

FIELDS | TERRAINS

Anthropology Among Us

Winter | Hiver 2021 | Vol 11

McGill Undergraduate Journal of Anthropology

Revue d'anthropologie des étudiantes et étudiants au baccalauréat de McGill

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On behalf of the 2021 editorial team, we are pleased to present to you the 11th Volume of McGill’s Undergraduate Journal of Anthropology, *Fields|Terrains: Anthropology Among Us*. This year’s edition was created within a unique year of virtual university in response to the COVID-19 pandemic. Correspondingly, the title *Anthropology Among Us* provides a nod to the zeitgeist of the past year in referencing the multiplayer online game that exploded in popularity as one of the many creative ways to stay connected to one another. Yet, our title *Anthropology Among Us* additionally underscores the reflexivity demonstrated amongst our authors, a connecting thread throughout the journal despite the variety in our selections, from interviews to academic research papers to poems, to short stories.

We thank you for all your continued support and hope you enjoy this year’s edition of *Fields|Terrains*.

Audrey Gray
Editor-in-Chief

Au nom de l’équipe éditoriale 2021, nous sommes heureux de vous présenter le onzième volume du journal académique d’anthropologie de premier cycle de l’Université McGill, *Fields|Terrains: Anthropology Among Us*. L’édition 2021 a été créée dans le contexte unique d’une année universitaire entièrement en ligne en raison de la pandémie de la COVID-19. Pour faire écho à ce zeitgeist très particulier, le titre *Anthropology Among Us* est une référence cocasse au jeu en ligne multijoueur très populaire qui nous a permis de rester connecté•e•s les un•e•s aux autres durant cette dernière année. De plus, notre titre *Anthropology Among Us* souligne aussi la réflexivité démontrée par nos autrices et nos auteurs, une caractéristique qui unit la grande variété de textes qui composent notre publication, qui passent des travaux de recherches académiques, aux poèmes et aux nouvelles.

Nous vous remercions de votre précieux soutien et espérons que vous appréciez l’édition 2021 de *Fields|Terrains*.

Audrey Gray, Éditrice en Chef
Traduction par Sophie Ji

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Excerpts from “Intersecting Archaeology, Community and Heritage: An Interview with Lynn Meskell”

Emily Draicchio and Alannagh Maciw

Dr. Lynn Meskell attained her PhD in Archaeology from the University of Cambridge (1997) and at the time of this interview was the Shirley and Leonard Ely Professor of Humanities and Sciences in Stanford University’s Department of Anthropology. She is now the Penn Integrates Knowledge Professor, the Richard D. Green Professor of Anthropology, and the Professor in the Graduate Program in Historic Preservation at the University of Pennsylvania. Throughout her career, Meskell’s outstanding work has not gone unnoticed. She has been awarded fellowships by the National Science Foundation, the School of American Research, the Andrew W. Mellon Foundation, Oxford University, and Cambridge University for her archaeological research and cultural heritage work in several areas including Egypt, Turkey, and South Africa. Currently, Meskell is conducting fieldwork in India, where she examines the complexities of archaeological heritage and its impact on local communities.

Meskell was invited to present her lecture *Engineering Nationalism; the Cold War and UNESCO’s Victory in Nubia* on October 17th, 2019 at the Montreal Musée des Beaux Arts based on research from her latest book *A Future in Ruins: UNESCO, World Heritage, and the Dream of Peace* (2018).¹ Her lecture exemplified the impact of global engagement with historical sites through her eth-

nography of UNESCO, where she encapsulated the agency’s origin, mission, and analysed the significance of the Nubian Project, a pivotal moment in UNESCO’s history. Following her Global Antiquities lecture, Meskell hosted an equally evocative seminar called *Imperialism, Internationalism and Archaeology: Un/Making of the Middle East* at McGill University. This seminar deromanticized archaeology of the early 20th century and its relevant characters. She considered how the political entanglements archaeologists encountered regarding heritage, conflict, territorial claims and abuses of stately power have been omnipresent in the history of the discipline. This powerful message was reinforced by the discussion it produced, as attendees of the seminar shared their cross-disciplinary experiences and current research facing similar problems. One scholar described her own work with refugees in the Middle East that was being hampered by heritage and rebuilding projects, reminding the room that these problems have not yet been resolved. Meskell’s work urges the question “who is this helping?” that leaves a strong message for archaeologists and heritage workers not to get so caught up in protecting the past that it is privileged over the living.

Meskell has also dedicated a portion of her career to promoting junior scholars, particularly Indigenous and

¹ The lecture was organized by Global Antiquities, a pillar of the Yan P. Lin Center for the Study of Freedom and Global Order in the Ancient and Modern Worlds. Their mission is to apply a global perspective to historical and social impact paradigms.

female scholars. She does so by working with them and publishing their work in the *Journal of Social Archaeology*, of which she is the founder. The *Journal of Social Archaeology* promotes interdisciplinary research that draws upon feminist, queer, and postcolonial theories. The following interview took place on October 18th, 2019 at McGill University and highlights the importance of junior scholars being able to openly discuss archaeological issues with successful academics in their discipline and creates a platform to deliberate the future directions of archaeology and heritage work.

AM: When did you first become interested in Archaeology?

LM: My first memory of being interested or fascinated with archaeology was before I actually went to school. I got the *Time Life* book series for Christmas and there was one volume on Egypt; it had Tutankhamun's death mask on the front, and one on China and one on ancient Greece. I think I got hooked from that sort of encyclopedia of ancient cultures around the world. Particularly the Egypt one I was immediately drawn to. I think I was about four. [...] I don't know why I got these books, but yes, that sparked my interest, and no one could really understand why I was so interested in them.

AM: In your career, you have done some remarkable archaeology at a vast number of sites. However, you have obviously felt the need to push that envelope. Through your books and edited volumes, like *Archaeology Under Fire and Embedding Ethics*, you have problematized the origin of the discipline, as mentioned in your seminar today, and considered

the impact of archaeology on the living. Why did you feel you needed to go beyond more traditional archaeological careers?

LM: I guess I didn't think it was untraditional at the time, perhaps because I was trained in Australia, and Aboriginal people came to speak to us in class, and were very angry, understandably, at the behavior of archaeologists and the practice of archaeology. My first archaeology training was enmeshed in living issues and land rights, rights of self-determination and privacy of materials, so that was a rude awakening that archaeology was not going to be like the *Time Life* book series and that politics were inherent in all of our practices. Certainly, when I was a student working in Egypt, there were terror attacks at archaeological sites where I was working, which goes right through to when I was working in Turkey and there was an attempted military coup that I happened to be caught in. So, to me, the two things were from the outset interconnected and inseparable, and the first book I published, *Archaeology Under Fire*, which is an edited volume, was about archaeological responsibility in the Middle East and the eastern Mediterranean because I had worked as an undergrad in Cyprus and in Greece. And you could see the politics there after the Balkan wars playing out certainly, but also to study Egypt in an ancient context and then consider the modern context as well. So, they went hand in hand and that book was also about archaeologist's responsibility in contributing to how materials are used in those tensions. The book differs from the outcry we see now about the destruction of sites; it was more focused on our role which I think all my

work has tried to focus on. That means our actions and our responsibilities rather than looking externally for something else going on. So really putting “us” in the middle of the sort of political histories that unfolded in those regions.

AM: Yes, it is interesting to think about it as so interconnected for you from the get-go, because you’ll often hear a scholar’s biography and it very much follows a somewhat linear trajectory, and they’ll have different interests at different stages in their lives, like focussing on feminism for some time or a new research trend, but I find it interesting that your interests have followed you throughout your career.

LM: Well, it’s interesting that you mention feminism, because people often say to me how I’ve done so many different things, but actually, they’re all facets of the same thing, they are all concerned with inequality and social injustice and the people who get written out of history. And so that’s why I did my dissertation work on a community in Egypt, and I was looking at age, class, sex and those sorts of issues, which were not really so different from the inequalities I was working on in South Africa around race and apartheid, or even in India around religious difference and caste and class issues. Someone said I am very good at doing the archaeology of the underdog, and I’m ok with that label as well, but that’s probably because of my own upbringing and just being trained in Australia that you can’t ignore those issues anymore and it’s sort of coming of age of our discipline, hopefully.

AM: What do you think is most important about generating a conversation about archaeological ethics? How do you think this

discussion will change the ways future archaeologists are trained to think about the past?

LM: My training of students, certainly at Stanford and now at Penn, is that ethics have to infuse every class, whether we study the history of archaeological thought or classes on World Heritage and conflict, or the intersection with socio-cultural anthropology. I teach all those kinds of classes and they all have an ethical base, which is not just that we look at ethical guidelines or codes, because they never do enough work; they are a starting point, but they don’t encompass all the issues that we face. We talk a lot about fieldwork, about how to do ethical fieldwork, how to work well with the people who are hosting you, your collaborators, and co-publishing, so that it is a sort of win-win situation for ourselves, for our students, but also for the people who have to put us with us in their countries, or communities within these countries that we think are familiar with, but we’re not. It has to be much more a practice than a theoretical nod to ethical topics or doing your IRB (Institutional Review Board). It has to be much more part of the everyday archaeological encounter, and that doesn’t mean that it’s just about people who work on present issues, but we also expect students working on pre-history to be considerate of this as well. So, you can’t package ethics off, like “I did that component, now I can carry on as usual.”

ED: Returning to the feminist lens discussed earlier, based on your experiences since you began your academic career as a woman archaeologist, how has the traditionally white-male dominated field of archaeology changed? Has it become more inclusive?

LM: I don't think it has transformed enough really. When I think of the number of students that we get, many of which are graduate students that are both international students and women, and yet you still see the bulk of the jobs going to young men. So, I do think there's still a lot to be dealt with, watching the experiences of my young female students going into jobs and the things that they've encountered, so it's not just again on a theoretical basis, but in terms of equity and discrimination. I've had long conversations with people here too [McGill University] about their career trajectories and not simply discrimination, but also harassment and these issues have not gone away. In fact, there are just more clever legal ways of institutions covering up for that. I think that, unfortunately, it's more common than we like to think. Even though we do things like harassment training, it's still alive and well and it can come in many different guises and it's still very difficult for female students. At Stanford, I see it all the time, women having to deal with that. So, it's unfortunate because we think that we're farther along, but actually we're not. In my career, I also encountered a generation of women archaeologists, some of whom were not very welcoming and open to the sorts of approaches I was pursuing, because there was also a backlash of an older generation who felt that they had struggled to the top and why shouldn't this new generation have the same experiences. It is still a very male-dominated field, even though that's not the generation of students we're producing, it's not reflective of that.

ED: Do you think the *Journal of Social Archaeology*, which you founded, has helped the field of archaeology become more inclusive? Has it fostered a space for

female archaeologists and archeologists from diverse backgrounds to publish their research?

LM: That's a good question, I often say that JSA is the best thing I've done in my career, because it's about supporting the work of others. It is a very enabling thing for other people, particularly junior scholars, students, people writing from all around the world with different approaches, Indigenous scholars and Indigenous issues. So, in a way we can be inclusive, and we can try new sorts of things. We don't always have to impose a Eurocentric notion of what archaeology or archaeological theory is. Also, we are very encompassing in terms of the disciplines and people crossing over disciplines. I think it is quite innovative in that way and lots of people do write and say they had a good experience. Many say the review process was very fast and quite nurturing, so I hope that that's the case. I'm sure not everybody feels that way, but I do think it has been good for a lot of junior scholars.

ED: How do you think that your career as an archaeologist has equipped you with the tools to offer a different perspective on UNESCO and World Heritage?

LM: That's a great question. There have been a lot of accounts written about UNESCO, some as books but mainly as articles, many of which are coming out of a critical heritage studies perspective, which I had great difficulty with because [...]the way it [UNESCO] was caricatured worried me, that we hadn't really fully understood the organization; and partly that was because, as an archaeologist, I'd worked at World Heritage sites. So, I was also interested in the history and the archives. I had on the

ground experience, but was also interested in the documents, so I wanted to look at long term statistical patterns as archaeologists do. No ethnographer wants to do statistics on twenty years of archival documents from meetings, looking at information like who spoke when and who supported what country and I did all of that with my colleagues. I actually worked with cultural economists, because I think archaeologists are trained to work across disciplines a lot, we collaborate a lot. So, I worked with economists, with lawyers and also political scientists. If I wasn't an archaeologist, I wouldn't have understood the issues on the ground, I wouldn't have visited so many sites or worked at those sites, and then brought to bear those types of methods that we are trained to do; quantitative methods as well as qualitative.

AM: Do you see your research going in a particular direction now that you are less focused on archaeological fieldwork, and more on heritage policy and infrastructure?

LM: I have been studying agencies like UNESCO that use heritage work to manage issues of water and infrastructure, particularly pollution and open defecation. These are huge issues that affect millions of people and the government has chosen programs like Swachh Bharat, which is about Gandhi and cleanliness and has now shifted their focus towards water, but it uses archaeological sites to clean up India [...]. It is very politicized as well and heritage is this vehicle. Because it's so in the spotlight, like in no other country I've seen, it's used and deployed so much and so visually, it can be great at mobilizing things and organizing corporate social responsibility, but it can also leverage a lot of violence between communities. Conflicts

about who was there first, what is a monument and underneath that monument there may be a mosque or a temple, so there can also be a lot of heritage violence. The work is probably on those two ends, the well-being, clean/pollution modernisation, and neoliberal front, and the other is looking back at this sort of mythical past, which are the two sides of the same issue. One very forward looking, modern, competitive on the market, and the other looks back to the mythical, ancestral origins stories that then enable all sorts of violence and claims to sites.

AM: Is there anything more you would like to comment on about the lecture, seminar or your current research?

LM: I would just say what I said to the incoming students; to do doctoral research, we are very privileged to do this kind of work. It is both a luxury and a responsibility. I would just say, do something that matters, something that matters to people in the places you work. My usual saying is that we are not out there curing cancer, so let's try and at least, when we choose our topics and places in the world we will work at, that it matters not only in the past, but that it matters to people in the present.

ED: Thank you Lynn for your thoughtful answers and advice to students thinking about pursuing a career in archaeology. We look forward to reading more on your current fieldwork in India regarding heritage sites and the needs of living communities.

Concluding Remarks

Heritage work and the discipline of archaeology both exist in the spirit of discovering and preserving the past. As exemplified in

this interview, Professor Meskell's work has shown that even if efforts are made in good faith, it is always important to consider who is benefitting. Her research is especially meaningful for young scholars who are entering the field of archaeology with the ambition of doing so ethically. Specifically, Meskell proposes open communication between researchers, local communities, and global heritage organizations., which would promote the consideration of diverse voices in the management of heritage sites. Furthermore, as underscored above in the discussion of her career trajectory, Meskell does not limit archaeology to a singular discipline. Instead, she actively encourages scholars to participate in interdisciplinary research for it offers productive scholarship,potential for outreach programs, and fosters more opportunities for feminist and Indigenous scholars,. Therefore, Meskell emphasizes the positive directions archaeology can take to be more inclusive and continues to inspire junior scholars to embed ethics in their research by considering those who will find meaning in their research. Overall, as this interview highlights, the field of archaeology has both changed, remained the same, and has significant room to improve.

Acknowledgments

This interview was made possible by the Yan P. Lin Center for the Study of Freedom and Global Order in the Ancient and Modern Worlds and McGill University. We would like to thank the directors Hans Beck and Griet Vankeerberghen as well as Professors Peter G. Johansen and Darian Totten for organizing this event. We are especially thankful to Dr. Johansen who introduced us to Professor Meskell last year in India. Finally, thank you Lynn

Meskell for your generous answers and enthusiasm.

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The Priestess-Matriarch?

An Investigation of Gendered Behaviour in the Mature Harappan Period

Sophie Thompson

In the archaeological study of ancient societies with complex material culture but few textual sources—such as the Indus Valley Civilization (IVC)—it is difficult to investigate gender and gendered experiences. There is little to suggest that women in the Mature Harappan Period were valued solely for their ability to bear children, or that their sexualities and reproductive resources were particularly policed or controlled. In fact, family life seemed to be matrilocal. However, there is firm evidence that males received preferential treatment; daughters seemed to experience more food stress than sons, and women seemed to eat lower-quality food than men throughout their lives.

How can this duality be reconciled? I argue that while IVC society was overall male-dominated, women had agency as spiritual mediators, progenitors of familial identity and contributors toward crafts and commerce. I have based my thesis on an analysis of the various arguments surrounding the use of terracotta figurines found at Harappa and Mohenjo-Daro, as well as housing spatial distributions and, crucially, paleopathology. Investigation into the IVC has been fraught with colonialist tropes and misogyny since John Marshall's initial 1931 report. It is therefore important to reconsider the evidence while remaining cognizant of our contemporary assumptions about sex and gender and avoiding the projection of these assumptions onto the past. Attempting to recon-

struct the abstract beliefs of an ancient society without contemporary texts involves considerable interpretation, resulting in conclusions with some degree of uncertainty. However, differences in behaviours between sexes can indicate gendered performance based on gender roles, allowing valuable interpretations to be drawn.

Understanding Terracotta Figurines

I will base this study primarily on an analysis of terracotta figurines recovered from IVC urban centers, which have been the subject of great debate over function and interpretation, especially regarding gendered behaviours. In his first analysis of the figurines found at Mohenjo-Daro, John Marshall concluded that the figurines, a “great majority” of them near-nude women, represented a pre-Aryan Mother Goddess cult (Marshall 1931, 49). He based his interpretation on the recovery of similar figures in Syria, Mesopotamia, Iran, and other ancient societies, where they were taken to represent the ‘Great Mother’ or ‘Nature Goddess’, as well as their nudity, which Marshall took to signify fertility and motherhood. (Ratnagar 2016, 114).

Marshall’s interpretations have faced much criticism, such as from Shereen Ratnagar. Firstly, only a very small percentage of the figurine corpus comprises females holding infants (Ratnagar 2016, 115). Additionally, the rural Indian

mother-goddess worship Marshall took as the continuation of Indus religion is aniconic; the iconography itself may not actually visually represent its intended supernatural recipient (Ratnagar 2016, 114). This means we cannot take images interpreted as offerings literally. Ratnagar emphasizes the difference between sacral icons and votive objects, arguing that the clay figurines do not share the supernatural iconography of female figures on seals and tablets (Ratnagar 2016, 129). This suggests that the figurines acted as representations of living people. It is, however, generally agreed that the figurines served some ritual purpose. Ratnagar argues that while the figurines represent ordinary people, they served as ritual objects in a women's cult (2016, 129). She cites the facts that definitively female figurines occur almost exclusively in the large cities of Harappa and Mohenjo-Daro and are often found broken in city middens following deliberate disposal, perhaps after they had served their ritual purpose (Ratnagar 2016, 130). Additionally, many are clumsily made, as though by non-specialists (Ratnagar 2016, 130). Furthermore, while many male figurines are completely nude, most females are not. Ratnagar postulates that the female makers of the figurines were attempting to protect their own modesty, even if near-nudity may have been required for ritual purposes (2016, 129).

As Sharri Clark writes, Marshall's original interpretations were based on faulty identification of the sexes of the figurines; he used fractional figurines as though they were unbroken and likely underrepresented the number of male figurines (Clark 2003, 307). Clark was unable to definitively identify the sex of 38.7% of the figurines in her assemblage:

while 16.1% were probably or certainly male, and 45.2% were probably or certainly female (Clark 2003, 312). Clark further states that the female figurines do not have exaggerated sexual organs. A number of scholars agree that the waists, breasts, and hips are in proportion with one another, and male figurines are never ithyphallic (2003, 308). This supports Ratnagar's theory that the figurines are not meant to invoke the idea of fertility or specifically represent a fertility deity. Despite the varied interpretations, figurines cannot be ignored as sources even if they do not explicitly represent concepts of Indus sex and gender, but because, as Clark states, they "implicitly embody" these ideas (2003, 308).

While I find Ratnagar's idea that female makers of the figurines were attempting to protect their modesty tenuous, I am convinced that the figurines do not represent supernatural beings, but ordinary people. This is supported by the fact that the figurines do not display supernatural body parts as beings depicted on seals do, and their bodies are not exaggerated into proportions that emphasize generative organs. Additionally, the use of clay suggests a household or unspecialized origin; in comparison to materials such as bronze, clay would have been more readily accessible to non-specialists. Marshall's initial Mother Goddess theory was based on fundamentally flawed methods of interpretation, including sexing fragmentary figurines and using dubious analogy; furthermore, I do not believe it was epistemologically sound to make extensive claims on the religion of an ancient culture in the first report after its rediscovery, as he did. His theory is also steeped in colonialist tropes of a stagnant India. Marshall

summarizes Indus religion as “so characteristically Indian as hardly to be distinguishable from still living Hinduism or at least from the aspect of it which is bound up with animism and the cults of Siva and the Mother Goddess” (Marshall 1931, vii). As Ratnagar has asserted, his interpretations were heavily skewed by the idea that India was incapable of undergoing development or change on its own.

I also believe that at least some of the figurines had a ritual purpose, especially those that seem to represent householders. It should be noted that not all are believed to represent ordinary people; Ratnagar proposes that the small, unadorned, kneeling figures, which are invariably male, may have been used to count prisoners (2016, 125). It is unlikely that they were toys as both representations and actual toys have been recovered in courtyards (Kenoyer 1998, 133). Terracotta seems a poor material for dolls, especially when more durable materials such as wood were available. In contrast to toys found in courtyards, figurines were most often found in middens, suggesting they were no longer useful. Clark highlights the black and white bone pigments found on the figurines; she suggests that the use of bone rather than more readily available mineral pigments was deliberate, to confer a power or ‘life force’ upon the figure for ritual use (Clark 2009, 252). If this were the case, it is possible that the figurines were discarded once their ‘life force’ was used up.

Additionally, I agree that terracotta figurines had a household origin, and were likely created by the people who used them. Female Indus figurines were not idealized as, for example, Bronze Age Egyptian female depictions were. It is difficult to make a claim as to what an ‘ideal’ Indus woman may have looked like. What can be

seen, however, is that female terracotta figurines are much more varied in body shape than Ancient Egyptian depictions of women. The female place in Ancient Egyptian society is fairly well-understood; they had respected status and a high level of personal agency (Goelet 1993, 25). Despite this, they are depicted as invariably youthful and slender, and often wearing diaphanous dresses (Goelet 1993, 26). By contrast, males in Indus terracotta are more likely to be totally nude. Bronze Indus figurines of women—and, for that matter, men—differ greatly in shape from terracotta ones. Bronze figurines have slender, elongated bodies and carefully modelled forms compared to terracotta ones, suggesting that their production process was more highly specialized. I postulate that the stylistic differences represented differences in intended audience and use. For instance, more time-consuming and complicated statues may have been for public viewing. Their slender forms may have been idealized for aesthetic purposes, whereas those of the ‘homemade’ terracotta figures were not. This suggests a level of female sexualization on the part of bronze sculptors for commercial works. Furthermore, the lack of clear temples, monumental architecture, or large, shared worship spaces in Indus urban centers indicates that spiritual life was likely centered in the home. If small-scale worship, spiritual communication, and magical rituals centered around female figurines were performed in the home, I find it plausible that the performance of ritual and spiritual management of the household was a female role.

Woman the Priestess

The ‘archetypal’ Indus terracotta figurine represents women wearing

jewelry and what are either headdresses or elaborate hairstyles. I argue that the elaborate headdresses or hairstyles themselves had a specific ritual role and were not part of everyday dress. Firstly, paleopathology from Harappa reveals that both men and women had spinal arthritis in somewhat equal numbers, likely from carrying heavy objects on their heads (Wright 2010, 264). Wearing such elaborate headdresses or hairstyles would make this near-impossible; only women exempt from physical labour could have worn such elaborate headwear in daily life. I am disinclined to believe that these figurines represent elite women, as the headwear is so ubiquitous among female figurines, and there is little evidence of elite burial in the IVC. Furthermore, there are no such headdresses or hairstyles on bronze figurines representing women. This link is fragile, as the stylistic and iconographic differences between bronze and terracotta figurines could be interpreted as a difference of ethnicity or class. However, considering all other factors, this attribute should not be ignored. Finally, this theory would offer a solution as to trends among ambiguously gendered figurines; these form a small fraction of the figurines recovered from Harappa and almost always have male physical attributes, but are dressed in clothing associated with women, including the headdress or hairstyle (Clark 2003, 319). While many explanations have been offered, such as cross-dressing traditions or the representation of eunuchs, I speculate that some of these figurines may represent men who have taken on a usually female ritual role within a household, perhaps after the death of the matriarch. Even if men did not wear such headdresses or hairstyles in real life, the representations in clay may have symbolically included these potentially ritual

components.

Evidence of bodily ornamentation may also support a ritual female role. As Mark Kenoyer notes, jewelry and ornamentation in pre-industrial or ‘traditional’ societies often has a distinct ritual purpose, necessitating the use of specific prescribed materials and forms (1991:81). These ornaments are utilitarian as well as essential to societal function, as they “protect, identify, and preserve an individual’s place in society” (Kenoyer 1991, 82). There appears to be a greater need for women to ornament themselves than there is for men, as evidenced by jewelry found in graves. Female burials at Harappa’s two known cemeteries, Cemetery H and R37, show that women are buried with more jewelry than men (Wright 2010, 266). This is illustrated by the following contingency table from Kenoyer (1991, 92) of primary context burials from Harappan excavations between 1986-88. There are in total ten definitively male burials and thirteen definitively female burials. Men have ornaments in three of the ten cases, while women have ornaments in nine of the thirteen cases: a much higher proportion. Terracotta figurines of women usually wear one if not more necklaces, while males wear fewer ornaments, if any (Clark 2003, 315). While amulets that would not have been on display have been found in both male and female graves, truncated cone-shaped pendants have been found only in association with female graves (Kenoyer 1991, 94). Beads of the same shape but in raw materials from carnelian to terracotta have been recovered (Kenoyer 1998, 162). Kenoyer notes that all beads would have looked very similar from a distance, communicating the same concepts about their wearers (1991, 96). It is possible that the shape was important for ritual purposes

Harrapa	Male		Female		Uncertain		Infant	Totals
Burials	Full	Partial	Full	Partial	Full	Partial	Full	
With Ornaments	2	1	7	2	0	2	0	14
Without Ornaments	3	4	4	0	0	5	1	17
Totals	5	5	11	2	0	7	1	31

Table 1: Ornamentation in Harappan Burials, reproduced from Kenoyer (1991, 92).

and that they therefore needed to be widely available. However, they may also simply have been copies of more luxurious jewelry.

Sexuality

It seems unlikely that female value was linked chiefly to their sexuality or their fertility. As I have discussed so far, spiritual status would have been a significant element in an Indus woman's identity. Only approximately 4% of figurines show females holding infants (Clark 2003, 317). Less than 3% are 'globular' or round women, which are possible references to pregnancy (Clark 2003, 317). If childbearing and rearing was the chief purpose of a Harappan woman or the source of her social status, references to this role would occur most likely occur more frequently. Additionally, it is uncertain if 'globular' representations even refer to pregnancy rather than obesity, maturity, or prosperity.

The figurines' clothing permits interpretations concerning female sexual and

reproductive agency. Most female figurines wear only belts or small skirts (Clark 2003, 310). Attitudes toward nudity within a culture can give insight into that culture's concepts of chastity; while the climate of a region may have some bearing on a society's dress and the relationship between nudity and sexuality, it is not always the most important factor. This is demonstrated in ancient Mesopotamia, an IVC contemporary. Bronze Age Mesopotamian sexuality was not bound by concepts of virginity as a morally desirable choice (Clark 2003, 310). Nudity had varied meanings in Mesopotamian depictions, from defeat to heroism, while sex was related to civilization (Bahrani 1993, 13). The lack of idealization of the female body in Indus terracotta figurines further differentiates nudity and reproductive sexuality; here, women are not portrayed as sex objects or progenitors of a family line. Furthermore, their genitals are rarely exposed, while their breasts virtually invariably are. This would seem to indicate that women had personal control over their sexual receptivity, and there

was little shame attached to partial nudity. While I cannot claim that terracotta figurines represent the actual Indus woman's everyday dress, I believe their near-nudity, in conjunction with their representation of everyday householders and lack of emphasis on fertility or idealization, can be used to infer a relative lack of policing of female sexuality, and a level of female agency within such a social paradigm.

Home and Commercial Life

From figurine representations, paleopathology, and the spatial patterning of private houses, it seems that while women were not powerless or entirely devalued, they did not receive some of the benefits afforded to men. Enamel hypoplasia found on teeth in Mature Harappan cemeteries H and R37 shows periods of dietary stress and disruptions of growth during childhood. Lines appear more frequently on women's teeth than on men's, indicating that sons may have received preferential treatment (Coningham and Young 2015, 207). Additionally, females have poor dental health in comparison to males; higher numbers of cavities indicate that women ate more carbohydrate-rich foods than men, who probably consumed more animal-based protein (Wright 2010, 264). The preferential treatment of males and the exclusion of females from certain commensal activities caused embodied physical suffering beginning in childhood, potentially indicating that men were valued over women in Mature Harappan society.

The apparent matrilocal practices at Harappa complicate this interpretation. The group at Cemetery R37 represented a restricted fraction of Harappa's population, displaying strong genetic affinity among the females (Wright 2010, 263).

Males had weak genetic affinity, showing a trend of inward male gene flow from outside Harappa (Wright 2010, 267); this provides strong evidence for the practice of matrilocality. Matrilocality implies matrilineality, as housing and identities tied to land are more likely to stay with the spouse that does not move. If property and identity were passed down a female line, women would have strong social value of their own as matriarchs and inheritors. I doubt, however, that this represents a system of simple female primogeniture, because in such a situation daughters would be less likely to be neglected than sons. Matrilineality does not necessitate female authority in a household: for example, Nayar people of Kerala, India have practiced a complex matrilineal tradition wherein property is passed down along female lines, but the *karnavan*, or eldest brother of a matrilineal family group, wields ultimate authority (Panikkar 1918, 262). However, the Nayar family model is not a perfect analogue for the Indus family. For instance, Nayar men live with their sisters, not their wives, while Harappan men not only seem to be buried with their wives' families but sometimes come from outside the urban center entirely (Panipakkar 1918, 260). Additionally, houses generally seem too small to serve as Nayar-style familial compounds, except for the few largest. However, it is possible that extended families lived in nearby houses. Anna Sarcina claims that the nuclear family was the main player in the Indus economy, and the small residential models would seem to support this interpretation (1979, 445). While the Nayar tradition is not a perfect analogy, it shows that matrilineality does not equate to female supremacy in a household. It seems likely that Indus family or clan identity passed from mother to child, as evidenced

by the burials within female-line genetic groupings.

Women seem to have been involved in specialized commercial practices, on local scales and possibly further. With regard to exclusively female labour, little can be definitively known. However, clay figurines depict women grinding a material that could be grain (Possehl 2002:183). As Clark notes, the substances in these figurines could also be minerals for faience work, or clay for pottery-making (2003, 318). Whatever the material, figurines and incidences of spinal arthritis indicate that women performed physical labour (Wright 2010, 264).

Residential structures provide strong evidence for a female role in production. In some cases, businesses and workshops may have run from the ground floors of residential buildings. Sarcina's 'blue' and 'brown' models especially seem suited to these purposes; the 'brown' model has a courtyard that covers almost half of the floor plan, with only a few covered rooms on the south side (Sarcina 1979, 439). The 'blue' model appears to be a workshop space. The rooms are too similar to support residential needs, though the thick walls indicate upper levels which may have constituted living quarters (Sarcina 1979, 440). Indeed, a potter's workshop at Harappa seemed to have been used across many generations as a family-operated production site (Wright 2010, 187).

Wright hypothesizes that in such family operations, everyone participated in production (2010, 188). Women at pre-Harappan Mehrgarh were likely textile workers (Wright 2010, 59); if this had continued into the Mature Harappan Period, women participated in the production of textiles for trade with Mesopotamia (Wright 2010, 226). Furthermore, as Sarcina's research reveals, there are no residential areas in Mohenjo-Daro where no commerce or business took place (1979, 445). Women would therefore have been in contact with such activities whenever they left their houses. There seems to be little effort to segregate commercial and possibly 'masculine' areas from non-commercial ones. While the female ritual role seems centered in the home, there is little to support a strict male-commercial female-domestic dichotomy. The IVC was a trading powerhouse spanning a vast area with a high population. It is highly improbable that women were not active participants in craft production and trade, especially with businesses established in such close proximity.

Conclusion

Ultimately, it is likely that IVC women had important roles as ritual mediators; religion was performed in the home rather than at shared places of worship. It is also likely that they regularly participated in craft production and trade, which would have been economically important in a society with a strong 'international' commercial presence. It is important to investigate gender and gendered behaviours archaeologically; as Roberta Gilchrist stresses, doing so can produce more empirically accurate data (1999, 27). Considering sex and gender in the past while acknowledging our modern, biased standpoints is a crucial part of elucidating ancient experiences, motivations, and politics. In this paper I have used terms which indicate a gender binary because it is clear that Mature Harappan society treated the biological sexes differently and so likely had at least two genders. However, we cannot know whether their genders were treated in

opposition to each other, nor whether they fell on a spectrum. It is also difficult to pinpoint what physical work each sex was performing, and there was much I was unable to ascertain or evaluate within the scope of this paper. It seems to me that much more paleopathological work could be done, including analysis of arm bones to determine differences in physical labour, and isotope analysis to investigate differences in diet as well as immigration patterns which may give further insight into matrilocal practices. Overall, this work must be done with the caveat that understanding the abstract thinking of a culture that has left no understandable texts invariably involves speculation; nevertheless, it is important to contribute toward the discussion surrounding ancient gender and sexuality by including diverse voices and acknowledging our own biases.

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Étégraphie

Sophie Ji



Assise sur un banc de parc
derrière un cahier de notes
elle tient un café glacé
qu'elle finit par déposer.

Elle sort un crayon, observe,
note, dessine, observe,
barre, observe, sourit.
Elle observe, sort un stylo,
observe, dessine, note,
sourit, observe, barre.

(14h30. Parc bondé.)

à la recherche de l'histoire
de ce hamac détaché
de cette poubelle renversée

(Un enfant grimpe sur un arbre
—ça y est, il est en haut—
Un adulte court le chercher
« Heille! Là, tu restes assis. Assis. »)

Puis elle analyse
les conversations
qui parfument l'odeur
des barbecues bien entamés

(« -Je suis libre! À ta droite!
-Non! T'es trop loin du but! »
Le ballon plane. Hors-jeu.)

l'expérience de la chaleur;
le flirt constant
entre la crème solaire
et les coups de soleil

(Un cycliste à vélo. Trois personnes
en file au comptoir de location.)

Et si dans son cahier
elle analysait
les murmures du soleil
pour pouvoir les rejouer
dans les pages de l'hiver

(« Maman, j'veux dormir! »
Moi aussi.)

Elle note que son stylo
ne dessine pas ce qu'elle observe
il lui faudrait un autre crayon
et pourquoi pas un autre cahier

puis elle a fini
son café glacé

C'est l'ethnographie
de l'été en après-midi.

The Chilling Truth Behind Arctic Ethnohistories

Confronting the Myth of the Peaceful Eskimo

Susannah Clinker

Archaeology, as a discipline, plays a crucial role in the maintenance of cultural heritage and in the dissemination of the cultural past of peoples across the world. To do this, archaeologists must carefully decipher the archaeological record and often make use of ethnography as a tool to strengthen and develop their archaeological explanations (Jordan and Cumming 2014, 1-2). In some areas, like those occupied by hunter-gatherer groups in the Arctic, archaeology “may merely be ethnography with a shovel” (Wobst 1978, 303). That is, without ethnographies and ethnohistories, deciphering the archaeological record as the remnants of human behaviours would be nearly impossible. However, as archaeology has long been practiced through a colonial perspective and relied on the accounts of non-Indigenous peoples, the ethnographic frameworks that are so heavily relied on are often deeply flawed and perpetuate colonial perspectives. For instance, Arctic hunter-gatherers have traditionally been described and represented as inherently peaceful and without the political sophistication for formal warfare. This view stemmed from ethnohistorical descriptions of Native hunter-gatherers living in the Arctic that continue to in-

fluence modern archaeological research and popular belief. However, this notion of the peaceful Eskimo¹ is far from the historical reality of Arctic Native hunter-gatherers (Fienup-Riordan 1990), and such ethnohistorical descriptions work to strip Native hunter-gathering groups of their particular historical and cultural identities. These misconceptions must be confronted in order to replace a false image of Arctic hunter-gatherers, not with an image of inherent violence, but with a more realistic image that is corroborated within archaeological ethnohistorical evidence (Fienup-Riordan 1990, 146). By doing so, we enable ourselves to not only further understand the tradition and culture of hunter-gatherers indigenous to the Arctic, we may also more readily understand current socio-political actions taken by them today.

The establishment of the peaceful Eskimo trope can be associated with some of the original accounts of Arctic hunter-gatherers made by early Euro-Americans. These accounts often establish a superiority of the white identity (Fienup-Riordan 1990, 162) and aim to distance ‘them’ from ‘us.’ This is not uncommon in early accounts of Native/non-Native interactions, where Native

¹ While the term ‘Eskimo’ is used by Fienup-Riordan (1990), this term will not be used unless directly referring to the trope of Arctic hunter-gatherers being inherently peaceful. This is out of respect for Indigenous communities who feel as though the term ‘Eskimo’ is derogatory and lacks the acknowledgement for different Indigenous communities who are linguistically, ethnically and culturally distinct.

peoples are described as: “primitive, closer to nature, prehistoric, inefficient, and backward” (Fienup-Riordan 1990, 242). As Euro-Americans began to settle into North America, Arctic hunter-gatherers were described as having a child-like innocence and lacking political sophistication compared to the mature, refined, and adult Euro-Americans (Fienup-Riordan 1990, 242-3). There are many reasons why non-Natives may have perceived Arctic hunter-gatherers this way but “[Indigenous groups] were often differently regarded depending on how they treated the colonizers” (Fienup-Riordan 1990, 162). Since Euro-Americans did not seek to strip Native Americans of their land in the Arctic or to enslave their peoples, like in many other areas of the New World, there would have been little reason to engage in hostility towards non-Natives (Fienup-Riordan 1990, 147). However, there were still many instances of tense encounters as Arctic hunter-gatherers demonstrated a “general friendliness and willingness to trade when it suited them, mixed with the capacity to murder their guests and take what they wanted by force if they thought they could get away with it” (Fienup-Riordan 1990, 147). Nevertheless, the desire to trade with and learn from Indigenous peoples in the unforgiving climate of the Arctic led to relatively cooperative and stable interactions. However, tense interactions and violence were not limited to Native and non-Native encounters because, “left to their own devices, [Arctic Indigenous societies] were no more peaceful than anyone else” (Chacon and Mendoza 2007, 28). In fact, the presence of Euro-Americans and associated trade in Arctic regions, “brought an end to the violent inter-regional struggles that had

characterized the region before the arrival of Euro-Americans” (Fienup-Riordan 1990, 147).

Warfare and conflict between and within Arctic Indigenous groups, “were regularly characterized interregional [exchanges]” (Fienup-Riordan 1990, 153) both before and well into the period after European settlements. In the early 1800s, there were twelve Yup’ik nations (Burch 1988, 229) accounting for a population of roughly fifteen thousand people on the coast of western Alaska (Fienup-Riordan 1990, 153). Each of these nations “viewed [themselves] as socially and territorially distinct and [were] willing to wage war to remain so” (Fienup-Riordan 1990, 153). In addition, Native historians have also listed revenge as being one of the primary reasons for war (Chacon and Mendoza 2007, 19). This is not altogether surprising as the origin of warfare in western Alaska is described by a single oral history described by Fienup-Riordan (1990, 153). The story begins with an incident between two boys, where one injures the eye of the other. In a fit of rage, the injured boy’s father blinded the offender completely instead of poking just one of his eyes out as had been agreed upon by the two boys’ fathers. The violence escalated to the involvement of the entire region (Fienup-Riordan 1990, 153). The oral histories of contemporary Arctic Indigenous peoples not only highlight the general occurrence of intragroup and intergroup violence but contradict the stereotype of the peaceful Eskimo in itself. The inaccuracy of the stereotype is further supported by language. For example, the word for visitors “from another village for a feast are referred to as ‘attackers’” (Fienup-Riordan 1990, 153). The archaeological record

also directly contradicts the notion that Arctic Native peoples were inherently peaceful and incapable of formal warfare. While skeletal evidence in the Arctic is scattered, there have been findings consistent with the oral histories of violence between Indigenous groups in the Arctic like the traditional Yup'ik bow-and-arrow warfare. Most notably, the remains of at least thirty-five individuals, all evidencing use of lethal violence and torture, were discovered near the MacKenzie Delta in the Northern Territories dating to the fourteenth century. This corroborates the oral history of Inuit's in that region who describe an attack while men were away whaling. With the men away, those left in the camp (primarily elders, women and children) were mutilated and exterminated (Chacon and Mendoza 2007, 219). This kind of archaeological evidence illustrates the intergroup violence which was present in the Arctic, as well as how distorted the trope of the peaceful Eskimo truly is.

The peaceful Eskimo trope also suggests that Arctic hunter-gatherers were small non-complex societies that lacked the sophistication to not only conduct formal warfare but also engage in intragroup conflict. This notion was likely derived from the perceptions of Indigenous societal structures as understood by early Euro-Americans. Whereas the capitalism of Euro-Americans societies inevitably opposed equality, hunter-gatherer groups were able to redistribute and trade to maintain a level of material equality (Fienup-Riordan 1990, 165). This does not mean that hunter-gatherers were without socio-political organization. On the contrary, hunter-gatherers in the Arctic specifically, had "definite social hierarchy according to age and degree of relation" (Fienup-Riordan 1990, 165). Without such hierarchies,

group decision-making would not have been possible as a mechanism is needed "to overcome scalar-communications stress problems that [do not] involve what we would normally recognize as hierarchical organization" (Johnson 1982, 396). On the Northwest Coast of North America, hunter-gatherer groups were ranked by the status of individuals and kin groups and "grew progressively more formalized and rigid" (Ames 1985, 162) In other words, despite these hierarchies being different from the typical Western social organization, they were essential to process information and maintain stability within the group (Ames 1985, 159), a fact that many early ethnohistories underestimate.

While there are many useful methods employed by hunter-gatherer societies for stability, it is impossible to avoid all intragroup conflict entirely. The fissioning of groups was used to restore a certain level of intragroup stability following conflict by separating into smaller like-minded subgroups and according to oral histories, was the cause for some of the many hunter-gathering groups in the Arctic. As discussed by Fienup-Riordan (1990), it is said that conflict within a group arose after a Yukon man became aggravated when the group he had married into and his original group engaged in warfare. The man ultimately killed his hunting companions and sparked armed conflict anew, creating "distinct social groupings out of an original unity" (Fienup-Riordan 76).

Another cause of intragroup instability is resource strain. As it was described to Lewis Binford:

When we first move into a valley everything is good, people want visitors, people want to see friends, people want to share, but as time goes on, things get

used up and the place gets full of flies, then people start to fight. When that happens it's time to move to a place where nobody has lived for a long time [1983, 383-3].

The precarious nature of plant food sources and the reliance on hunting as primary subsistence, "makes [Arctic hunter-gathering] life the most precarious human adaptation on earth" (Lee 1968, 40). While it is true that subsistence in the Arctic can be arduous and precarious, this should not be exaggerated as fishing conducted by hunter-gatherers in high latitudes allows for a far more reliable food source (Lee 1968, 41). In the case of the Nunamiut interviewed by Binford (1983), hierarchies were established to mitigate resource distribution, and a shift in the group's geographical range allowed for the renewal of stability. In the aftermath of group fissioning in the Arctic following the murder of the Yukon man's hunting mates, social stability was regained during a famine as trade and cooperation were restored between the feuding groups. Again, these stories directly disprove the validity of the stereotypical peaceful Eskimo. Not only is this depiction of Arctic hunter-gathering peoples narrow-sighted, it also neglects to consider the complex relationships of northern Native tradition (Fienup-Riordan 1990). Citing Knud Rasmussen (1932, 17), Fienup-Riordan (1990) states that the majority of men in one northern native community had committed murder themselves, and that nearly every adult male had been a part of a killing in some capacity (Fienup-Riordan 1990, 150). From these oral histories and accounts that describe the propensity of war and conflict amongst the many complex groupings in the Arctic, it becomes clear that violence was a reality

for northern Native peoples, yet it has, and continues to be, overlooked in ethnographic history and public belief.

Ethnohistories are important tools in the archaeologists' kit as they can be used as the basis for analogic reasoning (Jordan and Cumming 2014, 8). However, "[if consumed] without prior testing, there is a great danger that they [will] merely reproduce the form and structure of ethnographically perceived reality in the archaeological record" (Wobst 1978, 303). Unfortunately, this has become the case with the trope of the peaceful Eskimo, and it has had continued influence in academia and "continues to inform American popular culture to this day" (Fienup-Riordan 1990, 163). By silencing these parts of Arctic Native history and tradition we encourage ignorance from non-Natives and the loss of Native American history. The origin of these stereotypes must be confronted and put to rest, as it is impossible to understand the lives and histories of Arctic hunter-gatherer groups without taking into account intersocietal violence (Chacon and Mendoza 2007, 28). By doing so, we enable ourselves to not only further understand the complex historical traditions and cultures of Arctic Native Americans, we may also more readily understand current socio-political actions taken by them today. By dispelling the trope of the peaceful Eskimo, we free Arctic Indigenous communities from the framework we have forced them to reside in.

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An Impossible Distinction

Emily Handfield

The silence in which I had found myself was formed by the rhythmic pitter-patter of the gently falling rain and the hushed rushing of cars on the nearby boulevard. The waterfront park was a narrow patch of greenery with a couple of trees, a singular winding path and a few benches. The younger trees had plastic shields around their trunks, indicating that someone was tending to their wellbeing. Fallen autumn leaves littered the grass and the stony path that paralleled the street right above it. The road had a wide biking path on one side and suburban homes stood on its other side. I placed a tablecloth on the wet bench before sitting down. Ahead of me, the still water of Lake Saint-Louis created a silvery expanse that blended with the misty sky into a curtain of grey. I was worried that no one else would brave the unfavourable cold weather, but people soon started to wander into my view.

Influencing Movement

In my hour spent in the park, I observed fifteen people. Twelve of them had a purpose other than the pleasure of walking; eight people were exercising on the road, while four others were walking their dogs and later came into the park. These two latter purposes seemed to affect the way people interacted with their environment.

Most of the exercisers maintained their gaze straight ahead of them to focus on their tasks of self-care. Some exercisers glanced at the park and the misty curtain beyond. Some noticed me gazing at them and returned my stare. None of the exercisers stopped to contemplate the view, nor passed through the park. If they seemed so uninterested by the park, why did they choose to exercise along this particular path? Perhaps being aware of the park and the water's presence was enough to make them choose this route.

The Sounds of Interaction

After visually assessing my environment, I attuned to its noises. The wheels of the cars passing behind me projected the already fallen water droplets back into the air to create a rushing sound. A person's foot pressed against the stony path and ground the pebbles together. Later on, another person's steps crunched the leaves beneath them. One sound in particular leaped out from this orchestra of human and non-human interactions: a cough. In fact, one-third of the people I observed either coughed or sniffled. Even through all their attempts to protect and distinguish themselves from the environment by wearing jackets, hats, gloves, pulling hoods over their heads, and carrying umbrellas, people's coughs proved the impossibility of their separation from the environment. The exchange of a cold breath in and a warm breath out created a strain on their bodies, leading them to either cough or sniffler. This interaction between warm bodies and the cold environment was not only audible, but also visible through the vapor that formed in front of people's mouths. People were not simply walking through the environment; they were breathing it in and leaving parts of themselves in it when exhaling. When I returned home, and was reminded of what warmth felt like, I too sniffled.

The environment impacts us in the most unexpected ways. It influenced people's exercise routes, their dog walking habits and even impacted the ease with which they breathed. Although we might perceive ourselves as being detached from the environment, it is clear that we are always in intimate exchange with the world around us.

The CRISPR Embryo

Germline Genome Editing and Selective Reproductive Technologies¹

Cynthia Lazzaroni

Introduction

November 25, 2018 – The Second International Summit on Human Genome Editing in Hong Kong was set to begin for a two-day (November 27 to November 29) series of conferences and panel discussions about the science and ethics of genome editing in humans.² Scientists and experts were landing in Hong Kong, getting ready to discuss pressing concerns about genetic technologies' advancement. Gabrielle³ was in her hotel room, preparing for the kick-off of the International Summit. She got distracted by the buzzing sound of her cellphone. “All of a sudden, Twitter and the Internet exploded with the announcement of the germline babies that were born,” she recalled. Gabrielle told me how she suddenly found herself in the middle of the mediatic explosion surrounding the news that Dr. He Jiankui, a leading scientist from the Southern University of Science and Technology in Shenzhen, China, had come to term with the first germline genome editing experiment on human embryos destined to pregnancy. Dr. He’s experiment resulted in the birth of the first gene-edited babies: twin Chinese girls nicknamed Lulu and Nana, for which he attempted to create a resistance against HIV by disabling their CCR5 gene.⁴

1 This article is an excerpt from my honours thesis The CRISPR Embryo - Germline Genome Editing, Bioethics And Selective Reproductive Technologies: Pathways To An Anthropological Approach To The Birth Of Lulu And Nana (Fall 2020), supervised by Professor Sandra Hyde.

2 The Second International Summit on Human Genome Editing was organized by the U.S. National Academy of Science, the U.S. National Academy of Medicine, the Royal Society of the United Kingdom, and the Academy of Sciences of Hong Kong.

3 This research received approval from the McGill Research Ethics Board (REB file # 20–03–043). The names of participants have been changed to protect the confidentiality and safety of research participants. In the few cases where participants requested that their real name be used, an asterisk is added next to their names when they first appear in the writings.

4 For his experiment, Dr. He recruited seven Chinese couples struggling with fertility, where the father was HIV positive, and the mother was not infected with HIV. Lulu and Nana were created with IVF, and edited with CRISPR before implantation in their mother’s womb.

Gabrielle's cellphone kept ringing. Amid the mediatic wave that surged the Summit, Gabrielle sensed an accompanying wave of shock. "I was not aware of anything, and neither were any of the people that I usually work with. So it really was a shock," she explained. Gabrielle was not the only attendee at the Summit who felt the wave of shock that surrounded the surprising announcement of the first gene-edited babies. Ayo* recalled how everyone attending the Summit strongly felt this wave of shock. As I asked him why he sensed this feeling of shock among the scientists and experts present at the Summit, he expressed what several of my interviewees mentioned: concerns about the scientific, technical, and ethical questions related to the safety of germline genome editing at this time and inquiries about what germline genome editing means in light of selective reproductive technologies. Since the First International Summit on Human Genome Editing in 2015, it was believed by many that a sort of consensus against the use of genome editing technologies in human embryos was in effect. Neither the science nor the ethics of human germline editing were deemed safe and acceptable in the wake of the Second Summit. Indeed, Ayo pointed out how the whole point of the Second International Summit in Hong Kong was "for the scientific community to show that it wants to stay on top of the ethical issues, and debating them and discussing them before proceeding to trials in humans." The gathering's whole point was for the scientific community to discuss these kinds of questions and whether genome editing on humans should occur. Part of the agenda was to assess the science and ethics behind a recent genome editing

technology: CRISPR (short for 'clustered regularly interspaced short palindromic repeats'). As it happened, Dr. He Jiankui used CRISPR to alter Lulu and Nana's genes.

When news about the CRISPR babies leaked, a sense of crossing a controversial line was felt at the conference in Hong Kong, a controversial crossing-point that quickly reached the international scientific community and public media. Dr. He was not supposed to talk about the CRISPR twins at the Summit. He was invited to speak on a panel about nonviable embryo editing and some other research he designed. With the news now on everyone's mind, the Summit organizing committee asked Dr. He to talk about Lulu and Nana. The mediatic wave followed him as he walked on the stage of the Ran Ran Shaw auditorium at Hong Kong University. Dr. He answered questions from fellow scientists and left the international community hanging about the immanent controversy of his experiment and a sense of disappointment that he had moved forward with the birth of genetically modified embryos. At the end of the Second International Summit on Human Genome Editing, the organizing committee issued a statement judging the "unexpected and deeply disturbing claim that human embryos had been edited and implanted, resulting in a pregnancy and the birth of twins" (referring to Dr. He's work, while not naming the scientist) irresponsible and a failure to conform with international norms (2018).

For Ayo and many of my interviewees, germline genome editing in embryos moves upstream within of what Gammeltoft and Wahlberg (2014) call selective reproductive technologies (SRTs): the practices aiming to prevent

or allow the birth of certain kinds of children (2014, 201), echoing similar ethical concerns as reimplantation genetic diagnosis (PGD) and in vitro fertilization (IVF).

This research draws from Lulu and Nana's birth to raise, explore and analyze the bioethical questions behind the use of genome editing in human embryos. To do so, I situate germline genome editing within larger discussions of SRTs, arguing that germline genome editing concerns align with ongoing ethical questions accompanying the increased routinization of SRTs, such as PGD. However, CRISPR embryos move the discussion from choosing which embryo to implant or not with PGD and IFV, or choosing whether to terminate or not pregnancy after genetic testing or amniocentesis, to a discussion about actively creating the kind of child to be born by altering their DNA as embryos. As with ethical discourses surrounding genetic testing and genetic selection with reproductive technologies, I also turn to diversity issues. Disability studies and anthropological inquiries about eugenics frame this discussion, echoing questions of what kind of world we want to live in, the value of diversity, who decides what counts as diversity, and the complex relations that may arise between genetic selection and eugenics. Ultimately, this thesis frames CRISPR embryos within discussions about reproductive technologies to argue that the ethical concerns surrounding the birth of Lulu and Nana are not new.

In this short excerpt from my honours thesis,⁵ I reproduce the concluding section of Chapter One, titled *The Selective Reproduction Arena*. I inquire into the ambivalence of reproductive choices with Anne Kerr (2009) and Abby Lippman (1994), and I turn to the geneticization of reproduction and questions of needs. With Rayna Rapp (1988, 1999), I posit that participants to the He experiment were moral pioneers, and I argue, following Sarah Franklin and Celia Roberts (2004; 2006), that moral decisions evolve with the technologies that require them. By situating germline genome editing within the realm of selective reproductive technologies, and following Amy, a leading bioethicist, I aim to show that CRISPR embryos do not require new ways of thinking. I then conclude with a short discussion of *How to Live with Difference* – how to live with new beings like Lulu and Nana.

The Selective Reproductive Arena

As discussed in the Introduction, for Ayo and many of my interviewees, germline gene-editing in embryos moves upstream within selective reproductive technologies, echoing similar ethical concerns as genetic testing, amniocentesis, and reimplantation genetic diagnosis (PGD). I situate CRISPR germline genome editing into the spectrum of selective reproductive technologies. As anthropologists Ayo Wahlberg and Tina M. Gammeltoft argue

⁵ The full thesis has three chapters. The first chapter revolves around the ethical questions related to the use of CRISPR germline genome editing. It explores the details of the He experiment, and turns to the ethical questions from the perspective of the science and of selective reproductive technologies (this the topic of this article). The second chapter turns to diversity issues and discusses concerns about a renewal of eugenics with CRISPR embryos. It then inquires into who gets to decide about germline genome editing. The final chapter concludes with a discussion of how to live with difference (which is partially discussed in this article) and what anthropology brings to the table.

(2014, 2018), selective reproductive technologies refer to practices aiming to allow or prevent the birth of specific kinds of children. While assisted reproductive technologies are specifically designed to overcome fertility issues, selective reproductive technologies add other layers of possibilities for couples who aim to overcome fertility issues and couples who do not have fertility issues but still turn to SRTs for their reproductive choices. What the authors frame as nature assisted (ARTs) and nature directed SRTs (2018, 6) may be called “nature made” or “nature created” with CRISPR germline editing. Unlike established SRTs, CRISPR embryos move the discussion to concerns about actively *making* changes to an embryo’s genome. Rather than genetically testing and then selecting embryos made via IVF (as with PGD) before implantation or testing the fetus during pregnancy with amniocentesis, CRISPR germline genome editing makes it possible to actively modify, change, add, or delete part of embryos’ genomes before implantation with established IVF procedures.

As we discussed CRISPR germline genome editing, Ayo described how CRISPR embryos are a continuation of SRTs:

“So through prenatal screening, and through prenatal diagnosis, you identify embryos, which, you know, perhaps have a serious genetic condition or trisomy. There are a number of different conditions that can be detected now. And for some parents, they then use that information to decide to terminate (pregnancy). That is a form of selective reproduction because the pregnancy was wanted, but it was terminated because of the kind of child that would potentially be born. So abortion has really been and continues to be the

major selective reproductive technology to this date. And then, as we know, it is moving further and further upstream. You could put it like that in the reproductive process. So now we can make embryos in laboratories, and indeed we do this on a huge scale with IVF. So getting access to the embryo allows you then not to test the fetus but to biopsy the embryo. And otherwise, it is the same discussion. So you can get access to the DNA of an embryo.”

As Ayo pointed out, CRISPR embryos come as the next option on the list, from making babies in the lab, to testing and then selecting or discarding them, and now actively changing their DNA. The increasingly widespread availability of ARTs and SRTs paved the way for introducing CRISPR and germline genome editing into the fertility clinic, a path that Dr. He Jiankui followed. With regards to selective reproduction, ethical debates are strongly linked to diversity issues. From which conditions to screen for, to what makes a condition severe and/or undesirable enough to justify pregnancy termination, and women and families’ reproductive rights and choices, concerns around SRTs touch on matters of diversity, disability, reproductive rights, and women’s choices. As Ayo told me: “it raises questions of what kind of world do we want to be living in? Are we selecting in a eugenic sense? Which condition should we allow that to happen for? Do we need to edit embryos if we have PGD? What is it that we allow PGD for? Can a case be made for CRISPR embryos?”

Disability rights scholars and activists such as Tom Shakespeare (1998) have long argued that selective reproductive technologies “rest on and reinforce a reduction of the value of disabled lives”

(Wahlberg & Gammeltoft 2018, 17), echoing essential questions about the ethics of selective reproductive technologies. Further on in our conversation, Ayo traced how current discussions around the ethics of PGD apply uncannily to concerns about CRISPR embryos:

"What is it that we allow PGD for? You know, there are parts of the world where, in principle, you could use PGD to create blue-eyed children. It does not happen on any large scale. I am sure it has happened somewhere in the world, and perhaps it is ongoing right now. We know it for sex selection, for example, which is for so-called social reasons. So there are areas where we are expanding the boundaries of what is the purpose of this selection to socially balance the family versus to avoid sex-linked disease, which would cause suffering. So this is the core question, I think, that the editing of embryos enters into, but not exceptionally. We have had this selective reproduction ethical debate for a long time, and we need to continue it. Every country needs to continue debating what it is that we should, as a society, be condoning/legislating/allowing in terms of selection? Should it be a serious disease therapeutic consideration, or should it not? And it is a diverse legal situation in the world, which, of course, leads then to those people who travel across borders to access certain technologies. So we know that if it is available somewhere, the market will arise."

Ayo's concerns echo the blurred line between enhancement and treatment and what conditions are deemed serious enough for reproductive selection. Whether for PGD or CRISPR embryos, these questions re-

quire similar thoughts and ethical debates, which are ongoing. While what is allowed for PGD at some time and place is likely to change and evolve over time, so is what may be allowed with CRISPR if the technology is deemed safe.

Within selective reproduction debates, polarities emerge between those who reject the use of selective reproductive technologies at all cost, regardless of the condition; those who accept the use of selective reproductive technologies for certain conditions deemed severe and undesirable; and those who promote the use of selective reproductive technologies regardless of the condition. With regards to CRISPR germline genome editing, similar polarities arise. Most of my interviewees located themselves in the middle ground of the spectrum between rejection and promotion of germline genome editing. On their mind, and as Sofia told me, "there are some cases in which it makes sense to do something in the germline, the embryo or sperm or egg rather than waiting until you have a child born with a condition." She noted that these cases are certain genetic problems for which waiting until a child is born may be too late to prevent or treat. Genetic diseases where no cure exists and where both parents carry the affected gene, for example, could eventually justify the use of CRISPR to prevent this family's children from carrying on the inevitable genetic disease. For these specific cases, like Huntington's disease or sickle cell anemia, modifying the embryos' genome could eradicate the disease for these children and prevent them from passing on the gene inherited by their parents.

These discussions are already ongoing with regard to preimplantation

genetic diagnosis. However, as Sofia put it, it is only theoretically possible for some people to have access to ARTs and SRTs, and then to do IVF and PGD and choose to transfer unaffected embryos, if there are any. As she mentioned, people often get few embryos to choose from with PGD, and there is still a chance that perhaps either all of them are affected with the genetic disease, or none of them are viable, or the IVF implantation of, let us say, the sole embryo that was not carrying the defective gene does not take. “While it [PGD] would be a better option for most people, just to do PGD, it is not gonna be an option for everybody in this situation ... I feel like it is all about the nuances of the different cases and the different conditions and even the different situations that different couples find themselves in,” she noted. What Sofia pointed out is the complexity of selective reproductive technologies to be guided by predefined conditions. Indeed, each family’s nuanced background, experiences and particularities bring to the fertility clinic complicated reproductive and moral choices. In some cases, these intricacies could justify turning to CRISPR embryos.

Reproductive choices are often ambivalent, requiring families to navigate unknown moral, ethical and social territory, where important choices must be made for them and their potential children. Anne Kerr (2009) explains that decision-making about PGD and genetic reproduction are ambivalent and bounded to uncertainty (Kerr 2009, 4). She sees how ambivalence or doubt accompanying new genetic technologies inform moral or ethical decisions, individually and socially (4-5), and notes how reproductive choices are not made in isolation from broader social networks. Lack of information, ambivalent opinions, social pressure, or lack of time and space

for contemplating possible decisions frame how choices are made in reproductive genetics (9). While providing families with a plurality of options, ARTs and SRTs also complexify decision-making processes.

Even before the turn of the century, discussions about the increasing use of genetic technologies in prenatal screening were ongoing as prenatal diagnosis (ultrasounds, amniocentesis, PGD) became routinized among (mainly, as Lippman notes) white, middle-class women in North America (Lippman 1994, 113). Abby Lippman explores the ‘geneticization’ – the reduction of biology and reproduction (and more broadly, diseases and behaviors) to the vocabulary of genetics – behind stories of early prenatal diagnosis and selective reproduction. When pregnant women are drawn into the world of SRTs, which emphasizes the importance of genetics, they come to fetishize genetics and ‘good genes.’ Increasingly ‘geneticized,’ prenatal diagnosis is also fetishized, creating the *need* for pregnant women to test, screen, and ‘confirm’ that their embryos are ‘normal.’ Lippman inquires into the stories that reinforce the ideas that pregnant women *need* a prenatal diagnosis, and, I may add, SRTs. With regard to the ‘need’ for prenatal testing, Lippman identifies three lines of thoughts: “(1) as an assembly-line approach to the products of conception, separating out those products we wish to develop from those we wish to discontinue; (2) as a way to give women control over their pregnancies, respecting (increasing) their autonomy to choose the kinds of children they will bear, or (3) as a means of reassuring women that enhances their experience of pregnancy” (1994, 114). However, she argues that neither of these is a complete story. Even taken together, they fail to account or how, beyond ‘product control,’

'reassurance,' and 'choice' for pregnant women, "posing a 'need' for testing to reduce the probability that a woman will give birth to a child with some detectable characteristic rests on assumptions about the value of [genetic] information, about which characteristics are or are not of value and about which risks should or should not be taken" (117). It also rests on assumptions that certain fetal conditions are unwanted and that women's 'choices' are their own. However, Lippman argues that these 'needs' and assumptions of 'free choice' are construed within Western biomedical beliefs, often by experts and specialists that determine "who uses prenatal diagnosis and for what reasons" (119), and what conditions are deemed reasonable for fetal abortion or embryo destruction.

For Lippman, prenatal screening and diagnosis' 'needs' come from above. They are informed by who makes decisions about these technologies, their regulation, and their accessibility. Behind stories of increasing and reassuring women's reproductive choices and control (119), political, social, and economic contexts shape who can and how they can access these technologies. Hence, following Lippman, I raise questions about who chooses and who is at the table discussing SRTs. With the rise of international discussions around CRISPR and how scientists should proceed with this technology, especially regarding the use of CRISPR for germline genome editing, who is at the table matters. I now want to turn to how women and families interacting with SRTs are moral pioneers. Drawing from the works of Rayna Rapp (1988; 1999) and Sarah Franklin and Celia Roberts (2004), I posit that Paul, Mary, Mark and Grace,⁶ and the other par-

ticipants of the He experiment are moral pioneers. I argue that, in practice, particularly for the families involved, moral decisions evolve with the technologies that require them.

Rayna Rapp's famous ethnography of amniocentesis and prenatal diagnosis in the United States (1999) explores the social impact and cultural meaning of these increasingly routinized reproductive practices. Exploring the various perspectives of women accepting or refusing the test, of scientists, doctors and geneticists involved, and of parents of children living with disabilities often diagnosed (and aborted) following prenatal testing, Rapp contends that:

"The construction and routinization of this technology is turning the women to whom it is offered into moral pioneers: situated on a research frontier of the expanding capacity for prenatal genetic diagnosis, they are forced to judge the quality of their own fetuses, making concrete and embodied decisions about the standards for entry into the human community" (3)

For Rapp, women and families accepting (or refusing) to undergo prenatal testing (as SRTs) become moral pioneers as they must navigate complex decision-making processes, often with relatively new or controversial technologies, in the face of critical ethical questions related to the kinds of offspring they will bear. These ethical decisions often blur the lines between what Rapp calls reproductive consciousness (women's reproductive options and decisions) and disability consciousness (biomedical imaginaries about what

⁶ Some of the participants of the He experiment; Mark and Grace (pseudonyms) are Lulu and Nana's parents.

t kinds of children may or may not be born according to their medical conditions and the social meanings of disability they carry). As such, Rapp argues that all women she met participated, whether or not they accepted prenatal testing, “in the meeting between reproductive consciousness and disability consciousness which technoscientific advances are helping to shape” (Rapp 1999, 310). Rapp concludes that “at the same time that the offer of prenatal diagnosis forced women to consider the concrete limits they might individually place on atypical maternity through selective abortion, it also forced an engagement with disability consciousness” (310). Following Rapp, I contend that engaging with SRTs such as amniocentesis, PGD and CRISPR embryos requires women to make decisions while navigating the complicated threshold of their reproductive desires and the limits they may place on motherhood – both influenced by their individual experiences and collective socio-cultural contexts – a new or renewed encounter with disability imaginaries, and the significant ethical decisions they must make. More often than not, the specifics of each woman’s experience creates a tricky moral territory that predefined ethical discussions cannot account for.

For Mary (and Paul), and Grace (and Mark), navigating the morality of participating in Dr. He’s experiment required them to consider two forces. On the one hand, they had to consider their respective private and public social contexts (the father’s HIV positive status and related discrimination, and their socioeconomic

position) and their desire for a healthy child. On the other hand, they had to consider the novelty of CRISPR germline editing, the uncertainties of the experiment, and safety concerns for their families and potential children. As explained earlier, participants in Dr. He’s experiment were educated and able to discern what they were enrolling in, to the extent of the information they were given.⁷ They knew they were getting involved with a form of selective reproductive technology involving genetic modification. Putting aside the ethical concerns with regard to the degree of informed consent, I posit that Paul, Mary, Mark, Grace and the other participants of the He experiment acted as moral pioneers. Indeed, they navigated the unknown territory of engaging with CRISPR embryos and embodied critical decisions for their families, amidst complex social and moral choices. With Franklin and Roberts (2004), I argue that as moral pioneers, the participants of Dr. He’s experiment navigated the ethics of their choices alongside their progressive involvement with SRTs. I follow the authors in arguing that, in the realm of SRTs, parents’ moral decisions evolve with the technologies that require them. Indeed, I posit that while the current controversy around the use of CRISPR germline editing is well ever-evolving, so will the ethical questions surrounding its introduction within reproductive medicine, and perhaps even, in the future, its routinization within SRTs.

With regard to germline genome editing entering the realm of reproductive technologies, Martin noted how radical

⁷ It is worth mentioning that, according to Eben Kirksey, Dr. He was not entirely honest with the participants on the support he claimed having from the Chinese government, his conflicts of interest, and the fact that such an experiment was the first of its kind (Kirksey 2020, see chapter 18).

new medical and genetic techniques are often greeted with a high degree of skepticism before eventually being integrated into medical practice. Often, the controversies of their origin stories remit over time. Martin – and all of my interviewees – referred to Louise Brown’s birth, the first IVF baby, in the 1970s. As we discussed the famous controversy of the first test-tube baby, Martin said:

“Test-tube babies – that was a real kind of new technique in reproductive medicine that was really controversial and hotly debated at the time. Now, if you are walking in certain neighborhoods in New York or you know very suburbs cities, you could barely walk a block without passing someone pushing a baby stroller that has a kid created with artificial, assisted reproduction. So you know controversial technologies are sometimes born with a high degree of controversy, but they pretty quickly get assimilated and integrated into the office such as they are no longer really controversial...”

For Martin, although currently very controversial, CRISPR germline genome editing may well be, over time, integrated into reproductive medicine. Accordingly, he noted that “what is perhaps exotic to us today would cease to be exotic to us probably within a span of you know ten years, it would pretty soon be integrated into the new norm of options that are available...” – considering, of course, that the technology is proven safe.

I conclude with my discussion with Martin to emphasize how new reproductive technologies often end up being routinized over time, losing their ‘exotic’ and

controversial qualities. As such, I want to reiterate that the parents involved in the first ARTs and SRTs innovations are moral pioneers (as Louise Brown’s parents may well be called), in Rayna Rapp’s sense. Even if Louise Brown’s birth can still be criticized as an infamous example of dubious ethical procedures within reproductive sciences, as is Lulu and Nana’s birth, IVF nevertheless became the familiar birth story of many (and counting) children. As I asked Martin what he thought the future of CRISPR germline genome editing might be, he answered by saying how “the ways new medical technologies look to us at this moment change over time as they quickly get assimilated into a new norm and a new norm that is not necessarily more morally problematic than the old norm.” Martin’s thought implies that, just as Franklin and Roberts (2004) argue that scientific innovation and society’s morality evolve together, ethical concerns evolve alongside the technologies that require them.

Amy (the bioethicist) does not see CRISPR as exceptional in terms of the ethical questions debated. She does not see that CRISPR requires an engagement with entirely new ways of thinking or new ethical challenges. In her words, CRISPR “raises the same questions we always dealt with” with other forms of selective reproductive technologies since the routinization of IVF and PGD. By situating germline genome editing within the realm of SRTs, I aim to show, following Amy, that discussions around selective reproductive choices, including germline genome editing, are not new. Indeed, CRISPR germline genome editing enters the realm of selective reproduction and brings forth similar concerns than what has been discussed with other selective techniques, from abortion to prenatal ultrasounds, amniocentesis and PGD.

How to Live With Difference

In 1991, Donna Haraway posited that we were all cyborg, mi-human, mi-machines, always transforming our bodies and environments with new technologies. Following Haraway, Sarah Franklin (2006) wrote about the cyborg embryo. For Franklin, biology is increasingly *made and born* with assisted and selective reproductive technologies, normalizing ideals of cyborg embryos (Franklin 2006, 171): embryos made and born by reproductive technologies. With CRISPR, embryos are literally *made*. CRISPR embryos force us to think about how to live with difference; that is, how to live with different sorts of beings, bodies and minds. In my conversation with Eben, we discussed this very issue. For him, CRISPR embryos are not an exceptional case. Indeed, living with difference concerns as much living with differently able bodies and minds as living with gene-edited babies or other forms of being. For Eben, how to live with difference is an ethical problem in itself. It involves discussions about how we value (or disvalue) difference, and how we accommodate or reject different beings. With CRISPR embryos, he said, “we are also seeing a diversity of new forms [of life] emerge. So, I think there are ethical questions or legal questions about how to regard these new forms of life.” Genetically modified babies like Lulu and Nana, like the first test-tube babies conceived via IVF, are such new forms of life. As Eben said:

“An earlier point of comparison is also the birth of Louise Brown, who was deemed monstrous when she was born, you know, everyone from the pope to scientific authority and way down said that it was abominable that an experi-

ment like that was done. And you saw a lot of the same language repeating itself [with the CRISPR babies]. And even esteemed bioethicists at the University of Oxford deemed the He Jiankui experiment monstrous, by implication that the two children were also monsters. And, you know, I think we must critically step back from these figures of abomination to think about ways that new objects are being formed by science. And I think it would be wrong to deny full humanity to genetically modified children. I think it is important to embrace and celebrate the full diversity of humanity.”

I asked Eben how we could relate to the twins, born out of controversy, and what living with genetically modified beings could look like. His answer was simple and revolved around letting Lulu and Nana live a normal life, undefined by their difference. As he said:

“I think in this particular case, it could look like letting them enjoy normality. One big contrast between this moment and the moment when Louise Brown was born is that Louise Brown was a public person from the moment she was born. Perhaps, I mean, there are other people who are public persons like princes... But she was perhaps the first person who became an internationally known figure at the moment of her birth. And in contrast, the secrecy surrounding the experiment by Dr. He had in my mind one ethical result, which was protecting these children from unwanted scrutiny by journalists, by members of the curious public, by neighbors, by friends of the family. So right now, they are living a life of privacy. And so embracing them as normal people I think is one way of

living ethically and responsibly.”

Regardless of the controversy behind their birth, allowing the twins to live a normal life is the first step towards normalizing their birth and living with their way of being. Letting the twins out of mediatic scrutiny, allowing them to have a normal childhood, and perhaps “forgetting” that Lulu and Nana have “a little something different” is the first step to relate to the twins. Indeed, like any other different being, Lulu and Nana have the right to be equally treated and valued. They deserve equality, respect and full recognition of their humanity. How to live with difference is perhaps one of the most critical questions regarding the first CRISPR babies. Indeed, how we will relate to the twins will shape what the future of CRISPR embryos may hold, and what will be discussed and by whom at the decision-making table.

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The Real Crisis of Representation

Towards a Decolonized Anthropology

Simona Bobrow

In *Anthropology as Cultural Critique*, George E. Marcus and Michael M. Fischer identify periods of time where scholars became acutely aware of the limitations of paradigmatic theories and methods for adequately understanding, describing and explaining social life in their disciplines. They refer to this problematization of prevailing theories as the ‘crisis of representation’ – and they argue that the contemporary discipline of anthropology is experiencing this crisis. In this paper, I will be critiquing Marcus and Fischer’s depiction of the crisis of representation, particularly in the context of how anthropology as cultural critique can be henceforth cultivated. My critique will draw mainly from the decolonization theories of Lewis R. Gordon and Zoe Todd in order to show that the true crisis of representation can only be resolved through the non-metaphorical decolonization of anthropology. After an overview of Marcus and Fischer’s illustration of the crisis of representation, I will explain how they fall prey to what Gordon terms ‘disciplinary decadence,’ and how the failure of anthropology to recognize and reconcile with its historical relationship to colonialism perpetuates violent colonial power relations. In light of this, I reframe the crisis of representation as an issue of anthropology’s favouring of white Western voices and European, enlightenment-era epistemologies, arguing alongside Todd that decolonizing anthropology is necessary to resolving the real

crisis of representation.

The crisis of representation is seen as the current “intellectual stimulus” for the experimental nature of anthropological research at present (Marcus and Fischer 1999, 8). This crisis arises from “uncertainty about adequate means of describing social reality” (Marcus and Fischer 1999, 8). In other words, the limitations of the prevailing theoretical frameworks and methodological techniques which previously guided the discipline are being more broadly recognized, namely their failure to account for and explain important aspects of social reality (Marcus and Fischer 1999, 12). Marcus and Fischer argue that the entire direction of anthropology is not in jeopardy as a result, but rather that current experiments in anthropology will enable the discipline to stay in line with its promises to accurately depict cultural difference and use this knowledge to critically assess ‘domestic’ or Western social realities (Marcus and Fischer 1999, 42). The authors discuss one current methodological trend of experimentation in anthropology, which addresses anthropology’s ineffectiveness in confronting issues of historical context and political-economy within ethnographies which is “relevant not only to its own subjects, but also to its own research process” (Marcus and Fischer 1999, 34). In other words, the depiction of traditional ethnographic subjects as isolated and pristine cultural units, rather than historically bound and

perpetually in flux, represents a limitation in previous anthropological paradigmatic theories and methods, and thus the discipline as a whole. The current trend attempts to remedy this issue by incorporating questions of political-economy and history into the ethnographic process (Marcus and Fischer 1999, 85). These experimental anthropologists operate under new guiding questions, asking how they can better account for “a reciprocity of perspectives” now that we understand ethnographic subjects as “far from being isolated from the same world system that forms the anthropologist’s cosmopolitan consciousness” instead of their traditional, ahistoric, ‘self-contained’ understanding of ethnographic subjects (Marcus and Fischer 1999, 86).

To answer this question, they investigate “indigenous historical consciousness,” or traditionally Indigenous ethnographic subjects’ self-determined histories, juxtaposed with the “Western narrative of their experiences” in effort to correct anthropology’s ahistoricity, which also putatively provides a way to critique Western scholarship (Marcus and Fischer 1999, 78). Marcus and Fischer posit that if anthropology can better represent the perspectives of other cultures with respect to their historical context and political-economy positionality, then the underdeveloped opportunity of repatriating anthropology as cultural critique can be better cultivated. Traditional comparative methods in anthropology which relied on social evolutionist logic inevitably resulted in critiques which served to reaffirm the “superiority of modern European or American society” (Marcus and Fischer 1999, 129). Though this logic is now commonly seen as outdated and defunct, the underlying schemas it

produced – such as traditional/modern, underdeveloped/developed – remain embedded in anthropological theory today.

Marcus and Fischer point out that while these problematic schemas need to be overcome in repatriated anthropological critique, there must also be a balance in proposed alternatives which do not fall “prey to overly romantic or idealist representations of the exotic” ways of life (Marcus and Fischer 1999, 117). Ultimately, the authors portray a vision of ‘domestic’ anthropological cultural critiques as works which undertake “dual projects of ethnography equally committed to their own contexts and equally engaged in cultural criticism,” rather than the common problem of imbalanced focus on either the ‘domestic’ ethnography or the ethnography of the ‘other’ (Marcus and Fischer 1999, 138). They see the strongest form of repatriated cultural critiques as those in which the anthropologist views their own culture just as unfamiliarly as they view the ‘other’ culture, which can be best achieved through rigorous research in both domains by the anthropologist, though they point out that a successful work of this kind does not exist, at least to their knowledge.

Despite Marcus and Fischer’s critiques of prevailing anthropological traditions, they continue to uphold the traditional role of the anthropologist as an objective, rational, white supremacist agent from the West. To truly view one’s own culture as unfamiliar, as they argue is necessary to provide an apt domestic cultural critique, wouldn’t it be logical to listen to the perspective of someone who inevitably views it this way? In other words, it seems that listening to the actual perspective of the cultural ‘other’ would be vital to a critique of the

Western world. However, Marcus and Fischer see the possibility of, say, Trobrianders and Islanders conducting reciprocal cultural critiques of the US as impossible because “by the time such others are trained as anthropologists...they of course are no longer radically other” and thus deem their critique invalid on these grounds (Marcus and Fischer 1999, 156). I would be remiss to not point out the hypocrisy here – they seem to think ‘domestic’ anthropologists can become ‘radically other’ from their own cultures more than an actual person from another culture could. They default to a conception of resolving the crisis of representation in anthropology as cultural critique as the responsibility of the traditionally white, Western anthropologist to engage in more rigorous ethnographic defamiliarization with their own society, in addition to this application to the cultures of others, as opposed to listening to the voice of so-called ‘others’.

Considering anthropology’s entangled history with colonialism, as well as Edward Said’s famous critique of anthropology’s traditional rhetoric imposing passivity upon their subjects (often those situated in worlds dominated by Western colonialism, I contend that Marcus and Fischer fall prey to what Lewis R. Gordon terms ‘disciplinary decadence’ (Gordon 2014, 36, 2). This term refers to the tendency of disciplines to turn inward and away from the reality which “recognizes its own limitations,” instead cultivating a sort of methodological solipsism where “becoming ‘right’ is simply a matter of applying, as fetish, the method correctly” (Gordon 2014, 86). Marcus and Fischer’s work is an instance of disciplinary decadence because, despite their critiques and argument for new experimental techniques, they fetishize the traditional, colonial roles which

demarcate who qualifies as an anthropologist and who is confined to the role of the ‘subject’. Gordon identifies the best response to disciplinary decadence as “*trans-disciplinarity*, where disciplines work *through* each other” in order to produce knowledge rather than solely under their own theories and methods, which Marcus and Fischer’s propositions for incorporating history and political-economy into anthropology seem to reflect (Gordon 2014, 87). However, Gordon goes further and suggests that doing this will raise questions that require the “*teleological suspension of disciplinarity*” in order to answer– questions that challenge the core purpose and goals of the discipline (Gordon 2014, 87). This suspension of traditional disciplinary principles is an “epistemic decolonial act” because it questions the foundations of the discipline, which are implicitly colonial, and allows for discourse to occur outside of these bounds (Gordon 2014, 87). Unlike Gordon’s, Marcus and Fischer’s proposals for how anthropology ought to develop continues to uphold implicit colonial relations.

Gordon delineates the way that the production of knowledge has historically been “enlisted in the service of colonisation,” not just at the epistemological level, but also at the methodological level (Gordon 2014, 85). Gordon points out how through processes of colonisation, epistemological developments led to the formation of groups of people who “are indigenous to a world that, paradoxically, they do not belong to” (Gordon 2014, 84). Victims of colonization are ‘othered’ even when they could not exist anywhere else from the ‘non-other’ world. More specifically, and similarly to Said’s critique, he explains how colonizers maintain power through the “elimination of discursive opposition”

from the subordinate group (Gordon 2014, 88). In his words, “It is not that the colonised group fails to speak,” but that their words are not transformed into speech; they are not listened to with sincerity nor attributed with legitimacy (Gordon 2014, 88). While Marcus and Fischer acknowledge anthropology’s colonial legacy and the inadequacy of such related basic concepts – the notion of their subjects as ‘primitive’, ‘isolated’, and ‘self-contained’ cultures – they fail to envision a reversal of these traditional roles as a response to the crisis of representation. They even take space to ensure that when producing a cultural critique of a ‘domestic’ or Western society, the anthropologist does not ‘over romanticize’ (read: legitimize) the social reality or epistemology of the ‘other’ when suggesting alternative social organizations in their own society, in effect aggrandizing Western, enlightenment-era epistemologies (Clement 2019). In fact, their very depiction of this crisis – as one of not knowing how best to represent cultural others in light of the changing world – fails to fully realize itself. I propose that the real crisis of representation, in light of Gordon’s perspective, is that the voices of the ‘other’ are not empowered to represent themselves and their own cultures, thanks to the discipline’s colonial structural traditions and decadence.

The problem I have identified in Marcus and Fischer’s assessment of the current state of the discipline remains prominent, despite the book being published just over two decades ago. Métis anthropologist Zoe Todd understands the colonial nature of representation in anthropology well, departing in her work from the unsettling reality that anthropologists of the day appropriate ideas from Indigenous cultures without reference to

any Indigenous scholar who represents themselves, deferring instead to old ethnographic texts where white anthropologists represent Indigenous peoples – an act of “epistemic colonial violence” (Todd 2016, 17). She is living proof of the insidiousness of what I called the real crisis of representation: even where Indigenous or ‘other’ anthropologists exist, the inherently colonial structures of the academy prevent their voices from truly being heard (Todd 2016, 12). As a white-passing Indigenous person, Todd explains her “curious access” into anthropology as ‘white public space,’ where white scholars can “say what what they really think” about Indigenous issues or People of Colour when they assume everyone in the room is Caucasian” – a space she identifies as the “gulf between ‘what is’ and ‘what can be’” in the discipline (Todd 2016, 12). Todd critiques the structural and routinised whiteness in the academy, which serves to reinforce colonial and neocolonial relations in both in the discipline and in social reality. Even if anthropologists such as Marcus and Fischer acknowledge anthropology’s colonial history, they must go further to acknowledge colonialism as an “extant, ongoing reality” and engage in material decolonization in order to truly achieve the liberatory and radical reconceptualization of the discipline that their critique demands (Todd 2016, 7). The resolution of anthropology’s true crisis of representation is constrained by the “white supremacist, imperial human dimensions of the academy,” and without material structural change theoretical decolonization cannot be realized (Todd 2016, 19). Simply put, “decolonization is not a metaphor” rather it requires decolonial material change – that is, the repatriation of Indigenous lands and lifeways – in order to truly achieve its goal (Tuck and

Yang 2012). True representation of the ‘other’ cannot be realized in an academy, nor a world, where colonial power dynamics and structures continue to be upheld.

Marcus and Fischer assess the current state of anthropology as being in a crisis of representation, a phrase they use to describe that the discipline is in crisis because its founding practices of how to portray cultural differences are being progressively deconstructed, revealing its problematic foundations. Gordon’s theory of decolonization points out that questioning the discipline is a necessary result of diverting from disciplinary decadence, and brings us closer to more accurate knowledges of social reality. I have argued that the true crisis of representation is not simply a matter of *how*, technically or methodologically, to represent other cultures and people, but *who* can represent ‘others’. Marcus and Fischer fall short of arguing for what is truly needed to remedy anthropology’s colonial foundations, succumbing to disciplinary decadence. Gordon and Todd provide insight into the historically colonial nature of the production of anthropological knowledge and the implicit structures within the academy which uphold the white, colonial silencing of the ‘other’. In order to resolve the crisis of representation as I have reconceptualized it, non-metaphorical decolonization is necessary. If this throws the discipline of anthropology as we know it into question, perhaps this is not a bad thing, but a necessary move towards achieving anthropology’s foundational goal of representing cultural diversity, in a way that can achieve unprecedented justice in the academic world of cultural representation.

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Vyvanse as an Affectively Cybernetic Technology of the ‘Self’

An Exploration of the Contemporary Extimate Subject

Tanya Geggie

What does it mean to know yourself? Is it possible? To embark on an understanding of the ‘self’ is to look outward, toward expansive exteriors, toward interactions. To investigate particular encounters, perceptions, and inhibitions of the world is to dive into the webbed systems that establish it. Subjectivity is an ever-changing registration of various practices, technologies, discourses, apparatuses, and behaviors –what we might call *dispositives* (Han 2017, 12)– that come into contact with the ‘self.’ In studies of selfhood, internality and externality bleed into one another. Although it is perhaps impossible to insert ourselves into the chaos of perpetual synergism that structures being, there is much to learn from trying.

My sense of self as a young adult began to develop at university. Living away from home for the first time and maneuvering what was an unprecedented degree of unstructured time compared to my high school experience, my academic efforts and performance felt newly individuated. In the post-secondary educational setting, I was seemingly in complete control of my position as a student along with the potentiality of success; it was my own prerogative to manage myself as well as my time. Although diagnosed in high school, it was within this new framework that I came to define my attention deficit hyperactivity disorder (ADHD) as a major impediment to scholarly achievement. This

paper explores my personal experiences that emanated from receiving an ADHD diagnosis as an adolescent. I will analyze the measures I assumed and employed ensuing the identification of my learning disability through notions of care of the ‘self’ and intentional self-adjustment (Foucault 1997a; 1997b), cybernetic circuits of inflection, and cruel optimism (Berlant 2010). I intend to apply my own subjective progression to these relevant theories to better cognize the fundamentally extimate nature of the ‘self.’

I was diagnosed with ADHD when I was seventeen years of age, only months away from completing my secondary school education. For the duration of my life prior to this psycho-medical pronouncement, my academic performance had been consistently positioned in the above-average range. At school, I was frequently typified as an extroverted, semi-disruptive class clown; I regularly encountered difficulty in time-management, organization, and prioritization, as well as with extended durations of concentration or sitting idly. However, I kept pace with my peers at our academically rigorous International Baccalaureate school and did not recognize my dispositions as substantially disadvantageous. Upon affirming the impairment of my cognition, the psychologist proposed a series of techniques to manage my ADHD in personal and academic settings. Included in these propositions were exam-writing accommodations,

and, most consequentially, pharmaceutical intervention. I was introduced to Vyvanse –a stimulant medication commonly used to treat ADHD– which I now customarily utilize in academic environments. Although I securely completed two concurrent high school diplomas at a challenging school, unaided by accommodations or medications, in the years since, my scholarly success has become thoroughly implicated with my association to Vyvanse. The adoption of ADHD as part of my selfhood shaped my phenomenology: I understood my condition as a neurological impairment requiring remediation or counterpoising, which I responded to with pharmaceutical medication, in turn modifying my mode of being.

The incitement to begin using Vyvanse upon my identificatory introduction of ADHD is closely tied to Foucauldian notions of self-domination and freedom. Michel Foucault examines ideas pertaining to the cultivation of the self and personal advancement through his concept of “technologies of the self” (1997a). As the mechanisms and applications that bodies operate upon themselves to undergo processes of subjectivation as independent, individualist, and unique agents, technologies of the self are activated with a certain optimizing objective (Foucault 1997a, 225). These intentional self-constituting techniques are affectively, materially, and discursively unstable, contingent upon their historic-cultural contexts. The treatment of my ADHD with Vyvanse can thus be appreciated as a contemporary ‘technology of the self.’ Transformational practices of negotiating personal freedom are enacted through the “care of the self”: a critical attitude entangled with a self-knowledge

of one’s distinct mode of being and the relations of power which define it (Foucault 1997a, 231; Foucault 1997b, 287). Fundamental to this approach of exercising freedom and mobility within existing power relations is the means by which the subject comes to know itself.

By agreeing to participate in the psychoeducational assessment that ultimately classified my cognition as disordered, I submitted to medical epistemology and its discourses. It was in this setting that I came to recognize myself as a psychologically impaired subject-form in need of amelioration. Thusly, modern medical discourse mediated my self-knowledge. With this newfound perception of my consciousness, albeit externally realized, I became compelled to enlist Vyvanse as a technology of the self (Foucault 1997a, 237). I accordingly embraced this medication in the name of transforming myself into a subject with an improved capacity for systematicity, attention, and productivity. As a technology for the care of the self, Vyvanse stimulated my sensorium, affecting my brain to discipline my impulses and hyperactivity.

In a contemporary context, notions of technologies of the self as the means for transformational self-enhancement are immersed in neoliberal ideology that presents itself with excessive, unlimited promises of the potential for self-determination. Natasha Dow Schüll discusses cybernetic modification to problematize western neoliberal conceptions of the ‘self’ as defined, autonomous, and telic; the narrative of this ontological paradigm is merely a technological construct. Subjectivity is constituted and constantly modulated by a dialectical relation between self-intervention and docile

dependency, existing in cybernetic feedback loops of interaction with external dispositives (Schüll 2006, 233). As an emergent property of such circuits, the ‘self’ dwells in perpetual inflection, pursuing an equilibrated state rather than purpose-driven advancement.

I intentionally began taking Vyvanse for the function of transcending my impetuousness, distractibility, and inefficiency that I came to accept as a truth of my actuality. Guided by the advice of my doctor, I intermittently administered partial doses of Vyvanse to my system for tasks and circumstances that required especial assiduity, occupation, and endurance. The drug quickly revamped my work-ethic, heightening my focus and effectiveness to unprecedented levels. My inclinations toward socialization and physical activity were inundated by an urgency to enter the “zone” that Vyvanse provided, suspending my natural affects and temporality within the rhythm of my schoolwork (Schüll 2006, 234-235). My understanding of this stimulant as highly advantageous within academic settings grew simultaneously with an intensified perception of its crucial role in my attention proficiency and in my accomplishment. Within this circuit of self-transformation, I found myself using Vyvanse with accelerating regularity. Apprehending a tolerance that my body had developed for the drug, I proportionately increased my dose (Schüll 2006, 236). The transformative emanations of Vyvanse altered my academic approach and general mode of being in a cybernetic fashion. What was inserted into my subjectivity as a ‘technology of the self’ to free myself of my spontaneity and disorderliness became enfolded into the affective modulation that fuels my neoliberal existence: one which interminably strives toward maintaining a

balanced conduct (Foucault 1997b, 287; Schüll 2006, 230). For this reason, I have remained thoroughly engaged with Vyvanse despite its considerably virulent affects.

My always-adjusting interaction with and submission to Vyvanse as connected to interminable cycles of technological interplays within the neoliberal conviction in self-maximization can be elucidated through Lauren Berlant’s model of “cruel optimism.” Through this concept, the affective and political complexity of one’s devotional adherence to codes of normativity are asserted as a mode of idealism. Cruel optimism is enacted through an enduring attachment to a given disposition that debilitates the realization of the very desires it endeavors to fulfill (Berlant 2010, 94). The subject capitulates to technologies of attrition with the distorted anticipation of phantasmagorical, telic self-fulfillment by the means of such technologies. As an affective structure of attachment, the fantasies of cruel optimism simultaneously license subjective survival and reinforce conventionality.

At the time of my diagnosis of ADHD, I was drawn toward Vyvanse as a pharmaceutical technology of the self that carried the potentiality of self-transformation into a well-adjusted individual who could productively and auspiciously complete tasks in a comprehensive manner. Although this practice of self-mediation appears to be conducive to my desires of academic success –the pills modified my temporality, enabling me to work at a desk for several consecutive hours– it is accompanied by toxic side effects. My usage of Vyvanse inflames my anxiety, suppresses my appetite, often spawns irritability and anti-sociality, and engenders insomnia. It transforms my subjectivity from outgoing

and excitable to tirelessly industrious and single-minded. Yet, I continuously return to this self-depleting practice with the moral and affective neoliberal fantasy of “the good life,” whereby the subject is endowed with a host of technologies through which self-mastery is sensed as feasible (Berlant 2010, 100; Schüll 2006, 240). Despite the palpable harms of my medication, my unconscious aspiration for advancement and prosperity configures an affective attachment to the specific aspects of Vyvanse that promise such ideals. As such, my cruel optimism towards Vyvanse cybernetically cycles through technological variations that forge my lived experience as constantly seeking stability (Schüll 2006, 233). In the modern context of neoliberalism, technologies of the ‘self’ function in a cyclical instead of unidirectional manner. Rather than intentional practices of care for oneself to cognize subjectivity-shaping power dynamics and gain transformational mobility within them, contemporary technologies of the self are bound up with unconscious desires that incite causal cyclicity (Foucault 1997b, 287). The subject is caught in a circuit of compulsions to return to and embrace subjectivity-shaping apparatuses –such as Vyvanse– in pursuit of neoliberal ideals of meritocracy and autonomous freedom.

In conclusion, my identification with ADHD as a teenager affected my subjectivity. The diagnosis of a neuro-behavioral deficiency causally implicated a necessary and significant repair in the form of prescription medication. My experience in attempting to counteract my ADHD-subjectivity can be used to address theoretical interpretations of the contemporary ‘self’ as inextricably shaped by its environment. I intentionally began to use Vyvanse for the purpose of transformational ameliora-

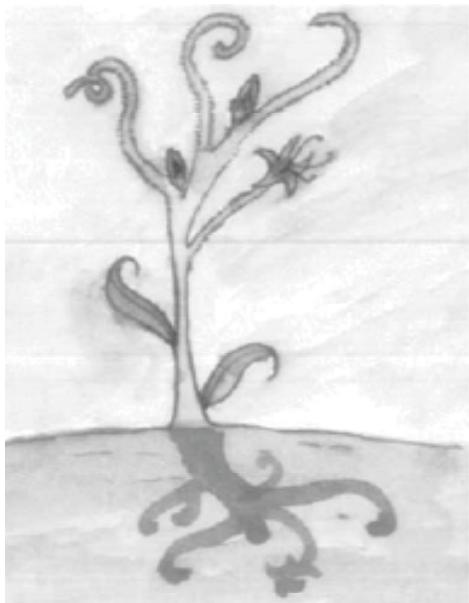
tion of my focus-directing abilities so as to succeed in school, corresponding to Foucault’s technologies of the self. However, as a subject caught in a circuit of subjectivity-shaping apparatuses, my capitalization upon Vyvanse functioned merely as another device contributing to my modulation toward a normative neoliberal self. Thus, despite its negative impacts, I continue to return to Vyvanse-use, adjusting my doses and frequency to correspond with my fantastic desire for freedom and upward mobility, a desire that drives subjectification. It is a method of striving for the ‘good life,’ an intangible, transcendental idea whose attainment taunts so many.

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Not Knowing

Viola Ruzzier



The Muhans knew everything. Every animal that lived nearby, every tree that provided them with shade, every particle of every atom of every thing around them – they knew about it. The Muhans had spent the past seventeen and a half millennia trying to understand how the world worked, and why this happened and that didn't, and how many times you could poke an iridescent fly-worm before it bit you, and things like that. There were no uncertainties, no unproven theories, no individual beliefs that went against what was known to be true. Knowledge was everything, and everything was known.

And then the plant arrived. It grew overnight in someone's backyard, and the next morning there was this little pale-yellow stem with a couple of curling green leaves timidly poking their tips out to see what

this world was like. No one had seen anything like it before, and everyone was excited – excited, but a little scared, too, for here was something new. A group of botanical specialists came, and counted the number of fuzzy, tentacle-like limbs that grew from the top of the plant and excitedly pointed to the pale pink buds that were forming between the tentacles. They wrote down measurements and observations and what kinds of shadows the plant produced, which somehow never perfectly matched its shape, and soon they knew a lot more about it than they had before. But they still didn't know everything.

Some people started to think it was an alien come from space. Others thought it a new god, and started to worship it as such. At least three people swore their grandmothers had had a plant just like it in their gardens, and if only their grandmothers were still alive the mystery would be elucidated. But no one knew where it had come from.

Meanwhile, the buds had blossomed into beautiful star-shaped flowers that gave off a sweet but unnamable smell. The flowers stayed open until one day they didn't, and they withered and fell off one by one, and soon it became clear that the plant would die, and with it, the possibility of knowing everything about it. So the Muhans watched helplessly as the plant grew, and aged, and wilted, and, as the first snowflakes fell, swayed gracefully to the ground to rest its tentacles on the frosted earth.

Modern Human Dispersal to South Asia in the Paleolithic Period

Louise Tremblay

For decades, archaeologists have debated over the ambiguous origins of anatomically modern humans in South Asia. The complicated colonial archaeological history of India, combined with a lack of hominin fossils found in the region, sub-par execution of excavations, and “poor chronological controls” (Petragna and Allchin 2007, 3) in past excavations have ultimately blurred our understanding of early *Homo sapiens* movements to and within the Indian subcontinent during the Paleolithic period. Two prevailing models for the dispersal of anatomically modern humans (AMH) out of Africa and into the Indian subcontinent occupy the center of the greater debate (Mellars 2006; Petraglia et al. 2012; Blinkhorn and Petraglia 2014; Haslam et al. 2010; Ambrose 1998; Athreya 2015). The first model, known as the “Southern Dispersal Hypothesis” (Athreya 2015, 62) or post-Toba model, suggests AMH achieved a single dispersal out of Africa and arrived in South Asia between 65 and 50 thousand years ago (kya), well after the eruption of the Toba volcano in 74 kya (Mellars 2006, 796; Blinkhorn and Petraglia 2014, 65). The second theory, known as the pre-Toba model, proposes that the arrival of AMH into the Indian subcontinent occurred sometime before the Toba eruption (Petraglia et al. 2007; Haslam et al. 2010; Petraglia et al. 2012). While neither of these models provide an unequivocal answer to the larger question of human dispersal to South Asia, the ev-

idence upon which the pre-Toba model is based is substantial and brings into question the reliability of the post-Toba dispersal model. In order to examine the validity of the principle alternative models presented for the arrival of AMH into the Indian subcontinent, it is imperative to evaluate the evidence used to support them. This includes archaeological, paleoclimatic, environmental, and genetic data. Based on the interpretive strength of these data, it will be demonstrated how the pre-Toba model disrupts the narrative of a single later dispersal to South Asia.

Model 1: A Single Successful Post-Toba Dispersal

This model for the single successful dispersal of AMH out of Africa and into South Asia suggests that a population of *Homo sapiens* left eastern Africa and arrived in southern Asia between 65 and 50 kya. This was during the boundary between the marine isotope stages 4 and 3, and well after the 74 kya super-eruption of the Toba volcano in Sumatra (Mellars 2006, Mellars et al. 2013, Blinkhorn and Petraglia 2014). The reason for this departure is connected to anatomically modern humans’ development of “newer adaptive advantages” (Oppenheimer 2009, 8) between 80 and 70 kya (Blinkhorn and Petraglia 2014) that were likely spurred by serious changes in the environment—possibly linked to the

widespread effects of the Toba super-eruption (Ambrose 1998). These advantages ultimately contributed to their exit from Africa and their swift migration along the South Asian coast (Oppenheimer 2009; Blinkhorn and Petraglia 2014). According to this position, the southern coastal route was taken as it would have provided greater access to water and marine resources required for survival than northern corridors which would have been far too arid at the time to sustain human life (Oppenheimer 2009). Accompanying these “newer adaptive advantages” (Oppenheimer 2009, 8) were cultural and behavioural adaptations which led to the development of a microlithic tool industry and ‘modern’ symbolic elements including the production of etched beads made of ostrich eggshell (Mellars 2006, 789, Mellars et al. 2013, 10701-10702). This “modern package” (Clarkson et al. 2012, 176) saw its development in Africa but was carried out to South Asia in exodus (Mellars 2006; Mellars et al. 2013). It is noteworthy that the pre-Toba model does not argue that there were no other dispersals of AMH prior to the southern coastal migration. For instance, there may have been an earlier dispersal of AMH to the north, reaching the Levant circa 130 to 70 kya, but there they failed to replace the Neandertal population occupying the region (Ambrose 1998; Lahr 1996). Therefore, the emphasis in the rapid southern coastal migration model is that AMH achieved a *successful* relocation to South Asia. According to Mellars, Europe was eventually colonized through a “later and secondary” (2006, 800) migration out of southern or western Asia.

Model 2: Pre-Toba Dispersal

Alternatively, the pre-Toba model sees a far earlier successful dispersal of *Homo sapiens* from Africa to South Asia. This migration occurred sometime before the Toba super-eruption in Sumatra circa 74 kya, within the marine isotope stage 5, likely by 78 kya (Petraglia et al. 2007; Petraglia et al. 2012; Haslam 2010; Clarkson 2014; Blinkhorn and Petraglia 2014). Blinkhorn and Petraglia argue that AMH travelled via “continental routes” (Blinkhorn and Petraglia 2014, 66), finding select habitable spaces in inland basins (Korisetar 2007), rather than along the coast. Furthermore, these populations arrived in South Asia with a Middle Paleolithic tool industry (Petraglia et al. 2007; Petraglia et al. 2012; Haslam 2010; Clarkson 2014; Blinkhorn and Petraglia 2014) and gradually developed an Indian microlithic industry *in situ* by approximately 35 kya (Petraglia et al. 2012, 131; Mellars et al. 2013, 10701) as a result of environmental pressures (Blinkhorn and Petraglia 2014, 66). According to Petraglia, this long and relatively uninterrupted continuity of lithic industries, visible before and after the Toba super-eruption, implies that “populations facing the Toba eruption were flexible enough” (Petraglia et al. 2012, 130) to overcome whatever environmental modifications may have occurred as a result of this natural event. This model also holds that it is likely populations spread from South Asia to Australia by 50-40 kya (Blinkhorn and Petraglia 2014; Athreya 2015), and Europe was populated only by a later dispersal of *Homo sapiens* from Africa to the north (Clarkson et al. 2012, 175).

Related Research

Archaeological Evidence

In order to support their hypothesis, proponents of the post-Toba migration model cite archaeological evidence from certain South Asian sites including Patne, located relatively near the west coast of India, and Jwalapuram in the Kurnool district of Andhra Pradesh in southern India, as well as Batadomba-lena, a cave site in Sri Lanka (Mellars 2006, 797). Archaeological assemblages found at these locations consist of a microlithic industry composed of crescentic and backed-segment stone tools (Mellars et al. 2013, 10701), as well as beads that demonstrate deliberate production and depict etched symbolic designs (Mellars 2006, 797). According to Mellars (2013), these collections of archaeological materials bear “striking resemblances” (2013, 10702) to assemblages found in southern and eastern Africa dating to 55 to 65 kya consisting of microlithic tools that were similar to the African Howiesons Poort industry. Mellars and others who support a post-Toba dispersal model argue that this evidence represents a “modern [African cultural and behavioural] package” (Blinkhorn and Petraglia 2014, 73), developed in Africa only after the Toba super-eruption, that was brought into South Asia as anatomically modern humans migrated out of Africa along a coastal route (Mellars 2006; Mellars et al. 2013; Athreya 2015).

Supporters of the pre-Toba dispersal model also consider the Jwalapuram site in the Jurreru Valley to be a significant source of evidence for their conclusions. Excavations at several localities of this site revealed Middle Paleolithic tool assemblages located in the stratigraphic layer beneath an ash layer deposited by

the Toba volcanic eruption determined to be the Youngest Toba Tuff (YTT), dated to approximately 74 kya (Petraglia et al. 2007, Petraglia et al. 2012, Blinkhorn and Petraglia 2014). Similar lithic assemblages were also found in the stratigraphic layer directly above the ash (Petraglia et al. 2007, Petraglia et al. 2012, Blinkhorn and Petraglia 2014). The assemblages of both layers differed only slightly in composition of tool types, and both contained Levallois and discoidal cores (Clarkson et al. 2012). Optically stimulated luminescence dating was used to obtain the date range of 77 ± 6 kya for the lithic tools located below the Toba tephra, and 74 ± 7 kya for those above the ash layer (Petraglia et al. 2007). According to Clarkson (2014), these tools were diagnostically Middle Paleolithic, rather than Late Acheulean. With this in mind, as well as the dates obtained for these assemblages, they were possibly the products of AMH rather than archaic hominins, indicating that *Homo sapiens* could have reached the southern region of the Indian subcontinent before the Toba eruption and remained there after the event (Petraglia et al. 2007, Haslam et al. 2010, Clarkson 2014, Jones 2010). Furthermore, excavations demonstrate that an Indian microlithic industry does not suddenly appear in the stratigraphic record as proponents of the first model would maintain. Rather, there is a gradual shift in the lithic sequence from Middle Paleolithic tool types to a microlithic industry that becomes fully apparent by 35 kya (Clarkson et al. 2012). This continuity and gradual tool evolution is taken to indicate a successful and continuous occupation of AMH in the Indian subcontinent, before and after the Toba super-eruption (Petraglia et al. 2007, Haslam et al. 2010, Clarkson et al. 2012, Blinkhorn and Petraglia 2014).

Paleoclimates and Environments

The study of paleoclimates and environments of the regions in question at the time of either a post- or pre-Toba dispersal can also be used to buttress the arguments made based on interpretations of recovered archaeological material. For instance, Blinkhorn and Petraglia (2014) discuss the shifts in climates between the marine isotope stages 4-3 (MIS 4-3) and marine isotope stage 5 (MIS 5) and the implications this might have had for migrating populations. Supporters of the post-Toba dispersal model argue that migrations of AMH populations from Africa to South Asia occurred sometime between the end of MIS 4 and the beginning of MIS 3 (Mellars 2006; Mellars et al. 2013; Oppenheimer 2009). For instance, Oppenheimer suggests that the arid climate of MIS 4 marked a “window of opportunity” (2008, 8) for migration. Alternatively, Blinkhorn and Petraglia (2014) assert that AMH pursuing a coastal route to South Asia during the MIS 4 would have certainly run into “the expanded Thar Desert [that] appears to have formed an arid barrier to dispersal” (2014, 69-70) during this stage. While these extreme arid conditions may have improved slightly by the onset of the MIS 3, there still would not have been a significant amount of water in this region to support human life and would have proven troublesome to traverse (Blinkhorn and Petraglia 2014). Conversely, during the earlier MIS 5, Blinkhorn and Petraglia maintain that the Thar Desert would have provided a useful continental corridor as it would have supported small river systems (2014, 69, 70), ultimately providing a relatively resource-rich ecosystem for AMH (Korisettar 2007; Blinkhorn and Petraglia 2014).

Furthermore, there are also questions surrounding the degree to that the Toba eruption would have affected populations living on the Indian subcontinent (and potentially reaching further to Australasia and Africa) during the YTT event (Williams et al. 2009; Williams et al. 2010; Williams 2012; Jones 2010; Petraglia et al. 2012). After analyzing marine pollen spectra containing YTT ash from the Bay of Bengal, as well as the carbon isotopic levels within various pre- and post-Toba soil samples, Williams and his co-authors (2009) dismissed Petraglia’s lithic evidence for hominin occupational continuity in South Asia presented by Petraglia and others (Petraglia et al. 2007, Petraglia et al. 2012, Blinkhorn and Petraglia 2014). Instead, Williams (2012) proposed that the Toba eruption would have devastated hominin populations living in South Asia, and the long-lasting and far-reaching effects may have eventually prompted cultural and behavioural adaptations in Africa that may have facilitated the move out for AMH (Williams et al. 2009, 312). Alternatively, Jones (2009) and Haslam et al. (2010) counter that Williams failed to establish a secure and credible chronological frame for the duration of the environmental and climatic shifts caused by the major volcanic event. This contributed to their arguments that not only might there have been a regional variance in the severity of the effects caused by the deposition of YTT ash (Jones 2010), but that its impacts may not have been all that severe (Haslam et al. 2010).

Genetic Data

Much of the genetic evidence presented on this topic emerges in support of the single successful southern dispersal of

AMH after the Toba eruption. According to Forster (2004), the mtDNA haplogroup L3 developed in Africa c. 60 to 80 kya, which eventually generated the nested founder haplogroups found outside of Africa, namely M and N. While N is seen in South Asia, Europe and the Levant, the somewhat older haplogroup M seems to only be detectable in South and East Asia, as well as Australia, but is missing in Europe and the Levant (Athreya 2014). This haplogroup distribution is perceived as evidence for a small, recently genetically delineated group of AMH migrating out of Africa and into South Asia and beyond via the southern coast, only to disperse to Europe and Levant via a later secondary dispersal (Forster 2004; Mellars 2006; Athreya 2014). The calculated coalescence of both haplogroups M and N is approximately 60 to 65 kya (Endicott et al. 2007; Blinkhorn and Petraglia 2014), indicating that AMH could not have arrived in South Asia prior to that time (Forster 2004; Endicott et al. 2007), thus dispelling the hypothesis for a successful pre-Toba dispersal event. Furthermore, Forster maintains that the post-Toba dispersal along the southern coast route would have occurred rapidly, otherwise mtDNA analysis of populations living along this coast and further to East Asia and Australia would have developed their own “region-specific haplogroups” (2007, 236). This rapid dispersal is argued to be supported by the *Homo sapiens* fossil remains located at Lake Mungo in Australia dated to between 40-50 kya, as well as the Niah Cave skull found in Borneo, dated to 40-42 kya (Blinkhorn and Petraglia 2014). Therefore, according to proponents of the first model, the only exit out of Africa that would have provided such a quick

dispersal would be via the South Asian coast. While genetic analysis may seem like a solve-all solution, there are several inherent issues with this method that go unaddressed by several supporters of the pre-Toba dispersal model (Blinkhorn and Petraglia 2014; Athreya 2014). These are challenged by Petraglia (2014) and other proponents of the post-Toba model and will be discussed in the following section.

Discussion

While it does not provide a complete solution to the question of anatomically modern human dispersals from Africa, the second model described above manages to shake the foundations upon which the Southern Dispersal Hypothesis is based (Athreya 2015, 62). First, they challenge the genetic data that currently occupies a central space in the debate. For instance, Blinkhorn and Petraglia (2014) note the possibility that populations possessing the mtDNA haplogroup L3, and eventual founder haplogroups M and N, may have successfully dispersed from Africa at an earlier date between c. 100-80 kya, but do not appear “in modern populations owing to extinction events or patterns of sampling” (2014, 65; Petraglia et al. 2012). Further supporting the somewhat ambiguous nature of genetic analysis, Endicott et al. (2007) claim that calculations for the coalescence of haplogroups M and N have significant error margins (2007, 235). These margins range from 57-87 kya, ultimately meaning that an earlier successful dispersal of these haplotypes cannot be ruled out (Petraglia et al. 2012, 131 Athreya 2015).

Secondly, advocates of the pre-

Toba model question the legitimacy of the “Microlithic First” position maintained by Mellars (Clarkson 2014), in which migrating AMH would have arrived in South Asia with microlithic industries. As such, the stratigraphic sequences would demonstrate the direct replacement of lithic assemblages created by archaic hominins, by a microlithic material culture (Petruglia et al. 2012). Petraglia argues that the sudden occurrence of microlithic tool types in India without any precursors is unlikely. In addition, careful lithic analysis of the assemblages at the Jwalapuram site performed by Clarkson (2014) suggests an intermediate Middle Paleolithic industry utilized by AMH, appearing after a Late Acheulean industry and gradually developing into the Indian microlithic industry by 35 kya (Petruglia et al. 2012; Clarkson 2014). Simply put, proponents of the pre-Toba model denounce this diagnostic method of equating a particular lithic practice with a particular people (Blinkhorn and Petraglia 2014, 72). Finally, Petraglia et al. (2012) note that anatomically modern humans appear in Australia 10,000 years before microlithic tool types arrive on the scene in the Indian subcontinent, as evidenced by the burials of Lake Mungo, therefore indicating the likelihood that these tools were developed gradually, long after AMH dispersed into South Asia (2012; Endicott et al. 2007).

Considering the climate and environment that hominins would have faced during the Paleolithic period, particularly in regards to the effects of the Toba super-eruption, Petraglia and others, including Jones (2010) and Haslam et al. (2010), argue against Williams’ (2012) conclusions that the volcanic event would have had a widespread

and extreme impact on the surrounding ecosystems—including the hominins who may have occupied them—and the global climate (Williams et al. 2009). Furthermore, the climatic chronology established by Williams via pollen and carbon isotopic soil composition analyses is still far too imprecise to claim for certain the devastating impact of the Toba super-eruption (Williams 2012; Petraglia et al 2012), thus, nullifying—at least for now—the power of this argument against a successful pre-Toba dispersal and continual occupation of AMH in the Indian subcontinent.

The pre-Toba dispersal model is not a perfect solution to the questions surrounding the initial migrations of *Homo sapiens* out of Africa and into South Asia, and its weaknesses warrant discussion. Most significant is the reliance on the comparative analysis of lithics found at the Jwalapuram site as the foundational evidence for this hypothesis, despite certain depositional and diagnostic concerns raised by Mellars et al. (2013), Williams et al.(2010), and Oppenheimer (2009). More specifically, these scholars signal the possibility that the Middle Paleolithic assemblages identified at this site may have been located in secondary contexts, meaning the dates obtained for them would only correspond to their re-deposition, rather than a more accurate date which would reflect initial deposition (Williams 2010). Moreover, this apparently Middle Paleolithic assemblage bears similarities to the Late Acheulean industry that preceded it and could have been the product of an archaic hominin species rather than AMH (Athreya 2015; Blinkhorn and Petraglia 2014). The reliance on debatably diagnosed Middle Paleolithic material assemblages only

offers vague support for the hypothesis for AMH in South Asia prior to the Toba super-eruption. Furthermore, utilizing these tool industries as proxies for people fails as adequate archaeological reasoning.

The question of the initial dispersal of anatomically modern humans out of Africa and into South Asia and the role the Indian subcontinent may have played in such a migration is particularly challenging to answer. In attempting to provide a solution to this question, both models presented here agree on the timing of the emergence of AMH in Africa but fail to agree on the sequence that led to their eventual dispersal, the route taken during this exit, the climatic and environmental shifts that may have impacted their movements, as well as the cultural and behavioral adaptations brought with them to new territories. While archaeological, genetic, paleoclimatic and environmental data can provide significant evidence detailing the arrival of anatomically modern humans in South Asia, the recovery of *Homo sapiens* fossil remains in the Indian subcontinent, of which there is a frustrating absence, is ultimately required to confirm or deny the current hypotheses (Athreya 2015).

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« WHY IS IT A BAD THING TO BE A PUTA? »

Regard critique sur la prostitution hétérosexuelle au nord-est du Brésil

Savannah Dubé

« We are never going to get the common person to call us anything else. To try is a waste of time and resources. What we need to ask is ‘**Why is it a bad thing to be a puta?**’ That puts the finger squarely into the wound, doesn’t it? There’s no getting around that question unless you want to be a moralist, and once people openly assume their moralism.... ah, then we can talk and maybe change minds! »

– Gabriela Leite, prostituée et activiste brésilienne (Blanchette & Da Silva 2018)

Bien qu'il s'agisse du pays avec la plus grande économie en Amérique latine et d'un acteur mondial de plus en plus important, le Brésil est caractérisé par de grandes inégalités raciales, sociales et économiques (Williams 2013). La pauvreté est endémique à travers tout le pays, particulièrement au nord-est (Ferreira Filho et Horridge 2006). Longtemps confrontés à des enjeux de pauvreté et de chômage élevé, les gouvernements du nord-est du Brésil ont vu une opportunité de s'attaquer au problème en profitant des longues plages paradisiaques de la région pour y développer une industrie touristique. Depuis les années 1980, le tourisme a pris un essor considérable pour devenir un des secteurs d'activité essentiels de l'économie de la région (Andrade et al. 2017).

En marge du tourisme international, une industrie parallèle de tourisme sexuel et de prostitution s'est établie dans

la région (Piscitelli 2007); un haut taux de prostitution caractérise le nord-est du Brésil. Face à cette réalité, les gouvernements locaux se sont engagés dans une lutte contre le tourisme sexuel, la prostitution et la traite d'humains, des réalités enchevêtrées et associées uniformément à la violence (Carrier-Moisan 2013 ; Carrier-Moisan 2020 ; Williams 2013). Souvent, les travailleuses du sexe sont perçues comme des êtres ayant besoin de protection et comme étant « vulnérables » ou « à risque » (Williams 2013). Ces discours dominants effacent la subjectivité des prostituées (Blanchette et Da Silva 2018 ; Carrier-Moisan 2013 ; Williams 2013). En se concentrant sur les femmes prostituées¹ au nord-est du Brésil, cet article cherche à démontrer qu'elles ne sont pas seulement des victimes d'oppression, mais aussi des figures complexes, dotées d'agencéité et porteuses d'espoir. Cet article tente de répondre à la question suivante : qu'est-ce

1 Dans cet article, le terme « femme » fait référence à la notion de genre Euro-Américaine et non au sexe biologique d'une personne. Toute personne s'identifiant comme une femme est ainsi considérée comme une femme. Cet article se concentre principalement sur les femmes prostituées, non pas parce que les hommes ne s'adonnent pas à la prostitution eux aussi, mais parce que la prévalence de la prostitution féminine est beaucoup plus élevée. En outre, les études sur la prostitution masculine restent peu développées (Pliley 2018).

qu'être une prostituée dans le nord-est du Brésil?

Portrait de la prostitution hétérosexuelle² au nord-est du Brésil

Les définitions de la prostitution peuvent varier considérablement. Dans sa définition la plus large, la prostitution comprend l'échange de relations sexuelles contre de l'argent, de la drogue, un abri ou d'autres commodités (Weiner 2013). Lorsque l'on parle du travail du sexe au Brésil, il est toutefois intéressant de noter que la prostitution ressemble rarement à la prostitution au sens typique du terme. Bien qu'on observe parfois des transactions monétaires en échange de services sexuels, la majorité des activités de prostitution se dissimule sous les «relations» que les femmes locales entretiennent avec des hommes étrangers ou, plus rarement, avec des *caroas* locaux (hommes riches, généralement âgés et blancs). Par exemple, ces hommes payent les hôtels et les repas, subviennent aux besoins des femmes et leur offrent parfois même de l'argent en «cadeau», mais rarement donnent-ils explicitement de l'argent en échange de relations sexuelles (Carrier-Moisan et al. 2020; Williams 2013; Williams 2014). Pour reprendre les mots de Samuel Veissière : «the woman saves face by never explicitly asking for money, and the man saves face by never explicitly offering to pay» (Veissière 2018, 275).

Le travail sexuel étant considéré comme moralement répréhensible, les hommes et les femmes concernés évitent donc d'y faire référence (Veissière 2011; Williams 2013). Par ailleurs, la majorité

des femmes impliquées dans le commerce sexuel évitent de s'identifier comme étant des «prostituées» afin de limiter la stigmatisation qui accompagne le terme (Carrier-Moisan et al. 2020; Piscitelli 2007). Au Brésil, le terme «garota de programa» (traduit en anglais comme «call-girl») est employé plus fréquemment que «prostitua» (prostituée). Le terme «garota de programma» a une connotation plus positive que celui de «prostitua» et désigne généralement des prostituées «haut de gamme». Or, le terme fait aussi l'objet d'une stigmatisation (Williams 2014). Il est important de noter que la pratique de la prostitution des adultes elle-même est légale au Brésil, mais diverses activités connexes, telles que l'exploitation d'un bordel ou la promotion de la prostitution, sont illégales (Bureau of Democracy, Human Rights, and Labor 2006).

Plusieurs chercheurs en sciences sociales ont soulevé les limites des termes «prostitution» et «travail du sexe» pour expliquer les relations d'échanges complexes impliquant sexe, argent, affection et biens matériels. Cependant, en l'attente d'un terme plus adéquat pour capturer la véritable complexité de ces relations, ces termes restent couramment utilisés (Garcia et Olivar 2020). Dans cet article, le terme «prostitution» ne se limite pas seulement aux transactions monétaires en échange de sexe, mais il inclut également le large éventail de liaisons et de relations ambiguës dans lequel l'argent est impliqué indirectement.

Vers une juste représentation de la prostituée brésilienne

² Bien qu'au Brésil, le sexe entre les hommes n'est pas nécessairement considéré comme homosexuel, ici, l'hétéosexualité est définie au sens Euro-Américain du terme et fait référence aux relations sexuelles entre des personnes de genres opposés (Kulick 1997).

Il y a quelques décennies, Gayle Rubin (1984), aujourd’hui considérée comme l’une des pionnières de la théorie queer, a écrit un article révolutionnaire intitulé «Thinking Sex : Notes for a Radical Theory of the Politics of Sexuality» dans lequel elle argumente que la culture populaire est aux prises avec une hiérarchie sexuelle malsaine qui trace irrationnellement une ligne imaginaire entre le «bon» et le «mauvais» sexe. Le «bon» sexe, celui qui est sain et normal, serait composé de couples hétérosexuels, mariés et monogames qui ont des relations sexuelles à la maison à des fins reproductives. Quant à lui, le «mauvais» sexe, celui qui est malsain et anormal, serait celui dans lequel des travestis ou des transsexuels ont des relations sexuelles intergénérationnelles pour de l’argent. Selon Rubin, tout ce qui se trouve entre ces deux opposés, incluant la prostitution hétérosexuelle, est un territoire contesté.

Toujours pertinente aujourd’hui, l’analyse de la stratification de la sexualité de Rubin aide notamment à comprendre la stigmatisation entourant la prostitution. Le sexe est un domaine chargé de fortes tensions morales. Même la culture dominante au Brésil, pays qui a pourtant une réputation de «liberté sexuelle», traite toujours le sexe avec suspicion et n’échappe pas aux imaginaires de «bon» et de «mauvais» sexe dans lesquels la prostitution est regardée d’un mauvais œil. D’une part, on le remarque dans le fait que les prostituées brésiliennes, et les hommes qui

entretiennent des relations avec elles, essaient de se conformer autant que possible à l’idéal du «bon» sexe, en s’engageant dans des relations monogames qui n’impliquent pas directement de l’argent en échange de sexe. D’autre part, on le remarque aussi dans la couverture médiatique et la littérature scientifique : le travail du sexe au Brésil est principalement couvert sous l’angle de la violation des droits de l’homme, de la violence ou de la perspective des infections transmises sexuellement (Garcia et Olivar 2020).

Que quelques chercheurs examinent l’agencéité, le bien-être ou l’activisme des prostituées. Ces études plus «positives» se situent notamment en anthropologie (voir Blanchette et Da Silva 2012; Blanchette et Da Silva 2018; Blanchette et Da Silva 2016; Carrier-Moisan 2013; Carrier-Moisan et al. 2020; Gracia et Olivar 2020; Piscitelli 2007; Veissière 2018; Williams 2013; Williams 2014), faisant écho à une tendance récente dans la discipline : se concentrer sur le «bien» (Ortner 2018).³ En effet, la plupart des travaux anthropologiques sur la prostitution au nord-est du Brésil se sont, à juste titre, consacrés à donner une meilleure image du travail du sexe.

Les prostituées du nord-est du Brésil : des femmes polyvalentes aux multiples facettes

Une erreur courante dans les médias et dans la littérature scientifique consiste à

³ Dans «La face sombre de l’anthropologie», Sherry B. Ortner (2018) soutient que le problème des anthropologies du bien est qu’elles se sont majoritairement développées en s’opposant radicalement aux anthropologies sombres. Ici, l’anthropologie sombre, réfère à un type d’anthropologie qui aurait pris forme au début des années 1980 et qui se serait exclusivement concentré sur des questions d’inégalité, de pouvoir et de violence (Ortner 2018; Robbins 2013). L’anthropologie du bien se serait ainsi développée en contre-réaction au tournant sombre en explorant plutôt des questions comme le bien-vivre, l’espoir ou le changement.

à catégoriser uniformément toutes les prostituées, comme si leurs expériences en tant que travailleuses du sexe étaient, en quelque sorte, homogènes. Or, à l'intérieur du Brésil, l'expérience de la prostitution dans le nord à Ponta Negra est très différente de celle plus au sud à Rio de Janeiro. Chaque prostituée a un récit unique. Ce ne sont pas seulement des «prostituées»; ce sont aussi des femmes, des transsexuelles, des sœurs, des filles, des étudiantes, des travailleuses domestiques, des activistes.

En fonction de leurs situations, elles ont toutes des intérêts très différents –parfois contradictoires– qui les poussent à s'engager dans le commerce sexuel. Certaines femmes veulent un meilleur statut social, d'autres cherchent l'amour, d'autres veulent s'amuser et d'autres désirent s'enrichir, ou obtenir toutes ces choses à la fois (Carrier-Moisan 2012). Cette idée de la multiplicité des intérêts parfois contradictoires est bien véhiculée dans le travail de l'anthropologue Marie-Ève Carrier-Moisan. Carrier-Moisan relate les paroles d'*Isabella*, une femme qui s'identifie comme *garota de programa* à Ponta Negra : «I don't know what I want. Sometimes, I feel like I want to make money, money, money, money, but there are other times when the only thing I want is for him to come to sleep beside me, to call me *amor* (love), to give me affection» (Carrier-Moisan 2012, 84). En effet, Carrier-Moisan remarque qu'il existe souvent un brouillage entre les vies privées des prostituées et leur travail, particulièrement en ce qui concerne les sentiments (Carrier-Moisan 2012). Cependant, les femmes ne sont pas toujours en mesure de choisir entre vie privée et travail (par nécessité financière ou durant les périodes «mortes») (Carrier-Moisan 2012; Veissière 2018).

Activisme et «*putafeminismo*»

Le travail du sexe est un domaine lourdement chargé de dilemmes moraux. Il existe peu de données fiables et de recherches scientifiques impartiales sur le sujet, amenant le politique à primer sur la science dans les discours dominants. Trop souvent, on ignore les voix des travailleuses du sexe au profit des voix des «survivantes de la prostitution», des féministes radicales ou des travailleurs sociaux critiquant le travail du sexe. Mais cette situation n'est pas sans contestation : de nombreuses travailleuses du sexe s'engagent activement à protester contre l'image négative qui leur est attribuée (Blanchette et Da Silva 2018).

Depuis quelques années, une pensée intellectuelle et politique alternative est en pleine émergence au Brésil. Il s'agit du *putafeminismo*, qui postule que la lutte contre la stigmatisation sociale de la *puta* est une condition préalable nécessaire à toute lutte de justice sociale impliquant des travailleuses du sexe. Inspirées de la défunte Gabrila Leite, une grande activiste, sociologue et prostituée brésilienne qui avait repris le terme «*puta*» pour s'auto-identifier, les *putafeministas* cherchent justement à restituer ce terme. Les *putafeministas* luttent contre ce qu'elles appellent la «whorephobia» (la phobie de la putain), soit la peur, l'aversion ou la discrimination à l'égard des femmes qui vendent des services sexuels. Les *putafeministas* insistent sur l'universalité du «whoredom», afin que leur mouvement puisse rejoindre le plus de personnes possible. Tel que l'ex-prime Indianara Siqueria, une *putafeminista* et conseillère municipale, lors d'une manifestation :

« You're all whores too, you know. You

don't do what you do because you love your job or are loyal to your boss. You do it for money. Those of you who are teachers, you don't get out of bed at 5 AM and bus to work because you love your students and are dedicated to your profession: you do it for a salary. Sure, you may ALSO love your students and occasionally even your job. Even whores occasionally cum. But *dinheiro na mão, calcinhas no chão* [no money, no honey]. **Every worker is a whore and every whore, a worker** » (Blanchette et Silva 2018, 12).

Le *putafeminismo* et son «whoredom» sont des mouvements intéressants à examiner, car ils montrent la préoccupation fondamentale que partagent de nombreuses travailleuses du sexe : la légitimation de leur travail. Ils montrent aussi, une fois de plus, qu'elles sont bien plus que des «prostituées» et qu'elles sont des figures dotées d'agencéité qui doivent être invitées à parler pour elles-mêmes.

Conclusion

Alors que la littérature scientifique et les médias ont tendance à considérer les prostituées brésiliennes comme des «victimes», la littérature anthropologique, de son côté, a tendance à vouloir contrer ce mouvement en se penchant plutôt sur l'agencéité et le bien-être de ces femmes. Dans l'ensemble, chacune de ces femmes est unique, a des intérêts particuliers et sait comment naviguer dans l'adversité face à la stigmatisation du travail du sexe.

Malheureusement, le travail du sexe demeure un domaine d'activité largement incompris et trop souvent jugé comme moralement répréhensible. Bien que de nombreuses travailleuses du sexe militent

afin de changer ce phénomène, les idées de «bon» et de «mauvais» sexe restent profondément ancrées dans la culture populaire. On observe d'ailleurs depuis quelques années plusieurs campagnes de sensibilisation dans le nord-est du Brésil voulant «sauver» les femmes de la prostitution (Blanchette et Da Silva 2012; Carrier-Moisan 2013; Williams 2013). Cette rhétorique refuse systématiquement l'agencéité des travailleuses du sexe et met ces femmes déjà marginalisées dans une position qui les stigmatise encore plus. Ironiquement, les prostituées brésiliennes jouent un rôle nécessaire dans l'industrie du tourisme de la région et apportent, tout de même, d'importantes retombées économiques (Piscitelli 2007). De ce fait, au cœur de la «lutte contre l'exploitation des femmes» au nord-est du Brésil se trouve une profonde incompréhension de ce qu'est la prostitution, de ses impacts et de ce qui motive les femmes à s'y engager. Dans cette optique, le *putafeminismo* et une meilleure compréhension de la prostitution sont essentiels à l'amélioration des conditions des prostituées brésiliennes.

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“Into the Minds of Anthropologists”

Anthea Fleming

Ghostly Subjects and the Queer Affordances of Grave Goods

A queer analysis of the Tomb A71S ceramic assemblage from Bab-edh-Drah

Oscar VG Chisholm

Introduction

In this paper, I address an anxiety, an ethical problem which often appears unresolvable; I address the possibility that grave goods actively continue to hold affordances for the subjects they are interred with, even if we cannot perceive these affordances. Indeed, I argue that the paradigms we deploy which assume this possibility impossible may well be exactly what impedes our perception as archaeologists. Therefore, addressing this anxiety, these ghosts, also involve addressing the narratives that are spun to placate the situation and erase this ethical problem.

For my analysis, I use a queer phenomenological approach, because in many ways, this anxiety is a queer one. It is a possibility which falls outside the dominant narrative, but is nevertheless present and ought to be engaged with. Sara Ahmed, citing Judith Butler, delineates how a heterosexual background shapes itself by renouncing the possibility of homosexuality (Ahmed 2006, 87). Here, a parallel can be made, to say that the scientific environment shapes itself by renouncing possibilities deemed ‘supernatural’, more often with the more diminutive implications of the term ‘religious.’ In my theoretical framework, I outline how these possibilities truly exist and why they ought to be addressed.

Theoretical Framework: The Presence of Ghosts and Queer Affordances

A queer phenomenology orients us to objects which appear to be deviate or deviant (Ahmed 2006, 3). In this paper I address the ghostly affordances of these grave goods as queer because they emerge from recognizably deviant forms of relating to these objects. A queer phenomenology rejects the normative assumptions that archaeological practice and discourse often operates on, where the meaning of objects is only relevant to no-longer-living subjects who ‘believed’ in them. Archaeology depends on the queerness of ghosts, and a representationalist notion which renders the discipline’s thinking as ‘straight’ to legitimize its methods and practice. Scholars of the material and ontological turns have stressed a “relational rather than essential constitution of the world”, whereby the meaning or properties of objects emerge from a variety of relationships, rather than being essential or imposed by the human mind (Conneler 2012, 8; Alberti & Jones 2013, 26). Conneler discusses the properties of matter as “interfaces”, and therefore emergent from interaction, which challenges dominant essentialist assumptions surrounding matter (Conneler 2012, 8). Furthermore, she argues that “tracing these connections reveals past worlds” (Conneler 2012, 9). By looking at properties of objects through this lens, it becomes possible to grapple

with properties which do not ‘exist’ to the scientific eye, because properties are subject-specific. ‘Affordance’ is a term that I use to refer to a group of these queer ‘properties’ in this essay. Affordance is a theoretical term defined by Gibson as being “a specific combination of the properties of [the object’s] substance and its surfaces taken with reference to an animal” (Gibson 1977, 67). He further specifies that “properties” is a term used here which goes beyond simply those “described in classical physics” (Gibson 1977, 67). It is important to observe properties as constituents of collective relationships, rather than as discrete elements, and a crucial part of that collective is the subject which these properties exist in reference to (Gibson, 1977, 68). Gibson challenges the subjective/objective dichotomy because it renders the subjective less real, and counter-argues that our realities are shaped by our subjective perceptions (Gibson 1977, 70). In this essay, I am reworking Gibson’s definition by replacing the ‘animal’ as the subject of reference, with the figure of the ‘ghost’. Gordon understands ghosts and hauntings from a sociological stance and sees haunting as “neither premodern superstition nor individual psychosis”, but rather a social phenomenon of consequence (Gordon 2008, 7). She argues that locating ghosts is “the paradox of tracking through time and across all those forces that which makes its mark by being there and not there at the same time” (Gordon 2008, 6). A dead subject inhabiting a tomb is as much present as it is absent, because absence also affects (Gordon 2008, 19). Once we perceive a tomb as being actively inhabited by ghosts, there is the likelihood that their possessions continue to afford something

to them as long as they are interred together. Furthermore, ghosts can continue to haunt these objects, in the gaps of understanding we create as archaeologists. By separating them from their original context and transforming them into media, despite our desire for preservation through this transformation, those absences maintain affect (see Figure 1).

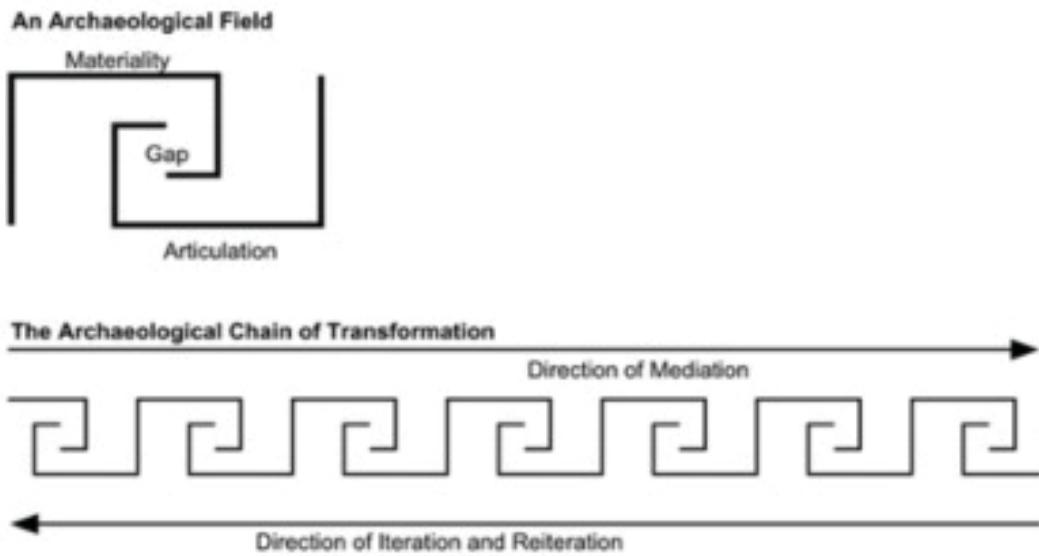
Robb discusses a “flow of action” whereby objects can act to further or interrupt human agency (2015, 167). Here, I explore the possibility that as an archaeologist opens, enters and excavates a tomb, they are interrupting a flow of action which is already taking place. Ahmed describes the moment of disorientation as a loss of stability in one’s inhabitation of space, where “the ‘loss’ itself is not empty or waiting; it is an object, thick with presence” (Ahmed 2006, 158). I argue we should engage with the possibility that as archaeologists we disorient and queer dead subjects by erasing the possibility of their presence. To put this engagement in practice, we must understand the narratives and factors which allow us to do so.

This theoretical framework of engagement is mobilized in this paper to the examine the affordances of tombs and pots for dead subjects. This examination is intended to disorient the archaeologist by challenging the normative discourses which are used to erase an ancient intentionality observable in the materiality of the tomb and its contents.

The Archaeologist’s Desire and Orientation

One reason that a queer phenomenology has applications in an archaeological context is its engagement with the

Figure 1: The gaps that exist in archaeological interpretation (Witmore 2007, 551)



“Figure 2: The transformation of the material world into media involves many small gaps (augmented from Latour 1999: 70, fig. 2.21: also Witmore 2004a: fig. 12)”

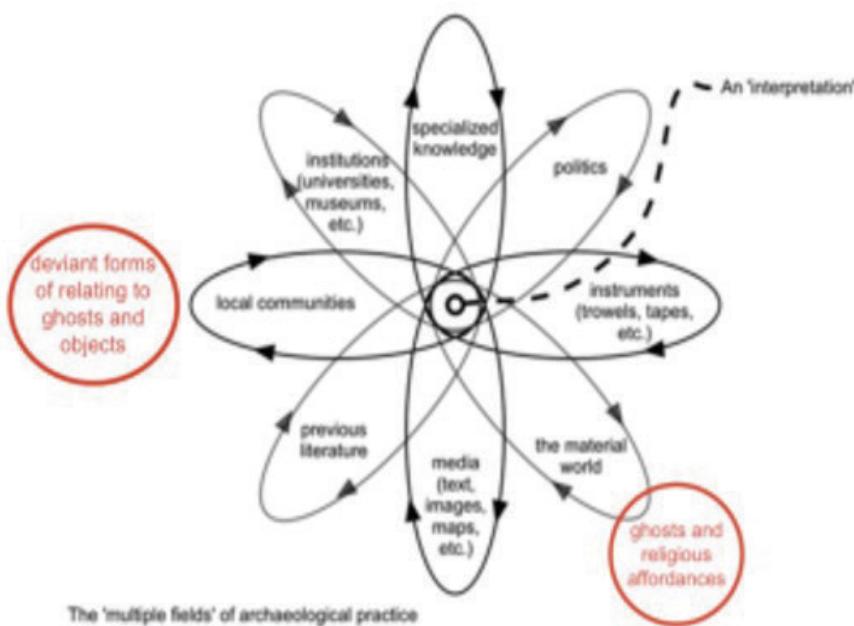
This model acknowledges that as the archaeological method translates material object into media or information, there is a gap in which information or understanding can be lost, because of the limits of the archaeological method itself.

issue of desire. Specifically, a queer phenomenology examines the desires of the archaeologist to acquire objects and information; “Desire involves a political economy in the sense that it is distributed: the desire to possess, and to occupy, constitutes others not only as objects of desire, but also as resources for world making” (Ahmed 2006, 115). I argue that the methods of archaeologists are shaped by a heterosexual background, to produce discourses aligned with heterosexual desires (Ahmed 2006, 70). Ahmed defines a background as something which can be spatial or temporal, and which gives form to objects, or to the entire foreground (2006, 38). Alignment is a process by which two

narratives or discourses parallel each other; not only in function, whereby discourses work to legitimize certain relations of power, but also in form, wherein narratives mimic each other or work metaphorically to legitimize each other. Alignment is a process which renders some objects or bodies legitimate, normal and “reachable”, while it queers others (Ahmed 2006, 112). When aligned with a heterosexual background, archaeological desires appear natural and correct, and deserving of satiation (Ahmed 2006, 87).

Ahmed discusses consciousness as intentional, and directed toward something (Ahmed 2006, 27). Jordanian archaeologists express their own orientation toward

Figure 2: The archaeological construction of interpretation (Witmore 2007, 551)



"Figure 1: What is gathered behind an 'interpretation.'" (modifications in red)

Witmore seeks to delineate the myriad factors and objects involved in constructing archaeological interpretation. I have modified this diagram to include examples of factors, objects and subjects which are excluded or marginalized by the archaeological method.

information; Kersel and Hill state that "an artifact's contextual information can be more important than the object" (2019, 306). I argue that upholding the information an object yields as even more important than the object itself has significant consequences for how these objects are understood. Archaeologists approach artifacts as opportunities for knowledge, attainable by the methods which they are familiar with. If consciousness is "embodied, sensitive, and situated," then archaeologists are only primed to be aware of the information that can be attained by their methods. The archaeologist is therefore likely ignorant and not concerned with any phenomena which are not observable through their method-

ological practice (Ahmed 2006, 27)(see Figure 2). This is problematic because the archaeologist becomes neglectful of any phenomena, ghosts, affordances, or interests which might haunt their acquisition of information.

"Orientation is a powerful technology insofar as it constructs desire as a magnetic field: it can imply that we were drawn to certain objects as if by a force of nature: so women are women insofar as they are orientated toward men and children" (Ahmed 2006, 85). By aligning with a heterosexual background, an archaeologist is an archaeologist insofar as they are drawn to the collection of information, and their practice is considered a natural result

of that desire.

Queer Critique of the Biographical Model's Application to Jordanian Grave Goods

The biographical model is a prime example of this alignment with the heterosexual background. The biographical model is an analytical tool used to examine objects as social actors, by metaphorically applying terms such as ‘life’ and ‘birth’ to reflect an object’s introduction to different social contexts, and ‘death’ to refer to

the end of their participation in a society (Holtorf 2002). Joyce argues that “object biographies can impede our understanding of how objects work in the manner of things, instead personifying them in the manner of humans” (Joyce 2015, 21).

Stutz et al. (2013) have deployed a biographical model in discussion of the looting of Jordanian cemeteries, arguing that “excavating and looting force a rebirth or re-production of the object and an entirely new life course” (Stutz et al. 2013, 686). I posit that when discussing a tomb and its contents using the biographical

Figure 3: Photograph of the shaft of Tomb A76 (Lapp 1968, 13)



“Fig. 1 Looking down the shaft of tomb A76: entrance to east chamber right of meter stick; stones blocking west chamber visible at upper left of shaft base” (emphasis added)

model, wherein grave goods are ‘birthed’ into the archaeological record or the illicit antiquities market, it aligns this process with a heterosexual background, rendering the tomb as a womb.

Ahmed argues that the heterosexual line creates the sexist assumption whereby “women’s bodies [are] conceived of as ‘containers’ or as vessels that are ‘ready’ to be filled by men” (Ahmed 2006, 71). Once a tomb is seen as a ‘ready’ and ‘waiting’ container pregnant with information, rather than as a material expression of someone’s last wishes for their body and possessions, the ‘birth’ of those objects becomes predestined, and “decent” (Ahmed 2006, 70). When the archaeologist perceives the tomb as a body ripe for excavation, the ethical dilemmas of archaeology as a practice are obscured.

Discursive Alignment of the Tomb, and a Queer Spatial Analysis of its Structure

Here I analyse the tomb, namely the affordances of its space, structure and layout, to highlight the discrepancy between the intentionality perceivable in its construction, and the discourse mobilized to align with archaeological desire. Gibson writes about the earth as affording concealment or hiding, noting that the builders of the shaft tombs at Bab-edh-Drah clearly aimed to magnify this affordance, not only to conceal or hide these tombs, but seal them completely, to permanently prevent entrance to this tomb (Gibson 1977, 74).

In Lapp’s article on Tomb A76, Fig. 1 (see Figure 3) is a photo of the shaft above Tomb A76, with vertical walls, and in Fig. 63 (see Figure 4) of Schaub and Rast, a drawing of the shaft leading to A71’s tombs shows the same structure, approximately 1.5 meters deep (Lapp 1968, 13) (Schaub

and Rast 2003, 112). These shafts are clearly not built to make a safe descent possible. Furthermore, Lapp describes the shafts of the tomb as fully sealed with packed earth (Lapp 1968, 15). This indicates that these shafts were constructed with the purpose of hiding and isolating these tombs from the earth’s surface and the realm of the living.

In addition to the shafts, Lapp describes the base of these shafts usually containing “blocking slabs”, blocking anywhere between one to five chambers (Lapp 1966, 109). In Tomb A76, Figures 2 and 3 (Figures 5 and 6) show a layer of mortar placed over a sealing of packed chipped stone, which had “perfectly sealed” a hole — referred to as an “entrance” — into the tomb just less than a meter wide, “since the late fourth millennium BC” (Lapp 1968, 14).). Lapp notes that “[t]he most exciting experience of the dig came with the removal of a blocking stone from the first perfectly sealed chamber we discovered” (Lapp 1966, 110). This statement from Lapp’s account of the excavation shows the interaction of archaeological desire in relation with these tombs, and is haunted by the ghosts who were not considered in the researcher’s pursuit to satiate archaeological fetish. This desire influences a choice of wording which legitimizes that desire and its satiation as innate and natural to archaeology. Tiny holes, which in fact acted as final exits for those who buried the dead in these tombs, are named ‘entrances’ for the archaeologist. Slabs used to protect the tomb from invasion are referred to as ‘blocking’, as obstacles which hinder the archaeologist, rather than as the defenses of the dead subjects they were placed to protect.

A Queer Spatial Analysis of the Pots in the Tomb

Ahmed argues that an object is “shaped by the conditions of its arrival,” meaning that our perception of objects as phenomena is shaped by their spatial and temporal context, and our own conditions of arrival (2006, 40). As researchers, we come to know the pots of Bab-edh-Drah through their separation as objects dislocated from their tomb assemblage, encountering them as an isolated assemblage within the Redpath Museum. To understand what the pots would have afforded to dead subjects, I examine the spatial relationships they maintained within the tomb, alongside the human remains they accompanied, to observe how the pots were shaped by the conditions of their arrival to the tomb, rather than through the conditions by which they arrived in the museum.

However, Ahmed asserts that “objects and bodies ‘work together’ as spaces for action” (2006, 57). An archaeological drawing or photograph renders these objects, both human remains and their accompaniments, as passive rather than active, as they are perceived by the archaeologist. Archaeological methods are thus far not oriented toward the affordances or presences which I am trying to grasp. These methods do not afford “conjuring”, which in Gordon’s terms is “a particular form of calling up and calling out the forces that make things what they are” (2008, 22). To conduct a queer spatial analysis of this tomb’s contents also involves interacting with an absence of information, media which are haunted by what has not been considered, or what has been erased (see Figure 1).

Schaub and Rast describe the buri-

als in Tomb A71S as “disarticulated, with six skulls grouped in a semicircle around the bone pile”, and in Figures 64 and 65, (see Figures 7 and 8) we see that the pots are similarly oriented in a nearly full circle around the human remains (Schaub & Rast 2003, 110). Ahmed states that “to be orientated around something is not so much to take up that thing, as to be taken up by something, such that one might even become what it is that is ‘around’. To be orientated around something means to make that thing central, or as being at the center of one’s being or action” (2006, 116). If we apply this statement to the arrangement of the pots in proximity to these entombed individuals, we see the potential for these pots to act as a constitutive element of their subjecthood in death, to afford a maintenance and production of that subjecthood.

Gregoricka et al. suggest that there is very little evidence for socioeconomic stratification in the Early Bronze Age of the Dead Sea Plain, supported by evidence of non-administered, collective storage (2020, 322). They go on to suggest that “the arena of death permitted one of the only avenues in which family groups could display and maintain social capital, power, and/or authority that differentiated them from other kin based groups at the site” (Gregoricka et al. 2020, 322). This is an interpretation which is predicated on a representationalist notion, assuming that these objects could at most be ‘displaying’ some form of social meaning. But once one is open to the possibility that these objects were actively affording something to who they were buried with, this interpretation could support the assertion that these objects were crucial in constituting the person who they were buried with, in death.

Figure 4: Sections of the shaft of Tomb A71 (Schaub and Rast 2003, 112)

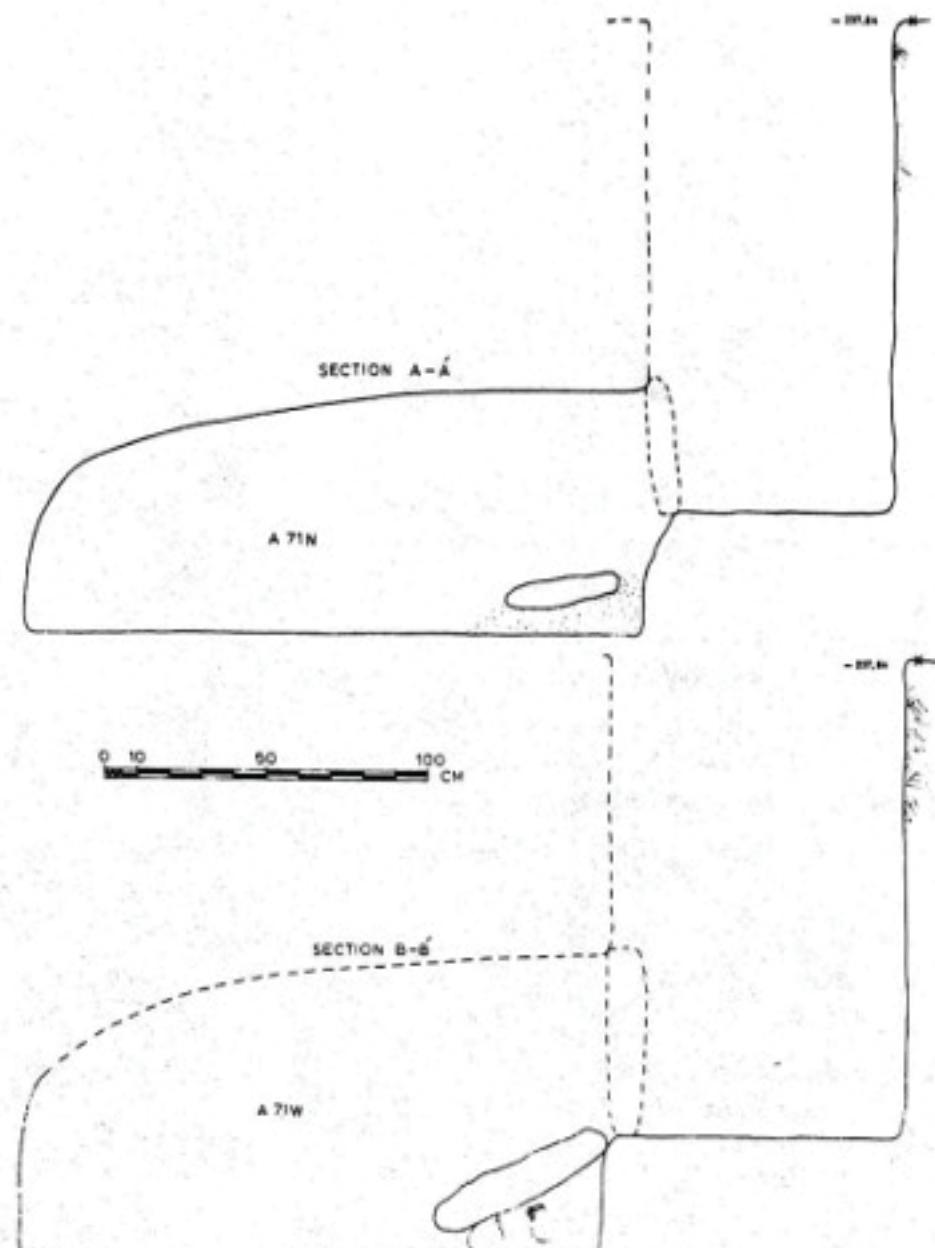
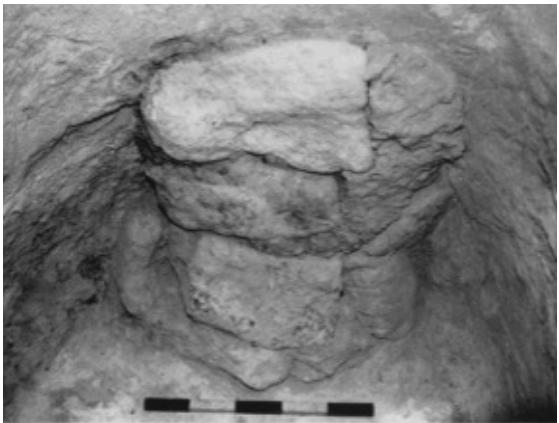


Figure 63. Sections of Tomb A 71N, W.

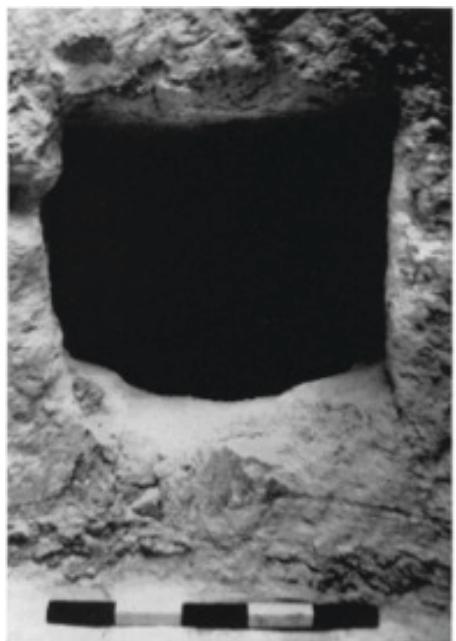
“Figure 63. Sections of Tomb A71N, W”

Figure 5: Chipped stones sealing Tomb A76 (Lapp 1968, 14)



“Looking east at blocking of east chamber to Tomb A76 after outer coating of mortar had been chipped away; blocking had perfectly sealed chamber since late fourth millennium B.C.” (emphasis added)

Figure 6: Exit of Tomb A76 (Lapp 1968, 15)



“Looking east at entrance to east chamber of Tomb A76 after removal of blocking; meter stick rests on floor of shaft in place where blocking stones rested” (emphasis added)

The Role of the Jordanian Nation-State as Patriarch

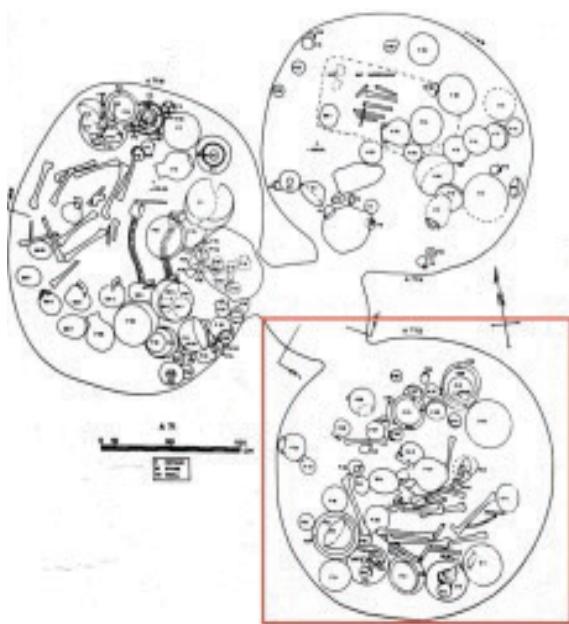
Archaeologists like to differentiate themselves from looters in that they pursue their desires only so long as they have permission to do so. However, that permission is not requested from the tomb’s inhabitants; the archaeologist makes the presumption that the inhabitant no longer own the tomb, because that tomb is inherited by the Jordanian nation-state. This presumption neglects the presence and interests of dead subjects in the excavation of their own tombs.

Ahmed discusses the concept of a ‘life-debt’, by which the child ought to reproduce to repay the gift of their own life to their parents, making one’s sexuality of familial and social importance (Ahmed 2006, 21). She brings forth an example of one of Freud’s patients, and how her homosexuality was treated as an issue of familial importance, where the father’s interests in the sexuality of his daughter are considered more important than her own (Ahmed 2006, 73). With this in mind, I argue that the Jordanian nation-state parallels the role of the father in this case, as the state acts as the legal owner of all land within its borders. Entities which occupy space within these borders now owe a ‘space-debt’, and a tomb pays this debt by ‘reproducing’ resources for worldmaking.

The Jordanian government has certainly concerned itself with protecting the ancient cemeteries within its borders, insofar as these cemeteries can provide resources for worldmaking. Kersel and Hill state that “whether the response is training and outreach, law and policy, or archaeological fieldwork, the goal is the same: the protection of Jordanian cultural heritage through a curtailing of archaeological loot-

ing” (2019, 324). This statement shows that the government is not concerned with preventing the desecration or disturbance of burials for the sake of the burials themselves, but is instead interested in asserting control over ‘cultural resources’ which produce something *for* the state. The Jordanian Department of Antiquities “buy-back” program, and the Jordanian Provisional Antiquities Law no. 12 of 1976, which “made it illegal to trade in antiquities, forcing local Jordanian looters and dealers to find other markets for the material,” are both evidence that the Jordanian government’s initiatives are aimed at protecting *against looters*, rather than protecting tombs and cemeteries from any disturbances (Kersel & Hill 2019, 311; Stutz et al. 2013, 681).

Figure 7: Plan of the chambers of Tomb A71 and their contents (Schaub & Rast 2003, 113)



“Plan of Tomb A71N, W, S.” (modifications in red)

Disturbances by the archaeologist are legitimized and encouraged, and distinguished by those disturbances caused by the looter. In this model, the archaeologist becomes the sole suitable suitor, approved and supported by the figure of the patriarch, as one who can procure information from the tomb ‘correctly.’

The Figure of the Looter as Queer

Archaeologists go to great lengths to distinguish themselves from looters, with “the need to excavate [...] tombs scientifically,” becoming the main criterion of difference (Lapp 1966, 104). The way that looters are figured as detrimental is centered on the “negative effect on understanding the past” and sees the unrecorded excavation of burials, destruction of human remains, and sale of grave goods in the marketplace as “ultimately mean[ing] a loss of knowledge” (Kersel & Hill 2019, 307)(Stutz et al. 2013, 677). Archaeological discourse expresses a concern with looters making it to the tombs before the archaeologist, and thus becomes ignorant of its own desecration and destruction of tombs.

Looters, on the other hand, do not emphasize such a distinction between themselves and archaeologists, and in fact, “looters come to view archaeologists as looters themselves, but looters who operate above the law” (Kersel 2007, 91). Politis has argued that the “unemployed local population [of Jordan] has, over the years, taught itself the skill of tomb robbing to survive” (2002, 259). Politis looks at the ways in which governmental action, taking form in the militarization or dispossession of land, has led Jordanian people to loot as a way of providing for themselves (2002, 257).). Similarly, Kersel outlines how “the

"the motivations for looting involve notions of nationalism, the forces of globalism, conflicting preservation and management plans, colonialism, and long-entrenched traditional practices" (2007, 82). In Figure 1 (see Figure 9), Kersel demonstrates how demand for looting is created by actors occupying the top of the pyramid, such as museums and antiquities collectors overseas (2007).

Looting is often provoked and comes about via the same institutions which support the archaeological project. This means that looters do not operate outside archaeology, but through a "perverse" form of meeting the same demand that is created by archaeological desire (Ahmed 2006, 78). Looting becomes queer and wrong when operating against an archaeological background.

Practices termed 'looting' appear worldwide and are likewise operationalized through the archaeological background described in this paper. Kersel talks about traditional looting practiced by Bedouins and others in Jordan as practiced with a "set of specific rules of conduct, superstitions, and knowledge that is handed down through the generations" and goes on to draw parallels between these looters and *tombaroli* in Italy (Thoden van Velzen 1996; Thoden van Velzen 1999) or *huaqueiros* in Peru (Smith 2005; Kersel 2007, 88; Kersel 2007, 90).

Archaeologists in Jordan have already profited immensely from knowledge given to them by looters. Archaeologists and looters operate as integrated communities, despite a dominant conception of existing in opposition to each other; Lapp called on "local experts", who were suspected looters, to help him locate tombs (Stutz et al. 2013, 680). If archaeologists truly desire to understand burials holisti-

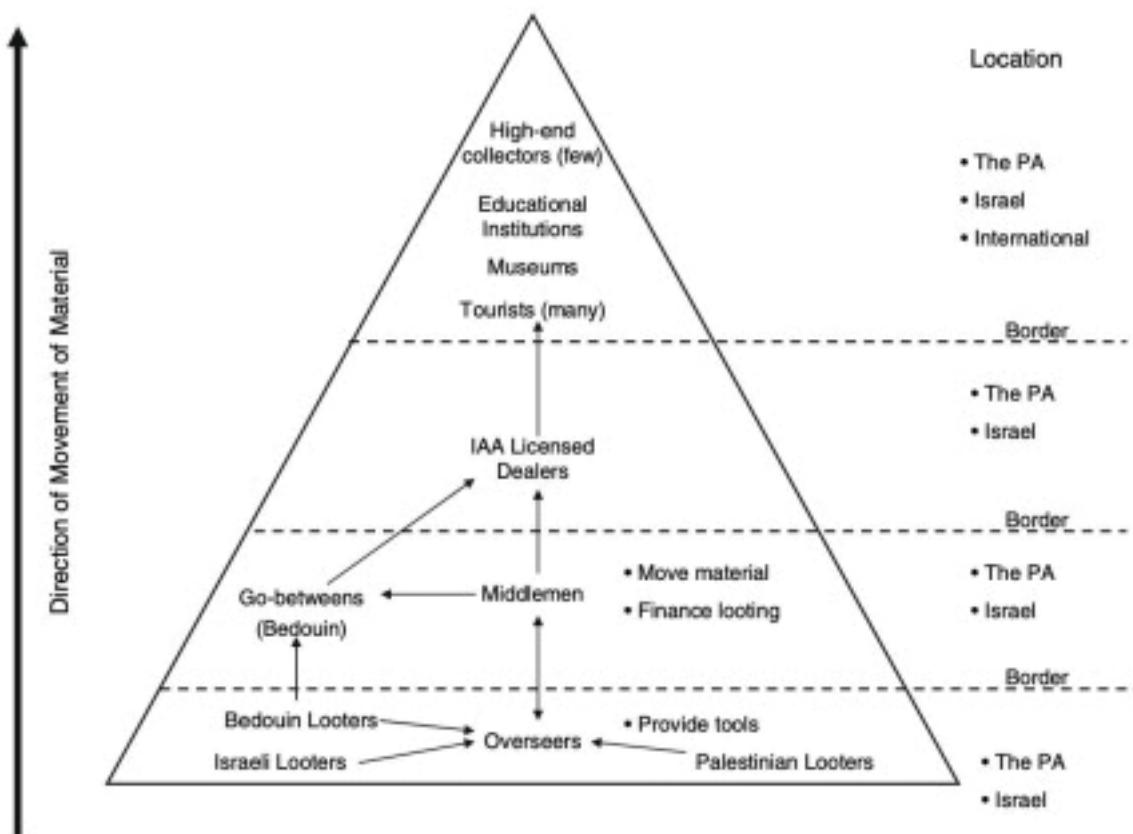
cally, I argue that they ought to challenge their own reservations about looters, and consider the ways by which looters can inform archaeological practices. Furthermore, I argue that the archaeologist needs to look beyond their own ontology to ethically engage with human remains and the possessions of dead subjects. Looter communities possess means of acquiring archaeological material, in acknowledgement and congress with ghosts who are present, whereas archaeologists depend on a narrative which erases the possibility of a ghostly existence, in order to carry out their work within an archaeological background shaped by heterosexual desires. Our ineptitude at addressing these ghosts

Figure 8: Photo of pots arranged around the human remains of Tomb A71S (Schaub & Rast 2003, 114)



"Figure 65. Tomb A71S looking south at bone pile and surrounding pottery vessels."

Figure 9: Pyramid of demand for looting (Kersel 2007, 87)



“Figure 1. Israeli and PA looting pyramid (after Ganor 2003)”

is a result of “bloodless categories, narrow notions of the visible and the empirical, professional standards of indifference, institutional rules of distance and control, [and] barely speakable fears of losing the footing that enables us to speak authoritatively with greater value than anyone else who might...” (Gordon 2008, 21).

Conclusion: Possibilities

Gordon states that “a different way of knowing and writing about the social world, an entirely different mode of production, still awaits our invention” (2008, 21). By mobilizing a queer critique of archaeological practice and epistemology, I have aimed to disorient the archaeologist and dismantle

the narratives they depend on to legitimize archaeology as a discipline. In Ahmed’s words, “the point is what we do with such moments of disorientation, as well as what such moments can do- whether they can offer us the hope of new directions, and whether new directions are reason enough for hope” (2006, 158).

Both Stutz et al. (2013) and Gregoricka et al. (2020) have put forward evidence that could suggest the first shaft tombs on the Dead Sea Plain were secondary burials for the long dead, which means that there were prescribed ways of exhuming and interacting with these dead subjects and their belongings respectfully. Furthermore, many experts currently deal with the issues and possibilities I have

outlined, and have already been a major influence on the archaeological production of knowledge. However, their expertise has also been, to some extent, rejected due to the limitations we place on scientific investigation.

I do not argue for a complete disengagement with desires to learn from and interact with the dead; quite the opposite, I argue that we ought to enthusiastically seek out ways to engage with the dead, more holistically than we ever have.

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