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Title

Developing the Field Site Concept for the Study of Cultural Evolution

Permalink

<https://escholarship.org/uc/item/5d38j7j0>

Journal

Cliodynamics, 7(2)

Authors

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Publication Date

2016

DOI

10.21237/C7clio7233542

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SOCIAL EVOLUTION FORUM

Developing the Field Site Concept for the Study of Cultural Evolution.

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As the study of human cultural evolution matures, field sites will increasingly have a role to play, just as they have in the study of genetic and cultural evolution in nonhuman species. Progress, however, may not be easy due to complex intellectual histories and disciplinary norms. Cultural anthropology and sociology, the two most field-oriented disciplines in the human behavioral sciences, have been among the most avoidant of evolutionary theory. In other branches of the human behavioral sciences, the bulk of research is conducted on college students in the laboratory without any reference to their cultures or everyday lives.

The newly formed Cultural Evolution Society (CES) is in a unique position to facilitate the creation of field sites around the world. The Social Evolution Forum is therefore pleased to feature two essays on the topic by David Sloan Wilson, an evolutionary biologist by training, and Harvey Whitehouse, a social anthropologist by training. Together with commentaries by authors with diverse perspectives on field research, we hope to catalyze the formation of field sites for the study of cultural evolution around the world.

David Sloan Wilson. *Developing the Field Site Concept for the Study of Cultural Evolution: An Evolutionary Biologist's View*

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The study of human cultural evolution has made enormous strides over the last three decades. For most of the 20th century, evolutionary biology was highly gene-centric and the human behavioral sciences developed largely without reference

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*Citation: Wilson, David Sloan and Harvey Whitehouse. 2016. Developing the Field Site Concept for the Study of Cultural Evolution (with comment). *Cliodynamics* 7: 228–287.*

to evolution. Now, the study of evolution is increasingly becoming centered on the concept of heredity, with genes constituting only one mechanism of inheritance. Other mechanisms include epigenetics, forms of social learning found in many species, and forms of symbolic thought that are distinctively human (Jablonka and Lamb 2006). The human capacity to transmit large amounts of learned information across generations is now properly seen as both a product of genetic evolution and a process of evolution in its own right. More than ever before, human cultural diversity is being studied with the same set of theoretical and empirical tools as the study of biological diversity (Henrich et al. 2008; Henrich 2015; Richerson and Christiansen 2013; Richerson and Boyd 2006; Wilson 2012; Wilson et al. 2016).

Field studies are the backbone of research in evolutionary biology because the only way to understand the properties of species is in relation to their environments. Laboratory research is also essential but must be informed by field research; otherwise, it runs the risk of asking misleading and nonsensical questions. Reliance on field studies is second nature for an evolutionary biologist. Field studies take place at geographical locations, or field sites. Many biological field sites are just the places where individual scientists conduct a single study, but some field sites become locations where studies build upon other studies. Famous examples include the field site established Peter and Rosemary Grant for the study of Darwin's Finches (Grant and Grant 2014) and Gombe Park in Tanzania for the study of Chimpanzees (Goodall 2010). Some field sites are established and operated in a top-down fashion, such as the [Hubbard Brook experimental forest](#) in New Hampshire, USA, or the Long Term Ecological Research ([LTER](#)) Network funded by the National Science Foundation in America. Others become established in a bottom-up fashion starting with a single modest study, an important point to which I will return below.

Field studies have a different status in the human behavioral sciences. They form the backbone of research in cultural anthropology and sociology, but these disciplines have historically been the least inclined to adopt an evolutionary perspective and many cultural anthropologists also eschew scientific methods. Most of the other branches of the human behavioral sciences do not study people in relation to their past and present culturally influenced environments or base laboratory research on field studies, with the attendant risk of asking misleading and nonsensical questions.

Since field studies have such a marginal status in the human behavioral sciences, it follows that the concept of field sites is also underdeveloped. The best examples come from quantitative sociological research such as the Project on Human Development in Chicago Neighborhoods ([PHDCN](#)) headed by Robert J. Sampson. While site-based research projects such as this one are admirable and

sophisticated in many respects, they are typically not informed by a modern biocultural evolutionary perspective.

It follows that work is required for field studies and field sites to play a role in the study of human cultural evolution comparable to the role that they play in evolutionary biological research. Some of the work is *conceptual*—making the role of field studies second nature as part of adopting an evolutionary perspective. Some of the work is *physical*—creating an infrastructure at geographical locations for studies to build upon other studies.

The purpose of this target essay is to place the development of field sites firmly on the radar screen of the newly formed Cultural Evolution Society (CES) and other individuals and organizations that want to promote the study of human cultural evolution. I plan to do this in a conversational way, through the lens of my own experience as someone trained in evolutionary biology, who conducted numerous field studies on nonhuman species earlier in my career, and who now conducts human-related field research in my city of Binghamton, New York (Wilson 2011). A companion essay by my friend and colleague Harvey Whitehouse will relate his experience as a cultural anthropologist who conducted traditional ethnographic research earlier in his career and is now actively engaged in field-oriented human evolutionary research in sites around the world.

Before telling our own stories, we want to stress that we regard ourselves as fellow travelers, rather than leaders, in developing the field site concept for the study of cultural evolution. Our efforts have been marked by failures in addition to successes and we hope that both will be instructive. Others have made as much or more progress than we have and some of them will be sharing their stories in commentaries on our target articles. We hope that the combined experience of the commentators and ourselves will help to catalyze the creation of field sites for the study of cultural evolution around the world, with the CES playing a lead role.

The Experience of an Evolutionary Field Biologist

I was lucky to enter graduate school in the 1970's when the historically separate disciplines of ecology, evolution, and behavior were growing together. This was the decade that included Dobzhansky's (1973) declaration that "nothing in biology makes sense except in the light of evolution", the award of the Nobel Prize in Medicine to Konrad Lorenz, Karl von Frisch, and Niko Tinbergen, and the publication of E.O. Wilson's (1975) *Sociobiology: The New Synthesis*.

Tinbergen's (1963) now classic article titled "The Methods and Aims of Ethology" was part of my core reading as a graduate student. In his effort to establish ethology (the study of animal behavior) as a branch of biology, Tinbergen pointed out that four questions must be addressed for all products of evolution, concerning their function, history, mechanism, and development. Ever

since, “Tinbergen’s Four Questions” have been cited as a compact description of a fully rounded evolutionary approach—and they are relevant to *all* products of evolution, no matter what the mechanism of inheritance (see Wilson and Gowdy 2013 for a discussion of Tinbergen’s Four Questions in relation to economic theory and practice).



Figure 1. The author in South Africa.

All four questions require knowledge of the organism in relation to its environment. This goes without saying for the function and history questions, but it also holds for the mechanism and development questions. To illustrate this point, imagine being told to study the developing and mature brains of two species of birds without being told anything about their ecology. Unbeknownst to you, one species migrates south during the winter and is adapted to memorize the night sky as a nestling. The other species does not migrate and is adapted to memorize the locations of thousands of food items that it stores every fall. How

many decades would be required for you to discover these brain mechanisms, in the absence of information about each species in relation to its environment?

As the study of human cultural evolution matures, field sites will increasingly have a role to play, just as they have in the study of genetic and cultural evolution in nonhuman species. Progress, however, may not be easy due to complex intellectual histories and disciplinary norms. Cultural anthropology and sociology, the two most field-oriented disciplines in the human behavioral sciences, have been among the most avoidant of evolutionary theory. In other branches of the human behavioral sciences, the bulk of research is conducted on college students in the laboratory without any reference to their cultures or everyday lives.

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Centralized planning plays a larger role at some field sites, especially when supported by major funding from public and private sources. An example is the [Long Term Ecological Research Network](#), which is supported by a dedicated branch of the National Science Foundation. This kind of “Big Science” is required to tackle some questions, but there can also be inefficiencies associated with centralized planning and large bureaucracies of all sorts.

Envisioning a Comparable Role for Field Sites in the Study of Cultural Evolution

Against this background, what would the study of cultural evolution be like if it were comparable to the study of biological evolution? Researchers would employ Tinbergen’s fully rounded “Four Questions” approach. They would use quantitative observational and experimental methods in the field whenever possible and their laboratory experiments would be based upon a foundation of fieldwork. Colleges and Universities would maintain field sites for the study of cultural evolution and at least some centrally planned “Big Science” projects would be funded by public and private foundations.

Obviously, the actual study of cultural evolution is a far cry from this description, but it is useful to keep it in mind as something to work toward. In addition, it is encouraging to know that long-term field sites aren’t required for many kinds of field research (any location will do) and can come into existence

incrementally using the “termite” model, without requiring the major initial investment required by the “Big Science” model.

It is especially feasible for any college or university to create a field site for the study of cultural evolution. Biological field sites require a relatively large area of natural habitat, living facilities and laboratories on site, and so on. For the study of cultural evolution, the community surrounding the college or university can be the field site, faculty and students already live on the site, and the laboratories are already located on campus. All the ingredients of a field site are present without formal designation required.

Something that is not always present, however, is the right mindset for the faculty and students conducting the research. As I stressed at the beginning of this essay, *conceptual* work is required in addition to *physical* work for field sites to play the same role for the study of cultural evolution as for the study of biological evolution. At almost all colleges and universities, the administration is eager to foster good relations with the community and numerous faculty and students are already doing community based research and action. At my university, which is located several miles away from the city of Binghamton, a downtown building was recently constructed that houses the College of Community and Public Affairs, including the departments of Social Work, Human Development, and Public Administration. Without wanting to disparage the research that takes place in these departments (at many colleges and universities, not just my own), the following statements are empirically supportable.

Most of the research is oriented toward the solution of practical problems and does not contribute much to basic scientific knowledge.

Each problem tends to be considered in isolation, resulting in an “archipelago” of research communities with little communication among “islands”.

The quality of empirical research is highly variable. The best is very good indeed, but many studies are entirely descriptive and many programs are poorly designed and assessed.

All of these problems can be solved by adopting evolution as a unifying theoretical perspective.

If basic scientific knowledge requires studying people in relation to their culturally influenced environments, then field research in one’s community can merit publication in top academic journals in addition to addressing practical problems. It is possible to have one’s cake and eat it too with respect to basic and applied research.

Evolutionary theory provides a common theoretical language that can integrate previously isolated research communities.

A unified theoretical perspective and enhanced communication among research communities can improve the average quality of empirical research.

The Binghamton Neighborhood Project

My own attempt to use my city as a field site, which I dubbed the Binghamton Neighborhood Project (Wilson 2011), can be used to illustrate these points. The concept is transferrable, as Daniel Nettle has shown with his Tyneside Neighbourhood Project in the UK (Nettle 2015). In termite-like fashion, the BNP started with a single study that was conducted without any external funding. With my PhD student Daniel O'Brien, I collaborated with the Superintendent of the Binghamton City School District to give a survey to public school students in grades 6–12. In practical terms, the survey measured internal and external assets that are required for healthy human development. In basic scientific terms, the survey measured the conditions required for prosociality (behaviors and attitudes oriented toward the welfare of others and society as a whole) to evolve as a social strategy in a Darwinian world—whether by genetic evolution, cultural evolution, or the expression of behaviors in phenotypically plastic individuals. Very simply, for prosociality to succeed as a social strategy, those who give must also receive. We were able to demonstrate an impressively high correlation between the prosociality of the individual student and the prosociality of the student's social environment, including family, neighborhood, school, religion, and extracurricular activities. In fact, the correlation coefficient was higher than the correlation coefficient between full siblings (r) in simple models for the genetic evolution of altruistic behaviors (Wilson, O'Brien and Sesma 2009).

Although social support need not be spatially based, it has a very strong spatial component, as we saw when we combined our survey data with the residential locations of the students from school records as shown in Figure 1 (abiding by human subject research guidelines, of course). On a scale where individuals can vary between 0 and 100 in their self-reported prosociality, the average prosociality of students in a neighborhood can vary by as much as 50 points.

This study led to other studies to validate and extend the survey results. We made naturalistic observations of prosocial behaviors in neighborhoods, played experimental games with students in their classrooms, and employed the “lost letter method” from the field of social psychology to experimentally demonstrate variation in behaviors in the different neighborhoods (see Wilson 2011 for a book-length account). In one set of experiments, Binghamton University college students viewed photographs of different neighborhoods and then played experimental economic games with public school students from the neighborhoods (O'Brien and Wilson 2011). We could do this because we had previously played economic games with the public school students in their classrooms. Knowing their residential locations, we could pair their responses to the responses of the college students in the subsequent study. The results showed

that merely viewing a photograph of a neighborhood strongly influences the propensity to cooperate or defect in an experimental game. Daniel Nettle (2015) has gone even further by bussing students into different neighborhoods to complete surveys and play experimental games, with huge effects compared to the same surveys completed and games played on campus.



Figure 2. Mapping prosociality in Binghamton, New York.

This research is immensely interesting and relevant to the practical concerns of our community partners, while also resulting in publications in top-ranked academic journals such as the *Journal of Personality and Social Psychology* (O'Brien and Wilson 2011) and *Behavioral and Brain Sciences* (Wilson, Hayes, Biglan, and Embry 2014), illustrating the positive tradeoff that can exist between basic and applied research. It unites previously isolated research communities by showing that prosociality is a master variable: Having it results in multiple assets and not having it results in multiple liabilities (Biglan 2015).

Challenges to Establishing a Field Site

Based on my experience and Nettle's parallel effort, I am convinced that every college and university, anywhere in the world, can become the nucleus for a field site for the study of cultural evolution. The field sites can grow incrementally based on resources at hand, like a termite mound, without requiring a large initial "Big Science" investment. However, I have also encountered severe challenges in my efforts to develop Binghamton into a field site, which can be grouped into the following categories.

Lacking the evolutionary perspective. The need for a unified theoretical perspective cannot be overstated. It makes the difference between the unfocused and often low quality research taking place in communities everywhere and focused research that contributes to basic scientific knowledge while also addressing practical problems. It sounds imperious to make this claim—especially to people who do not yet “get it”—but the power of a unified theoretical framework has already been proven by the history of the biological sciences. A community of scientists and their students who understand the meaning of Tinbergen’s fully rounded “Four Question” approach is therefore necessary to get started. One reason that I was emboldened to begin field research in Binghamton in 2006 was because I had previously established a campus-wide evolutionary studies program in 2003 (Wilson 2007; Wilson et al. 2011), providing a critical mass of faculty, graduate students, and undergraduate students who could become involved. Even in my case, it was primarily my own laboratory that started the BNP and there is a constant need to educate people—both inside and outside of the Ivory Tower—about the evolutionary perspective.

Social instability and disruptive influences beyond one’s control. Even when a research project can be conducted with the resources at hand, it requires the cooperation of community partners, such as my collaboration with Binghamton’s school superintendent. Whenever a community partner retires or moves on to another position, the collaboration must be renegotiated with his or her replacement. It is distressing how often this happens. In a school for at-risk youth that the BNP helped to design in collaboration with the same superintendent (Wilson, Kauffman, and Purdy 2011), we had to orient four principals that were assigned to us over a period of three years. Then the whole program was terminated by a new school superintendent who trusted her gut instincts more than the results of our randomized control trial. Other projects that began with the help of a progressive mayor withered with the election of a more conservative mayor. The turnover of college presidents, provosts, and deans—each anxious to establish their reputation as a change agent before moving on—is also distressingly high. A long-term field site must be designed to withstand these disruptive influences, a point to which I will return below.

Putting one foot in front of the other. As if these problems weren’t bad enough, even for projects that receive unanimous support, it can be difficult to collectively put one foot in front of the other. As one example, the county health department reports a treasure trove of information at the spatial scale of zip code, but this information would be much more valuable and commensurate with our own data at the spatial scale of census block groups. In principle this should be doable, both

technically and legally, but the work required proved to be insurmountable given the financial and human resources at hand. As a second example, the idea of sponsoring a friendly competition among neighborhoods to increase school attendance, using attendance maps (similar to the prosociality map shown above) that are updated monthly, met with universal approval. The school had the necessary information, but it was entered into commercial software packages sold to schools that are designed to issue reports, not to work with the data in unscripted ways. The small IT staff of the Binghamton City School District was too preoccupied with more immediate concerns and efforts to interest faculty and graduate students of Binghamton University's computer science department also failed, so an interesting project in applied cultural evolution, with the possibility of an important educational outcome (increasing school attendance) didn't materialize.

I hope that these examples give a flavor of what it's like to create a field site for the study of cultural evolution in "termite" mode, without requiring the massive support required by the "Big Science" mode. The good news is that it can be done with whatever resources are at hand. The motto of the BNP is "Don't wait for the money!" Indeed, over-reliance on external funding has been the death of many research programs. The capacity of a college or university to conduct community-based research without dedicated funding is impressive when one pauses to think about it. The faculty are already on salary. Many of the graduate students are supported as well on teaching or research assistantships. Faculty and graduate students alike are on the lookout for interesting new research projects and many have an intrinsic motivation to help their community. Undergraduate students are eager to work for course credits, very affordable hourly wages, or on a volunteer basis. Community partners also have a latent capacity with their paid staffs and operation budgets, although often to a lesser extent due to understaffing and the short-term demands of their jobs. These latent capacities can be activated by having a clear sense of what to do.

One hidden benefit of the "termite" model is that trying to accomplish positive cultural change with the resources at hand is an education in cultural evolution all by itself. The challenges that my associates and I encounter aren't always fun, but they teach us things that we never would have learned from the purely academic study of cultural evolution. We are developing street smarts to go along with our book smarts.

Of course, I'm not suggesting that the investment of additional resources for the creation and support of field sites wouldn't help, especially when targeted to solve some of the instabilities and disruptions described above. The contractual obligations that come with dedicated funding can help in addition to the actual

money. And some of the biggest and most important questions in the study of human cultural evolution will require the “Big Science” model to address.

The larger the community of scientists who adopt the evolutionary paradigm, the sooner the field site concept will acquire the same status for the study of cultural evolution as for the study of biological evolution. This is why the creation of the Cultural Evolution Society is of historic significance. For the first time, over a thousand scientists and scholars from around the world who speak a common theoretical language have a means to communicate and coordinate their actions. I hope that the creation of field sites will be among their top agenda items.

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Harvey Whitehouse. *Developing the Field Site Concept for the Study of Cultural Evolution: An Anthropologist's View*

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The birth this year of the new Cultural Evolution Society is an exciting and historic development, and I am privileged to have served on the steering committee responsible for its initial conception and gestation. Cultural evolution research faces many challenges in the years to come. One of the most fundamental, perhaps, is to establish the extent to which cultural evolution is Darwinian.

This could be broken down into many sub-questions. For example, is cultural variability due to cultural evolution or some other process? If culture evolves then what are the units of selection? Does the evolutionary process involve random variation and selective retention as observed in natural selection? To what extent does it depend on deliberate design and innovation? To what extent is culture evoked by biologically evolved mechanisms or transmitted? While all of these questions and more can be addressed using theoretical models and running lab experiments, there is also an inescapable need for field research. Indeed,

restricting the study of cultural evolution to university campuses would arguably be equivalent to trying to study biological evolution exclusively in a zoo or aquarium.

To illustrate the high importance of field research in the study of cultural evolution I propose to focus here on just one of the fractionated questions above: Is culture evoked or transmitted? Culture is *evoked* to the extent that some putatively innate behavioural tendency (let's say incest avoidance) is triggered by the presence of some standard environmental cue (e.g. sharing the same mother). By contrast, culture is *transmitted* to the extent that some putatively learned behavioural tendency (let's say incest avoidance again) is passed down through the generations as part of a set of traditional beliefs and practices (e.g. sexual mores and sacred taboos). Surely nobody doubts that there is evoked culture and transmitted culture, but Evolutionary Psychology (hereafter EP) strongly emphasizes the former over the latter whereas Evolutionary Theories of Culture (hereafter ETC) tend to place the emphasis the other way around. To adjudicate on this question we need to seek data beyond (as well as within) the lab. In explaining why I will draw heavily on my personal experience of field research as an anthropologist. But let's begin by fleshing out some key features of the evoked-versus-transmitted problem.

Cultural as Evoked or Transmitted

A concise account of the differences of emphasis between EP and ETC was conveyed by the debate held in San Diego earlier this year between Leda Cosmides and Joe Henrich at an SPSP Annual Convention symposium entitled "[Big Questions in Evolutionary Science and What They Mean for Social-Personality Psychology.](#)"

Cosmides, a captivating exponent of the EP tradition, argued that much of the content of culture is evoked rather than learned. That is, many cultural representations are the way they are because they are anticipated by evolved psychological architecture and, as such, would be motivating or memorable and therefore 'catchy' in any normal human being placed in a suitable environment. Henrich, an equally captivating exponent of cultural evolution theory, argued that much of our cognitive architecture evolved to facilitate the acquisition of useful information that could not have been inherited genetically. That is, we have evolved to recognize and preferentially learn useful information wherever we may find it. Both Cosmides and Henrich clearly agree that many specialised cognitive adaptations have evolved through natural selection, and both agree that culture provides an important context for the activation of these cognitive systems. The points of disagreement between advocates of EP and ETC, however, are as subtle and multifaceted as they are theoretically portentous.

Firstly, Cosmides emphasizes the role of evolved psychological capacities that emerge similarly in development across all human populations, being somewhat ‘canalized’ or genetically pre-specified (Waddington, 1957). Henrich, by contrast, emphasizes the role of evolved psychological capacities for learning in flatter epigenetic landscapes (Whitehouse 2013). So while both acknowledge that human psychology is an outcome of biological evolution, for Cosmides the emphasis is on inherited cognitive specialisations (a ‘modular mind’) whereas for Henrich the emphasis is on learning capacities (if not a more general intelligence, then at least a mind specialised for learning new skills rather than simply pulling out preformed gadgets to suit the terrain).

Secondly, to the extent that having a certain socially learned cultural skill (e.g. cooking) can have significant consequences for anatomy, cognition, and behaviour (e.g. digestive system), Henrich argues that culture and genes can co-evolve. But whereas for Henrich this insight should have profound implications for our understanding of human psychology, Cosmides argues that most cultural innovations are too recent to have had much effect on cognitive evolution via natural selection.

Thirdly, lurking somewhat in the background of this particular debate is a question about whether or not culture itself evolves. For Henrich, and perhaps many other founding members of the new Cultural Evolution Society, it may seem obvious that culture evolves. Even Cosmides recognizes that of course cultural representations can accumulate in a population so as to form distinctive cultural traditions and that particular domains of culture, such as technology, can become progressively more effective and efficient via processes of winnowing and selection. But she doubts whether such processes constitute a separate system of inheritance, alongside genetic inheritance, such that the two might be said to co-evolve (see also Sperber, 1996). In her talk in San Diego, Cosmides says that her main reasons for doubting this is that the notion of a cultural inheritance system either requires or tends to lead to a ‘mind-less’ (psychologically implausible) view of cultural transmission (see also Powell and Clarke, 2012).

Of course, the discussions in San Diego were designed around an adversarial debating format, veiling much common agreement. And it would surely be a mistake to reduce the differences between entire subfields such as EP and ETC to the views expressed by only two individuals at a single event. Nevertheless, a puzzling conundrum surely lurks beneath the surface here. While some leading exponents of EP and ETC may indeed agree on many fundamental points of theory, by emphasizing different aspects they wind up concluding that what the other is studying is not what they think it is. I am reminded of the story of the three blind men who each feel a different part of the elephant (e.g. the tail, the trunk, and the ear) and, as a consequence, reach very different conclusions about

the nature of the object before them (claiming respectively that the object is a rope, a branch, and a fan). In much the same way, proponents of EP and ETC arguably fail to identify the same objects or to agree on how they should be connected up.

Seeing the bigger picture is easier if instead of engaging in abstract theoretical debates one starts to grapple with the messiness of real-world observation. Maybe in the field we have a better chance of seeing the whole elephant...

The Experience of an Anthropologist in the Field

My life as an anthropologist began in the late 1980s, as a doctoral student at Cambridge University. My mission was to travel deep into Papua New Guinea's rain forest and conduct participant observation among the Mali Baining, a group whose language had yet to be described and whose culture was unknown to anthropology (Whitehouse 1995). Houses in my village were built of bush materials and lacked electricity and running water. Because of limited access to medical care (the nearest aid post being too far to walk when seriously ill), many succumbed to malaria and other potentially treatable maladies. Materially the culture was simple and life was often brutish and short. But the rituals and beliefs of the community were contrastingly rich and vibrant.

In my village there were various temples: two large communal ones built close to the cemetery and numerous smaller ones tended by individual households. In each of these buildings, offerings to the ancestors of food and drink (and sometimes also money, if available) were laid out as part of an unremitting schedule of daily rituals associated with a secretive organization known as the 'Kivung'. In *tok pisin* the word *kivung* means 'a meeting' and it is certainly true that my friends spent a lot of their time in meetings, discussing how best to prepare for the great day when their ancestors would return from the dead. It was often said that the returning ancestors would take the appearance of white men and women, wielding powerful magic and technology. They would summon a vast fleet of bulldozers to flatten the rainforest and construct, overnight, a vast urban sprawl with high-rise buildings and wealth beyond ordinary people's imaginings.

At Kivung meetings the community would often dwell on the forces of darkness that prevented the ancestors from returning and endeavour to root out sinners and have them ritually absolved and cleansed. Sin was understood as any breach of the Kivung's Ten Laws (loosely based on the Ten Commandments taught by the nearby Catholic Mission). Only when sinfulness had been completely eradicated among the living would they finally be reunited with the ancestors. A period of great plenty, known as the *taim bilong kampani* (period of the companies), would ensue during which Kivung members would be granted

vast wealth. During this time, there would be a great temptation to indulge the sins of the flesh. At the Day of Judgement to follow, those who resisted temptation would enter an everlasting paradise on earth known as the *taim bilong gavman* (period of the government). The rest would be cast into Hell to suffer eternal damnation. The leaving of offerings provided a measure of the community's progress towards this goal. Consistent evidence that the ancestors were receiving the offerings indicated that they would soon return. Evidence that the offerings had been rejected showed that the community was still sullied by breaches of the Ten Laws, delaying the miracle.

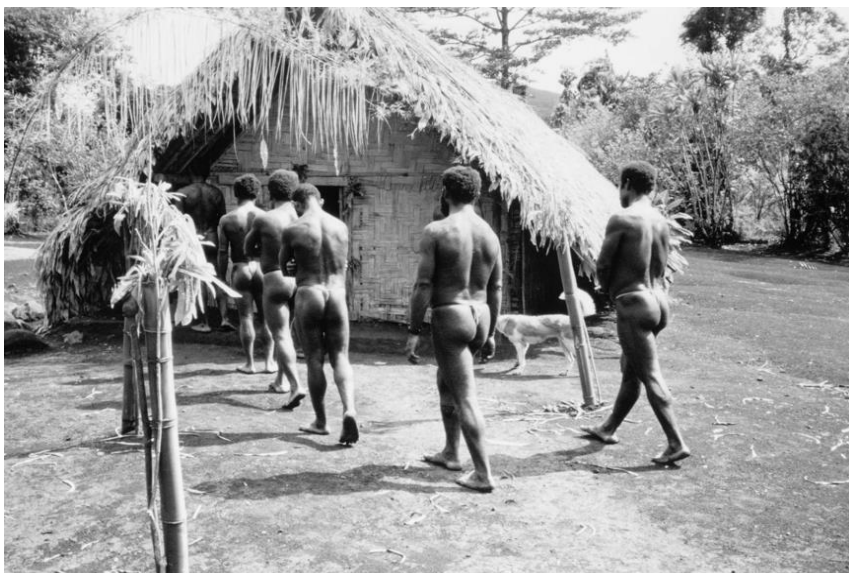


Figure 3. Men of the village process solemnly to one of the Kivung temples to lay out offerings to the ancestors (from Whitehouse 1995).



Figure 4. Given Kivung teachings that the white-skinned ancestors kept the living under constant surveillance, my habit of following people around taking notes on everything they said and did caused something of a stir.

The procedure for laying out offerings in the larger temples begins with a task force of men processing solemnly through the front entrance of the building to arrange the food, water, money, and decorations on tables, rather as the servants of a grand house might have prepared for a banquet in Victorian England. Great care is paid to cleaning and straightening the containers of the offerings, and adjusting flowers and other adornments on the tables, all with a certain flourish and exaggerated attention to detail. Once the laying out of offerings has been completed to the satisfaction of all concerned, the temple is vacated except for one man (known as a ‘witness’) who remains behind, seated in solitude in a small cubicle in the heart of the building. It falls to the witness to note signs of ancestral presence such as the clinking of plates or cutlery, the sounds of eating, the creaking of benches, or even snippets of conversation among the visiting spirits. When his vigil is over the witness emerges blinking in the daylight before a gathering of the entire community. All are eager to hear whether the ancestors have accepted the offerings or, as disappointingly is so often the case, have refused them because of the sinfulness of one of more Kivung members (who must therefore be rooted out and encouraged to confess).

Temple rituals are only some of the many complex beliefs and practices associated with the Kivung. A similarly elaborated set of ideas and practices focus around communal gardens associated with ancestor heroes closely linked to the Genesis story as taught by the Catholic mission. Except that Eve's misdemeanor was not to partake of a forbidden apple but to harvest the betel nuts of a taboo palm tree. As Eve slid down the trunk of the tree with her stash of nuts, she cut her groin on a sharpened stone that Adam had cunningly embedded in the trunk, causing her to produce a strong flow of menstrual blood from that time forward. She then passed on this curse to all her daughters and her daughter's daughters, causing them to bleed every month. Kivung followers, unlike most neighboring groups in the region, repudiate betel chewing because they say the red substance it produces in the mouth is equivalent to menstrual blood.

Some years ago a group of us attempted to build a model of the Kivung meaning system in which some of the core ideas and practices were captured as nodes in an elaborated network (Whitehouse et al. 2012). The model incorporated four special nodes, depicted as black rectangles. We referred to them as 'intuitive anchor points' and what made these special was that they constituted universal implicit beliefs that are *evoked* by the environment (to use Cosmides' terminology) rather than being sponged up via some sort of general learning capacity. The intuitive anchor points in question were selected for illustrative purposes and not because they were the only or even the most obvious intuitive beliefs that the Kivung evoked. Indeed, a more comprehensive model would have taken many other possible anchor points into account and the socially transmitted nodes in the meaning system would have been vastly more numerous and their crisscrossing interconnections unfathomably more complex. Nevertheless, our model helps convey the complex interplay between evoked and learned cultural representations in a given tradition.

In our model, one of the anchoring beliefs was mind-body dualism, the intuition that minds and bodies are distinct and can in principle be detached (Bloom 2004). This was clearly essential to the notion that ancestors could invisibly enter the temple and partake of the offerings without being physically present. Kivung members did not need to be taught that ancestors were bodiless, they inferred this from the fact that nobody ever entered or left the temple after the offerings had been laid out even though the witness might sometimes hear them talking or eating. The second was promiscuous teleology, the over-attribution of intelligent design to natural phenomena (Kelemen and DiYanni 2005). Consider, for instance, the Kivung creation myth about the causes of menstrual bleeding. The idea that this biological function was caused by the actions of primordial ancestors did not need to be explicitly taught but was simply inferred from the fact that Eve cut herself on a betel palm, women

menstruate, and Kivung followers do not chew betel nut. The third anchoring belief in our model was the notion that offerings to the ancestors should be handled like potential contaminants, triggering hazard-precaution routines (Boyer and Lienard 2006). That the men entering the temples should walk slowly and deliberately, manipulate the offerings with great care (paying attention to separating, cleaning, and boundary maintenance), and communicate only in whispers were all intuitively obvious and did not need to be explicitly taught. All that was needed to generate these psychological responses and outward behaviours were cues that the food and the context for its preparation and laying out were somehow sacred and therefore potentially dangerous, serving as salient input to each participant's hazard precaution system. And finally, the notion that God and the ancestors would punish sinners by delaying the miracle or casting them into Hell was implicitly informed by immanent justice intuitions (Callen, Ellard and Nicol 2006). Nobody needed to be explicitly taught that sinners, even the hapless ancestral Eve, deserved to be punished—this was immediately evident by virtue of their transgressions. The point, then, is that Kivung beliefs and practices were grounded in a set of deeper, maturationally natural intuitions (McCauley, 2011) delivered by people's evolved psychological equipment rather than having to be explicitly taught. This much is entirely consistent with the EP view of culture.

But at the same time, many of the details of the Kivung belief system certainly did have to be explicitly taught and learned. If one were to dig deep into each of the core concepts of the Kivung, such as the Ten Laws, or the movement's eschatology, one would soon find many explicit beliefs that were relatively remote from the intuitive anchor points described above and some downright difficult to conceptualize and remember. For example, the law proscribing murder was interpreted to refer to a great many different kinds of sins involving a kind of metaphorical 'killing' including gossip, certain forms of which were understood as a form of 'character assassination'. In Kivung meetings details of what the ancestors would regard as homicidal behavior were elucidated at great length and repeated with such frequency that the risks of garbling or forgetting were greatly reduced. In our agent-based model such repetition served to 'refresh' the nodes in our network, allowing them to persist in a stable form. But when the frequency of repetition in a simulation was reduced, the links between nodes furthest away from intuitive anchor points would be at risk of fading and disappearing, with the possibility that a node could become isolated and so be extinguished from the system altogether. Our model also took account of the motivational strength of particular nodes and other variables that were affected not only by intuitive foundations but also how recently they had been first encountered and other relevant factors. Nevertheless, my concern here is not

with details of a computational model. What matters for the present argument is that some beliefs, qua the EP viewpoint championed by Cosmides, are anchored in evolved intuitions and so are largely evoked rather than learned. But, at the same time, cultural systems also incorporate beliefs that are more distant from intuitive anchor points and, qua the ETC viewpoint advocated by Henrich, have to be explicitly learned, practised, and rehearsed if they are to be culturally transmitted.

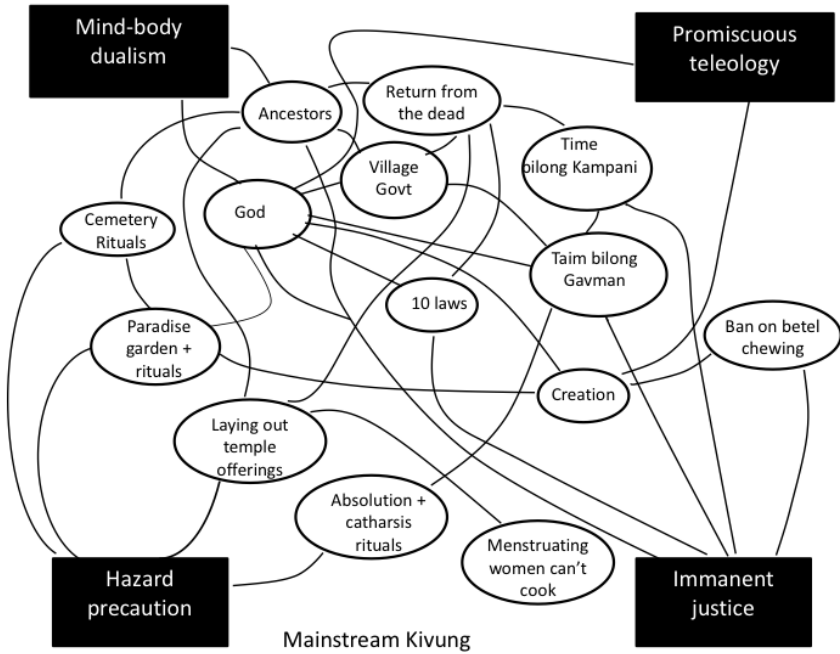


Figure 5. Mapping the mainstream meaning system (from Whitehouse et. al. 2012).

For any meaning system to achieve stability it must ensure not only that its less intuitive constructs have adequate mnemonic support but also, more challengingly still, that the complex webs of interconnections between component nodes are sustainable over time within bounded populations. If a regional tradition is to maintain homogeneity across the landscape it must overcome various kinds of threats to its integrity and coherence as a belief

system. Among these threats are forgetting, innovation, and demoralization. Much of my own previous research has been devoted to showing how highly routinized religious traditions reduce the risk of forgetting simply by repeating the creed over and over. Moreover, frequent repetition also makes it easier to spot unauthorized deviations from the orthodox canon (Whitehouse 2004), and mechanisms leading to tighter norm-enforcement may also contribute to the stabilization of beliefs and practices (Gelfand et al., 2011). But faithfully remembering and accurately reproducing a set of beliefs and practices also presents motivational challenges—not least that routinization can become mind-numbingly boring! The insistence that the faithful turn up day in, day out and week in, week out to hear the same speech acts over and again can lead to the so-called ‘tedium effect’ (Whitehouse 2000), lowering motivation and risking splintering events or other expressions of rebellion.

To the extent that the contents of beliefs systems stray from their intuitive anchorages, they may easily stretch credulity to the limit. Doubters pose a threat to retention of the group’s membership not only because they may vote with their feet but also because they may also cause others to lose their faith. The ETC perspective has generated a number of theories to help explain how cultural traditions can inoculate themselves against such problems. An instructive example is the theory of Credibility Enhancing Displays or ‘CREDS’ (Henrich, 2009).

Time consuming and materially costly rituals, such as the laying out of offerings to the ancestors in temples, serve to advertise the commitment of adherents to Kivung teachings. According to the CREDS hypothesis, beliefs are more likely to spread in a population if those espousing them act in a way that would be costly if they were only pretending to believe. Beliefs may be said to be costly if they reduce the holder’s fitness, if they make great behavioural demands, or if they stretch credulity to the limit. Such costs are commonly exacted by supernatural beliefs. For example, the Kivung belief that the ancestors will soon return from the dead and magically transform the rainforest into a modern city, incorporates many assumptions that fly in the face of everyday observation and experience. Declaring one’s commitment to such a belief system is relatively easy to do but also likely to prompt skepticism. When such declarations are combined with costly behaviour such as an unremitting regime of ritual observance, however, they appear more sincere and therefore *imitable*. By backing up one’s beliefs with actions, by ‘walking the walk’ as well as ‘talking the talk’, CREDS may facilitate the spread of otherwise incredible beliefs. Insofar as groups with different kinds of beliefs come into competition, those with beliefs that are better adapted to cooperation may prevail. So, for example, a group that can stabilize beliefs in an all-knowing punitive ancestors may be better able to deter cheaters

and defectors allowing more effective cooperation in communal projects (Bulbulia et al. 2013). The idea is that CREDs can play a crucial role in such a process, leading to the spread of cultural traditions like the Kivung, at the expense of others.

Rituals serve as admirable CREDs because they are actions rather than merely words. Whereas advocates of EP, like Cosmides, emphasize that what causes beliefs to spread and persist in a population is their ‘catchy’ cognitive content, exponents of ETC, like Henrich, can point to mechanisms like CREDs that allow cultural groups to override various kinds of content bias, making implausible or personally deleterious beliefs more transmissible. One of the great advantages of conducting qualitative fieldwork is that it allows us to see how both EP and ETC perspectives can be right, that evoked intuitive beliefs as well as elaborated systems of belief that need to be learned and supported in various ways can and are combined in the real world. From this field-based perspective, EP is just one element, extended-able by advocates of ETC, and culminating in something much larger and more impressive—if not an elephant exactly, then something roughly as majestic and impressive.

Building a Cross-Cultural Field-Based Approach: The AnthroLab Model

Participant observation affords us the opportunity to describe cultural systems holistically and that is perhaps its greatest strength. Nevertheless, observation and description are not the same as explanation. To understand the causes of the phenomena we observe in the wild requires carefully controlled experiments and systematic comparison across space and time. Field sites have a vital role to play at this level as well. But when experiment and comparison are the goal, fieldwork starts to look very different. Research teams get larger, field sites need to start communicating with each other, and the whole enterprise of data gathering and analysis needs to be scaled up and, to some extent, centrally coordinated. In this section we briefly consider a couple of examples of collaborative research in the field based on a hub-and-spokes model, pioneered by [AnthroLab](#) in Oxford’s [Centre for Anthropology and Mind](#).

[AnthroLab](#) is currently collecting data in 12 countries worldwide, in some of these countries at multiple field sites. We chose this particular spread of countries and regions for several reasons. Above all, we wanted to maximize cultural variability, something that is important whatever one’s theoretical orientations. The question whether culture really does evolve in a genuinely Darwinian fashion—that is via generated variation, inheritance, and selection—is clearly an empirical question that requires field-based research as well as

theories, lab experiments, and secondary data analysis to resolve. As we have seen, however, another of the big questions facing the evolutionary human sciences is whether particular behaviours are evoked or transmitted. Adjudicating on this question requires empirical studies that put these possibilities head to head. It is all too easy to fall prey the widespread, if implicit, misconception that just because behaviour is universal it must be intuitive/ evoked whereas if it is variable is must be learned/ transmitted. For example, Henrich argues that because fairness judgments differ cross-culturally this must reflect differences in transmitted norms but as Cosmides observes evoked culture can also vary. Indeed, to the extent that behaviours vary as a consequence of environmental differences the fact that physical and social environments have diversified so dramatically over the past ten thousand years or so should be a powerful motivation for advocates of the EP approach to get out of the lab and enter the field.

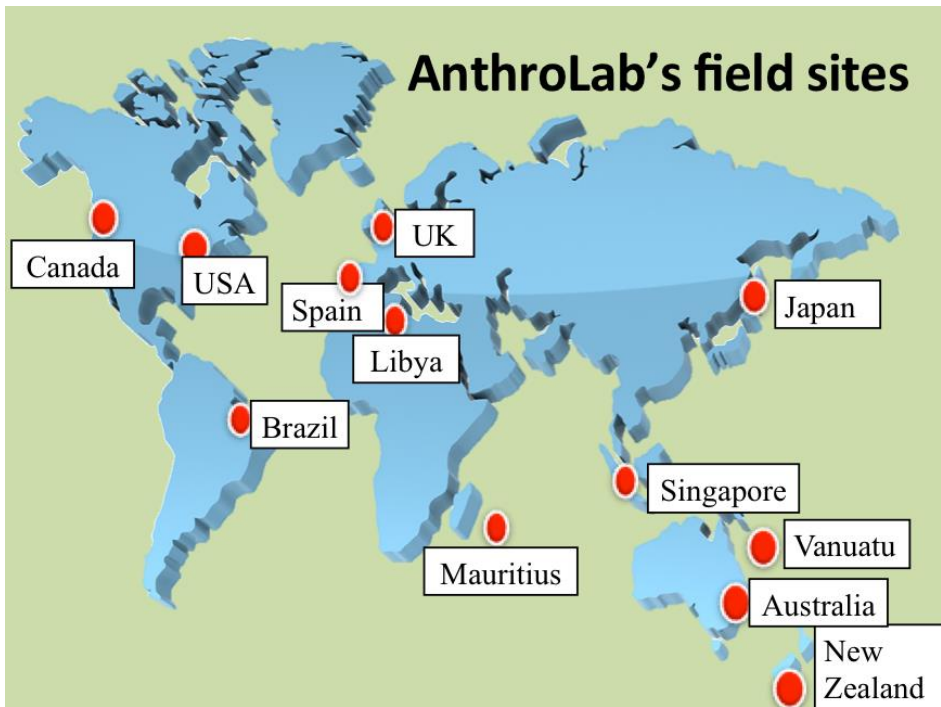


Figure 6. AnthroLab's current field sites.

To illustrate the role that field research can play in these areas, consider the work we have been doing on ritual learning in early childhood. For several years we have been conducting experiments with 4–6 year olds in the USA showing that young children are very sensitive to cues that modelled behaviour is ritualistic rather than instrumental and tend to imitate the former more slavishly than the latter (Legare et al., 2015). In particular one of my doctoral students, Rachel Watson-Jones, spearheaded a series of studies suggesting that the imitation of rituals is motivated by a desire to affiliate with a group (Watson-Jones et al. 2014; Watson-Jones et al. 2015). Since this would appear to be an important mechanism of social learning, a crucial question from an ETC perspective is whether more ritualistic cultural environments (e.g. Melanesia) foster greater sensitivity to cues for conventional rather than instrumental learning. In 2012, Cristine Legare, Quentin Atkinson and I went to Vanuatu, a Melanesian archipelago, to lay the foundations for future fieldwork projects aimed at addressing this important question. In 2013, Atkinson and I returned with a larger entourage of research assistants to begin data collection. Although we each had our own teams dedicated to distinct projects, we were able to join forces to collect basic demographic data, covering more ground much more rapidly than would have been achievable using more conventional ethnographic methods. Moreover, teamwork was essential to carrying out field-based experiments. For example, we wanted to run a study of the effects of ritual versus instrumental primes on cooperation, trust, and preparedness to delay gratification. The procedure involved participation in an artificial ritual involving four experimenters and only one participant per session, a design that would have obviously been impossible to implement by a researcher working alone.

Conducting surveys and experiments as teams in countries like Vanuatu has enabled us to carry out rigorous comparisons between field sites. For example, we have been able to show that although Vanuatu is a more tradition-bound, ritualistic environment than much of Eurasia, children of the same age in both regions are more or less equally sensitive to the effects of ritualistic versus instrumental cues on executive control and delayed gratification tasks (Rybinska et al., *in press*). Currently we are studying the effects of ritual participation across multiple field sites simultaneously and planning even more ambitious research involving thousands of participants from scores of countries (more on this below).

Barriers to Progress

Team-based field research in remote rural settings isn't easy—or cheap. One of the most obvious barriers to progress is funding. While [AnthroLab](#) has been fortunate to secure continuous funding from large grants from the EU, Research

Councils, and various charities since its establishment more than a decade ago, fundraising is always a perilous business. To be sustainable in the long-run, field-based approaches in the human sciences need to be embedded in universities serving the regions in which data collection takes place, via collaborations with permanent faculty and their renewable teams of student RAs. We cannot afford to rely only on postdocs supported by ‘soft’ money and fixed term contracts.

There are also undoubtedly intellectual barriers to overcome. We all know about the ‘two cultures’ problem that for many decades has made collaboration between scientists and humanist scholars so difficult or even impossible. But this, at least, is an obvious and longstanding problem being addressed from many angles. A potentially more pernicious barrier to progress—at least if it is allowed to persist unabated—is the unwarranted dislocation of EP and ETC perspectives. The need to combine these productively may seem less pressing from the comfort of one’s armchair or even the university lab, but for those of us committed to rolling up our sleeves and going into the field it is a high priority.



Figure 7. Doctoral candidate Veronika Rybanska (background left) and a local research assistant (foreground right) help me run a priming study with and adult male participant (foreground left) while another research assistant, Joseph Watts (then a student of Atkinson’s) participates off-camera by managing props, keeping notes, prepping participants and carrying out other essential tasks.

One solution is to get more EP and ETC people into the field working together. [AntroLab](#) is committed to training and hiring researchers to engage in scientific research all around the world based on collaborative fieldwork. To this end, we are continually hiring researchers to work on our many projects overseas and anticipate advertising several more full-time postdocs positions before the end of the current year as well as many opportunities for research assistants. Those interested in applying should look for updates [here](#).

Our field-based approach not only combines the perspectives from EP and ETC but more generally adopts a methodologically eclectic approach to problem solving. By doing so, our research will help to illuminate the essentials of being human—our language, rituals, religion, and morality. The best way to help research on cultural evolution inch forward is to wrestle with empirical problems, forcing the theorists catch up. That, in my experience, is what going into the field is all about.

Acknowledgments

I am grateful to Michael Buhrmester, Tom Currie, Oliver Scott Curry, Pieter Francois, Jonathan Jong, Chris Kavanagh, and David Sloan Wilson for commenting on a draft of this essay. This work was supported by a Large Grant from the UK's Economic and Social Research Council (REF RES-060-25-0085) entitled "Ritual, Community, and Conflict".

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Commentaries

Russell Schutt. *Developing the Field Site Concept for the Study of Cultural Evolution: A Sociologist's Perspective*

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The two-part essay by David Sloan Wilson and Harvey Whitehouse, "Developing the Field Site Concept for the Study of Cultural Evolution" presents compelling applications of field research methods and evolutionary theory that make clear the value of their combination. Wilson's Binghamton Neighborhood Project (BNP) and Whitehouse's studies in Papua New Guinea and then in the AnthroLab field sites are not only models for research on cultural evolution but also examples of how to investigate fundamental questions about the social world within a scientific program of theoretically-guided research. My commentary on the essay will focus on the implications of these models for research in sociology and on the persuasiveness of the authors' arguments from a sociological standpoint.

Three recommendations underlie the "field site" vision elaborated in David Sloan Wilson's part, although the first recommendations are subsumed within the third: (1) Use field study as a methodology; (2) Use evolutionary biology as a theoretical framework; (3) Apply field study methods within the theoretical framework of evolutionary biology in order to explain cultural variation and change. The first recommendation is the easiest for a sociologist to accept, since as Wilson points out, there is a strong tradition of field research in sociology. Nonetheless, there is much to be learned from reviewing this tradition and considering its current status and challenges. The second, theoretical recommendation requires overcoming what Wilson aptly characterizes as

sociology's historical disinclination to adopt an evolutionary perspective, and so I will review that history and consider whether there is some potential for a disciplinary reset. Of course, it can be argued with respect to the third recommendation that most sociologists do not study cultural evolution, but if behavior ranging from sociality in Binghamton schools (Wilson, 2016) to community rituals in Melanesia (Whitehouse, 2016) are best explained by evolutionary theory, it is clear that "cultural evolution" is meant to be broadly applied.

Recommendation #1: Field Study Methods

Sociology emerged as a discipline in the late 19th century during the social transformations associated with urbanization and industrialization of Western society, and in the midst of growing support for the scientific method and of Darwin's theory of evolution by natural selection. With increasing numbers of people moving from small, homogeneous villages to urban areas and leaving home- or workshop-based agricultural and craft work for factory (and then office) labor, early sociologists focused attention on the corresponding changes in the nature of social ties. Émile Durkheim (1893) conceptualized the change as involving a shift from "mechanical" solidarity based on likeness to "organic" solidarity based on interdependence; Max Weber (1915) described the shift from traditional society to legal-rational authority and bureaucratic organization; Ferdinand Tönnies (1887) distinguished *gemeinschaft* social groupings bound by feelings of togetherness from the new *gesellschaft* pattern of instrumental relations.

Concern with the fragility of social ties associated with the more modern form of social organization stimulated research ranging from Durkheim's seminal study of suicide rates in France to Robert and Helen Merrell Lynds' mixed methods field study of *Middletown*. This focus found perhaps its most concentrated expression in the development of what came to be known as the "Chicago School," which was centered at the University of Chicago's Department of Sociology. With the rapidly growing metropolis of Chicago as their laboratory, Chicago School sociologists used ethnographic methods to study neighborhood social life holistically, at times making explicit analogies to the ecological approach in biology. Since the 1920s, the Chicago School approach has inspired field studies of neighborhoods and organizations by sociologists in Chicago and beyond, including Harvey Warren Zorbaugh's (1929) *The Gold Coast and the Slum* (Chicago's Near North Side), William Foote Whyte's (1943) *Street Corner Society* (Boston's North End), Gerald Suttles's (1968) *The Social Order of the Slum* (Chicago's Near West Side), Kai Erikson's (1976) *Everything in Its Path* (Buffalo Creek, West Virginia), and, more recently, Carolyn Ellis's *Fisher*

Folk (community in Chesapeake Bay), Elijah Anderson's (1999) *Code of the Street* (North Philadelphia), Mitchell Duneier's (1999) *Sidewalk* (New York's Greenwich Village) and Eric Klinenberg's (2002) *Heat Wave* (Chicago's North and South Lawndale).

I hope I have made it clear that field studies have a long tradition in sociology that has continued to inspire new research even before Rob Sampson's (2012) brilliant new contribution, *Great American City: Chicago and the Enduring Neighborhood Effect*. It is therefore important to consider why relatively few sociologists conduct field studies.

The explanation again takes us back to the discipline's founders. The other side of the coin of concern with the loss of community was sociologists' conceptions of the society that was replacing it. Although differing in their specifics, these conceptions highlighted decreasing communal ties and traditional values and increasing individual autonomy and rationally determined goals. Each also expressed various degrees of apprehension about this shift, but all understood that the social world's tectonic plates were shifting. Arguably expressed most clearly in Talcott Parsons's (1951) "pattern-alternatives of value-orientation," the direction of change included a shift in orientation from affectivity to affective neutrality, from concern for the collectivity to concern for the self, from ascription to achievement, and from particularistic to universalistic values. To the extent that society thus shifted from a more collectivist to a more individualist culture, sociologists could favor methodologies that treat individuals as the units of analysis and slight the role of local social context.

The appeal of a theoretical rationale for methodological individualism was complemented by rapid development of national communications, transportation, and educational and computational infrastructures that facilitated collection and analysis of data with surveys and other quantitative methods. The ability to represent large populations, measure myriad variables, and test complex causal chains diminished for decades the appeal of intensive, locally-focused field studies. As globalization has increased the scale of interconnection (Friedman 2006), "neighborhoods are seen as an anachronism displaced by global networks of interchangeability." (Sampson, 2012: 355)

But attention to social context is on the rebound in sociology, both because of the discipline's foundational concern with social relations and as an indirect consequence of interrelated social and technological developments. Four examples convey the increasing contemporary sociological focus on social context: (1) Migration has created new social patterns within cities across the globe. Like Harvey Whitehouse's multi-sited AnthroLab project, the [Max Planck Institute's comparative field studies of social diversity in New York, Singapore, and Johannesburg](#) is showing how traditional field research methods can relate

global issues to local communities. (2) While the [Project on Human Development in Chicago Neighborhoods](#) set a new standard for quantitative research on contextual effects, the “ecometrics” method Rob Sampson developed for the PHDCN can now be [facilitated with Google Street View data](#). Place-based analyses are now also possible with [GPS data](#) pertinent to many social science questions (Sampson, 2015, p. 360). (3) A large body of sociological research has highlighted the value of social ties for health and well-being, but individually-based measures of social supports are increasingly being supplemented by assessment of the social networks ([Schutt 2015](#))—the social context—in which they are embedded (Christakis and Fowler, 2009). (4) Statistical analyses of quantitative data collected from different social contexts, including organizations and communities, increasingly use multi-level modeling techniques that take account of contextual influences (e.g., Schutt, 2005).

So while traditional ethnographic field studies focused on single communities may never comprise more than a fraction of sociological research, attention to social context and group-level processes is evident increasingly throughout sociology. We can place a solid checkmark in the box corresponding to Recommendation #1.

Recommendation #2: Evolutionary Theory

After decades of disciplinary disengagement followed a troubled rupture between sociology and evolutionary biology at the turn of the last century, the path to implementation of Recommendation #2 contains many obstacles. However, the development of multi-level selection theory in evolutionary biology—so well represented in the Social Evolution Forum—as well as related developments in genetics and neuroscience require reexamination of the bases of the rupture and provide multiple means for productive and sustained reengagement. As I discussed in a contribution to [This View of Life](#), this reengagement is still in a nascent stage in sociology. I will limit myself here to a summary of the major disciplinary changes and new points of convergence, after a brief reconsideration of the past points of contention.

The application of Darwin’s (1871) theory of evolution by natural selection to the context of human society was shaped largely by Herbert Spencer’s (1852) conceptualization of the struggle for “survival of the fittest” as the causal mechanism that explained social stratification in contemporary society: The “evolution” of human societies involved a “natural process of elimination” of the “good-for-nothings” (Spencer 1874, p. 286). In the words of William Graham Sumner, Spencer’s American acolyte, the only alternative to the “survival of the fittest” was the “survival of the unfittest” (*The New York Times*, 1883). The enduring popular—and sociological—understanding of the implications of

Darwin's theory for human society was that it was a reductionist perspective in which critical human abilities that were fixed by biology at birth resulted in a competition between individuals that those less endowed were destined to lose. As Mendelian genetics added to Darwinism a mechanism to explain inheritance, as Nazi "science" distorted genetics to justify genocide, as Lysenko's Soviet "science" of environmentally-determined heredity was discredited (Mukhernee, 2016), it seemed appropriate to consider the scientific study of society a matter apart from biology and the theory of evolution as irrelevant to understanding human cultures.

But while this construction of Darwinism supported social prejudices of the time, it is important to recognize that it overlooked much of what Darwin (1889/1874) actually believed. Two of the seven chapters (4 and 5) of *The Descent or Origin of Man* focus on the importance of human sociality and the role of group selection pressures in the evolution of *Homo sapiens'* most distinctive characteristics. Human dominance of the earth is in part due to "social habits, which lead him to aid and defend his fellows" (Darwin, p. 52); social motives override self-preservation and extend beyond kin to one's social group (p. 114); the "social instinct" is a more powerful influence on human behavior than "the base principal of selfishness" (p. 125); these social instincts can be extended to larger groups and communities (p. 127) and can become hereditary (p. 128); groups are disadvantaged in the course of evolution if they are composed of more "selfish and contentious people" (p. 135). In other words, Darwin believed strongly in evolutionary selection at the level of groups as well as individuals! (Wilson, 2015)

The recent development of multilevel selection theory finally capitalizes on this long neglected aspect of Darwin's theorizing and recasts evolutionary theory in a way that supports sociologists' recognition of the importance of human altruism and social processes (Durkheim (1884/1893) rather than standing in opposition to it. Complementary developments in neuroscience, genetics, and anthropology also strengthen the need for this disciplinary reconnection. Social neuroscience has identified the structures and neurochemical processes in the brain that evolved to support social connection and the plasticity that allows the brain to develop in response to social experience (Schutt, Seidman, & Keshavan, 2015): "We now know that the human brain, considered in isolation from its social functions, is like a cell without chromatin." (Brothers, 1997: 67) The explication of epigenetic processes reveals that the connection of genes to their environment is bidirectional, rather than deterministic (Mukherjee, 2016). Henrich (2015) and others have connected biological evolution to cultural change, in part by showing how cultural learning shapes reward circuitry in the brain, and thus preferences

and behavior so as to allow adaptation to different environments without genetic change.

Some sociologists have also begun to connect the dots that link human biology and social behavior. Sociologists have collected biomarkers in order to understand gene-environment interactions (Guo & Adkins, 2008; Guo, Tong, & Cai, 2008), “neurosociologists” have contributed new insights about evolutionary and cognitive processes (Franks, 2010; TenHouten, 2013), and others have accorded evolved emotions a key role in social organizational processes (Turner, 2011). New sections have formed within the American Sociological Association to support scholarship on [evolution, biology, and society](#) and on [altruism, morality, and social solidarity](#).

Emerging from these efforts are principles that can be the foundation for sociological research informed by evolutionary theory: (1) sociality, the capacity and need for social connection is instantiated in human biology—most importantly in the brain; (2) social contexts, ranging from families and peer groups to organizations and neighborhoods shape individual orientations and behaviors; (3) influence between individuals and groups flows in both directions, in part through environmental influences on biological processes—including cultural variations—and in part through biologically-based behavioral predispositions toward social contexts; (4) the evolutionary processes of variation, selection, and retention are important at each social level, from individuals to states, and natural selection at the level of groups can be more consequential than at the level of individuals; (5) tension inevitably emerges between motives to act on the basis of self-interest and on behalf of group well-being and is managed through social psychological and social control processes.

Although relatively few sociologists frame their scholarship explicitly in terms of evolutionary biology theory, many more investigate research questions that are tied closely to these basic principles and that could be reframed to good explanatory effect in terms of these principles. The concept of “collective efficacy” used by Sampson (2012) to explain neighborhood effects is related closely to the concept of variable group effectiveness that underlies group selection theory, while Thomas Scheff’s (1990) “microsociological” theory of the centrality of social bonds as a human motive and the role of shame as an emotional indicator of their disruption lacks only a connection to the evolutionary theory that explains this fundamental aspect of human nature. Much the same could be said of the extensive sociological literature on social support (Song, Son, & Lin, 2011). My own research has focused for many years on the bidirectional process of influence between individuals and their social contexts ([Schutt, 1985](#); [Schutt, 2011](#)), but has only recently included attention to the biological dimensions of this process ([Schutt, Seidman, & Keshavan, 2015](#)).

In the discipline of sociology, there is much room for more progress toward Recommendation 2, but momentum is building in that direction.

Recommendation #3: Use Field Studies and Evolutionary Theory to Understand Culture

This disciplinary history and these contemporary developments suggest that some sociologists are already predisposed to endorse Recommendation 3—the overarching point of the Wilson/Whitehouse essay—and that many more could be convinced of its value. Few sociologists would question the value of David Sloan Wilson’s comparative study of schools in Binghamton neighborhoods or of Whitehouse’s cross-cultural research spanning the globe. Most sociologists would endorse the proposal to use a theoretical framework to guide more community-based research projects and to frame interconnected research questions. But to what type of social phenomena should this approach be applied and which methods and/or theories should it displace?

The disciplinary boundary between anthropology and sociology—as is true in relation to each of the social sciences—is to some extent arbitrary and permeable; but it does reflect an anthropological tradition of primary attention to the small-scale communities of the past as compared to a primary sociological focus on the large societies of the present. Therein, as I mentioned at the outset, lies the origin of our discipline, but also the appeal of methods that allow collection of data from large numbers of people sampled from areas that transcend physical, social, and political boundaries. While Rob Sampson (2015) has provided persuasive theoretical and empirical justification for reemphasizing the importance of neighborhood location—of “place,” this does not obviate the need to understand local cultural variations within the context of overarching cultural patterns nor the importance of determining empirically the appropriate units for comparison to answer particular research questions. To analogize to evolutionary biology, when should we consider neighborhoods to be like organelles that evolved endosymbiotically within cell boundaries, or to be like organs within an organism, rather than as different independent “sites” that can best be understood as functionally integrated cultural units and compared as such to each other? While we do not want to fall back into what can be seen as Durkheim’s circular reasoning about “organic” solidarity, in which different social parts are assumed to have the position they do because it supports the functioning of the whole society, we cannot escape the need to take account of macro-level processes. And for this reason I suggest we need to take advantage of opportunities to conduct field studies within broader mixed methods investigations (Schutt, 2015) that can inform us about cross-cultural

interconnections and structural constraints that must be understood as part of a larger social context—even as that context is increasingly global and virtual (Chayko, 2017).

This evaluation of the appropriate context for understanding social life also requires attention to the role of emotions. As part of the evolved neurobiological mechanisms that enhance survival, social emotions are attuned to interpersonal interaction and the context of face-to-face communication (Damasio, 1999). Is this a reason to suppose that interaction that transcends local contexts can therefore be understood apart from human biology? Research on engagement in both written texts and electronic forms of communication suggests that the brain responds in similar ways to social information irrespective of the medium, but this connection needs more investigation in order to better frame our understanding of the relevant “field” for particular investigations and the role of evolved capacities (Chayko, 2017; Pinker, 2011). And the emotional ball also still bounces in evolutionary biology’s court, as the role and even relevance of emotions in human evolution remains unsettled (*cf.* Boehm, 2012; Brothers, 1997; Tomasello, 2014; Turner, 2000).

If more sociologists are to adopt Recommendation 3, they must be convinced that their theorizing about culture should reflect or at least be compatible with the tenets of evolutionary biology. In some respects the development of evolutionary views of culture makes this a harder sell. The argument is as follows: The evolution of the human capacity for cultural learning created both the possibility of gene-culture coevolution (as in the development of lactose tolerance after the domestication of milk-producing animals) and the means for evolution of social practices without genetic change and at a pace much more rapid than is possible through natural selection (Henrich, 2015; Turchin, 2016). Yet this remarkable consequence of biological evolution returns us to a basic argument first made by those who argued for a complete separation between biology and the human sciences: *Homo sapiens* crossed an evolutionary Rubicon from genetic determination to cultural malleability (Kroeber, 1915).

As Whitehouse notes, evolutionary psychologists have looked back across the river and found constraints on human behavior and psychology in evolutionary adaptations during the Pleistocene era; culture is therefore more “evoked” than “transmitted” (Tooby & Cosmides, 1992; Whitehouse, 2016). But the less constrained “Evolutionary Views of Culture” that Whitehouse endorses is more compatible with the perspectives of sociologists—even the many who do not see any need to view through an evolutionary lens the extension of human cooperation to larger units or the spread of such patterns as monogamy or monotheism, and so it is ETC on which I focus (Turchin, 2016).

I think that the tie binding sociology and evolutionary biology at the hip is the evolved capacity for and importance of human sociality. Neither human altruism, group-level cooperation, or social identity, nor the importance of secure attachment or of neighborhood cohesion—to name a few examples—can be explained adequately without taking into account the neural and other biological processes involved in sociality (Schutt, Seidman, & Keshavan, 2015). It is a fundamental consequence of human evolution within social groups that is still too often overlooked even by sociologists who recognize the need to engage with biology (Shostak & Freese, 2010). If sociologists come to recognize that sociality and group process underlie the evolution of our species and are inherent in our biology, Recommendation 3 will become not just a means of framing sociological research but a clarion call for transdisciplinary recognition of the centrality of our discipline (Wilson, 2012).

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Martha Newson. *Response to: Developing the Field Site Concept for the Study of Cultural Evolution*.

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The creation of field stations for the study of cultural evolution akin to the ecological stations that Wilson describes represents a significant departure from traditional anthropology where data are collected by just one or two researchers at a time using largely qualitative methods. At the *Centre of Anthropology and Mind*, Oxford, we have been pioneering a new kind of field research with special populations known for their high levels of in-group loyalty and inter-group rivalry: football fans. There is a wealth of sociological and psychological literature on football cultures, but, as Whitehouse points out, descriptions are not the same as explanations derived from controlled experiments. Using a pan-global network of football fans (starting with the UK, Brazil, and Australia) we have been investigating social cohesion, altruism, and out-group hostility.

Following observational fieldwork and interviews with fans in the UK, we generated research questions and took them to the field: the FIFA World Cup, held in Brazil, 2014. Here we collaborated with a research group at a local university who specialised in obtaining physiological measures and provided us with local RAs, who we trained in our approach. We then collected data from around 400 football fans during live national matches: at fan sites, in stadia, and in field laboratories, which we set up in hostels and community centres to collect physiological measures including salivary cortisol. For this study, most participants talked to each other, many were hard to contact for our follow up measures, and some consumed alcohol—everything you wouldn't want in a laboratory. However, what we lacked in control, we made up for with ecological validity and supporting qualitative data to improve previous designs.

Field sites provide a setting not only for observational work but also experimental methodologies. Football fandom is a good example of how field laboratories enable experimentalists to access the richness of human culture. In the study above, the intensity of a live game, surrounded by other fans shouting, cheering, and crying, provided us with emotionally charged participants and created rich, textured data; helping us to set future research questions that were grounded in reality.

Dimitris Xygalatas. *Bridging the gap between laboratory and field.*

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In a set of two captivating essays, Harvey Whitehouse and David Sloan Wilson share their views on the role of the field site concept in the study of cultural evolution. They each present a vision for a holistic research paradigm that combines contextual sensitivity with methodological rigor. I salute this view, and I attempt to add to the point by drawing from my own research experience.

As someone who was trained across disciplines and has spent several years in the field as well as in various laboratories, I have come to appreciate the benefits of each research paradigm, but also to be wary of their respective limitations. More importantly, I have come to realize that the lab and the field are not antagonistic modes of inquiry, but two sides of the same coin. I find the “two cultures” problem raised in Whitehouse’s essay to be the greatest impediment to a holistic understanding of human nature, because it creates a false dichotomy, expressed along numerous dimensions: explanation *versus* understanding, measurement *versus* observation, laboratory *versus* field, and so on. I find such dichotomies counter-productive, and often detached from reality, as they are primarily the product of specific socio-political factors pertaining to academia itself rather than a response to discontinuities found in the natural world.

It is true that methodological specialization is necessary to deal with the complexity of our world. However, one should not mistake a discipline’s limitations for its virtues, or confound what is convenient with what is desirable. Laboratory experiments afford precision and simplicity. But while precision is always desirable, simplicity is a mere methodological tool that helps us increase precision, and it comes at a steep cost: in the human behavioral sciences, the more we simplify the phenomena we examine the more we move away from them, as the complexity we are trying to reduce is an inextricable part of what we want to isolate (Boster, 2011). Inversely, field observation allows social scientists to study precisely the things that matter to them (real-life), but is very difficult to

conduct in truly scientific fashion (although, as Wilson points out, not impossible). But to argue, as some cultural anthropologists do, that scientific methods are undesirable rather than simply hard to implement, seems like the result of cognitive dissonance.

As a result of this fallacious thinking, we are led to not only methodological but also epistemological and even ontological encapsulation between disciplines. This leaves us with a landscape in the study of human behavior where the vast majority of studies gravitate around one of two main attractors: on the one hand (upper-left in the graph), we have ethnographic field studies, which are high in ecological relevance and validity, but low in control and precision; and on the other hand (bottom-right), we have lab studies, which are high in control, but low in relevance. What we all want, of course, is the closest possible approximation of that (possibly unattainable in itself) ideal top-right corner. And the best way to reach that space is by building a methodological bridge that connects field and laboratory methods. This bridge can provide a path for cross-disciplinary dialogue and mutual enrichment, as well as the scaffolding for inter-disciplinary approaches that combine the strengths of each type of method.

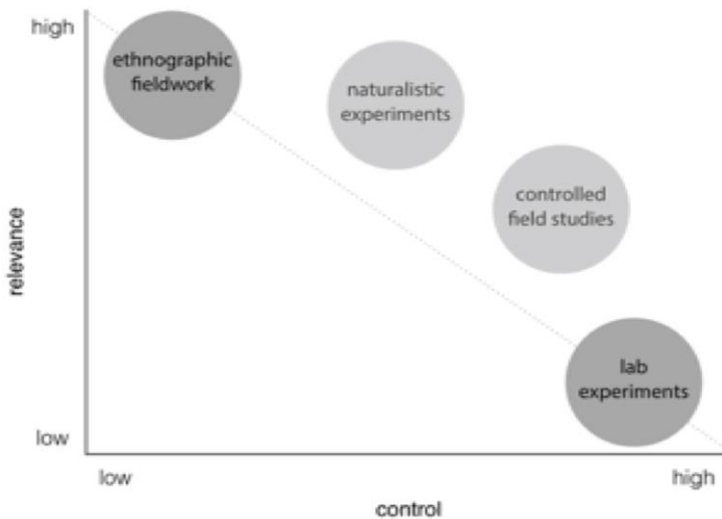


Figure 8. Participant observation and laboratory experimentation are mirror-images of one another in this landscape of relevance and control (each thrives where the other withers). Although all methodological choices have specific costs and benefits, interdisciplinary forms of research can often provide a better cost-benefit ration, thus reaching a better approximation of the ideal top right corner.

Naturalistic experiments (typically pseudo-experiments that employ precise quantification in a real-life setting without random assignment) may allow the researcher to assess phenomena that cannot be studied either in controlled experiments or by participant-observation. For example, my colleagues and I have studied the inter-personal alignment of psycho-physiological states during the performance of large-scale, highly arousing collective rituals (Bulbulia et al., 2013; Konvalinka et al., 2011; Fischer et al., 2014). Such studies would have been impossible to conduct in a laboratory setting because, a myriad practical reasons aside, these rituals are heavily laden with meaning that cannot be evoked at will in an artificial context. Neither would we have been able to approach our questions on the basis of ethnography alone, as we were interested in internal states that are often inaccessible to participants (Xygalatas et al., 2013).

Sometimes controlled experiments are also possible in the field. For example, when we wanted to study the effects of environmental cues on behavior, instead of using a highly controlled but artificial laboratory environment, we went out to the real world and used temples, restaurants, and libraries, and either randomly assigned participants to different locations or made minor interventions to the environment itself (Krátký et al., 2016; Xygalatas et al., 2016). Thus, by embracing the complexity of the real world while giving up only limited control, these designs too offered cumulative benefits that neither ethnographic observation nor laboratory experiments alone could offer.

Needless to say, some things are better studied in specific ways. If we are interested in the favorite topics of gossip within a community, conversations with trusted informants will reveal more than any quantitative method. But if we are interested in neurological reactions to gossip, the controlled environment of the neuroscience laboratory is ideal. Which brings us to another important reason for building that bridge between the lab and the field.

The systematic, incremental production of scientific knowledge consists of a circle which connects observation, theory, prediction, testing, and re-evaluation. Each time the circle is repeated, we (hopefully) become a little bit wiser. As Wilson points out, laboratory research must be informed by field observation in order to ask the right questions. But as Whitehouse emphasizes, observation and description are not the same as explanation—that requires experimentation and systematic comparison. Any individual study can only tell us so much. To gain a holistic understanding of social evolution, we need to consider cumulative evidence, completing the puzzle one piece at a time. And to do that, we need to move back and forth between field and lab studies, but also, crucially, to understand that neither do the former always need to be devoid of control, nor must the latter always be detached from real-life settings. The two authors offer

compelling arguments on why and how to do this, including the nitty-gritty of establishing, running, maintaining, and connecting field projects. They warn that this is not an easy task: it is expensive, time-consuming, and requires overcoming long-established disciplinary boundaries. I know all of that to be true from personal experience. But I also know the cumulative benefits of combining laboratory and field methods to be greater than the sum of their parts.

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Peter N. Peregrine. *On Field Sites for the Study of Cultural Evolution*.
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The essays by Wilson and Whitehouse introduce an important topic that may lay the groundwork for new approaches to the study of cultural evolution: the development of field sites specifically for investigating cultural evolution. Wilson's vision for such field sites is modeled on those used in evolutionary biology and ecology—specific locations of biological diversity that are studied over time by multiple researchers. Whitehouse looks toward a slightly different model, one more common in anthropology and psychology, in which multiple investigators located in different sites examine cross-cultural variation and change in coordination. Both are valuable models. Wilson's site-specific model provides a diachronic perspective by examining stability and change in one location over multiple time periods, but is limited in the range of variation that can be observed from this one site. Whitehouse's approach provides a broad view of variation, but does not provide the time depth that a site-specific approach does. An obvious thing to do is to combine these two—to have a range of focal sites with good temporal depth that can be compared to one another, and, as Wilson points out, this is precisely where ecology is heading.

How might we create field sites with both time depth and broad regional coverage? One model is provided by Seshat, which is attempting to use historical and archaeological data from 30 specific locations to examine cultural stability and change over long periods of time (Turchin et al. 2015). This is a model that has already been used in archaeology and history, and by combining the two Seshat will provide an extremely useful resource for the study of cultural evolution. However, Seshat is focused on gathering and coordinating extant data, not on collecting new data. How might we create a field site concept for cultural evolution that provides depth and breadth but that is based on new data?

Looking back a century (actually a bit more—to 1896) Franz Boas, in his widely-misread but seminal work on “The Limitations of the Comparative Method in Anthropology”, suggests that in order to understand general “laws” of cultural development anthropology “must not confine itself to comparing [cultures], but wherever such is feasible it must compare processes of growth, and these can be discovered by means of studies of the cultures of small geographical areas.” (p.907–908). What Boas suggested were regional studies of cultural evolution (“development” he called it, as “evolution” implied materialist theories of progress at the time), and he attempted such a study on Vancouver Island. Unfortunately, Vancouver Island did not (and still does not) have a fully explored

archaeological record, so time depth is only partially available, even though the historical and ethnographic records are exceptionally rich. Other areas of the world, for example the Southwestern United States and Mesoamerica, have rich ethnographic, historical, archaeological records that could be harvested to create the basis of a regional field site. Those regions have extant indigenous populations as well, and they might serve as informants for new investigations such as those discussed by Whitehouse.

What I suggest as a field site for the study of cultural evolution, then, is really a “field region” with multiple sites in a small area that provides both time depth and cultural variation. Perhaps a city like Binghamton, with a diverse population in a relatively small area, is an example of a “field region”. But Binghamton does not have the time depth that I think is necessary for a true cultural evolution field site (although as an archaeologist my view of time depth is probably much greater than either Wilson’s or Whitehouse’s). So, while I agree that the idea of a field site for cultural evolution is an excellent one, I think we need more discussion to determine exactly what such a field site would look like. Would it be a location, a region, or a group of spatially dispersed sites? What time depth would be desired? What degree of cultural variation would the site need to encompass? These are all answerable questions, and I hope that in time we will address them in order to initiate a serious effort to develop one or more cultural evolution field sites.

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Christopher Kavanagh¹ and Yo Nakawake². *Developing the Field Site Concept for the Study of Cultural Evolution: The Promise and the Perils.*

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D.S. Wilson & Harvey Whitehouse’s essays offer a timely call for a reappraisal of the role of ‘field sites’ when attempting to explore processes of cultural evolution.

Since one of us is a cognitive anthropologist and both of us have collected data ‘in the field’ at ritual events in Japan, we fully recognise the value of cultural evolution researchers entering ‘the field’ and conducting studies amongst actual communities. And whether this involves learning new methods to translate research protocols for the field or collaborating with those who already have such expertise is immaterial. The important point is that the central role of field research is acknowledged.

In this response, however we do not seek to offer just cheerleading approval. Indeed, as individuals who have worked closely with Harvey Whitehouse on ritual research projects we can hardly be considered unbiased commentators. Consequently, we do not provide an in-depth critical review of the target articles but instead offer, first, a complementary recommendation and, second, an important note of caution concerning ‘field site’ research.

In Wilson’s article, he raises an argument that he has stated repeatedly: that evolution can serve as a general framework to unify research in the social sciences, including that conducted in the ‘field’. We agree with this and note that there are several researchers who have already demonstrated how productive such a perspective can prove. However, we also wish to emphasise that rather than being just an effort to recommend in the future it is equally important for previous social science research that was not necessarily collected within an evolutionary framework to be reappraised.

Alex Mesoudi’s (2008) research, for example, incorporates a selection of theoretical models from social psychology, including work addressing the conditions for imitation and social learning (e.g. Asch, 1951; Festinger, 1954; Bandura; 1977), but explores their implications from an explicitly evolutionary perspective. Moreover, we also have numerous examples of how adopting an evolutionary perspective can help to dissolve interdisciplinary boundaries, enabling biologists and social scientists to work together (Conradt & List, 2009; List, Elsholtz & Seeley, 2009; Kameda, Wisdom, Toyokawa, & Inukai, 2012). Pre-Darwinian theoretical models in political science from as early as the 18th century (Condorcet Jury Theorem; cf. List, 2004) are also being used productively as one of the basic models of collective intelligence that can be used to model collective competence (Sumpter & Pratt, 2009; Wolf et al., 2013). These are just a few examples which demonstrate that taking account of evolutionary theory does not automatically require that the extensive existing social science findings be discarded. Instead, a critical reappraisal is necessary, to avoid wasting time ‘reinventing the wheel’.

A clear parallel can be drawn here from the immensurable benefits extracted in post-Darwinian biological research from the diverse body of observational data collected prior to the development of the theory. Social science research,

including material from the arts and humanities, could prove equally important to researchers of cultural evolution in the 21st Century. There are already examples of how productive such efforts can prove, including research based on phylogenetic analyses which rely on pre-existing data from linguistics (Grey & Atkinson; 2003), anthropology (Atkinson & Whitehouse, 2011; Wats, Sheehan, Atkinson, Bulbulia & Grey, 2016) and archaeology (O'Brien, Darwent & Lyman, 2001). The field of cultural psychology also represents a vast and still developing repository of information that researchers of cultural evolution should be advised to consult regularly. This is not to endorse the methods or the robustness of all the various cross-cultural theories, but to emphasise that much work has already been done in identifying cultural landscapes with relation to geo-political or ecological factors (e.g. Gelfand et al., 2011, Talhlem et al., 2014).

Clearly there is still much work to be done and a vast quantity of existing second hand material that should be consulted and analysed. Yet, it is also the case that first-hand experience with field research is hard to overestimate. Both Wilson and Whitehouse, despite long productive careers, clearly regard their own early experiences in the field as being formative and of immense value for their later research. Based on our own experiences, we would fully echo this sentiment.

However, the note of caution we wish to raise arises precisely due to the romantic allure of the field and the extra credibility that (often) accompanies 'field site' data. Although the unpredictable nature of field research can leave those trained in traditional tightly controlled laboratory studies feeling very uncomfortable, it is paradoxically the case that field research which is cross-cultural, and includes non-WEIRD (Western Educated Industrialised Rich & Developed) samples is accorded a certain level of prestige and attention. At the time of writing, google scholar records 2247 citations for Henrich et al.'s paradigmatic 2001 paper in this mould and 1062 citations for a 2006 follow up on costly punishment. Admittedly these were ground-breaking papers, but the point we wish to emphasis is that while there remains steadfast opposition to field site studies in certain quarters, in many respects the battle for the need for field-based research is a battle which has already been won. And the interest in Henrich et al.'s papers reflect that.

But the increasing prestige for studies with diverse field site research also brings with it potential risks. The first is a point raised by traditional social and cultural anthropologists, that empirically-minded researchers occasionally seek to extract data from a field site a) without being willing to put the necessary time into understanding the local context and b) with little thought of 'giving back' to the community, except maybe through acknowledgements in papers that most of their participants will never read. The unofficial term for this is "helicopter research" because it involves parachuting in and then quickly departing from a

research site (see Flicker et al. 2007). This is a serious issue and is one of the reasons that attempting to develop genuine collaborations with anthropologists and other experts who engage in long-term research is essential for those who may be new to 'field site' based research and do not have contacts with a targeted community/area. Opportunistic short-term collaborations are sometimes appropriate, but we argue that the standard procedure should be to develop long term relationships and a deeper personal familiarity with any field site that is the subject of research.

There also needs to be efforts made to offer meaningful benefits to the communities studied, where this is possible. Compensating participants for their time is a basic requirement, but there are many other less-direct ways to provide something valuable to communities without compromising research ethics. For example, when collecting responses for an online survey on ritualised promotional experiences amongst Brazilian Jiu Jitsu (BJJ) practitioners, we also collected more general information about training experiences and then presented the information as infographics on a freely accessible website (www.bjjsurveys.com). This was warmly received by the wider BJJ community and entailed no substantial financial cost. This is not a call to adopt the advocacy model widely found in social and cultural anthropology, which brings with it the potential for a host of conflicts of interest, rather it is to advocate that as a field we should seek to early on establish good standards of practice.

Another general risk of field site research relates to the eventual presentation of research and the potential increase in researcher degrees of freedom that uncontrolled field environments provide. Consider, for example, the image below:



Figure 9. Set-up for economic game in a Buddhist temple in Japan. Photo by authors.

This is an isolated booth we built in the corner of a Buddhist temple in Japan to enable participants to take part in a simple economic task designed to measure trust. Upon completing our survey, participants were escorted to the booth to collect a small monetary reward for taking part. Before entering, they were instructed to select one of two envelopes: the first had included a set amount of money (300 Yen) and the second an amount divided by another attendee at the festival which could range from (0–1000 Yen). The rationale for the task is that if the participant *trusts* that the other festival attendee was fair then they should expect them to have split the 1000 Yen award evenly, meaning that there should be 500 Yen in the envelope (the other attendee took the remainder for themselves) and that it is therefore the more attractive choice. Alternatively, if the participant suspects that the other attendee would have been greedy and took more than 700 Yen for themselves, then they should select the other guaranteed amount envelope which they know contains 300 Yen.

The image above could easily be presented in an article as evidence that we built a semi-controlled, private, and somewhat sterile environment during a busy firewalking festival. But that would be misleading. For a start, it would ignore that directly facing the boxes was the following image:



Figure 10. Photo by authors.

Not exactly a neutral environment, especially with the claims made about eye-spots and prosociality. However, that's not all. Here's another shot of the outside of the 'booth', which you might note contains a rather prominent picture of the Dalai Lama, a figure prominently associated with moral behaviour.

These are the kind of environmental details which are very difficult to avoid

when collecting data in a Buddhist temple, but they could easily be omitted from a journal article. What could also be omitted is that it became clear during data collection, based on what many respondents said when inside/leaving the booth, that they were trying to select the envelope with the lowest amount to be more generous or not appear greedy. Following the logic of the trust task, such a response technically indicates the participant has less trust in other festival attendees, but this was clearly not the motivating factor for these participants. And this is a serious problem because the trust task protocol only works if it is reasonable to expect that people are profit maximising, but this did not appear to be a valid expectation within the surrounding religious context.



Figure 11. Outside of the booth. Photo by authors.

In addition to the issue of motivation, there was also a significant problem with comprehension. We had designed and piloted a trust task that we thought would be very simple to understand and we were collecting data in Japan, where there is a high standard of education amongst the general population. However, even with detailed written and verbal instructions, the task proved to be very confusing, with several participants exiting the booth to ask the experimenter which envelope to choose or inviting friends to join them in the private area. The point here is not to denigrate our participants, but to emphasise that even in a country with a literate, highly educated population, a simple economic behavioural task can prove very challenging to implement. For researchers

working with isolated tribal communities that lack experience with currency, who rely on local translators, such issues are likely to be magnified greatly. See, for instance, the dismay of the economist Andreas Ortmann (2005) about the various ‘idiosyncrasies’ in framing, participant recruitment, and experiment instruction reported in Henrich et al. (2001).

Ultimately, we did not make use of the trust task data because of the myriad of methodological issues. However, this decision itself also represents the exercise of a potentially problematic researcher degree of freedom. The non-reporting of outcomes and selection of only ‘successful’ results can be a serious problem and result in inaccurate biases seeping into the research literature. Selective reporting appears to be a significant factor in what has come to be known as the ‘replication crisis’ in psychology and other disciplines. The solution here lies both in field site researchers being honest about limitations (we intend to report the failure of the task in the final paper) and in adopting more contemporary research standards, including: the pre-registering of studies and outcomes and providing open access to data for other researchers to explore.

While the above might sound negative, we would like to end our response by reiterating that we are in full agreement with both Wilson and Whitehouse’s arguments in favour of the importance of field site research and that an evolutionary perspective can serve to unite future research efforts. We believe that such an approach, if conducted with appropriate care can provide a wealth of new insights and even potentially serve as a bridge to bring together disparate disciplines. But to achieve this it will be essential to both reappraise existing social science research—regardless of its evolutionary underpinnings, and take due consideration of the methodological and ethical issues we raise above.

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Veronika Rybanska. *Addressing the Field Site Concept: A Cognitive Anthropologist's View.*

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In the essays presented by Whitehouse and Wilson, both authors present an account on developing field sites for studying cultural evolution. The target essays are presented from the viewpoints of an evolutionary biologist (David Sloan Wilson) and anthropologist (Harvey Whitehouse). While I am generally supportive of their proposition, there are also logistical concerns with maintaining field sites that should be discussed. This commentary is presented from the viewpoint of a cognitive anthropologist and a recent doctoral graduate who, despite a relatively short career, has conducted her own research at multiple field sites.

There is a particular point, stressed by Whitehouse, which I believe should be a focus of future research projects. Namely, evolutionary approaches should be the theoretical foundation for the investigations at field sites such as those described by Whitehouse and Wilson. Whitehouse has noted that the differences between Evolutionary Psychology (EP) and Evolutionary Theories of Culture (ETC) may not be that great. This is not a new stance, as he has argued for it before (Whitehouse, 2004). Synthesizing the different evolutionary approaches to culture should be a target for projects such as those outlined in the target articles. This is because finding a theoretical framework that is appropriate for investigating the wide variety of cultures targeted by projects such as [AnthroLab](#) must walk a fine line between generalizability and contextual sensitivity. However, as outlined here and elsewhere, it has been noted that both EP and ETC have their merits in helping to develop new hypotheses for research. Although

many question the utility of ETC as anything more than an analogy to biological evolution (Knudt, 2015), the general focus of cultural evolution on information that is socially learned provides a focus to what it is about human social groups that make them unique. This focus on unique socially learned behaviours can be combined with evolutionary psychology, which posits that human minds evolved in order to process such social information. By assuming that all naturally developed human minds share a suite of cognitive mechanisms which evolved to process different information—including socially transmitted information (a point stressed in earlier writing on evolutionary psychology, e.g. Tooby & Cosmides, 1992, p. 24)—we can use evolutionary theory as a foundation for the study of culture.

I find the proposition of synthesizing EP and ETC quite persuasive, as I have embraced it in my own research. During my most recent fieldwork, I studied how the development of executive function and the ability to delay gratification in children can be manipulated by adopting a ritual stance or instrumental stance (Rybanska, McKay, Jong & Whitehouse, in press). Briefly, individuals adopt an instrumental stance towards learning instrumental skills, assuming that the modelled actions are performed in the service of a specific concrete goal in accordance with normal expectations about physical causation (e.g. washing hands). On the other hand, individuals adopt a ritual stance towards learning conventional, ritualised behaviours, i.e. actions are executed in a certain way simply because it is demanded by a convention, with no clear links between actions and goals (e.g. ritual washing). These different ways of approaching actions (as instrumental or ritual) should have effects on our cognitive mechanisms. More specifically, because a ritual stance demands close attention to actions and the necessity to perform these actions correctly, it puts greater demands on executive function. This generated the hypothesis that performing actions that promoted the adoption of a ritual stance—as opposed to an instrumental stance—should have positive effects on executive function. I took this hypothesis to not one, but two field sites: Slovakia and Vanuatu. What I found was that adopting a ritual stance increased executive function and the ability to delay gratification in both field sites, and there were no significant differences between the two (Rybanska, et al., in press). In this way, one can argue that the cognitive underpinnings of executive function, the ability to delay gratification, and even those that govern the adoption of ritual or instrumental stances are likely evolved psychological mechanisms that recur cross-culturally and develop at similar points in childhood.

However, it does not take a keen eye to see that there are vast differences in the rituals performed in Vanuatu and Slovakia. While most rituals in Slovakia would be familiar to anyone with basic knowledge of Central European cultures,

rituals in Vanuatu can sometimes involve rare, dangerous rituals, such as land diving, where men jump from a wooden tower with only vines tied around their ankles. At the same time, some communities in Vanuatu have been exposed to European missionary efforts, and have adopted many of their ritual practices. The historical contexts that outline the shifts from one form of ritual to another can be interpreted through the lens of cultural evolution, as noted by both Whitehouse and Wilson.

Although as a cognitive anthropologist I greatly appreciate both Wilson and Whitehouse stressing the importance of fieldwork, as a researcher who operated in multiple field locations in both Europe and the South Pacific I would like to stress a logistical issue that cannot be overlooked; namely, the issue of funding. While Wilson suggests that funding is not of utmost importance, the idea that this sort of research can be sustained at a university without funding is unrealistic. Students, particularly graduate students—who are producing much of the work in the field—cannot sustain themselves, pay the fees requested by universities, and sustain a field site without additional funding; even paying undergraduates “very affordable wages” is an additional cost not afforded to most researchers out of tenure track. Although Wilson appears to recognize that tenured faculty have certain liberties to pursue such research because they are permanently salaried, such positions are increasingly rare (The American Federation of Teachers, 2003) and currently some universities are employing as many as 70% of their employees on temporary contracts (Chakraborty & Weale, 2016). Such a system is not conducive to setting up and sustaining field sites in and around universities. Furthermore, setting up and sustaining field sites in multiple remote locations entail additional costs of travel and lodging which are not feasible given the economic circumstances of many researchers who are not beneficiaries of research grants. As such, the sustenance of the field sites may be subject to fits and spurts of research as they fall between cracks in funding cycles.

Whitehouse acknowledges that “one of the most obvious barriers to progress is funding”. However, one could add that it is not just securing funding, but the efficient and appropriate allocation of funds to sustain a project as ambitious as that outlined by Whitehouse, which involve costs such as research assistance, travel, lodging, equipment costs, and other research expenses. It is true that some research expenses can be cut down. Using my own research project as an example, I studied the vernacular language of Vanuatu (Bislama) which enabled me to not only conduct all of my research in the field without a translator and thus eliminating significant costs, but also, as an anthropologist I was able to create greater rapport with the local communities. Although this is an imperative for establishing a field site, it is being neglected by many researchers, creating distance and lost meanings between researchers and communities. From an

anthropological perspective, creating bonds with the local communities is of high importance, although this is not always the case as some researchers treat local communities as merely their own personal participant pool.

Logistical concerns notwithstanding, it is admirable that both Whitehouse and Wilson are emphasizing the importance of fieldwork for the study of culture. It is true that this has been underrated and neglected and many researchers have felt that field work can be replaced by lab experiments with college students. Wilson and Whitehouse are right that in order for us to understand culture, it is in relation to our environments, both biological—as stressed by evolutionary psychology—and social—as stressed by cultural evolution—and that a well validated theoretical perspective can generate insights and explanations that lab experiments alone cannot provide.

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Michele J. Gelfand and Joshua Conrad Jackson. *Defining and Implementing Field Sites in Cultural Evolution Science.*

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Harvey Whitehouse and David Sloan Wilson write two important commentaries on the role of field sites in the study of cultural evolution. Their contribution is especially timely considering the recent formation of the Cultural Evolution Society (CES). We have had the pleasure of working with Wilson and Whitehouse as part of the society's steering committee and in this commentary we offer a psychological perspective on their vision of field sites in cultural evolution science. First, we seek to clarify what exactly a "field site" is in the context of research on human behavior. Next, we offer some recommendations for the types of field sites that may best serve cultural evolution as the field gathers momentum.

Briefly Considering Definitions

Consider whether the following data-collection centers should qualify as "field sites":

Center A: A zoo-based research laboratory, where non-human animals are raised and studied in captivity.

Center B: A study of fraternities and sororities at a large public university.

Center C: A study of online gaming communities which offer small amounts of money for participants to fill out online research surveys.

Center D: A small village in Papua New Guinea where a team of scientists are collecting behavioral data from local inhabitants.

Based on their commentaries, it is fairly clear that Wilson and Whitehouse do not consider Center A to qualify as a field site. Both authors point out that field sites take place at specific geographical locations where organisms can be observed in their natural habitat. These sorts of investigations can yield insights that experiments on captive animal populations could never offer. Wilson cites birds' migratory patterns—which could never be observed in captive populations—as an example of such insights.

However, it is less clear whether under the authors' definitions, Centers B-D would qualify. Whitehouse appears to disparage university campus research, noting that "restricting the study of cultural evolution to university campuses would arguably be equivalent to trying to study biological evolution exclusively in a zoo or aquarium," yet human subjects in a university laboratory actually have far more in common with Peter and Rosemary Grant's finches than any animal in captivity; both laboratory undergraduates and wild finches are living in a rich naturalistic environment—such as the above case of living in fraternities and sororities—that is informing their responses to experimental conditions and stimuli.

Similarly, in a study of online communities, participants may be taking a survey on a computer, but well-designed surveys of the dynamics of such communities approximate real life experiences in meaningful ways. In this sense, a high quality computer survey could rival any in which researchers fly to far-flung areas of the world to personally run experiments using non-WEIRD samples ("Center D"). In both cases, experimenters are testing valid theories of cultural evolution using targeted populations.

This exercise suggests that we need to be wary of using a narrow definition of "field sites" in the study of human behavior. Are they data collection centers where scientists study a specific population's interaction with their environment? If so, many centers (virtual or face-to-face) where people collect data from human subjects could qualify as a field site. Or are they data collection centers where researchers study a small sample over time? If so, then hardly *any* mode of human subject data collection qualifies as a field site; indeed, this latter definition seems prohibitively narrow to serve as the paradigmatic foundation for a field as broad as cultural evolution.

Moving Beyond Definitions

The previous section's semantic puzzle suggests that we should be doing more than simply recommending that researchers use field sites. Instead, we should be offering guidelines for *how* researchers should be employing their field sites broadly construed (i.e. centers of data collection on human behavior) to best serve cultural evolution theory. To this end, we offer three simple methodological recommendations—based on our own research experience—for how field sites should best be employed in cultural evolution scholarship.

1. Field Sites Should Be Cross-Cultural. Each human society operates within unique ecological constraints, and cross-cultural research is ideal for mapping the influence of ecological variance on cultural evolution. For example, in our research, we have found that levels of ecological threat facilitate the development

of stronger cultural norms (cultural tightness). This relationship occurs because cultures under threat face increased pressure to coordinate their behavior to sufficiently compete with other cultural groups, and we would never have observed it had we not been able to collect survey and archival data from 33 countries alongside dozens of international collaborators. We define “cross-cultural” very broadly—it could include but is not limited to variation across national, state, community, religious, class, and ethnic groups.

2. Field Sites Should Be Multi-Method. Research on human behavior should operate at multiple levels of analysis using multiple methods to test for theoretical convergence. Human behavior varies from society to society, from state to state, from situation to situation, and from person to person. A strong cultural evolution theory should make predictions at each of these levels of analysis, and doing so requires methodological breadth. In our research on culture and norms, a combination of big data analysis, experimental designs, survey data, computational models, and neuroimaging have revealed that tightness-looseness has a fractal nature—no matter what the context, threat facilitates the development of stronger norms, which results in a range of downstream effects on attitudes and behavior which we refer to as the tight-loose trade-off for nations, states, groups, and individuals. Tight groups, for example, have greater order and self-regulation but greater ethnocentrism; loose groups have much more disorder and self-regulation challenges, but are more open and creative. By investigating this trade-off across levels and with multiple methods, we can begin to build general principles for the field of cultural evolution.

3. Field Sites Should Be Interdisciplinary. One exciting feature of cultural evolution research is its post-disciplinary nature. The CES membership already involves dozens of fields, and research in cultural evolution often involves interdepartmental collaboration. This approach to science minimizes the risk of theoretical redundancy, and helps researchers learn from one another, rather than talking past each other. Field sites should be no different, and we have tried to capitalize on interdisciplinarity in our research on tightness-looseness. At the moment, we are collaborating with anthropologists, computer scientists, political scientists, sociologists, management scholars, neuroscientists, and biologists in an effort to better understand the relationship between ecology and the strength of social norms. These collaborations have expanded the questions we ask, the way we can test them, the samples on which we test them, and have helped us broaden the implications of our findings.

We derive these insights from our own research, but see no reason why they

shouldn't apply to cultural evolution methodology in general. Indeed, given that field sites connote very different meanings across human and animal behavior, we believe that simply recommending them may not be enough to paradigmatically guide cultural evolution scholarship. If field sites are cross-cultural, multi-method, and collaborative across disciplines, however, they can improve the quality of our field, and help us make major steps toward understanding the evolution of human behavior.