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Supporting Frontier research: which institutions and which processes. Some initial considerations.

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Section 1- Introduction

The objective of this chapter is to discuss the relevance of the European Research Council as an engine for promoting ‘frontier research’ in Europe and bridging the perceived gap highlighted by most policy documents of the early 2000s.

There has been a number of analyses of the rationales and processes that explain the creation of the ERC. Many analysts see its roots deep in the construction of the European Community, and more specifically at the creation of the European Commission and its perspective about European research with its 4 dimensions (Guzzetti, 1995, André 2006, Laredo 2009). Nedeva (2010) proposes an elegant answer to the unfolding of the ERC with her notion of science built as a relationship between “research fields” and “research spaces”¹. She sees the ERC as an answer to the tension “between the inherently global nature of the research fields and the localised, mostly national, research spaces”. She suggests that such a social process can only materialise if three conditions are fulfilled: the existence of a change champion (here the elite of life sciences, see the 2003 Paris meeting organised by ELSF and EMBO), some level of institutionalisation and organisation building (here the Commission which strikingly changed its views on the issues within one year, see Dublin conference 2004), and the progressive emergence of conditions (commensurability of funding rules, organisational set-up for research) that render the enlargement audible by national spaces (here the dominance of the agency model of funding with in particular the creation of the French ANR, and the central role given to universities as research performers in most countries at the turn of the 21st century).

I fully share this approach. There is however one aspect that is not explained with this analysis that is the institutional focus given to the ERC: it is not only dedicated to funding academic or fundamental or basic research, the classical OECD categories; it is focused on ‘frontier research’ as is well outlined by the few extracts taken from the 2008 work programme (Box1). At the same time these extracts show that the concept is not that clearly established: is the research ‘frontier’, or is it ‘frontier’ because it is located at the ‘frontiers of knowledge’ (which could correspond to the fields that the ISI web of knowledge qualifies as ‘research fronts’), or is it qualified as such because it is ‘unconventional’ (others say heterodox) and/or of a ‘groundbreaking nature’?

Box 1- Extracts of the 2008 work programme of ERC

- The fundamental principle for all ERC activities is that of stimulating investigator-initiated

¹ “Research fields” are empirically outlined by three inter-connected elements, namely converging *knowledge communities*, consistent *bodies of knowledge* and *research organisations*. “Research spaces”, on the other hand, are defined by the ‘essential’ relationships of the research organisations and by notions of utility of knowledge. The emphasis is on the relationships and the exchange(s) in which the organisational actors are involved rather than on the attributes of the organisations.

frontier research across all fields of research, on the basis of excellence.

- Support excellent, innovative investigator-initiated research projects
- ERC Advanced Grants provide an opportunity to established scientists and scholars to pursue frontier research of their choice.
- Advanced Grants are intended to promote substantial advances in the frontiers of knowledge, and to encourage new productive lines of enquiry and new methods and techniques, including unconventional approaches and investigations at the interface between established disciplines.
- (projects should) demonstrate the ground-breaking nature of the research.

The focus of this presentation is not to inquire how such a focus was arrived at. It is to take it for granted and discuss the coherence of this objective with the organisational arrangements arrived at.

Section 2 will focus on frontier research as a politically-driven concept looking on both sides of the Atlantic. Section 3 will link these politically driven developments to existing literature not in a view to delineate this concept further but with the objective to grasp its institutional and organisational contents. This will help to identify key organisational conditions for the implementation of a policy objective that would be to increase the amount of frontier research undertaken in Europe. Finally personal views will be elaborated about the future of the ERC.

Section 2- Frontier research as a politically-driven concept

In recent work done on rationales for research and innovation policy-making, L. Bach (2007) highlighted the existence and interplay of two sources: production policy rationales and governance policy rationales, the former being associated to scholarly conceptual developments and the latter deriving from practice and causal beliefs built within the course of political action. This symmetrical approach to constructs that are mobilised by policymakers is specifically useful here to address the notion of ‘frontier research’. We all know the 1945 report by V. Bush, Science the endless frontier. However the term ‘frontier research’ does not resonate much in academic work. A quick overview shows that its main use is linked to agenda setting for discussing the challenges faced by disciplines or derived from new issues (e.g. Baltes et al (2003) on the future of ageing or Berkowitz et al (2003) on urban ecosystems). The other central use has also been operational, with scientometrics and ISI identification of ‘research fronts’. This explains why I focus first on ‘governance policy rationales’. A number of initiatives have been recently developed under this conceptual umbrella: the European Union ERC and the US Department of Energy initiative on “Energy frontier research centres”. Other agencies in the US, following the National Science Board (2007), have devised a related concept of ‘transformative research’ and have embedded it in their activities.

There has been quite a number of official texts about the ERC (for a review, see Nedeva 2010), however the only one to attempt a detailed definition of ‘frontier research’ is the report of a high level group set up by the European Commission (2005) some members of which are central figures in the field of science policy studies (e.g. Ben Martin, Stefan Kuhlmann, Andrea Bonaccorsi or Paula Stephan). It highlighted four central characteristics for this new terminology: be at the forefront of new knowledge, be risky and uncertain, potentially merging the classical dimensions of applied and basic research, and pursuing questions irrespective of established disciplinary borders (see Box 2 for an enlightening paragraph).

Box 2 - Defining Frontier Research : the HLEG 2005 report (p. 18)

“Classical distinctions between basic and applied research have lost much of their relevance at a time when many emerging areas of science and technology (e.g. biotechnology, ICT, materials and nanotechnology, and cognitive sciences) often embrace substantial elements of both. We therefore prefer to use the term frontier research to basic research to reflect the following characteristics:

- 1) Frontier research stands at the forefront of creating new knowledge and developing new understanding. Those involved are responsible for fundamental discoveries and advances in theoretical and empirical understanding, and even achieving the occasional revolutionary breakthrough that completely changes our knowledge of the world.
- 2) Frontier research is an intrinsically risky endeavour. In the new and most exciting research areas, the approach or trajectory that may prove most fruitful for developing the field is often not clear. Researchers must be bold and take risks...
- 3) The traditional distinction between ‘basic’ and ‘applied’ research implies that research can be either one or the other but not both. With frontier research, researchers may well be concerned with both new knowledge about the world and with generating potentially useful knowledge at the same time (as with the concept of Pasteur’s Quadrant developed by D. Stokes)...
- 4) Frontier research pursues questions irrespective of established disciplinary boundaries. It may well involve multi-, inter- or trans-disciplinary research that brings together researchers from different disciplinary backgrounds, with different theoretical and conceptual approaches, techniques, methodologies and instrumentation...

The US National Science Board in its report on transformative science (2007) proposes a quite similar definition but locates it within an overall view of the dynamics of science. It suggests to differentiate between evolutionary and transformative science (see box 3). The report explained why NSF is poor at doing it (see later) and proposed the development of a new initiative. The same analysis was made by the NIH (NIH 2004 Roadmap). But both agencies have selected different approaches to address it: the NIH have developed a specific programme with its pioneer awards ‘to support individual scientists of exceptional creativity’ while NSF has chosen to add one selection criterion in all its panels (“To what extent does the proposed activity suggest and explore creative, original, or potentially transformative concepts?”).

Box 3 – locating and defining ‘transformative science’ in science dynamics

Source: National Science Board (2007)

“Science progresses in two fundamental and equally valuable ways. The vast majority of scientific understanding advances incrementally, with new projects building upon the results of previous studies or testing long-standing hypotheses and theories. This progress is evolutionary—it extends or shifts prevailing paradigms over time. The vast majority of research conducted in scientific laboratories around the world fuels this form of innovative scientific progress. Less frequently, scientific understanding advances dramatically, through the application of radically different approaches or interpretations that result in the creation of new paradigms or new scientific fields. This progress is revolutionary, for it transforms science by overthrowing entrenched paradigms and generating new ones. The research that comprises this latter form of scientific progress (is) termed *transformative research*... This pathway is marked by its challenges to prevailing scientific orthodoxies.

The DoE offers a very different answer, which is simultaneously procedural, cognitive and organisational. It is based on an initial strong assumption: ‘Incremental advances in current energy technologies will not address the energy challenges of the 21st century. History has demonstrated that radically new technologies arise from disruptive advances at the science frontiers’ (2008, p.2).

The de facto definition proposed first accounts for a process that started in 2001 with the work of an advisory committee (Basic Energy Sciences Advisory Committee, report in 2003) followed by ‘basic research needs workshops’ gathering 1500 participants over the next 3 years and producing each a specific report (12 in total). These in turn enabled to identify ‘scientific challenges which no longer were discussed in terms of traditional scientific disciplines’ and which ‘described a new era of science — an era in which materials functionalities would be designed to specifications and chemical transformations would be manipulated at will’ (ibidem, p. 3). This de facto definition thus entails a second dimension: it is not only procedural, it is also cognitive: frontier research is related to given challenges or problems, and it is associated to potentialities offered by sciences to address them. To discuss frontier research, one needs to enter into contents. The core of DOE 2008 text is about describing the five ‘science grand challenges’ identified.

The third component of ‘frontier research’ builds an organisational answer: ‘Energy Frontier Research Centers will bring together the skills and talents of multiple investigators to enable research of a scope and complexity that would not be possible with the standard individual investigator or small group award’ (ibidem, p. 4).

These politically-driven rationales propose a similar vision of the dynamics of science. By and large they share definitions of what is looked for. But they widely differ in the ways of implementing it. Europe has created a new funding agency, NSF proposes to include a new criterion in its panels, NIH has established a new initiative based upon individual scientists while DoE initiative is focused on centres. These different organisational answers raise questions about the reasons that underpin them. They also drive us to consider the theories and concepts that underlie them.

Section 3- Conceptual backgrounding to ‘frontier research’

How does this politically driven construction relate to established theories? Should it drive us to develop new conceptual frames as was the case at the beginning of the 1980s to face the construction by policymakers of a new type of policy instrument, collaborative or technological programmes (Callon et al., 1997, Larédo et al., 2010)? My tentative answer is no, considering that two established streams help us address the organisational issues raised.

One stream derives directly from the science dynamics proposed (especially by the NSB) which has strong connections with work done in innovations studies on breakthrough innovations or disruptive technologies, at the encounter of economics, management and sociology. It further resonates much with the very classical work about Kuhnian science dynamics. This stream focuses on processes through which transformation occurs, and one important dimension is about how new scientific or technological paradigms, new breakthrough products and services are institutionalised and how adoption, generalisation takes place.

Following Nedeve’s approach, such transformations deal with the reshaping of research fields, their boundaries and the communities that they entail. We are there associated to a long tradition of sociological studies dealing with the structuration of fields and the elegant theorisation by Diana Crane of invisible colleges and subsequent work on transepistemic communities and changing modes of production (with the famous ‘mode 2’ of Gibbons and colleagues, 1994). This offers one way of taking account of diversity by establishing peer-based selection processes within all-embracing institutional settings. New approaches to knowledge dynamics, and in particular work by Bonaccorsi (2005, 2008), question whether this is enough to take into account the diversity of ‘search regimes’ and the institutional

conditions that favour or constrain their growth. Said otherwise can there be ‘one size fit all’ institutional answers to different knowledge dynamics?

31- Frontier research as a process: Organisational implications

The classical reference is clearly linked with Kuhn’s approach of science dynamics. One can see the use of ‘frontier research’ as a call for more support to those research activities that question established paradigms which organise normal science. In technology, evolutionary economists have also highlighted the role of technological paradigms (Dosi, 1982) and have associated the long term dynamics of economies to shifting paradigms. Whether qualified as radical or breakthrough innovations, or disruptive technologies, there is an important body of work to analyse the journeys through which such transformations take place (Cheng and Van de Ven 1996). Studies have focused on the emergence of new designs or paradigms and on the ways they become dominant. Abernathy (2005), Tushman and Anderson (1986) and others propose a convergent approach to these transformation dynamics.

The core of innovative activities undertaken are cumulative and come to deepen and reinforce the ‘dominant design’ (here the dominant paradigm). This is the normal state of affairs or normal science. There are different views to explain the progressive exhaustion of this dominant design. The two main explanations put forward for innovation deal with the banalisation of the knowledge base and with the progressive exhaustion in the exploitation of market segmentation. The former drives to a competition via prices (‘produce the same thing cheaper’) and the second one focuses on deepening differentiation associated to stronger and stronger connections with different ‘lead users’. Whatever the reasons, this provides incentives for inventors and innovators to try and pursue alternative alleys. Proponents of dominant designs speak of breakthrough innovations not as one off events (Collarelli O’Connor and Rice, 2001), but as a progressive unfolding of new designs with often a long fluid phase whereby options to turn the new approach into innovations, multiply and compete. This raises strong debates about the ‘narrowing process’ that will drive to the emergence of a dominant design and the conditions which those ‘market shaping’ activities have to address (Courtney et al., 1997).

Work done highlight three complementary and intertwined dimensions: technological, utility (as perceived by users), and institutional, the latter dealing with rules (the North way), regulations and infrastructures that support them (such as patent offices for IP or drug authorisation agencies for the pharma industry). In turn this has shed light on processes that enable such processes to take place: we for instance have developed instruments for managing the ‘societal robustness of breakthrough innovations’ (Larédo, Rip et al., 2002, final report of the EU Socrobust project) which emphasize organisational issues, both in term of ‘implementation structures’ (Rip et al, 1986) and of operational aspects (the portfolio of instruments mobilised). A later study on the emergence of a new approach to chip design, asynchronous logics, on the International Technology Roadmap for semiconductors (ITRS, Delemarle and Larédo, 2008) showed that not only the portfolio of instruments was important but also the sequence and conditions of their deployment.

From this parallel, I derive a first line of interrogation about developing frontier research in Europe: Organisational dimensions are critical to the materialisation of the objective followed. Furthermore we should not only consider the overall ‘implementation structures’ established, but also the portfolio of instruments proposed and the conditions of their deployment.

32- Knowledge dynamics, search regimes and the need for specificity

There is a lasting tension about work done on scientific production. On one side, there has been a constant search for a generic approach to structure government intervention. Merton's republic of science has witnessed two main institutional materialisations in the 1950s, the US vs the Soviet or the British vs the French models, putting universities and principal investigator project based funding at the core on one side, or making of dedicated research organisations and research collectives the central mechanisms on the other. Germany was then a clear outlier having developed a balance between both. Of course this was only the dominant feature and both co-existed in the different countries. And we all know about the strong blurring of these differences during the last 20 years, but the constant search for a generic approach to the research system remains.

On the other, empirical work has emphasised the importance of differences between fields, between big and small science, between experimental and theoretical, between laboratory based vs observational, between curiosity vs problem solving driven among others. The most elegant theorisation of this variety for me still lies in the work by Diana Crane on invisible colleges. Colleges however are not so invisible, they only exist through all the tangible and intangible infrastructures required to maintain them. Together they provide a powerful definition of what an established 'field' or 'discipline' is. Some play at the level of the discipline itself (in particular journals, conferences, prizes, professional associations) while others are embedded into national and local organisational settings (in particular teaching curricula, departments and/or research groups). At Government level, this approach enabled to operationalise the Mertonian republic of science, embedding diversity by transforming invisible colleges into institutional constructs based on peer-reviewing (in agencies or research organisations). In a way research fields were collapsed into research spaces, a few nations being central in this process. And we would now face the limitations associated to this assimilation, especially in smaller or mid-size states as are European countries.

Three aspects of later developments are of importance for our discussion: the universe of actors that populate these colleges, the connections between 'disciplines' and the internal vs external sources for agenda setting. Readers will recognise work done by K. Knorr (1982) on transepistemic arenas of research, all the issues associated with inter-, multi-, pluri- or trans-disciplinarity, and the ever-growing discussion on problem solving research and the third mission of universities.

They join in building a new 'storyline' on the production of knowledge. The idea is that societal pressures (and in particular from firms faced with difficult problems, such as offshore exploitation forty years ago) propose new challenges to science, drive to new interactions between discipline-based knowledge and, in a few cases, drive to paradigmatic shifts and the emergence of new communities. We face a beautiful example with homogeneous catalysis and the 2008 Nobel prize given to one researcher employed by a mission oriented institution (Chauvin from IFP) that has been at the birth of a completely new speciality within chemistry.

From these developments, we can deduce that conditions under which new 'frontier' knowledge is developed differ widely between fields. A conceptualisation like this of Stokes (1997) that is often mobilised when discussing 'frontier research' (with his Bohr's and Pasteur's quadrants) is at best a categorisation of existing situations. Bonaccorsi (2005, 2009) has proposed a new approach to these differences with his 3 dimensions of 'search regimes': rate of growth, complementarities (cognitive, technical and institutional) and degree of diversity. Using this approach, I have shown (Laredo 2006, 2009) at the macro level how different production conditions have been for the successive leading sciences of the time, and how it interacted with institutional conditions (see also Bonaccorsi, 2008). In a recent paper (Larédo et al., 2010), we underline that the trend to replicate policy mixes and instruments that worked well in preceding waves often prevailed before new mixes were developed that

better fit with the on-going dynamics (see the striking examples of the French Plan Calcul for information technology of Nixon's war against cancer).

These elements tend to highlight the clear interaction between the dynamics of fields and this of research spaces to follow Nedeva's terminology. Institutional 'one size fits all' solutions might have very different effects depending upon the dynamics of different fields (see Jansen on unintended effects, this book). Thus we should take into consideration the research fields – research spaces coupling not only in a spatial dimension (moving from the national to the European level) but also in its cognitive dimension: what different mechanisms are needed within a research space to cater for the variety of research dynamics. This may well explain why in the US 'research space', answers proposed by the NIH widely differ from those developed by the DoE.

Section 4- Reflecting upon organisational issues for European developments

Focusing on the European situation I derive from the above developments that it is not enough to decide to create a global 'implementation structure' (the European Research Council). Operational aspects are critical and need to deal not only with the portfolio of instruments mobilised and their conditions of deployment, but also with their ability to cater for different dynamics: what might be relevant for some biotechnology developments may not be adapted for say nanotechnology based new materials.

In order to discuss these points one has to enter into more organisational details of the ERC. The central mode of operation selected is peer reviewing. The ERC has created 25 panels to cover the whole range of science domains (more than 340 areas or specialities singled out). It only works out through calls and has devised two instruments addressing single principal investigators: the starting grant scheme and the advanced grant scheme.

This situation requires that we address two complementary issues: does such an organisational setting favour or hinder the selection of 'frontier science'? and second is it fit to take into account different knowledge dynamics?

41- Peer reviewing and 'frontier research'

Even the 'greatest' supporters of peer review in their 'systematic review' (Wood and Wessely, 2003) concluded that they "are unable to substantiate or refute the charge that peer review suppresses innovation in science" (p14). They account for the work of Horrobin (1990, 1996) and relay the strong interrogations by R. Kostoff, a well known specialist of evaluation procedures. Their citation of the Nobel prize winner Yalow (1982) is typical of this: "the need to promote scientific revolutions and the outcome of peer review are in opposition" (p. 26). Should we then consider, with Horrobin and others, that peer review is malformed for funding frontier research? The conclusions by NSB go in that direction (see box 4). Still one has to recognise that even long ostracised Nobel Prize winners such as Prusiner, were funded by the US funding agencies.

Box 4- NSB analysis about why NSF has difficulty to fund 'transformative research'

Source : NSB, 2007

- In practice, distinguishing between innovative and transformative research is difficult at best and, some would argue, only possible in hindsight. Indeed, the two forms of scientific progress do exist side-by-side and, often, proceed hand-in-hand and overlap each other.
- Transformative research frequently does not fit comfortably within the scope of project-focused, innovative, step-by-step research ... nor does it tend to fare well wherever a review system is dominated by experts highly invested in current paradigms or during times of especially limited budgets that promote aversion to risk.

- The Board finds that investigators are reluctant to submit radical or paradigm-challenging research ideas to NSF given the low conventional success rate. ...
- Experts in the areas being challenged (many of whom may sit on review panels) may dismiss such ideas by pronouncing the research overreaching or without basis.

Looking at interdisciplinary grant committees, Lamont et al (2006) propose a more nuanced answer. Analyzing how committees build their criteria and rules, they show that “procedural fairness” is warranted on “respecting disciplinary sovereignty”, that this drives to recognising and accepting different “epistemological styles”, but that this also allows reviewers to have “their tastes and idiosyncrasies ... play a greater role”: as mentioned by one of their interviewees “excellence is in some ways what looks most like you”. I infer from these results two central conclusions.

First I consider, following others (Knorr Cetina or Schimank to mention a few) that epistemic communities largely frame the behaviour of reviewers, who will tend to respect and thus reinforce disciplinary standards. This applies not only for criteria for “robustness” (what makes good proposals) but also for topics addressed. By this I mean that most researchers share the research agenda of their discipline or speciality - what they recognise as the important questions to address. Committees are thus faced with two types of issues: one dealing with ‘empirical rigor’ (is there one or more epistemological styles considered?²), and one dealing with ‘positioning’ (is this part of the research agenda of the discipline/speciality?). This enables to rephrase the issue of frontier research as those cases that do not follow dominant disciplinary styles and/or position themselves outside the ‘mainstream’ research agenda.

Second, I take it for granted that committees are quite good at curtailing the long tail of ‘bad proposals’ (van der Besselaar & Leydesdorff, 2008). Thus we should only consider the other cases. There I distinguish between normal science or what the NSB calls evolutionary science (projects well located in the mainstream agenda with robust accepted methodologies) and other projects. My second assumption (based on a long-standing practice, and multiple anecdotal evidence, again a source for more systematic research), is that committees are quite good at identifying ‘evolutionary’ projects and at recommending them for funding. Committees only then appraise these other ‘interesting’ but ‘unorthodox’ projects, making of ‘frontier research’ a left-over of having addressed the pressure for satisfying the mainstream agenda.

Why should then the ERC, having a similar approach to selection processes, differ in its outcome? We can then anticipate, especially when taking into account the level of pressure observed (well under 20%), that a large portion of the work supported will be ‘evolutionary’ rather than ‘frontier’. Thus we can assume that only one fraction of the 5 years anticipated 7 billion euros will nurture ‘frontier research’ – redefined as research that does not follow dominant disciplinary styles and/or is positioned outside the ‘mainstream’ research agenda.

42- Can the ERC help coping with the perceived difference between Europe and the US?

It thus drives to a complementary issue: can it help coping with the perceived gap between the US and Europe in ‘frontier research’. We shall see that discussing this issue drives to focus on our second question: can it accommodate different knowledge dynamics?

² Here I adopt the categorisation proposed by [Mallard et al. \(2008\)](#) with its four types: constructivist, comprehensive, positivist and utilitarian.

We had in Europe at the end of the 1990s a lively discussion on the European paradox (Caracostas and Muldur 1997): Europe is good in Science and poor at transforming it into innovation. As soon as the paradox was issued, there were voices to demonstrate that this was untrue. Focused on science-based industries, and looking at science shaping new directions and new paradigms (manifested by highly cited researchers or Nobel prizes), Europe was no longer leading, it was mostly strong in cumulative areas and weak in new fast growing fields (Dosi et al, 2006, and Bonaccorsi 2007 for the versions published). How then explain that spending as great an investment than the US, drove to such a ‘poor’ record? Was it because there was a constant brain drain? Working with NSF (see Laredo, 2004) a check was made on 30 years of US Nobel prizes to demonstrate that almost all of them had done their prize winning research in the US and that this could not explain a one to four ratio in Nobel prize winners. The EC translation was that research performers were too fragmented, thus that it was important to introduce a process of amalgamation. This provided a background rationale for the development of networks of excellence and of the European Institute of Technology. Again voices were raised to challenge this opinion, showing that the problem did not lie in research groups being “sub-critical”, but was associated with institutions themselves, and in particular with institutions in charge of the allocation of resources³.

Could then the European Research Council be THE solution? In order to address this issue, I developed some time ago a conjecture taking into account the different institutional settings between both spaces and estimating the respective levels of ‘frontier research’ faced with a similar level of overall investment. Box 5 presents the overall reasoning and the conclusions arrived at. This conjecture helps to highlight when comparing the US and Europe, why, with similar investments, there should be 3 to 4 times more frontier research - and Nobel prizes - in the US than in Europe. This is not an issue of intelligence or attractiveness, it is an organisational issue which has been intuitively identified by European policymakers and coined under the term of ‘fragmentation’. Fragmentation is not here an issue of research performers, but an organisational problem in funding mechanisms to academic research overall, having strong consequences on the levels of funding dedicated to “frontier research”.

Box: An hypothesis on the reasons of the EU-US difference in ‘frontier research’

Source : Larédo, 2004.

Let us consider a specific area, for instance catalysis in chemistry (see PRIME ERA Dynamics works on Chemistry and Catalysis, Bonn Workshop 2007). Let us make the following starting hypothesis: overall efforts in this area are similar in Europe and the US.

In the US, the core of public funding is concentrated in a few Federal agencies, here NSF, DoE (department of Energy) and DoD (department of Defence). In other fields NSF will be replaced by NIH and the DoD may be by the newly created department of homeland security (DHS). These few agencies are used to coordinate and to share tasks (even if it is never easy), as is well illustrated by the “National Nanotechnology Initiative” (NNI). For the same field in Europe we shall have to account for at least 10 agencies with meaningful activities complemented by at least 4 to 5 “national programmes”, as is well illustrated by the two ERA-Nets in Chemistry and in Catalysis. This case is further simplified since there is no FP specific programme to add to the picture.

Analyses done have often shown that ‘research agendas’ clearly identify the research directions at short and medium term, and that long term issues identified are in continuity with the prevailing ‘dominant’ paradigm. And that, to anticipate on new breakthrough direction, it is better to leave “a thousand flowers bloom”, thus leaving room in the allocation of resources to unanticipated bottom-up initiatives.

Let us now suppose that for catalysis research, the 3 main US agencies spend 100, that 70 is focused on the “mainstream agenda” and that 30 support different options which we suppose to be “frontier research”.

³ For a full demonstration, see the [report](#) “Challenging Europe’s Research: rationales for the ERA” by an expert group chaired by Luke Georghiou (March 2008).

What will happen in Europe? There are large enough communities and strong exchanges, so that anticipations made are shared on both sides of the Atlantic. As each national agency has strengthened its management over time, they will ask for achieving critical mass, and even if the whole mainstream agenda is not covered, this will drive the agency to focus more means to this agenda. Globally the agencies in Europe will probably devote 85 of the 100 they invest on the mainstream agenda, to enable their national teams to remain in the world competition. In the end, Europe will over-invest on the mainstream agenda, which exploits present paradigms. Furthermore, as there is no coordination between the 10 main agencies or programmes, there will be significant redundancy in the ‘frontier research’ supported. Twice less funds associated to strong redundancy, produces, with equal investments and human capabilities, between 3 and 4 times less options being explored, and, if we accept that there are similar success ratios between both sides of the Atlantic, between 3 and 4 times less ‘nobelisable’ science... which is exactly the ratio observed in term of Nobel prize winners over the last thirty years.

43- Coping with diversity in knowledge dynamics: the ERC as the ‘agency of agencies’?

If we follow the reasoning pushed by such a conjecture, the central issue is not to add another independent agency in the already fragmented European landscape of funding, but rather to discuss how the existence of this new fund could drive to a greater amalgamation of funding bodies in Europe. My answer is that it could become the ‘agency of agencies’. Let me explain this apparently exotic solution.

We do not start from an empty space. There is a long tradition of bilateral collaborations between funding agencies, even if funds mobilised are generally small. Of course research organisations and funding agencies inherit the outstanding failure of ESF in term of amalgamating their means. Many observers were sceptical when the Commission proposed the ERA Net instrument. 5 years later, the surprise was the other way round: how could it be that it had been so attractive? And even if we will no doubt count many short lived experiences, this has demonstrated that in many cases national agencies were interested to join in specific areas and for specific issues. Even if it is anecdotal, the largest European call in social sciences on a given topic, migrations, has not been issued by the European Commission but by an ERA Net. And this short lived ERA Net has given rise to a now joint DfG – ANR – ESRC – NWO joint call. This shows that times have changed and that the idea of targeted pooling of resources is no longer just a ‘dream’ and could be seriously fostered by EC incentives, as seems the case in 2010 when dealing with toxicology issues of nanotechnology. This latter ERA Net also highlights another dimension: the specific construction by actors of the need for a European-level action has been taken up by funding agencies which discuss about original mechanisms to implement it. This ability to take into account the specific knowledge production requirements is reinforced by other anecdotal evidence, comparing for instance the strong differences in the positioning and projected developments of the two ERA Nets dealing with chemistry⁴.

This builds a learning path. The next step would be that the ERC does not use all its funds in its own ‘all over the board’ calls, but keeps a significant share to experiment, with national agencies, new forms of ‘joint programming’. ERA-Net like structures with national agencies would be in a position to accommodate developments such as this made by the DoE with its procedural, cognitive and organisational dimensions. It would not break from the central requirement of competitive funding but would adapt it to the perceived needs of the area looked at, and to the anticipated ‘basic research grand challenges’ (to use a DOE terminology). One issue is about the identification of areas/issues that require such approaches: one option could be the development of a forum of funding agencies with multiple processes, including bottom-up calls for want to be ‘basic research grand

⁴ see 2007 Bonn Conference by the PRIME ERA dynamics project; [add link](#)

challenges'; lessons for this could be derived from the ESFRI forum for European-level research facilities that has demonstrated its ability to operate such processes. This is what I call turning progressively the ERC into the agency of agencies.

To conclude

It is not because new concepts emerge from political dynamics that they should be considered as just fashions which will fade away. Collaborative research was born this way... This is why we should take seriously this political urge to consider 'frontier research'.

What I have tried to show is that we have both the conceptual apparatus and the organisational knowledge to address it. It drives to very different paths than the one presently followed at EC level. And one could even say that there are intuitions of this within quite a number of governments that advocate for more 'joint programming' on grand societal challenges. Whether we need to wait for an anticipated failure to generate a substantial level of 'frontier research', or whether we can influence the trajectory followed remains to be seen!

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