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Some remarks on the division of cognitive labor

Marco Viola*

Abstract: since the publication of Kitcher's influential paper The Division of Cognitive Labor, some philosophers wondered about these two related issues: (1) which is the optimal distribution of cognitive efforts among rival methods within a scientific community?, and (2) whether and how can a community achieve such an optimal distribution? Though not committing to any specific answer to question (1), I claim that issue (2) does not depend exclusively on an invisible hand like mechanism, since both intra-scientific and extra-scientific institutions may play a major role. Finally, I examine some practical difficulties of reallocating scientists from a method to another, which leads to stress the importance of well-planned training and recruitment.

Keywords: social epistemology; economic epistemology; division of cognitive labor

1. Introduction

Among researchers, it sometimes happens that practitioners of a heterodox methodology complain about the (either real or alleged) tyranny of a mainstream school of thought. Whereas some of these quarrels depend on private rivalries, it is almost indisputable that, within many disciplines, there actually are two or more rival schools of thought whose followers periodically engage in disputes and try to ostracize each other. I argue that, as far as these rivalries are genuine (i.e., as far as they are rooted in some genuinely epistemological disagreement), such disputes highlight social phenomena that have important epistemic consequence.
In this paper, I will present and discuss the following two issues:

> Division Issue (DI): What is the optimal division of cognitive labor between rival methodologies within a scientific community in order to achieve some epistemic goal?

> Attainment Issue (AI): Which mechanisms can lead to this optimal distribution?

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* IUSS Pavia e Università Vita-Salute San Raffaele. marco.viola@iusspavia.it
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In this paper, I do not claim any solution concerning DI; rather, I assume that DI is a genuine epistemological problem, that is, that different positions toward the division of cognitive labor affect a scientific community in epistemologically relevant ways.

Subsequently, I settle for supporting the following claims concerning DI:

a) External factors regulated by research policies can (and actually do) affect the division of cognitive labor.

b) Since the shifting of a scientist from a method to another always comes with some cost, an adequate planning of training and recruitment of scientists assume a key role for an efficient division.

I frame my discussion within the perspective of Goldman's (2011) system-oriented social epistemology. That implies that I consider science neither as a mere sum of individual scientists nor as a collective agent (e.g. "the scientific community")[1]; rather, I will look at science as "a social system that houses a variety of procedures, institutions, and patterns of interpersonal influence that affect the epistemic outcomes of its members" (Goldman 2011: p. 13).

This approach has two relevant features. First, many topics in social epistemology are framed through the theoretical lens and/or the empirical tools of social sciences[2]. The very questions w are going to solve became central in the so-called economics of scientific knowledge (Zamora Bonilla, 2012), and are sometimes analyzed using computer simulations (for an extensive review, see Payette, 2012). Although these approaches may provide powerful (though maybe controversial; cf. Hands, 1997, Maki, 2005) tools, the following discussion will be confined to merely theoretical grounds.

Second, system-oriented social epistemology has both descriptive and normative ambitions: it aims to "examine the systems in question to see whether its mode of operation is genuinely conducive to the specified epistemic ends" but also to "identify alternative organizational structures that might be epistemically superior to the existing systems." (Goldman 2011: p. 11)[3].

In the next section, I briefly examine two classical and antithetical answers to DI. Then, in section 3 I will expound the most influential modern discussion of both DI (§3.1) and AI (§3.2), namely Philip Kitcher's (1990, 1993) account of the division of cognitive labor – as well as some of the more recent literature on the topic (§3.3).

Subsequently, in sections 4 and 5 I present some considerations concerning the idealization operated by Kitcher and other authors; more specifically, in section 4 I claim that the division of cognitive labor does not depend exclusively on an invisible hand mechanism, but may be and actually is affected by scientific social institutions. Finally, in section 5 I stress that, because of the effort required from scientists to reallocate themselves into a different methodology, an efficient planning of the division of cognitive labor should focus on the training and hiring phase of new scientists.

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[1] These two perspectives would correspond to the other varieties of social epistemology described by Goldman (2011): namely, individual doxastic agents with social evidence and collective doxastic agent.

[2] Much social epistemology concerning science could be conceived as an essay to defend the authority of science from the critics of some sociologists of scientific knowledge such as Latour or the proponents of the Strong Programme in SSK (such as Bloor and Barnes). See for instance Goldman (2010), sections 2 and 3.

[3] Kitcher (1993: p. 315) shares similar ambitions about the policy-advising role of social epistemology, though he also invites us to be cautious in the interpretation of results based on idealizations.

2. Historical roots of DI

How much room should there be for dogmatism in science? Before the publication of Thomas Kuhn's famous The Structure of Scientific Revolution (1962/1970), the commonsensical answer would likely had been "the less, the better". Notably, Karl Popper championed a falsificationist account of science, which leaves none or little[4] room for dogmatism (Popper, 1959).

The publication of Kuhn's book, however, prompted a Gestalt Switch of the debate. Albeit being most famous for his groundbreaking claims on scientific revolutions, Thomas Kuhn maintained that history of science is, above all, a history of normal science – as opposed to a history of revolutions. During normal science periods, a scientific community is engaged in puzzle-solving activities: scientists follow the protocols of a shared paradigm, and systematically overlook those problems that fall outside the boundaries of their tradition.

Therefore, Kuhnian account of normal science leaves no room for heterodoxy: during normal science, a scientific community is (and must be – although this is a more controversial point, on which I will come back soon) hegemonized by a single paradigm. Although most theses of Kuhn (1962/1970) stands on a historiographical (descriptive) ground, I argue that he is also implicitly maintaining the normative claim that one scientific community must converge on a single paradigm in order to do normal science[5]. In his words:

> Men whose research is based on shared paradigms are committed to the same rules and standards for scientific practice. That commitment and the apparent consensus it produces are prerequisites for normal science, i.e., for the genesis and continuation of a particular research tradition (Kuhn 1962/1970: p. 11)

If ever a scientific community is not hegemonized by some paradigm, it is either because it has not achieved the status of mature science yet, or because it is shifting from an old paradigm to a new one, i.e., it is undergoing a revolutionary phase. In the first case, science makes slow (if any) progresses. In the second case, things are a bit more complex: Kuhn regards the shift from an old paradigm to a new one as a major leap forward for that science – although it comes with a price[6].However, revolutions don't come in the void: they are triggered by the accumulation of a considerable mass of anomalies which highlight the limits of some currently adopted paradigm. Hence, it is reasonable to assume that the hegemony of a single paradigm is necessary for both the

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[4] As highlighted in Rowbottom (2011), even Karl Popper admits a role for some dogmatism in science: "[T]he dogmatic scientist has an important role to play. If we give in to criticism too easily, we shall never find out where the real power of our theories lies." (Popper, 1970: p. 55; quoted in Rowbottom, 2011).

[5] One could argue that what Kuhn really means here is not a normative claim of the form "a scientific community ought to converge on a single paradigm in order to begin doing normal science", but rather a transcendental claim of the form "converging on a single paradigm is a necessary condition to fulfil before you can do normal science". However, since my main interest is theoretical rather than historiographical, for the sake of simplicity I will set aside this controversy and only consider the normative reading.

[6] According to Kuhn, each paradigm entails its own World view, which is usually incommensurable to the World view entailed by other paradigms. Therefore, some of the problems addressed within an old paradigm may simply cease to exist, and thus their solutions may become unavailable, within (the World of) a new paradigm. These losses were later called "Kuhn—losses".

kinds of progress, that is, the cumulative progress obtained by puzzle-solving activities during normal science, and the revolutionary progress that sets new challenges.
Despite the widespread popularity of Kuhn (1962/1970), his very account of history of science as a succession of hegemonic paradigms was all but hegemonic itself. In those same years, Imre Lakatos, developed an alternative account of the history of science, moving from his teacher's falsificationist account. He called his own framework the methodology of scientific research programmes (Lakatos 1970a). In a nutshell, according to Lakatos science is[7], and must stay, a 'battlefield' where competing research programs face each other in order to provide the best explanations for already known phenomena – as well as the better predictions for those yet to come.

While exposing his framework, Lakatos explicitly rejects Kuhn's claims on both descriptive and normative grounds:

> The history of science has been and should be a history of competing research programmes (or, if you wish, 'paradigms'), but it has not been and must not become a succession of periods of normal science: the sooner the competition starts, the better for progress. 'Theoretical pluralism' is better than 'theoretical monism': on this point Popper and Feyerabend are right and Kuhn is wrong (Lakatos 1970a: p. 69. Emphasis is mine)

Though not central in their writings, I claim that this dispute between Kuhn and Lakatos set two milestone answers concerning DI. Their accounts represent two antithetical answers: whereas Kuhn states that a single paradigm ought to eschew any rival in order to have normal science and thus significant progress, Lakatos argues for the proliferation of several competing methodologies research programs.

This dualistic picture would be a bit too simplistic to withstand an accurate historiographical scrutiny. For instance, this image of Kuhn as a standard-bearer of dogmatism only holds when we consider the science battlefield synchronically, while diachronically speaking Kuhn considers scientific revolutions and the ensuing paradigm-shifts both physiological and beneficial for scientific progress. However, my purpose here is not to shed light on the actual history of philosophy; rather, I discussed this controversy because Kuhn and Lakatos' answers represent two sharp-cut solution to DI, which I think to be a legitimate and also useful way to employ history of philosophy, as far as we are conscious and transparent about its sketchiness. Accordingly, from now on I will speak about "lakatosian" distribution referring to those situations in which the members of scientific community split their efforts within different methodologies during the same time, whereas we will call "kuhnian" those situations where that does not happen.

[7] Lakatos' historiographical claim is actually a bit more complex – and controversial. In his (1970b) he proposes a criterion for assessing the adequacy of a philosophical theory of history of science compared to other rival theories. His criterion states that the better theory is the one which allow the greatest part of history of science to be accounted for on purely rational ground – *i.e.* by appealing to internalist (epistemic) explanation – rather than invoking the intervention of externalist factors (such as political interests).

3. Kitcher on the division of cognitive labor

In order to address a challenge posed by Kuhn (1977), that is, how could science balance between tradition and innovation, Philip Kitcher adopted an economic model first employed by C.S. Peirce (1979). His The Division of Cognitive Labor (Kitcher, 1990; further developed in Kitcher, 1993: ch. 8) set the terms of both DI and AI so as we are discussing them.

3.1 Kitcher on DI

Regarding DI, in his 1990 paper he formulates the following thought experiment:

> Once there was a very important molecule (VIM). Many people in the chemical community wanted to know the structure of VIM. Two methods for fathoming the structure were available. Method I involved using X-ray crystallography, inspecting the resultant photographs and using them to eliminate possibilities about bonding patterns. Method II involved guesswork and the building of tinker-toy models. Everybody agreed that the chances that an individual would discover the structure of VIM by using method I were greater than the chances that that individual would discover the structure by using method II. Since all members of the community were thoroughly rational, each chemist used method I. They are still working on the problem (pp. 11).

Then, adopting the fictional perspective of "a philosopher-monarch, with the prerogative of directing the course of scientific research" (1990: p. 8), he wondered how should such a monarch allocate the cognitive workforce among the two methods[8].

His answer requires the following idealization: imagine that each method has a probability p(n) to provide a solution for each number n of scientists who pursue it, assuming each scientist's contribution to the discovery being equal (or, at least, that a brilliant scientist's contribution may be represented as many scientists' contribution). Given that the whole community is made up by a number N of scientists, the optimal solution to DI requires to maximize the probability that either method I or method II discover the structure of VIM, that is, to maximize

p1 (n) + p2 (N - n) - Prob (both methods deliver).

Kitcher considers reasonable to assume that, in most cases, function p(n) is both increasing monotonically with n and tending asymptotically to some value p when n goes to infinity. The direct consequence of these assumptions is that there is a kind of "saturation point" for method I such that, albeit it is intrinsically preferable to method II, given a sufficiently high number n of scientist pursuing it, a new scientist would increase p2(n) by pursuing method II more than he would increase p1(n) by pursuing method I. That is because, since both p1(n) and p2(n) tend asymptotically to 1, as

[8] Kitcher employs a programmatic broad notion of "method", encompassing "a set of rival theories, research programs, methods for approaching a problem, etc." (1990: p. 10). This umbrella terms encompasses both weaker meanings, that can peacefully co-exist even within a single paradigm/research program (e.g., rival theories, instruments and techniques for making observations), and stronger meanings that entail the inconsistence between different 'methods' (e.g., different and inconsistent ontologies).

they are probability functions, it is likely that a more fruitful method becomes less fruitful than its rival(s) because it is "too crowded".

Therefore, Kitcher gives no short nor sharp-cut answers to DI, because in his framework the community optimum depends on both the size of the scientific community (N) and the 'sloppiness' of the functions p(n) of each rival method. Consequently, whenever there are too few scientists to fully exploit the potential of a better method, Kitcher's account would prescribe what we have called a kuhnian distribution – i.e., focusing every effort on the better method (cf. §2). However, he claims "that we sometimes want to maintain cognitive diversity even in instances where it would be reasonable for all to agree that one of two theories was inferior to its rival, and we may be grateful to the stubborn minority who continue to advocate problematic ideas" (1990: p. 7). In our terms, he concedes that, on many occasions, we should seek for a lakatosian solution to DI.

3.2 Kitcher on AI

After having dealt with DI, Kitcher turns to AI: he asks how – by which means – can the community achieve such an optimum. In order to frame DI, Kitcher proposed to put ourselves in the shoes of a philosopher-monarch. However, since such a monarch does not exist, he assumed that the distribution of scientists into various methods relies on the individual scientists' rationality (that is, on a set of norms regulating their behavior so to maximize some utility function). Thus, he compared different rationality norms, assessing how far these would lead from the *desideratum*.

To begin with, he considers what would happen if scientists were driven by purely epistemic goals, regardless of their personal profit. Scientists may simply choose to pursue the intrinsically better method (that is, the method in which the contribution of a single scientist generates the greater increase of the probability function), while being unaware of what their peers do[9]. This account of rationality is clearly unsuited to achieve a lakatosian distribution, because everybody would pursue a single method. A similar outcome would result even if the rationality norms of individual scientists prescribe them to maximize their chance to follow the method that yields the right answer.

Of course, one could elude the problem by tautologically redefining rationality so to let it fit the desired outcome. For instance, one could say that "that an individually rational agent is a person who chooses so as to belong to a community in which the chances of discovering the correct answer are maximized" (Kitcher, 1990: p. 14). Besides its "begging the question" flavor, Kitcher is skeptic about any account of rationality in which scientists are conceived as unnaturally altruistic agents.

[9] Here Kitcher makes the (unrealistic) assumption that all scientists' estimates of the intrinsic qualities of each method are both approximatively correct and universally shared. However, as Goldman (1999) points out, "scientists do not always agree in their estimates of the probability that a given method will yield an answer. […] This alone might guarantee a substantial diversity of research paths […] even if scientists have purely epistemic goals, and only wish to pursue (what they regard as) the most probable route to an answer" (p. 257. See also Muldoon and Weisberg's 2011 critics in §3). Nonetheless, there is a worst possibility: scientists may agree on the wrong answer, and thus disregard a potentially fruitful method in favor of a less fruitful one. In such a case, extra-epistemic factors may counterbalance an otherwise disastrous outcome.

Then, he moves to consider a different scenario, in which scientists are motivated by a purely egoistic goal, that is, the quest for credit – represented by winning a prize for the discovery. Since in the history of science, as Merton (1957) pointed out, there is no place for second comers, they only seek to be the first to make a discovery. Assuming that each scientist pursuing a method has the same probability to make a discovery as her colleagues, the probability that she wins the prize is directly proportional to the overall p(n) of the method that she is pursuing and inversely proportional to the number of scientists pursuing that methodology. To put it simple, "by choosing a method, [she] buy[s] into a lottery that has a probability of paying up, a probability dependent on the number of ticket holders; [her] chance of collecting anything is the probability that the lottery pays up divided by the number of tickets" (Kitcher, 1990: p. 15).

Kitcher observes that, whenever a lakatosian distribution is preferable, such an egoistic community would have far more chance of approximating it, since scientists would be discouraged to place their bet on the majority approach in case it gets too crowded. This allows him to draw the following counterintuitive conclusion:

> The very factors that are frequently thought of as interfering with the rational pursuit of science – the thirst for fame and fortune, for example – might actually play a constructive role in our community epistemic projects, enabling us, as a group, to do far better than we would have done had we behaved like independent epistemically rational individuals. Or, to draw the moral a bit differently, social institutions within science might take advantage of our personal foibles to channel our efforts toward community goals rather than toward the epistemic ends that we might set for ourselves as individuals. (Kitcher, 1990: p. 16).

3.3 Kitcher's account and its legacy

The framework developed by Kitcher in *The Division of Cognitive Labor* became the starting point of any ensuing enquiry concerning both DI and AI. His answers can be summarized as follows:

> [K-DI] In many cases, focusing all the scientists' efforts to a promising but overcrowded method would enhance the whole community's epistemic benefit (i.e. the probability to attain an answer to a given question in a given time) less than it would if they split their efforts in two or more rival methodologies.

> [K-AI a] Since there is no monarch in science, the division of scientific labor has to rely solely on scientists' individual urges.

> [K-AI b] A community made of sullied scientists thriving for credit would allocate scientific efforts better than a community made of altruistic scientists would.

The relevance of Kitcher's account primarily comes from its methodological legacy in the subsequent literature[10], rather than to its specific answers to the questions: Kitcher framed the discussion of both DI and AI within a quasi-economic perspective, treating the problem of optimally distributing cognitive labor as a resource allocation problem.

Exploiting the very same Peircean approach, Michael Strevens (2003) argued for the rationality of the so-called priority rule (i.e., the fact that credit for a discovery is given only to the researcher or

[10] For a comprehensive review of this literature, see Weisberg (2010) or Muldoon (2013).

research team who first made it, first described by Merton, 1957). He claims that priority rule is preferable over other options for allocating credit to scientists (e.g. rewarding them for their contribution of a project), since it encourages scientists to "hedge their bet".

This optimistic picture depicts science akin to a self-regulating market, in which scientists split their efforts toward different projects optimally thanks to the priority rule. However, as Strevens himself stressed in a later paper, real scientific communities often exhibit herding phenomena, that is, the number of scientist who pursue mainstream projects is often greater than it should be in order to achieve an optimal distribution. He explains this fact with the following argument: since in many cases there are no objective measurements of the success of a given scientific project (at least in the mid-term), its assessment rests on the shoulders of scientists themselves. Therefore, scientists are less prone to indulge in projects that are unpopular among their peers, since their success would risk to be underestimated and the project funding cut off before their project thrive enough for achieving objectively measurable success (Strevens, 2013).

Other authors exploited the economic approach in order to explain away the possible tension between individual (i.e., single scientists) rationality and collective (i.e., the scientific community as a whole) rationality. As long as one wishes to account for the rationality of the scientific enterprise, an economic theoretical framework seems particularly fit for the job: it allows to depict an optimistic picture in which, as Mäki (2005) puts it, public virtues (may also) flow from private vices.

Goldman and Shaked (1991) developed a bayesian model where scientists are conceived as agents that receive rewards on the basis of how much they affect their peers' priors about world views. According to their model, credit-motivated agents do well, although there is a range of cases for which truth-motivated agents outperform them.

Nonetheless, some thinkers blamed the so-called "economic epistemology" for acritical borrowing tools and concepts from economics (e.g. Hands, 1997; Mäki, 2005). Muldoon and Weisberg (2011) raised criticisms over Kitcher and Strevens' economic framework, which they call *marginal contribution/reward* (MCR) approach, showing that it rests on four unrealistic idealizations:

> First […] that scientists are utility-maximizers, responding rationally to the established incentive system. Second, […] that the division of cognitive labor can be represented as a choice amongst a number of pre-defined projects. Third, [that] every scientist knows the distribution of cognitive labor before she chooses what project to work on. […] Finally, [that the objective probabilities of success] functions are known by all of the scientists in the model. (Muldoon and Weisberg, 2011: p. 163-164).

In a similar fashion, in the remainder of this paper I am going to raise two issues that need to be accounted in order to make Kitcher-like models more realistic: first, I will cast doubts on the idea that, since there is no science monarch, then the division of cognitive labor relies solely on an invisible-hand-like mechanism. Second, I will stress that re-allocating scientists on new projects is not an effortless endeavor, and thus an efficient division of cognitive labor should take place mostly *ex ante*, i.e., during the training and the recruitment of future scientists.

4. Isn't there any alternative between a philosopher-monarch and the invisible hand?

Although his formulation of DI is framed from the point of view of a philosopher-monarch, before facing AI Kitcher (1990) dismisses this platonic regnant, claiming that such a point of view was just a useful fiction. Therefore, he assumes that the division of cognitive labor depends solely on the personal choices of individual scientists, regulated by an invisible-hand-like mechanism (see above, K-AI a).

Likewise, other authors take for granted that, since there is no science monarch, the division of cognitive labor (as well as other aspects of the social organization of science) relies upon an invisible-hand-mechanism that harmonizes the individuals' choices. Zollman, for instance, asserts that "in science, there is no social planner. Individuals are left to their own devices to choose how much of their time to dedicate to science" (2014: p. 6)[11]. Strevens considers that "in modern research science there are no more superintendents" (2013: p. 5) – though he also points out that Peirce's (1879) seminal work, from whom Kitcher (1990) draws upon, was written in order to advise a real science superintendent who had to actually split the resources between scientists.

Whereas it is true that nowadays there are no centralized monarch/superintendent (at least in Western countries), concluding that individual scientists are the only players in the game is hasty – as well as false. Surprisingly few thinkers took into account the role of scientific institutions. Most notably, Wray (2000) argued that the role played by invisible hand mechanisms in accounting for the success of science is overrated, and a more comprehensive account should explicitly consider institutions designed by scientists[12]. Another one is Goldman, who openly argued against Kitcher (1990)'s account by considering the role of funding agencies:

> Kitcher oddly ignores the fact that although there is no science "monarch," there are centralized agencies whose decisions powerfully influence the allocation of research efforts. These are the funding agencies, both governmental and extragovernmental. Very little of modern science can be conducted without funding, and as long as individual scientists at least propose to use different methods, funding agencies are in a position to encourage diversity by financially supporting it. Centralized decision making is one part of the larger social practice that determines the allocation of research effort (Goldman, 1999: p. 257).

Assessing the contribution of scientific agencies toward the division of cognitive labor is a tricky task. The main point here is not the trivial consideration that, as Strevens (2013, fn. 2) puts it, their power is minuscule compared to that of the superintendent[13]. Rather, the relevant difference is on

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[11] Although in another paper (Kummerfeld and Zollman, forthcoming) he argued for the desirability of social regulation.

[12] Oddly enough, Wray (2000) only takes into account those institutions whose design has been planned by scientists themselves, omitting to consider the role of external social factors such as the political decisions to pursue some or other scientific project; see the discussion of Human Brain Project below.

[13] The degree of power of the funding agencies is very sensitive to some sociological features concerning the scientific profession. While in models like Kitcher (1990), scientists' freedom to choose the topic and the method of their research sounds a reasonable idealization with respect to tenured professors, the same does not hold for untenured postdocs

the qualitative ground: whereas a monarch can directly allocate scientists into one or another method, central agencies can only exercise indirect influences on individual scientists' decisions, slightly affecting the attractiveness of some method over others.

Moreover, some agencies can (and indeed do) affect the division of cognitive labor by other means than funding. Though I cannot indulge into details here, it is worth mentioning that evaluation agencies and research assessment exercises all around the world profoundly affect the behavior of scientists, usually discouraging heterodoxy and thus fostering a kuhnian distribution[14].

Sometimes, funding agencies' role in shaping the division of cognitive labor can be rather prominent. Consider the *Human Brain Project* (HBP), one out of two flagship research project funded by the European Commission. While its aim is to comprehend the functioning of the human brain, surprisingly enough the HBP is not a neuroscientific project in a traditional sense; rather, it is an informatics project. As a matter of fact, its main research strategy will be to simulate a human brain through computer science tools, employing a bottom-up approach (from single neurons to brain functions).

However, many neuroscientists were disappointed about the enormous amount of funding allocated to HBP, whose promise to provide a realistic simulation of the human brain they regarded as very premature with respect of the limits of our current knowledge. Their discontent was given voice in an open letter sent to European Commission on 7th July 2014[15], signed by almost eight hundreds European scientists. Their aim was to invite the EC to reconsider the allocation of funding, in order not to eschew cognitive neuroscience, that is, the study of high level function from a mainly top-down perspective (from brain functions to single neurons).

This real case closely resembles Kitcher's (1990) hypothetic case: even if the object of inquiry here is the brain rather than a hypothetical VIM, scientists could decide either to build tinker toys (as HBP proponents suggest), or to undertake more traditional empirical investigation (as invoked by those who subscribed the letter). But unlike Kitcher's model, the decision does not only – nor mainly – bears upon the shoulders of individual scientists. Rather, it is very likely that the decision of such a powerful backer as the EC would profoundly determine the future of neuroscientific research by directly choosing which method to finance[16].

Despite its brevity, I consider this episode a compelling proof that there can be – and there actually are – other actors apart from individual scientists who affect the division of cognitive labor. Surely, recognizing such factors compels us to give up with the idealized picture of science as a self-regulating system (even in Kitcher-like models, where this "self" also includes non-epistemic

(which are growing in number, as witnessed for instance by Finkelstein 2010 for the US and by Bonatesta 2015 for Italy) or private sector scientists, since their job is often more or less strictly tied to some specific project.

[14] Notably, Gillies (2008) raises major skepticisms against the Research Assessment Exercise in UK. On a worldwide perspective, Origgi and Ramello (2015: p. 12) considered that "ranking systems do not foster interdisciplinarity and research diversity. Rather, they encourage 'gaming the system', and force competitive practices that are not always compatible with the norms of production of knowledge"

[15] http://www.neurofuture.eu/.

[16] I don't want to imply that scientific efforts equals to funding; as Kitcher, I take scientists working hours as the better (though still rough) measure of cognitive labor. However, since in many Big Sciences there is little to no possibility to work without appropriate funding, an appropriate funding may be considered a necessary condition for some scientists to work within a methodology.

motives). If science were such an impermeable system, social epistemology would have nothing to say from a normative point of view. But it looks like this isn't the case.

5. Switching method does not come for free

In his 1999 book, Alvin Goldman correctly points out that Kitcher's model "assum[es] that there is an adequate supply of competent researchers ready to pursue each method, if funded". However, he continues, "[i]f workers have different skills and specialties, they may not be transferable across methods, as Kitcher assumes" (Goldman, 1999: p. 258).

We will refer to Kitcher's assumption as the "Painless Transferability Assumption" (PTA), and defining it as follows:

> [PTA] A scientist may stop pursuing one method M1 in order to begin pursuing a rival method M2 without any cost in efficiency, time and/or other resources.

Of course, this formulation of PTA is largely untenable, since even the smallest methodological switch comes with some cost. In Kitcher's (1990) terms, even if a chemist who always worked with the X-ray crystallography is skilled enough to efficiently work also on tinker toys, in order to do that she may probably spend a few weeks to prepare her laboratory and reading the most recent literature.

Some costs may be worth paying: provided that her motivation to switch method is strong enough, the aforementioned chemist may be gladly willing to invest a few weeks and some money into it; yet, in other cases, the switch may not be worth the trouble.

The costs of method switching can vary greatly from case to case, also because of the vagueness of the notion of "method(s)" (see *fn.* 8). While passing from the endorsement of a theory to another demands a relatively small effort, undertaking a new kind of empirical investigation usually requires some training, which can take up from some weeks to several months.

We can also consider the radical cases in which, as Thomas Kuhn (1962/1970) maintained, scientists who lived and operated within a paradigm will be unable to convert to a new one, no matter what[17].

However, even eschewing these radical cases, we should look at the division of cognitive labor under a new light. Since they were not interested in the practical aspect of DI (how to achieve an optimal division of cognitive labor?), Kitcher and other authors modeled the distribution of scientific efforts as an idealized hydraulic system with no attrition, where scientist can effortlessly flow from one method to another. My claim is that any model on the division of cognitive labor that aims to be relevant for science policy has to replace these idealized fluids by viscose fluids whose transferability comes at a cost – or even, if we admit kuhnian impossibility of conversion, that some scientists are solids rather than fluids.

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[17] In the authoritative words of Max Planck: "a new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it" (Planck, 1949: pp. 33-34, quoted in Kuhn, 1962/1970: p. 151).

Considering the effort of switching method has one important corollary: namely, that a scientific community, considered diachronically, is the more efficient the less switches it undertakes. In rough words, we may say that, since training scientists and buying new instruments are expensive activities, we could want to avoid do it twice (or even more times). Still, many of these costs has to be paid at least once, i.e., during scientists' training. Therefore, if we want a lakatosian division of cognitive labor, it would be more efficient to attain it by promoting the differentiation of doctoral programs. On the contrary, if we seek for a kuhnian concentration of the cognitive efforts, it is our best interest to uniform the training of young scientists so to make them all capable of efficiently pursuing the one and only paradigm.

Furthermore, since the centerpiece of science are still tenured professors in universities (despite the growing amount of untenured professors, see Finkelstein, 2010), the hiring stage is tantamount important for planning the desired division of cognitive labor. Hiring professors who are highly trained in/committed to some method rather than another would be far more efficient than trying to make a Copernican out of a Ptolemaic. Any such choice could easily trigger a path *dependency* effect: when she comes to judge for the future candidates, it is very unlikely that a Copernican would choose a Ptolemaic (or viceversa).

Of course, since the DI is yet difficult to solve on a synchronic level ("how much heterodoxy should I tolerate/foster?"), I can hardly imagine that someone has clear intuitions as for the diachronic level ("how much heterodoxy should we want in the next decade?"). As I mentioned in the introduction, I have no reply to all these questions. Nevertheless, it would be in and of itself a good thing if whoever has the power to exert some influence on either the training or the hiring of scientists becomes aware of the impact her choices may have even on the division of cognitive labor.

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