Which forms of limitation of the autonomy of science are epistemologically acceptable (and politically desirable)?

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This paper will investigate whether constraints on possible forms of limitation of the autonomy of science can be derived from epistemological considerations. Proponents of the autonomy of science often link autonomy with virtues such as epistemic fecundity, capacity to generate technological innovations and capacity to produce neutral expertise. I will critically discuss several important epistemological assumptions underlying these links, in particular the "unpredictability argument". This will allow me to spell out conditions to be met by any form of limitation of the autonomy of science to be epistemologically acceptable. These conditions can then be used as a framework to evaluate possible or existing forms of limitations of the autonomy of science. And it will turn out that the option of direct public participation (a lively option in philosophy of science today) might not be the best way to go to democratize the setting of research agenda.

1. Introduction. Pleas for a democratization of the setting of research agenda are often made, and rightly so, on political and moral grounds. In a nutshell, citizens, it is argued, are affected in their daily life by scientific breakthroughs (genetic tests, nanotechnologies, GMO, etc.), and research is (at least partially) funded by their taxes, therefore, in a democratic society, they should have their say in the choices made about research priorities. In itself, this line of argument (which I will endorse here without further arguments) leaves open the issue of which political forms of limitation of the autonomy of science are preferable - a lively option being theses days in philosophy of science some form of direct public participation in the setting of research agenda (e.g. Kitcher 2001, 2011). My aim in this paper is to investigate whether constraints on possible forms of limitation of the autonomy of science can be derived from epistemological considerations.

My starting point will be traditional, utilitarian lines of defense of the autonomy of science. In that perspective, autonomy (in the sense of self-governance) is first considered as a necessary condition for the epistemic and practical successes of science. In other words, when science is left free to define internally its priorities and epistemic aims, it produces more and better knowledge, directly or indirectly useful to society, via in particular technological innovation. Second, autonomy (in the sense of independence and self-regulation) is considered as a necessary condition for the epistemic authority of science. Only when protected from outside influences (commercial, political special interests), so the argument goes, can science deliver the neutral expertise necessary for the proper functioning of a democracy. Note that this link between autonomy and utility for society is at the core of the very influential view of scientific governance defended by Vanevar Bush in his well-know report published in 1945, *Science, The Endless Frontier*. In that document, Bush makes a case for a very broad societal utility of science: "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). And the condition of that progress is, according to Bush, a complete autonomy of science: "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).[1]

This kind of utilitarian arguments in favor of scientific freedom associates (more or less implicitly) autonomy with various virtues such as epistemic fecundity, capacity to respond to societal practical needs and neutrality. I will first identify and critically discuss several important epistemological assumptions underlying these links, such as what I will call here the unpredictability argument and the diversity argument. I will distinguish in my discussion autonomy in the sense of self-governance as regards the setting of research agenda and autonomy in the sense of independence and self-regulation, in particular as regards the functioning and composition of scientific communities. This will allow me to spell out conditions to be met by any form of limitation of the autonomy of science to be epistemologically acceptable. These conditions can then be used as a framework to evaluate possible or existing forms of limitations of the autonomy of science. And it will turn out that the option of direct public participation might not be the best way to go to democratize the setting of research agenda.

2. Autonomy and epistemic fecundity. "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

[1] For a more extended discussion of this link between autonomy and societal utility in Bush's report, see for instance Stokes (1997).

results of a research program. Such retrospective observations feed a central argument often invoked in favor of the autonomy, which can be dubbed the *unpredictability argument*. A somewhat lyrical form of this argument was given by Polanyi in his classical essay "The Republic of Science" (1962). Science, says 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." Therefore scientists must be free "to assess … the depth of a problem and the importance of its prospective solution primarily by the standards of scientific merit accepted by the scientific community." But what exactly is behind this unpredictability argument?

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. From this perspective, a problem is deemed important in light of considerations internal to a field of scientific inquiry, such as the potential impacts of its resolution on other epistemic issues central to the field, and not (at least not primarily) in light of external considerations, such as societal utility. The unpredictability argument thus boils down to the claim that because of the unpredictable nature of scientific development, choices of research priorities in a field must only be based on considerations internal to its own dynamic. And if one also buys into the idea that only scientists can master such considerations, then scientists must be left free to define research priorities. But it is easy to see that, as it is, the argument is incomplete. To work in favor of scientific autonomy, the argument must be enriched and reformulated in a comparative form, as follows:

> A field of research is epistemically less productive when its objectives are defined externally than when they are defined internally because, in the latter case, unexpected fundamental discoveries and practical applications are more likely to happen.

This claim immediately raises a simple, empirical question (but which is surprisingly rarely really addressed): is that the case? Does history of science, in particular, show us that a research program whose objectives are defined externally is systematically epistemically less fecund than a research program whose objectives are "disinterested"? After all, examples of finalized research programs having produced along the way unexpected fundamental discoveries are not so rare. For instance the motivations of the Nobel Prize's winners Arno Penzias and Robert Wilson who discovered the 3 Kelvins cosmic microwave background were very practical and had to do at the beginning with improving the quality of transatlantic radio communication. But along the way, this research program led to this very fundamental discovery in cosmology. Industry research on the giant magnetoresistance effect in the 1990s is another telling example of research undertaken under considerable pressure to produce applicable results but which nevertheless produced, along the way, very fundamental knowledge (Wilholt 2006). Of course, an accumulation of examples that could reassure the epistemic pessimistic as regards finalized research will not be enough to invalidate the unpredictability argument, because of its comparative form. Recall that the argument states that a field of research is epistemically *less* productive when its aims are defined externally (i.e. not primarily according to considerations internal to its own dynamic). But the problem is that history of science does not offer any control group. It is just not possible to compare the fecundity of a field when it is left free to define its priority with the fecundity of the same field whose research agenda would be defined externally in order to respond to societal needs. Thus the debate cannot be closed empirically, other considerations are needed.

On the face of it, a promising way could be to draw on what I will call the *diversity argument*. In a nutshell, the argument is the following:

- You cannot predict which line of research (problems + approaches) will turn out to be epistemically fecund or dead ends.
- Maximizing the fecundity of a scientific field thus requires maximizing the diversity of the lines of research (problems + approaches).
- Leaving the scientists free to define their own research agenda is the best way to maximize the diversity of the lines of research.

Clearly, the validity of this argument hinges on the third step, which brings us to considerations partaking of social epistemology. But as far as I know, social epistemology does not provide any good reasons to believe that scientists' freedom of research promotes diversity of lines of research. And given the natural tendency in some fields to monoculture (see for instance the domination of the Big Bang model in cosmology), it seems that we may even reasonably doubt that it is the case[2]. So my first intermediate conclusion is the following: there is no good epistemological reasons to reject any form of externalization of setting of research agenda on the ground that it would diminish the epistemic fecundity of science (*contra* the traditional argument *à la* Polanyi). For all that, claiming that an externalization of the setting of research priorities might be epistemologically acceptable does not mean of course that *any* form of such externalization is epistemologically acceptable. On the contrary, my previous discussion of the link between autonomy and epistemic fecundity establishes that as regards the epistemic productivity of science, what matters is not that

[2] Not everybody would agree though, see for instance Wilholt (2010, 176) for whom the alternative to free choice of projects would be the existence of central authorities who would organized diversity. But it is not clear why an externalization of the setting of research agenda would require that centralized authorities possess "complete and detailed global and local knowledge".

research aims are defined internally, but that the setting of research priorities promotes a diversity of lines of research. Hence the first condition that must be met by any form of limitation of the autonomy of science: ensuring diversity of lines of research.

3. Autonomy and accountability. Let us discuss now a second underlying assumption of the defense of the autonomy of science on utilitarian grounds, that is, the link between autonomy and accountability. The notion of accountability may refer to two distinct types of expectations. Firstly, one can expect from science that it actually delivers the anticipated societal benefits. In other words, to put it trivially, the funding bodies want results for their money. Secondly, expectations can be of a moral nature: scientists can be hold responsible, not only of course for the methods they use, but also for the potential negative impacts their results may have on the citizens' lives[3]. I will focus here on the first kind of accountability, in terms of efficiency to provide the expected societal benefits.

In Bush' views, recall that efficiency is linked to autonomy: autonomy is seen as a necessary condition for science to deliver the expected societal benefits. But is it the case that science is better able to produce what society expects in terms of applicable knowledge and innovations when it is autonomous? Note first that historically, Bush's views started to be called into question on efficiency grounds. Funding bodies, such as the American federal government considered that the return in terms of technological innovations and economic productivity was insufficient. To oversimplify a complex story, American science was considered as too "selfish": too many Noble Prizes and not enough technological innovations (Guston 2000, 138). Challenging the capacity of an autonomous science to actually deliver the expected gains in terms of technological innovations is certainly a good reason to question Bush's views. But I will not discuss further here this delicate and complex issue of the

[3] For a recent discussion of the various dimensions of the notion of scientific responsibility, see Douglas (forthcoming).

efficiency conditions of technological transfer. I would rather draw attention on another essential reason to reconsider the link made between autonomy and efficiency. My proposal is that we should look more closely at the nature of society's expectations and take into account their evolution. For, as noticed by Neal Lane (1997), who was former director of the *National Science Foundation*: "It is not that science did not deliver in so many ways over so many years, but rather that different times require different types of accountability."[4] So what are today the types of societal expectations that science must respond to, and to what extent do they differ from the society's expectations at the heyday of the Bush's model?

I will suggest that the significant feature of the evolution of society's expectations is that they have become more specific, more targeted. First because of the increasing "scientifization" of politics (more and more political decisions call upon scientific expertize on precise issues such as the evolution of the climate or the dangerousness of GMO); second, society's expectations in terms of technological benefits have also become more specific, more targeted. Technological solutions to particular problems are expected (such as how to store photo voltaic energy), and not technological innovation *tout court*, such as the next laser, which would not answer pre-existing needs. So my point is that an autonomous science might well be able to answer global, unspecific expectations (Bush's global expectations - more jobs, better health, technological progress, etc.), but it is very likely less able to meet specific, targeted needs.

My previous analysis of the assumptions underlying the unpredictability argument has shown that a limitation of the autonomy of science (in the form of an externalization of the definition of its research priorities) is epistemologically acceptable as long as it fulfills the condition of diversity of lines of research. The above analysis of the accountability of science in terms of efficiency shows that such a limitation is not only epistemologically acceptable

[4] I borrow this quotation from Guston (2000, 1).

but also necessary. Since societal expectations toward science have become more targeted, an autonomous science whose research priorities are set internally will be less efficient in responding to these expectations.

4. Autonomy and neutrality. Another line of utilitarian defense of the autonomy of science states that only when protected from outside influences (e.g. commercial, political special interests) can science deliver the neutral expertise necessary for the proper functioning of a democracy. Autonomy (here in the sense of independence and self-regulation) is thus considered as a necessary condition for the epistemic authority of science (as long as, of course, this self-regulation obeys proper basic methodological norms). The central issue is then the following: Is a self-governing scientific community more likely to function according to methodological canons that maximize neutrality and impartiality?

Two kinds of considerations may be relevant here: empirical considerations, in that case historical, and considerations provided by social epistemology. Empirical considerations immediately suggest that the condition of self-regulation and independence is far from being enough to guaranty the neutrality of the results produced. Thanks in particular to feminist philosophical and historical studies of science, cases of ideological biases are now well documented in various disciplines. And those cases are not cases *à la* Lyssenko (that is, cases departing from basic methodolgical norms), but cases where a scientific community, largely independent from political power or interest groups, conforming to traditional canons of good science, nevertheless produces non neutral results, which are influenced by dominating ideologies in the broader society. Examples of such "good" but biased science can be found in particular in primatology, archeology, biology, etc.[5] How is that possible? Let me just refer here to Helen Longino's now well-known work, which offers a precise analysis of how

[5] See for instance Keller and Longino (1996).

contextual values may influence the very content of scientific results via the adoption, in the process of empirical justification of hypothesis, of background assumptions. These background assumptions, when they are shared by all members of a scientific community, are invisible and thus avoid the process of mutual criticism at the core a social view of objectivity. Hence the possibility of "good" but biased science. I do not need here to go into more details, for I just want to emphasize one of the main consequences of Longino's analyses in social epistemology, as regards the issue of how a scientific community should be organized in order to reduce the permeability of its results to contextual values. In a nutshell, the (general) idea is that a multiplication of different perspectives on a problem within a scientific community promotes the suppression of biases linked to individual preferences, to the extent that the heterogeneity of viewpoints promotes the identification and the intersubjective critic of the background assumptions involved in the process of empirical justification.

In light of this (rather unproblematic) contribution from social epistemology, our question "Is a self-governing scientific community more likely to function according to methodological canons that maximize neutrality and impartiality?" can be reformulated as follows : does autonomy favor heterogeneity of perspectives within a scientific community? This is admittedly a complex and delicate question. Let me just give here some hints for a negative answer. Scientific communities are not really spontaneously at the cutting edge as regards the social diversity of their composition… So counting on the internal social dynamic of scientific communities to maximize the heterogeneity of perspectives might seem a bit optimistic and even naïve. Therefore, an autonomous scientific community (i.e. not subject to an external control of its composition) might not ensure a very high degree of heterogeneity of perspectives, and thus might not maximize neutrality and impartiality. Some form of external control of the composition of scientific communities, as long as it encourages the

heterogeneity of perspectives, might do better on that terrain. In other words, a limitation of the autonomy of science, in the form of an external control of the diversity of the composition of a scientific community might be necessary to maximize the neutrality and the impartiality of the scientific results produced by these communities, and hence their epistemic authority[6].

Let me just take stoke here. As regards the link between autonomy and epistemic fecundity: the analysis of the validity of the epistemological arguments underlying a defence of freedom of research (in the sense of freedom of choices of research priorities) has established that an externalisation of the setting of research agenda is epistemologically acceptable as long as it fulfils the condition of diversity of research. Analysis of the accountability of science in terms of capacity to respond to societal needs has then established that such an externalization is not only epistemologically acceptable, but also desirable, because of the evolution of the nature of these needs (specific, *targeted* needs are unlikely to be better fulfilled by an autonomous science). As regards now the link between autonomy and neutrality, insights from social epistemology invites to challenge the idea that a self-regulating, self-organized scientific community is better able to produce neutral results: some form of external control of its composition might on the contrary better ensure a heterogeneity of perspectives on a given problem, thus enhancing the neutrality of the results and expertise produced.

5. Evaluative framework. The two aforementioned conditions – condition of diversity of lines of research and condition of heterogeneity of perspectives – provide a framework to evaluate the epistemological acceptability of existing or possible forms of limitation of the autonomy of science. Consider first a form of external control of research priorities already in place and often decried by scientists, to wit, definition of research priorities in light of short-

[6] I borrow from Leuschner (2011) the example of the IPCC as a scientific community whose pluralistically organization is regulated by a political instance (in that case NATO).

term economic interests. In light of the previous analysis, is this form of limitation of the autonomy of science acceptable? The answer is straightforward: such a limitation does not fulfill the first condition of diversity of lines of research and therefore leads to an epistemic impoverishment of science. But note that the problem is not that the objectives assigned to science are defined externally; the problem is rather that these objectives correspond to a very limited subset of the vast collection of objectives assignable to science. In other words, this form of limitation of scientific autonomy should not be rejected on the ground that science should remain "free and disinterested"; it should rather be rejected on the ground that when it comes to the definition of research priorities, considerations of short-term economic profitability should be integrated into a larger collection of considerations, reflecting the diversity of interests, both practical and epistemic, of the *whole* society.

A possible way to realize this integration would be to involve citizens in the choices made on research priorities. This public participation option is indeed widely discussed today and has started to be implemented in scientific institutions, albeit in ways that remain largely anecdotal and purely advisory. In philosophy of science, the ideal of well-ordered science developed by Kitcher (2001, 2011) has become a reference on this matter. In a nutshell, well-ordered science aims at promoting a collective good defined in a non objectivist way, by a process of deliberation involving tutored citizens. This form of direct public participation does indeed offer an alternative to a choice of research priorities in the interests of special groups (such as economic ones): in so far as deliberators are supposed to make evolve their preferences both in light of scientific expertise and in light of others' preferences (hence the notion of "tutored" preferences), the outcomes of the deliberations are supposed to provide an adequate representation of the interests of the *whole* society. For all that, this representativeness does not guaranty that the option of public participation fulfills the two conditions of epistemological acceptability (condition of diversity of lines of research and

condition of heterogeneity of perspectives). As proponents of the autonomy of science would fear, citizens may have rather selective expectations toward science, with a bias toward practical expectations (better cellphones and cures of cancer).

But is this fear grounded? Answering this question would require empirical studies of actual processes of deliberation leading to "tutored" preferences in Kitcher's sense. There exists a relatively large body of literature on consensus conferences and other forms of direct public participation, but these conferences often focus on a particular issue (for instance the societal acceptability of nanotechnology) and not (at least to my knowledge) on the much more general issue of what the research priorities should be at a national or supranational level. In any case, I take the crucial question here to be of a comparative nature: in light of the two conditions of epistemological acceptability, is public participation a form of limitation of the autonomy of science *preferable* to other forms, for instance to some form of control exercised by our elected bodies? My claim is that epistemological considerations do not favor the public participation option over other forms of democratic control. The conditions of diversity of lines of research and heterogeneity of perspectives can also be fulfilled by appropriate forms of control exercised by our elected representatives. One can very well conceived that some appropriate subset of our elected representatives get also "tutored" in Kitcher's sense by scientific experts. Of course, proponents of the public participation option may immediately discard this option on the ground that this would be a far too optimistic view of the capacity of our elected representatives to come up with a large range of scientific priorities, both epistemic and practical, and not just with short-term, economically profitable priorities, under the influence of various powerful lobbyings[7]. But I think this prevention (which may be to a certain extent country-dependant) can be questioned and, in any case,

[7] This seems to be Kitcher's view, for it is striking that governements and elected bodies are completely absent from his picture of a democratized science, or when they are invoked, they are immediately discarded in not very kind terms (2011, 24).

arguments are needed to establish that the public participation option is significantly less prone to biases towards short-term, practical expectations.

6. Concluding remarks. A first conclusion of this paper was that some forms of limitation of the autonomy of science are not only epistemologically acceptable but also desirable. Forms of externalization of the setting of research agenda are epistemically acceptable in so far as they fulfill the condition of diversity of lines of research. And appropriate forms of external control of the diversity of the composition of a scientific community may allow to increase the degree of heterogeneity of perspectives on a given problem, thereby increasing the neutrality of the results produced (see the IPCC example in footnote 7). The next step was then to investigate whether some forms of limitation of the autonomy of science score better than others on these epistemological counts. My claim is that there is no good (epistemological) reason to choose public participation over other forms of democratic control, in particular via our elected representatives. I am very aware that this step has remained very sketchy: much more need to be said to evaluate the comparative merit of the various options of democratic control as regards their capacity to fulfill my two epistemological conditions. In any case, epistemological criteria need to be supplemented by other criteria, such as political representativity, in the sense of "acting for" a larger group (Brown 2004, 86), and integrability within our existing, *representative* systems of democracy. And one can question whether the option of public participation scores well on those two counts. After all, we live in representative democracies where government and elected assemblies are those who are, *in fine*, responsible for the way public money is spent on research. How public participation *à la* Kitcher would articulate with them?

Keeping this issue open, I will just conclude on a general note concerning the type of contributions philosophy of science can bring to the topic of the democratization of science.

In the same way as philosophical reflections on science benefit from taking into account how science actually works, a political philosophy of science should take into account how our democratic systems of decision actually work, as well as the specificities of existing practices in science policies. And given their variety from one country to another, not to the mention supranational levels, this *naturalist* turn will invite a certain degree of localism: rather than trying to come up with a *general* normative proposition on how to democratize science, political philosophy of science should try to elaborate "local" propositions, that is, propositions that take into account the specificities of the relevant institutional and political context and more broadly, the specificities of the relevant "political culture"[8].

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