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CONTROVERSIAL MANAGEMENT THEORIES: IMPLICATIONS FOR TEACHING AND RESEARCH

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Abstract

In today's world of management research and teaching, we often behave as if there were a widely accepted paradigm and that we only had to discuss minor points, or we could even ignore them. I claim this is not true, and that there are at least five problem areas of disagreement or controversy: (i) methodology, (ii) ideology, (iii) relationship with practice, (iv) incentive systems for academics, and (v) narrow specialization.

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CONTROVERSIAL MANAGEMENT THEORIES: IMPLICATIONS FOR TEACHING AND RESEARCH[1]

The idea of this session originates from a private conversation between Eduard Bonet and myself after the Grenoble meeting, where we were commenting and beginning to realize that in EDAMBA, like in many other organizations related to the world of management and business administration, we typically behave as if there were no big controversies, i.e., as if there were one accepted paradigm that everybody shared; and therefore all we have to discuss are perhaps some minor adjustments (in the case of EDAMBA) to our doctoral programs so that they are more effective at transmitting this paradigm.

This does not happen to be true. In the last few years, we have heard loud voices of dissent, mainly coming from some of the "old generation of academics". Interestingly, the opposite seems to be happening of what often happens in the world of arts, sciences or any other branch of knowledge: typically, it is the younger generation that criticizes the status quo; in our world of management, it seems to be the older generation that does not like what it sees and manifests its disagreement, which is quite harsh at times. Pfeffer, Mintzberg, Donaldson and the late Sumantra Ghoshal, just to name a few, have been harsh critics of what is going on in the world of teaching and research, i.e., our world (see, e.g., Donaldson, 1995, 2002; Ferraro, Pfeffer and Sutton, 2005; Ghoshal, 2005; Ghoshal, Bartlett and Moran, 1999; Ghoshal and Moran, 1996; Mintzberg, 1996, 2001; Mintzberg, Simons and Basu, 2002; Pfeffer and Fong, 2002).

I would now like to summarize the basic points of disagreement and the basic problem areas that we have to solve. I believe there is (i) a methodological problem, (ii) an ideological problem, (iii) a "relevance" problem (I use quotes because, as I will argue, I don't quite like the word), (iv) an incentive-system problem for academics, and (v) a problem of narrow specialization. Let me briefly describe each one.

(I) The methodological problem

There is today in most fields of management (not all, of course, business ethics being one of the exceptions for obvious reasons) an almost blind acceptance of a form of empiricism that is in general rather naïve, and consists of the belief that "scientific" and "empirical" are practically

synonymous. The natural sciences are often proposed as the natural model to follow, and there is widespread belief that the natural sciences are entirely empirical. To begin with, this is not so. Not too long ago, in many universities there were courses on what was called "Rational Mechanics", to emphasize that reason alone was sufficient to fully grasp the subject: there was no need for any empirical reference. It is even possible that today there are places where the name still exists, but in general it has all but disappeared, because of course "rational" mechanics has an empirical basis, like any other science, and the name of the subject has been changed to "Newtonian Mechanics" or a similar expression. But for many years (indeed, for centuries), reason alone was considered to be the essential solid basis of science, if not the only one. Today, we would find this idea ridiculous, but it is just as ridiculous to accept as "scientific" only those propositions that can be readily tested empirically, which, implicitly or explicitly, is the idea that has come to dominate management research of late. This idea is a spurious child of logical positivism, a now dead philosophical movement also called logical empiricism. Unfortunately, to make things worse, that child might even be missing the "logical" part of the expression. Or, in other words, "logical empiricists" knew better. This is more or less what has happened in my own field with what is called "positive accounting theory" (or "PAT"), just to cite one example.

All these problems don't have easy solutions, but we shouldn't behave as if they didn't exist. On the contrary, we should discuss them and find the right place for each research method (if it has one!). Controversies are good. And we should be trained in Methodology (with a capital "M") as something different from mere research methods (lower case "m"), which nowadays have come to mean essentially regression and structural equations.

It is not possible to discuss all this in detail here. I attempted to go a little bit further a few years ago elsewhere, in an informal paper for a faculty meeting at IESE (Rosanas, 2006). And I believe EDAMBA, as an organization, should go further in the study of such issues by other means (debates, possibly conferences, or even a journal). But I would like to stress two points before I proceed to my next problem. First, management seems to be a discipline where it is most appropriate to apply some of the methodological ideas of Friedrich von Hayek, both in his classic *The counter-revolution of science* and in his Nobel Prize Lecture, "The pretence of knowledge". Hayek's basic point is that social sciences often try to imitate the *methods* of natural science. According to him, this is wrong, because social sciences deal with phenomena of organized complexity where quantitative methods fall short of the variables they should measure. Instead, they should imitate the *spirit* of the natural sciences, which is perfectly valid in a social-science context.

Second, the Ghoshal (2005) idea (taken originally from Elster, 1983) that there are three kinds of explanations (causal, functional, and intentional), and that while causal and functional explanations may sometimes be useful in management, intentional explanations are crucial if we want to attempt to understand complex human phenomena in their entirety.

(II) The ideological problem

Economics has come to be possibly the most influential discipline in management thinking. And "mainstream economics has, in the main, always worked on the assumption of *Homo Economicus*, a model of people as rational self-interest maximizers. (…) Even practitioners of sociology and psychology (…) have increasingly adopted the notion of behavior as being self-seeking as a foundational assumption" (Ghoshal, 2005).

The hypothesis of self-interest used in utility maximization may be rather innocuous if the goal of the theory is to show the optimality of the price system. But when it is used to analyze organizational problems, as in transaction-cost economics and agency theory, it may often become perverse. But not always: sometimes, the hypothesis is only the beginning, just as in Newtonian mechanics you study first the frictionless case, and later on you add friction. The problem in social sciences, and mainly in management, is that by making certain hypotheses, you may make them self-fulfilling (Ghoshal and Moran, 1996; Ferraro, Pfeffer and Sutton, 2005).

Again, according to Ghoshal, the pessimistic model of people as purely self-interested beings comes from the ideology Milton Friedman calls "liberalism". According to him, the root of such an ideology lies in two convictions. First, that the ethical problem should be left to the individual and thus has no place in social theory; and second, that the liberal way of looking at social-organization problems is "as much a negative problem of preventing bad people from doing harm as of enabling good people to do good". "And given that much of the social science until then had focused on the second part of the problem, the agenda of social theorists thereon, that is, for the last 40 years has focused on the first part, that is, the 'negative problem'. Hence the pessimistic, the ideology-based gloomy vision" (Ghoshal, 2005). I think it was Seneca who said that there is only one thing that is more foolish than trusting everybody: not trusting anybody. Which is just what the gloomy vision does.

The gloomy vision is also at the origin of the expression "lean and mean", which seems to be many people's idea of how organizations should be run (Mintzberg, Simons and Basu, 2002). Fear as the basis of organizational "commitment"? Well, we have seen that being argued in many management publications (see, for instance, "Leadership and the Fear Factor", *Crosstalk*, *MIT Sloan Management Review*, Winter 2004). Fear instead of trust as a basis for organization. Is this the kind of organization where we want to work or the kind we would like our children to join?

(III) The relationship-to-practice problem

This problem is often referred to as the "relevance" problem. I hate that word, because it is typically contrasted with "rigor" as if one had to sacrifice a little bit of one to get a little bit of the other. This is not so: rigor (related to the methodological problem) consists of having a reasonable assurance that what is being affirmed is true; and, thus, nothing that has no rigor can have any relevance for any practical problem.

The real problem is that research, no matter how rigorous and how relevant it may be for *some* practical problems, is often perceived to be far, far removed from the interests of practitioners, and practitioners do not understand it all; and yet they often turn for guidance to books and journals with no rigor at all, written by "gurus" that may have done a lot of superstitious learning in their practical experience.[2] Then academics typically think that there is merely an implementation and pedagogical problem, where academics should have an easier job of

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[2] March and Olsen call superstitious learning the kind of learning that attributes the wrong causes to actual events. For instance, the belief that success in a given firm comes from superb management or a specific management practice, when in fact the favorable winds of the economy have been the crucial factor. (March, J. and Olsen, J., "The Uncertainty of the Past: Organizational Learning under Ambiguity", *European Journal of Political Research*, **3**, 1975, pp. 147-171).

communicating their wonderful ideas to practitioners; while practitioners believe academics are up in the clouds. A few years ago, I overheard a practitioner asking an academic friend, "How many years ahead of us are you in solving problems we don't have yet?"

It is true that there are often conceptual problems that need to be solved in theory because they are crucial steps toward solving the problems practitioners actually have, while they might seem to be unimportant to them. This has happened in all sciences. It is true that some academics have never seen a firm, except at a distance, and thus they interpret as real problems situations that are in fact conceptual problems of no practical interest. But what this shows is precisely the need for dialogue and contact (which is not easy, and that is precisely why it is a problem that does not have a solution yet), not the generally accepted belief that one side is right and the other wrong. And I fear that the gap between those two beliefs (one on each side) is widening. Witness the lists of best-selling management books. Few of them have been written by academics, or have a minimum of rigor that goes beyond simple rules of thumb.

That is parallel to what often happens in MBA programs. Instructors have no idea of what the problems in the real world are, because they have been trained (not too well, sometimes, as it turns out) in Econometrics rather than in their own disciplines. Besides, academics have developed (and, of course, teach) what Donaldson (2002) qualifies as anti-management theories, as the antithesis of something helpful to decision-makers. Employers complain. Then deans try to have the schools turn to "practicalities" and, all too often, this consists of folklore and anecdotes with no rigor, or in teaching students mere information that turns out to be obsolete in just a few years (or even months!).

(IV) The incentive problem

Those of us who are (or have been) in the field of management control know pretty well what happens with strong incentive systems and the problems they typically induce. It would therefore be surprising if this didn't happen in Academia.

Following in the footsteps of C. S. Peirce, S. Haack (1998) has summarized the problem quite well. On the one hand, we have to distinguish between genuine inquiry and pseudo-inquiry. "Genuine inquiry seeks the truth with respect to some question or topic; pseudo-inquiry seeks to make a case for the truth of some proposition or propositions determined in advance. (...) But there are two kinds of pseudo-inquirers. The sham reasoner is not primarily concerned to find out how things really are, but to make a case for some immovably held preconceived belief. The fake reasoner is not primarily concerned to find out how things really are, but to advance himself by making a case for some proposition to the truth-value of which he is indifferent." Peirce never used the expression "fake reasoning", but his "remarks about the corrupting effect of 'vanity'" indicate that he was aware of the dangers of fake reasoning as well as of the sham.

When so many incentives are based on the number of articles (or even the number of pages) published, one should not be surprised to find so many "fake reasoners". A phenomenon, on the other hand, closely related with the methodological problem, whereas sham reasoning is more related perhaps with the ideological problem. In any case, the type of incentive systems that operate on management researchers really favors both. In our Doctoral programs, the questions we should be asking are: "Do we train genuine reasoners, fake reasoners or sham reasoners? Do we teach them right, but, then tell them to become sham reasoners to get a job?". Isn't it true that, very often, an author includes a paragraph, or even an important part of the article, "just for the referees"?

One of the most cited articles in the management literature is probably Steven Kerr's "On the Folly of Rewarding A While Hoping for B", originally published in *Academy of Management Journal* in 1975. Twenty years lately, the article was published again in *Academy of Management Executive* because of the big impact it had had (Kerr, 1995), with the comment that a whole empirical section had been taken out because it had not been in the original article. It had only been added when someone insisted in the refereeing process that something empirical was necessary if the article was to be published. Clearly, the "empirical part" did not add anything to the article, and the reason it was successful was the basic conceptual issue it addressed: how the incentive systems are very often wrong. One of his examples, of course, was Academia.

(V) The narrow specialization problem

The first author who wrote about management in an organized way, French engineer Henri Fayol, said almost a century ago that managers should have *culture générale.* In the classical Urwick edition of the work, this expression was translated as "general education", but I think *culture générale* has a broader meaning. It means knowing things that are not directly related to the practice of your job, that go beyond what Fayol intended to do in his study of Administration, just in case you need them to solve complex problems: in such situations, you never know exactly what you need to know. What would Fayol say today of our Ph.D.s in Finance who are completely ignorant about strategy or organization, or of our Ph.D.s in Organizational Behavior who can't solve an elementary problem in finance or cost accounting? In real-world management, problems don't come with a label on them, and if in order to control costs you have to create a serious organizational problem, it might be better for the firm not to control costs. But then how can instructors teach students to look at real-world problems in a holistic way if the instructors themselves don't know the basics of the neighboring disciplines, let alone *culture générale*? It is not too surprising that Pfeffer and Fong (2002) say that "a large body of evidence suggests that the curriculum taught in business schools has only a small relationship to what is important to succeed in business". This, combined with their assertion that "…grade inflation is pervasive in American higher education (…) Every student who wants to (and avoids financial and emotional distress) will graduate", gives us a picture of the results of teaching practice. Which, for most instructors, began with their doctoral program…

Just one additional comment: many doctoral programs nowadays favor the narrow specialization process from the beginning so that their students can churn out publishable papers (specialized, of course) as early as possible for the job market. And this closes a vicious circle.

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