Policy Paper ESF/ScienceConnect, EuroScience, ISE on missions in FP9

1. Executive Summary

"Missions" will be introduced as a central new tool in the architecture of FP9, though many think they already exist in prior Framework Programmes. They can provide a new perspective but it is quite an exaggeration to say that "most technologies have emerged from missions": curiosity-driven research led to many. Several issues need more elaboration. What share of FP9 should missions get? How will they relate to existing instruments? Since the professed aim is that they should be realistic, what does it mean? Do we have the management tools to pilot? How to go about 10-year planning periods? Can goals really direct research? How can public engagement be organized effectively? Would it not be wiser to experiment with missions, by starting a limited number first, and in a phased approach to learn from them after a few years?

2. Introduction

a) Contents of this policy paper

Clearly, missions will be a major new focus in FP9. The past three years have seen a flurry of activities to elaborate what that concept should mean as an overarching concept in FP9[1]. EC representatives have mentioned in several workshops that 10 missions have been more or less identified to which the EU would contribute 1 billion € each for a period of ten years. Big missions (some call them Moonshots) have a definite appeal but some deep policy discussion is needed as to how and with which scope they are going to be introduced in FP9. This contribution of EuroScience, ESF/Science Connect and ISE first summarises the concept of mission as it is elaborated in the Mazzucato and ESIR reports. In the next section we propose key elements for the policy discussion that we think is warranted. In the final section we offer a few recommendations on how to introduce this new type of missions (we will call them 'big missions') in FP9.

b) Are most technologies spillovers from past missions?

We want to begin, however, by stating that the underlying belief that such missions are the best way for science and innovation to serve society is not supported by a careful reading of the history of science and technology. Mariana Mazzucato formulates the belief explicitly when she states that "most of technologies in smart products emerged as spillovers from missions from the past". Even if one would accept the limitation to technologies in smart products (however one would define smart products) this is not the case. Many technologies can be traced back to curiosity-driven scientific research of the past which was not driven with specific applications, let alone missions in mind. The example of GPS based on lasers and special relativity is an evident one. Moreover, the fact that the mission proposals extend to health, environment, quantum computing or even all of the SDGs that such a limitation to 'smart product' technologies is not what guides the thinking in missions. For the role of curiosity-driven research one should read Abraham Flexner's "The usefulness of useless

[1] In December 2017 the Expert Group on Economic and Societal Impact of Research (ESIR) published a report "Towards a Mission-oriented Research and Innovation Policy in the EU." A key member of that group, prof. Mariana Mazzucato, had been asked by Commissioner Moedas to write a separate report (Mission-oriented Research & Innovation in the European Union, Mariana Mazzucato, EU, February 2018)

knowledge" from 1939; or the companion essay by Robbert Dijkgraaf, Flexner's successor as director of the IAS in Princeton, in the 2017 republication of Flexner's essay. Mazzucato mentions that innovative spillovers may not be known beforehand and can have unforeseen applications, but the point is that they were not spillovers from missions. It is putting the history of science and technology in an economist's straightjacket. The Internet which Mazzucato cites was not developed as a mission but originated (with hindsight's view) when DARPA wanted to connect its scientists' computers.

3. Summary of key characteristics of missions, as defined by Mazzucato ('big missions')

- Missions reflect the desire to introduce direction in the framework of policies for Science, Technology and Innovation.
- Defining scientific research as a horizontal domain, one could say that the difference between a mission-oriented programme and an ordinary Horizon 2020 programme is that a mission-oriented programme has this mission as its vertical part, whereas the ordinary Horizon 2020 programme has technologies as its vertical component.
- Moreover, one should rather speak of mission-oriented policies rather than mission-oriented programmes, because to achieve a mission one needs much more than R&D programmes (or policies): regulatory policies may be important or general fiscal policies. The need to involve a great variety of stakeholders might bring in local community policies.
- Missions are diverse: one key category aims at accelerating technology development, another drives systemic change.
- A mission:
  - is bold, inspirational, and of wide social relevance;
  - has a clear direction, has measurable targets, and is time-bound;
  - is ambitious but realistic;
  - requires cross-disciplinary, cross-sector, cross-actor innovation;
  - requires multiple, bottom-up solutions.
- Implementation of a mission requires:
  - engaging may regional and local stakeholders as well as businesses, hence requires reinvigorating capacity building in the public sector;
  - setting goals and milestones, and measuring continuously achievements;
  - managing portfolios of bottom-up projects and funding instruments;
  - building in flexibility and pro-active management, and using strengthened in-house expertise.

4. Policy issues around missions requiring deeper discussion

There are many valuable elements in the set of characteristics of missions quoted in the previous section. But it is obvious that they encompass a very broad variety of situations and it is unlikely that a single concept of missions can be applied to all of them. Moreover, serious issues, partly reflecting the thinking of past decades about science, technology and innovation policies, need further consideration.

a) Does Horizon 2020 not have missions?

Most people would argue that in line with the use of the word 'mission-oriented' research as it was prevalent in discussions about distinctions between basic, applied, application-oriented, curiosity-driven, mission-oriented and other types of research in the seventies and eighties, Horizon 2020 has several (sub)pillars that can be called mission-oriented. The Societal Grand Challenges, the Flagships, the EIT KICs are obvious examples. Surely, criticisms have been offered. As an example, EuroScience and ISE have identified in their Manchester statement of 2016 to Commissioner Moedas, as points for

serious concern, the short-term and prescriptive nature of the Calls for Societal Grand Challenges, requiring mostly technological solutions which moreover are sometimes even constrained to specific technology readiness levels. Trying to provide ontologies of different types of missions, and ordering them in a grand scheme which also comprises Societal Grand Challenges and Flagships as members, which was a subject of heated debates in the discussions on missions, is probably aiming for the impossible, and at odds with the historically multiple ways science and technology have benefited society.

b) 'Big missions' should not drive out everything else

Often one gets the impression that all of the existing programmes under Horizon 2020 should now be replaced by or reformulated as missions. Mazzucato's report states, for example, that missions "can provide policymakers for the first time a privileged view over the different elements of … [Horizon 2020]". That is not a modest claim. But of course one cannot object to identifying, for instance, which ERC-projects are related to a mission-oriented programme so that conversations can start about how eventual results of such projects can contribute to achieve the mission goals. But one has to recognize, as the Mazzucato report and certainly the ESIR memorandum seem to do, that various progammes serve different purposes, so missions should not lead to specific ERC calls, neither should ERC projects related to a mission be subordinated to portfolio managers of a specific mission.

In general, when discussing how large a share 'big missions' should receive in FP9 one should bear in mind the note of caution US Nobel prize winner William Phillips, now in charge of a major US quantum computing initiative, expressed at an EP-STOA organized workshop in January 2018. In response to suggestions that the EC should define a mission to have not only an operating quantum computer in 2025, but one that is specifically designed to solve the climate change science questions, he said that this is not the approach that has brought success to US science and innovation. Diversity is what counts, in funding mechanisms, goals or the absence of precise goals, and so on.

c) What will be the relation to existing instruments?

For years there has been much discussion about the complexity (organizationally, financially, managerially, for example) of the existing instruments and their overlap. JTIs, Flagships, the KICs of the EIT and others are not always clearly demarcated. What will happen when missions will be added? In public discussions the EIT already goes on record saying that what they do is the same as missions. And how does the FET Flagship on Quantum Technologies relate to the example quoted in section 3b of the operating quantum computer in 2025? The discussion now is too much about drawing up, in a somewhat arbitrary way, general distinctions between missions and all of these other instruments. In practice, it seems only feasible to elaborate a number of specific examples and on that basis try to clarify any distinctions that might exist.

d) "Ambitious but realistic"?

Ambitious but realistic, is what 'big missions' should be. Worthwhile; but how realistic is it when:

- Missions seem to inherently require effective coordination between the R&D efforts at the level of the EU and those at national or regional levels. This has proven to be a goal that has eluded the EU and its member states so far.
- One of the examples the Mazzucato report gives goes even one step further: clearing the oceans from plastic. That would require not only globally coordinated R&D efforts, but more importantly globally coordinated policy actions. Focusing on what the EU can contribute to this important goal might be a more realistic starting point and a basis for engaging in wider international debates.

- The Mazzucato report rightly stresses that implementing 'big missions' require new management capabilities at the level of the EC as well as the member states. Those required portfolio management capabilities are absent and cannot be built up overnight. The DARPA agency of the US Department of Defense is cited as an example for this portfolio management. But it is obvious that the EC is very different from a single US government agency. Would management have to be done by sectoral DGs, or by DG Research and Innovation, or by a new Executive Agency? But if so, how would it collaborate with existing EU Directorates and Agencies? When elaborating first missions this has to be thought through in practical detail.

e) Implications of missions spanning more than one Framework Programme?

The long timeframe of 'big missions' extends beyond a single Framework Programme. That is not in itself an objection. Instruments need to be adapted to the requirements of research, not the other way around. But one has to consider in advance how to do this, and what the implications are. Political decision making, budgetary rules or bureaucratic continuity or rather discontinuity all provide hurdles which have to be cleared.

f) To what extent can goals direct research?

One should reflect much more deeply about the extent to which scientific research can be directed by defining *a priori* goals. It is highly questionable whether major contributions from scientific research to the development, often much later, of technologies and innovations would have occurred if one had restricted scientific research to cases where a goal could be defined in advance. That includes setting an overarching goal for a whole broad, interdisciplinary field of research. The goal to eradicate cancer by 2035 (or 2050) cannot probably be meaningfully divided in subgoals if one is thinking of basic biological understanding; however subgoals (making significant progress in curing this or that cancer) might be realistically achievable in a given timeframe in the way of the portfolio management mentioned in section 2. Most importantly, a mission should have very broad goals, but not define the path to get there to invite innovative approaches one cannot and must not define at the start.

g) Public engagement

Public engagement is important, but one cannot speak about it in general terms. The Mazzucato reports mentions involvement in the selection and implementation stage of research. Right as this is as direction one has to be more specific.

The discussion on citizen science tells us that one has to be specific. Science and scientists not only have to be accountable and prepared to engage in discussions about consequences, risks or impacts. And there are good examples where it is not only possible but crucially important to involve groups of citizens in elaborating research questions: patient groups in medical research or citizens – in the literal sense – and local businesses when it comes to urban development. 'Professional amateurs' in astronomy or botany or zoology stand in a long tradition, which gets only new dimensions with the vast potential of ICT. But one cannot translate these examples in prescriptions for each and every field of research. The German Energy Transition (Energiewende) is probably a good example; green policies in Germany have since long a high level of public support on which ambitious and abrupt policy changes could build. And the many choices of how to implement renewable energies with significant geographical, physical, social and cultural impacts on a country do warrant wide-scale and active engagement of citizens. But for the quantum computer engaging the public would surely look very different. Implementing general concepts such as missions requires taking into account this wide variety. That was one of the lessons learned over the past years when thinking about how to implement the important concept of Responsible Research and Innovation.

5. How to proceed with introducing 'big missions' in FP9

Learning by doing is the key recommendation we proffer. The first 'big missions' should be treated basically as experiments. That is: try to define a small number of limited missions and address explicitly for each of these proposed missions all of the issues listed, such as

- can one define realistic goals which will not restrict the potential of science to contribute;
- how are they related to existing instruments;
- how will the management be done;
- can the EU alone achieve them;
- how will the cooperation between an EU-defined mission and necessary involvement and contributions from member states look like;
- how to go about getting commitment beyond one Framework Programme;

and think about how putting together the scientists and companies and other stakeholders who set out to achieve these 'big missions' (is a Call the appropriate mechanism, for example?). Moreover, it seems imperative to phase in such missions gradually, similarly to what has been done with KICs of the EIT, and learn lessons from ongoing missions, without waiting ten years for them to be completed, so that new missions can benefit from how the earlier ones developed.

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