International Food and Agribusiness Management Review
Volume 14, Issue 5, 2011

An Epistemology for Agribusiness: Peers, Methods and Engagement in the Agri-Food Bio System

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Abstract

Agribusiness scholars face a significant tension between the research demands of industry peers and academic peers. This tension is created by the difference in how the two sets of peers know what they know—a difference of practical knowledge versus positivistic knowledge. The article explores the epistemologies of practice and positivism, and proposes a third epistemology, grounded theory, that can allow agribusiness scholars to produce rigorous research acceptable and relevant to both sets of peers. A more recent and growing need to address "wicked problems" pushes agribusiness scholars even further toward an epistemology of engaged scholarship. Seven recommendations are provided for guiding future agribusiness research efforts.

Keywords: agribusiness, epistemology, research methods, wicked problems, engaged scholarship, research rigor, grounded theory

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Personal Prologue and Introduction

If one envisions a continuum from scholarly scholar to practical practitioner, I am somewhere between a practical scholar and a scholarly practitioner. As Director of the MSU Product Center, I lead a boundary organization where we translate (1) practitioner knowledge and needs for scholarly work to scholars, and (2) scholarly knowledge and needs for empirical work to practitioners. Those of us who are scholars in the field of agribusiness management often find ourselves in this translational space serving two sets of peers—our academic peers in agricultural, food and resource economics (convert this phase into whatever your department name may be these days) and our industry peers in agricultural, food and bioeconomy firms. What these two sets of peers want from us are distinctly different, and this difference creates pressures on our scholarship that can be difficult to manage.

On the one hand, our industry peers want relevant, actionable prescriptions for firm and market behavior. They are critical of "ivy tower" vocabulary and method. These peers want us to be much the same as they are—to use methods that mimic their ways of knowing and understanding. On the other hand, our academic peers want elegant, rigorous contributions to knowledge. They decry much of agribusiness research as inappropriately qualitative or subjective. These peers also want us to be much the same as they are—to use methods that mimic their ways of knowing and understanding. Two distinct sets of peers, two distinct sets of demands for our behavior. The differences between our two sets of peers arise largely from two distinct ways of knowing what they know—differences of epistemology. If agribusiness researchers cannot find a reasonable epistemology for serving both sets of peers, we will not be effective either because (1) our scientific credentials will be continually at risk within the academy, or (2) our relevance will be continually questioned in industry.

I have taken on this topic before. In 1997, I presented an invited paper to the Agribusiness Research Forum. The paper was never formally published but it has been used nonetheless in various research methodology courses in several agribusiness graduate programs. A significant portion of this paper is an updating and recasting of this earlier work. The message is still needed as long standing tensions between our academic and industry peers are even more relevant today than 14 years ago. The next to last section of the paper is entirely new and comes from recent work in "wicked problems." The paper ultimately argues that agribusiness scholars need to base their research on an epistemology of grounded theory and, for some particularly complex messy problems, an epistemology of engaged scholarship

The Traditional Tension: Epistemologies of Practical and Positivistic Knowledge

The Epistemology of Industry Peers

Our industry peers have knowledge that arises from doing. Their epistemology is straight forward: They know what they know because their knowledge works. Their knowledge is derived from action. Their methods of learning are through practice, stories, rules of thumb, and imitation. Practical knowledge is concrete, emerging from the actual complex and ambiguous context in which action is taken. Since it arises from action, practical knowledge is actionable. It is used to predict and to prescribe. Such knowledge is in part intuitive. The decision maker may be guided by nothing more than a gut feel or a nearly instinctual response.

Practical knowledge is non-scientific.[1] It is inherently subjective in that it is dictated by the unique perceptions, experiences, and practices of the actor. Practical knowledge has low data integrity (Bonoma 1985) in that it can be prone to error and bias. This in turn makes practical knowledge highly specific and thus weakly generalizable. In addition, the decision maker makes little explicit attempt to discern any underlying structure to the situations faced. Situations can thus be either viewed as largely unconnected or too easily assumed to be alike. In either case, past experience tends to be consulted as a guide, rightly or wrongly.

In the vocabulary of the knowledge management literature, practical knowledge is largely tacit (Takeuchi and Nonake 2000) being context-specific and informal arising from experience and practice. It is only made explicit in the form of how-to manuals and best practices lists, but these explicit versions can never carry the full knowledge and nuance embedded in the practitioner's experience. Transferring practical knowledge to others is thus best accomplished through apprenticeship and mentoring (Takeuchi and Nonaka 2000) when knowledge can arise from guided practice.

The agribusiness scholar cannot take on the decision maker's epistemology of practical knowledge for two reasons. First, the agribusiness scholar is part of the academy and thus needs to take on a scientific perspective in order to have legitimacy with academic peers. Knowledge needs to be discovered and communicated in explicit form. Second, even if adopting the epistemology of practical knowledge were legitimate academically, it would add no value to what decision makers already do. To adopt the decision maker's epistemology is to become a decision maker. We cannot in our classrooms, our research, or our outreach merely mimic the decision maker. We can add no value by doing so because we can never know the complete context of the decision situation as well as the decision maker does. Adopting the decision maker's epistemology is thus not a feasible strategy for knowing. Agribusiness research methods should not adopt the epistemology of our industry peers. However, we cannot be so far removed from the decision maker's way of knowing that we cannot contribute to it. We are, in the end, applied scientists.

The Epistemology of Academic Peers

The most obvious contrast with practical knowledge is scientific (or theoretical) knowledge. However, our academic peers tend to pursue a specific form of scientific knowledge that tends to sharpen the contrast with practical knowledge. As evidenced by the American Journal of Agricultural Economics and closely related professional journals, the prevailing academic epistemology of agricultural economists is that of positivism. Its methods are nearly always quantitative (Derbertin and Pagoulatos 1992). This way of knowing is inherently scientific. Positivistic knowledge is derived from theory and learned through deduction. Such knowledge is abstract in

[1]Science like many terms is used extensively but we fail to be precise about its meaning. For this article, science is defined as a method of obtaining knowledge that is objective and verifiable (Titus). The problem is discerning what is objective and verifiable when in practice they are not absolute terms.

that the detail and noise of context are filtered and reduced in search of an underlying cause-and-effect structure and thus it aspires to be objective knowledge. Yet, it attempts to be correspondent with actual data (Johnson 1986), to have construct validity (Yin 1994) and be verifiable. The knowledge is generalizable (has external validity) within the bounds of appropriate assumptions. When positivistic knowledge is properly correspondent, generalizable, and strongly causal, it can have significant predictive power. Such knowledge is, in this sense, often useful to decision makers, even if it solves no specific problem.

So central is the notion of objectivity to positivistic knowledge that some further exploration of what objectivity means is in order. Knowledge is in part deemed objective when it has high data integrity (Bonoma1985); it is free of error and bias. The desire for data integrity drives the academic researcher to seek statistical validity in empirical findings. Objectivity also arises in knowledge that has clarity and coherence (Johnson 1986). Knowledge has clarity if it unambiguous with a unique interpretation, as in a just-specified econometric system. Knowledge has coherence if it follows logically from relevant theory and has no internal contradictions. Coherence and internal validity are comparable concepts. The desire for clarity and coherence drives the academic researcher toward mathematical models with well-defined variables, exact identification, and controlled measurement. In fact, the greatest strength of positivistic knowledge is its clarity and coherence. Finally, knowledge is objective in part because the researcher who discovered it was objective in the search. The researcher applies the tests of clarity, coherence, and correspondence and willingly abides by the results (Johnson 1986).

Positivistic knowledge is ultimately limited by its level of abstraction. The search for underlying structure, clarity, and coherence causes positivistic knowledge to ignore much of the detailed richness of a whole situation. Its currency is limited (Bonoma 1985) in that its contextual relevance is low and thus its applicability to any particular situation is limited. For example, the generalizability of statistical validity has little relevance to a decision maker who must take action in a setting that resembles but does not meet the exact conditions under which the positivistic knowledge was found to hold. Positivistic knowledge is predictive in terms of what may happen and can thus contribute to the analysis of a decision, but it alone is not prescriptive in telling a decision maker how to make it happen. In sum, positivistic knowledge has limits on its ability to guide action precisely because its clarity and coherence does not lead to adequate detail about how and why to do specific actions. Further, positivistic knowledge is weakened if the underlying cause-and-effect structure it claims to explain is itself under change. If the structure is changing, then all insights gained from the knowledge are open to question.

Examining the Tension between Peers

The practical knowledge of industry peers and the positivistic knowledge of academic peers are in clear tension with one another. Agribusiness researchers are pushed in two directions. Industry peers want enhancement of practice while academic peers want confirmation of theory. Certain types of research questions (many of which have been the forte of traditional agricultural economists) allow the distance between practical knowledge and positivistic knowledge to be relatively small resulting in little tension between peers.

Bonoma provides a critical characteristic of research problems that exhibit this short distance. This characteristic is the nature of the phenomena of interest, and here he proposes two subcategories of concern. First, can the phenomena of interest be studied separate from their natural setting? Second, are the phenomena amenable to quantification? If the answer to both questions is yes, then positivistic knowledge and research methods are likely preferred. In this case, concerns about correspondence and prescription can be minimized, and clarity and coherence can be given primary sway. Industry peers will find the research reasonably applicable. On the other hand, if the answer to either question is no, then positivism is of far less use.

The research problems of agribusiness management often exhibit both characteristics of phenomena that fall outside the strengths of positivism. For example, the nature of effective practice in the strategic management of an agribusiness firm is not reducible to a quantifiable issue due to its complexity, but it is an important one for agribusiness researchers to examine nonetheless. The claim here is that in reducing strategic management to its quantifiable elements so much of reality is lost that the positivistic results are of little use. The issue is not merely one of small numbers of observations; the issue is relevant complexity that defies quantification and separation from context. Likewise, if one wishes to understand the dynamics of contract negotiations as they are evolving in the vertical coordination of the agri-food-bio system, some aspects of the phenomenon cannot be easily studied separate from being immersed in the full context of both parties to the negotiation. Some quantification of contract terms and frequency of application can be achieved, but positivistic research alone cannot reveal full insight into the dynamics of how and why. These examples also suggest that institutional economics generally is not likely successfully studied by positivistic methods. Again, the limiting factor is not a matter of small numbers of observations or some restricted ability to quantify, but the fundamental inability to separate the phenomena from context.

Beyond Bonoma’s two characteristics, one additional characteristic of the research setting determines when a positivistic epistemology is appropriate: To what extent is the underlying causal structure stable or changing? If the structure is stable, positivistic knowledge is possible and its methods can be pursued. However, positivistic knowledge can be of very limited use in times of significant structural change.

Again, most research issues of greatest relevance to agribusiness scholars fall outside the purview of positivism. First, the fundamental shifting of agricultural business structures and market arrangements (for example, the emergence of the food and fuel controversy as energy and food markets synchronize) suggests that underlying structure is changing dramatically. No stable underlying structure exists to study in many key situations. Second, research into the area of business strategy for agribusiness firms is the study of how firms can create and choose strategic alternatives that have as their fundamental motivation altering the structure of the industry in which the firms operate. When the goal of the phenomenon being studied is to alter structure, how can the phenomenon be studied with methods that assume the stability of structure? Third, even when phenomena of interest appear stable, there may be no fundamental underlying structure to find. Long ago management researchers gave up the notion of a general theory of management in that there is no one true way to manage. The study of management only gives rise to what can be termed contingency or substantive theory (Gummesson 1991)—theory only made relevant in specific contexts. Management research seeks to define the contingent characteristics of circumstances that determine which of many alternative managerial approaches is best suited to a particular situation. Positivism is of limited use in this effort.

When the theory is strong, the phenomena are quantifiable and separable from context, and the structure is stable, positivistic epistemology and methods are appropriate. The distance between our academic peers and our industry peers is relatively short in that theory leads to application rather efficiently and effectively. However, most current phenomena of greatest interest to agribusiness scholars (the food and fuel controversy or strategic management generally) are amenable in only limited ways to the positivistic approach. Positivism can address side questions (frequency, trend, and correlation) but not the main questions (how and why).

Further, when underlying cause-and-effect structures are shifting or too complex to be reasonably understood even in the abstract, practical knowledge is probably also at its weakest usefulness. Rules of thumb cease to apply, standard operating procedures become ineffective, and normally reliable business instincts mislead. It can be hypothesized that the distance between our peers becomes a chasm in this case. Industry peers want scholars to provide guidance precisely because the changing context takes them beyond the bounds of their experience. Our traditional scholarly peers respond based on their normally reliable models, but end up making recommendations that prove ineffective precisely because the changes take them beyond the bounds of their theory. Our industry peers become very distresses with scholars at that point. When this occurs, there is an alternative epistemology that can resolve the tension between our peers and offer a unique contribution for agribusiness research endeavors.

An Alternative Epistemology: Grounded Theory

Practical knowledge and positivistic knowledge both have great strength in their relevant domains—practice and theory respectively. Both face limits (perhaps severe) when: (1) new phenomena fall outside the realm of their existing domains—in which either practice fails to be transferable or known theory does not apply, or (2) the causes of phenomena are so complex that practice cannot effectively deal with them and positivism provides ineffective partial explanations.

Professional schools—law, medicine, business—have recognized the limits of practice and positivistic science for a long time. They have a history of case research and teaching as a significant part of the answer to the epistemological limits of other approaches. More generally, so-called qualitative methods have emerged to fill the gap identified. The methods are not themselves an epistemology but imply the existence of one. Qualitative methods encompass a wide variety of approaches: hermeneutics (Gummesson 1991; Jankowicz 1995) as well as naturalistic inquiry, social constructionism, and new paradigm inquiry (Easterby-Smith, Thorpe and Lowe 1991); reflection-in-action (Schon 1995); various forms of direct reference as qualitative research methods (Jankowicz 1995; Cassell and Symon 1994; Easterby-Smith, Thorpe and Lowe 1991; Ghauri, Gronhaug and Kristianslund 1995). Bitsch also lists ". . . phenomenological research, ethnomethodology, ethnography, qualitative case study, participatory action research, and grounded theory." (Bitsch 2005, 77) All of these approaches have at least some intellectual ancestry in philosophical pragmatism (Johnson 1986).

In the 1997 version of this paper, phenomenology was used as the term to reference these collective approaches. More compelling for this update is Bitsch's use of grounded theory as the best representation of these methods and one that comes closest to describing an epistemology. As she states, "Grounded theory, first published in 1967 by Glaser and Strauss, is the master metaphor of qualitative research." (Bitsch 2005, 77) Grounded theory is about extracting theory from the data and information of actual context. It involves a cycle that starts with a phenomenon of research interest, moves to collection of rich context-based information, induces a first round of theory based on the concepts and constructs that arise from the information, continues with another round of data gathering and induction, and ends when the induction-deduction cycle ceases to evolve the theory. Bitsch is highly effective in elaborating the approach in substantial detail. An abbreviated discussion is presented here.

At the heart of a grounded theory epistemology is the notion that the phenomena of interest cannot be separated from their context. To study human phenomena, the researcher must understand the holistic nature of the situation that created it. Behavior and context are fundamentally interdependent. In this view, reality is socially constructed by the actors involved in the phenomena. To understand the phenomena, the researcher must understand the meanings and motivations of the actors.

Similar to Schon's concept of reflection-in-action, grounded theory knowledge can be thought of as built upon making explicit what decision makers know implicitly; and, by making it explicit and testable, the knowledge can become more objective rather than merely subjective. Schon argues that knowledge arises from "subjecting to critique and testing the strategies, assumptions, or problem-settings implicit in a whole repertoire of situational responses." (Schon 1995, 31).

Grounded theory knowledge is derived from an iterative process that is both inductive and deductive. The academic researcher must observe the actual situation and the actions taken. To these observations, the researcher attaches meaning through classification and comparison based on existing theory and/or the logic of the situation itself. The researcher forms a tentative hypothesis about the action, its causes, and its results. This hypothesis can then be tested against other decision situations. Subsequent testing will determine whether the hypothesis holds, needs to be modified, or abandoned. This is what Bonoma calls the theory/data/theory revision cycle that he recommends to drive the process of case research. It is also akin to some of the defining characteristics of qualitative research more generally in which the researcher seeks "to formulate new hypotheses and alter old ones as the research progresses, in the light of emerging insights." (Cassell and Symon 1994, 4).

Decision makers themselves often engage in such an iterative process in real time. Schon gives an example of how a decision maker engages in action, is surprised by the results of the action, and instantly restructures his or her understanding of the situation. "On the basis of this restructuring, he invents a new strategy of action and tries out the new action he has invented, running an on-the-spot experiment whose results he interprets, in turn, as a 'solution,' an outcome on the whole satisfactory, or else as a new surprise that calls for a new round of reflection and experiment." (p. 30) The academic researcher can make this process explicit, expand it to multiple situations, and bring theory and objectivity to the iterative process.

Grounded theory knowledge is scientific. Its Kantean cycle of induction, deduction, and verification is a form of the scientific method (Kenemy Titus). Grounded theory knowledge is abstract in that it is articulated in the medium of words and ideas. It can meet the criteria of objectivity, clarity, coherence, and data integrity. (The degree to which it meets these criteria will be addressed in the next section.)

The ability to generalize grounded theory knowledge is an obvious and central issue in its legitimacy as an epistemology. Citing and elaborating on Guba and Lincoln, Bitsch presents transferability as paralleling external validity and generalizability in quantitative research. "Transferability refers to determining the extent to which findings can be applied in other contexts or with other respondents . . . . the burden of prove shifts from the researcher to the person who wants to apply the research results." (Bitsch 2005, 85) The user is aided in transferability by the researcher's use of (1) "thick description" (Geertz) that provides interpretative and rich enough detail for judging probable alternative applications, and (2) purposeful sampling that assures the research process examined typical and atypical cases to test the limits of application scope. (Bitsch 2005) Schon calls for generalization ". . . not as covering laws[2] but through what I call 'reflective transfer,' that is, by carrying them over into new situations where they may be put to work and tested, and found to be valid and interesting, but where they may also be reinvented." (p. 31) Yin posits that generalizing case findings is not the same as statistical generalization in positivism. Rather, case studies, as with experiments in the natural sciences, rely on *analytical* generalization from a particular set of results to some broader theory. Gummesson argues that local theory applicable to particular situations has value in and of itself even if broad generalization is not possible. The situation adds new richness to the understanding of possible behaviors and responses.

Grounded theory knowledge has an inherent dynamism that makes it highly useful in times of change. Grounded theory methods can be used even if the underlying structure is not stable. Working hypotheses can be readily altered and expanded in order to maintain correspondence with emerging conditions. The methods of grounded theory reflect the claims of Cassell and Symon for qualitative methods more generally in that these methods ". . . are sensitive enough to allow the detailed analysis of change. . . . With quantitative methods we may be able to assess that a change has occurred over time but we cannot say how (what processes were involved) or why (in terms of circumstances and stakeholders)." (p. 5)

The differences between practical, positivistic and grounded theory knowledge are presented in Table 1. Grounded theory knowledge can add value for decision makers because of its increased levels of objectivity and generalizability versus practical knowledge. Decision makers can be less given to error in experience transfer and in understanding what factors actually matter in the decision situations faced. In contrast to positivistic knowledge, grounded theory knowledge finds its greatest applicability to research settings in which established theory is weak or nonexistent, the phenomena of interest are not readily quantifiable nor separable from context, and the underlying cause-and-effect structure is unstable or not given to general theory.

[2]By covering law Schon means "a general, perhaps statistical, proposition applicable to all instances in which certain combinations of variables are present." (p. 31)

Table 1. Comparative Aspects of the Three Types of Knowledge

Aspects of Knowledge	*Practical Knowledge*	*Grounded Theory Knowledge*	*Positivistic Knowledge*
Goals for Understanding	How things are done. Replication of past success. Development of standard operating procedures.	Why things happen in a socially-constructed world. Development of “local” theory.	Why things happen in an external and objective world. Development of “general” theory.
How Learned	Practice, story, experience, rules of thumb, imitation. Trial and error.	Induction-deduction-validation cycle with emphasis on induction. Scientific method.	Induction-deduction-validation cycle with emphasis on deduction. Scientific method.
Relevance of Context	Mostly concrete Holistic	Moderately abstract Holistic	Mostly abstract Reductionistic
Form of Knowledge	Mostly tacit with explicit expression of best practices or procedures	Mostly explicit with cautions about users sensitivity to tacit practice	Explicit to the point of precise replication
Objectivity	Mostly subjective	Mostly objective with qualitative safeguards	Mostly objective with quantitative safeguards
Reliability			
• **data integrity**	Low	Potentially high	High
• **construct validity internal validity**	N/A	High	Potentially high
• **clear causality**	Low	Moderate	High
• **coherent causality**	Low	Moderate	High
Ability to Generalize (External Validity or Transferability)	Limited by experience	Analytically transferable	Statistically transferable
Predictive Power	High within bounds of experience	High within bounds of working hypotheses	High within bounds of theory
Prescriptive Power	High	High	High to limited depending upon level of detail needed
Actionable	High	High	High to limited depending upon complexity of context
Ability to Address Changing Structure	Moderate	High	High to limited depending upon method of derivation

The three types of knowledge—practical, positivistic, and grounded theory—lie on a continuum. Some decision makers pursue practical knowledge in a nearly scientific manner, searching for underlying structure and attempting to drive out bias and thus moving across the continuum toward grounded theory knowledge. Positivists can and do give up some of their clarity and coherence to improve the correspondence of what they know about the world, and thus they move across the continuum. Some quantitative techniques, such as factor analysis, occupy a spot on the continuum between grounded theory and positivistic knowledge (although purists on both sides may not agree.) Objectivity and subjectivity, as well as concreteness and abstraction, are not absolute terms empirically. The issue really becomes what tradeoffs scholars or practitioners are willing to make in order to know what they know.

Grounded Theory Knowledge and Rigor

To a strict adherent of positivistic epistemology, the objectivity and generalizability of grounded theory knowledge may appear questionable. Positivists will argue that this third way of knowing lacks what has come to be called "rigor." Ultimately, academic legitimacy hinges on whether or not an appropriate rigor can be defined for grounded theory knowledge. If for no other reason, agribusiness scholars must find reasonable ways to signal in journal articles and other scholarly writings that the standards of grounded theory rigor were known and followed by the author. This signaling will not be easy since the more qualitative nature of grounded theory knowledge necessitates lengthy output that may strain usual editorial standards for article length or the reader's patience in wading through the material (Cassell and Symon 1994). A corollary to this point is that the reviewers for journals must accept the legitimacy of grounded theory methods and be prepared to provide appropriate critique.

If rigor is defined by the careful adherence to tests of scientific validity and reliability, the evaluation of grounded theory research can achieve both tests. Appropriate standards of rigor can be articulated, but these standards differ from positivistic standards. Based on the complexity and ambiguity of real decisions, grounded theory knowledge will never achieve the level of clarity or coherence argued earlier to be the hallmarks of positivistic knowledge. The tradeoff is heightened correspondence and improved prescription. Grounded theory knowledge is actionable in that the richness of context can be significantly preserved while some level of abstraction is sacrificed.

Table 2 attempts to provide a starting point for defining rigor for grounded theory knowledge. It is beyond the scope of this paper to go further. (See Bitsch for a more extensive examination of this issue.)

First, grounded theory knowledge has been rigorously derived if appropriate research methods were used. The preferred methods of conducting grounded theory research include, but are not limited to, case studies, archival analyzes, semi-structured or fully-structured interviews and surveys, field experiments, critical incident analyzes, repertory grid techniques, cluster analysis, factor analysis, conjoint analysis, and structural equation modeling. The earlier entries in this list are largely qualitative, but the latter entries involve quantitative analysis, albeit mostly of qualitative (often categorical) data. Rather than define and elaborate on each of these techniques here, the author simply wants to establish that these techniques exist and have a supporting literature of their own.

Second, construct validity, internal validity, reliability and external validity can be achieved for grounded theory approaches, and the key questions related to assessing each of these is presented in the table. Most especially, researchers should focus on high correspondence and effective prescription as standards by which grounded theory knowledge is judged. In addition, data integrity must be a critical concern and should be based on (1) proper triangulation (Bonoma, Cassell and Symon, Yin, Bitsch) in one or more of four forms self-reported and archived information, multiple investigator perspectives, multiple theoretical perspectives, or multiple methods of gathering and interpreting data, and (2) appropriate precautions against researcher bias arising from close interaction with decision makers. Clarity arises from careful description, classification, and

Table 2. Comparative Characteristics of Grounded theory and Positivistic Knowledge

Characteristics of Knowledge	*Grounded Theory of Knowledge*	*Positivistic Knowledge*
Researcher Goals:	Focus on meanings. Try to understand happenings. Look at the totality of each situation. Develop ideas through induction from data. Develop "local" theory.	Focus on facts. Seek causality & fundamental laws. Reduce situation to simplest elements. Formulate hypotheses and then test them. Develop "general" theory.
Applicable Research Setting:	Theory construction Phenomena need not be quantifiable Phenomena not separable from context Unstable or nonexistent structure	Theory confirmation Quantifiable phenomena Phenomena separable from context Stable underlying structure
Preferred Methods:	Using multiple methods to establish different views of phenomena. Small samples investigated in depth or over time.	Operationalizing concepts so that they can be measured. Taking large samples.
Construct Validity	Has the researcher gained full access to the knowledge and meaning of inform-ants?	Does an instrument measure what it is supposed to measure?
Internal Validity (Clarity and Coherence)	Has the researcher uncovered the logic of the phenomena observed either by applying existing theory or laying bare the inherent order of the situation itself in new theory?	Has the researcher properly deducted and tested the hypothesis?
Reliability (Data Integrity)	Will similar observations be made by different researchers on different occa-sions? Has triangulation of data been appropri-ately handled?	Will the measure yield the same results on different occasions (assuming no real change in what is to be measured)?
Generalizability (External Validity or Transferability)	How likely is it that ideas and theories generated in one setting will also apply in other settings?	What is the probability that patterns observed in a sample will also be pre-sent in the wider population from which the sample is drawn?

Source. Rows 1, 3, 4, 6 and 7 are adapted from Easterby-Smith et al.

comparison of observed situational phenomena rather than from precise definitions and measurements. Thus clarity is qualitatively achieved (based on experience) rather than quantitatively. Coherence arises by bringing logical order to the phenomena observed either by applying existing theory or laying bare the inherent order of the situation itself in new theory. Objectivity arises from the clarity, coherence, and data integrity already mentioned, and in addition from subjecting both the methods and results to peer review. Rigor is attainable for grounded theory knowledge, and agribusiness scholars have a responsibility to properly define it and practice it.

The New Tension: Wicked Problems and Engaged Scholarship

An epistemology of grounded theory rigorously carried out in methods can go a long way toward allowing agribusiness scholars to serve their two sets of peers, industrial and academic. But, such an epistemology may not be enough for a certain class of problems that we are increasing asked to address, so-called wicked problems (Rittel and Webber 1993; Conklin 2006; Batie

2008). Wicked problems, a term from the 1970s social planning literature, have the essential characteristic that they are not solvable; they can only be managed. As part of their management, special roles exist for both new knowledge and full stakeholder engagement in the research process itself.

Sustainability serves as one example of a wicked problem that many agribusiness scholars (as well as many agricultural natural scientists) are being asked to address today. Table 3 presents a list of defining criteria for a wicked problem and how sustainability meets these criteria. Fuel vs. food, global warming, and even business strategy itself (Camillus) are other examples.

Table 3. Sustainability as a Wicked Problem

Criteria for a Wicked Problem	Sustainability
No definitive formulation of the problem exists.	Ideal definition lacks specificity and is reduced to slogan or tagline such as triple bottom line (economic, social and environmental) performance
Its solution is not true or false, but rather better or worse.	One can never know if sustainability has been achieved. Only progress in its trajectory can be predicted.
Stakeholders have radically different frames of reference concerning the problem, and are often passionate in their position on the problem.	Businesses strongly favor economic outcomes. Environmental groups strongly favor environmental outcomes. Social justice groups strongly favor social outcomes, such as fair wages and equitable access.
System components and cause/effect relationships are uncertain or radically changing.	Many claims are made about what is sustainable (such as local food systems are sustainability while global food systems are not) with unclear knowledge of what system characteristics assure or even promote sustainability.

Based on the criteria, one realizes why wicked problems cannot be solved—they have no closed-form definition, their "solution" can only be thought of in relative terms, stakeholders will be in conflict over solutions and actions, and the system is not understood well enough to effect entirely purposeful change. Wicked problems can be managed and their effective management focuses on actions toward two desired system outcomes:

- *Improved impact*, moving system components in a desirable direction
- *Meaningful process*, effectively responding to the relevant stakeholders who can veto as well as enable action in any direction

Potential options to improve impact can be meaningless if the process results in stalemate, while endless process can result in no action to improve impact. Impact and process outcomes must be achieved simultaneously.

Further, each stakeholder brings strongly held existing knowledge to the management process. This existing explicit and tacit knowledge is deficient in two respects:

- Existing knowledge of one stakeholder is suspect to other stakeholders because of issues arising from trust, transparency and credibility of sources.
- Existing knowledge freezes the system tradeoffs that give rise to the conflicting system outcomes that divide the stakeholders in the first place.

The argument runs that only new knowledge can overcome these deficiencies. The process has legitimacy when the new knowledge is derived together with the stakeholders. When the creation of the new knowledge centers on system innovation, then more acceptable impact tradeoffs can emerge even to the point of converting existing tradeoffs into new complements through deeper systems understanding and redesign.

Knowledge institutions and their scholars have a role in managing wicked problems like sustainability when they understand how research can be beneficial to the process outcomes as well as the more traditional impact outcomes. Given the messy underlying understanding of the system at work in a wicked problem, the grounded theory epistemology already advocated in the paper would seem to have great fit to this context. Agribusiness scholars would seem to be of significant value to the context as well.

An epistemology of grounded theory may be a necessary condition for contributing to the management of wicked problems, but is likely not sufficient on two counts:

- Many more disciplines are needed than those of agribusiness scholars to address the full system analysis and synthesis needed for impact outcomes. These problems are even beyond multidisciplinary approaches (pooling individual disciplinary knowledge) demanding instead transdisciplinary approaches (collective disciplines creating new knowledge together).
- The stakeholders need to be engaged throughout the research enterprise in order to have its results be meaningful and legitimate to the desired process outcomes. The stakeholders cannot merely be there at the beginning of process (to articulate their needs) and at the end of the process (to receive the results). They must be there throughout the process to assure that the research stays on track and will have stakeholder credibility when the results are known. The researcher will need to manage the rigor of the research, but the research will be done in a fishbowl unlike our traditional research expectations of objective separation.

An epistemology of engaged scholarship that encompasses all of the above is essential when working in the arena of wicked problems. This realization is entirely consistent with the historic and contemporary literature that surrounds wicked problems. (See Peterson 2009; Batie 2009; Fear et al. 2006; and Bitsch (2009) for contemporary analyses related to agricultural and natural resource systems.) If agribusiness scholars are to excel in this arena, then they must work with rigor not just in grounded theory but also in engaged scholarship. There is no rest for the weary. We are called to even greater challenges by our peers.

Recommendations

What then should we do as agribusiness scholars to assure that we serve our traditional industrial and academic peers and the even broader set of stakeholder peers facing wicked problems? Seven recommendations are made.

First, we should pursue grounded theory knowledge and adopt its methods when our research calls for such an approach. As already argued, grounded theory knowledge adds value for our business peers and keeps us in the academy with our academic peers. But beyond that, the rapid changes occurring in the agri-food-bio system and the presence of wicked problems signal that causal relationships are in a state of flux or system complexity makes them extremely hard to uncover. In either case, grounded theory knowledge is especially appropriate. The phenomena we study, both inter-firm and intra-firm, are not easily studied separate from the richness of context and are not readily given to quantification. Grounded theory knowledge fits well with the situations in which we conduct the vast majority of our research as scholars.

Second, as members of the academy, we bear a responsibility to define further an appropriate level of rigor for grounded theory knowledge and its methods. This paper only begins this process. We need to consult the research methods of related social sciences and mine the richness of methodological diversity found there.

Third, we must teach grounded theory methods to our graduate students and learn how to use them ourselves. As agricultural economists, most of us have been trained in positivistic methods and most of our graduate programs require that our students learn positivistic methods. Qualitative and grounded theory methods must be adequately represented in our curricula. Quantitative techniques more appropriate to qualitative data, including conjoint analysis, factor analysis, cluster analysis, structural equation modeling, must also be part of the curricula. We will quickly need to determine to what extent the traditional agricultural economics doctoral program can produce scholars that have adequate backgrounds to do both positivistic and grounded theory research. Two distinct, yet compatible programs may well be called for.

Fourth, as agribusiness scholars, we must willingly recognize when positivistic knowledge will be most helpful. When the theory is strong, the phenomena are quantifiable and separable from context, and the structure is stable, we need to recognize the legitimacy of the positivistic epistemology. In addition, our grounded theory insights can enhance positivistic theories and methods in order to improve their correspondence to the world we encounter. We need broad collaboration across methods, and not intellectually pure camps trading barbs. At the same time, our research cannot be merely derivative of or subservient to our traditional agricultural economics peers. We must add value through our different perspectives and approaches.

Fifth, in a world of wicked problems, we must use our command of grounded theory to contribute to transdisciplinary research and to engage with stakeholders in this arena. Our contributions here may be even more challenging to our existing peers in the academic, but I suspect that our industry peers need our participation in managing these even more intractable problems.

Sixth, we must test our research-derived knowledge with our industry peers and not just our academic peers. Do practicing managers find our research results actionable? Do our research results further the evolution of management practice? Industry peers need to answer these questions in the affirmative if we are to be judged relevant. Further, we need to have our industry peers engage in our work in exchange for our delivering relevant research-based knowledge. We need access to qualitative and quantitative data and information. Continuing privatization of data and limiting access to industry decision makers make meaningful grounded theory research harder to pursue effectively. Industry peers need to open appropriate access to us, and we need to honor that access with appropriate confidentiality. We also need them to open their minds to the limitations of their own practice and to the usefulness of science and theory when it comes to transferring knowledge from one application to another or one situation to another. We need to intellectually spar with each other and not merely have one side or the other represent what they know as ultimate truth (practically or theoretically) rather than the best available knowledge for the moment. This process requires rich, vital relationships between industry and scholars, and not incidental meetings here and there at conferences, nor encounters merely about students for employment.

Finally, we must reach out to our two sets of peers and ask that they understand our potential and our limitations as well as their own if we are to work together effectively. Our business peers must understand that we cannot mimic their way of knowing or that of practicing business consultants. Their world is one of practical knowledge. Our academic peers must understand that we cannot mimic, in most instances, their positivistic knowledge because it removes us from the context in which actual decisions must be made. In return, we must strive to retain our commitment to science and to a research rigor that is appropriate to grounded theory and ultimately to engaged scholarship.

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