Dialogue on Organization and Knowledge

Anna Grandori • Bruce Kogut
University of Bocconi, CRORA Department, Viale Isonzo 23, via Sarfatti, 25—20136 Milano, Italy
Wharton School, University of Pennsylvania, 2041 Steinberg Hall-Dietrich Hall,
Philadelphia, Pennsylvania 19104-6370
anna.grandori@uni-bocconi.it • kogut@wharton.upenn.edu

Preface

In lieu of writing an introduction that runs the risk of adding more smoke than fire, the guest editors elected to engage in a dialogue with one another. Its purpose is to give readers a sense of the thinking behind the special issue, of the debates that ensued, and of the possible directions for further work.

We met for lunch in a café-bar in Greenwich Village in April 2001 to discuss the special issues introduction. Through a series of exchanges, the discussion emerged. What follows is a dialogue about Knowledge, Knowing, and Organizations.

What do you feel are the most pertinent intellectual roots of the knowledge approach to understanding what firms do?

AG: The primary roots are the philosophy and sociology of knowledge. Writers such as Berger and Luckmann, Polanyi, Kuhn, and recently even Giddens, have influenced the thinking on knowledge in organizational and economic life. In this respect, let me note how those authors are contributors mainly in the sociology of knowledge, and have been concerned to point to the weaknesses, limits, "stickiness," "embeddedness," subjectivity, and biases of human cognition. Because of the influence of these authors, there has been an overemphasis on this direction in organizational studies. Without denying the usefulness of that descriptive approach, let me also notice that the philosophy of science and of knowledge tradition is surprisingly underutilized, though it could provide fruitful stimuli for further progress. In fact, *organization science* is more interested than sociology in improving and crafting the rules and structures that in turn govern us. Hence, the relatively prescriptive contributions in the philosophy of knowledge should be more inspiring in an organizational perspective. Rather than being satisfied with observing that rationality and knowledge are limited and subjective, the insights by philosophers as Popper, Lakatos, and even Feyerabend acknowledge the difficulties, and yet propose methods to overcome them and to build safe and powerful scientific constructions, taking into account the "weaknesses" and the real functioning of rationality and knowledge. For example, they point at how to sustain knowledge growth in a context of limited comparability of theories and knowledge, improve knowledge validity and reliability in a context of "fallible" knowledge, and permit communication and innovation in a context of subjective knowledge. Karl Weick early noticed that organizations may be usefully interpreted as "scientific communities with self-interest." Models of organizational learning, rather than of organizational knowledge, have drawn on those conceptual underpinnings, especially for distinguishing incremental from radical learning, as in the elaborations by Argyris and Schon, and more recently by Jim March. Most notably, however, these contributions have somehow lost the "self-interest problem" along the road.

BK: In my view, the chapter by Sid Winter on knowledge and competence in the volume edited by Teece in 1987 surely watered the deep intellectual roots that laid dormant. For Udo Zander and myself, as well as for David Teece, Richard Nelson, and Sid Winter, the most proximate roots are in the massive literature on technology transfer. It should be remembered that one of the observations that perplexed Michael Polanyi, who has played the most prominent role in setting out the idea of tacit knowledge, was why a Hungarian light bulb factory did not work even though it licensed the technology under a turnkey contract. The fascinating study by Hall and Johnson on the transfer of aircraft technology to Japan asked why was this so hard and discussed three levels of context that might matter. Early on, it was recognized that technology transfer is not mechanical, but interactive and embedded in existing capabilities on both sides and in the social relationships between both sides of the transaction. People thought about this as occurring at different levels of analysis, the institutional, the firm, and the project. Paul Streeten and Sanjay Lall discussed the problem of

ORGANIZATION SCIENCE, © 2002 INFORMS
Vol. 13, No. 3, May–June 2002, pp. 224–231
1047-7039/02/1303/0224/$05.00
1526-5455 electronic ISSN

what they called absorptive capacity, an idea that came from Paul Eckaus on the ability of developing countries to absorb foreign capital. Dasgupta and Stiglitz specified more clearly the idea that there is a "learning to learn" that could explain differences in growth and wealth among countries. Of course, this kind of learning is the Type II learning that is to be found in the early work of Karl Deutsch and Gregory Bateson, and later in Argyris and Schon.

This idea has reemerged as an important element in understanding transfer due to Cohen's and Levinthal's recognition that firms will invest in learning depending upon the degree of competition and imitation. The knowledge literature still overfocuses on the learning dimension, which has a century of research behind it, and misses the competitive aspect. The studies on learning sometimes place the bar of proof too low. It is not surprising that Latin helps learning of Italian or more generally, prior knowledge influences future learning. As a hypothesis, this is equivalent to testing the "null;" it is bound to be accepted unless the tests have high power and are very discriminatory.

What is not clear is what makes some learning "distant" from current knowledge. I doubt if this "distance" is determined by an abstract technology rather than by social understandings and a "situated cognition." Development economists soon realized that importing capital equipment did not magically transform countries, and the social and technical capabilities of countries mattered deeply. (Still, economists such as Brad DeLong and Larry Summers argue that capital imports are the driver, without realizing that their use relies upon indigenous capabilities that vary by country and firm.) Here the work of Linsu Kim, Larry Westphal, Howard Pack, and Nathan Rosenberg among many others was so important in discussing how technical capabilities are built and how firms are heterogeneous and differ in their abilities to absorb foreign technologies. Dick Nelson's projects on national systems reflect exactly the point that firm capabilities evolve in interaction with, among other things, institutional resources. Organization studies focus on the internal or external aspects without looking at how markets and firms coevolve in a competitive context. Of course, there are many philosophical and sociological roots, but as some of us looked across countries, and tried to understand multinational corporations as agents of knowledge dispersion, the literature on technology transfer loomed large.

If these are some relevant roots, how do we get to a contribution of knowledge approaches to organization theories?

AG: One way in which it has contributed, I think, is in providing a new "contingency" factor for understanding organizational arrangements, as well as to suggest new ways to conceive the nature of organizational contingencies. Knowledge complexity, differentiation and specialization, complementarity, and interdependence are emerging as important contingencies affecting effective organization and governance solutions (as the articles by Birkinshaw and Takeishi, in this issue, contribute by further clarifying). At the same time, if contingencies are expressed in the broader language of knowledge, rather in the traditional language of information processing costs (as they were in Lawrence and Lorsch, Thompson, and many others afterwards), some new possibilities loom out. In fact, there has always been some ambiguity in the main inspirers of the so-called information processing view of organization (Simon and March), between an information cost and a knowledge-based interpretation of it. In an information cost perspective, one is led to compare different organizational mechanisms in terms of their relative efficiency in attaining a given outcome (as has been most precisely done in organizational economics). However, this perspective restricts our attention to domains in which more than one mechanism can be applied and to come up with one superior solution. In other terms, that focus has limited our attention to relatively structured tasks, ruling out realms of radical or epistemic uncertainty, where the issue is to construct valid knowledge and not to process information more efficiently. In those realms, some knowledge governance mechanisms "fail" not in the sense that they are more costly but equally applicable than others, but because they are "cognitively unfeasible" rather than just costly. As a corollary, one realizes that many more coordination and governance mechanisms are available to manage "simple" conditions (and in that case their choice may well be driven by comparative cost considerations), while the possibility set shrinks as one adds complications. The notion of "contingency" is itself contingent.

Knowledge concerns have brought about another enlargement of perspective in the (vast) part of organization theory that has been influenced by the notion of "routines." Routines are programs of action—not only a form of "rule-guided behavior," but more precisely as context-specific, experience-based, action rules. Hence, studying routines has somehow restricted our attention, traditionally, to "automatic" behavior, generated by knowledge accumulated through past experience and "retained" as a quasi-genetic trait. If one starts from a knowledge perspective, one is naturally led to consider routines as a component, and a quite particular case of knowledge. One is led to enlarge the perspective to include processes of knowledge articulation and deliberate hypotheses testing (a direction towards which Zollo and Winter are pointing

in their piece here). And more varied trajectories of learning open up, if learning processes are modeled in less deterministic ways than that provided by natural selection (which otherwise is the only one way to proceed if the unit of analysis is the "gene-analogue" of routines).

I think that there is growing agreement and shared interest in exploring other and more far-reaching consequences of taking into account knowledge and cognition, rather than just information and information costs; and heuristic, theoretical, and paradigmatic knowledge, rather than just routine-based knowledge in organizational analysis.

BK: I largely agree with what you said, but I will confess to being stumped on how to move from this articulation to theories grounded in feasible empirical strategies. For me, the article by Cohen and Bacdayan that appeared in *Organization Science* represents the most interesting attempt to explore the relationship between cognition and routines, as well as the possibility of "negative learning," the application of knowledge to the wrong context. There is an implication in your comments that I fully endorse, namely that the emergence of knowledge as a fundamental perspective on what organizations do suggests normatively that we should teach students, including our professional business students, the value of stating hypotheses as testable propositions and then to provide assistance in thinking about ways to validate these tentative ideas.

In that respect, the consideration of knowledge helps move the traditional focus of organizational theory to take practice and history seriously. It indicates that it is important to look at what firms do, not just at what factors influence their boundaries, that is, what they don't do! It is interesting that industrial sociology has had such a spectacular and yet vulnerable history in the United States. I think the problem has been the discomfort of organizational theory with feeling comfortable to study phenomena that are situated in particular historical contexts. If we reread the great studies of Joan Woodward today, what is so striking is that she is discussing the transfer of American mass production methods to the United Kingdom in the postwar period. The Aston School results, that scale drives productivity, is a reflection of this historical Zeitgeist in which American size seemed to be the explanation for American success. The reemergence of industrial sociology in the 1980s and 1990s was associated with the wave of new methods coming from Japan. I simply found it suspect to ignore the historical dimensions of this wave, of its cultural roots, and to try to desanitize these practices into ahistorical terms, such as collaborative groups. To this day, management journals such as *Administrative Science Quarterly* or *American Management Journal* remain largely uninterested in understanding historically situated practices in their context, especially those that involve cross-border diffusion. And they remain uninterested in history, even despite the eloquent pleas of Alfred Kieser and others. I don't think this central message of situated practice has been accepted, except in studying Goffman-like phenomena at the micro level. It is a bit odd that knowledge as routines is so amenable to the March and Simon tradition, as you say, and yet knowledge as embedded in historical understandings and organizational practices (e.g., Taylorism, multidivisional form) appears so alien. I would like to see a better bridge to historical and contextual studies.

After a decade of writing, has the "knowledge approach" changed our understanding of the theory of the firm?

BK: It would be surprising if I said no. Udo and I were quite clear in saying that incentives and governance are important. In arguing that the theory of the firm is overdetermined—that is, knowledge and incentives are independently sufficient conditions—we never said that incentives can't explain firms. We said let's spend a bit of time in trying to work out more clearly the logical argument for how knowledge can explain the firm. I think the work of Kathleen Conner and Sid Winter made progress along this path. There is still more work to do, and that is why Postrel's contribution to this issue is so interesting. Still, I think the emphasis upon incentives focuses too much on pecuniary rewards.

AG: Well, let me formulate an antithesis to that: The theory of the firm is *under*-determined—as neither incentives nor knowledge independently get us to a need for constructing islands of shared property rights; and even if complex knowledge and conflict between interests come together, I am not sure that there are no governance alternatives to setting up a firm *strictu sensu*. Perhaps you came to the conclusion that firms can be explained as communities of shared knowledge and identity because you looked (as some papers in this issue still look) only at what happens within firms, finding examples which confirm *the possibility* that knowledge sharing and integration does occur in firms. I have no objection to this procedure for purposes of discovery: Looking for hypotheses from which observed phenomena would naturally flow from is a perfectly legitimate logic of discovery. However, sooner or later one has to come to theory justification and testing; then one has to look for counterexamples as well. Then, let me propose to look at what

happens between firms in knowledge integration respect. There we do find counterexamples: Communities of shared knowledge do exist across firm boundaries (as papers like Postrel's and Takeishi's reconfirm in this issue), and the mechanisms governing them are pretty much like those working within firms (face-to-face interactions, common computer-based knowledge exchange networks, shared practices and theories, certification bodies and rules).

We would also find that knowledge exchanges are more intense in some cross-boundary relations (e.g., in co-makership subcontracting relations) than in some internal interunit relations (e.g., relations between divisions in pure M-forms). We can find counterexamples also by looking at knowledge-sharing failures, rather than successes, within firms. There is little evidence that incentives to share knowledge are generally stronger within the same organization than across its boundaries, or at least one can maintain the reverse as well. This may be especially true for personal and tacit knowledge, as it is mostly a property of people who are less likely to share it with their more proximate competitors in the internal labor market than outside . . . As Machiavelli said, "people make wars with neighborhoods and alliances with far located partners." So (as I have more extensively argued in a recent paper in publication in the *Journal of Management and Governance*) we do have both communities of knowledge (roughly correspondent to conditions of shared objectives) and knowledge markets (roughly correspondent to conditions of differentiated knowledge and conflicting interests) both within and across firm boundaries. Even when we come to conditions of complex knowledge (difficulty of compensated exchange) and conflicting interests (need for incentives and "objectives realignment") we might not get precisely to the firm. In fact, the creation of islands of shared property rights colocated with the knowledge to be integrated are still not necessarily coterminous with firm boundaries: There can be "internal" islands of shared rights (in a "sea" of hierarchically separated rights)—such as internal corporate ventures; as there can be "external" islands of shared rights (in a "sea"of market contracts)—such as interfirm joint ventures.

Therefore, while I have nothing against the procedure of suspending incentive considerations for seeing what we can explain without making recourse to that variable, I disagree on the merit of the conclusion that we have or can get to an explanation of the firm on that road. But perhaps we disagree because we are using different definitions of incentives and of conflicting interests. Neither of them need be monetary. . . .

BK: Clearly incentives can be other than monetary. Yet we should be careful, or else this does not lead us anywhere; letting the utility function be infinitely elastic does not explain anything. The whole interest is to tease out social influences on behavior without a reduction to individual utilities. The effort as I have seen it is to understand firms as consisting of patterns of behavior that persist even if there is turnover in individuals.

AG: I agree with this, and yet it seems quite meaningful to me to say that if two people want, say, the same organizational position, they hold conflicting objectives, even though no money is at stake, and incentives come in "status utility points." In addition, conflict in the firm is not simply the problem of lack of incentive compatibility, but the conflict in rooted judgement. People have different training and orientation. There are conflicts in how they see the world.

BK: Then what are we arguing about? For here we agree that identity is so important. This has been the remarkable contribution of gender and ethnic studies that is to make us aware of the powerful effect of how we see ourselves in terms of social classifications. The importance of the division of labor is that it also organizes our occupational identities, how we see the word, how we learn.

AG: Well, there still may be something to argue about. For why do you think these identities are specific to firms? Isn't this idealizing firms too much? If people hold occupational identities, then this implies that their competences are embedded in larger networks that transcend firm boundaries. Identities won't get you there; occupational identities, industrial identities (e.g., in industrial districts), social group identities may be and often are (perhaps more in Europe than in the United States?) stronger than firm identities.

I am also thinking, however, that part of the disagreement may come from working with too aggregate and imprecise a notion of "firm boundaries." We are criticizing transaction cost economics in many respects, but we have remained trapped into its far too coarse-grained (at least to our purposes) notions of what a firm and a firm boundary are. We should say that there are many boundaries to a firm: At the very least, we should distinguish between a (legally defined) property rights boundary, a (de facto) knowledge possession boundary, and contractual boundaries, which distinguish (if they do so) "internal" human assets from human resource service provisions.

Maybe this is a way out of the dilemma defined by your thesis and my antithesis; perhaps we or others will find a synthesis.

BK: I guess I will have to respond to such a direct line of questioning! I certainly agree that identity is not limited to the firm. Industries also have boundaries and identities, a point emphasized by J. C. Spender. I am struck by how clever March and Simon were not to endorse a definition of the organization. Your proposition on types of definitions is reasonable, because it maintains this pragmatic orientation.

This allows me to double back and protest mildly over your claim that we saw knowledge as only within the firm. I recall Reinhard Bendix, whom I was very lucky to have as a professor for several classes when I was an undergraduate, saying that to understand a position you have to understand against what it was taken. Udo and I clearly set out a figure that discussed four levels of knowledge, one of them the network. However, our aim was to do two things: One was to show that one could do empirical work in this area and the second was to show that we could model an approach very much on Williamson's contribution of looking at the microanalytic details, and yet explain boundary choice by other factors than incentive conflicts. There has now been considerable empirical work on knowledge transfer. The work by Argote and her colleagues, Szulanski, Hansen, Poppo, Zenger, and Zander among many others, develop measures from raw data to test propositions. There are also excellent field studies, such as the article by Orlikowski in this issue. The many fine studies in transaction cost economics have set out the appropriate focus: Look for the microanalytic details whenever possible. Therefore, I disagree with your claim that this work has remained on the grounds of "discovery" without fully entering into theory testing and the "logic of justification."

AG: . . . But you are still not confronting the importance of identities even outside the firm.

BK: I guess I am because I accept the observation as true but disagree with your conclusions. Over the past decade, researchers such as Gordon Walker, Powell, Uzzi, Dyer, Fujimoto, Shan, Stuart, Ahuja, Gittelman, Gulati, and myself among others have looked at knowledge in the network. Still, by endorsing this perspective of networks, I don't want to sacrifice the analytical gains we have made in understanding the firm. We are talking about differences, not absolutes. Knowledge and incentives are influenced by firm boundaries; this influence need not be large itself to produce large outcomes. Of course, the interaction between knowledge and incentives interacts sometimes to produce unexpected outcomes. That this is harder than what we suggested is certainly true. The hope for some linear relationship between costs and organizational form is not realistic; we cannot create a continuum of organizational forms from the market to the firm. Yet, sometimes predictions are easy. Mark Casson, for example, has argued persuasively that firms can internalize transactions solely to arbitrage tax regimes that differ across countries. There is no incentive conflict, or competence issue, simply institutional arbitrage. We should look for the easy cases to provide anchors, rather than dismiss them for lacking complication. Similarly, we should look at important phenomena, such as transfer, without criticizing such studies for being too focussed.

Much has changed since a decade ago when the only viable theory of boundaries was transaction costs. Theories of capabilities, competence, and knowledge have forced a major rethinking of the source of differences among firms. It is heterogeneity that Coase emphasized, and why he protested so strongly that he did not observe incentive conflicts in his field research. The Coase article of 1937 provides the insight by which we can construct why identities are analytically useful: In firms, coordination, learning, and communication are facilitated, but at the cost of a lost in variety.

I think this captures nicely the problem of an organization; identities rule out viable paths of exploration, alternative visions of organization, and competence. Organizational behavior has long emphasized socialization, but the cost of socialization is homogeneity, a point emphasized by March. Coase showed how markets and firm capabilities jointly determine boundaries. So does a theory of knowledge by looking at competence in the firm and between firms. The lines cross, like every good economic theory should. But let's be clear; we are talking about differences among firms and about increasing costs incurred by variety. Your criticism can't simply be that there is knowledge and identity everywhere and hence nothing can be determined. I know we agree on this point.

Clearly, identities, learning, and coordination are to be found in the interstices among firms and in the occupational communities described by Barley and van Maanen. When they are stronger than those among firms, they may well switch the activity from the firm to a different entity; I hesitate to say to the network itself though. It is increasingly clear that communities of practice are critical to understanding regions and transfer of ideas by the movement of people. All of this is not conceded, but endorsed.

AG: In fact, we agree that knowledge integration and identities are not everywhere; we seem to disagree on where the zones of their maximal density lie with respect to where (which) firm's boundaries lie. But you seem to come along, by saying that some external networks may exibit stronger identities and more intense knowledge-sharing properties than firms. Hence, there may be a lot

of fungibility between internal and external mechanisms. I would then raise the bar, and propose that the first version of a knowledge-based analysis of the firm we got (and thanks to efforts like yours we luckily got one) was too deterministic and perhaps too close, rather than too distant or "rival," to some of Williamson's procedures, if not contents. In particular I wonder whether we can really hope to characterize "governance structures"—among which the very broad structural alternative of "the firm"—by finding exclusive attributes that characterize them (and not other forms). In my own work (which I would readily admit is also otherwise indebted to organizational economics), I am trying to explore an opposite procedure: Suppose that any governance and organization form is actually compounded by the same raw material, by the same basic elements, and that, as in forms of life (and of artificial material), *the combinations* of those elements matter, rather than the exclusive use of them, the distinctiveness of attributes. And these elements are in large part made of a range of coordination mechanisms—including not only prices, authority, and identity-based communities, but also other forms of communities, rule-based "automatic" mechanisms as routines, as well as "democratic" but not identity-sharing mechanisms like voting and negotiating.

In this respect, I think that your work has been timely and useful in reminding and extending our appreciation that coordination within firms can rest on mechanisms which have little to do with hierarchy and "fiat," and that the equation between firms and hierarchies is at best ill founded. For *organization science* there can be such things as nonhierarchical firms, as well as there can be interfirm hierachical contracts. Even more, we converge, I believe, in thinking that it should be possible to explain governance arrangements (and firms among them) by means of a more general theory which does not rely on the particular configurations of power that came to be most diffused in a given phase of the history of "capitalism." If we look at history, as you rightly suggest, we do find that both the "old" and "new" economy are very effective and efficient examples of enterpreneurial, peer-group-like, and still other internal governance arrangements. The interpretation of them, however, should not rely, at the same time, on "idyllic" assumptions of trust as opposed to the "cynic" assumption of opportunism. All that is far too ideological, and should be overcome. And I think we also converge at least on the hypothesis that knowledge is an important factor in this more general theory.

BK: I agree with the view of combination as an avenue by which to understand how firms dynamically develop. And I think you are right to emphasize that these blocks to be combined are to be found outside as well as in the firm. A firm grows in the context of making the choice of what elements to combine, and in the understanding and exploration of complementarities among them. One way to put forward a knowledge argument is that firms and communities consist of coordination problems, independent of cooperation dilemmas. That is, firms do not consist of resolution to prisoner-dilemma kind of problems, but of mechanisms by which action is coordinated. Postrel takes this insight as a starting point, and his paper represents a sophisticated analysis of the implications.

But I would invite readers to go further and to understand identity in the context of power, authority, and position inside the firm. Hierarchy is present, even if overstated. Indeed, the many network studies that ignore formal hierarchy perhaps are finding this in their empirical results; we don't know because they don't tell us! We gather data on communication, we calculate centrality statistics, and perhaps we are only rediscovering the formal hierarchy. I am not convinced that network studies themselves discount hierarchy if hierarchy remains a latent and unexplored construct.

Power, then, returns us to your emphasis on cognition. Combinations arise not from abstract relationships (such as technological distance), but in response to power and cognition. Power is an important issue, and I sympathize but disagree with Williamson's efforts to dismiss it. Power is important, partly because it influences cognition, what people are willing to see. The costs of laying off workers in Europe clearly not only influences the hiring decision, but also changes the power of employees and workers inside the firm; this influences boundary decisions as well as the development of competence inside the firm. Game theory does a rather elegant job of analyzing this kind of situation. I am, though, very wary of studies that claim coherence among combinations (such as diversification patterns) that do not see how these patterns rose in response to institutional and contextual influences. I am not entirely comfortable with social construction theories, but I surely accept their basic premise, that technologies should not be seen as exogenous. Organizational sociology is right to insist upon this.

What I fear, though, is that we lose analytical sharpness as we add in more and more important concepts, such as power. I would hate to think that the ultimate contribution has been to loosen the obsession with transaction costs only to plunge into a fog. I think we should be willing to admit that theoretical perspectives are partial guides, and to avoid trying to run around like a butterfly catcher with a net in the wind. Still, I am coming around to the view that since identity is hard to establish in large firms—a

point upon which Larry Prusak insists, claiming that community can only exist along a few hundred people—a theory of power and authority is unavoidable. But I resist this perspective, as I suspect do Brown and Duguid, Cohen and Prusak, and many others, perhaps because of the idyllic hope that organizations could be built on shared knowledge as opposed to hierarchy. Here we both agree. Fortunately, we have Morten Hansen's article in this issue, which speaks well to the issue of knowledge sharing in relation to social capital.

AG: Yes, a clear point on which we agree among ourselves and disagree with transaction costs, is that a theory of the firm cannot be a theory of hierarchy. In spite of this, or perhaps just because of this, I am not only sympathetic but I actually agree with Williamson that we should not soften up the task by using concepts like power or trust that can easily explain everything, and can easily lead us to lose any capacity to discriminate between effective and ineffective arrangements. For example, layoffs or underhiring due to "power" reasons would simply be evaluated as inefficient boundary design in organizational economics, i.e., it would be analyzed as an inefficient allocation of power (of decision, control, and reward rights and obligations). We do need a theory of effective authority (and actually I think that we do have one, in the line of thought connecting Weber to Simon to Demsetz), but I suspect (actually having devoted some efforts in my past works to demonstrate it) that theories of "effective power" or of "effective trust" are likely to be equivalent to other, more precise, already available economic and organization theories.

What are some important methodological issues confronting organization and knowledge research?

AG: Despite some differences about how to achieve a different or more general theory of the firm with respect to the one received from transaction cost economics (which has to be thanked, by the way, for providing one to start with), it seems to me that we both encourage the development of models that have the courage of being precise and predictive. I fully concur with your concern with simply stating that knowledge, identity, hierarchy, or any other organizational element, are everywhere and nothing can be determined. Actually, as you know, this is the very reason why I am concerned about the diffusion of something that we may call "a Giddensian syndrome" in organization studies, and in knowledge studies in particular. It is not my job to conduct a critical review of what Giddens' true contribution to sociology has been—I know that this debate is still quite open in sociology itself. I talk from the different discipline of *organization science* and I am evaluating how the "structuration" discourse is being used in our field. While assertions such as "everything depends on everything else," that "Così è se vi pare" (title of one of Pirandello's masterpieces, sounding like "So it is, if you so believe") can usefully sensitize deterministic cultures to think otherwise, they are likely to have fairly dangerous effects on cultures that are already quite "flexible" and sophisticated, and on scientific work. There is the risk that research becomes messy and inconclusive rather than sophisticated, and sophistication becomes a game of words and legitimation.

BK: Having grown up in Los Angeles, described as suburbs in search of a center, I can hardly disagree with your quotation from Pirandello. I have found Giddens and this tradition to be useful, especially in response to the central traditions in American management studies. However, in my heart, I prefer an approach that is grounded and yet leads to an empirical research strategy.

It is almost a professional reflex that every academic has a few ready thoughts on what constitutes knowledge. There is also often a perceptible edginess in the academic discourse regarding knowledge. This edginess stems from a few sources. One is the distrust of philosophical intrusions in applied research, a distrust more profoundly felt in American circles than those in Europe. Even articles of indisputable quality submitted to this special issue fell into the spiral of venting an exasperation with the "highbrow" aspirations of published work in the area. The need to eliminate crankiness was a common refrain in the reviews and editors' letters. At the same time, there was the opposing tendency to denounce efforts to "capture" knowledge in constructs and measures. Knowledge is described as interactive, embracing both its individual embodiment and the structured context by which individuals engage in coordinated action. The argument then questions how efforts to create "codified" codes of measures can capture the social dynamic between individuals acting on tacit beliefs. Given this gap among research traditions, it is not surprising that efforts to understand organizations as the constructed utilization of knowledge should stir up epistemological debates that appear to have no natural resolution.

AG: We should honestly admit that the knowledge concept itself is on the edge of running the risks that broad concepts like power and trust confront: to be a low value adding "superstructure" to the component concepts in which they could be operationalized and the microprocesses into which they could be disentangled. I do think it should be salvaged from that risk (and from being thrown away, after the faddish phase, together with the bad water). The main far-reaching potential that the

knowledge concept seems to have is to favor an enlargement of the cognitive foundations of *organization science* from a view of rationality that is "bounded," limited, tacit, uncertainty avoiding, information cost reducing; to include a view of rationality that is creative, uncertainty seeking, problem solving, hypotheses testing and self-aware (let say the "scientific" view of rationality). To achieve that, it is important, whatever epistemological stance we embrace on knowledge and organization, to be quite precise, predictive, and even prescriptive (concerned in the growth and validity of the knowledge used in economic and organizational life). It is possible to be so even if we want to trace tacit microinteractions—we have quite solid examples to follow in cognitive and behavioral research on decision making in this respect. To conduct qualitative and process studies does not provide a license for being less rigorous than in what became the standard construct-and-measures quantitative approach. Rather, the opposite holds; it obliges us to be even more precise, detailed and empirically grounded—the obsession with documentation and observational accuracy is in fact a classic of high standing traditions of qualitative research as ethnographic and historical research. Hence, it is not for eclecticism that we tried to be open to all approaches in this issue, but because we are convinced that there is no point in ranking research strategies as such with respect to one another, nor that it is possible to state that one particular approach is superior to others on one particular subject, knowledge included.

BK: And yet we have our preferences, as any discerning reader would accuse us! Perhaps we would admit to the disappointment that there was less grounded empirical research, especially statistical studies coupled with field work, than expected. There was disagreement among these articles regarding the value of continued exploration of the philosophical foundations. There was also agreement that there had been considerable advance in research over the past 10 years and still there was disagreement over the characterization of this progress.

There are plenty of signs that the themes of the knowledge perspective are important, because one sees them emerging across so many disciplines. Tacitness appears as a topic in an article published on nuclear bomb diffusion in the *American Journal of Sociology* and in a series of research projects headed by Paul David, Dominique Foray, and Patrick Cohendet, on science. Identity is discussed by Jane E. Dutton, Janet Dukerich, and Celia Harquail in *American Science Quarterly* and by George Akerlof and Rachel Kranton in the *Quarterly Journal of Economics*. I know that you are publishing a special issue of the *Journal of Management and Governance* on knowledge, rationality, and governance involving people like March, Levinthal, Foss, Loasby, Langlois, and others. *Organization Science* has been an important forum for these ideas, but not the only one.

It would be nice, of course, to see convergence in ideas. We are all impatient, but progress is being made. Surely, one sees this insofar in that the articles in this special issue, even though so different, speak to one another even when they disagree.

And hey, we didn't do too badly speaking to each other during our disagreements either. I guess it must be that Italian coffee.

AG: Mhm . . . I thought I was drinking American tea.