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Authorities and Methodologies: Unmanageable Complexity?

The present essay is a response to Josep M. Rosanas' occasional paper on methodology and research in management that examines the logical and empirical foundations of knowledge. The author teaches accounting and control at IESE, the Barcelona-based school of management, sharing the same upscale residential hill with its major rival ESADE in competing for the title of the best Spanish business school. This ambitious examination lists as the keywords research methodology, logic of research, philosophy of science and management research.

Let us begin with the logic of research and the philosophy of science. The latter is the area in which the authority of science is examined and various attempts to conduct research are organized. The philosophy of science has been a very robust and popular branch of academic inquiries – at least until recently. It peaked around the Popper-Kuhn debate (in the early 1970s) and gradually lost the struggle for mainstream attention of research communities. Thomas Kuhn unwittingly contributed towards making it irrelevant by legitimizing the struggles between generational cohorts fighting for power, money and recognition in academic communities. Kuhn was no hippie, and yet he had made the flower-power countercultural movements legitimate. He barely made it into Harvard (he was repeatedly rejected by his peers, but pushed by politically minded academic managers), but once he got there, his influence soared. There are many reasons for the relatively rapid decline of the status of philosophy of science. Most of them are linked to the specialization and subdivisions of the domains of inquiry and the shift of focus from physics to molecular biology and medical genetics in attracting new talent, but Kuhn certainly provided prefabs for the postmodernist scaffoldings around the cathedral of knowledge.

The logic of research in 2012 is what philosophers of science and the researchers themselves reconstruct and prescribe as desirable and legitimate procedures aimed at the production of scientifically validated knowledge. Research methodology is the flesh around the skeleton of the logic of research, and management as in *management research*

is the area, a specific domain of inquiry, namely *management, managing*, which basically means *organizing and coordinating within complex and very different organizations, usually formal professional bureaucracies* (universities, research laboratories, joint projects, but also business corporations, family firms, symphonic orchestras, semi-legal networks and standing armies).

Rosanas clearly understands that there has been remarkable progress in academic bureaucratization and its ideological counterpart, a standardization of research methodologies. Together, bureaucratization and ideologization may threaten and undermine the expected relevance of research results by strict control of creativity along with organized blindness and learned ignorance.

He begins his essay with the remark that “a good deal of the knowledge that is useful to managers - is not in the least scientific” (“in the least” is an exaggeration). However, he then starts wondering about the origins of the decline in authority, which used to be taken for granted, and he surprises the reader with the following statement:

“Curiously, while science is overrated as a source of useful information, and useful information is overrated as regards scientific credentials, the general public has come to somewhat distrust scientists.”

What I find curious about the above remark is the word “curiously”. If the general public is exposed to a critical view of every branch of human activity – medical health care, religious spirituality, or scientific knowledge production – and if the general public comes to believe that science is overrated as a supplier of relevant (useful) information, then why would it be so curious that the authority of scientists is being questioned? If, as Rosanas suggests, truly relevant (useful) information is less dependent on scientific credentials than we originally were led to believe, then why wonder that the general public thinks less highly of scientific credentials and is more likely to check them rather than blindly obey and accept?

Thus I would have replaced the word “Curiously” with “No wonder that”. But this is a rhetorical point. The arguments I have in mind are not sheer rhetoric. The decline of scientific authority is both a decline in authority in general and a decline in status of scientificity. Rosanas mentions two reasons for this decline of the authority of scientific enterprise in the eyes of the beholders who pay taxes and finance the salaries of most academic researchers.

The first is historical. The world-wide protests of the ‘60s and ‘70s are explicitly mentioned as the direct cause for the decline of social authority of experts. Rosanas does not go deeper into this argument, which basically rests on a critical distrust towards the scientific-military-cultural mobilization of the Cold War establishment. Distrust was curbed when aggressive Soviet policies justified defense of the “free world”. Curbing became difficult after the decline of a direct Soviet nuclear threat. The Cuban missile crisis

was won by Kennedy and Soviet nuclear missiles were not installed in Havana. Seven years later, US astronauts landed on the moon and the Russians have been unable to duplicate this achievement.

The second is political. Rosanas mentions the failure of the US think tanks of experts to predict the disastrous course of the war in Vietnam. It is true that “the best and the brightest” minds of McNamara, fresh from MIT, Harvard, Stanford and CalTech, could not foresee the willingness of the North Vietnamese communist party to sacrifice hundreds of thousands of soldiers and civilians, nor the power of the US media to lower domestic support for the military actions, thus paving the way for the communist conquest of South Vietnam. Nobody could.

The third is social. He blames the increasing complexity of our societies for the growing difficulty in distinguishing between “real” authorities and “self-proclaimed experts” that are encountered “in today’s more sophisticated professions”.

I am not certain if I understand this last point very well: what exactly is meant by the growth of sophistication in professionalization? The way I understand it, contemporary professions are more sophisticated than earlier ones because they are increasingly more standardized and managed by academic bureaucracies legitimized by merit, hence by meritocracies. Huge systems of ranking and accreditation, of certifying and diploma-granting, of recognition and acknowledgment are built into the very fabric of the daily life of members of research communities. Therefore the example of Sowell’s milkmaid or milkman does not sound very convincing to me. The problem is not with the supposedly direct proof of the pudding in the eating and indirect proof of the design and plan to milk in future within a large organization. The difference between the empirical test of knowledge of a milkmaid or of a marketing manager is not a difference of an individual result of a single act of direct participant observation as opposed to a complex and collective effort. In evaluating a milkmaid, neither are we driving to the farm; we rely on statistics, on delivery quotas and dates, on lab tests of milk quality, on brand popularity if she is selling her milk, etc. In both cases we are evaluating indirect numbers computed and prepared by many sophisticated intermediaries in a complex and collective effort. Methodology comes in as a constitutional tribunal of cognitive justice in both cases. And in both cases, this methodology is bound to reflect our reflections on the way things are (ontology), the way we get to know them (epistemology) and the way we decide to prefer some over others (axiology, both as ethics and as aesthetics).

The philosophy of science does not have to start with Aristotle, but it may well do so. Aristotle did notice that we have to start with some theoretical assumptions and then to follow them with rigorous empirical research. It is a long way from an ontology visualizing the world as a storage hall of objects linked by various relations to one another (and to ourselves) to the ontology of, say, “eventism”, visualizing everything as a huge stream of multidimensional events (to use Whitehead’s brave attempt to draw conclusions from quantum physics in general and relativity theory in particular). But ontology is usually

the least of the methodological worries of academic researchers. These concerns are usually solidly entrenched on two fronts: epistemological and axiological. Rosanas visits the epistemological trenches. He talks of scientific theories, comparing them to maps (quoting Polanyi and Christenson) and allows for pragmatic omissions (no need to reproduce more than needed for navigation at hand). He talks of the need to operate with abstract concepts rather than concrete ones, for reasons of efficiency and generalizability. True enough – but not sophisticated enough. The main reason we are talking about actionable knowledge as if it was a map of reality is not that Polanyi or anybody else used to do so, but because we have already industrialized the transformation of information into abstract maps with the help of computers and telecommunications. The battles in information space (if I may use the term coined by the late Max Boisot) are about search engines, networking rules and communicative socialization of the new generations. Hume and Kant and Russell may be quoted at length, but the quotes and respect for their skeptical attempts to build the sound scaffolding for all future builders of knowledge does not change the fact that today we are more into an inter-subjective mood of post-Popperians (e viva Wikipedia) and post-Kuhnians (e viva tenured tracks in academic competition) than into either the tradition of pure skepticism (Hume) or solid stage design for any drama a knowledge pursuing mind might write (Kant).

I agree, however, with my Spanish colleague, that a sophisticated view of knowledge production (say, a non-naïve philosophy of science) may presuppose an attempt to “discover the complete set of possible states and their logical relations, that is, the logical structure of the set of possible worlds”. May, but does not have to. What emerges are often non-hierarchic and extremely transparent networks, projects and – yes – structures. Wikileaks are anti-hierarchic and to hell with complete sets of possible states. Of course, I cannot but agree with him that in order to apply epistemological intuitions to management research one has to “become more direct and personal”, though I do not share his belief that this necessarily makes me less academic. Margaret Archer did not become less academic when dealing with self-reflection and complex embedding and shifts of actionable knowledge of self-transforming agents. Why would it have to be so? Being personal and self-reflective and building my critical self-reflection into the very fabric of my methodological awareness, I do not deviate from the course of a true professional in the academic research community. Au contraire, I should stay on this side of the Pyrenees.

Rosanas expresses a belief that we should, as members of a research community in good standing, “keep abreast of what everybody else thinks”. This is true; we should know what our peers are doing when trying to answer the questions, which trouble and perplex us as well. However, it is not enough to simply write down what we think in a structured manner and let it float through the communication and information space. The problem is that our communication and information space becomes a very crowded and very subdividing universe full of black holes and feudal fiefs. Delivering cumulative knowledge, which, according to Koza and Thoenig (whom Rosanas quotes approvingly),

is what the European researchers viewed from the US perspective are unable to do, depends crucially on organizing and managing research, implementation, marketization and streamlining of public-private-creative networks and projects (Richard Whitley had once tried to see if he can come up with national recipes for a successful mix of basic research, applied projects and market products and services). Perhaps a solution will emerge from our recent experiences with very large scale giga-networks and mega-complex projects like growing and transforming bunches of experiments linked to the large hadron collider run by CERN deep under Geneva, Switzerland.

The first empirical studies of these research processes are already emerging (one of them co-edited and co-authored by the late Max Boisot) and we should pay close attention to them. The real test of the European Union is not only the management of a fiscal crisis and the bailing out of Greece or prevention of a bankruptcy of Spain and Italy. It depends also, and perhaps much more so in the longer run, on the organizing and managing skills of our complex and heterogeneous elites enlisting researchers, managers, politicians, PR and media specialists and other professionals, who can persuade tax-paying citizens that in spite of diminishing authority, it is still worth our while to finance basic research in hope that someday more GPS navigation, mobile phones and tablet computers will make our life easier, better, more in line with the ideals of a creative enrichment of socialization patterns. Let's hope so. In fact, some hopeful signs can already be detected. The extreme populism of the new right wing parties has not scored a sustainable success in re-shaping the European public opinion. The attempts to understand new forms of political participation, which might change the unfortunate image of the EU integration as a plot of the elites against the masses, are already acquiring a form of a new activism against exclusion from the tele-communicational clouds of the web (ACTA) or of the emergent concepts of "sustainable technological citizenship" (cf. Valkenburg, 2012, Sassen, 2012). Academic professionals, who had often in the past evolved into the first-rate public intellectuals (Erasmus, Voltaire, Chomsky) do have their chance. Especially when they write that: "euroscepticism is part of the democratization of the EU. It results from the uncertainty about the quality and scope of the EU polity and the fuzziness of the underlying demos" (de Wilde, Trenz, 2012).

Academic science can progress in making us understand the world better and in bending it to serve our needs better by a managed reinventing of itself. Rosanas calls for this reinvention, when he urges us, academic professionals, to keep abreast of what non-academics and non-professionals think. Steve Jobs had once said that he is not interested in marketing research: he wants to invent something that all people will want to have, although they still have no idea that this is what they will want in future (and so asking them about it by marketing researchers makes no sense). Same holds true with respect to our new forms of managing our political affairs (EU as polity) and of the emergent forms of sustainable solidarities (demos, or the citizens of the EU member states). A democratic community emerges and acquires a voice due to democratization,

which also has which need to be reinvented all the time. Reinventing itself, democratic communities make choices and decide, often on the basis of insufficient evidence, who is to be followed as an expert and who is to be distrusted as a non-reliable manipulator. Academic professionals are not the best and the brightest all the time, but we can extend organized skepticism of our cold laboratories and calculated polemics to the hotbeds of pent up emotions, heated sentiments and baroque rhetoric of the real life politics. Rigor and relevance in an approximate balance. Yes, we can. Moreover, we should.

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