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EVALUATING PHILOSOPHY AS EXPLORATORY RESEARCH

ROGIER DE LANGHE AND ERIC SCHLIESSER

Abstract: This article addresses the question how philosophy should be evaluated in a research-grant funding environment. It offers a new conception of philosophy that is inclusive and builds on familiar elements of professional, philosophical practice. Philosophy systematically questions the questions we ask, the concepts we use, and the values we hold. Its product is therefore rarely conclusive but can be embodied in everything we do. This is typical of explorative research and differentiates it from exploitative research, which constitutes the bulk of funded research activity. This article argues that exploratory research is crucial for long-term progress and requires a distinct evaluative regime.

Keywords: exploitation, exploration, general purpose technologies, research evaluation, research policy.

> I mean by planning for freedom the orchestration of variety.
>
> —O. Neurath (qtd. Kallen 1946, 533)

1. Introduction: Exploitative and Explorative Research

Let us start by introducing two distinctions, one familiar and one less so, and tweak them for our present purposes. First, alongside the rise of the welfare-military-industrial state, research and research funding came to be distinguished along "basic" (or "pure," "fundamental") and "applied" research.[1] An intuitive way to think about the distinction is in terms of application: applied research is primarily about applying scientific results, which aim at understanding or truth, in technology, engineering, or medicine. One reason this is intuitive is because it matches up with important self-conceptions in the sciences and their

[1] For a recent, philosophically sophisticated treatment with special attention to origins of the distinction between pure and applied science, see Douglas. She claims, "The Great Exhibition of 1851 that applied science came to mean something like what we mean today.... The pleas for pure science began by the 1870s, but reached their full fruition in the 1880s" (Douglas 2014, 57). Douglas calls attention to the many myths that have historically surrounded the distinction and the ideological uses made of it. So, the distinction precedes the period we are discussing.

divisions in the modern research university. This is *not* the way we will use the distinction in what follows. By contrast, in this article we understand the distinction between basic and applied in terms of returns and profit. So, we understand applied research as that research of which expected profits can flow back to the originator or her employer. Applied research can sustain itself through the profits it generates. This means that, in principle, with a system of property rights, funding applied research can be left to the market and the economic agents operating in it. Obviously, in practice, applied research, in the sense that we use it, can also occur in universities, foundations, religious institutions, government research institutes, and so on.

The rewards of basic research do not primarily flow back to the originator but spill over to society at large. Therefore, it must receive external support because it cannot sustain itself despite the net societal profit it generates. So obtaining funding is a function of one's ability to demonstrate beforehand the expected results, why these are relevant, and how they will be achieved. Basic research requires non-market sources of funding, primarily the government. Of course, foundations, philanthropists, and even some large corporations (for example, the famous Bell Labs) may fund it, too, in practice.

This traditional distinction between pure and applied research sheds little light on the relation between philosophy and other basic research. A more useful distinction for the purposes of this article is therefore the distinction that some economists have made between exploitative research that enhances productivity and explorative research that enables exploitative research. In order to simplify discussion we treat this distinction and the kind of technologies they give rise to as a sharp one, although in practice it is probably better understood as a continuum.

Most technological innovations are the consequence of exploitative research—this is the bulk of industrial research and development. Explorative research has the capacity to generate more disruptive innovations, for example, writing, electrification, and the Internet.[2] The Internet, for example, does not in itself increase productivity much if at all, but it does enable the development of other technologies that do, such as cloud computing and software as a service. This is why Lipsey, Carlaw, and Bekar (2005) speak of "general purpose technologies," which have three main characteristics: (i) many different areas of potential application, (ii) strong complementarity with existing and potential new technologies, and (iii) room for incremental improvement as it enables exploitative technologies.

Although all productivity gains by definition derive from exploitative research, exploitative progress is always constrained by the potential of

[2] Of course, in practice the implementation of disruptive technologies may still require quite a number of technologies that rely on exploitative research.

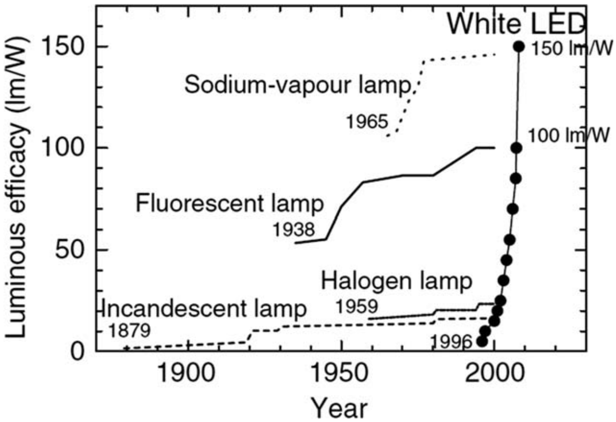

FIGURE 1. The evolution of luminous efficacy of various lamp technologies (source: Narukawa et al. 2010)

the explorative research that has enabled it.[3] Sustained, long-term progress in technological productivity is therefore only possible with sufficient exploratory research. For an illustration of the interaction between marginal short-term increases in progress through exploitation and sustained long-term progress through exploration, see Figure 1.

Whereas economists apply the distinction between explorative and exploitative research to applied research, in this article we apply the distinction, in turn, to basic research.[4] Exploitative basic research directly adds to our stock of knowledge. Examples include filling up Mendeleev's table, finding the Higgs boson, and deducing the gravitational constant. This is "normal science" in Kuhn's familiar terms. By contrast, explorative basic research is research aimed at enabling exploitative basic research. Scientific examples are the invention of the concept of the periodic table, Einstein's theory of general relativity, and Newton's mechanics. Turning Newton's mechanics into scientific and technological applications required a lot of hard, extra work after the *Principia*; textbook Newtonian mechanics was the fruit of this labour.

We understand explorative research as research that produces general purpose technologies (hereafter GPT)—intellectual, material, and conceptual—that (i) have a large variety of uses with multiple purposes;

[3] Of course, this is a simplification. But, for example, there are physical constraints on, say, the speed or minimal size of silicon-based chip technology.

[4] For an agent-based model of how the pattern in Figure 1 can emerge from the interactions of individual rational scientists adapting to research programmes, see De Langhe 2014a. For an application of this model to paradigm shifts in economics, see De Langhe 2014b.

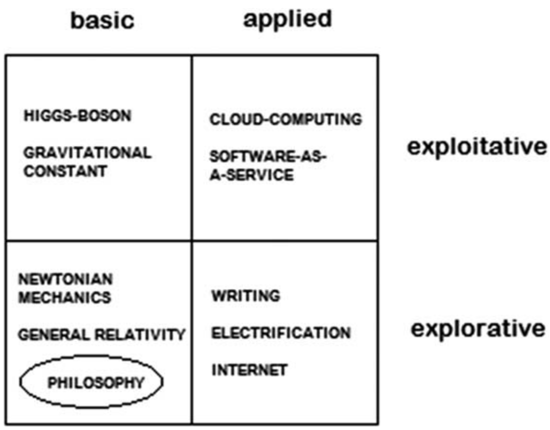

FIGURE 2. The quadrant of research

(ii) can be used in multiple intellectual and social domains; and (iii) can be used in tandem with a host of other existing technologies (social, material, conceptual, and so forth). Moreover, (iv) there is also a "performative" aspect to a GPT: (a) it can be developed in tandem with its use, and (b) so using it entrenches the technology. In the next section we treat philosophy as a species of explorative research.

In sum, research can be categorized on the basis not only of funding and expected profits but also of whether the research itself is aimed directly at increasing productivity through (incremental) exploitation or enabling future productivity increases through (disruptive) exploration. This results in "the quadrant of research" shown in Figure 2.

In practice, lots of sciences and disciplines can be put in many of the quadrants. Given that profits/returns are, in part, context sensitive, one cannot place domains of inquiry or scientific disciplines on one side of the quadrant once and for all. Something similar applies to the exploitative/explorative distinction: fields can undergo periods of incremental progress punctured regularly or infrequently with disruptive technologies. Below, we defend the idea why professional philosophy belongs primarily in one quadrant.[5]

The purpose of the quadrant of research is to help us think about evaluation regimes. So, the content of the distinction is meant to capture non-trivial descriptive, albeit context-sensitive, facts, but we use the quadrant in a normative fashion in an allocative context.

[5] In this article we ignore the core/applied distinction one often finds in philosophy, especially in ethics and applied ethics.

2. The Aims and Dynamics of Explorative and Exploitative Research

Explorative research and exploitative research have all but opposing aims and dynamics. This demonstrates the relevance of this often overlooked distinction.

2.1. Aims

Explorative research aims to carve out a new niche, an entirely new puzzle, or a new assembly line. Hence it aspires to "vertical integration," that is, to successfully connect as many processes as possible that were previously unconnected or did not even exist. The challenge is to subsume as many phenomena as possible under a single header, thus maximizing economies of scope—for example, the unification by which Newton achieved his new mechanics.[6] This mechanics was subsequently used for centuries to divide labour within physics. Apple combined innovations in software and hardware to invent an entirely new kind of product, the tablet computer.

Exploitative research aims to specialize in a single part of the puzzle or the production process. This is only possible if it has stable expectations about the expected output of other processes within the same assembly line. For this reason it aspires to "horizontal integration." An example is Intel, a company specializing in the production of processors; it became extremely successful thanks to its horizontal integration with Microsoft. Horizontal and vertical integration is shown in Figure 3.

In summary, explorative research aims to achieve economies of scale, and exploitative research aims to exploit them. Given that a shared standard (niche, puzzle, assembly line) is a condition of possibility for division of labour to be possible, vertically integrated explorers tend to precede the rise of horizontally integrated exploiters. A good example is the evolution of the PC market. Pioneering work was done by vertically integrated, mainframe computer manufacturing. But as the market matured horizontally, integrated players like Intel and Microsoft took over.[7]

2.2. Dynamics and Organization

Researchers exploiting the same general purpose technology (GPT) have a shared standard that can facilitate division of labour. Ever since Adam Smith it has been generally accepted in economics that dividing

[6] For literature on explanatory unification in science see Friedman 1974 and Kitcher 1981.

[7] In the philosophy of science, a focus on scientific revolutions may, thus, lead philosophers and scientists to overemphasize the benefits of unification. For example, Einstein (1934) even went so far as to call unification "the method of theoretical physics."

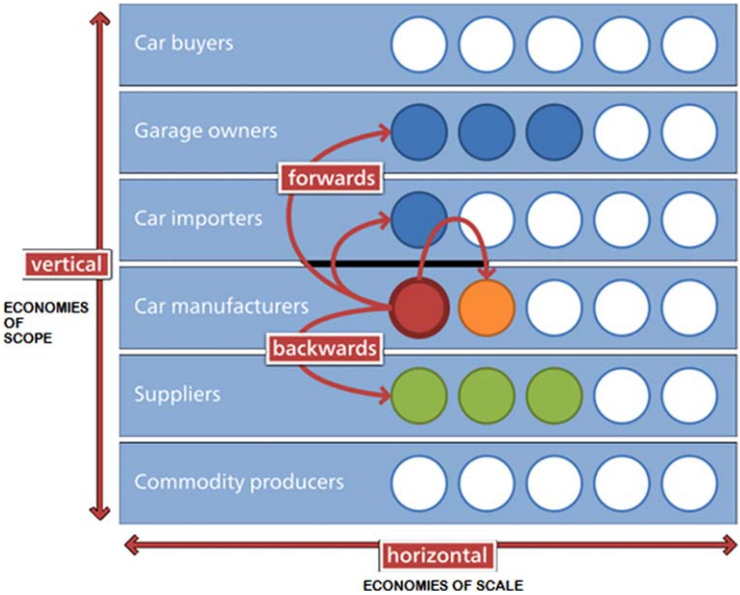

FIGURE 3. Horizontal and vertical integration [Colour figure can be viewed at wileyonlinelibrary.com]

labour increases productivity. As a consequence, exploitative research is a positive-sum game. The size of the group is limited only by the carrying capacity of the standard (for example, the size of market, income, existing technologies, and so on) for dividing labour.[8] Exploitative research is therefore capable of being organized within large, homogenous groups called schools, paradigms, or disciplines. Think of research at CERN or the Human Genome Project.

By contrast, in exploring new GPTs there is no shared standard. Alternatives being developed by different researchers tend to be substitutes, if related at all. From the vantage point of developing GPTs, exploratory research is a zero-sum game. Explorers tend to compete instead of to collaborate. Explorative research, therefore, tends to be organized in small, relatively independent groups. While there are a few larger formal and informal research groups, in philosophy the ongoing practice conforms to the explorative research model across nationality, research funding regimes, and philosophical schools.[9]

[8] There is, of course, a very detailed literature on the so-called production frontiers for countries, industries, and firms.

[9] In the developing field of experimental philosophy (so-called x-phi), the norms of authorship are similar to those found in social psychology; these, too, are fairly small research groups.

3. Philosophy as Explorative Research

In this section we argue that philosophy is explorative research, analogous to research aimed at producing a GPT. One subset of explorative research is primarily conceptual. We understand philosophy as conceptual explorative research. This approach subsumes within it philosophy understood as conceptual analysis, conceptual articulation, conceptual development, the history of philosophy, the empirical testing of intuitions about concepts (x-phi), formal philosophy, and so forth. Undoubtedly, we exclude important philosophical practices, but our intentions are inclusive, and we deliberately aim not to take sides in familiar divides among continental and analytic philosophy, critical theory, feminist theory, and other approaches.[10] Obviously, philosophy has other tasks; some of these are very dear to us, including teaching a great number of skills and values, calling students of philosophy to the good life, and a general receptivity toward wisdom. But in this article we focus on research assessment and funding.

In some areas of philosophy it is easy to see that philosophy generates GPTs: a variety of formal approaches are, if not topic neutral, then at least quite general: modal logic, Bayesian epistemology, game theory, and so on. Formal philosophy is very good at establishing visible GPTs. But normative philosophy is just as capable of generating GPTs: utilitarianism, Rawls's difference principle, Foucault's bio-power, voting theories, stakeholder theory in corporate capitalism, theories of art, the exploration and diagnoses of reasoning fallacies, and so forth.

As the examples in the previous paragraph suggest, the scope of GPTs need not be universal. What matters is that they are capable of being quite general. Moreover, a GPT need not be true for it to be quite useful. For example, even a deontologist may admit that for many purposes, a utilitarian calculus can be a useful tool in some policy environments (say, *within* a context of a strong regime of basic rights). We do not deny that many philosophers understand their own conceptual activity as aimed at truth, explanation, criticism, or articulation. All we require is that in such cases GPTs be foreseeable byproducts (even if not so intended).

Moreover, we argue that understanding philosophy in terms of explorative research that may (but not only) lead to conceptual GPTs can address two common worries about the general value of philosophy: (1) that philosophical questions are notoriously unanswerable and

[10] Having said this, two caveats: (i) some features of scientific philosophy that fully embrace the intellectual division of labor are more properly understood as exploitative research; (ii) some philosophies may well resist any species of external evaluation on political, methodological, or normative grounds. While we respect adherents of (ii), we think the stance self-defeating in the context of funding regimes. For the long history of the intellectual division of labor within scientific philosophy, see Schliesser 2011.

that such answers as are offered tend to be contested; and (2) that professional philosophy lacks the extent of coordination and organization found in science. The two worries are the root of the image that treats the origin of individual sciences as branching off from philosophy.

One argument for conceiving philosophy as basic explorative research is that this conception can successfully address these concerns. We argue that these two worries stem from a failure to distinguish philosophy as explorative research from more familiar and prevalent exploitative research. For this purpose we draw on the claims from section 2 that both kinds of research have fundamentally different aims and dynamics. Philosophy as explorative basic research explains (1) why its value is all but invisible and nearly impossible to measure using standards of evaluation appropriate to exploitative research and (2) why it tends to be organized differently from exploitative research. We addressed (2) in section 2.2, so now we focus only on (1).

3.1. The Invisibility of Philosophical Results

Philosophy as basic exploratory research is not concerned with settling questions of fact. Philosophers are not empirical scientists. This is not to deny that much philosophy draws on facts; nor does it deny that many philosophers aim at producing scientifically respectable truth or collaborate with scientists. Of course, there are philosophical projects that aim to aid in the development of scientific theories (say, in philosophy of the special sciences). But when philosophers do understand their own activity as uncovering facts, these tend to be quite general (see Paul 2012 on metaphysics) or aimed at the transcendental ground of experience (for example, in phenomenology). Some philosophical projects are aimed at developing concepts that make (possible) experiences more visible and easier communicable to others (Deleuze and Guattari 1994; Schliesser 2013).

Much philosophical activity pertains to contested areas; often much work is done to articulate what the question should be. It does not say how to do something but can help to explain why it should be done. This involves clarifying the concepts used to formulate the questions and explanations and exploring the logical entailments of their interrelations. These in turn predetermine what kind of answers could count as satisfactory answers and thereby constrain the ontological and methodological domain for the resulting quest for answers. Indeed, it is only after this constraint has become sufficiently restrictive that sufficient coordination and resulting specialization is possible to enable the structured and highly disciplined productive activity we call science.

As a consequence, it is rare that a product or action is ever the direct result of philosophical inquiry. What philosophers produce does not stand on its own but is embodied in the very fabric of our knowledge, actions, and institutions. The philosophical frameworks that underpin them often make them possible or intelligible. Just as electricity is useless without the appliances it enables, many concepts, methods, "isms," and "ologies" have their roots in philosophical inquiry. (It follows from this conception that one can be philosophical without having a doctorate in philosophy.)

Constitutive structures provide stability, and accordingly, while they can be improved and developed, they tend to remain fairly unchanged for longer periods of time. And when something does not change it is easy to forget it even exists. Not unlike other explorative basic research, philosophy therefore tends to be underappreciated because its role is confined to short but nevertheless crucial episodes in, say, the history of science (Friedman 2001).

Another reason for the structural invisibility of the fruits of philosophical research is common to explorative research: substantive effects are not only indirect and far between, they are also typically delayed, sometimes by decades, because newly explored technologies require other changes to occur before they can realize their full potential. Writing, for example, was not particularly useful when it was invented, since nobody could read. Technological innovations do not coincide with the growth in which they result. For example, Huygens experimented with steam engines during the middle of the seventeenth century, but steam only became a driver of productivity during the final decades of the eighteenth century. The same argument is invoked to explain why computers did not substantially increase productivity in the 1980s (the "Solow-paradox"; Solow 1987, Brynjolfsson 1993).

In summary, organizational differences between research fields can be explained, in part, by distinguishing between the explorative or exploitative nature of their mission. We do not mean to be revisionary here. Exploitative research is characteristic of the so-called mature sciences (Kuhn 1970, 69); even so, we do not like the pejorative nature of Kuhn's terminology (Schliesser 2012). An additional reason we have resisted using Kuhn's terminology here is pertinent to our focus on philosophy. We do not wish to claim that generally philosophical research intrinsically and teleologically strives to become mature science; we only claim that this is a possible and sometimes foreseeable side consequence.

4. Horizontal Evaluation

We have argued here that exploitative and explorative research are both necessary components of long-term progress and that explorative and exploitative research have fundamentally different aims and dynamics.

Evaluators of exploitative and explorative research are evaluating very different things. An exploitative research project will be an extremely specialized piece of a larger, well-established puzzle. An explorative research project will aim to produce a new puzzle. It is therefore remarkable that almost all publicly funded research evaluation is based on the same, one-size-fits-all principle that those closest to the researcher are best suited to evaluate what research ought to be funded. We call this idea that researchers ought to be evaluated by their peers "horizontal evaluation." Examples are peer review for journals, tenure committees, and grant review committees. The dominance of horizontal evaluation is unsurprising because (a) it takes quite a bit of advanced, skilful training to understand extremely esoteric scientific domains; (b) the bulk of research is exploitative in nature; and (c) there are important norms of autonomy that mandate that politicians and other interested parties remain at some distance from decision making that impacts the future of scientific research.[11]

The rationale for horizontal evaluation is straightforward from the perspective of exploitative research. Exploitative research is research that exploits a pre-existing research framework. This given framework allows the dividing of labour within it. Division of labour increases productivity by allowing individuals to specialize in complementary tasks. In this way goals can be achieved that no single individual could have achieved alone, such as building a cathedral, mapping the human genome, and finding the Higgs boson. These can be physically embodied in the production process, like for work at an assembly line, but most often it is socially enforced by a community of peers or equals. Peers will be willing and able to enforce rules because they share an interest in the collective goal and a standard for assessing the relevance of anticipated results; they know the customary methods to achieve them. As Kuhn has described, such evaluation practices promote a certain form of homogeneity (around shared standards, answers, methods, disciplinary boundaries, and so on), while fostering specialization in fine-grained puzzles and problems. Of course, as specialties become increasingly distinct, it is possible that they develop incompatibilities between them. But for present purposes we can disregard this.

Applying horizontal evaluation to exploratory research is problematic, however. Exploratory research lacks widely shared norms and criteria or measures internal to it that can be evaluated. Indeed, such norms and criteria or measures are often a side consequence of highly successful exploratory research. Exploratory research about concepts,

[11] There is a huge literature on these issues, of course. Within recent philosophy two notable books that address some of the complex considerations involved are Kitcher 2003 and Douglas 2009.

that is, philosophy,[12] is (inter alia) about finding out what the question should be, clearing up confusions, articulating concepts that make the previously though improbable visible, developing new standards of evaluation, and the like. Often exploratory research starts from a vision or a nagging feeling, and once this vision is in sight, it proceeds by working its way back to the objectives and methodology that will serve to articulate it and often retells and reinterprets the history of philosophy along the way. Its goals, methods, and standards for evaluation are therefore not the starting points but the end results of such research.

5. Vertical Evaluation

As a method to arrive at an alternative mode of evaluation for exploratory research, we propose to isolate the rules that translated the aims and dynamics of exploitative research into horizontal evaluation and apply them to the aims and dynamics of exploratory research we formulated in section 2. We will call the resulting mode of evaluation "vertical evaluation."

5.1. Let Research Be Evaluated by Those Who Will Benefit from Its Success and Not from Its Failure

Recall that exploitative research is research that exploits a pre-existing research framework. This given framework allows the division of labour within it. Division of labour increases productivity by allowing individuals to specialize in complementary tasks. Collectively, goals can be achieved that no single individual could have achieved alone. Rules are enforced because exploitative researchers rely on each other for the achievement of collective goals. In a division of labour, intellectual workers are all in the same boat. It can therefore be expected that peers will have the desire *and* knowledge required to further their interests.

Explorative research explores alternative research frameworks. Accordingly, the products of explorative research are not complements but substitutes. Explorative peers are not colleagues but competitors in the context of (limited) grant evaluation. We do not mean this in the ordinary sense of healthy emulation; nor are we blind to the fact that even in exploitative research there are all kinds of incentives that can make peer evaluation function as a means of excluding scientific competitors (Tullock 2005, chap. 7, "The Backwardness of the Social Sciences"). Rather, we mean it in the sense that competing visions

[12] To reiterate: there are exploratory research projects in some of the sciences, too, that may also be primarily conceptual.

(traditions, approaches, schools, and so on) often view each other as offering incompatible pathways towards the (dramatically alternative) future(s). This helps explain the often virulent and belittling opposition among partisans of such alternative approaches. Interested parties will likely not be readily classified as peers but might be found in other areas, fields, or disciplines that would stand to benefit from the integrative promise of the explorer.

Peers lack a natural incentive to select the most promising research because of the competitive nature of exploratory research. To the contrary, the more disruptive a project, the more likely the research will be to encounter a referee with a horse in that particular race. And because of the substitutive or even unrelated nature of alternative exploratory projects, referees will likely be unqualified to evaluate the research anyway. Note, however, that it might not be clear for years or even decades exactly which parties will benefit.

5.2. Let the Incentive Structure Be a Reflection of the Risk-Reward Ratio

Rewards from exploitative research are relatively low-risk but high-cost. Exploitative rewards carry low risk because exploitative research is embedded in an established research context with a proven track record of success. But conducting such research tends to become very costly because of its reliance on specialization involving the development of specific and often very expensive instruments. Investments in instruments are more efficient if they can be used more intensively and when their results are relevant to a greater number of other researchers. Accordingly, rewards tend to increase as more people join the same research programme. Exploitative research can benefit from strong economies of scale. A group adopting a single standard or a model organism is more productive than a group adopting two different standards. For exploitative research it is therefore wise to concentrate resources on so-called Big Science.[13] These economies of scale are reflected in the well-documented "rich get richer" incentive structure in science (Merton 1968, Strevens 2006).

By contrast, rewards from explorative research are low-cost but high-risk. Because of the delays, indirectness, and uncertainty of the effects of exploratory research, it is typically very difficult to predict which innovations will eventually turn out to lead to genuine GPTs. This is especially so because philosophers understand their own

[13] A recent example of such policy is the FET Flagship Initiative by the European Commission, which awarded ten-year, one-billion-euro grants to be used for the focused study of graphene and the human brain: http://cordis.europa.eu/fp7/ict/programme/fet/flagship/home_en.html.

activities as primarily focused not on the production of GPTs but on truth, understanding, meaning, justice, and the like. So, peers or no peers, assessing future exploratory research based on its objectives is all but impossible.

Because explorative research is mainly conceptual, its costs tend to be very low in comparison to those of exploitative research. Given this (opposite) risk-reward ratio it would be foolish to put all eggs in the same basket. The risk of failure must be hedged by fostering diversity (Kuhn 1970, Kitcher 1990, Zollman 2010, Muldoon 2013). Explorative research benefits not from economies of scale but from economies of scope.

It follows, then, that the size of average grants in explorative research should be relatively small but more researchers should have the possibility of obtaining such grants (see Burgoon et al. 2017). Rather than trying to pick research "winners" from the top down in advance, it makes more sense to promote a deep, heterogeneous pool of researchers. If big grants are to be advocated at all, they should be awarded as retrospective "achievement" prizes. If grant agencies and the publics that fund them wish to see work on particular problems or domains, they should encourage this indirectly by way of prizes.

The same "rich get richer" incentive structure that fosters specialization in exploitative research hampers innovation in explorative research. This "Matthew effect" has become stronger as bibliometric indicators have become a dominant tool in research evaluation. Counting citations and measuring impact factors favours research for which there already is a market with researchers to cite such research over research that creates new markets that researchers still need to enter. By the time the research does get picked up, the metrics work against it again because they are often used to track the so-called research frontier, a concept privileging relatively recent work (often at most two to five years old).[14] And in the meantime there are also non-trivial endowment effects: young, small scientific fields or ones with restrictive citation practices are at a disadvantage in comparison to older and often larger fields.[15]

6. Conclusion

The same incentive structure that allows exploitative research to reap the benefits of peer review and division of labour by fostering interdependence and homogeneity culminates for exploratory research in

[14] "Research frontier" is part of the official glossary of the European Research Council: http://erc.europa.eu/glossary/term/267. The locus classicus is de Solla Price 1965. For some of the relevant issues, see Nederhof 2006.

[15] For a revealing history of the role of trust and expertise, see Porter 1996.

stakeholder conflicts and potentially stifling conformity. Exploitative research evaluation practices foster short-term growth, but applied to explorative research these generate strong conservative forces that hinder long-term growth.

It follows from our analysis that if one wishes to foster explorative research within philosophy, this must be done by evaluating it against an incentive structure that promotes not interdependence and homogeneity but independence and heterogeneity.[16] Of the handful of seeds that are sown, some will grow to be fully functional, autonomous plants, and some will not. We have called this view of evaluation that is specifically designed for exploratory research "vertical evaluation" because its success depends on its ability to vertically integrate the different aspects of what constitutes an autonomous research programme, viz., a set of objectives and methods.

Vertical integration gives the independence and focus required to carve out a new niche. Despite the trend to the opposite for basic research, a trend towards vertical integration has emerged with innovative technology companies doing exploratory applied research (Vergara and Del Rosario 2012). For example, the success of Apple at creating not just new products but also new kinds of products, such as the tablet computer, has often been attributed to its vertical integration. Apple focuses on just a few products for which it produces both software and hardware, unlike more exploitative companies like Microsoft that have a wider range of products, the components of which are produced by a variety of external firms. Vertical integration makes it much easier for Apple to enforce innovative ideas top down (making possible the emergence of visionaries like Steve Jobs; see Isaacson 2011), whereas the coordination of chain-wide innovative changes is much harder for companies that have no direct control over the entire chain of production.

In an age of control it has become particularly difficult to embrace uncertainty. We have warned that the elimination of uncertainty from the evaluation process ignores the very essence of explorative research. The lesson from our present article can be summarized as follows: you cannot capture a wild horse by building a fence around it, you can only make your meadow fresh enough so that it comes to you. If you could build a fence around it, it would not be a wild horse. Analogously, if there would be a standard for evaluation of it, it would not be explorative research. The search to substantiate and objectify the results of philosophical inquiry is to ask the wrong question, because

[16] Basically we arrive here at an argument for a specific form of scientific pluralism called "antagonistic pluralism." See De Langhe 2010 for a categorization of scientific pluralism, a defense of antagonistic pluralism, and an application to a controversy in economics. For an independent argument for the same conclusion, see Lefevere and Schliesser 2015.

no product or action is ever the direct result of philosophical inquiry. Just as the challenge is not to keep the wild horse but to find it.

What philosophers produce does not stand on its own but is eventually embodied in the very fabric of our knowledge, actions, and institutions; the philosophical frameworks that underpin them are what made them possible to begin with. The rewards of philosophy are simultaneously the most intangible but also the most important for fostering long-term human progress. Without appreciating the philosophical contributions to our daily actions we forget who we are and where we come from, and so will leave the choice of where we are going to those who do. To put it in terms of a slogan: a focus on the actual without allowing for the unimagined possible is a recipe for a Great Stagnation.[17] Just as the bird might be deceived into thinking it will fly faster without the resistance of the air, policy makers aspiring to reduce science to its core business might be tempted to defund philosophy for its lack of demonstrable results and its failure to organize itself systematically. We have argued that without the resistance of the air the bird will not fly faster but will fall from the sky.

Philosophy has been and can keep on being an engine of progress if one accepts the uncertainties inherent in exploratory research.[18] And to achieve this, philosophical research ought to receive an incentive structure appropriate to its explorative mission rather than be pushed into a role that is not its own.

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[17] This process of a Great Stagnation might already be under way (Cowen 2011). For example, progress in medicine appears to be diminishing despite substantive increases in funding over the years (Scannell et al. 2012).

[18] Clearly, not all "progress" is to be welcomed. But if one prefers a status quo bias one ought to be explicit and transparent about it.

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