UDK: 316.776:005.941
https://doi.org/10.2298/FID1904535P
Original Scientific Article
Received: 14.01.2019. Accepted: 07.04.2019.

PHILOSOPHY AND SOCIETY
VOL. 30, NO. 4, 463–644

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TEAM AND PROJECT COMPOSITION IN BIG PHYSICS EXPERIMENTS

ABSTRACT
Identifying optimal ways of organizing exploration in particle physics mega-labs is a challenging task that requires a combination of case-based and formal epistemic approaches. Data-driven studies suggest that projects pursued by smaller master-teams (fewer members, fewer sub-teams) are substantially more efficient than larger ones across sciences, including experimental particle physics. Smaller teams also seem to make better project choices than larger, centralized teams. Yet the epistemic requirement of small, decentralized, and diverse teams contradicts the often emphasized and allegedly inescapable logic of discovery that forces physicists pursuing the fundamental levels of the physical world to perform centralized experiments in mega-labs at high energies. We explain, however, that this epistemic requirement could be met, since the nature of theoretical and physical constraints in high energy physics and the technological obstacles stemming from them turn out to be surprisingly open-ended.

KEYWORDS
social epistemology, networks, science, physics, technology, innovation

1. Organizing Experimental Science: Epistemological Approaches

What are the best ways to organize big scientific communities and big scientific networks, big physics experiments, in particular?[1] The organizational issues common to modern large physics laboratories were not common in a typical physics laboratory the size of a house basement at the beginning of the 20th century. There was, of course, a network of different laboratories that communicated, so organizational issues appeared at a higher level of organization. Today, however, there are a hundred times more professional physicists than before WWII (Kragh 2002, Ch 2). This is a staggering increase, much larger than the increase in the overall population of the respective societies. In addition, vastly more resources are invested in physics experiments today than in

1 This work was presented at the 4th LOGiCIC international workshop at the University of Amsterdam, November 26–28, 2015. (https://logicicworkshop2015.wordpress.com/programme/) It was also presented at an international conference *How to Act Together: From Collective Engagement to Protest* held in Belgrade in November 19–21, 2015, under the title "Epistemic and Social Networks in Big Science".

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the age of small laboratories (Ibid.). The latest result of these trends is the Large Hadron Collider at CERN, which houses about ten thousand professionals, including thousands of physicists. Discovery papers coming out the laboratory are sometimes signed by thousands of collaborators. Several obvious questions emerge. How should communities of this sort be organized? Has this kind of organization been implemented anywhere? And can large laboratories be organized into networks in optimal ways?

There is often a political element to such questions, especially when funding agencies enter the picture. Is organizing science best left to scientists? Or should funding agencies be the ones to determine organization? The default response among academics seems to be to let scientists do their work because that is how they will perform best. To ensure results are advantageous to society, the argument goes, funding agencies should not interfere substantially with the way scientists want to plan and perform their research. Yet this is a vacuous answer. To respond properly, we need to know what happens once the agencies grant the money to scientists and let them organize the way they do science.

In fact, all sorts of outcomes can and do happen. Institutional inertia frequently shapes long-running research (Torrisi 2014), or the funding structure can influence decisions and determine the organizational structure of research (Hallonsten and Heinze 2012). Both can be harmful to productivity and to the efficiency of research. The politics at all levels inevitably shape large research operations, often adversely (Chompalov et al. 2002; Greenberg 1999). One example is the organization of CERN; during the first 15 years of its existence, it consistently performed worse than labs in the US. One of the main reasons was that at CERN, the quota of representatives of each donor nation was mandated. This prevented hiring based on merit alone. A strong top-down hierarchical organization was required to oversee the process (Herman et al. 1987).

Thus, while we agree that the science is best left to the scientists, we argue that this is only true if they approach the organizational issues as meticulously as they approach the subject of their research. But is there an optimal way of organizing teams and projects in large laboratories, i.e. an optimal organization that provides optimal epistemic conditions for generating experimental knowledge? If so, can we identify it, and how?

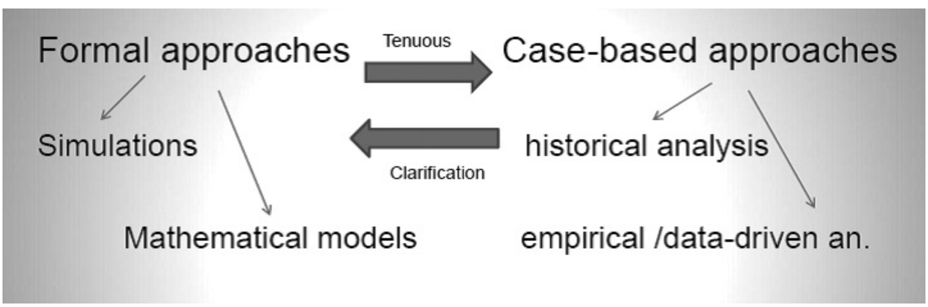

Diagram 1: Epistemological approaches to the organizational structure of scientific networks.

We can approach the question in two different ways (Diagram 1). The first approach is formal and makes use of mathematical modeling and computer simulations. Philosophers, science policy analysts, computer scientists and others have used this approach to address similar questions. For example, computer simulations utilizing graphs have been used to examine how networks of various structures – centralized, loosely connected, or decentralized – affect efficiency and accuracy in performing certain kinds of tasks (Zollman 2007). Modelling techniques typically used in economics, utilizing, for instance, decision theory, have been used for the same purpose (Charn et al. 1978). The second approach employs case-based analysis: a historical or data-driven analysis of particular cases can be performed to examine the networks in question (Perović et al 2016; Maruyama et al. 2015; Cetina 1999).

There are strengths and weaknesses to both approaches, so ideally we should do comparative analysis as well. On the one hand, when relying on case-based analysis, we rarely arrive at succinct or formalizable conclusions. Thus, we can combine those sorts of insights with modelling and simulations to improve understanding. On the other hand, the parameters in simulations and abstract formal models are usually not connected in obvious ways with actual cases, so case-based studies can help establish this relationship in a direct and informed manner.

2. Efficiency and Structure of Scientific Networks

There are two sets of questions that these two approaches can address in our discussion of organizing science: quantitative and qualitative. First, what is the optimal team composition in terms of the number of researchers? How many members does an efficient research group require, and what is the optimal number of researchers in the laboratory, given a certain task? Second, in terms of the optimal project composition, what is an optimal division of researchers into groups (sub-teams)? Project leaders and managers have to grapple with these quantitative and qualitative questions and solve them under time constraints while having only a vague idea of how the actual research will unfold. If there are too few researchers per team, it is easy to end up with like-minded approaches to the problem, and the required diversity is lost. If there are too many, communication may not be effective. There is also a problem of inertia that sets in if the team has been working together for too long; this is a major issue in long-lasting experiments.

These questions have been studied extensively by science policy scholars (Cook et al. 2015; Carillo et al. 2013; Maruyama et al. 2015; Torrisi et al. 2014), as they are of a general epistemological interest as much as they are a matter of practical concern. A recent example is a public debate among biologists on the optimal number of team members in a typical biology laboratory (Cook et al. 2015). In fact, these issues come up in other kinds of organizations, especially in industry where they were studied systematically in various ways much earlier than in science. The answers are naturally context-specific, although we may find an organizing rule or two that is epistemically beneficial across contexts.

The question about the size of scientific networks and their ability to solve problems or make accurate predictions about natural phenomena is closely related to the question of the role of cognitive diversity in groups. Industries are interested in exploring diversity as a way to increase the efficiency of their operations. Scott Page (2007) says: "Diversity matters because it can increase the bottom line by introducing more perspectives, heuristics, interpretations, and predictive models. Diverse cognitive tools can, in turn, improve an organization's ability to solve problems and make accurate predictions." He has developed a theorem (Page 2011) to capture the idea that a group will be better off if a new member is somehow different than the existing members, while the value of the contribution of each new member of the same type will keep diminishing with each addition. The theorem is applicable under the assumption of the law of diminishing returns, a standard assumption in Utility Theory and in the assumption of the absence of interactions (interactions can be destructive). This abstract theorem is meant to be a baseline – and to provide motivation, as it were – for studying the impact of diversity across various contexts and as a general argument for the benefit of cognitive diversity.

3. Big Physics and Epistemic Norms

In our case, the question is how to optimally organize a large laboratory when attempting to discover a fundamental particle. How many researchers should work on a project and how should they be divided? How many laboratories will most efficiently result in discovery? Can a demanding discovery be made with only one laboratory?

A recent quantitative study addressed the effect of team composition on efficiency in one of the major particle physics mega-laboratories, Fermi National Laboratory (Fermilab) (Perović et al. 2016). Efficiency was measured by determining the weighted citation counts in 12 categories. Using citation counts in this case was an accurate measure of the significance and fruitfulness of experiments because the usual troubles of bibliometric analysis were absent. First, the field is very isolated, so only the experts working in the field read and cite (or fail to cite) the papers. The citations do not come from outside the group. Furthermore papers producing the same results will not be overlooked because only a handful of labs do research on the subject; thus, physicists cannot fail to take them into account. In short, the bibliometric data pretty much reflect peer agreement on the adequacy of experiments and the importance of their results.

To analyze the data, the study used data envelopment analysis, a standard way of assessing efficiency of units in an organization (e.g. bank branches) (Cooper et al. 2011). The method identifies efficient and inefficient units based on the same inputs and outputs. In this case, inputs were the number of researchers and research teams for each experiment (a unit), and the output was the above-characterized citation counts. It turned out that all efficient experiments were comparatively small, and all inefficient experiments were comparatively large.

A preliminary conclusion of this study and the conclusion of similar studies across various fields of science (Bonaccorsi and Daraio 2005; van der Val et al. 2009; Campion et al. 1993) is that it is generally better to organize a number of small experiments and smaller teams. Moreover, at least in the cases similar to the ones studied, scientists should introduce diversity at some level, preferably very early on, when teams pick the hypotheses they will test and when they design the experiments. Small groups avoid hierarchical and atmospheric issues, as flat structures tend to be less hierarchical and provide more direct communication (Ibid.).

Do such studies have a normative value? In other words, the conclusions of the analyses may be sound but it may be impossible to change much in organizational terms, so they may only offer insight into the limitations of real scientific networks. The logic of discovery in high energy physics, for instance, pushes us to build large laboratories. The discovery of fundamental particles requires collisions at high energies, and this, so the argument goes, requires large experimental machines. Eventually, "in science, as in war, big science becomes unavoidable" (Rescher 1999).

In fact, there is no indication of sharp limits as experimental approaches to desired phenomena in fundamental physics do not have very specific, but rather general requirements to start with. We can test the Standard Model in a number of ways that do not require high energies achieved by large colliders and in much smaller laboratories.

The background physical theories (Quantum Field Theory and Quantum Electrodynamics) define possible physical phenomena within a very wide range of energies and processes. The general constraints are then defined at the level of Model Theories (in agreement with the background theories; e.g. the Standard Model or Super Symmetry – SUSY) defining the "particle signatures" (i.e. kind decays desired particles ought to produce) to be detected. The actual physical constraints affecting actual experiments are shaped only at the level of phenomenological theories which tell us, for example, what sort of intensity of gamma radiation we may expect in a certain kind of detector for presumed particle decay. This is the level of theory at which the choice of the processes to detect happens. This is where energy domains are determined and particles of interest and the expected observable outcomes of the postulated processes are defined. And these offer a wide range of direct and indirect ways of reliable and substantial detection.

In fact, detection of particles that decouple at high energies and their properties does not necessitate production of such high energies at all. Thus, we can observe cosmic rays that harbor high energy particles instead of producing them in accelerators. Detection of this sort is an unstable process and the parameters cannot be controlled the way they can in particle colliders. This led to its early sidelining in the development of High Energy Physics. Yet recent developments, due to a staggering increase in computing power, mean that the symbiosis of simulations and observation, enabled by new detecting techniques, has become a potent tool – perhaps as potent as controlled experimentation.

Similarly, astrophysics of high energy events can provide insights that even colliders cannot, as transient events at much larger energies in observational astrophysics are inaccessible to accelerators. Moreover, in the so called neutron guides, "testing basic principles of physics does not necessarily require a high-energy accelerator" (Geltenbort 2013). Neutrons are susceptible to all four basic physical forces. As they are electrically neutral, they cannot be guided and bunched by electric currents as protons in hadron colliders. But in 1959, Y. B. Zel'dovich realized that if they are super-cooled, neutrons can be slowed down (to 2m/s) and bunched. A phenomenological theory explains how this can be achieved. A neutron in a gravitational field can be used to test for the existence of the fifth force, the existence of which both the Standard Model and Super Symmetry theory predict. The decay time of neutrons is also relevant, as decay is based on weak force. Finally, neutrons are composed of *u* and *d* quarks with opposing fractional charges. If the charges do not exactly coincide, the neutron is characterized by such an Electric Dipole Moment which would imply the violation of Charge Parity and Time reversal symmetries. The shortcoming of the Standard Model is that the violation of Charge Parity in weak force is insufficient to explain the dominance of matter to anti-matter. The alternatives to the Standard Model propose a small Electric Dipole Moment. These alternatives can all be tested in neutron guides.

Alternatively, we could employ a very different strategy and improve the situation by creating favourable epistemological conditions at an early stage of experimentation. For example, instead of investing a large sum in a collider with the current technology, we could conceivably invest in pioneering developments of technologies that will eventually decrease the price of experimenting at desirable energies. If we opt for a portfolio strategy of diverse investing across laboratories, our innovating in experimentation across energies may bear fruit. In fact, an example of the potential for this strategy to work in experimental particle physics is the development of the detecting techniques and accelerating tools (magnets and superconductors, above all) for linear colliders. These colliders provide a much clearer picture of particle interactions than circular ones, since they can collide leptons – particles that are not composed of more elementary parts (quarks) and, thus, do not produce a large number of background interactions – at the requisite high energies. When the decision was made to build LHC at CERN, technology for linear collisions at sufficiently high energies for testing the Higgs boson hypothesis was not available, but building a giant circular hadron collider was an achievable goal (Panoffsky 1994). In the meantime, even with a comparatively small investment, the length of the linear collider performing at sufficiently high energies was reduced by 50%, and the detecting problems these colliders initially faced were solved by investing in techniques at much lower energies (related to the magnets, criogenics, and detecting techniques).[2] But the granted lump sum (of about ten billion dollars) was already spent on building the LHC, so the linear collider that would provide much more precise insights into relevant particle interactions could not be built.

How far would physics have advanced if the money had been spent on the development of diverse technologies for linear collisions? There are, of course, political and funding reasons why physicists need to invest in technology that will produce results within a set time span with high certainty. But that only means funding agencies are enforcing a less efficient strategy of organizing experimentation, not that there are clear technological and physical limits pushing physics to develop against the epistemologically beneficial norms. Institutional inertia and the traditional way of organizing established during WWII and the Manhattan project may go against these norms as well.

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2 See all the technical data at http://www.linearcollider.org/.

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Struktura timova i projekata u velikim eksperimentima u oblasti fizike

Apstrakt

Identifikovanje optimalnih načina organizovanja istraživanja u mega laboratorijama fizike čestica je izazovan zadatak koji zahteva kombinaciju studija pojedinačnih slučajeva i pristupa formalne epistemičke analize. Studije zasnovane na podacima ukazuju na to da su projekti koje izvode manji master timovi (manji broj članova, manje pod-timova) znatno efikasniji od onih većih u različitim oblastima nauka, uključujući eksperimentalnu fiziku čestica. Manji timovi takođe prave bolje izbore projekata na kojima će raditi od većih, centralizovanih timova. Pa ipak, epistemički opravdani zahtev sa što manjim, decentralizovanim i raznolikim timova je u suprotnosti sa često naglašenom i navodno neizbežnom logikom otkrića koja prisiljava fizičare koji istražuju osnovne nivoe fizičkog sveta da izvode centralizovane eksperimente u mega-laboratorijama na veoma velikim energijama. Naš je argument, međutim, da bi taj epistemički zahtev mogao ipak biti ispunjen, jer su priroda teorijskih i fizičkih ograničenja u fizici visokih energija i tehnološke prepreke koje iz njih proizilaze iznenađujuće otvoreni.

Ključne reči: socijalna epistemologija, mreže, nauka, fizika, tehnologija, inovacija