COMPUTER SIMULATION AND ARCHAEOLOGY

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On the cover: Excavation of a Birnirk culture house at the Rising Whale Site (KTZ-304), Cape Espenberg, northwestern Alaska, in the summer of 2017. The excavation of the Rising Whale site is part of the NSF-funded Cape Espenberg Birnirk Project (NSF #1523160). The project proposes to conduct interrelated research in archaeology, anthropology, and paleoecology to explore the question of the origin of Inupiaq culture in northwestern Alaska at the end of the first millennium AD. The project documents the cultural histories, social interactions, population diversity and dispersal, and environmental changes from data collected at Cape Espenberg and in relation to the greater Bering Strait and Arctic regions. For more information: https://www.nsf.gov/awardsearch/showAward?AWD_ID=1523160
Photo courtesy of Claire Alix and Owen Mason.
EDITOR’S CORNER

Anna Marie Prentiss

Anna Marie Prentiss is a professor in the Department of Anthropology at the University of Montana.

In 1989, the paleobiologist Stephen Jay Gould encouraged us to “replay life’s tape…” in the expectation that minor evolutionary tweaks early-on could cause dramatically different outcomes down the line. Gould’s provocative argument stimulated a wide-ranging discussion and debate in the biological and paleobiological sciences about the inevitability of different life-forms. Archaeologists address similar concerns. A couple of generations back, anthropological theorists proposed a stair-step model of cultural change that always led to a socioeconomic and political configuration typically termed a state. More recent perspectives implicate diversity in pathways to a wider variety of stable and not so stable cultural configurations. While paleobiologists have invested substantially in computer simulations to rigorously imagine diverse evolutionary histories, archaeologists have been slower to make use of these tools. But this situation may be changing. In recent years, a relatively small group of archaeological scholars has begun to make the case for computer simulations to help us to imagine alternative pathways to cultural diversity, and interest in this topic is growing.

In this issue of The SAA Archaeological Record, guest editor Cheyenne Laue develops a collection of essays about computer simulations in archaeology. The contributors to the special section offer a number of arguments about archaeological modeling using computers. Bocinsky challenges us to consider improbable pasts. Given the potential for diverse pathways, perhaps no specific cultural configuration might be considered highly probable, and one way to explore alternatives is with simulations. Crema points to the importance of simulations in the process of hypothesis testing. Good examples of such research are evident in recent advanced studies of radiocarbon distributions. Premo explores student perspectives on simulations, recognizing that simulations create abstractions that help us to understand a complex world. Davies and Romanowska present data on the demographics of modelers and consider recommendations for developing communities of practice. Finally, Laue draws on artificial life concepts to challenge us to think outside of the box as we creatively imagine alternative pasts and futures. These contributions make the case that simulations can significantly enhance our research whether for creatively replaying the tape and imagining improbable outcomes or more rigorously exploring our data. Perhaps the time is ripe for an expansion in these endeavors as a new generation of tech-savvy scholars enter our classrooms and offices ready to explore our archaeological past in new ways.

Additional contributions to this issue include Fernandez-Diaz and colleagues’ discussion of ethical concerns associated with the application of remote sensing technologies, SAA President Susan Chandler’s column, and our Volunteer Profile from Tanja Hoffmann. Hoffmann not only encourages diverse forms of volunteering but also reflects on themes regarding community and indigenous archaeology raised in recent issues of The SAA Archaeological Record (see September 2017 and January 2018 issues).
I am pleased to announce the results of the SAA 2018 Elections. I wish to offer my congratulations to those who were selected and to offer my sincere thanks to all of those who stood for election.

Joe E. Watkins has been elected the SAA President-Elect. Joe received his PhD in 1994 from Southern Methodist University and is currently the Chief of Tribal Relations and American Cultures at the National Park Service in Washington, DC. He will serve on the SAA’s Executive Committee while job-shadowing me this coming year and will become the SAA President at the annual meeting in 2019, serving for two years thereafter.

Teresita Majewski was elected Secretary-Elect. Terry received her PhD in 1987 from the University of Missouri Columbia and is a vice president at Statistical Research, Inc. and adjunct associate professor in the School of Anthropology at the University of Arizona. She will, too, will become a member of the SAA Executive Committee, serving one year learning the job of secretary before assuming that position from 2019 to 2021.

Two persons were elected to the SAA Board of Directors: Lynne P. Sullivan (PhD 1986, University of Wisconsin Milwaukee) and Heather A. Lapham (PhD 2002, University of Virginia). Their terms begin at the 2018 Annual Meeting and extend until the 2021 Annual Meeting. Lynne is the Curator of Archaeology Emerita/Professor at the McClung Museum of Natural History and Culture, University of Tennessee. Heather is a research archaeologist at the Research Laboratories of Archaeology and an adjunct associate professor of anthropology at the University of North Carolina, Chapel Hill.

Scott Van Keuren and Erin Baxter were selected for one-year terms on the Nominating Committee. Scott received his PhD in 2001 from the University of Arizona and is an associate professor in the Department of Anthropology at the University of Vermont. Erin received her PhD in 2016 from the University of Colorado Boulder, where she now serves as an adjunct professor in the Department of Classics and Department of Anthropology.

Three SAA Board members will complete their terms of office at the 2018 Annual Meeting: Deborah Nichols, Treasurer; John Douglass; and Gordon Rakita. I have enjoyed serving on the Board with them and thank them for their hard work and dedication to the Society.

The 2019 Nominating Committee will be chaired by former Treasurer, Deborah Nichols. The committee will include the two members recently elected by the SAA membership as well as two members to be appointed by the Board at the Annual Meeting. The Chair of the Nominating Committee is a nonvoting member of the committee except in the case of a tie. This year, the Nominating Committee will be searching for two candidates for each of the following positions: Treasurer-Elect, Board position 1, Board position 2, Nominating Committee position 1, and Nominating Committee position 2. Any SAA member in good standing can also suggest the names of candidates (or themselves) for any of the open positions. The call for nominations will be published in the May issue of The SAA Archaeological Record.

I hope that if you are asked to run for office that you will accept the nomination. Although a commitment of time and energy, I believe that serving SAA is an important professional responsibility and one that can be rewarding on both a professional and personal level.

In this era of #MeToo revelations, SAA is aware that the list of persons affected by sexual harassment include a number of our
SAA, as a sponsoring organization of the Register of Professional Archaeologists (RPA), urges our qualified members to register. The Register is in the process of adding to its Code of Conduct an explicit statement on harassment in all its forms. There is no recourse for addressing violations of the Register’s Code of Conduct or Standards of Research Performance if the SAA member is not an RPA. On the flip side, the Register will also support and defend an RPA who has been falsely accused of violating the Code or Standards.

New “Area of Interest”

Following the suggestion of the Rocky Mountain Anthropological Association Board of Directors, SAA has added the Rocky Mountain region to the list of Areas of Interest for member profiles and for future Annual Meeting programs. Previously, members conducting research in the Rocky Mountain region had to either choose “Other” or default to an adjacent listed region, such as Plains or Great Basin.
Working for First Nations in support of community-directed research agendas has shaped the majority of my career in archaeology. The pursuit of community-directed research has meant that I have been asked to apply my skills in a variety of interdisciplinary settings. These range from archaeological excavation, to land claim negotiations, to cultural impact assessments. Each new project presents new research challenges. Moreover, the research agendas themselves, whether created to pursue community goals or activated in response to the initiatives of an outside agency, have required me to expand my own knowledge beyond that typically afforded the archaeologist. My desire to meet the research requirements of community-directed initiatives led me to pursue an MA degree in First Nations Studies from the University of Northern British Columbia, and a PhD in Resource and Environmental Management at Simon Fraser University. My PhD thesis was completed in collaboration with the Katzie First Nation of the Lower Fraser River Valley, in British Columbia, Canada. The dissertation considers how Katzie responded to the cultural impacts generated by the construction of a multimillion dollar bridge through the centre of their traditional salmon fishing grounds.

I began working directly for First Nations communities as a 23-year-old graduate student, and 25 years on I can safely say that the communities for whom I have worked have raised me, both personally and professionally. Many First Nation Elders, leaders, and community members have contributed to my education, and they have taught me several difficult but necessary lessons along the way. Working directly with and for Indigenous communities means that I have been asked to confront the assumptions embedded in Western archaeological thought, especially those concerning privilege and ownership. Throughout my years working in Indigenous communities, I have volunteered at many community events. I can safely say that among my most rewarding personal and professional experiences have been my volunteer activities.

I now live in Cambridge, UK, far from familiar people and places of Canada’s Northwest Coast, but very close to what many might consider the birthplace of Western archaeology. Upon moving to England I volunteered for a local community archaeology group that conducts surveys and excavations of a range of interesting sites. Volunteering with this group has allowed me to discover more about my own English heritage as well as about English archaeology; it has also given me firsthand experiences that I can reference to compare methodological and theoretical approaches in North American and English archaeological traditions.

Additionally, I have volunteered to deliver a number of public lectures on ongoing collaborative research initiatives. In particular, the results of the excavation of a 3,800-year-old Northwest Coast (Katzie) wapato (“Indian potato,” Sagittaria latifolia) gardening site has generated considerable interest among UK academic and lay audiences. Most of my lectures highlight projects, like the wapato garden excavation, where Indigenous leaders and community members were directly involved in developing and implementing the research goals for a particular project. I feel as though volunteering to speak about these projects contributes in a small way to furthering our collective understanding about how archaeological practices have changed (in my opinion for the better) as a direct result of collaboration with Indigenous communities.
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Dramatic changes have occurred over the past 10 years in the practice of archaeology due to the increasing use of remote sensing technologies (RST), and, in particular, the use of mapping airborne light detection and ranging (lidar) in forested areas. Although archaeologists have used remote techniques such as aerial photography since at least World War II, nearly all archaeological projects today incorporate some sort of advanced geospatial data, equipment, or analysis. These might include combinations of a geographic information system (GIS), ground-penetrating radar (GPR), geophysical sensing, satellite or airborne imagery, or airborne and terrestrial lidar, among many others. The incorporation of these tools has led to significant scientific observations at the global scale. They have transformed the way that we collect data as well as identify areas for further investigation or that are at risk of looting, deforestation, or other threats, and they contribute to debates about the Anthropocene, citizen science, and democratizing research (e.g., Chase et al. 2012; Chen et al. 2017; Evans et al. 2013; Fisher et al. 2017; Khan et al. 2017).

While many scholars agree that RST has revolutionized the way that we conduct archaeology, there has been less attention to how these tools shift our perspectives and their ethical implications. We argue that it is critical to identify and understand these changing perspectives, and their associated ethical impacts. The following discussion is informed by our experience with lidar and RST archaeological projects in Mesoamerica, but particularly by our experiences in Honduras (Figure 1).

Our work in Honduras began with a lidar survey in 2012 for the archaeological prospection of uncharted valleys in the Mosquitia region and subsequent field verification of the results in 2015. In 2016, we excavated a cache of partially exposed stone artifacts and conserved a site at risk of deforestation and looting (information on these activities can be found in Fisher et al. 2016; Preston 2013, 2015). More recently, we have been analyzing lidar data for Honduras acquired in 2000 for the US Geological Survey (USGS). These data were collected to develop flood risk maps for 15 municipalities after the devastation caused by Hurricane Mitch in 1998. The data also contain substantial archaeological information. The Mosquitia projects received a large amount of public attention due to media coverage in the US and Honduras, but they also received criticism from indigenous groups and academics. Some of the critiques had elements of merit while others were the result of misinformation and media spin. The most interesting of these centered on the ethics of lidar and RST.

We have given significant thought to these critiques raised with respect to ethics. We believe that our experiences and perspective can be informative to other researchers working with or about to embark upon large-scale RST or lidar activities. Through the increasing use of lidar and other RST analyses, we are experiencing a series of perspective shifts in terms of how we view landscapes and ancient sites, and how we conduct our research activities. We believe that we should consider updating or modifying our ethical principles, or, perhaps we need to reconsider our attitudes to the principles that we currently follow. Below we provide some historical context on the current set of SAA principles, followed by a discussion about the shifting perspectives instigated by RST, and the associated ethical opportunities and challenges informed by our experiences in Honduras.
Many archaeological societies around the world have codes of ethics, which often specify requirements of truth telling and fidelity (Wylie 2003). These codes emphasize that archaeologists have a responsibility to benefit, and not harm, the living peoples who have an interest in archaeological sites, the archaeological record itself, and human cultural heritage in general. The SAA Principles of Archaeological Ethics follow this pattern. Resulting from a community–participatory effort that began in 1991 with the establishment of an ad-hoc Ethics in Archaeology Committee, a draft of principles was first produced in a 1993 workshop and formally adopted in 1996 (Lynott and Wylie 1995). These principles are works in progress, and they are suggestions about what practitioners should do when negotiating their complex responsibilities (Lynott and Wylie 1995:8). The current nine principles relate to stewardship, accountability, commercialization, public education and outreach, intellectual property, public reporting and publication, records and preservation, training and resources, and safe educational and workplace environments. Details of each principle are available on www.saa.org.

At the time that the current principles were developed, RST, and the interconnectivity of individuals across the world were a tiny fraction of what they are today. In 1991, the World Wide Web was just two years old. By 1995, the internet was accessed by about 0.4% of the global population, and the Multispectral Scanner (MSS) sensor onboard the Landsat 5 satellite was producing publicly accessible images with a resolution of 60 m per pixel. In addition, while there have been some prototype airborne lidar sensors since at least 1985, they did not have the capability to penetrate thick vegetation canopies, and they produced low-resolution data.
Today, about 52% of the global population has access to the internet. Web-based platforms like Bing or Google provide free public access to worldwide imagery at 30 m resolution or better, and in certain cases such as urban areas, there is imagery with a 60 cm resolution or better. Currently, lidar provides researchers with the ability to see what a few decades ago was concealed under forest canopies, and to map hundreds or thousands of square kilometers in a few days with sub-meter spatial resolution. Embedded in these Big Data—some of which are publicly available—are pieces of the archaeological record, which can be used for both the preservation and destruction of cultural resources.

Perspective Shifts

From Ground-Level to a Bird’s-Eye View

The most transformative aspect of RST to archaeology is the ability to rapidly prospect and map large areas at a relatively low cost. Traditional pedestrian survey has driven archaeology and contributed to important theoretical perspectives. Nevertheless, with RST, we can dramatically expand our sample by gaining access to remote areas where performing traditional archaeology is too complicated, dangerous, or costly. By employing lidar, we can see sites in relation to the landscape at sub-meter level resolution. Many features, which could not be identified during field surveys or were classified as natural features, have been reclassified as human-made or modified after accessing high-resolution topographic data.

Importantly, however, RST comes with limitations, including that not all cultural features are recorded. A few challenges that arise from this changing perspective include the possibility of becoming myopic toward monumental sites, detachment from the sociocultural and environmental complexities of the regions under study, and the difficulty of establishing long-term and sustained research programs in remote areas. Several ethical questions immediately arise. Should we engage in large-scale archaeological prospection projects to identify and map cultural resources that we are unable to protect? Can the datasets fall into the wrong hands and facilitate looting? How does the role of foreign versus local archaeologists change with the use of RST?

Over 40 years ago, William D. Lipe (1974) argued for a conservation ethic by instructing archaeologists to identify, protect, and conserve archaeological resources for maximum longevity. More specifically, he recommends that we forego excavation of sites that are not threatened or if we can meet the data needs of a problem from salvage site datasets. For Lipe, conservation is a means of ensuring that future archaeologists with better tools will have datasets with which to work. Should we apply this conservation ethic to RST? Alternatively, since RSTs are not destructive, should we maximize their use in our efforts against deforestation, development, and other factors that threaten cultural heritage? How might these questions differ when applied to various contexts in the US and elsewhere?

Throughout our Honduran experiences, we faced these questions. Our 2012 lidar survey had to be reduced because analysis of satellite images obtained shortly before the flights indicated that substantial areas within one valley had been clear-cut (Figure 2). In our analysis of the 2000 USGS lidar, we found that many sites that were identifiable in 2000 are indiscernible today. This highlights the rapid pace of cultural and environmental destruction, which is certainly not exclusive to Honduras. Moreover, such rapid destruction should be enough to justify conducting lidar surveys as early as possible even when we may not be able to protect all identifiable cultural resources. Having these datasets along with detailed site inventories allows us to prioritize and to optimize whatever resources we as a community and/or government have for research and preservation. Ultimately, these datasets are part of the archaeological record that does not degrade, and, in many cases, will be the only record that remains. Waiting for better tools is not an option in many parts of the world.

From Local to Regional

RST enables us to expand our research from local to regional scales, and in some cases, it can help us transcend current political boundaries that did not exist in the past. Here, the main challenge is that the number of stakeholders and groups of people impacted by remote datasets grows exponentially. While we may want to comply with ethics such as Principle 2—consulting with affected group(s)—this becomes harder as our mapping scales expand. In some cases, affected groups (e.g., descendant communities, landowners, squatters) may have conflicting views of a mapping project, and there may be conflicts between governments and affected groups. As researchers, we have the duty to comply with regulations regarding cultural resource protections at the local and national levels while we observe professional ethics guidelines. In certain jurisdictions, regulations stipulate that cultural patrimony belong to the entire nation rather than to a specific community. Thus, conducting a large-scale survey, which will improve the country’s site inventory and which helps to develop research and protection plans, may benefit the broader nation despite the opposition of a few. How are we to balance between our stewardship and accountability duties?

After our 2012 survey and 2015 verification, we faced a similar dilemma. While we had full support and permissions from the government authorities and many academics prior to our research, certain academic and indigenous representatives expressed opposition and criticism. We view these as learning opportunities, and in current and future work, we are intensify-
From Physical to Digital

Central to the discipline of archaeology is the tangible, physical object. We work with sites, structures, and artifacts, and we record these objects through maps, drawings, photographs, and replicas. RST, digital mapping, and surveying have facilitated a shift from physical to digital domains. Technology now allows us to record most elements of the archaeological record in digital multidimensional datasets that can reside in many places at the same time. These digital datasets become the archaeological record and some are potentially accessible to researchers and interested publics around the world. In many cases, these digital artifacts will be the only remnant of the archæological record as their physical counterparts may degrade, vanish, or remain off-limits while respecting indigenous practices.

The digital archaeological record is clearly subject to the guidelines set in Principles 5 through 8. What is less clear, however, is who curates and archives these datasets. Who can access them? How do we ensure that these datasets are translated into formats that will last for posterity? In addition, how do we acknowledge intellectual property (i.e., of the creators, enhancers, and curators)? In Honduras, we began with the historical record and proceeded to airborne lidar survey, but during our ground verification and excavation, we documented the archaeological and environmental contexts in 3-D. We employed a combination of terrestrial lidar, handheld scanners, drones, structure-from-motion, and photogrammetry (Figures 4 and 5). We have learned that these tools and datasets are not the only ways to excel in our duties set forth in Principle 7 (records and preservation), but they are also ideal for Principle 4 (public education and outreach). We still face the challenge of finding adequate solutions in terms of long-term hosting, curation, and distribution of these digital artifacts, especially within the academic peer review process. As a research community, we must build a collaborative and open infrastructure that will allow for the optimal development and use of digital datasets.
We also learned that the press is very interested in stories that combine the old with innovation, especially those about “lost” or “discovered” “civilizations.” While public interest in such topics is not new, considering the best strategies for communicating accurately with the press needs to be paramount. Certainly, engaging with the media is an excellent and necessary way to reach the broader public; however, the challenge is in articulating the scientific message for public outreach across our hyper-connected world. As scientists and scholars, we face an uncertain future with funding resources, politics, and broader institutional change, and we must consider how best to engage with the media. After all, the public often funds our research and is interested in our interpretations and results.

From Carefully Designed to Opportunistic Analysis
After we design our research, national authorities oversee our fieldwork to ensure that cultural resources are properly handled and protected. Construction and other natural phenomena can expose archaeological materials, and archaeologists may then conduct quick assessments. Sometimes, archaeologists conduct salvage excavations to recover and protect these cultural resources. RST data are collected daily from satellite-based sensors at the local and regional scale for a variety of non-archaeological purposes, and some of these data are publicly accessible and contain archaeological data. Similar to salvage archaeology, should we comb through these and other datasets to make sure that materials are properly documented and protected in accordance with Principles 1 and 7 (stewardship and records)? How might we modify our carefully designed methodologies into more opportunistic research to take advantage of the RST data deluge?

In many jurisdictions, archaeologists need to conduct cultural impact assessments for infrastructure projects (dams, highways, etc.). Sometimes lidar data are collected for the design of these projects; however, often the professionals conducting the impact assessments are not allowed access to these data, or they may not have the tools to conduct a thorough analysis. After our 2012 Honduras lidar survey, we searched for other lidar datasets...
and forms of RST data that would help improve our understanding of settlement patterns in Northeastern Honduras. We were fortunate to discover the 2000 USGS lidar collection that was conducted as part of the aid package from the US to Honduras to assist in hurricane recovery. We were able to access the original raw data because the project was funded as an aid package, and because the original time limit had passed from its original use. We have also identified other lidar datasets that were collected for the design of a hydropower dam and road in Northeastern Honduras. These datasets will be paid for with public funds, but we have not been able to access them. Should our professional associations be lobbying our governments for greater access to these infrastructure-oriented datasets? And, how can we engage in conversations to improve the relationship between archaeologists and infrastructure developers?

From an Academic Aristocracy to a Citizen Scientist Democracy

Perhaps the largest impact that RST data may have on the archaeological community is that we will have to become more open, not only within our field, but also with the public. Because some of these RST data are accessible to anyone with the internet, and because of the amount of data produced, the reality is that our community cannot analyze and sift through the data deluge, making it almost prohibitive to protect the archaeological record according to our current ethical codes. One possible path is to engage and recruit the public and to develop an archaeological citizen scientist community, such as Sarah Parcak’s project with GlobalXplorer (https://www.globalxplorer.org/).

Other than the practical and logistical challenges that establishing this community would entail, it may be useful to reconsider...
Principles 4 (public education) and 8 (training). Such an approach may even require the development of ethical principles for these citizen scientists.

It is not only necessary to look outwards—we also need to look inward. Through our experiences with lidar in Honduras and in Mesoamerica, we have pondered the difficult issue of data sharing among our academic community as discussed in Principles 5 and 6 (intellectual property and reporting). While it is easy to share digital data, national regulations and academic interests make this an enormous ethical challenge brought by RST. As with any other tool, the use of RST data can have both positive and negative consequences. One can argue that given its high up-front cost, lidar has facilitated disparity and the rise of a “lidar elite.” Rather than democratizing the practice of our science, only a select few can afford to collect and analyze lidar data. How can we pool resources for the benefit of a broader community and particularly for young researchers?
Conclusion

We understand that we raise issues and questions that archaeologists and their collaborators privately debate. We believe, however, that we need to have a holistic discussion that carefully weighs the opportunities with the challenges of integrating RST data like lidar. We emphasize that archaeologists should think about the issues above (and others) when conducting their research in any context. While lidar affords opportunities such as the ability to generate extensive datasets, to collect data at multiple scales, and to ask different research questions, ethical issues arise. We could modify a few SAA principles, but as a community, we should adjust our attitudes to the current principles that are conducive to record sharing for the greater good. We invite others to contact us to discuss these issues via email or at future conferences.

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INTRODUCTION TO COMPUTER SIMULATION AND ARCHAEOLOGY

Cheyenne Laue

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While archaeology remains a discipline reliant on field-based methods, the articles in this special issue highlight an alternative methodology that is increasingly employed in archaeological research: computer simulation. A growing body of literature on archaeological applications of simulation highlights how simulations may help archaeologists generate possible pasts and match these possibilities against patterns seen in actual data recovered from the material record. However, as several articles in this issue note, this process is not unproblematic and the degree to which computer simulations can or should be used to emulate particular pasts is still a matter of debate. Indeed, many of the authors featured here touch on common themes surrounding the uncertainty inherent in simulated methods as well as the degree to which simulations should be used as an exploration of cause-and-effect processes, as a method to facilitate understanding of past change, and ultimately as tools to build theory, rather than as mechanisms for simply recreating the known past.

In “Skipping Stones Off Improbable Histories,” Kyle Boinskiy discusses the intersection between temporality, probability, and history in the context of simulation. He notes the improbability of any particular history (real or simulated) as well as the degree to which small events often coalesce and “cascade” into the major historical occurrences we hope to model and thereby understand. Computer simulation as the creation of multiple probabilistic pasts, Boinskiy suggests, is much like a “choose your own adventure novel” where improbability rather than likelihood should be embraced.

In “Statistical Inference and Archaeological Simulations,” Enrico R. Crema highlights the similarities and differences between the possible uses of computer simulations: theory building, method development, and hypothesis testing. Focusing on the latter, Crema notes that issues of equifinality and multifinality may both interfere with the process of hypothesis testing based on comparisons of real-world data with simulation results. Crema suggests that methods such as approximate Bayesian computing, which allow comparison of multiple parameter values and multiple models against data of interest, allow archaeologists to choose likely explanations from a set of generated possibilities.

In “Don’t Take My Word for It: Students’ Thoughts on Agent-Based Modeling,” L. S. Premo discusses the process of teaching students how to create simulations, outlining the process of model development from formulating a research question to honing simulation parameters. Premo highlights a critical aspect of simulation design: Good models are inherently simplifications of complex processes, and it is this that provides a vantage point on processes of interest. Based on this, Premo reminds us, the goal of simulation is not to accurately reproduce the real world but rather to create abstractions that facilitate deeper understanding.

In “An Emergent Community?: Agent-Based Modelers in Archaeology,” Benjamin Davies and Iza Romanowska discuss data from a recent survey designed to assess the demographics of the modeling community in archaeology. With the idea that forming a “community of practice” is essential to motivating the continued use and development of simulation methods in archaeological research, the authors detail statistics of computer simulation research in archaeology, including research practices, gender, and education.

Finally, in “Excavating the Artificial: Archaeology, Temporality, and Digital Space,” Cheyenne Laue discusses the implications of new computational research, such as Artificial Life, for the future practice of archaeology. Laue asks the reader to consider that computer simulation is not just a method for examining the past, but that it might also provoke yet-unforeseen disturbances in both archaeological practice and perspectives on temporality.

In conclusion, the pieces in this issue provide important insights into both the promises and perils of using computer simulation in archaeological research. While many archaeologists will continue to choose not to incorporate simulation methods into field-based research programs, the articles here offer a glimpse of the possibilities open to those who choose to do so. An ever-widening community of archaeologists who collaborate with modelers or incorporate simulation into research or curriculum is certainly a worthy goal. More broadly, however, and with the aim of theory building always in mind, it is my hope that these articles provoke thought and discussion on the roles of alternative methodologies in a rich but always uncertain archaeological future.
The perhaps universal human pastime of skipping stones off a lake or stream has gotten a surprising amount of attention in the scientific literature lately, with analyses of the physics of skipping showing up in Nature, Fluid Mechanics, and Physical Review Letters, among other high-profile academic journals. Researchers in France have described the physics of stone skipping using mathematical models of gravity, resistance, and fluid dynamics, and have derived a theoretical maximum number of skips (38) that may be achieved under just the right circumstances. The key is to throw the stone at a 20-degree incidence angle with the surface of the water, while also maintaining a leading edge of the stone 20 degrees above its trailing edge; to throw it at 40 feet per second; and to use a flick of the wrist to give it approximately 14 rotations per second (Bocquet 2003; Clanet et al. 2004). Physicists have performed experiments validating these mathematical models, and have shown that a “perpetual skip” may be achieved if stone velocity can be maintained. NASA engineers have built on skipping theory to describe a type of spaceflight where a ship skips off the surface of earth’s atmosphere, saving fuel but undoubtedly leading to a bumpy ride for passengers. Looking at the research, it seems like we’ve finally got stone skipping nailed down.

But what if, instead of lab settings and Newtonian principles, we only had a sample of skipping attempts at a professional skipping competition from which we were asked to derive skipping theory? We can imagine perhaps having data on the number of skips per throw, personal statistics on each skipper, and the distance each stone traveled in its recorded number of skips. We might then describe the relationship between the number of skips and the distance traveled, or explore the impact of biophysical metrics of the skippers. But could we reconstruct Newtonian mechanics and fluid dynamics from these limited observations alone? Or could we predict the paths of future skips, or who is likely to win the tournament? How should we organize our analysis of the tournament data if we wanted to do both?

The dual challenges of deriving a theory of the mechanics of stone skipping while simultaneously attempting to predict the outcome of a stone-skipping competition—all from scant data on a few skips and skippers—is an apt analog to our task as archaeologists. As recorders of human life in the past, we are interested in exploring the trajectories of history in minute detail; as anthropologists, we seek to understand not only the diversity of the human experience, but also humanity’s common heritage. We strive to know what makes us human. Anthropological observations of repeated historical trajectories cross-culturally and throughout time present compelling evidence that there must be structuring principles of human histories. Still, when observing particular histories, anthropologists are made acutely aware of their contingency, leading to the suggestion that History itself—what has come before—is the most important structuring force of all. To return to skipping stones, we recognize that the environment (waves on the surface of the water, for example) has a profound impact on the trajectory of the stone, and that humans (previous skippers) can have a profound impact on the surface of water, and on the surface of history.

A Probabilistic Theory of History

Imagine a short sequence of coin tosses. For an evenly weighted coin, the probability of realizing a particular sequence of tosses—say, heads, tails, heads (HTH)—is 1/2^3, or 1/8. In fact, this result is the same regardless of the realized sequence; the probability of flipping HHH is the same as for TTT, or THT, or THH, etc. Now, imagine we add another toss to the mix; the probability of any given sequence goes to 1/2^4, or 1/16. As more and more tosses are added—as the coin flip history gets longer—the number of possible outcomes increases and the probability of any particular outcome decreases (and even in a binary system like a coin toss, quite quickly!).

What if it were a weighted coin, where a heads flip occurs with a 90% probability? Our potential three-toss histories would look like this:

- HHH: (9/10) × (9/10) × (9/10) = 0.729
- HHT: (9/10) × (9/10) × (1/10) = 0.081
The probability of a particular history is unsurprisingly related to the number of heads it contains, with all heads being the most likely outcome. But what happens with additional flips of the coin? The probability of the most likely outcome, all heads, decreases to 0.6561 \((9/10)^4\). Just as with the unweighted coin, as the number of flips increases—as our history becomes longer—the probability of any given history decreases. In the limit of time, the probabilities of all unrealized histories converge toward zero and are thus ultimately equally unlikely!

But this has all been theoretical up to this point... we haven't yet started flipping! How do the probabilities of histories behave once a history is underway? Take a coin out of your pocket and give it a flip—I just did. If you get heads, the probabilities of the four histories with tails at the first flip immediately drop to zero, and those of the four remaining histories (with heads as their first flip) jump from 1/8 to 1/4. Take another flip—tails—and the probabilities of the two histories that have heads in the second flip also become zero, leaving the remaining two histories with a probability of 1/2. Finally, a third flip—heads—pushes HTT to zero, and the probability of the realized history is absolute. All this is to say that Time prunes the tree of possible histories, and leaves only one realized (“true”) History in its wake.

So, we have an apparent paradox: Any given history of sufficient length is highly improbable at the start, but among them a History is definitely going to happen with probability one. Time (at least for non-relativistic entities... don't try this near a black hole) both races to catch up with the present, pruning histories as it goes, and extends forward toward infinity. The key to determining a certain History among histories is to choose a time for that History to “end”—for a measurement of reality to be taken; for the state of history to be resolved.

How are the human histories that archaeologists study different from a series of coin tosses? Well, for one, we know that the types of events that happen in reality aren't easily or naturally represented by binary probabilities. Instead, they might better be thought of as discrete (binned) probability distributions over a set of mutually exclusive options. Also, unlike fair coin tosses, real probabilistic events don't occur in isolation—instead, they are the product of past events and are subject to direct and indirect manipulation by historical agents. We might even go so far as to define agency as the capacity to influence the probability distributions of events, and therefore the probabilities of eventful histories.

The tendency for the outcome of one event to affect the probability distributions of subsequent events can have a profound impact on an ultimately realized History. Causal cascades of probabilistic events precipitate into the Big Events of History that are so often the topic of archaeological thought. Can we learn to identify these Big Events, and their antecedent “little” events, in the past? Could we predict them into the future? Are there limits on the extent to which humans can exercise agency and affect the causes and consequences of Events in History?

Simulating Uncertain Histories

I think we can, but doing so requires a framework for knowing that builds on and extends the strengths of anthropological archaeology as it is currently practiced. It requires we go beyond recording thick descriptions of particular idiosyncratic histories, and even beyond the comparative approach where we seek common patterns in multiple realized (and artificially segregated) histories. Instead, we must somehow fully explore the history space to identify convergences in probabilistic pathways. To do so is to construct narratives for how certain Big Events of History came to be, and their effects, without regard as to whether a particular narrative has actually been realized. We must tell possible stories about history; derive probable stories as sets of isomorphic histories; and through empirical research reveal actual histories that may or may not be among our probable or even our possible stories.¹

One way that some of us—likely including many of the authors in this special issue—tell probabilistic stories is by simulating them in a computer. We write code that generates sequences of events, and we model the historic interactions between events as well as agency processes that affect their probabilities. Early computer simulations in archaeology were used to bring analytical models—say, a series of differential equations defining a migration pattern—to their logical conclusion; to “solve” them, so to speak. But computer simulations really shine at representing uncertain, probabilistic histories—the kind of histories that cannot be “solved” in the traditional sense, but must be told and told again to reveal their secrets.
As I said, computer simulations are written in code. Computer languages, like all languages, impose a grammar on the stories we tell—they provide the stories with structure and rigor. Probabilistic simulations don’t allow us to be wishy-washy with how we tell our stories—even if they do permit the construction of uncertain histories. They lay bare the assumptions that we make about how histories evolve—about the models that represent the probability distributions of events and how those distributions change as a result of prior events and agent actions.

To add yet another analogy to a paper overflowing with them, telling probabilistic histories is like reading (or, more aptly, writing) a Choose-Your-Own-Adventure (CYOA) novel. Like such books, which require readers to make (usually binary) decisions, computer simulations must make a series of choices about events to compose a complete story. Reality is like an infinitely long CYOA book. History is a single adventurous reading of such a book. But everyone who has read a CYOA book knows that what makes them so fun is reading them again, making different choices, and seeing where the new adventure takes you. Such is the joy of computer simulation.

Our Improbable History

Much more could be said about the virtues and pleasures of digital storytelling through computer simulation. We are at the dawn of quantum computing, which promises to allow us to simulate probabilistic histories using probabilistic particles instead of the deterministic logic gates of classic circuit boards, and at speeds that are nearly instantaneous. Simulating probabilistic histories of increasing complexity is a natural use case for this next generation of computing.

Probabilistic histories are diverse histories—they are representations of how individuals, families, and communities might have lived in the past, and they invite cultural embellishment to further link them with lived human histories. I’ve imagined a cross-cultural study where we use simulation to describe many such histories of, say, a family, and invite people from diverse backgrounds and cultures to fill in the blanks in the histories that a computer (or dare I say an anthropologist) would never be able to fill. What were these digital family members’ hopes? What were their fears? How did “Household 112” in the Village Ecodynamics Project2 simulation (a project that I’ve worked on for a decade) feel about their daughter marrying that boy from Mesa Verde? Or about their turkey dying in last year’s drought? Or their child?

These digital stories, while undeniably fictions, can nevertheless be “true” in the sense that we humans can imagine ourselves in them—that they are isomorphic, albeit imperfectly so, to the phenomena we wish to study. And they have the power to reveal truths about Reality—about History—as any form of storytelling does. They also teach us important lessons about the metaphysics of Reality: about the structural regularities that emerge in sets of histories, especially ones at the confluence of physical, cultural, and social worlds. They reveal common actions on probabilistic pathways—and even though these common actions may not reflect Reality, they enable us to explore their causes and consequences in a systematic way.

One final thing I’ve also learned from computer simulations is a healthy respect for the improbable: that we ultimately have very little reason to expect that any particular realized history we record archaeologically was a likely one from the outset. The results of interacting probabilistic events—not to mention human agency—can be chaotic. Embracing improbability frees us to consider alternative, counterfactual histories not as merely “wrong,” but as paths that could have been taken but weren’t—and to ask why and wherefore these counterfactual histories were pruned away. It invites us to not only study the trajectory of our particular stone as it skips across the surface of History, but to map the surface in its own right, and, ultimately, to celebrate archaeology as a field of inquiry that invites us to do both.

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1. James McGlade (2014) provides a nuanced and compelling argument for considering computer simulation as narrative construction, as opposed to representations of reality.
The last decade saw an exceptional increase in the number of archaeological simulations covering a range of topics as diverse as settlement dynamics, spread of farming, origins of inequality, and cultural evolution. The wider accessibility of dedicated programming languages (e.g., NetLogo for Agent-Based Modelling), and the flexibility of general-purpose data science languages (e.g., R and Python) are enabling a new generation of scholars and students to dive into the field of archaeological simulations with less effort than ever. Retrospective papers are continuously being published, and it is becoming increasingly common to see research projects including a “modeller” postdoctoral position. Simulations are certainly not new in archaeology (see Lake 2014 for a historical review), and future archaeologists will know whether we are finally approaching the “plateau of productivity” of the notorious hype cycle or if we are still ascending the “peak of inflated expectation” (and about to face a “trough of disillusionment”).

The question of where we are now and where we are headed becomes harder (if not pointless) to answer once we consider the many flavours of archaeological simulations that have been proposed in the last 40 years. Several classifications have been suggested to make sense of this rich variation, using criteria ranging from thematic content of the model to its degree of realism or abstraction. One such classification, originally devised by Mithen and discussed in detail by Lake (2014), focuses on the ultimate objective of the simulation model. Simulations can thus be used to support theory building by providing a heuristic device to explore the implications of one or more behavioural assumptions; be part of method development, by generating artificial datasets to test the efficiency and the limitations of an analytical technique; or be based on a particular historical-geographic context, with outputs that are directly comparable to observed data, and hence be employed for hypothesis testing. Pure theory-building models have a long history across different fields of studies, and although occasionally criticised for their high levels of abstraction (cf. Dronamraju 2011: Ernst Mayr’s critique of “beanbag genetics” in population genetics), they reached substantial maturity in subfields such as cultural evolutionary studies. The success of method development simulations are harder to evaluate, partly because of the comparatively small number of archaeologists engaged in the development of new statistical techniques. Nevertheless, these simulations are successfully being employed to assess the reliability of existing techniques (within the field or “borrowed” from other disciplines), often to ascertain whether they are robust to different forms of archaeological biases such as spatially uneven sampling strategies, time-dependent taphonomic loss, and time-averaging (e.g., Crema et al. 2017; Premo 2014).

The third category in this classification—hypothesis-testing models—is the main focus of this article. As pointed out by Lake (2014), the distinction between hypothesis-testing models and theory building is not always clear-cut, and very often the two objectives coexist informally in the same simulation. This is particularly the case for empirically grounded models designed to emulate a specific historical window and from this to explore more general aspects of human behaviour such as the emergence of hierarchical societies (Kohler et al. 2012). While there have been discussions on the effectiveness of such a “realist-particularist” approach (Costopoulou 2015; Kohler 2015), it is undeniable that a substantive number of archaeological simulations are designed to emulate, whether for hypothesis testing or external validation, some aspects of reality in a predefined window of time and space. Yet, formal comparisons between simulation outputs and the empirical record are not as frequently carried out as one might expect, and in many cases attempts have been limited to visual or qualitative inspections. Moreover, the dominant focus of many works has been model building and description (and to a lesser extent parameter exploration), with far less attention given to the fit (or the lack thereof) between simulation outputs and observed data as well as the broader implications of the whole exercise.
Two, Four, and Six

In 1960, P.C. Wason published the results of a psychological experiment that aimed to explore a particular form of inferential bias. Participants were presented with a numerical sequence—2, 4, and 6—and were asked to identify the underlying “rule” generating the numbers. To aid the process, they were allowed to propose as many “test” sequences of three numbers as they wished and were informed whether the proposed triplet could also be generated by the rule. Most participants proposed sequences designed to confirm their initial clue (e.g., that the rule was “increasing intervals of two”, suggesting for example 10, 12, and 14). When informed that their proposed triplet could also be generated by the algorithm, the participants would stop the testing procedure (or continue further tests with the same hypothesised rule), ultimately concluding that their algorithm was the correct one. The right answer, however, was “increasing numbers,” a simple rule that can yield a wide range of sequences (e.g., 1, 2, and 3; 5, 25, and 125; 10, 98, and 99; etc.), matching outputs from a variety of alternative and more complex rules (e.g., “increasing intervals of two,” “increasing multiple of the first number,” etc.). Because the majority of participants were aiming to reproduce the observed pattern and hence seeking to “prove” their hypothesis, they failed to identify the correct answer. In contrast, an approach designed to disprove an initial clue (e.g., by testing 4, 5, and 6) would have avoided such a mistake.

Wason’s study highlights our natural tendency to seek confirmation (rather than rejection) of our theories and hypotheses and, more importantly, how this can lead to erroneous conclusions when dealing with patterns that could have been generated from more than one possible generative process—i.e., when we are dealing with equifinality. The theoretical implications of this problem and the related issue of multifinality (same process, multiple possible patterns) have been discussed in the literature (see, for example, Premo 2010), and it is known to have even contributed to the abandonment of the whole enterprise by early adopters such as Ian Hodder (Lake 2014).

It would follow that if we are pursuing hypothesis testing, and wish to avoid Wason’s inferential pitfall we should be designing simulation models to disprove our theories rather than seeking their confirmation. This, however, introduces a paradoxical situation where the worst outcome in the external validation of a computer simulation is a perfect fit to data. A complete lack of fit can help dismiss parameter ranges, or question the validity of key assumptions, while a partial fit can generate new ideas on “what is missing,” with the simulation model acting as a comparative template (Kohler et al. 2012). A perfect fit, which arguably would be rarely achieved (especially with highly realistic models), would be less informative—there would be nothing left to explain; alternative explanations are not considered and hence cannot be dismissed a priori and, because of equifinality, we are not able to conclusively state that the proposed model is the “correct” one.

A “Generative” Statistical Inference

It is surprising that the issues of equifinality and multifinality, which are at the core of this problem, have not been discussed in relation to inferential statistics where the comparison of model and data is the disciplinary bread and butter. At its foundation, statistics is based on probability distributions, which capture the expected variation in the observed data given a parameter value of a statistical model (e.g., what are the probabilities of getting 0, 1, 2, 3, and 4 heads given 4 tosses of a coin with a probability of heads equal to 0.5?), and likelihood functions, which capture the variation of the most likely parameter values given the observed data (e.g., what are the odds of getting 3 heads out of 4 tosses, using a coin with a probability of heads equal to 0.1, 0.2 ,0.3, 0.4, and so on). Although within the realm of a single model, the former is a depiction of multifinality (a parameter value generating different outcomes) and the latter of equifinality (multiple parameter values generating the same outcome), and in both cases variations (of the outcome or the parameters) are formally quantified in probabilistic terms. How does inferential statistics then deal with equifinality and multifinality? Either by aiming at the rejection of a particular model (i.e., the frequentist null-hypothesis testing approach) or by comparing multiple models using information criteria. The latter approach in particular can be used to directly test competing hypotheses against each other, providing the possibility of formally comparing alternative explanations for an observed pattern (Rubio-Campillo et al. 2017), potentially drawn from distinct bodies of archaeological theories (Eve and Crema 2014). If the objective of our modelling enterprise involves some comparison with the empirical record, why are we not adopting these, arguably better, inferential tools?

There are at least three sets of reasons. First, pure hypothesis-testing models in archaeological simulations are not common. As mentioned earlier, the great majority of empirically ground, realistic simulations are simultaneously also theory-building devices. Models are constructed on the basis of a given historical-geographic context, but a substantial effort is spent on exploring the parameter space to evaluate the consequences of the embedded assumptions. One reason why we do not observe pure hypothesis-testing models is that ideal
null hypotheses or established alternative explanations that are readily formalised in the literature are rarely available. In other words, model-based archaeology is still in its early stages, whereas theory building is still central and there are no “off-the-shelf” models ready to be tested against data. It is no coincidence that the few notable exceptions where a simulation-based, generative statistical inference has been used are those with a well- and long-established body of formal models already available. For example, Crema and colleagues (2016) have recently reexamined the Neolithic pottery assemblage from Merzbach Valley in Germany, comparing outputs of a simulation model with different modes of social learning (unbiased, conformist, and anti-conformist) derived from cultural evolutionary theory, whilst Por and Nikoli (2016) studied the demographic changes at the Mesolithic site of Lepenski Vir in Serbia, using long-established population growth models.

The small number of parameters and the comparatively high levels of abstraction in these and other examples illustrate the second reason why the adoption of a statistical inference for the analysis of computer simulations is difficult. The great majority of these models cannot be analytically “solved,” so expected outputs of a given parameter value can be obtained only through a simulation run. To obtain the approximate equivalent of a probability distribution, we would thus need to rerun a model with the same parameter settings many times, and, crucially, to obtain something comparable to a likelihood function, we would need to do this for every possible combination of model parameters and record how often, and under which circumstances, the output perfectly matches the observed data. The number of simulations required to achieve such a task becomes almost immediately intractable with the increasing number of parameters. However, an approximate solution based on some measure of distance to the observation (rather than a perfect match) can drastically reduce the computational requirements, making the combination of statistical inference and simulation modelling feasible. One of the most promising approaches in this direction is approximate Bayesian computation (ABC), a computational method that enables probabilistic estimates of parameter values as well as comparisons of different models against the same observed data. Still, such an approach is possible only by using modern computer technology, as the number of required simulations is in the order of magnitude of millions—well above the typical number of runs observed in archaeological simulations.

Third, the choice of what exactly we are trying to “fit” can severely limit model design and even bias the inferential process. Summary statistics that numerically describe complex phenomena (e.g., diversity indices) are very often insufficient, i.e., they entail a loss of information compared to the full dataset and can introduce further levels of equifinality. This is worsened by the fact that observed archaeological data are also profoundly affected by postdepositional events, sampling strategies, and loss of crucial information (e.g., via time-averaging) that are rarely reproduced within simulation models despite potentially shaping a large component of the observed pattern.

A Future for Hypothesis-Testing Models?

Is there a future for pure hypothesis-testing models in archaeology? Increased computational resources and a wider development of formal models within archaeology can certainly benefit the use of approaches similar to ABC. This seems to be the case for fields with a longer tradition and a greater role of formal models such as population genetics. Similarly, in ecology, attempts have been made to formalise the comparison between the output agent-based simulations and empirical data using ABC (van der Vaart et al. 2016), or to devise alternative model selection criteria (Piou et al. 2009). The ABC approach itself is also benefiting from continuous methodological development and refinement by the statistical community, showcasing how the combination of a consolidated inferential paradigm with the flexibility of formal simulation-based modelling is both an attractive and promising cross-disciplinary research agenda. Within archaeology, different bodies of theory will inevitably have different stances towards this approach, and the temptation to exclusively rely on borrowed models from adjacent fields or to limit the inferential exercise to tractable problems, data, and hypotheses will be the greatest limit of its wider application. The lack of a unified body of theory in the social sciences will on the one hand impede the spread of reusable models, but at the same time will offer a unique opportunity to contrast a wider range of alternative explanations directly against data. Whether the latter will be achieved, or even sought, remains an open question.

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1. Although it can be argued that seeking to identify a mismatch between simulation and data shares some similarity to the null-hypothesis testing approach.
DON’T TAKE MY WORD FOR IT
STUDENTS’ THOUGHTS ON AGENT-BASED MODELING

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This special issue provides a welcomed opportunity to share and reflect upon what my students have told me they have gained from learning agent-based modeling. I hope this essay is useful to researchers who are interested in agent-based modeling but remain unclear about what it entails and what they can expect in return for their investment of time and effort.

The Student Becomes the Teacher

I have been teaching agent-based modeling to graduate students since 2005. I have taught my course at universities as well as at a Max Planck Institute. Students have come from a range of disciplines including archaeology, evolutionary biology, ecology, primatology, and paleoanthropology. The course involves many hours of programming in NetLogo (Wilensky 1999). The final project requires that students analyze and then report on an agent-based model they created during the semester.

I often ask students a simple and purposefully open-ended question: What was the most important or surprising thing you learned in this course? I am not too proud to say that I find their answers illuminating. Some students highlight aspects of modeling to which I had not given sufficient thought. Others explain how concepts like emergence and complexity provide the opportunity to reconsider their own research questions from a different perspective. Many of their answers would be of interest to anyone currently considering investing the time and energy required to learn agent-based modeling. But given the space available here, I will briefly comment on just three of the most common and/or most interesting responses. Each anonymized and lightly edited student response is followed by my thoughts on the topic.

It Is All About the Question

For some reason, I was under the impression that I would think of research questions after programming my model, but now I understand just how important the research question is to model development. I could not have programmed my model without a question to guide me.

I have found that many students harbor the notion that they must program a “realistic” model of some real-world process, society, or period first and then worry about research questions second. I warn them that attempting to program an agent-based model without a question is likely to induce panic. After just 30 seconds of staring at the cursor, immobile and blinking in a sea of white like an avalanche beacon lodged in a heavy blanket of snow, it suddenly becomes painfully clear just how many decisions one must make about model design before any code can be written. Clearly, it would be undesirable, not to mention implausible, to include all of the real-world details of the target system. But how, exactly, does one go about deciding which factors to include and which to leave out? In other words, what criteria does one use to discern which aspects of the real-world system are necessary to one’s model?

The answer to those questions highlights an important but underappreciated role for one’s research question. Remember that the primary goal in modeling is not to create a passable substitute for the real thing—a kind of simulacrum—that can be studied as if it were the real thing. Rather the goal is to create a purposeful abstraction of a particularly interesting aspect of the real thing that lends itself to experimentation and thus leads to deeper understanding (see Lake 2015; McGlade 2014; Premo 2007, 2008, 2010 for more on this topic). Because the research question provides the purpose for one’s model, it also guides model construction. That is to say, the research question is the ultimate arbiter of which aspects of the system are necessary to include in one’s model. Students quickly come to appreciate the multiple roles—blueprint, guardrail, filter, midwife—their research question plays throughout the arduous but worthwhile task of building an agent-based model. Invest
heavily up-front in crafting a good question and the model will follow. Disregard the research question at your own peril. Do not say we did not try to warn you.

Verbal Models Leave Much Unsaid (and Undone)

Even “simple” human behaviors depend on a surprisingly large number of assumptions and parameters for which we often have very little empirical data. The task of programming such a behavior from scratch quickly exposed numerous deficiencies in what I had previously thought was a comprehensive and detailed verbal model.

Verbal models are common in the anthropological literature. A verbal model is a written description of the author’s thoughts on how a system works and how it responds to perturbation. As one might suspect, it does not take long for students to realize that an agent-based model is a different kind of beast. Verbal models are not bad or unimportant by comparison, but rather they are underspecified—many, woefully so. The student who provided the response above certainly was not the only one surprised by the amount of work needed to “translate” a well-formulated verbal model into working code. Most students are overwhelmed by the number of important decisions required to bridge the chasm between the airy prose of verbal models and the more exacting demands of executable code.

Happily, there is a payoff for thinking hard about what we do not know. While it is impossible to run sensitivity and robustness analyses on a verbal model, an agent-based model naturally lends itself to experimentation. Well-designed experiments allow one to isolate the parameters that have the greatest effect on model dynamics. Verbal models cannot offer a comparable level of understanding mainly because they cannot be “run” or “executed” under varied conditions. This is not to imply that agent-based models simply offer up a deeper level of understanding on a silver platter. But because agent-based models are fully specified and can be iterated in a computer, one can employ experiments to methodically build a better understanding of the causal relationship between process and pattern.

Lose Your Illusion

This course has given me a “sixth sense”—the sense to figure out for myself how a system works, at least in theory. Because I can design and run my own simulation experiments, I now possess the means to deepen my own understanding of assertions made by others.

Cheyenne Laue invited me to contribute this piece around the time that I was reading Sloman and Fernbach’s (2017) book, *The Knowledge Illusion: Why We Never Think Alone*. Sloman and Fernbach begin with the provocative assertion that although humans collectively know more about how the world and cosmos work today than at any other time in the past, individually we know less about how the world works than ever before. In a similar vein, Dennett (2017:375) discusses “distributed comprehension,” which he defines as the “idea that we as a group might understand something that none of us individually could fully understand.” If the notion that individual comprehension does not (or cannot) keep pace with distributed comprehension sounds preposterous, that may be because most of us are blissfully unaware of our own blind spots—we do not appreciate just how little each of us knows. Consider the following question: How well can you explain how a zipper, battery, light bulb, flush toilet, or internal combustion engine works, let alone something more nuanced like the reasons behind your neighbor’s vote for city council in your last local election? The answer is humbling, is it not? Despite the fact that most of us cannot explain how relatively mundane items like zippers work in sufficient detail, studies show we sure think we can. It is only when we are put on the spot that the surprisingly large gap between our inappropriately high self-regard and embarrassingly spotty knowledge is exposed. Sloman and Fernbach argue that this so-called knowledge illusion is a product of our uncanny ability to convince ourselves that a sizable proportion of the knowledge humans have accumulated and stored in books, engineering plans, web pages, mass-manufactured items, scientific findings, patents, etc.—Dennett’s distributed comprehension—also resides in each of our heads, ready to be pulled out and applied at a moment’s notice.

Perhaps we should not find the knowledge illusion so surprising. After all, there is little impetus to distinguish what “is known” from what “I know” because in most aspects of our lives it is possible to exhibit competence without comprehension (Dennett 2017). Driving to the grocery store does not require that one understand how the engine in the car works, how it was made, or even how to fix it if it breaks down. One does not need to know how a toilet works to operate the flush. That is someone else’s job, and thankfully so, because who has the time to study all of those unnecessary details when the family needs those groceries ASAP? Most people recognize at some level that each of us is merely an “end-user” rather than a repository of humanity’s accumulated knowledge. And yet, for some reason (which I cannot name because, ironically, I do not know what it is), we operate under the vaguely defined presumption that each of us possesses the kind of knowledge that might in fact only be knowable at the level of the group.

The knowledge illusion plays a role in scientific endeavors as well. Given the many demands on one’s time and the limits...
of one’s expertise, one is often tempted to accept someone else’s answer to a particularly vexing problem, especially if the answer seemingly allows one to move on to bigger and better questions. But because the act of recounting someone else’s solution does not require that one actually understand how it solves the problem—again, competence does not necessarily require comprehension—there is some danger in relying too heavily on borrowing. Although this very human form of social learning underwrites rapid advances and important scientific breakthroughs, when left unchecked it can also lead to intellectual cul-de-sacs populated by shambling “zombie” explanations that live on far beyond their usefulness.

Here is where the student’s response comes into play. The student uses the term “sixth sense” to describe the ability to use a model to learn for oneself how a process works, to not only identify which conditions cause which effects but also to explain why. I think she hit the nail on the head. Agent-based modeling allows one to toggle from social learning to individual learning in an effort to mitigate—if not eliminate—one’s knowledge illusion. The ability to leaven social learning with individual learning might help explain why researchers proficient in modeling are often able to go beyond merely stating their results (This is what I observed in my data) to explaining them (This is what I learned from my experiment). I think it also helps explain why modelers are much more likely than non-modelers to be cognizant of the holes in their own understanding. An acute awareness of what remains unknown drives fruitful research. Because modelers are more familiar with the boundaries of their knowledge they are also better positioned to articulate the steps required to address known deficiencies. If I may, the student’s response could be rephrased, in the context of Sloman and Fernbach’s argument, as “agent-based modeling provides an invaluable tool for confronting one’s knowledge illusion.”

Summary

So, what have we learned from the students? First, one’s research question is of utmost importance to modeling. The research question serves as both the purpose of the model and the blueprint for its construction. Second, below the placid surface of even the best verbal model the water is teeming with questions of all shapes and sizes, and these pesky details must be dealt with in order to construct an agent-based model. And third, exploratory agent-based models provide the opportunity to combat—if not vanquish—one’s knowledge illusion about a process or system of interest. Conducting experiments with simple abstract models provides an avenue to a deeper understanding of the relationship between cause and effect in complex systems, including human societies. Clearly, this is not an easy task. But consider the alternative.

Acknowledgements

I thank Cheyenne Laue and Dr. Anna Prentiss for the kind invitation to contribute to this special issue on computer simulation and modeling. I thank Prof. Bill Lipe for recommending Dennett’s new book and loaning me his copy; sorry that I held onto it for so long. And to the students of my agent-based modeling courses over the last 13 years: Thank you for sharing your thoughts, and I hope you enjoyed the class as much as I enjoyed teaching (and learning from) you.

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Agent-based modeling (ABM) is a powerful tool for understanding the dynamics of past social systems. A form of computer simulation, ABM is oriented around computational “agents”: autonomous entities that have the capacity to interact with each other and/or their environment based on a set of rules. The rules that govern agent behaviors are predetermined; however, agents can be given the capacity to change which rules they follow based on information they receive over time, producing complex and sometimes unexpected outcomes. This kind of ‘bottom-up’ modeling approach has been well received in human-centered sciences like health science (Reiner et al. 2013), economics (Hamill and Gilbert 2015), and, increasingly, in anthropology and archaeology.

ABM is an old-but-new method in archaeology. Computer simulations in the discipline have a heritage that spans half a century (some of the earliest conceptualizations of which would be described as “agent-based” today, e.g., Doran 1970; Thomas 1972), while ABM specifically has been around since the early 1990s (Lake 2014). At the same time, there is a sense that the approach is still in its early development, with attention recently turning to mainstreaming and systematizing practices (see Cegielski and Rogers 2016; Costopoulos and Lake 2010; Lake 2015).

The development and promotion of methodology depends in part on the shared ideas of a community of practice. Communities of practice are identified by interest in a common domain and collective recognition of the value of improving and sharing their knowledge of that domain (Wenger 1998). These emerge from, and are maintained by, common experiences and learning traditions among members.

To systematically target issues related to practice, it is helpful to understand what a community of ABM practitioners in archaeology looks like. Formal organizations are not easy to find: at present, the SAA does not have a dedicated ABM or simulation interest group, although there is often overlap with the domains of other communities of practice in existence today, such as the SAA Digital Data Interest Group (SAA-DDIG) and the Open Science Interest Group (SAA-OSIG). The Computer Applications and Quantitative Methods in Archaeology (CAA) Complex Systems Simulation Interest Group, started in 2014, is probably the most visible manifestation of a community of ABM practitioners in archaeology. This group organizes workshops and themed symposia at conferences, and maintains an email listserv that is used to seek information from community members and provide updates on ongoing projects and opportunities. These kinds of activities generally distinguish a community of practice from other kinds of organizations.

The number of ABM practitioners in archaeology is small but growing, increasing the opportunities for developing and sustaining discussions about methodology. Therefore, in the near term, how one gets into archaeological ABM, who ends up doing so, and what their current practices are will have influence over what become standard practices in the future.

Examining the ABM in Archaeology Community

To better describe this community, we conducted a survey of self-identified ABM practitioners in December 2017. The survey was publicly visible, distributed via the CAA Complex Systems Simulation mailing list, the simulatingcomplexity1 blog, and via social media (e.g., Twitter, Facebook). A total of 65 respondents completed the survey, which we consider a sizeable proportion of active ABM practitioners in archaeology. The data, analysis script, and survey have been made openly accessible for use by other researchers².

Gender

Survey respondents included more than three times as many male researchers (77%) as female researchers (23%; Figure
1). This stands in contrast to the general population of archaeologists where the percentage of female participation varies between 40% (US; Zeder 1997), 46% (UK; Aitchison and Rocks-Macqueen 2013), and 50% (EU; Lazar et al. 2014). By comparison, the percentage of survey respondents is far closer to the levels of women’s representation in science, technology, engineering, and mathematics (STEM) positions, which fall around 25% for North America and Europe (Beede et al. 2011; UNESCO Institute for Statistics 2016).

The gender gap in STEM representation is a well-documented and persistent phenomenon in many Western countries. Several potential explanations have been put forward, including fewer faculty role models for women, less flexibility with family commitments (Beede et al. 2011), discrimination, and inequalities in funding and remuneration (Shen 2013). It is probable that similar root causes apply to the gender imbalance among ABM practitioners in archaeology.

Education

Most survey respondents have trained or are currently training as archaeologists and anthropologists, with 28% coming to study archaeological problems from other disciplines (Figure 2). This suggests that the draw of taking up ABM methods comes predominantly from an interest in their applications to archaeology rather than the other way around. Most of the non-archaeologists in our sample come from STEM subjects.

Among the coding platforms used by participants, NetLogo was by far the most popular (78.5%), followed by general purpose programming languages: R (28%), Python (20%), and Java (20%). NetLogo is an easy platform to learn on, but lacks some of the flexibility of general purpose languages. When we asked respondents how they had learned coding, a total of 55.4% described themselves as self-taught, either...
with no support or support from peers only, while an additional 15.4% were self-taught with support from an academic supervisor. In contrast, only 13.8% described their training as a formal component of their degree program.

Employment
The largest share of respondents to the survey held permanent or tenure-track academic appointments (30.8%); this was followed closely by PhD students (29.2%) and post-doctoral researchers (13.8%). Despite the proportion of permanent positions held, a clear majority of those surveyed are early career researchers (<4 years since terminal degree; 64.6%). Again, there is a large disparity between female and male researchers\(^1\), where 46.7% of women using ABM are PhD students (versus 24% of men) but only 6.7% (that is, one respondent) hold a permanent academic post (versus 38% of men)\(^2\). This disproportion in the representation of women in permanent academic positions is larger than the one reported for archaeology in general 20 years ago (Zeder 1997).

In addition to the large number of early career researchers, a small but statistically significant grouping of respondents completed their terminal degree around 30 years ago. This corresponds to the beginning of a ‘renaissance’ period identified by Lake (2014) during the early 1990s, associated with the advent of ABM and the start of spatially explicit simulation.

Research Practices
Despite most participants having archaeology/anthropology degrees, most drew limited theoretical inspiration from long-established archaeological theory. The most prominent theoretical frameworks identified by respondents were complexity science (34.4%) and evolutionary theory (32.8%)\(^3\). This agrees with an assessment by Lake (2014), who identi-
fied complexity science as driving the 1990s renaissance, and a surge of evolutionary theory models in current practice. Theoretical paradigms that historically align with simulation and ABM (e.g., systems theory) were less prevalent among current modelers, showing a strong discontinuity between early simulations and the current applications.

On the other hand, the scales of the models used by respondents suggest that ABM modelers take notice of the current theoretical landscape of the discipline. ABMs can simulate processes at range temporal, spatial, and organizational scales, from pedestrian movement recorded in seconds and minutes to evolutionary-driven species-level changes modeled over generations. Survey respondents were asked to describe the scales of agents and time steps in their most recent model. While a range of different perspectives were reported, models in which agents are defined as individuals or households (70%) and time moves at relatively short intervals (<1 year; 88%) were dominant. This squares with a general trend of seeking the “individual” in the archaeological record (e.g., Gamble and Porr 2005).

Respondents were also asked about validation methods used in their simulations, and the results indicate a mix of theory-driven and data-driven simulations being developed for archaeological case studies (Figure 3). The former usually do not require a validation because they function as subjunctive models (of a type “if x and y then z”) or can be validated against a “stylized fact”: a broad generalization of an empirical pattern (e.g., “first hominins reached East Asia before Western Europe”). On the other hand, data-driven simulations try to replicate patterns found in archaeological and other data (e.g., precise arrival dates for archaeological sites related to the first hominin dispersal). Trade-offs between model realism and generality, and their implications for archaeological applications, have been discussed elsewhere (e.g., Lake 2015), but a general preference is not indicated in the survey. Fewer respondents sought to replicate results from earlier models, which in many disciplines is a starting point for simulation research, but this may be related to comparatively lower numbers of simulations in archaeology when compared to other disciplines.

With respect to collaboration, over 80% of respondents confirmed that they had built models with input from other researchers. Life scientists and STEM researchers were the most common collaboration partners (Figure 4), suggesting that interdisciplinary expertise and technical skills are fre-
Practices from the wider domain of agent-based modeling have been adopted in mixed degrees within archaeology. For example, the “Overview, Design Concepts, and Details” (ODD) protocol (Grimm et al. 2010) provides a universal method for documenting models to improve replication, and is becoming a regular accompaniment for published models in forums like the OpenABM consortium. About half of survey respondents reported using the ODD protocol in their research. It is not known whether this means that other methods are being used to document models, or whether documentation procedures are not being used. About two-thirds of respondents who did not use ODD indicated awareness of it, suggesting that knowledge of documentation practices is widespread.

**Going Forward**

Cegielski and Rogers (2016:284) recently argued that ABM has the potential to make “revolutionary advances within the overall archaeological research paradigm” but that mainstream acceptance depends on greater capacity building. We agree, and would add that the mechanisms for building that capacity can be enhanced through active engagement and reflection within the existing community of practitioners. This could be accomplished in a number of ways. At SAA Annual Meetings, for example, simulation and ABM-themed symposia were once few and far between. Since 2013, they have occurred every other year. This is an improvement, and important for sustaining discussions about practice, but could be further enhanced by making them an annual feature. More in-depth considerations and critiques of practice within these forums may help draw important questions about the direction of ABM in archaeology into the open. This survey raises some fundamental ones like “Why are there so few women ABM practitioners?” or “Is the ODD protocol the best documentation solution for archaeological ABMs?”

A major hurdle to capacity building is the lack of formal training within the discipline, which may also be contributing to the gender gap. To move away from a reliance on self-teaching, shared educational resource bases could be developed specifically for training archaeologists in formal modeling methods. As more ABM specialists are promoted into permanent academic posts, we hope the number of archaeology students receiving formal training in ABM methods will increase. It is encouraging to see courses teach-
ing ABM and simulation methods appearing at institutions like Washington State University, Arizona State University, and Leiden University. More training will undoubtedly increase interaction within the community as instructors and students seek resources. We believe that this will also have a positive influence on the discipline as more archaeologists are exposed to thinking about archaeological questions in terms of formal models, and have more opportunities to make interdisciplinary connections through computational applications.

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1. simulatingcomplexity.wordpress.com/2017/12/04/help-us-characterise-the-abm-community/
2. Supplementary data, code, and graphics can be found at github.com/izaromanowska/Emerging_community
3. See supplementary information SL_10 and SL_11
4. See supplementary information SL_2
5. See supplementary information SL_15
6. See supplementary information SL_25
In archaeology, data move. Methodological necessities of the modern age slide artifacts to hard drives and ancient material culture to servers in the cloud. Backups ensure that every object stays excavated, stays ordered, and stays remembered: a most well-organized past. Such digital archiving is perhaps predictable in a disciplinary culture for which reconstruction and meaning are embedded in the processes of dating and chronology. Here, in the messy world of trenched dirt and gritty prehistory, the tidy order and timelessness of digital storage holds innate appeal. We might suggest, along with anthropologist Nurit Bird-David, that such an obsession with chronology represents “a past-present distinction grounded in process, along with the sense of the past it selects” (2004:407) and that archaeological acts of social reproduction result in the construction of temporal order, the very act of excavation birthing a sense of time in which the past is irreparably breached from whatever comes next.

We might also notice that the process of archaeological reconstruction is much the counterpart to ethnographic study, and indeed, some archaeologists note the extent to which “archaeology is [often] about doing the ethnography of the past” (Holdaway and Wandsnider 2008:1). Archaeologists, then, are in many ways storytellers, reanimating the past through narratives of forgotten lives, drawing them forward into the present. If we look more closely at ethnographic methods, however, we notice that the construction of temporal asymmetries is inherent in that process as well, and that time is also used to structure the “others” of ethnographic investigation (Fabian 1990, 2014). By freezing the lives of those under study in the narrative of ethnographic texts, Johannes Fabian famously claimed, they are denied a status that is coeval with that of the researcher and are thus staked out as permanently different, alter, and obscure.

However, the implications of constructing this coeval difference persist outside of defining cultural groups, or for archaeologists, their cultural objects. Anthropologist John Comaroff (2010), forced to question the future of ethnography in a world where traditional subjects have slipped from romanticized relics to normative contemporaries, expressed lingering apprehensions that his discipline might simply die “with the demise of the last primitive” (2010:524). Ironically, Comaroff said, in an era of “partial truths,” “provisional readings,” and “an empiricism of ever greater descriptive complexity” (2010:525), his focus emphasized the future of ethnographic work and, as such, he declined to dwell “on the archaeology of anthropological crisis” (2010: 525). This statement makes apparent the extent to which the process of archaeology remains a handy synonym for digging up the past, and indicates that time itself may be archaeology’s last primitive. The question, however, remains: Are past/present distinctions essential to archaeological practice, or do they simply linger at the periphery, relics themselves of our own methodological inheritance?

In answer to this question, and far more radically than the suggestions made above, we might draw our attention to the methods emerging at the nexus of archaeology and computation, and propose that they portend an eminent destabilization of seemingly firm temporal boundaries, and represent archaeological movements that go far beyond digital storage. In particular, computer simulations represent dynamic, exploratory, and descriptive attempts to revive prehistory using data recovered from surveys and excavations. These efforts at simulating the past rely on a set of initial conditions reconstructed from archaeological and environmental databases, behavioral rules derived from cultural, residential, or kinship patterns, and a form of temporality that is highly malleable, allowing researchers to adjust the tempo and direction of change as it unfolds (Bonabeau 2002; Epstein 2006). By tracing the complex exchanges between people, environments, and material culture, such simulations reconstitute the physical characteristics of artifacts, restore momentum to long-inert human communities, and allow...
the forgotten to be resurrected in *silica* (Lake 2014). The results are reconstructions that are often uncanny in their ability to mirror reality, as in the case of the Artificial Anasazi Project (Diamond 2002), which was implemented using archaeological, geological, and climatological data in order to reproduce social and ecological conditions in Arizona’s Longhouse Valley between AD 800 and 1350 (Axtell et al. 2002; Dean et al. 2000; Janssen 2009). Small flaws in demographic realism notwithstanding, the project was accurately able to model the rise and fall of Anasazi society, including the eventual abandonment of the valley.

Programmers involved with the project note the complexity of modeling cultural change on such a large scale; they do not, however, indicate the epistemological problems that arise in creating such accurate reproductions of the past, or the ontological crisis potentially invoked by manipulating time to such an extent that events from a thousand years ago unfold again on computer screens in the present.

Indeed, all is not as it seems. Too neat, perhaps, our archaeological simulations, with their sanitization of time as something that can be manipulated or ignored entirely, and altogether too complete their comfortable perspective on events that occurred in a largely inaccessible past. This is, after all, a time of “partial truth, provisional readings” and empirical complexity (Comaroff 2010:523).

Untidily, unexpectedly, and entirely unapprehended, something else waits on the horizon. That something began as a subtle recognition among a few computer programmers that life, rather than an intrinsic property of certain types of matter, is more likely related to systems of organization and the complex patterns that give rise to a larger, less definable process (Langton 1989). Thus what we understand as life—the animating, invisible force unique to organic matter—may be, on a more fundamental level, simply a set of rules governing the biology of information storage and processing, metabolism, and self-reproduction (Farmer and Belin 1990). This logic implies that “defining life [is] more of a value judgement than an empirical observation” (Helmreich 1998:61). These ideas gave rise to a new generation of computer simulation that now seeks to create artificial organisms in the dynamic topography of virtual space (Aguilar et al. 2014). Organisms largely unrecognizable in terms of form or substance, but possessing epistemologies that conform to the same set of assumptions as those enabling the existence and evolution of organic beings; organisms that are “artificial in the sense that they [are] originally designed by humans… [yet] alive under any reasonable definition of the word” (Farmer and Belin 1990:816). In Baudrillard’s terms, these are not “referential beings with substance, they are models of a real without origin or reality: a hyperreal” (1994:1).

Such artificial life (ALife) simulations are now common in a number of disciplines as scientists use them to model everything from ecosystems to biological immunity (Langton 1997; Mitchell and Forrest 1994; ). Computational anthropologist Stefan Helmreich (1998) notes that ALife simulations increasingly hold a place somewhere between theory and experimental methods, while programmers involved in ALife projects suggest that computers are more appropriately definable as places, rather than tools (Ray 1993). ALife applications specific to archaeology are still rare; however, research using artificial life principles has now successfully propagated the vegetation of ancient plant communities, allowing unique investigation of the interactions between past peoples and the landscapes that they inhabited (Chñg and Stone 2006). While such research demonstrates, as noted above, the increasingly shaky distinction between theory and method, it also suggests the generative potential of archaeology in a world scarred by the Anthropocene and a growing view on the part of some scientists that computers are places to “begin again” in a reality where the “word for world is no longer forest but computer” (Helmreich 1998:65).

What then are the implications of these (possibly) “really real” artificial worlds for archaeology, particularly an archaeology attempting to extricate itself from a grounding in the past? In early work on computational social research, programmer Joshua Epstein and his colleague Robert Axtell (see Epstein and Axtell 1996; Epstein 2006) devised strategies to implement and study the development of what they called proto-cultures in communities of artificial agents. They and a number of researchers since (e.g., Berry et al. 2002) have repeatedly demonstrated the emergent properties of the complex cultural systems in artificial societies that are “grown” from simple computational rules. Following from this computational approach to social science research, anthropologist Nicholas Gessler (1994, 1995) proposed that artificial societies are even now evolving nascent histories that pose important questions for both ethnographers and archaeologists alike. His work urges the development of research strategies that integrate with programming primitives and that take into account the radically different structuring of artificial and real and social worlds. In considering these digital beings, their strange physiologies and interactions, and the complex temporal entanglements that leave them existing somehow outside of time, the question for archaeologists becomes what doing archaeology in such places might entail: What does it mean to excavate the artificial? After all, artificial landscapes are simply mirages to dig-
While the larger meanings and implications of these transitions are still obscure, much depends on the willingness of archaeologists to recognize the intrinsic value of alternative forms of life, and the importance of conducting research in unconventional places. Thus, and most importantly, future archaeologies of artificial worlds must recognize the propensity for ALife organisms to become a new form of “digital native” to whom ancient tropes of similarity and difference based on temporality and spatiality may be applied. If allowed, such tropes may once again permeate our work, and we must therefore, as Gavin Lucas suggests, continue to question our relationship to our archaeological others (Lucas 1997:14). In this particular case, the imbrications may be far more obscure than those existing between archaeologists and peoples of the prehistoric past; after all, in posing the question “Who are they to us?” Lucas implies that our alter others, although sometimes strangers, are always after all human. In the case of ALife organisms, the status of our subjects has yet to be determined: Are the digital inhabitants of these proto-worlds and proto-cultures anything at all like our ancestors? For that matter, are they anything like our subjects, as traditionally defined? Or, are they somehow shadows of ourselves evolved under different circumstances, a different physics giving rise to different forms of life?

In any of these events, Lucas’ advice regarding forgotten pasts still resonates: Perhaps the nature of their stories is unknowable to us because these stories are not ours to remember (1997:14). Or, perhaps as Baudrillard (1994) suggests, the only recourse is to turn to fable—to narrate the stories of lives that don’t really exist and to create ethnographies, as such, of partial truth. In light of these uncertainties and partialities, I will close very simply by quoting again from Baudrillard: “The simulacrum is never what hides the truth—it is truth that hides the fact that there is none. The simulacrum is true” (1994:1). Invoking once more the words of Gavin Lucas, we are also reminded that “there is more at stake here than simply the truth” (1997:8).

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Louis Carl Kuttruff, Jr., died on July 23, 2017, in his native Baton Rouge, Louisiana, at age 73. Childhood pictures show him, at age one, digging in a sand pile; at age three, excavating a mud hole; and, at age four, playing at the controls of a backhoe. At age 15, with his Boy Scout troop, he journeyed to Luzon, site of the tenth World Jamboree. By the end of the trip Carl had visited Corregidor, Angkor Wat, the Taj Mahal, the Egyptian pyramids, many Classic Greek and Roman ruins, and the lava fields of Iceland. Also, in this year he first walked the fields of Louisiana’s Poverty Point. In 1964 he enrolled in William Haag’s introductory anthropology course and never looked back.

Carl received his BA degree from Louisiana State University (LSU) in 1965. Encouraged by Haag, he attended his first Southeastern Archaeological Conference. There he met James Ford and Richard Orlandini. The latter convinced Carl to enroll in graduate school at Southern Illinois University (SIU), and introduced him to Jenna Tedrick, his wife-to-be. As part of SIU’s contract program, he worked at threatened prehistoric sites. At SIU, Walter Taylor pointed Carl toward Mesoamerica, and soon he was working with Kent Flannery and Richard Blanton at Monte Alban. He earned his MA and PhD from SIU in 1970 and 1974, respectively.

Later, he joined the Tennessee Division of Archaeology (1973–1989). There he expanded into historical archaeology, primarily at early fortifications. His work at Fort Loudoun became his magnum opus, *Fort Loudoun in Tennessee, 1756–1760*. He and Jenna also periodically participated in investigations at World War II Pacific installations on Wake Island, Kwajalein Atoll, and Corregidor.

In 1989, after Jenna accepted a position at LSU, Carl returned to Baton Rouge. Thereafter, he dug at Poverty Point, Marksville Mounds, and Kleinpeter Mounds. As a consummate reader of dirt, Carl recognized patterns where others saw trivia. And he knew the cultural significance of the artifacts. But he did not eschew academia. He taught at several institutions, and helped with several University of Southwestern Louisiana field schools. His work includes nearly three dozen journal articles/book chapters, and four dozen papers at professional meetings, plus four dozen CRM reports. He was preparing several reports when he died. He also served on Tennessee’s review board for National Register nominations, as program chair for meetings of the Louisiana Archaeological Society and the Southeastern Archaeological Conference, and was a past president of the Louisiana Archaeological Society.

But the Carl many of us knew best was equally at home drinking a beer or watching a ribeye sizzle on a campfire. He was always soft-spoken, but we knew to listen closely to his quiet wisdom.

Carl also had an inner compass with regard to integrity and deportment. He was an old-style gentleman, at a time when the breed was becoming extinct. Though modest, he had little tolerance for witless colleagues. He fumed at the notion that archaeological deposits should be preserved instead of excavated—“preserving ignorance,” he called it—and he hated bureaucratic red tape. He once lamented, “I’d like to live long enough to see what a Poverty Point house looked like, but I probably never will because they’re too timid to allow anybody to look for one.”

Working with Carl was an egalitarian experience. He was the ultimate teacher, patiently showing rather than telling. One of his maxims was that “if you start off with a bad pit you’ll wind up with a bad pit.” His movement in a test pit and skill with a Marshalltown were sheer poetry. His commitment to archaeology was as pure as his friendship.

Carl is survived by wife Jenna; father Louis Carl, Sr.; sisters, Gail, Alma, and Katty; and brothers, Kirby and Claude.

Dorothy Hosler was elected 2017 Fellow of the American Association for the Advancement of Science (AAAS). She is Professor of Archaeology and Ancient Technology in MIT’s Department of Materials Science. Her research integrates the tools of materials engineering and geoscience to investigate materials processing technologies in the ancient Americas. Hosler established a chronology for the metallurgy’s development in west Mexico, then demonstrated that this copper-based metallurgy, including Cu-As and Cu-Sn alloys, was introduced into western Mexico from the Andean region. Aspects of that technology were incorporated, then reconfigured in accordance with local social precepts. She has been recognized by AAAS for her “distinguished contributions to integration of materials science and social theory for understanding ancient technologies and what they indicate about social aspects of ancient societies.”

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