

Management with Chinese Characteristics

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Abstract: Millenia of writings on organizational issues and the longest running bureaucracy in the world notwithstanding, China's rise as a management research magnet has been associated more with the country's economic ascent than with the opportunities it accords for conceptual and theoretical development. Starting with my scholarly journey as "an outsider looking in", I review the evolution of management research on China, as it transformed from the curious to the legitimate, yet still suffering forced accommodation within extant theories and methods. Rejecting both universalism and an indigenous Chinese model of management, I recommend a hybrid approach where China's unique characteristics are leveraged but used as a stepping stone towards the eventual development of a context variant approach to organization theory and management.

Keywords: Chinese management; indigenous research; scholarly universalism

Introduction: A Scholarly Journey

To illuminate to the reader the lens through which I am looking at China, it is useful to start with the background. Such disclosure is common among scholars interested in culture -- I count myself as one—which is based on the belief that we all have biases, acquired through our upbringing and life path, which color our view of the world. I am often asked how I have become engaged with China at a time when most Westerners have had little interest in the country. I suppose it had to do with the desire to look at what then seemed a "black hole", unknown yet strangely enticing. When I enrolled in what was then the Department of Chinese and Japanese Studies (since renamed the Department of East-Asian Studies) at the Hebrew University of Jerusalem in 1972, China was a country with a stellar past, a somber present and a cloudy future. So much so that the University required us to double-major, believing that our China education was unlikely to ever lead to gainful employment. I chose sociology as my second major, as I saw it a useful companion to my interests in China and in its organizations. Upon graduation, absent graduate studies on China in Israel at the time, I pursued a Master in Sociology at the Hebrew University, focusing on organizations, while also taking business school classes. Concomitantly, I started working as a researcher at the Truman Institute, where I have had the opportunity to work with the late S.N. Eisenstadt, learning the craft and lens of comparative analysis, something that continue to serve me to this day.

Having completed my Master, I debated whether to pursue doctoral studies in sociology, business, or Chinese Studies, settling on sociology at Columbia University. At that time, rigorous research on organizations was done in sociology departments rather than in business schools, whereas Chinese Studies departments and or institutes offered regional depth but no management

expertise. Still, China remained a main draw. I obtained permission to qualify in the sociology of China, the first at Columbia to do so. Writing a dissertation on the Chinese bureaucracy, with committee members drawn from Sociology, the East-Asian Institute, and the graduate school of business, I was soon to learn the promise but also the challenges of interdisciplinary research. My advisors did not speak the same language, some opining that scholars outside their field were ill equipped to advise on the subject matter. Yet, I have learned the value of using multiple lenses to explore different facets of the same phenomenon, an epiphany that would follow me in the years to come but which often stood in the way of “mainstream” writing and publication within a disciplinary organized framework.

Upon graduation, my expectations for a rich and rewarding academic life were quickly challenged. The year was 1981, and sociology departments were becoming, institutionally speaking, endangered species. Business schools were flourishing, but China was the last thing on their mind. The then isolated nation accounted for mere three percent of global GDP, and, although reforms have already been launched, there were very few if any signs that the country would one day become an economic heavyweight. Finally landing a university position, I have had my China related manuscripts returned unread, typically with an appended apologetic note from the editor that the work was irrelevant for his or her readership. Area study publications were more forthcoming, but unfortunately did not count for much in the business school rigid ‘publish or perish’ world. I vividly recall sharing what I thought were good news with my dean, that my book on Management in China was reviewed by the *China Quarterly*, the leading journal in Chinese Studies, only to face a dismissive response. To survive, I expanded into other areas, a diversion which proved valuable in expanding both my skills and my horizons and helped me to place China in a broader context, to my eventual scholarly benefit.

Still, China beckoned. So in 1987-1988, I disregarded the advice of my academic superiors to spend a sabbatical in the United States, optint to go to the Chinese Univerity of Hong Kong. When there, I have had the opportunity to travel to the Mainland, visiting universities, government offices, and firms, something I continue to do virtually every year since. Following several years of joint appointments in the United States and Israel, I settled in Columbus, Ohio, as the first Ford Motor Company Chair in Global Business Management and a Professor of Management and Human Resources at the Fisher College of Business, the Ohio State University, where I am also a member of the Center for Asian Studies and the China Institute. At Ohio State, I am part of a small group of international business scholars, set within the department of Management and Human Resources.

“Chinese Management”: The Evolution of a Concept

When I first taught a class on Chinese management at the University of Hawaii in 1987, it did not immediately occur to me even what the title implied. At that time, China still did not register on the screen of Western management scholars; if it did, it was mostly as a curiosity, an opportunity to look at something different and almost bizarre. Still, driven by the reality of an emerging business opportunity, practioner interest has started to show up. I recall hosting in class the Human Resource Manager of a major US multinational establishing its first joint venture in China, who was seeking prospective expatriate assignees among the students. Yet, from an academic perspective, I was not clear what the scope of such a course should be. Was

the class about how the Chinese manage? Was it to constitute a comparative analysis of US and Chinese management systems, somewhat in the same vein as the then popular classes on the United States and Japan? Back then, Japan was the utopian darling of American management scholars and practitioners, with bestsellers recommending partial or wholesale US adoption of the venerable Japanese model. Then again, no one in the United States thought then (and very few think now) of China as a model to be emulated. Should the class focus on the nascent foreign direct investment in the country, which US multinational executives might have appreciated? Or, should it be an occasion to show the students that management had to be adapted to its contextual variants? With a template lacking, my class was likely a mix of all the above, though I always felt drawn to the last question in particular, as I thought this could be the “linking pin” connecting my China interest with my broader interest in organization and management.

Obviously, a major reason why I was “flying blind” is that there was not much to rely on. In early years of the second half of the twentieth century, when the modern field of management was coming of age, China, and for that matter, most other “foreign” (i.e., non-US) countries, have hardly mattered. The field as a whole was deeply rooted in the United States, its culture, history, economy, corporate evolution, and major dilemmas, whether governance challenges or the “proper” organizational structure. Such anchoring was largely implicit: With very few exceptions, such as the writings of Max Weber, most organization behavior and management classics talked about “people” and “organizations” in general but were actually talking about the United States. Even when teaching Weber, who was, among other things, a major comparative scholar, his extensive writings on different civilizations, China among them, were not even mentioned. What was followed extensively were Weber’s supposedly universal models, such as those of bureaucracy, which were conveniently extracted from the environmental context in which they have explicitly varied. Incidentally, and as a result of that unfortunate legacy, this is still the way Weber is taught in the United States, and by extension, in most countries today.

Decades later, I regret to say that the globalization narrative notwithstanding, this is very much still the case. While the coverage of other countries has vastly expanded, the United States is still the source of almost all management theories, the dominant ground for field research, and, in the more limited space of comparative studies, the constant and overwhelming referent. In other words, if a different country is studied, it is more often than not as a comparison point to the United States (which, as I will explain later, makes China a newly popular destination). To borrow from the political realm, the United States is, in a sense, the “core country” of the field. Thus, while most of us may hardly notice, the “audience” we write for was, and very much remains, the corporate executive at the helm of a US company, or, perhaps more accurately, the American researcher, as the institutions of the profession have by and large admitted their failure to connect with the real world, numerous pleas towards “relevance” notwithstanding.

The situation is not different in other functional areas of business, be they finance, operations, accounting, marketing, or logistics, or, to a somewhat lesser degree, in the applicable social sciences, e.g., economics, sociology, and political science. When they appear at all, other countries are still assessed by how similar or dissimilar they are to the US, with the latter now often preferred as exotic examples. This has not always been the case however. Up until the 1990s, if a Western scholar did develop a rare interest in a foreign country, China would have been an unlikely choice. The country was very difficult to access, with political, cultural, and

linguistic obstacles abound. Chinese scholars, on their part, were only beginning to reconnect with the rest of the world. The combined result was that China, the most populous country in the world, hardly showed up in the management literature. This was the case even in the area presumably most motivated to do such research, namely international business. In a review of the leading journal in the field, the *Journal of International Business Studies (JIBS)*, Thomas, Shenkar and Clarke (1994) found a single China article, or less than one percent of the total, published in the 1970-1977 period. This rose to 2 percents in 1978-1985, still under one percent of the total. Only in the 1986-1993 period, when China began to emerge as a global exporter and a substantial foreign direct (FDI) investment target, did the number rise to nine articles, still just under 4 percent. Not a single mainland China based author appears during the entire 1970-1993 period, though the last two periods (1978-1993) feature two Hong Kong- and three Taiwan-based authors. The situation was roughly the same for *Management International Review* and marginally better for the *Columbia Journal of World Business* (since renamed) (Thomas et al., 1994).

Fast forward several decades: China has now become among the most popular research destinations, in line with what Thomas et al. (1994) would have predicted when they zeroed in on a country's trade relationship with the United States as the ultimate predictor of the probability of its inclusion in the journal. The authorship picture has also changed and quite dramatically. In 2016, Cantwell, Piepenbrink, Shukla and Vo reviewed *JIBS* authorship from 1972 to 2014, reporting not less than 77 authors with Chinese affiliations (Hong Kong appears to be included in the total but Taiwan was listed separately, which would have added 10 more authors) for the 1995-2004 period, for a combined total of just about 10 percent, roughly the same as in the subsequent 2005-2014 period. With a PhD from abroad increasingly the norm, China led in the number of returnee authors (57), implying a foreign (mostly Western) education. China also leads in the number of authors who are presently based elsewhere around the world, mostly in the United States and Europe. These scholars have been instrumental in "putting China on the map", enhancing its profile as a major research target, and, without a doubt, have greatly increased our knowledge of management in China., but it came with a price.

Foreign education has taught the returnees, as well as Chinese scholars who remained in the West how to converse with the broader research community, a good thing; but it also colored the lenses with which they look at the world, to the detriment of us all. For I would argue, perhaps controversially, that in and of themselves, these seemingly encouraging developments did *not* fundamentally change the management field in a manner that would make China, or for that matter any other country, a meaningful input into our management codex, that is, this did not change the manner in which we look at management phenomena nor the concepts and assumptions behind it. Yes, we now have "management in China", that is, management knowledge that is applied in China, perhaps even tested in China, but we still lack "management with Chinese characteristics" customized to address management in the country, let alone extended, in reach and depth, into the world. Worse, in some ways, as I will illustrate later, and mostly unrelated to China itself, the situation is getting worse rather than better.

In addition to foreign education, there are several other reasons for the current state of affairs. Globalization narrative notwithstanding, business PhD programs in the US, Europe and increasingly in the rest of the world, are or in the process of becoming, fairly identical, in essence a replica of each other. Schools are teaching, and their scholars are studying, supposedly universal principles despite the fact that they have not been globally vetted, let alone validated, and are hardly if at all adapted to a particular business environment. PhD programs in China, at least at its elite universities, are no exception, as they eagerly pursue international accreditation, in the process shedding their own identity. An important study of the teaching case studies used in Chinese business schools (Liang and Lin, 2008) revealed, alarmingly, that over time, these cases have shed their Chinese characteristics and have become indistinguishable from Western case studies: As the authors note, “the holistic approach to management, prevalent in early Chinese MBA cases and typical of traditional Chinese culture, has largely disappeared and Chinese cases now exhibit many of the same weaknesses and deficiencies that have been documented in Harvard Business School cases” (2008, p. 603). Gone are, for example, the intricate network of relationships underlying a business transaction and the social institutions that contextualize them, in come “rational” decision making and detached strategic “thinking”. In other words, such a case may have a Chinese firm at its center, but the way it is narrated, staged, discussed and analyzed, is anything but.

Chinese Exceptionalism

The term “exceptionalism” has been used in political science and history, among other fields, to denote the United States’ unique historical path which presumably sets it apart of virtually all other nations, with connotations ranging from global destiny to “light upon the nations” and anything in between. Applied to our subject matter, Chinese exceptionalism would imply that thanks to a ecological conditions as well as a long and unique history and institutions forged under consecutive imperial and Communism tutelage, China developed a special culture and its “market socialist” economy, like other cultural, social, political and ideological elements, can be imitated but not replicated. If one accepts this premise (for China or for any other nation), then a call for indigenous management knowledge, in particular a theory base, may well be in order. However, if management in and by China can be accommodated within existing theories, models and frameworks, even with outlying scores, then management in China is nothing more than an interesting application ground for the existing body of knowledge, inclusive of its theories and methods. Without a doubt, even if implicit, the latter is the prevailing view in the management field. Has it really been put to the test? The answer is clearly no.

It is important to remind the reader at this point that this essay maybe China-centric but is by no means only about China. Broadly speaking, a failure to use a lens that can properly capture context will result not only in lack of validity but also in a lost opportunity, to understand the role played by context variables. Ignore institutions, as Harvard cases typically do (Chetkovich and Kirp, 2001), and you will miss the chance to analyze and comprehend their real impact. Look at institutions exclusively the way promoted by Douglas North (1995), and you have largely missed on culture and other “informal” institutions within which economic institutions are intricately embedded. Limit your disciplinary lens to economics, and you have chosen to build on the most insular of all the social sciences (Fourcade, Ollion and Algan, 2014) while missing on critically important sociological, psychological, and political science perspectives,

among others. Use the United States as the perennial reference point, and you will end up labelling Chinese institutions “weak” due to ambiguous treatment of property rights, neglecting the overwhelming and continuing role of those institutions in the local economy, now spreading its impact into the world.

Truly, challenges to the prevailing view of institutions in connection to China have been posited (e.g., Nee, 1992; Boisot and Child, 1988, 1996), but the irony of the matter was often lost on the reader. For example, the title of Nee’s oft cited article on the Chinese reforms denotes a “mixed” economy, but a mixture of what, exactly? If you want to oversimplify, China was then a hybrid of market and socialist systems, but are these really the right goal posts within which it should be placed? Nor was there much follow-up discussion of this dilemma in conceptual and theoretical terms, except, perhaps, by Boisot and Child (1996). Incidentally, the Boisot and Child paper has coined the term “network capitalism”, later cited as one of only three (!) Chinese indigenous concepts to have made their way into the mainstream management literature. Here too, the question is whether such categories as “capitalism” and “network” have similar meanings and repercussions in the Chinese context as they do in the context in which they were developed, in other words, are we right to use a universal scale or are we mixing apples and oranges as a matter of convenience? If we fail to even ponder the question, we not only limit our ability to understand the functioning of management in China, but, more generally, we forfeit an opportunity to properly capture variations in institutional systems, or, for that matter, identify other context variables and dynamics, so we can better comprehend their interplay with management phenomena.

The Relativity of Organizational Theories

Every “publish or perish” scholar knows that theory development is the holy grail of the so called “A journals” which will pave your way for success in a research intensive institution. Yet only a few scholars stop to ask whether the theories we use are truly universal or may be biased. The only group which seems to be preoccupied with the issue are cross-cultural researchers, who, as the name suggests, are primarily occupied with the role of culture rather than that of other contextual variables. Furthermore, an overwhelming majority of those scholars hail from the micro, behavioral side, rather than the macro side or organizational analysis, where institutions reign and where cultural differences have been reduced to the oversimplistic “cultural distance” formula. Yet theories are not only about “publish or perish” – they are fundamental blocks of perceptions, assumptions, and thought that guide our research. Should they not be universal, we may as well go back to the drawing board and rethink how we approach management on its various phenomena. So far, we do not know that they are. Efforts to apply Western-developed theories in other parts of the world seem not to be lacking, but hardly any of those efforts goes beyond a cursory examination of supposed “validity” based on a superficial reading of results that may represent different things in different contexts.

Among the authors raising the spectre of the cultural relativity of organizational theories (e.g., Hofstede, 1993), observations have been mostly anecdotal, with a systematic analysis of the possibly culturally biased variations still sorely lacking. Hofstede (1996) piece represents one of the few attempts to unearth the roots of several specific economic and organizational theories and the assumptions behind them, but his astute observations have hardly been built on, let alone

empirically applied in a search for systematic variations, not to mention attempts to proceed towards either context specific theories or the setting of boundaries delineating theory-specific domains.

The above situation is striking because of the reality that virtually all of the theories used in the management field originated in the West, predominantly in the United States. You could take the position that management is perfectly universal, however not only is there no evidence for that, there is ample evidence to the contrary, which is beyond the scope of this essay. If, on the other hand, you accept the variability, that is, that, in the same way a company may have a unique culture stemming from its historical path, there is a cultural and institutional path shaping the emergence of a theory, the question becomes whether theories developed in one context will apply in another. For example, given that the United States is found (with one or two exceptions) to be the most individualistic culture on earth, would it be appropriate to utilize the motivational theories that have all sprouted there in another national environment? And remember, just because a theory or a framework has been applied to a country, does not necessarily mean that this will yield a valid observation. Note, for example, Trieman's (1977) often cited observation that occupational prestige rankings were by and large similar across countries, concluding they were universally construed as a function of the division of labor, yet failing to appreciate that occupation was *not* the key designator of prestige in many societies. Indeed, only a handful of scholars are willing to admit that such adjustment is even necessary, as Adler, Cambell and Laurent (1989) bravely did, showing the scholarly community how using a Western instrument in China has yielded results so disorted to the point of being completely unusable. Since then, we have occasionally seen greater care in terms of instrument adjustment, however the fundamental questions regarding "true fit" have rarely been asked, let alone answered. For example, given Chinese penchant to focus on satisfying one's superiors, are current leadership models which dwell on relationship with one's subordinates relevant and meaningful? And given the predominance of the US perspective, shouldn't we start by asking what are the fundamental differences between Americans and Chinese, and do so from a historical social and philosophical perspective (e.g., Hsu, 1981) before we move to the managerial applications that rest on them?

"Chinese Exceptionalism" and "Indigeneous" Management Knowledge

The above discussion leads us to the next question, which I would label that of "Chinese exceptionalism", its manifestations, and its repercussions. In other words, if China is exceptional, than in what? In its national culture? Its political, social, and or informal institutions? In other contextual elements? Their combined mixture? And, again, can China be assessed in terms of existing frameworks (e.g., extant cultural classifications) or is it so unique that the readings on current scales do not mean much because they are rooted in a context so different that it is incompatible with the current models? And how should China be positioned vis-à-vis the current body of knowledge from which it has been hitherto excluded? It is important to note here that, in addition to its trade and FDI rise, a major reason why the Western management community has finally shown an interest in China has been the country's seemingly pole position to management's major country of reference, the United States, on such key variables as culture (e.g., Oyserman, Coon, and Kemmelmeier, 2002). Because it was often presented as a contrast, China has ironically served to reaffirm the existing ethnocentric bias where countries were assessed by their difference, or "distance", from the United States. In other words, the legitimacy

of including a China focused publication was argued in terms of providing a mirror image to the mainstream, a point of comparison and perhaps contention but certainly not parity let alone substitution.

To be sure, China has its own biases. In this sense, the concept of “The Middle Country” is not so different from that of the “Far East”, which is how the region in which China is embedded was called in the West for a long time. In both instances, there is a tendency to look at the world from one’s own vintage point, with all trajectories viewed and measures as deviations from a given ideal. So the suggestion here is not to switch from a US centered to a China centered approach, but rather to promote an approach that enables to analyze China, or, for that matter, other countries, by identifying contextual factors that are currently missing, distorted, or little understood, all the while contributing to mainstream management knowledge that will be able to handle such contingencies as opposed to generalized concepts such as “turbulence” or “dynamism” that fall short on multiple accounts. Stated differently, the goal is to “take China to the world.”

From China to the World

With the goal being not only “apply in China” but also “learn from China”, that is, take Chinese concepts to the world beyond, let us first take account of where we are now. On that, there is little room for a celebration. Following an extensive literature review, Jia, You and Du (2011)) found only three (!) concepts that have been imported so far from China into the Western management paradigm: market transition, network capitalism, and *guanxi*. Of the three, market transition is not necessarily unique to China (the category of “transition economies” is hardly a cohesive or even common group) while network capitalism is a feature of other post-Communist collectivistic societies in Eastern and Central Europe, for example. This leaves *guanxi*, the only concept retaining its Chinese name – implying that it has no direct equal in other environments (even though this too has been challenged). This meager selection shows the limits of so called “indigenous research” (Li, and Leung, Chen and Luo, 2012). The authors note the rich mine of Chinese philosophies as one source for the development for such research, but how often do we see this happening? Thirty years since its publication, the concept of “Confucian Dynamism” (later labelled Long Term Orientation in an attempt to highlight a universal application), developed by Michael Bond and The Chinese Cultural Connection (1988, some in cooperation with Geert Hofstede) remains a rare example of a Chinese indigenous concept going mainstream. Nor does this solve the challenge of capturing the multiplicity of overlapping philosophies existing side by side Confucianism, e.g., Mohism, Taoism and Legalism, not to mention the evolution of Confucianism (e.g., e.g., Mencius and Neo-Confucianism).

If an indigenous approach to Chinese management is not the answer for it will preclude comparability as well as contribution to the outside world, yet universality is challenged, so say the list, the road ahead should consist of a hybrid approach. In this route, context relevant to Chinese (and other countries) should be induced to produce contextual variables and eventually comprehensive frameworks that can be used to gauge management globally. For now, we are far from that. Part of the problem rests within the management field as a whole, where the ascent of strategy as a dominant paradigm has either removed context altogether or oversimplified it to the point of rendering it unusable, a predicament that has now spread into other areas, including

international business, where the temptations of two by two tables is proving irresistible. If China can provide a lasting contribution to the management field, reminding us all of the dangers of extreme parsimony would be a very important and valuable start.

To escape such undue parsimony, the very idea of “uniqueness” should be reassessed not only in terms of its direct meaning but also in terms of its research repercussions. To say that a country is unique does not help much until and unless this uniqueness is leveraged. China’s long record of civilization, combined with the changes it underwent, provide a great opportunity to examine crucial issues in management. For instance, McClelland (1963) examined Chinese children literature during the imperial, Republican, and Communist periods, gauging potential changes in culture, whose stability or change is a crucial question. In my dissertation (Shenkar, 1981), I examined how the Chinese bureaucracy adapted to changes in ideology and task during and over the same three periods. Such longitudinal research would be in line with the Academy of Management’s mostly futile call for longitudinal research, and China, given its long and documented history, is uniquely placed to provide the setting for such historical research. For this to happen, however, we need editors who appreciate the importance of historical research and will not desk reject a manuscript because “it is not timely.” In the same vein, looking at China from a comparative perspective is a good idea, but think long and hard about what the comparison country should be. While it may be tempted to continue and use the United States as the point of reference given its predominance in management publication and its supposedly opposite position to China on many fronts, this is not necessarily a good idea. One of China’s advantages in the existence of multiple Chinese societies, so a comparison, say, of the PRC, Taiwan, Hong Kong and Singapore (see Shenkar and Ronen, 1987) is an opportunity to compare “partially similar cases” (Przeworski and Teune, 1970), yielding superior controls.

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