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FOREWORD

Dr. Brian Spooner

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Foreword

The articles you will read in these pages are the works of the next generation of anthropologists. They represent the best from the current stage of student progress in the study of the human condition in the changing world around us, works of original research presented by juniors and seniors at our annual undergraduate research conference in March 2017. What we have selected for publication includes not only some of the projects from the lesser known parts of the modern world, but also work on our understanding of the past, both biological and evolutionary, and early historical, in the light of the present—how we came to be what we are today. Sharon Ashok investigates an episode in the history of research on a crucial phase in the evolutionary emergence of our species. Ivana Kohut writes about the organization of healthcare in a society (Cuba) that has been cut off from social change in the rest of the world for the last half-century. Jessie Lu tells us about the relationship between government and healthcare in the neglected eastern province of Uganda. From Emma McNamara we learn about work on the problem of how and when language began. In Arielle Mae Pierson's article we are introduced to research on the prehistory of the lower Mississippi Valley.

Anthropology is the global social science, the study of the human condition—biological, social, cultural and semiotic—from the evolutionary emergence of our species to the present. Before the modern interest in breaking down the boundaries between academic disciplines, anthropology straddled the natural and social sciences and the humanities. It was internally inter-disciplinary. Recently we have added a medical dimension that explores global health. Starting in the middle of the 19th century, before globalization, we set out to document and analyse human social life throughout the world and collect the data that would allow us to create a cultural map of the world, a map of human diversity. We understood diversity in historical terms, explaining why some communities had grown more than others and achieved more at various times. Soon after we began, in the 19th century, before the rate of change had accelerated to its modern pace, we learned that unlike the typical geographer's map our cultural map had to be continually updated. Not only did everyday social life on the ground continue to change at different rates in different places, but as we became better informed and our society became more complex, our ways of making sense of it also changed. We had to study not only change in general, but different types of change, the relationship between change in different parts of the world, and change in the relationship between them and us. Our project was made more difficult by the fact that the rates of change everywhere were uneven. Each community changed at a different rate. Each community had adapted to a different environment, developed an economy from the use of different resources, experienced different rates of population growth and become increasingly complex at a different rate from its neighbors.

Anthropology gained its place in the curriculum in the 20th century with the development of the idea of culture as an analytical concept that would liberate the global comparative study of different ways of life from the pseudo-biological concept of race. Race had implied inequality. Culture was neutral. In the part of their curriculum where they apply what they

have learned—their research—our students have been investigating how the unevenness of our global development so far is evening out in the current crucial phase of globalization.

Anthropology is at the center of the curriculum, keeping up with the modern world as it continues to change at an ever-accelerating pace. Our students understand how we got to where we are today, and are well prepared for whatever career they choose for the next phase.

Dr. Brian Spooner
Anthropology Undergraduate Chair

The History of Race in Anthropology: Paul Broca and the Question of Human Hybridity

Samantha Sharon Ashok

A B S T R A C T

Paul Broca (1824-1880) was a French physician and anthropologist whose belief in the polygenesis of human origins was inextricably linked to the question of human hybridity. His studies on hybridity began in 1857 after he observed leporids, the crossing of a hare and a rabbit. He applied his methods of analyzing these animal hybrids to mixed-race individuals, a task he believed would prove polygenism. His studies laid the basis for French anthropology and led to the founding of the *Société d'Anthropologie de Paris*, the world's first anthropological society. Broca's background in mathematics led him to expand upon methods of quantifying human difference and to develop new anthropometric measurements, which are arguably his greatest contribution to biological anthropology. Broca was one of the purest scientists of his age and exclusively relied on quantitative data. His portrayal in secondary literature does not demonstrate this fact and he is often considered a key figure in 19th century racist science. Drawing from the work of philosophers of science Georges Canguilhem and Ian Hacking, this paper argues that Broca develops anthropometry to classify mixed-race individuals, effectively creating the biological concept of a human hybrid, a type of person that did not exist before.

I N T R O D U C T I O N

French physician and anthropologist Pierre Paul Broca (1824-1880), the founder of French anthropology, was one of the most influential scientists of his time, and one of the key figures in mid-19th century debates on the origins of human difference (Schiller, 1979). Though Broca originally planned to pursue studies in mathematics, the unexpected death of his sister led him to a career in medicine (Schiller, 1979, 15). He did not, however, lose his appreciation for quantitative data and his trust in numbers, rather than assertions, was the hallmark of both his medical and anthropological careers (Blanckaert, 2009, 122).

As a young physician, Broca's primary focus was neuroscience and he is perhaps best known for discovering the area of the brain associated with speech production (Broca's area) and defining the eponymous disorder (Broca's aphasia) that results from damage to this area (Schiller, 1979, 192). Though he was initially bored by osteology, his interests in natural science and anatomy grew and he would go on to become one of the greatest craniologists and physical anthropologists of the 19th century (Broca, 1886, 30, 131; Schiller, 1979, 20)

One single event sparked Broca's interest in anthropology. In 1857, he was presented a leporid, the hybrid offspring of a rabbit and a hare. He became fascinated by the creature and it led him to study the hybrid offspring of humans. As a polygenist, Broca believed in the multiplicity of human origins and primeval human difference and maintained that this view was linked to the study of human hybridity. By proving that distinct species could produce fertile

offspring, Broca thought he could prove the polygenesis of the human races (Broca, 1877).

After realizing that discerning human hybrids from either pure race could not be based on hair or skin color alone, Broca developed a series of anthropometric measurements to systematically quantify human difference (Stocking, 1968, 57). He borrowed cranial measurement techniques from anatomists before him (such as Camper, Morton, and Retzius), and to them added not only measurements of the cranium, but of the entire human body (Broca, 1871; Broca, 1861a; Broca, 1862a; Broca, 1865). Though he preferred laboratory work on skeletal remains, Broca developed a field guide to measuring and analyzing human variation among living people and he dedicated an entire section to identifying hybrids (Carson, 2007, 100; Broca, 1865).

In developing these measurements to systematically categorize the offspring of racial crossings, Broca effectively created the concept of a human hybrid, following the models of philosophers of science Georges Canguilhem and Ian Hacking. Canguilhem argues that experimentation is key in forming biological concepts, whereas Hacking's idea of "making up people" asserts that the classifications that scientists impose on individuals can create entirely new types of people. By experimenting first with leporids and then measuring human hybrids, Broca was effectively quantifying their existence. Though his work on hybridity has been largely forgotten, Broca has left a clear impact on racial science and anthropology as a discipline.

This paper aims to discuss Broca in three ways. Although he dabbles in the humanistic fields of medicine and anthropology, Broca is perhaps one of the purest scientists of the 19th century, focusing primarily on numerical data rather than *a priori* assumptions. Broca is cast as a racist in most secondary literature, a judgment that does not demonstrate his results. Finally, Broca codified many categories and methods that still exist today to some degree.

Overall, this paper will: 1) give a biographical sketch of Broca; 2) describe the history of 18th and 19th century racial theories and introduce the key figures in the study of human origins and variation prior to Broca; 3) mention early attempts to systematically measure human difference; 4) explain Broca's research on hybridity in animals and in humans, which he believed illuminated the polygenist view of human origins; 5) introduce Broca's novel anthropometric measures and argue that they were developed to classify hybrids; and 6) connect Broca's study of hybrids and anthropometry with the work of philosophers of science, Georges Canguilhem and Ian Hacking to suggest that Broca effectively created the concept of a human hybrid.

B I O G R A P H I C A L S K E T C H

Pierre Paul Broca was born on June 28, 1824 to a Calvinist Protestant family in Sainte-Foy-la-Grande, Gironde, France (Schiller, 1979, 11). From childhood, he was an exceptional student, dedicating most of his time to his secondary education (Schiller, 1979, 13). As a young professor, his history lectures were his first foray into paleontology and what would later be called paleoanthropology. In August of 1840, he received a bachelor's degree in letters (history and

literature). Despite his father's wishes that he attend medical school, Broca secretly pursued a bachelor's degree in mathematics and was set to matriculate into the Ecole Polytechnique de Paris the year after receiving his degree (Schiller 1979, 14). Had it not been for the unexpected death of his 18-year-old sister, Léontine, Broca would not have decided to become a physician. Abandoning his previous desires to become an engineer, in October 1841, he began his medical studies at the Collège Sainte-Barbe in Paris' Latin Quarter (Schiller, 1979, 15-6; Brace, 2005, 144).

Broca worked as a *pion*, or prefect, in order to pay his way through medical school (Schiller, 1979, 18). But when it came time to compete for the *concours*, a qualifying examination for the one-year extern and multi-year intern positions, he was forced to resign from his position to focus primarily on his studies (Schiller, 1979, 20). At the Collège Sainte-Barbe, he took courses by leaders in the sciences, many of whom continued to influence him throughout his career (Schiller, 1979, 18). Broca was fascinated by the basic experimental sciences, but ironically found osteology to be rather mundane (Broca, 1886, 30). By the end of his first year, he was one of ninety students to study anatomy at the Ecole Pratique, setting him up for a career as a surgeon. He also received a position as extern at the *Hôtel-Dieu* to work under Dr. Philippe-Frédéric Blandin, a gifted surgeon and one of Broca's professors at the *Collège Sainte-Barbe* (Schiller, 1979, 23-4). After passing the qualifying exam for the externship in 1843, he was assigned to M. Philippe Ricord at the Hôpital du Midi to study venereal diseases (Schiller 1979, 25-6). Despite Broca's antipathy to the specialty, Ricord proved to be an excellent mentor (Schiller, 1979, 26). In 1844, he completed the qualifying exam for his internship and, much to his chagrin, was placed under the supervision of François Leuret at the Bicêtre, a mental institution. Broca was not yet aware of the impact that Leuret's work on comparative neuroanatomy would have on his future endeavors (Schiller 1979, 28-9). By the time he became Pierre Nicolas Gerdy's surgical intern at the Hôpital de la Charité in 1846, Broca had been an extern at a number of Parisian hospitals (Schiller, 1979, 32-4). No later than the spring of 1846, however, Broca was suspended from his internship, likely due to his burgeoning political interests and letters featured in the revolutionary republican publication, *Les Ecoles*, during the July Monarchy (Brace, 2005, 144; Schiller, 1979, 37). Despite this, towards the end of July 1846, Broca passed the *concours* for aide in anatomy at the top of his cohort becoming, at the age of twenty-two, the youngest to ever achieve this position (Schiller, 1979, 41). He was finally vindicated when, within the final months of 1846, he was offered an internship to work again with Blandin at the Hôtel-Dieu (Schiller 1979, 42).

Broca's intelligence and ambition were reflected in his success in his field and self-confidence. Before the end of 1847, his parents realized his full potential and allowed him to compete for the *agrégation*, the most prestigious and competitive public education qualifying examination (Schiller, 1979, 46). When, in February 1848, the July Monarchy fell and the Second Republic was declared, Broca, the young, self-proclaimed moderate was ecstatic and the revolutionary spirit was spreading among the interns (Schiller, 1979, 50-1). Mid-July 1848, Broca completed the prosector qualifying exam and was named prosector of the Ecole Pratique (Schiller, 1979, 58).

Broca began his work on cancer research under the supervision of Hermann Lebert, whose

1845 publication, *Physiologie pathologique*, was one of the first publication on the cytology of tumors (Schiller 1979, 59-60). Lebert stressed the importance of a “dynamic” approach to pathology, combining clinical, experimental, and microscopic analyses. He separated tumors into two categories based on their tissues. Broca, too, became heavily involved with cytology, likely due to his fascination with the microscope. He bought one of his own in 1847 and declared in 1849, “the microscope has replaced hypothesis by evidence. . . Any observation unconfirmed by the microscope must be regarded as null and void,” (Schiller 1979, 61). Before the microscope, cytology was based on assertions, but now the new tool microscope allowed for detailed analysis and precise measurement of microscopic organisms that was previously impossible. This marks the start of Broca’s dependence on instruments and quantitative measurements, hallmarks of his anthropological career. In 1850, he won the *Prix Portal*, an award given by the Académie de Médecine de Paris, for his 364-page monograph summarizing Lebert’s works, as well as Broca’s own discoveries, most notably, the identification of the venous spread of cancerous tumors (Brace, 2005, 144; Schiller, 1979, 62-3). His and Lebert’s microscopic work helped transform French pathology into a laboratory-based science (Schiller, 1979, 74). His doctoral dissertation entitled *De la propagation de l’inflammation -- Quelques propositions sur les tumeurs dites cancéreuses (On the spread of inflammation: Some Statements on tumours called cancerous)* was published in 1849 (Schiller, 1979, 78).

In the spring of 1853, Broca successfully completed the *agrégation* qualifying exam and was the first candidate to be selected as *agrégé*, the most prestigious title in higher education (Schiller, 1979, 88-9). The rest of Broca’s career is characterized by breakthrough discoveries and engagement with scholarly societies. Since the French Revolution, higher learning institutions had been stigmatized and, as a result, often operated in secret. Pre-1860, there were roughly a dozen biomedically-related societies in France; by 1902, there were 85. In the spring of 1847, 23-year-old Paul Broca became one of the greatest contributors to the Société Anatomique and was named a titular member in 1849 (Schiller, 1979, 90-1). His research, aside from cancer studies, was consecrated at this time to musculoskeletal disorders. In 1852, Broca was the first to identify the tissue modification that resulted in rickets, namely, a malnutrition-related dysregulation in ossification, for which he received a prize of 500 francs from the *Académie des Sciences*. His next topic was osteoarthritis, which he distinguished from other joint conditions and whose point of origin he identified as the synovial membrane. Additionally, he noted each of the modifications to blood vessels and connective tissue. He introduced the concept that cartilage, unlike hair and nails, was more similar to teeth and cornea and kept alive by diffusion of nutrients from nearby blood vessels (Schiller, 1979, 91-2). In 1850, Broca was elected Vice President of the Société. He then began work on muscular disorders, starting in 1847 with club-foot. His 1851 *Description of the muscles in a case of club-foot* refutes the claim that the malformity is the result of lesions of the central nervous system, and instead states that it is due to idiopathic muscle changes resulting in atrophy and eventual loss of muscle fibers. This made him the first to fully report on the pathology of muscular dystrophy (Schiller, 1979, 93-4). One of his later contributions was an explanation of the mechanism of blood clotting (Schiller, 1979, 107). His best-known publication in the late 1850s was *Traité des anévrismes et leur traitement*

(*On Aneurysms and their Treatments*), published in 1856. Today, the 931-page oeuvre is still the most comprehensive analysis of aneurysms and their treatment (Schiller, 1979, 106). After its founding by his colleague at the Ecole Pratique, Broca joined the Société de Biologie, the society to whom he presented his research until he began his studies on hybridity (Schiller, 1979, 112).

By the end of 1853, Broca was a well-known name not only in Paris, but abroad. He experienced contemporaneous success in his personal life. In July of 1857, he married Adèle Augustine Lugol and together they had three children, Pauline (1858), Auguste (1859), and André (1863). They lived in the same house at 1 Rue des Saints-Pères from 1857 until Broca's death in 1880 (Schiller, 1979, 125, 127).

Influenced by research conducted by American anthropologists Samuel George Morton and his student Josiah Nott, Broca began his studies on human hybridity which he presented before the Société de Biologie in 1858 (Brace, 2005, 148). Once the president of the Société de Biologie insisted for the final time that Broca stop his research, Broca founded the *Société d'Anthropologie de Paris* in 1859 and acted as Secretary/General Secretary until his death. The society was the first "anthropological" society in the world and effectively established its founder as the father of French anthropology (Schiller, 1979, 130-1).

The first year of the society was focused on the study of the hierarchy of races (Schiller, 1979, 137). Across the western world, mid-19th century scholars were tackling the question of monogenism and polygenism, or the unity or plurality of the origin of races. Broca aligned himself with the American polygenists, a viewpoint that pervaded every aspect of his research. One of the more important aspects of his work was the development of anthropometric measurements, for which Broca is credited with making physical anthropology a true science (Schiller, 1979, 150, 163).

Broca's next endeavor, and perhaps his most enduring legacy, was his work on the brain. Rather than focus on brain size as a measure of intelligence, like many of his contemporaries, namely Samuel George Morton of Philadelphia, Broca suggested that the shape of the brain ought to be considered instead (Schiller, 1979, 174). He studied the brains of individuals with disordered speech and found damage to localized parts of the brain (Schiller, 1979, 183). He discovered that a lesion in the left hemisphere is associated with articulated speech (Schiller, 1979, 192). On May 19, 1861, he presented a paper on aphasia to the Société Anatomique and in 1877 presented his final paper on this topic (Schiller, 1979, 197, 200).

In 1865, Broca was elected president of the Société de Chirurgie de Paris and he continued his work in varying aspects of science including physiognomy and public health (Schiller, 1979, 216, 219). His work gained popularity not only among academics, but within the public due to its focus on the hierarchy of races and French superiority (Schiller, 1979, 274). He took on a new administrative role; beginning in 1872, he published the periodical *Revue d'anthropologie* and by the end of 1876 he opened his School of Anthropology (Schiller, 1979, 275, 278). He became *un homme politique* (politician), running for a seat on the Senate. After one failed attempt, he was elected in

January 1880 with the help of the slogan, “France et Science!” (Schiller, 1979, 280). During his campaign, he was subject to intense mudslinging highlighting his most controversial contributions to anthropology and science and entirely avoiding the innocuous content of his career (Schiller, 1979, 281). As a senator, he effortlessly balanced medicine and politics and one of his most impactful pieces of legislation provided public high school education to all girls (Schiller 1979, 285).

In 1880, Broca was struck by a coronary thrombosis, a rare condition in the late-19th century (Schiller, 1979, 286). The condition began to wear him down and on July 7, just one week after his fifty-sixth birthday, he complained of pain in his left shoulder, but went about his day working with students at the hospital. The next day at the Senate, his severe chest pains forced him to return home where he died later that night (Schiller, 1979, 288). He was buried on Sunday, July 11, 1880 in the Cimetière de Montparnasse. His wife, children, and other family members are today buried in the same grave. Nearly 2,000 people were in attendance, a testament to his lasting impact on the fields of anthropology, anatomy, and biomedicine (Schiller, 1979, 289).

HISTORICAL BACKGROUND

Through the 18th and 19th century, monogenism and polygenism were competing theories concerning the origin of human races. Monogenism was “the doctrine of single origins” which was an interpretation of the Judeo-Christian Biblical origins story that named Adam and Eve as the original pair from which all mankind descended (Brace, 2005, 42). In contrast, polygenism was the doctrine of multiple origins whose interpretation of the Biblical account of human origins stated that Adam and Eve were the ancestors of the Jews, but the gentiles were descended from pre-Adamites, or people that lived before Adam (Brace, 2005, 39). Both theories had their own views on concepts such as race, types, species and hybridity.

The history of these concepts prior to Broca traces through some of the most prominent figures in 18th and 19th century natural history and medicine. The first iteration of the modern species concept was recorded in 1686 by English naturalist John Ray (1627-1705) who defined species as “a stable product of original creation and subsequent propagation” and used hybrid sterility to distinguish them (Douglas, 2015, 2; Brace, 2006, 86). Through the 18th and 19th centuries, the concepts of species, race, and type were used differently by monogenists and polygenists based on the assumed degree to which they admitted the influence of the environment in changing human form over time, and their views on the possibility of human mixture among different types. To some degree, each of the following figures influenced the formation of Broca’s polygenist views on species, race, type, and hybridity.

Carl Linnaeus (1707-1778), Swedish botanist and father of modern taxonomy, began to systematically classify living forms in his 1735 publication *Systema Naturae* (Douglas, 2008, 35-6; Brace, 2005, 24-5). He was the first to place *Homo sapiens* in the order Primates, breaking down the distinction between the human and animal world (Brace, 2005, 26). A strict creationist and monogenist, he drew from the work of French physician François Bernier (1620-1688) to under-

stand and systematize human biological variation. Using Bernier's four quadrants of the globe as the basis for his classification, Linnaeus established four varieties of human beings: *Homo sapiens europaeus*; *H. sapiens asiaticus*; *H. sapiens americanus*; and *H. sapiens afer*, corresponding to Europe, Asia, America, and Africa, respectively (Brace, 2005, 27). Linnaeus' *scala naturae*, or "Great Chain of Being," the notion that all beings were placed on a hierarchical scale with God at the top, living forms in the middle, and inorganic materials at the bottom, was integral to his classification of life forms. His categorization of living entities placed Primates as the highest level, with humans at the peak within the order (Brace, 2005, 28). This system of hierarchical classification, coupled with the delineation of human varieties, was key to 19th century anthropology.

German physician Johann Friedrich Blumenbach (1752-1840) spent his entire career as professor of medicine at the University of Göttingen. His doctoral dissertation, *De Generis Humani Varietate Nativa* (1775) was released to the public in 1776. He criticized Linnaeus' four varieties for being "artificial," but expanded upon it to create his own "natural" classification of humankind. He maintained Linnaeus' European, American, and African groups, but split the Asian group into Mongolian and Malayan, thus creating his five races: the Caucasian, or white race; the Mongolian, or yellow race; the Malayan, or brown race; the Ethiopian, or black race; and the American, or red race (Brace, 2005, 44).

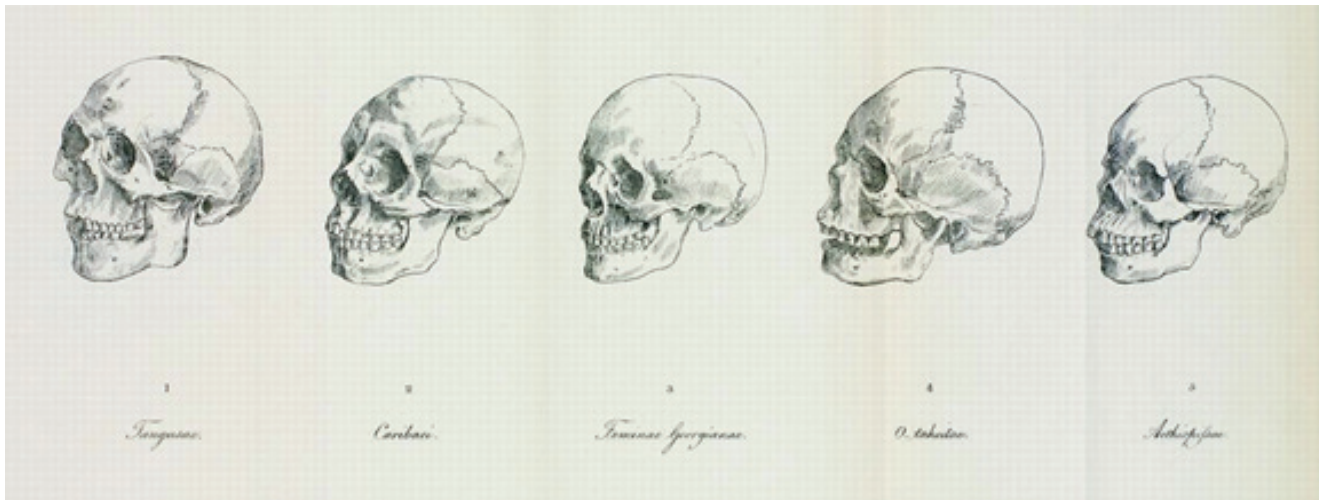


Figure 1: Blumenbach's Five Races Represented with Crania

There are two explanations for Blumenbach's choice of the word "Caucasian" to describe the white race. First, he considered the Georgian skull he studied to be the most beautiful, and thus the Caucasus would best represent this beautiful race (Brace, 2005, 44). Second, it was a nod to his Christian beliefs as the Caucasus, specifically Mount Ararat, was the landing place of Noah's ark (Brace, 2005, 44-5). Like many religious persons at the time, Blumenbach was a monogenist and believed that the difference in skin color between the races was a result of degeneration due to external factors, rather than a primordial difference (Douglas, 2008, 38). He preferred a more fluid take on human difference, suggesting the varieties blended into each other to such a degree that his divisions were arbitrary (Brace, 2005, 46). Ironically, Blumenbach's five types served as

the building blocks for classifying human difference for the next 200 years (Brace, 2005, 46).

Georges Cuvier (1769-1832), the French naturalist and comparative anatomist best known for his work in vertebrate paleontology was a dominant figure in early 18th century anthropology and natural history (Douglas, 2015, 5). His predominant focus was on race which he defined as “the permanent inherited physical differences which distinguish human groups,” (Stocking, 1968, 30). Although his conservative religious background rendered him a monogenist, Cuvier established many of the tenets of 19th century polygenism. He believed in the fixity of species and denounced Lamarckian notions of the role of environment in establishing human difference, favoring a theory of aboriginal variation (Douglas, 2008, 59). He classified humans into three distinct races (Caucasic, or white; Mongolic, or yellow; Ethiopic, or black) based on cranial morphological differences, a practice that continued into the mid-19th century (Douglas, 2008, 41 & 45, Stocking, 1968, 39). In particular, Cuvier utilized Camper’s measurement of the facial angle to morphologically differentiate races (Stocking, 1968, 29-30). His stance on human hybridity, as defined in his *Discours sur les révolutions du globe* (1826), also persisted into the next era as he argued that the species are alterable only by mixing, which nature has taken care to avoid by establishing a “mutual aversion” between separate species (Blanckaert, 2003, 48).

Georges-Louis Leclerc, Comte de Buffon (1707-1788), was a French naturalist who, in his *Histoire naturelle, générale et particulière* (1749), went against Linnean abstraction and asserted the reality of species (*espèce*) which he defined as a concrete category of individuals who are interfertile (Douglas, 2008, 36; Blanckaert, 2003, 44). Extrapolating this definition to humans, Buffon contended that due to their ability to freely reproduce, all human beings belonged to one single species. As a monogenist, he believed that all human diversity was the result of climate-induced degeneration from a primordial form (Blanckaert, 2003, 44-5). Buffon was among the first scholars to engage with the question of human hybridity¹, insisting that no mixed-race individuals (*métis*) are entirely infertile, but rather possess varying degrees of hybridity (Blanckaert, 2003, 45). He developed this idea further through experiments in animal breeding and determined that the degree of fertility of both pure and mixed species was dependent on the similarities (*convenances*) shared by the parent species. Thereby, he redefined species as the number of similarities or dissimilarities (*disconvenances*) which differentiated them (Douglas, 2015, 5). Buffon’s legacy for 19th-century monogenism was based on two key concepts: that the human species was entirely interfertile; and that miscegenation improved a species (Douglas, 2015, 5).

In many ways, Buffon’s disciple, Julien-Joseph Virey (1775-1846), acted as his foil. Virey distinguished species based on phenotypic characteristics, rather than reproductive capabilities (Douglas, 2008, 50). As a polygenist, he was an advocate of Voltaire and Lord Kames and divided the genus *Homo* into two species, the first including white, yellow, and brown races, the second including black and “blackish” races (Bernasconi & Dotson, 2005, vii; Stocking, 1968, 39; Brace, 2005, 41). He believed in the fixity of species and denied the effects of climate on diversity (Douglas, 2008, 50). Although he agreed with Buffon’s take on the fecundity of human hybrids, Virey spun his mentor’s focus on interfertility to invali-

date the unity of the human species. He argued that since adjacent animal and plant species were capable of producing fertile offspring, thus the creation of new human species was plausible. Whereas Buffon asserted the ameliorative effects of miscegenation, Virey was equivocal in regard to the adequacy of cross-bred individuals (Douglas 2008, 61).

Often considered Broca's greatest inspiration, Samuel George Morton (1799-1851) was a prominent Philadelphia physician and perhaps the most influential polygenist of his time (Brace, 2005, 78; Stocking, 1968, 39). Considered to be the Founder of the American School of Anthropology, Morton adopted Blumenbach's five races but, lamenting that he did not possess a cranium representing each variety, began to compile his immense cranial collection that would come to be known as "The American Golgotha," (Brace, 2005, 81). Meticulously documented and researched, the Morton collection contains skulls from across the globe and became the basis for Morton's major anthropological works, *Crania Americana* (1839) and *Crania Aegyptiaca* (1844). Although his racial categories were adopted from those of Blumenbach, Morton believed in primordial differences between human races (Brace, 2005, 83-4). As he continued to study human variation, he obfuscated his definitions of "race" and "species" such that "any possible distinction between 'race' and species had vanished," (Brace, 2005, 86). Morton began his studies on human hybridity in an attempt to develop a more adequate definition of "race". In his 1846 treatise, *Hybridity in Animals, Considered in Reference to the Question of the Human Species*, Morton refutes the notion of fertile offspring as the touchstone of species, arguing instead that "the mere fact that the several races of mankind produce with each other, a more or less fertile progeny, constitutes, in itself, no proof of the unity of the human species," (Morton, 1846, 211-2). Morton favored a differential fecundity of hybrids and the "degrees of hybridity" which he developed served as the basis for Broca's expansive study on human hybridity (Douglas, 2008, 66-7).

Among Morton's American supporters was Josiah Clark Nott² (1804-1873), a prominent polygenist and physician from Mobile, Alabama. Nott, along with George Gliddon (1809-1857), continued the tradition of the American School of Anthropology after the death of Samuel George Morton in 1851 (Brace, 2005, 93). In their co-edited polygenist manifesto, *Types of Mankind* (1854), they maintained Morton's assertion that different human groups represented separate species (Bernasconi & Dotson, 2005, xiv; Blanckaert, 2003, 56). Their primary goal was to prove the plurality of human species, thus establishing a scientific basis for chattel slavery in the American South (Bernasconi & Dotson, 2005, xvi). Nott recognized that the question of polygenism was entangled with that of hybridity and, drawing from census data, sought to break down the monogenist argument of interfertility "on the basis of weakened progeny" of whites and blacks (Bernasconi & Dotson, 2005, xiv & viii).

Etienne Serres (1786-1868) and Armand de Quatrefages (1810-1892) were among Broca's French contemporaries whose monogeny was in stark contrast with Broca's polygeny (Douglas, 2008, 56). In 1839, Serres was appointed chair of "anatomy and the natural history of man" at the *Muséum national d'histoire naturelle* where he lectured on racial classification and sought to improve the anthropological collections at the museum. In 1855, he stepped down as chair, re-

named the position to include “anthropology and the natural history of man”, and appointed Quatrefages as his replacement. By that time, the museum’s anthropological collection was the largest in the world and was only expanded upon by Quatrefages who, alongside his assistant E.T. Hamy, published *Crania ethnica* (1882), an oeuvre dedicated to skull types, much like Morton’s *Crania Americana* and *Crania Aegyptiaca* (Williams, 1985, 340-1). Quatrefages was, in many ways, Broca’s chief rival. Just as Broca appropriated the monogenist claim of indefinite fertility of hybrids to support his polygenist agenda, Quatrefages’ 1869 anthropology course at the Muséum and his publication *L’espèce humaine* (1877) relied on miscegenation to refute polygenism in favor of a unified human species (Douglas, 2015, 10). Quatrefages explicitly defined the distinction originally made by Buffon, that a métis was the fertile offspring of two distinct races, whereas a hybrid was the infertile offspring of two distinct species (Douglas, 2015, 4). These categories were often conflated by polygenists, namely Broca, who freely used the term hybrid, but distinguished between degrees of fecundity (see, for example, Broca, 1877, 327).

Joseph Arthur, Comte de Gobineau (1816-1882), French aristocrat and social thinker, published in four volumes (1853-55) his *Essai sur l’inégalité des races humaines*, a work which won him the nickname “the father of racism” (Douglas, 2008, 66; Brace, 2005, 119). In Gobineau’s ideology, the white, or Aryan, race was the founding race from which all other populations were derived (Brace, 2005, 120). Although he rejected polygenism for its discontinuity with the Catholic story of creation, he believed in the hierarchy and permanence of racial types and was strongly opposed to racial mixing (Brace, 2005, 120-1). To Gobineau, hybrids were always inferior to the pure races of their parents, incapable of reproducing on their own (Gobineau, 1855, 397-8; Douglas, 2008, 66). He believed that miscegenation was a precursor to degeneration and sterility, both of which signified the end of civilization (Blanckaert, 2009, 94). Though popular in Europe, Gobineau’s work gained its largest following in America where the issue of slavery was approaching its apex (Brace, 2005, 121).

Though their conflicting views of human origins put polygenism and monogenism in direct opposition, many of the nuances of each argument could be adopted by either side. Both theories were compatible with different views on the fixity of human types, the role of the environment in changing human form, and the possibility of compatible hybrids.

HISTORY OF MEASURING HUMAN FORM

As the desire to understand human variation grew, anatomists developed methods of measurement to systematize it. The earliest attempts to quantify human difference can be traced to the 4th century BCE, when Aristotle used the facial angle as an indicator of intelligence (Haller, 1971, 9). Aristotle did not record systematic measurements of this feature, which was left to 18th century Dutch painter and anatomist Petrus Camper (1722-1789). The facial angle, as defined by Camper, is the angle made between the line tangent to the forehead and through the upper lip (the facial line) and the line from the base of the nose to the ear canal (Brace, 2005, 33). The ancient Greeks gave their statues a superhuman facial angle mea-

asuring 100 degrees, setting an exaggerated standard for human perfection (Haller, 1971, 9). Camper revitalized this practice in 1784 after Linnaeus called for a method “by which to distinguish between Man and Ape,” (Haller, 1971, 9; Brace, 2005, 33). Camper measured the skulls of Europeans, Africans, central Asians, a monkey and an orangutan and came to conclusions that were readily accepted by the scientific realm (Brace, 2005, 33; Haller, 1971, 9).

Camper determined that the human facial angle ranged from 70 to 80 degrees; “everything above eighty degrees belonged to the realm of art, everything below seventy degrees to the animal kingdom,” (Brace, 2005, 33). He also claimed that the facial angle of the African skull bore a considerable resemblance to the orangutan and such strong low-face prognathism was associated with stupidity (Brace, 2005, 33; Haller, 1971, 9-11). Camper’s facial angle was the first attempt to demonstrate explicit morphological differences between the races and establish that cranial measurements could determine intelligence and morality (Stocking, 1968, 29-30). His measurements were to quantify Linnaeus’ Great Chain of Being in which ‘the further a race departed from the ideal form represented by Greek statuary, the lower it must rank on the scale of humanity,’ (Brace, 2005, 33; Meijer, 1991, 6). Despite his own monogenist beliefs, by 1860, Camper’s facial angle became a commonly used method of distinguishing and ranking human species (Meijer, 1991, 5; Haller, 1971, 11).

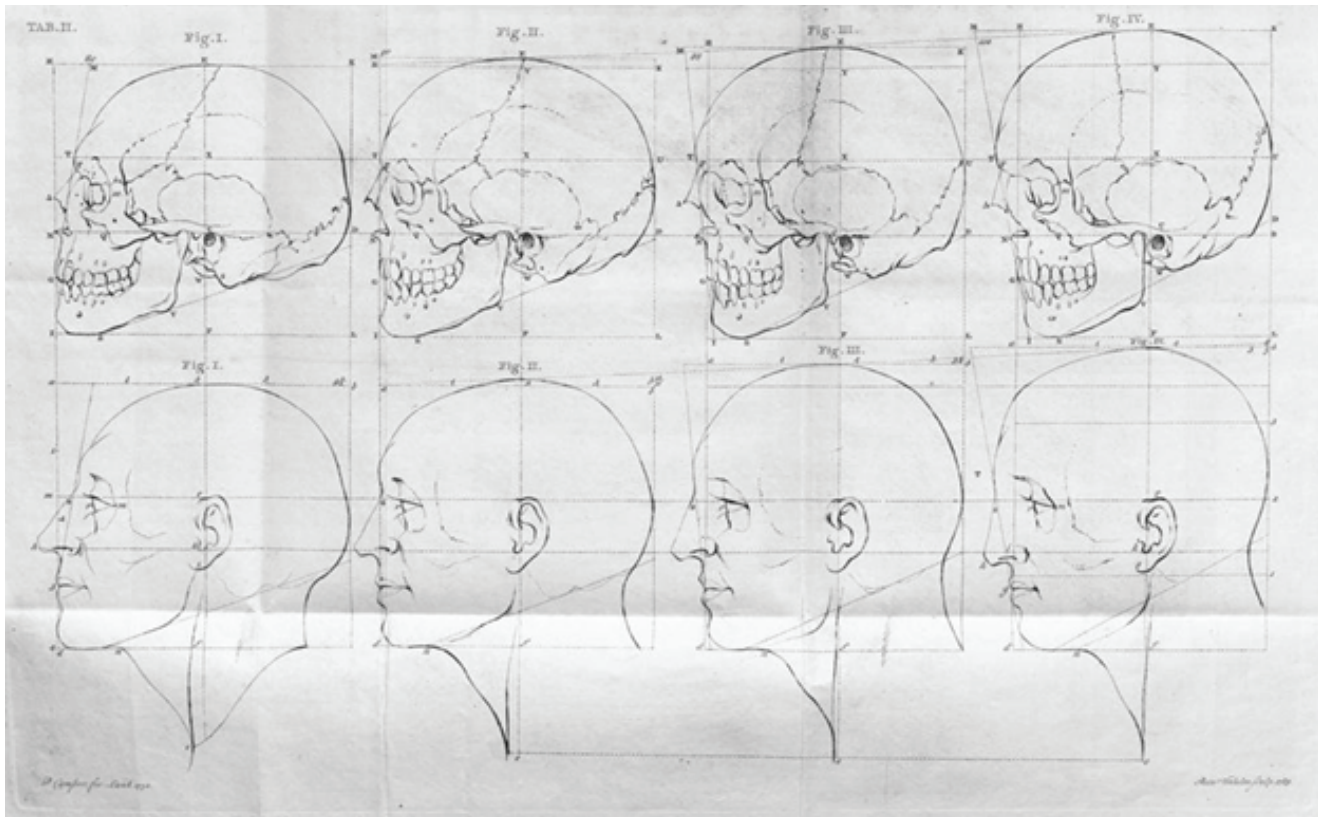


Figure 2: Camper’s use of the facial line.

As the field of craniometry formed, studying the brain became popularized. Some anatomists who dabbled in phrenology³ transitioned to measuring cranial capacity. The first to do so was German anatomy and physiology professor Friedrich Tiedemann (1781-1861) (Brace, 2005, 75).⁴ Tiedemann, who viewed the brain as “the noblest part of the body,” published in 1836 *On the Brain of the Negro, compared with that of the European and the Orang-Outang*,⁵ his study of the brain sizes of Blumenbach’s five races (Tiedemann, 1836, 498; Brace, 2005, 75). He procured his information in two ways. First, he simply weighed the brains (Tiedemann, 1836, 500). He then measured cranial capacity by weighing the empty skull, filling it with millet-seed through the foramen magnum, re-weighing the filled skull, and subtracting the empty weight from the filled weight (Tiedemann, 1836, 504). After conducting both experiments, Tiedemann found that the skulls of the “Ethiopian race” are generally no smaller than those of other races and condemned the naturalists⁶ who argued otherwise (Tiedemann, 1836, 511).

Ironically, Samuel George Morton, Tiedemann’s contemporary, used the same method of measuring cranial capacity to rank the five races based on brain size (Gould, 1981, 51; Morton 1839). He initially used white mustard seed, instead of Tiedemann’s millet, to measure cranial capacity by filling the cranium with sifted seed and then pouring the seed back into a graduated cylinder to measure the cranial volume in cubic inches. He soon discovered that the seeds did not pack well, when he was unable to replicate his results, so he switched to 1/8” diameter, BB-sized lead shots and received consistent measurements with an error of no more than one cubic inch for any given skull (Gould, 1981, 53). Morton published the resultant measures of hundreds of crania (as well as the facial angle for the same specimens) in three major works, *Crania Americana* (1839), *Crania Aegyptiaca* (1844), and his *Catalogue of skulls of man and the inferior animals* (1849). From his measurements, Morton developed a hierarchy of Blumenbach’s five races and twenty-two subgroups based on his measurements of brain size, setting the stage for Broca to develop his own methods of quantifying racial difference (Gould, 1981, 53-4).

In 1842, Swedish anatomist Anders Retzius (1796-1860) developed the cranial index, a new technique of craniometric measurement, which he defined in his 1843 book *Om Formen paa Nordboernes Cranier* (Carson, 2007, 102; Andreassen, 2015, 38). The index was the ratio between the width and length of the head, effectively measuring the relative size of the anterior lobes of the brain. From these measurements, Retzius divided crania into two types: dolichocephalic (long-headed) and brachycephalic (round-headed) (Carson, 2007, 102). He contended that his ancestors, the Nordic peoples, were descended from the dolichocephalic Bronze Age Europeans, whereas all non-whites were descended from brachycephalic Stone Age Europeans (Andreassen, 2015, 39). The cranial index garnered the most attention when Retzius argued that the Swedes and other long-headed blonde-haired Aryans were intellectually and morally superior to all other Europeans because of their highly developed anterior lobes (Carson, 2007, 102). In fact, one of Broca’s first forays into anthropometry was revisiting the cranial index in a study of Basque skulls (Carson 2007, 102; Schiller, 1979).

B R O C A A N D H Y B R I D I T Y

For Broca, the question of the plurality of species was inseparable from the question of human hybridity. He called his *Mémoires sur l'hybridité* (1877), the republication of his major work on hybridity, a “*plaidoyer polygéniste*”, or a polygenist appeal (Broca, 1877, 321). He did not want to deeply explore the conflict between monogenism and polygenism, he simply wanted to put monogenist reasoning in contestation with the facts and thus prove that their doctrine of the unity of the human species was hypothetical, arbitrary, and uncorroborated (Broca, 1858, 408). Broca believed that the human species had already undergone all possible changes and constantly cites the continuity between modern Egyptians and their depictions on ancient Egyptian monuments, following Morton (1844), as evidence that distinct types have existed since time immemorial (Broca, 1877, 334, 346-7). Broca splits the opposing monogenist view into two camps: those that adhere to the Biblical time scale and those who believe in deeper time (Broca, 1877, 348). He starts his attack with the first group who believes that the black race is a result of the curse of Noah on his second son, Ham (Broca, 1877, 348). Harkening back to his Christian upbringing, Broca recounts the story, noting that the curse only mandates servitude and says nothing about skin color. He shames the monogenists that used this story to justify slavery, a practice that Broca did not support (Broca, 1877, 349-50).

The second group of monogenists suggests that climate is the cause of phenotypic variation (Broca, 1877, 349). Broca argues that the time-frame accepted by these Christian monogenists requires racial differentiation to occur post-flood, that is, within 300-400 years. He argues that Europeans have been in tropical climates for similar amounts of time and have not begun to transform into Africans (Broca, 1877, 351). He begins to refute this claim with skin-tone, which he finds to be the most striking difference between races (Broca, 1877, 353). If the monogenists were right in stating that latitude is responsible for these differences, one would expect to see a single, consistent cline of skin color variation. Broca disproves this theory with numerous Native American and African populations whose skin colors are not distributed in a light-to-dark/north-to-south pattern (Broca, 1877, 362-4). He then introduces hair texture as a marker of distinct species (Broca, 1877, 372). Broca posits that the monogenists, who argue again for the effects of climate, must recant because not all dark-skinned people have what they refer to as woolly hair (Broca, 1877, 380).

To regain legitimacy, the monogenists developed a new, more scientific argument for the unity of the genus *Homo*. They state, “All the human races descend from one common origin and belong to the same species because they are all capable of producing fecund cross-breeds with one another,” (Broca, 1877, 408, translated by the author). Broca was unconvinced, and sought to discover whether this definition of species was real, or invented by the monogenists to support their agenda (Broca, 1877, 322). It is this question that he tackles in his major work on hybridity, *Recherches sur l'hybridité animale en général et sur l'hybridité humaine en particulier considérées dans leurs rapports avec la question de la pluralité des espèces humaines* (1858-1860).

Broca defines hybridity as “the crossing of species”, but posits that this definition is only ap-

appropriate if a species can be defined based on precise anatomic characteristics and are thought of as immutable, a feat which has not yet been accomplished (Broca, 1877, 411). Thus, he believes that making hybridity the “touchstone of species”, as Buffon did, is “turning in a vicious circle” (Broca, 1877, 411). Broca does not seek to claim that hybridity has no relation to the question of species, but rather argues that it is evidence against, not in favor of, this definition. According to Broca, when two individuals are incapable of producing offspring or produce offspring with limited fertility, they clearly belong to two distinct species. But when they produce offspring that are freely fertile with each other, this does not necessarily prove that they are of the same species, but rather that their genitals are compatible (Broca, 1877, 411). Broca gives this compatibility the name *homœogénésie*, which he defines as “a similitude in reproductive function” of which there are various degrees (Broca, 1877, 418). With the introduction of this term, Broca begins to present his rules on hybridity.

Broca contends that the most important condition to produce hybrid offspring is comparable gestation periods (in mammals) and incubation periods (in birds) (Broca, 1877, 425). Only when this condition is met can progeny be produced. When a male from species A can produce offspring with a female from species B and a male from species B can produce offspring with a female from species A, this is an example of *bilateral hybridity*. When a male from species A can produce offspring with a female from species B, but a male from species B cannot do so with a female from species A, this is called *unilateral hybridity*. Broca attests that the more dissimilar the species, the less the degree of *homœogénésie*, but it is no more possible for proximate species to produce offspring than it is for distant ones (Broca, 1877, 421). Thus, the only way to fully understand hybridity and *homœogénésie* is to abandon *a priori* reasoning for *a posteriori* methods and methodically study the formation of hybrids (Broca, 1877, 426).

For Broca, “*homœogénésie* is directly proportional to the degree of perfection of the hybrids” (Broca, 1877, 426; translated by the author). He defines a perfect hybrid as one that is either equal to or better than one or both parent races. It can perpetuate its own race, as well as breed with the parent races to produce offspring (Broca, 1877, 426-7). On the other hand, an imperfect hybrid is one that is always inferior to the parent species, incapable of reproducing, and, borrowing from Nott’s views on hybridity, the most imperfect hybrid is weaker than the parent species (Broca, 1877, 427). Based on this scale, Broca defines the highest form of *homœogénésie* as “*homogénéité complète*” (complete homogeneity) and the lowest form as “*hétérogénésie*” (heterogenesis), or “absolute sterility” (Broca, 1877, 428). Broca places all hybrids somewhere between these two limits (Broca, 1877, 428).

According to Broca, the lowest form of hybridity, which he hesitates to define as a degree on the basis that it is “hypothetical”, is *l’hybridité abortive* (abortive hybridity), which is when the crossing of two species results in a miscarriage or stillbirth. Broca calls this the “transition between *hétérogénésie* and *homœogénésie*” because progeny is produced, but does not come to term (Broca, 1877, 428). Broca establishes and outlines four degrees of hybridity (Figure 3), which he adapted from those of Samuel George Morton (1850, 9) (Figure 4) (Broca, 1877, 433-4).

GLOSSARIAL NOTE.

The significations of the following words, habitually used by Dr. Broca, are appended :—

- AGENESIC.** Mongrels of the first generation, entirely unfertile, either between each other, or with the two parent species, and consequently being unable to produce either direct descendants or mongrels of the second generation.
- DYSGENESIC.** Mongrels of the first generation, nearly altogether sterile.
- a. Unfertile with each other, therefore with no direct descendants.
 - b. They sometimes, but rarely and with difficulty, breed with one or the other parent species. The mongrels of the second generation, produced by this interbreeding, are infertile.
- PARAGENESIC.** Mongrels of the first generation having a partial fecundity.
- a. They are hardly fertile or infertile *inter se*, and when they produce direct descendants, these have merely a decreasing fertility, tending to necessary extinction at the end of some generations.
 - b. They breed easily with one at least of the two parent species. The mongrels of the second generation, issued from this second breeding, are themselves and their descendants fertile *inter se*, and with the mongrels of the first generation, with the nearest allied pure species, and with the intermediate mongrels arising from these various crossings.
- EUGENESIC.** Mongrels of the first generation entirely fertile.
- a. They are fertile *inter se*, and their direct descendants are equally so.
 - b. They breed easily and indiscriminately with the two parent species; the mongrels of the second generation, in their turn are, themselves and their descendants, indefinitely fertile, both *inter se* or with the mongrels of all kinds which result from the mixture of the two parent species.

Figure 3: Broca's four degrees of hybridity.

Hybridity.

Hybridity, whether in plants or animals, has been singularly neglected by naturalists. It has generally been regarded as a *unit*: whereas, its facts are as susceptible of classification as any other series of physiological phenomena. Hence, I have, on a former occasion, proposed *four degrees* of hybridity, which I will briefly recapitulate in this place.

The 1st degree is that in which the hybrids never reproduce; in other words, where the mixed progeny begins and ends with the first cross.

The 2d degree is that in which the hybrids are incapable of reproducing, *inter se*, but multiply by union with the parent stock.

The 3d degree is that in which animals of unquestionably distinct species produce a progeny which is prolific, *inter se*.

The 4th degree is that which takes place between closely proximate species—among mankind, for example, and among those domestic animals most essential to their wants and happiness.

There is, moreover, what may be called a mixed form of hybridity, which certainly has had a very great influence in modifying some do-

Figure 4: Morton's four degrees of hybridity.

He explains the differences between his four degrees of hybridity and Morton's and describes why he chose to change Morton's classifications in the way he did (Broca, 1877, 432-3). Broca's and Morton's first degrees of hybridity are identical, Broca simply gave Morton's a proper name (agenesic). Broca split Morton's second degree into dysgenetic and paragenetic hybridity because he found that their pattern of fertility was distinct enough to justify separate classifications. Morton's third and fourth degrees correspond to Broca's eugenesic hybridity. He finds Morton's division into two groups "entirely insufficient", arguing that this distinction was "evidently no more than a sacrifice to the demands of the moment"⁷ and that "the fourth degree of hybridity is entirely imaginary" (Broca, 1877, 433-4; translated by the author).

Broca divides his four degrees into two distinct classes: *l'hybridité inférieure* (inferior hybridity), which includes the first two degrees (agenesic and dysgenetic); and *l'hybridité supérieure* (superior hybridity), which includes the last two degrees (paragenetic and eugenesic) (Broca, 1877, 434). Broca attests that the two types of inferior hybridity have no influence on the parent species, whereas the two types of superior hybridity can modify a species by creating new races, thus falsifying the doctrine of the permanence of species (Broca, 1877, 434).

Though Broca attests that paragenetic hybrids are able to generate new races, he admits that the creation of a permanent type has yet to be supported by evidence. Instead, what is observed is a *croisement de retour* (back crossing), when the hybrid offspring of the first generation mates with one of the parent species and produces a hybrid offspring of the second generation that is two parts parent species, and one part hybrid. If the crossings continue to follow the same pattern, the offspring will eventually revert to the parent species with which it has been crossed (Broca, 1877, 437, 454).

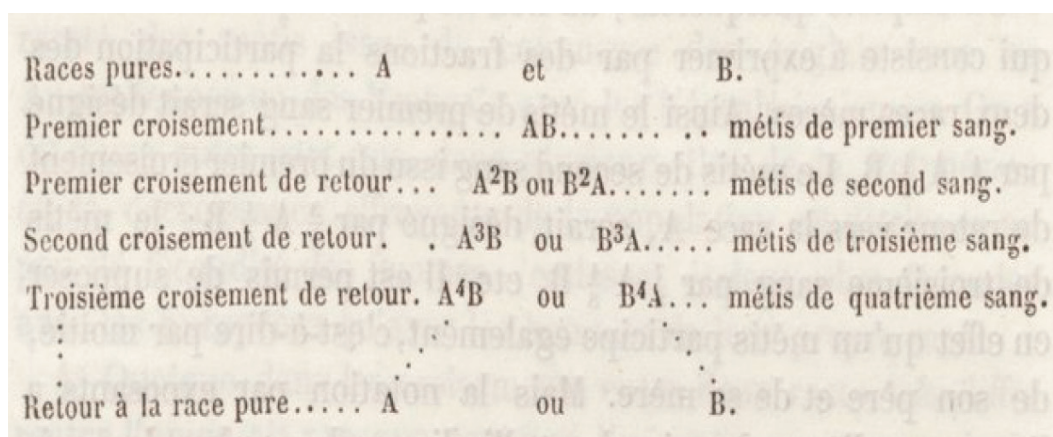


Figure 5: Graphic of Broca's *croisements de retour* (back crossings).

On the contrary, eugenesic hybridity can create new races (Broca, 1877, 445). To demonstrate this fact, Broca cites numerous examples of eugenesic animal hybrids, such as dogs and wolves, sheep and goats, one-humped and two-humped camels, and llama, alpaca, and vicuña hybrids, all of which can reproduce freely, but are clearly members of distinct species (Broca, 1877, 453, 448, 457, 460-1). To understand and describe these hybrids,

Broca mentions a number of observational methods, including counting the ribs of half-breed buffaloes to see whether they take after the mother or father species and whether there is a difference in rib-count in first or second generation hybrids (Broca, 1877, 443-4). He also cites the abbot Molina who, in his *Histoire naturelle du Chili* (1782), describes the differences in wool texture of the cross-breeds of goats and sheep (Broca, 1877, 448).

It is worth noting that for most of these examples, Broca cites the experiments of other naturalists, most often his fellow Frenchman, Comte de Buffon. Broca's first, and most impactful, hands-on experience with hybrids was with crossing hares and rabbits, giving rise to a hybrid which Broca named *léporides* (leporids⁸) (Broca, 1877, 475, 468). Broca asserts that the hare and rabbit are clearly distinct species, going so far as to say that "it seems superfluous to prove" it (Broca, 1877, 468; translated by the author). They differ in their social behaviors (hares are solitary, rabbits are social), their habitat (hares live out in the open, whereas rabbits live in the bushes, hares have larger ears and longer back legs, thicker skin, and coarser hair (Broca, 1877, 468-70). Broca states:

The difference in their forms, in their coat, in their flesh, in their smell, in their fecundity, in their intelligence, in their habits, cannot be attributed to accidental influences: it is the work of nature, and no one would ever dream to find two species so distinct that could come from a common origin (Broca, 1877, 470; translated by the author).

Despite being natural enemies, the hares and rabbits have an identical gestation (thirty days), and thus can be bred in captivity (Broca, 1877, 469, 425). In the 18th century, Buffon attempted this crossing to no avail, but the first successful crossing of a hare with a rabbit was recorded by the naturalist abbot Carlo Amoretti whose experiments suggested paragenetic hybridity between the two species (Broca, 1877, 471-2). Broca was not the only of his 19th century contemporaries to consider the hybrids of hares and rabbits; Samuel Morton, the biggest influence on his hybridity studies, was presented the hides of an unknown species of the genus *Lepus* by John Bachman, who claimed that they were a new hybrid species, an assertion that Morton accepted (Broca, 1877, 474).

Broca first became involved with the leporids in October 1857 when his friend, M. Léonce Bergis, presented him two female hybrids of the first generation (half hare, half rabbit) and one male hybrid of the second generation (three-quarters hare, one quarter rabbit) which he had brought from Angoulême where a man by the name of M. Alfred Roux had been breeding them since 1850. Each of the females had five young, each of which was five-eighths hare and three-eighths rabbit. M. Bergis gave Broca one of these young hybrids, which he brought back to Paris where it was raised in the Jardin des Plantes by Broca's colleague, M. Vulpian (Broca, 1877, 475). After establishing the existence of the leporids, Broca wanted to better understand their fertility, so he went to Angoulême that same month (October 1857) to meet with M. Roux (Broca, 1877, 475-6). Upon arrival, he found that the hybrids had reproduced to the sixth or seventh generation and he could immediately see varying degrees of hybridity amongst them and three or four types intermediate to the parent species. Broca states that at this moment he could not help but think of the public festivals in Havana "where men of all colors, from white to black,

mix and collide,” (Broca, 1877, 476; translated by the author). M. Roux continued his breeding experiment and by March 1859, he had reached the tenth generation of leporids. M. Roux had succeeded in creating a new race that was not only, in Broca’s opinion, stronger and more beautiful than either parent species, but he had created a eugenic hybrid race that was able to proliferate on its own, proving that distinct species are able to produce fertile offspring, thus invalidating the assertion that hybridity can serve as the touchstone of species (Broca, 1877, 476, 485).

Broca presents his leporid before the Société de Biologie in 1858 and was promptly told to discontinue his study because it presented a threat to Biblical creationism and “was in contradiction with the law of species” (Brace, 2005, 148; Broca, 1877, 323). Broca was shocked and became skeptical of the monogenist definition of species. In his memoir he says, “What I did not know is that the definition of species based on interfertility had become a dogma and that everything, up until my evidence, had to bend before it; for is there anything more evident than the species-level difference between the hare and the rabbit?” (Broca, 1877, 324, translated by the author). Broca realized he either had to abandon his study and give up on the possibility of studying the hybridity of the human genus, or found a new society where he was free to study whatever he pleased. Therefore, in 1859, he founded the Société d’Anthropologie de Paris whose main purpose was “the scientific study of the human races” (Broca, 1877, 325; Société d’Anthropologie de Paris, 1864, v). Hybridity was the foundation upon which his society, and French anthropology as a discipline, was built and after presenting his first major work, *Recherches sur l’ethnologie de la France*⁹ (1859) before the Société, his primary focus became the study of human hybridity (Brace, 2005, 149; Broca, 1877, 516).

Des phénomènes d’hybridité dans le genre humain, the third and final part of Broca’s *Mémoires sur l’hybridité* (1877), was published in two parts from 1859 to 1860 and translated into English as *On the Phenomenon of Hybridity in the Genus Homo* for the Anthropological Society of London in 1864. Having completed his studies on eugenic hybridity in animal species, Broca’s goal is to show that the human races are subject his four degrees of hybridity and to use this differential fertility to prove the plurality of human origins. To understand Broca’s argument, one must first understand what he means by “race”. To differentiate between two races, Broca says that “a single character, however slight, is sufficient, provided it be hereditary and sufficiently fixed,” (Broca, 1864, 7). There are, thus, an immense number of human races, but they tend to naturally separate into groups, or “types”, based on broadly shared characteristics (Broca, 1864, 7-8). Broca defines type as “the ensemble of the characters common to each group” and bases his “types” on Blumenbach’s five races (Broca, 1864, 8). He insists that types are nothing more than abstractions and although an individual may belong to a certain type, no individual will ever personify the type to which it belongs (Broca, 1864, 8). Ironically, given his penchant for conflating terms such as hybrid and métis, Broca criticizes those polygenists who conflate race with type because if they argue, for example, that a light-haired German and a dark-haired Celt belong to the same race, then there is nothing stopping monogenists from arguing that a Celt and a Berber belong to the same race (Broca, 1877, 503). Finally, Broca stresses the care one must take when using the word “species” which refers to “both the idea

of a special confirmation and a special origin” (Broca, 1864, 11). Many polygenists refrain from using the term “species” because they believe in the plurality of human origins, thus rendering it impossible of determining every primitive stock and its characteristics (Broca, 1864, 11). Other polygenists, whom Broca has deemed “pentagenists” use Blumenbach’s five races to constitute five distinct, primitive stock, whereas monogenists argue that these five races all emerged from the same primitive stock (Broca, 1864, 12). Broca ascribes to the polygenist approach and uses race, rather than species, to refer to the different human groups (Broca, 1877, 508).

At the start of his treatise, Broca cites Gobineau who, in his *Essai sur l’inégalité des races humaines* (1855) attests that the crossing of human races “constantly produces disastrous effects and physical or moral degradation is sooner or later the inevitable result,” (Broca, 1877, 493). Broca admits that he does not share this view and goes against those who believe that no mixed race is capable of self-preservation (Broca, 1877, 493). He argues instead that, much like the animal species he studied, human races reproduce with varying degrees of *homœogénésie*, and thus some crossings are entirely eugenesic, others occupy an intermediate paragenesic or dysgenesic space, and others are potentially agenesic (Broca, 1877, 508).

Drawing from the teachings of his teacher and companion, Pierre Nicolas Gerdy, Broca contends that most races, including the French, have arisen from the eugenesic crossing of two or more races (Broca, 1877, 509). He does not extend this degree of *homœogénésie* to all races of men, however, and only mentions Caucasian races in this section on eugenesic hybridity (Broca, 1877, 517). Citing other naturalists and anthropologist, he discusses a unilateral hybridity between Negro¹⁰ and Caucasian races. Broca attests that the union of a Negro man with a Caucasian woman is often sterile, whereas the union of a Caucasian man with a Negro woman is entirely fertile (Broca, 1877, 521). The mulatto offspring of these unions are extremely fertile when mixed with either one of the parent races, constituting paragenesic hybridity like that which Broca observed between animal species (Broca, 1877, 523). Broca remains objective in his exploration of human hybridity and is hesitant to accept explanations for why certain mixed races are unable to proliferate, either because there is not enough evidence or because he does not believe that the authors he cites consider all potential influences on or barriers to fertility¹¹. He also finds that, just as in animals, proximate races of humans do not necessarily produce more fertile offspring than more distant races, but, instead, citing M. Omalius d’Halloy, President of the Belgian Senate, claims that the most distant races (the Europeans and the Tasmanians) produce weak and sterile offspring bilaterally, thus representing agenesic human hybridity (Broca, 1877, 537, 542). Broca takes a more pragmatic approach to this claim, arguing that he does not have enough information to explicitly argue that the Australian mulattos are inferior hybrids, so he calls upon all physicians and travelers to go to Australia and observe the patterns of racial mixing (Broca, 1877, 555). Broca sums up his study on human hybridity in an eight-point list of observations (Figure 6). It is important to note that Broca presents very few of these assertions as facts, but rather expresses a need for more data and more observation to fully form his opinions (Broca, 1864, 60).

1. That certain intermixtures are perfectly eugenesic.
2. That other intermixtures are in their results notably inferior to those of eugenesic hybridity.
3. That Mulattoes of the first degree, issued from the union of the Germanic (Anglo-Saxon) race with the African Negroes, appear inferior in fecundity and longevity to individuals of the pure races.
4. That it is at least doubtful, whether these Mulattoes, in their alliances between themselves, are capable of indefinitely perpetuating their race, and that they are less prolific in their direct alliances than in their re-crossing with the parent stocks, as is observed in paragenesic hybridity.
5. That alliances between the Germanic race (Anglo-Saxon) with the Melanesian races (Australians and Tasmanians) are but little prolific.
6. That the Mulattoes sprung from such intercourse are too rare to have enabled us to obtain exact particulars as to their viability and fecundity.
7. That several degrees of hybridity, which have been observed in the cross-breeds of animals of different species, seem also to occur in the various crossings of men of different races.
8. That the lowest degree of human hybridity in which the homœogenesis is so feeble as to render the fecundity of the first crossing uncertain, is exhibited in the most disparate crossings between one of the most elevated and the two lowest races of humanity.

Figure 6: Broca's eight observations on human hybridity.

Broca concludes his memoir by stating that the human group is one genus made up of multiple species (Broca, 1877, 556). By proving that not all human intermixtures are eugenesic, he feels he has overturned the monogenist doctrine which argues, based on the belief animals that produce eugenesic offspring are of the same species, that human crossings are entirely eugenesic and thus humans belong to a single species (Broca, 1877, 559, 561). It may come as a shock that Broca ends his long treatise on hybridity, whose polygenist agenda often labels it a racist manifesto, by stating, "It might be said that the polygenist doctrine assigns to the inferior races of humanity a more honourable place than in the opposite doctrine. To be inferior to another man either in intelligence, vigour, or beauty, is not a humiliating condition," (Broca, 1864, 71). As he begins to expand his methods of experimentation, it becomes evident that Broca's only goal is to prove there is a primordial difference between the human races.

BROCA AND ANTHROPOMETRY

Before becoming a physician and anthropologist, Paul Broca initially studied for a bachelor's degree in mathematics (Schiller, 1979, 14). He is quoted saying, "A number is always better than an assertion: it leaves a more durable imprint on the memory," (Blancaert, 2009, 122, translated by the author). This background and appreciation for quantitative data underlies much of his approach to anthropology and explains why he developed such comprehensive anthropometric measurements after his first foray into craniometry.

As a medical student, Broca wrote to his father, “Currently, I am studying the skull. I do not find osteology very amusing. Besides being a drudgery, it offers the disadvantage of being so easily forgotten. But I must get through it,” (Broca, 1886, 30, translated by the author). Ironically, he would go on to become one of the most important craniologists of 19th century anthropology. Just over a year later, he wrote his mother of his growing taste for natural history and anatomy, claiming to like them “just as he once liked mathematics, because they gave him results that satisfied his mind,” (Broca, 1886, 131-2, translated by the author). By combining his objectivity with his studies of anthropology, Broca became especially interested in analyzing anatomical and morphological variation, most importantly the facial angle, the brain, and craniology, but also other physical characteristics such as size, skin tone, and body hair (Broca, 1871, 7). For traits such as eye color, skin color, and hair texture, which are not measurable in the traditional sense, Broca created scales on which to place individuals and satisfy his need for systematization (Broca, 1865, 137). It was only after he finished publication of his treatise on human hybridity that Broca began to experiment with measuring skulls and bodies. Whereas in controlled experiments, Broca was able to categorize animal hybrids as intermediates between the parent species in terms of color and hair growth pattern, this was harder to accomplish with human crossings and he had to develop more exact ways of identifying hybrids (Broca, 1877, 491).



Figure 7: Scale of eye colors.

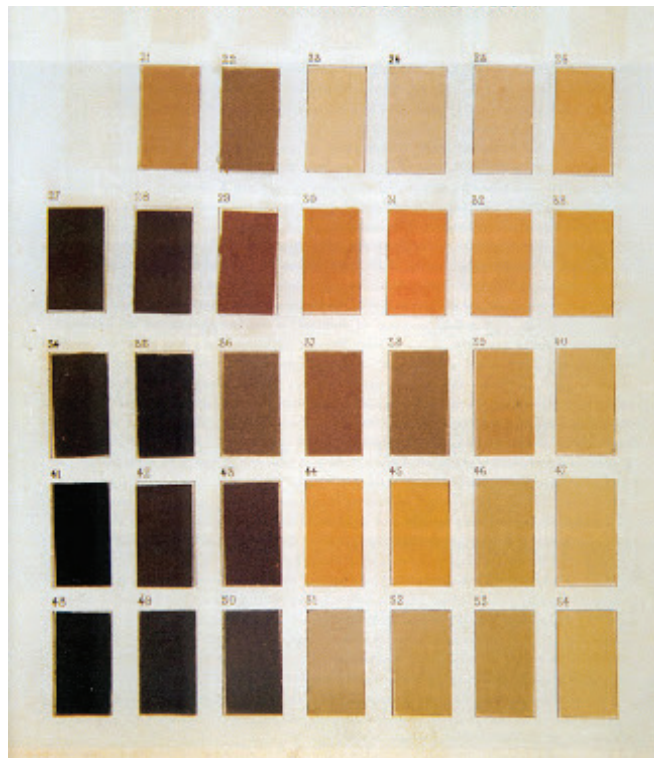


Figure 8: Skin colors and hair system.

As a neurosurgeon, it is no surprise that Broca's first experience with anthropometry was measuring the size and the form of the brain. In 1861, he published *Sur le volume et la forme du cerveau suivant les individus et suivant les races*, in which he sought to answer whether intelligence is linked to the form of the brain, rather than size (Broca, 1861a, 3). Following Morton's method of measuring cranial capacity in cubic inches, Broca plugged all openings of the skull, but the foramen magnum, and filled the cranium with equal sized grains, achieving sufficiently precise measurements (Broca, 1861a, 46). Broca did find correlations between intelligence and cranial capacity, but he concluded that brain form was just as, if not more, important as cranial volume (Broca, 1861a, 50-1).

Broca's next experiment in craniometry was reworking the cranial index in an attempt to prove wrong Retzius' assertion that Basque skulls were brachycephalic (Carson, 2007, 102; Broca, 1862a, 1). Broca claims that the Basque are an autochthonous race predating the arrival of the Celts (Broca, 1862a, 2). After studying and measuring 60 skulls, Broca's results did not match up with Retzius' theory. He found very few brachycephalic skulls and the average cranial index was 77.67 (Broca, 1862a, 3). Broca criticizes Retzius for his lack of numerical data, which he believes have only relative value and could have been the reason the Basque were deemed brachycephalic (Broca, 1862a, 4). Broca reminds the reader that he had redefined the cranial index into pure dolichocephaly (under 75), sub-dolichocephaly (75-77.77), mesaticephaly (77.77-80), sub-brachycephaly (80-85), and pure brachycephaly (above 85). Based on these distinctions, none of the Basque skulls that he measured was truly brachycephalic, as the highest cephalic index only measured 83.24 (Broca, 1862a, 6).

INDICE CÉPHALIQUE.	NOMBRE DES CRANES de chaque catégorie, en centièmes.					TOUT Paris.	BAS- QUES.
	CITÉ.	INNO- CENTS.	SÉPUL- TURES parti- culières.	FOSSE com- mune.	MORGUE.		
Au-dessous de 75.	14.4	17.1	12.2	14.3	5.9	14.3	15.0
De 75 à 77.77....	23.2	21.4	17.8	14.3	5.9	19.8	33.3
De 77.78 à 80....	28.8	20.5	26.7	22.9	29.4	25.2	31.7
De 80.01 à 83.24...	16.8	20.5	23.3	28.5	35.3	21.4	20.0
au-dessus de 83.24.	16.8	20.5	20.0	20.0	23.3	19.3	0.0
Nombre de crânes.	100.0	100.0	100.0	100.0	100.0	100.0	100.0

Figure 9: Broca's refinement of Retzius' cephalic index.

Broca not only expanded upon previously existing craniometric measurements, but he began to develop his own measurements, as well as various instruments to aid in anthropometry (Broca, 1861b). Broca was the first to name singular points on the cranial vault, the same which are used today for craniometric data (Broca, 1862b). According to Broca, "the ordinary anatomic methods of measurement are entirely insufficient and in order to fruitfully compare species, races, and individuals, one must resort to using special instruments," (Broca, 1861b, 42, translated by the author). One such instrument was the *craniographe* (craniograph) which Broca developed to measure the auricular angles in order to map the curve of the skull and the placement of the auditory canals (Broca, 1861b, 44). Evidently, Broca had a very specific purpose in mind when developing this tool, but he also used it to obtain three other measurements which he equated with inferior or superior races: facial prognathism, anterior projection, and posterior projection (Broca 1861b, 60).

	1° EN MILLIMÈTRES.			2° EN MILLIÈMES.		
	EUROPÉENS.	NÈGRES.	DIFFÉRENCE.	EUROPÉENS.	NÈGRES.	DIFFÉRENCE.
Projection antérieure... faciale.	12,385	27,676	+15,291	61,80	137,58	+72,78
... crânienne	78,351	72,628	- 5,723	409,95	361,04	-48,91
Projection postérieure..... crânienne	100,385	100,857	+ 0,472	525,24	501,37	-23,87
Total.....	191,121	201,161	+10,040	1000	1000	

Figure 10: Measurements of the anterior and posterior projections of the crania of Europeans and Negros.

Shortly after developing the craniograph, Broca published *Sur les projections de la tête et sur un nouveau procédé de céphalométrie et d'anthropométrie* (1862c). In this work, rather than simply taking measurements, Broca is attempting to quantitatively define inferior and superior races and to do so, he borrows both Camper's facial angle and Gratiolet's three groups: frontal races (Europeans), parietal races (Mongolians), and occipital races (Negroes) (Broca, 1862c, 79; Broca, 1861a, 51; Gould, 1981, 97). Preferring to call the measurement "anterior facial projection", rather than facial angle, Broca was measuring prognathism, which he associated with inferiority (Broca, 1862c, 86). Most likely due to his background in neuroscience, Broca quickly abandoned cranial capacity as a measure for intelligence in favor of the "relative development of the parts of the brain", where larger anterior portions are associated with intelligence, whereas larger posterior portions are associated with inferiority (Schiller, 1979, 151; Gould, 1981, 97-8). He thus focused on measuring the relative sizes of the anterior and posterior portions of the skulls of Europeans and Negroes.

He noticed that to make a Negro skull from a European, one must not only increase the degree of facial prognathism, but also decrease the projection of the anterior lobe and increase the projection of the posterior lobe (Broca, 1862c, 92).

Longueur du radius, l'humerus étant cent.

NÈGRES.			EUROPÉENS.		
	Neuf Hommes.	Six Femmes.		Cinq Hommes.	Quatre Femmes.
Minimum . . .	78.00	76.66	Minimum . . .	70.58	70.91
Maximum . . .	83.33	82.00	Maximum . . .	76.67	76.33
Moyenne . . .	79.43	79.33	Moyenne . . .	73.82	74.02
Moyenne des 15 nègres des deux sexes,	79.40		Moyenne des 9 Européens des deux sexes,	73.88	

Figure 11: Length of the radius, the humerus being 100. Relative length of the radius to the humerus in male and female Negroes and Europeans.

As he continued to study morphological differences between human races, Broca moved from taking just cranial measurements to measuring the rest of the body. His first instance of using postcranial data to compare races was in 1858 in the first part of the *Recherches sur l'hybridité animale*. . . He describes Negroes as an intermediate form between apes and humans. He cites numerous anatomical measurements including pronounced facial prognathism, small facial angle and cranial capacity, and their relative forearm-to-arm lengths (Broca, 1877, 397). He took these measurements from skeletons in September 1858 and published them

four years later in *Sur les proportions relatives du bras, de l'avant-bras et de la clavicule chez les nègres et chez les Européens* (1862) (Broca, 1862d, 164). Broca cites Charles White who determined that the forearm of the Negro is proportionally longer than that of the European (Broca, 1862d, 163). Broca's first set of measurements found that, on average, the radius of the Negro is longer than that of the European, both in males and in females (Broca, 1862d, 165).

Longueur de la clavicule par rapport à l'humérus = 100.

	NÈGRES.		EUROPÉENS.	
	Neuf hommes	Sept femmes	Cinq hommes	Quatre femmes
Minimum	41.53	44.36	40.95	43.46
Maximum	52.00	52.00	48.40	46.66
Moyenne	45.89	47.40	44.32	45.04

Figure 12: Length of the clavicle in relation to the humerus – 100. In male and female Negros and Europeans.

Broca uses the marked differences in arm-to-forearm length to argue for primordial human difference by claiming that climate, which many believe is the reason for racial diversity, could not have played a role in altering these characteristics (Broca, 1862d, 168). He then chooses to determine whether an elongated radius is a sign of inferiority. Using Hottentots, Eskimos, and Australians as examples of inferior races, he finds that the radius of the Australian measures roughly the same as that of the shortest European and that they all measure shorter than that of the shortest Negro (Broca, 1862d, 169). Thus, Broca is unable to name an elongated radius as an inferior trait (Broca, 1862d, 169-70). Broca then moves to measurements of the clavicle, which are longer in both European and Negro women than they are in their respective male counterparts. Broca also observes that the clavicles of Negros are longer than those of Europeans, demonstrating a larger racial difference than sexual difference in clavicle length (Broca, 1862d, 171).

In 1867, Broca, having been met with controversy for not taking his measurements from living individuals, publishes *Sur les proportions relatives des membres supérieurs et des membres inférieurs chez les Nègres et le Européens*, a work compiling every possible measurement from every skeleton in the Museum where Broca conducted the study for his 1862 publication (Broca, 1867, 641-2). He limits himself to discussing the arm, forearm, and clavicle in order to respond to his critics (Broca, 1867, 642). Though he is criticized for using skeletons rather than living people, Broca contends that only on a skeleton does one know the true superior and inferior limits of the long bones, allowing for more accurate measurements (Broca, 1867, 645). Broca measures the length of the upper limb in comparison to the length of the lower limb and, surprisingly, the highest median length measurement belongs to a European male.

**LONGUEUR DU MEMBRE SUPÉRIEUR, LE MEMBRE INFÉRIEUR
ÉTANT REPRÉSENTÉ PAR 100.**

	Nombres.	Maximum.	Minimum.	Moyennes.
Nègres.....	10	73.04	65.70	68.30
Nègresses.....	0	70.20	65.96	68.15
Nègres des deux sexes.	10	73.04	65.70	68.27
Européens.....	8	70.54	68.97	70.04
Européennes.....	0	71.42	66.37	69.33
Europ. des deux sexes.	14	71.42	66.37	69.73

Figure 13: *Length of the upper limb, the lower limb represented by 100.* In male and female Negroes and Europeans.

Broca asserts that the young male Negro individual whose long arm measurement raised the Negro average probably experienced arrested development in his lower limbs (Broca, 1867, 647). Broca explains that his results do not at all conform to the popular opinion of blacks being intermediate between the anthropoid apes and Europeans. Since the apes have much longer arms in comparison to their legs, this in humans is considered an inferior trait, but do Broca's measurements demonstrate the opposite? (Broca, 1867, 648) Broca then compares the length of the humerus to the length of the leg. The humerus length of Negroes is considerably less than that of Europeans, which is not a shock if one considers that in black individuals, the radius is longer compared to the humerus than it is in European individuals. The radius of the Venus of Hottentot, a member of the Khoi people of South Africa, a race which is considered inferior to most, measures 41.20, higher than the average for male Europeans (Broca, 1867, 649).

**LONGUEUR DE L'HUMÉRUS, LE MEMBRE INFÉRIEUR
ÉTANT REPRÉSENTÉ PAR 100.**

	Nombres.	Maximum.	Minimum.	Moyennes.
Nègres.....	10	39.84	35.93	38.03
Nègresses.....	6	39.73	37.14	38.51
Nègres des deux sexes.	10	39.84	35.93	38.20
Européens.....	8	41.20	39.04	40.35
Européennes.....	6	41.20	38.83	39.79
Europ. des deux sexes.	14	41.20	38.83	40.11

Figure 14: *Length of the humerus, the lower limb represented by 100.* In male and female Negroes and Europeans.

Broca continues by comparing the length of the humerus to the length of the femur.

LONGUEUR DE L'HUMÉRUS, LE FÉMUR ÉTANT REPRÉSENTÉ PAR 100.

	Nombres.	Maximum.	Minimum.	Moyennes.
Nègres.....	10	72.79	66.93	69.03
Négresses.....	6	72.28	66.13	69.38
Nègres des deux sexes.	16	72.79	66.13	68.97
Européens.....	8	74.51	69.97	72.16
Européennes.....	6	75.37	68.00	71.87
Europ. des deux sexes.	14	75.87	68.00	72.90

Figure 15: Length of the humerus, the femur represented by 100. In male and female Negroes and Europeans.

Again, his measurements show that the Europeans have longer humeri across sexes and the Hottentot race measures more closely to the European than to the Negro type. Broca adds that in the anthropoid apes, the humerus is oftentimes equal in length or longer than the femur. In these measurements, he notes that the Negro differs more from the apes than does the European (Broca, 1867, 650). Broca concludes that the Negro is characterized by a short humerus when compared with the radius, with the lower leg, and with the femur. He then tries to determine whether this is the reason for the short upper limb measurements he has obtained from the Negro skeletons and expects to find that the radius is relatively the same length when compared to the lower limb.

LONGUEUR DU RADIUS, LE MEMBRE INFÉRIEUR ÉTANT REPRÉSENTÉ PAR 100.

	Nombres.	Maximum.	Minimum.	Moyennes.
Nègres.....	10	33.20	28.23	30.36
Négresses.....	6	30.71	29.68	30.14
Nègres des deux sexes.	16	33.20	28.23	30.28
Européens.....	8	30.35	29.12	29.75
Européennes.....	6	30.22	27.53	29.28
Europ. des deux sexes.	14	30.35	27.53	29.54

Figure 16: Length of the radius, the lower limb represented by 100. In male and female Negroes and Europeans.

This is not the case, however, and Broca finds that the radius is longer in Negroes than it is in Europeans, but they still have shorter upper limbs in comparison to their lower limbs (Broca, 1867, 651).

Broca summarizes his results in five points: 1) based on the comparative arm-to-leg length, the Negro is further from the apes than is the European; 2) based on the length of the humerus compared to that of the femur, the Negro is again further from the apes than is the European; 3) based on the length of the humerus compared to the radius, the Negro is closer to the apes; 4) the extreme length of the radius in comparison to the humerus depends, in some degree, on the shortness of the humerus and the radius of the Negro is longer than that of the European compared to the lower limb; and 5) the upper limb of the Negro both approaches the European type (radial length) and the simian type (humeral length) (Broca, 1867, 652-3). Broca accepts that in studying the skeleton one often finds mosaic characteristics, despite being a superior or inferior race. He mentions again that the Hottentot, which is well-understood to be inferior to the Negro, often resembled the European type in its measurements. For Broca, this is clear evidence in support of the polygenist belief in the plurality of human origins (Broca, 1867, 653).

Surprisingly, Broca, unlike Morton, does not engage in anthropometry with a clear racist framework. In fact, he has stated, in reference to his measurements:

Incidentally, we are not looking to establish the superiority or inferiority of this or that part of the genus Homo; we are only looking to see if Ethiopians and Caucasians could have the same origin and if the considerable differences seen in the cranial morphology of both types could be explained by anything but the diversity of human origins. The volume of the brain is nothing more than a question of curiosity for us. (Broca, 1858, 405).

This is in direct opposition with claims made by scholars, such as Stephen Jay Gould, who explicitly accused Broca of using “numbers not to generate new theories but to illustrate a priori conclusions,” (Gould, 1981, 74). If this were true, Broca would have never published his 1867 anthropometric piece for it completely contradicted every pre-existing accepted notion of the inferiority of Hottentots and the intermediate placement of Negroes between Europeans and the anthropoid apes. I argue instead that Broca was using numbers to generate new theories on human hybrids.

When conducting these anthropometric studies, Broca takes special note of hybrid individuals. For example, in his early facial projection measurements, he wonders where to classify intermediary types (Broca, 1862c, 87). Additionally, he measures the radii and humeri of a mulatto and mulatress when doing his preliminary anthropometric measurements of Negro and European arm bones in 1858. He finds that the individuals do not represent an intermediate form, but that the Negro characteristics have persisted, making Broca wonder whether upper limb length is one of the characteristics transmitted matrilineally, the first time he makes mention of such a theory of inherited traits (Broca, 1862d, 167). Broca measures the clavicle of the mulatress as well and gets a measurement longer than the median clavicle length of a pure race female Negro, more evidence that the mixed-race individual is not taking on an intermediate form, but adhering more to the black race, which is probably that of her mother (Broca, 1862d, 171). He redoes these measurements in 1867 along with the other skeletons but does not discuss them in detail (Broca, 1867, 649). Interestingly, though Broca borrowed both his four degrees on hybridity and his method

of measuring cranial capacity, from Samuel George Morton, Morton himself deliberately excluded hybrids from his craniometric samples as did his students, Nott and Gliddon, when they took over the American School of Anthropology after his death (Morton, 1849, ix; Nott & Gliddon, 1854, 371). Hybrids only become theoretically interesting with Broca and he is the first to attempt to quantify these hybrids, developing anthropometry to do so and dedicating twelve pages in his anthropological field guide to studying hybrids (Blanckaert, 2003, 61).

Broca's *Instructions générales pour les recherches et observations anthropologiques (anatomie et physiologie)* (1865/1879) is a compilation of all of Broca's observations of human physiology and measurements of the human body. It is unique in that it outlines how to study living people, rather than skeletal remains. In the twelve pages dedicated to hybrids, Broca establishes a guide to identify, measure, and evaluate racial métis. He poses a number of observations and tips for describing hybrids and then poses the question:

Do métis of the first degree resemble one race more than the other, or do they present a more intermediate type? Describe the characteristics of this métis and document the on the individual observation sheets; one should not be limited to noting skin color, hair type, and facial morphology; one should practice measurements of the head, the trunk, and the limbs that are indicated in our table (Broca, 1865, 113).

Broca explicitly defines the link between the development of his anthropometric measurements and his desire to quantitatively measure and describe hybrids. It is worth noting that in his 1919 pieces on Anthropometry, Aleš Hrdlička, founder of the American Journal of Physical Anthropology and the first curator of physical anthropology at the Smithsonian Institution National Museum of Natural History, who mentions Broca as the “father of anthropometry”, also mentions how to, if possible, recognize and identify mixed-race individuals. It is interesting, however, that just forty years after the publication of the second edition of Broca's *Instructions générales pour les recherches anthropologiques à faire sur le vivant* (1879) that the progeny of members of different races are again being excluded from measurements (Hrdlička 1919, 187). Perhaps Broca's untimely death in 1880 is to blame for the sudden halt in studying hybrids (Schiller, 1979, 288). Though hybridity, both in animals and plants, had been studied for centuries before him, Broca was the most involved and attached to these studies and was the first to deliberately fashion categories of hybrids and develop measurements for their study.

M A K I N G U P P E O P L E

Ian Hacking is a Canadian philosopher of science who served as chair of Philosophy and History of Scientific Concepts at the Collège de France from 2001 to 2006, becoming the first Anglophone elected to a permanent chair in the Collège's history (Collège de France; Miller, 2006, 1480).

Both in his inaugural lecture at the Collège de France and in several papers, Hacking makes reference to a philosophical project aimed at the analysis of practices used in classifying peo-

ple, how these classifications affect said people, and how these people then change their classifications (Hacking, 2006, 23). He maintains that people, and particularly scientists, sort people into finite groups based on particular characteristics with the goal of controlling, organizing, or simply understanding the groups of people under classification (Hacking, 2006, 23). For Hacking, though, the process is not unidirectional, since these classifications are interactive: the people we classify are “moving targets because our investigations interact with them, and change them” (Hacking, 2002, 11; Hacking, 2006, 23). This is what he calls the “looping effect”, when the categories we impose on individuals change the ways in which they act, thus rendering the categories false, requiring a historical perspective on the construction and application of classificatory concepts (Hacking, 2006, 23; Hacking, 2002, 11).

To understand the classifications of people and the looping effect, one must look to the philosophical debate between nominalism and realism. Nominalists are particularists and deny the reality of kinds of things and categories made to organize these kinds. Instead, they suggest that only the names applied to things are what groups of things have in common. Using stars as an example, an extreme nominalist would say that the only thing stars have in common is their name. On the contrary, a realist would argue that there is something inherent to all stars, some single characteristic or set of characteristics that are sufficient and necessary criteria for inclusion in to the class of “stars,” justifiably situating them in that category (Hacking, 1986, 164). It is not hard to argue on the side of realism when dealing with some classifications of people, such as male or female, short or tall, fat or thin. But nominalism has its merits, especially when dealing with borderline cases such as hermaphrodites or people of average or middle height or weight (Hacking, 1986, 164-5). When discussing the categorization of people, Hacking adopts a position he calls dynamic nominalism, eschewing static nominalism and realism in preference of the claim that “a kind of person came into being at the same time as the kind itself was being invented” (Hacking, 2002, 10-11; Hacking, 1986, 165, 170). Hacking admits that this idea traces back to other thinkers, such as Friedrich Nietzsche (Hacking, 2006, 24). In his inaugural lecture at the Collège de France, Hacking quotes an aphorism from Nietzsche’s *The Gay Science*: “Unspeakably more depends on *what things are called* than on what they are . . . creating new names and assessments and apparent truths is eventually enough to create new ‘things,’” (Nietzsche, 1974 [1887] quoted in Hacking, 2002, 7). To Nietzsche, the act of classifying abstractions and giving them names creates real entities that did not exist before.

Classification is fundamental to the natural sciences and, according to Hacking, there has been a push in the human sciences, such as anthropology and medicine, to treat people as “objects of scientific inquiry” and classify them in the same way (Hacking, 2002, 11; Hacking, 2006, 23). He says, “Sometimes, our sciences create kinds of people that in a certain sense did not exist before. I call this ‘making up people,’” (Hacking, 2006, 23). To explain what he means by this, Hacking cites numerous clinical examples, one of which is multiple personality disorder, which experienced a sudden burst in diagnoses during the 1980s (Hacking, 2006, 25). Hacking presents two sentences:

A. There were no multiple personalities in 1955; there were many in 1985.

- B. In 1955 this was not a way to be a person, people did not experience themselves in this way, they did not interact with their friends, their families, their employers, their counsellors, in this way; but in 1985 this was a way to be a person, to experience oneself, to live in society (Hacking, 2006, 25).

For Hacking, both A and B are true. Although some would disagree with the validity of A, whether due to a preference for the name Dissociative Identity Disorder, or simply because they do not believe the disorder exists, Hacking argues that everyone would agree with B. B is an example of making up people (Hacking, 2006, 25).

Hacking refers to multiple personality, or Dissociative Identity Disorder as a “transient mental illness”, transient meaning of “existing only at a certain time and place” (Hacking, 2006, 3). These ideas of transience and historical context were deeply explored by French physician and philosopher of science, Georges Canguilhem (1904-1995) who theorized on the formation of a concept and the idea of historical epistemology, or the idea that “philosophical problems are inseparable from their historical milieu” (Méthot, 2013, 112). For Canguilhem, these “philosophical problems” include “genuine (scientific) concepts”, which he defines as “dynamic and complex entities comprising three components: a phenomenon, a denomination, and a definition” (Méthot, 2013, 112, 119; Schmidgen, 2014, 245). He contends that there is a strong relation between experimentation and the formation of concepts “that gestures towards the wider social and cultural contexts in which those processes operate” (Méthot, 2013, 120).

Hacking refers to multiple personality, or Dissociative Identity Disorder as a “transient mental illness”, transient meaning of “existing only at a certain time and place” (Hacking, 2006, 3). These ideas of transience and historical context were deeply explored by French physician and philosopher of science, Georges Canguilhem (1904-1995) who theorized on the formation of a concept and the idea of historical epistemology, or the “a posteriori reconstitution of forms of knowledge in a given domain” (Méthot, 2013, 112-3). Canguilhem contends that there is a strong relation between experimentation and the formation of concepts and that in biology especially, “the core issue is perhaps not so much ‘using experimental concepts’ but rather ‘experimentally constituting authentic biological concepts’ (Méthot, 2013, 119).

Broca’s and M. Roux’s experiments crossing hares and rabbits to produce leporids were an attempt to establish the concept of animal hybridity. After M. Roux’s breeding experiment was complete, Broca claimed that “the scientific experiment still leaves something to desire” (Broca, 1877, 476, translated by the author). He thus began to observe M. Roux’s leporids of varying degrees in an attempt to systematically differentiate them from the parent species based on their physical appearance. He took note of their size, fur color and texture, ear length, and length of their hind limbs (Broca, 1877, 479-81). Eventually, Broca introduces several other measurements, such as nostril size, size of the head and eyes, and both absolute and relative (to the hind limbs) length of the forelimbs (Broca, 1877, 480-1).

Broca uses these observations as a launchpad for his study of human racial mixing. He admits that conducting similar breeding experiments on humans is both physically and morally impossible, so one must observe the hybrids that emerge from the natural crossing of human races (Broca, 1877, 491). In the twelve pages dedicated to identifying hybrids in *Instructions générales pour les recherches anthropologique à faire sur le vivant (anatomie et physiologie)* (1865), Broca demands that hybrids be studied based on hair and skin color, hair texture, head size, body size, and limb lengths, the same qualities that he studied in the leporid hybrids (Broca, 1865, 113).

By applying his methods of studying animal hybrids and to the human domain, Broca demonstrates Canguilhem's notion of the "filiation of concepts" or "the historical process by which concepts emerge out of scientific ideologies and are captured by a theory or succession of theories" (Kritzman & Reilly, 2006, 469). The scientific ideology from which Broca's concept of hybridity emerges is the polygenesis of the human species, which he claims to have proven by demonstrating that distinct species can produce fully fertile young. The theories that Broca develops from his experimentation with hybrid leporids and his studies of human hybrids allow him to formulate new biological concepts, such as his four degrees of hybridity, which, according to Canguilhem, "are the result of newly opened spaces of experimentation and foster at the same time the developments of new forms of practice" (Méthot, 2013, 119). By experimenting with leporids, Broca introduced, and took advantage of, the possibility of studying human racial mixing, thus creating the concept of an identifiable human hybrid that can be systematically distinguished from the parent races through various measurements and physical observations.

Together, Canguilhem's and Hacking's ideas propose various methods by which concepts are created. By arguing that experimentation is the method by which concepts are formed, Canguilhem introduces one of the ways in which people are made up (Méthot 2013, 119).

Hacking presents a five-part framework for "making up people": "(e) the experts or professionals who generate (d) the knowledge, judge its validity, and use it in their practice. They work within (c) institutions that guarantee their legitimacy, authenticity and status as experts. They study, try to help, or advise on the control of (b) the people who are (a) classified as of a given kind" (Hacking, 2006, 24). Operating within this five-part framework are several "engines of discovery" – that is, technoscientific imperatives -- that Hacking (2006, 1) suggests are used by the human sciences as "engines for making up people". Some of these engines are:

1. Count!
2. Quantify!
3. Create Norms!
4. Correlate!
5. Medicalise!
6. Biologise!
7. Geneticise! (Hacking, 2006, 6)

These engines are used to develop part (d) of Hacking's framework and can be associated with the experimentation that Canguilhem argues is key in the formation of a concept (Hacking 2006, 1; Méthot, 2013, 119).

With his studies on human hybrids, Broca was, in effect, "making up people". Extrapolating Hacking's five-part framework to Broca's 19th century work, we have (e) the expert, Dr. Pierre Paul Broca who introduced (a) the classification, the term "hybrid" which is used to describe (b) the people, which are mixed-race persons. The word was used to denote the crossing of individuals of different races as early as 1843 by Josiah Clark Nott, but it first appeared in the Oxford English Dictionary in 1861, the year following Broca's final publication in his series on hybridity, *Des phénomènes d'hybridité dans le genre humain* (1860) (Young, 1995, 6). The OED states, "A few examples of this word occur early in 17th cent.; but it was scarcely in use till the 19th" (OED). Before 1861, a human hybrid did not exist. The word of choice was "mongrel", "mixing", or "métis" (Young, 1995, 6; Douglas, 2015, Broca, 1858, 433). But Broca, who was known to conflate species and race, also conflated the terms mongrel, mixing, and métis with hybrid, thereby creating an entirely new way of naming and understanding human difference (Broca, 1858, 433). The word hybrid's "appearance between 1843 and 1861, therefore marks the rise of the belief that there could be such a thing as a human hybrid" (Young, 1995, 6). This new scientific classification gave rise to a new social classification, which created the possibility for scientific *and* social changes that would affect and be affected by the mixed-race individuals who were the target of the classification.

Continuing with Hacking's framework, (c) the major institution in Broca's making up hybrids is the Société d'Anthropologie, which he founded with the sole purpose of continuing his work on hybridity after his preliminary papers were rejected and his research was halted by the Société de Biologie in 1858 (Schiller, 1975, 129, 130-1). Aside from his illustrious Société, in 1867, Broca created an anthropological laboratory dedicated to (d) the knowledge, or the science of anthropometry (Carson, 2007, 100). In producing fine distinctions among human hybrids, Broca needed more precise measures:

As anthropologists moved from the classification of primary to secondary races, the number of morphological peculiarities necessary to separate races increased, and these were more and more subject to quantification. Color alone would usually distinguish Negro from Caucasian; but to separate Nordic and Mediterranean one must observe - and measure - not only pigmentation, but stature and headform as well" (Stocking, 1968, 56).

As he shifted his focus from intelligence to the more complex question of hybridity, Broca developed and applied these precise measurements - not just of the head, but of the entire body -- to not only distinguish between the races, but to scientifically analyze the products of their crossings.

Broca was the first to systematically quantify the human body, not just the cranium, and he developed these methods to better understand hybrids, a concept that depended upon as well as buttressed his fundamental belief in the plurality of human origins. No matter Broca's

personal beliefs on racial equality¹² his scientific re-inscription of profound human racial difference introduced methods and concepts which extended racist science¹³. Although Broca's phrase "eugenesic," and indeed his entire hybrid classificatory system, has been largely forgotten, it is notable that Francis Galton's word "eugenic" has a shared etymology. Galton coined this word in 1883, in his *Inquiries into Human Faculty and Its Development*, defining it as:

the science of improving stock, which is by no means confined to questions of judicious mating, but which, especially in the case of man, takes cognizance of all influences that tend in however remote a degree to give to the more suitable races or strains of blood a better chance of prevailing speedily over the less suitable than they otherwise would have had" (Galton, 1883, 25).

Galton did not cite Broca in this work (although he did read him, e.g. Eaton 2013, 148), but the parallels between *eugenic* and *eugenesic* are striking. A few pages after the definition of "eugenic," Galton (1883, 40-41) laments the want of anthropometric laboratories and registers: "When shall we have anthropometric laboratories, where a man may, when he pleases, get himself and his children weighed, measured, and rightly photographed, and have their bodily faculties tested by the best methods known to modern science?" By 1865, Broca had developed these registers and opened such a laboratory in Paris by 1867, but it was not until 1883, three years after his death, that his measurements and methods were appropriated to support a racist agenda (Carson, 2007, 100).

C O N C L U S I O N

In the historical background sections of this work, various 18th and 19th century views on human origins and variation were discussed in relation to Broca's own polygenist way of thinking. Then, the earliest attempts to measure human difference were mentioned as a preface to Broca's own experience with craniometry. The bulk of the paper focused on Broca's attempt to prove polygenism through his study on hybridity for which he developed his anthropometric measurements. The final section related Broca's attempts to identify hybrids to Georges Canguilhem's ideas on concept formation and Ian Hacking's "making up people", arguing that Broca developed anthropometric measurements to systematically categorize human hybrids, thus "making up" a human hybrid, a concept that did not exist before. Although studies of hybridity did not continue after his death, Broca's word "eugenesic" was appropriated by Francis Galton to coin the term "eugenics" and Broca's anthropometric methods were used by others in racist science.

Although in secondary literature Broca is often portrayed as a racist who uses his measurements to prove *a priori* expectations, perusal of the primary literature shows that he was perhaps the purest scientist of the 19th century. He developed precise measurements in order to quantify human difference, but did not explicitly enlist them to establish a hierarchy of human races. Instead, he sought only to prove the theory of polygenesis.

In many ways, Broca was the most influential figure on the field of anthropology as it exists today. His treatise on hybridity formed the basis of French anthropology and led to the foundation

of the Société d'Anthropologie de Paris, the first society devoted to the science of anthropology, which Broca defined as “the study of the human group, considered in its entirety, in detail, and in reference to its relation to the rest of nature” (Stocking, 1968, 40; Broca, 1871, 1). His contributions to the field transcend his own country as his version of anthropology served as a model for biological anthropology as a discipline in Germany, England, and the United States (Brace, 2005, 149).

Arguably, Broca’s most important contribution to physical anthropology was the development of anthropometric measurements of not only the skull, but the entire human body. Today, these measurements are commonplace and are used in studies of not only anthropology, but human biology and evolution, biomedicine, and forensics. Although the methods themselves are not racist, it is important to recognize their historical context and that they are predicated on a polygenist way of thinking and developed in response to an existing, 19th century claim of a primordial difference between the human races.

E N D N O T E S

1. Buffon did not use the term “hybrid” -- according to his logic, hybrids, in their traditional definition, do not exist (Douglas, 2015, 4).
2. Josiah Nott was also among Gobineau’s followers, stating “I have seldom perused a work which has afforded me such pleasure and instruction as the one of Count Gobineau, ‘*Sur l’Inégalité des Races Humaines*,’ and regard most of his conclusions as incontrovertible,” (Nott, 1856, 463).
3. Phrenology, a popular 19th century pseudo-science begun by Viennese physician Franz Joseph Gall (1758-1828), was the study of the size and shape of a person’s skull, used to determine a person’s character and mental abilities. It was based on the notion that certain mental traits are associated with certain parts of the brain, for which the cranium can be used as a proxy (Brace, 2005, 66). The divisions between early craniology and phrenology were not always clear: Scottish phrenologist George Combe (1788-1858) popularized phrenology in America, and Morton allowed him to write an appendix to his *Crania Americana* (1839).
4. Although Tiedemann was not fully aligned with phrenology, he did proceed from certain of its tenets:

There is undoubtedly a very close connection between the absolute size of the brain and the intellectual powers and functions of the mind. This is evident from the remarkable smallness of the brain in cases of congenital idiotismus, few much exceeding in weight the brain of a newborn child. Gall, Spurzheim, Haslam, Esquirol, and others, have already observed this, which is also confirmed by my own researches. The brain of very talented men is remarkable, on the other hand, for its size (Tiedemann 1836, 502-3).

5. This was his own, abbreviated English translation of his German book *Das Hirn des Negers mit dem des Europäers und Orang-Outangs verglichen* (1837).
6. He specifically names Petrus Camper, anatomist Samuel Thomas von Sömmerring (1755-1830), naturalist Georges Cuvier (1769-1832), surgeon and naturalist Sir William Lawrence (1783-1867), and Julien-Joseph Virey (1775-1846). He directly attacks Camper's facial line and angle as "unsatisfactory in determining the capacity of the skull, the size of the brain, and the degree of intellectual powers," (Tiedemann, 1836, 511).
7. Broca contends that Morton made this distinction to differentiate between individuals that clearly belong to different species (e.g. dogs and wolves, goats and sheep) and those whose origin is not agreed upon by all naturalists (e.g. principal races of dogs, horse, and humans) (Broca, 1877, 433).
8. The French word "*léporide*" refers specifically to the métis produced by crossing a hare and a rabbit and does not have an English equivalent. The English word "leporids" usually refers to members of the family *Leporidae*, including rabbits and hares, and translates into French as "*léporide*". For the purposes of this paper, I will use the English "leporid" to refer to Broca's term "*léporide*".
9. Broca's *Recherches sur l'ethnologie de la France* (1859) was his first attempt at describing the effects of racial mixing on a population. He stated that mixed individuals are easily distinguished from their mother races (17) and usually take on intermediate characteristics (Broca, 1859, 18). Based on the diversity in France, he attested that French people presents all the characteristics of a mixed race (Broca, 1859, 19).
10. Wherever relevant, direct translations of Broca's categorical terminology will be used because they were his delineators of populations. Many of these terms are today considered to be derogatory. They do not, in any way, reflect the opinions of the author, nor does she advocate their use.
11. Broca is hesitant to accept Nott's explanation for the differential fecundity of mulattoes at different latitudes before getting an alternative explanation (Broca, 1877, 527-8).
12. "Incidentally, we are not looking to establish the superiority or inferiority of this or that part of the genus *Homo*; we are only looking to see if Ethiopians and Caucasians could have the same origin and if the considerable differences seen in the cranial morphology of both types could be explained by anything but the diversity of human origins" (Broca, 1877, 405, translated by the author).
13. "From another point of view, it might be said that the polygenist doctrine assigns to the inferior races of humanity a more honourable place than in the opposite doctrine. To be inferior

to another man either in intelligence, vigour, or beauty, is not a humiliating condition. On the contrary one might be ashamed to have undergone a physical or moral degradation, to have descended the scale of beings, and to have lost rank in creation” (Broca, 1864, 71).

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A Cuban Medical *Herstory* An Analysis of Primary Care Delivery Systems to Women in Cuba Part Two

Ivana Kohut

A B S T R A C T

This study specifically evaluates the primary-level healthcare infrastructure that is currently made available and used by women in rural Mayajigua, Cuba. In making use of ethnographic interviewing techniques with women of various ages (three age cohorts: ages 18-32, ages 33-69, ages 70 plus) paired with intensive participant observation in the field, this study achieved a thorough analysis of how women access, perceive, and interact with their primary-level healthcare system at large, specifically the *Consultorio Médico de la Familia* [CMF] and local polyclinic. Two salient themes pertaining to the primary tier of the socialist Cuban healthcare system (primary-level healthcare is analogous to general medicine in the US healthcare system) include: CMF experience in the clinic and the medical irony of access to “acute” versus “active” care. In this study, discussions of the CMF experience and of care access more generally revolve around the central theme of access to healthcare in Cuba being defined as a “fundamental human right” as written in currently upheld health policy. Additionally, this study aimed to better assess the translation of written health policy into practice so as to better interrogate how primary-level healthcare in Cuba is delivered to women as a “fundamental human right” in practice.

I N T R O D U C T I O N A N D B A C K G R O U N D

When the average American hears the word “Cuba” what frequently comes to mind are images of beautiful beaches, vintage cars, and Spanish colonial architecture. But what is often not associated with the island is an intricately designed healthcare system that has enabled Cuba to achieve incredible healthcare feats on the state-wide level, notably: an “infectious disease mortality rate of 1.1%” in 2014, an HIV/AIDS transmission rate of “0.1% among the population between 14 and 49 years of age” in 2009, and an infant mortality rate “of 4.6 in 2012” (WHO IRIS 2014). Scholars, such as those at WHO IRIS, have argued that because the “Cuban Social policy defines the state as responsible and the citizen as beneficiary” the socialist healthcare platform achieves competitively low global mortality disease rates since the system is inherently “oriented towards sustainable human development with the goal of wellbeing and the highest standard in quality of life” (2014). Correspondingly, scholars have largely focused on the *noso-politics*¹ that uphold the Cuban healthcare’s social and political infrastructures despite Cuba’s Third-World status.

A general theme among scholars studying Cuban healthcare has been the importance of the state’s provisioning of care as a “fundamental human right” (Iatridis 1990, 29). This notion of health has been particularly powerful for scholars like Iatridis and Sixto, since the system’s “success requires political commitment to equitable socioeconomic development and community participation”

¹ The politics of health. Specifically, *noso-politics* refers to the theory that it is the government’s responsibility to create and uphold care systems that will allow healthcare to be made available as a basic human right.

(Iatridis 1990, 29). Upon extension, the socialist healthcare system is not successful based on the socialist nature of the Revolution alone, but rather on “mutualism” (Sixto 2002, 325). Sixto defines this mutualism as the major cornerstone of the health equity policy since mutualism involves active health participation on all societal levels. For Sixto, this mutualism provides an additional explanation as to why the Cuban healthcare system is categorized into four levels: primary (general non-specialized care that occurs at the neighborhood level), secondary (specialized care that occurs at the municipal level), tertiary (high specialty care that occurs at the provincial level), or quaternary (super specialty care that occurs at the national health research level).

Similarly, scholars have argued that the primary level is the single most imperative and uniquely identifying aspect of the Cuban healthcare system. For example, Huage argues that “the burden of the public health system rests on the shoulders of [...] consultorio (clinic) [CMF] teams [...] or the assignment of a nurse and doctor to every 150 families [which] established continual community-based patient care” (2007, 40). In this way, the doctor quite literally becomes “of and for the people” (Randall 1981) because s/he is tasked with living in the community. Further, Randall claims that the Consultorios Médicos de la Familia² (CMFs) not only make healthcare more accessible to rural dwellers, but also ensure a high quality of community, public health. For example, in the event of a disease outbreak, it is the responsibility of the CMF doctors to lead health “charlas” (talks) and to inform the community members on how they can continue to live healthy lives. It is also the responsibility of the CMF care providers to go on “terrenos” (Huage 2007, 40) and visit ill comrades when they are unable to come to the doctors’ office themselves. Further, for some scholars such as Reed and Keck, this “embedding [of] health professionals in the communities meld[s] public health with clinical medicine” (2012, e2). In this way, for many scholars, the primary care level acts as a safeguard and the first line of defense to ensuring community health.

Though the research conducted has been instrumental in analyzing how the Cuban state has achieved disease rates comparable to a First-World country, much of the data has been grounded in health policy and statistics. To date, there is no health ethnography nor health-focused qualitative study on primary care system implementation. Because most of the research and work conducted has prioritized statistical and policy analysis, the voices of the Cuban citizens and their experiences with the primary care system have been neglected. Moreover, there has not been an emphasis on women’s healthcare research, which is particularly interesting since the Cuban healthcare system explicitly prioritizes women’s and maternal health. Consequently, this study analyzes the question: How do the experiences Mayajiguan Cuban women (of ages 18+) have with the post-Revolutionary Family Medicine healthcare system [CMF] compare with the family medicine healthcare system as legally written? Subsequently, the aim of this research has been to give the statistics a social context and to forefront the stories and experiences that *women* have had with the healthcare as is outlined in policy in order to best analyze how healthcare is not only made available but used by Cuban women daily. In this way, healthcare efficiency

² The CMF is a numbered two-story building in which the doctor lives on the second floor (often for years or decades at a time) and sees patients on the first floor. The CMF is the vehicle for primary care delivery as established through the Cuban healthcare system, given that each 150-family community has their own CMF.

in practice (not only in policy) can be better evaluated, especially as made available to women.

This qualitative approach to analyzing primary care systems is hence critical since it would offer a completely new perspective in academia and policy making given there is no qualitative study on women's health that involves the active participation of women. The topic also matters personally since my family is Cuban and I have witnessed both successful and troubling aspects of the primary care health system, many of which cannot be captured by the statistics alone. Ultimately, the aim is to collect and forefront these "herstories" so as to inform policy that will allow Cuban women to have even better access to care, especially considering the changing geopolitical relationship between the United States and Cuba.

M E T H O D S

Mayajigua, Cuba lies in the central province of Sancti Spiritus, Cuba approximately 407 kilometers southeast from La Habana capital city. It was in Mayajigua, a small, rural farming town that is often referred to as a "barrio" (neighborhood) of Yaguajay³, where the month-long 2016 qualitative study was conducted. Mayajigua is particularly known for its natural thermal springs, which are in turn famed for their dermatologic, gastric and arthritic healing capacities. Furthermore, although the springs have attracted medical tourists from around the island, Mayajigua is also known as one of the poorest "barrios" in central Cuba. The historic lack of humanitarian aid sent to Mayajigua has caused poverty rates to rise, which manifests itself very physically in the deteriorating community infrastructures and homes which are mostly composed of large outdoor seating areas (porches) coupled with minimal indoor space. Moreover, the town is unique in that it has a strong "Revolutionary" nature that manifests itself not only in town programs but also on smaller, social levels.

Because homes in Mayajigua are largely composed of outdoor seating areas and porches, the "Woman on a Porch"⁴ recruitment strategy was implemented. In the summer months, the porch serves a double function since it is the coolest place in the house and a social center (especially when young people are on vacation and when adults return from work). The specific strategy was to walk randomly from street to street at least twice a day (once in the morning and once at night) and seek women who would be willing to participate. Additionally, this technique proved particularly powerful considering that women were comfortable and open in the interview since the space where the interview was conducted was one in which the women quite literally felt "at home." Secondly, this recruitment strategy enabled snowball sampling. Women who enjoyed being interviewed would either insist that their relatives, neighbors or friends (on other porches) also be interviewed (and they would provide introductions) and/or would call to people from other porches to co-participate. Likewise, the interviews themselves were semi-structured, but the more fluid "river-approach" (Rubin and Rubin 2012, 124) was utilized as-needed—as with larger groups.

³ Yaguajay City is 28 km from Mayajigua's town line.

⁴ Effectively the "Man on the Street" approach.

Moreover, formal interviews were not randomly selected. Since the research question overtly involved the gathering of experiences, it was critical to have three different age cohorts represented so as to obtain data that could specifically clarify how each stage of development in a woman's physiology (from young to aged) experiences and interacts with the healthcare system. This need ultimately led to "purposive interviewing" in which I "decided the purpose [I wanted interviewees] to serve and [went] out [to] find some" (Bernard 2006, 43). The cohorts created were as follows: young women (ages 18-32), middle-age women (ages 33-69) and elderly women (ages 70 plus). Consequently, a total of 12 formal qualitative interviews lasting approximately 45 minutes to an hour and a half with 4 women from each age cohort were conducted. Six informal qualitative interviews (interviews that were not audio recorded and that were conducted with women due to random snowballing or networking) were additionally conducted to obtain more information and were recorded through jottings post-interview or note-taking during the interview depending on immediate access to writing materials.

When analyzing the data, the 12 formal interviews were transcribed using the audio recordings saved to an iPad. These formal interviews were then coded in NVivo and were specifically reviewed for common themes in which parent nodes were first outlined and then subcategorized into child and grandchild nodes. From these nodes, a coding tree was created to better analyze emerging themes and trends in the experiences collected. Coding was also intentionally executed using not only the information presented by women in the interview (inductively), but also based on the question (deductively). The deductive coding was particularly powerful considering that the interview questions constructively guided the coding tree's formation and facilitated the emergence of themes that were found not only in individual interviews but also between interviewees, which allowed for a cross-comparison of information.

However, because the informal interviews were not audio-recorded, collected notes were added to field notes at the end of each day. When writing the field notes, information was written as-is while personal impressions, reflections and opinions were included in brackets in the body of the text. Once the field notes from the trip were compiled upon return, the field notes were coded in Word based on salient themes emerging from not only observations but also from the informal jottings that were included in the set of notes. The field notes were also coded inductively. Consequently, the nodes found from the field notes were then compared with the nodes from the interviews. This triangulation of data sources also made possible the juxtaposition of obtained information which further prompted analytic cross-comparisons between data sources.

Also, note that because all interviews were conducted in Spanish, transcription writing, jotting writing, and coding all occurred in Spanish. Translation into English was the last step of the process in which major quotes and anecdotes are either translated or summarized. In turn, policy analysis and additional information from a previous Women's Healthcare study in 2015 serve as context for the 2016 findings. Major policy points from the Cuban 1983 Public Health Law are compared with the field findings to analyze how the ideal execution of healthcare (as established in policy) compares with the primary care that is made available/used.

Lastly, IRB Exemption status was achieved in 2015 from Penn's Review Board. This status was renewed for the 2016 study, which was approved as a partner to the 2015 study. Similarly, because all interviews were conducted anonymously and all recordings were destroyed after transcription, informed consent was received verbally prior to the start of the interview.

R E S U L T S

Upon analysis of the coded 2016 data, two general findings emerge. The first major theme is that, although the written policy attempts to make health available equally to all as a "human right," in practice there are large discrepancies in how primary care is delivered through the CMF. Salient minor themes include a discussion on privacy, wait-period, and the use of "terrenos" (home-visits). A second major theme is that in practice there is an emphasis on making only "acute forms" of medicine and care readily available. For instance, in 2015, one of the women interviewed stated that because of the Cuban healthcare policy, "I can receive IVF treatment for free. [...] I don't have to pay a buck." It is possible for women to have free access to extremely expensive and potent therapies, including IVF, chemotherapy, and various inoculations, but women do not have equal access to less acute forms of medicine notably feminine care products, non-prescription pain medications, band-aids, Alka-Seltzer, body soap, diapers, baby cream for diaper rashes, Benadryl—this list is not exhaustive. More simply, Cuban women do not have equal access to "active care," or care that an individual can actively seek and obtain for themselves without a prescription; this in turn indicates that many Mayajiguan women struggle in leading consistently hygienic lives with access to means that provide efficient relief in times of discomfort/pain (that do not require acute care).

A. Results Collected from Interviewees

Discrepancies in access to privacy when visiting the physician proved to be a key, identified sub-theme since there is no written and legally upheld HIPPA law or equivalent. More than half of the women interviewed stated that when they visit their CMF physician there is no privacy.⁵ Instead, the trend is that a patient is seen in a room with other patients so as to increase the efficiency of the system: it is easier for the physician to view six or seven patients at once especially when, as interviewee Kristina⁶ stated, "the doctor is seeing you for something normal." Here, Kristina defined "something normal" as a check-up in which the doctor would not conduct an invasive exam or test where a patient would be required to undress. For Kristina, when the doctor sees several patients at once for "normal" checkups, the physician time is significantly cut because the doctor can quickly assess and move on. Likewise, of the women who stated that they do not see their CMF physician in private, only Yulia admitted that the lack of privacy unnerves her. Specifically, Yulia stated that, "I wouldn't like it if I had to divulge personal information, or if I were getting negative results to a test." Upon subsequent discussions, for Yulia, the lack of privacy was truly only uncomfortable if she had to describe

⁵ Even when women go to hospitals or to polyclinics, upholding patient privacy is not a priority and doctors will often divulge identifying medical information in front of staff and other patients.

⁶ Note that to uphold the privacy of the women interviewed, pseudonyms are used in this academic paper.

a severe physiological complaint that would other her from either healthy individuals or individuals that would heal relatively quickly without severe, stigmatizing health consequences.

On the contrary, the women who stated they saw their doctor in private explained that they could have a more one:one consultation because they knew the doctor personally. For example, some of the women interviewed stated that their physician was either blood related (their child or niece/nephew) or was such a good friend that it was “as if the doctor were part of my family.” For these individuals, the close relationship established with their care provider prior to the visit, not because of the visit, was responsible for a treatment that was more one:one and personal. This discrepancy in consultation privacy correspondingly relates to how well a woman knows the physician considering that women who have more personal relationships with their doctor are frequently found to have the added benefit of a more private consultation.

Likewise, wait-period emerged as an interesting sub-theme to the CMF experience. All of the women interviewed stated that the wait to see a physician “depends on the line.” As Edna stated, “I get in the line and I wait my turn because that’s how it works here.” The question then becomes not if a woman will be seen by her CMF physician, but when. For example, Kristina explained that when specialists come to visit the CMF and the local polyclinic, the line is particularly long because “everyone in town wants to see a specialist.” Due to volume, patients often wait hours in line though a consultation is not guaranteed. For example, Kristina stated that she “waited all day to see an orthopedist, but my turn didn’t come.” As a result, Kristina had to travel 35 kilometers to see the specialist. She also stated that even when she travels she is not guaranteed a visit because her turn with the specialist also “depends on the line.” Hence, her healing process has been delayed on multiple occasions because her “turn” in the line did not occur while the physicians were visiting Mayajigua or while the physicians were having visiting hours. Similarly, this fact was echoed by Maya who explained that she and some of her peers (especially those in need of orthopedic care) would wait long periods of time but would not be seen. Maya further explained that when this occurs she or her friends would, “look for him, travel to the hospital [in Yaguajay] because there are always doctors in the hospital, or see the doctor in the street.” When prompted to elaborate, she stated that “if you see a doctor in town, and you have a question, and your turn didn’t come, then you ask him right there.” Again, the question is not if one gets care, but when and where. Conversely, for Bella and Yulia, certain circumstances allow two groups of people to be expedited through the line. For example, Yulia stated that when her first-born child was still an infant and was sick, the people in the line allowed her to be seen by the doctor first. Similarly, Bella stated that “because I have this cane, and white hair, and wrinkles, people mistake me for a little old lady and they allow me to go first in line.” Correspondingly, though the CMF experience is often categorized by a long wait- period, if one has a sick child or if one is elderly (Bella was 94 yo when interviewed) it is a generally socially accepted norm to allow the elderly and sick infants to be seen first.

Another sub-theme was the experience women have with “terrenos.” As defined in the health policy, a terreno is a recorded home-visit. Similarly, as outlined in the health policy, the CMF

physician is expected to spend only the morning in the consultation center considering that in the afternoons s/he is expected to conduct home-visits with each family (especially households who have ailing loved ones). The purpose of the home-visit is two-fold. The first, as stated by several women, is to demonstrate “healer humanity” in which the healer makes a valiant effort to conduct a thorough and personal *terreno*. The second purpose is to increase accessibility to the doctor (especially if the patient is physically unable to visit the doctor in the consultation center). For example, Marí stated that she has, “arthritis, diabetes, problems with my heart [... And my doctor] comes here, to my house [... because] going around walking is not easy.” The *terrenos* thus are useful because they facilitate physician access to not only sick individuals, but also to older individuals. By extension, public transport exists, but is expensive and uncomfortable (consists mainly of riding in the flatbed of a truck, the back of a trailer or tractor, or in a cramped, hot bus). In this way, the *terrenos* are an alternative. And though this alternative exists, one of the women who was informally interviewed stated that “doctors only come to visit you in your home if you are friends, or if you can offer them something like a meal.” Thus, in principle the *terrenos* should be a way to enable easy access uniformly, especially to those who are physically unable to “go” to the doctor, but in practice this is not the case. The “obsequio,” or gifts that are given to physicians (not mandatory by any means granted that physicians are paid by the state), can also serve as encouragement for certain doctors to visit and can also be interpreted as a bribe. The *terreno* also allows the doctor to actively assess the health status of the community because they allow the physician to actively study the impact of patient lifestyle to patient health. For Marí this has particularly been the case. For instance, when Marí was hospitalized, her doctor reported information on her living situation to the State in the medical report, and Marí received funds from the State to refurbish her house. “80,000 pesos⁷ they sent me and they fixed my house with a table, bathroom, sink, everything.” However, although this has been the case for Marí, Bella was also hospitalized when she was diagnosed with cholera. Even so, her living conditions were not reported and she continues to live without running water and with only an outhouse (unlike Marí).

A second general theme is the lack of access to over-the-counter products and medications. As I found in a 2015 exploratory healthcare study in Cuba, 75% of the 89 women interviewed stated that they do not have access to over-the-counter (OTC) products or medications. In 2016, I found that it is this fact that makes living in Mayajigua “not easy” which was echoed either implicitly or explicitly by Yulia, Kristina, Bella, Marí, and Edna. For instance, when I met Yulia her second-child was a few weeks old. The child did not have many diapers

and due to the heat and the reusable cloth, had a terrible diaper rash. Yulia explained that the non-prescription medication for the diaper rash was sold at the international pharmacy. However, she was unable to go to the pharmacy for several reasons: a) it was too long of a walk, b) the medication was too expensive (over-the-counter products and medications

⁷ This figure is given in CUP—not the national money. This figure is equivalent to 3,200 CUC or 2,720 USDS (using the currency conversion from 2016 of 1 CUC = 0.85 USDS).

are not freely made available unlike expensive treatments), and c) the international pharmacy caters to tourists and so the likelihood of her even being able to enter the store was low. Yulia's experience reflects healthcare inequality. Moreover, there are products that are simply no longer being made nor entering the country which further reduces access to "active care" as defined earlier. For instance, during the interview, Bella called the pharmacy to verify if there was any Biripilina (Cuba's version of aspirin). The pharmacist explained that there wasn't because the main ingredient used to make the drug was no longer available in Cuba. However, despite a lack of access, Edna stated that she has access to OTCs because of the generosity of her American family who send her these products. This again demonstrates that healthcare is not equally made available to women considering that women who have family or connections from outside the country have a better access to "active care."

Furthermore, a sub-theme was the use of "Green Medicine" as an alternative to OTCs. Because finding OTCs is extremely difficult and expensive, all women stated that they use "Green Medicine," or alternative medicine, as a healing technique. The state also supports "Green Medicine" and has health "charlas" (talks) and television programs that explain which herbal concoctions, vegetables, fruits, etcetera provide healing. For instance, Mari used the meat of a pumpkin to treat her daughter's acne, while Yulia's grandmother eats pumpkin seeds as an alternative to Alka-Seltzer, granted "the pumpkin was nothing like Alka-Seltzer, but was better than nothing." And though "Green Medicine" offers some comfort and healing, it is by no means a worthy substitute for OTCs. For example, Bella stated that she cannot make use of most of the "Green Medicine" techniques because of a medical condition that prohibits her from consuming various roots and vegetables that are often used in the herbal concoctions.

B. Results Collected from Participant-Observation in the Field Site

Though Mayajigua is considered a "small" town by comparison to larger urban centers such as La Habana City, it still is relatively large with approximately 350 households. This fact in of itself is interesting since the CMF was designed to service approximately 150 households, as mandated in health policy. Moreover, the CMF physician does not live in Mayajigua because, as casually explained by several women, the doctor lives in Yaguajay with his family. Moreover, the fact that the physician does not live in Mayajigua negatively affects patient wait-period. For example, when I was in the field I had an allergic reaction and went to the polyclinic for treatment since the CMF physician had returned home already. At the polyclinic, I waited an hour before being seen by a physician (there was only one physician on staff at the polyclinic) who told me that they did not have any Benadryl at the clinic and that I instead had to find transport to the Yaguajay hospital. In turn, at Yaguajay, I waited three hours before being seen and an additional hour and a half before being medicated. As a result, I found that there are not enough physicians for the volume of patients given that the Mayajigua polyclinic must service not only Mayajigua but also other Cubans that live on the outskirts of the town. Likewise, the Yaguajay hospital must service patients that are "forwarded" to them from other towns, like Mayajigua. The volume of patients is unmatched by physician volume.

In addition to a lack of doctors and medication in Mayajigua, my experiences in the field also demonstrate a general lack of food sources. In Mayajigua, I lived directly in the community and there were days where I would only be able to eat one meal because of food allotment. In speaking to one woman about the food rationing, she suggested that I travel to the Keyes, which are a high-end resort for foreign tourists.⁸ Towards the end of my visit I took a day-trip to the Keyes where I witnessed the difference in resource availability. There was simply more in the Keyes—more food, feminine care products, Internet access, consistent electricity, soap, milk, fruit, rice, sugar.

Consequently, based on these themes from the interviews and field observations, in practice the primary healthcare system was not found to be ubiquitously available nor accessible to all—both in access to physicians and in access to more basic, active forms of care. Tables 1 and 2 specifically illustrate this lack of access to active forms of care, especially in comparison to acute care (notably vaccines).

Table 1 demonstrates several examples of resourceful workarounds women in modern Mayajigua use so as to have some form of self-care in response to everyday pains or discomforts. Again, this resourcefulness is often the solution to a lack of access to OTC products as earlier discussed.

Symptom	Unavailable OTC Solution	Green Medicine Solution
Stomach Pain or Discomfort	Antacid	Pumpkin, Anon, Hot Water with burnt bread crumbs
Acne, Burns and Rashes	Burn Creams	Aloe gel, Tomato meat, Pumpkin meat, Banana leaves
Arthritis Pain	Ibuprofen	Concoction of Cuban garlic, pure Cuban alcohol, and Cuban tobacco [fermented for 20-21 days in the Cuban sun on the roof]

Table 1. Examples of Green Medicine Alternatives

Table 2 provides some illustrative quotes obtained in formal interviews with the women of Mayajigua in 2016. The quotes primarily illustrate the medical irony in providing acute forms of care over active ones, in addition to illustrating access to physicians more generally.

⁸ The Keyes are a gated community. In order to have access, one must present a valid foreign passport.

Theme	Notable Quote	Interviewee
Active Care Accessibility	~ “Here in Cuba, a kid can be without shoes, but never without vaccines.” ~ “Oh, Alka-Seltzer. [...] Here, there aren’t band-aids, nor Alka- Seltzer, none of that.” ~ “Oh, how I wish I were in the United States. Mmm-hmm. In that country, the supermarkets have rows and rows of milk for miles.”	~2015 interviewee quote ~Kristina ~Bella [stated in informal conversation]
Doctor-Patient Accessibility	~ “[The doctor] gave the vaccines to my daughter here in the living room.” ~ [In reference to medical transport] “Like I said to you before, you find a way to get there.”	~Yulia ~Marí

Table 2. Care Accessibility: Illustrative Quotes

D I S C U S S I O N

I began this study to better understand the implementation of primary care infrastructures in the field in Mayajigua in search of a clearer understanding of how healthcare is made available and subsequently accessed by women. The resulting data has in turn emphasized that lived experience constructs multiple health realities for the women in Mayajigua, which ultimately has demonstrated that healthcare is not made available equally to all. Moreover, women who require more acute health services as earlier described are prioritized over women who seek and require just as important active forms of care. Upon reflection of the described themes, two interesting medical ironies arise. The first is that healthcare is made available equally to those who have the means to foster stronger relationships with physicians. The second is that healthcare is made equally available only if a patient requires acute treatment instead of active care.

What is particularly striking about the privacy minor theme is that women who had a more personal relationship with the physician benefited more from their healthcare than women who did not have these kinds of relationships. Upon interpretation, this might have been the case because the doctor already had an established, familial relationship with the patient and the doctor would thus want to care better for this person out of respect and obligation to this previously established relationship. A dilemma arises because not everyone is fortunate enough to personally know or be related to a physician, thus the difference in treatment further suggests that care provision is not universally equal in practice. Moreover, the treatment delivery can also differ considering that women who have weaker personal relationships with the doc-

tor will not be able to benefit from a more holistic medical review. This is to say that when a woman has a very strong relationship with her doctor (established well in advance before a visit or medical introduction), the doctor can create courses of action that are tailored to her specific sociocultural contexts because the physician knows not only the woman but the life that she lives. For instance, when a doctor knows a woman well (and knows her family) s/he will have access to information that will help him/her create a course of action that is feasible for that woman specifically. Because not all physicians establish these relationships with their female patients, women who do not have strong relationships with their doctors are inherently at a disadvantage because the doctor is incapable of more fully understanding the sociocultural contexts that inform her illness narrative and experience (compared to those patients that have well established relations). Furthermore, because the CMF physician does not live in Mayajigua, the discrepancy is propagated since women who are not intimately connected to their physicians do not even have a means to forge a more intimate relationship with their doctors.

The second interesting point pertains to wait-period. As stated in the Results, the general lack of doctors and the lack of a physician who lives in Mayajigua's CMF is significant since Mayajigua is left without a senior healthcare professional that is simultaneously healer and citizen. Although it can be argued that there is no need for a CMF physician who lives in the community considering that Mayajigua has a 24/7 polyclinic, this violation of the health policy has impacted wait-period. Firstly, there is not enough consistent healer manpower to service the population. Secondly, because the doctor does not live in the health community, s/he does not have the opportunity to forge strong relationships universally with patients because s/he is not simultaneously healer and comrade. The result is thus not only differences in the strength of the relationship, but once again elongation in wait-period since the physician must take more time with each patient to understand the medical history since s/he is not living in the community and understanding the effect of other social and public factors in the patient's life at least somewhat before the patient arrives in the office. A third explanation for long wait-periods is Cuba's political tendency to conduct health diplomacy in which thousands of doctors are exported to countries such as Brazil, Haiti, and Angola in return for political compensation. The consequence is that there are fewer doctors at home nationally who can service the domestic sick which again results in longer wait-periods.

Additionally, the lack of physician-community immersion and the lack of ubiquitous healer humanity can also explain why not everyone has the opportunity of a "terreno." As stated in the findings, some women explained that only those who know the doctor have the benefit of the terreno. Because the doctor is not living with the community, s/he is not able to forge personal relationships with each person and thus, not everyone knows the doctor personally which in turn means that not everyone will benefit from a terreno especially if the doctor only visits those people s/he knows personally. Furthermore, realistically, not all doctors have healer humanity as discussed in the Results, and are instead driven by monetary compensation. For example, the average educated, working Cuban (including professionals) makes approximately 17.50 USDS a month. And though price is not the defining factor for a career choice, such a small salary especially among educated professionals does indeed impact how such healers provide care. In other words, fam-

ilies that can spare extra food or even gifts for the doctor, “obsequios,” have been known to have more private, home-visits with the doctor. Because not everyone can afford to give the doctor an obsequio, the care delivered is not universally equal. In this way, though the obsequio is not mandatory (doctors are employed and receive salaries from the state), for some women who do not get home-visits the obsequio is a socially mandatory form of payment they cannot afford to give.

Correspondingly, the historic lack of availability and national access to OTCs has its root in the historic lack of humanitarian aid sent to Cuba. In 1962, John F. Kennedy officially enacted the Embargo against Communist Cuba. Since then, it has been difficult for Cuba to have access to many products that are relatively accessible in the United States, notably US pharmaceuticals and OTCs. Even so, in the twentieth century and in the first few years of the twenty-first century, Cuba was still able to have access to equivalent products through the political alliances the country had with China, the USSR and later Russia, Venezuela, and Brazil. Granted, Cuba was not able to obtain higher brands of products (name brands for OTCs) or even large-scale product quantities, but the political alliances allowed for products to at least be generally made available and rationed. However, because Cuba has heavily relied on long-term foreign assistance, the country’s political alliances have not been able to “give” enough to satisfy Cuba’s great need. An unfortunate consequence has thus been a cycle of numerous Special Periods since 1962. Special Periods are defined as periods of time in which it is difficult or impossible to find certain needed goods/products that are imported. For example, in a Special Period the government states that for the good of the commune the people must “stand together” and make use of what they have since certain products will become impossible to find. These Special Periods over the course of 56 or so years have been responsible for rising poverty levels in Cuba and the increased use of “Green Medicine” as an alternative to OTC products and medications. For instance, doctors have also been known to suggest “Green Medicine” concoctions. This does not mean that the doctor is writing a prescription for a Green Medicine alternative, but rather that the physician recognizes the difficulty in obtaining certain products and has consequently legitimized alternative medicine as a plausible improvisation to providing care.

Moreover, in June 2016, the Regime declared that Cuba entered the worst Special Period in Revolutionary history. Products that were previously scarce became and continue to be non-existent since June. As stated in the Results, even products that Cuba used to independently manufacture can no longer be made since the main ingredients are unavailable. This means that the government now provides even less active care than it did prior to the 2016 Special Period. The root of this aggressive and recent Special Period also lies in Cuba’s political circumstance. For instance, the US Embargo is still currently in place. Additionally, China decided that it could no longer afford to send products to Cuba as it has previously, Russia cut funding to Cuba by approximately 5 billion USDS, and with the political unrest in Venezuela and Brazil, Cuba has taken a significant cut in the amount of benefits it receives from these countries as well.

Similarly, with the Presidential visit Obama made to Cuba earlier in 2016, the Cuban regime decided to redirect the resources it has towards American tourism. This redistribution is effectively a

domestic cut of available products to the people since the government is prioritizing tourism over citizen interest. The experiences elaborated on in the Results (specifically the difference between Mayajigua and the Keyes) also suggest that the Special Period is not only the result of foreign aid cuts and the Embargo, but that there is indeed a tangible redistribution of resources away from Cubans which has in turn reduced access to active care, healthy nutrition, and a higher quality of life.

These findings are hence particularly interesting because they add to the body of literature by elucidating the significance of active care and the Special Periods. In the past, scholars such as Huage and Randall, have incorrectly stated that there was one singular Special Period in Cuba. However, in practice there have been multiple Special Periods that have each been related to Cuba's financial and political predicaments. This data particularly demonstrates that the Special Period is dynamic and that it has not technically "ended" nor will it. That is, many scholars including Sixto and Paul Farmer, portray Cuba as a medical paradise based on policy. Nevertheless, because there are limited studies that involve direct analysis of Cuban citizens' experiences, much of the human suffering due to lack of access to active care goes ignored. The current statistics simply do not provide information on how many people get only one meal a day, how many girls skip school because they do not have enough feminine care products for their period or access to adequate pain-medication for menstruation cramps, or on how many old women live without soap or milk access. Moreover, these findings add to the body of literature because they validate the importance and necessity of reliable active care to rural Cuban women. Further, the findings suggest that access to active care is intricately linked to not just the Embargo, but to the Special Periods (plural) more generally. Thus, the broader implication of these findings is to use these "herstories" to dispel the notion that Cuba is a medical paradise. Indeed, Cuba has achieved incredibly low disease and mortality rates on the national level. Nonetheless, the suffering and discomfort of average people due to inaccessibility to equal care delivery and active care has been silenced by these statistics. The implication is thus to stimulate conversations that elevate the importance of active care to acute care when writing and executing health policy so that the "average" Cuban (not just the terminally ill) can truly have access to a higher quality of life.

That said, there are still limitations in the data that can be used to inform future research projects. The first is to conduct a quantitative study to numerically describe how many people do not have access to basic yet critical forms of "active care" like nutrition, pads, and so on. A second future direction is to analyze exactly why certain towns, such as Mayajigua, do not have a CMF physician living in the community as legally established. It would be specifically interesting to study who decides if a doctor can live outside of the community s/he serves and under what circumstances. Likewise, it would be interesting to quantify the "severity" of each Special Period in relation to the historical or political contexts of the State at the time, and also to track how Cubans have been able to improvise workarounds. Lastly, it would be interesting to conduct a study based on healthcare delivery in relation to political affiliation. For example, in the field, Bella was known as a sympathizer of the Counter-Revolution (her closest relatives were exiled). However, Marí was known as a Revolution sympathizer and was a close neighbor of the commune's political president. Considering that both Marí and Bella are elderly women who were hospitalized

for acute care due to harshness of living conditions, yet only Marí received the added benefit of compensation for her living quarters, it would be interesting to analyze if there is any difference in treatment delivery to those who actively support the Revolution versus those who do not.

Upon conclusion, it is critical to reiterate this project's broader implications for policy and practice. As the relationship between the US and Cuba continues to change, research such as this project is significant because this research centers the conversation on benefitting Cuba over American tourist or business ventures—particularly the Cuban town instead of the Keys. Such research also promotes peace because the research demonstrates that Cubans and Americans can work together for a common good—better access to care—as opposed to working against each other for a political conversion. In this way, conducting research on the healthcare system in Cuba can lead to not only conversations, but even to the sharing of policy and resources so that healthcare access truly becomes a universal human right not just on the page but in practice in both Cuba and the US.

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When Healthcare Becomes Governance
Excerpted from:
Health Citizenship in The “Forgotten District”: The Intersec-
tion of Non-Profit Governance and the Ugandan Welfare State
Jessie Lu

A B S T R A C T

The increasing presence of non-profit and non-governmental organizations as healthcare providers in Uganda has altered the role of the nation state in providing health services. This alteration of the nation state is especially salient in Bududa district, located in a region of Uganda that has been labeled as “the forgotten district,” where traditional modes of the government oftentimes do not reach. Through three months of ethnographic fieldwork conducted in Bududa, this thesis attempts to address the ways in which individuals understand their relationship to the government vis à vis the health welfare system, and the ways in which the presence of the non-profit alters this understanding. I argue that the non-profit clinic alters citizen relations to the state because of its ability to provide the health services that the state cannot. I establish that political forms of the state do not directly reach the rural district of Bududa. Rather, healthcare becomes the primary way through which individuals interact with the Ugandan government.

M A I N T E X T

Modern ethnographies of the state have increasingly begun to examine the changing role of the government of the nation state in the globalized world. In their introduction to *The Anthropology of the State*, Aradhana Sharma and Akhil Gupta (2006) write about the state as an entity constantly reconstructed through culture and context and thus substantiated differently in the everyday life of various populations. The anthropology of the state in the globalized world moves beyond examining the state as bound to the nation and instead examines a type of transnational governance that transcends national boundaries and allows institutions beyond the state to begin to exercise power. Within this form of governance, Sharma and Gupta note the importance of rethinking hegemonic ideas of the role of the state and instead focusing on attempting to understand the ways in which Western ideals of poverty, health, and development all co-constitute a “culture of governmentality,” which dictate the ways in which government should be enacted, but become embodied differently in various contexts around the world. Within this framework, institutions outside of traditional state apparatuses fulfill the functions of the government and offer alternative ways to think about governance. This understanding has been appropriated by governments in low-income countries, where welfare that promotes development has become prioritized tasks of the state as a method of exercising power, at least in theory.

Sharma and Gupta’s analysis on the changing nature of the state in a globalized world is situated in a broader discourse examining the ways in which the state currently exists. Drawing from Philip Abram’s 1988 distinction between the state-system as the practices of the state and the state-idea as the ways in which the perception of the state becomes reified as a societal construction,

scholars have noted the difficulties around studying the state as a well-defined entity. Rather, the state can be studied through what Timothy Mitchell (2006) terms a “state effect,” the processes by which the state becomes imagined as an entity separate from civil society. Other scholars of the state such as Michel-Rolph Trouillot and his colleagues (2001) argue that any analysis of the state should be examined in relation to civil society, since the ways in which the state manifests itself on the ground exists within a social contract between state and society. The state, though nebulous and difficult to define, can become examined through the effects it has on civil society.

In addition to the state, government and governance also operate as key terms in this section. Throughout this chapter, I want to think of governance through the definition provided by Didier Fassin in his introduction to his book *Humanitarian Reason* (2012). He argues for an expanded definition “as the set of procedures established and actions conducted in order to manage, regulate, and support the existence of human beings: government includes but exceeds the intervention of the state, local administrations, international bodies, and political institutions more generally”(1-2). Through this definition, Fassin introduces both the notion that governance may apply broadly to aspects of individual existence and this notion may be enacted through institutions outside of standard governing institutions. His expanded definition becomes particularly pertinent in my examination of healthcare, in which I see governance as specifically applying to the ways in which institutions affect the physical existence of human beings at the most foundational level.

Given these definitions of state and governance, I establish in this chapter the ways in which Uganda’s health welfare system becomes one of the primary ways through which individuals in Bududa understand their relationship to the state. I show how in Bududa, the welfare system—and the health welfare system in particular—becomes an institution through which state governance is enacted and citizenship is negotiated. In the rural district, the national democratic framework of the Ugandan nation state breaks down and political participation becomes defined through institutions outside of the Ugandan government. Within this process, other forms of citizenship, such as economic citizenship (in terms of inclusion in a global economy) or medical citizenship (in terms of the receipt of adequate and globally competitive healthcare), become more important than a strictly Ugandan citizenship. In this context, the health welfare system, and specifically the site of the clinic, emerge as an institution through which individuals in Bududa interact with the state because that is the only entity that is available.

THE BREAKDOWN OF DEMOCRACY IN BUDUDA

In his ethnography examining the translation of Ugandan national development policies on the ground in the Teso region of Eastern Uganda, Ben Jones (2009) finds that individuals in rural Uganda feel that the government has pulled out of their lives. Government policies in rural districts cater more toward abstract international development expectations than toward the tangible and urgent needs of the rural poor. As a result, rural Ugandans feel disconnected from the state and other forms of social connection, such as connection

to and service provided by religious institutions, become more influential in initiating local social change, especially for the sake of economic development. From here, Jones notes a larger trend in which individuals in rural districts in Uganda have felt increasingly marginalized by and excluded from the state. James's thoughts on the disconnect between rural districts and central governments in African countries is not new; he cites Goran Hyden, who coined the controversial term "uncaptured peasantry" in 1980 to describe populations in the Tanzanian countryside ineffectively reached by a weak post-colonial government. Indeed, scholars of foreign aid in Uganda have questioned the ways in which external donor assistance and funding has been used by the Ugandan government to promote its own political image rather than to eradicate poverty in practice (Oloka-Onyango and Barya 1997).

These same sentiments of disconnection between the Ugandan state and its rural citizens are echoed by many of the individuals in Bududa, just Southeast of the Teso region where Ben Jones performed his fieldwork. In Bududa, distrust of the government is situated in expectations around welfare provision and in feelings of isolation from the democratic processes that create the state. Because of this, distrust of the government fuels political disengagement, which in turn reinforces further distrust. Put another way, for individuals in Bududa, the failure of democratic voting processes to place preferred leaders into power reinforces a desire to exist in isolation from the formal government. This distrust and subsequent loss of faith in government leaders leads to a functional absence of the state for individuals in the district. Or rather, a functional absence of the state effect as defined by strictly political terms. The state becomes less visible in its political effects and more visible in other effects.

Discourses of distrust of the government circulated constantly around the Bushika non-profit clinic throughout my time in the field. Negative stories were shared between staff members and among patients. Throughout the summer, the clinic faced ongoing struggles with the district health officer (DHO), who refused to recognize the legitimacy of the new clinic because she believed the clinic leadership owed her a bribe. Government officials who had previously lent benches to the clinic for patients waiting to be seen took away the benches overnight for a town meeting in July and then never returned them. In the summer of 2016, the entire country was coming out of an election in which the opposition leader had supposedly won the popular vote, but the longstanding Museveni administration remained in power (Karimi 2016).

For Sarah, the in-charge of MCH, one of the midwives at the clinic, and a 28-year-old woman from the capital of Kampala who moved to the Eastern region for work, the election systems often favor political leaders unsupported by rural populations. Elections in Uganda are mediated and governed by the Electoral Commission (EC), which was established as part of an article in the 1995 New Constitution for Uganda. The EC was tasked with independently overseeing elections in Uganda, which are currently held every five years, and was created partly in response to the political instability that characterized Uganda's early government in the second half of the twentieth century post-independence. Despite its theoretical creation as part of the constitution, the EC did not successfully organize an election un-

til the 2006 elections, more than a decade after its formation (Electoral Commission 2016). For Sarah, however, the EC excludes rural populations rather than includes them. She notes,

The people can support somebody else, then by virtue of anything like popularity elsewhere, maybe by the electoral commission, someone else goes through whom the public does not wish. So, the majority of the public, most of the people, the poor people in the villages, just keep looking, they look at whatever is going on as if life just continues.

In her assessment, Sarah describes the process by which decisions made by the EC and voters in other areas of Uganda become more important than the voices of the individuals in rural areas who vote for elected leaders. Through such a system, the rural poor observe the events from what Sarah describes to be an outsider point of view. Through her narrative, Sarah shows a distance between individuals in rural areas and the government.

In her analysis of the creation of the state in modern Haiti, Chelsey Kivland (2012) identifies a form of what she terms “disordered governance” in the post-dictatorial democracy. In Haiti, despite the institution of a new regime of a supposedly stable government, individuals perceived a sense of statelessness. She argues that “Ultimately, the notion of statelessness serves to express both the need for a normal or proper ‘state’ of public life and the inability to locate a sovereign authority that is responsible for providing this life”(250). In Uganda, a similarly post-dictatorial democracy, these same ideas of statelessness could be applied to the individuals in rural areas who cannot find a state that they perceive to be serving them in public life.

This sense of statelessness through a lack of perceived government authority becomes accentuated through stories of distrust that circulate through communities. Phiona, an eighteen-year-old secondary school student visiting the clinic, described her perceptions of the government.

Okay, you can just hear from some people I’m talking to in the village. Everyone knows those people [government workers] are not really working. Like, they can send them money to help a certain village, for helping poor families, widows, and people who cannot help themselves. For them, they just use on their own things, they don’t take that money to those people whom their government has directed them to take for them.

Phiona describes a normalized and generalized knowledge around government workers as corrupt and selfish, which is underscored by narratives shared by the other members of her village. These government workers, under the control of a larger “government,” which perhaps has better intentions than the local government, hoard money and results are not seen on the ground. Of note in Phiona’s narrative, these discourses are premised under the assumption that government work, especially at the local level, must be measured by the welfare benefits given by the state and by the use of funds for the needy who cannot help themselves. In this sense, the efficacy of the nation state government becomes strongly tied to the ability of the government to distribute welfare benefits to its poor. On the other hand, scholars have also noted how govern-

ments use welfare to reify their power and organize their subjects (Jessop 1999). Thus, the Ugandan government, in its failure to help the poor, both becomes perceived as important precisely through its potential to provide welfare benefits to the poor, and yet fails in its perceived power.

These themes, in addition to and informed by Jones's work, highlight the importance of understanding how individuals find alternative means of interacting with the government, given this disconnect between rural Ugandans and the state. In the context of the abstract feelings of isolation from the government experienced by individuals in Bududa, it is even more important to untangle the specific and concrete ways in which people do interact with the government. Traditional ways of understanding citizenship begin to break down as individuals choose not to participate in democratic processes; nor do they have extensive interaction with the state because of their distrust of the government's ability to conform to their expectations of the ways in which the state should serve them.

B U D U D A A N D A U G A N D A - K E N Y A C I T I Z E N S H I P

Throughout the course of my research, I began to see Bududa as particular in its location close to the Kenyan border, which placed Kenyan urban centers and Ugandan urban centers as equally accessible. I offer this section as a way of problematizing a notion of Ugandan national citizenship and as an example of a circumstance where alternative forms of citizenship become necessary for survival, especially in an increasingly globalized economy. In his theorizations of citizens in the context of social change, Maurice Roche (1987) describes three aspects of citizenship: a civil element that consists of individual rights to freedom, a political element that consists of the right to participate in institutions of power, and a social element that consists of the rights granted by the economic welfare state, which compasses rights to security and rights to adequate living standards. I want to think about how social citizenship becomes a dominant form of citizenship for which individuals in Bududa strive, which is most easily seen in the ways in which they search for economic opportunity in Kenya. Of course, this discussion opens up a whole new area for exploration, which is beyond the scope of this paper. Still, it offers an example of the multiple other ways in which citizenship in Bududa is negotiated and necessitates the need to examine other forms of citizenship and governmentality in the district.

The geographical proximity of Bududa to Kenya allows for free movement of individuals across international borders. Since the independence of Uganda and Kenya in the mid-twentieth century, border towns have served as essential centers for the movement of resources, items for sale, and people across the border (Lorch 1994). Out of the twenty individuals interviewed for this study, four had been to Kenya in search of economic opportunities or had family members who had done so. By comparison, only one individual had moved to Kampala, the Ugandan capital, in search of economic opportunity. Researchers examining internal and international migration in sub-Saharan Africa have noted the drive of economic opportunity for border crossing as compared with internal migration. Especially in the case of South Africa, where high levels of economic development mean increased employment opportunities,

researchers have noted that employment concerns drive cross-border migration while internal migration tends to be characterized by both employment concerns and social concerns (Wentzel, Viljoen, and Kok 2006). Migration in sub-Saharan Africa has thus increasingly become an economic survival strategy in light of stagnant socioeconomic conditions and political instability (Adepoju 2000). Movement from Uganda to Kenya is additionally facilitated by similarities in language and culture (Adepoju 2006). Individuals who leave for Kenya generally return to Bududa, but their return to the country is precipitated by family and economic situations rather than for a desire for civil or political citizenship within the nation of Uganda.

In Bududa, movement into Kenya is motivated by multiple factors. Mercy, a nineteen-year-old mother cited the strength of the Kenyan shilling against the Ugandan shilling. She described, “let’s say you work [in Bududa], you work but the payment is low, now people prefer Kenya. Whereby you get something little, you convert it to here, it’s a lot.” The promise of stronger currency and its somewhat free circulation across the border allowed for wealth that would be otherwise difficult to obtain in Bududa, but also in the entire country of Uganda. Other informants offered stories of jobs and better education in Kenya, which contrasted with the subsistence farming prevalent in Uganda and the low quality education offered in the rural districts.

In the reverse direction, movement into Bududa is equally unregulated. Individuals return to Bududa for various reasons, which include marriage, a desire to be with their families, or the loss of economic profitability in Kenya. For example, another of the clinic midwives Jane had been born in Kenya to parents originally from Bududa. Her family had moved to Kenya in search of employment opportunities, and her mother found work as a housemaid and supported the family as a single mother after she separated from her husband. Later, her mother contracted HIV and was forced to return to Uganda, fearful of stories of forced euthanasia of HIV positive individuals in Kenya. Once driven back to Bududa at the age of seventeen, Jane spoke vaguely about the process through which she reclaimed her Uganda citizenship when she and her family returned to her mother’s family in Bududa. When asked about the citizenship process, she replied, “Okay, the process I don’t know very well. Because when we came this side, I live permanently this side and I have ID for Uganda.” She was unable to speak of how she got this ID for Uganda, nor did she share information on her experiences—they were, from her point of view, insignificant and uneventful.

Given this freedom of movement across borders in search of economic opportunity—specifically economic opportunity rooted in the stability and strength of the globalized Kenyan economy—what happens to perceptions of citizenship and feelings of loyalty to the Ugandan state? Relationships with the Ugandan state seem to be pushed aside in favor of broader forms of inclusion. The freedom of movement in search of economic opportunities between Uganda and Kenya demonstrates a deprioritization of formal connections with the state. Given this lack of interest in explicit forms of governance from the Ugandan state, compounded with a general lack of trust and interest in the government as articulated in the previous section, how can relationships between citizens of Bududa and the state can and should be examined in other ways.

THE WELFARE CLINIC AS STATE

In the context of these fluid forms of Ugandan citizenship in Bududa, and as mentioned before, social welfare services become a key way through which individuals interact with the government. Social welfare in Bududa is predominantly delivered through the institutions of health and education. Especially when juxtaposed against a neglected physical infrastructure and a corrupt law enforcement system, social welfare in Bududa become a consistent and indeed essential way through which individuals access the services given by their government. In this context, though not the only institution through which governance can be enacted, the clinic becomes an important way for understanding how individuals interact with the state. This is not to say that the clinic is the only site through which to understand citizenship in Bududa, but it provides insights into understanding rural citizenship vis à vis institutions outside of the classical understandings of governance, which fail to properly reach the population of the district.

Michel Foucault (1984) writes about the concept of biopower to describe how power in modern society is based off of governance that regulates the life of individuals. For Foucault, the government and its associated laws become normalizing institutions that regulate populations by creating strict definitions of normality in life. This comes in contrast to the regulation of populations through death found in most exercises of power prior to modern times. He extends this analysis to examine health in the eighteenth century, where he traces new state experiments that treat the body as an object to be controlled. Thus, government politics in the eighteenth aimed to improve the health of populations as a means of enacting power, and the clinic became a site through which governments could reinforce their power over populations by defining health standards and regulating treatments. Such a reformulation of government politics opens the door for the analysis of health as a way through which power can be enacted. Through this framing, the clinic in Bududa and its ability to regulate health can be seen as a way through which the state may exercise authority. That is, the clinic becomes a site through which government power is enacted and through which this power must be analyzed. This is especially important given the political system in Bududa in which government control in a politically democratic sense is perceived as absent by the people.

Individuals in Bududa, though they feel distanced from the government, nevertheless recognize the ties between the clinic and the state. This knowledge is especially well explained in the viewpoints of healthcare workers. James, the in-charge of the clinic and a clinical officer at a HCIV in a neighboring district, notes the driving forces behind the Ugandan government's creation of the free health care system in 2003. For him, the government changed its policy "because now everything, they are like pushing it to politics. They want to make it free so that people can think that the government is helping." In this statement, James gives the government credit for recognizing its failure to properly attend to its citizens. Simultaneously, he notes that government uses its supposedly democratic health care system to construct an image of efficacy. Health care from the top down becomes a way through which the government connects with its citizens on paper. This mentality is consistent with much of the government rationality behind creating a decentralized system. Decentralization ideally brings

government services closer to the people, constructing an image of a competent and accessible government. Scholars have noted, however, that as decentralization has become normalized, its motivations have changed and it has increasingly also become a means through which the government consolidates power (Awortwi and Helmsing 2014). In the end, the creation of a theoretically accessible government serves to reinforce the power of the state.

The intrinsic ties among the government, citizenship, and health are further illustrated by the expectations of the clinic midwife Sarah, who in describing the Ugandan healthcare system notes that “the citizen is a business of the government.” When asked to elaborate on this statement, Sarah uses the example of death in the clinic, explaining that a death in a health facility would be a reason for the government to “come in with its long arm [and] reach you wherever you are.” In this blunt statement relating regulation of the state to the biology of the individual, protection of citizen life becomes a responsibility of the health clinic. When the clinic fails, the government comes in and intervenes. The clinic thus becomes an intermediary institution. The government oversight and regulation of the clinic for the sake of protecting its citizens further underscores the presence of the government specifically as it relates to health. Indeed, every health clinic is responsible for reporting statistics for each month to the Ministry of Health. Surveillance of health thus becomes a main task of the state and the clinic allows for its execution.

The perspectives of James and Sarah support the idea of the post-eighteenth century clinic as a site of governance, especially as it pertains to Bududa. The government exercises its control in Bududa precisely through its ability to regulate the health of populations. In people’s lived experiences, the government perhaps does not concretely execute power over populations. Nevertheless, the image that the government strives to create underscores the importance of the clinic as a site through which the government can create an image of exercising control for both itself and for its citizens. These connections between the government, its free clinics, and its potential to care for its citizens underscore the importance of examining governance specifically as it relates to health in Bududa.

Though the interrelatedness of the government and healthcare clinics are less explicitly stated in opinions of the general public, there is nevertheless an appreciation of the government specifically because of the healthcare it provides. That is, individuals in Bududa recognize the potential of the government run healthcare clinics. When asked about the benefits of the free government clinics, almost every individual answered that the free clinics were a good thing. Mercy, the nineteen-year-old mother from Kenya visiting the clinic notes that the free clinics are “good mostly in the village ‘cause more people here are so poor actually mostly in the village, they cannot be able to afford themselves actually in private hospitals.” The government, with its ability to provide welfare benefits to the poor if only on paper, is more appreciated when speaking in the context of healthcare than when speaking of the state in general. Still, though individuals praised the government’s ability to provide healthcare for the poor, many individuals would add that they personally would never use the clinics though they themselves identified as poor. In this way, the government successfully creates an image

of providing for its citizens. This image, however, does not necessarily become internalized by individuals in Bududa and the benefits of the clinic remain inaccessible and thus unrealized.

Thus, the clinic becomes a significant way through which individuals understand their interactions with the state. It is through the clinic that individuals most potently feel the state effect. Though they feel disconnected from their government officials or disappointed by their ability to participate in the governmental system, people in Bududa recognize the state within their healthcare systems. As a result of this, citizenship, as navigated through the healthcare system, is essential for understanding how individuals feel included or excluded by the state, and how they exist under a Ugandan government and within national identity that is not necessarily viewed as the most important element of citizenship with which they chose to identify.

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Gestural Communication and the Social Complexity Hypothesis

Emma McNamara

A B S T R A C T

Whether language has vocal or gestural origins remains a heated debate among researchers. The social complexity hypothesis for communication, which posits that heightened complexity in social interactions drives heightened communicatory complexity, could play a role in deciding this debate. McComb & Semple (2005) provided a piece of potential evidence in favour of the vocal origins of language when they showed that vocal repertoire size correlated positively with social group size across a sample of 42 primates, or, broadly, that primate vocal communication is consistent with the social complexity hypothesis. To see if this trend also holds true with primate gestural communication, a regression analysis was performed with the gestural repertoire data from Call & Tomasello (2007), the only comparative study of primate gestural communication to date. No significant correlation was found between social group size and gestural repertoire size in this dataset. While this could reflect that gestural communication is not, in fact, as strong of a candidate for the origins of language as vocal communication, this lack of correlation could also be related to issues with the dataset, or issues with quantifying social and communicative complexity. In order to truly elucidate the relationship between gestural communication and the social complexity hypothesis, however, further comparative research on intentional gesture use in primates needs to be carried out.

I N T R O D U C T I O N

Language can be defined as, “a system for representing and communicating complex conceptual structures, irrespective of modality” (Fitch, 2000:258). Because speech is so central to our idea of language today, it is easy to assume that language and speech are, in some ways, synonymous, and that vocalisations have always been at the centre of language’s origins and evolution. Yet the “irrespective of modality” statement is actually central to our understanding of language. As Arbib, Leibal & Pika point out, “the performance of language is multimodal, not confined to speech” (2008:1053). Gestures often factor heavily into human language, with children using gestures to communicate before they develop vocal speech, and manual gestures frequently accompanying adult speech (Arbib, Liebal & Pika, 2008). This has led many scholars to argue that language initially evolved from gestures. Hewes (1973) is largely credited for reviving the gestural origins of language hypothesis with his article in *Current Anthropology*. He argued that the ability to learn gestural signals is shared between humans and the great apes, and therefore, “manual communication may thus come close to representing the deep cognitive structure on which not only language but all our intellectual and technological achievements can rest” (Hewes, 1973:65). Since this suggestion, there has been a renewed focus on the debate between the vocal versus the gestural origins of language (Armstrong & Wilcox 2007), (Corballis, 2003), (Arbib, Liebal & Pika, 2008), (Snowdon, Brown & Peterson, 1982), (Seyfarth 1987).

A great deal of the evidence for the vocal or gestural basis of language comes from comparative studies of nonhuman primate vocalisations and gestures. Those who see gestural communication as the more likely candidate point to the high flexibility in the form, or morphology, of nonhuman primate gestures, as well as their functional flexibility, especially relative to primate vocalisations. Vocal repertoires in primates tend to be smaller than gestural repertoires, and have fixed acoustic features that get modified very little over the course of development (Seyfarth & Cheney, 2010). Corballis (2003) goes further to argue that primate vocalisations are largely involuntary, and that, unlike human speech, they cannot be combined into sequences or broken down into individual, transposable parts. Tomasello & Call offer a similar argument stating that, “nonhuman primates seem to have only limited control over their vocalizations—very little in terms of call morphology and a bit more in the case of call usage” (2007a:5). These perceptions of primate vocalisations have largely been substantiated by experimental work. For example, a study comparing chimpanzee and bonobo facial/ vocal displays with their manual gestures found that while the use of facial/ vocal displays were very similar between the species, there was great variety both within and between species in terms of gestural communication (Pollick & de Waal, 2007). In another example, Owren et al. (1993) illustrated the limited flexibility in morphology in the vocalisations of two species of macaques. Here, they cross-fostered two rhesus macaques (*Macaca mulatta*) and two Japanese macaques (*Macaca fuscata*) in order to understand the potential effects on auditory and vocal development. Despite the cross-fostering, all the infants adhered fairly strongly to their species-typical vocalisations, suggesting that vocal communication in these primates is largely innate and inflexible (Owren et al., 1993). Similarly, Cheney & Seyfarth (1990) found limited flexibility of use in their study of Japanese macaque vocalisations. In their experiment, they examined whether the females’ behaviour changed when shown food or a predator either in the presence of their offspring or alone. Despite the changes in audience, they found little difference in the reaction of the mothers, again suggesting limited flexibility of use in primate vocalisations. It also seems that primates have a very limited ability to learn new vocalisations (Cheney & Seyfarth, 1990). This contrasts to the demonstrated ability of the apes to learn gestural signals. As described by Corballis, “chimpanzees, gorillas, and an orang-utans have been taught a simple form of sign language, and both chimpanzees and bonobos have learned to use a keyboard containing symbols, which they point in sequence to deliver messages” (2003:204). Kanzi, a male bonobo that learned an extensive repertoire of gestural signals, has also been documented inventing gestures to add to his repertoire (Savage-Rumbaugh, Shanker & Taylor, 1998).

Yet the evidence for the gestural origins of language is not constrained solely to the limitations of the nonhuman primate vocal repertoire. Other characteristics of primate life and biology may have predisposed our ancestors to evolving a gestural form of language initially. Corballis (2003) notes that, as a result of adaptations to life in the trees, primates have exceptionally good intentional control over their limbs, and especially their hands and arms. Furthermore he points out that primates are largely visual creatures, with estimates suggesting that over half of the brain is, “involved, one way or another, with the analysis of the visual world” (2003:202). Neurological evidence for the gestural origins of language is also presented with the concept of

mirror neurons. Rissolatti & Arbib (1998) found that these neurons discharge in monkeys whenever they grasp or manipulate objects as well as when they observe the experimenter grasping or manipulating objects. They claim that this association marks the neural basis of gestural communication. Furthermore, they found that mirror neurons were located in an area of the primate brain homologous to that of Broca's area in the human brain, which has been associated with language processing (Rissolatti & Arbib, 1998). Beyond the higher degree of flexibility observed in primate gestural communication, there is a wide range of neurological and physiological evidence that suggests that language may have arisen from gestural communication.

However, many still hold that vocalisations are the more likely candidate for the predecessor to human language. Dunbar (2003) notes that there are some aspects of gestural communication that present clear disadvantages to functional communication. The visual nature of gestures, he points out, would prevent their use at night, and require that those communicating maintain a direct line of site. The latter would be particularly disadvantageous given the prevalence of arboreality in primate relatives, and would also be potentially maladaptive in the scrub woodland environment that human ancestors likely found themselves in. Dunbar (2003) argues that given these circumstances, a gesture-based language would have no advantage over the grooming behaviour that promotes sociality in other species of primates.

Others note that there is, in fact, a degree of flexibility in nonhuman primate vocalisations. Hauser (1992) conducted a study on the "coo" vocalisation in rhesus macaques (*Macaca mulatta*) where he found that members of one matriline produced coos that were acoustically distinctive from all the other matrilines. He posited that, "learning may be the primary cause of such intrafamilial similarities," illustrating that nonhuman primate vocalisations are sometimes subject to minor alterations (Hauser, 1992:2175). Further experimental research has also shown that vocalisations are not involuntary, emotional responses, as suggested by others. For example, Cheney, Seyfarth & Silk (1995) exhibited that female baboons (*Papio hamadryas ursinus*) decide whether or not to give a reconciliatory grunt to their opponents. In a playback experiment, they found that the baboons reacted more strongly to a casually inconsistent sequence (a grunt from a lower-ranking female followed by a fear bark from a higher ranking female), showing that some call combinations elicit a strong response while others do not. The inconsistencies in responses indicate that these baboons' vocalisations are not involuntary responses to a stimulus, but rather a reaction under the control of their own volition (Cheney, Seyfarth & Silk, 1995).

One paradigm that might play a role in deciding this debate is the social complexity hypothesis for communication. According to Freeberg, Dunbar & Ord, "The 'social complexity hypothesis' for communication posits that groups with complex social systems require more complex communicative systems to regulate interactions and relations among group members" (2012:1785). In other words, the more complicated social networks in certain species exert selection pressures for the development of more complex communication modalities. This hypothesis would also serve as a reasonable explanation for the evolution of language, for as our ancestor's groups grew larger, a more complex form of communication would have then

been selected for. Social complexity is most often quantified via measures of group size, yet is sometimes quantified via other measures that try to take aspects of social complexity beyond number of interactions (e.g. kinds of interactions) into account as well. There are, however, two fairly common approaches to measuring communicative complexity. One is the information theory approach in which, “the complexity of a signalling channel is measured in terms of its entropy or uncertainty—bits of information”(Freeberg et al., 2012:1788). Researchers use this when quantifying complexity based on the number of elements or components making up any single signal. The other, more frequently used quantification of communicative complexity are measures of repertoire size, which count the, “distinct displays or signals in a signalling repertoire” (Freeberg et al., 2012:1788). In this case, researchers argue that larger repertoires, with more discrete displays and signals, are more complex than smaller repertoires.

The repertoire size method has been applied successfully when testing the social complexity hypothesis on primate vocal communication. McComb & Semple (2005) used a phylogenetically controlled analysis to see if repertoire size increased with social group size and time spent grooming across 42 species of primates. It was found that vocal repertoire size correlated both positively and significantly with social group size and time spent grooming (McComb & Semple, 2005). In other words, they found that vocal communication in primates is consistent with the social complexity hypothesis. This finding has clear implications on the language origins debate. Dunbar (2003) theorizes that language initially evolved as a means of maintaining social relationships when groups grew too large for grooming to serve this role in primates. It would then follow that whichever modality language arose from would increase in complexity with the heightened demands from social group size. Therefore, the finding that the vocal repertoires of primates are consistent with the social complexity hypothesis provides additional evidence for the idea that vocalisations were, in fact, the root of language evolution in our ancestors.

It is then natural to question whether or not the social complexity hypothesis also holds true in the gestural communication of primates. While two studies (Dobson, 2009), (Maestriperi, 2005), have explored implications of social factors on visual and gestural communication, a comparable study in gestural communication to the work of McComb & Semple (2005) in vocal communication has not been conducted. In terms of visual communication, Dobson (2009) used facial expression repertoires from 12 nonhuman anthropoid species in order to see if the facial repertoire increased with group size. Controlling for both body size and phylogenetic relations, he found that group size is a good predictor of facial mobility (Dobson, 2009). Despite the fact that this is not a direct study of gestural communication, nor is it as comprehensive as the McComb & Semple (2005) study, it does show that an aspect of primate communication in the visual modality is consistent, to an extent, with the social complexity hypothesis. Maestriperi (2005) examined the potential connection between social relationships and gestural repertoires in three species of macaques. He found that the species with the most despotic social hierarchy, and therefore relatively limited social interactions, used significantly fewer gestures than the two more socially complex species (Maestriperi, 2005). While Maestriperi (2005) does seem to illustrate that gestural communication in primates is consistent with the social complexity hypothesis,

this study is certainly limited in its scope—with only three species of one genus being analysed.

Seeing this clear gap in the literature, it is then the goal of this paper to see if gestural repertoire size, like vocal repertoire size, correlates positively with group size among the primates. Considering gestural communication's strong candidacy for the origins of language, and the strength the social complexity hypothesis has shown in other modalities, I would expect this correlation to hold true for gestural repertoires across primates as well. Therefore, I would predict a positive correlation between social group size and gestural repertoire size in primate species.

M E T H O D S

The initial aim of this paper was to compare gestural repertoires across a wide variety of primate species, in the same vein as McComb & Semple (2005). By utilizing primarily online databases, an exhaustive search of the literature was done by searching for key words and terms such as gesture, manual gesture, tactile communication, and visual communication. Unfortunately, only a small number of studies were found, and those that were found were not suitable for comparison due to differences in study design and methodology. The majority of research on gestural communication is performed on the great apes; therefore, I then decided to try and focus my further literature search on the great apes. Despite the fact that there were considerably more studies examining gestural communication in apes than in other primate species, again, due to differences in study design and methodology, the vast majority of the data was not suitable for cross comparisons. Ultimately, a broad scale comparison, as was done in McComb & Semple (2005), was not possible due to the dearth of comparative data covering gestural communication in primates.

However, in my literature search, I found Call & Tomasello (2007), which is widely cited as one of the only comparative gestural communication compilations. This group of studies is presented as chapters in a book, and included approximate gestural repertoire sizes from six species, including four great ape species (*Pan troglodytes*, *Pan paniscus*, *Pongo pygmaeus*, and *Gorilla gorilla*), siamangs (*Symphalangus syndactylus*), and Barbary macaques (*Macaca sylvanus*). Because this was the only set of studies I found in my literature search where methods were controlled enough to justify comparison, I used data solely from this group of studies. This was useful in the sense that the data was already controlled for comparative purposes, but limiting in that it prevented broader comparisons between studies.

In these studies, gesture was defined as, “those intentional movements of the limbs, head, and body as well as body postures used to communicate with conspecifics” (Tomasello & Call, 2007b:23). Details of the number of individuals and groups observed, as well as the amount of time spent observing is presented in the results section (Table 1). For all studies, with the exception of the macaque study, facial expressions were not included in final gestural repertoire counts. Gestural repertoire sizes were obtained through focal animal sampling primarily, though *ad libitum* sampling was also used to supplement observations in the macaque study (potential bias introduced by this discrepancy is addressed later). The reported repertoire sizes from Call

& Tomasello (2007) were used directly in all species, with the exception of common chimpanzees. In this case, the reported repertoire size in Call & Tomasello (2007) was a compilation of five different study periods, so instead, the common chimpanzee repertoire size was taken from Tomasello, Gust & Frost (1989), where the same methodology was applied to measure the repertoire size of a group of chimpanzees in a more comparable period of time. Furthermore, the facial expressions were removed from the final macaque repertoire. Unlike McComb & Semple (2005), though, gestural repertoires could not be restricted to those only performed by adults, as the majority of the studies within Call & Tomasello (2007) were focused on the gestural communication of infants and juveniles. The implications of this limitation will be discussed later.

Measures of average group size were taken from McComb & Semple (2005) directly. Because this particular species of macaque was not included in the vocal repertoire analysis, average group size was found in Rowe (1996), which was the source initially used by McComb & Semple (2005) for measures of group size. After these data were gathered, a regression analysis followed. Unfortunately, unlike the McComb & Semple (2005) study, analyses could not be phylogenetically controlled because there were not enough species included to render a phylogenetic regression meaningful (i.e., lambda would be zero in such a small sample).

R E S U L T S

Table 1 presents data on social group size and repertoire size for each of the six species included in this analysis. It also presents a review of variables from the five studies from Call & Tomasello (2007), as well as a review of the corresponding common chimpanzee variables from Tomasello et al. (1989), where gestural repertoire data was derived from. Potential biases that could have resulted from differences among these studies are discussed later.

Figure 1 presents the results of the correlation analysis. Regression analysis, with social group size serving as the independent variable and observed gestural repertoire size serving as the dependent variable revealed a p value of .2827, and an R^2 value of .28. The results showed a slight, but insignificant, trend of gestural repertoire size decreasing with group size, which is the opposite of what was initially anticipated.

Species	Common Name	Social	Reper-toire Size	Number of Individuals observed	Number of Groups Observed	Age Range of Individuals Observed	Hours spent observing (direct and videotape)	Type of Sampling	Environment
<i>Pan troglodytes</i>	Common chimpanzee	26.5	28	7	1	Juveniles	180	Focal-animal sampling	Captive
<i>Pan paniscus</i>	Bonobo	125	20	7	2	Subadult	278	Focal-animal sampling	Captive
<i>Pongo pygmaeus</i>	Orangutan	2	29	16	2	Infants, juveniles, subadults, and adults	160	Focal-animal sampling	Captive
<i>Gorilla gorilla</i>	Gorilla	9	33	13	2	Subadults	455	Focal-animal sampling	Captive
<i>Symphalangus syndactylus</i>	Siamang	3.5	20	14	4	Infants, juveniles, subadults, and adults	140	Focal-animal sampling	Captive
<i>Macaca sylvanus</i>	Barbary macaque	24	25	52	3	Infants, juveniles, subadults, and adults	N/A	Focal-animal sampling, and <i>ad libitum</i> observations	Semifree

Table 1. Gestural repertoire sizes, and further variables by species. Common chimpanzee data was taken from Tomasello et al. (1989), and all other data was taken from Call & Tomasello (2007).

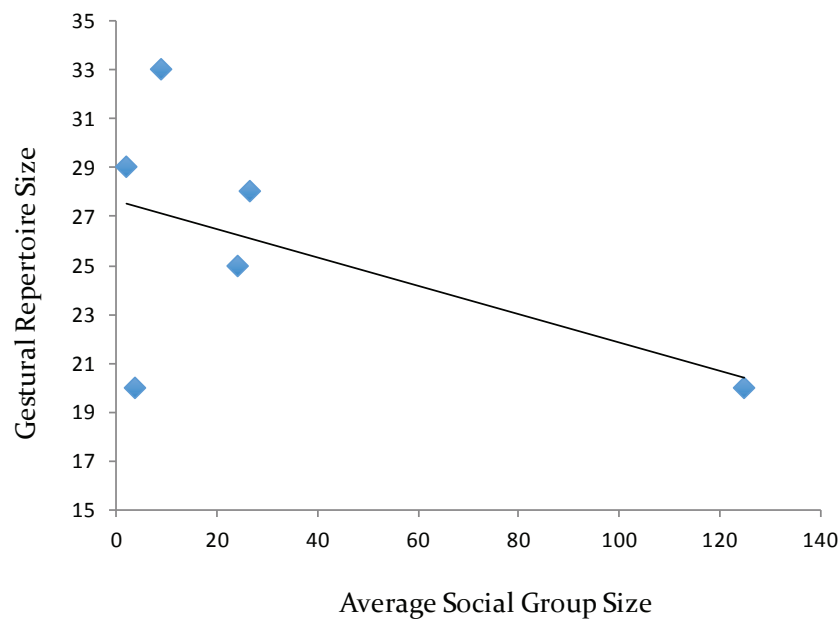


Figure 1. Gestural repertoire sizes correlated with average social group size. Common chimpanzee data was taken from Tomasello et al. (1989), and all other data was taken from Call & Tomasello (2007).

D I S C U S S I O N

This particular set of data suggests that gestural communication in primates is not consistent with the social complexity hypothesis for communication. This could insinuate that trends in gestural communication in these species were impacted by other ecological or evolutionary factors other than a heightened drive of social complexity. However, there are many other reasons why this lack of correlation may be the case, including limitations and biases inherent in the dataset as well as issues with how both social and communicative complexity were measured.

First, it should be noted that Call & Tomasello (2007) use this data comparatively for several analyses of their own. Furthermore, this compilation is frequently cited in other works as being a ground-breaking study for comparative work in gestural communication. One paper suggests that, “Call, Tomasello, and colleagues conducted the only study of ape gestures that is fully comparable between species” (Cartmill & Byrne, 2010:795). While there is a fair amount of consistency in the methods between these studies, there are also significant differences whose potential to introduce bias warrant attention and analysis. Variables that could have introduced bias include differences in the number of individuals observed, differences in the number of groups observed, differences in the ages of individuals observed, differences in the overall observation time, differences in the primate living environments, and differences in the sampling methods.

There are discrepancies in both the number of individuals and groups analysed in each study. The macaque study, in particular, is an outlier in terms of individuals observed, with the next closest study having 36 fewer individuals being analysed (Hesler & Fischer, 2007). This is of particular concern because idiosyncratic gestures, or gestures only performed by one individual, were observed in each of the Call & Tomasello (2007) studies, as well as the Tomasello et al. (1989) study. Given that even one individual can impact the size of the observed gestural repertoire, having such large differences in the number of individuals considered could certainly impact repertoire sizes in significant ways.

Because of intergroup differences, these comparisons could also be biased by differences in the number of groups observed in each individual study. In their studies of common chimpanzees, bonobos, orang-utans, gorillas, and siamangs, Call & Tomasello (2007) analysed gestural communication data for group differences, and found fairly universally that there was low concordance between groups both in the types of gestures performed and the entire gestural repertoire. This suggests that there is a high variability in gestural communication in groups of the same species; therefore, differences in the number of groups observed between studies could artificially inflate or deflate observed repertoire sizes.

Perhaps the relatively limited sample of bonobo individuals and groups observed could, in part, explain why their repertoire size is particularly small relative to their group size (Pika, 2007a). In his comparison between the communicatory repertoires of common chimpanzees and bonobos, De Waal (1988) described over 25 distinct bonobo gestures and postures, in addition to six common facial expressions. These gestures were based on observations of ten bonobos in three dif-

ferent subgroups at the San Diego Zoological Garden. A great amount of similarity in complexity in chimpanzee and bonobo communication, generally, was also noted (De Waal, 1988). If the social complexity hypothesis were to hold true, a repertoire greater than 25 gestures would be closer to what one would expect of bonobos, compared to the size of the other species repertoires in this study. However, this particular ethogram did not take into account the intentionality of gestures, so it therefore cannot be directly compared to the Call & Tomasello (2007) repertoires.

Another concern arises from the fact that while some studies included gestures performed by individuals of all ages, others only included those performed by younger individuals. McComb & Semple (2005) controlled for age, ensuring that any vocalisation performed exclusively by immature individuals was not included in the final vocal repertoire measurement. This is of particular concern because in each of the species analysed for differences in gestural use by age, significant patterns were found. In common chimpanzees gestural communication increased with age; it was noted that, “this does not simply reflect that older subjects were more active, but it also shows that they had a greater variety of gestures” (Tomasello & Call, 2007b:32). Similar patterns were found in orang-utans, gorillas, siamangs, and macaques as well, with certain age groups performing more gesture types than others. Furthermore, several gestures seemed to be exclusive to certain age classes (Call & Tomasello 2007). The more restricted age samples of the common chimpanzee, bonobo, and gorilla studies as compared to the other studies could have easily reduced their relative gestural repertoire size.

Bias could have also been introduced as a result of the different lengths of time spent observing the individuals in each study. The gorilla study, for example, reported the highest repertoire size, and those researchers also logged nearly double the number of observational hours than the next highest study (Pika, 2007b). In their study of gestural communication in wild chimpanzees, Hobiater & Byrne (2011), by plotting repertoire size against active gesture time, found that an asymptote was reached at approximately 15 hours of active gesture time. This amounts to around 150 days of field observation time (Hobiater & Byrne, 2011). While it is unclear whether this benchmark was reached in the Call & Tomasello (2007) studies, their repertoire sizes are generally low when compared to other studies of gestural communication in these species. Hobiater & Byrne (2011), for example, observed 66 distinct gestures in their observations of common chimpanzees. Another study of orang-utans noted 64 different gesture types (Cartmill & Byrne, 2010), and a study focused on gorilla gestural communication found 102 distinct gestures (Genty et al., 2009). However, these discrepancies could also be due to a lack of standardization in what exactly defines a distinct gesture, as Hobiater & Byrne (2011) argue that the 102 gestures observed in gorillas (Genty et al., 2009) could be lumped into 66 distinct gestures under their definitions. Clearly there needs to be more standardization in the lumping and splitting of gestures in order to ensure greater accuracy in the measurement of gestural repertoire sizes.

Discrepancies between the Hobiater & Byrne (2011) study and the Call & Tomasello (2007) study of common chimpanzees could have also arisen because Hobiater & Byrne (2011) focused on a wild population, while Call & Tomasello (2007) focused on a captive population. Researchers

have observed differences in the use of gestures in wild versus captive populations of apes, perhaps with referential signalling being the most widely discussed issue. For a long time, referential gestures had, “been reported only in captive chimpanzees interacting with their human experiments and human-raised or language-trained apes” (Pika & Mitani, 2006:191). Recently, Pika & Mitani (2006) observed wild chimpanzees using directed scratches to initiate grooming in designated areas of their body, but the performance of referential gestures in wild primates remains controversial. This provides one example of how gestural communication may differ between captive and wild primates. While most of the studies in Call & Tomsello (2007) were conducted on captive populations, the macaque population observed was semifree, which is yet another instance where bias may have been introduced into this supposedly comparative sample.

The macaque study also differs in that they used *ad libitum* sampling in addition to the focal animal sampling utilized by all other studies in the compilation (Hesler & Fischer, 2007). In the study of intentional gestures in orang-utans, Cartmill & Byrne (2010) used *ad libitum* sampling because a, “pilot study suggested that focal-animal sampling would significantly reduce the number of gestures observed, since active social interaction occurred primarily in punctuated bursts” (2010:796). It is then clear that discrepancy in observation methods could lead to differences in the number of gestures observed. Because *ad libitum* sampling was used in the macaque study, but not for observations in any other species, it could be that the other species repertoire sizes were underestimated compared to that of macaques.

While this dataset is widely cited as the only interspecific comparative study on gestural communication in primates, there are a number of concerning variables that could certainly have influenced this analysis of gestural repertoire sizes. Differences in the number of individuals and groups observed, as well as differences in observation time and methods used could plausibly account for the lack of correlation between social group size and repertoire size in this dataset. However, even without these discrepancies in the dataset, there are a number of other queries that could potentially explain these results.

The lack of correlation seen between social group and gestural repertoire size in this dataset could also be related to poor means of quantifying social and communicative complexity. Social group size, for example, may not necessarily be the best measure of social complexity. Freeberg et al. (2012) present a more intricate view of social complexity where the number of social roles individuals can take on, and the diversity of morphology in interactions is considered in addition to the number of individuals interacting. They note that while a pair-bond may seem like the simplest of social groups, “members of a pair-bond need to have a much more subtle appreciation of each other’s needs and intentions than is the case when relationships are more casual and ‘of the moment’” (2012:1793). Based on this observation, they suggest that, “what makes these social systems complex is that the network structure does not necessarily parallel the size of the social group, but rather is additionally affected by the fact that relationships in different layers have different qualities” (Freeberg et al., 2012:1794). Dunbar & Schultz (2007) who have shown that, in all taxa except the anthropoid primates, large brain size is not associated with

social group size but rather pair-bonded monogamy further substantiate this. Their results suggest that it was the complexity of social interactions in, “pair-bonding that triggered the initial evolution of large brains across the vertebrates” (2007:1346). Given that the complexity of the social network does not necessarily correlate with the number of individuals in a social group, it follows that group size is not the best measure of social complexity. Additionally, there are many ecological factors that could influence group sizes, so it would seem that focusing on the number of individuals in a social group alone is a rather arbitrary measure of social complexity.

Furthermore, repertoire size may not be the most accurate measure of communicative complexity. Researchers often face difficulty when trying to quantify repertoire sizes. Freeberg et al. point out that, “there is often graded variation among the signals of a repertoire,” which makes it difficult to distinguish separate units (Freeberg et al., 2012:1793). Measures of repertoire size also often fail to take into account how variations in signals impact the receiver. For gestural modality specifically, discrete units may not be the best way to measure communicative complexity. Issues with how exactly to define a distinct gesture are certainly a source of debate among researchers, with, for example, Genty et al. (2009) describing 102 distinct gesture types in gorillas, while Hobiater & Byrne (2011) suggesting that these gestures could instead be lumped into 66 distinct forms. Because the flexibility of gestures is central to their strong candidacy for the origins of language, perhaps measures of flexibility would be a more appropriate measure of communicative complexity. For example, quantifying how many gestures are used in more than one situation, or how many different gestures are used in one situation could be used to quantify communicative complexity in this modality.

However, some of the qualities that make gestural communication an appealing candidate for the origins of language are beginning to be called into question. Call & Tomasello (2007) argue that the majority of primates’ gestural repertoires are acquired via the process of ontogenetic ritualization. They describe the process of developing a gesture as:

Individual A performs behaviour X (not communicative signal); individual B consistently reacts by doing Y; subsequently B anticipates A’s performance of X, on the basis of its initial step, by performing Y; and subsequently, A anticipates B’s anticipation and produces the initial step in a ritualized form (waiting for a response) in order to elicit Y (Tomasello & Call, 2007a:5).

In this way, once functional movements are conventionalized into non-functional gestural signals. This mechanism of mounting a gestural repertoire illustrates the potential flexibility of this modality in different species of primates, which strengthens the argument for the gestural origins of language. Under this model, rather than gestures being innate or species-specific, there is a learning component similar to that of human language. Call & Tomasello (2007) claim the six studies in their compilation support ontogenetic ritualization as the primary means by which primates acquire their gestural repertoires, and point to examples of idiosyncratic and one-way gestures as strong evidence for this hypothesis.

More recent studies examining the gestural repertoires across the three genera of great apes, though, suggest otherwise. After conducting a study on gestural communication in gorillas, Genty et al. concluded that, “no support was found for the ontogenetic ritualization hypothesis as the chief means of acquisition of gestures” (2009:527). Instead, they argue that gorillas have a species-typical gestural repertoire, suggesting that gestures are more innate rather than learned or conventionalization (Genty et al., 2009). Hobiater & Byrne (2011) took this conclusion a step further in their study on wild chimpanzee gestural communication. Again finding no support for the ontogenetic ritualization hypothesis, they instead contend that the chimpanzee gestural repertoire is species-typical and, in part, family typical. They found that 24 of the 66 distinct gestures they observed in chimpanzees were also observed in orang-utans and gorillas (Hobiater & Byrne, 2011). However, it remains worth noting that even if it is the case that primate gestures are phylogenetically, rather than ontogenetically, ritualized, “they are not totally inflexible because, at the very least, they are deployed in the right circumstances and the existence of appropriate substrates/ elements determines their appearance” (Liebal & Call, 2012:122). Yet, another study conducted on orang-utan gestural communication found that gestures might not be used as flexibly as once thought in this species (Cartmill & Byrne, 2010). They concluded that, “despite their contextual flexibility, orang-utan gestures are made with the expectation of specific behavioural responses and thus have intentional meanings as well as functional consequences” (Cartmill & Byrne, 2010:793). More recent work in gestural communication has shown that gestural communication may be more innate and less flexible than researchers may have initially thought. Perhaps, then, the lack of correlation seen between gestural repertoire size and social group size in this dataset could be because gestures are not, in fact, a strong candidate for the origins of language. If that is the case, other factors, outside of vocal complexity, could have influenced the evolution and development of repertoire size.

Other researchers have pointed to other social and ecological factors that may influence the gestural repertoire size in primates. Maestriperi (1999), for example, suggests that species with more despotic social hierarchies will have reduced variety in gestural communication as compared to those species with more relaxed social structures. He argues that, “in a social system in which relationships are strongly influenced by differences in dominance and in which cooperation occurs mainly within clusters of kin—that is between individuals who are very familiar with each other—there may be little pressure to develop a sophisticated system of communication,” whereas, “in societies that are more egalitarian and individualistic, flexible individual strategies may be favoured,” (Maestriperi, 1999:57). His study of gestural communication among three species of macaques seemed to confirm this idea with the species having the most despotic social system observed using the least diversity of gestures (Maestriperi, 2005). Interestingly, in Call & Tomasello (2007), common chimpanzees, despite having a more despotic social organization, have more variability in their gestural repertoire than the more egalitarian bonobos, which seems to go against the observations in Maestriperi (1999, 2005).

Marler (1976), on the other hand, suggests that gestural repertoire size varies depending on how much time a species spends in the trees. He contends that vocalisations would be more

prominent in arboreal species, whereas gestural communication would be more frequently utilized in terrestrial species (Marler, 1976). While not directly opposing Marler's theory, Dobson (2009) did find no correlation between arboreality and diversity in facial expressions in his study on variation in facial expressions in 12 species of anthropoid primates. This suggests that at least part of the visual modality is not impacted heavily by time spent in the trees.

Some suggest, more generally, that other theories should be considered alongside the social complexity hypothesis to explain what drives communicative complexity on the whole. Freeberg, Ord & Dunbar note that, "Despite a number of studies providing support for the hypothesis, a key limitation of the hypothesis as it currently stands is that it has rarely been tested against alternative explanations for communicative complexity" (2012:1782). Other ecological factors, as well as stochastic evolutionary forces, could limit or inflate communicative repertoire sizes, including habitat, and predation (Freeberg et al., 2012). Ord & Garcia-Porta (2012) attempted to test the social complexity hypothesis against other ecological explanations for communicative complexity in a variety of taxonomic groups, including birds, lizards, frogs, and ants. They concluded that, "we found that sociality—based on the metrics we were able to compile from the literature and electronic databases—was not as influential in the evolution of signal complexity as we had anticipated. Indeed, it appears to have only been an important factor in the evolution of signal complexity in lizards" (Ord & Garcia-Porta, 2012:1823). Instead, they found ecological and allometric, or body size, factors to be more significantly correlated with the development of communicative complexity. Conceivably, then, the lack of correlation between social group size and gestural repertoire size in this dataset could be due to other factors influencing gestural communicative complexity, or communicative complexity on the whole, more than social complexity.

C O N C L U S I O N

There is no significant correlation between social group size and gestural repertoire size with the data provided by the studies of Call & Tomasello (2007). This could be because gestural communication is not, in fact, a strong candidate for the origins of language, or it could be related to greater issues with how complexity is quantified, the strength of the social complexity hypothesis itself, or simply issues with bias in the dataset at hand. Unfortunately, not enough research into the nature of gestural communication in primates has been done to draw any real conclusions based on this discussion.

As Tomasello & Call themselves point out, "By far the greatest amount of research has been done on primate vocal as opposed to gestural communication" (2007a:5). While the interest in primate social behaviour is by no means new, only recently have there been investigations into gestures as intentional signals. In the past, researchers like Jane Goodall, took a more ethnographic approach to describing primate social interactions in a way that did not distinguish between effective movements and intentional gestures that served as signals between individuals. Therefore, while there may be a larger body of literature describing primate social movements, there are only a handful of studies that actually address the use of intentional gestures.

Furthermore, within the small body of work that does address intentional gestural communication, the focus is overwhelmingly on the apes, with very few studies investigating this modality in other species of primates. In order to truly see if the social complexity hypothesis holds true across primate gestural communication, it is necessary first to investigate this behaviour across a wider variety of species. Finally, It is also necessary to standardize research methods in gestural communication, so cross-comparative studies are more easily conducted.

The relationship between gestural communication and social complexity in primates could certainly provide interesting information regarding the origins of language, among other pertinent topics; however, much more research needs to be done before the nature of this relationship becomes clear.

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The Concretions of Smith Creek: A Preliminary Geoarchaeological Analysis

Arielle Mae Pierson

A B S T R A C T

Concretions, small to medium-sized formations of hardened sediment, found during the 2015 Smith Creek Archaeological Project season are in the process of being analyzed using a geoarchaeological approach that includes examination of geomorphology, stratigraphy and X-ray Powder Diffraction. This analysis is used to support the conclusion that the complex mound building by native tribes during the Late Woodland Period was highly planned and undertaken communally. It required vast knowledge, skill and devotion to cultural and ritual practices to actualize the mounds. Through close examination of the dirt and the placement of artifacts and ecofacts within the dirt it is evident that great care and understanding went into the construction of mounds and this supports the understanding that these mounds held vast religious, cultural and symbolic importance to the people who built them.

I N T R O D U C T I O N

The mounds excavated at Smith Creek are located in the Lower Mississippi Valley region. The site is located along the bluffs overlooking the Mississippi River, and is in present day Wilkinson County, Mississippi (Fig. 1). There are over 30,000 recorded archaeological sites in Mississippi including Emerald Mound in Adams County, the second largest earthen mound in the United States, and Carson Mounds in Coahoma County, the second largest mound group in the United States with 88 mounds. At one time there were 21 Native American tribes in Mississippi, those who constructed the mounds at Smith Creek would likely have been ancestors of the Natchez, a tribe largely decimated by the French in the 1730s (Barnett 2007, 119)

As shown by the pale green strip along the western edge of the state on the geological map (Fig. 2) the site area is entirely made up of loess, a sediment formed by windblown silt that was deposited along the eastern edge of the Mississippi River and compacted to form the bluff line along which the site sits.

The mounds at Smith Creek were constructed by Native groups during the end of the Late Woodland Period, which we identify locally as Coles Creek (750-1200 C.E.), and continued in use into the Early Mississippi Period (1200-1350 C.E.), which we identify locally as Plaquemine. This time period also includes the transition from hunter/gatherers to agriculturally based subsistence and egalitarian to more hierarchically organized communities.



Figure 1, showing Mississippi counties. The large red dot is the location of Smith Creek

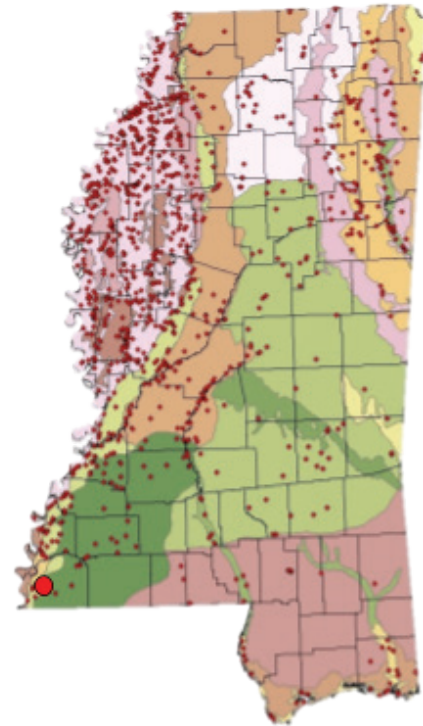


Figure 2, showing Geological Regions and mound sites. The large red dot is the location of Smith Creek.

B A C K G R O U N D

Mound construction began in North America during the Middle Archaic period (6,000-3,000 BCE) in the Lower Mississippi Valley. Initially these mounds were conical and dome-shaped and used primarily as burial grounds. While these dome-shaped mounds continued to be built sporadically throughout the Archaic and Woodland Periods a major change occurred at the end of the Woodland Period. The once conical mounds gave way to the larger, platform mounds used as foundations for activities and structures, often found in mound-and-plaza complexes. Thousands of these earthen mounds were built throughout the Mississippi period. Situated at the beginning of this major transition, Coles Creek mounds increased in size and complexity through time while non-mound settlements began to burgeon around them, plants were cultivated and ceramic decoration and vessel styles shifted. An egalitarian structure is evident through mortuary practices, lack of surplus distribution and changes in mound summit uses. These ranges of change are why Coles Creek can be considered both a terminal Woodland and Mississippian culture (Kassabaum 2014, 1-15). The shifting of these communities over time was directly reflected in the changing construction and uses of their mounds.

Coles Creek mound-and-plaza centers were often designed to bring together many of the smaller surrounding and largely egalitarian communities for communal feasting and ritual activities. These mounds have long been accepted by archaeologists as ritually and politically important gathering spaces. These flat-topped structures served four primary functions: elite residences,

temples, mortuaries/shrines, and as platforms for other non-residential buildings (Lindauer and Blitz, 1997,175). Previously the majority of study was focused on the mound summits and the four functions they most commonly served, and none on the building process that took place before these summit activities. This has changed and there is now an outpouring of compelling research showing that the process of mound construction was just as essential as the activities that took place on the completed mound summits. Mound building was both an art and a science requiring vast amounts of knowledge, skill and planning as well as an emphasis on aesthetics and symbolism (Sherwood and Kidder, 2011, 84). Mounds, much like present day religious structures or major municipal buildings, were built carefully and skillfully with the finest materials and various other expressions of ritual significance. Mounds were built by basket loading, a method in which individuals would carry baskets filled with large amounts of sediment from surrounding areas by hand and layer them in loads until the desired mound height was achieved. (Fig. 3). Some mounds were built slowly over time and others quickly, such as in the case of Mound A at the Poverty Point site in Pioneer, Louisiana. The second largest mound in eastern North America, Mound A took only a matter of three months to construct by basket loading. That's 238,500 cubic meters of dirt, moved in basket loads by hand by individuals with no access to pack animals or metal tools in only ninety days (Ortmann and Kidder, 2012, 67,76).



Figure 3, Artist's depictions of Mound Building, Basket Loading

Nowadays most mounds that are still standing look a lot like overgrown hills blending into the natural topography, but it is important to note two things. First, that many of these mounds are over a 1,000 years old and have not yet given way to weathering or erosion. One mound at Lake George, located in Yazoo County, Mississippi, even survived a demolition attempt with dynamite (Phillips, 1970, 280), showing the magnificence of their engineering. Second, while unkempt and overgrown now, the original mounds would have been maintained with the utmost care. Though publically visitable mounds are typically seen today covered with nicely manicured grass, this would have been quite impossible prehistorical-

ly with no grazing creatures or lawn mowers. Yet an aesthetic appearance clearly mattered. There is growing evidence that they veneered the sides of the mounds by decorating them in an array of contrasting colors of sediment (Kassabaum 2014,39). This could serve aesthetic, symbolic and practical functions. The smooth surface would have been both attractive and protected the mound from erosion. (Sherwood and Kidder, 2011, 80-81) (Fig. 4,5).

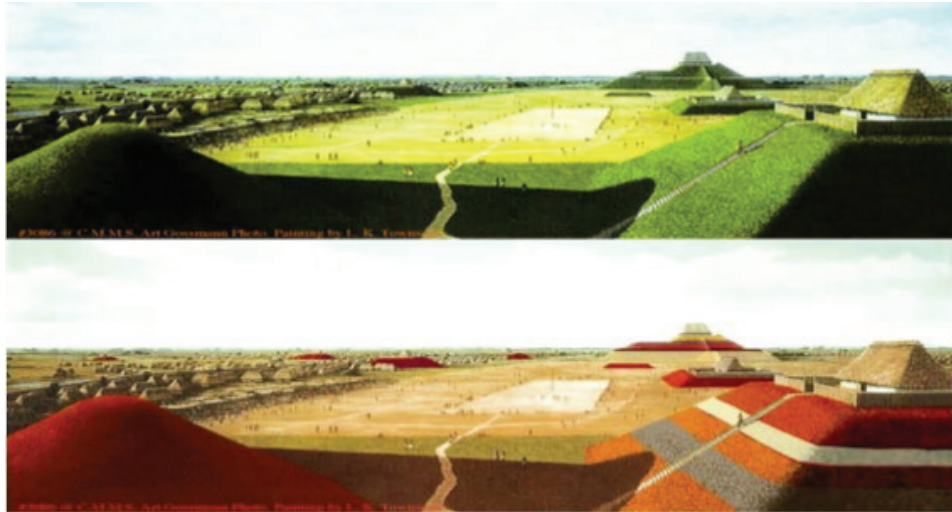


Figure 4 (top), Artist's depiction of Mound, shows the previously held belief that flora grew and was maintained on the exterior of the mounds.

Figure 5 (bottom), Artist's depiction of Mound, shows the beautiful veneer and clay used to preserve and protect the exterior of the mounds.

In addition to learning more about the appearance of mounds, recent excavations have also highlighted the care taken in maintaining the shape of the mound through time. The stratigraphy evident from excavations within the mounds at sites such as Smith Creek and Feltus (another Coles Creek mound center in the Lower Mississippi Valley) show floors, veneers and restoration done to maintain and reinforce the structures. This demonstrates that the love and care extended to maintaining the structural integrity of the mounds even after construction was complete (Fig.6,7).



Figure 6, Evidence of restoration after erosion on Mound A at Feltus, Jefferson County, Mississippi



Figure 7, Stratigraphy of floors on Mound A at Smith Creek, Wilkinson County, Mississippi

G E O A R C H A E O L O G I C A L A N A L Y S I S

In an attempt to better understand the process of mound building, sediment choice and the care and attention visible throughout the mound's stratigraphy, I chose to analyze concretions found in Mound A at Smith Creek. The Smith Creek site is composed of three mounds surrounding an open plaza, due east of the Mississippi River and situated just west of its namesake, Smith Creek (Fig. 8). Excavations thus far have included units in Mound A, Mound C, and the Plaza area. Within Mound A the particular unit in which the concretions were found was Unit 1026R466 (Fig. 9). It was excavated during the 2015 field season and contained, along with the concretions, a mix of cultural materials such as animal bones, ceramics and flakes as well as naturally occurring snail shells. These materials differentially presented themselves at various levels throughout the unit and in varying soil types.

The analysis of these concretions is a first step in research focused on understanding often-ignored aspects of the mound construction process such as sediment selection. The concretions were collected in the field because they were assumed to be naturally forming yet had interesting variations in size, color and shape and came from various levels and soil types within the Unit 1026R466. We chose two representative samples from these to analyze. First, I undertook a macroscopic examination of the unit within Mound A where the concretions were found. This included studying the stratigraphy, soil types and the context of material within Unit 1026R466.

The larger, white concretions, Sample 74, were found in Level 5 of unit 1026R466 (Fig.10). This was a zone of mound fill from the construction process that was yellowish-brown in color. The surrounding area contained very little material other than a few ceramics, animal bones and naturally occurring snail shells. The concretions were recovered through half-inch dry screening.

The small, reddish concretions, Sample 144, were found in Level 7 of unit 1026R466 (Fig. 11). This was from within a large midden deposit. The base material and surrounding material were composed of dark, highly organic loess containing a great deal of cultural material such as ceramics, animal bones and stone debitage. These concretions were also recovered through half-inch dry screening. This midden could have built up over time as a trash deposit or been pushed off the side of the mound while cleaning the summit. Level 7 has been radiocarbon dated to around AD 780-990 (Personal Communication, Meg Kassabaum) (Fig. 12).

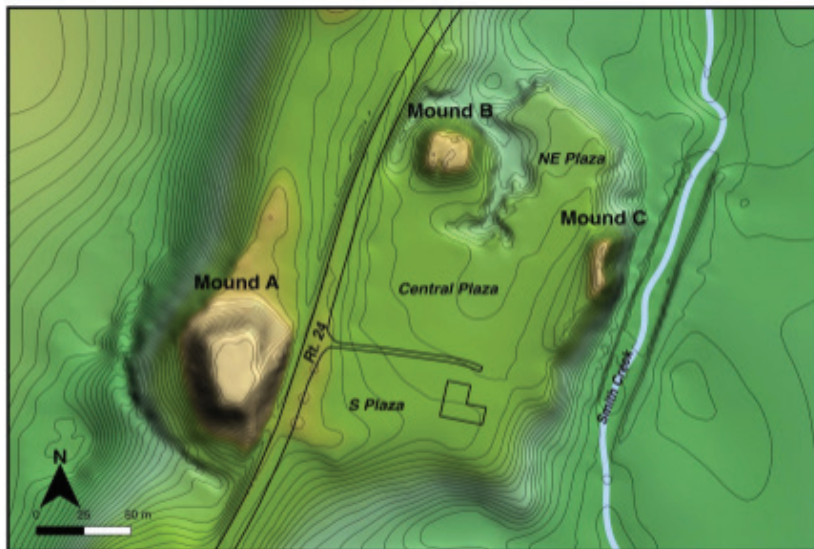


Figure 8, Map of Smith Creek Archaeological Site, Mounds and Plaza

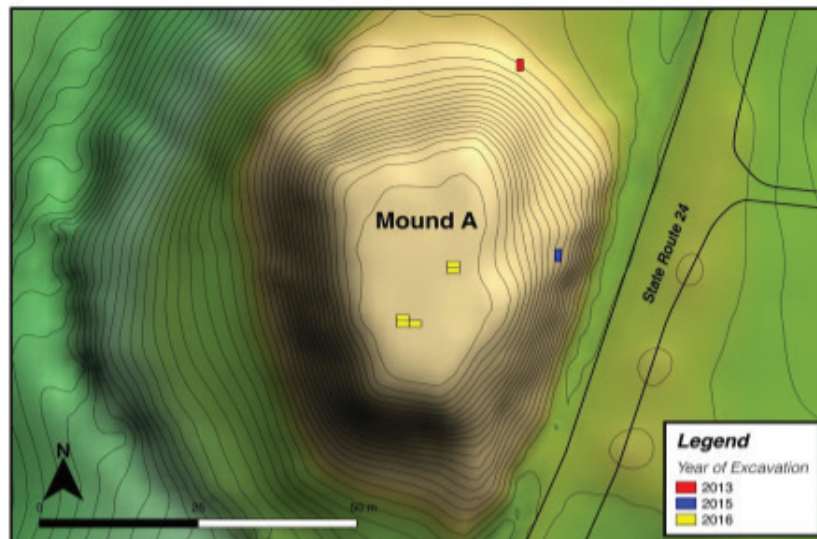


Figure 9, Map of Mound A, 2015 (blue) shows the unit in which the concretions were excavated



Figure 10, Sample 74



Figure 11, Sample 144

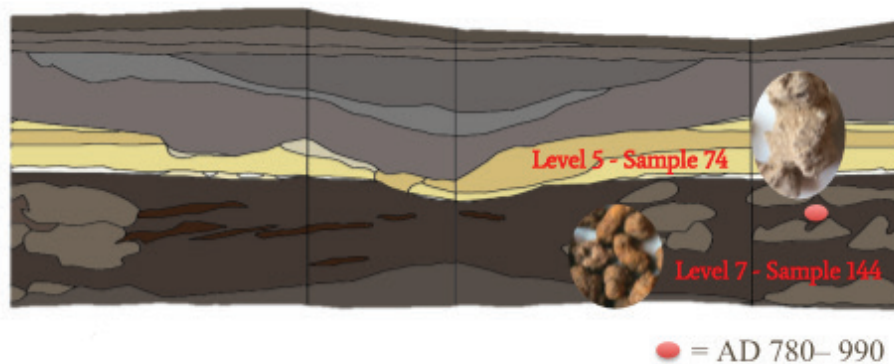


Figure 12, Complete wall map of unit on Mound A showing level locations of concretions and date.



Micromorphological analysis was then used through the process of X-Ray Powder Diffraction (XRD) to analyze Sample 74 and Sample 144. This was done with the assistance of geology Professor, Dr. Gomma Omar. Using a ball mill the samples were crushed into a fine microcrystalline powder and transferred to sample slides to be X-rayed. The data presents itself as seen in Figures 13 and 14. The y-axis represents the concentration of minerals in the sample. The x-axis represents the 2 degree theta, or the the angle of distance or spacing between the atomic plane of the mineral, which can be used to determine the structure of minerals. The peaks show primary mineral content.

Sample 74 (Fig. 13) showed that the white concretions are formed primarily of quartz and calcite with some dolomite. The carrots above the chart tell us that the computer algorithm believes there are other smaller peaks also represented in the data, but upon further observation we noted that they were either of no significance or not even present given that the computer randomly selects minerals and relies strongly on the local geological knowledge of the individual processing the information. Further work on understanding the local geology will certainly improve the accuracy of these results. This first sample is unsurprising as both quartz and calcite are some of the most common minerals in the earth's crust. We believe that the concretions of Sample 74 are naturally occurring in the local, quartz rich, loess. Further research will be necessary to determine how commonly these concretions occur in the local, dry, well-drained loess soil or if it is possible they were transported from elsewhere.

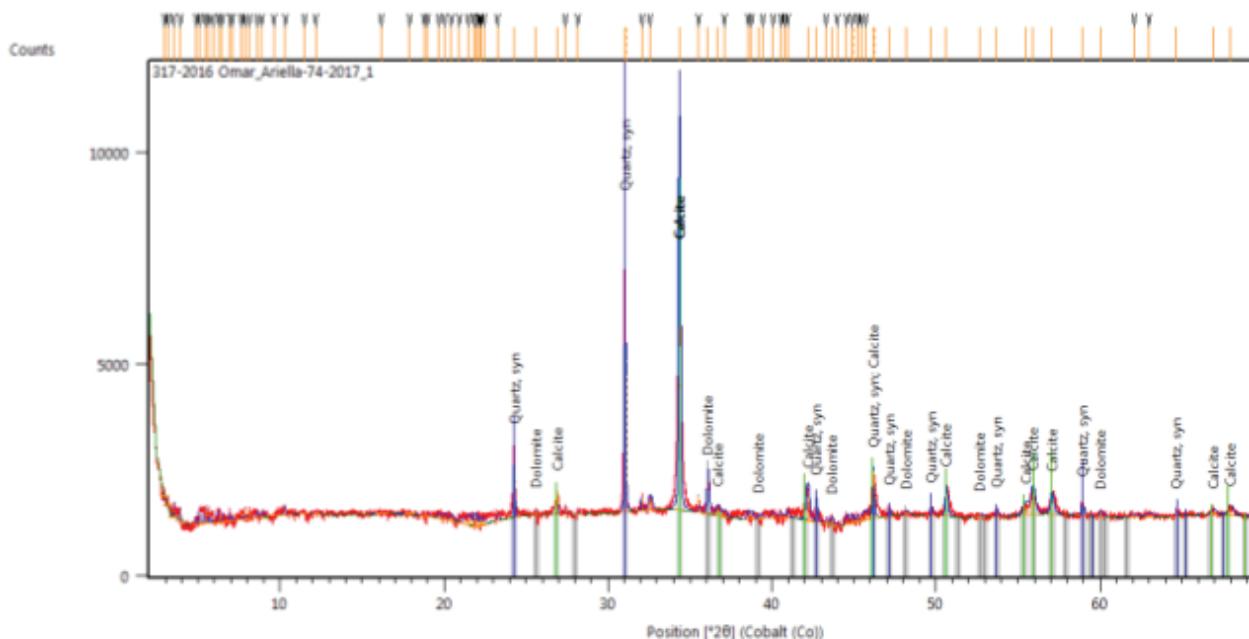


Figure 13, Sample 74 - Composed of Quartz and Calcite

Sample 144 (Fig. 14) is still in the process of being analyzed. We are very fascinated by the results because, while composed almost entirely of quartz, a whitish, opaque mineral, the formations are reddish-brown in color and have a dust like coating that leaves a reddish residue. Yet, no particular mineral was identified besides quartz. Professor Omar weighed the possibilities of magnetite, hematite, rutile and cinnabar, all red-hued minerals, but as of now we are unsure. Cinnabar is not found in Mississippi and the others barely present themselves in the data from Sample 144, leaving little explanation for the coloration. It is possible that the color could have been added as some sort of pigment unrelated to the minerals listed above, but we plan to run further tests in which we dissolve the quartz and analyze the remaining minerals. This, alongside further investigations into types of pigments used to color ceramics and sediment, as well as soil analysis of Mound A could give us further clues. The question of whether Sample 144 contains pigment added intentionally as part of the mound building process or whether the color occurred naturally will give us further insight into the importance of color in mound building practices in the Lower Mississippi Valley.

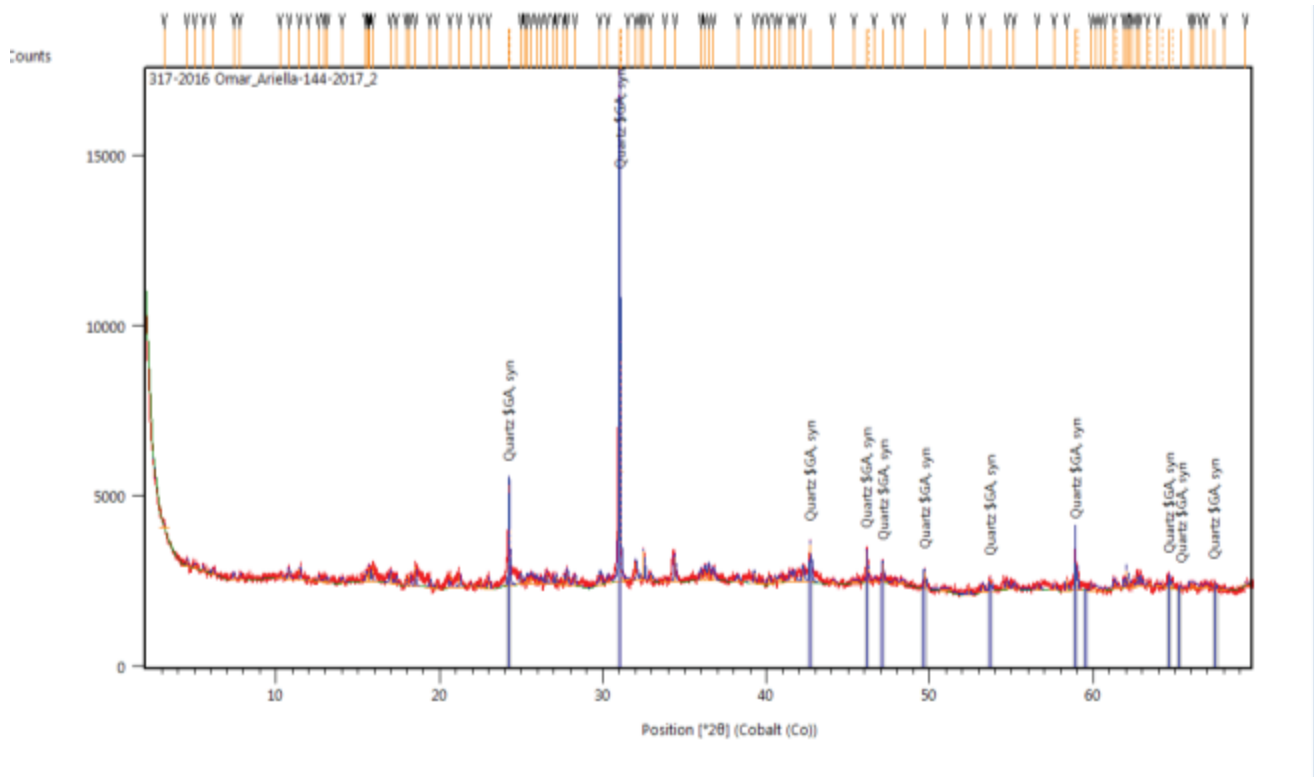


Figure 14, Sample 144 - Composed entirely of Quartz

C O N C L U S I O N

The use of geoarchaeology to understand behavior through the analysis of materials used to build mounds is relatively new. It provides a framework to understanding the techniques of labor and the engineering knowledge needed to accomplish such acts of creation. It can be used to emphasize the immense amount of artistry and science that went into creating these mounds and explain how the labor process played such a symbolic role in the overall cultural significance of the mounds. Through examining these concretions, the stratigraphy of the mounds, the placement of artifacts and ecofacts within the soil of the mounds and the local geomorphology, there is an opportunity to showcase the legacy of these innovative and eloquent mound builders.

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F I G U R E S

Figures 1-9: Courtesy of Smith Creek Archaeological Project

Figure 10: Courtesy of Smith Creek Archaeological Project and Authors additions

Figures 11-14: Property of Author