter upon which scientists are in the best position to decide. But once acknowledged that science should also respond to society specific needs and interests, then it is not obvious at all that scientists are still in the best position to do so. Indeed, the priorities that would be defined by scientific communities "following their curiosity in their own discipline" as the British scientist J. Cadogan puts it in the above mentioned quotation, are unlikely to coincide with the ones defined in light of society needs.

Who, then, should be in charge? This issue will be addressed in the last part of my chapter, and the option of direct public participation in these matters is certainly an increasingly considered option worth being assessed. But let us turn before to the epistemological dimension of the normative issue of the acceptability of a limitation of scientific autonomy.

11.3 Epistemological Soundness of the Unpredictability Argument in Favour of Scientific Autonomy[7]

"I didn't start my research thinking that I will increase the storage capacity of hard drives. The final landscape is never visible from the starting point." This statement made by the physicist Albert Fert (2007), winner of the 2007 Noble Prize for his work on the giant magnetoresistance effect, expresses a very common belief, especially among scientists, about the unpredictable nature of the development and results of a research program. Such retrospective observations feed a type of 'unpredictability argument' often invoked in favor of curiosity-driven science, in contrast with interest-driven science. Polanyi gave a somewhat lyrical form of this kind of unpredictability argument in his classical essay "The Republic of Science" (1962). Science, says

[7]The following section draws directly on Bedessem and Ruphy (2019) which offers a more elaborated version of the arguments.

Polanyi (1962: 62), "can advance only by unpredictable steps, pursuing problems of its own, and the practical benefits of these advances will be incidental and hence doubly unpredictable. … Any attempt at guiding research towards a purpose other than its own is an attempt to deflect it from the advancement of science… You can kill or mutilate the advance of science, but you cannot shape it." In Polanyi's view, claims about the unpredictable nature of scientific development go hand in hand with a plea for an *internal* definition of research priorities: a problem should be considered important in light of considerations internal to a field of scientific inquiry and not (at least not primarily) in light of external considerations, such as practical utility or relevance for political decisions (expertise). The orientation of the inquiry by such external objectives is then deemed epistemically counter-productive and vain: one should not attempt to predict the unpredictable. In this section, I will challenge a crucial but often implicit assumption in the traditional defense of scientific freedom based on scientific unpredictability (such as Polanyi's or Fert's), namely the assumption that a free, curiosity-driven science is more likely to generate unexpected facts and hence to be pioneering, creative and fecund. But what are actually the conditions favoring the emergence of novelty in the course of a scientific investigation? This important issue has not received much epistemological attention.[8] I will fill this gap by first distinguishing two kinds of unpredictability arguments often mixed when debating on scientific freedom, to wit, unpredictability as unforeseen practical applications and unpredictability as unforeseen new lines of research and discoveries. Focusing on the latter, I will identify epistemological conditions that favor the occurrence of unexpected facts in the course of a scientific investigation and discuss, in light of these conditions, whether curiosity-driven research is more, or less, hospitable to the unpredictable than interest-driven research.

11.3.1 Two Kinds of Scientific Unpredictability

When unpredictability refers to unexpected applications, the argument is the following: freedom of research should be preserved since a free, curiosity-driven science is needed to generate a reservoir of fundamental knowledge, which then can be used to develop applications. This argument was typically developed by Vannevar Bush who appealed to the now classically called linear model of innovation linking pure science and practical applications:

> Basic research leads to new knowledge. It provides scientific capital. It creates the fund from which the practical applications of knowledge must be drawn. New products and new processes do not appear full-grown (1945: 20).

[8]Wilholt and Glimell (2011: 353) do touch upon this issue when discussing the link made by proponents of the autonomy of science between freedom of research and diversity of approaches favoring the epistemic productivity of science. But they just note that it is a strong assumption and do no further discuss its validity.

The development of the A-bomb in the frame of the Manhattan project is a paradigmatic case. As Bush emphasizes (1945: 20), accumulating fundamental knowledge about the structure of the matter is what allowed the development of the A-bomb. Another frequently cited example of unpredictable application is the invention of the laser, a widely-used technological device nowadays, made possible by pure theoretical developments in quantum physics during the first half of the XXe century. I will not discuss further this first version of the unpredictability argument, if only because its underlying linear model of innovation linking pure science and practical applications has already been challenged on several grounds, as mentioned earlier (see footnote 5). Rather, I want to focus on the second (and also widespread) type of unpredictability arguments, whose validity has been much less scrutinized.

In this second type of argument, unpredictability refers to cases when unexpected observation or result opens up a new line of research leading to a fundamental discovery. A very well-known historical episode illustrating this kind of unpredictability is the invention of the first antibiotic by Fleming, after he had accidentally observed the effect of a fungus (Penicilium) on bacteria colonies (Fleming 1929). Also often cited is the discovery of radioactivity by Becquerel (1896): when working with a crystal containing uranium, Becquerel noted that the crystal had fogged a photographic plate that he had inadvertently left next to the mineral. This observation led to posit that uranium emitted its own radiations.

When unpredictability refers to such unexpected developments, freedom of research is defended on the grounds that scientists should be able to freely change the direction of their research or open up new lines of inquiry in order to be able to follow up on unexpected results, thereby generating new knowledge and innovation. But to properly work in favor of scientific autonomy, the argument actually presupposes that the occurrence of surprising facts is more likely to happen in a curiosity-driven system of science than in an interest-driven one. For increasing the production of new knowledge and innovation does not only depend on being able to freely follow up on unexpected facts, it also (obviously) depends on whether occurrences of unexpected facts are favored, to start with. It is thus necessary to clearly distinguish between two types of considerations, too often mixed in defense of scientific freedom: considerations on the occurrence of unexpected facts and considerations on the (institutional, material) possibility to follow up on them.

I will not discuss for the moment the second type of considerations (management of the unexpected) and focus on the first (genesis of the unexpected), which has been largely neglected in the literature on scientific freedom, namely the epistemological conditions that actually favor the occurrence of surprising facts. The central epistemological issue can then be reformulated as follows: is it the case that when the inquiry is interest-driven, unexpected facts are less likely to occur than when the orientation of the inquiry is set internally by scientific communities following their curiosity?

11.3.2 Epistemological Conditions Favoring the Occurrence of Unexpected Facts

By 'unexpected facts' occurring in the course of an inquiry, I simply mean here results (observations, outcomes of an experiment, etc.) that cannot be accounted for within the theoretical framework in which the empirical inquiry has been conceived and conducted. This kind of "exteriority" is what leads scientists to move away from the initial explanatory framework and open up new lines of inquiry in search of an alternative one that could accommodate the unexpected results. My central claim is that occurrence of unexpected facts follows from our partially uncontrolled intervention on the (complex) real world. Consequently, as I argue in more details below, there are no good epistemological reasons to claim that curiosity-driven research is more hospitable to the unexpected than interest-driven research. Let us turn now to a first epistemological condition favoring the unexpected.

Isolation and purification of phenomena It is now a well-known feature of contemporary experimental sciences that many of their objects under study are "created" in the laboratory rather than existing "as such" in the real world. When drawing our attention to this epistemologically important feature, Hacking (e.g. 1983, Chap. 13) specified that we should not read this notion of "creation" of phenomena as if *we* were *making* the phenomenon, suggesting instead that a phenomenon is "created" in the laboratory to the extent that it does not exist outside of certain kinds of apparatus. This is typically the case for a phenomenon like the Hall effect: it did not exist "until, with great ingenuity, [Hall] had discovered how to *isolate*, *purify* it, create it in the laboratory" (Hacking 1983: 226, *our italics*). In other words, Hall created in 1879 the material arrangement—a current passing through a conductor, at right angles to a magnetic field, for the effect to occur and "if anywhere in nature there [was such an arrangement, *with no intervening causes*, then the Hall effect [would] occur" (1983: 226, *our italics*). Isolation, purification, control of intervening causes (i.e. control of physical parameters) are noticeable features of an experimental protocol that have a straightforward consequence directly relevant: they tend to limit the number of causal pathways which can influence the response of the object or phenomenon under study experimentally. Unknown causal pathways existing in the real world are thus inoperant (or less operant), thereby limiting the occurrence of unexpected results. Hence our first criterion to evaluate whether a certain system of science favors the occurrence of surprising results: the more the phenomena under study in that system are isolated, purified in highly regimented experimental conditions, the less likely the occurrence of unexpected results is.

Theoretical unifying ambition Another relevant factor is the degree of generality of the theoretical framework within which the inquiry takes place. Scientists working within a theoretical framework with a large unifying scope will be reluctant to "leave" it and search for an alternative one when facing an unexpected result, and for good epistemological reasons: there is (obviously) a high epistemic cost of abandoning a theoretical framework that provides explanations for a large set of phenomena.

The right move is rather to try to accommodate the surprising result by adopting, if necessary, ad hoc hypothesis or tinkering with some ingredients of the existing theoretical framework, so that the result loses its "exteriority" and ends up being integrated. And because of this well-known "plasticity" and integrative power of well-established theoretical frameworks with a large unifying scope,[9] when a (at first sight) surprising result occurs, it rarely leads to the opening up of a new line of inquiry in search of an alternative explanatory framework, but rather gets integrated within the existing one, thereby losing its unexpectedness.

There is another reason why a high degree of theoretical generality does not favor the occurrence of unexpected results. By constraining the type of experimental procedures developed and the type of data generated, a theoretical framework with a large unifying scope tends to *homogenize* the experimental works conducted to probe the various phenomena that it accounts for. And since a diversity of experimental approaches increases the possible sources of emergence of surprising facts, we can conclude that by reducing this diversity, theoretical generality makes the occurrence of unexpected facts less likely to happen.

11.3.3 Comparative Analysis

In light of the criteria proposed above, how does curiosity-driven science score compared to interest-driven science when it comes to favoring the occurrence of unexpected facts? Let us first compare the two in light of our first criterion based on the degree of isolation and purification of the phenomena under study. A directly relevant feature of interest-driven science is the use of what Carrier (2004) calls "contextualized causal relations" rather than full causal chains. Interest-driven science, or use-inspired science as it is also called, typically aims at directly intervening on a process or phenomenon often disposing only of a partial knowledge of the causal chains involved and without being able to isolate it from various causal influences exerted by the rest of the physical world. A direct consequence of this feature of use-inspired science is the low degree of control of its experimental protocols. By contrast, to the extent that pure, curiosity-driven science aims primarily at answering fundamental theoretical questions about the world, it designs highly regimented experimental procedures that isolate and purify phenomena in order to be able to get empirical answers about the specific fundamental processes questioned in the theoretical investigation.[10] Moreover, building highly regimented experimental procedures requires knowledge of full causal chains in order to be able to better control

[9]Classical references on these ideas of plasticity or integrative power are of course Kuhn's description (1962) of scientists being busy working on resolving anomalies in normal science and Lakatos' concept of "protective belt" of a research program (1978).

[10]Carrier sums up this contrast as follows: "Empirical tests often proceed better by focusing on the pure cases, the idealized ones, because such cases typically yield a more direct access to the processes considered fundamental by the theory at hand. But applied science is denied the privilege of epistemic research to select its problems according to their tractability (…). Practical challenges

the response of the system under study. The outcome of the application of our criterion is straightforward: compared with pure, curiosity-driven science, use-inspired, or interest-driven science favors the occurrence of unexpected facts to the extent that its experimental procedures tend to be less controlled and based only on partial knowledge of the causal influences exerted on the phenomenon under study.

The etiology of cancer provides an interesting illustration of this claim. Indeed, many current cancer therapies built in the frame of use-inspired research are based on contextualized causal relations. Typically, if a cellular agent is found to be massively expressed in cancer cells, drugs are designed to inhibit it, even if the whole causal chain determining its action is not known. For instance, a large amount of proteins promoting angiogenesis (the growth of blood vessels), notably VEGF (Vascular Endothelial Growth Factor), was found in tumoral cells, leading to the design of anti-VEGF molecules (Sitohy 2012). These molecules are used without considering the complete causal chain in which the VEGF is embedded. Only their known action on angiogenesis is considered. The clinical tests have led to unexpected observations: the use of an anti-VEGF molecule (Avastin) can stimulate tumor growth (Lieu et al. 2013). This example shows that the use of contextualized causal relations promotes the occurrence of surprising facts by allowing unknown mechanisms to intervene in the experimental procedure.

Let us now compare curiosity-driven science and interest-driven science in light of our second criterion. Whereas pure, curiosity-driven science often aims at providing comprehensive and unifying theoretical frameworks (think of the Standard Model in particle physics or the Big Bang model in cosmology), interest-driven research is often characterized by the coexistence of numerous local models, each determining the development of specific experimental procedures. An extreme case of this locality are for instance the design-rules used in the industry, which are built as laws guiding action (Wilholt 2006). They are experimentally confirmed rules providing relations among different relevant parameters to manufacture industrial products. These rules are extremely specific: they apply to a very few numbers of situations and each of them determines a singular experimental practice. The use of local models is also widespread in the biomedical sciences, a typically interest-driven field of research. I will draw again on oncology to illustrate my point. Consider for instance the case of the development of radiotherapy protocols in the first half of the XXe century. The aim was to intervene on cancer to cure it, without any general model describing the mechanism of carcinogenesis. This program promoted the development of a variety of exploratory approaches using X-rays against cancer (Pinell 1992). As there were no standardized protocols, many experimental procedures were tested, changing the density of X-rays received, the distance of emission, the frequency of the radiotherapy sessions. In order to improve the efficiency of the therapeutic methods, scientists tried to build various local models describing the action of X-rays on cancer, corresponding to the variety of experimental procedures implemented. Grubbe (1949) formulated a model based on the inflammatory reaction to explain the effects of radiotherapy on

typically involve a more intricate intertwinement of factors and are thus harder to put under control" (2004: 4).

cancer: the inflammation of the surrounding tissue beyond the effects of X-rays is responsible for the decrease of tumoral mass. This model reflects his specific use of X-rays: he applied very high doses, necessary to generate an inflammatory response. In parallel, Tribondeau and Bergonié, using more moderate doses, developed a model based on the proliferation of the cells in tumoral context, which led to the "Bergonié law": X-rays have a higher impact on proliferating cells (Bergonié and Tribondeau 1959). The outcome of the application of our criterion is then again straightforward. By promoting the use of a diversity of local models and heterogeneous experimental protocols, interest-driven science favors the occurrence of unexpected facts, whereas the penchant of pure, curiosity-driven science for comprehensive unifying theoretical and explanatory frameworks, hence homogenized experimental protocols, does not.

11.3.4 Intermediate Conclusion

Our previous analysis has established that several features of pure, curiosity-driven science make it no more hospitable than use-inspired, interest-driven science to the occurrence of unexpected facts. For all that, it does not follow that proponents of freedom of science cannot appeal anymore to the unpredictability argument to make their case. For the issue of which conditions favor the occurrence of unexpected facts is only half of the story. The other half is the possibility to actually follow up on these occurrences and open new lines of inquiry. And this other half raises different issues. What are the institutional, organizational structures of science that make it easier for scientists to re-orient their research when needed? To what extent an initial orientation of a scientific investigation by external practical needs is less compatible with the opening of new lines of inquiry than an initial orientation by epistemic considerations internal to the dynamics of a scientific field? When appealing to the unpredictability argument, proponents of free, disinterested science not only presuppose that it is the best system of science to generate unexpected facts to start with—a contention that I have challenged in this section—but also that it actually gives more freedom to scientists to follow up on unexpected results. In other words, the issue of the possibility for researchers to change the direction of their line of inquiry when needed is somewhat mixed, confused with the normative issue of what the aims of science should be (in short, increase knowledge following considerations internal to science *vs.* answer external needs). But the two issues, I contend, should be kept separate. After all, one can very well conceive a system of science whose aims are primarily to answer society needs but which nevertheless leaves scientists free to choose the lines of inquiry that seem *to them* the most promising ways of fulfilling these needs (which includes changing research directions if needed). Otherwise put, one can very well conceive a use-inspired, interest-driven science which is not a *programmed* science in which scientists are asked to plan every step of their inquiry in order to achieve a given aim. And note that a pure, curiosity-driven science may be as much programmed as an interest-driven one: the fact that scientists are left free to choose the aims of their research does not protect them from having to plan every step to reach these aims. In any case, my purport in this section is not to attack pure, curiosity-driven science. There may be, no doubt, many good reasons to defend it, but the widespread, traditional one appealing to the unpredictability of scientific inquiry is certainly not the most cogent and solid one. Consequently, from an epistemological point of view, resistance to a limitation of the autonomy of science (hence in particular to public participation in the setting of research agenda) grounded in views on the very nature of the dynamics of scientific development and innovation is not well-founded.

11.4 Pro's and Con's of Public Participation

Now that epistemological room has been made for external, interest-based guiding of scientific research, let us turn to a comparative discussion of the shortcoming and virtues of various possible options to define priorities in the setting of research agenda, focusing in particular on the public participation option. When discussing such options, a first step, I suggest, is to distinguish between two main ways of identifying the interests that should shape research and innovation agenda, which are 'objectivist, substantialist' and 'non-objectivist' ones. According to an objectivist, substantialist conception, the needs and interests of society that scientific research and innovation should respond to can be defined independently of what members of the society, the citizens, would identify and express as being these needs. By contrast, according to a non-objectivist conception, these needs and interests are just those identified and expressed by the citizens (by some appropriate process—more on this crucial issue later).

11.4.1 Objectivist, Substantialist Conceptions of the Goals of Scientific Research and Innovation

In an objectivist, substantialist approach, some subset of citizens are in charge of defining the needs of society, that is, "the collective good that scientific inquiry is supposed to fulfill", as Kitcher puts it (2001: 137). Who are the candidates and by virtue of what quality can they be considered as being legitimately in charge of defining this collective good? Given the antecedents of philosophers in matters pertaining to the definition of the common good, it should come as no surprise that philosophers of science are inclined to take up the task. And indeed, some philosophers of science with a taste for a socially relevant philosophy of science (admittedly a rather rare species in the contemporary philosophical landscape) have come up with propositions. Kourany (2012) for instance developed an ideal of "socially responsible science", whose agenda should be shaped by "sound social values", so that its outputs meet the needs and expectations of society (2012: 348). This kind of propositions clearly partakes of an objectivist, substantialist approach: the guiding "sound social values", whatever they are exactly, are taken as being universally shared and Kourany insists that neither "the market" nor "the politicians" should be in charge of defining them (2012: 346). Note that when defined in an objectivist, substantialist way, the goals of science and innovation may admittedly coincide with the goals that would be defined in a non-objectivist way by the whole set of members of society, but it *needs not* be so.

11.4.2 Epistemic Elitism

An objectivist, substantialist approach of the definition of the main goals of science and innovation such as Kourany's goes hand in hand with what can be described as 'epistemic elitism': a subset of members of a society is considered as being legitimately in charge in virtue of having some privileged epistemic position in that society. This is by no means a recent feature of the organization of science. As Kitcher (2001: 137–138) for instance reminds us, epistemic elitism was at the core of what can be considered the first document of science policy, namely, Francis Bacon's description of Salomon's House in his fable the *New Atlantis* (Bacon (1627) 1966). The wise inquirers of Salomon's House were in charge of defining the human needs, taken as being universal, that scientific inquiry should fulfill, independently of the actual needs that the rest of the citizens might have expressed, had they been consulted.

In our contemporary societies, given the central role assigned to the natural sciences and technology, researchers in these fields (rather than from the social and human sciences) account, not surprisingly, for most of the members of our modern versions of Salomon's House. Consider for example in France the *Conseil stratégique de la recherche* (Research Strategic Council) reporting to the French Prime Minister, whose mission is to "identify and propose a limited number of big research and technological priorities to prepare and construct the future of France".[11] Looking at who is involved in the choices made about research priorities at the national level in France today is telling. Eminent researchers from the natural sciences make the bulk of it and France is, in this regard, no exception. I will comment later on the other members of the Council (a minority, at least numerically). For the moment, I just want to emphasize that the option of epistemic elitism is (still) alive, both descriptively and normatively, in our democracies. But is it satisfactory?

There are, at least, two reasons to be unhappy with epistemic elitism (restricted to the natural sciences). First, one can challenge that the privileged epistemic position of scientists is *relevant* in those matters. Second, one can challenge the very idea of an objectivist, substantialist conception of the goals of science underlying epistemic elitism.

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[11] http://www.enseignementsup-recherche.gouv.fr/cid75958/www.enseignementsup-recherche.gouv.fr/cid75958/conseil-strategique-de-la-recherche.html. Accessed August 2016.

With regards to the first source of concern, remember first that, in this chapter, a *contextualized* view of the aims of science and innovation is taken as being the appropriate framework of discussion, both for normative and descriptive reasons (see end of Sect. 11.2). That means, I would remind, that scientific research is supposed to trigger new knowledge and innovation *responding to the needs of society*, not only to produce new knowledge for its own sake. Eminent scientists are members of research guiding bodies as epistemic experts in their field of speciality. That would suffice to qualify them for the task if the aims of science were decontextualized. In that case, yes, practicing scientists are in the best (epistemic) position to determine what are the most promising lines of research to increase knowledge in their fields. But why would this kind of expertise put them in an epistemically privileged position when it comes to defining what the needs and interests of society are? To put it otherwise, their epistemic expertise is not the kind of epistemic expertise needed to grasp what the needs of society are at a certain time of its history. Who, then, could have this relevant kind of expertise?

It would certainly be interesting to contemplate an articulation of epistemic expertise coming from both the natural sciences *and* the human and social sciences (after all, the epistemic aims of the latter include precisely providing knowledge about the needs and interests of society). This kind of interdisciplinary endeavour is for instance what Kourany has in mind to define the proper goals for science (2012: 346). I will not, however, pursue here this line of thought, because I want to question the very idea of defining the goals of science and innovation in an objectivist, substantialist way. In other words, rather than trying to improve the functioning of Salomon's house, one should, I contend, rebuild it on new foundations altogether.

11.4.3 Non-objectivist Views on Defining the Goals of Science and Innovation

The main reason for giving up an objectivist, substantialist approach is straightforward: democracy. Indeed, pleas for a democratisation of the setting of research and innovation agenda are easily made on political grounds[12]: citizens are affected in their daily life by scientific developments and innovations (just think of genetic tests, nanotechnologies, genetically modified food (GMO), etc.), research is (at least partly) funded by their taxes, so why shouldn't citizens have their say? But if so, in what matters exactly should citizens have their say and how? It could first be noted that various kinds of participatory devices, as for instance deliberative forums open to lay citizens, have already been implemented in relation to science and innovation. However, they usually focus on a particular issue (e.g. innovations in nanotechnology), not on the broader issue of what are the needs and interests that science and

[12] A more sophisticated philosophical case in favor of a non-objectivist approach to the definition of the goals of science taking the form of a democratisation of the setting of research is offered by Kitcher (2001, Chap. 11).

innovation should respond to in priority. Moreover, many of these democratic experiments have been a recurrent source of dissatisfaction, and rightly so, for the people involved, and criticised on the ground that, to put it briefly citizen participation was only an alibi for diminishing resistance to new technologies. It is not my purpose here to further discuss the actual role played by these democratic experiments, nor the actual influence of their outputs, nor either the actual intentions and aims of the institutions implementing them.[13] The rest of the chapter will rather map out, from a philosophical point of view, pro's and con's of various possible ways of democratizing the setting of research agenda.

11.4.4 Comparative Discussion

Let us start by going back to the composition of the French research guiding body evoked earlier, the *Conseil stratégique de la recherche*. Along with eminent scientists, three other types of people are involved (again, seemingly marginally, at least by their number): a few representatives of big French companies (e.g. Total, Orange, EADS), three elected representatives and… a rather well-known novelist, Marie Darrieussecq. It is not clear by virtue of what quality this novelist is included in the council, but let us suppose here that it is as a lay citizen. This composition is telling in that it reflects the main options currently on the table, implicitly mixing objectivist approaches and non-objectivist ones. The epistemic elitism option, lying within the objectivist framework, is (still) dominant (at least, again, by the number of eminent scientists involved), but our modern version of Salomon's house has become a bit more inclusive: citizens are invited in, via their elected representatives or directly (the novelist) and "the market" is also on the guest list. What these new guests have in common is that their expectations towards science go beyond "seeking the knowledge of causes and secret motions of things", which was the core expertise of the members of Salomon's House (Bacon (1627) 1966: 288). Let us discuss each of these options in turn, while keeping in mind that what is sought for is a better alignment between the outputs of scientific research and innovation and the needs and expectations of society.

Assessing the "market option" as a mean to respond to the needs and interest of society takes us back of course to a much broader and long-standing political debate. It suffices here for our purpose to emphasize that when guided by economic interests (be it directly when conducted by private companies or indirectly by cooperation agreement between private and public laboratories), research and innovation can only respond to a limited (albeit still central) subset of the needs of society (to wit, the economic ones). Moreover, as Hiroi (2019) suggests, this limited subset may even become less central in our societies, as alternative measurements of wealth, such as "happiness studies", are on the rise.

[13]This issue is especially worth being addressed in the case of RRI (Responsible Research and Innovation) actions implemented in H2020.

On the face of it, the two other options (our elected representatives and direct participation of lay citizens) allow avoiding the pitfall of a scientific and innovation agenda overly restricted to economic interests. After all, our elected representatives are supposed to convey the whole range of needs and interests of the people they represent. But the fact is that the rhythm and demands of political life are hardly compatible with the kind of long term engagement needed for the development of a scientific program. Consequently, our elected representatives may be biased toward short-term, practical expectations, neglecting long-term needs and interests in matters of science and innovation of the citizens they represent.

In principle, contrary to the two preceding options ("the market" and our elected representatives), the option of direct participation of lay citizens in the setting of research agenda avoids, by design so to speak, the pitfall of a possible gap between the actual needs and interests of the citizens and the needs and interests actually taken into account in the setting of research and innovation priorities. This presupposes, admittedly, that the participatory processes are well designed so that their outcomes do correspond to the collective needs and interests of the concerned citizens. The implementation in the real world of such participatory devices is notoriously difficult, with all kinds of unwanted biases, and I can only refer here to the numerous studies in sociology and political sciences dealing with these issues, as well as to the lessons drawn from past experiences (e.g. Fishkin 2009). I will take for granted that appropriate participatory devices can be implemented and discuss another problem facing, this time in principle, the public participation option. To what extent is the small subset of citizens involved in a participatory device representative of the rest of the citizens? The participating citizens are not elected, so they cannot "act for" the rest of the citizens and be accountable to them in the way that elected representatives "act for" and are accountable to their constituents. As Brown (2004: 86) explains, participating citizens, at best, just "stand for" the rest of the citizens. But then how do they grasp and make representations of the views of the rest of the citizens on what the agenda of research should be? "Without input from their constituents, the deliberators must rely upon introspection, intuition, or speculation to assess popular preferences", says Brown (2004: 86). This might be all very nice, but incompatible, as Brown warns us, with the expectation that the recommendations so obtained "have binding force on elected officials" (2004: 86). I thus see this lack of political representativeness of the participating citizens as a serious, in principle, shortcoming of the public participation option, especially when one requires more than the granting of a mere consultative role to these participatory devices.

11.5 Concluding Remarks on Pending Issues

As could be expected, no clear winner has emerged from the previous comparative analysis, so that a crucial pending issue is the following: how should these various options (including the 'epistemic elitism' option) be articulated? In the present state of affairs, the articulation mainly boils down to a mere *juxtaposition* of them: eminent scientists, elected representatives, representatives of the private sectors, sometimes other stakeholders from the lay society (but the participation of lay citizens remains anecdotal) sit around a table and deliberate (see for instance, again, the French Research Strategic Council). This is unsatisfactory because it tends (not surprisingly) to lead to a play of power between various groups defending their own agenda (not to mention the accompanying lack of transparency unfortunate in a democracy). In particular, fundamental research is then held, regrettably, in tension with interest-driven research. What is needed is a *non-competitive, integrative* articulation. Such an integrative framework should be based on a contextualized view of the ends of science. Note that there can be plenty of room for fundamental, curiosity-driven research within this framework [see for instance the notion of "use-inspired basic research" analyzed by Stokes (1997)]. Another specification for the integrative framework is that our elected representatives cannot be left out of the picture, as it is the case in Kitcher's widely-discussed (at least in philosophy of science) ideal of "well-ordered science" (2001), nor can they just occupy a back seat, given their accountability and central roles in our *representative* democracies. How then direct participation of the citizens can be articulated with indirect participation (via their elected representatives) is, no doubt, a central challenge to be met, but not one specific to the science and technology sphere. Also, epistemic experts (practicing scientists both in the natural and human and social sciences) must keep a central role in the integrative framework but this role should be redefined as being informative and not decisional. Moreover, interdisciplinary work between the natural and social and human sciences is needed to clearly identify the needs and interests of society that cannot be addressed by the market. Interdisciplinary work is also needed to anticipate on the evolution of the needs of society, so that scientific research can adjust its agenda in time.

I have only offered in this chapter some general specifications for the building of an integrative framework. Much more work remains to be done to elaborate detailed blueprints for the democratic rebuilding of Salomon's house. Successful experiments at local levels of inclusive research practices such as "community-based collaborative action research" (Kusago 2019) may provide insights for more general plans, the aim being to come up with a renewed, more democratic and workable conception of the role that society should play in the course taken by scientific developments and innovation.

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