



Editor's Introduction

Introduction: economic sociology and new methods in research

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European Management Review (2007) 4, 133–139. doi:10.1057/palgrave.emr.1500088

There is often a debate echoed in Europe between two schools of thought. Sometimes these schools are labeled American and European, or as deductive-empirical and post-modern. There has been much published on this debate, including in this journal. In all, behind these debates lie many things: different visions of how truth is attained – or can ever be attained, how careers are made and advanced, how epistemic communities are maintained and sometimes even lost.

The *European Management Review* has provided a forum for these discussions in its early issues. The proposal, for example, by Richard Whittington (2004) to study strategy more as a practice offers a distinctly different vision of research than what is found in the leading American management and strategy journals. It calls for a closer study of the practice of strategy, how strategy is 'done.' Whittington implicitly asks the researcher to be an expert in strategy, much like the sociology of science demands a degree of fluency with science in order to understand the values and claims of practicing scientists.

In many ways, this call is akin to the growth in economic sociology of the studies done on financial markets. The finest study in this tradition is the remarkable article by Donald MacKenzie and Yuval Millo (2003). They examine the 'discovery' of the Black–Scholes formula in the early 1970s to its rapid introduction in financial markets. Relying on the notion of 'performativity' proposed by Michel Callon (1998), they demonstrate the inscription of the Black–Scholes formula in the hand-held calculators used by traders. Option trading exploded with the deregulation of markets and thus with the introduction of massive volatility in exchange and interest rates. The Black–Scholes formula facilitated these complicated markets by supplying a trading formula that 'determined' the value of an option as a function of five parameters: the stock price, the risk-free interest rate, the duration of the option, the exercise price, and the volatility of the stock. Thus, the diffusion of the Black–Scholes formula in turn created financial markets; price disagreements could only be due to differences in these five parameters. Since the stock price, time, and exercise price were given, and the risk-free rate is close to the return on a government treasury bill, the only real area of disagreement was volatility. Of course, behind these five parameters was a significant body of theory that proposed particular interpretations of price formation (e.g.

log normal distributions) that might at a particular time prove to be wrong. While Fisher Black was careful to remind continuously whoever might listen that a model is as good as its assumptions, the practitioner heedlessly uses the technology at hand – be it a hand-held calculator or the vastly more powerful machines driving computer-driven arbitrage in current markets.

The growing securitization of 'everything' created more and more markets for trading assets. If these assets could not be traded, then markets quickly developed for 'correlated assets.' Thus, even if one could not write a contingent security on the weather in Brazil, it would be possible to purchase a commodity option on Brazilian coffee. I doubt if many of us considered the utility of such an option for our own use. However, the next time you are vacationing in Rio Janeiro and you watch the pouring rain destroying your trip, remember that you could have hedged this risk by buying a call option on Brazilian coffee – whose price will increase with the fall in supply caused by too much rain and insufficient Sun.

The brilliance of the MacKenzie and Millo story is to carry forward the innovation of the Black–Scholes formula to that black October day when option markets crashed. On that day, hand-held calculators gave the wrong result. It is not clear at what point traders realized that these instruments failed, but I have this fantasy image of a collective realization dawning upon the floor, with hundreds of calculators flung upward in the air like those hats we see tossed in the old pictures of a great event, such as announcing the end of war in Europe. Their conclusion is the cautionary instruction of the limits of formal models to address 'all states of the world.'

Frequently, one senses an anti-market bias in the treatment of markets by sociology, perhaps an echo of the older tradition of *Gemeinschaft* and *Gesellschaft* that imprinted the earliest literature in economic sociology. The refreshing aspect of some of the new economic sociology is the acknowledgment that financial markets perform useful functions that can bring major social benefits. In their analysis of financial arbitrage, Beunza *et al.* (2006) note:

In constituting markets, arbitrage has wider consequences for economies and political systems. For example, in the late 1990s arbitrageurs in hedge funds

and investment banks began to perceive growing similarity between the bonds issued by the government of Italy and those issued by other European countries, notably Germany. For a variety of reasons (including distrust of the fiscal efficiency of the Italian state and consequent fears of it defaulting on its bonds), the prices of Italian government bonds had traditionally been low relative to those of countries such as Germany, thus imposing high debt-service charges on Italy. As arbitrageurs began to buy Italian bonds, their relative prices rose and the proportion of Italy's government expenditure devoted to debt service fell. The process – which was assisted by the liquidity created by the MTS electronic bond-trading system, set up by the Italian treasury in 1988 and discussed below – helped Italy meet the Maastricht criteria for European Economic and Monetary Union (EMU). Arbitrageurs' beliefs thus had a self-validating aspect – they prompted trading that made more likely the event, Italy's qualification for EMU, on which the beliefs were predicated (MacKenzie, 2003) – and arbitrage helped to create a European government bond market, rather than separate national markets.

The financial firm that opened this arbitrage and thus by the classic logic of Adam Smith improved the welfare of the Italian government and presumably Italy by pursuit of profit was called long-term capital management (LTCM).

Over the past days, I could not put down a book, *When Genius Failed*, written by a financial journalist named Roger Lowenstein that treats the story of LTCM that exploded in 1999 and 2000. In one perspective it is the story of greed but in another, it is the story of a community of believers who understood the power of leverage to exploit tiny openings in market inefficiencies to reap incredible wealth. I went to graduate school with a few of the management at LTCM, and I could almost hear the echoes of graduate school debates over models and policies. Such debates lead to an accumulation of over \$4 billion in capital in 4 years of operation, with an estimated \$1.3 trillion of option liability. Now this is what I would call leverage.

Move forward 7 years, and we are in the closing days of the year 2007. The American sub-prime market has rippled through world markets. A sub-prime mortgage is not simply the financing of a home to those who normally lack sufficient credit. It is a security that is stapled with other securities, from which the principal and interest components are separated and then both are sold separately on world markets. The buyers are large institutions, including European banks, insurance firms, and investment houses. In the US, these institutions include even universities, whose endowed investments have grown aggressively in the past years due to their large share of investments in hedge funds, private equity, and mortgage-backed securities. These investments all involve unusual leverage (i.e. debt) and to a large extent embedded options. These options are often extremely hard to price, such as a mortgage-backed security that allows the holder of the mortgage to refinance (or effectively, 'to call the original mortgage').

The current crisis is the product of falling housing prices in the US, which have led to a collapse in the secondary market for mortgage-backed securities as institutions scramble to unload their positions. Without these markets,

liquidity dries up quickly for the borrower in the primary market, as the originating mortgage companies do not want to hold the undiversified risk and buyers are wary to increase their exposure. The illiquidity causes rates to rise, exposing arbitrageurs in the secondary market to unexpected changes in their positions. Forced to sell to cover their positions, prices of securities fall further, that is, the promised rate of return on mortgages must increase. Homeowners holding mortgages with variable rates face higher mortgage payments. As rates increase, defaults increase, and thus a vicious cycle is born. The solution is for central banks to increase liquidity. This is a blunt instrument, causing all financial markets to rise with falling interest rates. Thus, the not-so-funny irony is that financial disaster in one market leads to market interventions that benefit all financial markets.

Of course, the puzzle is why experienced institutions, such as Merrill Lynch and Citicorp, held massively such contingent assets in their portfolio. Rather than having hedged, or having intermediated the mortgages by selling them to other investors, these sophisticated institutions piled astronomical open positions in mortgage-backed securities into their portfolios – some of which were held off-the-balance sheet in special investment vehicles. Faster institutions, such as Goldman Sachs, moved rapidly to recapitalize their positions and to hedge their risk. Goldman avoided the error of LTCM: when markets know you must sell, they are ruthless.¹ But then again, Goldman – according to Lowenstein – was one of the institutions that relentlessly exploited the illiquidity of LTCM's portfolio, even while it was orchestrating a syndicate – to which it belonged – to take over the company. Clearly, there is learning in markets.

But what kind of learning? One theory of the current financial disaster is 'moral hazard.' Mr. Greenspan, the libertarian-inclined central banker, opened the floodgates of liquidity repeatedly throughout his celebrated tenure. The LTCM debacle closes with the top management of LTCM provided \$250,000 a year contracts with \$500,000 bonus opportunities following the acquisition by the syndicate. There are many good reasons to provide this contract, but this soft-landing (in the eyes of most people) surely is not the brutal discipline exercised by libertarian markets. The moral hazard is of course the effects of the belief that markets may crash, but money managers simply bounce. In this view, traders learn that leverage has no downside.

The other view is that learning is always local, rarely fundamental. A popular proponent of this view is Nassim Taleb, whose book *The Black Swan: The Impact of the Highly Improbable* has been widely reviewed. The title comes from the story of the belief that all swans were white until Europeans arrived in Australia to find black swans. The story is the metaphor for the limits of all models that are built upon particular assumptions of the world, such as constant volatility – one of the principal assumptions in the original Black-Scholes formula at odds with the world. Taleb echoes Lowenstein's verdict: a believer's addiction to theories and models is bound in the rough waters of financial markets to deliver the ship on the shoals.

However, far from criticizing the US for the export of disastrous market ideas, Taleb praises the US for its



mastery of trial-and-error learning. The consequence of this cultural aptitude is to assign pragmatic creativity to the US and dull formalization to foreign, namely European, cultures.

Globalization allowed the US to specialize in the creative aspect of things, the risk-taking production of concepts and ideas – that is, the scalable part of production, in which more income can be generated from the same fixed assets through innovation. By exporting jobs, the US has outsourced the less scalable and more linear components of production, assigning them to the citizens of more mathematical and culturally rigid states, who are happy to be paid by the hour to work on other people's ideas.²

This is an odd conclusion to a study on the limits of mathematical models in trading, in which the basic models were invented by American and American-based researchers. It also rings of that Hayekian (almost Greenspan-like) credo that markets are collectively more intelligent than individuals and assuredly more intelligent than abstract models.

To the contrary, one might say in response to the Lowenstein and Taleb thesis, the evidence points incredibly to the significance of people. Markets have a dual character: their power is their weakness. In reading the histories of Black October, the LTCM bankruptcy, and the sub-prime mortgage crisis, I am struck by the reappearance of the same characters and sometimes the same institutions. Thus, John Thain at Goldman plays a major role in the dismemberment of LTCM, continues on to become the head of the New York Stock Exchange after its governance crisis, and is now the head of the much troubled Merrill Lynch. Richard Rubin, also of Goldman, having underwritten the crisis in Mexico, is now heading Citicorp in its moment of crisis. Mr. Greenspan, ever so accommodating with the soft-landing but so skeptical of regulation, has been the Methuselah (the biblical character who continually reappears) of financial disasters.

Economic sociology has begun to address these major issues of people, technologies and models, institutions, and systemic risk. It is possible that these studies will lead to a correction in the ideology of markets as self-correcting if left by themselves, because the histories show quite clearly that markets are not left to themselves at the moments of their crisis. Whittington's reflexive strategist engaging in practice can be transposed to understanding the practice of finance and of financial intervention. Would it be a bad result that sociology and management studies should lead to a better understanding of the practical knowledge in financial markets and the control of systemic risk? Like the farewell to a knight mounted on a horse in a medieval tale, we should say to such studies, godspeed!

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The articles in this issue represent the use of novel methodologies for the study of management and organizations. At least two of the articles present methods that generate the data to test their ideas. This inversion of the inductive approach thus is contrary to the mainstream approach in both American and European journals (yes, there is a European mainstream) that analyze data as given

by the world. What though if history and nature are miserly and do not give us enough samples of the many worlds possible? Why not test our ideas out on data that we generate?

The study by Marco Villani, Stefano Bonacini, Davide Ferrari, Roberto Serra, and David Lane represents the percolation of a major stream of research orchestrated around an abstract understanding of the origins of innovation. They propose the *Exaptive Model in Innovation Studies* (EMIS) that consists of an agent-based model in which agents consist both of producers and users and both have categorical partitions (understandings) of the world. Innovations consist of functional attributes proposed by producers that are evaluated by users according to their categorical understandings of the world. Obviously, there is a type of coordination problem in which functions and categories do not perfectly correspond. Thus, the innovation process consists of a constant process of revision of artifacts (innovations), their evaluation by users, and the transmission of information by users to innovators of their level of satisfaction.

In their model, an exaptation arises adaptively. An exaptation is a category change in the interpretation of the artifact. Through success iterations, categories increasingly improve insofar that the artifact functionality corresponds closer to the users' desires. At a lower probability, a new category can emerge that dominates the adaptive category. This emergence is an example of exaptation.

The simulation, as always, simplifies the world while incorporating nevertheless complex ideas regarding the communication between users and producers. Given the complexity of the treatment regarding functions and categories, Villani *et al.* reduce the number of agents to two. As a result, the action is evaluated not as a distribution over agents or agent choices, but over the iterations of artifact evolution. The results highlight that misinformation ('asymmetric information') permits greater exaptive innovation.

As there is no loss of function in the model, we do not get a sense of how much junk a user has to tolerate before getting something really interesting. The image that the model generates is not the innovation centers located at a modern design house, but rather the atelier of an artist located near the junkyard of a large urban city. The latter might be the more proper image of innovation rather than models of linear search, but the sight is surely less pretty.

Being very abstract, the article by Villani *et al.* might appear to be absent of much controversy. The interesting comments by Anna Grandori and Daniel Beunza indicate much appreciation for the model and substantial disagreement. Grandori offers succinctly an excellent summary of three positions on innovation: innovation is the outcome of blind trials (Campbell), innovation is too improbable if the result of blind search and thus is the outcome of a pattern recognition heuristic (Simon), and innovation can be produced by science-like heuristics that utilizes experimental design to infer directionality in search efforts.

Grandori favors the third position and notes that exaptation fits better to this type of purpose-driven discovery process. However, the weakness in the model, she notes, is that it sees innovations as largely blind in their variations. Instead, designers might in ambiguous settings

design products that are ‘multi-purpose.’ In some ways, Grandori is positing an artifact to consist of features that are directly relevant and others that have an option value. The Villani *et al.* model embodies this approach, except that the *a priori* variety is not itself the outcome of design.

In the tradition of the jaundiced eye always sees yellow, this debate reminds me of the results of a study that Dong-Jae Kim and I did on dead-end technologies in semiconductors (Kim and Kogut, 1996). Very simply, new firms who specialized in semiconductors that were more multi-purposed survived longer than those which did not. Highly specialized semiconductors were dead-ends. If there were no direct markets for the device, the firm failed. The platform technology, or the multi-purposed innovation, appeared to be harder to innovate (entry was lower) but subsequently more successful (exit was lower). I never thought of this embedded option value of platform technologies as ‘exaptive,’ but Grandori is right: the original functionality may be relatively far from the eventual functionality that evolves in the dialogue between user and producer.

The comment by Daniel Beunza frames the Villani *et al.* model of exaptation in the lens of sociology. Beunza is an economic sociologist working with some of the leading figures in the new sociology of financial markets (e.g. David Stark and Donald MacKenzie). His work spans the experience of traders working on the day of September 11 to the interactions of arbitrageurs seeking to make large profits out of thin spreads by taking large leveraged positions.

Acknowledging the novelty of the exaptive agent model, Beunza argues that the model does not reflect the sociology of material things. Much like Grandori, he emphasizes the potential of design (in this case, organizational design) to create multiple paths of communication and exploration. As an example, he cites the trading floor that puts traders operating in different markets at the same desk so that they can aggregate holistically a more macro understanding of events. He also emphasizes the centrality of artifacts, of materiality, that centers the interactions. These artifacts are the calculators, the models, or the spatial organization of work. Like all of us, he observes that the ultimate question is the origins of innovations. This question is hard to define *a priori*, but Beunza, more than Grandori, proposes that the solution lies in shifting the focus from the person as the sole actor but as one type of agent in a broader network. In this sense, Beunza and the Villani *et al.* model are convergent.

The second article in this volume is the paper by Matthew Checkley and Christian Steglich. The article addresses one of the principal observations on the sequence of financial crises described above: crises and institutions come and go, but individuals persist. Checkley and Steglich wish to examine the movement of venture capital (VC) managers and whether they take their relationships with them. The pedestrian interpretation of this study is that they want to determine if organizations or people matter to the social networks that bind VC firms. The broader, though only implicit, question is: do theories of social networks and venture capital have to be sensitive to how this question is answered.

The study analyzes 39 VC firms that conduct 80% of the investments in the UK. There are 663 VC syndications and

95 general partner movements over the period 1996–2003. A VC firm has general partners who manage the investments. When they leave one VC firm for another, it leaves open the vitally strategic question whether they take the ‘social capital’ of the VC firm with them or whether they must start afresh albeit in the context of a new VC firm with its own social capital. We thus have a multi-level network of VC companies and their syndications and general partners and their affiliations.

This question and design are very novel but they also pose difficult challenges in estimation. Consider the problem. At the beginning of the observation period in 1996, there is a network of VC firms. Over time, partners leave to other firms. New investments are also made (the network evolution ignores repeated ties among VC firms). In a network of 39 firms, there is the potential of 741 undirected links. The overall number of ties is 663, suggesting a fairly dense network. The analysis estimates the transition probabilities for 95 moving general partners over 7 years. Thus the probabilities of transition from one node to another are very low, requiring the use of extreme value distributions.

Moreover, the probability of a transition is conditional on all possible configurations of the network. This problem has created massive headaches for studies of dynamic networks. This particular study is aided by the small number of VC firms (39) but still the problem is massive. The solution, which they employ, developed by Tom Snijders, is to treat these transitions by a Markov Chain Monte Carlo process. By this method, they estimate their model over all the possible configurations of the network iterated many times. The large number of iterations justifies confidence that the parameter distributions are approximately normal (Student’s *t* distributed given the small sample) and thus open to testing.

The results of their paper show that general partners, when they move to a new VC firm, do not ‘drag’ their ties. Checkley and Steglich write:

These findings are coherent with an ‘institutional’ interpretation ... of inter-VC cooperation. That is, partner VC firms are chosen on the basis of relatively stable institutional attributes. Partner VC firms are not chosen on the identity of the GP who might initiate a syndicate relationship. That GPs are a core resource of a VC firm ... was consistent with our analyses.

In other words, general partners constitute a resource in the VC firm that attracts ties, but their relationships with other firms are not a resource. Thus when they move, there is no subsequent evidence that their past syndication histories matter for future partner choice.

This result is surprising given past literatures, as they note, on the importance of personal networks in R&D. This literature finds that patent holders or scientists do drag their knowledge, and their relationships, with them. Checkley and Steglich do not find this result, arguing that what matters is the inter-organizational tie. How should we reconcile these findings?

The one troubling technical observation is that they eliminate repeated ties. We find that two-thirds of the ties among VC firms are redundant. This percentage is similar, though smaller, to the percentage found in a much larger

sample of US VC investments over 45 years (Kogut *et al.*, 2007). Given the high density of ties among firms, and the low mobility of general partners (0.3 per firm-year), Checkley and Steglich are unable to sort out the mobility effect from the neglected repeated ties.

Still, the results are important for indicating greater specificity in theorizing for the importance of syndication. One interpretation of the number of general partner as influencing syndication partners is simply that the number captures bigger firms. It would not be surprising that investments flow to and from bigger players. This observation would suggest that the degree of distribution should be skewed and that the correlation in degrees (assortative correlations) among partnering firms should be high. These results would indicate the possibility of a sort of preferential attachment, by which the richer get richer and the rich consort with the rich. However, we know from the US study that the degree of distribution is not skewed nor is their assortative correlations in degrees (Kogut *et al.*, 2007). What is skewed are repeated ties.

I am suggesting the following. Since partners leave very rarely and since VC firms repeat deals so often, it is altogether possible that the effects of personal social networks explain the very high rates of stability in repeated investments. It seems odd to dismiss personal ties because those that move do not take ties with them when in fact the more stunning observation is that general partners hardly ever move. It would be all the more extraordinary that the defection of one partner should shift the social capital.

Surely, extraordinary things do happen. If the Checkley and Steglich arguments are right, they suggest that the value of networks in VC have far more to do with competitive and strategic dimensions than personal ones. One kind of consideration is whether the low rates of mobility reflect the abilities of VC firms to 'bond' their partners. If they move, they lose some (how much?) vested interest in the firm. In addition, VC firms build up powerful brands that the departing partner loses. The economics of bonding is the standard story of partnerships. Is this what Checkley and Steglich are finding, namely that VC firms have found ways to bond their partners, and hence their personal social capital, to the firm? It would be valuable thus to see further studies. This is what they promise us at the end, and we look forward to the future research.

The third article is the novel paper by Rachel Croson, Jaideep Anand, and Rajshree Agarwal that asks why are experiments so rarely used in strategy research. This question has a *prima facie* aura of truth, for there are surely very few studies that come to mind. They argue that experiments are able to test theory that has useful predictions for behavior outside the laboratory. In many ways, their thesis is diametrically opposed to the advocacy of Whittington for more studies of the practice of strategy *in situ*.

Their argument is compelling, for the making of strategy and its implementation is messy stuff. Experiments have been very helpful in showing that people respond to intrinsic as well as extrinsic motivations. Of course, these experiments are not directly testing competitive strategies. They also have a naivete and a reference point that are not as interesting for strategy as for economics, but they often also provide powerful insights – especially if conducted in the field. This claim requires an explanation.

Experimental economics has its roots in trying to test game theory. Game theory is highly epistemological and concerns the choices that people make given their information. Change the information and they might change their choices – assuming the payoffs are the same. There are deeply engrossing and fascinating studies along these lines. These are often highly constrained studies, designed to emphasize the epistemological problem solving of individual choice. Ultimately, the test of the experiment is the null hypothesis that people conform to the game theoretic model.

The results of these experiments have often been along the lines of people deviating from such models, but they also conform. The latter news leads to some rejoicing and the claim is that the model is confirmed. Yet, given that the design of the experiment is so heavily influenced by the requirements of the model, one suspects that chaos might quickly enter the picture if my Uncle Harry suddenly walked into the room, that is, if the setting was less constrained.

It is all the less appealing that these heavily funded experiments have justified Nobel Prizes for largely confirming, with tolerable deviations, the theory – (the major exception being of course the work of Daniel Kahneman).

The current frontier is neuroeconomics. This term is very unfortunate, for if the certain social behaviors are hardwired in the brain, then the correct label should be neurosocial science. I will not review these studies here. However, the gist of these studies is that people are highly programmed to be social. The use of prefrontal cortex processing of new information – which would be required in figuring out the optimal strategy in most game theoretic descriptions – is avoided in favor of more reflexive reliance on implicit memory and learned repertoires. The evidence is far from confirming the economic model. To some eyes, it disconfirms.

Nevertheless, national research bodies fund implicit contracting models of the brain whereby the principal, the prefrontal cortex, seeks to achieve credible commitments from the wily and impulsive amygdala. This baggage has, for many, handicapped the appeal of experimental economics for strategy.

By this objection, I would not though reject experiments. To the contrary, the magisterial experiment by Michael Cohen and Paul Bacdayan (1994) on routines brilliantly supports an understanding of routines as learned and implicit knowledge that has the benefit of speeding response times and the cost of false learning, that is, the inappropriate transfer of a routine to a new setting. Compare this experiment to the study by Weber and Camerer (2003) on the use of epistemological coordination games for understanding cultural conflict. Cohen and Bacdayan are studying practice in a laboratory. Weber and Camerer are studying thinking in a laboratory. They both have their value, but an empirical field interested in knowledge as practice is likely to favor the former.

There are some very exciting developments happening in the area of field experiments. An example of this type of experiment intermediate between the laboratory and the field are the studies published in the book by Joseph Henrich *et al.* (2004) that conducted the ultimatum game in real field settings around the world, usually among

less-developed societies. The results are many, but a principal finding was that the results were sensitive to the society.

Now this result deserves a moment of reflection. If the results were sensitive to the society in which they were conducted, then laboratory experiments often (how often?) do not exclude unobserved external factors. So much for the methodological observation. The more substantive observation is that society matters, and hence what matters is potentially not what people have in their heads but in the social interactions that constitute society. After all, society is not a bunch of firing neurons or epistemological puzzles. Society is people, their relations with each other, and their categorization of social groups and behaviors.

I suspect that the authors of these papers were being too modest and they share many of these reservations. The sociological reality is that experimental economics must heed the standard economics to be published in economics journals. The interesting implication of their article in the EMR is whether experiments must be economic – in the less interesting definition of the word – to be useful for strategy.

It is possible that the most promising arena of experiments for strategy and management is neither those in the laboratory nor those in the brain but those in the field. The Henrich *et al.* study indicates this much. A fascinating example of a field study is the paper by Shang and Croson (2005) on public giving and public radio. In the United States, public radio is partly supported by voluntary contributions. Shang and Croson gained the cooperation of a radio station and manipulated the message to isolate the effects of ‘social influence.’ They found strong effects for social influence in relation to appealing to intrinsic values: people care about social comparisons.³

The article by Croson, Anand, and Agarwal is an important challenge to the state of empirical research in strategy that often has difficulty in sorting out effects from complicated data. While I disagree with the framing of the approach too much in the tradition of experimental economics, the potential for laboratory and field experiments in strategy is significant. The interesting question will be the ability of these approaches to capture the ‘social influence,’ which is surely an important dimension to the practice of strategy. We welcome the submission of articles using experiments.

The final article is the project report written by Nicolai Foss. The Copenhagen Business School (CBS) is one of the largest business schools in Europe. Organized around entrepreneurial energies, it boasts several research centers of international reputation. The Centre for Corporate Governance, directed by Steen Thomsen, is an example of the bubbling ideas at CBS that coagulate into research organizations. There are many more.

One of the most interesting centres at CBS is the Centre for Strategic Management and Globalization, directed by Nicolai Foss. Foss is a leader in the study of incentives in organizational governance. Influenced by the writings by Oliver Williamson, Foss has sought in his writings to couple the strict logic of transaction cost economics to the creation and management of knowledge in organizations. He has been a frequent critic of knowledge studies that rely upon

metaphysical arguments as opposed to measurable dimensions that can be observed and tested.

Despite a well-articulated adherence to a particular school of organizational economics, Foss’s article reflects above all the workings of a successful research community. The very vastness of CBS means that the feeding of the functional requirements of an educational institution is not minor: classes must be taught, large numbers of students and faculty must be administered and orchestrated, and committees must be attended. It is easy for research in this environment to be lost in the general clang and bang of the daily press for time.

The remarkable achievement of Foss and his colleagues has been to create a research center of international reputation that is self-sustaining. One suspects that the incentives for research have increased over time, with local reputations increasingly linked to international reputations. At the same time, the development of the concept of the academic research, and the shared identity of what this means for the organization of knowledge and effort, cannot be missed in this story, at least in the eyes of some observers.

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This issue consists then of three articles that offer us new ways of understanding organizations and management: agent-based modeling, dynamic modeling of network evolution, and laboratory experiments. The final article by Nicolai Foss is a project report on a successful research center on management and globalization. Although all different, they provide collectively evidence of the research depth in management studies, in Europe and globally, and also to the importance of breaking away from deadlocks between points of view (e.g. statistical vs field research) by entering new arenas of theory and practice.

Notes

- 1 For an excellent treatment of this meltdown in arbitrage markets, see Beunza *et al.* (2006).
- 2 See his article in Forbes, http://www.forbes.com/2007/05/23/nicholas-taleb-innovation-tech-cz_07rev_nt_0524taleb.html.
- 3 Bruno Frey and Stephan Meier (2004) pioneered this particular use of field experiments in public giving.

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