

## RESPONSE

### VAN DE VEN AND JOHNSON'S "ENGAGED SCHOLARSHIP": NICE TRY, BUT . . .

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Practitioners find little value in academic research. Some see it as a knowledge flow problem; others see practitioner and academic knowledge as unrelated. Van de Ven and Johnson propose a pluralistic collective of researchers and practitioners using "engaged scholarship" and intellectual arbitrage to create practitioner-meaningful research. It's a nice dream, but not a solution; bias, disciplines, and particularism remain. Neither discipline-centric nor practitioner-driven research offers a solution. Earthquake science offers a better model for business school research.

Van de Ven and Johnson (2006) draw on Simon (1967) to state the obvious: a key mission of business schools is to produce research that advances practice. They also cite observers who argue that this mission, by and large, has failed (Anderson, Herriot, & Hodgkinson, 2001; Beer, 2001; Beyer & Trice, 1982; Brief & Dukerich, 1991; Gibbons et al., 1994; Hodgkinson, Herriot, & Anderson, 2001; Lawler, Mohrman, Mohrman, Ledford, & Cummings, 1985; Rynes, Bartunek, & Daft, 2001; Van de Ven, 2002; Weick, 2001). Pfeffer and Fong (2002), Bennis and O'Toole (2005), and Ghoshal (2005) are recent additions. Most of the dates are post 2000. If anything, business schools apparently are getting worse. I'll term this the *knowledge failure problem*. Before turning to the proposed fixes, I'll take a quick look at the knowledge "food chain." Next, I'll offer comments on problems I have with the Van de Ven and Johnson solution. I'll then provide a suggestion of my own. Whatever one thinks about the various solutions discussed, there is no question that the knowledge failure problem is the dominant issue as business schools start the twenty-first century. Carnegie Commission, where are you?

I figure Andy and Paul are tough guys, so it won't be the end of their world if I sharpen the dialectic. In any event, their scholarship is impressive; I have learned much from reading their article.

#### THE KNOWLEDGE FOOD CHAIN

Van de Ven and Johnson see the positioning of management research as equivalent to the po-

sitioning of engineering relative to the physical sciences and medicine relative to the biological sciences (2006: 808). Knowledge production and consumption are not unlike the biological food chain. At the left end we have, say, mosquitoes; at the right end we have *T. rexes*. Reading from the left, we see the production of ever-larger and more complex creatures; reading from the right, we see the consumption of ever-smaller kinds of animals/plants. As Van de Ven and Johnson describe the knowledge food chain, "Knowledge is created and tested by academic researchers, taught to students by instructors, adopted and diffused by consultants, and practiced by practitioners" (2006: 805). In earthquake country, the engineering food chain looks like this:

Physics, Earthquake Science, Engineering,  
City Building Code Departments, Builders,  
Buyers

Medicine looks like this:

Biology, Medical Research, Medical Schools,  
Ph.D.s/M.D.s, 4th- to 1st-Level Hospitals, GPs,  
Patients

I would describe the business school food chain as follows:

Disciplines, Management Research, Ph.D./  
M.B.A. Students, Consultants, Practitioners

Food chains can be read from either direction. Thus, in life science, the discovery of DNA eventually leads to new molecules in drugs that cure patients. The increasing prevalence of Alzheimer's disease in patients leads to stem cell re-

search. Arguably, on the one hand, business school research is increasingly held hostage to the epistemology of basic disciplines—a problem. On the other hand, we have the following quote:

The only way we can make our field more useful is to start doing—and rewarding—work that can be read and applied by business people (Davenport, quoted in Lytras, 2005).

Should management research be held hostage to people who seem mentally challenged when reading the *Harvard Business Review*? This is one danger in Van de Ven and Johnson's approach.

### REVIEW OF THE VAN DE VEN AND JOHNSON ARGUMENT

Van de Ven and Johnson review two existing solutions to the knowledge failure problem and then detail their own. I review these briefly so as to highlight the differences.

#### The Knowledge Transfer Problem

The first school of thought the authors review holds that knowledge failure is a knowledge transfer problem. I list key elements of their argument below. Needless to say, in this view knowledge flow is left to right and it stops just before it gets to M.B.A.s and consultants.

##### *The Knowledge Transfer Problem:*

1. Academic research isn't put into a form that can be applied in practice.
2. Little attention is paid to the transfer problem.
3. Researchers don't take responsibility for knowledge transfer.
4. Authoritarian and coercive styles of imparting knowledge, defensiveness by teachers and researchers, and self-interested recommendations by consultants inhibit the flow.
5. Academic research interpretation fails because researchers don't collaborate with practitioners.
6. We know little about what makes research useful.
7. We don't appreciate just how much knowledge changes as it goes through the transfer process.
8. We don't understand (Aristotle's) art of persuasion.
9. Researchers don't take time to appreciate or understand the context of the practitioner.

#### Theory and Practice As Distinct Kinds

A second school the authors review holds that academics and practitioners live in two different knowledge worlds. Expecting knowledge to flow left to right is like expecting round pegs to fit square holes—no wonder we have a knowledge failure problem. Since we already know very well how academics do research and produce knowledge, this school starts at the right-hand end. How do practitioners, and how should academics, learn about practitioner problems? Beginning with Kondrat's (1992) work, I show key elements of their view here.

##### *Theory and Practice As Distinct Kinds:*

1. What knowledge does a practitioner actually use and how does he or she obtain it? How does he or she construct an action?
2. What do competent practitioners know and what do they know about knowing?
3. The "knowledge transfer" school privileges academic knowledge and devalues practitioner-created knowledge.
4. Practitioner knowledge is a distinct form of knowing in its own right.
5. In Aristotelian terms, *phronesis* (practical knowledge) is just as important as *episteme* (basic knowledge) and *techne* (applied technical knowledge).
6. Practical knowledge is tacit and embodied in action; only immersion in the job produces relevant *techne*—even scientists rely on task-immersed knowledge construction.
7. Scientists study generalizable problems that are, as much as possible, context free; practitioners use knowledge that is site specific and date stamped—it is customized, derived from experience, and aimed at specific situations.
8. Practical knowledge is a distinct form of knowledge having epistemological status equal to that of academic knowledge.
9. The epistemological rules of good scientific knowledge are fundamentally different from what is necessary for valid practical knowledge; practice-aimed inquiry cannot stand outside practice, as scientific epistemology dictates.
10. Practitioner-relevant knowledge can also be produced with detachment; it can achieve "objectivity" by relying on multiple observers to rise above idiosyncratic viewpoints.
11. Practitioners construct new theories for new contexts.
12. Valid practitioner knowledge has to be actionable.

This school takes a no-flow-needed stance. Each kind of knowledge exists, but this school writes off the left end as unfathomable, although not necessarily irrelevant. Left unanswered is the following: If practitioner knowledge is independent, validly produced, and useful to practitioners, why should business schools bother with academic knowledge? Should they do it just to look good to promotion committees at universities or to keep journal editors happy? Should they do it even though it appears to have little, if any, practical relevance? Now one can see why the debate is heating up (Grey, 2001; Huff, 2000; Kilduff & Kelemen, 2001; Weick, 2001). Interestingly, Van de Ven and Johnson report that citation rates and practical relevance are somewhat correlated (Baldrige, Floyd, & Markoczy, 2004), as citations are with researcher familiarity with research sites (Rynes, McNatt, & Bretz, 1999).

#### **Van de Ven and Johnson's Solution: Knowledge Production via Engaged Scholarship**

Van de Ven and Johnson try to bridge the "distinct kinds" gap by what Boyer (1990) and Pettigrew (2001) call "engaged scholarship." I list the essentials of their logic below, mentioning key aspects such as intellectual arbitrage, conflict management, and dialectical form. While I summarize a fair amount of the Van de Ven and Johnson story line, readers need to read their article to fully appreciate the depth and nuanced contextual richness of their well-argued discourse.

##### *The Van de Ven and Johnson Solution: Knowledge Production via Engaged Scholarship:*

1. Engaged scholarship is a "set of reforms to break down the insular behaviors of academic departments and disciplines" (2006: 809):
  - a. Calls for a fundamental shift from disconnected, distanced researchers to researcher collectives involving both business school scientists and practitioners.
  - b. Calls for this collective to jointly produce research meeting academic standards and practitioner needs.
  - c. Shifts organizations from data collection sites to idea factories where "practitioners and scholars coproduce knowledge" (2006: 809).
2. A key aspect of the research collective's behavior is "intellectual arbitrage" (2006: 809):
  - a. Idea is to take advantage of differing academic and practitioner perspectives so as to design better research—multiple discipline/academic and different functional practice perspectives.
  - b. Advocates a "dialectical method of inquiry," rather than research from opposite ends of the food chain, and a confrontation of "divergent theses and antitheses."
  - c. Eschews narrow technical research strategies in favor of triangulating in on problems from different perspectives.
  - d. Advocates Azevedo's (2002) "pluralistic methodology."
3. Needless to say, conflict resolution becomes a key issue in intellectual arbitrage:
  - a. Given the research collective's pluralistic views, conflict is inevitable; conflict and power relations take center place.
  - b. Creative conflict management becomes the central challenge; suppressing conflict defeats the purpose of the pluralistic collective.
  - c. Task conflict is encouraged; personal conflict is to be avoided.
4. The "dialectical form of engaged scholarship" consists of five "dimensions" (2006: 809–815), which occupies a third of their article. Engaged scholarship needs the following:
  - a. A focus on *big questions grounded in reality*—that is, large, complex problems offering no immediate payoffs to academics or practitioners; both ends of the food chain have to be motivated; scholars from different disciplines and practitioners from different functions need to be involved.
  - b. A *collaborative learning community*; the pluralistic collective needs to meet regularly and members need to come to know and respect each other, practice concilience, find ways to rise above conventional scientific requirements, proprietary concerns, or pragmatic pressures, and have rules of engagement.
  - c. An *extended time* over which to build relationships, to find ways to make significant advances, to get academics closer to the practitioners' phenomena, to get practitioners in touch with academic concerns, and to conduct longitudinal research.
  - d. *Multiple models and methods*, scientific pluralism, triangulation of methods, propositions that "carve at the joints," and methods of testing alternative plausible hypotheses.
  - e. A *reexamination of researcher assumptions* and researcher self-reflection; researchers should warm up to the interventionist model of action research, use

arbitrage to work out conflicts stemming from traditional "detachment" versus "action research" values, and intertwine clinical and researcher roles to pool insights.

### PROBLEMS WITH THE VAN DE VEN AND JOHNSON SOLUTION

Most charitably, one can see that Van de Ven and Johnson attempt to resolve *both* the "knowledge transfer" and "distinct kinds" problems. Arguments aside, their program shows some resemblance to Chris Argyris's well-known "action research" perspective (1970, 1980; Argyris, Putnam, & McLain Smith, 1985; Argyris & Schön, 1974, 1978, 1996] and Ed Schein's (1987) work. Argyris might say, "Been there, done that." In fact, what is new is Van de Ven and Johnson's idea of the *pluralistic collective* of academics and practitioners collaborating on difficult issues over an extended time period; they add *arbitrage*, *big questions*, and *method triangulation*. This is surely different from the isolated Argyris, Schön, Schein, and many others consulting/researching with some firm. One might wonder how anyone can be even a little negative about the "goods" in italics.

In principle, a lot of things should happen that in reality don't: CEOs not cheating on shareholders; husbands not cheating on wives or wives on husbands; students working to get the best out of a class rather than cheating on the exam; consulting firms actually accomplishing something positive for their clients rather than selling useless best practices for high fees; buildings that survive the next big quake; politicians that don't lie, cheat, and steal; Presidents that don't pay off big business and friends at the expense of virtually everything else; and on and on.

In dreamland, engaged scholarship, intellectual arbitrage, conflict resolution, and Van de Ven and Johnson's five dimensions could work. In reality. . . I am not so sure. The joint probability that all their required elements would line up simultaneously at the right time and place seems low. Worse, there could be outright deal breakers. A number of possible downsides come to mind.

#### Bias

Firms have particularistic, specific, time-dated interests and proprietary concerns. Is this

a good platform for scientific truth claims? Action researchers and academic consultants always have a conflict of interest—keeping the client happy and getting consulting fees versus doing what would be best for science. Engaged scholarship consists of pluralistic interests and conflict; there is the risk of decision by committee, power contests, and settling for the lowest common denominator. Assuming the less likely—that the parties remain (statistically) independent—there is the risk that the "average" across many of these projects would be decision by committee. More likely, given the obvious interdependence, the behavior of the collective could spiral into very constructive or very dysfunctional outcomes via positive feedback cycles, but outside (disconnected) scientists wouldn't necessarily know which to accept as truth claims. Besides, has any significant, novel, science-type "truth" actually emerged from the decades of action research?

Personally, I think *consulting addles the scientific mind*. Judging from the business media books, most managers are seemingly incapable of aspiring even to the "intellectual" level of the *Harvard Business Review*. They are phobic against the word "academic." I remain unconvinced that science is well served by constant dumbing down to four-cell tables. I don't see any evidence that academics who thrive on "interpreting" to practitioners are ever at the forefront of scientific novelty. All the Nobel laureates do their creating first and then write their "pop" books. It seems unlikely that business school profs could do the opposite. I don't see that action research has ever risen above the simple wish some professors have of trying to get rich doing research.

Finally, there is the famous phrase "What is good for GM is good for the country." Is it equally true that what is good for GM is good for science? Even business school science?

#### Food Chain Problems

Suppose we accept that there is a gap in the knowledge food chain. Bridging the gap via engaged scholarship accepts that both ends of the chain remain unchanged. One could argue that Van de Ven and Johnson are bridging the ineffective. Consider that knowledge flows each direction. Parsons' (1951) *universalism/particularism* dimension is a key aspect.

**Going left to right—the discipline effect.** Disciplines create the wrong basis for management research; they focus on would-be universalist but discipline-specific theories and terms, discipline-centric methods, and the like. Discipline perspectives are seemingly not useful to practitioners, nor are discipline-based truth claims. Academics writing papers for disciplines have different success criteria than practitioners. The foregoing statements reflect much of what is implied by both knowledge failure schools, so I won't expand further here.

Much of business school prestige is now defined in terms of achieving close ties with underlying disciplines. Top-ranked business schools generally hire discipline-centric Ph.Ds. In Starbuck's (2005) just-out essay on publication quality in A journals, the *Administrative Science Quarterly* is the only "business school" journal included in his analysis. But is it really a business school journal any more? For some of my sociologist colleagues, the *Administrative Science Quarterly* is now seen as the "third" place to publish organizational sociology papers, after the *American Sociological Review* and the *American Journal of Sociology*. The institutional structure imposing on business schools exerts an irresistible pull toward discipline-centric research.

**Going right to left—the firm effect.** Practitioners need immediate help; they can't wait for scientists' lengthy conception-to-publication time cycle. They need site- and time-specific insights. Tomorrow is what counts. They are not especially helped by longitudinal studies based on questions and data defined one or more decades ago. There are three additional "negatives" in going from competitively advantageous site-specific findings to findings independent of time and place and then back again to application in a specific firm at a specific time.

1. While Southwest Airlines may have "moderate complexity" that makes it inimitable (Porter, 1996; Rivkin, 2001), most firms, having found some kind of distinctive competitive advantage via good research, would be stupid to share it with other firms.
2. In addition, if we were to actually accumulate particularistic research—via engaged scholarship—from a collection of individual firms into some kind of "average," we would still have the same problem we already

have, which is going from the average to the dated context of a specific firm.

3. Any truth claim based on site-specific research having value to a specific firm could have little value to another—Toyota is GM's competitor; Toyota is not Toyota's competitor. They live in different niches.

It is arguably illogical to have "science" go from right to left in the knowledge food chain. To sum up my concerns, at the worst, engaged scholarship could produce the following:

Conflict of Interest × Conflict × Decision by  
Committee × Particularism = Bad Science

### Campbellian Realism

For epistemological justification, Van de Ven and Johnson cite Jane Azevedo (1997, 2002), who, in turn, cites Campbell (1974), Hooker (1987), Hahlweg and Hooker (1989), Campbell and Paller (1989), and my "Campbellian realism" (1999, 2002). Collectively, these scholars develop the evolutionary naturalist realist epistemology. Whereas *scientific realism* stems from natural science and Campbell's realism embraces the notion of objective reality as the criterion variable, Campbell also accepted scientists' idiosyncratic interpretations of that reality, as well as the follow-on social construction by a scientific community. The latter gave rise to Kuhn's (1962) paradigms.

The quote from Azevedo (2002: 730), which Van de Ven and Johnson use on page 809, was written within the context of her concern about paradigmatic narrowness. Thus, her advocacy of pluralism is to get out from under the constraints of a single paradigm. Her book is titled *Mapping Reality* because she wants us to think of theories as maps. A map is a simplified, idealized view of some part of our world, usually created for a specific purpose like locating roads and towns; rivers, mountains, and plains; or earthquake fault zones and state boundaries. Sometimes a theory or map can be used for a purpose other than its initial specific intention. In their engaged scholarship, Van de Ven and Johnson claim that different paradigm perspectives, like different maps, offer usefully different views of the same territory or firm. So far, all okay for the authors.

In the knowledge food chain, discipline paradigms are at the left end. Van de Ven and Johnson's starting problem is not that there is a

dearth of paradigm perspectives; it is that any one of them comes to a halt before it offers useful information to practitioners. Adding multiple paradigm perspectives—that is, pluralism—appears to me to simply complicate the practitioner problem, not help it. I don't see much reason to think that more paradigms help the knowledge flow problem. It seems as though they could make it worse.

While I think Campbellian realism offers sound legitimacy on which to base organization science, if anything, it makes the flow problem worse by reinforcing the scientific legitimacy of the several castle-like, strong-paradigm disciplines at the left end. There is nothing in the current rules of scientific realism that allows paradigms to accept site- and time-specific findings as broad "scientific" truth claims. Improvements to action research of any kind, virtually by definition, can't overcome this. Another way needs to be found.

### REDEFINING THE FOOD CHAIN

It is hard to imagine mosquitoes and *T. rexes* collaborating on anything, although tickbirds and hippos have a symbiotic relationship. I have never heard of builders collaborating with engineers to design earthquake-safe buildings (although architects obviously do). Practicing physicians, however, often become clinical professors with research grants at medical schools. Thus, in some chains the ends are symbiotic; in others they are not. Why symbiosis in one and not the other? Why physicians doing science and not consultants? M.D.s are different from M.B.A.s?

Mostly, I think the answer lies in the nature of the phenomena. In people, biomolecules are mostly the same from one end of the chain to the other; hearts, lungs, brains, and bones are mostly the same from one end to the other; and quantitative research based on sampling from populations works the same from one end to the other—scientific findings reduced to averages work pretty well. Not perfectly, needless to say. Just listen to the drug ads on TV every night—all those horrible side effects. While most bodies are helped by a particular drug "solution," some clearly are not. Still, averages work quite well for most patients.

With organizations and management I don't think this is the case. What gets stamped as

legitimate research at the left end is discipline-centric quantitative research with large samples, Gaussian statistics, findings reduced to averages, and confidence intervals for statistical significance based on finite variance. Practitioners live in a world of extremes—Toyota, eBay, Google, Southwest, Wal-Mart, and GE are good; Alitalia, Enron, Anderson, WorldCom, Lucent, and the FBI are bad. All of the cases used in M.B.A. classrooms are stories about good and bad examples—extremes, never averages. If one scans "business media" books, such as *In Search of Excellence* (Peters & Waterman, 1982), *Built to Last* (Collins & Porras, 1994), *Hidden Value* (O'Reilly & Pfeffer, 2000), and *Good to Great* (Collins, 2001), one sees that they are mostly about good and bad examples, never about "averages." If one thinks of organization and management phenomena as appearing in all sorts of weird shapes, what happens in discipline research is that all these weird shapes are crammed into the square hole of Gaussian statistics. It's called "robustness." "Extreme" science is spelled out in Andriani and McKelvey (2005).

For example, leadership research keeps producing findings about averages. Practitioners don't give a damn about averages. They want to know how to identify good and bad leaders. The cost of a bad leader at the top is horrendous. Board members and CEOs don't care about average firms; they want firms that generate above-average profits—in any industry only the top few firms generate most of the economic rents. There is nothing in an "average" that tells a company how to have a competitive advantage. Nothing. Yet that is the "knowledge" they get from all the academics at the left end of the strategy food chain. How can studies about averages point to idiosyncratic advantage? It's illogical. All the economists' math in the world can't fix this.

### CONCLUSION

I think the food chain is not so much broken; it's that the wrong stuff is flowing—or would flow, if we all took Van de Ven and Johnson seriously. Practitioners keep looking for T-bone steaks, but what keeps flowing are turkeys. I don't quite see how any amount of engaged scholarship, paradigm pluralism, arbitrage, conflict resolution, big questions, and so forth is

going to turn turkeys into T-bones, even if the flow from the left is renewed. Starting from the right end produces bad science, and, besides, why should firms let the good stuff out? This goes counter to their competing for idiosyncratic advantage. Not that it isn't common knowledge what the problem is, but why would Dell go out of its way to give the good stuff to HP? I hate to say it, Andy and Paul; your dream sounds great, but your path is full of potholes, and, besides, it leads in the wrong direction.

To repeat, the problem is that what has plugged up the food chain are findings about averages. Management researchers need to learn from earthquake science. It is the only legitimate science studying extremes. California has 16,000 "average" quakes per year. No one cares. We design buildings for the "big one" that will come some day. Managers don't worry about averages; they live in a world of extremes, and they want more of the good ones and wonder how to better avoid the bad ones.

Yes, I agree, there are phenomena here and there in firms and industries and societies that fit the Gaussian world. Our bodies live in the world of Newtonian dynamics, but relativity theory is "out there." The managerial world is not so simple; practitioners face *both* independent and interdependent data points (phenomena) in firms and industries. They don't know when their world shifts from Gaussian distributions to extreme events, power laws, and Paretian distributions. They just know that it does and that what they get from academia is invariably from an assumed Gaussian world (McKelvey & Andriani, 2005).

So, Andy and Paul, I say Thanksgiving for practitioners is not about getting turkeys to flow better from the left. Nor does it come from starting from the right; even *T. rexes* can't create their own food—no flow, no food. It's about reinstitutionalizing business school research toward a science of extremes rather than averages. A rigorous beginning in this direction is taken by Baum and McKelvey (2006). Martin, are you listening?

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