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The Lives and Afterlives of Skulls. The Development of Biometric Methods of Measuring Race (1880-1950)

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The Lives and Afterlives of Skulls.

The Development of Biometric Methods of Measuring Race (1880-1950)

A dissertation submitted in partial satisfaction of the requirements

for the degree Doctor of Philosophy in History

by

Iris Isabelle Clever

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ABSTRACT OF THE DISSERTATION

The Lives and Afterlives of Skulls.

The Development of Biometric Methods of Measuring Race (1880-1950)

by

Iris Isabelle Clever

Doctor of Philosophy in History

University of California, Los Angeles, 2020

Professor Soraya de Chadarevian, Chair

This dissertation is history of how researchers have trusted biometric technologies to operate objectively but have perpetuated racial bias in the technologies' design and output. It explores the origins and development of the biometric study of race and skulls during the rise and hardening of colonialism. A turn to quantification marks this period: researchers increasingly relied on measurements and statistical methods to develop racial classifications of the world's populations. With a central focus on racial data and the practices that produced the data, the dissertation is a transnational history that follows the data from measurement encounters in colonial spaces, to laboratories in the United States and Europe, to printed form in publications. It transcends disciplinary boundaries and integrates anthropology, anatomy, statistics, and genetics, thus offering a fresh perspective on the history of racial science.

I reveal a methodological crisis around 1900, spurred by a heterogeneous approach to studying race. Measurements and instruments like the skull-measuring caliper were introduced in the 19th

century to infuse anthropology with precision. Meanwhile, researchers continued to study skulls through observations with a "trained eye." By 1900, racial data had piled up without clear taxonomic value, creating a distrust in quantification and confusion about the direction of racial research. In the first half of the 20^{th} century, statisticians like Karl Pearson began transforming anthropology with new biometric methods to make racial research more "scientific." The dissertation argues that biometricians quantified and automated racial research: they made new use of the caliper by combining it with disembodied statistical formulas. Automation entailed a critique of the anthropologist's subjective "trained eye" expertise and a reduction of human intervention in favor of objectivity. The biometricians, however, never challenged racial research itself and continued to reproduce old racial biases in their new methods and theories. Even in challenging Nazi race theories, they never questioned the existence of race. The dissertation thus uncovers how biometric practices were considered objective *and* reproduced racial prejudices.

The dissertation of Iris Isabelle Clever is approved.

Andrew Apter

Lynn A. Hunt

Theodore M. Porter

Soraya de Chadarevian, Committee Chair

University of California, Los Angeles

2020

To my family

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Acknowledgements

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My decision to pursue graduate education in the United States began with a visiting Graduate Student Fellowship that was made possible by the UCLA-Utrecht exchange program, founded by

Professors Margaret Jacob and Wijnand Mijnhardt. In the Fall of 2012, I spent three transformative months at UCLA's History Department, taking classes with the three professors that later would become my dissertation committee members. I cannot express my gratitude enough to Margaret Jacob and Wijnand Mijnhardt for giving me this opportunity and for their encouragement to pursue an academic career in the United States.

I am so grateful to Professors Andrew Apter, Lynn Hunt, and Theodore Porter for their unwavering support, help, and kindness in the past eight years. You have made me feel at home at UCLA right from the start. Andrew Apter and I both participated in the UCLA-Utrecht exchange and I have such fond memories of both our visits. I am so grateful that we have been able to continue our conversations about anthropology and theory. You have supported me in exploring new theoretical avenues and applying them to my research. Thank you for showing me that no idea is ever too crazy. Lynn Hunt's work on cultural history and the body has been a continuous source of inspiration since my undergraduate days and it has been a great gift to work with you. You teach me how to ask historically urgent questions and remind me that the body always matters. Thank you for encouraging me to pursue a career as a historian, your words at the end of my exchange were transformative. Ted Porter gave me the confidence that I too could write a history of statistics. You have consistently pushed me to develop my own perspective about the source materials, to explore what lies beneath. Thank you also for showing me that deep academic rigor can be combined with wittiness and lightness. Those of us who have had the privilege of teaching your Modern Science class will not forget Spencer's song. Being a student and a TA in your class is a truly wonderful experience.

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The graduate students of UCLA's History Department have given me an incredible community these past years. I am so proud to call you my friends and colleagues. Three friends have made this journey particularly meaningful. Scottie Buehler has been my partner in crime from my first day in the program. Thank you for being such a wonderful friend and supportive colleague. I couldn't have made it to the finish without Maia Woolner, who read and commented on every page of this dissertation. Thank you for your extraordinary insights and feedback. Your friendship means so much to me. Elle Harvell has become family. My time in LA has been marvelous with you by my side.

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2018 I. Clever, "Book Review: 'Measuring the Master Race' by Jon Røyne Kyllingstad," *Journal for the Royal Anthropological Institute* 24:1 (2018) 204-205.

2016 I. Clever, "Book Review: 'Constructing Race' by Tracy Teslow," *History of Anthropology Newsletter* 40:1 (2016).

Additional publications

2016 I. Clever, "Voices from the Field: A Collection of Personal Experiences and Advice to Graduate Students," *UCLA Historical Journal* 27:1 (2016) 18-20.

2014 I. Clever, "Science Inside Out. Objectiviteit en Marginalisering in Wetenschapsgeschiedenis," *Historisch Tijdschrift Aanzet* 29:3 (2014).

Selected Presentations

2020 "The Anthropological Politics of Standardizing Man's Measurements." *Contested Data: What Happens When the Givens Aren't Taken Academic Workshop*. Data & Society Research Institute, 6 March.

2019 "Skulls and Statistics: Karl Pearson and Competing Methods of Classifying Races in the Early 20th Century." *History of Science Society Annual Meeting*. Utrecht University, 23-27 July.

2019 "'Tiresome Anthropometric Affairs.' Standardization Efforts in the History of Physical Anthropology, 1880-1950." *International Society for the History, Philosophy, and Social Studies of Biology Biennial Meeting*. Oslo, 7-12 July.

2018 "Reinventing the Caliper: Transforming Physical Anthropological Practices after World War II." *American Association for the History of Medicine Annual Meeting*. Los Angeles, 10-13 May.

2017 "Finding a Formula for Race. Quantifying Racial Differences in Early Twentieth Century Physical Anthropology," *Histories of Anthropology: Transforming Knowledge and Power* (1870-1970). University of Cambridge, 18-19 September.

2016 "The Anthropological Politics of Scientific Universalism. Standardization in Anthropometry and Miriam Tildesley's International Standardization Committee (1928-1945)," *History of Anthropology Conference: the rise of university departments 1918-1945*. Royal Anthropological Institute, London, 13-14 December.

Introduction

Faces, Races, and Bias

On the 26th of August 2019, a major Dutch newspaper, the *NRC*, published an article titled "Algoritmes discrimineren niet," translated "Algorithms do not discriminate." In this short piece, professor of Data Science in Crime and Safety Peter de Kock responded to recent outcries surrounding the widespread use of predictive algorithms in various Dutch governments. A new study revealed that these algorithms were at risk of producing discriminatory outcomes.[1] De Kock responded with three claims: first, the debate was based on emotions. Second, the thesis that algorithms discriminate was nonsensical – their precise function was to class data based on characteristics and affinities, i.e. to discriminate in the literal meaning of the word. Third, to discriminate in the sense of making unjust and prejudicial distinctions based on markers such as race and sex was exactly what the algorithm did *not* do. "The algorithm is amoral…it is not bothered by a poor night's sleep or an annoying downstairs neighbor." It therefore could not make unjust distinctions. The problem, according to Kock, was the data that trained the algorithm. People introduced their biases in the data, not the algorithm. Biased data, however, was easy to detect and

[1] J. Schellevis and W. de Jong, "Overheid gebruikt op grote schaal voorspellende algoritmes, 'risico op discriminatie,'" *NOS* 29.05.2019. The article cites researcher Marlies van Eck who argues that "discrimination is inherent to predictive algorithms."

correct. He thus concluded that "if checked by people, algorithms can contribute to a society in which everyone is treated equally in equal cases."[2]

De Kock's defense of algorithms and their neutrality is very different in tone than recent scientific publications on algorithmic bias. Whereas de Kock sets up a fundamental distinction between the nonhuman amoral algorithm and the flawed biased human, information scholar Safiya Umoja Noble rejects this opposition in *Algorithms of Oppression*. Noble argues that the artificial division between humans and algorithms lies at the heart of the issue of algorithmic bias. She states that "we have automated human decision making and then disavowed our responsibility for it." Instead, we need to acknowledge that the mathematical formulations that drive automated decisions are produced by humans. "While we often think of terms such as 'big data' and 'algorithms' as benign, neutral, or objective, they are anything but. The people who make these decisions hold all types of values, many of which openly promote racism, sexism, and false notions of meritocracy."[3] We have come to believe that humans are separated from the technologies and machines they produce. As a result, we trust technologies to be objective. Indeed, the separation between observer and observed object lies at the heart of scientists' trust in the objectivity of their research. But as feminist scholars Karen Barad, Donna Haraway, and Annemarie Mol have argued, this separability is not a fixed reality, but a fragile relationship performed or enacted in practice. Disembodiment may seem to promise scientific objectivity and a view from nowhere, but science remains embodied and situated all the way through.[4]

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[2] Peter de Kock, "Algoritmes discrimineren niet," *NRC,* 23 August 2019.

[3] Safiya Umoja Noble, *Algorithms of Oppression: How Search Engines Reinforce Racism* (New York: New York University Press 2018) 1-2, 181.

[4] Donna Haraway, "Situated Knowledges: The Science Question in Feminism and the Privilege of Partial Perspective," *Feminist Studies* 14:3 (1988) 575–99; Karen Barad, "Posthumanist Performativity: Toward an Understanding of How Matter Comes to Matter," *Signs* 28 (2003) 801-831; Annemarie Mol, *The Body Multiple: Ontology in Medical Practice* (Durham: Duke University Press 2002).

Algorithmic bias becomes especially problematic in one realm of implementation: biometric technologies that identify and surveil individuals through phenotypic data and machine-learning algorithms such as facial scanners. Developers trust that biometric technologies operate objectively and are superior to human reasoning. Biometrics specialist John D. Woodward writes about facial recognition:

> While humans are adept at recognizing facial features, we also have prejudices and preconceptions…Facial recognition systems do not focus on a person's skin color, hairstyle, or manner of dress, and they do not rely on racial stereotypes. On the contrary, a typical system uses objectively measurable facial features, such as the distances and angles between geometric points on the face, to recognize a specific individual. With biometrics, human recognition can become relatively more 'human-free' and therefore free from many human flaws.[5]

The mathematical formulas and geometric shapes that drive facial recognition algorithms are assumed to transcend the human context of scientific inquiry and production, but developers revive historical racial prejudices in the algorithm's design. As a result, these scanners fail to recognize certain populations or over-identify them in criminal databases, as scholars across the disciplinary spectrum have begun to show.[6] Headlines about racist biometric technologies continue to surface in the media.

This dissertation traces the *longue durée* history of how we have come to trust biometric technologies that interact with faces and skulls to be objective and neutral, while in practice they perpetuate racial bias. The dissertation shows that today's biometric practices are not neutral and

[5] John D. Woodward et al, *Biometrics: Identity Assurance in the Information Age* (New York: MacGraw-Hill/Osborne 2003) 254.

[6] See for examples and extensive discussion: Joseph Pugliese, "Biometrics, Infrastructural Whiteness, and the Racialized Zero Degree of Nonrepresentation," *boundary 2* 34:2 (2007) 105–33; Joseph Pugliese, *Biometrics: Bodies, Technologies, Biopolitics* (London: Routledge 2010); Joy Buolamwini and Timnit Gebru, "Gender Shades: Intersectional Accuracy Disparities in Commercial Gender Classification," *Proceedings of Machine Learning Research* 81 (2018) 1–15; Virginia Eubanks, *Automating Inequality: How High-Tech Tools Profile, Police, and Punish the Poor* (New York: Picador 2018); Safiya Umoja Noble, *Algorithms of Oppression: How Search Engines Reinforce Racism* (New York: New York University Press 2018); Ruha Benjamin, *Race After Technology: Abolitionist Tools for the New Jim Code* (Medford, MA: Polity 2019).

novel but build on older biometric technologies developed during the apex of colonialism and racial science.

The Lives and Afterlives of Skulls presents the origins and development of the biometric study of race and skulls between 1880-1950. A turn to quantification in racial research marks this period: researchers increasingly relied on measurements and statistical methods to develop racial classifications of the world's populations. With a central focus on racial data and the practices that produced the data, the dissertation is a transnational history that follows the data from measurement encounters in colonial spaces, to laboratories in the United States and Europe, to printed form in publications. It shows how racial researchers circulated data and measuring instruments across the Atlantic and strove to standardize measurement techniques internationally. Its central intervention, however, is to ground the global study of human variation in a laboratory that has long been ignored in histories of race and anthropology: Karl Pearson's Biometric Laboratory in London. Pearson, known today as one of the founders of mathematical statistics, became deeply engaged with racial science in the early 20th century. My dissertation sets out to show how Pearson became paramount to a long-term turn to measurement in racial science and radically transformed methods of quantifying race. It also seeks to answer to what extent the technologies he introduced continue to inform facial biometry today.

Approach: Quantification and the Bodies of Racial Science

Scientists have long been engaged in the study of human variation. Anthropology and the study of race emerged during the establishment and expansion of European colonial power and was devoted to the description and analysis of non-European societies dominated by European powers. Thus, the anthropological methods of measuring people's bodies and bones are "rooted in an unequal power encounter between the West and the Third World."[7] In the United States, for instance, white settlers claimed Indigenous land and lives, while scientists studied their cultures and bodies and collected the remains of their deceased relatives. In developing racial taxonomies and theories of man's racial evolution, this research brought together methods from natural history and comparative anatomy. The natural history tradition of collecting specimens took the form of shipping thousands of human remains back to Europe's museums and laboratories, where researchers meticulously described the foreign bones and skulls and compared them to anthropoid and European bony remains.[8] While researchers analyzed and measured bones, blood, hair, skin texture and color, and teeth, skulls played a central role in unraveling the racial links between humans: researchers believed that they varied markedly between races. They considered skulls more "stable" than the living body because personal qualities relating to age, sex, and health left minimal imprints on the bony structure. Skulls of people long gone furnished researchers with the scientific material to dig further back in time and explore human evolution.[9] Finally, as the seat of the brain, racial researchers

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[7] Quotation from Talal Asad (ed.), *Anthropology and the Colonial Encounter* (Itacha: Itacha Press 1973) 16. See also Londa Schiebinger, *Nature's Body: Gender in the Making of Modern Science* (New Brunswick: Rutgers University Press 1994); Bronwen Douglas, *Science, Voyages, and Encounters in Oceania,* 1511-1850 (Basingstoke: Palgrave Macmillan 2014); Suman Seth, *Difference and Disease: Medicine, Race, and Locality in the 18th-Century British Empire* (Cambridge, UK: Cambridge University Press 2018). Early modern scientists not only studied humans, but also plants: Londa Schiebinger, *Plants and Empire: Colonial Bioprospecting in the Atlantic World* (Cambridge, MA: Harvard University Press 2009); Daniela Bleichmar, *Visible Empire. Botanical Expeditions and Visual Culture in the Hispanic Enlightenment* (Chicago: University of Chicago Press 2012). See for a wider discussion of science, medicine, and empire: James Delbourgo and Nicholas Dew (eds.) *Science and Empire in the Atlantic World* (New York: Routledge 2008); Harold Cook, *Matters of Exchange: Commerce, Medicine, and Science in the Dutch Golden Age* (New Haven: Yale University Press 2007); Kapil Raj, *Relocating Modern Science: Circulation and the Construction of Knowledge in South Asia and Europe, 1650-1900* (Basingstoke: Palgrave Macmillan 2007).

[8] Martin Legassick and Ciraj Rassool, *Skeletons in the Cupboard: South African Museums and the Trade in Human Remains 1907-1917* (Cape Town: South African Museum 1999); Ann Fabian, *The Skull Collectors: Race, Science, and America's Unburied Dead* (Chicago: University of Chicago Press 2010); Sadiah Qureshi, *Peoples on Parade: Exhibitions, Empire, and Anthropology in Nineteenth-Century Britain* (Chicago: University of Chicago Press 2011); Samuel J. Redman, *Bone Rooms: From Scientific Racism to Human Prehistory in Museums* (Cambridge, MA: Harvard University Press 2016). The history of collecting and museums reaches back to the early modern period: Paula Findlen, *Possessing Nature: Museums, Collecting, and Scientific Culture in Early Modern Italy* (Berkeley: University of California Press 1994); James Delbourgo, *Collecting the World: Hans Sloane and the Origins of the British Museum* (Cambridge, MA: Harvard University Press 2017).

[9] Western scientists also perceived non-western folks or "primitive" races as the embodiment of an earlier time in man's historical development. See Johannes Fabian, *Time and the Other: How Anthropology Makes Its Object* (New York: Columbia University Press 1983).

assumed that the skull bore marks of the brain's intellectual powers, which were also believed to vary among races.[10]

As the cranial collections grew along with Europe's imperialist activities, researchers found themselves perplexed by increasingly complex racial relationships. They struggled to capture the small links between closely related races with observations and descriptions. In the 19th century, they began developing a system of racial measurements, centered around the various lengths, breadths, and indexes of the body. The skull again proved to be a central resource: its hard, bony structure allowed for precise measurements that could be repeated. Even though the quantification of the skull, *craniometry*, did not supersede the skull's observation and description, *craniology*, from the end of the 19th century craniometry became the norm in racial science. Quantification continued to grow in importance throughout the first half of the 20th century. These developments coincided with the disciplinary development of a new research field that brought together the above-mentioned questions and approaches: physical anthropology.

This dissertation examines the historical conditions of the rise and development of quantification in racial science and discusses major transitions and continuities in methodology up to 1950. It approaches this history in three ways. First, it grounds these developments in Karl Pearson's Laboratory at University College London. A trained mathematician, Pearson was one of the first scholars to lay the foundation for present-day mathematical statistics. In developing novel statistical methods, Pearson turned to the quantitative study of biological problems such as variation and

[10] There are obvious historical connections between the craniometric study of skulls and the phrenological study of heads. American anthropologist Samuel Morton, for instance, is generally considered one of the first to systematically collect and measure a large number of skulls for racial purposes. Morton was also an advocate of phrenology, a science that promoted the idea that the lumps and bumps on the head's surface revealed information about certain "centers" of the brain. Phrenologists set up considerable cranial collections and developed specialized instruments such as the spreading caliper to measure the head's surface. See: James Poskett, *Materials of the Mind: Phrenology, Race, and the Global History of Science, 1815-1920* (Chicago: University of Chicago Press 2019). One phrenological collection ended up at Pearson's Laboratory, today known as the "Robert Noel Collection." Pearson, who dismissed phrenology as a pseudoscience in a lecture on the "Association of Mental and Physical Characters in Man," happily accepted this collection when donated by a family member. Karl Pearson Archive, Box 48, 2/1/15; Galton Laboratory Archive, Box 38, 5/4 correspondence with Lady Lovelace.

heredity and helped establish the new research field *biometry*. The available historical literature has amply discussed Pearson's contributions to statistics and heredity studies.[11] His engagements with racial research, however, remain largely unexplored. This project offers a fresh perspective on the history of racial research by bringing into view biometric scientists and research practices that are rarely discussed in histories of race and anthropology but played a crucial role in racial science's development. It exposes how Pearson became engaged with racial research and deeply transformed anthropology's methodology by applying statistical methods to questions of race in the early 20th century. I discuss how he and the workers and students of his lab amassed a 7,000-piece skull collection, mined publications for racial data and created a racial database, and developed statistical formulas for racial classification. Grounded in mathematics and geometrics, these biometric approaches were far removed from anthropology's more common methodology of measurement, observation, and description. Indeed, the "biometricians" strongly believed that their methodology made racial research more "scientific" as they called it, which meant more objective and less biased. Their statistical methods had the power to unlock more "truthful" racial classifications and histories of racial evolution than anthropologists had been able to do before.

Second, the dissertation researches racial science's methodology through the instruments, technologies, and bodies that made it possible. In the 19th and 20th century, scientists developed various instruments and technologies that enabled the quantification of race. My story begins with the spreading caliper, an instrument specially designed for measuring skulls and heads of living people. Properly wielding the caliper required a controlled, coordinated performance during which the researcher held the instrument in one hand, balanced the caliper's legs on the fingers of the other hand, while carefully reading the scale on the instrument. It created an intimate encounter, the researcher's body touching the skull or head of another person, while pinching the flesh or bony

[11] See further below for a discussion of this literature.

surface with the caliper's steel legs. Even though measuring a skull in a lab was in many ways different from measuring living people in the field, as chapter 1 discusses, I argue that both cases were about encountering *humans* and turning them into objects of research.[12] Other instruments, such as *coordinatographs* and *stereographs*, transformed the morphological skull into geometric paper representations. These instruments propped the skull in front of a piece of paper and drew the skull's landmarks and form onto the paper with movable pencil arms, steered by the researcher's hands. Technologies such as Pearson's *Coefficient of Racial Likeness* barely engaged the morphological skull. They "reduced" the skull's measurements, combined it with the data from its racial group, and provided numerical classification tools. The researcher put the required data into the formula and made the calculations with pen, paper, and calculation machines. The dissertation thus probes the entanglement of bodies, dead and alive, instruments, and technologies in racial research practices. It aims to understand how racial knowledge emerged during these encounters and how they enacted *objectivity*.

Third, racial data, the product of these encounters, plays a central role in this project. While looking for archival evidence of human encounters and measurement practices in the field, data was the main *thing* I found in the personal papers of American, British, German, and Dutch racial researchers: I pulled heaps of measurement cards, notebooks with cranial measurements, and scraps of paper with calculations from the archival boxes and folders. Clearly, racial researchers found these data and calculations worth holding onto: they could revisit the data throughout their career with new technologies and methods of analysis, possibly revealing new insights about race. They shared the data with colleagues at home and abroad. Today, researchers in economic history and the life

[12] I'm following Annemarie Mol's interpretation of death, who writes: "[the] corpse is active. It tells that someone's life has ended. It tells of death." Annemarie Mol, *The Body Multiple: Ontology in Medical Practice* (Durham: Duke University Press) 49-50.

sciences reuse this historic data that is housed in archives across the globe.[13] *The Lives and Afterlives of Skulls* traces data's *life cycle* to offer new insights into the relationship between measurement practices and racial theories. Historians have pointed out that data and statistics played an increasingly important role in racial science,[14] but this is the first study that explains how and why this happened. A focus on data reveals how morphological skulls transformed into measurements written down on forms and cards and how those measurements were grouped in indexes, averages, probable errors, and standard deviations. I show how statistical formulas created clusters of data that were used to develop racial classifications. These classifications fueled racial theories published in journals and monographs along with the data and sometimes the raw measurements. Data, then, connects the encounters between racial researchers and research subjects to racial theories. Moreover, in taking data as a vantage point, I cut across disciplinary boundaries and reveal that the study of racial science and its data requires a wide perspective beyond anthropology. The dissertation brings together the interactions and conflicts between physical and cultural anthropologists, anatomists, statisticians, and eugenicists.

In taking up the history of quantification and classification in racial science, the dissertation builds on a rich literature on the histories of measurement, bureaucracy, and statistics. Scholars such as Ian Hacking, Sarah Igo, John Carson, and Dan Bouk have explored how histories of the census, citizen surveys, and intelligence tests have shaped modern societies and the way they engage with their subjects. These histories unsettle social and bureaucratic categories that seem "natural" by revealing the power dynamics involved in imposing categories onto people. Such interventions often

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13 Richard Jantz, "Franz Boas and Native American Biological Variability," *Human Biology* (1995) 345–53; Emöke Szathmáry, "Overview of the Boas Anthropometric Collection and Its Utility in Understanding the Biology of Native North Americans," *Human Biology* 67:3 (1995) 337–44; Richard Jantz, "The Anthropometric Legacy of Franz Boas," *Economics & Human Biology* 1:2 (2003) 277–84; Alexander Moradi, "Towards an Objective Account of Nutrition and Health in Colonial Kenya: A Study of Stature in African Army Recruits and Civilians, 1880–1980," *The Journal of Economic History* 69:03 (2009) 719–54.

14 Stephen Jay Gould, *The Mismeasure of Man* (New York: W.W. Norton & Company 1996); Andrew Zimmerman, *Anthropology and Antihumanism in Imperial Germany* (Chicago: University of Chicago Press 2001) 87-88.

build on Michel Foucault's notion of biopolitics, governance through the regulation of bodies, and urge that categories have held people in place. These histories uncover that categories are devices of control.[15]

Scholars interested in data and measurements have paid increasing attention to data science's historical conditions of emergence and development, compelled by current-day claims of "Big Data's" novelty. Historians have begun to demonstrate how today's data practices are connected to a much longer history of information recording, storing, communicating, and processing.[16] One result of this interest is new attention paid to the present-day reuse of historic datasets. Recent studies criticize the unproblematized reuse of old data, especially body measurements, and argue that such reuse perpetuates the oft exploitative origins of the data.[17] The dissertation's focus on racial data builds on these new directions in the history of data. The conclusion will discuss why deep histories of racial data's origins matter in light of present-day reuse of historical anthropological data.

The dissertation combines this approach to the history of data and quantification with another exciting vein of scholarship that explores the relationship between humans, matter, and nature in novel ways. New approaches in feminist science studies that can largely be grouped under "New Materialism" question the distinction between body-mind, nature-culture, and subject-object

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15 Michel Foucault, *The History of Sexuality* 1st American edition (New York: Pantheon 1985); Geoffrey C. Bowker and Susan Leigh Star, *Sorting Things Out. Classification and Its Consequences* (Cambridge, MA: MIT Press 1999); Ian Hacking, "Making up people," *Historical Ontology* (Cambridge, MA: Harvard University Press 2004) 99-114; John Carson, *Measure of Merit: Talents, Intelligence, and Inequality in the French and American Republics,* 1750-1940 (Princeton: Princeton University Press 2007); Sarah Igo, *The Averaged American: Surveys, Citizens, and the Making of a Mass Public* (Cambridge, MA: Harvard University Press 2008); Dan Bouk, *How Our Days Became Numbered: Risk and the Rise of the Statistical Individual* (Chicago: University of Chicago Press 2015); Heinrich Hartmann, *The Body Populace: Military Statistics and Demography in Europe before the First World War* (Cambridge, MA: MIT Press 2019); Jacqueline Wernimont, *Numbered Lives: Life and Death in Quantum Media* (Cambridge, MA: MIT Press 2019).

16 Elena Aronova, Christine von Oertzen, and David Sepkoski, "Introduction: Historicizing Big Data," *Osiris* 32 (2017) 1-17; Soraya de Chadarevian and Theodore Porter, "Introduction: Scrutinizing the Data World.," *Histories of Data and the Database.* Special issue of *Historical Studies of the Natural Sciences* 48:1 (2018) 549–56.

17 See especially Jenny Reardon and Kim TallBear, ""Your DNA is History": Genomics, Anthropology, and the Construction of Whiteness as Property," *Current Anthropology* 53:S5 (2012) S233–45; Joanna Radin, ""Digital Natives": How Medical and Indigenous Histories Matter for Big Data," *Osiris* 32:1 (2017) 43–64.

and argue instead for the analysis of events during which material-discursive realities are temporarily performed or enacted. Scholars such as Annemarie Mol direct focus to the situated encounters, practices, and moments where matter and culture act together and produce temporary and ever-shifting meaning or reality. This approach enables me to analyze how biometric technologies enacted objectivity in various data practices, but did not solidify a "human-free," objective approach to researching human race.[18]

Argument: Trust, Automation, and Bias

The dissertation shows that quantification of racial research did not take place without struggle and controversy. I reveal a crisis around 1900, spurred by a heterogeneous approach to studying race. Measurements and instruments like the skull-measuring caliper were introduced in the 19th century to infuse anthropology with precision. Meanwhile, researchers continued to study skulls with a natural historian's "trained eye." By 1900, racial data had accumulated without clear taxonomic value. Along with a variety of racial schemes, researchers began to express distrust in quantification and confusion about the direction of racial research. It is in this context that Pearson and his colleagues began to intervene with biometric methods. Pearson did not simply introduce new methods to anthropological research in the early 20th century but also criticized and attacked anthropologists along the way. In letters and his journal *Biometrika*, he expressed profound dissatisfaction with anthropology's methods of observation, description, and simple measurement. His controversialist attitude was met with resistance – not all racial researchers welcomed the

[18] Donna Haraway, "Situated Knowledges: The Science Question in Feminism and the Privilege of Partial Perspective," *Feminist Studies* 14:3 (1988) 575–99; Karen Barad, "Posthumanist Performativity: Toward an Understanding of How Matter Comes to Matter," *Signs* 28 (2003) 801–31; Annemarie Mol, *The Body Multiple: Ontology in Medical Practice* (Durham: Duke University Press 2003); Jane Bennett, *Vibrant Matter* (Durham: Duke University Press 2010); Iris van der Tuin and Rick Dolphijn, *New Materialism: Interviews & Cartographies* (Utrecht: Open Humanities Press 2012). See for the application of new materialism in the realm of historical research: Geertje Mak, *Doubting Sex: Inscriptions, Bodies and Selves in Nineteenth-Century Hermaphrodite Case Histories* (Manchester: Manchester University Press 2013); Iris Clever and Willemijn Ruberg, "Beyond Cultural History? The Material Turn, Praxiography, and Body History," *Humanities* 3:4 (2014) 546–66; Willemijn Ruberg, *History of the Body* (London: Red Globe Press 2020).

"gospel" of biometry. Some anthropologists mistrusted these interventions coming from researchers without formal anatomical or anthropological training and continued to value the trained eye approach. Other statisticians pointed out problems with Pearson's biometric methods and dismissed them. In the background of these controversies, a lack of standardization of racial measurements occupied researchers around the globe. Throughout the late 19th century and early 20th century, efforts to internationally standardize methodology both united racial researchers and pitted them against each other.

At the core of quantification and biometry was a desire to make racial research *objective*, meaning to minimize human intervention and eliminate human presence in the research process.[19] In racial research, this included denying the subjectivity of the researcher and the research subject. Quantification technologies attempted to turn the researcher into an *automaton* that mechanically moved instruments and passively recorded data produced by these technologies. Geometric cranial projections, for instance, aimed to eliminate the embodied vantage point of the researcher. In drawing geometric projections, the researcher departed from the more-common perspective drawings and had to "consent to become a mere machine, which does nothing but mark with pencil or pen the point which indicates the perpendicular ray," as German anthropologist Carl Vogt wrote in 1863.[20] Pearsonian interventions furthered these ambitions: they entailed a critique of the anthropologist's subjective trained eye expertise and made new use of the caliper by combining it with disembodied statistical formulas. These formulas "reduced" the measurements of bodies to

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[19] In *Objectivity* (New York: Zone Books 2007), Lorraine Daston and Peter Galison argue that from the mid-19th century, a new epistemological virtue of scientific sight took hold in science: mechanical objectivity. In earlier times, scientists had celebrated the subjectivity of the scientists and their skilled judgment and embodied expertise. This new form of objectivity, however, largely denied the subjectivity of the researcher and aimed to produce knowledge that bore no trace of the knower. I don't follow Daston and Gallison's periodization here, but much of my thinking about objectivity and biometry has been shaped by *Objectivity*. Importantly, I argue that transcendental seeing ultimately was impossible and a form of violence, following Donna Haraway's "Situated Knowledges." The conclusion will further discuss this perspective.

[20] K. C. Vogt (translated by James Hunt), *Lectures on Man: His Place in Creation, and in the History of the Earth* (London: Longman, Green, Longman, and Roberts 1864) 76.

mathematically modified and simpler forms and appeared to calculate racial classifications without any foreign help. The dissertation thus traces a shift in producing classifications from the hands and eyes of anthropologists to disembodied instruments and technologies such as measurements and formulas. I theorize that late 19th and early 20th century quantification technologies attempted to *automate* racial research before the computer: they were the product of racial researchers' disembodied dreams and desire to mechanize and quantify the research process, to make it more *objective*.

Quantification also erased the subjecthood of the research subjects, first by turning humans into passive bodies to be measured, and second by converting those bodies into data. The earlier mentioned *stereograph* transformed the research subject, the morphological skull, into a geometric projection. The instrument reduced the skull to lines on paper, far removed from its three-dimensional shape that once housed a living, breathing person. Racial measurements and data fragmentized the skull into the bits and pieces of indexes, lengths, and breadths and pieced them back together in one-dimensional, univariate aggregates. When reading the history of quantifying race, one must heed another meaning of *reduction*: to bring a person under control, to subdue. Quantification and statistical reduction conquered subjects through numbers.

However, the chapters that follow also show that it was impossible to erase subjectivity from the research process. The work involved was deeply embodied, from the field to the lab. The caliper and the intimate encounter it necessitated between researcher and research subject remained at the base of *all* cranial data. Subjects could resist and instruments and skulls broke. The lab was a site of physical labor, where researchers carefully cleaned and measured skulls and processed calculations with Brunsviga calculating machines. The researcher's self could not be suppressed: *how* to be impersonal and objective became an emotional and moral question for many researchers who were proud of their skills. As this dissertation shows, improving and standardizing anthropology's methodology became infused with parochialism and chauvinism. It thus reveals that racial data and biometric technologies not only represented disembodied dreams but also the messy practices in which the relationship between objectivity, data, and race was anything but clear.[21] What is more, new biometric methods never undermined the existence of fundamental racial differences. Instead, it gave a new voice to old ideas about race. Thus, deep methodological changes in the study of race only partially transformed ideas about race: old racial biases were built into new technologies. Scientific rigor was no cure for racial bias, as researchers like De Kock today believe.

At the same time, the history of methodological controversy and messy data practices were forgotten or made irrelevant, while belief in the objectivity of quantification and biometry took hold and deepened in the 20th century. Automation, I argue, built a powerful sense of objectivity into biometric technologies of faces and races.

A brief note on terminology will help situate the reader. In discussing race and quantification, I use the terms *racial science* and *racial research* to indicate a wide field of study that brought together scientists from a range of fields, such as physical anthropology, anatomy, zoology, and biometry. While fields such as physical anthropology and biometry developed into specialized disciplines in the late 19th and early 20th centuries, they are treated here as new branches of racial science, a field of study with a long tradition. I employ the term *data practices* to refer to a range of quantitative and qualitative methods, technologies, and activities involved in the production of racial data. As we shall see, racial data was not restricted to numerical form alone: it could take shape in

[21] Ted Porter comes to similar conclusions about the presence and absence of subjectivity in discussing Pearson and scientific life in the statistical age. Scientific life was riddled with tensions, Porter concludes, between detachment and disappearance of the self on the one hand, and connectedness and egoism on the other. About the disappearance of the self, Porter writes that statistics stressed that individuals were merely part of a collective and that Pearson "became the voice of impersonal objectivity." At the same time, "science should not annihilate the self but raise it up to higher social standard. The right way could not be achieved mechanically, merely by calculation, but required the wisdom and discernment of the cultivated scientific person." Thus, according to Porter, Pearson did not advocate mechanizing or routinizing the scientific method but advocated a "self-overcoming that demanded a strong self and not a weak one." Theodore M. Porter, *Karl Pearson: The Scientific Life in a Statistical Age* (Princeton: Princeton University Press 2004) 305-309.

the form of descriptions, graphs, maps, contours, and photographs. Below, terms with quotation marks signify actors' terms, such as "primitive" and "civilized." The reader will notice, for instance, that my actors do not use the term "objectivity" themselves but instead talk about making anthropology and racial science more "scientific." I have italicized words that are either important analytical concepts or terms in languages other than English.

Background & Literature: A Fraught History

Historians have offered various interpretations of the origins of modern racial classification and have traced it back to antiquity, the Medieval period, and the early modern period.[22] Most historians, however, argue that the 18th century was a central turning point. The word "race" had come into use in the 16th century and initially referred to lineage, family, and nation. From the 18th century, scholars began to use the term to refer to fundamental human types that differed in physical characteristics, mainly skin color. Enlightenment thinkers such as Johann Blumenbach and Georges-Louis Leclerc, Comte de Buffon understood race in a dynamic way and believed that bodily characteristics could be changed by climate and civilization. From the 19th century, race "hardened," as scientists increasingly saw racial physical markers like skin color and skull size as innate and permanent and believed that these biological qualities shaped mental and moral traits. They developed various theories of racial differentiation, many of which hierarchically organized races with the "white race" occupying the top position. The measurements of skulls, bones, and bodies gave credibility to such theories and hierarchies. A science of race began to be established, which from the late 19th century developed into the discipline physical anthropology. Racial science and anthropology, however, was not a

[22] See: Benjamin Isaac, *The Invention of Racism in Classical Antiquity* (Princeton: Princeton University Press 2004); Geraldine Heng, *The Invention of Race in the European Middle Ages* (Cambridge, UK: Cambridge University Press 2018); David Nirenberg, "Was There Race Before Modernity? The Example of 'Jewish' Blood in Late Medieval Spain," in: M. Eliav-Feldon, B. Isaac, and J. Ziegler (eds.), *The Origins of Racism in the West* (Cambridge, UK: Cambridge University Press 2010) 232-265; Jean E. Feerick, *Strangers in Blood: Relocating Race in the Renaissance* (Toronto: University of Toronto Press 2010).

purely academic endeavor. The production of racial knowledge about "primitive" and "savage" races became intertwined with various nationalist, racist, and imperialist campaigns in the 19th and 20th centuries. Indeed, historians have shown how it provided justification for the dehumanization of non-white folks, slavery, racial segregation, and imperialism.[23] Notions of race carried political valence.

These developments coincided with the emergence of the eugenics movement in the late 19th century. Eugenics, "the science of improving stock," built on the assumption that the biological body, as well as mental and moral traits, were unchanging and determined by heredity. Scientists, public health officials, and policy makers began promoting the idea that social problems such as crime and mental illness resulted from hereditary defects and could be eliminated through "proper breeding." Alarmed by the perceived rapid "degeneration" of civilized society, advocates across the political spectrum became enthralled with improving society's population by encouraging reproduction of those with favorable traits and discouraging those with unfavorable traits. Karl Pearson's mentor Francis Galton not only coined the term *eugenics* but also pioneered a new research field with his statistical study of the inheritance of intelligence between parents and offspring, *Hereditary Genius* (London: Macmillan and Co. 1869). By the turn of the 20th century, various countries around the world established eugenics organizations and put policies in place that aimed to control populational breeding through marriage regulations and sterilization programs.[24]

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[23] For a grand overview of racial science's development and reach, see Ali Rattansi, *Racism. A Very Short Introduction* (Oxford: Oxford University Press 2007) and George M. Fredrickson, *Racism: A Short History* (Princeton: Princeton University Press 2016). See also Keith Breckenridge, *Biometric State. The Global Politics of Identification and Surveillance in South Africa, 1850 to the Present* (Cambridge, UK: Cambridge University Press 2014).

[24] Daniel J. Kevles, *In the Name of Eugenics: Genetics and the Uses of Human Heredity* (Cambridge, MA: Harvard University Press 1995); Hans-Walter Schmuhl, *The Kaiser Wilhelm Institute for Anthropology, Human Heredity and Eugenics, 1927-1945: Crossing Boundaries* (Berlin: Springer 2008); Alison Bashford and Philippa Levine (eds.), *The Oxford Handbook of the History of Eugenics* (Oxford: Oxford University Press 2010); Nathaniel Comfort, *The Science of Human Perfection: How Genes Became the Heart of American Medicine* (New Haven: Yale University Press 2012); Stefan Kühl, *For the Betterment of the Race: The Rise and Fall of the International Movement for Eugenics and Racial Hygiene* (Basingstoke: Palgrave Macmillan 2013); Alexandra Stern, *Eugenic Nation: Faults and Frontiers of Better Breeding in Modern America* (Berkeley: University of California Press 2015); Philippa Levine, *Eugenics: A Very Short Introduction* (New York: Oxford University Press 2017).

The ideology of controlled human breeding in the face of degeneration was in many ways restricted to fears and policies *within* the nation or race. Nevertheless, it also became intertwined with ideas about racial difference and hierarchies. Karl Pearson, for instance, expressed this notion in his lecture "National Life from the Standpoint of Science," delivered at the Literary and Philosophical Society of Newcastle upon Tyne in 1900. He introduced the audience to the "urgent" problem that increased civilization and the lessened struggle for existence led to the over-fertility of the unfit and the lessened fertility of fitter stocks. "You cannot change bad stock to good…until it ceases to multiply it will not cease to be," he concluded.[25] This logic also applied to "the lower races of man." "How many centuries, how many thousand of years, have the Kaffir or the negro held large districts in Africa undisturbed by the white man? Yet their intertribal struggles have not yet produced a civilization in the least comparable with the Aryan. Educate and nurture them as you will, I do not believe that you will succeed in modifying the stock."[26] "The only healthy alternative," Pearson urged, "is that he [the white man] should go and completely drive out the inferior race." He pointed to Australia and the United States as successful examples.[27]

Indeed, eugenics advocates in the United States began understanding racial mixing as a form of degeneration in the context of Jim Crow and immigration waves from southern and eastern Europe. In Germany, eugenic zeal fused with fears of and violence against Jewish populations, which surged in the early 20th century. Anthropologists such as Eugen Fischer played an important role in the process of defining strangers and enemies to the German nation-state by using skull measurements and hereditary studies to claim "Aryan" superiority and demonstrate that "lower"

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25 Karl Pearson, *National Life from the Standpoint of Science* (London: Adam and Charles Black, 1905) 19.

26 Karl Pearson, *National Life from the Standpoint of Science*, 21.

27 Karl Pearson, *National Life from the Standpoint of Science,* 23. See also Margaret Jacobs, *White Mother to a Dark Race: Settler Colonialism, Maternalism, and the Removal of Indigenous Children in the American West and Australia, 1880-1940* (Lincoln: University of Nebraska Press 2009). Jacobs explores the ways in which the histories of racial relationships and settler colonialism differed and aligned between the United States and Australia regarding Indigenous child removal practices.

races such as Jewish people weakened the nation mentally and physically. With the rise of Nazi power, racial purification efforts commenced, first with sterilization and marriage restrictions, and from 1939, with the mass-murder of millions of Jews, people with disabilities, and other minority groups.[28]

Scholars began questioning eugenic policies and challenging racism in the 1930s. With new insights from the study of genes, geneticists criticized eugenics' simplistic understanding of the relationship between genes, the body, and heredity. In anthropological circles, a small group of scientists began speaking out against Nazi scientific racism. Although most anthropologists considered Nazi theory nonsense, "many scientists in the United States and Britain…had long believed that public advocacy on controversial political issues was incompatible with the 'objectivity' assumed to be the hallmark of scientific practice, Michelle Brattain explains.[29] As a result, "most scientists were hesitant to join the political frontier in the intellectual battle to discredit racism," Elazar Barkan concludes.[30] Anthropologists such as Ashley Montagu, Julian Huxley, Alfred Haddon, and Franz Boas, however, publicly combatted racism in publications, newspaper articles, and public lectures.[31]

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[28] Robert Proctor, *Racial Hygiene: Medicine Under the Nazis* (Cambridge, MA: Harvard University Press 1988); Mario Biagioli, "Science, Modernity, and the 'final Solution,'" in: Saul Friedlander, *Probing the Limits of Representation. Nazism and the 'Final Solution'* (Cambridge, MA: Harvard University Press 1992) 185-204; Sheila Weiss, *The Nazi Symbiosis: Human Genetics and Politics in the Third Reich* (Chicago: University of Chicago Press 2010).

[29] Michelle Brattain, "Race, Racism, and Antiracism: UNESCO and the Politics of Presenting Science to the Postwar Public," *The American Historical Review* 112:5 (2007) 1386–413, quote on page 1390.

[30] Elazar Barkan, *The Retreat of Scientific Racism: Changing Concepts of Race in Britain and the United States Between the World Wars* (Cambridge, UK: Cambridge University Press 1992) 280.

[31] British biologist Julian Huxley and anthropologist Alfred Haddon addressed the public in 1935 in *We Europeans: A Survey of 'Racial' Problems* (London: Jonathan Cape 1935), "a scientific statement written in popular form" that targeted the "pseudo-scientific" Nazi racial theories and the widespread ignorance about the term race, according to Barkan. Quite radically, the authors suggested replacing the term "race" for "ethnic groups." Historians have widely recognized American-German anthropologist Franz Boas as the most active anthropologist to combat racism. In the mid-1930s, Boas made several attempts to mobilize scientists to speak out against Nazi racial theories and racist policies. His efforts within American anthropology proved unsuccessful. Together with anthropologist Earnest Hooton he drafted the "Ten Statements about Race," but only found one anthropologist, Aleš Hrdlička, willing to sign it – others did not want to meddle with politics or engage in controversial topics. Boas then directed his attention to a wider scientific circle and

In the aftermath of World War II and the horrors of Nazi racial policies, anthropologists arguably felt the need to distance themselves from the recent associations of the discipline with eugenics and Nazi science. One effort came out of the United Nations Educational Scientific and Cultural Organization, founded in 1945. UNESCO formed a group of scientific experts to combat racial hatred and scientifically "false" ideas about race. Headed by Ashley Montagu, this group drafted the "UNESCO Statement on Race" in 1950, which bore Montagu's imprint of rejecting "race" in science and replacing it with "ethnic groups." This statement and its definition of race, however, proved controversial within anthropological and genetic circles and in 1951 a new expert panel published a Second Statement on Race. This revised statement claimed that race was a classificatory device and suggested that genetic differences between races possibly existed.[32]

Another attempt to push physical anthropology into a new direction came from American anthropologist Sherwood Washburn. In his 1951 article "The New Physical Anthropology," he argued that anthropology was moving away from the static measurement of races. "The old physical anthropology was primarily a technique," centered around the caliper, measurements, statistics, and classification. "The new physical anthropology is primarily an area of interest, the desire to understand the process of primate evolution and human variation by the most efficient techniques available." Its focus should be the experimental study of genes and breeding populations, Washburn urged. Furthermore, it needed to adopt the "Modern Synthesis," a new theory of evolution that had recently synthesized claims about the mechanisms for evolution from genetics, systematics,

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sent out a drafted statement to scientists in various disciplines in 1938. This "Scientists Manifesto" gathered 1284 signatures and prompted the American Association of Anthropology to also publish a statement. The American Association of Physical Anthropology did not succeed in drafting a statement. See Tracy Teslow, *Constructing Race: The Science of Bodies and Cultures in American Anthropology* (Cambridge, UK: Cambridge University Press 2014) for a fuller account on Boas. Finally, historians often mention British-American anthropologist Ashley Montagu in the combat against scientific racism. From the 1940s, Montagu started attacking the concept of race in physical anthropology. Most famous in this respect is his publication *Man's Most Dangerous Myth* (New York: Columbia University Press 1942), in which he denied the biological existence of race. Chapter 4 will demonstrate that biometrician Geoffrey Morant's work deserves historical attention as another important example of published critiques of Nazi racism.

[32] Perrin Selcer, "Beyond the Cephalic Index: Negotiating Politics to Produce UNESCO's Scientific Statements on Race," *Current Anthropology* 53:S5 (2011) S173–84.

paleontology, and botany.[33] Several physical anthropologists redirected their focus away from race to population studies. Cultural anthropologists, who before the 1950s largely understood cultural differences between "primitive" and "civilized" peoples as a result from different developmental stages in unilinear evolution, began producing nuanced studies of non-western cultures. Through participant observation and with a plural, relativistic culture concept, both already introduced by Franz Boas well before 1945, anthropologists began understanding cultures by themselves, not by judging or ranking them against others.[34]

Histories of Race and Anthropology

The available literature has long followed this narrative of the "rise and fall" of race in science. The work of George W. Stocking, Nancy Stepan, and Elazar Barkan have particularly been pivotal in establishing a historiography on racial science in the 19th and 20th century.[35] Especially Stepan and Barkan developed a teleological and somewhat triumphalist narrative of the disappearance of race in science after 1945. The rise and fall narrative continues to frame new historical studies on race and anthropology. Indeed, we can identify three "rescue stories" in the literature that shape a common understanding of the development and periodization of race in science in the 20th century. First, the idea that the "good" cultural anthropology prevailed over the "bad" physical anthropology in the

33 Sherwood Larned Washburn, "Section of Anthropology: The New Physical Anthropology," *Transactions of the New York Academy of Sciences* 13:7 Series II (1951) 298–304. The following piece is also often mentioned in this context: Stanley M. Garn, "The Newer Physical Anthropology," *American Anthropologist* 64:5 (1962) 917–18. Betty Smocovitis, "Humanizing Evolution: Anthropology, the Evolutionary Synthesis, and the Prehistory of Biological Anthropology, 1927–1962," *Current Anthropology* 53:S5 (2012) S108–25. See also: Michael A. Little and Kenneth A.R. Kennedy, *Histories of American Physical Anthropology in the Twentieth Century* (Lanham, MD: Lexington Books 2010).

34 Melville J. Herskovitz, "Past Developments and Present Currents in Ethnology," *American Anthropologist* 61 (1959) 389-398.

35 George W. Stocking, *Race, Culture, and Evolution: Essays in the History of Anthropology* (New York: The Free Press 1968); Nancy Stepan, *The Idea of Race in Science: Great Britain, 1800-1960* (Hamdon, Conn: Archon Books 1982); George W. Stocking (ed.), *Bones, Bodies, Behavior: Essays on Biological Anthropology* (Madison: University of Wisconsin Press 1988); Elazar Barkan, *The Retreat of Scientific Racism: Changing Concepts of Race in Britain and the United States Between the World Wars* (Cambridge, UK: Cambridge University Press 1992).

20th century lives on in historical monographs today. Relativist and qualitative, descriptive approaches towards culture won out over deterministic and tenacious measurements of race.[36] In a recent historical study on race, sex, and science in America, historian Melissa Stein, for instance, credits the scientific trend towards cultural relativism as a major challenge to biological understandings of race in the 20th century.[37]

Second, historians propose that a turn to the study of human prehistory and evolution involved a move away from racial classification. Historian Samuel Redman argues that "scholars interested in studying human remains began to change their language from one centered on *race* to discourses surrounding *population, migration,* and *evolution*" in the late 1920s and early 1930s. They began using ancient skeletons to "solve the riddles of the human past" rather than rank races.[38]

Finally, historians and anthropological textbooks alike often identify the rise of population genetics and its novel statistical and genetical approaches to studying human difference as a crucial scientific attack against the notion of race. Central to this offense was a "new" understanding of within-group variation. American evolutionary biologist Richard Lewontin demonstrated in the 1970s that variation *between* groups was much smaller than variation *within* groups through the analysis of various genes associated with blood group systems and red blood cell enzymes. This showed that "our perception of relatively large differences between human races…is indeed a biased perception" and no justification for racial classification could be offered, Lewontin claimed.[39] With

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[36] See for a fuller discussion the introductions of Alice L. Conklin, *In the Museum of Man: Race, Anthropology, and Empire in France, 1850-1950* (Ithaca, NY: Cornell University Press 2013); Tracy Teslow, *Constructing Race: The Science of Bodies and Cultures in American Anthropology* (Cambridge, UK: Cambridge University Press 2014).

[37] Melissa Stein, *Measuring Manhood: Race and the Science of Masculinity, 1830–1934* (Minneapolis: University of Minnesota Press 2015).

[38] Samuel J. Redman, *Bone Rooms: From Scientific Racism to Human Prehistory in Museums* (Cambridge, MA: Harvard University Press 2016) 229.

[39] Richard C. Lewontin, "The Apportionment of Human Diversity," *Evolutionary Biology* 6 (1972) 381–98, quotation on page 397.

new theories such as these, Stepan concludes, the old racial science declined, and a "new, non-racial, populational genetical science of human diversity" emerged.[40]

More recently, historians have criticized and complicated the rise and fall narrative by revealing that physical and cultural anthropologists continued to support racial science before and after the war. Alice Conklin and Tracy Teslow show that even scientists who actively criticized racial dogmas and Nazi racism retained older typological and reductionist notions of human variation. Thus, scientists could oppose scientific *racism* without rejecting *race* – race was too deeply embedded in the prewar sciences for anyone to fundamentally question it.[41] Other historians have offered revisionist accounts of postwar human variation research. Susan Lindee, Rosanna Dent, Joanna Radin, Jenny Bangham, Veronika Lipphardt, and Soraya de Chadarevian have argued for the persistence of prewar approaches in the supposed postwar shift from "race" to "populations." In the aftermath of World War II, scientists aimed to move away from hierarchical racial research, but continued to exhibit an interest in isolated human populations. Like the prewar anthropologists, postwar population geneticists valued isolated populations for scientific study as they were understood to be closer to nature and more "pure." In population genetics, this meant that isolated populations were considered reservoirs of unique genes. Despite its associations with German racial politics and scientific racism, populational "mixing" and heredity also remained topics of interest.[42] Science scholars such as Jenny Reardon, Jonathan Kahn, and Steven Epstein have positioned the

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[40] Nancy Stepan, *The Idea of Race in Science*, 171. See also: Robert L. Welsh & Luis A. Vivanco, *Cultural Anthropology. Asking Questions about Humanity* (New York: Oxford University Press 2018) 252-253.

[41] Indeed, facial-racial measurements were even used to subvert Nazi racial policies. During the Second World War, Dutch anthropologist Arie de Froe used facial measurements to claim that many of Amsterdam's Jews were of a "superior" Sephardic origin than Ashkenazi Jews. This research saved hundreds of people from deportation. See H.U. Jessurun d'Oliveira (eds.), *Ontjoodst door de wetenschap: de wetenschappelijke en menselijke integriteit van Arie de Froe tijdens de bezetting* (Amsterdam: Amsterdam University Press 2015).

[42] The essays in the special issue "Human Heredity After 1945: Moving Populations Centre Stage," *Studies in History and Philosophy of Science Part C: Studies in History and Philosophy of Biological and Biomedical Sciences* 47 (2014) are especially insightful.

resurfacing of race in recent genetic research, molecular biology, and medicine within the longer history of racial science, stressing a continuance of old approaches and concerns.[43]

This project adds to the growing body of literature that challenges the conventional periodization of racial science. *The Lives and Afterlives of Skulls* uncovers major divisions, developments, and shifts in racial science's methodology taking place *before* 1945. Chapter 1 reveals 1900 to be a central turning point. Chapters 2-4 argue that Pearson's biometry was a major intervention from *within* racial research, taking place long before the outside critiques of post-war cultural anthropology and population geneticists. The dissertation's conclusion will reflect on the extent to which the biometric study of race and skulls transformed after 1945.

What is more, the chapters question the three rescue stories. While the dissertation does not explore the connections between physical and cultural anthropology, it does reveal that physical anthropology was not all about quantification but deployed qualitative methods as well. It thus obscures the dichotomy between quantitative and qualitative methods in racial science and shows how both were part of the project of classifying race. The chapters also uncover that the biometricians actively researched within-group variation and turned it into a racial characteristic rather than a weapon against race. Finally, the history of race and quantification is already very much a story of scientists obsessed with prehistory and time. What was at stake for racial scientists was how the story of human origins should be told and who should tell it. As we shall see, race played a crucial component in the history of mankind. The discussions about prehistory between racial scientists are part of a well-known story: as Johannes Fabian has argued, in order to keep certain races different and unequal, they had to be held back on a unilinear timeline as "primitive savages"

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[43] Steven Epstein, *Inclusion: The Politics of Difference in Medical Research* (Chicago: University of Chicago Press 2007); Jenny Reardon, *Race to the Finish: Identity and Governance in an Age of Genomics* (Princeton: Princeton University Press 2009); Jonathan Kahn, *Race in a Bottle: The Story of Bidil and Racialized Medicine in a Post-Genomic Age* (New York: Columbia University Press 2013).

not yet ready for civilization.[44] Prehistory and the story of humankind consolidated who was modern and who was not.

Histories of Statistics, Pearson, and Biometry

Histories of statistics and hereditary research pay ample attention to Karl Pearson and biometry. Classic studies such as Theodore Porter's *The Rise of Statistical Thinking*, Porter's 2006 biography on Pearson, Donald MacKenzie's *Statistics in Britain,* and Alain Desrosières's *The Politics of Large Numbers* introduce Pearson as one of the founders of mathematical statistics. In varying degrees of technical difficulty, these studies detail how Pearson developed modern statistical methods such as the goodness of fit test and the product-moment method by applying statistical ideas to a wide variety of social and biological problems. They stress the importance of Pearson's Biometric School, the Eugenics and Biometrics Laboratory, and the journal *Biometrika* he founded in 1902 for the development and spread of statistical theory. Pearson's controversialist nature and the disputes he sought with scientists in other disciplines also surface in these works. Most famous in this respect is the "biometrician-Mendelian" controversy about whether the study of heredity should be based on statistics or Mendelian principles. This debate was mostly fought between Pearson and his colleague Ronald Fisher, another figure central to statistics' historical development. Porter's most recent publication, *Genetics in the Madhouse*, explores how Pearson and several other scientists researched heredity through data collected by alienists in mental institutions. It reveals the intertwined history of quantification, psychiatry, heredity, and genetics.[45]

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[44] Johannes Fabian, *Time and the Other: How Anthropology Makes Its Object* (New York: Columbia University Press 1983).

[45] Theodore M. Porter, *The Rise of Statistical Thinking, 1820-1900* (Princeton: Princeton University Press 1986); Donald A. MacKenzie, *Statistics in Britain: 1865-1930; the Social Construction of Scientific Knowledge* (Edinburgh: Edinburgh University Press 1981); Alain Desrosières, *The Politics of Large Numbers: A History of Statistical Reasoning* (Cambridge, MA: Harvard University Press 1998); Theodore M. Porter, *Karl Pearson: The Scientific Life in a Statistical Age* (Princeton: Princeton University Press 2004); Theodore M. Porter, *Genetics in the Madhouse: The Unknown History of Human Heredity* (Princeton: Princeton University Press 2018).

Porter's biography on Pearson includes a brief discussion of his interests in racial differentiation and human evolution.[46] Historian Eileen Magnello, based at University College London, has offered a more extensive exploration of racial research at Pearson's Laboratory. In her 1999 article, she discusses the development of the Biometric Laboratory and explains how the anthropological study of race and skulls fit into the lab's larger focus on statistics and biology. She argues that the work done at the Eugenics and Biometric Laboratories, both run by Pearson, were very different in kind. Magnello criticizes the widespread assumption among historians that these laboratories shared a unified approach and methodology. "The widely-held assumption that Pearson's statistical techniques for analyzing biological variation were driven by his eugenic concerns," she concludes, is not correct.[47] Another London-based historian, Debbie Challis, has discussed how University College archaeologist Flinders Petrie helped Pearson establish his Biometric Laboratory through a large collection of Egyptian skulls that Petrie brought back from an expedition and donated to the lab.[48]

In general histories of anthropology or racial science, Pearson and biometrics are mostly discussed as footnotes to or diversions from larger and more important developments. No entries dedicated to biometry, Pearson, or statistics are found in Frank Spencer's *History of Physical*

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46 See Porter, *Karl Pearson*, 263.

47 While both laboratories collected and analyzed large amounts of data, they developed different methodologies: the Biometric Laboratory used statistical methods, solid geometry, and various instruments to research a wide variety of biological problems. Central to this research were Pearson's curve-fitting and goodness of fit testing, along with his correlation methods. The Eugenics Laboratory used family pedigrees and actuarial death rates to study eugenic problems such as the heredity of intelligence and mental deficiency. This research required methods that did not rely on continuous or discrete variables. Thus, the Eugenics Laboratory used different methods from the Biometric Laboratory. Furthermore, Magnello writes: "there was, in every other respect, a complete lack of correlation in the laboratories in all points, including the principal methods, finances, personnel, architectural juxtaposition and the methodological style of the journals." M. Eileen Magnello, "The Non-Correlation of Biometrics and Eugenics: Rival Forms of Laboratory Work in Karl Pearson's Career At University College London, Part 1," *History of Science* 37:1 (1999) 79–106; M. Eileen Magnello, "The Non-Correlation of Biometrics and Eugenics: Rival Forms of Laboratory Work in Karl Pearson's Career At University College London, Part 2," *History of Science* 37:2 (1999) 123–50.

48 Debbie Challis, "Skull Triangles: Flinders Petrie, Race Theory and Biometrics," *Bulletin of the History of Archaeology* 26:1 (2016) 1–8. See also Debbie Challis, *The Archaeology of Race: The Eugenic Ideas of Francis Galton and Flinders Petrie* (London: Bloomsbury 2013).

Anthropology Encyclopedia (1997).[49] Pearson and his lab often appear in relation to Franz Boas, who corresponded with Pearson in the late 19th century and shared an interest in the statistical study of race.[50] In the "rise and fall" narratives of racial science, Pearson's biometry receives significant mention as a failed enterprise that was still part of the "bad" racial typology and did not fundamentally challenge the biological concept of race. Nancy Stepan's seminal account discusses how Pearson offered a very direct challenge to racial science, often overlooked by historians, but introduced a populational approach that remained embedded in racial typology. Yet Stepan concludes that this was a problematic contradiction: "Insofar as Pearson's critique of anthropological conceptions of race was contradicted by his own, casual use of a racial typology, the contradiction was not recognised and therefore never acknowledged by Pearson himself." Moreover, she argues that his critiques "fell upon deaf ears" with anthropologists and thus his work had little effect on racial biology.[51] Elazar Barkan's account takes the notion of failure further and argues that by the 1920s and 1930s, "biometrics…was more likely a scientific ghost, one that was put to rest upon Pearson's retirement in 1933." He interprets the debate between Pearson and Fisher and Fisher's dismissal of craniometry (discussed in Chapters 3 and 4 of this dissertation) as a rightful and successful excommunication of Pearson's biometric school and evidence that Pearson's approach was "leading to a dead end." Craniometry and physical anthropology, he concludes, "a once important branch of biology…had lost touch with scientific progress."[52]

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49 Pearson is only very briefly mentioned in the entry on eugenics.

50 See for instance George W. Stocking, *Race, Culture, and Evolution: Essays in the History of Anthropology* (Chicago: University of Chicago Press 1968) 167-173, 218; Michael A. Little, "Franz Boas's Place in American Physical Anthropology and Its Institutions," in: Michael A. Little and Kenneth A.R. Kennedy, *Histories of American Physical Anthropology in the Twentieth Century* (Lanham, MD: Lexington Books 2010); Tracy Teslow, *Constructing Race: The Science of Bodies and Cultures in American Anthropology* (Cambridge: Cambridge University Press 2014) 46-49.

51 Nancy Stepan, *The Idea of Race in Science: Great Britain, 1800-1960* (Hamdon, Conn: Archon Books 1982) 134-139.

52 Elazar Barkan, *The Retreat of Scientific Racism*, 151-162.

Stepan and Barkan's interpretation of Geoffrey Miles Morant, Pearson's student and the lab's craniometry expert, deserves further discussion. Central to their interpretation of biometry's failure is Morant's *The Races of Central Europe. A Footnote to History* (London: Allen & Unwin 1939). In this book, Morant used the lab's biometric insights to challenge the Nazi racial theory that Europe consisted of several races. With biometric data and statistical observations, Morant argued that there were no European races, only complex mixtures due to centuries of migration and mixing. Stepan argues that anthropologists dismissed this book because of its challenge against racial classification, which was the anthropologist's main object.[53] Barkan, who introduces Morant with the middle name "Mackay," argues that this book showed that Morant "had finally yielded to his data" and at this point only enjoyed support from race-critic J.B.S. Haldane, "but this for humane rather than scientific reasons."[54] Based on Morant's larger oeuvre and newly discovered archival material, Chapter 4 of this dissertation instead reveals that Morant never challenged racial classification as a whole but merely criticized the idea of European races. It shows that these conclusions logically followed from decades of research at the Biometric Laboratory and were not sudden new insights. Furthermore, Morant very much enjoyed support from Haldane for scientific reasons – he even wrote the preface to *The Races of Central Europe* – as well as other leading race-critics such as Ashley Montagu who received the book very positively.

At the heart of these misunderstandings is a hesitancy to separate *race* from *racism*. Both Stepan and Barkan struggle to understand how Pearson could challenge typological strongholds such as the purity of races while at the same time putting his populational approach to typological use. How could Morant, a dedicated student of race, be outspoken against racism? Barkan and

53 Stepan, *The Idea of Race in Science*, 138-139.

54 Barkan, *The Retreat of Scientific Racism,* 158-162.

Stepan end up shelving all biometric research under eugenics – now questioned by Magnello[55] – and the failure of biometry. In this light, Pearson and Morant's work in the 1930s simply become examples of their "adamant" desire to "salvage their discipline."[56]

As discussed above, several historians dismiss the notion that race disappeared from science after 1945. They have pointed out that scientists before and after the war could hold anti-racist positions *and* study race. The "retreat of race" narrative obscures more subtle developments of racial science's methodology and its complex entanglement with racial theories. Instead, this dissertation treats the history of race as a data science and thus reveals *longue durée* efforts to collect, reduce, and reuse cranial data for questions about human evolution and variation. It is the first book-length study of the biometric study of race and skulls. It integrates the currently disconnected histories of race, anthropology, and statistics and broadens our knowledge of how problems of biological variation were foundational to the development of mathematical statistics.[57] It reveals how Pearsonian approaches *critiqued*, *quantified*, and *automated* racial typology and suggests that this had long-lasting consequences. This is also the first study that exposes the disputes between biometricians and anthropologists instead of their well-known conflicts with supporters of Mendel's gene theory. The conclusion discusses how biometric methods and data led important afterlives in postwar physical anthropology and human evolutionary studies, rather than succumbing to the "good" cultural anthropology and population genetics. By moving beyond a teleological framework that stresses the failure of biometry and the retreat of race in science, we start seeing longer trajectories and more subtle developments of racial quantification's history.

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[55] Considering this dissertation's focus on the biometric study of races and skulls, I have limited my research to the work produced in the Biometric Lab and published in *Biometrika*. Occasionally, craniometric research was published in Pearson's journal *Annals of Eugenics*, but my observation is, not unlike Magnello's, that most craniometric research was published in *Biometrika*. Magnello explains that *Annals* mostly reprinted lengthy public lectures. On the contrary, a more specialized audience read *Biometrika.*

[56] Barkan, *The Retreat of Scientific Racism,* 161.

[57] Theodore M. Porter, *The Rise of Statistical Thinking,* xi.

Scales of Research: The Universal, Transnational, Colonial, and the Local

This project builds on a longstanding interest in the colonial context of the sciences and postcolonial theory. In the past decades, postcolonial historians have challenged the long-assumed centrality and universality of western knowledge and the dichotomy between European knowledge centers and non-western peripheries. Instead, postcolonial histories stress the importance of non-western actors, cross-cultural encounters, and local knowledge in the production of western knowledge. By placing the view outside the west, they reveal that these "peripheries" were major research sites.[58] A more recent "global turn" in the history of science has further dissolved the west-rest dichotomy by scrutinizing the notion of *travel* in science, such as the travel of scientists, knowledge, and scientific materials. These studies position local encounters within the globalized world of scientific exploration, colonial expansion, and worldwide trade. A focus on travel requires a wider frame of analysis, one that connects multiple localities. Following the movement of data, brains, instruments, scientists, and "go-betweens" across the globe uncovers the various obstacles encountered along the way. Sometimes travel was smooth, other times it was denied, forced, or challenged.[59] Oftentimes travel relied on colonial networks. Some historians have adopted a transnational approach to show the interplay of science, politics, and diplomacy in movement. They track the movement of scientists

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58 Gayatri Chakravorty Spivak and Sarah Harasym, *The Post-Colonial Critic: Interviews, Strategies, Dialogues* (London: Routledge 1990); George W. Stocking (ed.), *Colonial Situations: Essays on the Contextualization of Ethnographic Knowledge* (Wisconsin: University of Wisconsin Press 1991); Frederick Cooper and Ann L. Stoler, *Tensions of Empire: Colonial Cultures in a Bourgeois World* (Berkeley: University of California Press 1997); Dipesh Chakrabarty, *Provincializing Europe: Postcolonial Thought and Historical Difference* (Princeton: Princeton University Press 2000); Mary Louise Pratt, *Imperial Eyes: Travel Writing and Transculturation* (New York: Routledge 2008); Ricardo Roque, *Headhunting and Colonialism: Anthropology and the Circulation of Human Skulls in the Portuguese Empire, 1870-1930* (Basingstoke: Palgrave Macmillan 2010); Ricardo Roque and Kim Wagner (eds.), *Engaging Colonial Knowledge: Reading European Archives in World History* (London: Palgrave Macmillan 2012).

59 Warwick Anderson, "Introduction: Postcolonial Technoscience," *Social Studies of Science* 32:5/6 (2002) 643–58; Kapil Raj, *Relocating Modern Science: Circulation and the Construction of Knowledge in South Asia and Europe, 1650-1900* (Basingstoke: Palgrave Macmillan Basingstoke 2007); Harold John Cook, *Matters of Exchange: Commerce, Medicine, and Science in the Dutch Golden Age* (New Haven: Yale University Press New Haven 2007); Simon Schaffer, Lissa Roberts, Kapil Raj, and James Delbourgo (eds.), *The Brokered World: Go-Betweens and Global Intelligence, 1770-1820* (Sagamore Beach: Science History Publications 2009); Sujit Sivasundaram, 'Introduction. Focus: Global Histories of Science,' *Isis* 101:1 (March 2010) 95-97; Fa-ti Fan. "The Global turn in the History of Science." *East Asian Science, Technology and Society: An International Journal* 6, no. 2 (2012): 249-258.

and knowledge in and between the regulatory state and international networks.[60] These non-western, global, and transnational scales of research thus bring into view important actors and places that have been previously ignored in the history of science, without flattening out the power dynamics involved in the travel of science.

Despite these developments, the history of anthropology has often been written from a national perspective. Like other disciplinary histories, historians have tracked the development of physical and cultural anthropology through the founding actors, the first disciplinary institutions and chairs at universities, and membership in disciplinary societies. Some of these disciplinary histories move beyond national frameworks by tracing the travels of anthropology's "founding fathers" to other countries or anthropology's reliance on the (former) colonies of the nation.[61] Lyn Schumaker and Johannes Fabian have offered particularly revealing accounts of the various ways in which anthropological knowledge was developed in Europe's colonies in Africa.[62]

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[60] John Krige (ed.) *How Knowledge Moves: Writing the Transnational History of Science and Technology* (Chicago: University of Chicago Press 2019).

[61] The understanding of the relationship between anthropology and colonialism has changed in the past 40 years. In the 1970s, the issue of anthropology's colonial origins struck at the discipline's core in the wake of decolonization. A boundary was erected between "pure" and "applied" anthropology, thus separating the development of ideas about race from the practices of measurement and fieldwork. By the 1980s and 1990s, these boundaries were dissolved when historians began to argue that anthropology was a "handmaiden of colonialism." See: Herbert Lewis, "Was Anthropology the Child, the Tool, or the Handmaiden of Colonialism? in: idem, *In Defense of Anthropology. An Investigation of the Critique of Anthropology* (New Brunswick: Transaction Publishers 2014). Anthropological knowledge provided colonial administrators with detailed ethnographic knowledge about the peoples they sought to subject to their rule. It gave colonial powers the scientific justification of racial superiority and domination. More recently, scholars have complicated this account by stressing that anthropological knowledge was often too esoteric for governmental use and that other "knowers" on the ground, such as merchants, missionaries, and administrators, produced more practical knowledge. Moreover, the circumstances were often too messy to obtain bones, measurements, and information. Within this newer historiography, a fruitful debate has formed on the variety and exact nature of the relationship between anthropology and colonialism in practice. See: Talal Asad (ed.), *Anthropology and the Colonial Encounter* (Itacha: Itacha Press 1973) 9-19; George W Stocking (ed.), *Colonial Situations: Essays on the Contextualization of Ethnographic Knowledge* (Wisconsin: University of Wisconsin Press 1991); P.J. Pels and Oscar Salemink. "Introduction: Locating the Colonial Subjects of Anthropology," in: idem, *Colonial Subjects. Essays on the Practical History of Anthropology* (Ann Arbor: University of Michigan Press 1999); Henrika Kuklick (ed.), *New History of Anthropology* (Malden, MA: Blackwell 2008).

[62] Johannes Fabian, *Out of Our Minds: Reason and Madness in the Exploration of Central Africa* (Berkeley: University of California Press 2000); Lyn Schumaker, *Africanizing Anthropology: Fieldwork, Networks, and the Making of Cultural Knowledge in Central Africa* (Durham: Duke University Press 2001). See also: Emmanuelle Sibeud, "A Useless Colonial Science?: Practicing Anthropology in the French Colonial Empire, Circa 1880–1960," *Current Anthropology* 53:S5 (2012) S83–94;

Anthropology and the scientific study of race requires an analytical framework that moves beyond national boundaries and connects multiple localities. Prior research of racial measurements taken during a Dutch scientific expedition to New Guinea in 1902 signaled not only the importance of non-western actors in the production of racial data; it also revealed that racial data transformed when it moved between several localities, from measurements, to statistical constants, to racial theories.[63] Building on this research, *The Lives and Afterlives of Skulls* analyzes the travel of racial data and technologies on various scales. It connects Pearson's laboratory to a wider anthropological community in England, the United States, France, Germany, and the Netherlands. It tracks the reuse of racial data between these communities through the shipping of measurement cards across the Atlantic and Pacific and the travel of books and article offprints with printed measurements. Like other sciences that relied on data-sharing,[64] this project reveals the desired global scope of racial research and explores how scientists relied on data produced on populations around the globe for their racial comparisons and classifications. It shows the problems with the international use and reuse of this data in light of a lack of standardization of measurement techniques. It connects these scales of research to a desired "universality" of scientific research and racial researchers' ambitions to produce objective knowledge that transcended knowledge producing practices. Finally, where possible, I uncover the practices of non-scientific actors in order to demonstrate that the collection of skulls and measurements of living people relied on the labor of various actors outside the laboratory.

Ross L. Jones and Warwick Anderson, "Wandering Anatomists and Itinerant Anthropologists: The Antipodean Sciences of Race in Britain Between the Wars," *The British Journal for the History of Science* 48:1 (2015) 1–16.

[63] Iris Clever and Willemijn Ruberg, "Beyond Cultural History? The Material Turn, Praxiography, and Body History," *Humanities* 3:4 (2014) 546–66.

[64] Sabina Leonelli and Niccolo Tempini (eds.) *Data Journeys in the Sciences* (Springer 2020).

Organization of chapters

The first chapter discusses how physical anthropology developed out of anatomy and natural history and created unique methods to measure skulls and classify races. Through the study of key anatomical and anthropological textbooks, the chapter distills two data practices: the morphological and metrical method. With morphology, the visual and manual observation of the skull, anthropologists studied the skull as a whole and ordered them in racial series and "ideal types." Systems of measurement were designed to make these visual observations more precise. As skeletal collections grew, anthropologists increasingly used measurements to reveal racial links. Despite this growing use of measurements, they continued to value the morphological method. Racial classifications continued to be the result of the anthropologist's morphological expertise and measurements. The chapter ends with a discussion of methodological problems arising from this approach.

The second chapter introduces British statistician Karl Pearson and his Biometric Laboratory at University College London. From 1902, the lab amassed large collections of skulls and developed a durable and consistent approach to reducing morphological race to statistical data. This biometric "scheme" studied race through statistical formulas, means, standard deviations, probable errors, and correlations. This approach was a major challenge to the traditional anthropological methods discussed in Chapter 1 which Pearson and his colleagues considered deeply inadequate. They argued that novel insights into man's racial history should depend on strong statistical methods, not the researcher's expertise. Furthermore, their research debunked common anthropological assumptions such as the existence of pure races. "Pearsonian anthropology" was an attempt to improve racial research, not overthrow it. Biometricians incorporated new statistical methods and insights into the existing large-scale project of mapping human evolution and racial classification.

Chapter 3 examines how formulas and instruments were supposed to replace the researcher's wisdom, paying special attention to how biometricians visualized racial data. With devices such as the Coefficient of Racial Likeness and cranial coordinatograph, Pearson transformed anthropology's typology into a quantified typology that heavily relied on geometry and statistics. While these disembodied methods and instruments were designed to make racial research less subjective, they continued to reflect the choices and data work of the researcher. Furthermore, these devices transformed race in important ways but also continued to generate traditional racial schemes and assumptions. New methods and understandings of race thus remained within the realm of the traditional racial research.

Chapter 4 provides an example of the unwavering trust in the neutrality of biometry through the case study of biometrician Geoffrey Morant. The chapter discloses newly discovered archival material and moves Morant center stage in the historiography of race science as one of the few racial researchers who publicly discredited racism but continued to embrace race. In the 1920s and 30s, Morant was the lab's expert on race and biometry. From the mid-1930s, he began to publicly denounce Nazi racial theories using biometric methods and arguments. These efforts culminated in the publication of *The Races of Central Europe: a Footnote to History* (1939). This 159-page "protest" showed how physical characteristics were gradually distributed across Europe and argued that European races, especially pure Aryan ones, did not exist. Morant even admitted that racial classifications were arbitrary. But his ideas about race, biometry, and data never made him question or reject the existence of global racial differences. Instead, he blamed the misuse or ignorance of anthropological facts for the racism of his day. He believed Pearson's biometric methods were a safeguard to producing "truer" anthropological evidence that could dispel dangerous racial fallacies and bring world peace.

Chapter 5 continues the story of the standardization of anthropological measurements in the interwar period. Race scientists needed comparable, universal data, not subjective observations, in order to classify global human variation. The lack of standardization was especially problematic for biometricians who reused published measurements to amass data for statistical analysis. From the late 1920s, Pearson's former student, biometrician Miriam Tildesley, attempted to establish an international standardization committee. While the need for standardization brought together racial researchers across disciplinary and national boundaries, the hurdles this committee faced demonstrate how racial data practices continued to be subjective and resistant to automation.

In the conclusion, I briefly examine how the computer transformed the biometric study of race and skulls in the 1950s and 1960s. In the aftermath of Nazi racial policy, researchers claimed to discard prewar racial research but continued to use skulls, race, and biometry in evolutionary biology, now aided by the electronic computer. The conclusion also bridges the historical development of the biometric study of race and skulls to the current developments in physical anthropology and artificial intelligence and the problems of racial bias that have surfaced.

Chapter 1

Anthropology's Data Practices.

Balancing the Subjective and the Objective

Introduction

"The importance of the cerebral cavity in man, and its influence on the external configuration of the skull, early engaged the attention of anthropologists, with a view to determine its capacity," French anthropologist Paul Topinard wrote in one of the first anthropological textbooks, *L'Anthropologie* (1876). His teacher Paul Broca, generally considered the founder of physical anthropology, had transformed the method of measuring the cavity into "a mathematical operation upon which we can now depend." "The following is his mode of proceeding, to the minutest details." Topinard continued:

> The orbit being filled with cotton wool and the vault of the cranium placed in a wooden bowl, the first litre of shot is poured into its cavity; then, the skull being grasped with both hands, is shaking so as to allow the shot to pass into the anterior part of the cavity. It is then turned about, and at the same time a wooden spindle is used to ram down the shot, until the cavity can hold no more. Then pressing hard with the thumb, the shot is rammed in until it is on a level with the occipital foramen. The contents are then emptied into a vessel, and from this turned quickly into a tin litre, the surface of which is leveled with a flat rule. The remainder is passed into a glass gauge, graduated in cubic

> centimetres, through a funnel, the neck of which is fixed in a wooden disc fitted to the gauge like a cover.

By meticulously following these instructions, one could calculate the skull's volume, also known as cranial capacity, of a series of skulls and measure the racial differences between them. "The inferior races have a less capacity than the superior," Topinard observed, with capacity attaining "a maximum in whites" and a minimum in the Australian peoples. For anthropologists, cranial size corresponded to intellectual endowment: the more the better. Therefore, this measurement offered solid proof for the difference in intelligence between the races of man, the French anthropologist concluded.[1]

Measurements such as cranial capacity came to define a new discipline that had begun to enter the university by the late 19th century: physical anthropology, generally known as the natural history of man.[2] Its central focus was not only the link between man and other animals but also the relationship between man's various races, which researchers often understood in the hierarchical manner exemplified above. The roots of the study of human variation reach far back into the past, but the study of race in particular took flight in the 18th century.[3] Our story begins around the mid-19th century, when the new discipline physical anthropology began to take shape and the practice of measuring skulls, bones, and bodies evolved. The study of race, *racial science*, brought together researchers from various fields of study: anatomy, medicine, zoology, the newly self-identified physical anthropologists, and, as subsequent chapters will discuss, mathematicians, biometricians, and eugenicists.

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[1] Paul Topinard (translated by R. Bartley), *Anthropology* (London: Chapman and Hall 1878) 226-30.

[2] K. C. Vogt (translated by James Hunt), *Lectures on Man: His Place in Creation, and in the History of the Earth* (London: Longman, Green, Longman, and Roberts 1864) 6.

[3] J. Smith, *Nature, Human Nature, and Human Difference: Race in Early Modern Philosophy* (Princeton: Princeton University Press 2017); S. Stuurman, *The Invention of Humanity: Equality and Cultural Difference in World History* (Cambridge, MA: Harvard University Press 2017); Devin J. Vartija, "The Colour of Equality: Racial Classification and Natural Equality in Enlightenment Encyclopaedias" (Ph.D. diss., Utrecht University, 2018).

How did racial researchers understand human variation in the late 19th and early 20th centuries? Which methods and practices did they use in the formulation of racial theories and classifications? In the 1950s and 60s, physical anthropologists judged colleagues in preceding decades to narrow-mindedly focus on simple methods of measurement, description, and racial typology.[4] This chapter places these metrical, qualitative, and classificatory methods under the microscope and explores how they came about and how they developed. I show that these methods were more complex than postwar anthropologists judged: they formed a unique approach that combined skilled vision, geometry, and abstraction of human variation. Observation and measurement were data practices that together reduced humans to abstracted information on paper. How these practices transformed and persisted throughout the 20th century will be explored in subsequent chapters.

In examining the notions, theories, and methods of racial science, we do not find a monolithic scientific landscape: racial researchers hotly debated the origin and classification of races. Their various backgrounds created an interdisciplinary research field where multiple approaches and methods coalesced. The chapter shows that, on the one hand, racial researchers increasingly relied on measurements for the determination of racial origins and relations in the second half of the 19th century. At the same time, they began to question racial science's methodology towards the end of the 19th century.

Textbooks form the main source for this chapter: they offer insight into racial science's practices and theories and their development over time. What is more, textbooks *shaped* the budding discipline of physical anthropology by putting in print a "standard" approach to studying human

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[4] Stanley M. Garn, "The Newer Physical Anthropology," *American Anthropologist* 64:5 (1962) 917–18. See also the conclusion of this dissertation.

variation and by circulating it among students, laboratory researchers, and explorers.[5] The chapter draws from a wide range of anthropological and anatomical textbooks from various national contexts from the 1860s to the 1920s. As chapter 5 will reveal, what constituted a "standard" methodology became a topic of international debate from the 1880s onwards because research approaches differed between disciplines, research institutes, and countries. Textbooks played an important role in standardization efforts as they often reflected the writer's ambition to record locally developed theories and methods as well as standardize these approaches within disciplines, countries, and sometimes even between countries. Textbooks are therefore particularly useful for our purposes here: they reveal the differences, tensions, and commonalities in methods and theories of studying race.

First, the chapter discusses how and why researchers increasingly turned to bones and measurements in the study of race. It then explores how notions of typology, abstraction, and biological determination underlay the project of racial classification. The third part digs into the epistemology of observation, measurement, and typology and the sense of crisis that came to be felt about these data practices around 1900.

Interdisciplinarity and disciplinarization

The origins of physical anthropology's methodology lie in natural history and comparative anatomy. 18th century naturalists were primarily concerned with *classification*, with understanding humans as part of the natural world as species to be classified among other organisms. The world they desired to order radically expanded with the ongoing conquests and discoveries made in the New World and other spaces of European colonization. European travelers sent back an increasing number of exotic plants and animals that scientists at home compared and classified as a way to make sense of the

[5] Marga Vicedo, "Introduction: The Secret Lives of Textbooks," *Isis* 103:1 (2012) 83–87.

organism and its place in nature's organization.[6] The native peoples travelers encountered and subjugated in faraway places were rapidly incorporated into these scientific schemes. Travelers were struck by their difference in appearance and behavior, most prominently their hair and skin color. From the 18th century, scientists began to explore human variation through the systematic study of skulls, bones, and other bodily and cultural objects that were sent back to Europe for study.[7] By the late 18th century, "cabinets were filled with human skulls and pottery shards, and notebooks were crammed with measurements and descriptions of facial angles, cranial diameters, and shades of skin color."[8]

In classifying human races, 18th– and 19th – century scientists used the same methods for studying the relationships between plans and animals and also desired to arrange humans in "chests, drawers, and pigeon-holes."[9] First, the researcher examined and described each group, its divisions, and its differences from allied groups. Then, "by synthesis," he determined the race's position in the classification of organisms by establishing the variety, species, genus, order, and class to which it belonged, Topinard explained.[10] This taxonomy was rooted in Carl Linnaeus's classification system as introduced in his first edition of *Systema naturae* (1735). Linnaeus had controversially departed from Scripture by including humans in his taxonomy of organisms and shelving them under the class of *mammalia* and species *Homo Sapiens*.[11] In the first edition, Linnaeus merely mentioned four

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[6] Vartija, "The Colour of Equality," 47, 55; Bruno J Strasser, "The Experimenter's Museum: Genbank, Natural History, and the Moral Economies of Biomedicine," *Isis* 102:1 (2011) 64; Staffan Müller-Wille, "History Redoubled: The Synthesis of Facts in Linnaean Natural History," in: C. Zittel, G. Engel, R. Nanni, and N. Karafyllis (eds.), *Philosophies of Technology: Francis Bacon and His Contemporaries* (Leiden: Brill 2008) 515-38.

[7] Nancy Stepan, *The Idea of Race in Science: Great Britain, 1800-1960* (Hamden, Conn: Archon Books 1982) ix.

[8] E. Williams, *The Physical and the Moral: Anthropology, Physiology, and Philosophical Medicine in France, 1750-1850* (Cambridge, UK: Cambridge University Press 1994) 10.

[9] Vogt, *Lectures on Man*, 214.

[10] Topinard, *Anthropology,* 2-3.

[11] Vartija, "The Colour of Equality," 47.

races as subspecies (*Europaeus, Americanus, Asiaticus, Africanus*) – by the tenth edition, he added general descriptions of each type: the *Americanus* type was "red, choleric, erect, with thick black hair, and open nostrils." The *Europaeus* was "white, sanguine, muscular, with long flowing hair and blue eyes."[12] Whether human races were varieties of the same species or different species became a topic of debate in the 18th and 19th century.

The anthropological study of race was also shaped by anatomical practices, where the examination of the body played a central role. Because knowledge of the human body was foundational in comparing different races, physical anthropology and the study of race became an extension of anatomy.[13] It is therefore not surprising that many of the first physical anthropologists were trained anatomists and physicians, such as Samuel Morton, Paul Broca, Rudolf Virchow, and Aleš Hrdlička. During their medical training, these men had learned about morphology, the study of the factors that determined the shape, structure, and functions of the body. With comparative anatomy, they had studied how to search for homologies and differences in the build of different animal species and groups. These morphological and comparative practices revolved around careful observation and description.[14] American anthropologist Aleš Hrdlička stressed in his textbook *Anthropometry* that "the best and in fact the only sufficient *preparation* for scientific anthropometry, are the studies which lead to the degree of doctor of medicine." The racial researcher needed training in anatomy, physiology, and pathology – anyone without it will "remain a bird with a paralyzed wing."[15]

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[12] Carl Linnaeus, *Systema naturae* first edition (Leiden: 1735); Carl Linneaus, *Systema naturae* tenth edition (Leiden: 1758) 20-22.

[13] Ashley Montagu, "Physical Anthropology and Anatomy," *American Journal of Physical Anthropology* 28:3 (1941) 263.

[14] T.B. Johnston (ed.), *Gray's Anatomy. Descriptive and Applied. 27th Edition* (London: Longmans, Green and Co., 1938) xxxi; A. Robinson (ed.), *Cunningham's Text-Book of Anatomy. Fourth Edition* (New York: William Wood and Company 1913) 4.

[15] A. Hrdlička, "Anthropometry B: Introduction to Anthropometry," *American Journal of Physical Anthropology* 2:2 (1919) 180.

Race thus brought together researchers and methods from various fields of study. Broca emphasized the methodological diversity inherent to race and anthropology in his founding of the *Société d'Anthropologie* in 1859, which grouped around the study of the human races "the medical sciences, comparative anatomy, and zoology, prehistoric archaeology, palaeontology, linguistics, and history, and designated under the title Anthropology the science whose domain was thus largely extended."[16] By 1904, British anatomist Wynfrid Duckworth concluded that anthropology had taken on a "protean nature."[17]

Late 19th and early 20th century racial researchers determined that Broca's founding of the *Société d'Anthropologie* in Paris chimed the beginning of physical anthropology's disciplinarization.[18] While the word "anthropology" had appeared in German and French scholarly writing since the late 18th century, scientists only began calling themselves "anthropologists" from the second half of the 19th century. The first university chairs in anthropology appeared in the late 19th century.[19] Physical anthropology began to develop as a separate science and continued to build on the methods of natural history, comparative anatomy, and morphology.

Bones and Quantification

Racial researchers increasingly turned to cranial collections for their studies on race. Whereas 18th-century travelers, naturalists, and philosophers such as Linnaeus provided descriptions of the physical appearance of living people, the focus of scientific interest shifted considerably to deceased

[16] Paul Broca in Topinard, *Anthropology*, vi.

[17] Wynfrid Duckworth, *Morphology and Anthropology: A Handbook for Students* (Cambridge, UK: Cambridge University Press 1904) 10.

[18] Topinard, *Anthropology*, 17.

[19] These were often tied to the anatomy or medicine departments of universities, as was the case in the Netherlands and Norway. See Fenneke Sysling, *Racial Science and Human Diversity in Colonial Indonesia* (Singapore: NUS Press 2016); Jon Røyne Kyllingstad, *Measuring the Master Race: Physical Anthropology in Norway, 1890-1945* (Cambridge, UK: Open Book Publishers 2014).

remains in the 19th century. In comparison with living humans who could resist observation, bones and skulls were easier to obtain and ship home. Bones were also considered to be more "stable" than living bodies, meaning that the bony structure, uncovered from human flesh, revealed the body's unchanging essences. They allowed for more precise measurements than fleshy bodies.[20] Moreover, bones unlocked human's prehistoric racial past in ways that living populations could not. Topinard explained that bones "have the inestimable advantage of presenting to us all that remains of ancient peoples of which there are no longer any living representatives; some extending back to one or two thousand years, others to ten and twenty thousand, when the various types had become less changed."[21] Finally, researchers viewed the skull as the most important part of the body as it housed the organ of the mind, the brain, which they understood to play a central role in human development. German scientist Carl Vogt pointed out in his textbook *Lectures on Man* that the study of skulls uncovered the mental differences between "primitive" and "civilized" races because the brain's "chief features are impressed on the inner surface of the cranium."[22]

Colonial expansion and warfare continued to provide anthropologists with cranial collections in the late 19th and early 20th century. Primary sources often attest to grave robbing practices. Even though several Western countries shut down the trade in corpses for medical schools throughout the 19th century, body snatching and grave robbing practices were less controlled in the colonies and less frowned upon when it concerned the remains of "primitive" peoples.[23] In 1920, Georgetown anatomist Philip Newton reported about his procurement of cranial specimens from the "Negritos

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[20] Franz Boas, "Some Criticisms of Physical Anthropology," *American Anthropologist NS* 1:1 (1899) 98-99; Charles Davenport, *Guide to Physical Anthropometry and Anthroposcopy* (Cold Spring Harbor: Eugenics Research Association 1927) 10-11; Nancy Stepan, *The Idea of Race in Science: Great Britain, 1800-1960* (Hamdon, Conn: Archon Books 1982) 100.

[21] Topinard, *Anthropology*, 206.

[22] Vogt, *Lectures on Man,* 8-9. Throughout the 19th and early 20th centuries, racial researchers were also very much invested in the study of the brain, its weight, and its characteristics. While this dissertation alludes to this topic of study in a few places, it largely remains outside of its scope.

[23] See for the British, Scottish, and Australian case H. MacDonald, *Possessing the Dead: The Artful Science of Anatomy* (Carlton, Vic: Melbourne University Press 2010).

of the Philippine Islands:" "The negrito will not willingly part with the remains of his dead, and this part of the work was very difficult. In addition to the opposition on the part of the negritos, the graves themselves are widely separated and in the most out-of-the-way places. When found they must be opened at night to escape the watchfulness of the negritos."[24] German-born American anthropologist Franz Boas also robbed a grave when he traveled to British Columbia in the early 20th century: "It is the most unpleasant work to steal bones from a grave, but what is the use, someone has to do it…I hope to get a great deal of anthropometric material here."[25] Historian Ricardo Roque has revealed how anthropologists in Portugal retrieved skulls for scientific study through Portuguese colonizers and the organized violence they conducted in East Timor.[26]

The turn to skulls and bones was accompanied by an increasing use of measurement systems. In the 19th century, more and more researchers expressed racial differences in numbers. While it remains unclear exactly why skulls began to be viewed as measurable objects,[27] the primary sources offer some suggestions. Boas reflected about the past:

> Studies of the human skeleton had not been carried very far when it was found to be not quite easy to determine racial characteristics with sufficient accuracy by mere verbal description. This led to the

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[24] Philip Newton, "Observations on the Negritos of the Philippine Islands," *American Journal of Physical Anthropology* 3:1 (1920) 3.

[25] R. Rohner (translated by Hedy Parker), *The Ethnography of Franz Boas. Letters and Diaries of Franz Boas Written on the Northwest Coast from 1886 to 1931* (Chicago: University of Chicago Press 1969) 88.

[26] Ricardo Roque, *Headhunting and Colonialism: Anthropology and the Circulation of Human Skulls in the Portuguese Empire, 1870-1930* (Basingstoke: Palgrave Macmillan 2010). See also Fenneke Sysling, *Racial Science and Human Diversity in Colonial Indonesia* (Singapore: NUS Press 2016) 37.

[27] Dutch historian Siep Stuurman, expert on the intellectual history of equality and difference, posed this question in a private conversation with me. In exploring the conditions for data in natural history, Staffan Müller Wille has posited that "experiential" pressure was not at the basis of the quantification of 18th century natural history, but the experimental nature of natural history and "the instruments and infrastructures brought into play to manage and enhance flows of data" such as Linnaean names and taxa. This generated "unforeseen, and, indeed, never-before-seen phenomena that were difficult to reconcile with long-held intuitions." Müller-Wille's paper asks us to find answers for the turn to quantification inside the development of racial science and its main research object rather than pointing towards a "teleological necessity through the accumulation of data" and the problems that this accumulation caused, as Boas suggests here. The question what was fundamentally different about skulls and bones that brought about quantification remains unanswered. See Staffan Müller-Wille, "Names and Numbers: "data" in Classical Natural History, 1758–1859," *Osiris* 32:1 (2017) 109–28.

> introduction of measurements as a substitute for verbal description. With the increase of the material, the necessity of accurate description became more and more apparent, because intermediate links between existing forms were found with increasing frequency.[28]

Thus, the need to express smaller racial differences required the use of numbers. Historian of anthropology George W. Stocking underwrites this explanation: "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."[29] Anthropologists could easily separate white from non-white peoples but required measurements of stature and head-form to differentiate between European races. Indeed, data played a central role in the determination of presumed racial differences between closely allied European types, for instance in American racial theorist William Ripley's textbook *The Races of Europe*.[30]

Measurements gave racial science a level of accuracy and precision that was considered impossible through visual observation and verbal description. Unlike observations of living people in foreign countries, which were often chaotic and riddled by European anxieties and frustrations,[31] skulls could be measured at home, in laboratories, where "everything is done carefully and methodically…conducted with calmness, and every available source of information is brought into requisition," Topinard remarked. Here, one could take "perfect measurements" that were reliable to within a millimeter.[32] Anthropologists further viewed the transition from descriptive observations to quantitative data as part of the professionalization of anthropology and its development as a serious

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[28] Boas, "Some Criticisms of Physical Anthropology, 99.

[29] George W. Stocking, *Race, Culture, and Evolution: Essays in the History of Anthropology* (Chicago: University of Chicago Press 1968) 57.

[30] George W. Stocking, *Race, Culture, and Evolution*, 68.

[31] See Johannes Fabian, *Out of Our Minds: Reason and Madness in the Exploration of Central Africa* (Berkeley: University of California Press 2000).

[32] Topinard, *Anthropology*, 82; 204; Charles Davenport, *Guide to Physical Anthropometry and Anthroposcopy*, 11.

and rigorous science.[33] But, as will be discussed below, quantification did not replace observation and description; both were considered essential to racial classification.

As Boas remarked, by the turn of the 20th century, the growing collection of bones and skulls "have led to a most extensive application of the metric method in the study of the human skeleton and also in the study of the external form of the living."[34] Racial researchers developed measurement systems for skulls and bones (craniometry and osteometry) as well as living people (anthropometry). Moreover, they developed specialized instruments. The spreading caliper (fig. 1.1), designed to measure heads and skulls, was first created by Broca in the late 19th century. For this instrument, he repurposed the pelvimeter used by midwives. In collaboration with surgical instrument maker Lucien Mathieu, Broca altered the pelvimeter's design by changing its shape, thinning the branches, and placing buttons on the tips. This cut the weight in half and produced an instrument that could be easily slid into a jacket pocket and carried along scientific voyages.[35]

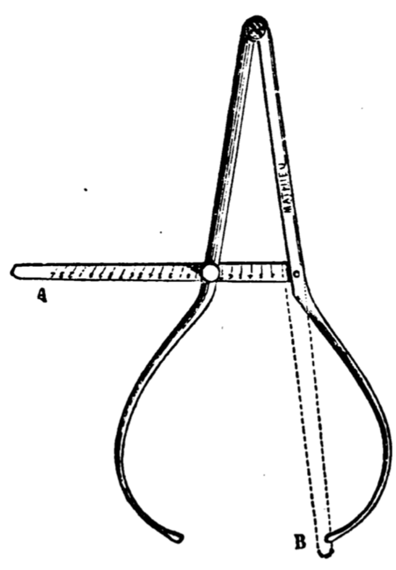

Figure 1.1. The spreading caliper. Source: P. Broca, *Instructions générales pour les recherches anthropologiques. A faire sur le vivant* (Paris: G. Masson, 1879 [2nd edition]) 35. Source is in the public domain.

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[33] Topinard, *Anthropology*, 224.

[34] Boas, "Some Criticisms of Physical Anthropology," 99.

[35] P. Broca, *Instructions générales pour les recherches anthropologiques. A faire sur le vivant* (Paris: G. Masson 1879) 35-38; L. Mathieu, *Catalogue des instruments anthropologiques* (Paris) editions from 1862, 1864, 1867, and 1873.

The caliper consisted of two steel legs ending in pear-shaped knobs. The left leg carried a ruler, inscribed in centimeters, which could slide back and forth when positioned in the right leg socket. In Broca's method, the anthropologist would pull the legs apart, place the tips on the head, and balance the legs on the fingers, while the other hand held the caliper. Instead of observing from afar, measuring with the caliper required a more intimate encounter, the researcher touching the skull or the head of a living person (fig. 1.2). Properly wielding the caliper required precision and training in a certain *habitude*, Broca cautioned: new researchers should practice measuring the same locations on the head over and over again until the results were similar up to a millimeter. They should pay attention to finding the anatomical landmarks, to preventing the legs from slipping on the skin, and to memorizing the measurements read from the scale. Wielding the caliper, then, was a controlled, coordinated, and mechanized performance.[36]

Figure 1.2. The encounter between the caliper, the researcher, and the research subject. Source: A. Hrdlička, *Anthropometry* (Philadelphia: Wistar Institute of Anatomy and Biology 1920) 70. Source is in the public domain.

[36] P. Broca, *Instructions générales,* 35-38. This controlled performance was echoed by Aleš Hrdlička, *Anthropometry* (Philadelpha: Wistar Institute of Anatomy and Biology 1920) 41.

The caliper was an essential technology for the quantification of racial differences: it turned people into bodies to be measured. What is more, the instrument *transformed* the messy encounters in the field into ordered measurements and systematic analytical data. This portable little instrument allowed the anthropologist to produce quantitative data written on anthropometric forms which he could take home while leaving the subjects behind. Even though it vied with other instruments, such as the Vernier sliding caliper and the anthropometer, instruments designed for linear measurements, the spreading caliper was particularly indispensable because it measured skulls and heads in laboratories and the field. Some researchers made the instrument more portable and practical: British anatomist William Flower combined the sliding and spreading caliper into one instrument;[37] German anthropologist Rudolf Virchow added extra pivots to the legs, making it more compact.[38] Aleš Hrdlička and Swiss-German anthropologist Rudolf Martin developed calipers with wider openings to measure bigger heads and skulls.[39] German anthropologists developed heavier, more precise calipers for laboratory measurements.[40] Apart from calibration to the metric system, the design of anthropological instruments such as the caliper was not standardized. Nevertheless, many researchers purchased the same instruments from instrument makers Lucien Mathieu in Paris and P. Hermann in Zürich, the leading international distributors in the late 19th century. Like data, anthropological instruments moved across the globe. This international flow of instruments was cut

[37] William Flower, *Catalogue of the Specimens Illustrating the Osteology and Dentition of Vertebrated Animals, Recent and Extinct, Contained in the Museum of the Royal College of Surgeons of England* (London: Taylor and Francis 1879) xv-xvi.

[38] Emil Schmidt, *Anthropologische Methoden: Anleitung Zum Beobachten Und Sammeln Für Laboratorium Und Reise* (Leipzig: Verlag von Veit & Comp. 1888) 63.

[39] Rudolf Martin, "Anthropometrisches Instrumentarium," *Correspondenz-Blatt der deutschen Gesellschaft für Anthropologie, Ethnologie, und Urgeschichte* 30 (1899) 131-132; Paul Huston Stevenson, "Adaptation of Hrdlička's Compass for Direct Head-height Measurements," *American Journal of Physical Anthropology* 13:3 (1929) 469–75.

[40] John Carson, "Craniometer," in: R. Bud and D. Jean Warner, *Instruments of Science: an Historical Encyclopedia* (New York: Garland Publishing 1998) 157-158.

short during World War I, however, leaving some anthropologists with the task of finding manufacturers at home willing to produce instruments for a small market of racial researchers.[41]

The measurement of bones and bodies also came to serve more practical purposes in the 19th and 20th centuries. Craniometry became one of the central methods of the new field of forensic anthropology. Indeed, from the early 20th century onwards, physical anthropologists were increasingly asked to assist with the identification of skeletal remains in criminal cases. Aleš Hrdlička, for instance, assisted with several FBI cases during his career.[42] The quantification of the living body also became dominant in branches related to physical anthropology, such as military and criminal anthropology. Military doctors used these body measurements to measure recruits and determine their fitness for combat.[43] In prisons, doctors not only measured prisoners' bodies to identify them, but also to examine their supposed degree of degeneration. Most famous in this respect is Bertillonage, the criminal measurement system developed by French criminologist Alphonse Bertillon, and the work of Italian criminologist Cesare Lombroso, who attempted to identify the physical origins of crime.[44] Physical anthropologists also linked up with these efforts: American

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41 A case in point was Aleš Hrdlička, who undertook several efforts to find a manufacturer in the United States to produce anthropometric instruments, but without success. He established an import business with Czech colleague Jindrich Matiegka in Prague in the 1920s and 1930s and sold the Czech instruments to American colleagues who were eager to purchase these items. Aleš Hrdlička Archive, Box 35, Folder "Manufacture of Instruments." See also Lucile E. Hoyme, "Physical Anthropology and Its Instruments: An Historical Study," *Southwestern Journal of Anthropology* 9:4 (1953) 408–30.

42 Douglas Uberlaker, "A History of Forensic Anthropology," *American Journal of Physical Anthropology* 165:4 (2018) 915-923.

43 Heinrich Hartmann, *The Body Populace: Military Statistics and Demography in Europe before the First World War* (Cambridge, MA: MIT Press 2019).

44 Alphonse Bertillon, *Identification Anthropométrique: Instructions Signalétiques* (Melun: Typographie-Lithographie Administrative 1885); Cesare Lombroso, *L'uomo delinquente: Studiato in rapport alla antropologia, alla medicina legale ed alle discipline carceraria* (Milan: Hoepli 1876); Amos Morris-Reich, *Race and Photography: Racial Photography as Scientific Evidence, 1876-1980* (Chicago: University of Chicago Press 2016); Angelo Matteo Caglioti, "Race, Statistics and Italian Eugenics: Alfredo Niceforo's Trajectory from Lombroso to Fascism (1876-1960)," *European History Quarterly* 47:3 (2017) 461-489.

anthropologist Earnest Hooton's *The American Criminal* (1939), for instance, attempted to determine whether criminal behavior was linked to physical and racial factors.[45]

These practical measurements were similar in type to the practices of physical anthropologists and were produced with instruments like the caliper. Moreover, like racial research, these projects were often concerned with the relationship between race, civilization, and the body. But where the measurement of bones and bodies in forensic, military, and criminal contexts largely centered on learning more about the individual, racial measurements were designed to look beyond the individual and explore the characteristics of a racial population.

Typology and Biological Determinism

Like other sciences that centered on finding patterns among varied phenomena and revealing the order of nature,[46] racial science's principal method relied on the *abstraction* of human variation by forming racial "types." The ontological status of this type was a complex matter. Historian Nancy Stepan tells us: "To the typologist, every individual human being belonged in some way or another to an undying essence or type. However disguised or hidden the individual's membership in the type might be, the scientist expected to be able to see behind the individual to the type to which he belonged." The constructed racial type, then, was an abstraction or an idealization, obscured by racial mixture, selection, and migration. Stocking writes that "to *recreate* these types out of the heterogeneity of modern mixed populations was the tremendously difficult task of the physical anthropologist. But once accomplished, it produced only an imaginary entity," a "fictive individual who embodied all the characteristics of the 'pure type'…obliterating the individual variation of his

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[45] Earnest Hooton, *The American Criminal: An Anthropological Study* (Cambridge, MA: Harvard University Press 1939).

[46] See L. Daston and E. Lunbeck, *Histories of Scientific Observation* (Chicago: University of Chicago Press 2011).

fellows, until he stood forth for them all as the living expression of the lost, but now recaptured, essence of racial purity."[47]

Topinard was well aware of the current-day "confusedness" of races: "Races have been divided, dispersed, intermixed, crossed in various proportions and in all directions, for thousands of years…We find ourselves no longer in the presence of races, but of peoples, the origins of which we have to trace or to make a direct classification of." "Ancient races" were thus the subject of anthropology. "There are no longer any pure races," he concluded.[48] Nevertheless, he considered it possible to determine pure races, types, or ideals, conflating the terms throughout his textbook:

> By *human type* must be understood the average of characters which a human race supposed to be pure presents. In homogeneous races, if such there are, it is discovered by the simple inspection of individuals. In the generality of cases it must be segregated. It is then a physical ideal, to which the greater number of the individuals of the group more or less approach, but which is better marked in some than in others.[49]

Creating types, races, and classifications required a lengthy, laborious, and minute analytical process of finding traces of "autochthonous groundwork" or ancient characters through observation and measurement. It was the only way to deduce a "negro ideal," a "Mongol ideal," or a "white ideal."[50] Topinard recognized that this was an "artificial method" since nature had only formed individuals, not classes or unvarying species. Nevertheless, "classifications are valuable, and, indeed, indispensable. They assist study, bring together animated beings, generally in a natural way, and mark the measure of progress accomplished." They introduced order into the medley of individuals found

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[47] Stepan, *The Idea of Race in Science,* xvii-xviii; Stocking, *Race, Culture, and Evolution*, 58-9; 63.

[48] Topinard, *Anthropology*, 443-444.

[49] Topinard, *Anthropology*, 446-447.

[50] Topinard, *Anthropology*, 316; 447.

in the world. These classifications always remained provisional and arbitrary, as long as racial science was in development.[51]

Topinard was not the only anthropologist to realize that racial types were abstractions and idealizations or that current-day populations were not racially "pure." In his textbook on European races, American racial theorist William Ripley argued that "pure types must be exceedingly rare" because European peoples had crossed and absorbed one another throughout history. Nevertheless, "it matters not to us that never more than a small majority of any given population possesses even two physical characteristics in their proper association; that relatively few of these are able to add a third to the combination; and that almost no individuals show a perfect union of all traits under one head, so to speak, while contradictions and mixed types are everywhere present." Ripley "boiled down" three European races by determining the physical traits that had been common to the type throughout time: the "Teutonic," "Alpine," and "Mediterranean" races. He compared his method to the geologist's approach that "restored" the ideal mountain chain from what was left behind today: "The geologist is well aware that the uplifted folds as he depicts them never existed in completeness at any given time." Similarly, "far be it from us to assume that these three races of ours ever, in the history of mankind, existed in absolute purity or isolation from one another." Ripley's three racial types "exist for us nevertheless."[52]

Ripley included several "portrait types" of the European races in his textbook that were supposed to be illustrations of types, not pictures of individuals (fig. 1.3). "Eminent authorities in all parts of Europe" had loaned Ripley the photographs of living representatives of their country's racial type. But finding representatives was not always easy. Ripley had asked German anthropologist Otto Ammon for photographs of a "pure" Alpine type from the Black Forest. Ammon "had

[51] Topinard, *Anthropology*, 20-21.

[52] William Zebina Ripley, *The Races of Europe: A Sociological Study* (New York: D. Appleton 1899) 107-112; 600.

measured thousands of heads, and yet he answered that he really had not been able to find a perfect specimen in all details. All his round-headed men were either blond, or tall, or narrow-nosed, or something else that they ought not to be."[53] Rather than undermining the belief in racial typology, such experiences fortified the idea that racial mixing had confused the original types. "Faith in the reality of the type was deep," Stepan reminds us. This belief in fundamental, unchanging, and discrete physical and behavioral essences of primordial races made the project of racial classification truly essentialist.[54]

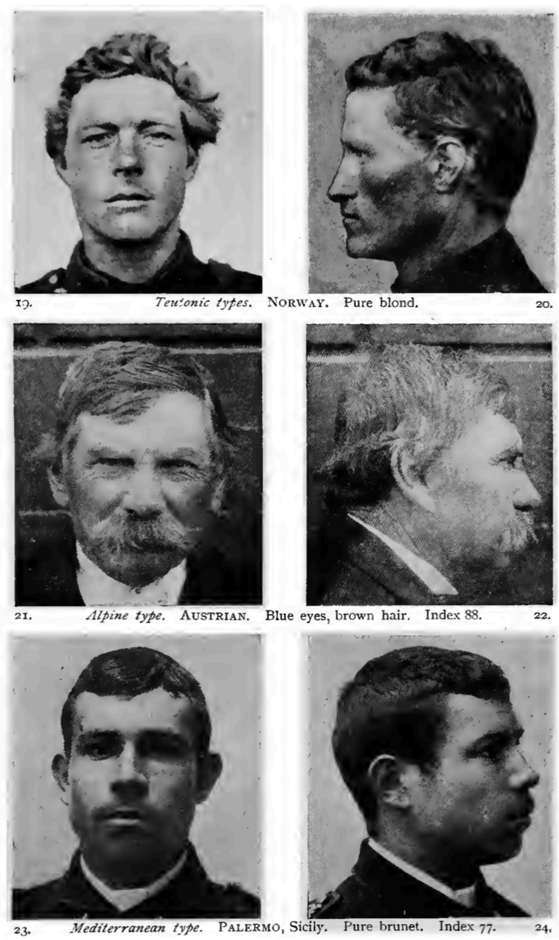

Figure 1.3. The three European racial types. Source: William Zebina Ripley, *The Races of Europe: A Sociological Study* (New York: D. Appleton 1899) 121. Source is in the public domain.

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53 Ripley, *The Races of Europe*, 108.

54 Stepan, *The Idea of Race in Science*, 94.

Biological determinism often accompanied essentialism in understanding racial differences, meaning that the physical and behavioral racial essences were intimately linked and mutually reinforcing.[55] Racial researchers commonly believed in the correlation between skull size, brain size, and intelligence or level of civilization. Besides the skull and the brain developing together and influencing each other,[56] researchers assumed that racial differences in head size explained racial differences in intelligence. Scottish anatomist Arthur Cunningham's *Textbook of Anatomy* proposed: "It may be generally assumed that the size of the skull in the more highly civilised races is much in excess of that displayed in lower types. The size of the head is intimately correlated with the development of the brain." It was therefore no surprise that gifted white men such as Cuvier, Schiller, and Napoleon had very large skulls "in proportion to their height."[57] As this chapter's opening demonstrated, researchers estimated the size of the skull, brain, and intelligence through cranial capacity. Vogt claimed that "Australians, Hottentots, and Polynesians, nations in the lowest state of barbarism" had the lowest capacity. "No one can deny that the place they occupy in relation to cranial capacity and cerebral weight corresponds with the degree of their intellectual capacity and civilisation." Broca measured and compared skulls from Parisian graves of the 12th and 19th centuries and argued that, through time, skull size increased along with an increase in civilization.[58]

Another measure deployed to indicate skull size and intelligence was the cephalic index, which measured the ratio between the length and the breadth of the skull, thus capturing both size and shape. English naturalist Charles Darwin wrote in the *Descent of Man* (1871): "one of the most

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[55] See for a more complex discussion of the historical relationship between essentialism and biological determinism in anthropology: Rachel Caspari, "From Types to Populations: A Century of Race, Physical Anthropology, and the American Anthropological Association," *American Anthropologist* 105:1 (2003) 65–76.

[56] Vogt, *Lectures on Man*, 79.

[57] Researchers generally realized that differences in stature, weight, and sex exerted influence on the size of the skull. Robinson (ed.) *Cunningham's Text-Book of Anatomy,* 284.

[58] Vogt, *Lectures on Man,* 91-92; Topinard, *Anthropology*, 229-30.

marked distinctions in different races of man is that the skull in some is elongated, and in others rounded." Swedish anatomist Anders Retzius introduced the ratio in 1840 along with a classification system: those with a short, round skull and a high ratio were "brachycephalic" and longer skulls with a low index "dolichocephalic." Topinard argued that the cephalic index was the most important cranial measurement and presented the "universally adopted nomenclature," adding several categories to Retzius' scheme (fig. 1.4).[59]

CEPHALIC INDICES.

Dolichocephali	75·00 and under
Sub-dolichocephali	75·01 to 77·77
Mesaticephali	77·78 „ 80·00
Sub-brachycephali	80·01 „ 83·33
Brachycephali	83·34 and above

Figure 1.4. Cephalic index classification scheme. Source: Paul Topinard (translated by R. Bartley), *Anthropology* (London: Chapman and Hall 1894) 238. Source is in the public domain.

This naming scheme enjoyed wide popularity and application. Various specialized measurement schemes adapted the cephalic index's categories. An article in the *American Journal of Physical Anthropology* in 1925, for instance, presented a racial scheme for the length of the large intestine and classified European groups "of dolichocolic type" and "brachycolic" type. The terms brachycephalic and dolichocephalic are still used in medicine today.[60]

While many considered the cephalic index to be one of the most important racial measures, some complained that researchers often relied on this measure alone, ignoring other important characteristics. British biometrician Karl Pearson, whom we will meet in the next chapter, bemoaned

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[59] Charles Darwin, *The Descent of Man, and Selection in Relation to Sex* I (London: John Murray 1871) 148; Topinard, *Anthropology*, 238-41. See for more on the cephalic index: Jon Røyne Kyllingstad, *Measuring the Master Race: Physical Anthropology in Norway, 1890-1945* (Cambridge, UK: Open Book Publishers 2014) especially chapter 1.

[60] A. Robinson (ed.) *Cunningham's Text-Book of Anatomy,* 284-9; Edward L. Miloslavich, "Contributions to Anthropological Splanchnology. I. Racial Studies on the Large Intestine," *American Journal of Physical Anthropology* 8:1 (1925) 11–22.

in 1924 that "a considerable number of anthropologists seem to believe that all racial problems can be solved by merely ascertaining the mean cephalic index within a given group or district."[61]

The linking up of physical and behavioral racial essences with levels of civilization had far-reaching consequences, well beyond anthropological discussions. In several cases, as Stephen Jay Gould shows, these deterministic conclusions about race provided the justification for practices of slavery, colonialism, and eugenics.[62]

Prehistory and the Origins of Races

Racial scientists in the 18th, 19th, and 20th centuries theorized and debated how racial differences came about. Many 18th century researchers followed the "monogenetic" creation story that all humans originated from Adam and Eve and that non-white races developed as a result of degeneration. They speculated, for instance, that black skin was the result of the biblical curse of Ham or Canaan. Scholars such as German physician Johannes Blumenbach also suggested that climate, environment, and "mode of life" created racial differentiation. Blumenbach further observed that races "shaded into each other", which to him proved the unity of mankind. He wrote in *De Generis Humani Varietate Nativa:*

> For although there seems to be so great a difference between widely separate nations, that you might easily take the inhabitants of the Cape of Good Hope, the Greenlanders, and the Circassians for so many different species of man, yet when the matter is thoroughly considered, you see that all do so run into one another, and that one variety of mankind does so sensibly pass into the other, that you cannot mark out the limits between them.[63]

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[61] Karl Pearson and L.H.C. Tippett, "On Stability of the Cephalic Indices Within the Race," *Biometrika* 16:1/2 (1924) 118; Louis R. Sullivan, *Essentials of Anthropometry. A Handbook for Explorers and Museum Collectors* (New York: American Museum of Natural History 1923) 12-13.

[62] See Stephen Jay Gould, *The Mismeasure of Man* (New York: W.W. Norton & Company 1996).

[63] J.F. Blumenbach (translated and edited by T. Bendyshe), *The Anthropological Treatises of Johann Friedrich Blumenbach* (London: Longman, Green, Longman, Roberts, & Green 1865) 98-99.

Other scientists abandoned Scripture and argued that races had distinct origins. Support for this "polygenist" theory increased with the colonization of peoples in the New World. The theory also lent itself as a justification for slavery. Polygenism remained a minority discourse in the 18th and 19th century due to its conflicts with biblical origin stories.[64]

Charles Darwin's theory of evolution revived the debate between monogenists and polygenists. In his 1859 *On the Origin of Species*, Darwin suggested a dramatic expansion of the world's timeframe which called into question both monogenist's creationism and polygenists' multiple origins. At the same time, Darwin's theory offered support to both sides of the debate: "the monogenists continued to construct linear hierarchies of races according to mental and moral worth; the polygenists now admitted a common ancestry in the prehistoric mists, but affirmed that races had been separate long enough to evolve major inherited differences in talent and intelligence," evolutionary biologist Stephen Jay Gould explains.[65] Both polygenists and monogenists assumed that racial traits were fixed a long time ago and were probably no longer under the influence of natural selection. Thus, race formation lay in the prehistoric past and had placed racial essences in the bodies of men and women today. Darwin himself used the same argument as Blumenbach to defend the common origin of species in *The Descent of Man*: "The most weighty of all the arguments against treating the races of man as distinct species, is that they graduate into each other, independently in many cases, as far as we can judge, of their having intercrossed." He also argued that the divergence of races took place "at an extremely remote epoch" when races already "differed but little from each other."[66]

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[64] Vartija, "The Colour of Equality," 56; 63; Terence Keel, *Divine Variations: How Christian Thought Became Racial Science* (Stanford: Stanford University Press 2018).

[65] Stephen Jay Gould, *The Mismeasure of Man* (New York: W.W. Norton & Company 1996) 105; Stepan, *The Idea of Race in Science*, 85.

[66] Charles Darwin, *The Descent of Man* I, 226; 229; George W. Stocking, *Race, Culture, and Evolution*, 46-48.

Racial science's typological approach with fixed, primordial essences could thus be married with Darwinian variation and the notion of a struggle between races was attractive to many budding anthropologists. But they questioned Darwin's theories of sexual and natural selection well into the 20th century. Several anthropologists instead turned to French naturalist Jean-Baptiste Lamarck's theory of species transformation to explain evolution. Lamarckism postulated that species gradually adapted to their environment and conditions of existence during life and passed these "acquired characteristics" onto their offspring. Researchers could observe such gradual adaptations during a man's lifetime – Darwin's selection mechanisms could not be detected. Stocking tells us that "Darwinism did not lay all the issues between monogenists and polygenists." In the aftermath of Darwin's publications "the various positions became in a sense free-floating, and people who were in general Lamarckian or explicitly monogenist might hold what were in fact quite polygenist ideas on a special racial issue such as miscegenation."[67]

Discussions about racial origins and their evolution created an increasing interest in the study of human prehistory. If man evolved from the ape and racial differentiation happened a long time ago, "are we able to point out the gradations which bridge over the gulf which still exists between the Negro and the ape, and follow them step by step from the anthropoid ape to the Negro, and from the Negro to the white man?" Vogt asked.[68] The 19th-century discovery of fossils and the development of archaeology, geology, and paleontology also considerably spurred research into human prehistory. The study of "early man" developed slowly as it took years to discover and

[67] Stepan, *The Idea of Race in Science*, 87; Stocking, *Race, Culture, and Evolution*, 47; 55. See also Topinard, *Anthropology*, 519-535.

[68] Vogt, *Lectures on Man*, 193.

describe human fossils. Researchers further disagreed on the continental origins of human ancestors.[69]

Hierarchy, Variation, and Temporality

The biblical creation story not only postulated the monogenetic origin of man, but also his special creation as the child of God. The attendant "Great Chain of Being" supposed a hierarchical scheme of the universe with the most perfect creatures at the top – God and then man – and other organisms occupying lower ranks according to decreasing perfection. Many scientists also subjected human variation to this hierarchical scheme: rather than understanding all humans as equally special, they ranked the white man above non-white men, thus infusing the doctrine with racial prejudices and notions of white superiority. The Great Chain of Being became "one of the most powerful doctrines governing theories of race in the eighteenth century," as historian Londa Schiebinger concludes.[70]

The findings of budding sciences such as paleontology and comparative anatomy popularized the doctrine again in the mid-19th century: fossils and skeletons suggested the progressive organization of organisms and thus offered proof for older hierarchical notions. Darwin's theory of evolution also lent itself to the notion: Darwin argued that species were linked through evolution like tree branches and improved with every new line of descent (fig. 1.5). Natural selection, he wrote in the conclusion of *The Origin of Species*, was the "progression towards perfection." In *The Descent of Man*, he proposed that "man bears in his bodily structure clear traces of

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[69] See Samuel J. Redman, *Bone Rooms: From Scientific Racism to Human Prehistory in Museums* (Cambridge, MA: Harvard University Press 2016) 228-229; Emily Kern, "Out of Asia: A Global History of the Scientific Search for the Origins of Humankind, 1800-1965" (Ph.D. diss., Princeton University, 2018); Ann Gibbons, *The First Human: the Race to Discover our Earliest Ancestors* (New York: Anchor Books 2007).

[70] Londa L. Schiebinger, *Nature's Body: Gender in the Making of Modern Science* (New Brunswick: Rutgers University Press 2004) 145; Stepan, *The Idea of Race in Science*, 1-19. See also: Arthur Lovejoy, *The Great Chain of Being: A Study of the History of an Idea* (Cambridge, MA: Harvard University Press 1936).

his descent from some lower form," like his nearest ally and early progenitor the ape. The interval between man and ape, then, were "filled up by numberless gradations," including prehistoric and present-day "savage" races. With Darwinism, "the 'lower races' were now races that had 'evolved' least far up the evolutionary ladder, had lost out in the 'struggle for survival,' and were 'unfit' for the competition between tribes. Or they represented the evolutionary 'childhood' of the white man," Stepan summarizes. Older racist notions thus lived on in newer scientific theories and practices.[71]

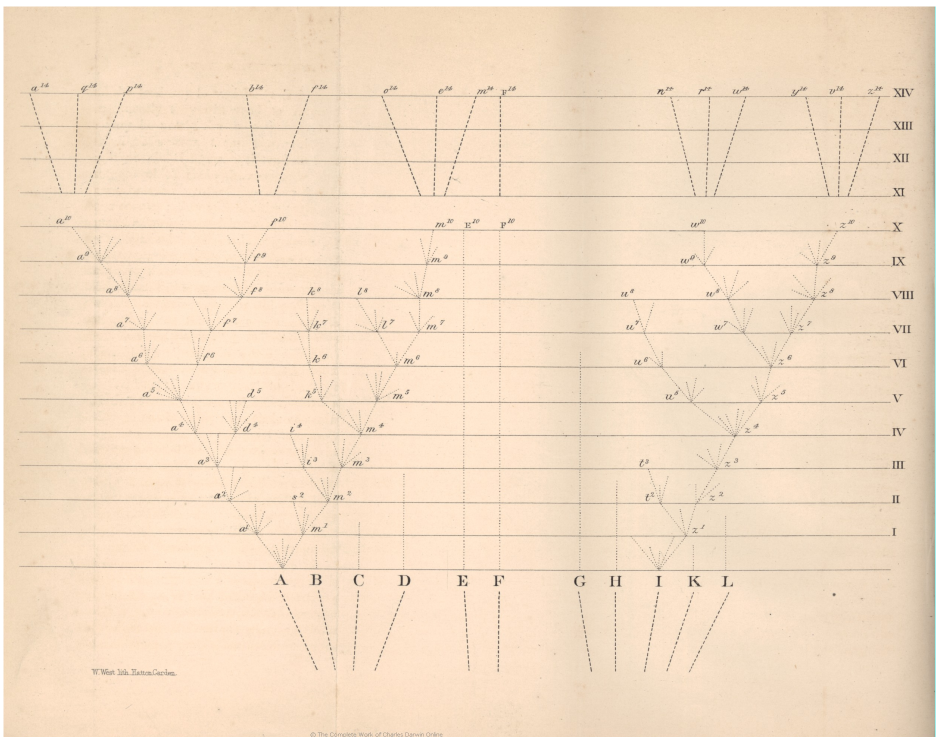

Figure 1.5. Evolution and descent, linked like tree branches. Source: Charles Darwin, *On the Origin of Species by Means of Natural Selection, or the Preservation of Favoured Races in the Struggle for Life* (London: John Murray 1859) 116-117. Source is in the public domain.

The hierarchy between man's races appear in multiple places in anthropological textbooks. Vogt's *Lectures of Man* is littered with comparisons between the "lower races" and apes and assertions that they represent earlier stages of mankind. Topinard claimed that "at the summit is Man, many of whose types approximate in many of their features to the Anthropoids." Certain characteristics

[71] Charles Darwin, *On the Origin of Species By Means of Natural Selection, or the Preservation of Favoured Races in the Struggle for Life* (London: John Murray 1859) 119; 489; Darwin, *Descent*, 34-35, Stepan, *The Idea of Race in Science*, 83.

"have an affinity in negroes to those which they exhibit in apes, and establish the transition between these and Europeans." Racial researchers chose the white man as a blueprint for understanding all human races. In introducing his reader to the physical characters of living subjects, Topinard stated: "and in the first place let us give the terms of the modern canon, as taught in the schools of art, where the white is the standard for the anatomy of the figure, as it is in the dissecting-rooms for ordinary anatomy."[72] For reasons unknown, talk of a hierarchy of races became somewhat more subdued or implicit in textbooks from the early 20th century. Terms like "primitive" and "civilized," however, continued to be liberally used, along with the white body as the standard for scientific study.[73]

One facet of the hierarchical understanding of organisms was that groups increased in complexity as one moved up the ladder, with simple creatures at the bottom and complex humans at the top. Some researchers also applied this logic to races. Individuals of a race shared racial essences but varied among themselves; some "Europeans" were blonde, others had dark hair. A few researchers suggested that such *within-group variation* increased as one moved up the racial ladder and claimed that white races were more varied and complex than non-white races. This logic could also be applied to human evolution: researchers generally supposed that "modern" people had more within-group variation than prehistoric people.[74] The two ideas easily went hand in hand: if anthropologists considered "primitive" people to represent man's evolutionary past, then they also were less variable, more homogeneous, and more "pure" than "civilized" races. Purity resulted from remaining more isolated throughout history while civilized races had migrated and intermixed more.

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72 Vogt, *Lectures on Man*, 171-2; 191-2; Topinard, *Anthropology*, 221; 316; 525.

73 See for a discussion about the role of the standardized white body and the inclusion of non-white bodies in scientific research today: Steven Epstein, *Inclusion: The Politics of Difference in Medical Research* (Chicago: University of Chicago Press 2007).

74 Karl Pearson Papers, 11/1/13/117 Correspondence with C.S. Myers, Myers to Pearson 14.3.1902.

Ripley, for instance, argues that "pure types physically are always to be found outside the great geographical meeting places…Competition, the opposite of isolation, in these places is the rule."[75]

In the age of quantification, determining the purity or within-group variation of a race became an exercise in creating frequency distributions and determining the amount of deviation from the average and the "sharpness" of the distribution's peaks. Vogt, suggested that "the confinement of variation in size within narrow limits [may] be regarded as a proof of the purity of a stock." Ripley agreed that "a sharp pyramid generally denotes a homogeneous people...On the other hand, a flattened curve indicates the introduction of some disturbing factor, be it an immigrant race, environment, or what not."[76]

Researchers of the 19th and 20th centuries believed that evolutionary pressures, colonization, and the spread of Western civilization endangered racial purity. Due to these forces, primitive races were believed to either soon go extinct or begin intermixing with other races. According to Topinard, in the future, "crossing will have diminished the number of pure types; the native race of America will have entirely disappeared – there will be no Esquimaux, or Aïnos, or Australians, or Bosjesmans."[77] Anthropologists all over the world feared that the primitive qualities that certain races embodied, the biological keys to the mysteries of a prehistoric past, would soon be lost to science. Racial science was therefore urgent and was the only way to salvage racial characters that would disappear in the immediate future.[78]

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75 Ripley, *Races of Europe*, 56. For a discussion on isolation and purity, see: Veronika Lipphardt, "Isolates and Crosses in Human Population Genetics; or, a Contextualization of German Race Science," *Current Anthropology* 53:S5 (2012) S69–82. Lipphardt also observes that talk of isolation (temporality) and purity (stability) went hand-in-hand.

76 Vogt, *Lectures of Man*, 50; Ripley, *Races of Europe*, 114.

77 Topinard, *Anthropology*, 427.

78 Jacob W. Gruber, "Ethnographic Salvage and the Shaping of Anthropology," *American Anthropologist* 72:6 (1970) 1289–99.

Data Practices: The Examination Procedure

Racial researchers such as anatomists and anthropologists developed a critical corpus of physical features for the determination of racial types and their differences, evolution, and "purity." Even though the size and shape of the skull took a prominent role in this research, textbooks listed a range of other valuable characters. Anthropologists considered the soft parts of the face, such as the shape of the nose, lips, ears, and eyes, to hold important clues about racial origins, as well as hair and skin color.[79] Most racial researchers measured *and* observed when examining living people and skeletons: skin color and nose shape could easily be examined with the naked eye; the cephalic index required the use of the caliper.

Obtaining sufficient samples was a prime concern. Textbooks recommended acquiring at least 20 adults per sex, striving for as large of a group as possible.[80] Ripley stated that "the day when one could…formulate an entire theory as to the origin of European types by the study of two crania alone is happily past. Modern craniometry must rest for its justification upon a few simple measurements, taken, however, upon a large number of subjects."[81] This could be challenging in field research where people could resist examination. On how to obtain subjects for measurement, Aleš Hrdlička's *Anthropometry* advised that while "in the case of schools, institutions, and recruiting stations, matters may be easily arranged," with tribes one should start with chiefs, elders, and other influential members of the group and then "work down." Researchers should leave out any measurements that could be easily obtained on skeletons or that would call for "resented exposures," referring to any measurements that required people to take off their clothes. When examining women, Hrdlička cautioned to "retain the attitude of the methodic, abstract investigator."

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[79] Vogt, *Lectures on Man,* 74; Robinson (ed.), *Cunningham's Text-Book of Anatomy*, 286.

[80] Topinard, *Anthropology*, 219.

[81] Ripley, *The Races of Europe,* 591.

Researchers should offer subjects a small payment for their participation. "The honest, friendly and able worker, with earnest, dignified procedure, will have little difficulty in succeeding among any class of people."[82]

During examinations, researchers attempted to achieve the highest levels of accuracy and precision – this was particularly important for field research on living people. All sorts of distractions lurked beyond the walls of the laboratory that could interrupt focus. Hrdlička instructed researchers to develop a habit of minute care in taking measurements and using instruments until these practices became "automatic." Examinations should take place in well-lighted spots that were the least subject to interruptions. "No conversation with the subject or a third person should be carried on during the examination, in order that the whole attention of the observer may be concentrated on the work itself." The researcher registered his measurements and observations on special recording blanks with entries for name, sex, age, place of study, and tribe. The blanks also listed the measurements and observations to be taken in order of their importance. Most textbooks provided their own recording blanks – there did not seem to have been a standardized practice (fig. 1.6).[83] Hrdlička suggested that the examiner recorded his measurements and observations himself to guard against error. Number should be written down in permanent ink "that will not fade out in the course of years, for some of the records secured may be of value long afterwards."[84] "If the above rules are followed, the well-trained, earnest observer will find his work reduced to a mechanical procedure of high order…the precision of which will be a source of constant gratification."[85]

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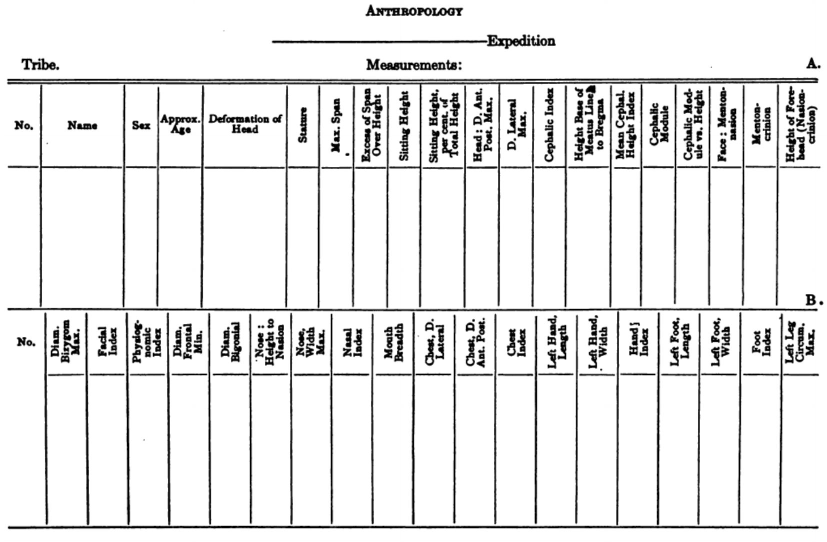
[82] Hrdlička, *Anthropometry,* 36; 41-43.

[83] Hrdlička, for instance, preferred the use of centimeters and their decimals, not the "German method" of using meters, centimeters, and millimeters.

[84] See the dissertation's conclusion for a discussion of the reuse of historic racial data.

[85] Hrdlička, *Anthropology,* 35; 41-42. Quotations on page 41-42.



ANTHROPOLOGY

——————————Expedition

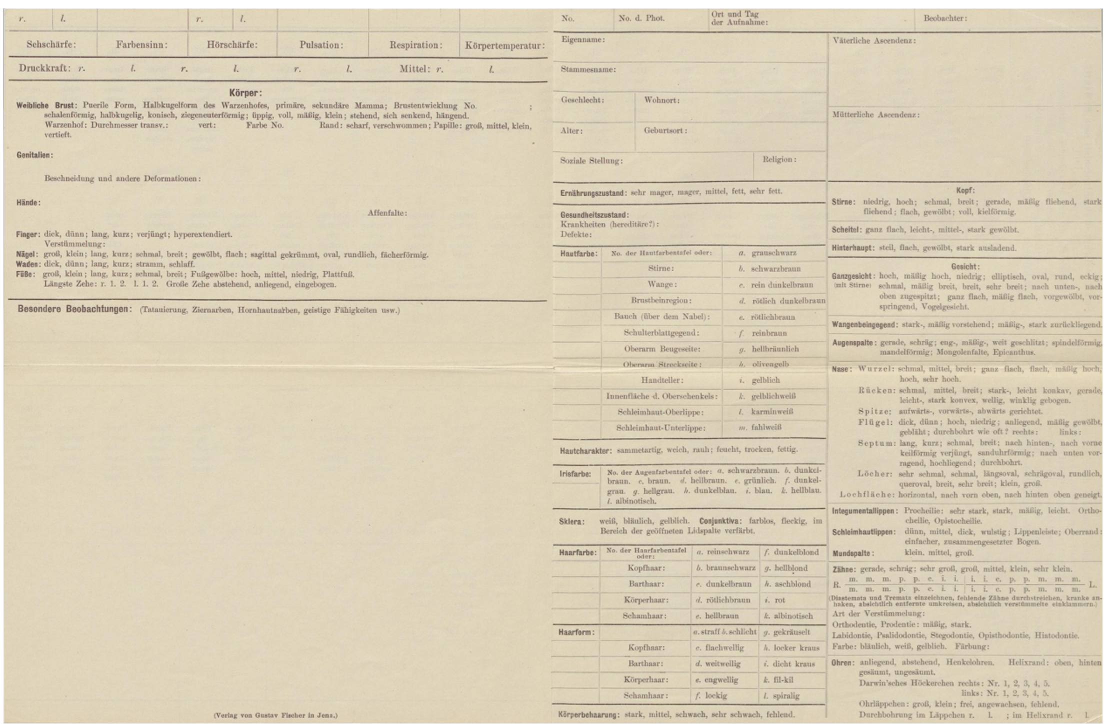
Tribe. Measurements: A.

No.	Name	Sex	Approx. Age	Deformation of Head	Stature	Max. Span	Excess of Span Over Height	Sitting Height	Sitting Height, per cent. of Total Height	Head: D. Ant. Post. Max.	D. Lateral Max.	Cephalic Index	Height Base of Meatus Line to Bregma	Mean Cephal. Height Index	Cephalic Module	Cephalic Module vs. Height	Face: Menton-nasion	Menton-crinion	Height of Forehead (Nasion-crinion)

B.

No.	Diam. Bizygom Max.	Facial Index	Physiognomic Index	Diam. Frontal Min.	Diam. Bigonial	Nose: Height to Nasion	Nose, Width Max.	Nasal Index	Mouth Breadth	Chest, D. Lateral	Chest, D. Ant. Post.	Chest Index	Left Hand, Length	Left Hand, Width	Hand Index	Left Foot, Length	Left Foot, Width	Foot Index	Left Leg Circum. Max.

Figure 1.6.a. Hrdlička's recording blank. Source: A. Hrdlička, *Anthropometry* (Philadelphia: Wistar Institute of Anatomy and Biology 1920) 63. Source is in the public domain.



r. l. r. l.

Sehschärfe: | Farbensinn: | Hörschärfe: | Pulsation: | Respiration: | Körpertemperatur:

Druckkraft: r. l. r. l. r. l. Mittel: r. l.

Körper:

Weibliche Brust: Puerile Form, Halbkugelform des Warzenhofes, primäre, sekundäre Mamma; Brustentwicklung No. ; schalenförmig, halbkugelig, konisch, ziegeneuterförmig; üppig, voll, mäßig, klein; stehend, sich senkend, hängend. Warzenhof: Durchmesser transv.: vert: Farbe No. Rand: scharf, verschwommen; Papille: groß, mittel, klein, vertieft.

Genitalien:

Beschneidung und andere Deformationen:

Hände:

Affenfalte:

Finger: dick, dünn; lang, kurz; verjüngt; hyperextendiert.
Verstümmelung:
Nägel: groß, klein; lang, kurz; schmal, breit; gewölbt, flach; sagittal gekrümmt, oval, rundlich, fächerförmig.
Waden: dick, dünn; lang, kurz; stramm, schlaff.
Füße: groß, klein; lang, kurz; schmal, breit; Fußgewölbe: hoch, mittel, niedrig, Plattfuß.
Längste Zehe: r. 1. 2. l. 1. 2. Große Zehe abstehend, anliegend, eingebogen.

Besondere Beobachtungen: (Tatauierung, Ziernarben, Hornhautnarben, geistige Fähigkeiten usw.)

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Eigenname:
Stammesname:
Geschlecht: | Wohnort:
Alter: | Geburtsort:
Soziale Stellung: | Religion:

Väterliche Ascendenz:
Mütterliche Ascendenz:

Ernährungszustand: sehr mager, mager, mittel, fett, sehr fett.

Gesundheitszustand:
Krankheiten (hereditäre?):
Defekte:

Hautfarbe:	No. der Hautfarbentafel oder:	a. grauschwarz
	Stirne:	b. schwarzbraun
	Wange:	c. rein dunkelbraun
	Brustbeinregion:	d. rötlich dunkelbraun
	Bauch (über dem Nabel):	e. rötlichbraun
	Schulterblattgegend:	f. reinbraun
	Oberarm Beugeseite:	g. hellbräunlich
	Oberarm Streckseite:	h. olivengelb
	Handteller:	i. gelblich
	Innenfläche d. Oberschenkels:	k. gelblichweiß
	Schleimhaut-Oberlippe:	l. karminweiß
	Schleimhaut-Unterlippe:	m. fahlweiß

Hautcharakter: sammetartig, weich, rauh; feucht, trocken, fettig.

Irisfarbe: No. der Augenfarbentafel oder: a. schwarzbraun. b. dunkelbraun. c. braun. d. hellbraun. e. grünlich. f. dunkelgrau. g. hellgrau. h. dunkelblau. i. blau. k. hellblau. l. albinotisch.

Sklera: weiß, bläulich, gelblich. **Conjunktiva:** farblos, fleckig, im Bereich der geöffneten Lidspalte verfärbt.

Haarfarbe:	No. der Haarfarbentafel oder:	a. reinschwarz	f. dunkelblond
	Kopfhaar:	b. braunschwarz	g. hellblond
	Barthaar:	c. dunkelbraun	h. aschblond
	Körperhaar:	d. rötlichbraun	i. rot
	Schamhaar:	e. hellbraun	k. albinotisch
Haarform:		a. straff b. schlicht	g. gekräuselt
	Kopfhaar:	c. flachwellig	h. locker kraus
	Barthaar:	d. weitwellig	i. dicht kraus
	Körperhaar:	e. engwellig	k. fil-kil
	Schamhaar:	f. lockig	l. spiralig

Körperbehaarung: stark, mittel, schwach, sehr schwach, fehlend.

Kopf:

Stirne: niedrig, hoch; schmal, breit; gerade, mäßig fliehend, stark fliehend; flach, gewölbt; voll, kielförmig.

Scheitel: ganz flach, leicht-, mittel-, stark gewölbt.

Hinterhaupt: steil, flach, gewölbt, stark ausladend.

Gesicht:

Ganzgesicht (mit Stirne): hoch, mäßig hoch, niedrig; elliptisch, oval, rund, eckig; schmal, mäßig breit, breit, sehr breit; nach unten-, nach oben zugespitzt; ganz flach, mäßig flach, vorgewölbt, vorspringend, Vogelgesicht.

Wangenbeingegend: stark-, mäßig vorstehend; mäßig-, stark zurückliegend.

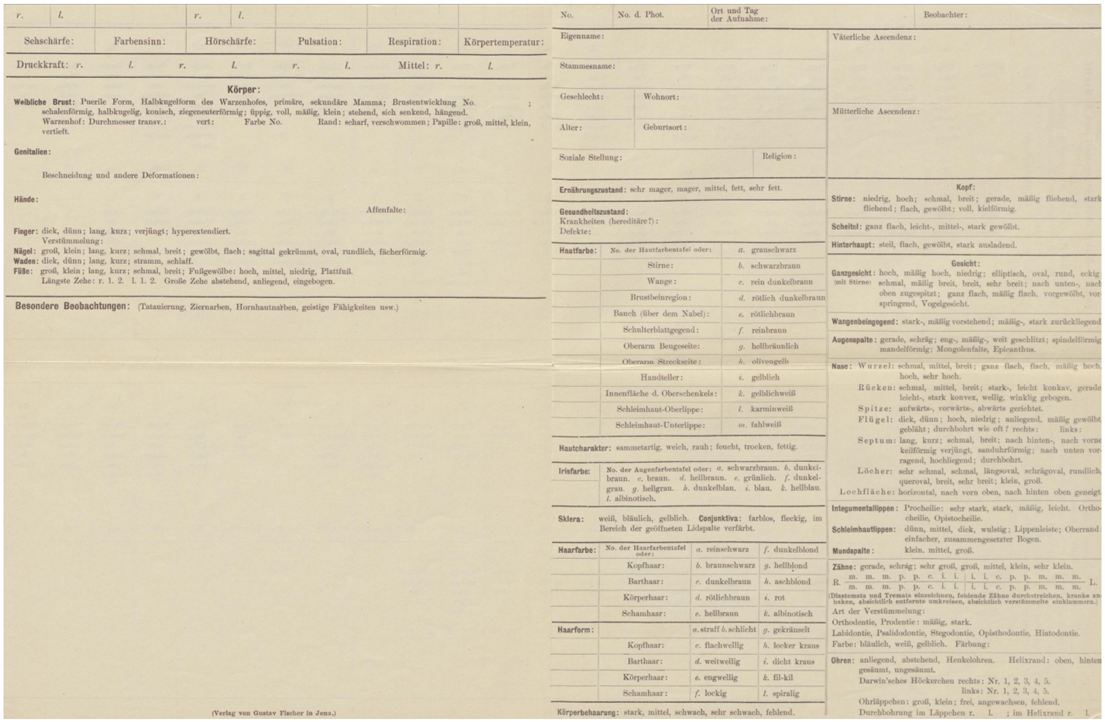
Augenspalte: gerade, schräg; eng-, mäßig-, weit geschlitzt; spindelförmig, mandelförmig; Mongolenfalte, Epicanthus.

Nase: Wurzel: schmal, mittel, breit; ganz flach, flach, mäßig hoch, hoch, sehr hoch.
Rücken: schmal, mittel, breit; stark-, leicht konkav, gerade, leicht-, stark konvex, wellig, winklig gebogen.
Spitze: aufwärts-, vorwärts-, abwärts gerichtet.
Flügel: dick, dünn; hoch, niedrig; anliegend, mäßig gewölbt, gebläht; durchbohrt wie oft? rechts: links:
Septum: lang, kurz; schmal, breit; nach hinten-, nach vorne keilförmig verjüngt, sanduhrförmig; nach unten vorragend, hochliegend; durchbohrt.
Löcher: sehr schmal, schmal, längsoval, schrägoval, rundlich, queroval, breit, sehr breit; klein, groß.
Lochfläche: horizontal, nach vorn oben, nach hinten oben geneigt.

Integumentallippen: Procheilie: sehr stark, stark, mäßig, leicht. Orthocheilie, Opistocheilie.
Schleimhautlippen: dünn, mittel, dick, wulstig; Lippenleiste; Oberrand: einfacher, zusammengesetzter Bogen.
Mundspalte: klein, mittel, groß.

Zähne: gerade, schräg; sehr groß, groß, mittel, klein, sehr klein.
R. m. m. m. p. p. c. i. i. | i. i. c. p. p. m. m. m. L.
m. m. m. p. p. c. i. i. | i. i. c. p. p. m. m. m.
(Diastemata und Tremata einzeichnen, fehlende Zähne durchstreichen, kranke anhaken, absichtlich entfernte umkreisen, absichtlich verstümmelte einklammern.)
Art der Verstümmelung:
Orthodontie, Prodentie: mäßig, stark.
Labidontie, Psalidodontie, Stegodontie, Opisthodontie, Hiatodontie.
Farbe: bläulich, weiß, gelblich. Färbung:

Ohren: anliegend, abstehend, Henkelohren. Helixrand: oben, hinten gesäumt, ungesäumt.
Darwin'sches Höckerchen rechts: Nr. 1, 2, 3, 4, 5.
links: Nr. 1, 2, 3, 4, 5.
Ohrläppchen: groß, klein; frei, angewachsen, fehlend.
Durchbohrung im Läppchen r. l. ; im Helixrand r. l.



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Körper:
Weibliche Brust: Puerile Form, Halbkugelform des Warzenhofes, primäre, sekundäre Mamma; Brustentwicklung No. ; schalenförmig, halbkugelig, konisch, ziegeneuterförmig; üppig, voll, mäßig, klein; stehend, sich senkend, hängend. Warzenhof: Durchmesser transv.: vert: Farbe No. Rand: scharf, verschwommen; Papille: groß, mittel, klein, vertieft.
Genitalien:
Beschneidung und andere Deformationen:
Hände:
Affenfalte:
Finger: dick, dünn; lang, kurz; verjüngt; hyperextendiert.
Verstümmelung:
Nägel: groß, klein; lang, kurz; schmal, breit; gewölbt, flach; sagittal gekrümmt, oval, rundlich, fächerförmig.
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Längste Zehe: r. 1. 2. l. 1. 2. Große Zehe abstehend, anliegend, eingebogen.
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Stammesname:
Geschlecht: | Wohnort:
Alter: | Geburtsort:
Soziale Stellung: | Religion:
Väterliche Ascendenz:
Mütterliche Ascendenz:
Ernährungszustand: sehr mager, mager, mittel, fett, sehr fett.
Gesundheitszustand:
Krankheiten (hereditäre?):
Defekte:

Hautfarbe:	No. der Hautfarbentafel oder:	a. grauschwarz
	Stirne:	b. schwarzbraun
	Wange:	c. rein dunkelbraun
	Brustbeinregion:	d. rötlich dunkelbraun
	Bauch (über dem Nabel):	e. rötlichbraun
	Schulterblattgegend:	f. reinbraun
	Oberarm Beugeseite:	g. hellbräunlich
	Oberarm Streckseite:	h. olivengelb
	Handteller:	i. gelblich
	Innenfläche d. Oberschenkels:	k. gelblichweiß
	Schleimhaut-Oberlippe:	l. karminweiß
	Schleimhaut-Unterlippe:	m. fahlweiß

Hautcharakter: sammetartig, weich, rauh; feucht, trocken, fettig.
Irisfarbe: No. der Augenfarbentafel oder: a. schwarzbraun. b. dunkelbraun. c. braun. d. hellbraun. e. grünlich. f. dunkelgrau. g. hellgrau. h. dunkelblau. i. blau. k. hellblau. l. albinotisch.
Sklera: weiß, bläulich, gelblich. Conjunktiva: farblos, fleckig, im Bereich der geöffneten Lidspalte verfärbt.

Haarfarbe:	No. der Haarfarbentafel oder:	a. reinschwarz	f. dunkelblond
	Kopfhaar:	b. braunschwarz	g. hellblond
	Barthaar:	c. dunkelbraun	h. aschblond
	Körperhaar:	d. rötlichbraun	i. rot
	Schamhaar:	e. hellbraun	k. albinotisch
Haarform:		a. straff b. schlicht	g. gekräuselt
	Kopfhaar:	c. flachwellig	h. locker kraus
	Barthaar:	d. weitwellig	i. dicht kraus
	Körperhaar:	e. engwellig	k. fil-kil
	Schamhaar:	f. lockig	l. spiralig

Körperbehaarung: stark, mittel, schwach, sehr schwach, fehlend.

Kopf:
Stirne: niedrig, hoch; schmal, breit; gerade, mäßig fliehend, stark fliehend; flach, gewölbt; voll, kielförmig.
Scheitel: ganz flach, leicht-, mittel-, stark gewölbt.
Hinterhaupt: steil, flach, gewölbt, stark ausladend.
Gesicht:
Ganzgesicht (mit Stirne): hoch, mäßig hoch, niedrig; elliptisch, oval, rund, eckig; schmal, mäßig breit, breit, sehr breit; nach unten-, nach oben zugespitzt; ganz flach, mäßig flach, vorgewölbt, vorspringend, Vogelgesicht.
Wangenbeingegend: stark-, mäßig vorstehend; mäßig-, stark zurückliegend.
Augenspalte: gerade, schräg; eng-, mäßig-, weit geschlitzt; spindelförmig, mandelförmig; Mongolenfalte, Epicanthus.
Nase: Wurzel: schmal, mittel, breit; ganz flach, flach, mäßig hoch, hoch, sehr hoch.
Rücken: schmal, mittel, breit; stark-, leicht konkav, gerade, leicht-, stark konvex, wellig, winklig gebogen.
Spitze: aufwärts-, vorwärts-, abwärts gerichtet.
Flügel: dick, dünn; hoch, niedrig; anliegend, mäßig gewölbt, gebläht; durchbohrt wie oft? rechts: links:
Septum: lang, kurz; schmal, breit; nach hinten-, nach vorne keilförmig verjüngt, sanduhrförmig; nach unten vorragend, hochliegend; durchbohrt.
Löcher: sehr schmal, schmal, längsoval, schrägoval, rundlich, queroval, breit, sehr breit; klein, groß.
Lochfläche: horizontal, nach vorn oben, nach hinten oben geneigt.
Integumentallippen: Procheilie: sehr stark, stark, mäßig, leicht. Orthocheilie, Opistocheilie.
Schleimhautlippen: dünn, mittel, dick, wulstig; Lippenleiste; Oberrand: einfacher, zusammengesetzter Bogen.
Mundspalte: klein, mittel, groß.
Zähne: gerade, schräg; sehr groß, groß, mittel, klein, sehr klein.
R. m. m. m. p. p. c. i. i. | i. i. c. p. p. m. m. m. L.
m. m. m. p. p. c. i. i. | i. i. c. p. p. m. m. m.
(Diastemata und Tremata einzeichnen, fehlende Zähne durchstreichen, kranke anhaken, absichtlich entfernte umkreisen, absichtlich verstümmelte einklammern.)
Art der Verstümmelung:
Orthodontie, Prodontie: mäßig, stark.
Labidontie, Psalidodontie, Stegodontie, Opisthodontie, Hiatodontie.
Farbe: bläulich, weiß, gelblich. Färbung:
Ohren: anliegend, abstehend, Henkelohren. Helixrand: oben, hinten gesäumt, ungesäumt.
Darwin'sches Höckerchen rechts: Nr. 1, 2, 3, 4, 5.
links: Nr. 1, 2, 3, 4, 5.
Ohrläppchen: groß, klein; frei, angewachsen, fehlend.
Durchbohrung im Läppchen r. l. ; im Helixrand r. l.

Figure 1.6.b. Martin's recording blank, front and back. Note that it provides space for both observations and measurements. Source: R. Martin, *Lehrbuch der Anthropologie* (Jena: Gustav Fischer Verlag 1914) appendix. Source is in the public domain.

Sources reveal that such ideal circumstances of focus, compliant subjects, and lack of interruption were not always met. During a scientific expedition to Dutch New Guinea in 1903, the research team, on boat, made drum sounds to make villagers aware of their impending arrival and peaceful intentions. In some cases, inhabitants deserted their villages after hearing the drum sounds. When the scientists did encounter villagers willing to be examined, physical anthropologist Gijsbert van der Sande struggled to accurately record pulse frequency because subjects were nervous during the examination.[86] In another example, from 1928, Italian anthropologist Lidio Cipriani took anthropological observations, measurements, and plaster casts in present-day South Africa. In his report, he wrote that the Indigenous people found the measuring process "ridiculous" and started laughing when instruments touched them. Cipriani's attempts to take plaster casts provoked an

86 Hendrik Lorentz, *Eenige Maanden Onder De Papoea's* (Leiden: Brill 1905) 273; G.A.J. van der Sande, *Nova Guinea. Uitkomsten Der Nederlandsche Nieuw-Guinea-expeditie in 1903 Onder Leiding Van Dr. Arthur Wichmann* (Leiden: Brill 1907) 342.

uncontrollable laughter from bystanders. He had to ask his interpreter to remove bystanders from the scene because they were causing the subjects to smile and thus crack and ruin the plaster casts.[87] The results from these examinations, the filled-out blanks, thus mask the messy encounters that made them possible and the translation process enabled by the caliper.

As discussed above, racial researchers increasingly turned to measurements for racial identification and classification. At the same time, quantification did not replace observation and description: both data practices remained essential to racial classification.

Data Practices: Measurement, Geometry, and Statistics

The measurement of the skull and body centered on geometry. For measurements, anthropologists first relied on anatomical "landmarks:" the relevant morphological points on the surface of the body, bones, and skull that all humans share. The practice of taking landmarks as the basis for measurements derived from medicine. British anatomist Luther Holden explained in his textbook on clinical anatomy that "medical or surgical landmarks" were "surface-marks, such as lines, eminences, depressions, which are guides to, or indications of, deeper seated parts."[88] Landmarks could be identified on the bony structure or through the skin. Even though their precise location varied slightly per individual, these fixed points furnished researchers with a system that increased the uniformity of observations and measurements. One of the most important landmarks on the skull, for instance, were the auditory openings or "auricular points" – they formed a central landmark from which several cranial measurements were determined and were easily located on both the living and the dead.[89]

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87 Lidio Cipriani, "Sette Mesi in Africa," *Universo* 9 (1928) 640.

88 Luther Holden, *Landmarks, Medical and Surgical* (London: J & A Churchill 1876) 1-3.

89 A. Robinson (ed.), *Cunningham's Text-Book of Anatomy,* 285; Vogt, *Lectures of Man*, 25.

Second, racial researchers determined cranial and bodily surfaces called "anatomical planes." Borrowing from a long tradition in anatomy, these planes hypothetically divided the body into several geometric sections that facilitated in locating landmarks and describing the body. The three standard body and skull slices, the median sagittal, the transverse vertical, and horizontal plane, were ideally mutually perpendicular (fig. 1.7).

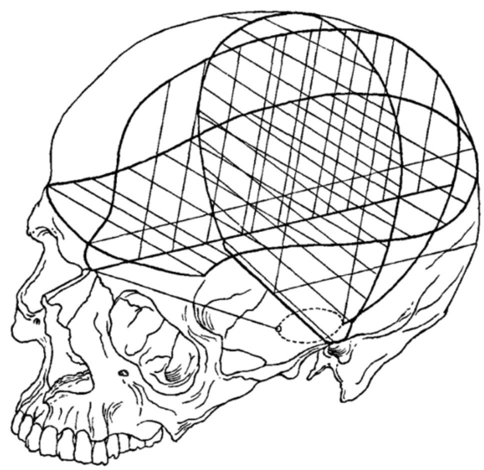

Figure 1.7. The three standard cranial planes. Republished with permission of Oxford University Press from William Howells, "The Cranial Vault: Factors of Size and Shape," *American Journal of Physical Anthropology* 15 (1957) 24.

The planes tied together measurements, thus preventing the *fragmentation* of the skull or body into bits and pieces. Davenport explained:

> It is desirable that the dimensions should be capable of being tied together so that outlines of the person measured could be drawn. If a series of measurements, as, e.g., of the profile, are made without tying any one of them to these measurements made from the floor, the group of these float in space, as it were, and their position is not fixed to the rest of the figure. Also, points should be measured with reference to the 3 fundamental planes by three coordinates. The floor is one plane, the sagittal plane of the body, in a symmetrical organism, is a second. For the third a vertical wall against which the subject is in contact may serve. Points in the sagittal plane of the body should be referred, so far as possible, to the other two rectangular planes.[90]

[90] Davenport, *Guide to Physical Anthropometry and Anthroposcopy,* 11.

These anatomical planes are still used today for medical imaging techniques such as CT, MRI, and PET.[91]

Before landmarks could be determined and distances between them measured, skulls first needed to be placed in a definite position. Anthropologists positioned the skull in a horizontal plane of orientation, following the natural shape of the skull as much as possible. Broca suggested the alveolo-condylar plane, which ran from the teeth through the rounded end of the occipital bone. As Chapter 5 will discuss, this horizontal plane became the source of controversy and bitter rivalry in the late 19th century.[92] Measurements, then, could be located and taken across racial samples with the help of landmarks and planes. This planar approach turned the body and skull into a grid or Cartesian coordinate system. It envisioned the relationship between morphological points in a geometric way.

Anthropologists developed various technologies to transform the skull into a geometric object on paper. These approaches reconstructed the skull into a two-dimensional visual made of lines, triangles, and contours. It simplified the skull's form and brought out its racial essences. Transforming skulls into geometric objects was a practice of abstracting individual variation and erasing subjecthood: it divorced the skull from its context as the house of the brain of a once-living individual or its morphological existence in the lab. German anthropologist Hermann Welcker created a polygon-style projection called the "Schädelnetz" or cranial net (fig. 1.8). Inspired by the netlike designs to make paper figures of crystals, this projection used mean measurements of a set of skulls to create an "averaged" skull "net" of four squares and four triangles that captured the type skull's characteristic forms, shapes, and spaciousness:

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[91] They are the same but with different names. The horizontal plane is today called the transverse; the transverse is now called the coronal plane. Keith L. Moore, Arthur F. Dalley, A.M.R. Agur, *Clinically Oriented Anatomy* 7th edition (Philadelphia: Wolters Kluwer/Lippincott Williams & Wilkins 2014) 6.

[92] The horizontal plane was possibly first determined by Camper. Paul Broca, "Sur le plan horizontal de la tête et sur la méthode trigonométrique," *Bulletins et Mémoires de la Société d'Anthropologie de Paris* 8:1 (1873) 48–96.

The skull net…is different in different skulls in a highly characteristic way; the length of its lines and its angles often make differences and peculiarities more rapidly apparent than is possible on the skull itself. But if one does not want to recognize the skull net for what it is really is – an abstracted [*abgekürztes*] image of the skull – then it should at least be recognized as an extremely effective and clear graphic compilation of the most important cranial dimensions.

The method exposed a mean diameter network of the type skull that allowed for the measurement of several distances between landmarks.[93]

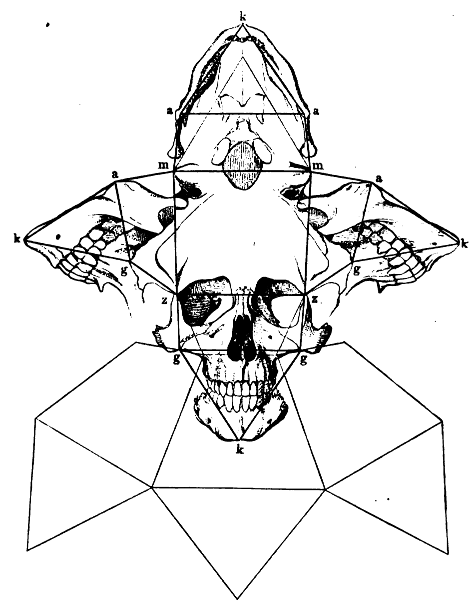

Figure 1.8. Welcker's cranial net. Source: Hermann Welcker, "Kraniologische Mittheilungen," *Archiv für Anthropologie* I (1866) 108 fig. 39. Source is in the public domain.

Broca also developed several instruments to create a skull's geometric projection. His *stereographe*, for instance, recreated the skull's landmarks on paper (fig. 1.9). The instrument positioned the skull between two parallel arms; one exploratory needle that touched the skull and one pencil that touched the paper. Moving the needle along the skull's surface also moved the pencil and thus enabled the researcher to draw a contour on paper. He could also add details, visible and

[93] H. Welcker, *Untersuchungen über Wachstum und Bau des menschlichen Schädels: 1: Allgemeine Verhältnisse des Schädelwachstums und Schädelbaues, normaler Schädel deutschen Stammes* (Leipzig: Verlag von W. Engelmann 1862) 24-26 (quotation from page 26); Vogt, *Lectures on Man*, 45-46; Hermann Welcker, "Kraniologische Mittheilungen," *Archiv für Anthropologie* I (1866) 102-112.

invisible to the eye, to the contour by replacing the arm with a curved rod that poked the skull's interior.[94]

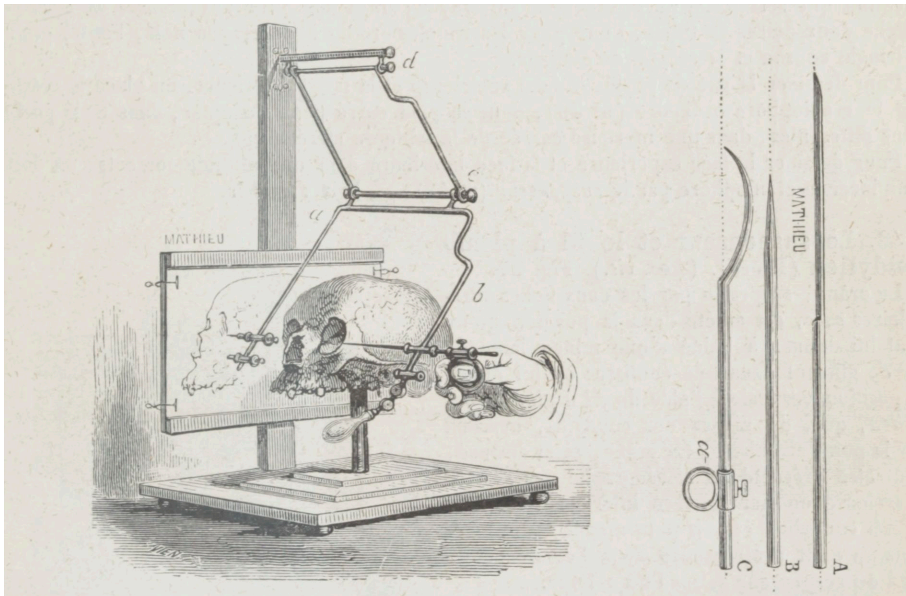

Figure 1.9. Broca's stereographe. Source: Mathieu, *Catalogue des instruments anthropologiques* (Paris: H. Plon 1873) page 15. Source is in the public domain.

These geometric projections differed from the "central projections" that the eye and the camera created. With central projections of the skull, the lines converged towards one point, thus slightly distorting the skull's shape. Geometric projections created parallel lines. Broca therefore argued that geometric projects were "the only ones which give exact measurements applicable to craniometry." They created undistorted, objective shapes on paper from which the anthropologist could directly measure lines, curves, and angles accurately to the millimeter, "more readily than on the skull itself," Broca claimed. Not only did the instrument remove the distorted view of the anthropologist in developing an accurate projection, it shifted the labor of creating the image to the instrument itself. Topinard assured that the *stereographe* required hardly any skill; the researcher only

[94] Broca, "Sur le Craniographe et sur la determination de plusieurs angles nouveaux nommes angles auriculaires," *Bulletin de la Société d'Anthropologie* (1861 t. II) 673-80.

needed to guide the instrument. Like an automaton, the mover of the parts became one whole with the instrument. Technologies for geometric projections thus attempted to *automate* the research process by removing the subjectivity of both the researcher and the skull.[95] They were designed to engage with the situated and spirited being of the researcher and remove any bias that might result from his positionality, not just his viewing perspective.[96]

Data Practices: The Morphological Method

Racial researchers also employed an embodied, subjective methodology that relied on the researcher's memory and deep knowledge: the morphological method. Anatomists and physical anthropologists continued to greatly value the study of the morphological skull with the naked, trained eye. These morphological observations were generated from years of experience of handling skulls. It also stemmed from the anatomical training that many anthropologists had received which taught them how to see and feel the body. This training had instilled in students "the habit of examining the living body with 'anatomical eyes' and 'surgical fingers'" and trained their hands and eyes to act together, as the medical textbook *Gray's Anatomy* made clear in 1887.[97] In inspecting the skull, researchers used their hands and eyes to observe the anatomical landmarks, the bone's structures, and the skull's shape. They used descriptors such as "proturberance," "crest," "pits," "depressions," "grooves," and "furrows" to illustrate the prominences and surfaces of the skull's

[95] Broca, "Sur le Craniographe et sur la determination de plusieurs angles nouveaux nommes angles auriculaires," 674-6; L. Mathieu, *Catalogue des instruments anthropologiques* (Paris: H. Plon 1873) 14-16; Topinard, *Anthropology*, 263-277. Martin argued that the *stereographe* was not that easy to use and required precise handling. Martin, *Lehrbuch der Anthropologie* 2nd edition (Jena: Gustav Fisher Verlag 1928) 54.

[96] I thus take a more forceful position than Andrew Zimmerman, who likewise argues that geometric projections effaced the researcher's subjectivity, but only his viewing position, not "anything psychological." Andrew Zimmerman, *Anthropology and Antihumanism in Imperial Germany* (Chicago: University of Chicago Press 2001) 104-106.

[97] H. Gray, T. Pickering Pick, and R. Howden, *Anatomy, Descriptive and Surgical* (Philadelphia: Lea Brothers & Co 1887) 1025.

bones, characteristics that were difficult to capture in measurements.[98] Anatomical textbooks like Cunningham's described at length how to inspect various parts of the skull: "By the removal of the skull-cap the cerebral aspect of the cranial cavity is exposed. The deep surface of the cranial vault is grooved medially for the superior sagittal sinus, on either side of which are seen numerous depressions for the lodgment of arachnoideal granulations. On holding the bone up to the light, the floor of these little hollows is oftentimes seen to be very thin."[99] Rather than expressing these characteristics in numbers, anthropologists described and scored them. Martin explained that "for many descriptive characteristics, however, it is neither possible nor worthwhile to draw up schemes…it is usually sufficient to distinguish between 5 levels: 0 missing, 1 weak, 2 medium, 3 high, and 4 very strong. Finer distinctions only pretend an accuracy that is not feasible." Such scoring was done on craniometric recording blanks.[100]

Thus, not all aspects of the skull could be quantified – some could only be seen and felt, like sex. Because sexual dimorphism was often greater than racial difference, the researcher first needed to separate cranial samples by sex before analyzing racial origins so as to not mistake sexual difference for racial difference. The skull's sexual characteristics were often visible to the eye: "the head of the woman is smaller and lighter, its contours more delicate, the surfaces smoother, the ridges and processes not so marked," Topinard clarified. In fact, "all the parts of the female skeleton are lighter and more frail; the general contour is more soft and graceful; the eminences, processes, or tubercules are smaller and less marked."[101] After establishing sex, the researcher could explore racial characteristics that "defied all attempts at measurement" through morphological inspection. For

98 Robinson (ed.), A., *Cunningham's Text-Book of Anatomy,* 82.

99 Robinson (ed.), A., *Cunningham's Text-Book of Anatomy*, 179.

100 Martin, *Lehrbuch der Anthropologie,* 589.

101 Topinard, *Anthropology*, 143; 145. Note the sexualized language here in describing female bones.

instance, Topinard argued that cranial sutures were simple in inferior races, but complex in more superior ones and that the shape of the parietal bones were flat in "negroes of Africa", bulged among the "Lapps."[102]

Researchers also utilized the morphological method in determining the racial characteristics of living people. Descriptive characters included skin color, hair color and shape, eye color, and shape of the nose and the lips. These characteristics were also filled out on the recording blanks with descriptors such as "straight," "wooly," or "prominent." Some examination tools combined the morphological and the metrical method. Anthropologists such as Paul Broca, Rudolf Martin, and German anthropologist Felix von Luschan developed standard scales for skin, hair, and eye color. In using the scales to determine shades of the body, the anthropologist listed the corresponding number on the form.[103]

Even though Broca argued that geometric projections were the only way to ensure exact measurements, he also advocated the morphological approach. He explained that the anthropologist obtained, after years of training, a special wisdom to observe racially distinctive traits at a first glance. The anthropologist could "penetrate the details" and move beyond the racial characters that were obvious to lay observers. This "certainty of glance," a "special sense which makes the artist," assisted in determining racial types.[104] British anthropologist Arthur Keith wrote about this expertise:

> when a skull of unknown history is placed in [the craniologist's] hands for racial identification," he does not at once sit down to measure its angles and indices "and then sets out to search for skulls possessing similar angles and indices…*A cast of the eye is sufficient for a diagnosis in making a racial identification of a man or of a skull.* The anthropologist follows the practice of everyday experience; wherein he differs from the ordinary man is that he becomes conscious of the points which lead him to a diagnosis; he turns a subconscious empirical process into one which is conscious and scientific in

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102 Topinard, *Anthropology*, 208-213.

103 See Davenport, *Guide to Physical Anthropometry and Anthroposcopy,* 40-44; 52-53; Hrdlička, *Anthropometry*, 59.

104 P. Broca, *Instructions Générales Pour Les Recherches Anthropologiques*, 26-27.

> that he notes the various points on which a recognition is made, measures them and learns the value which should be attached to them.[105]

Everyday experience of types was not enough; the expertise necessary to racially differentiate skulls was only gained by the long experience of holding, inspecting, and arranging skulls. "Absolute measurements are of great assistance, but the use of indices and angles by themselves…can and do give most misleading results. One other qualification is needed for the equipment of the craniologist – a complete and intimate knowledge of the skulls of all races of mankind."[106]

While geometric projections *removed* the subjectivity of the viewing researcher, morphology *reintroduced* it. Quantification fragmented the skull into bits and pieces to be measured; the morphological approach maintained a more holistic, *Gestalt* appraisal of the skull, borrowing a term from psychology that referred to the principle that the whole was greater than the sum of its parts.[107] The morphological method exemplifies the "sciences of subjectivity" that generate scientific knowledge rather than obstruct objective knowledge.[108] The subjective and the objective, the morphological and metrical approach thus co-existed and researchers used them to different extents. The methods supplemented each other in the construction of racial types. Broca explained that typology "consists in carefully examining a great number of persons, in grasping what is common between them, in abstracting individual variations, in grouping in an ideal type the features and characters, which, taken one by one, clearly predominate in the great majority, and to consider as the true representatives of the race the individuals who come closest to this type."[109] These racial types

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105 Arthur Keith, "Was the Chancelade Man Akin to the Eskimo," *Man* 2 (1925) 187, italics mine.

106 Arthur Keith, "Was the Chancelade Man Akin to the Eskimo," 189.

107 T. Sjøvold, G.N. Van Vark, and W.W. Howells, *Multivariate Statistical Methods in Physical Anthropology* (Dordrecht: D. Reidel Publishing 1984) 2-3.

108 Steven Shapin, "The Sciences of Subjectivity," *Social Studies of Science* 42:2 (2012) 170–84.

109 P. Broca, *Instructions Générales Pour Les Recherches Anthropologiques*, 25-26.

were not only reproduced in geometric projections and average measurements, but also in photographs of individuals that closely represented this ideal type. Thus, some racial essences could only be observed, others only measured. Both methods offered the required abstraction tools. Quantification, therefore, was not antithetical to typology or more subjective modes of producing racial knowledge.

Crisis

Towards the end of the 19th century, researchers began to doubt racial science's methodology. Over the years, scholars had presented very different racial schemes, determining anywhere between 4 to 60 races globally. Darwin pointed out in the *Descent of Man* that this diversity of opinion resulted from the fact that races graduated into each other and thus defied clear distinctive characters.[110] Felix von Luschan observed that the number of races increased from one author to another. Along with no fixed definition of race, he mocked that determining the number of races was "just as pointless as determining how many angels can dance on the top of a needle."[111]

Racial researchers further disagreed on the value of the metrical and morphological methods. American anatomist Harris Hawthorne Wilder clarified in his *Laboratory Manual of Anthropometry* in 1920:

> Concerning the actual value of anthropometric measurements, of whatever sort, and the extent to which measurement may be profitably carried, both opinion and practice differ widely…Certain investigators are bound to be more interested in the mathematical than in the biological side, and there is always danger that, in their hands, the latter cause may suffer, and the work be viewed as a mathematical problem, in which the goal is reached when the new relations involved are expressed in the form of formulae and tables. Others, on the other hand, view Physical Anthropology as wholly morphological, and place their reliance upon forms and form-comparisons as revealed to the eye, being very wary about expressing any character in a mathematical form.

[110] Darwin, *The Descent of Man*, 226.

[111] Felix von Luschan, *Völker, Rassen, Sprachen* (Berlin: Welt Verlag 1922) 2-3.

On the "mathematical extreme," Wilder named Hungarian anthropologist Aurel von Török "in whose hands the whole subject becomes an endless series of measurements." Török's textbook, published in 1890, included 5371 measurements of the skull, and hundreds of indices, angles, triangles, and polygons.[112] Török's goal, Wilder argued, seemed to be quantify the skull in its totality so that it could be "faithfully reproduced if destroyed."[113] Other researchers rejected craniometry. Italian anthropologist Giuseppi Sergi urged that anthropologists should racially research skulls with the eye: "as a zoologist can recognize the character of an animal species or variety belonging to any region of the globe or any period of time, so also should an anthropologist if he follows the same method of investigating the morphological characters of the skull," Sergi argued in 1909.[114] According to Duckworth, "Sergi has carried the purely descriptive method to such an extreme as renders its use almost impracticable, owing to the minuteness of detail which suffices for the creation of new specific cranial types."[115]

Wilder instead recommended that the "rational" anthropologist used measurements to bring out and express differences already perceptible to the eye of the trained observer.[116] Likewise, Topinard argued that the descriptive method of using the eye and the craniometrical method were "equally usual and mutually perfect…Some of the differences which skulls exhibit … are more readily appreciable by their general appearance, others by measurement."[117] Numerical data *supplemented* descriptive data and could make verbal descriptions more accurate.[118]

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112 Aurel von Török, *Grundzüge einer sytematischen Kraniometrie* (Stuttgart: Verlag von Ferdinand Enke 1890).

113 Harris Hawthorne Wilder, *A Laboratory Manual of Anthropometry* (Philadelphia: P. Blakiston's Son & Co. 1920) 5-6.

114 G. Sergi, *The Mediterranean Race. A Study of the Origin of European Peoples* (London: Walter Scott 1909) 36.

115 Duckworth, *Morphology and Anthropology,* 234.

116 Wilder, *A Laboratory Manual of Anthropometry,* 6.

117 Topinard, *Anthropology*, 206.

118 Duckworth, *Morphology and Anthropology,* 234; Boas "Some Criticisms of Physical Anthropology," 103.

But the place of the morphological method became precarious if physical anthropology was to develop into an *exact* science centered on numerical data alone.[119] Topinard wrote that:

> However striking certain characteristics furnished by the eye and the forms thus recognised may be *a priori*, both are insufficient to lay the foundation of an exact science…The traits of character so judged are entirely individual in the majority of cases, and their estimate depends upon the mental disposition of the observer, as well as upon the accurate recollection of his latest visual impressions. These can only be committed to writing in a very imperfect way. According to the way in which the light falls upon the skull so do appearances vary, and Monsieur Broca is daily exhibiting to his pupils the fallacies to which any one of these characteristics, looked upon by craniology as of the highest importance, may be exposed.[120]

Morphological observations were hard to standardize, control, or replicate.

But problems with metrical methods had also begun to accumulate towards the late 19th century. Quantification came with its own problems of inaccuracy, such as personal equation. This term appeared in the early 19th century to describe the worrisome fact that astronomers recorded different transit times while observing the same events. Experimental psychologists then began investigating the differences in sense data between observers.[121] By the late 19th century, personal equation had become a topic of concern in racial science: researchers taking the same measurement on the same body often produced slightly different results. While total elimination of such inherent bias was impossible, standardization of measuring methods would greatly assist and guide researchers on *how* to take measurements in exactly the same way.[122] It turned out to be incredibly

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119 Vogt, *Lectures on Man,* 22.

120 Topinard, *Anthropology*, 217.

121 Simon Schaffer, "Astronomers Mark Time: Discipline and the Personal Equation," *Science in Context* 2:1 (1988) 115–45; Henrika Kuklick, "Personal Equations: Reflections on the History of Fieldwork, With Special Reference to Sociocultural Anthropology," *Isis* 102:1 (2011) 1–33; Jimena Canales, *A Tenth of a Second: A History* (Chicago: University of Chicago Press 2010).

122 Wilder, *A Laboratory Manual of Anthropometry,* 7-8; M.L. Tildesley, "Racial Anthropometry: A Plan to Obtain International Uniformity of Method," *The Journal of the Royal Anthropological Institute of Great Britain and Ireland* 58 (1928) 351–62; Standardization Committee, "109. The International Committee for Standardization of the Technique of Physical Anthropology. A General Statement of Aims and Methods," *Man* 34 (1934) 83–86.

difficult, however, to find agreement on the standardization of racial measurements, as Chapter 5 explores in depth. Indeed, there was hardly any national or international agreement on the methods of measuring living people, skulls, and racial traits and various schools of anthropology had developed their own approaches to racial research. Standardization and personal equation were thus pressing concerns within racial science at the dawn of the 20th century.

What is more, the taxonomic value of the data accumulated remained unclear and anthropologists struggled to determine which cranial characters were most important to racial differentiation. Some researchers believed that *more* measurements and data would bring greater accuracy to their work, but Topinard warned that there was "a great danger of exaggeration in making craniometrical measurements" and a "tendency to run into minutiae."[123] Von Török's textbook with thousands of measurements and indices exemplified such metrical excess. Others subordinated everything to the study of a single measurement, such as the cephalic index. According to Boas, anthropologists who limited their work to a mechanical application of measurements or a single measurement, did not apply the metric method in the correct way:

> It must be borne in mind that measurements serve the purpose only of sharper definition of certain peculiarities, and that a selection of measurements must be adapted to the purpose in view. I believe the tendency of developing a cast-iron system of measurements, to be applied to all problems of physical anthropology, is a movement in the wrong direction. Measurements must be selected in accordance with the problem that we are trying to investigate….Measurements should always have a biological significance.[124]

Which measurements were most fundamental to racial differentiation, however, remained unsettled: "we have not been able to find any criterion by which an individual skeleton of any one race can be distinguished with certainty from a skeleton belonging to another race."[125] English physician Charles

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123 Topinard, *Anthropology*, 224.

124 Franz Boas, "Some Criticisms of Physical Anthropology," 103-104.

125 Franz Boas, "Some Criticisms of Physical Anthropology," 99.

Myers wrote in 1903 about anthropometry and racial science that "people have so far been searching vainly for the philosopher's stone, the ideal test, the infallible measurement."[126]

Myers and Boas also highlighted another problem with the metrical approach in racial science: it lacked "rigid" statistical methods. When using measurements to differentiate races, anthropologists and anatomists generally reduced the data with simple statistical procedures. Averaged measurements played a central role: where photographs of type-like individuals visually represented the racial type, the mean numerically captured the characteristics of the type. In distribution curves, researchers assumed that the peaks reflected races – a curve with two peaks indicated the presence of two races.[127] Even though some anthropologists recognized that within-group variation was a racial characteristic, in practice, generally little attention was paid to the variation within samples. Measurements were approached from the point of view of morphology, not from the science of statistics.[128] Furthermore, textbooks suggested sample sizes of at least 20 adults per sex, but researchers could not always obtain sufficient participation or find that many skeletons and often ended up using smaller samples.[129]

By the turn of the 20th century, the call for more thorough statistical approaches in the study of racial types surged. With its new methods of studying large numbers, researchers believed that statistics held the promise of metrical accuracy that other approaches to the study of human variation lacked. For Boas, anthropology was not the study of individuals but of local varieties and therefore researchers should measure the distribution of human forms of *groups* rather than classifying *individuals*. These distributions not only brought out the prevalent type through averaged

[126] C.S. Myers, "The Future of Anthropometry," *The Journal of the Royal Anthropological Institute of Great Britain and Ireland* 33 (1903) 39.

[127] See for instance C.B. Davenport and J.W. Blankinship, "A Precise Criterion of Species," *Science* 7:177 (1898) 685–95.

[128] Duckworth, *Morphology and Anthropology,* 257-8.

[129] Van der Sande, for instance, measured 40 men in Dutch Papua New Guinea, but only 3 women. G.A.J. van der Sande, *Nova Guinea,* 328.

measurements, but also revealed the character of the variation within the group. An unequal or irregular distribution, meaning one that did not follow a symmetrical bell curve, could show, for instance, whether a group was undergoing changes or whether the type was "dishomogeneous." "These facts are very strong arguments for the assumption of a great permanence of human types," Boas claimed.[130] Investigations of this character required the measurement of very extensive series of individuals and "rigid statistical methods." "Anthropological classification must be considered as a statistical study of local or social varieties," Boas concluded.[131]

Myers determined in 1903 that anthropometry had fallen into disrepute: time and again, the wide variation between individuals obstructed the racial identification of bones through measurements, and as a result, researchers had begun to distrust the metrical method. Like Boas, Myers blamed the misuse of the metrical method and the sole reliance on a single average index obtained from a handful of skulls: there was little doubt that the many-peaked curves resulted from an insufficient number of measurements, not racial mixing. With bigger samples, the curves would have likely smoothed out. "Anthropometry has become well nigh sterile by its persistence in one sole line of research after racial averages. Its activity can only be revived by the infusion of new blood, the adoption of improved methods, the pursuit of new problems," Myers proclaimed. The way forward was with the analysis of *more* data with *statistical* methods. "If physical anthropology is to be a science, its results *must* be capable of expression in mathematical formulae…the study of living forms is passing from the descriptive to the quantitative aspect, and it is by experiment and observation on biometrical lines that future progress is clearly promised."[132]

130 Boas argues here for the prevalence of heredity over environment in the distribution of types, an argument that he would counter in the 1910s in Franz Boas, "Changes in the Bodily Form of Descendants of Immigrants," *American Anthropologist* 14:3 (1912) 530–62.

131 Franz Boas, "Some Criticisms of Physical Anthropology," *American Anthropologist NS* 1:1 (1899) 98–106, quotations on pages 100 and 104. See also Franz Boas, "A Precise Criterion of Species.," *Science* 7 (1898) 860–61.

132 C.S. Myers, "The Future of Anthropometry," 36–40, quotations on pages 38 and 40.

Myers and Boas pointed towards a new player on the anthropological stage that could give racial science new life: Karl Pearson. With newly developed statistical and biometrical methods, Pearson provided novel means to analyze racial data, and, Boas and Myers urged, had begun to show how anthropology could develop into a quantitative, exact science. The next chapter unravels Pearson's program for physical anthropology and racial science.

Chapter 2

Pearsonian Anthropology. Biometric Interventions in Anthropology's Data Practices

Introduction: Race as a Metrical Aggregate

On the wall of Karl Pearson's Biometric Laboratory at University College London a phrase was printed, that read: "I will accept nothing as fact which cannot be measured or demonstrated mathematically."[1] Pearson, an important early figure in biometry, was one of the first to apply mathematical statistics to questions of race in the early 20th century. From 1902, his lab amassed large collections of skulls and developed a durable and consistent approach to "reducing" morphological race to statistical data, to transforming skulls into mathematically simpler and modified forms. This biometric "scheme" studied race through statistical formulas, means, standard deviations, probable errors, and correlations. For the biometricians, race lay in the aggregate, not in ideal types. These biometric transformations came at a time of crisis: a lack of standardized measurements, an accumulation of data without clear taxonomic value, and a variety of racial

[1] Recalled by Wilton Krogman in *The Yearbook of Physical Anthropology* (New York: Viking Fund 1946) 16.

schemes had created a distrust of anthropological measurements. Indeed, the biometric approach to racial research was a major challenge to the conventional anthropological methodology. Pearson and his colleagues considered these practices deeply inadequate: they relied on the "unscientific" and idiosyncratic morphological method, very small samples, and "kindergarten" arithmetic. With statistical methods, the biometricians desired to make anthropology more "scientific," rigorous, and precise. They argued that novel insights into man's racial history should depend on strong statistical methods, not the researcher's subjective wisdom or intuition. Their research innovatively debunked common anthropological assumptions such as the existence of pure races and the relationship between skull size and intelligence by demonstrating that the numbers simply revealed otherwise. But fundamentally, this was a transformation from within: biometricians wanted to *improve* racial research, not *overthrow* it. They incorporated new statistical methods and insights into the existing project of mapping human evolution and racial classification.

Below, the chapter opens with a discussion of the history of statistics and the development of the Biometric Laboratory. It then examines the lab's racial research practices: how did the biometricians obtain skulls and data for their research? Which methods did they develop for analyzing racial differentiation? The final section discusses how Pearson and his colleagues challenged common anthropological notions in their racial research, sometimes in controversial ways. The chapter thus reveals the extent to which the biometrician's view of and approach to race differed from other anthropologists.

The Rise of Statistics: Quetelet, Galton, Pearson, and the Biometric Laboratory

Statistics, as the origin of the word reminds us, emerged in the context of state administration and the survey of individuals and their behavior in the 18th century. Early statisticians collected demographic and economic information and used averages to regulate and predict the behavior of state subjects. Thus, they developed statistical methods to "objectify the social world," to abstract individual variation and reduce it to "things that hold" like aggregates.[2] In the second half of the 19th century, statistics transformed from an administrative practice into a specialized branch of mathematics, especially in England. Central figures such as Francis Galton, Karl Pearson, and Ronald Fisher laid the foundations of present-day mathematical statistics between 1890-1930. This approach did not necessarily arise in mathematics departments, as one may expect. Historian Theodore Porter points out that statistics' historical development was notably interdisciplinary. Biology played an especially important role: "The quantitative study of biological inheritance and evolution provided an outstanding context for statistical thinking, and quantitative genetics remains the best example of an area of science whose very theory is built on the concepts of statistics." The statistical study of biological variation and heredity, termed *biometry*, led to the development of several important statistical tools.[3]

Scientists began studying topics like averages and aggerates around the mid-19th century. In surveying aggregates of individuals, Belgian astronomer Adolphe Quetelet observed that measurements such as births, marriages, deaths, and physical traits produced a similar distribution: a "normal curve" with most measurements clustered around the average. Quetelet argued that the curve exposed a deep social order beneath a population's diversity. He invented a new concept, an ideal average man or *homme moyen* who embodied all average characteristics of a population, physical

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2 Alain Desrosières, *The Politics of Large Numbers: A History of Statistical Reasoning* (Cambridge, MA: Harvard University Press 1998) 9; 67.

3 Theodore M. Porter, *The Rise of Statistical Thinking, 1820-1900* (Princeton: Princeton University Press 1986) 270. See also Stephen M. Stigler, *The History of Statistics: The Measurement of Uncertainty Before 1900* (Cambridge, MA: Harvard University Press 1986); Ian Hacking, *The Taming of Chance* (Cambridge, UK: Cambridge University Press 1990); Gerd Gigerenzer et al, *The Empire of Chance: How Probability Changed Science and Everyday Life* (Cambridge, UK: Cambridge University Press 1997).

and moral, and stood forth as the Creator's symbol of perfection.[4] Normal variation was further explored by Francis Galton, who researched major statistical questions from the 1860s to 1880s, occasionally with the help of mathematicians. Although Quetelet saw human variation as "error" of the average ideal, Galton understood it as a law of deviation that enabled the classification of individuals. The study of deviation also led him to determine his law of regression, which posited that for any given trait, offspring were less "extreme" than their parents and "regressed" towards the mean or mediocrity. His cousin Charles Darwin's work on evolution inspired him to measure human traits and research heredity as a way to influence the effects of regression and improve the population as a whole. Galton termed this field of study *eugenics* in 1883.[5]

While Galton was not a mathematician, his protégé and disciple Karl Pearson was. After taking the mathematics Tripos at Cambridge in 1879, Pearson studied physics and metaphysics in Germany and developed a strong interest in history and Darwinist theory. In the 1880s, Pearson returned to mathematics. He was appointed chair of Applied Mathematics and Mechanics at University College in London in 1884 and held a professorship in Geometry at London's Gresham College from 1891-1894. Through his contacts with Gresham colleague and Professor of Zoology W.F.R. Weldon, he became very interested in the statistical study of biological problems and began reading Galton's work on eugenics and heredity. Pearson quickly became convinced of its importance but wished to improve Galton's approach. He further developed Galton's mathematical ideas and continued his research on inheritance.[6] In the 1890s, he started collaborating, publishing,

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[4] Alain Desrosières, *The Politics of Large Numbers*, 73-77.

[5] Francis Galton, *Inquiries into Human Faculty and its Development* (London: Macmillan & Co. 1883); Francis Galton, "Section H. Anthropology. Opening Address by Francis Galton," *Nature* 32:830 (1885) 507–10. For more on Quetelet and Galton, see Elise Smith, "'Why Do We Measure Mankind?' Marketing Anthropometry in Late-Victorian Britain," *History of Science* (2019) 1–24.

[6] For an extensive discussion of Pearson's life and scholarly development, see Theodore M. Porter, *Karl Pearson: The Scientific Life in a Statistical Age* (Princeton: Princeton University Press 2004).

and lecturing on topics such as heredity and evolution, craniometry, and racial history.[7] These interests led him to develop the new field *biometry*. Pearson's biometry was more concerned with the exact measurement and description of heredity and variation than theorizing or explaining the underlying mechanisms. The problems of genetics and evolution, according to Pearson, were "in the first place statistical, in the second place statistical, and only in the third place biological." This position brought him into heated debates with the so-called "Mendelians," who explored the inheritance of discontinuous traits through experimental studies with, for instance, fruit flies.[8]

Pearson's "Biometric school" and Laboratory developed concurrently and grew into the most important representative of this new branch of study in Great Britain. "The so-called Biometric Laboratory started about 1890 in a course of lectures on statistics which I gave to one or two members of the staff of the Department of Applied Mathematics and one or two voluntary research workers," Pearson wrote in a report in 1927. These lectures were not part of his professorial duties, but as his research on biometry progressed and his lectures attracted increasing amounts of students, a "biometric school" began to develop.[9] An annual grant from the Worshipful Company of Drapers financed the Biometric Laboratory from 1903 onwards.[10] Work in the laboratory centered on the collection and reduction of data and the development of tables, graphs,

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7 Pearson's first paper on the subject is Karl Pearson, "Mathematical Contributions to the Theory of Evolution. Iii. Regression, Heredity, and Panmixia," *Philosophical Transactions of the Royal Society of London* 187 (1896) 253–318, in which he determined the correlation of skull parts.

8 Quote from a letter from Pearson to Galton, 12.2.1897, quoted in Gerd Gigerenzer et al, *The Empire of Chance*, 147. See for a discussion of the historiography of this debate and a refreshing new take: Theodore M. Porter, "The Curious Case of Blending Inheritance," *Studies in History and Philosophy of Science Part C: Studies in History and Philosophy of Biological and Biomedical Sciences* 46 (2014) 125–32. On the history of Mendelism and genetics see: Robert E. Kohler, *Lords of the Fly: Drosophila Genetics and the Experimental Life* (Chicago: University of Chicago Press 1994).

9 Karl Pearson Archive (KPA), Box 117, 4/4/8, "Report on the Galton and Biometric Laboratories especially with regard to their Income and Expenditure."

10 The Drapers Company awarded several grants to educational institutions in London, including University College. The College decided to give the award of £1000 to Pearson, who welcomed this unexpected gift. The first Drapers grant was thus not specifically made out to Pearson, but the Company continued to fund his laboratory with £500 per year until 1932. See: L.A. Farrall, "The Origins and Growth of the English Eugenics Movement 1865-1925 (Ph.d. diss., Indiana University, 1970) 129-131.

and models for various problems in mathematical statistics. Equipped with measuring devices and Brunsviga calculating machines, the laboratory became like a "biometric registry office."[11] The lab's research was diverse and ranged from the development of statistical methods such as curve-fitting to the study of inheritance in poppies. Craniometry had a central place in the research agenda. The lab had a massive skull collection, a special room for craniometric research with specialized instruments, and a museum that exhibited the history of early man and his artefacts (fig. 2.1a and b).[12]

Figure 2.1.a. Instrument room. Source: Karl Pearson Archive, box 115, 4/1. Image courtesy of UCL Special Collections.

11 Porter, *Karl Pearson*, 261.

12 KPA, Box 117, 4/4/7, "Report for the years 1922, 1923 and 1924."

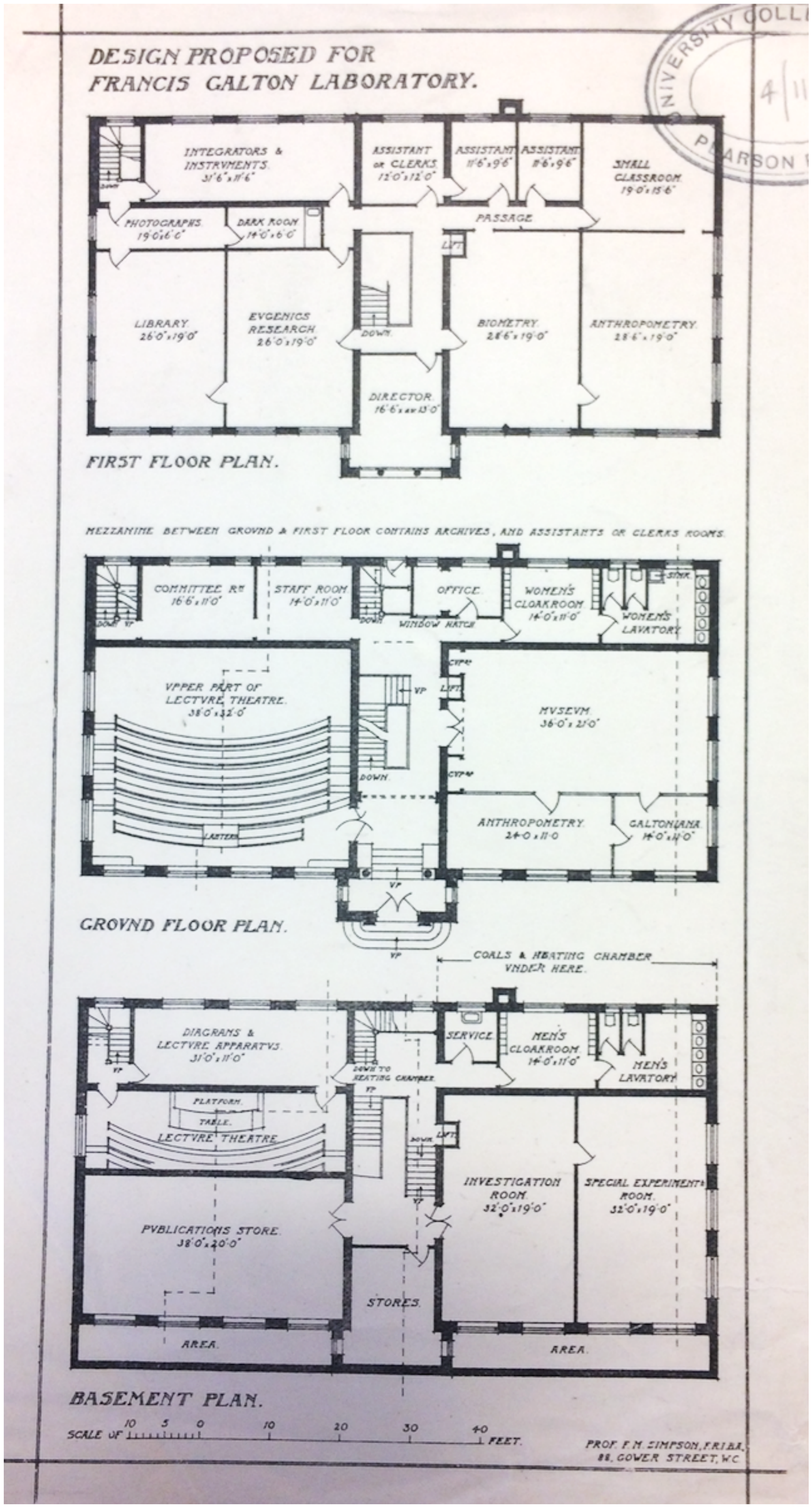

Figure 2.1.b. Proposed design for the Francis Galton Laboratory with spaces for biometry, anthropometry, and a museum. Note how the design includes separate spaces for eugenics research and biometry. Source: Karl Pearson Archive, Box 117, 4/11/1. Image courtesy of UCL Special Collections.

The "gospel" of biometry[13] spread through two channels. Besides a steady influx of British students, the laboratory attracted scholars from America, Europe, China, India, South Africa, and Japan. Some of these foreign "student visitors" had academic appointments in zoology, psychology, or mathematics and set up their own biometric laboratories upon return home, such as Indian statistician Prasanta Mahalanobis.[14] Indeed, teaching was an important aspect of life at the biometric laboratory. Pearson made daily rounds consulting his students and workers and often rewrote their papers to ensure that the lab's work was coherent and of the highest quality.[15] The journal *Biometrika*, founded in 1902, was another important medium for Pearson's biometry. In the editorial of the journal's first volume, Pearson wrote that "the spirit of *Biometrika*" revolved around the statistical study of evolution with "the mathematics of large numbers…to interpret safely our observations." Pearson wished to collect "under one title biological data of a kind not systematically collected or published in any other periodical, but also of spreading a knowledge of such statistical theory as may be requisite for their scientific treatment." *Biometrika* would "form a manuscript collection of such data available for further research" and become an archive that could be revisited at all times. The journal would ideally facilitate interdisciplinary collaborations between biologists, mathematicians, and statisticians in ways that university programs did not offer.[16]

From 1906, Pearson was also in charge of Galton's Eugenics Laboratory where researchers applied statistical methods to questions of heredity. With the founding of the Galton Professorship of Eugenics after Galton's death in 1911 and the establishment of the Department of Applied

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13 Porter, *Karl Pearson*, 7; 250.

14 KPA, Box 117, 4/4/9, "Report for the years 1925, 1926, 1927, 1928-1929."

15 Morant recalled: "He [Pearson] was always ready to do this if he found that students working under his direction were unable themselves to bring out and put down clearly the conclusions to which their investigations had led; he tried to ensure to the student the maximum fruits of his labours, but at the same time jealously guarded the library standards of his laboratory publications." G. Morant and L.B. Welch, *A Bibliography of the Statistical and Other Writings of Karl Pearson*. (London: Issued by the Biometrika Office, University College 1939) vii.

16 Karl Pearson et al, "Editorial," *Biometrika* 1:1 (1901) 1-6; Francis Galton, "Biometry," *Biometrika* 1:1 (1901) 7–10.

Statistics at University College, both laboratories were incorporated into a unified research institute under Pearson's leadership. Craniometry and racial research were mostly done at the Biometric Laboratory.

Skulls and Data at the Biometric Laboratory

The Biometric Laboratory applied statistics to a wide variety of subjects regarding evolution and heredity, but it remains largely unknown that most of that research lay within the scope of race and physical anthropology. Indeed, racial variation and man's evolutionary history became core research topics from 1902 onwards and craniometry was *Biometrika*'s most durable topic until the late 1930s.[17] For this research, the laboratory amassed a large skull collection that researchers carefully measured and reduced to racial relationships with biometric methods. The lab's craniometric focus attracted independent racial researchers, who regularly came to the lab for advice and instructions on cranial measurements. From 1911, the lab had a special fellowship for craniometric research, the Crewdson Benington studentship. This one-year stipend allowed students to conduct craniometric research on race and evolution and publish their results in *Biometrika*.[18] Pearson wrote about craniometry in a departmental report in 1929: "There is at present little limit to the extent to which this section of the work could be expanded if our funds were more adequate for the payment of permanent workers and for the purchase of material."[19]

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17 G.M. Morant, "118. Professor Karl Pearson," *Man* 36 (1936) 90; E.S. Pearson, "Karl Pearson: An Appreciation of Some Aspects of His Life and Work," *Biometrika* 29:3/4 (1938) 178; John Aldrich, "Karl Pearson's Biometrika: 1901–36," *Biometrika* 100:1 (2013) 6.

18 KPA, Box 244, 11/1/14/33 and /34, Correspondence with Mary E. Norman-Robinson and W.A. Norman-Robinson. First receiver: H. Dorothy Smith, "Observations on the Occipital Bone in a Series of Egyptian Skulls," *Biometrika* 8:3/4 (1912) 257-266. Last receiver: D.L. Risdon, "A Study of the Cranial and Other Human Remains from Palestine Excavated at Tell Duweir (Lachish)," *Biometrika* 31:1/2 (1939) 99-166.

19 KPA, Box 117, 4/4/9, "Report for the Years 1925, 1926, 1927, 1928-1929."

Skulls and race were central to Pearson's biometry from the get-go. It remains unknown where and when Pearson learned how to measure skulls and become "a master of craniometry"[20] but he introduced the study of crania and race in one of his first papers on biometry, "Variation in Man and Woman." The paper was printed in the bundle of essays *Chances of Death and Other Studies in Evolution* (1897) and challenged, with statistical evidence, the widespread assumption that men were more variable than women. Pearson argued that these claims were erroneously based on the frequency of abnormalities and pathologies among the sexes, instead of the normal variation of bodily characters. Skulls offered such a normal and random sample because "death strikes all ages, sexes, and conditions." Moreover, they were "comparatively easy of definite measurement; they vary markedly with different races; if not in themselves a test of intellectual fitness, they are the seat of the brain, and their variation may be justifiably assumed to be more or less closely correlated with those variations in the brain upon which the progressive evolution of mankind largely depends." Museums and anthropologists had already created large collections of skulls that "can be at once used for mathematical calculation." Whereas the mathematician would ideally have hundreds of skulls at his disposal, Pearson admitted that "such ideal samples are in most cases practically impossible. The craniologist has often to be satisfied with 20, 30, or 50 skulls of one race and one sex, which are all that are at his command. He is in fact delighted with 50, overjoyed with 100, and the expression of his emotions in the unique case known to me in which more than 1000 are available exceeds all description." Despite its smallness, skull samples gave a good approximation of male and female normal variation in races. Pearson concluded that variability was more a racial than a sexual characteristic, much in line with the common anthropological assumption discussed in chapter 1. In accordance with the principles of natural selection "the more intense the struggle [for survival] the less is the variability, the more nearly are individuals forced to approach the type fittest

[20] Theodore M. Porter, *Genetics in the Madhouse: The Unknown History of Human Heredity* (Princeton: Princeton University Press 2018) 241.

to their surroundings, if they are to survive. This conclusion is amply verified by the variability of civilised races being greater than that of savage races, when both are compared with regard to the same organs." Instead of a marked difference in variability between the sexes, Pearson concluded that the civilization level of races determined variability: the more "savage" the race, the lesser the variability.[21] Pearson's conclusions anticipated the Biometric alboratory's research into differing "intra-racial variation" in the years to come.

Thus, Pearson built a tight connection between biometry, the study of evolution in man, and "normal" skeletal series early on. He also built a private collection of skulls and bones at the Biometric Laboratory. In the late 19th century, he began requesting skulls from archaeologists, explorers, and military officers – with success. His contact with colleague Flinders Petrie, Professor of Egyptian Archaeology at University College, was most lucrative. Before Petrie left for an expedition to Egypt in 1894, Pearson asked him to collect, if possible, a hundred skulls for biometric study. Petrie promised to "bring every skeleton or skull of which the age can be fixed," broken or unbroken.[22] Near Naqada, a town in Upper Egypt, Flinders and his team of student assistants and local workmen began clearing over 400 graves, many of which had already been plundered by others searching for grave goods to sell. For clearing the tombs, the smallest and lightest boys were lowered down into narrow digging spaces "like a little digging machine" and dug around until they touched pottery or bones. The digging was then proceeded by Flinders and other more experienced men. Whenever a workman broke a skeleton upon excavation, Flinders immediately fired him to ensure that others would be more careful.[23] Within four months, Petrie

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21 Karl Pearson, "Variation in Man and Woman," *Chances of Death and Other Studies in Evolution* (London: Edward Arnold Publishers 1897) 256-377. Quotations from pages 258, 268, and 280.

22 KPA, Box 260, 11/1/16/94, Correspondence with Flinders Petrie, 3.11.1894; 13.11.1894.

23 W.M. Flinders Petrie and J.E. Quibell, *Naqada and Ballas* (London: William Clowes and Sons 1896) vii-viii.

collected as many as four hundred skulls, "a huge mass of material" for Pearson's "aethereal mathematics," as he called it.[24] Petrie reported to Pearson in 1895:

> When I began here I stacked skulls and bones on a broad shelf in my bedroom, with a pleasingly perfect [illegible] lying below. Soon I had to stack them in boxes and await packing. Then they overflowed and formed a heap, which encroached on our courtyard until I could hardly get into my room. Now the heap is extending daily and threatening to cut off the entrance to our visitor's room. The skulls were laid on shelves across the end of the court, but have now filled all the ornamental openings of the brick wall. And still every day more come in.[25]

Every bone was marked with the number given to the grave and packed up in specially made boxes for transport.[26] With the financial help of Pearson's brother, the collection of skulls and bones was transported from Egypt to University College in London.[27] Flinders believed he had discovered a new "cannibal race occupying upper Egypt about 3000 B.C," the prehistoric "Naqada race" with "small hook noses and strong brows." Since the cranial series showed little variability, "you could not have a better lot of material for homogeneousness and age," he wrote to Pearson. "I think it deserves to be worked up into a classical memoir in anthropology."[28]

Flinders continued to send Pearson skulls from Egypt throughout the next decades.[29] In 1910, he sent as many as 1800 prehistoric crania, excavated from a cemetery south of the Gizeh pyramids. This "E series" found permanent storage in the laboratory and provided research for decades; it took the biometricians fourteen years to complete the measurements and arithmetical

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[24] KPA, Box 260, 11/1/16/94, Correspondence with Flinders Petrie, 13.11.1894.

[25] KPA, Box 260, 11/1/16/94, Correspondence with Flinders Petrie, 1.2.1895. Also quoted in Debbie Challis, "Skull Triangles: Flinders Petrie, Race Theory and Biometrics," *Bulletin of the History of Archaeology* 26:1 (2016) 4.

[26] KPA, Box 260, 11/1/16/94, Correspondence with Flinders Petrie, 1.2.1895; 7.3.1895.

[27] Cicely D. Fawcett and Alice Lee, "A Second Study of the Variation and Correlation of the Human Skull, With Special Reference to the Naqada Crania," *Biometrika* 1:4 (1902) 411.

[28] KPA, Box 260, 11/1/16/94, Correspondence with Flinders Petrie, 1.2.1895.

[29] Geoffrey Morant Papers (GMP), Durham UK, Karl Pearson to Geoffrey Morant, 8.9.1920; KP to GM 16.7.1935. In 1935, Pearson writes to Morant: "It may as well be asserted that all the Egyptian series we have are due to the individual assistants like Engelhof, Sir G. Thompson, etc., who dug them up as part of Petrie's expedition."

work on the skulls. Petrie's donations thus provided the lab with uniquely long series of skulls, "long enough to give really adequate representations of a statistical population."[30] Historian Debbie Challis concludes that the Petrie collection helped to establish the Biometric Laboratory;[31] indeed, it formed the foundation for many racial studies and the development of various statistical tools.

This was not the only "skeleton business"[32] that Pearson was involved in: his skull collection also grew with the development of London. As London expanded, old church cemeteries, plague pits, and crypts were discovered, opened, and cleared. University College's anatomical department procured the bones dug-up in London and often made them available to Pearson's lab. One such area of construction was the neighborhood Whitechapel, where old houses were demolished and excavations undertaken to lay the foundations of new buildings. In 1893, the construction of a whiskey store revealed an old bone yard with five to six hundred scattered skeletons, quickly acquired for scientific study. Pearson investigated the series with W.R. Macdonell, an "early member of the Biometric School."[33] No archaeological inspections were done at the time of discovery, so Pearson and Macdonell turned to historical maps and plans from the British museum to research the history of the cemetery and characterize the material found. Using their literary and historical skills, Pearson and Macdonell suggested that the burials dated from the 17th century, "most probably from the time of the Great Plague" of 1665-6.[34] University College anatomy Professor George Thane also granted access to the "Moorfield crania." During excavations for a public urinal in Liverpool street in 1903, workers discovered a collection of bony remains. Again, no on-site investigations were done

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[30] Karl Pearson and Adelaide G Davin, "On the Biometric Constants of the Human Skull," *Biometrika* (1924) 329-30; GM Morant, "A Study of Egyptian Craniology from Prehistoric to Roman Times," *Biometrika* 17:1-2 (1925) 1.

[31] Debbie Challis, "Skull Triangles," 6-7.

[32] KPA, Box 260, 11/1/16/94, Correspondence with Flinders Petrie, 1.2.1895.

[33] Macdonell joined the Biometric lab after retiring as a businessman. He studied Pearson's new biometric techniques and acted as assistant editor of *Biometrika* for many years.

[34] W.R. Macdonell, "A Study of the Variation and Correlation of the Human Skull, With Special Reference to English Crania," *Biometrika* 3:2/3 (1904) 191–244, quotation on page 196.

because the workers had stacked the bones in piles. Pearson and Macdonell returned to historical maps and determined that the workers had revealed another 17th century plague pit in Moorfields.[35] The Whitechapel and Moorfield series came to represent a 17th century "English type" and regularly appeared in racial comparative studies in *Biometrika*.

By the 1920s, biometricians visited excavation sites themselves in order to secure collections and gather better on-site data regarding age.[36] In 1924, Pearson's employee Geoffrey Morant monitored excavations for new buildings in Farringdon Street because the site was "densely packed with human remains." Workers unearthed a graveyard with a "jumbled mass" of 600 skeletons piled against older building foundations. The "Farringdon Street crania" provided another sample of the 17th century English type. A few years later, the extension of the Spitalfields Market brought to surface the skulls of 950 individuals. Again, Morant visited the site for over a month to secure the collection for the laboratory.[37]

Because the lab was in competition with Oxford and Cambridge for these exhumed remains,[38] getting to the site first was an urgent matter. When a flower market was planned at the Spitalfields Market in 1933, Pearson immediately asked the City Surveyor to authorize his lab to examine any "possible finds of Archaeological interest on the site." His request was granted and in 1934 the architect informed Pearson that excavations had commenced. Pearson urged Morant to visit the site at once, "You need to be <u>first</u> on the field!" He told Morant to bring cash: "I believe you would get a better collection on the part of the workmen, if 1/- a dozen were offered to the

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35 W.R. Macdonell, "A Second Study of the English Skull, With Special Reference to Moorfields Crania," *Biometrika* 5:1/2 (1906) 86–104.

36 The form of burial, depth of the grave (including geological stratification), storage of corpses, and presence of grave goods could provide information about age of the skeletons. See Martin, *Lehrbuch*, 29-30.

37 Beatrix G.E. Hooke, "A Third Study of the English Skull With Special Reference to the Farringdon Street Crania," *Biometrika* (1926) 1–55; G.M. Morant and M.F. Hoadley, "A Study of the Recently Excavated Spitalfields Crania," *Biometrika* 23:1/2 (1931) 191–248; GMP, KP to GM 17.9.1930.

38 Porter, *Karl Pearson*, 263.

workmen for skulls. This would not amount to more than £5 if there were 1000." He ordered that the skulls were to be kept near the site "as we expect that more than one burial ground will be involved and photographs of bodies in situ ought to be taken to determine nature of burial. Also buttons, pipes or pottery to determine dates."[39]

The publications discussing the Whitechapel, Moorfield, Farringdon Street, and Spitalfields crania often covered "the whole story" of how the lab determined the racial origins, homogeneity, age, and sex of the skeletal finds. They cited state records and church minute books and reprinted historical maps to identify historic burial grounds and trace their histories (fig. 2.2). These papers demonstrate the interdisciplinary nature of race research at the lab: biometricians combined historical and literary skills with anatomical, anthropological, and statistical expertise.

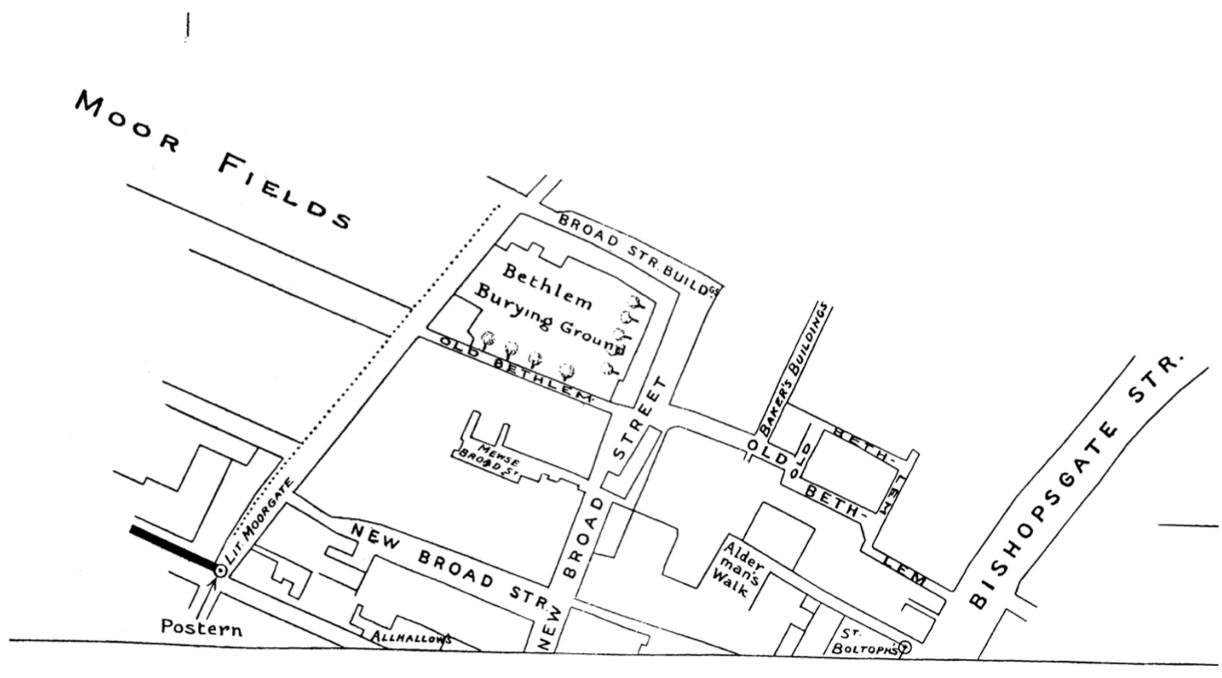

Figure 2.2. Location of Moorfield sample traced from a map of 1746. Republished with permission of Oxford University Press from W.R. Macdonell, "A Second Study of the English Skull, With Special Reference to Moorfields Crania," *Biometrika* 5 (1906) 88.

[39] GMP, KP to GM 19.4.1934.

Pearson's laboratory thus became a "storehouse" for skulls. When he died in 1936, he left a collection of more than 7000. Morant claimed in 1936 that his collection was "one of the largest in the world, and its English and Egyptian series are far longer than any other preserved."[40] Occasionally, the lab had skeletal series on loan or received collections promised to other institutions first.

Before the lab's workers could measure the skulls they received, they first needed to clean them. Bones and skulls retrieved from the earth were often too soft and fragile to be cleaned on-site and first needed to be dried in the sun. They were then individually wrapped, placed in larger boxes, and transported to the laboratory where they could be carefully cleaned. With softer and harder brushes, the lab's workers dusted off sand and mud and pulled roots and earth from the skull cavity with a small hook. The skulls often remained too fragile to be rinsed with water. Fragmented bony parts were glued back together with a thin glue solution.[41] The lab's workers spent hours on these preservation practices. Morant wrote to Pearson about his cleaning of a collection of East African skulls: "they are by no means free from mud now, but it would not be possible to much more without soaking them, which would be rather dangerous. This operation has taken 51 hours and I have rather grudged the time." Once cleaned, the skulls were stored in cupboards, special "skull stores," and in the lab's museum.[42]

Besides building his own cranial collection, Pearson relied on the collections and measurements of others for his racial research. He developed connections with anthropologists and anatomists across the globe, as evidenced by his correspondence and the acknowledgements in *Biometrika*'s craniometry papers. Through Pearson's relations with anthropologists in and around

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[40] E.S. Pearson, "Karl Pearson: An Appreciation of Some Aspects of His Life and Work," *Biometrika* 29:3/4 (1938) 215-216; G.M. Morant, "118. Professor Karl Pearson," *Man* 36 (1936) 89.

[41] Martin, *Lehrbuch*, 29-32.

[42] GMP, KP to GM 16.7.1921; GM to KP 8.8.1930; KP to GM 16.7.1935.

London, Pearson and his lab's affiliates had access to various other collections of bones and skulls. William Henry Flower, director of London's Natural History Museum, invited Pearson to come measure a set of "Negro skulls" in his collection. Anthropologists Dudley Buxton and W.H.L. Duckworth connected Pearson to the collections of the Oxford University Museum and the Cambridge Anthropological Laboratory. Several students worked with anthropologist Sir Arthur Keith at the Royal College of Surgeon's Hunterian Museum. Sometimes gifts came from abroad: Swiss-German anthropologist Rudolf Martin had a case of a femur sent to Pearson from Zurich.[43]

For most studies, however, biometricians did not need to measure the bones themselves but instead relied on measurements produced by others. A personal collection of skulls always remained restricted in size, but printed numbers provided the mass of data needed for statistical analysis. In fact, the data itself were more important than the bony objects they represented: biometricians were mainly concerned with reducing morphological race to statistical data. For that project, they engaged in "data-mining" *avant la lettre*, mining journal articles and craniological catalogues, and asking befriended anthropologists for racial data to reuse and reduce with new statistical methods. Again, Pearson's network came in handy. As data was easier to ship than bones and skulls, Pearson could broaden the scale and scope of his requests. French anthropologist Léonce Manouvrier sent Pearson data of a thousand French skulls from the catacombs, measured by himself and Paul Broca, as well as data of modern "Negro" skulls.[44] In the United States, anthropologist Thomas Wingate Todd at the Anatomical Laboratory of Western Reserve University shipped data on thousands of skulls,

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[43] KPA, Box 229, 11/1/2/151, correspondence with Dudley Buxton; Box 233, 11/1/4/51, correspondence with Duckworth, KPA, Box 236, 11/1/6/29, correspondence with Flower; Box 240, 11/1/11/8, correspondence with Keith; 11/1/13/48 correspondence with Martin, Martin to KP 7.7.1919.

[44] Karl Pearson, "On Some Application of the Theory of Chance to Racial Differentiation. From the Work of W.R. Macdonell, and Cicely D. Fawcett," *The London, Edinburg and Dublin Philosophical Magazine and Journal of Science* 6:1 (1901) 110–24; Karl Pearson, "Craniological Notes. Explanatory. I. Professor Von Török's Attack on the Arithmetical Mean. II. Homogeneity and Heterogeneity in Collections of Crania. III. With S. Jacob and a. Lee: Preliminary Note on Interracial Characters and Their Correlation in Man," *Biometrika* 2:3 (1903) 338–56; R. Crewdson Benington and Karl Pearson, "A Study of the Negro Skull With Special Reference to the Congo and Gaboon Crania," *Biometrika* 8:3/4 (1912) 292–339.

skeletons, and brains of various races and anthropoids. Biologist Raymond Pearl mailed Pearson his data on brain-weights. In 1895, Pearson asked Franz Boas to help him collect data for his research on family heredity. A few years later, Pearson received two packages of a thousand anthropometric cards with measurements of Native Americans.[45] He also made appeals for data in journals. In 1908, he asked the readers of the *British Medical Journal* to continue sending him cases of albinism in man, having already received pedigrees covering nearly "500 albinotic stocks" from "many medical men in various parts of the world."[46] A year later, he asked *Biometrika*'s subscribers to send him any data or photographs of mixed race individuals.[47] Like his heredity studies, Pearson collaborated with various scholars and institutions that held racial data of interest to him.[48]

Reusing *published* racial data, however, did not require any interactions with or consent from authors and measurers. Rather than waiting for packages with data to be shipped across the Atlantic, published data was immediately available to biometricians. Indeed, "innumerable anthropometric, including craniometric measurements, had been made and published but very little had been done in determining scientifically their statistical constants," the lab's biometricians Cicely Fawcett and Alice Lee concluded in 1902.[49] Craniological catalogues were especially convenient for reuse, such as Flower's catalogue of the Royal College of Surgeons Museum skull collection.[50] Biometricians also retrieved data from journal articles and monographs. German anthropologist Johannes Ranke's *Beiträge zur physischen Anthropologie der Bayern* (Munich: Riedel 1883) and Japanese anthropologist

45 KPA, Box 269, 11/1/19/45, correspondence with Wingate Todd; KPA, Box 245, 11/1/16/19, correspondence with Pearl, date 23.1.1905; KPA, Box 228, 11/1/2/83, correspondence with Boas, dates 22.7.1893; 14.4.1987; 20.5.1897; 14.10.1897; 22.11.1897; 4.12.1897.

46 Karl Pearson and E. Nettleship, "Albinism in Man," *British Medical Journal* 2:2487 (29 Aug. 1908) 625.

47 Karl Pearson, "Note on the Skin-Colour of the Crosses Between Negro and White," *Biometrika* 6:4 (1909) 348–53.

48 Porter, *Genetics in the Madhouse*, 224.

49 Cicely D. Fawcett and Alice Lee, "A Second Study of the Variation and Correlation of the Human Skull," 409.

50 William Henry Flower, *Catalogue of the Specimens Illustrating the Osteology and Dentition of Vertebrated Animals, Recent and Extinct: Contained in the Museum of the Royal College of Surgeons of England* (London: Taylor and Francis 1879).

Koganei Yoshikiyo's *Beiträge zur physischen Anthropologie der Aino* (Tokyo: Verlag der Universität 1893), for instance, contained long lists of measurements ordered per skull. These numbers could easily be reduced to means, probable errors, and other statistical constants and gave the lab data on "a highly civilised modern race and an extremely primitive race."[51]

Through published measurements, Pearson's own cranial collection, and the datasets he received through his friendly network, the lab produced a large racial database. An article Pearson wrote with Adelaide Davin, a worker of the lab and former Crewdson Benington student, shows this racial database at work. The table in figure 2.3 shows the various racial constants of the occipital index, a new biometric index that measured the development of the cerebellum, a region of the brain associated with motor-coordinating capacity. High curvature or a low index suggested higher development. The footnotes demonstrate the breadth of collections used to produce the table: museum collections, craniological catalogues, the cranial collection of the Biometric lab, as well as various publications, especially German studies. Fawcett and Lee explained in 1902 that German anthropologists had published "by far the largest mass of material yet measured by a nearly uniform system." German publications were therefore ideal for the data-mining and -collecting.[52]

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[51] Cicely D. Fawcett and Alice Lee, "A Second Study of the Variation and Correlation of the Human Skull," 428. The biometricians considered these samples homogeneous, because it was "known" that these populations had been isolated for a number of generations and had freely interbred. See: Karl Pearson, "Craniological Notes: Remarks on Professor Aurel von Torok's Note," *Biometrika* 2:4 (1903) 511.

[52] Karl Pearson and Adelaide G. Davin, "On the Biometric Constants of the Human Skull," *Biometrika* (1924) 332, 334-335; Fawcett and Lee, "A Second Study of the Variation and Correlation of the Human Skull," 412-13. J.B.S. Haldane wrote in 1957 about Pearson and Davin's study that "in my opinion nothing since written on human craniometry has in anyway superseded [this] great memoir." See: J.B.S. Haldane, "Karl Pearson, 1857-1957," 310.

Species or Race, etc.	Number	Male	Number	Female
Old World Monkeys, various ...	—	♂ + ♀ 7	69·3	69·0
New World Monkey, Ateles ...	—	— 1	67·3	
Gibbons*, various. Adult	—	♂ + ♀ 5	67·9	
„ Adolescent	—	♂ + ♀ 6	77·5	73·8
„ Young	—	♂ + ♀ 5	75·2	
Orangs*, (all ages)	—	♂ + ♀ 8	64·2	—
Chimpanzees*, Adult	—	♂ + ♀ 18	62·9	
„ Adolescent	—	♂ + ♀ 6	60·1	62·5
„ Young...	—	♂ + ♀ 8	63·2	
Gorillas*, Adolescent and Young ...	—	♂ + ♀ 10	61·0	—
Rhodesian Man†	—	68·0	—	—
Piltdown Skull	—	59·2 (??)	—	—
La Chapelle Man	—	61·0	—	—
Homo mousteriensis (*Hauseri*) ...	—	58·3 (??)	—	—
Předmost Crania	—	63·4	65·3	—
Gibraltar Skull	—	—	59·5	—
Homo aurignacensis (*Hauseri*) ...	—	58·4	—	—
Engis Skull	—	57·1	—	—
Tilbury Skull	—	56·2	—	—
Cromagnon Skull	—	56·5	—	—
Short Cist Skull	—	60·5	—	—
Neolithic, Trent Skull	—	59·4	—	—
„ Langwith Cave	—	57·2	—	—
Gaboon[1] (1864)	49	68·3	44	69·2
Gaboon[1] (1880)	15	66·3	19	69·5
Congo[1]	48	68·8	27	67·0
Juvenile Negroes[1]	5	66·7	—	—
Angoni[2], Nyassaland	15	60·7	8	62·7
Cameroon[3]	83	65·6	—	—
Modern Abyssinia[4] (Tigre) ...	57	62·0	23	61·8
„ „ (Galla and Somali)	8	62·2	—	—
„ „ (Kohaito) ...	17	59·4	8	59·6
Bushmen[5]	9	59·6	13	58·7
Egypt, Pygmy Crania[6]	—	—	12	63·3
„ "Middle" Dynasties[7] ...	30	62·3	—	—
„ "Early" Dynasties[8]... ...	66	62·0	—	—
„ 26th to 30th Dynasties[9] ...	885	61·5	592	61·98
„ 18th to 20th Dynasties[10] ...	54	61·4	—	—
„ 18th Dynasty[11]	25	60·6	—	—
„ Naqada, B, T, and R Graves[12]	23	60·5	—	—
„ Naqada, A and Q, Graves[13].	84	60·2	—	—
„ 1st Dynasty, Abydos[14] ...	32	59·9	—	—
Egyptians: Total unweighted ...	—	61·05	—	—
„ Total weighted ...	1199	61·4	—	—
Bismarck Archipelago[15]	43	64·7	34	65·3
Santa Crux Islanders[16]	27	60·0	22	59·9
Solomon Islanders[17]...	♂ + ♀ 43	63·0		—
Moriori[18]	32	62·2	20	64·2
Maori (Weisbach)[19]	15	61·1	—	—
„ (Mollison)[20]	♂ + ♀ 16	64·7		—
Aborigines (N. S. Wales)[21]	21	60·1	11	63·8
Typical Australian Skull[22]	1	60·2	—	—

* The development of the occipital ridge in the anthropoids obscures this index. All the calvarial sutures seem to close completely very soon after the eruption of the third molars, and the determination of the lambda becomes in the case of adult gorillas impossible, one adult gorilla gave K. Pearson a very doubtful 69·6. "Adolescent" have all teeth, but third molars and canines fully erupted, "Young" are those with no 3rd molars or canines and with 2nd molars not fully erupted. The material does not suffice to separate local varieties. Sir Arthur Keith had in most cases determined age and sex.

† The measurements of the palaeolithic and neolithic crania are based on casts, and in several cases the reconstructions involved in preparing these are open to grave doubt. Professor Macgregor's complete reconstruction of Heidelberg man from the Mauer jaw gives an occipital index of 55·0, which seems to us quite out of the question.

References. [1] *Biometrika*, Vol. VIII. pp. 298—300, Table II. [2] Brit. Mus. (Nat. Hist.). [3] *Archiv f. Anthropologie*, Bd. XL. S. 176. [4] Sergi: *Crania Habessinica*, pp. 306, 395. [5] R. C. of S.s' Museum. [6] *Biometrika*, Vol. VIII. p. 265. [7] Toldt: *Denksch. d. k. Akad. d. Wissensch. Wien, Math. Nat. Kl.* Bd. XCVI. Tafeln S. 51–6. [8] *Idem.* [9] Present Memoir, Table II. [10] Stahr: *Die Rassenfrage in antiken Aegypten* 1907. Tafel. [11] Schultz and Oetteking: *Archiv f. Anthro-*

Species or Race, etc.	Number	Male	Number	Female
Chinese Northern[23]	39	62·9	9	63·3
" Southern[24]	19	62·1	—	—
Tibetan, A-Race[25]	37	61·8	—	—
" B-Race[25]	15	59·2	—	—
" Non-adult[25]	2	68·0	—	—
Nepalese[26]	47	60·4	[6	59·9 ?]
Hindoos[27]	[9	60·5 ?]	—	—
Burmese, A-Race[28]	44	62·8	39	64·1
" B-Race[28]	[7	61·5 ?]	17	66·1
" C-Race[28]	[8	62·8 ?]	18	61·4
" Non-adult[28]	♂ + ♀ 7	68·4		—
Mongol Torgods[29]	12	60·7	—	—
Altai Telenghites[29]	60	61·0	15	61·4
Buriats[30]	14	60·4	—	—
Kalmucks[31]	19	60·2	[6	59·6 ?]
Esquimo (cast)[32]	1	67·3	—	—
" "[32]	16	59·3	—	—
Turks[32b]	70	63·3	—	—
Greeks[33]	94	62·9	—	—
Serbo-Croatians[34]	80	62·8	—	—
Rumanians[34b]	40	62·7	—	—
Slovenes[35]	60	62·1	—	—
Guanches	79	60·6	44	61·8
Czechs[36]	101	60·5	49	60·5
Swiss (Daniser)[37]	40	60·3	—	—
" (Alamans *c.* 300 A.D.)[38] ...	♂ + ♀ 176	58·9		—
Bavarians (Alps)[39]	101	59·0	44	61·1
Tyrolese (Austria)[40]	24	59·6	14	59·7
Walser (Vorarlberg)[41]	43	58·1	25	58·8
Austrians[42]	36	58·3	—	—
Friesians (Terps)[43]	♂ + ♀ 35	59·2		—
" (Marken)[43]	♂ + ♀ 13	59·1		—
Anglo-Saxons[44]	26	58·4	26	59·5
English, 17th Century[45]	40	58·5	49	59·2
" Murderers[46]	7	58·2	—	—
Robert Burns (from cast)[47]	—	60·0 (?)	—	—
Professor Cullimore[48]	—	59·0	—	—
Robert, the Bruce[49]	—	57·2 (?)	—	—
Marshall Hess[50]	—	57·1	—	—
Friederich Mohs[51]	—	57·0	—	—
Schubert[52]	—	56·5	—	—
Descartes[53]	—	56·1 (?)	—	—
J. S. Bach[53]	—	56·0	—	—
Sir Thomas Browne[54]	—	55·8	—	—
Kant[55]	—	55·4	—	—
Henry Stuart, Lord Darnley[56] ...	—	55·4	—	—
Schiller[53]	—	54·4	—	—
Haydn[53]	—	54·5	—	—

pologie, Bd. XXXVI. Tafeln S. 70—89, Bd. XLIV. S. 78–9 (Sexing). [12] Biometric Lab. Cranial Collection. [13] *Ibid.* [14] *Ibid.* [15] *Archiv f. Anthropologie*, Bd. XLII. S. 196. [16] *Ibid.* Bd. XLVII. S. 127. [17] *Ibid.* Bd. XL. S. 272, Tafeln. [18] *Biometrika*, Vol. XI. S. 90. [19] *Mitth. d. Anthrop. Gesellsch. in Wien*, Bd. XX. S. 32. [20] *Zeitschrift f. Morph. u. Anthrop.* Bd. XI. S. 580. [21] *Mitth. d. Anthrop. Gesellsch. in Wien*, Bd. XLV. S. 79. [22] Cast in Biometric Laboratory. [23] *Zeitschrift f. Morph. u. Anthrop.* Bd. XV. S. 544, Tafel 6 and Haberer: *Schädel...aus Peking*, 1902, S. 86. [24] S_3' for 19 from Schmidt's Katalog, S_3 mean of 74 from various sources; Schmidt does not provide S_3. In all other cases S_3 and S_3' are from the same individual crania. [25] *Biometrika*, Vol. XIV. p. 206. [26] *Ibid.* Vol. XVI. pp. 28—31. [27] Biometric Lab. Cranial Collection. [28] *Biometrika*, Vol. XIII. p. 219. [29] *Zeitschrift f. Morph. u. Anthrop.* Bd. XV. S. 544 u. Tafeln. [30] *Ibid.* S. 544 u. Tafeln. [31] *Ibid.* S. 544 u. Tafeln. [32] Cast of Typical Eskimo skull in Biometric Laboratory; 16 Crania in R. C. of S.s' Museum. [32b] *Mitth. d. Anthrop. Gesellsch. in Wien*, Bd. III. S. 206—213. [33] *Mitth. d. Anthrop. Gesellsch. in Wien*, Bd. XI. S. 72. [34] Weisbach: Suppl. 1884, *Zeitschrift f. Ethnologie*, S. 70–2. [34b] *Denksch. d. k. Akad. d. Wissensch. Wien, Math. Nat. Kl.* Bd. XXX. S. 107—136. [35] *Mitth. d. Anthrop. Gesellsch. in Wien*, Bd. XLII. S. 84, Tabelle I—II. [36] *Archiv f. Anthropologie*, Bd. XXXIX. S. 282. [37] *Zeitschrift f. Morph. u. Anthrop.* Bd. XV. Tafeln S. 544. [38] *Ibid.* Bd. XIV. S. 649. [39] *Beiträge z. Anthrop. u. Urgesch. Bayerns*, Bd. XVIII. S. 1. [40] *Mitth. d. Anthrop. Gesellsch. in Wien*, Bd. XXXIX. S. 17—22. [41] *Zeitschrift f. Ethnologie*, Bd. XLIV. S. 509. [42] *Mitth. d. Anthrop. Gesellsch. in Wien*, Bd. XVIII. S. 79. [43] *Zeitschrift f. Morph. u. Anthrop.* Bd. XVI. S. 396, S. 478. [44] R. C. of S.s' and London Museums Skulls. [45] *Biometrika*, Vol. V. p. 92. [46] Skulls in R. C. of S.s' Museum. [47] Cast in R. C. of S.s' Museum. [48] *Journal of Anatomy*, Vol. LII. p. 282 from plates. [49] From cast in Department of Anatomy, University of Edinburgh, approximate. [50] *Mitth. d. Anthrop. Gesellsch. in Wien*, Bd. XVIII. S. 91. [51] *Ibid.* Bd. XVIII. S. 92. [52] *Ibid.* Bd. XVIII. p. 79. [53] From photographs. The occipital index as obtained from photographs is only approximate, the arc and chord are measured on the *norma lateralis*, the former with a map-wheel. The position of the opisthion, if no *norma basalis* is available, is the chief difficulty, and it has to be placed with careful consideration of the "springings" of the mastoids. [54] *Biometrika*, Vol. XV. p. 59. [55] *Archiv f. Anthropologie*, Bd. XII. S. 405–7. [56] From skull in R. C. of S.s' Collection.

Figure 2.3. The racial database at work. Republished with permission of Oxford University Press from Karl Pearson and Adelaide G. Davin, "On the Biometric Constants of the Human Skull," *Biometrika* 16 (1924) 334-335.

In sum, Pearson's craniometric research depended on his relationships with, the goodwill of, and the research done by anthropologists and anatomists around the globe.

Although skulls and bones were essential to his study of man's racial evolution, Pearson was also interested in data of living humans for his studies on intelligence and family heredity patterns. The lab had the measurements from Galton's Anthropometric Laboratory at the 1884 London International Health Exhibition at its disposal. This data archive included long series of measurements for both sexes, various classes, and a wide age-range.[53] Pearson and his workers also collected long series of measurements of school children, families, and students. The lab had good connections with the Cambridge Anthropometric Committee, which shared measurements of undergrads and collaborated with the lab in some studies. The scope of this living data exceeded far beyond the numbers of dead skulls he could collect.[54]

Collecting measurements of living people, however, was often difficult: it relied on voluntary cooperation. Pearson found that many people either had a "superstitious objection" to being measured or did not see any immediate profit for themselves.[55] For his family heredity research, Pearson hoped to collect information on two thousand families but only found eight hundred families willing to provide data. In obtaining student measurements for a study on the correlation between physique and intelligence, he experienced a different type of resistance:

> There seems to have been a desire on the part of some of the measured to test the accuracy of the measurer by repeating the process as often as possible, and subjecting him to various artifices. One senior wrangler was measured no less than five times! Considering that the measurer had not the means of a prison warder for controlling his subject, he appears to have managed fairly well. When

[53] Karl Pearson and L.H.C. Tippett, "On Stability of the Cephalic Indices Within the Race," *Biometrika* 16:1/2 (1924) 119. For more on Galton's Anthropometric Laboratory, see: Frans Lundgren, "The Politics of Participation: Francis Galton's Anthropometric Laboratory and the Making of Civic Selves," *The British Journal for the History of Science* 46:03 (2013) 445–66.

[54] Theodore Porter has recently shown how Pearson received masses of human data from prisons, medical, and educational institutions. Porter, *Genetics in the Madhouse*, 250.

[55] Karl Pearson, "Variation in Man and Woman," 267.

> the duplicates were hopelessly irreconcilable – generally in those characters depending upon the agency of the subject – they were all rejected.[56]

Although skulls lacked the numbers that the living could offer, they did not pose such problems – its bony structure never resisted the controlling touch of the researcher. Its stiffness was the gateway to objectivity and accuracy.

The above-discussed exchanges of bones, casts, and craniometric and anthropometric data point towards the presence of a moral economy of racial researchers who together worked on the global project of mapping human variation. This type of sharing and reuse, we may remember, was the intention of launching *Biometrika*, to make data widely, even internationally, available. It is noteworthy that the diversity of measurement methods did not prevent the exchange and reuse of data. Perhaps the biometricians believed that statistics could fix these problems: by turning to the mathematics of large numbers, these individual differences may have become less important. Perhaps anthropologists found obtaining the data and moving forward with their research simply more urgent.[57]

These exchange relations, however, were not entirely balanced: Pearson was very guarded of his bone collection and desired to keep the material for his own lab. He was therefore reluctant to share any details about the collection or share the bones with researchers unaffiliated with his lab.[58] Sharing data in journals also came with certain problems: publishing data was expensive. As *Biometrika*'s editor, Pearson had to issue auxiliary publications such as the *Tables for Statisticians and Biometricians* (London: Biometric Laboratory of University College London, 1914 and 1931) to cover

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[56] Karl Pearson, "On the Correlation of Intellectual Ability with the Size and Shape of the Head (Preliminary Notice)," *Proceedings of the Royal Society of London* 69 (1901) 333.

[57] See for more on the moral economy of laboratories: Robert E. Kohler, *Lords of the Fly: Drosophila Genetics and the Experimental Life* (Chicago: University of Chicago Press 1994).

[58] GMP, KP to GM 16.7.1921; KP to GM 1.2.1931.

the cost of printing the pages of data and graphs that were central to the journal's ambitions.[59] Sometimes his mathematical tables were pirated and published in other countries.[60] Pearson was not the only journal editor that struggled with the costs of printing data. American anthropologist Aleš Hrdlička restricted the publication of detailed measurements in his journal *The American Journal of Physical Anthropology* to save costs or asked authors to cover the costs of printing pages of data.[61]

Researching Race: The Biometric "Scheme"

Under Pearson's leadership, the lab developed a durable and consistent approach to reducing morphological race to statistical data. The first craniometric paper published in *Biometrika*, the 1902 article written by Fawcett and Lee mentioned earlier, gives a good impression of the questions asked and the methods used. Two women[62] wrote the paper: Cicely D. Fawcett, a computer at University College with a Bachelor's in Science, and Alice Lee, a lecturer in applied mathematics at the University's Bedford College for women as well as computer for Pearson's lab between 1896-1927. Lee did the calculations for many craniometric papers published in *Biometrika* and was often listed as co-author.[63] "A Second Study of the Variation and Correlation of the Human Skull, with Special Reference to the Naqada Crania" was the first study to present the measurements, statistical

[59] W.P. Elderton, "Biometrika 1901-1951, *Biometrika* 38:3/4 (1951) 267-268, here 267.

[60] J.B.S. Haldane, "Karl Pearson, 1857-1957," *Biometrika* 44:3/4 (1957) 311.

[61] Moreover, Hrdlička argued that no one ever read the data and thus the publication of lengthy tabular matter was simply insignificant. In fact, Hrdlička was very wary of the statistical treatment of physical anthropology, as chapter 5 will discuss. See: Aleš Hrdlička Archive, Box 8, Folder "American Journal of Physical Anthropology (1918-1941).

[62] Pearson pioneered in offering careers in statistics to female scientists in his Biometric Laboratory. See: Rosaleen Love, "'Alice in Eugenics-Land:' Feminism and Eugenics in the Scientific Careers of Alice Lee and Ethel Elderton," *Annals of Science* 36:2 (1979) 145–58; Marsha L. Richmond, "'A Lab of One's Own,' the Balfour Biological Laboratory for Women At Cambridge University, 1884-1914," *Isis* 88 (1997) 422–55; David Alan Grier, *When Computers Were Human* (Princeton: Princeton University Press 2005); Claire Jones, *Femininity, Mathematics and Science, 1880–1914* (New York: Palgrave Macmillan 2009).

[63] E.S. Pearson, "Karl Pearson: An Appreciation of Some Aspects of His Life and Work," *Biometrika* 28:3/4 (1936) 225; KPA, Box 241, 11/1/12/24, correspondence with or regarding Alice Lee.

constants, and racial conclusions derived from Petrie's Naqada skulls. What is more, the paper introduced the lab's main methods. Despite the development of new methods and formulas over the years, Fawcett and Lee's article introduced "a scheme" that consistently returned in the lab's future craniometric publications. Biometricians often referred to the article as a "classical memoir" and copied its methods.[64] The paper thus became an important hinge in the lab's ongoing racial research. At the time of publication, Pearson expected that the piece would attract new *Biometrika* subscribers.[65]

It took Fawcett and Lee six years to measure the four hundred skulls and reduce them to their relevant statistical constants. Although Fawcett received some help from anatomist Thane, she did most of the skull measurements herself at home.[66] In choosing which characters to measure, Fawcett followed two guides. First, she modelled her approach after Pearson's *Chances of Death* and the characteristics of variability he had deemed important. Moreover, "the chief use of craniometry is for comparative purposes, and what will be of most value will be, not to add new types of measurement, however desirable in themselves, but to make such measurements as will bring the Naqada skulls into relationship with as many measured series as possible." Instead of introducing new characters, Fawcett took the standard measurements of German anthropology which had already produced a good amount of comparative data. The article presented a total of 48

[64] G.M. Morant, "Studies of Palaeolithic Man. I. The Chancelade Skull and Its Relation to the Modern Eskimo Skull," *Annals of Eugenics* 1:3 (1926) 262.

[65] Challis, "Skull Triangles," 6. Along similar lines, Challis argues that "this paper cemented the importance of skulls in biometric work and illustrated the concerns with defining race scientifically in the early years of the twentieth century, whether in statistics, anthropology or archaeology." She also suggests that this article played an important role in the receiving of the Drapers grant.

[66] Challis, "Skull Triangles," 6.

measurements and indices, including various lengths, breadths, widths, circumferences, correlations, and angles, which became the lab's standard practice or "our usual methods."[67]

These raw measurements, however, were not sufficient. Craniometry "must adopt the methods of modern statistical investigation, tabulating means, variabilities, correlations, and their probable errors in order to draw safe inferences and make racial comparisons." To make these methods known to other researchers, the authors presented the full statistical treatment of the skulls, resulting in pages with "masses of technical detail and data" as Challis remarks.[68] They also published the long tables of data for "future interpretation," thus fulfilling the promise of data sharing set out in *Biometrika*'s first editorial. Subsequent craniometric articles continued to print this ambition along with many pages of raw data.[69] Thus, biometricians not only mined publications for data, but also contributed to future data-mining practices themselves. The transnational comparison of data, central to anthropology as discussed in chapter 5, was at the base of racial research.

Fawcett and Lee regarded the most important statistical insights to be the variability and homogeneity of the racial sample. This required knowledge of standard deviations and probable errors. Homogeneity built on the assumption that means larger than twice the probable error indicated differences beyond the limits of random sampling and thus suggested heterogeneity. The final test of homogeneity was the comparison of the sample's variability with other known homogeneous series, such as Ranke's Bavarian and Koganei's Aino samples introduced earlier. After careful consideration, Fawcett and Lee concluded that: "it would appear that the Naqada series is quite comparable in homogeneity with any modern series of skulls of like number…we think therefore that we are justified in treating our material as homogeneous and in speaking of a Naqada

[67] GMP, KP to GM, 8.8.1930.

[68] Challis, "Skull Triangles," 6.

[69] See for instance M.A. Lewenz and Karl Pearson, "On the Measurement of Internal Capacity from Cranial Circumferences," *Biometrika* 3:4 (1904) 394.

race and not merely of the Naqada crania." The variabilities of known homogeneous races thus determined the racial ontology of other samples.

Indeed, a sample's variability played a central role in biometric racial research. It was the starting point of Darwinist theory that deeply informed Pearson's understanding of man's evolutionary history. Evolutionary development required the "gradual selection of slight normal variations," creating a continuous ebb of change within the human race, Pearson theorized in *Chances of Death*. Whereas anthropologists represented race with "ideal types," the biometric scheme considered within-group variation or "intra-racial variation" a crucial racial character. Variability not only determined whether the sample was a separate or mixed race – it also contained crucial information on the development of that race through time. Pearson already posited in "Variation in Man and Woman" that "civilized races" were more variable than "savage races" because of the lesser struggles for survival. Along similar lines, Fawcett and Lee used the Naqada sample's variability to determine where the race sat on the evolutionary scale. Was it more "primitive" or "civilized"? They added Ranke's and Koganei's data for comparison, which represented "a highly civilised modern race" and "an extremely primitive type" respectively. Upon comparison, the Naqada sample was not as variable as civilized Bavarians, but more so than the primitive Aino. The authors therefore concluded that "the Naqadas are not a race of markedly primitive character." The data and statistical analysis confirmed established theories about evolution and variability, much in line with Pearson's conclusions. Fawcett and Lee wrote: "The very wide-spread evidence of increased variation as we pass from uncivilised and primitive people may of course be due to increased racial admixture as man grows older, or it may, as we believe, be due to less stringent dependence for survival on the physical characters in civilised man." The authors left the categories "primitive" and "civilized" unexplained and also assumed the racial character of populations, primitive or civilized, to be self-evident.[70]

In measuring and assessing racial samples, the early biometric articles thus relied on a theory of evolutionary change "tending in a fixed direction" from primitive and less variable to civilized and more variable. Through time, the human race passed through stages of civilization. This materialized in the shape and size of the body and the variability of characters within a race. The biometricians thus introduced an important shift in focus towards within-group variation, generally neglected by anthropologists, and turned it into a racial character tied up with more traditional notions of biological determination and racial hierarchies. How races moved between stages remained unexplored. Only Crewdson Benington briefly reflected on whether races could succeed to different civilization stages. He discovered that a "Negro" sample was as variable as European samples but argued that this did not signify "that a few years of European education or civilised environment could convert the Negro cranium physically or develop it mentally into an instrument equivalent to the European cranium." Instead, European mental characters had developed over centuries. "Upward development" seemed to have been possible for biometricians, but only in the slow-moving pace of evolutionary time.[71]

The biometric "scheme" thus went beyond the examination of a few measurements and their means: only more complex statistical analysis could unlock accurate insights about race. In the hands of biometricians, race became a *metrical aggregate*. Besides the variability of a population, biometricians also considered correlation a relevant racial character. Several projects at the laboratory studied the correlation of the chief cranial characters. Correlation, first introduced by

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70 Confusingly, Fawcett and Lee used terms such as "race," "type," "people," "characteristics" without distinction and sometimes bracketed "primitive" while leaving it unbracketed in other cases. Quotations taken from pages 424; 428; 435; 440; 463-4.

71 R. Crewdson Benington and Karl Pearson, "A Study of the Negro Skull With Special Reference to the Congo and Gaboon Crania," *Biometrika* 8:3/4 (1912) 336.

Georges Cuvier,[72] was "the mathematical process of finding the best linear relation between the known value of one character and the most probable value of a second."[73] Where anatomists such as Cuvier analyzed correlation through the morphological study of the skeleton, the biometricians approached the subject with quantitative analysis, putting the problem, in their eyes, "on a sounder basis."[74] By mathematically defining the degree of correlation, biometricians predicted other measurements that were difficult to obtain, such as skull capacity. This was especially useful because many skulls were too fragile to pack with seed or sand and determine its volume. With external measurements of skull height, breadth, and length, biometricians developed race-specific formulas to predict the correlated skull capacity. Biometricians also studied other correlations, such as hair and eye color, head shape and pigmentation, and stature and long bones. In the case of the latter, biometricians reconstructed overall stature of an "extinct race" from the measurements of the long bones.[75]

Once researchers determined a correlation formula for a racial series with a set of measurements, they could insert new measurements of a skull in the formula and the formula would predict the skull's capacity. The formula acted like an automaton: its reductive operations simulated and mechanized human thinking while concealing the fact that it was man-made. It also eliminated the need for the morphological skull: the formula only needed the skull's measurements. Formulas

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72 Georges Cuvier, *Discours sur les révolutions de la surface du globe: et sur les changemens qu'elles ont produits dans le régne animal* (Paris: Chez G. Dufour et Ed. d'Ocagne 1826) 47. Cuvier's approach to the correlation of body parts was based on morphology and thus was rather different from the statistical correlation developed by Galton and Pearson.

73 M.A. Lewenz and Karl Pearson, "On the Measurement of Internal Capacity from Cranial Circumferences," *Biometrika* 3:4 (1904) 368.

74 A. Lee and Karl Pearson, "Data for the Problem of Evolution in Man. VI. A First Study of the Correlation of the Human Skull," *Philosophical Transactions of the Royal Society of London* series A, volume 196 (1901) 226.

75 Karl Pearson, "On the Measurement of Internal Capacity From Cranial Circumferences," *Biometrika* 3:4 (1904) 366–97; Karl Pearson and Brenda N. Stoessiger, "On Further Formulae for the Reconstruction of Cranial Capacity From External Measurements of the Skull," *Biometrika* 19:1/2 (1927) 211–14; Karl Pearson, "On the Correlation Between Hair Colour and Eye Colour in Man," *Biometrika* 3:4 (1904) 459–62.

such as these thus created a seemingly mechanized and automated operation that minimized the subjectivity of the researcher and the research subject.

Despite these efforts to make the calculation of correlations more precise and objective, the biometricians stressed the limited precision of these results: they were never entirely exact because of individual variation, which again necessitated knowledge of standard deviations and probable errors. Furthermore, these formulas were always race-specific. This boiled down to a difference between "intra-racial" and "interracial" formulas, terms introduced by Pearson. Whereas intra-racial formulas predicted results for individuals within a race, the interracial formulas only predicted values for race as a whole. "We cannot pass from intraracial to interracial conclusions," Fawcett and Lee warned the reader. Matters became even more complex when Pearson and his workers found that certain characters did not correlate intra-racially, or within a single skull, but could be interracially correlated as "due to racial differentiation."[76]

The lab further developed methods for determining the homogeneity of samples. Besides the skulls' ethnographic information, standard deviations, and probable errors, biometricians also plotted and analyzed a sample's distribution or frequency curves to establish its homogeneity. This built on the assumption that anthropometric and craniometric characters followed the normal distribution and generated a frequency curve with a bell-shaped form around the mean. If a distribution deviated from this shape and gave a "skew" curve, this indicated potential heterogeneity and the sample may consist of two normal curves of two races. With Pearson's "method of moments," one matched a complicated curve to a simpler bell-shaped curve and found the two normal curves by calculating higher moments. Pearson argued that his method of moments could

[76] Cicely D. Fawcett and Alice Lee, "A Second Study of the Variation and Correlation of the Human Skull, 465; Karl Pearson, "27. On the Reconstruction of Cranial Capacity from External Measurements," *Man* 26 (1926) 46; Karl Pearson, "Note on Section VI of Dr K. Wagner's Memoir," *Biometrika* 27:1/2 (1935) 133. Regarding last point: Karl Pearson and Adelaide G. Davin, "On the Biometric Constants of the Human Skull," *Biometrika* (1924) 347.

work as a control experiment of anthropologists' method of sexing skulls with the morphological method. Moreover, it gave insight into the survival of races and evolution.[77]

Pearson not only had his workers resolve heterogeneous frequency distributions using his method of moments, but also trained them to use his "chi-square" test or the "goodness of fit" method. This method determined whether a sample came from a known population or whether two racial samples were random samples of the same population of which the biometrician had no previous knowledge. In other words, the method compared predicted results against observations and determined the independence of two samples. This approach was used to determine whether a sample of skulls was random and homogeneous and whether cranial measurements fit the normal law of frequency.[78]

Pearson's methods such as the "goodness of fit" test became well-known in the field of statistics – we can see here how they were also marshaled in the realm of racial science. These methods were much more complex ways of determining racial differentiation and homogeneity than what anthropologists had been doing so far, as we have seen in chapter 1. Although Pearson faulted anthropologists for not learning these methods, he and his workers also stressed the hard and laborious work they required. Fawcett and Lee's six years of calculations was no anomaly: the arithmetic of the Egyptian skull data required the "heavy work" of measuring 50 characters on 1600 crania as well as the "preparing [of] as many schedules, sorting them out, forming over 250 correlation tables and working these out numerically." Authors often stressed that they received help

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[77] Pearson quickly found that homogeneous material could also create skew curves, caused by an "indefinite" amount of "contributory causes." Karl Pearson, "Contributions to the Mathematical Theory of Evolution. II. Skew Variation in Homogeneous Material," *Philosophical Transactions of the Royal Society of London* 186: Part I (1895) 343–424; Karl Pearson, "On Some Application of the Theory of Chance to Racial Differentiation. From the Work of W.R. Macdonell and Cicely D. Fawcett" *The London, Edinburg and Dublin Philosophical Magazine and Journal of Science* sixth series, volume 1 (1901) 110–24.

[78] Karl Pearson, "On the Probability That Two Independent Distributions of Frequency Are Really Samples from the Same Population," *Biometrika* 8:1/2 (1911) 250–54.

with their craniometric tables and that the conclusions of their studies were the cooperative product of various lab workers.[79]

Biometricians also regularly mentioned the lack of standardized measurements. As chapter 5 discusses, English, French, German, and American schools of anthropology had developed their own, distinct measurement methods. The biometricians, instead, stressed the importance of "doing exactly the same thing" rather than introducing new measurements and landmarks.[80] New insights only derived from the statistical comparison of large amounts of racial data, not from measuring a novel anatomical peculiarity on a handful of skulls. This was a dangerous activity for the craniologist: "If he neglects the past history of his science and adopts new and possibly better characters for record he cuts himself off from the possibility of forming comparisons with the wide range of measurements on innumerable races already made; and further he has no security that others will follow in his footsteps, so that his measurements may be of service to them." New measurements could be added only "*if the old are retained*," Pearson warned. He ensured that the lab's craniometric scheme was maintained "throughout all the work done in the Laboratory," in line with his desire to keep the lab's work coherent. It was crucial to "take a long view of matters" and press the importance on every worker who passed through the lab that their work was "simply a part of a scheme extending over a quarter of a century." To Pearson, racial research was a long-term project of which current workers, including himself "may never live to see the fruition."[81]

The biometricians thus wanted to innovate racial research and make it more rigorous with new statistical methods applied to common racial characters. Only the statistical constants derived

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[79] Karl Pearson, "Variation in Man and Woman," 271; Karl Pearson and Adelaide G. Davin, "On the Biometric Constants of the Human Skull," 330; Karl Pearson, "On the Relationship of Intelligence to Size and Shape of Head, and to Other Physical and Mental Characters," *Biometrika* 5:1/2 (1906) 136.

[80] R. Crewdson Benington and Karl Pearson, "A Study of the Negro Skull with Special Reference to the Congo and Gaboon Crania," 314.

[81] Karl Pearson and Adelaide G. Davin, "On the Biometric Constants of the Human Skull," 329; 333.

from large pools of data, they argued, revealed new insights about racial differentiation and human evolution. Biometricians implemented their results in the more traditional context of biological determinism and racial hierarchy. New methods did not always lead to new theories.

New was the biometric notion that race lay in the aggregate. And this was a weighty business that required knowledge of standard deviations, probable errors, and the difference between intra- and interracial constants. Biometric methods produced novel byproducts of racial objectification such as coefficients and correlations. Most anthropologists, anatomists, and medical men, however, had not learned these methods or were unwilling to depart from the practices that their instructors, in some cases the great "founding fathers" of physical anthropology, had taught them. This mathematical intervention came from a group of "outsiders" who, conversely, lacked the morphological expertise that came with a medically focused training. Pearson did not consider himself a stranger but a missionary preaching the biometric gospel to save anthropology. He launched an attack on the established practices of anthropologists and other racial researchers. In these challenges, he *did* introduce new racial theories.

Unsettling Anthropology's Data Practices

Pearson has been the center of much writing about the conflicts between Mendelians and biometricians, but as Porter points out, he challenged various disciplines where he believed that numbers and statistics were neglected or used incorrectly. Pearson's interactions with anthropologists offers another example of his controversialist and occasionally hostile attitude toward other disciplines and their data practices as well as his strong belief in the correctness of biometrics. Indeed, any anthropologist who did not recognize the value of biometric methods was considered a "sinner."[82] To Pearson, the methods commonly used in racial research were profoundly "inadequate." He desired to improve craniometry not only with a new methodology, but also by criticizing recent craniological work "with the sharpness of the surgeon's knife, which is handled in the real interests of the patient." He hoped that his published criticisms offered "reconstructive" advice for anthropologists.[83]

Biometricians' idea of "doing racial research right" differed from common anthropological practices. As his son Egon remembered, "Pearson was the first to insist on the necessity of obtaining large samples of skulls and bones," following from his belief that the study of evolution required the mathematics of large numbers.[84] Pearson and his team had demonstrated how to accumulate extensive series of racial data. Biometricians also had a different idea of what constituted a racial "type." Rather than an abstracted "ideal type" or a representative skull, a biometric racial "type" was a homogeneous race, determined by its means and standard deviations, and characterized by its unique intra-racial variation, correlations, and frequency distributions. Pearson reminded *Biometrika*'s readers:

> For the biometrician the type of any group (or "population" in the biometric sense) is fixed by the whole complex of statistical constants – means, standard deviations, correlations, skewnesses, etc., which suffice to differentiate it sensibly from other groups or populations. Very frequently the arithmetic means of a number of characters will suffice, if so they fix the type. Often we have to use a number of other constants – correlations or what not – and the question of whether two groups are different in type becomes an extremely delicate one, only solved by a careful consideration of the probabilities connoted by the probable errors of differences.[85]

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[82] Porter, *Genetics in the Madhouse*, 240; Porter, *Karl Pearson*, 270; Mackenzie, *Statistics in Britain*, 106; E.S. Pearson, "Karl Pearson: An Appreciation of Some Aspects of His Life and Work," *Biometrika* 29:3/4 (1938) 207. Historian Robert Kohler has suggested that the imperial attitude of the biometricians led biologists and taxonomists to reject biometrics. See: Robert E. Kohler, *Landscapes and Labscapes: Exploring the Lab-Field Border in Biology* (Chicago: University of Chicago Press 2002) 72.

[83] Karl Pearson, "Explanatory. Craniological Notes." *Biometrika* 2:3 (1903) 339.

[84] E.S. Pearson, "Karl Pearson: An Appreciation of Some Aspects of His Life and Work," *Biometrika* 29:3/4 (1938) 179.

[85] Karl Pearson, "Craniological Notes: Remarks on Professor Aurel von Torok's Note," *Biometrika* 2:4 (1903) 510.

Unlike many racial researchers, biometricians considered *all* the skulls of the racial sample in determining a metrical type. Indeed, anthropologists had proceeded along different lines, for instance by using the morphological method. Pearson argued that the method, which he called "anatomical appreciation," compelled "wonder and admiration" from the biometrician, who could not criticize that which belonged "to the *arcana* of the anatomist's training."[86] The ways in which anthropologists deployed the metrical approach, however, drew Pearson's sharp criticism. He pressed upon anthropologists the need to increase the number of skulls and characters measured far beyond 20 so that the sample's statistical constants could be accurately determined. Pearson was most frustrated with how anthropologists used statistical methods and did not move beyond the calculation of some averages. In his view, this was "kindergarten arithmetic."[87] Most anthropological work was based on "half-a-dozen measurements on half-a-dozen skulls screened by a smoke-fog of vague remarks."[88] "The data are either insufficient for any statistical conclusion whatever; or they are unaccompanied by any determination of their probable errors on which alone a judgment could often be based; or the very *principia* of the theory of statistics are clearly unknown to the handlers of the data." Anthropologists' inadequate statistical practices, he concluded, ended up obscuring their anatomical appreciations.[89]

Moreover, it drove the discipline into a deplorable state. "To classify a few individuals into different races by means of two or three measurements, such as the cephalic index, the length, or the facial angle, – before the correlation and the variations of these characters have been determined for even a single race – is a very dangerous proceeding, and calculated to bring craniometry into

86 Karl Pearson, "Explanatory. Craniological Notes," 338.

87 Karl Pearson, "On the Need of a New Technique in Anthropology," *The Lancet* 199 (1920) 679.

88 Karl Pearson, *The Life, Letters and Labours of Francis Galton. Researches of Middle Life.* (Cambridge, UK: Cambridge University Press 1924) 333.

89 Karl Pearson, "Explanatory. Craniological Notes," 338.

discredit," Fawcett and Lee cautioned.[90] Such research was "simply outside the field of science."[91] Biometry, however, was the solution: "Biometry is essentially a science of exact quantitative definition, and if it is to be of service in rendering anthropology an exact branch of science, it must replace vague ideas by numerically definite conceptions."[92] Biometric interventions were urgent.

Fellow anthropologists felt the sharpness of Pearson's biometric knife. In the early years of *Biometrika*, Pearson took swings at various scientists and criticized their methods. In 1903, Pearson began a short-lived series called "Craniological Notes" in *Biometrika*, which object was "to bring home to craniologists the need for the revision of their statistical methods."[93] These articles, "controversies" by his own determination, included his sharp criticisms and the responses from the disparaged anthropologists. These exchanges not only give insight into the encounters between Pearson and anthropologists, but also uncover Pearson's theories of race.

The first memoir Pearson criticized was Aurel von Török's latest publication in the German *Zeitschrift für Morphologie und Anthropologie*. The Hungarian anthropologist who defended the extensive use of measurements in craniology as discussed in chapter 1, raised questions about the use of statistical methods in determining racial "types." He pointed out that a racial type brought together characteristics found in the majority of the population but that the arithmetical mean failed to capture these common characteristics in absolute terms. What is more, he questioned the extent to which statistical methods could determine racial purity and racial mixing [*Blutmischung*] – "if this were possible, then the entire racial history [*Rassenlehre*] could be settled with pure mathematics!" Pearson was perplexed by von Török's observations. "The writer has clearly not the most elementary conception of the theory of statistics…The whole problem of graduating raw data is a sealed book

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90 Cicely D. Fawcett and Alice Lee, "A Second Study of the Variation and Correlation of the Human Skull," 409.

91 M.A. Lewenz and Karl Pearson, "On the Measurement of Internal Capacity from Cranial Circumferences," 397.

92 Karl Pearson, "Craniological Notes: Remarks on Professor Aurel von Torok's Note," *Biometrika* 2:4 (1903) 510.

93 Karl Pearson, "Explanatory. Craniological Notes," 339.

to him!" he declared. "All I demand is that he [the anthropologist] should not use, what are to the mathematician hopelessly inadequate and faulty statistical methods to justify his 'type' appreciations."[94]

Pearson countered that his mathematical methods *were* capable of solving questions of racial descent and mixing. Moreover, he claimed that no race existed without *Blutmischung* because humans descended from either a single or very few groups. "Probably every group of men is in this sense *blutvermischt*." Indeed, "race" was a relative term:

> I understand by a race of men, what I understand by a race of snails or birds or fox-terriers, i.e. a group which has intermixed freely with itself but not with other groups for a number of generations, and during this process has been equally freely subjected to the action of natural or artificial selection. Sensible isolation is generally needed for the first condition, probably fixity of locality for the second condition. The 'perfect race' would be that which had for many generations been isolated in one locality and had freely 'intrabred.' As a result we should have a distinct 'type,' more or less stable to its environment. Of course in man we only get more or less close approximations to these conditions, and from such approximations we have every shade of imperfect mixture down to the mixed population of a European settlement in Asia, with small *Blutmischung* among its different castes or subgroups...The 'purest' race is for me the one which has been isolated, intrabred, and selected for the longest period.[95]

Because of man's extreme mobility and his ready fertility with any group of his own species, evolution meant continuous blending. Pearson's understanding of evolutionary history thus questioned the traditional essentialist notion that races could be distinguished into pure, separate units. "I shall no doubt be told that this conception of race is quite invalid, that races can be sorted out by types, long after blood-mixture, by the expert anthropologist," he concluded.[96] Even though

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[94] Aurel v. Török, "Uber das gegenseitige Verhalten der kleinsten und grössten Stirnbreite so wie der kleinsten und grössten Hirnschädelbreite der Variationen der menschlichen Schädelform," *Zeitschrift für Morphologie und Anthropologie* Band IV (1902) 500-588; Karl Pearson, "Craniological Notes: Professor Aurel von Torok's Attack on the Arithmetical Mean," *Biometrika* 2:3 (1903) 339-345; Aurel von Török, "Craniological Notes: Note on Cranial Types," *Biometrika* 2:4 (1903) 508-509.

[95] Karl Pearson, "Craniological Notes: Remarks on Professor Aurel von Torok's Note," *Biometrika* 2:4 (1903) 511.

[96] Karl Pearson, "Craniological Notes," 511.

he considered races relative, Pearson did not dismiss racial classifications. Instead, one should start with groups that were known to have been isolated and interbreeding, such as Ranke's Bavarian and Koganei's Aino samples; their homogeneity "will give us a standard by which to judge of the comparative value of other groups."[97] The theory of evolution questioned the pure race concept, but its sidekick variation continued to be a classificatory tool that gave insight into relative purity of races.

Pearson's next target was Charles Myers, an English physician who had joined the Torres Straight Expedition in 1898 and dabbled in the measurement of living humans around the turn of the century. Pearson and Myers had corresponded in 1901 when Myers went to Egypt to collect 15.000 measurements of Egyptians and people from Sudan. Pearson made suggestions on collecting data and sent Myers Fawcett and Lee's memoir on the Naqada skulls. He also suggested that Myers publish his results in *Biometrika* if they proved interesting. After returning to England, Myers stored his "mass of data" at the British Royal Anthropological Institute which came to function as a physical database for the measurement cards.[98]

Before working up his Egyptian material, Myers wrote a review of Fawcett and Lee's memoir for the Anthropological Institute's journal *Man*. He called this "test specimen" of Pearson's biometric approach to race an "epoch-marking, if not epoch-making" publication. Even though ordinary craniologists would be "terrorised at the extremely mathematical character of the monograph," Myers assured that a slight acquaintance with statistics would unlock the meaning of most tables. Myers only questioned Fawcett's conclusion that the Naqada were a homogeneous race. The variability results puzzled Myers – if the standard deviation of the Naqada skulls was six, but

97 Karl Pearson, "Craniological Notes," 511.

98 KPA, Box 244, 11/1/13/117, correspondence with C.S. Myers; RAI Archive (RAI), MS 100 "Anthropometric measurements [of Egyptians. 1901-2]. The data cards are still stored there.

the standard deviation of a heterogeneous sample of skulls known to Myers was eight, then where did homogeneity end, heterogeneity begin, and which standard deviations represented the variability required for homogeneity?[99]

Pearson did not take Myers' comments lightly and wrote a spirited critique in *Biometrika*'s Craniological Notes. One should not subtract Myers' mean standard deviation from the Naqada's mean standard deviation but interpret these constants relatively and compare the variability of the variabilities. After demonstrating how this was done, Pearson concluded that the difference between the variabilities was not two but four times the standard deviation – a significant difference. The Naqada series was therefore dissimilar to Myers' heterogeneous population. Pearson further elaborated on the statistical odds: "it is such odds as these the combination of which can hardly fall short of 4,000,000 to 1 and which no sane man in practical conduct could disregard, that amount to 'small' differences from the standpoint of the old school of craniologists!"[100]

In response to this "vigorous denunciation," Myers stressed that homogeneity was always relative: "in our present ignorance none can define the exact meaning of racial purity." Because the Egyptian population was a mixture of several races, it was silly to speak of Egyptian "races." It ignored "every lesson which physical anthropology, philology and history can teach us. The truth is that at present we have no evidence of an isolated race, which has never been contaminated by admixture with other races."[101] Even though Myers' views on racial purity were in line with Pearson's understandings, Pearson responded that Myers was entirely mistaken, made "absurd" claims, and could never be converted to the view that statistics was an exact science. Despite the sharp tone of

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[99] C.S. Myers, "13. Reviewed Work(s): A Second Study of the Variation and Correlation of the Human Skull with Special Reference to the Naqada Crania. By Cicely D. Fawcett and Alice Lee," *Man* 3 (1903) 28–32.

[100] Karl Pearson, "Craniological Notes: Homogeneity and Heterogeneity in Collections of Crania," *Biometrika* 2:3 (1903) 345-347.

[101] C.S. Myers, "Craniological Notes: Homogeneity and Heterogeneity in Crania," *Biometrika* 2:4 (1903) 504-505.

his critique, he concluded that "mere controversy is very distasteful to me, even when it is striving to pull a great branch of science out of the discreditable rut it has been brought into by the use of hopelessly unscientific statistical methods."[102]

The "Craniological Notes" series was short-lived, but Pearson continued to attack anthropological work in the early 1900s.[103] These instances offer another episode of Pearson's aggressive conviction of the need for statistics in various disciplines and his preoccupation with "sinners" who used numbers incorrectly. These exchanges debated basic elements of racial research, such as type, purity, and measurements. Like Pearson, Von Török and Myers stressed in their memoirs the urgency for racial science to transform its methods: they raised questions about and offered new approaches to reducing measurements to racial data. Nevertheless, Pearson dismissed their efforts, seemed unwilling to consider alternative perspectives, and ignored similar ideas and critiques amongst praises. Only he had the final answer: modern statistics. He considered himself and those trained in his laboratory to be missionaries preaching the new gospel that "truth is only going to be reached by hard work and quantitative determination."[104] He rejected being an outsider, a mathematician with no formal anatomical, biological, or anthropological training, and argued that "genuine anthropology" could be done by mathematicians.[105] His later criticisms published outside of *Biometrika* proved somewhat milder.[106] Moreover, the biometricians realized it took a long time to reform a branch of science – perhaps Pearson grew more patient.[107]

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102 Karl Pearson, "Craniological Notes: Remarks on Dr. C.S. Myers' Note," *Biometrika* 2:4 (1903) 506-508.

103 See for instance: John Beddoe, "A Method of Estimating Skull-Capacity from Peripheral Measures," *The Journal of the Anthropological Institute of Great Britain and Ireland* 34 (1904) 266–83; KPA, Box 83, 3/6/50 "Papers relating to "On the Reconstruction of Cranial Capacity from External Measurements."

104 RAI, MS 294/17/2, letter Pearson to Miriam Tildesley, 9.8.1920. Also quoted by Porter, *Karl Pearson*, 250.

105 Karl Pearson, "Was the Skull of the Moriori Artificially Deformed," *Biometrika* 13:4 (1921) 338.

106 Karl Pearson, "Race Crossing in Jamaica," *Nature* 126:3177 (1930) 427.

107 W.P. Ker, "W.R. Macdonell," *Biometrika* 4 (1917) 281.

Pearson and his colleagues continued to challenge anthropological notions in their work, albeit without directly seeking controversy. First, they questioned the relationship between skull size and intelligence, the idea discussed in chapter 1 that able men had large heads. Already in the late-19th century, Pearson argued that the link between skulls, brains, and intelligence was more multifaceted. In the *Grammar of Science*, Pearson's popular taxonomy and philosophy of science, he wrote that intelligence was not linked to size, but "the complexity of its convolutions and the variety and efficiency of its commissures."[108] To estimate the correlation between brain size and intelligence, he measured the skull capacity of people known to be intelligent. Using external head measurements and correlation formulas, Pearson and his colleagues collected data of 35 anatomists measured at the Anatomical Society Meeting in 1898, of the 25 members of the teaching staff at University College, including Pearson himself, of college students at Cambridge and Bedford College, and school children. They found no marked, significant, or close correlation between skull capacity and intelligence. In fact, they found that "the most capable men" had small skull capacities. Indeed, Pearson's small capacity of 1452 cubic centimeters put him in the eighteenth place of his 25 colleagues. The data did not support any relationship between skull size and intelligence.

Pearson's most striking experiment involved the study of Jeremy Bentham's mummified head. When the British philosopher and social reformer died in 1832, he left his body to University College and requested it to be preserved. His severed, embalmed head, which is still kept at the University today, was pulled out of storage for the Biometric lab. One of the lab's workers took 37 measurements on the head and measured its capacity at 1475 cubic centimeters, just below the average English capacity of 1477. If judged by capacity, Bentham's head scored mediocre. Thus, Pearson and Lewenz concluded that "the head of this man of first-class intellect shows no single measurement – least of all its capacity – which would serve to differentiate it from that of the

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[108] Karl Pearson, *The Grammar of Science* (London: Adam and Charles Black 1900) 539.

average Englishman of his time." Biometric research on brain-weight offered similar conclusions. Thus, heads and brains could not provide individual predictions of intelligence or offer insight into sexual or racial differentiation regarding this relationship. These measures were always relative to physique and required allowance for weight, stature, and age.[109]

Although these conclusions undermined a traditional anthropological assumption in important ways, Pearson did not dismiss biological determinism. Instead, he argued that physiological characters offered better insights into intelligence. Lung functioning, grip, body temperature, pulse and respiration rate, vision, and hearing depended on the functioning of the body and were regulated by the brain. Thus, intelligence showcased itself through those capacities. Rather than size or weight, difference in intelligence "lies in the rapidity and efficiency of the functioning of the material of the brain." This argument was based on a materialistic understanding of the mind. Pearson considered it probable that bodily organs, the brain, and other physiological characters did not grow or operate independently but developed together. This was in line with his idea of evolution: man was part of a long ancestry of living and non-living things, all subjected to the same forces of evolution and selection that in turn produced psychical and physical characters. Although the association between the brain and physiological characters was hardly closer than "second cousins," he regarded research in this direction far more fruitful than studies on the relationship between skulls and intelligence.[110]

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[109] M.A. Lewenz and Karl Pearson, "On the Measurement of Internal Capacity From Cranial Circumferences," *Biometrika* 3:4 (1904) 366–97, quotation on page 395; J. Blakeman, Alice Lee, and Karl Pearson, "A Study of the Biometric Constants of English Brain-Weights, and Their Relationships to External Physical Measurements," *Biometrika* 4:1/2 (1905) 124–60; Karl Pearson, "On the Relationship of Intelligence to Size and Shape of Head, and to Other Physical and Mental Characters," *Biometrika* 5:1/2 (1906) 105–46; Karl Pearson, "The Relative Brain-Weight of Man and Woman," *British Medical Journal* 2359 (1906) 650. Porter also mentions that the biometricians were the first to discredit the claim that women had inferior intelligence. See: Porter, *Karl Pearson*, 263.

[110] Karl Pearson, "On the Inheritance of the Mental and Moral Characters in Man, and Its Comparison With the Inheritance of the Physical Characters," *Journal of the Royal Anthropological Institute of Great Britain and Ireland* 33 (1903) 179–237; Karl Pearson, "On Our Present Knowledge of the Relationship of Mind and Body," *Annals of Human Genetics* 1:4 (1926) 382–406, quotation on page 388.

Pearson also contemplated the inheritance and development of these characters. He strongly believed that physical characteristics were inherited and barely influenced by non-biological forces. He wrote to Morant in 1924: "I do not believe in direct environmental changes of the skeleton, because they would involve the inheritance of acquired characters (a blacksmith, who has developed his muscles by his trade, does not hand developed muscles to his progeny), I do think that the environment in 6000 years of action and reaction does tend to change the type," but only through natural and sexual selection.[111] If physical and psychical characters evolved in the same manner and intensity, then environmental factors had as little effect on the mind as on the body. Intelligence, then, could be fostered by good education, but "their origin, like health and muscle is deeper down…they are bred and not created." Hereditary laws determined intelligence. Pearson saw an important task for anthropology here. The history and evolution of man had to be put to practical use: anthropology "must suggest how those laws can be applied to render our own human society both more stable and more efficient. In this function it becomes at least the handmaiden of statecraft, if indeed it were not truer to call it the preceptor of statesman." This is one of the few places where we encounter Pearson's well-known eugenic spirit applied to anthropological research. He urged that British society needed more intelligent men, not better education: "the only remedy, if one be possible at all, is to alter the relative fertility of the good and the bad stocks in the community."[112]

Second, Pearson dismissed the present-day existence of pure races, as his exchanges with Von Török and Myers show. The very long history of evolution and natural selection made such a thing impossible: "such a view would mean an indefinite number of special creations or independent

[111] GMP, KP to GMM, 17.7.1924.

[112] Karl Pearson, "On the Inheritance of the Mental and Moral Characters in Man," 179–237; Karl Pearson, "Inheritance of Psychical Characters," *Biometrika* 12:3/4 (1919) 367–72.

evolutions of man." More in line with the arguments of post-war population geneticists, a "pure" race was a population that was isolated, selected, and had interbred for the longest period.[113] Pearson gave a more extended explanation of his position during his Presidential Address of Section H of the British Association for the Advancement of Science in 1920. He explained to the crowd of anthropologists that within-group variation expressed a population's "relative" purity. If a population was isolated for a long period, they experienced a prolonged subjection to the same environment and limited habitat. This resulted in a smaller intra-racial variability and a "purer" race. "But an absolutely pure race in man defies definition." Pearson urged that folk-wanderings, mixings, and absorptions had most likely been taking place for hundreds of thousands of years. "In the highly and recently hybridised nations of Europe there are really but few fragments of 'pure races' left." Note how Pearson refrained from using the terms "primitive" and "civilized," but echoed the older hierarchical notion that variability increased as populations moved through time.

Racial mixing also explained the lack of association between mental and physical characters: if there ever was a close association, "it would break down as soon as race mingled freely with race." Pearson again stressed that the progress of mankind, therefore, depended on "psycho-physical" characters such as reaction time and mental quickness, qualities that were far more important to the state than head measurements.[114] "If anthropometry is to be useful to the State it must turn from these rusty old weapons, these measurements of stature and records of eye-colour to more certain appreciations of bodily health and mental aptitude – to what we may term 'vigorimetry' and to 'psychometry.'" Knowledge of the minds and psychology of other nations may help prevent wars like World War I. He envisioned a fusion of physical and cultural anthropology that researched

[113] Karl Pearson, "65. Note on Dr. Keith's Review of Professor Arthur Thomson's 'the Ancient Races of the Thebaid'," *Man* 5 (1905) 118–19.

[114] See for an excellent discussion on the history of psychometric tests of reaction time and mental quickness: Maia Woolner, "Time to Cure: Social Acceleration and the Temporal Turn in Modern French Psychiatry, 1870s-1930s" (Ph.D. diss., UCLA, 2020).

psychological, cultural, and physical characters with quantitative methods. This, Pearson claimed, may lead to the recognition of anthropology as "the Queen of the Sciences," a title it deserved. Unlike some other anthropologists, including his own student Geoffrey Morant, Pearson did not believe in science for its own sake and desired to make anthropology a practical and useful discipline.[115]

Finally, the biometricians challenged the primacy of the cephalic index in racial research. In 1897, Pearson still claimed that the cephalic index was "a quantity closely associated with degrees of civilisation and capacity for racial survival in the struggle for existence." He concluded that "on the whole the extra-group struggle for existence does seem to have gone in favour of the brachycephalic races." This type was mostly found in the mainland and in civilized races. Dolichocephalic races had settled in borderlands and islands, "apparently driven out before victorious brachycephaly."[116] In the same year, Pearson published a study with Cicely Fawcett that questioned the unique racial quality of the index: compared to stature, the cephalic index was not more strongly inherited and did not show a different variability. They argued that the index was not a *special* racial character. Although valuable, the biometricians continued to stress that the cephalic index was no special indicator of racial relationships.[117] When Franz Boas questioned the stability of the cephalic index in his ground-breaking immigration study in 1912,[118] Pearson did come to its rescue by touting its stability but again stressed that the index had no unique position. More important, biometricians urged that racial

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115 Karl Pearson, *The Science of Man: Its Needs and Its Prospects* (Cambridge, UK: Cambridge University Press 1920) 1-5.

116 Pearson, "Variation in Man and Woman," 287-291.

117 Cicely D. Fawcett and Karl Pearson, "Mathematical Contributions to the Theory of Evolution. On the Inheritance of the Cephalic Index," *Proceedings of the Royal Society of London* 62 (1897) 413–17.

118 Franz Boas, "Changes in the Bodily Form of Descendants of Immigrants," *American Anthropologist* 14:3 (1912) 530–62.

research should never rely on one or a few characters – only a multiplicity of characters could define a racial group.[119]

Anthropological research and new understandings of race were thus clearly linked to eugenics for Pearson and had an important purpose for the state, well beyond the walls of academia. But the archive offers barely any evidence that the racial research developed in the Biometric lab was put to any practical use. Beyond the use of terms such as "primitive" and "civilized," the lab's publications on race reveal no eugenically spirited comments – Pearson's Presidential Address forms the exception. The biometric study of race seems to have developed as a rather narrow research field aimed at transforming racial science and providing new insight into the academic discussion of race, classification, and history.

Conclusion

With sharp criticism and new tools, Pearson and his colleagues did not wish to discredit anthropology but give it new life and "raise anthropometry and craniology in the future into the category of the more exact sciences."[120] Pearson considered biometry the only way to make racial research more rigorous, scientific, and to pull it out of its "rut." He dismissed other efforts to rejuvenate the science, as his attacks on anthropologists such as Myers and Von Török demonstrate. More generally, the biometricians' interventions were an important challenge to anthropology's common data practices: with larger samples, statistical methods, and a focus on within-group variation, they criticized anthropology's descriptive, typological approach. Not only did it make racial research imprecise, it also led to erroneous ideas about the purity of races, the cephalic index, and

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119 Karl Pearson and L.H.C. Tippett, "On Stability of the Cephalic Indices Within the Race," *Biometrika* 16:1/2 (1924) 118–38.

120 Karl Pearson, "On the Inheritance of the Mental and Moral Characters in Man, and Its Comparison with the Inheritance of the Physical Characters," 201.

the relationship between the skull and intelligence. Pearson offered evolutionary-based interpretations of racial development, centered on notions of temporality, isolation, and mixture, and questioned the idea of independent evolutions. With the biometric methods, race transformed from a morphological object into a metrical aggregate.

As this chapter has shown, these new approaches were incorporated within older frameworks: Pearson did not reject the hereditary relationship between intelligence and the body, nor did he dismiss the project of racial classification. Indeed, within-group variation, so often ignored by anthropologists, became a racial characteristic with linear development in the hands of Pearson. Pearson's theories of evolution and materialism and his statistical methods formed a mutually re-enforcing entangled complex that both challenged and retained anthropological beliefs.

The next chapter will reveal the epistemology beneath the attack on and transformation of anthropological practices, namely the biometrician's desire to remove the researcher's subjectivity from the research process. The technologies Pearson and his colleagues developed attempted to move the labor of producing classifications from the hands and eyes of the researcher to disembodied formulas and instruments. Indeed, the caliper, introduced in the 19th century to make racial classifications more precise, was put to new use by the biometricians. Instead of combining it with morphological expertise, it was coupled with mathematical formulas and newly designed biometric instruments that measured the cranium's form in novel ways. In this way, biometricians attempted to *automate* racial research.

Chapter 3

Biometric Visions of Race. The Skull's Geometric Projections and Paper Representations

Introduction: Becoming a Machine

In his 1863 textbook *Lectures on Man*, German anthropologist Carl Vogt compared geometric drawings of skulls to "ordinary" perspective drawings and wrote: "in practising [geometric drawings] one must abandon all the rules one has hitherto followed, and *consent to become a mere machine*, which does nothing but mark with pencil or pen the point which indicates the perpendicular ray."[1] We have already seen that anthropologists produced geometric projections of skulls on paper as a way remove the subjectivity and the distorted view of the researcher, while at the same time deeply valuing the subjective expertise that was associated with the morphological appraisal of skulls. In assessing skulls for racial origins and relations, the anthropologist was both a machine and a body of knowledge.

[1] From the English translation: K. C. Vogt (translated by James Hunt), *Lectures on Man: His Place in Creation, and in the History of the Earth* (London: Longman, Green, Longman, and Roberts 1864) 76. Italics mine.

Pearsonian anthropology deeply extended this geometric approach with new instruments and projection models. Meanwhile, Pearson began to criticize the morphological method as "unscientific" and imprecise. According to him, racial research could only become rigorous if the researcher's subjectivity was reduced and the labor of producing classifications shifted from the hands and the eyes of the researcher to disembodied statistical formulas, instruments, and cranial projections. Pearsonian anthropology, then, desired to further *mechanize* and *automate* racial research. With this move, a sense of impartiality was built into the biometric methods and instruments. But in line with the analysis of the previous chapter, these transformations were only partial and were placed into existing frameworks of racial classification and human evolution. As we will see, biometric formulas and instruments transformed race into multidimensional data templates and fluid evolutionary objects but did not upend typology. They *quantified* typology.

This chapter first looks at the Coefficient of Racial Likeness, a formula that Pearson developed for racial classification. It explores the formula's application to racial research as well as the critique it received in the 1930s. The chapter then moves onto various biometric technologies that transformed skulls into geometric data templates, such as contours, silhouettes, and cranial elevation models. In the third section, the chapter discusses to what extent these biometric technologies automated research and moved beyond the morphological method.

Measuring Racial Relations: The Coefficient of Racial Likeness

$$\frac{1}{m} \mathrm{S} \left\{ \frac{n_s \, n_{s'}}{n_s + n_{s'}} \left(\frac{\mathrm{M}_s - \mathrm{M}_{s'}}{\sigma_s} \right)^2 \right\} - 1 \pm \frac{\cdot 67449}{\sqrt{2\, m}}$$

Figure 3.1. The Coefficient of Racial Likeness. Republished with permission of John Wiley and Sons, from Ronald Fisher, "'The Coefficient of Racial Likeness' and the Future of Craniometry," *Journal of the Royal Anthropological Institute of Great Britain and Ireland* 66 (1936) 60.

In the 1920s, the research focus of the Biometric lab shifted from correlation and variation studies of race to population comparisons. The biometricians began reconstructing prehistoric racial migration patterns and the effects of evolutionary changes on racial populations.[2] Early in the 1920s, Pearson developed a special formula for the study of migration, evolution, and racial classification: the Coefficient of Racial Likeness, which also became known as the "CRL" (fig. 3.1). With this method, Pearson aimed to make racial research more rigorous: in comparing two racial samples, it made use of all possible measurements, weighed the sample sizes against each other, and compared means and standard deviations with a probable error. The development and use of the CRL shows how Pearson and his workers put race "in the mathematical mill"[3] which ideally lent itself to producing "mathematical truths…independent of human activity."[4] The CRL's life-span also reveals that while the number of racial researchers using the CRL outside of Pearson's lab was scarce, the method was further developed by statisticians and eventually found its way back into physical anthropology later in the 20th century. Statisticians and anthropologists today consider Pearson's formula the first attempt at a multivariate measure of distance.[5]

The CRL distinguished various types of man by determining their racial affinity in measurements. The method integrated the data of *all* craniometric characters measured instead of comparing a few "sacrosanct" characters like the cephalic index one by one. Not only did it process and abstract more information in this way; the analysis of many cranial characters also compensated for the smallness of several cranial series. The formula measured the probability that two samples,

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[2] W.W. Howells "The use of multivariate techniques in the study of skeletal populations," *American Journal of Physical Anthropology* 31 (1969) 312.

[3] Alfred Haddon, "President's Address. Anthropology, Its Position and Needs," *The Journal of the Anthropological Institute of Great Britain and Ireland* 33 (1903) 15.

[4] David Rowe, "Mathematical Schools, Communities, and Networks," in: Mary Jo Nye (ed.) *The Cambridge History of Science 5: The Modern Physical and Mathematical Sciences* (Cambridge UP 2002) 113.

[5] T. Sjøvold, G.N. Van Vark, and W.W. Howells, *Multivariate Statistical Methods in Physical Anthropology* (Dordrecht: D. Reidel Publishing 1984) 2.

thought to represent two races, belonged to the same population. The following data went into the calculation: for each measurement and each sample, the size of the sample, its mean, and standard deviation. The formula then assisted in combining this data for all measurements and generated a positive number. This outcome was explained as a degree of racial likeness or divergence, a lower or higher coefficient suggesting more or less distance. Biometricians interpreted the results as indicators of human migration in time and place. With a single number, the CRL gave a much more direct overview of racial resemblance than long lists with means of cranial characters. After several tests, Pearson determined the following degrees of association and divergence, from 1 to 31 (fig. 3.2):

Degrees of Association			Degrees of Divergence		
Grade	**Range**	**Class**	**Grade**	**Range**	**Class**
I	Less than *1*	Very intimate Association	I	*13—16*	Slight Divergence
II	*1— 4*	Close Association ..	II	*16—19*	Moderate Divergence
III	*4— 7*	Moderate Association ...	III	*19—22*	
IV	*7—10*	Slight Association ...	IV	*22—25*	Marked Divergence
V	*10—13*	Doubtful Association ...	V	*25—28*	
—	—	—	VI	*28—31*	
—	—	—	VII	Over *31*	Very wide Divergence

Figure 3.2. Degrees of Association and Divergence. Republished with permission of Oxford University Press from Karl Pearson, "On the Coefficient of Racial Likeness," *Biometrika* 18 (1926) 112.

Pearson identified some theoretical difficulties with the CRL. First, the method gave an *indication* of resemblance or distance based on the given data. "It is not a true measure of absolute divergence, and must not for a moment be considered as such, but nevertheless we shall speak of it, for convenience, as if it were an absolute measure of racial affinity," biometrician Geoffrey Morant warned.[6] Association and divergence were relative but convenient terms. Moreover, an accurate measure of racial resemblance required the means of 30-40 cranial characters in 50-100 local races as well as the standard deviations and correlations of the means among themselves, the intra-racial

[6] G.M. Morant, "A First Study of the Tibetan Skull," *Biometrika* 14: 3/4 (1923) 205.

variations and inter-racial correlations. Because this data was not yet available, the CRL operated based on two assumptions. First, the biometricians presumed there was no high intra- and inter-racial correlation between cranial characters and thus eliminated correlation from the formula altogether.[7] Second, the CRL assumed equal variability for all races. Surprisingly, the biometricians departed from their earlier notion that within-group variation was a racial character, which had been central to several racial studies in the 1900s and 1910s, as the previous chapter discussed. Rather than maintaining that data on variability uncovered the "civilized" or "primitive" character of races, by the 1920s the biometricians argued that "the different races of men are not widely divergent in variability."[8] This statement was far removed from Fawcett, Lee, and Pearson's earlier work on racial variability. Unfortunately, the sources do not explain this shift in thinking. More practically, however, one could simply not accurately determine intra-racial variability of very small series. To produce more accurate outcomes, the biometricians used the variability data from their longest series of a single race for the CRL – Flinders' 1800 Egyptian skulls or the "E series." The within-group variation of the Egyptian series thus became representative for any population whose measurements were inserted into the CRL.

As long as the inter-racial differences were unknown because of lacking data, the CRL was a provisional measure, a "stop-gap" until more data and a better method became available. "If any one has a sounder coefficient to propose, I shall not be the last to welcome and use it," Pearson promised when he introduced the method in *Biometrika* in 1926.[9] Unlike his attacks on physical anthropology's methods discussed in chapter 2, Pearson was more humble about the formula's promise for craniometry and anthropology. "On the whole, while it contradicts some current

[7] This assumption aligned with previous findings: Fawcett and Lee determined in the early 1900s that cranial characters did not correlate much. See: Cicely D. Fawcett and Alice Lee, "A Second Study of the Variation and Correlation of the Human Skull, With Special Reference to the Naqada Crania," *Biometrika* 1:4 (1902) 408–67.

[8] Karl Pearson, "On the Coefficient of Racial Likeness," *Biometrika* 18:1/2 (1926) 108.

[9] Karl Pearson, "On the Coefficient of Racial Likeness," 111.

anthropological beliefs, and suggests some hitherto unsuspected relationships, it does not give results wildly discordant with the beliefs and impressions of anthropology. It seems rather to confirm, to extend and in special cases to correct them."[10]

The lab's workers and those who passed through it used this "serviceable tool" extensively. The method was first introduced in exploratory research published by the lab's craniometric workers Miriam Tildesley, Geoffrey Morant, and Prasanta Mahalanobis. Pearson formally introduced the formula in *Biometrika* in 1926, when more than 760 calculated CRLs had confirmed its usefulness. With the method, the biometricians created large classification schemes based on the found "associations" and "divergences." In the early 1900s, Pearson had classified races as "civilised" or "primitive" and placed them along a fixed scale of civilizational progress, as was common in racial research around the time. By the 1920s, he argued that "it is proper to look upon Anglo-Saxons and modern English as *associated* races, but on Chinese and English as *divergent* races."[11] These terms not only signaled new ways of understanding racial comparison, but also showed a different interpretation of human evolutionary history: if two races had a high degree of divergence, this meant that they diverged at an earlier point in time than more closely associated races. Rather than situating races along a linear line of development, Pearson deployed a more loose and flexible interpretation of evolutionary history with the CRL, stressing racial convergence, mixing, and separation in time and space. Moreover, as the name of the method indicated, inquiries were more focused on finding resemblances than on searching for widely divergent races.

The formula first appeared in 1921, in a paper of Crewdson Benington student Miriam Tildesley. With the CRL, she explored the racial origins and relations of 142 skulls from Myanmar (formerly named Burma), collected by a colonel for Pearson's lab. Tildesley took measurements of

[10] Karl Pearson, "On the Coefficient of Racial Likeness," 111-112.

[11] Karl Pearson, "On the Coefficient of Racial Likeness," 113. Italics mine.

the skulls and compared these to data of neighboring races, such as Malayan, Chinese, Hindu, and Dravidian populations. She explained that tables comparing these measurements only gave a vague impression of the racial relationships. The CRL, instead, measured these relations by combining *all* the data into a *single* number. The results gave "at once a much better appreciation of the degree of resemblance between the seven races they represent than could have been obtained from a comparison of seven columns of means for twenty to thirty individual characters." Tildesley concluded that the South Burmese population consisted of a pure Burman race, closely allied to the Malayans, a second race, allied to the Chinese neighboring type, and a hybrid race close to both Malayan and Chinese types.[12]

In 1923, Geoffrey Morant published the second paper that used the CRL to analyze 32 Tibetan skulls from the Royal College of Surgeons. He explained that Tibet was an important place for racial research: it bordered between Mongoloid and Indian races and was inhabited by seminomadic pastoral tribes. Using qualitative data from ethnographic and historical studies, Morant hypothesized that the cranial sample contained two types, one mixed Mongoloid type from Bengal and a non-Mongoloid type from the Tibetan province of Kham, possibly the "aboriginal" inhabitants of Tibet. For comparative purposes, he compiled data from publications on other Asiatic races, such as the "primitive" Aino and Moriori peoples, Egyptians, and Whitechapel English. The CRL, based on 31 cranial characters and 12 indices, confirmed a marked difference between the types. It also revealed that the first Tibetan type was associated with Burmese, Malayans, and Chinese types and thus likely a mixed race. This was "quite consistent with the philological and historical evidence which tells of Tibeto-Burmese invasions from north-west China and they show that all these oriental peoples are very similar in type."[13] The second type, markedly different, was

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[12] Miriam Tildesley, "A First Study of the Burmese Skull," *Biometrika* 13:2/3 (1921) 176–262, quotation on page 248.

[13] G.M. Morant, "A First Study of the Tibetan Skull," 209.

possibly a "pure stock," as Morant had hoped. Indeed, the Kham skulls were quite similar to Moriori skulls, which prompted the author to compare the sample to other "aboriginal peoples." The found similarities excited Morant:

> The Moriori, Maori, Aino, Fuegians and Eskimo are races of man as geographically remote from each other as any which could be named and it may be thought idle to seek for affinities between them. But they have this in common, that they are border-land peoples who have been pushed out to the far extremities of the great land masses of the world and having once arrived at their present habitats they were probably undisturbed by the great race upheavals with might exterminate or hybridise other races. Hence these primitive peoples appear to have preserved a more or less pure type since very early times.[14]

The Kham Tibetans could be of a similar sort, their isolation resulting from the inaccessibility of Eastern Tibet.

Morant proposed that a "fundamental primitive human type" had widely scattered and fragmented across the globe. Even though the data were meagre, the CRL suggested that "perhaps we are warranted in saying now that craniometric evidence may necessitate theories of very extensive prehistoric wanderings of human types."[15] Similarly, Pearson concluded that "a study of 'fringe' peoples by aid of the C.R.L. may lead us to new ideas on the passage of human racial waves over the whole earth's surface."[16] A lower or higher coefficient placed human migration in time and space. For instance, the close association between the Moriori and the Fuegians and the lesser association between Moriori and Maori could indicate an "early transfer" from Antarctic lands to South America. The CRL thus facilitated the study of prehistoric migration patterns and the development of human races across the landmasses of the earth. The term "primitive" remained part of the biometrician's vocabulary but no longer referred to hierarchical civilization levels, at least

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[14] G.M. Morant, "A First Study of the Tibetan Skull," 219-220.

[15] G.M. Morant, "A First Study of the Tibetan Skull," 224.

[16] Karl Pearson, "On the Coefficient of Racial Likeness," 114.

not explicitly. Instead, it referred to time and place, meaning an (ab)original race that survived through isolation and without hybridization.[17] With the CRL, the biometricians introduced a refashioned understanding of race, one that heavily relied on statistical constants and positioned the likeness of human types on a flattened plane of time and space. Variability and levels of civilization no longer bound races to a scale. Nevertheless, the mythology of pure, primitive races continued to capture the imagination of biometricians and their understanding of human prehistory and racial evolution.

The CRL was mostly used in racial studies published in *Biometrika*. It was an analytical tool for many Crewdson Benington students who Pearson then invited to publish their results in his journal. Despite its main use in the study of human races, the CRL held a larger promise according to Pearson: "it applies to a wide variety of statistical problems, and to the local races of many species other than those of homo sapiens."[18] Only once, however, was the CRL used for a non-human study and determined similarities between various silk-worms.[19]

In the late 1920s, biometricians began to draw out the racial relationships in distinctive two-dimensional diagrams and published them alongside the tables of calculated coefficients. These images connected the names of samples with lines representing the coefficients and thus visualized the arrangements suggested by the method. It is likely that biometricians began drawing these images to assist with the interpretation of the CRL's values: organizing the results on paper allowed them to visualize the ways in which the formula grouped samples together and helped establish a classification. These visuals proved to be convenient in "reducing to some order the tangle of inter-

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[17] The introduction discussed how post-war researchers studied "primitive" races in the context of their isolation. It is interesting to note that this shift in the meaning of "primitive" from hierarchical to isolated already took place before World War II, in the context of biometric studies of race.

[18] Karl Pearson Archive (KPA), Box 82, 3/6/42/1, "A preliminary attempt to obtain a world population of Homo Sapiens and a discussion of the resulting Coefficients of Racial Likeness."

[19] A.V. Anoochin, "Similarities and Differences in the Species of the Mulberry Silk-worm," *Biometrika* 20A (1928) 69-78.

relationships" and thus built on the idea that the method gave a quick and clear overview of racial relationships.[20] The diagrams further reduced and abstracted racial data and allowed the reader to see likeness and difference *at a glance*. Long tables of characters and measurements were collapsed into the CRL; the tables of CRL values were collapsed into the images. A single illustration visualized all the data available to the researcher.

Various examples of these diagrams show that there was no standardized practice. The number of groups analyzed and compared often determined the organization of the images. In smaller studies, lines simply connected the names of samples (fig. 3.3a). In bigger studies, biometricians created constellations of races that clustered samples belonging together based on their CRLs (fig. 3.3b). The lines connecting samples within and between clusters represented the coefficients. Biometricians created various schemes, whatever fulfilled their needs, using solid strokes and dotted lines to signify CRLs small and large (figure 3.3c.). Each scheme was unique and presented with a legend explaining the author's choices. "The method of the coefficient of racial likeness…is not a simple rule of thumb," Geoffrey Morant and anatomist Gerhardt von Bonin explained. "The way in which its values have to be interpreted in order to yield useful results has to be determined empirically." This also meant that Pearson's values of association were not uniformly adopted in practice. The dissimilarity between these diagrams suggests that authors often created these images themselves, which further indicates that these visuals assisted biometricians in drawing conclusions based on CRL values.[21]

The images gave insight into the correspondence between geographic distributions of populations and the spatial-racial arrangements suggested by the formula. In several cases, the

[20] G.M. Morant, "A Preliminary Classification of European Races Based on Cranial Measurements," *Biometrika* 20B (1928) 301–75, quotation on page 328.

[21] G. von Bonin and G.M. Morant, "Indian Races in the United States. A Survey of Previously Published Cranial Measurements," *Biometrika* 30:1/2 (1938) 96. One hand is recognizable in various diagrams which I believe to be Morant's. It suggests that Morant was responsible for drawing up various CRL schemes in the lab. As the main researcher on race in the lab and assistant editor for *Biometrika* (see chapter 4) this hypothesis is not unlikely.

authors found little agreement. This was probably due to migrations, they hypothesized. Migrations could also explain the close relationships between widely separated nomadic races. Occasionally, the results surprised the biometricians: classifications produced through "purely statistical means" sometimes challenged accepted theories. Crewdson Benington student Elisabeth Kitson, for instance, found in her study of African cranial series that the method grouped together series that were separated by hundreds of miles. She observed that "no sharp distinction can be made between Western, Eastern and Southern negro races." Her classifications, therefore, contradicted common theories that distinguished between African races. Kitson determined that these theories were based on imprecise measurements of living bodies. Skulls and statistical methods, on the other hand, offered the "most hopeful approach for future research."[22] In other cases, a close overlap between racial relationships and geographical positions confirmed existing theories. In 1938, Morant and Von Bonin published their study of Native American populations and determined that "the results of the cranial comparisons appear to be in favour of the hypothesis which postulates an immigration into the American continent via the Straits of Bering" (fig. 3.3d).[23]

[22] Elisabeth Kitson, "A Study of the Negro Skull with Special Reference to the Crania from Kenya Colony," *Biometrika* 23:3/4 (1931) 272-314, quotations on pages 299 and 300.

[23] G. von Bonin and G.M. Morant, "Indian Races in the United States," 120.

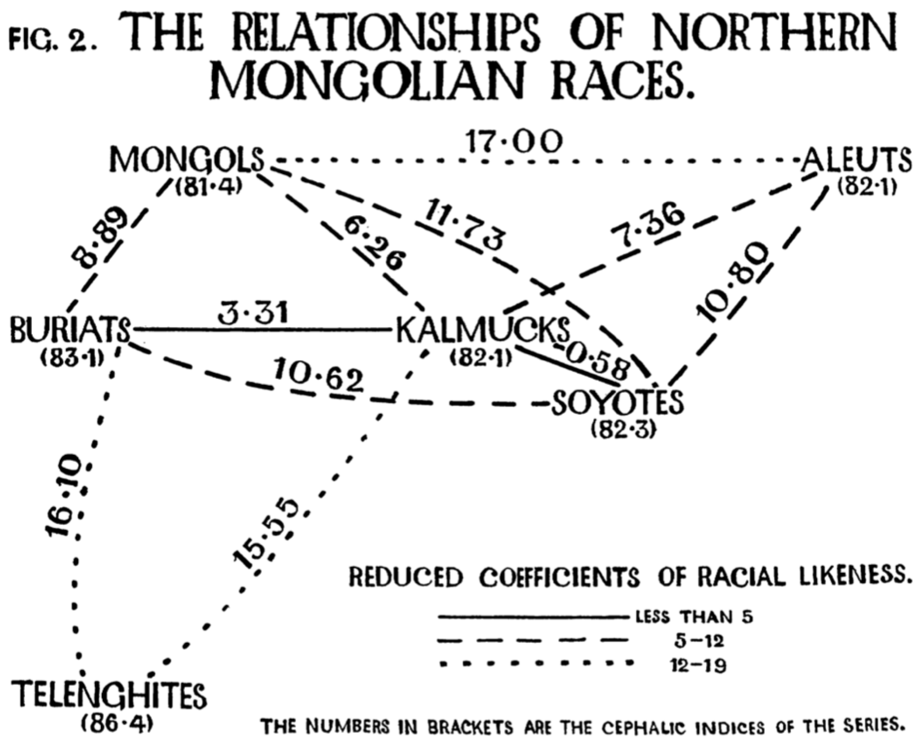

Figure 3.3.a. "The relationship of Northern Mongolian Races." Republished with permission of Oxford University Press from Ting Liang Woo and Geoffrey Morant, "A Preliminary Classification of Asiatic Races based on Cranial Measurements," *Biometrika* 24 (1932) 117.

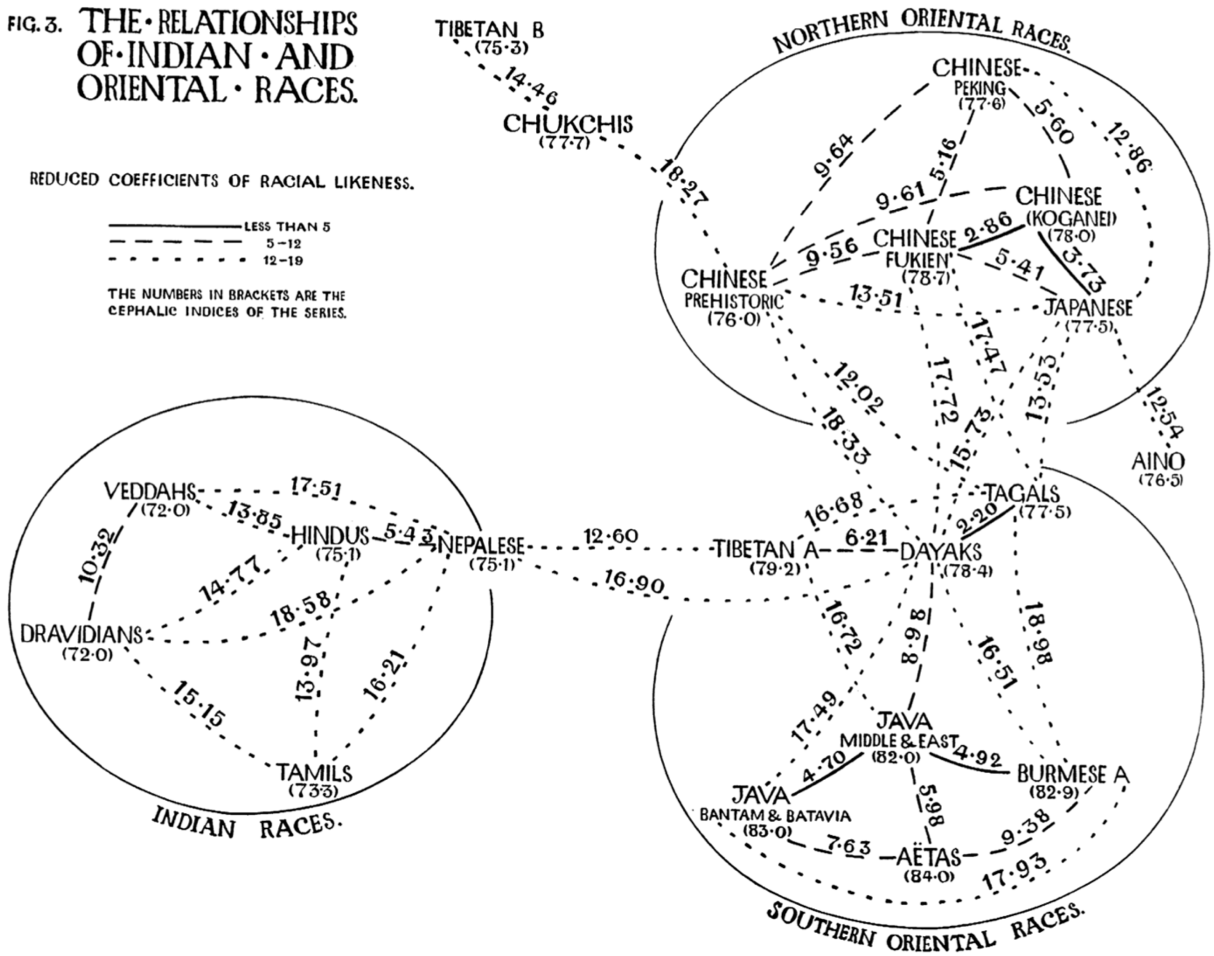

Figure 3.3.b. "The Relationships of Indian and Oriental Races." Republished with permission of Oxford University Press from Ting Liang Woo and Geoffrey Morant, "A Preliminary Classification of Asiatic Races based on Cranial Measurements," *Biometrika* 24 (1932) 123.

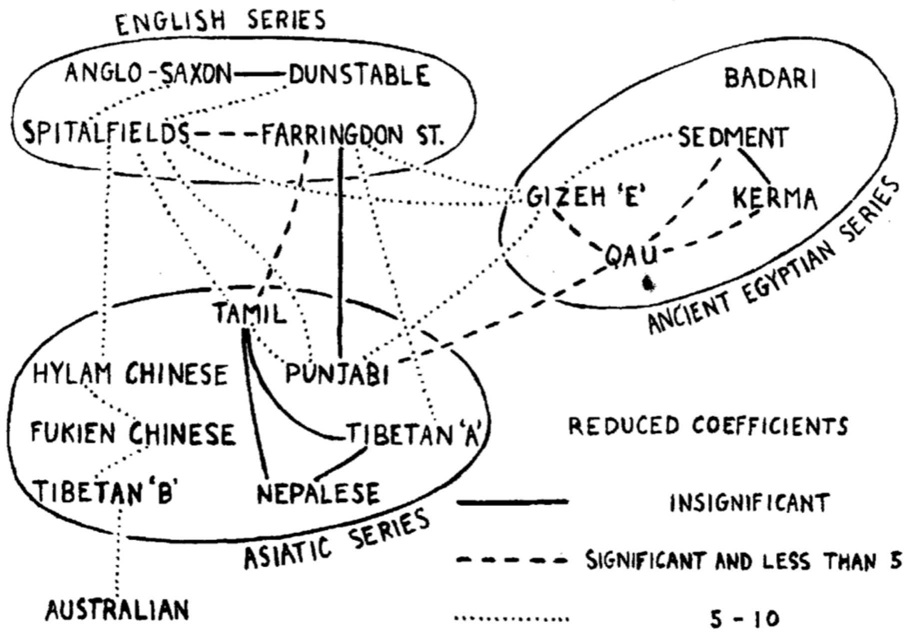

Figure 3.3.c. English, Asiatic, and Ancient Egyptian series. Republished with permission of Oxford University Press from Frank Cleaver, "A Contribution to the Biometric Study of the Human Mandible," *Biometrika* 29 (1937) 104.

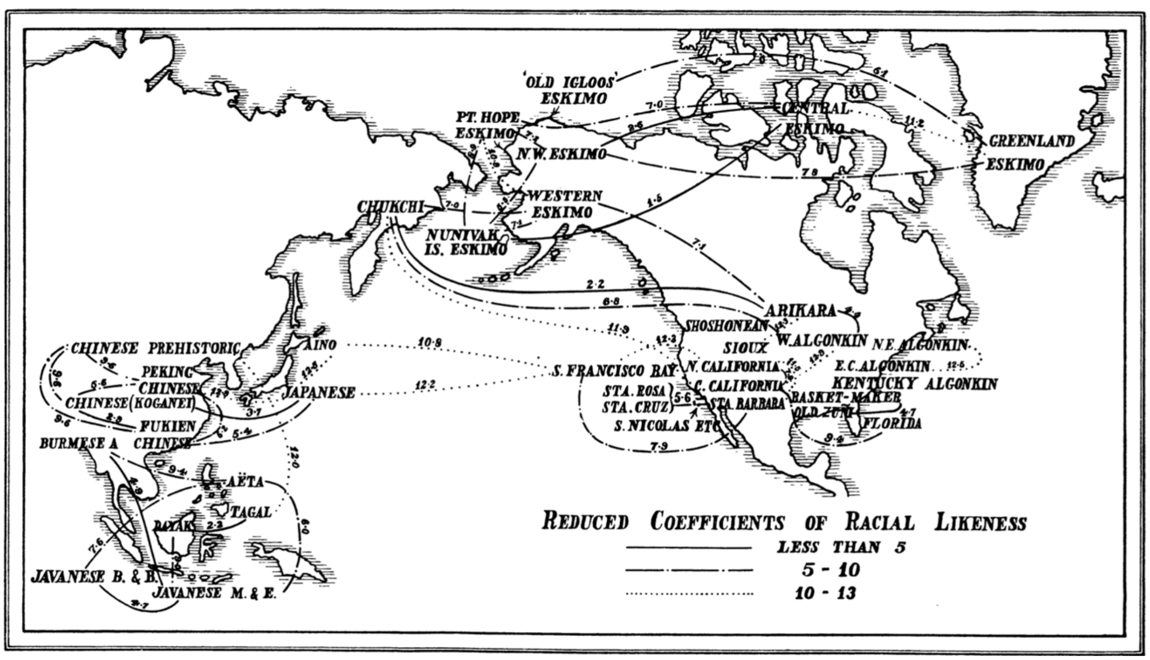

Figure 3.3.d. Reduced Coefficients of Racial Likeness of American and Oriental Series of Crania overlaid on a map. Republished with permission of Oxford University Press from Gerhardt von Bonin and Geoffrey Morant, "Indian Races in the United States. A Survey of Previously Published Cranial Measurements," *Biometrika* 30 (1938) 119.

Despite unexpected results and more fluid interpretations of racial evolution, biometricians often deployed pre-existing racial schemes in creating racial constellations. In a paper from 1932, Chinese anthropologist Wu Ding-Liang and Morant classified Asiatic races "by using purely quantitative methods." The CRL revealed surprising affinities between widely separated groups and thus exposed a long history of migrations in the region. In producing the classifications, however, they followed common anthropological schemes, clustering the many datasets around "Indian races" and "Oriental races" (fig. 3.3b). Indeed, the authors concluded that their classification "in its broad outline it agrees with most other classifications which have been suggested by anthropologists using less exact methods." More confusingly, they determined that in the end there *was* a close association between geography and racial affinity.[24] The persistence of racial assumptions also became evident when the method failed. In studying series of mandibles, Crewdson Benington student Frank Cleaver found that the CRL only gave unexpected results. Because the samples were too small, he argued, the method gave "insignificant coefficients in cases where distinct differentiation *would have been expected*." "In 6 cases out of 136 comparisons there is no evidence of a significant difference judging from the mandibular measurements, although the series would be expected to represent quite distinct races." These results, Cleaver argued, therefore revealed the inefficiency of the method for mandibular studies.[25] The studies above show that preconceived notions about racial relationships were always at play in the construction of classifications, even with "purely" statistical methods that seemingly automated the reduction of measurements to theories of race.

These diagrams were thus about race, place, and time. They offered a different type of family tree model than was common in anthropological practice, as discussed in chapter 1. On the one

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[24] Ting Liang Woo and G.M. Morant, "A Preliminary Classification of Asiatic Races Based on Cranial Measurements," *Biometrika* 24:1/2 (1932) 132.

[25] Frank H. Cleaver, "A Contribution to the Biometric Study of the Human Mandible," *Biometrika* 29:1/2 (1937) 80–112. Quotation on page 109 and 111. Italics mine.

hand, the lines suggested a fluidity between samples and populations. The images materialized the understanding that racial difference was the result of isolation and migration. They can be interpreted as tree models on a flattened surface that represented evolutionary time and space. On the other hand, the grouping and clustering occasionally followed traditional schemes. Moreover, equal within-group variability for all samples erased the variation and shading within and between groups that would later become central in "clinal" visuals. Through names, constellations, and lines, the method and visuals abstracted and erased individual difference. What is more, in grouping all the measurements, the images did not show how different measurements created different arrangements. These diagrams can thus be placed on a continuum between hierarchical monophyletic trees and fluid clines.[26]

Like some of his other biometric tools, the Coefficient of Racial Likeness did not appeal much to anthropologists not affiliated with the Biometric Laboratory. The large amount of hand computation it required hindered a wide adoption of the method.[27] Moreover, Pearson's sharp critiques of anthropological methods may have made some scholars reluctant to adopt his methods. Most anthropologists remained silent about the CRL, some made modest use of it.[28] They generally lacked the mathematical or statistical training required to adopt such methods, let alone critique them. Some did provide commentary: German anthropologist Emil Breitinger called the method "weak" when he reviewed a publication that used the CRL to determine racial affinities between

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[26] See for a fuller discussion about tree models and clines: Rachel Caspari, "From Types to Populations: A Century of Race, Physical Anthropology, and the American Anthropological Association," *American Anthropologist* 105:1 (2003) 65–76; Marianne Sommer, *History Within. The Science, Culture, and Politics of Bones, Organisms, and Molecules* (Chicago: University of Chicago Press 2016).

[27] E. Breitinger, "Gruppenrisse vom Hirnschädel. Zugleich ein weiterer Beitrag zur Differentialdiagnose zwischen nordischen und mittelländischen Schädeln," *Anthropologischer Anzeiger* 15:3/4 (1938) 302.

[28] T. Sjøvold, G.N. Van Vark, and W.W. Howells, *Multivariate Statistical Methods in Physical Anthropology,* 2.

peoples in India.[29] American anatomist Wingate Todd found the method "dubious" and did not believe it could reinvigorate anthropology.[30] More extensive critiques of the CRL came from those who managed to engage with its mathematics. Indeed, "the best criticisms of statistics are made by statisticians," American anthropologist Harold E. Driver wrote in 1954.[31] The criticism centered on the method, its assumptions, and outcomes, but never spoke about the diagrams. The first to express his concern was Indian statistician Prasanta Mahalanobis. In the summer of 1927, when he worked in Pearson's lab, Mahalanobis began improving the CRL. His improvement, the "D^2 method," measured the actual magnitude of distance between groups – the CRL was merely a test of divergence between two samples. Moreover, Mahalanobis found that larger samples affected the CRLs outcome: the bigger the sample, the bigger the degree of divergence. Instead, his D^2 measure remained constant with different sample sizes. Despite Pearson's promise that he would welcome any improvement of the method, Mahalanobis's suggestions fell upon deaf ears. Pearson rejected Mahalanobis's D^2 method and defended that the CRL had proven its value for racial research. Upon return in India, Mahalanobis continued to develop his approach and found that it was more in line with anthropological "facts" and thus a better classification tool than the CRL. The D^2 method, also known today as the Mahalanobis distance, continued to be developed by statisticians.[32]

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[29] Emil Breitinger, "Reviewed Work(s): The Racial Affinities of the People of India. Census of India 1931, Vol I P. III. By B.S. Guha," *Anthropologischer Anzeiger* 12:3/4 (1936) 271-273.

[30] Ales Hrdlicka Archive, Box 11, Todd to Hrdlicka, 27.12.1935.

[31] Harold Driver, "Statistics in Anthropology," *American Anthropologist* 55:1 (1952) 55.

[32] P.C. Mahalanobis, D.N. Majumdar, M.W.M. Yeatts, and C. Rao, "Anthropometric Survey of the United Provinces, 1941: A Statistical Study," *Sankhyā: The Indian Journal of Statistics* 9: 2-3 (1949) 89–324, see "Appendix I. Historical Note on the D-Statistic," 237-240; P.C. Mahalanobis, "On Tests and Measures of Group Divergence," *Journal of the Asiatic Society of Bengal* NS 26:4 (1930) 541-588; P.C. Mahalanobis, "On the Generalized Distance in Statistics," *Proceedings of the Natural Institute of Science* 2 (1936) 49-55. Pearson rejected Mahalanobis's research on the comparison of the D^2 method and the CRL but nevertheless published the data for this research in *Biometrika*, after ordering Morant to rework it considerably: Geoffrey Morant Papers (GMP), KP to GM, 13.7.1930; P.C. Mahalanobis, "A Statistical Study of Certain Anthropometric Measurements From Sweden," *Biometrika* 22:1/2 (1930) 94–108.

Polish statistician and anthropologist Jan Czekanowski criticized the CRL in 1932 for being unnecessarily complicated. "The computational burden of the researcher demanded by this procedure cannot be substantiated," he wrote in the German journal *Anthropologischer Anzeiger*. Czekanowski had developed his own distance measure for quantitative characters in 1909, which he called the *durchschnittlichen Differenzen* method. This method took the average absolute difference between various characters as a measure of racial difference. He tested the CRL against his simpler method and concluded that it gave equal results while requiring much less labor.[33] Possibly in response to this article, Pearson wrote to Morant that "you know I have never been satisfied with the C.R.L.; it was only a stop-gap, and the silly sheep may break that down – or the goats butt it down – any day, and be proud of their achievement."[34]

One of the most widely read critiques came from Pearson's University College colleague and statistician Ronald Fisher. In 1936, he published an article "'The Coefficient of Racial Likeness' and the Future of Craniometry," in the journal of the British Royal Anthropological Institute. It was not the first time that Pearson and Fisher budded heads – over the years, they had developed a heated rivalry. Pearson, 33 years his senior, had recognized Fisher's statistical talents early on and had offered him a position in the Galton laboratory. Fisher declined and moved to the agricultural field station in Harpenden, where he began a long career of experimental work on statistics and genetics. Fisher studied statistical tools in and by themselves, as opposed to the generation before him that developed those tools to study extra-statistical questions. Fisher began to criticize and correct Pearson's methods, such as his chi-square test and the method of moments, to which Pearson

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[33] Jan Czekanowski, "Zur Differential Diagnose der Neandertalgruppe," *Korrespondenzblatt der Deutschen Gesellschaft für Anthropologie* 40 (1909) 44-47; Jan Czekanowski, "'Coefficient of Racial Likeness' und 'Durchschnittliche Differenz,'" *Anthropologischer Anzeiger* 9:3/4 (1932) 227-249, quotation on page 248.

[34] GMP, Pearson to Morant, 16.8.1932.

strongly disagreed. The headstrong character of both men created a situation in which both "came to despise one another," Porter tells us.[35]

By publishing it in the journal of British anthropologists, Fisher directed his critique of the CRL towards the anthropological, not statistical, community. In the article, he did something that Pearson had refused to do: he explained in non-technical language the main statistical concepts that underlay Pearson's method. This was necessary because "the majority of anthropologists, as of biologists, feel so unfamiliar with statistical reasoning as to accept, in some cases, alleged statistical conclusions with something akin to credulous awe, or in others to reject them with indignation as introducing unnecessary confusion into otherwise plain issues."[36] Fisher clarified the statistician's main question – *were cranial samples drawn at random from the same population?* – main method – *calculating a probability* – and conclusions – *when was a difference between samples significant*? Fisher explained that the CRL was a test of significance, not racial difference. The name "Coefficient of Racial Likeness," however, *did* suggest that it was a measure of actual difference and thus created a misunderstanding of the method's capabilities. Moreover, a test of significance either gave affirmative or negative answers. Unlike what the biometricians had assumed, "high values of the coefficient of racial likeness do not demonstrate that the races showing them differ more in their cranial measurements than races showing lower, though significant, values."[37] Fisher disagreed with the idea that a higher coefficient indicated separation at an earlier point in time. Instead, the values only showed that the samples were larger than needed for demonstrating the significance of difference. Second, he considered the CRL "defective" because it did not distinguish between measurements and treated

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[35] Theodore M. Porter, *Karl Pearson: The Scientific Life in a Statistical Age* (Princeton: Princeton University Press 2004) 251-257; 277-278, quotation on page 251; MG Kendall, "Ronald Aylmer Fisher, 1890-1962," *Biometrika* 50:1/2 (1963) 1–15; Donald A. MacKenzie, *Statistics in Britain: 1865-1930; the Social Construction of Scientific Knowledge* (Edinburg: Edinburgh University Press 1981) 209-10.

[36] Ronald A. Fisher, "The Coefficient of Racial Likeness" and the Future of Craniometry," *Journal of the Royal Anthropological Institute of Great Britain and Ireland* 66 (1936) 57.

[37] Ronald A. Fisher, "The Coefficient of Racial Likeness," 61.

them as statistically independent. In ignoring the correlation between measurements, the method frequently gave very high or low values. Furthermore, the coefficient increased with the number of measurements put into the formula.

"It will have occurred to the reader who has followed this article so far that the science of craniometry must be in a very primitive condition, if it is still concerned with clarifying its fundamental notions at the stage we have been discussing," Fisher concluded.[38] Anthropologists had accumulated a mass of observational data, but their theoretical concepts lagged behind. Racial research should not merely center on the statistical analysis of this data; it should determine *which* measurements had any significant taxonomic value. Fisher argued that racial scientists should turn to living populations instead of skeletal material to study the history of race: "living heads" were available in much larger quantities and researchers often knew the sex, blood relations, and nationality of living people.

Fisher's article refrained from naming Pearson or any of the lab's workers – he only mentioned Morant in a footnote. Nevertheless, Morant and Pearson were unhappy with the piece. "He practically concludes that craniometry is a worthless science," Morant wrote to Pearson. Pearson responded that it was "sad when all we want as men of science is to advance our knowledge, that a man really competent to help – who makes however as many mistakes as most of us – should spend his time in idle attacks instead of showing the better way."[39] Fisher designed his own multivariate method for comparing human populations, the method of linear discriminant analysis.[40]

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[38] Ronald A. Fisher, "The Coefficient of Racial Likeness," 62.

[39] GMP, GM to KP21.7.1935; KP to GM 22.7.1935.

[40] Ronald A. Fisher, "The Use of Multiple Measurements in Taxonomic Problems," *Annals of Eugenics* 7:2 (1936) 179-188; Ronald A. Fisher, "The Statistical Utilization of Multiple Measurements," *Annals of Eugenics* 8:4 (1938) 376–86.

Another blow to the CRL came a year after Pearson's death. At the 1937 meeting of the American Association of Physical Anthropologists, Harvard anthropologist Carl Seltzer presented his critique. In a more critical tone than Fisher, Seltzer opened that "the Coefficient of Racial Likeness is neither a good nor a sound statistical measure for the use for which it is intended."[41] On a "constructional" level, the method ignored the correlation of measurements and the values went up or down depending on the size of the sample and the amount of characters used – arguments quite in line with Fisher's critique. Seltzer dismissed the biometricians' "fallacious" assumption of equal variability among racial populations and problematized the wide use of the Egyptian standard deviations. Regarding the theoretical assumptions of the CRL, Seltzer echoed Fisher that the method was merely a test of significance. He considered giving equal weight to all measurements "unmistakably erroneous." The assumption that human characters reacted in the same manner in racial mixtures disregarded the newly studied question of dominant and recessive elements. Finally, Seltzer dismissed the coefficient for giving "palpably absurd results." Morant, for instance, "would have us believe that two groups of English crania are no closer racially than a group of English and Chinese skulls." Seltzer rejected the method's surprising relationships and its suggestions about prehistoric migrations.[42]

Anthropologist Thomas Dale Stewart later recalled that no American physical anthropologist dared to use the method after Seltzer's critique. In 1938, Morant defended that Pearson and those who had used the method for the past ten years fully recognized its limitations and imperfections. Nevertheless, Morant still considered it the best method for determining racial bonds and affinities. He protested that theoretically more correct formulas that took correlations into account, i.e.

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[41] Carl C. Seltzer, "A Critique of the Coefficient of Racial Likeness," *American Journal of Physical Anthropology* 23:1 (1937) 101.

[42] Carl C. Seltzer, "A Critique of the Coefficient of Racial Likeness," 107.

multivariate approaches, involved much more arithmetical labor. The availability of high-speed computers in later years would change such considerations.[43]

Even though the CRL was mostly used within Pearson's circle, those who passed through the laboratory ensured the spread of the method. Via Gerhardt von Bonin and Raymond Pearl, the CRL was used in publications in the German *Anthropologischer Anzeiger* and Pearl's American-based *Human Biology*.[44] Via Prasanta Mahalanobis the method spread to India.[45] Chinese anthropologist Wu Ding-Liang studied statistics and anthropology at Pearson's lab between the late 1920s and mid 1930s. Wu continued his biometric research at the Anthropological Laboratory of the Chinese Academia Sinica, where he trained several Chinese physical anthropologists and introduced them to the CRL.[46] The critiques published in German, American, and British journals further show the transnational movement of Pearson's method. Even though anthropologists did not adopt the method as widely as Pearson possibly would have hoped, it traveled and was discussed internationally.

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43 T.D. Stewart, "Comments. Problems in Physical Anthropology of the Southwest," *American Anthropologist* 56:4 (1954) 619; G. von Bonin and G.M. Morant, "Indian Races in the United States. A Survey of Previously Published Cranial Measurements," *Biometrika* 30:1/2 (1938) 96; T. Sjøvold, G.N. Van Vark, and W.W. Howells, *Multivariate Statistical Methods in Physical Anthropology,* 3; W.W. Howells, "Yesterday, Today and Tomorrow," *Annual Review of Anthropology* 21:1 (1992) 10.

44 See for instance G. von Bonin, "Der Koeffizient der Rassenähnlichkeit und seine Anwendung am Lebenden," *Anthropologische Anzeiger* 7 (1930) 82–102; Raymond Pearl and John R. Miner, "On the Comparison of Groups in Respect of a Number of Measured Characters," *Human Biology* 7:1 (1935) 95-107.

45 Another user of the CRL was Indian anthropologist Biraja Sankar Guha. Guha obtained his Ph.D. in anthropology at Harvard (1924) and joined the University of Calcutta in 1926. For his use of the CRL, see: B.S. Guha, *The Racial Affinities of the People of India. Census of India 1931, Vol I India Part III Ethnographical, A. Racial Affinities of the Peoples of India* (Simla: Government of India Press 1935). It is possible that Guha learned of the CRL through Mahalanobis.

46 It seems that Wu, who published under the name Ting-Liang Woo, may have obtained his Ph.D. degree at University College based on his research done at Pearson's Lab and published in *Biometrika*. For more on Wu, see: "Wu Ding-Liang and the Anthropology Division of the Institute of History and Philology," Fortress Village – The Ethnic Minorities of Southwest China. http://ethno.ihp.sinica.edu.tw/en/remaking/main_4-09.html (accessed February 21, 2019); Ekaterina A. Pechenkina, "From Morphometrics to Holistics: the Emergence of Paleopathology in China" in: J.E. Buikstra and Charlotte Roberts (eds.), *The Global History of Paleopathology: Pioneers and Prospects* (Oxford: Oxford University Press 2012) 347-348; Frank Spencer (ed.) *History of Physical Anthropology. An Encyclopedia* (New York: Garland Publishing 1997) 273-282.

The CRL had an important afterlife in statistics and post-war population studies. Mahalanobis and Fisher further improved Pearson's method. Mainly, they introduced covariation between characters, thus transforming the method from a univariate analytical tool to a multivariate method. Modifications of these multivariate approaches have been used to measure genetic distances and cranial shape variation in population studies. The CRL has become a forgotten page in the history of anthropology but remains well-known as the predecessor of these distance measures in statistics and genetics.[47]

Geometric Visions of Skulls

The CRL attempted to mechanize and automate racial research by shifting the labor of producing racial classifications from the researcher to the formula. The biometricians claimed that, despite the method's limitations, this approach ensured a more "scientific" outcome: it considered all the available data and used the latest reduction methods. It abstracted morphological race by collapsing skulls into numbers, tables, and two-dimensional data visuals. The CRL, however, was not the only biometric technology that sought to automate racial research. By extending anthropology's geometric approach to crania, biometric techniques and instruments turned skulls into multidimensional data templates that captured cranial dimensions in new ways. Like the CRL, the projections' novelty did not move the method beyond common racial frameworks. Both approaches abstracted and transformed race into data templates that presented a *quantified typology*.

In his biography of Pearson, Theodore Porter discusses how the British statistician developed a commitment to "graphical vision" in his statistical work. Pearson found the geometrical means of solving problems most promising and "valued it for the mathematical virtues of power

[47] Masatoshi Nei, *Molecular Evolutionary Genetics* (New York: Columbia University Press 1987) 208-214; D. Slice (ed.), *Modern Morphometrics in Physical Anthropology* (New York: Kluwer Academic/Plenum Publishers 2005).

and generality."[48] By transforming numbers into forms and figures on paper, he created new visual representations that could function as powerful mathematical or statistical tools. Porter quotes Pearson lecturing in 1891-2 that "geometry is not merely a mode of representing research, *but it is essentially a mode of statistical research …. Most statistical conclusions which can be obtained by arithmetic, can be obtained also by geometry, and many conclusions can be formed which it would be difficult to reach except by geometry."*[49] Along these lines, "the statistical study of evolution was first of all visual and graphical" for Pearson. He became preoccupied with graphs and curves, especially normal curves and deviations from normality. Pearson theorized that evolution and pressures of natural selection could be analyzed through these curves, notably irregular curves. He became enthralled with geometry and the visual aspect of statistics, Porter concludes.[50]

Pearson also applied this graphical, geometric vision to his racial research and developed an instrument-heavy approach to the datafication of race and skulls. The racial work in his lab centered on turning skulls into data by expressing it more intensely in coordinates, lines, angles, ratios, and surfaces than anthropologists had done so far. With special instruments and methods, the biometricians created new geometric representations of the skull on paper. This was not a simple business. Fawcett pointed out that "it is extremely difficult to fix a conventional measurement by a printed statement. The skull is not a system of geometrical points."[51] It needed to be converted into such a system with landmarks, anatomical planes, and measurements.

The previous chapters discussed how both anthropologists and biometricians analyzed frequency distributions in order to determine the "purity" or "homogeneity" of a racial sample. The biometricians were critical of the common anthropological assumption that abnormal curves

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48 Porter, *Karl Pearson*, 232.

49 Porter, *Karl Pearson*, 236-7.

50 Porter, *Karl Pearson*, 239.

51 Fawcett and Lee, "A Second Study," 428.

indicated racial mixing. Instead, they argued that the distribution's peaks were often caused by very small samples, diversity in age, postmortem deformation of skulls, the occasional presence of foreign skulls, and the plundering of burial places. "All these causes tend to emphasize the irregularity of the distribution even beyond the limits of random sampling," Fawcett explained.[52] Moreover, as Pearson discovered in 1895, "not only may two or many peaks occur in perfectly homogeneous material but no peaks whatever in certainly heterogeneous material. It all depends on whether the peaks are significant or not."[53] Only the goodness of fit test could determine significance, "a very simple" formula that could easily "be applied by anyone able to do ordinary arithmetic."[54]

Pearson's geometric focus also materialized in visualizations of race other than curves. The biometricians further seized the skull's landmarks in angles, as anthropologists had done. After anatomist Thane had explained the importance of cranial angles to Pearson, he devised a specialized instrument with the Cambridge Instrument Company, a trigonometer. This instrument turned cranial lengths into a triangle from which various angles could be read, such as the nasal angle. Rather than measuring angles with a caliper or goniometer, the trigonometer readily obtained angles "with sufficient practical accuracy" (fig. 3.4).[55]

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52 Fawcett and Lee, "A Second Study," 441.

53 Karl Pearson, "On the Fundamental Conceptions of Biology," *Biometrika* 1:3 (1902) 329.

54 Karl Pearson, "On the Probability That Two Independent Distributions of Frequency Are Really Samples of the Same Population, With Special Reference to Recent Work on the Identity of Trypanosome Strains," *Biometrika* 10:1 (1914) 92.

55 Fawcett and Lee, "A Second Study," 418.

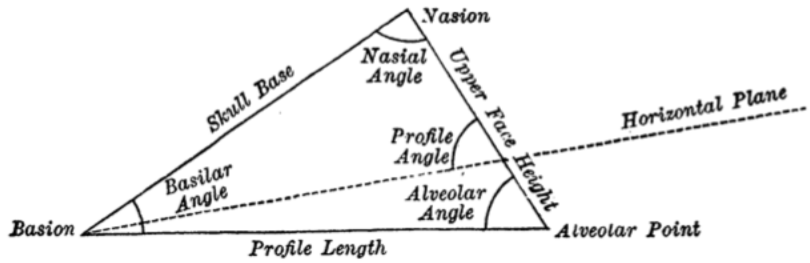

Figure 3.4. Cranial angles and triangles. Republished with permission of Oxford University Press from Cicely Fawcett and Alice Lee, "A Second Study of the Variation and Correlation of the Human Skull, with Special Reference to the Naqada Crania," *Biometrika* 1 (1902) 418.

Landmarks and measurements could also be captured in cranial contours. Unlike the anthropological instruments that produced contours of single skulls, the lab created an approach that quantified the anthropologist's averaged or "type" skull. In the late 1900s, Pearson hired gynecologist-turned-anthropologist R. Crewdson Benington, possibly already equipped with the expertise of wielding a pelvimeter or caliper, to assist in developing "type cranial contours." A few years later, Crewdson and Pearson presented their "standard scheme" for producing three cranial contours. First, the skull was placed in a skull holder called the *craniophor* and adjusted to the horizontal plane so that the relevant landmarks and planes could be determined. The researcher then moved the *craniophor* and skull to an instrument called the "Klaatsch contour tracer." For the first contour, the transverse vertical tracing, the skull was shifted so that the vertical plane became horizontal. The researcher traced the contour from ear to ear, passing the relevant landmarks along the way. Then, he drew a vertical axis in the middle of the contour, corresponding to the transverse vertical plane, and divided the contour into ten equal parts with proportional compasses. Finally, he drew lines parallel to the ear-to-ear line and numbered them. In total, 23 measurements were made on the transverse contour and written down in a recording book. These measurements were plotted and connected with a spline, a drawing device to create a continuous curve, thus producing the transverse contour (fig. 3.5). For the sagittal and horizontal contours, the skull was also first adjusted in the *craniophor* and then moved to the Klaatsch device. The researcher then drew the contour, marked the landmarks, and again divided the contour into ten numbered parts and recorded its measurements. Plotting the points and creating the three contours required a total of sixty measurements. The method turned the skull into a graphical organization of landmarks, lines, and angles (fig. 3.6).

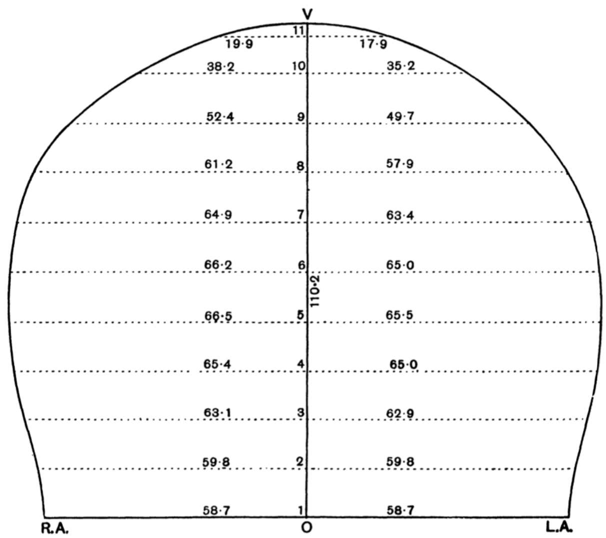

Figure 3.5. "Egyptian Crania. Transverse Contour." Republished with permission of Oxford University Press from R. Crewdson Benington and Karl Pearson, "Cranial Type-Contours," *Biometrika* 8 (1911) 175.

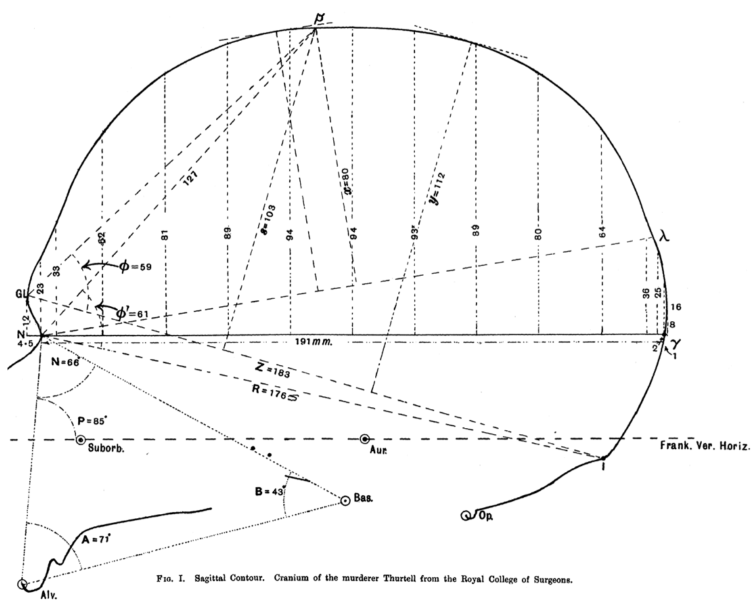

Figure 3.6. "Sagittal Contour." Republished with permission of Oxford University Press from R. Crewdson Benington and Karl Pearson, "Cranial Type-Contours," *Biometrika* 8 (1911) 139.

Morant explained that "drawings of cranial contours were originally proposed for the purpose of comparing and measuring crania individually and thus obtaining mean racial characters which could not be easily found otherwise."[56] The method, however, enabled the creation of average, mean, or type contours. The researcher arranged the measurements of individual contours in sixty columns and calculated the mean values and standard deviations of the sample. He then plotted the mean dividing line of the type contour, added the ten equal parts with the compasses, and plotted the mean measurements of various landmarks. With the aid of a spline, he drew the three type contours of each skull series. Sometimes biometricians drew a second tracing by adding and subtracting twice the probable errors of the mean measurements. This created a second type contour that represented both "less than type" (inside) and "larger than type" (outside). In the space between the two contours, called the "type zone," "practically fell the great bulk of the individual contours" and thus captured the variation of the sample (fig. 3.7).[57]

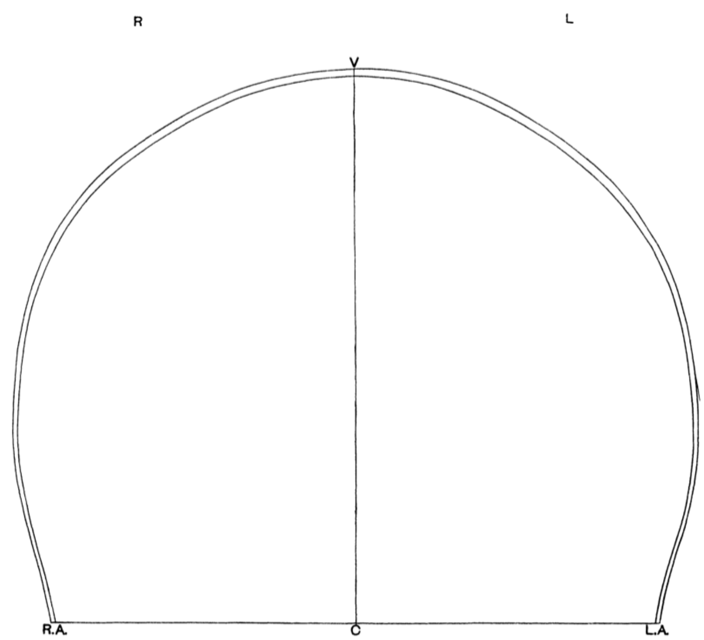

Figure 3.7. "English Type Vertical Contour, 17th Century, showing range of variation." Republished with permission of Oxford University Press from R. Crewdson Benington and Karl Pearson, "Cranial Type-Contours," *Biometrika* 8 (1911) 143.

[56] G.M. Morant, "A First Study of the Tibetan Skull," 228.

[57] R. Crewdson Benington and Karl Pearson, "Cranial Type-Contours," *Biometrika* 8:1/2 (1911) 129.

Like the CRL visuals, with the type contours, researchers could detect points of likeness and difference between racial samples more easily than by analyzing long tables of measurements side by side. The contours also revealed racial difference of *form* beside *size*. Crewdson and Pearson suggested that craniologists should start preparing type contours for large series in print and tissue that allowed for superposing contours. "A work of this kind would ultimately be the standard book of each craniologist's library, especially if it were accompanied by good photographs of the skull nearest to the type contour."[58] The type zones were especially convenient for racial comparison: if the zones for two type contours did not overlap, the races could be considered different and unrelated. In order to precisely compare type zones, Crewdson and Pearson suggested, the contours should be drawn double to scale on thin paper and placed on a large glass plate with diffused electric light.

Ultimately, the mean contours served as an aid in determining the morphological type skull that anatomists and anthropologists appreciated: "if a skull can be found in a given series which agrees practically with the three chief type contours…then that skull may, till a better method is forthcoming, be taken as the racial type," Pearson and Crewdson explained. The contours could "represent the type with all those qualitative features on which the anatomist lays special stress…until a better method is devised." Thus, the biometricians did not determine that the type skull was a "purely fictitious entity." Instead, "it is the skull possessing the maximum frequency."[59] The mean contours thus embodied the biometricians' notion that race materialized in the aggregate.

A standard work of printed type contours never appeared, but the contour method did become a standard practice in the lab. Indeed, the use made of these contours "in constructing average or type contours has become of far greater interest than the original purpose," Morant

[58] R. Crewdson Benington and Karl Pearson, "Cranial Type-Contours," 137.

[59] R. Crewdson Benington and Karl Pearson, "Cranial Type-Contours," 124.

remarked.[60] Type contours accompanied many of the racial studies published in *Biometrika*. This quickly generated a small database of racial type contours that could easily be compared to each other. Besides exposing racial differences in size, the contours allowed the biometricians to compare shape through the flattening or rounding of certain bones, the degrees of angles, and greater or lesser curvature of cranial parts. From the late 1920s, the contours often appeared in conjunction with the CRL, which biometricians considered a stronger tool for racial comparison. Morant explained: "When it is said, after comparisons have been made between type contours, that two races appear to be closely related or akin we are speaking only of relationship of form and do not for a moment mean to suggest that any such evidence can provide a criterion of remote consanguinity. Indeed…two races almost are remote as any imaginable, may have very similar sections."[61] "No single section of the human cranium is by itself a reliable guide to racial relationships…the types are supplementary and subsidiary to the mean direct measurements." The CRL, on the other hand, "takes into account all regions of the skull" and was therefore considered more reliable. Nevertheless, comparing the type contours to the CRL allowed for the analysis of both shape and size in racial differentiation.[62]

Although the contour method was a first attempt to quantify cranial form much like geometric morphometrics today,[63] the conclusions it generated fell in line with common racial assumptions. In comparing the Whitechapel crania with head measurements of English Royal Engineers through contours, Crewdson and Pearson concluded that the shape of the English head

[60] G.M. Morant, "A First Study of the Tibetan Skull," 228.

[61] G.M. Morant, "A First Study of the Tibetan Skull," 233; 195.

[62] G.M. Morant, "A First Study of the Tibetan Skull," 249; G.M. Morant, "A Study of Certain Oriental Series of Crania Including the Nepalese and Tibetan Series in the British Museum (Natural History)," *Biometrika* 16:1/2 (1924) 79.

[63] Geometric morphometrics developed as a field of study in the statistical analysis of biological shape and shape change in the early 1980s. Geometric morphometrics combines multivariate statistical analysis with solid and plane geometry and biomathematics "to support biological insights into the features of many different organs and organisms." With the help of computer programs, it studies, for instance, cranial shape variation in human populations. See D. Slice (ed.), *Modern Morphometrics in Physical Anthropology* (New York: Kluwer Academic/Plenum Publishers, 2005).

had remained rather stable from the 17th century onwards. Differences with other races were more visible, however. In fact, "the contours tell us nothing new," Crewdson and Pearson noted when they compared the Whitechapel type contours to a "Negro" series. "We all knew beforehand that the English and the negro cranium even in its least negro form were widely divergent."[64] Type contours did not overthrow the typological framework but *quantified* typology.

The biometricians developed two offshoots of the contour method that continued to turn skulls into geometric objects. In 1928, Ida McLearn, Morant, and Pearson introduced "type silhouettes": contours of en-profile living heads instead of dead skulls. These silhouettes included several racially important characteristics that had "no cranial existence," such as hair line, nasal tip, lip line, and chin. The silhouettes of individuals could be taken with a specially designed instrument available in the Galton Lab or through carefully standardized photographs. Like the contours, "type" or composite silhouettes could be created with the mean measurements of silhouettes.[65] This method was still in its infancy, but McLearn, Morant, and Pearson expressed hope that it would become of considerable anthropological value and would replace the "very unsystematic measurement of native races at present undertaken by anthropologists."[66]

Their first foray into the comparison of silhouettes revealed several racial and sexual differences. The male head had a bigger forehead, a longer nasal base, and a more vertical chin; the female contour looked like a reduced version of the male head. They then overlaid the contour of the male English head over a male West African head, ensuring that the auricular points coincided,

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64 R. Crewdson Benington and Karl Pearson, "Cranial Type-Contours," 134.

65 The method reminds of Galton's composite photography. This technique exposed several portraits in front of a copy camera onto a single plate, blending individual pictures into a generalized picture. It thus graphically captured the notion of within-group variation. In Galton's eyes, these composite photographs identified and visualized types. See Amos Morris-Reich, *Race and Photography: Racial Photography as Scientific Evidence, 1876-1980* (Chicago: University of Chicago Press 2016) 41-49.

66 Ida McLearn, G.M. Morant, and Karl Pearson, "On the Importance of the Type Silhouette for Racial Characterisation in Anthropology," *Biometrika* 20B (1928) 400.

and claimed that the English head was much larger. This superimposition also showed that the "Negro" had more protruding lips and chin, "this of course is a measure of negroid prognathism," the biometricians determined.[67] They also found that several racial differences overlapped with sexual differences. The biometricians concluded that, on the one hand "till we know more than we do at present of the origin of the modern races of man, we probably gain little by speaking of the Negro races as more 'primitive' than the European." However, they also pointed out that "those who believe this word [primitive] carries some significance, may feel themselves justified in affirming that the Englishwoman is more 'primitive' than the Englishman."[68] Leaving the reader with no explanation for such conclusions, the authors not only confirmed older racial theories with the method at hand, but also used it to conflate racist and sexist understandings of inferiority (fig. 3.8).

[67] Ida McLearn, G.M. Morant, and Karl Pearson, "On the Importance of the Type Silhouette for Racial Characterisation in Anthropology," 398.

[68] Ida McLearn, G.M. Morant, and Karl Pearson, "On the Importance of the Type Silhouette for Racial Characterisation in Anthropology," 400.

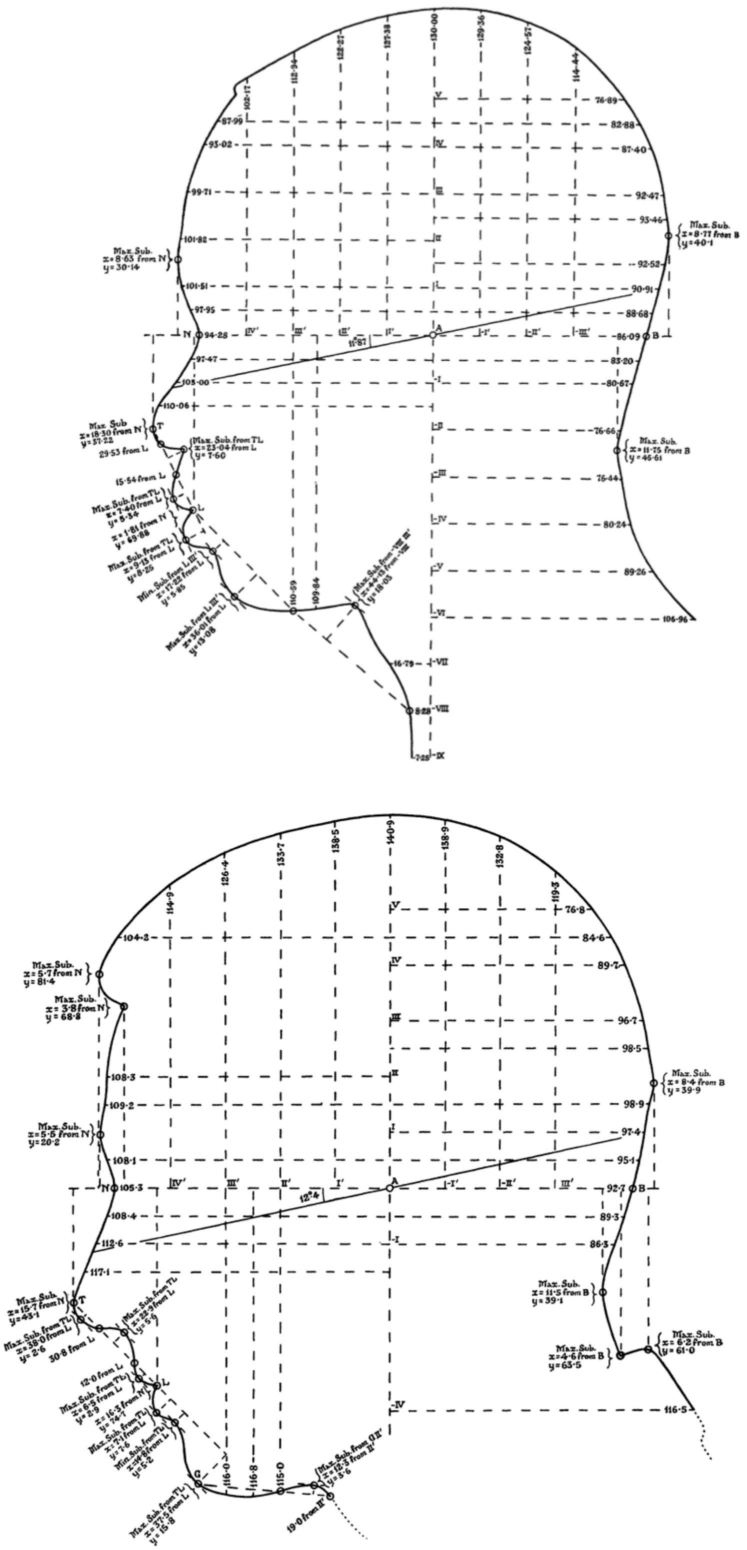

Figure 3.8. Mean silhouettes of "West African Negro" and "English Type." Republished with permission of Oxford University Press from Ida McLearn, Geoffrey Morant, and Karl Pearson, "On the Importance of the Type Silhouette for Racial Characterisation in Anthropology," *Biometrika* 20B (1928) 394; 396.

With another method related to the contours, Pearson and his colleagues compared the cranial outline of famous historical figures with paintings in order to determine which portraits were most "accurate." Rather than studying race in the aggregate, this approach narrowed the biometric focus to the individual. It presupposed that one could reconstruct the head of a person through their cranial measurements. This reconstruction produced a "true" outline of the head that could be overlaid on historical paintings. Pearson then determined which portraits were most accurate. This work required the biometricians to open up graves and dig up old bones of famous individuals long gone. Unlike the skulls in Pearson's collection, these skulls were returned to their graves after study. Pearson considered these exhuming practices important: "I can imagine a time, when public opinion being sufficiently educated, it shall be looked upon not as a desecration, but as a solemn duty reverently to exhume and study the crania of the departed great with a view of adequately correcting portraiture, or of supplying it where it is deficient."[69] Conversely, the comparison of skulls to portraits also enabled Pearson to determine whether a skull *claimed* to be of a famous figure actually belonged to that individual.

The approach combined Pearson's interests in history and craniometry. In long articles, he detailed the histories, personalities, and skulls of individuals such as George Buchanan, Henry Stewart, and Oliver Cromwell, researched the histories of their burial locations, and described how he obtained their skulls and what peculiarities the bony material revealed. In the case of Lord Darnley, the second husband of Queen Mary of Scots, Pearson carefully reconstructed Darnley's murder, effectively "playing the detective."[70] After providing tables with measurements, the three contours of the skull, and a discussion of its capacity and form, Pearson turned to historical

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[69] Karl Pearson, "The Skull of Robert the Bruce, King of Scotland, 1274-1329," *Biometrika* 16:3/4 (1924) 260.

[70] Karl Pearson and Walter Seton, "The Skull and Portraits of Henry Stewart, Lord Darnley, and Their Bearing on the Tragedy of Mary, Queen of Scots," *Biometrika* 20:1 (1928) 1–104; J.B.S. Haldane, "Karl Pearson, 1857-1957," *Biometrika* 44:3/4 (1957) 311.

portraits. A lab worker such as Ida McLearn had created measured drawings of the portraits and took outlines from the skull's photographs. Again through superimposition, Pearson analyzed to what extent the outlines of the skull "fit" the portraits, or more precisely, whether the face fit the skull. He separated "fancy" and "poor" portraits from "reasonable" or more "truthful" depictions (fig. 3.9).[71]

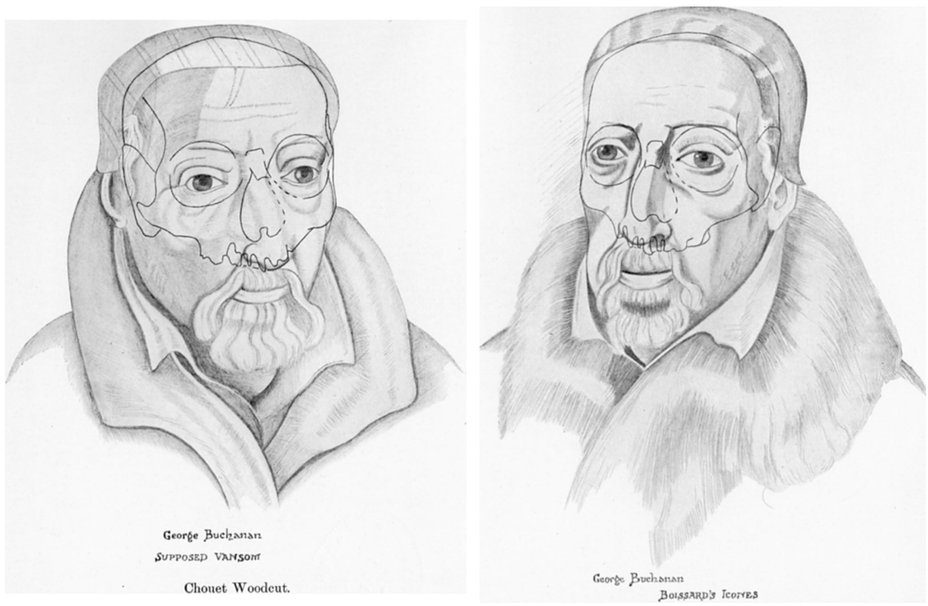


Figure 3.9. Shape of the skull overlaid on paintings of George Buchanan. Republished with permission of Oxford University Press from Karl Pearson, "On the Skull and Portraits of George Buchanan," *Biometrika* 18 (1926) Plate XXVII.

In the 1930s, Pearson began producing coordinate data of skulls, signaling a rising interest in the three-dimensional study of the skull that would accelerate from the 1960s with the introduction of the electronic computer. This interest grew out of the lab's ongoing research into the asymmetry of the skull and Pearson's resultant dissatisfaction with the "standard" skull planes, the sagittal,

[71] See, for instance: Karl Pearson, "The Skull of Robert the Bruce, King of Scotland, 1274-1329," *Biometrika* (1924) 253–272; Karl Pearson, "On the Skull and Portraits of George Buchanan," *Biometrika* 18:3/4 (1926) 233–56; Karl Pearson and G.M. Morant, "The Wilkinson Head of Oliver Cromwell and Its Relationship to Busts, Masks and Painted Portraits," *Biometrika* 26:3 (1934) 1–116. Race barely plays a role in these discussions. This type of analysis reminds of Google's recent Arts & Culture application that allows users to upload a photo of their face, scan it against a large database of historical artwork, and find a resembling portrait.

horizontal, and transverse planes mentioned in chapter 1 (fig. 3.10).[72] With coordinate data, he again advocated that mathematical or statistical analysis, not morphological analysis, made racial research more "true" and "scientific."

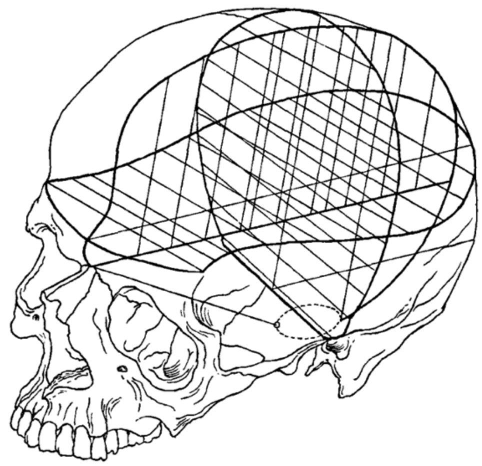


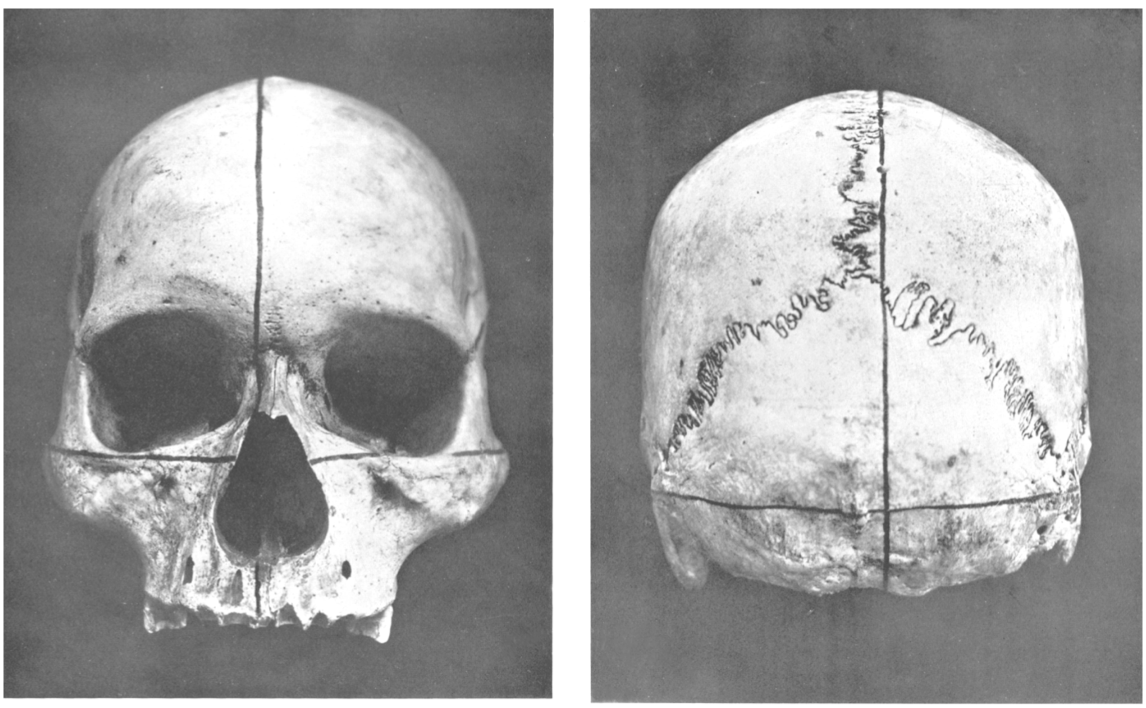
Figure 3.10. The three standard cranial planes. Republished with permission of Oxford University Press from William Howells, "The Cranial Vault: Factors of Size and Shape," *American Journal of Physical Anthropology* 15 (1957) 24.

Pearson complained that craniologists had determined cranial planes through *a priori* knowledge of the planes instead of finding the planes through the landmarks. "Is there not some need for a little mathematical logic – a little biometry to clear away these craniological fogs?" he asked.[73] More crucially, the asymmetry of various cranial parts made it difficult to accurately define the planes, an issue that anthropologists acknowledged but generally ignored. The standard planes largely relied on the location of the ears, the auricular points. Ideally, the horizontal and transverse planes were perpendicular to an imaginary horizontal line running through both ears called the "auricular axis." Ideally, the sagittal plane divided the skull in mirror symmetrical parts. In reality, however, the ears were often irregularly positioned: they shifted up, down, forward, backward, left

[72] On the asymmetry of the skull: Ting Liang Woo, "On the Asymmetry of the Human Skull," *Biometrika* 22 (1931) 324-252; K. Wagner, "Endocranial Diameters and Indices. A New Instrument for Measuring Internal Diameters of the Skull," *Biometrika* 27:1/2 (1935) 88-132; Karl Pearson and Ting Liang Woo, "Further Investigation of the Morphometric Characters of the Individual Bones of the Human Skull," *Biometrika* 27:3/4 (1935) 424–65.

[73] Karl Pearson, "The Cranial Coordinatograph, the Standard Planes of the Skull, and the Value of Cartesian Geometry to the Craniologist, With Some Illustrations of the Uses of the New Method," *Biometrika* 25:3/4 (1933) 227.

and right in regard to the sagittal mirror plane. This made it hard to define the plane, or, once determined, the plane created the wrong angle with the other planes. "The fiction of a median sagittal plane as a true *standard plane* of the skull seemed to vanish with the asymmetry of the skull," Pearson determined.[74] In no ordinary skull was this mirror plane perpendicular to the auricular axis. A photograph of a skull visualized the problem: the lines drawn on the bones exemplified the horizontal plane and the ideal symmetrical plane perpendicular to the auricular axis. The image showed that the latter failed to divide the skull into symmetrical halves (fig. 3.11).



(*a*) Hindu Skull. *Norma facialis.* (*b*) Hindu Skull. *Norma occipitalis.*

Figure 3.11. Planar lines drawn on a skull to show asymmetry. Republished with permission of Oxford University Press from Karl Pearson, "The Cranial Coordinatograph, the Standard Planes of the Skull, and the Value of Cartesian Geometry to the Craniologist, With Some Illustrations of the Uses of the New Method," *Biometrika* 25 (1933) Plate I.

"I could only look upon the Frankfurt Horizontal Plane and the Transverse Vertical Plane, both passing through the auricular axis, and the Median Sagittal Plane…as very temporary and inadequate expedients to obtain three mutually perpendicular standard cranial planes," Pearson

74 Karl Pearson, "The Cranial Coordinatograph," 217.

concluded.[75] Instead, he determined the mirror plane by finding the closest fit to thirteen mid-sagittal landmarks. In order to find these points, Pearson reduced the whole problem to solid Cartesian geometry. This not only required three planes that were more true to the skull's natural asymmetry, but also an instrument that determined the coordinates of cranial points in space in relation to the planes. Pearson set out to develop such an instrument, the "cranial coordinatograph". He described:

> The skull may be looked upon as a system of indefinitely numerous points. By aid of the cranial coordinatograph we can at once form tables of the coordinates in space of any number of these points we please. The instrument enables us to construct plan and elevation drawings of these points. We can then proceed to deduce the properties of the skull either by the methods of solid Cartesian geometry so familiar to the mathematician, or by the graphical rules of plan and elevation drawings so familiar to the engineer.[76]

The instrument replaced the caliper in measuring the distance between points and did a lot more: it determined the equation and angles of lines, the (in)adequacy of cranial planes, and whether two homologous points on the skull, such as the ears, were symmetrical. The heavy calculation labor it required was worth it: "The cranial coordinatograph seems to me to throw open a new field in craniometry, much as the modern theory of statistics did some forty years ago. It adds solid analytical geometry to the technique of the craniologist, and provides a valuable addition to his *instrumentarium*."[77]

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[75] Karl Pearson, "The Cranial Coordinatograph," 217.

[76] Karl Pearson, "The Cranial Coordinatograph," 219.

[77] Karl Pearson, "The Cranial Coordinatograph," 219.

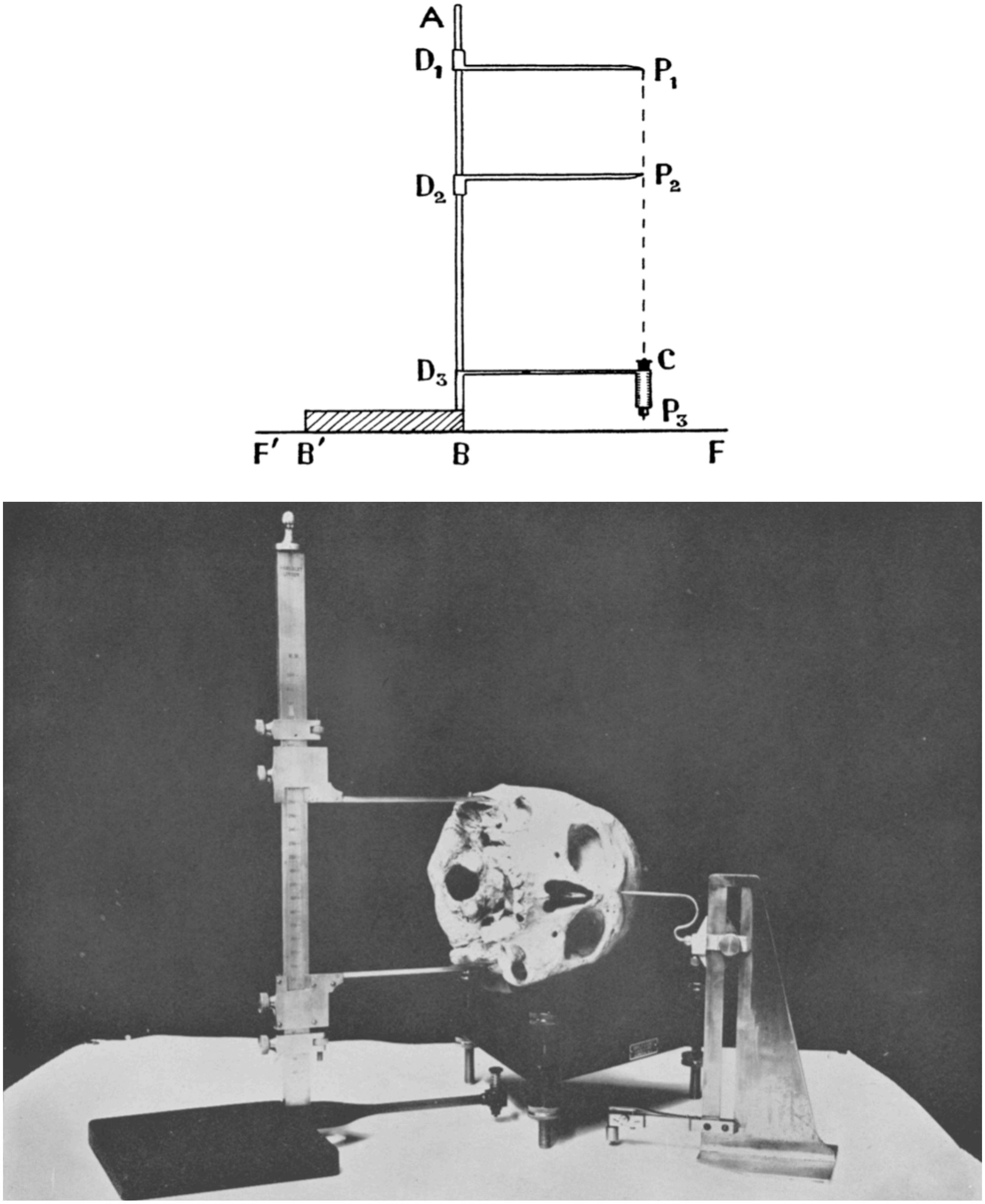

Figure 3.12. The Cranial Coordinatograph. Republished with permission of Oxford University Press from Karl Pearson, "The Cranial Coordinatograph, the Standard Planes of the Skull, and the Value of Cartesian Geometry to the Craniologist, With Some Illustrations of the Uses of the New Method," *Biometrika* 25 (1933) 222; Plate VII.

The instrument (fig. 3.12) consisted of a vertical rod with two movable arms with graduated scales that could be brought into contact with any two points (P1 and P2) so that they coalesced. A lower fixed arm carried a vertical cylinder with a needle point. With the press of the button (C) on top of the cylinder, the needle with ink came down and drew a point on the drawing board. In this way, the instrument could join any two cranial points with a line perpendicular to the drawing board, measure the height of any landmark above the board with the verniers, and record the plan and coordinates of cranial points. In this way, the three coordinates of any point could be found through the recorded elevations and measurements from the plan drawn on the board.

The coordinatograph converted the skull into a plan and elevation model (fig. 3.13). The horizontal line became the *x*-axis of the Cartesian coordinate system, the transverse vertical plane the *y*-axis. The craniometrist placed the skull in the instrument so that it hung en-profile above the paper, the paper becoming the *z*-axis or sagittal plane. The origin of the coordinate system was the point where the auricular axis passing through the skull met the drawing board. Anything positive of the *y*-axis went towards the face of the skull, anything positive of the *x*-axis towards the base of the skull. "If we now project onto the drawing-board all points we please of the skull, we shall have their plans on the plane of $z = 0$. These will give us their x and y coordinates."[78] After the biometrician had found the cranial coordinates with the instrument, he could analyze the properties of the skull with the "elementary formulae of analytical geometry of three dimensions."[79] With lengthy calculations, Pearson demonstrated the algebraic processes required to find his more "true" standard planes that took the skull's asymmetry into consideration. Pearson's "Plane of Maximum Symmetry" replaced the standard Sagittal plane and was the nearest fit to the thirteen mid-sagittal landmarks, determined by the cranial coordinatograph per skull. The researcher could then calculate the plane's analytical equation and use it to determine the right angles for the other two planes perpendicular to it.

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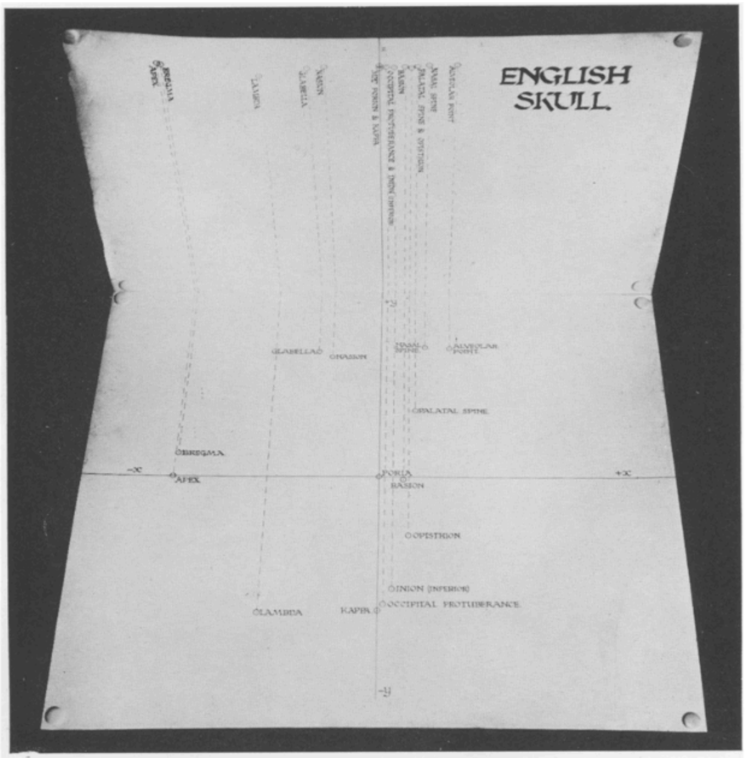
[78] Karl Pearson, "The Cranial Coordinatograph," 220.

[79] Karl Pearson, "The Cranial Coordinatograph," 236.



Plan and Elevation Model of an English Skull.

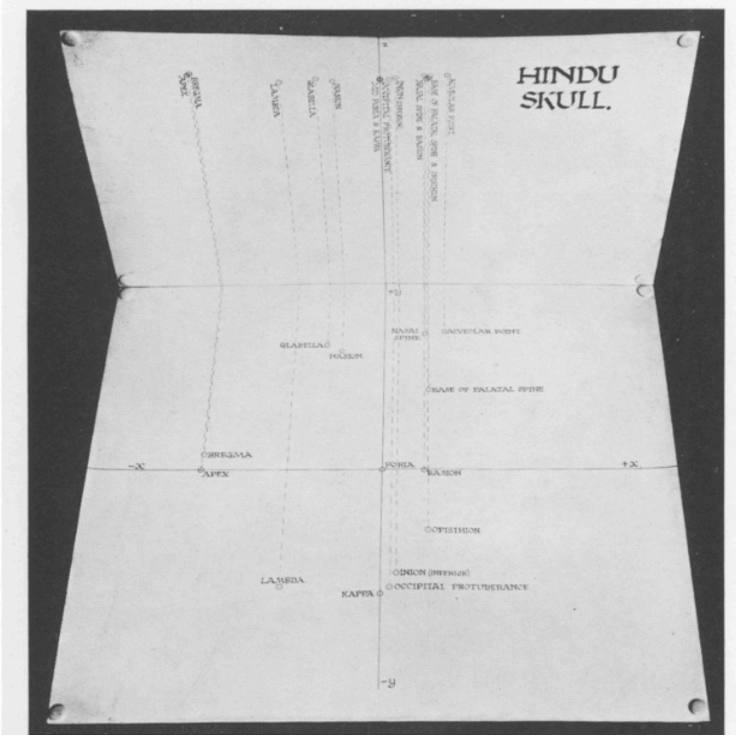


Figure 13. Cranial plan and elevation model. Republished with permission of Oxford University Press from Karl Pearson, "The Cranial Coordinatograph, the Standard Planes of the Skull, and the Value of Cartesian Geometry to the Craniologist, With Some Illustrations of the Uses of the New Method," *Biometrika* 25 (1933) Plate VI.

Thus, Pearson found the planes *a posteriori* through measurements, algebra, and geometry. The anthropological approach idealized the skull's shape; Pearson's mathematics captured its material reality. The application of solid geometry to craniometry, Pearson declared, "is a most promising field for the craniometricians who are the first to apply Cartesian geometry to the skull." The study of planes and angles with the cranial coordinatograph could give rise to new anthropological questions, like the study of various degrees of asymmetry in different races. "Does asymmetry increase as we pass from more primitive to more highly civilised groups?" he pitched as a new research topic. This new approach to the study of the skull thus continued to echo hierarchical notions of racial difference.[80]

Automating Research, Quantifying Typology

The cranial contours, silhouettes, and coordinatograph were not simply attempts to improve anthropological methodology: different epistemological intentions were built into them. They embodied a critique of the morphological expert vision that anthropologists valued and a shifting of trust from the researcher to disembodied biometric technologies.

The morphological method, as previous chapters discussed, centered on the study of the skull with the naked, trained eye and the subjective expertise of observing racially distinctive traits obscured to unexperienced viewers. Pearson's interactions with anthropologists Von Török and Myers, discussed in chapter 2, already showed that his writings on this method were filled with judgement. While he often stressed that this type of anthropological expertise was "outside of his plane," he nevertheless compared it to "medieval scholasticism" and considered it part of "old school anthropology" and the normal development of the sciences.[81] Even though he considered it

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[80] Karl Pearson, "The Cranial Coordinatograph," 253; 242.

[81] Karl Pearson, "27. On the Reconstruction of Cranial Capacity from External Measurements," *Man* 26 (1926) 50.

important for a science to remain purely descriptive for a long period, ultimately "we can really know very little about a phenomenon until we can actually measure it and express its relations to other phenomena in quantitative form."[82] This is why Francis Galton and the biometric school took up anthropology, Pearson explained: not because they were interested in biometric methods "but because, studying 'human material,' they found real progress was impossible unless new methods were invented and applied. And it is because of that desire to understand human development that they do not hesitate totally to condemn as nugatory and profitless the descriptive and appreciative processes in which [the anthropologist] puts his trust."[83] In determining racial origins and classifications, the researcher should trust statistical methods, not morphological expert vision.

For Pearson, statistical analysis was the most objective method. He assured that:

> we firmly believe that we have no political, no religious and no social prejudices, because we find ourselves abused incidentally by each group and organ in turn. *We rejoice in numbers and figures for their own sake* and, subject to human fallibility, collect our data – as all scientists must do – to find the truth that is in them. The tradition of the Laboratory has always been that until data are reduced and analysed no member of the staff holds the slightest opinion as to what might, ought or will come out of them.[84]

Pearson wrote this disclaimer at the beginning of a 250-page statistical inquiry into the physical and mental "fitness" of recent Jewish immigrant children into Great Britain. He concluded that because the alien Jewish population was "somewhat inferior physically and mentally" to the British population, unrestricted immigration was disadvantageous to the country. Because "the hot controversy over the alien Jewish population" in Great Britain, Pearson felt urged to stress that this inquiry was not funded by anyone "to teach results of a given bias" and did not reflect any biased errors. In the "cold light of statistical inquiry," truth rested in numbers and quantification.

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[82] Karl Pearson, *The Science of Man: Its Needs and Its Prospects* (Cambridge University Press 1920) 5-6.

[83] Karl Pearson, "Was the Skull of the Moriori Artificially Deformed," *Biometrika* 13:4 (1921) 346.

[84] Karl Pearson and Margaret Moul, "The Problem of Alien Immigration into Great Britain, Illustrated By an Examination of Russian and Polish Jewish Children," *Annals of Eugenics* 1:1 (1925) 8. Italics mine.

Pearson had a different idea of what vision could reveal. As race manifested itself in the aggregate, it could not be visualized by a single "ideal" skull, but in tables, graphs, and frequency distributions. Rather than the skillful glance entrusted by the anthropologist, the biometrician's peering expertise revealed itself in the prompt reading of tables. These needed to show racial difference at a glance. This desire motivated, for instance, Pearson to simplify the CRL's tables by reducing the CRL to a standard value of 75 skulls: "then the comparison of all such coefficients might be made by exhibiting them in a single table. We should thus be enabled to see *at a glance* the relative racial differences of all races."[85] At the same time, Porter points out that, for Pearson, "the eye alone was not sufficiently discerning to reach determinations on the vital questions of science with which he was now concerned." Mathematics and statistical determination were always required. Frequency distributions, for instance, could be deceiving in appearance and one should therefore never compare them with vision alone: "the distributions in appearance are wholly dependent on the choice of scales and the eye alone cannot possibly make any measure of the degree of accordance…the eye can never provide any judgment of value on such a point," Pearson wrote.[86] Even in biometric analysis, the researcher's eye was not to be trusted – only the statistics and data.

In practice, however, biometric research was not all about numbers: the lab's workers often employed commonly used morphological, anthropological, and ethnographic methods. The archival records reveal close to nothing on how Pearson or the lab's workers obtained anthropological measuring skills, but their research papers show that they had acquired this expertise. They used the same anatomical terminology, landmarks, and measurements, at the very least to ensure the comparability of their data. Their publications not only reproduced geometric cranial projections

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85 Porter, *Karl Pearson*, 239; Karl Pearson, "Note on Standardisation of Method of Using the Coefficient of Racial Likeness," *Biometrika* 20B:3/4 (1928) 378. Italics mine.

86 Karl Pearson, "On the Probability That Two Independent Distributions of Frequency Are Really Samples of the Same Population, With Special Reference to Recent Work on the Identity of Trypanosome Strains," *Biometrika* 10:1 (1914) 86.

but also routinely printed photographs of "typical" skulls, which recovered the 3-dimensionality that was flattened by turning skulls into data. What is more, morphological appreciation played a role in their research practices. Before measuring any sample of skulls or bones, the material had to be divided according to sex. The biometricians, like other racial researchers, had to explore sexual differences through touch and vision and by "seriating" samples, putting bones in succession from "most female" to "least female." Seriation allowed them to observe what a "typical" female bone looked like for a racial sample. In 1915, Pearson attempted to develop a method that mathematically divided samples according to sex, hoping it could replace the morphological method. He concluded that his evidence was not definitive, the calculations too laborious, and that the metrical determination of sex could merely function as an aid to morphological analysis.[87] Indeed, the biometricians often sexed the material through morphological analysis themselves and also routinely called in the help of anatomists such as fellow University College colleague Thane.

Furthermore, biometricians used ethnographic information in dividing cranial samples according to sex and race, such as the locations where samples were collected and burial practices. Chapter 2 detailed how biometricians consulted state records and church books in determining the age of skulls found in London's burial pits. For his study of Tibetan skulls, Morant developed hypotheses based on historical literature that the Coefficient of Racial Likeness then confirmed. Along similar lines, Miriam Tildesley wrote about her Burmese skull sample that "it was *obvious* in examining the whole series of 142 skulls that they were of rather mixed type, although one type predominated. It seemed desirable therefore to class together in separate groups those skulls which

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[87] Many physical anthropologists today prefer to sex bones through morphological observation. R. Panhuysen, "Geslachtsbepaling," Zomercursus fysische antropologie, University of Amsterdam, July 12th 2018. Karl Pearson, "On the Problem of Sexing Osteometric Material," *Biometrika* 10:4 (1915) 479–87.

qualitative appreciation led us to suppose of the same type, reserving the right to combine these groups after if the quantitative values obtained did not support this judgment."[88]

The word obvious is striking here. Elsewhere, Pearson suggested that the type of visual analysis he and his workers performed before measurement was not necessarily the morphological method, but the analysis of "the obvious." He wrote that the "old school anthropologist" "is quite sure that the mathematician does not look at anything but his figures, and so overlooks *the obvious* in his material, which he, the old school anthropologist, sees at first glance – not because he has handled more material, but because he is the Simon pure – the genuine anthropologist."[89] The biometricians thus both used and critiqued older methods.

Conclusion

The technologies discussed above transformed the morphological skull into a geometric object that exposed its racial form and characteristics. Biometricians presented these methods as a way to improve racial research and as a critique of morphological expert vision. The cranial paper representations were abstractions but not idealizations: Pearson's goal was to quantify the ideal type skull, to find "truer" planes, and to collapse data on racial relationships in simplified visualizations. His credo was that truth was only reached through quantitative determination, not expert vision. Novel insights into man's racial history should depend on strong statistical methods, not the researcher's subjective wisdom or intuition. With statistical formulas and geometric projections, the biometrician desired to *remove* the researcher's subjectivity from the research process. The above-discussed technologies *automated* racial research by transferring the work of producing classifications

[88] Miriam Tildesley, "A First Study of the Burmese Skull," 217. Italics mine. Qualitative appreciation means the morphological method here.

[89] Karl Pearson, "Was the Skull of the Moriori Artificially Deformed," 338. Italics mine.

from researchers to disembodied formulas like the CRL and instruments such as the coordinatograph.

These were partial transformations, not radical changes. While desires of objectivity and automation propelled the development of biometric technologies, research in the laboratory remained deeply embodied, where researchers meticulously cleaned and measured skulls, analyzed crania with several instruments, and spent hours grinding the Brunsviga calculators to produce data. Data templates divorced racial research from this embodied context but could not erase notions about racial differences and hierarchies common in the early 20th century. Furthermore, biometricians heavily criticized the morphological method but could not discard it. Biometric racial research was not all about the data and the methods, but also about existing historical, ethnographic, and racial assumptions. New technologies often confirmed accepted theories of racial difference and offered quantified racial typologies. Coordinate data, attention to within-group variation, and multivariate distances measures *by themselves* did not overturn the project of race. The fingerprints, intentions, and beliefs of racial researchers stuck to the research. Biometric technologies were not disembodied but situated all the way through.[90]

[90] Donna Haraway, "Situated Knowledges: The Science Question in Feminism and the Privilege of Partial Perspective," *Feminist Studies* (1988) 575–99.

Chapter 4

Weaponizing "Truthful" Biometry. Geoffrey Morant and Biometric Challenges to Nazi Racism

Introduction: "We are all racial hybrids"[1]

The biggest biometric challenge to anthropology came in the 1930s, delivered by the Biometric Laboratory's main racial researcher: Geoffrey Morant. Morant was the laboratory's in-house craniometry specialist and published over sixty papers on craniometry, race, and statistics. In the 1930s, Morant's writings became political. He began using his biometric insights to publicly denounce and debunk Nazi racial theories and racism, first in publications that warned other anthropologists, later in writings directed at a larger audience. In 1939, he informed the public about the fallacies of Nazi racial theory in a book titled *The Races of Central Europe*. With this publication, he joined a small group of scientists that publicly spoke out against Nazi scientific racism. Morant's antiracism publications, however, have been forgotten by historians. Instead, the historiography has prominently featured his friend Ashley Montagu and colleagues Julian Huxley, Alfred Haddon, and

[1] G.M. Morant, "Racial Fallacies and Realities in Europe" talk at the Royal Institute of International Affairs, 12.5.1942, 8.

Franz Boas as the leading anthropologists to publicly combat racism and Nazi racial theories.[2] Because few scientists openly spoke out against Nazi racism, Morant's work deserves historical attention.

In his challenge against *racism*, Morant actively supported *race*. He never interrogated the existence of race nor dismissed racial research like fellow Nazi critics Franz Boas and Ashley Montagu did. In fact, he argued that "truthful" biometric evidence of racial difference could bring peace to the world. This chapter thus contributes to a recent revisionist historiography that questions the "rise and fall" of racial science in the 20th century. Indeed, Morant's work challenges well-established historical timelines. In combating racism, he introduced new ideas about race that historians often associate with postwar population genetics, such as the notion that within-group variation was bigger than between-group variation. His statistical insights even led him to question the existence of racial boundaries. But whereas American evolutionary biologist Richard Lewontin would deeply challenge racial classifications with the notion of within-group variation in the 1970s,[3] Morant introduced it as a tool that would make racial research more "precise and accurate." In 1951, UNESCO asked Morant to write a pamphlet on racial differences in mental characteristics, in which he argued that such differences should be expected to exist, despite lacking data. Thus, Morant shows that researchers fought racism without rejecting race itself, before and after World War II. Antiracism did not necessitate the obliteration of race.

Morant's work may have escaped the historian's eye because no known archive holds his professional papers. This chapter introduces newly discovered archival material. A selection of his professional papers, correspondence, and treasured offprints have been sitting in boxes in the attic

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[2] See for example George W. Stocking (ed.), *Bones, Bodies, Behavior: Essays on Biological Anthropology* (Madison: University of Wisconsin Press 1988). For the main primary sources, see: Ashley Montagu, *Man's Most Dangerous Myth: The Fallacy of Race* (New York: Columbia University Press 1942); J. Huxley, A.C. Haddon, and A.M. Carr-Saunders, *We Europeans: A Survey of 'Racial' Problems* (London: Jonathan Cape 1935); Franz Boas, "This Nordic Nonsense," *Forum* 74 (1925) 502–11.

[3] Richard Lewontin, "The Apportionment of Human Diversity," *Evolutionary Biology* 6 (1972) 381-98.

of his son's house in Durham, England. This chapter presents these valuable archival materials for historical use.

Pearson's Most Trusted Worker

Geoffrey Miles Morant was born in Battersea, London, in 1899. After serving during the First World War, he studied statistics at University College London. In 1920, he became one of the first students in the United Kingdom to receive a Bachelor of Science degree in statistics, which the University had just recognized as a field of study. He obtained a Master of Science degree in Applied Statistics in 1922. In all probability, Morant was introduced to both Pearson and craniometry during his undergraduate or postgraduate studies and perhaps took Pearson's courses at University College. In 1920, Morant joined the Biometric Laboratory as the new Crewdson Benington student. From then on, Morant dedicated his academic career to craniometry and racial history. His first craniometric paper in *Biometrika*, the analysis of Tibetan skulls with the Coefficient of Racial Likeness discussed in the previous chapter, summarized the results of his studentship.[4]

Unfortunately, we do not know much about Morant's acquisition of cranial measuring skills. It seems that he had no formal anatomical training and obtained most of his knowledge from reading anatomy textbooks.[5] His travels to European museums and universities between 1924-1926, however, undoubtedly were formative to his development as a physical anthropologist. Morant obtained a two-year fellowship to do anthropological research on the continent, supported by the Royal Commissioners for the Exhibition of 1851. This research trip was partially prompted by personal tragedy. In 1923, a year after getting married to his first wife, she and their baby died during labor. With Pearson's support, Morant decided to "get away for two or three years" and widen his

[4] G.M. Morant, "A First Study of the Tibetan Skull," *Biometrika* (1923) 193–260.

[5] Ashley Montagu Papers (AMP), Box 35, correspondence with Geoffrey Morant, GM to AM 30.11.1959.

research experience.[6] Pearson introduced Morant to Léonce Manouvrier, the craniometry specialist at the *École d'Anthropologie* in Paris, and Morant departed in the Fall of 1924 to attend lectures and measure osteological collections in the famous Broca Laboratory.[7] He had his craniometry instruments sent to Paris and spent his time measuring a large collection of Neolithic skulls in the Broca collection and Paleolithic materials at the *Museum d'Histoire Naturelle*. His stay in Paris was an "extraordinarily interesting and pleasant" experience for Morant. In 1925, he traveled to museums in Lyon, Liege, Brno, Berlin, and Bonn.[8]

With two of his papers published in *Biometrika*, an examination committee granted Morant his Doctor of Science degree in the summer of 1926.[9] Pearson's Department of Applied Statistics hired him as an assistant and later as a lecturer in Anthropometry. In the Department, Morant supervised students from the UK, America, China, and India, and routinely instructed independent researchers in craniometry.[10] He developed a close relationship with Pearson, as their affectionate correspondence, collected by Morant in photo albums, demonstrates. In fact, their correspondence and Morant's publications show that Morant followed Pearson's approach to race, skulls, and statistics in most respects. The two men seemed to have only differed in opinion on the practicality of this research. Morant believed that science should be pursued for science sake and not serve any

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[6] Geoffrey Morant Papers (GMP), correspondence with Pearson, 12.7.1923.

[7] GMP, Pearson to Manouvrier, undated; Karl Pearson Archive (KPA), Box 243, 11/1/13/39, Manouvrier to Pearson, 27.10.1924; 14.7.1925.

[8] Morant and Pearson actively corresponded during this time. GMP, 4.11.1924; 5.11.1924; 8.11.1924; 17.11.1924; 2.12.1924; 13.12.1924; 16.2.1925; 1.11.1925; 9.3.1926; G.M. Morant, "Studies of Palaeolithic Man II. A Biometric Study of Neanderthaloid Skulls and of Their Relationships to Modern Racial Types," *Annals of Eugenics* 2:3/4 (1927) 109–214.

[9] G.M. Morant, "A Study of Egyptian Craniology From Prehistoric to Roman Times," *Biometrika* 17:1-2 (1925) 1–52; G.M. Morant, "A First Study of the Craniology of England and Scotland From Neolithic to Early Historic Times, With Special Reference to the Anglosaxon Skulls in London Museums," *Biometrika* 18:1/2 (1926) 56–98.

[10] The term "anthropometry" was used interchangeably with "craniometry" but sometimes specifically referred to the measurement of living bodies as opposed to skulls. Here, it was probably used for the measurement of both the living and the dead with a major emphasis on the latter. KPA, Box 117, 4/4/9, "Report for the years 1925, 1926, 1927, 1928-1929."

practical purposes. It is therefore not surprising that Morant steered clear of eugenics throughout his career.

Over time, Morant became the Biometric Laboratory's expert on biometry and race. The majority of Morant's racial studies published between 1923 and 1947 focused on the study of prehistoric and early historical races, their relations, and their "wanderings" or migration patterns. Using complex biometric methods, he developed theories about the historical sequence of racial types and hypothesized about migrations, "invading" races, racial mixture, and "dominant" types that pushed others to geographical fringes. Morant rarely measured living bodies for scientific purposes. He believed that studying ancient and prehistoric skeletal material was "the only safe way of tracing racial history" and of understanding how the intermixing of ancestral types produced modern populations.[11] Environmental and social conditions affected the skeleton far less than the living body and was "hence of more fundamental importance as a guide to relationships."[12] Morant had the large collection of skulls in the laboratory at his disposal and profitably used Pearson's connections with curators and anthropologists in London and beyond. In London, Pearson introduced Morant to the skeletal collections of the Royal College of Surgeons and the British museum. As other biometricians, Morant also reused racial measurements published in journals.[13] His unrestricted use of osteological materials explains the broad focus of Morant's biometric work: his publications cover populations from all corners of the world, from Basque country to Tibet, from Native Americans in the United States to the Irish. This breadth not only highlights the global scope of anthropology's racial classification project, but also suggests that methodology was equally

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[11] H.J. Fleure, G.M. Morant, L.S.B. Leakey, and M.L. Tildesley, "27. A Discussion on Methods of Description and Analysis in the Anthropometric Study of European Populations," *Man* 35 (1935) 28.

[12] G.M. Morant, "The Craniology of Ireland," *The Journal of the Royal Anthropological Institute of Great Britain and Ireland* 66 (1936) 54.

[13] See Chapter 2 for a discussion on the laboratory's skull collection and the biometricians' "data-mining" practices and re-use of published racial data.

important to biometric racial research as racial theory or focus. Morant's work did not become more thematically or geographically specialized during his career – his methodological expertise was his specialization.

Throughout his career, Morant faithfully used the methods for racial analysis he and others in the Biometric Laboratory developed. Pearson's Coefficient of Racial Likeness (CRL) appears in almost all Morant's publications on race. These methods guided Morant's conclusions and hypotheses about racial origins and classifications, as he stressed in several articles. "Using purely quantitative methods has some unexpected features," he explained.[14] Like the biometric studies discussed in previous chapters, his statistical evidence sometimes contradicted long-held assumptions in anthropology. In 1927, Morant questioned "the view advocated by some writers" that the Australian and Tasmanian races were "distinguished from all other modern races on account of their ultra-primitive characters," finding no support in the data.[15] Along similar lines, Morant found no proof for the assumption that the population of ancient Egypt was "sensibly affected at various times by the infusion of Negro blood," which was "very generally supposed."[16]

Early in his career, Morant developed arguments that would later become central to his antiracism publications. Although he still claimed in 1923 that "we cannot assert that geographical proximity naturally connotes racial affinity,"[17] the racial similarity of neighboring populations came to stand out more clearly in his statistical evidence between 1928-1932, especially when using the CRL. Particularly his work on European skulls demonstrated geographical affinities and he concluded that "it is interesting to note that the physical classification is entirely unconnected with

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[14] Ting Liang Woo and G.M. Morant, "A Preliminary Classification of Asiatic Races Based on Cranial Measurements," *Biometrika* 24:1/2 (1932) 134.

[15] G.M. Morant, "A Study of the Australian and Tasmanian Skulls, Based on Previously Published Measurements," *Biometrika* 19:3/4 (1927) 440.

[16] G.M. Morant, "A Study of Egyptian Craniology from Prehistoric to Roman Times," *Biometrika* 17:1-2 (1925) 8.

[17] G.M. Morant, "A First Study of the Tibetan Skull," *Biometrika* (1923) 195.

that suggested by language" which suggested sharp boundaries between neighbors. Along similar lines, Morant argued that the Basques were closely related to Western European races despite the present-day isolation and linguistic differences. By the early 1930s, he asserted that physical connections between neighboring types should be expected.[18] These conclusions were of particular interest, given that nationalism was on the rise in Europe and nation states had begun organizing their "national" borders on the basis of race and language.[19]

A Changing Scientific Landscape

In the second half of the 1930s, Morant's working conditions changed considerably. Under the tutelage of Pearson, who deeply valued racial research, Morant's position as the in-house craniometry expert was safe. But when Pearson retired in 1933, the old order was no longer ensured. Pearson continued to have a room for *Biometrika*'s editorial work,[20] but University College decided to split the Department of Applied Statistics into a department of Eugenics, chaired by statistician Ronald Fisher, and a department of Statistics, run by Pearson's son Egon. In 1937, a third department for Biometry was established with geneticist J.B.S. Haldane as chair.[21] In the summer of 1933, Morant and Pearson packed up the Biometric Laboratory and started a "troublesome" process

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[18] G.M. Morant, "A Preliminary Classification of European Races Based on Cranial Measurements," *Biometrika* 20B (1928) 351; G.M. Morant, "A Contribution to Basque Craniometry," *Biometrika* 21:1/4 (1929) 67–84; G.M. Morant and M.F. Hoadley, "A Study of the Recently Excavated Spitalfields Crania," *Biometrika* 23:1/2 (1931); Ting Liang Woo and G.M. Morant, "A Preliminary Classification of Asiatic Races Based on Cranial Measurements," *Biometrika* 24:1/2 (1932) 134.

[19] Eric Hobsbawm, *Nations and Nationalism since 1780. Programme, myth, reality* (Cambridge, UK: Cambridge University Press 1990).

[20] GMP, correspondence with Pearson, 10.8.1932.

[21] Donald A. MacKenzie, *Statistics in Britain: 1865-1930; the Social Construction of Scientific Knowledge* (Edinburgh: Edinburgh University Press 1981) 118.

of moving.[22] Finding new locations for the lab's instruments and objects did not go smoothly. Fisher disposed of various materials, including those that had once belonged to Francis Galton, and took ownership of shared research instruments such as photography equipment.[23] To safeguard the Laboratories' possessions, Pearson moved most of the skull and skeleton collections to the Duckworth Laboratory at Cambridge University.[24] He donated all of his anthropometric equipment, including some of Galton's instruments, to Morant.[25] Pearson lamented the split of his department. "Things here are getting shaken down to their new purposes," he wrote to Morant in the summer of 1933. "But of course the changes must be painful to me, who had spent much thought years ago in organising the building and its uses. I may be wrong but it seems to me as if, as years run by, so much will have to be restored."[26]

Morant decided to join Fisher's department. By 1935, his position at the College became precarious: Fisher was growing dissatisfied with Morant's field of work, craniometry. He began writing up his critique on the CRL discussed in chapter 3 and gave Morant a draft for feedback. Pearson didn't think it was an attack on Morant but was instead:

> written in anger at the removal of the palaeontological collections. He [Fisher] knows that they are really valuable, but in order to justify his turning them out, writes in some popular journal to say all palaeontology is rubbish. We know something, - do we not? – of the superiority of craniometry over the measurements on the living subject! But if you work only on the living subject, how can you hope to link modern man with palaeolithic man?[27]

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[22] GMP, correspondence with Pearson, 15.8.1933.

[23] GMP, correspondence with Pearson, 12.8.1933.

[24] B. Dickins et al, Annual Report of the Faculty Board of Archaeology and Anthropology on the Museum of Archaeology and Ethnology, 1952-1953 (Cambridge, UK: Cambridge University 1953); AMP, Box 35, GM to AM 21.10.1945.

[25] GMP, correspondence with Pearson, 16.7.1935.

[26] GMP, correspondence with Pearson, 30.8.1933.

[27] GMP, correspondence with Pearson, 22.7.1935.

Pearson grew distressed about Morant's position, Fisher's attacks on craniometry, and the breaking up of the Department: "I should like, were it in my power, that (1) you should stay at University College and (2) not be thrust out of physical anthropology."[28] Morant replied that Fisher "has kept to the letter of the bargain in leaving me alone to get on with my own work, but he has allowed me no assistance of any kind … and has been covertly hostile all the time."[29]

In 1936, the Provost of the University urged Morant to leave the college. "I have no wish to stay on in a department in which I am not wanted. There seems to be no way out," he wrote to Pearson.[30] He also wrote to British-American anthropologist Ashley Montagu about the situation, with whom he had started corresponding in the late 1930s.[31] In 1938, Morant told Montagu that he was able to continue his research while supervising students but without any assistance, financial or otherwise. "My present chief (R.A. Fisher) unfortunately has a fanatical hatred of the ideas descended from my former chief (K.P.) and I am told bluntly that my chances of securing promotion from the junior staff to which I belong now are absolutely nil." There were no jobs for physical anthropologists elsewhere in England. Moving to another country was not an option as he took care of his parents.[32]

These increasingly more difficult working conditions coincided with a changing political landscape in Europe. In January 1933, Adolf Hitler solidified his power as Chancellor and the Nazi party gained control over Germany. During the early years of the regime, the Nazi government

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28 GMP, correspondence with Pearson, 6.2.1936.

29 GMP, correspondence with Pearson, 7.2.1936.

30 GMP, correspondence with Pearson, 16.2.1936.

31 Morant and Montagu were possibly brought into contact by their mutual friend, Cambridge anthropologist Jack Carrick Trevor.

32 AMP, Box 35, GM to AM 12.1.1938; 19.2.1938. Morant wrote to Montagu that American anthropologist Wingate Todd offered him a job at Western Reserve once, but that Morant had declined it, still hoping that physical anthropology would develop in the UK (29.1.1939) He also wrote to Montagu that Herskovitz offered him a job in the US in 1945. (22.5.1963).

developed racial policies based on an Aryan-Nordic racial theory that had been brewing in German intellectual circles since the late 19th century. The theory promoted the existence of a hierarchy of races and a struggle between the lower races and the highest Aryan-Nordic race. Pure races such as the Aryans roamed the earth in ancient times and despite racial mixing, "pure" representatives of this race still existed among the German population. The theory claimed that ongoing racial mixing, however, threatened their existence and was disastrous for the nation[33] as "lower" races weakened its people both mentally and physically. The Nazis believed that Jewish people were the Aryans' eternal and most deadly enemies. Moreover, they argued that Europe contained various races of higher and lower worth and that one could identify racial origins in individuals – a belief that had disastrous effects in practice.[34]

Hitler discussed many of these ideas in *Mein Kampf*, which was published in 1925 and translated into English in 1939. Chapter 1 demonstrates that some of these ideas were based on long-held assumptions within anthropology, such as the existence of pure races, the idea that Europe contained various races, and a focus on racial "types." Nazi racial theory was a radicalization of such ideas.[35] This "pseudo-scientific theory" formed the basis for German political ambitions and

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[33] The "nation," according to Arthur Gütt, Head of the National Hygiene Department, did not comprise "all those who are citizens of the State concerned, irrespective of their race or their origin." "The science of heredity teaches us that such a view is but superficial, and that the term must be restricted to those persons who are racially akin to one another owing to their ancestry and to their physical and intellectual features. On the other hand, it should not be solely applied to those persons who are contemporary to one another at any given moment, but also to all their ancestors and descendants." Indeed, "'nation' implies the element of timelessness." Joachim von Ribbentrop, *Germany Speaks: By 21 Leading Members of Party and State* (London: T. Butterworth Limited 1938) 37.

[34] Morant described Nazi racial theory in various publications and talks between 1939-1944. G.M. Morant, "The People," in: Naval Intelligence Division, *Germany Volume II. History and Administration.* BR. 529A Geographical Handbook Series (March 1944) 1-17; G.M. Morant, "The Racial Doctrine of Mein Kampf," *The Modern Quarterly* 2:3 (1939) 248–61; G.M. Morant, "Racial Fallacies and Realities in Europe," talk at the Royal Institute of International Affairs, 12.5.1942. 13.

[35] See for a more extensive discussion of Nazi racial theory: Robert Proctor, *Racial Hygiene: Medicine Under the Nazis* (Cambridge, MA: Harvard University Press 1988), Sheila Weiss, *The Nazi Symbiosis: Human Genetics and Politics in the Third Reich* (Chicago: University of Chicago Press 2010), or in the words of German heads themselves, Joachim von Ribbentrop, *Germany Speaks: By 21 Leading Members of Party and State* (London: T. Butterworth Limited 1938). Morant mentions Ribbentrop's publication in his "Racial Theories and Political Propaganda" talk, especially the chapter on

allowed the Nazi party to dangerously "control the minds of German youth and to inflict the consequences of their theories upon Europe and the world," Morant wrote in 1944.[36]

From 1934 onwards, Morant mobilized biometry to criticize and debunk the Nazi Aryan racial theory. "The racial theories of *Mein Kampf* have often been criticised, and those who disagree with them have usually been content to say that they are obviously erroneous and fantastic. They are of such importance, however, that it is not sufficient to dismiss consideration of them so summarily."[37] Indeed, *Mein Kampf* had sold over 4.5 million copies by 1939, according to its publisher. Morant therefore took a more thorough approach to criticizing Nazism. "It should be shown *why* the views in question are fallacious, and how deductions drawn from them are affected if a *proper* view of the situation be accepted."[38] Morant did not just denounce Nazi racial theory as invalid but wished to demonstrate how and why it was scientifically flawed. Biometry became his political weapon against racism.

Initially, he aimed to advance agreement on racial matters among anthropologists so as to facilitate a united stance against Nazi racism. When this agreement did not materialize, Morant turned his attention to a wider audience.

"National Socialist Racial Thought," by Walter Gross, head of the Reich Bureau for Enlightenment on Population Policy and Racial Welfare.

36 G.M. Morant, "The People," 14.

37 G.M. Morant, "The Racial Doctrine of Mein Kampf," *The Modern Quarterly* 2:3 (1939) 249.

38 G.M. Morant, "The Racial Doctrine of Mein Kampf," 249. Italics mine.

Intervening Within Anthropology

Morant had long been aware of the disagreement between anthropologists on racial issues – the lack of international standardization of measuring methods was a concern close to his heart.[39] That anthropologists also barely agreed on a simple definition of race became clear to him in 1934. Shortly after Hitler's succession, the British anthropological community felt urged to respond to Nazism with a statement, stimulated by anthropologist Charles Seligman and the Czech-Jewish physician Ignaz Zollschan. Accordingly, the British Royal Anthropological Institute established a "Race and Culture Committee" in April 1934 that brought together a diverse group of anthropologists, including Morant. Historian Elazar Barkan has pieced together the few available sources on this committee. Barkan explains that the committee included scholars from both the antiracist and more racist positions but left out Jewish scholars so that the committee remained as "neutral" as possible. The group's diversity of opinion, however, made it impossible to come to any agreement on "the significance of the racial factor in cultural development."[40] After lengthy discussions, the committee published an interim report in 1936 that "showed all the signs of a divided and compromised authorship," according to Barkan. Only three of the 23 pages reflected the shared opinion of the committee and offered two different definitions of race. The rest of the booklet consisted of several addenda authored by committee members in which they presented their individual views on the subject.[41]

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39 G.M. Morant, M.L. Tildesley, and L.H. Dudley Buxton, "193. Standardization of the Technique of Physical Anthropology," *Man* (1932) 155–58. See chapter 5 for the many attempts to standardize racial measuring methods before World War II.

40 Royal Anthropology Institute; the Institute of Sociology, *Race and Culture* (London: Le Play House Press 1936) 2.

41 Elazar Barkan, "Mobilizing Scientists Against Racism: The Anthropological Communities in Britain and America in the 1930s," in: George W. Stocking (ed.), *Bones, Bodies, Behavior*: *Essays on Biological Anthropology* (Madison: University of Wisconsin Press 1988), 180-205, quotation on page 193.

Within a few months of forming the committee, Morant realized that the group was too divided to reach any agreement on the meaning of race.[42] He published an article in the July edition of *Man* detailing his perspective and method, "A Biometrician's View of Race in Man" – the article's summary appeared as an addendum in the interim report. It was the first time that Morant wrote for an audience other than the specialized readers of *Biometrika*. It was also his first solo publication in the journal run by the Royal Anthropological Institute, which was read by a wider audience of anthropologists, professional and amateur. Morant opened his article by stressing the "extreme complexity" of man's racial history. The measurement of just a few bodily characters was therefore insufficient to produce a clear picture of such a complex situation and could only provide "far too simple" linear or two-dimensional arrangements. Instead, the biometrician analyzed thirty-plus characters to imagine a "three-dimensional model" representing racial history that "would resemble a web of irregular pattern rather than a ramifying tree, since the crossing between different branches must have occurred frequently." He warned his readers, however, that the quantitative method "really gives a measure of the probability that the two samples represent the same race, not a *proof* of identity of race." It may, nevertheless, be used provisionally "as if they were definitely established facts."[43]

Moreover, "problems of race are essentially concerned with groups,"– not individuals. It was therefore extremely difficult – if not impossible – to determine racial origins in individuals. To explain this, Morant introduced the concept of normal variation and the bell-shaped curve that characterized the physical qualities of populations. As individuals fell somewhere along this curve, it was impossible to safely determine their racial origins based on their bodily measurements. He

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[42] He reflected on the committee's work in 1939. See: G.M. Morant, "Racial Theories and International Relations," *Journal of the Royal Anthropological Institute of Great Britain and Ireland* 69:2 (1939) 154-155.

[43] G.M. Morant, "126. A Biometrician's View of Race in Man," *Man* 34 (1934) 100; 102.

echoed the Coefficient of Racial Likeness approach by stating that populations often showed equal "intra-racial" variability and that the difference in within-group variation between "civilized and primitive communities" was small. What's more, based on cranial samples, "for the majority of the characters used intra-racial variability is usually greater – and for some characters very considerably greater – than inter-racial variability, that is to say, the individuals belonging to the same racially homogeneous population show greater differences, on the average, than those found between different racial types."[44] Furthermore, distribution curves overlapped. This argument would fundamentally challenge racial research in the 1970s, but Morant stressed that the wide range of "intra-racial variation" did *not* vitiate the concept of race. It merely questioned the racial identification of individuals and the existence of pure races: "The biometrician does not speak of a 'pure race' and it is not clear how such a term could be defined with reference to modern man." Due to a long history of contact and mixture, modern man was not only devoid of pure races, it also "may be found that two adjoining populations grade into one another and that no sharp line of demarcation can be drawn between them."[45]

Variation thus was "an essential feature of the material" and provided the basis for a number of important arguments that disputed long-held anthropological assumptions. These perspectives resulted from his experience of working in Pearson's Biometric Laboratory: since 1900, its racial research had analyzed large samples and variation instead of individuals and types. As chapter 2 discussed, Pearson also questioned the existence of pure types. Moreover, the laboratory had assumed equal variabilities in populations since the early 1920s. Morant had shown in his craniometric research that neighboring populations often resembled each other but now clearly stated that adjacent races shaded into each other, reviving Blumenbach's argument from the late 18th

[44] G.M. Morant, "126. A Biometrician's View of Race in Man," 104.

[45] G.M. Morant, "126. A Biometrician's View of Race in Man," 105.

century. This was the first time that he published the argument that intra-racial variability was larger than inter-racial variability. For him, it undermined the idea and practice of racially identifying individuals, not race itself. Morant hoped that anthropologists would take note of these biometric ideas and recognize the errors in their assumptions about race.

Morant next turned his attention to German anthropologists and published a critical piece in the German *Zeitschrift für Rassenkunde* – "the single most important anthropological journal in the Nazi period," according to historian Robert Proctor.[46] The article challenged the dogma that prehistoric and modern populations had different variabilities. He specifically targeted the Nazi notion that pure races roamed the earth in ancient times and that present-day "Aryan-looking" individuals represented this prehistoric type. Morant grouped racial data on nine modern populations from various published sources and compared their variabilities through standard deviations. Not only were their average standard deviations "remarkably alike for any particular character," when compared to data on prehistoric skeletal series, the similarity in variabilities undermined the assumption that ancient races were more pure than modern ones: "there is no reason to suppose that the existing populations of Europe, or of any other continent, are appreciably more variable than the extinct populations which are represented by series of skeletons." The article clearly criticized elements of the Aryan-Nordic theory that was gaining ground in Germany but did not draw any clear connections with the theory nor name German anthropologists, possibly not to antagonize the journal's editors.[47]

Thus, within-group variation was Morant's ammunition to challenge the existence of pure races, the racial identification of individuals, and claims of clear-cut racial differences between

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[46] Robert Proctor, "From Anthropologie to Rassenkunde in the German Anthropological Tradition," in: G.W. Stocking, *Bones, Bodies, Behavior: Essays on Biological Anthropology* (Madison: University of Wisconsin Press 1988) 162.

[47] G.M. Morant, "An Attempt to Estimate the Relative Variabilities of Various Populations," *Zeitschrift fur Rassenkunde* II:3 (1935) 296–311.

neighboring populations. But while anthropologists such as Haddon, Huxley, and Montagu began to question the use of "race" in anthropology, Morant continued to promote and justify his research on skulls, race, and biometry. He wrote to Ashley Montagu in 1938:

> Several people chide me for paying so much attention to craniology and perhaps you would too. Although the belief is unfashionable, that still seems to me to be the approach which is far more likely than any other to provide solutions to the major problems of anthropology. I would go further and say that most physical anthropologists nowadays neglect those mains problems – concerned with the unravelling of man's pedigree – and devote nearly all their attention to side issues. And I also believe that the advance of genetical theory will not help anthropologists much. But you may be thoroughly shocked by these views.[48]

Morant would change his views on genetics in the following years but maintained that biometry was the best approach to racial analysis. In 1939, he claimed that Pearson's methods were "universally accepted" but disparately applied because Pearson "did not codify a system or lay down any rigid rules for the guidance of those who wished to follow him." Morant hoped that his articles in *Biometrika, Man*, and *Zeitschrift für Rassenkunde* would clarify Pearsonian anthropology and show anthropologists how to produce "valid and useful anthropological conclusions."[49]

Morant's *The Races of Central Europe*

By 1938, Morant realized that physical anthropologists would not agree on definitions of race and would not draft a statement protesting the racial dogmas that were threatening peace in Europe. Anthropologists Franz Boas and Earnest Hooton had attempted to produce an official statement on behalf of American physical anthropologists but failed. Boas's decision to then collect signatures

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[48] AMP, Box 35, GM to AM 19.3.1938.

[49] G.M. Morant, "The Use of Statistical Methods in the Investigation of Problems of Classification in Anthropology: Part I. The General Nature of the Material and the Form of Intraracial Distributions of Metrical Characters," *Biometrika* 31:1/2 (1939) 72–98. Quotations from page 73. Morant had already partially written part II, he wrote to Montagu, which was scheduled to be published later, but it seems that this publication never came.

from the wider scientific community was much more successful: his "Scientists Manifesto" gathered 1284 signatures and prompted the American Association of Anthropology to also publish a statement.[50] British anthropologists, however, remained silent after the failure of the Race and Culture Committee. Morant blamed the controversial character of the issues involved for the committee's lack of success. "There was always the danger that if scientists who had specialised on these topics met to try to come to some agreed statement they would discuss the controversial issues rather than the basic questions upon which they agreed."[51] The topic was once more discussed during the 1936 British Association for the Advancement of Science meeting but no statement was drafted on behalf of British scientists. A second attempt in the late 1930s would probably have been successful, Barkan argues, but "their earlier experience at the Race and Culture Committee was apparently enough of a discouragement."[52] Morant considered initiating a statement independently but did not succeed and was disappointed that none eventually came off the ground.[53]

Rather than fostering agreement on racial "truths" among anthropologists, Morant began warning the larger public. He began writing a "short and scrappy" book that demonstrated the discordance between racial differences and language barriers in Europe, another bastion of Nazi theory. "It may do a little good to point this out," he wrote to Montagu.[54] The resulting publication,

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50 See Elazar Barkan, "Mobilizing Scientists Against Nazi Racism, 1933–1939" for an elaborate discussion of attempts by Boas. Earnest A. Hooton, "Plain Statements About Race," *Science* 83:2161 (1936) 511–13. The Geneticists Congress also published a manifesto in August 1939. See: H. Gruenberg, "Men and Mice at Edinburgh: Reports from the Genetics Congress," *Journal of Heredity* 30:9 (1939) 371–74.

51 G.M. Morant, "Racial Fallacies and Realities in Europe," talk at the Royal Institute of International Affairs, 12.5.1942. 13.

52 "Genetics and Race," *Nature* 137 (1936) 998–99; Barkan, *The Retreat of Scientific Racism,* 340.

53 AMP, Box 35, GM to AM, 4.2.1940.

54 AMP, Box 35, GM to AM, 3.11.1938.

The Races of Central Europe. A Footnote to History (London: Allen & Unwin, 1939),[55] "was written as a protest," Morant opened, "against the views regarding race which were almost universally accepted at the time of the Munich agreement" of 1938 that were used to legitimate the German annexation of portions of Czechoslovakia. These views fallaciously equated language "frontiers" with racial boundaries and thus claimed that German-speaking peoples in this new "Sudetenland" belonged to the "German race." What is more, politicians and writers in many European countries conflated race and language. The book's main object was to "discover how far the physical evidence relating to the peoples of Central Europe leads to conclusions regarding 'race' that are in accordance with those derived from cultural data and primarily from language." With biometric and craniometric evidence, Morant exposed that there were no racial differences in Central Europe and that language barriers therefore did not indicate racial barriers. His "narrow" expertise which at first sight might seem like "hair-splitting," as the new University College Professor of Biometry J.B.S. Haldane wrote in the preface to the book, was now of political importance and allowed him "to reach the utmost possible accuracy." "So far as almost all Dr. Morant's work has appeared in learned periodicals such as *Biometrika,* which neither the man in the street nor the politician reads." Haldane continued. But now "he has discovered that he can write for a wide audience."[56]

In 159 pages, Morant gave the reader a crash course in linguistics, physical anthropology, and statistics. Each of the ten chapters examined a racial characteristic, such as the cephalic index, stature, skin color, blood groups, and Central European prehistoric skulls. The chapters discussed the available racial data and were aided by maps that demonstrated the distribution of these

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[55] The book was printed at the start of the war, probably in late September 1939. GMP, "Statutory Rules and Orders 1939. Numbers 1083 + 1084," GMP, Note on the back of a letter from George Allen & Unwin publishers to Morant, 7.9.1939. Allen & Unwin was the same publisher that printed Huxley's *Evolution: the Modern Synthesis* a few years later.

[56] J.B.S. Haldane, "Preface," in: G.M. Morant, *The Races of Central Europe. A Footnote to History* (London: Allen & Unwin 1939) 5-8. Quotation from page 19.

characters in the area. In the chapter on the cephalic index, a distribution map compiled of datasets of 29 populations divided the area into four groups (fig. 4.1).

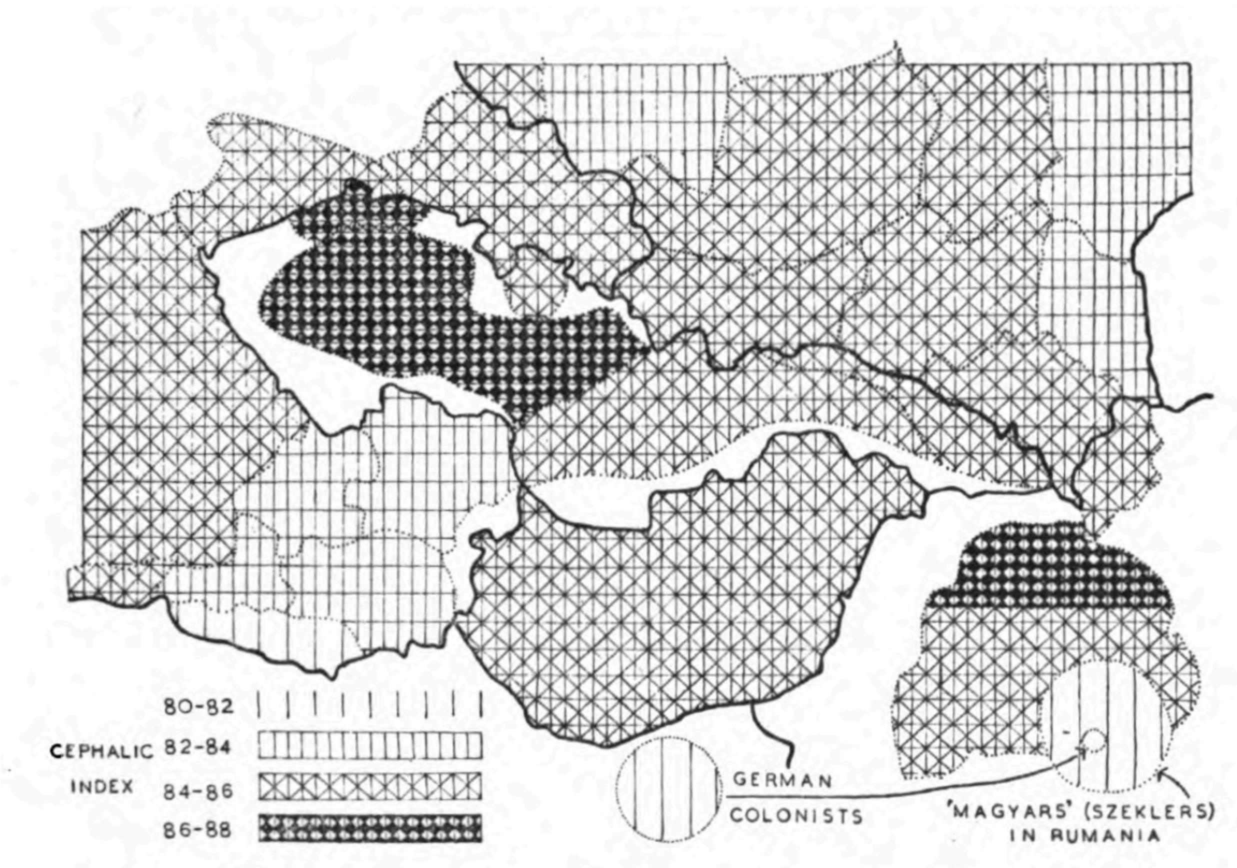

Figure 4.1. "Average cephalic indices for peoples of central Europe." "Full lines indicate national frontiers and dotted lines divisions, such as provinces, within countries," Morant explained. Blank areas indicated a lack of data. Source: Geoffrey Morant, *The Races of Central Europe: A Footnote to History* (London: Allen & Unwin Limited 1939) 37. Reproduced with the kind permission of Martin Morant.

Morant admitted that these groupings were arbitrarily based on an average index but conveniently gave a general impression of the distribution, which clearly spilled over national and linguistic boundaries:

> These comparisons … fail to suggest that there is any kind of correspondence between the distribution provided by the physical character and that provided by the languages spoken by the populations…In fact, the averages for the cephalic index indicate remarkable uniformity throughout the region. In general, they show a gradual transition on passing from one area to another and very few abrupt divisions between the peoples occupying adjoining areas.[57]

[57] G.M. Morant, The Races of Central Europe, 42-43.

These maps *showed* the reader the lack of sharp divisions in racial characteristics. Morant strongly believed that visual tools such as diagrams and maps effectively translated human biology research to the public and argued elsewhere that they should be incorporated in museum exhibits. Skulls in museums merely represented extremes, but diagrams illustrated variation and introduced the public to statistical generalizations.[58] Unsurprisingly then, Morant communicated his arguments with maps in this publication.

The maps also demonstrated that each anthropological character arranged the distribution differently. The map of average statures created different divisions than the cephalic index (fig. 4.2).

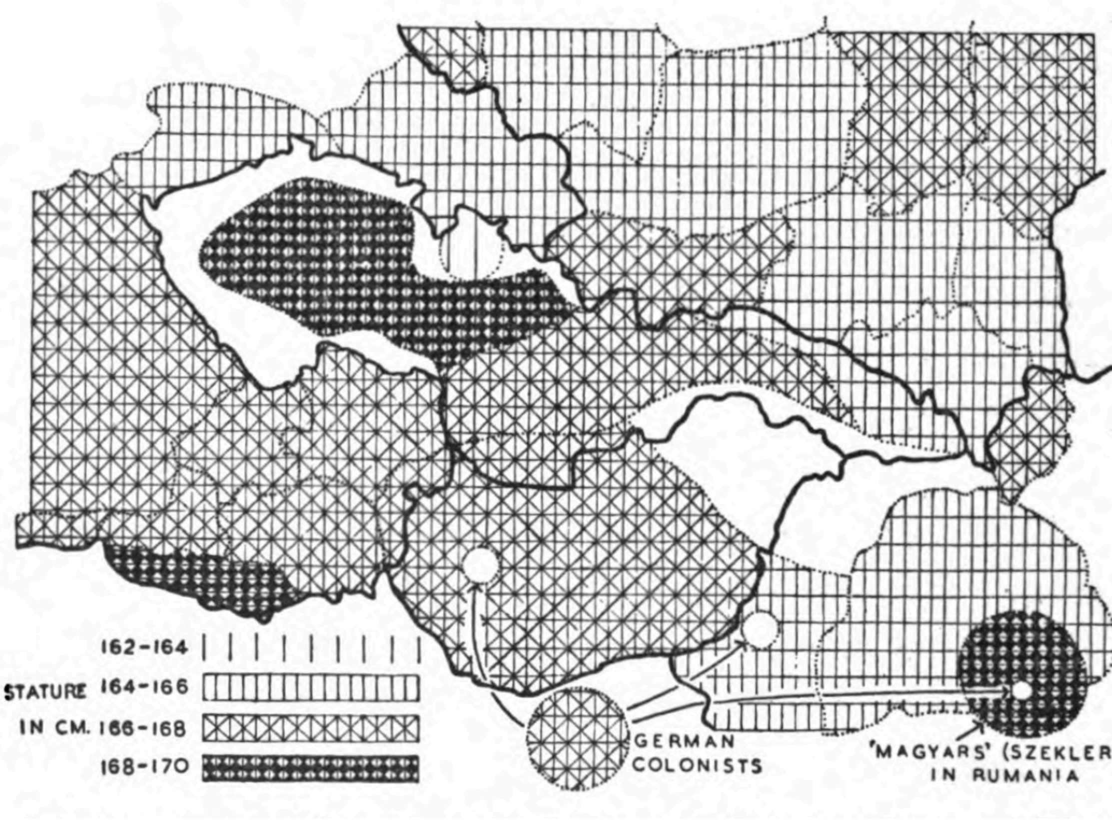

Figure 4.2. "Average statures for peoples of Central Europe." Source: Geoffrey Morant, *The Races of Central Europe: A Footnote to History* (London: Allen & Unwin Limited 1939) 46. Reproduced with the kind permission of Martin Morant.

58 G.M. Morant, "84. Museums, Human Biology and Diagrams," *Man* 38 (1938) 83–85.

The stature map also demonstrated that Hermann Göring's claim that Czechs were a "pygmy race" "was particularly inept: they are actually found to be taller than all the German-speaking peoples surrounding them."[59] Finally, the maps showed discordance between characters, for instance between skin color and the cephalic index. "There is no justification for picking out subgroups of people with particular combinations of these characters and supposing that these groups had different origins." Indeed, "it is usually quite unsafe to infer that a particular individual belongs to a particular race from an examination of his physical characters alone," he warned.[60] The chapter on blood groups discussed "amusing" data as he wrote to Montagu.[61] Indeed, the data revealed "forcefully" that "Germany is racially far more heterogeneous than any other country in Europe."[62] With prehistoric craniological evidence, Morant delivered another blow to the theory of Germany's Nordic origins by arguing that the migration of Anglo-Saxon and Norman peoples had completely wiped out Nordic peoples in Europe.

Morant introduced variation in the middle of the book. Here, he discussed the equal variability of prehistoric and modern populations and challenged the idea of pure races. He blamed the Roman historian Tacitus for the popular belief in pure races, who had described Germans as pure and unmixed. "Variation must have existed, but it was harder to assess and describe in verbal accounts, and hence its existence was conveniently ignored." Over time, it became customary to describe types without impressions of variability, as traveler's accounts of "primitive" peoples demonstrated. "Such inadequate descriptions tend to convey the impression that the differences

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[59] G.M. Morant, *The Races of Central Europe*, 52. See William L. Shirer, *Rise and Fall of the Third Reich: A History of Nazi Germany* (New York: Simon & Schuster 1960) page 383 for the full quote by Göring.

[60] G.M. Morant, *The Races of Central Europe*, 89.

[61] AMP, Box 35, GM to AM 3.11.1938.

[62] G.M. Morant, *The Races of Central Europe*, 113.

between the peoples are much greater than they are in reality."[63] Morant again stressed that the differences between populations were smaller than the differences between the population's individuals. As a result, physical characters formed a "continuous system, with every population overlapping every other to a large extent, so that the groups can only be distinguished on account of small differences between their averages."[64] They were like "a bridge, as it were, leading from a cluster of peoples showing close interrelationship on one side of the arbitrary line to a similar cluster on the other side."[65] Racial classifications were thus inherently arbitrary but convenient anthropological tools.

He concluded that his data showed no correspondence between Europe's linguistic barriers and "racial" differences. Furthermore, it was the Germans who stood out with their remarkably heterogeneous physical records. If one accepted the book's conclusions, "then it is obvious that certain racial theories current today are utterly absurd. When the language map is accepted as a racial map the differences between populations are raised to a fictitious maximum, and this exaggeration is admirably adapted to serve certain political ends…It is surely time that the anthropological view of the situation became more widely known."[66] Anthropology's "simpler truths" could expose racial lies, such as the "fallacious" and "utterly misleading" German dogmas. Crucially, "the security of Europe would be more assured if politicians in all countries made no statements regarding race except ones which could be substantiated by scientific evidence."[67] Thus, Morant's biometry of race aimed to engender peace.

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63 G.M. Morant, *The Races of Central Europe*, 64-65.

64 G.M. Morant, *The Races of Central Europe*, 72.

65 G.M. Morant, *The Races of Central Europe*, 139.

66 G.M. Morant, *The Races of Central Europe*, 142-143.

67 G.M. Morant, *The Races of Central Europe*, 9.

The Races of Central Europe did not cover all "articles of faith in Nazi Germany." In the epilogue Morant briefly discussed the lack of association between physical and mental characters in individuals. If there were an association, he argued, it would likely be similar in nature to physical characters, with more variety within than between populations. "All the peoples have some members who are exceptionally able and some who are exceptionally stupid."[68] Moreover, anthropological differences of any kind failed to indicate superiority or inferiority. Morant also explained why Jewish people and gypsies, groups targeted by Nazi discriminatory policies, were only peripherally discussed in the book: "Jews and Gipsies [sic] form minority groups in Central Europe, and they have not been included in considering the geographical distribution of other physical characters."[69] Morant possibly felt that this topic required a book of its own: he wrote to Montagu that he would "tackle Jewish material" in the nearby future and that "it would be good fun demonstrating that Jews are racially much more homogeneous than Germans," building on his arguments from *The Races of Central Europe*.[70] This publication, however, never appeared. He brought up the topic once more in a lecture in 1942 and stated that "the distinction between European Jews and non-Jews, judging solely by physical characters, was about the same as that between other European groups."[71]

Morant sent the book to 21 colleagues in the field, including Carleton Coon, Ashley Montagu, Aleš Hrdlička, Swiss anthropologist Otto Schlaginhaufen, and French anthropologist Henri Vallois. British racialist Arthur Keith also received a copy and responded that the book came at a crucial time. "As you know we interpret some things differently – but the ascertained things are

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68 G.M. Morant, *The Races of Central Europe*, 151.

69 G.M. Morant, *The Races of Central Europe*, 83-4.

70 AMP, Box 35, GM to AM, 29.1.1939.

71 G.M. Morant, "Racial Fallacies and Realities in Europe" talk at the Royal Institute of International Affairs, 12.5.1942, 10.

those which matter."[72] In his own copy of *The Races of Central Europe,* Morant collected several British reviews and newspaper clippings about the book (fig. 4.3).

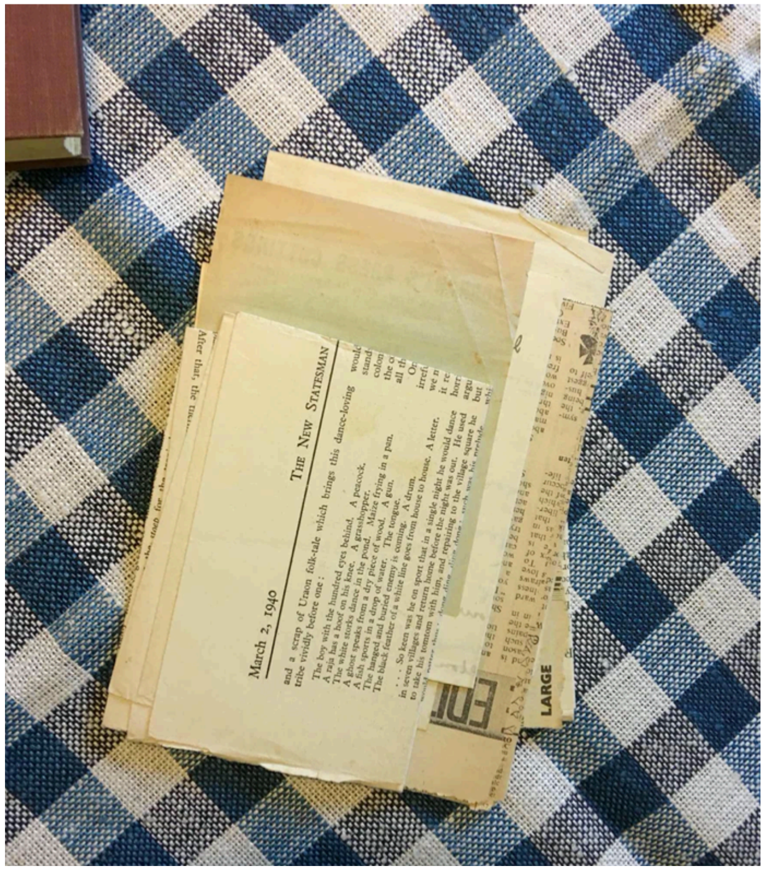

Figure 4.3. Collection of newspaper reviews that Morant kept in his own copy of *The Races of Central Europe*. Photograph by author.

The reviewers were overall quite positive and considered Morant an authority on physical anthropology. The *Sunday Times* wrote that Morant showed no signs of the narrow specialization that Haldane spoke of in the preface. The reviewers also appreciated Morant's honest tone. The reviewer for *John O' London's Weekly* wrote that "we feel convinced that if he found the Nazi theories justified, even in part, he would say so." All reviews stressed the main takeaway of the book: in Europe, linguistic boundaries were very different from racial differences and the physical records

[72] GMP, Keith to Morant, 11.10.1939. Unlike Morant, Keith was a staunch supporter of hierarchical racial differences.

showed a continuous system. They also excitedly reprinted the argument that Germany was racially more heterogeneous than other European countries. One review even headlined: "The Aryan Myth. Hitler's Germans are More Mixed than Their Neighbours!" Although Morant hoped his book would quell false ideas and educate the public on racial truths, the reviewer of *The New Statesman and Nation* was less optimistic: "this modest book is not likely to have the slightest effect on the statesmen who make the settlement at the end of the war."[73]

The reviews in scientific journals were also positive. American anthropologist Wilton Krogman called the book "one of the most sensible treatises on race which has appeared in recent years," declaring it to be "the perfect answer to the 'racial' dogma of *Mein Kampf*."[74] British anthropologist Wilfrid Le Gros Clark, Morant's fellow Race and Culture Committee member, agreed with Keith that the book came at a "very appropriate moment" when false concepts of race needed to be protested.[75] Morant's argument about German heterogeneity was also popular in the scientific reviews. Krogman exclaimed: "Anthropologist Adolf please note!"[76] *Nature*'s reviewer stressed that Morant represented the "older biometric school of anthropologists" to which some colleagues objected but pointed out that "Morant is clear that race cannot be used to rearrange the political map of Europe and that racialism as between Europeans is rubbish. All his fellow-workers will subscribe to this conclusion" despite possible misgivings about biometry. Some reviewers noted

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73 "The Races of Central Europe. A Footnote to History by G.M. Morant" *Times Literary Supplement* (7.10.1939); John Lehmann, "Danubia," *The New Statesman and Nation* (2.3.1940) 283-4; John Brophy, "The Aryan Myth. Hitler's Germans are More Mixed than Their Neighbours!" *John O' London's Weekly* (13.10.1939); "What is Race? *The Races of Central Europe*. By G.M. Morant, D.sc. Preface by J.B.S. Haldane, F.R.S."*Sunday Times* (12.11.1939).

74 Wilton Krogman, "*The Races of Central Europe*. By G.M. Morant," *American Journal of Sociology* 46:6 (1941) 612-613.

75 W.E. Le Gros Clark, "226. The Races of Central Europe. By G.M. Morant," *Man* 40 (Dec 1940) 191-192.

76 Wilton Krogman, "*The Races of Central Europe*," 613.

Morant's novel arguments about within-group variation and cited his claim that characters overlapped across Europe's populations.[77]

The reviewers welcomed Morant's book as a timely critique rather than judging him for using his scientific expertise to discuss controversial political issues, a fear that supposedly withheld others from discrediting racism according to Brattain and Barkan. Le Gros Clark wrote that:

> No one doubts now that for years the public conscience has been gulled and misled by skilful [sic] appeals to the doctrine of 'race,' and it is also certain that for years anthropologists have been aware of the confusion which has resulted from loose thinking and also from deliberate mis-statements on the subject. Occasionally one anthropologist or another has expostulated, but what has been most needed is a carefully prepared statement on the racial problems of Central Europe from the point of view of the biologist.[78]

Indeed, while many other anthropologists kept silent, Morant spoke out.[79] Despite his belief in science for science sake, he felt forced by the politics of the day to act as the voice of scientific reason in the public domain.

Neither statements against race nor Morant's *The Races of Central Europe* prevented the war from breaking out. Morant took stock of the situation in January 1940 in a lecture at the Royal Anthropological Institute and expressed feelings of disappointment. "The situation which culminated in the events of last September was, or should have been, a matter of special concern to anthropologists in all countries…What part did we play in that overture? Did we do anything?" He

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[77] See Chapter 2 on the discordance between biometricians such as Pearson and other anthropologists. Other reviews: "Race Questions in Europe," *Nature* No 3653 (4.11.1939) 767-768; M.B., "89*. The Races of Central Europe. A Footnote to History. By G.M. Morant. With a preface by Professor J.B.S. Haldane.," *International Affairs (Royal Institute of International Affairs)* No. 6 (Nov-Dec 1939) 846-847; Ales Hrdlicka, "The Races of Central Europe. By Morant (G.M.); with a Preface by J.B.S. Haldane," *American Journal of Physical Anthropology* 27:1 (June 1940) 172-173.

[78] W.E. Le Gros Clark, "226. The Races of Central Europe," 191-192.

[79] Morant printed his misgivings of Nazi racial theory in other places. A few months before the publication of *The Races of Central Europe*, in July 1939, he published an article about *Mein Kampf* in a new left-wing journal called *Modern Quarterly*. Early in 1939, the first English translation of Hitler's book, written by translator James Murphy, hit the bookstores. Within a few weeks Morant penned down a rebuttal of Hitler's views on race. Besides repeating his arguments against Nazi dogmas, including the "far greater" variation within populations than between groups, he stressed that none of Hitler's arguments were backed by any evidence. AMP, Box 35, GM to AM 21.4.1939; G.M. Morant, "The Racial Doctrine of Mein Kampf," *The Modern Quarterly* 2:3 (1939) 248–61.

concluded that anthropologists did nothing to reject the "cult of race." "Anthropologists have known perfectly well that such talk was nonsense, but they have failed to protest against it in any effective way," even when these beliefs threatened world peace. The lack of collective action, Morant explained, resulted from methodological and theoretical disagreement and prevented anthropologists from giving guidance on racial matters. What is more, racial data had accumulated faster than the crystallization of analytical methods, leaving the discipline in a transitional stage. "There is no unanimity of opinion among anthropologists regarding questions of racial analysis." The failures of the Race and Culture Committee were a prime example. Its pamphlet was far too abstract to have any value as propaganda. "It is unlikely that the pamphlet containing them was brought to the notice of Dr. Goebbels, and we must admit that it would not have caused him much anxiety if he had seen it."[80]

Due to disagreement between anthropologists, "it is not surprising that the popular conception of race is a confused medley," he continued, a "vague belief" based on historical accounts, language distinctions, and national traditions, nebulous enough to convince people of racial differences in Europe. Although some anthropologists supported these nebulous schemes, "honest" anthropologists had developed a different conception of race: as Johann Blumenbach pointed out centuries ago, gradual transitions and continuity characterized races, not sharp divisions. Races could therefore not be defined with any precision and only Pearson's statistical methods offered an effective way of arranging racial data. This was the "true conception" of race. He finished the talk on a positive note: there was still time to unite in protest against Nazi propaganda. "Now that war has come, the need for insisting on the recognition of a scientific view of racial

[80] G.M. Morant, "Racial Theories and International Relations," *Journal of the Royal Anthropological Institute of Great Britain and Ireland* 69:2 (1939) 151-152; 156-157.

problems is even greater than it was before." The boundaries between groups in Europe "are entirely artificial, and it is our duty to do what we can to break them down."[81]

Morant continued to urge anthropologists to draft a statement against racial dogmas throughout the war. He repeated that a "truthful" approach to race would "foster good international relations and go far towards mitigating the evils which may result from the illusions which the people of Europe cherish regarding their origins and presumed distinctions."[82] Historians would later debate whether the exceptionality of Nazi science and its deployment for horrific ends such as the Final Solution were the result of "softer" and "immature" sciences such as anthropology seeking power and prestige.[83] Morant blamed the lack of agreement regarding questions of race. "The fact that anthropologists were not agreed regarding methods of racial analysis need not have prevented them from making an effective protest."[84] He bracketed Nazi racial theory as dishonest and

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[81] G.M. Morant, "Racial Theories and International Relations," 161–62. It is interesting that Morant does not mention his own *Races of Central Europe* in this respect, perhaps feeling like this was not the right place to promote his own work but instead stress the lack of action that befell onto all anthropologists.

[82] AMP, Box 35, GM to AM 13.12.1939, 4.2.1940, Morant, "Racial Fallacies" quote on page 10.

[83] In the past 40 years, historians have begun to explore how and why German doctors and anthropologists came to play a crucial role in the justification, development, and implementation of Nazi racial policies. Some of the earlier works focus on naming the medically trained scientists and exposing their involvements in horrifying medical experiments, often arguing that their "bad science" was an anomaly of "normal" science. Here, the argument of "disciplinary blame" was offered to suggest that anthropologists were eager to acquire scientific status for their new discipline and thus willing to misuse science and collaborate with the Nazi regime. Historians from the late 1980s onwards, however, have largely moved away from scapegoating individuals and disciplines, instead embedding the research of Nazi anthropologists and geneticists in a larger historical-scientific context. Rather than dismissing their work as pseudo-science, historians like Robert Proctor have asked how and why these "medical transgressions" could have taken place. Works since have explored themes such as career opportunism and the international network of race scholars in which German scientists enjoyed prestige in order to explain why Nazi science took the trajectory that it did. Sheila Weiss, for instance, imagines the relationship between German anthropologists and the Nazi party as a "Faustian bargain," a symbiotic relationship of ever-changing negotiation with professional and political consequences for all parties involved. Thus, this newer body of work concludes that German scientists were not corrupted by Nazi politics or "unwilling puppets" of the Nazi regime. Instead, they appear as active members of an international community of race researchers, which Morant alludes to here as well. See: Benno Müller-Hill (translated by George Fraser), *Murderous Science: Elimination by Scientific Selection of Jews, Gypsies, and Others in Germany 1933-1945* (Oxford: Oxford University Press 1988); Robert Proctor, *Racial Hygiene: Medicine Under the Nazis* (Cambridge, MA: Harvard University Press 1988), Mario Biagioli, "Science, Modernity, and the 'Final Solution'," in: Saul Friedlander, *Probing the Limits of Representation. Nazism and the 'Final Solution'* (Cambridge, MA: Harvard University Press 1992) 185-204; Sheila Weiss, *The Nazi Symbiosis: Human Genetics and Politics in the Third Reich* (Chicago: University of Chicago Press 2010).

[84] G.M Morant, "Racial Theories and International Relations," 161.

misinformed. His biometric approach to race and anthropology, on the other hand, revealed the "true" meaning of race which could pave the road to peace. Racial studies itself were not to blame.

Holding Onto Race

At the beginning of World War II, the British government shut down most scientific activities and recruited several scientists for army purposes. While Fisher and Haldane continued their scientific research at University College London,[85] Morant was recruited as a censor by the Press and Censorship Bureau. The work left him with little time to continue his biometric research. He managed to carry on his editorial work for *Biometrika* and the *Journal of the Royal Anthropological Institute*, which continued to print despite great delays caused by the war. In 1943, Morant began working as a senior scientific officer for the Royal Airforce Institute of Aviation Medicine and became engaged in anthropometric enquiries for war efforts.[86]

In some scientific circles, resistance to the concept of race was growing. Morant's war-time correspondence with the well-known anti-race campaigner Ashley Montagu reveals how he continued to debunk "false" racial theories, while defending the biological existence of race. Morant and Montagu began corresponding in 1937 and maintained an active exchange of letters throughout the war. Montagu even offered to host his family in the United States during the war, sent Morant's children money to buy books, and dedicated two publications to Morant. In these letters, they discussed their views on race with each other and exchanged offprints and comments. Montagu frequently sent Morant batches of his offprints, which Morant greatly appreciated, especially during

[85] Montagu wrote that "servants of the college" assaulted Fisher and threatened Haldane for their antiwar perspectives and criticism of the British government. Ashley Montagu, "Review of: Adventures of a Biologist by J.B.S. Haldane," *Isis* 33:2 (1941) 297–98.

[86] AMP, Box 35, GM to AM 13.12.1939; 4.2.1940; 25.5.1941; 19.9.1943; GMP, "Curriculum vitae."

the war. "There was never a time when the kind of work you are doing interested me more," he wrote in 1941, "and I am in real danger of losing touch with recent writings on anthropological topics."[87]

In the 1940s, Montagu started attacking "race" and racism in talks, articles, and books. He combined a preference for genetical understandings of human difference with the rather radical desire to replace "race" with "ethnic groups," arguing that "race" had become an inadequate and dangerous concept for explaining human variation.[88] In 1941, he "shocked" the audience of the annual American Physical Anthropology meeting with his suggestion to drop race from the anthropological vocabulary.[89] A year before, he targeted biometry during the American Association for the Advancement of Science meeting. Montagu stressed the importance of morphological knowledge in physical anthropology and lamented that some anthropologists "have never so much as touched a scalpel, but have gone right on to the calipers. Such men, however competent they may be at their work, can rarely succeed in becoming anything more than good technicians." Montagu specifically attacked statistics, which threatened to assume the place once occupied by morphology. Statistics was a method, not a science, he argued. It demonstrated interesting relationships but could

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[87] AMP, Box 35, GM to AM 4.2.1940; 21.7.1940; 12.9.1941 (quote); publications dedicated to Morant: Ashley Montagu, *Adolescent Sterility. A Study in the Comparative Physiology of the Infecundity of the Adolescent Organism in Mammals and Man* (Springfield, IL: C.C. Thomas Publisher 1946); Ashley Montagu, *The Reproductive Development of the Female* (New York: Julian Press 1957).

[88] Montagu had obtained this insight from British biologist Julian Huxley and anthropologist Alfred Haddon who wrote *We Europeans: A Survey of 'Racial' Problems* (London: Jonathan Cape 1935), "a scientific statement written in popular form" that targeted the "pseudo-scientific" Nazi racial theories and the widespread ignorance about the term race. Quite radically, the authors suggested replacing the term "race" by "ethnic groups." The book's main achievement, Barkan concludes, was its ability to combine biological and anthropological approaches. Haddon and Huxley argued that anthropology had become outdated by its inability to incorporate genetical theory and its quantitative approach to human variation. Haddon and Huxley asserted that the statistical methods developed by Pearson and "his school" for biological and anthropological problems were "of very great value, since they provide an efficient method of guarding against generalization upon imperfect or inadequate data – a scientific stumbling block on which anthropologists have frequently tripped." In other places in the book, they pointed out the need for frequency distribution curves and correlation tables, the "essential descriptive work" for any classification. J. Huxley, A.C. Haddon, and A.M. Carr-Saunders, *We Europeans: A Survey of 'Racial' Problems* (London: Jonathan Cape 1935) 61, 142; See for discussion of the book Elazar Barkan, *The Retreat of Scientific Racism,* 296-310. See also Brattain, "Race, Racism, and Antiracism," 1394.

[89] "Anthropologists," *Time* (21.4.1941) 59.

never explain the biological causes of such relationships. Montagu criticized one of Morant's CRL papers and claimed that, without knowledge of morphology, genetics, and human prehistory, "the statistical anthropologist is not likely to contribute much towards the progress of his science, unless it be towards its greater confusion, and the obfuscation of its purposes."[90] After receiving a copy of the talk, Morant wrote back that he felt a strong urge to rebut these statements, but also stressed that he enjoyed discussing these matters with his American colleague. "It behoves [sic] us both to remember that: 'La critique est la vie de la science.'"[91]

Montagu continued to send publications that questioned the existence of race; Morant repeatedly responded with gratitude and disagreement. Late November 1942, he received a signed copy of *Man's Most Dangerous Myth* (fig. 4.4). In this groundbreaking bestseller, Montagu declared that the anthropological conception of race was "nothing but a whited sepulchre, a conception which in the light of modern field and experimental genetics is utterly erroneous and meaningless. As such it should be dropped from the anthropological as well as from the popular vocabulary, for it is a term which has done an infinite amount of harm and no good at all." The race concept was a meaningless "omelette" that had "no existence outside the statistical frying-pan in which it has been reduced by the heat of the anthropological imagination."[92] Morant marked several of the book's pages with exclamation points and question marks and wrote to Montagu: "In attacking the

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90 Ashley Montagu, "Physical Anthropology and Anatomy," *American Journal of Physical Anthropology* 28:3 (1941) 261–71. Quotations on pages 262 and 270-271.

91 AMP, Box 35, GM to AM 2.2.1941; 25.5.1941.

92 Ashley Montagu, *Man's Most Dangerous Myth: The Fallacy of Race* (New York: Columbia University Press 1942) 28; 32. In the passages referred to here, Morant doesn't seem to be criticizing biometrics. The statistical process of "anthropological race-making," included: "The process of averaging the characters of a given group, of knocking the individuals together, giving them a good stirring, and then serving the resulting omelette as a 'race'." A more proper description of a group included its wide variability, Montagu argued. Biometrics must have remained problematic in Montagu's eyes as it continued to rely on this "older" anthropological conception of race and remained engaged in "taxonomic exercises" of classification. Confusingly to Morant, Montagu wrote on page 4 that "in the biological sense there do, of course, exist races of mankind." Morant wrote "Thanks!" next to this passage. On page 74, Montagu claimed that "race" should be used when "referring to the five or six large divisions of man." Morant's question marks next to passages such as these suggest a sense of confusion about Montagu's position on race.

'anthropological' concept of race are you not concerned with the view of certain anthropologists only? I should say they were bad ones who are justly criticised, but that there is another concept of race which you ignore" – the "truthful" biometric concept.[93]

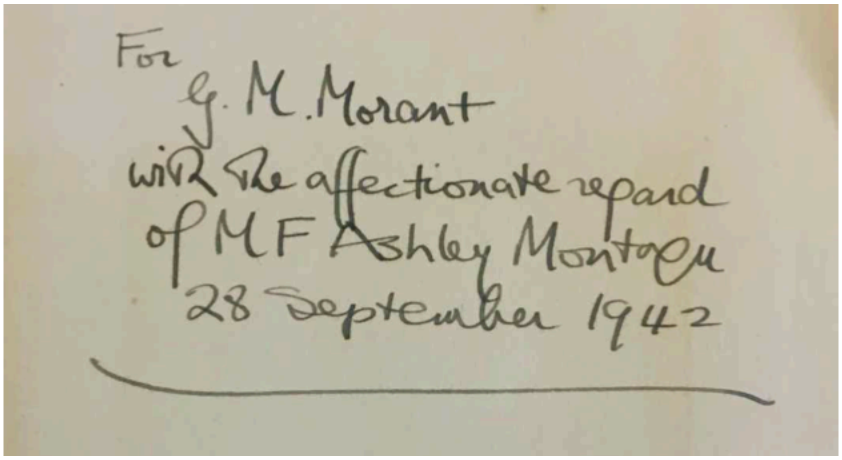

Figure 4.4. Personalized note from Montagu inside the copy of *Man's Most Dangerous Myth* that he sent Morant. Morant noted at the bottom of this page: "received 23/11/42." Photograph by author.

In a talk at the Marx Memorial Library and Workers' School, Morant countered that Montagu's claim about the meaninglessness of race was "merely the fruit of a literary exercise which shows little appreciation of the difficulties encountered in dealing with a complicated problem." Although he agreed that the incorrect use of the term had created much confusion, Morant conversely argued that race was fully charged with meaning. Even though anthropologists had failed to protest "bad anthropology" based on traditions, language, and history, there still was "a stock of reliable scientific knowledge which might well have been used to expose the absurdity of false racial doctrines." Indeed, "a proper understanding of the biological conception of race shows that the populations of neighbouring countries are essentially linked by sharing a measure of common origin, and it might well be used as counter-propaganda aiming at improving international relations

[93] AMP, Box 35, GM to AM 17.12.1942.

instead of embittering them." Racial data proved the reality of racial difference *and* likeness and such truths could combat racism.[94]

Morant also began embracing genetics as a transformative tool for anthropology. At the centenary meeting of the Royal Anthropological Institute of 1943, he argued that "the development of statistical procedure and the science of genetics…has led to the gradual abandonment by specialists of the earlier concept of race in man, and a striving towards a new concept which promises to be irreconcilable with all popular ideas regarding the topic." This change, however, had been little recognized because these ideas had not been made intelligible to others. Crucially,

> It seems to me that the time has come when anthropologists must fully recognize fundamental changes in their treatment of the problem of racial classification. The idea that a race is a group of people separated from all others on account of the distinctive ancestry of its members, is implied whenever a racial label is used, but in fact we have no knowledge of the existence of such populations to-day or in any past time. Gradations between any regional groups distinguished, and an absence of clear-cut divisions, are the universal rule. Our methods have never been fully adapted to deal with this situation.[95]

The problem continued to be about racial theory *and* its methods of analysis. All along the biometricians had urged anthropologists to adopt new methods to make their racial research more "scientific" and "true," to use statistical tools to explore variation within and between populations. Morant concluded that the traditional concept of race, one based on sharp divisions and pure races, must be recast and modified with new nomenclature.[96] He likely meant discarding old terms such as "Teutonic" and "Alpine," but certainly did not mean replacing "race" with "ethnicity" as Montagu had proposed. Nevertheless, Montagu believed that Morant finally had made the transition he

[94] GMP, G.M. Morant, "The Meaning of Race," talk at the Marx Memorial Library and Workers' School 18.6.1942, quotations on pages 1 and 10-11. This argument was also made in G.M. Morant, "Racial Theories and Political Propaganda," (undated).

[95] G.M. Morant, "6. The Future of Physical Anthropology," *Man* 44 (1944) 17.

[96] The centenary meeting took place on 30.10.1943. Morant sent the copy of *Man* with the full account of the proceedings, including Morant's talk, to Montagu on 28.1.1944.

desired for anthropologists and could now be ranked among others who espoused the meaninglessness of race. He published in 1944:

> Hogben, Haddon and Huxley, Morant, and myself, entertain no doubts as to the meaninglessness, not alone of the popular conception, but also of the anthropological conception of race. We do not consider that any of the existing conceptions of race correspond to any reality whatsoever; but we do consider that the persistence of the term and of the concept has been responsible for much confused thinking, and what is worse, has rendered possible much confused and confusing action resulting in the most tragic consequences for large numbers of mankind. It is for these reasons that several of us, as biologists, have recently urged that the term 'race' be altogether dropped from the vocabulary, at least, of the anthropologist.[97]

In the second edition of *Man's Most Dangerous Myth* (1945) Montagu wrote: "physical anthropologists must recognize that they have unwittingly played no small part in the creation of the myth of 'race,' which in our time has assumed so monstrous a form. I am glad to say that since the appearance of the first edition of the present volume a number of anthropologists have seen their responsibility clearly and are taking active steps to exorcise the monster and deliver the thought and conduct of mankind from its evil influence." Morant, "England's most distinguished physical anthropologist," exemplified this transition.[98] The biometrician, however, considered himself more "old-fashioned." This particularly meant that he argued, for instance in his 1943 lecture, that "anthropology should be pursued for its own sake as an academic study" and not serve some immediate practical use. After his lecture, American cultural anthropologist Margaret Mead told Morant "if that's what you mean by physical anthropology I don't wonder that there are so few jobs for you." Morant further complained that the "social fellows (including the ladies) are disrupting anthropology" by breaking up "the whole science of man" and its various branches into different disciplines, thus voicing his

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97 Ashley Montagu, "Two Articles on 'Race'," *ETC: A Review of General Semantics* 2:1 (1944) 53. Montagu cites Morant's 1939 "Racial Theories and International Relations" piece here.

98 Ashley Montagu, *Man's Most Dangerous Myth: The Fallacy of Race* (New York: Columbia University Press 1945) 36.

dissatisfaction with social and cultural anthropology, research areas that were gaining ground as distinct fields of study.[99]

UNESCO and the Postwar Defense of Race

Montagu's desired changes to race and science came after the war. The war's disasters and the horrors of the Holocaust were a "sudden and rude awakening" and urged geneticists and anthropologists to rethink the direction of anthropology and the meaning of race in science. The reality of Jim Crow in the United States and growing resistance against European colonialism in the postwar world order intensified these urges. Scholars again desired to enlighten the public about racism and racial dogmas with scientific knowledge. The United Nations Educational Scientific and Cultural Organization, founded in 1945, was "best equipped to lead the campaign against race prejudice and to extirpate this most dangerous of doctrines," according to its division on Racial Questions. Not unlike Morant's arguments of 1938, UNESCO claimed that "race hatred and conflict thrive on scientifically false ideas and are nourished by ignorance." To combat such hostility, UNESCO brought together a group of scientific experts to draw up a statement on race that was supposed to reflect scientific consensus. Headed by Ashley Montagu, the committee of eight consisted of mostly ethnologists, sociologists, and two physical anthropologists, Spanish-Mexican physical anthropologist Juan Comas and Montagu himself. The resulting "Statement on Race" argued that there was "no proof that the groups of mankind differ in their innate mental characteristics, whether in respect of intelligence or temperament" and claimed that man had an innate drive to brotherhood. Moreover, it suggested "to drop the term 'race' altogether and speak of *ethnic groups*," calling race a "social myth." These remarks clearly reflected the influence of Montagu. Released to the public on 18 June 1950, the statement received broad and positive international

[99] AMP, Box 35, GM to AM, 28.1.1944.

coverage. In scientific circles, however, it created a "firestorm." The press release accompanying the statement claimed that it summarized "the most recent findings in this field [race] which the world's biologists, geneticists, psychologists, sociologists and anthropologists agree are established scientific facts," but the document did not reflect scientific consensus. In fact, it displayed the diminished authority of physical anthropologists whose identity was now at stake.[100]

Especially British physical anthropologists took issue with the statement. They supported the combat against discrimination but felt that the statement did not reflect anthropological consensus. The editors of *Man* invited anthropologists to comment on the document, which elicited fifteen critical responses. These critics dubbed it "the Ashley Montagu Statement" and supposedly Montagu resigned from the RAI because of the hostility of these critiques.[101] To appease the situation, UNESCO convened a second expert panel in 1951 with more physical anthropologists and geneticists. The second "Statement on Race" was a long and confusing document, riddled with contradictory claims.[102] Rather than a social myth, it claimed that race was a classificatory device. It also suggested that there was a possible genetic difference between the intellectual and emotional response of different races.[103] Despite the conflicts surrounding the UNESCO statements and its

100 UNESCO, *The Race Concept: Results of an Inquiry* (Paris: UNESCO 1952); Man editors, "220. UNESCO on Race," *Man* 50 (1950) 138–39; Michelle Brattain, "Race, Racism, and Antiracism: UNESCO and the Politics of Presenting Science to the Postwar Public," *The American Historical Review* 112:5 (2007) 1386–413; Perrin Selcer, "Beyond the Cephalic Index: Negotiating Politics to Produce UNESCO's Scientific Statements on Race," *Current Anthropology* 53:S5 (2011) S173–84;

101 William Fagg, "UNESCO on Racialism, *Times* [London, England] 15 Aug. 1950: 6. *The Times Digital Archive*. Web. 15 Oct. 2018"; "220. UNESCO on Race," *Man* 50 (1950) 138–39; "101. UNESCO and Race," *Man* 51 (1951) 64; Anthony Q Hazard, *Postwar Anti-Racism: The United States, UNESCO, and 'Race,' 1945-1968* (New York: Palgrave Macmillan 2012) 55.

102 Brattain, "Race, Racism, and Antiracism," 1401; Selcer, "Beyond the Cephalic Index," S174. This was partially the result of issues around the publication. Montagu had published the draft version in a paper while comments and revisions were still being made. As a result, UNESCO decided to print the first draft with all the comments in UNESCO, *The Race Concept: Results of an Inquiry* (Paris: UNESCO 1952) instead of a revised final statement. Some of Morant's feedback to this second statement was also printed in this volume.

103 "It is possible, though not proved, that some types of innate capacity for intellectual and emotional responses are commoner in one human group than in another, but it is certain that, within a single group, innate capacities vary as much as, if not more than, they do between different groups." It also said that: "Available scientific knowledge provides

limited influence on reshaping ideas about race,[104] historian Staffan Müller-Wille concludes that the statement "seems to have deeply influenced research agendas in physical and evolutionary anthropology in the following three decades."[105]

The Statements on Race were part of the UNESCO Division for the Study of Racial Questions. After the publication of the first statement and the attendant controversy, the Division's head, anthropologist Alfred Métraux, contacted several anthropologists and biologists to write on points raised in the second statement. The resulting series of pamphlets named *The Race Question in Modern Science* functioned as a "second stage of UNESCO's campaign against race prejudice and discrimination." The nine pamphlets ranged from topics such as racial mixture to racial prejudice and were written by well-known scientists like Leslie Dunn, Claude Levi-Strauss, and Harry Shapiro.[106] UNESCO circulated these pamphlets among high schools and colleges in the hope that the booklets would educate students about racial matters and would remove racial prejudice.[107]

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no basis for believing that the groups of mankind differ in their innate capacity for intellectual and emotional development." See UNESCO, *The Race Concept: Results of an Inquiry* (Paris: UNESCO 1952).

[104] Brattain "Race, Racism, and Antiracism," 1405; 1412.

[105] Staffan Müller-Wille, "Claude Lévi-Strauss on Race, History and Genetics," *BioSocieties* 5:3 (2010) 330-31.

[106] Quote from Juan Comas, "'Scientific' Racism Again?" *Current Anthropology* 2:4 (1961) 305. Many of the authors of the pamphlets had participated in the production of the Race Statements. The full list of pamphlets: O. Klineberg, *Race and Psychology* (Paris: UNESCO 1951); L. Dunn, *Race and Biology* (Paris: UNESCO 1951); J. Comas, *Racial Myths* (Paris: UNESCO 1951); A.M. Rose: *The Roots of Prejudice* (Paris: UNESCO 1951)*;* M. Leiris, *Race and Culture* (Paris: UNESCO 1952); G.M. Morant, *The Significance of Racial Differences* (Paris: UNESCO 1952); C. Levi-Strauss, *Race and History* (Paris: UNESCO 1952); K.L. Little, *Race and Society* (Paris: UNESCO 1952) H.L. Shapiro, *Race Mixture* (Paris: UNESCO 1953). Some pamphlets were republished in newer editions, such as Shapiro's, Leiris's, and Dunn's pamphlets, but also Morant's booklet. In 1960, two more pamphlets were added to the collection: H.L. Shapiro, *The Jewish people: a biological history* and Marie Jahoda, *Race Relations and Mental Health*. Comas mentioned that two other series were published by UNESCO, *The Race Question and Modern Thought*, and *Race and Society*. See Y. Congar, *The Catholic Church and the race question*; GP Malalasekera and KN Jayatilleke, *Buddhism and the race question*; L Roth, *Jewish thought as a factor in civilization*; WA Visser't Hooft, *The ecumenical movement and the racial problem*; Thales de Azevedo, *Les Elites de couleur dans une ville brésilienne*; Morroe Berger, *Problemes raciaux: l'egalite par la loi*; M Leiris, *Contacts de civilisations en Martinique et en Guadeloupe*; Ch. Wagley, *Races et classes dans le Brésil Rural.*

[107] UNESCO organized a small pilot study in 1952 in the United States to test to what extent the pamphlets achieved these results with students. For high school students they were considered too difficult, but tests on college students gave positive results for informing students and reducing prejudice. A wider market was targeted with the booklet *What is Race?* (Paris: UNESCO 1952). See Gerhart Saenger, "The effect on intergroup attitudes of the UNESCO pamphlets on race," *Social Problems* 21 (1955) 21-27 and Gerhart Saenger, "The effectiveness of the UNESCO pamphlet series on

In 1951, Métraux invited Morant to write a pamphlet for the series. Morant's booklet, *The Significance of Racial Differences*, advanced his ideas about race and mental characteristics. He had already begun writing on the topic in the 1940s, wanting to tackle the "unscientific" assertion that no racial mental differences existed because none had hitherto been demonstrated.[108] His UNESCO pamphlet introduced the "modern scientific method" of researching race – his biometrics – to the study of mental differences. He explained that this research "has gone far already towards clarifying the nature of the problem and indicating the way it may ultimately be solved." The method of precisely exposing physical racial differences through data and statistics could therefore guide the racial investigation of mental differences.

The first Statement on Race had argued that there were no inborn psychological differences between human groups. Another pamphlet in the series, Otto Klineberg's *Race and Psychology* (1951), called this a premature conclusion and pointed out that mentality should be studied along similar lines as physical characteristics. Morant's pamphlet responded to this call and addressed the claim of the second Statement that "it was possible, though not proved, that some types of innate capacity for intellectual and emotional responses are commoner in one human group than in another, but it is certain that, within a single group, innate capacities vary as much as, if not more than, they do between different groups."[109]

Morant's pamphlet began with explaining why we knew so little about mental racial differences. As with physical characters, knowledge of the mental characteristics of non-Western races reached us through travelers' accounts that exaggerated differences between populations and reduced in-group variation to imaginary racial types. Moreover, travelers often judged mentality by

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race," *International Social Science Bulletin* 6 (1954) 488-502. See also Jenny Bangham, "What is Race? UNESCO, Mass Communication and Human Genetics in the Early 1950s," *History of the Human Sciences* 28:5 (2015) 80–107.

108 AMP, Box 35, GM to AM, 8.10.1951; GM to AM 12.9.1941.

109 Otto Klineberg, *Race and Psychology* (Paris: UNESCO 1951); UNESCO, *The Race Concept: Results of an Inquiry* (Paris: UNESCO 1952) 75-76; Brattain, "Race, Racism, and Antiracism," 1397.

customs and behaviors and concluded that people from "different civilization levels" were "savages." The study of mental racial differences thus lacked precise definitions of mental qualities, the systematic collection of records, and statistical analysis.

The pamphlet reads like a manual on how to apply Morant's biometry to the study of mental differences. This research also centered on variation: mental qualities, like physical qualities, exhibited continuous variation within populations. Variation was of the same order between races, only slightly lower for isolated island communities. The study of distribution curves showed that "the ways in which men differ are of small account compared with the ways in which they are alike…Distinctions between the groups in this respect are much less than is commonly supposed." Indeed, distributions always overlapped. Thus, Morant stated that we should assume that mental characters showed large variation within and small variation between populations. This also meant that there *was* a racial difference between populations. He reasoned that "if there is diversity within the groups then the existence of some real racial differences can be presumed" because distributions only overlapped to some extent. Thus, "variation within groups is always associated with variation between groups." "It seems to be impossible to evade the conclusion that some racial differences in mental characters must be expected."

"All men have basic mental qualities of the same kind," he continued. "Many of these are variable in the sense that different individuals may exhibit them in different degrees." Because these characters differed in degree but not in kind, populations could be compared. The "confused world of behavior" and social customs made studying mental qualities more difficult than bodily characteristics, especially when comparing "primitive" and "civilized" societies. Nevertheless, "evidence of logical thought is not absent among the former and the latter are not free from irrational beliefs." "The intelligence in a negro is comparable in every way with intelligence in a European," Morant argued.

Thus, Morant's proof of the existence of mental racial differences rested on the assumption that mental and physical characters were of the same kind.[110] It further built on his unshaken trust in the "scientific" biometric method and its capacity to pull significant racial differences out of the ever-changing social environment that obscured these biological realities. Indeed, the pamphlet's title referred to the statistical significance of racial differences, as revealed by the methods developed by Quetelet, Galton, Pearson, and himself. Race, to Morant, was fundamentally a statistical problem. Unshaken was also his belief in the ontological reality of statistical results, which pointed to biological realities and possibilities, a relationship that anthropologists such as Boas and Montagu critiqued.[111] "The general inference is that there are racial differences in mentality," Morant concluded, "although clear demonstration of them – regarding particular characters and particular pairs of populations – is not available yet." What is more, "it is still possible, or even probable, that there is association between some physical characters of racial significance and some mental characters of racial significance, the former perhaps being physiological or biochemical rather than morphological."

Morant realized his claims were controversial but felt misunderstood. His defense of the possible or probable existence of racial mental differences often resulted in misinterpretation because "the discussion of the problem has always tended to run to extremes" between those who argue for profound mental differences and those "who have vehemently denied the existence of any inborn inequalities between groups of people." "Any admission of racial differences is suspected by

[110] He also wrote: "Even if characters of mind and body were entirely independent, it would still be legitimate to infer that the conditions responsible for variation within and between groups for one class must be expected to have effects of like kind for the other class."

[111] In an unpublished lecture delivered at Columbia University's 50th anniversary celebration of *The Origin of Species* in 1909, Boas told the audience: "We can, however, see even now that the statistical methods provide us with a most powerful means of proving or disproving biological theories. We must, however, not expect too much of these methods. The statistical treatment of biological phenomena must not be expected to furnish biological explanations." Franz Boas Archive, Franz Boas Anthropometric Data and Early Field Notebooks, "The Relation of Darwin to Anthropology," unpublished lecture from 1909, page 16.

the 'levellers' to have a sinister implication, and the proponent of it is likely to be suspected of claiming superiority for the group to which he belongs." In the modern investigation of race, he continued, "it almost looked as though the ultimate solution of the problem might be a denial of the existence of any racial differences. But this conclusion is manifestly untrue in the case of physical characters, and in the writer's opinion is very unlikely to be proved true in the case of mental qualities." Morant sought an understanding of racial differences that was biologically "true" but not socially harmful. Like his argument that an "accurate" study of physical racial difference could foster peace, his defense of racial mental differences also stressed progress: "One group may be outstanding for one character and one for another, and all groups are unexceptional in most respects. Group diversity of such a kind tends to equalize all peoples when a final summing up is made for all characters. Variety among populations would be a boon to humanity if all had good opportunities to develop their potentialities." For Morant, equality lay within the realm of biological difference.

The UNESCO pamphlet was Morant's last publication on race and anthropology. After the war, he was asked to join the eugenics department at University College,[112] not the new department of anthropology which was closer to his expertise. The University's Provost made clear that Morant could not resume his anthropological research but also expected him to lecture on physical anthropology. Not wanting to "languish as a junior assistant in the Eugenics Laboratory," Morant drastically quit physical anthropology "in order to make his point of view plain to the authorities involved." "His career as an anthropologist seems to have come to an abrupt end at the age of 46," he wrote to Montagu. The situation affected him deeply. He described himself as a "second-rate sort of fellow who stood little chance of surviving in the academic world. His plodding researches were

[112] The department changed its name from Department of Eugenics to Department of Human Genetics in the mid-1960s.

voluminous enough, certainly, but too narrow in scope to lead him anywhere. His recent book, *The Races of Central Europe*, is thought little of in this country. I feel sorry for the fellow sometimes. His prospects now are certainly not bright."[113]

Morant joined the Institute of Aviation Medicine of the British Royal Air Force in 1946. As Principal Scientific Officer, he put his anthropometric skills to practical use for research in the realm of physiology and the design and sizing of cockpits, clothing, and seats. He enjoyed the work but continued to prefer research with no practical application. "I still believe that ideas are more important than things," he wrote. He also maintained his position on race. He wrote in 1961 to Montagu: "I still cannot believe that all races are equal in their distribution of innate mental characters."[114]

Conclusion

The biometric study of race had developed from a narrow and complex research field that attempted to transform racial research from within to a politically urgent weapon in the fight against pernicious racism. Biometry was put to political ends. Morant, who advocated the use of science for science sake, had entered the realm of politics because of his belief in "biometric truths" and his desire for peace. His fight was both political and methodological: "true" knowledge of race was his

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113 AMP, Box 35, GM to AM 29.7.1945. Morant wrote in the third person about himself here, in a response to a letter that Montagu had received from someone who was very excited about *The Races of Central Europe* and wanted more information about Morant.

114 AMP, Box 35, GM to AM 8.12.1946; GM to AM 1.8.1961; GMP, "Curriculum vitae." Morant doesn't mention any contemporary research on the mental characteristics of different races, here or in his UNESCO publication. See for the pre-war history of intelligence testing and race Stephen J. Gould, *The Mismeasure of Man* (New York: W.W. Norton & Company 1996); chapter 5 in Helen Tilley, *Africa as a Living Laboratory: Empire, Development, and the Problem of Scientific Knowledge, 1870-1950* (Chicago: University of Chicago Press 2011); the special issue A. Mülberger (ed.), "Mental Testing After 1905: Uses in Different Local Contexts," *History of Psychology* 17:3 (2014); Sebastiaan Broere, "Picturing Ethnopsychology: A Colonial Psychiatrist's Struggles to Examine Javanese Minds, 1910–1925.," *History of Psychology* 22:3 (2019) 266–86. Researchers would continue to study the racial differences in mental qualities in the period after World War II. See especially Rebecca Lemov's *Database of Dreams: The Lost Quest to Catalog Humanity* (New Haven: Yale University Press 2015) which traces the mid-twentieth century history of anthropological-psychological research into the cultural projections of non-western people, such as dreams.

weapon against false and dangerous ideas. While his anthropological career ended, the politics stuck: in the 1950s, Morant became actively involved in a local branch of the British United Nations Association and wrote a book about the history of the league of nations.[115]

Geoffrey Morant's life and work is this dissertation's last example of the argument that biometric innovations of anthropology challenged racial dogmas but not race itself. Morant took variation more seriously than previous biometricians and claimed that within-group variation was bigger than between-group variation. Because distribution curves of measurements overlapped, racial classification became an arbitrary yet convenient taxonomic exercise. Morant used these arguments in his fight against racial dogmas and hoped to intervene in the problematic political situation that racist creed was creating in Europe. Perhaps his political writings have gone under the historiographical radar because he left anthropology soon after the war and no archives hold his records, but contemporaries recognized Morant as a crucial player in the opposition of scientific racism. In 1961, Jean Comas ranked Morant among famous scientists such as Franz Boas, Ashley Montagu, Ruth Benedict, Louis Leakey, and Harry Shapiro "who published arguments to neutralize the pernicious effects of racial discrimination fostered by pseudo-scientific anthropology."[116] These new approaches to and interpretations of race, however, never made Morant question the validity of race itself. Indeed, he claimed that his biometric "truths" about race could foster peace. Biometry's "precise and accurate" methods could undermine false dogmas that resulted from ill-disposition and swayed the ignorant public. In this aspiration, he embraced a global biological likeness in kind and local racial difference in degree. Thus, Morant's fight against racism was not a fight against race. Antiracism did not necessitate the annihilation of race.

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[115] AMP, Box 35, GM to AM, 2.2.55; 16.1.1957; 1.8.1961. His book *A Short Account of the League of Nations* was rejected by Allen and Unwin because "few people are interested today in recent history," Morant wrote to Montagu.

[116] Juan Comas, "'Scientific' Racism Again?" *Current Anthropology* 2:4 (Oct 1961) 303.

Finally, this case study illustrates a complex relationship between evidence and race. Brattain explains that the "null hypothesis" in racial research had always been difference. In the 1950s, researchers still found it more difficult to prove a complete lack of racial difference even when there was little evidence in favor of such a difference. Because of the "*historic* formulation of the question itself," the argument that differences could be found in the future remained stronger than the will to embrace the lack of evidence, even though that accurately described the current state of affairs. Morant's UNESCO pamphlet on the "possible and probable" racial differences in mentality exemplifies this attitude. Even in the statistical study of race, the null hypothesis remained difference, not likeness. This historically powerful assumption remained a stronghold and hampered a more radical reconceptualization of race after the war.[117]

[117] Brattain, "Race, Racism, and Antiracism," 1403-05.

Chapter 5

The Anthropological Politics of Standardization

Introduction: National, International, Universal

In the early 20th century, Dutch physical anthropologists Gijsbert van der Sande and Hendrik Lorentz corresponded about the anthropological methods they had used during scientific expeditions in the Dutch Indonesian archipelago. In 1909, Van der Sande complained to Lorentz that a third colleague, Anton Nieuwenhuis, had measured head lengths on populations in the Dutch colonies from the chin to the crown instead of the more common measurement from the chin to the top of the head. The resulting measurements differed by a few centimeters, as he illustrated with a drawing, which made direct comparison impossible (fig. 5.1). Differing measurement practices made the data "of zero and no value, which is a damn shame." Indeed, anthropological measurements such as head length were not standardized, neither nationally nor internationally.

Considering that racial research strongly relied on comparable data, researchers around the globe considered the lack of standardization a serious problem.[1]

Figure 5.1. Variability in head measurements. Source: Letter from H.A. Lorentz to G.A.J. van der Sande, 11.06.1909. Collectie 384 Mr. H.A. Lorentz, toegang 2.21.183.51, inv. Nr. 8. Image courtesy of the National Archives of the Netherlands.

Standardization is often taken for granted: when successfully implemented, standards tend to become invisible. Stories of standardization can therefore be viewed by some as dull or boring. But histories of the creation of standards often show an entirely different picture. "Standards' objectivity, universality, and optimality are hard won victories that can be heavily contested by third parties lobbing accusations of bias and politicization," sociologists Stefan Timmermans and Steven Epstein conclude.[2] The history of standardizing racial measurements is case in point. The need for standardization in any discipline may seem obvious and one would assume that racial researchers should be able to agree on a set of standards. This was not the case: various political, personal, and scientific concerns and disputes surfaced when researchers attempted to negotiate racial measurements and data across nations and disciplines. A turn to quantification demanded the

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[1] Dutch National Archives, The Hague, Collectie 384 Mr. H.A. Lorentz, nummer toegang 2.21.183.51, inv. nr. 8, H.A. Lorentz to G.A.J. van der Sande, 11.06.1909.

[2] Stefan Timmermans and Steven Epstein, "A World of Standards but Not a Standard World: Toward a Sociology of Standards and Standardization," *Annual review of Sociology* 36 (2010) 69–89, quote on page 74.

standardization of data practices in order to make them less personal and more objective, but standardization practices brought out various interpersonal, parochial, and subjective concerns. The emotional stakes, this chapter shows, were high.

In the history of standardizing man's measurements, the interplay between subjective and objective desires played out in and between three spaces: the universal, international, and national. Researchers aspired to employ the scientific method and its *universalist* aspirations in their work on race, stimulated by the desire to professionalize and institutionalize the fledgling discipline physical anthropology. The scientific method proscribed that knowledge claims should not be based on subjective observations determined by personal or local beliefs, practices, and agendas. Instead, scientists should deduce and reduce empirical facts in such a way that others could replicate their experiments and test their hypotheses. Within the realm of researching man's variation, replicability and testing meant comparability of measurements and data, as Chapter 1 explained.

Sharing and reusing data was particularly valuable considering the *international* scope of racial research and its aim to trace human variation on a global scale. Researching race globally required the collection of large amounts of data of various races around the world and thus international cooperation and comparison. Research methods therefore needed to be standardized to achieve scientific universalism and successful comparison of data across countries. Standardization of man's racial measurements was further stimulated by a steady increase of international scientific cooperation in the 19th century, which lead to the founding of international journals, congresses and associations, and standardized methods and instruments in several sciences.[3] These universalist and

[3] Elisabeth Crawford, Terry Shinn, and Sverker Sörlin, "The Nationalization and Denationalization of the Sciences: An Introductory Essay," in: idem, *Denationalizing Science. The Contexts of International Scientific Practice* (Dordrecht: Springer 1993) 1-42; Elisabeth Crawford, *Nationalism and Internationalism in Science, 1880-1939: Four Studies of the Nobel Population* (Cambridge, UK: Cambridge University Press, 2002) 1-42; Ralph Jessen and Jakob Vogel, "Die Naturwissenschaften und die Nation: Perspektiven einer Wechselbeziehung in der europäischen Geschichte, in: idem, *Wissenschaft und Nation in der europäischen Geschichte* (Frankfurt: Campus Verlag 2002) 7-37. Examples of internationalism in science: Robert E. Kohler, *Lords of the Fly: Drosophila Genetics and the Experimental Life* (Chicago: University of Chicago Press, 1994); Ken Alder, *The Measure of All Things: The Seven-Year Odyssey and Hidden Error That Transformed the World* (New York: The Free Press 2002);

internationalist desires underpinned the standardizing of racial measurements and brought together anthropologists, anatomists, eugenicists, and biometricians in the early 20th century.

Anthropological measurements, however, were not comparable on an international scale nor did they meet the requirements of science's universalist ideal. Around 1900, there was hardly any national or international agreement on the methods of measuring bodies, skulls, and racial traits, and "schools of anthropology" in France, Germany, England, and the United States had developed their own approaches to racial research. What is more, racial research was often inextricably tied to the *nation*. Research projects often took place within the walls of local institutions and laboratories, or national bodies funded scientific expeditions to the nation's colonies. Furthermore, anthropology became an important nation-building device in the late 19th century: political heads deployed anthropological theories and classifications to justify their colonial activities and argue for the superiority of their nation's race.[4]

Those involved in the standardization of methods disagreed which "school of measurement" should be used for international standardization. Contrasting positions about the scientific value of embodied "trained eye" expertise and disembodied statistical methods further divided them. These methodological commitments became inextricably intertwined with the political tensions and warfare that characterized the early 20th century and soured the relationships between researchers internationally. Epistemological divisions, chauvinism, and prestige led researchers to *doubt* racial data.

Stefan Kühl, *For the Betterment of the Race: The Rise and Fall of the International Movement for Eugenics and Racial Hygiene* (Basingstoke: Palgrave Macmillan 2013).

[4] See Chris Manias, "The Race Prussienne Controversy: Scientific Internationalism and the Nation," *Isis* 100:4 (2009) 733–57 for a discussion of the tension between internationalism and nationalism in anthropology.

Unifying Schools of Anthropology

In the second half of the 19th century, several "schools" of anthropology emerged in Europe that attempted to harmonize racial research practices within countries. Prominent among them was the French *Institut d'Anthropologie*, formed by anatomist Paul Broca in the 1870s, which became a model for anthropological organizations in other countries. Broca and his workers developed a rigorous measurement system for the skull and the brain, "virtually reinvented the field of anthropometry, by measuring more carefully and more completely than any before them," historian Alice Conklin concludes. Broca introduced multiple craniometric methods, named several anatomical landmarks on the skull, and developed more than thirty new instruments. Broca's student Paul Topinard codified the institute's approach to race and anthropology in the first textbook on the subject, *Éléments D'anthropologie Générale* (Paris: A. Delahaye et É. Lecrosnier 1885), intended as a guide for students and a reference manual for practitioners. Broca's institute, methods, and instruments grew in fame beyond France in the second half of the 19th century. The *Institut* became the international leader for craniometric research and attracted students from various other countries. This ensured the international spread of the French methods.[5]

Other European countries also organized their anthropological methodology in the second half of the 19th century. The Royal Anthropological Institute of Great Britain and Ireland and the British Association for the Advancement of Science produced a field research handbook *Notes and Queries*, intended to assist travelers in producing accurate anthropological observations.[6] The

[5] Broca's *Institut* consisted of *the Société d'Anthropologie*, the *École d'Anthropologie*, a laboratory, and a museum. Frank Spencer, *History of Physical Anthropology* (New York: Garland Publishing 1997) 87, 221-222; Alice Conklin, *In the Museum of Man: Race, Anthropology, and Empire in France, 1850-1950* (Ithaca, NY: Cornell University Press 2013) especially chapter 1. Quotation on page 26. For more on Broca, see Francis Schiller, *Paul Broca: Founder of French Anthropology, Explorer of the Brain* (New York: Oxford University Press 1992). British anthropologist James Hunt founded the Anthropological Society of London in 1863 after Broca's institution. The French organization also served as the source of inspiration for Aleš Hrdlička's *American Association of Physical Anthropology*.

[6] L.H. Buxton and G.M. Morant, "The Essential Craniological Technique. Part I. Definitions of Points and Planes," *Journal of the Royal Anthropological Institute of Great Britain and Ireland* 63 (1933) 19-47, page 22; James Urry, "Notes and

handbook reflected the widespread British practice of following Broca's methods while introducing alterations and additions. *Notes and Queries*'s sections on hair and eye color, for instance, included copies of Broca's color tables. German anthropologists, however, decided to deviate from the French school. In 1873, two years after the unification of Germany, they began a decade-long quest to create a uniform German craniometry system. After several conferences, they reached an agreement in 1882 that became known as the "Frankfurt Verständigung" or the Frankfurt Agreement. Central to the system's divergence from the French method was a new horizontal plane for the skull, the "Frankfurt Horizontal." The German line ran from the top of the ear hole to the bottom of the eye socket, the auriculo-orbital plane. From this fixed line, various other measurements were to be taken, such as length, height, and breadth of the skull. This line was different from Broca's widely used horizontal, which ran from the alveolar bone past the occipital condyles (fig. 5.2).

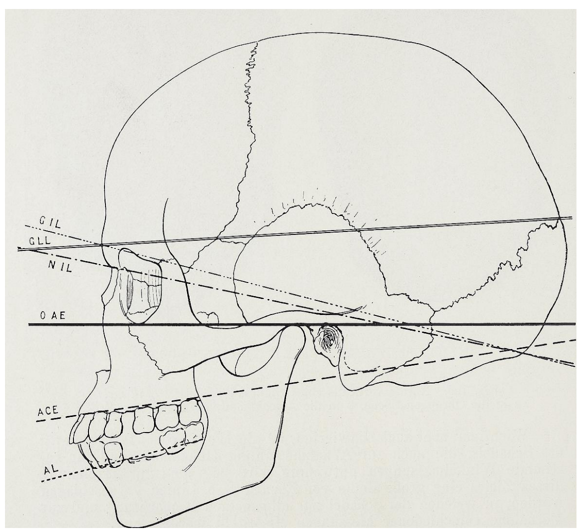

Figure 5.2. ACE is the Broca horizontal, OAE is the Frankfurt horizontal. Source: Rudolf Martin, *Lehrbuch der Anthropologie in systematischer Darstellung* (Jena: Gustav Fisher Verlag 1914) 480. Source is in the public domain.

Queries on Anthropology" and the Development of Field Methods in British Anthropology, 1870-1920," *Proceedings of the Royal Anthropological Institute of Great Britain and Ireland* 1972 (1972) 45–57. See also Henrika Kuklick, "The British Tradition," in: idem, *New History of Anthropology* (Malden, MA: Blackwell 2008). *Notes and Queries* included sections on physical anthropology in the first three editions (1874, 1892, 1899). From 1909, the physical anthropology section was significantly slimmed down in newer editions and readers were referred to a different measuring manual, *Reports of the Committee on Anthropometric Investigation*. The latter was drawn up for an anthropometric research project on the British Isles.

As a result, the same measurements differed 1 to 2 millimeters. Furthermore, only 8 of the 31 German measurements of the skull were directly comparable with the French system. The German system thus created a diversity in racial research methods that obstructed the comparability of data.[7] In his monograph on German anthropology, historian Andrew Zimmerman details the struggles of reaching agreement between German anthropologists. Zimmerman frames the Frankfurt Agreement as an important moment in forging a German scholarly community and a collective identity.[8] From a transnational perspective, however, German deviation from the French method seems to be part of a tense rivalry between French and German anthropologists following the Franco-Prussian war of 1870-71.[9] Moreover, researchers internationally bemoaned how French and German data was now incomparable. British anatomist John George Garson, who wrote the physical anthropology sections of *Notes and Queries* in 1894 and 1899, published a translation of the Frankfurt Agreement and included his critique. His gripe stressed the universal and international aspirations of the science of race:

> I must remark that it has hitherto been customary to consider those devoting themselves to any branch of science as belonging to one brotherhood, totally irrespective of the country to which they

[7] H. von Jhering, "Zur Reform der Craniometrie," *Zeitschrift für Ethnologie* 5 (1873) 121–69; J. Kollmann, J. Ranke, R. Virchow, "Verständigung über ein gemeinsames craniometrisches Verfahren," *Archiv für Anthropologie* 15 (1884) 1-8. L. H. Buxton and G.M. Morant, "The Essential Craniological Technique," 23. German anthropologist Otto Ammon attempted to find a formula that made data deriving from the French and German method comparable. See: Otto Ammon, "Über die Wechselbeziehung des opfindex nach deutscher und französischer Messung," *Centralblatt für Anthropologie, Ethnologie und Urgeschichte* 2:1 (1897) 1–6.

[8] Andrew Zimmerman, *Anthropology and Antihumanism in Imperial Germany* (Chicago: University of Chicago Press 2001) 86-94.

[9] This rivalry also manifested itself in other fields, like mathematics and experimental psychology. Historian Chris Manias argues that French and German anthropologists displayed tense international rivalry in the 1870s, following the Franco-Prussian war. In 1872, French anthropologists Arman de Quatrefages published *La race prussienne*, which became "the most politically contentious work of anthropology ever produced," according to Manias. Quatrefages argued that Prussia, now part of German unification, was racially distinct from Germany: its people were of inferior Finno-Slavic origins instead of Aryan-Germanic origins. German anthropologists were outraged by these conclusions, which they read as a rebuttal of German unification. "The bitter feelings that arose during the war had soured relations between French and German anthropologists on a number of levels," Manias concludes. A year after Quatrefages' publication, German anthropologists proposed to standardize German craniometry. See: Chris Manias, "The Race Prussienne Controversy: Scientific Internationalism and the Nation," *Isis* 100:4 (2009) 733–57.

> happen to belong, and beyond all national prejudices, with one common aim, namely, that of advancing the department to which they are giving their attention. That anthropologists in Germany should introduce the use of such terms as 'German' and 'our' to craniometrical questions is much to be regretted, as tending to destroy that feeling of unity which has always existed among scientific men…[The Frankfurt Agreement] seems to have been drawn up with little consideration of what has been done by other anthropologists over the world, save that of a few in Germany. Important measurements, which yield marked results in comparing skulls of different races, are entirely ignored, and methods of measuring accepted by anthropologists generally are altered without any reason being given for the change.

Garson argued that Broca's methods should be the basis of all craniometric research, "this being the system which has been adopted by anthropologists generally over the whole world." Broca's horizontal, for instance, aligned with the natural position of the skull and was easily found with French instruments. Instead, the Germans had separated themselves from the rest of the world.[10] A few decades later, American anthropologist Aleš Hrdlička agreed that due to the "schism" and "German individualism" "a great deal of work was lost."[11]

Most French, Belgian, Italian, Swiss, Russian, and Spanish anthropologists continued to use Broca's method, while anthropologists in German-speaking countries deployed the Frankfurt method. The British continued to pick and choose measurements according to their preference. Karl Pearson and the biometricians associated with his laboratory, for instance, chose the German system as the basis for their own skull measurements, statistical analyses, and racial comparisons, arguing that German researchers had produced the most and best craniometric data.[12]

Despite the popularity of Broca's system and German unification efforts, researchers continued to individualize their research methods. In the late 19th century, many German anthropologists abandoned the Frankfurt Agreement and the French school diverged in different

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[10] J.G. Garson, *The Frankfurt Craniometric Agreement, With Critical Remarks Thereon* (London: Harrison and Sons 1884) 10.

[11] A. Hrdlička, "Anthropometry A," *American Journal of Physical Anthropology* 2:1 (1919) 45.

[12] Cicely D. Fawcett and Alice Lee, "A Second Study of the Variation and Correlation of the Human Skull, With Special Reference to the Naqada Crania," *Biometrika* 1:4 (1902) 408–67; L.H. Buxton and G.M. Morant, "The Essential Craniological Technique," 24.

directions.[13] As researchers produced more racial data, however, their desire to standardize research methods internationally grew. Central to these ambitions was the definition of racially important characters, the selection of landmarks involved in measurement, and the technique of taking measurements with instruments. Members of the *Congrès International d'Archéologie et d'Anthropologie Préhistorique* (CIAAP), founded in 1867 with the aim to collapse international borders between its members, decided to take action. At the 1892 CIAAP meeting, two committees were formed and tasked to unify racial measurements: one for measurements on the living, one for craniometric methods. The committees were scheduled to report their standardization suggestions at the next meeting in 1900, but it proved too difficult to keep the momentum going after the congress meeting.[14] The CIAAP therefore decided in 1906 that a new craniometric committee should reach agreement on the unification of measurements *during* the sessions of the congress. The committee included French, German, Italian, and Swiss anthropologists, and met four times during the meeting. The choice between the French and German horizontal remained too controversial to be discussed. On the final day of the congress, members viewed the committee's final report in a dedicated room where the committee's secretary Georges Papillault assisted with questions.[15] During the next conference in 1912, a new committee decided on the measurements taken on living subjects. It represented Great Britain, Hungary, Spain, the United States, Poland, and Russia, besides France,

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[13] G. Papillault, "The International Agreement for the Unification of Craniometric and Cephalometric Measurements. Report of the Commission Appointed by the XIII International Congress of Prehistoric Anthropology and Archaeology at Monaco (1906)," *American Journal of Physical Anthropology* 2:1 (1919) 49.

[14] N. Zograf, "Note sur les méthodes de l'anthropométrie sur le vivant pratiquées en Russie et sur la nécessité d'établir une entente internationale pour arrêter les méthodes communes des recherches anthropometriques," in: Congres International d'Archéologie et d'Anthropologie Préhistoriques, *11-me Session a Moscou, du 1/13-8/20 aout 1892, Tome II* (Moscow 1893) 13-24; M. Kollmann, "Discours de M. Kollmann sur la craniométrie," in: Congres International d'Archéologie et d'Anthropologie Préhistoriques, *11-me Session a Moscou, du 1/13-8/20 aout 1892, Tome II* (Moscow 1893) 7-10, in "Proces-verbaux des séances" section; G Papillault, "The International Agreement for the Unification of Craniometric and Cephalometric Measurements," 48.

[15] G. Papillault, "Entente Internationale pour l'Unification des Mesures craniométriques et céphalométriques," in: Congres International d'Anthropologie et d'Archéologie Préhistoriques, *Compte Rendu de la Treizième Session Monaco 1906 Tome II* (Monaco 1908) 377-394; Frank Spencer, *History of Physical Anthropology* (New York: Garland Publishing 1997) 407.

Germany, Italy, and Switzerland. After four meetings, the committee presented their final report. Regarding the horizontal plane, it left decisions up to researchers themselves: "The Commission wishes to state that it is desirable that in the graphic representation of cranial forms, either the plane of Broca or of the Frankfort Agreement should be employed by anthropologists."[16]

With the 1906 and 1912 agreements, anthropologists had begun to organize their methods beyond national borders. Members of the standardization committees translated the agreements and published it in leading French, American, and German journals in order to stimulate the international adoption of the methods.[17] Anthropologists also began discussing the possibility of establishing a congress dedicated to the anthropological sciences. This congress would absorb the CIAAP and other congresses that centered on ethnography and linguistics. This new congress would not only bring together the various aspects of the study of man, it would also further stimulate the international unification of scientific terminology, methods, instruments, and procedures. A coordinating committee planned to organize the first International Anthropological Congress in 1916.[18]

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[16] "Entente Internationale pour l'Unification des Mesures Anthropométriques sur le vivant," in: Congres International d'Anthropologie et d'Archéologie Préhistoriques, *Compte Rendu de la XIVe Session Geneve 1912 Tome II* (Geneva 1914) 484-490. Translation from "The International Agreement for the Unification of Anthropometric Measurements to be Made on the Living Subject," *American Journal of Physical Anthropology* 2:1 (1919) 67.

[17] See for example: George MacCurdy, "International Congress of Prehistory Anthropology and Archeology, Geneva," *American Anthropologist* 14:4 (1912) 621-631; also in *Science* 36:931 (1912) 603-608; G. Papillault, "The International Agreement for the Unification of Craniometric and Cephalometric Measurements. Report of the Commission Appointed By the Xiii International Congress of Prehistoric Anthropology and Archaeology At Monaco (1906)," *American Journal of Physical Anthropology* 2:1 (1919) 46-60; A. Hrdlička, *Anthropometry* (Philadelphia: Wistar Institute of Anatomy and Biology 1920); Felix von Luschan, "Die Craniometrische Konferenz in Monaco," *Correspondenz-Blatt der Deutschen Gesellschaft für Anthropologie, Ethnologie und Urgeschichte* 37:7 (1906) 53-68.

[18] Franz Boas Archive, Boas Professional Papers, Box 19, "Committee for the Organization of an International Congress of the Anthropological Sciences," 1912; "Report of an International Conference, which met on June 4th, 1912, at the Invitation of the Royal Anthropological Institute to discuss the following Questions relative to a proposed International Anthropological Congress," *International Congress of Americanists. Proceedings of the XVIII. Session, London, 1912. Prepared by the Editorial Committee. Part I* (Nendeln/Liechtenstein: Kraus Reprint 1968 [1912]) 86-88; George MacCurdy, "International Congress of Anthropological Sciences," *American Anthropologist* 14:2 (1912) 408; George MacCurdy, "International Congress of Prehistory Anthropology and Archeology, Geneva," *American Anthropologist* 14:4 (1912) 621-631.

Standardization was further helped along by Swiss-German physical anthropologist Rudolf Martin. In 1914, he published a 1181-page textbook on anthropological methods, *Lehrbuch der Anthropologie in systematischer Darstellung* (Jena: Gustav Fisher Verlag 1914), "the first comprehensive representation of Physical Anthropology." The textbook, intended as a guide for laboratory research and instruction, was the result of a decade's work. Martin stressed the necessity of standardization in the introduction: "The fate of our science depends on the development of technique. As long as every new observer devises his own methods, or takes up methods without experience and critique, we will never get reliable and comparable results." For every measurement, the *Lehrbuch* listed several options: "when anthropological technique is more unified, to which this book hopefully contributes, these many variants will become superfluous and can be left out later." Martin listed the Frankfurt Horizontal first and Broca's second, arguing that the former was the only horizontal that could easily be determined in living people and thus enabled the comparison of anthropometric and craniometric data. Martin's *Lehrbuch* became a leading textbook in anthropology and went through four editions between 1914 and 1988.[19]

The First World War, however, made havoc of anthropology's organizational and unifying plans. When the war broke out, Martin fled his residence in Versailles and abandoned his personal library. He was only able to save his finished manuscript of the *Lehrbuch*.[20] The CIAAP was scheduled to meet in 1915 in Madrid but did not come together until 1930. The war interrupted plans for the international anthropological congress and anthropologists put the standardization agreements of 1906 and 1912 aside. The subject would not be revisited until the late 1920s:

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[19] Martin, *Lehrbuch der Anthropologie in systematischer Darstellung* (Jena: Gustav Fisher Verlag 1914) v-vi. Other editions: Rudolf Martin, *Lehrbuch der Anthropologie in systematischer Darstellung* (Jena: Gustav Fisher Verlag 1928); Rudolf Martin and Karl Saller, *Lehrbuch der Anthropologie in systematischer Darstellung* (Stuttgart: Gustav Fisher Verlag 1956); Rudolf Martin and R. Knussman (ed.), *Anthropologie. Handbuch der vergleichenden Biologie des Menschen* (Stuttgart: Gustav Fisher Verlag 1988).

[20] Andrew D. Evans, *Anthropology at War: World War I and the Science of Race in Germany* (Chicago: University of Chicago Press 2010) 103; Bruno Oetteking, "Rudolf Martin", *American Anthropologist* 28:2 (1926) 416.

problems with reviving the international scientific community after the war put unification of method on the backburner.

After the war, several initiatives were undertaken to revive the international scientific community. In 1918, French anthropologists of the *Institut d'Anthropologie* established a new international platform for anthropology, the *Institut International d'Anthropologie* (IIA). Unlike the CIAAP's focus on prehistoric studies, the IIA intended to bring together a wide variety of anthropological studies, from physical anthropology to linguistics. While international in ambition, the *Institut* was largely French in organization and character. Because the IIA was registered under French law and subsidized by the French government, its main office and Board of Directors had to be in France. While every participating nation could place 4 members on the Board, France was represented by 25 Board members and meetings would take place in France. Not the IIA but Broca's *École* elected the IIA's president and treasurer, who had to reside in France. Conference announcements and invitations were only published in the journal *Revue Anthropologique* and not mailed out to subscribers. Thus, the IIA was, in the words of British anthropologist John Myres, "practically French."[21] Yet its publications claimed an international scope. The booklet that the IIA distributed internationally in 1919 declared that "the Institute must be the fruit of the collaboration of all." The *Institut* could only advance the anthropological sciences if researchers internationally agreed on its organization. The booklet also stated that the IIA would encourage new initiatives in the realm of standardization of methods.[22]

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[21] "Statuts," in: "Réunion préparatoire pour la fondation de l'Institut International d'Anthropologie. Tenue a l'École d'Anthropologie de Paris, du 9 au 14 septembre 1920" printed in: *Revue Anthropologique* 30 (Paris 1920) 251-255; JL Myres, "International Congress," *Nature* 125:3148 (1930) 297–99; RAI Archive, A58/2/13/1.1 (including quotation).

[22] Aleš Hrdlička Archive, Box 34, Folder IIA Correspondence, "Rapports Préalables" (Paris 1919).

Anthropologists from various countries subscribed to the IIA,[23] but the British Royal Anthropological Institute declined membership because of its French character. Along similar lines, American anthropologist Aleš Hrdlička did not renew his membership in 1921 because the IIA had "not reached that harmony and truly international character that might be desired." Indeed, German, Austrian, and Hungarian anthropologists were excluded from the first IIA meeting in Liege in 1921 and the IIA continued to only invite researchers from neutral or allied nations.[24]

Tensions rose towards the end of the 1920s. For the IIA meeting of 1927 in Amsterdam, the Dutch organizers decided to invite previously barred colleagues from Germany.[25] Moreover, attendants complained about the lack of international representation in the IIA's organization and pointed out that its existence made it difficult to reestablish the CIAAP. Representatives of the British Royal Anthropological Institute (RAI) began pushing for an international body governed by representatives of various nations and international congresses organized by a host country without the dominance of one country. The IIA promised to organize a joint CIAAP-IIA meeting and change its organization to become more "international." At this combined meeting in 1930 in Portugal, however, it became clear that little had changed: only IIA subscribers, for instance, were allowed to attend the general assembly meeting. CIAAP members who were not members of the French institute were thus deprived of a voice in the affairs and future of the organization. "The *Institut* had, in fact, swallowed the Congress," RAI president John Myres complained. These affairs caused such consternation that the Portugal meeting was adjourned and a *petit comité* formed to discuss the future of the IIA, the CIAAP, and the international anthropological community at large.

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23 See L. Capitan, "Rapport Général," in: "Réunion préparatoire pour la fondation de l'Institut International d'Anthropologie, Compte Rendu," *Revue Anthropologique* 30 (1920) 193–255, especially 209-217.

24 J.L. Myres, "Presidential Address. Anthropology: National and International," *Journal of the Royal Anthropological Institute of Great Britain and Ireland* 60 (1930) 17–45; AHA Box 34, IIA Correspondence, Aleš Hrdlička to Papillault, 21.6.1921.

25 M. Diaz-Andreu, *Archaeological Encounters: Building Networks of Spanish and British Archaeologists in the 20th Century* (Newcastle upon Tyne: Cambridge Scholars Publishing 2012) 249.

After several rounds of negotiations, those involved decided to split the archaeological congress from the anthropological congress and establish an entirely new international anthropology organization with the old CIAAP statutes in 1932: the *International Congress of Anthropological and Ethnological Sciences*. This organization proved successful and remains active today.[26]

Biometric Interventions

While anthropologists struggled to establish an international organization, the biometricians of Karl Pearson's laboratory began concerning themselves with the lack of standardization of racial measurements, its extent, and resultant problems. It is not surprising that these topics resurfaced in biometric circles. As Chapter 2 discussed, the Biometric Laboratory studied race through large samples and statistical constants such as means, standard deviations, and correlations. This type of research required large amounts of data that biometricians obtained by mining publications for measurements. The incomparability of racial data thus posed a huge problem for biometric research.

Indian biometrician Prakash Mahalanobis, while working at Pearson's Lab in the late 1920s, made a systematic comparison of the French, German, British, American, and the 1906-1912 methods of measuring heads in the works of researchers from Sweden to Japan. He concluded that the data was barely comparable. He exposed the extent of the problem in an article in *Biometrika*:

> One would naturally expect that the works of such trained scientists would be free from ambiguities, and would be comparable with one another, especially when almost all the investigations…were conducted several years later than the date of the International Agreement of Monaco on

[26] RAI Archive, A58/2/13/1.1; A62/151/39; J.L. Myres, "20. International Congress and Institut International: An Interim Report of Recent Negotiations," *Man* 31 (1931) 17–20; J.L. Myres, "63. International Congresses, Anthropological or Prehistoric? A Further Report of Negotiations," *Man* (1931) 61–64; J.L. Myres, "94. International Congresses: A Third Report of Negotiations," *Man* 31 (1931) 87–90; J.L. Myres, "137. The Old Congress and the New. A Fourth Report of Negotiations.," *Man* (1931) 131–34. MAN editors, "84. International Congress of Anthropological and Ethnological Sciences," *MAN* 33: May (1933) 77–79. Nature Editors, "An International Congress of Prehistory," *Nature* 3227:128 (1931) 389-391.

> Anthropometric Measurements in 1906. To my great surprise and consternation I found that this was far from the actual fact. Fresh discrepancies in definitions or in technique continually cropped up.[27]

Researchers used different anatomical landmarks for the same measurements or simply failed to define the landmarks used. They took caliper measurements either with or without pressure on the skin, thus producing measurements that differed by a few millimeters. They also disagreed on whether the root of the nose was at the skull's *nasion* or the depression of the flesh. For the most common 11 head measurements, Mahalanobis concluded that "the appalling fact comes out that only 30% to 45% of the theoretically possible comparisons can actually be made in practice."[28] Restricted to measurements certainly known to be identical, the comparability rate dropped to 20% to 15%. Clearly the Monaco and Geneva agreements had failed to create any uniformity among researchers. He urged for new rounds of standardization, which would dramatically increase the value of the data.

A former student of Pearson's, Miriam Louise Tildesley, took up this call. After studying craniometry in Pearson's lab after World War I, she worked as a curator at the Royal College of Surgeons' Museum in London from 1920-1934. She also became an active member of the British Royal Anthropological Institute. In 1928, she published "A Plan to Obtain International Uniformity of Method" in racial anthropometry in the RAI's journal. This plan marked the beginning of her career-long path of organizing international standardization of racial measurements until after World War II.[29]

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[27] P.C. Mahalanobis, "On the Need for Standardisation in Measurements on the Living," *Biometrika* 20A (1928) 2.

[28] P.C. Mahalanobis, "On the Need for Standardisation in Measurements on the Living," 27.

[29] Tildesley's career exemplifies the widening opportunities for women during and after World War I. She writes in her CV: "Next three years spent in teaching, until the immense widening of the choice of occupation for women which took place during the First World War gave the opportunity for leaving a profession into which a reluctant entrant had been directed by lack of alternatives." During the war, Tildesley, who received the Cambridge Higher Local Honors Certificate in Mathematics, worked under Pearson in the Department of Applied Statistics at University College, doing "statistical work required for war purposes" and took a course on statistical theory and practice. After completing this course, she was given the Crewdson Benington studentship at age 35. Miriam Tildesley Papers (Cambridge, UK) "CURRICULUM VITAE."

Like Mahalanobis and previous pleas for standardization, Tildesley's plan centered around making racial data comparable. The project of measuring man's races was "so large as to necessitate the co-operative effort of a great many workers – more than one laboratory, or even one country, can supply. For their combined effort to be of any value, it is essential that the results of their work be comparable." Despite the Monaco and Geneva agreements, she diagnosed that a "lamentable lack of agreement" between racial researchers persisted, whose subjective expertise and institutional loyalty dictated their choice of measurement. Nevertheless, researchers continued to uncritically lump together data from different systems and draw conclusions from these comparisons. "We get no farther by these methods – such conclusions may merely muddy the wells of truth," Tildesley warned.

She suggested setting up a three-person international committee that would review the standard head, skull, and body measurements in various measuring systems. This committee needed to do more than merely choose the most important racial characters and select their landmarks: they should assess which characters, measurement techniques, and instruments offered the *best results*. This was a fundamentally biometric question for Tildesley: the best techniques reduced the possibility of observational error and the best characters demonstrated high inter-racial variabilities but little correlation with each other within races. The statistical constants required for such determinations would demand extensive research from the committee.

Tildesley also proposed taking the most popular measuring system as a starting point for standardization. She had surveyed this question at the IIA meeting in Amsterdam in 1927 and concluded that "more anthropometric work was being carried on on the basis of Martin's *Lehrbuch* than on any other system, and that, therefore, there was more hope of eventual general agreement on this basis than on any other." This was possibly a convenient result for Tildesley, who, like others who passed through Pearson's lab, most likely worked with the German method herself. She recognized, however, that this decision could upset those working with the French system. Indeed, standardization could be obstructed by "psychological" factors: "the psychological struggle in the anthropometrist's mind is more apt to centre around what he in particular, or his particular school, has used, rather than on the total amount of work done." The best way forward was for workers to use both the standardized methods and their own, so that their work was available for comparative work in the future.[30]

Tildesley sent offprints of her call to colleagues in Germany, France, Czechoslovakia, and the United States. Her plea was heard. In 1929, the Royal Anthropological Institute instructed Tildesley to set up an international committee on its behalf.[31] She determined that the most effective way to form this committee would be to link it to an international body of racial researchers. It took Tildesley years, however, to create the right committee at the right venue, due to several "psychological factors" of methodological and political kinds that divided racial researchers internationally.

In the late 1920s, another scientific group began to problematize the incomparability of racial measurements: the eugenicists. The subject had come up during meetings of the International Federation of Eugenics Organizations (IFEO). Eugenicists considered the uniformity of method essential to studying the genetic basis for racial differences in body and mind. Without standardization, they worried, anthropometric data and intelligence-test data were incomparable. As the prospect of a truly international anthropology platform hung in the balance, the IFEO provided Tildesley with the right "machinery" to organize international standardization. Indeed, she wrote that "the Eugenics Federation appears to be happily free from the kind of difficulty which at present

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30 Miriam Tildesley, "Racial Anthropometry: A Plan to Obtain International Uniformity of Method," *The Journal of the Royal Anthropological Institute of Great Britain and Ireland* 58 (1928) 351–62, quotations on pages 354 and 357.

31 Miriam Tildesley Archive, MS294/12; RAI Archive, A58/3/4, "British Joint Committee for Anthropological Teaching and Research."

hampers international co-operation between anthropologists." The IFEO invited Tildesley to speak about her plans at their 1930 meeting and in the next two years, the IFEO established a *Comité de Standardisation de la Technique Anthropologique,* short CSTA. The committee grouped together 12 researchers from 12 countries with Tildesley as chair and would be transferred to an international anthropology congress once such an organization came into existence.[32]

Meanwhile, Tildesley spoke to French anthropologists at the IIA and anthropologists in Germany in order to mend their relationships and hasten the process of establishing an anthropological venue for her new standardization committee. These activities got her into some trouble with John Myres, the president of the Royal Anthropological Institute. The RAI had not tasked her to get involved with IIA business and Myres was embarrassed by the way Tildesley had "gone about Europe" discussing the future of the IIA while the Royal Anthropological Institute pressured the French organization to change and become more international. He did not want to have anything to do "with your project of using the Eugenist Conference as a cover for international intrigue." Anthropological politics and international politics were still precarious and he believed that Tildesley's actions could spark new international conflict. He urged her to be patient and wait for the fate of the international anthropology conference to be settled. Tildesley explained that "my main object in taking part in anthropological politics has been to get machinery created by which the international standardization of techniques can be progressively accomplished."[33]

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[32] C.B. Davenport, "Aims and Methods in Anthropometry," in: Institut International d'Anthropologie, "III[e] Session Amsterdam 20-29 Septembre 1927," (Paris 1928) 108; RAI Archive, A62/151/35 Tildesley to Myres, 16.10.1930; A62/151/8, Tildesley to foreign anthropologists, 25.6.1930; also in AHA, Box 63, Correspondence Tildesley, C.B.S. Hodson to Aleš Hrdlička, 15.7.1930. On the CSTA: RAI Archive, A62/193/3 IFEO to International Congress of Anthropological and Ethnological Sciences. Members included: Chevket Aziz (Turkey); Czekanowski (Poland), Kleiweg de Zwaan (Holland); Lundborg (Sweden); Mollison (Germany); Schlaginhaufen (Switzerland); Sergio Sergi (Italy); Suk (Czechoslovakia); Vallois (France); Wagner (Norway); Woo (China); and Tildesley (Great Britain). A separate committee for the standardization of psychological measurements was also formed.

[33] RAI Archive, A62/151/1 Tildesley to Council; A62/151/9 Tildesley to Myres, 1.7.1930; A62/151/10, Myres to Tildesley, 2.7.1930; A62/151/11 Myres to Keith, 2.7.1930; A62/151/14 Myres to Keith, 6.7.1930; A62/151/35 Tildesley to Myres, 16.10.1930; A62/151/52 Myres to Tildesley, 3.4.1931. Anthropologist of the Wellcome Museum, L.W.G.

"Tiresome Anthropometric Affairs:" Competing Standardization Schemes

In 1934, IFEO chair Ernst Rüdin transferred Tildesley's standardization committee to the new *International Congress of Anthropological and Ethnological Sciences* (ICEAS). The committee had expanded to 22 members from 22 countries and published a general statement in the British anthropology journal *Man,* the French *L'Anthropologie*, the German *Anthropologischer Anzeiger*, the *Journal of the Anthropological Society of Tokyo*, as well as the USSR's *Anthropological Journal.* The RAI offered Tildesley a room at the institute that she could use for her CSTA business. At the first ICAES meeting in 1934, a special session was dedicated to anthropometry and standardization. So many papers were submitted, on topics ranging from standardizing head measurements to photography, that there was hardly any time for discussion.[34]

Instead of her earlier-proposed three-person committee, by now Tildesley envisioned a more complex scheme of organizing agreement within and between countries. She set up several sub-committees with members from different "schools" that would compare a particular measurement across methodological systems, such as head length or cranial capacity. The committee would test which system offered the most statistically consistent results for that measurement and then make a recommendation for a standardized measurement procedure and its landmarks and instruments to the larger committee. Once everyone agreed, the committee would publish the proposal in international journals and request written feedback and approval from researchers internationally. Tildesley believed that standards based on wide acceptance communicated through journals and

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Malcolm, wrote to Myres regarding Tildesley's actions: "We should be very careful indeed in dealing with certain International matters or we shall be having the Foreign Office to deal with." A62/151/13, Malcolm to Myers, 4.7.1930.

[34] RAI Archive, A62/193/3 IFEO to International Congress of Anthropological and Ethnological Sciences; RAI Archive, A106/1/11 "Report to the British Association on the Work of the Above Committee," 19.8.1938; G.M. Morant, "165. Section Ab. Anthropometry and the Standardization of Technique, Including the Application of Anthropometric Technique to Regional Anthropology," *Man* 34 (1934) 144–45.

conferences would be more effective than finding agreement within a small group and "hoping that the rest of the world would take note and modify its methods accordingly."[35]

Tildesley embarked on a world tour in order to mitigate any "psychological" obstacles that researchers might experience towards standardization and to solicit participation in the sub-committees. She visited racial researchers in 22 European countries and traveled to Dutch Indonesia, Japan, Australia, New Zealand, Canada, and the United States between 1935-1937. Apart from some funds from the ICAES and Arthur Keith, Tildesley bore the expenses herself, with the help from her father. Her tour was largely successful: several researchers joined the CSTA and signed up for her sub-committees. The committee grew to 35 members from 35 countries. In Italy and the United States, however, she met fierce resistance and competing schemes. These were largely the result of the "psychological" factors Tildesley had already anticipated in 1928.[36]

Tildesley's standardization committee might not have been the only one appointed by the IFEO. Italian physical anthropologist Fabio Frassetto had also been raising awareness for the need for standardization since 1918 and had presented his plans for setting up an international committee at the 1927 IIA meeting in Amsterdam.[37] He had discussed these plans at the IIA institute in Paris in 1929, and planned to formally establish a committee through the IIA at the 1930 Portugal meeting. When this meeting was adjourned, Frassetto also turned to the eugenicists and presented his scheme

[35] V. Bunak et al, "109. The International Committee for Standardization of the Technique of Physical Anthropology," *Man* 34 (1934) 83–86. Quotation from A106/1/11 "Report to the British Association on the Work of the Above Committee," 19.8.1938.

[36] RAI Archive, A106/1/7, Tildesley to H.S Harrison, 3.10.1935; RAI Archive, MS294/18 Correspondence with Franz Boas, Tildesley to Boas 30.3.1938; Miriam Tildesley, "84. Human Biology. Observations and Results of a European Tour in the Interests of Standardization," *Man* 36 (1936) 67–68; H.V. Vallois and R. Routil, "Internationaler Kongress der anthropologischen und ethnologischen Wissenschaften. Comité der Standardisierung der anthropologischen Technik (C.S.T.A.)," *Anthropologischer Anzeiger* 14:2 (1937) 170–73.

[37] Fabio Frassetto, "A Uniform Blank of Measurements to be Used in Recruiting. A Plea for the Standardization of Anthropological Methods," *American Anthropologist* 21:2 (1919) 175–81; Fabio Frassetto, "Proposta per la unificazione e standardizzazione dei metodi anthropometrici e biometici," Institut International d'Anthropologie, "Iiie Session Amsterdam 20-29 Septembre 1927," (Paris 1928) 109.

at the IFEO meeting in 1932. Here, Frassetto argued, IFEO chair Charles Davenport assigned *him* to set up an international standardization committee, which Frassetto called the *Standardisation Anthropologique Synthétique* (SAS). His committee promoted the uniformity of anthropometric, biometric, and eugenic methods. It would bring together researchers from various countries that would prepare and discuss proposals for standardization together. The SAS would also issue a yearly international brochure.

Frassetto was frustrated that the IFEO had also appointed the CSTA, which he considered a British committee; he did not acknowledge Tildesley as chair of an international initiative. In the 1934 Bulletin of SAS, he wrote: "faced with the British attempt, the S.A.S. wishes to claim itself in its entirety as the oldest and most concrete initiative of an international agreement for the unification of methods in anthropology and eugenics...The S.A.S. [has] the right of priority." Frassetto wanted to work peacefully with other committees by dividing the labor of researching methods. "Otherwise, the common aspirations for planned unification will be frustrated and we will find ourselves again in the current chaos."[38]

Privately, Tildesley expressed that Frassetto misunderstood the events at the 1932 IFEO meeting and probably misinterpreted "some show of hands after his lecture, or a polite remark from Davenport, as sufficient authorization: and of course the language difficulty can be responsible for quite a lot of imperfect understanding." The SAS committee held two meetings in 1934 and 1937 and some CSTA members also sat on Frassetto's committee. Frassetto continued to publish the SAS Bulletin in four languages until his death in 1953.[39]

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[38] RAI Archive, "Congress International des Sciences Anthropologiques et Ethnogiques," A62/196/90.1; A62/196/94.

[39] RAI Archive, A62/151/60, Tildesley to Myres 21.2.1933; UNESCO, Directory of International Scientific Organizations (Paris: UNESCO 1953) 42-43; B. Chiarelli and G. D'Amore, "Frasetto, Fabio (1867-1953) in: Frank Spencer, *History of Physical Anthropology* volume I (New York: Garland Publishing 1997) 408.

Tildesley's standardization committee faced more vigorous opposition in the United States. Her struggles with getting American representation on the CSTA largely revolved around the intransigence of physical anthropologist Aleš Hrdlička. Hrdlička had studied anthropometry at Broca's institute in Paris under Léonce Manouvrier and this experience had made him a loyal defender of the French method, which he called "the mother of anthropometry." He lamented the Frankfurt Agreement and the loss of work the German divergence caused. Between 1903-1943, Hrdlička headed the Physical Anthropology division of the National Museum of Natural History at the Smithsonian Institution, where he desired but failed to create an American version of Broca's institute. Instead, he set up the *American Association of Physical Anthropologists* (AAPA) as well as the *American Journal of Physical Anthropology* (*AJPA*). He also published his own anthropometry handbook, in which he reprinted the Monaco and Geneva agreements.[40]

Tildesley had already written Hrdlička about her standardization plans in 1930. He had responded that anthropometry in Europe was in a bad state. "Do not believe that peace of 1918 has done away with nationalistic feelings and pride and ambitions," Hrdlička warned about German anthropologists. "France is situated even worse" since Manouvrier had passed away. In England conditions were not much better, where biometry and mathematics reigned with Pearson's laboratory, "which are strictly speaking additions to and developments of anthropometry, rather than anthropometry proper." In the United States, some followed the German method, some the French, others the Monaco and Geneva agreements. Hrdlička claimed that international agreement needed to be organized by someone who was thoroughly acquainted with all existing methods and

[40] AHA, Box 11, folder 1, Hrdlička to fellow committee members, 29.1.1936; A. Hrdlička, *Anthropometry* (Wistar Philadelphia, 1920); Frank Spencer, "Ales Hrdlička," in: *History of Physical Anthropology* volume I (New York: Garland Publishing 1997) 503-504.

she should proceed with "extreme caution and patience." He invited her to "come over to us in person and see just exactly what we are doing."[41]

This was precisely Tildesley's motivation for her world tour. She was eager to invite American researchers to join the CSTA and wrote to the presidents of the *American Anthropological Association* (AAA), the Anthropology Section of the *American Association for the Advancement of Science* (AAAS), and the *American Association of Physical Anthropologists* (AAPA) to nominate representatives in 1935. She was particularly anxious to get American scholars to participate in the sub-committees so these committees could begin their work with no further loss of time. While the presidents of the AAA and the anthropology section of the AAAS desired to cooperate, the president of the AAPA, Raymond Pearl, along with Hrdlička and physical anthropologists Earnest Hooton and Adolf Schultz presented a very different plan during the 1935 AAPA meeting. They suggested that each country should set up a *national* anthropometric committee before realizing an *international* committee. "The national committees will have full power to act as they deem best in all matters relating to anthropometry in their own country…[they] will not be bound against their well-substantiated judgments by the actions of any other national committee," nor an international committee. Rather than appointing representatives to the CSTA, the AAPA formed an American anthropometry committee with Hrdlička as chair and American anthropologists Robert Terry, Thomas Wingate Todd, Earnest Hooton, and Adolph Schultz.[42]

Hrdlička's committee approached standardization in a very similar way as the CSTA. First, it also took the necessity of comparable data as its starting point: "Comparison in the vast field of anthropological problems necessitates the cooperation by many workers over many

[41] RAI Archive, MS 294/19/1 Hrdlička to Tildesley, 21.8.1930.

[42] Frank Spencer Archive, Box 19, Folder International Anthropometry, MLT to Pearl, 30.5.1935; AHA Box 11, folder I, Krogman to Pearl 22.7.1935; "Proceedings of the Sixth Annual Meeting of the AAPA, April 25, 26 and 27, 1935," *American Journal of Physical Anthropology* 20:1 (1935) 4-5.

generations…[and] work so well defined and regulated that its results may safely be utilized at all times and by all concerned." American research methods had become too individualized and needed to harmonized first. Hrdlička believed this would be possible within a few years. Moreover, Hrdlička developed a plan that broke the task apart into "several unit studies" of measurements, their landmarks, and techniques. But unlike Tildesley's statistical studies of observational error and between-group and within-group variation, Hrdlička envisioned "thoroughly sound historical and factual" reports that surveyed the origins and all subsequent developments of a particular method. These reports would form the basis for eventual recommendations and would be published in the *AJPA*. Hrdlička believed that this research would be fit for younger researchers and insisted that they leave out any personal observations. He rapidly formed a "corps of collaborators" of 22 American researchers. Finally, the committee also recognized that subjective desires and choices could impede standardization. Committee member Raymond Pearl wrote: "The thing at issue in this standardization business seems to me really to be a matter of deep-rooted human psychology…No mature and experienced man will adopt and use a standardized method for taking a particular measurement when he knows out of his own trained knowledge and experience that his own method is intrinsically better…if he did he would be false to his own soul."[43]

The internal correspondence of this committee evidences that its members, especially Hrdlička, had no interest in collaborating with Tildesley or appointing members to the CSTA and its sub-committees. Hrdlička wrote in the committee's first memorandum that American anthropology was competent enough to standardize methods of measuring race itself and "should not depend on others' initiative and directive. It should undertake to regulate its own house." Hrdlička had a very different timeline for international standardization in view than Tildesley: every country should first

[43] AHA, Box 11, folder 1, Pearl to Todd, 8.1.1936; Hrdlička to fellow committee members, 29.1.1936; folder 2, Hrdlička to Ciocco 26.1.1937; Frank Spencer Archive, Box 19, Folder International Anthropometry, Hrdlička to collaborators, 6.5.1936.

organize its own methodology and only then "the time will come, though perhaps not in the life of those of us who are older, when whatever differences have arisen can readily be settled by international agreement. To begin with an effort at such an international agreement from the start…would be much like trying to build a house from the top." In particular, Hrdlička felt that "we can no more rely wholly on Europe," since there were two independent, non-cooperative movements for standardization, an English and an Italian organization. Like Frassetto, Hrdlička did not consider Tildesley's CSTA an international organization, but a British initiative. Moreover, he did not consider Frassetto or Tildesley experienced or "representative" enough to lead an international movement.[44]

Some subjective factors were certainly at the root of Hrdlička's rejection of Tildesley and her committee. As a biometrician, she represented a turn to quantification in racial science that Hrdlička strongly objected to. He considered the morphological method and simple measurements of racial characters foundational to racial research and despised statistics and biometrics. In the first issue of the *AJPA* in 1918, he wrote that "lofty" and "disdainful" biometricians lacked the medical and anatomical training that racial research and physical anthropology required. Moreover, anthropology dealt with fundamentally irregular series that were deficient in number, so the problem of racial origins could never be approached through mere mathematics. As the editor of the *AJPA*, Hrdlička rejected articles that were too statistical in tone and disapproved of publishing lengthy tables with individual measurements. Towards the end of his career, he even considered statistics, "machine-made hash," a threat to anthropology. He wrote to Ashley Montagu in 1940: "There is a great danger indeed to our science in the unbaked products that are now being send out as 'physical anthropologists.'" In 1937, he wrote to Frassetto: "There is, I am afraid, a spreading illusion that

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[44] AHA box 11, "Committee on Anthropometric Affairs," folder 1, Hrdlička to fellow members, 5.12.1935; Hrdlička to Todd 6.1.1936; Hrdlička to fellow committee members, 29.1.1936; Frank Spencer Archive, Box 19, Folder International Anthropometry, Hrdlička to Frassetto 25.11.1936.

biometric procedures may bring more out of any work than there is in it; and especially a spreading tendency to use such easy methods instead of hard brain work." Statistical methods, however, were not at all easy for Hrdlička. In Hrdlička's obituary, committee-member Adolph Schultz suggested that Hrdlička's lack of mathematical training probably explained his resistance to statistics.[45]

Tildesley continued to ask Hrdlička to nominate representatives to the CSTA, assuring him that "Machtpolitik could hardly go…Our only hope of standardization rests I think upon the fact of a common interest in making our data comparable, and on a certain modicum of general reasonableness and goodwill in seeking this end." Hrdlička repeatedly responded that only the AAPA had the authority to appoint members to her committee, not himself or his committee. As a last resort to ensure American participation, Tildesley traveled to the United States in the Spring of 1937. After visiting colleagues in New York, Philadelphia, and Boston, she visited Hrdlička in Washington D.C.. This meeting did not go well, as Tildesley recalled years later: Hrdlička presented her with an "American front." He also did not take well to her plan to use Martin's *Lehrbuch* as a starting point for standardization, resulting from complex anti-German sentiments rooted in Hrdlička's Czech origins. Tildesley wrote about the situation years later: "This was in America's isolationist period, and Hrdlička, who somehow managed to be 100% American and at the same time 100% Czech and therefore anti-German and ipso facto anti-Martin." She also suggested that Hrdlička took issue with the fact that she, as a woman, chaired this international committee. Privately, Hrdlička expressed fears that appointing American representatives to the CSTA would mean the paralysis and death of his own committee.[46]

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[45] AHA box 11, "Committee on Anthropometric Affairs," folder 1, Hrdlička to fellow committee members, 29.1.1936; Aleš Hrdlička, "Physical Anthropology: Its Scope and Aims; Its History and Present Status in America," *American Journal of Physical Anthropology* 1:1 (1918) 3–23, 15; AHA box 8, correspondence regarding the *AJPA*, Hrdlička to Montagu, 20.1.1941; Hrdlička to Farabee, 28.5.1918; AHA Box 25, correspondence with Frassetto, Hrdlička to Frassetto, 24.2.1937; A.H. Schultz, "Biographical Memoir of Aleš Hrdlička, 1869-1943," *National Academy of Sciences* (1945) 313.

[46] Frank Spencer archive, Box 19, Tildesley to Hrdlička 26.3.1936 (the letter says 1935; this is a mistake); RAI Archive, MS294/12/2, Hrdlička to Tildesley, 16.4.1936; A106/1/24, Tildesley to committee members, 25.11.1954; MS

Meanwhile, Tildesley and Earnest Hooton found a solution to the problem of appointing American committee members: they decided that American researchers could cooperate with the CSTA as individuals in private capacity, with no official weight of the AAPA behind them. After Hooton agreed, Tildesley quickly wrote to American physical anthropologists William Howells, Ashley Montagu, Wilton Krogman, and Harry Shapiro, who all agreed to participate in the sub-committees.[47] Hrdlička was devastated by these developments and wanted to issue a warning against individual participation in the *AJPA*.[48] To his committee members, he wrote that "The Europeans will not get very far. Their Committee has a wholly inadequate conception of what anthropometry really is and means. They were envious of our strength and prospective program, and so they tried to engage our young workers for their own purposes."[49] He felt that European researchers did not see American anthropologists as equals and wanted them to acknowledge the maturity and power of American anthropology, in which Hrdlička had heavily invested since the early 1900s. He was also disappointed in the younger generation of American anthropologists: "Some of our young workers have preferred foreign tinsel for the interests of the country that has given them all they are and from which they expect everything further."[50]

These younger anthropologists, however, had grown resentful of Hrdlička and his anthropometry committee. They were eager to participate in international research teams that offered far more room for experimentation and testing than Hrdlička's historical surveys of methods. Moreover, a majority of AAPA members approved of collaboration with the CSTA and

294/20/12, correspondence with Frank Spencer, Spencer to Tildesley (undated), Tildesley to Spencer 6.12.1975; AHA Box 32, correspondence with Earnest Hooton, Hrdlička to Hooton, 1.5.1937.

47 RAI Archive, MS 294/19/5 Correspondence with Hrdlička, 8.12.1937.

48 AHA, Box 11, folder 2, Hrdlička to committee members, 4.1.1938.

49 AHA, Box 11, folder 2, Hrdlička to Pearl, 7.4.1938.

50 AHA, Box 11, folder 2, Hrdlička to Todd, 25.4.1938.

almost decided to discharge Hrdlička's committee. Unhinged by his unpopularity in the AAPA, Hrdlička pushed through one more resolution on behalf of his committee. While the AAPA approved of individual participation in the CSTA sub-committees, "it must continue to disassociate itself officially from the [CSTA's] deliberations, activities and conclusions."[51]

A final overture came from the CSTA's secretary, French anthropologist Henri Vallois. In a 6-page letter to Hrdlička, Vallois stressed that he would fight for the methods of Hrdlička's beloved mentor Manouvrier, that he was not a fan of Martin's methods himself, and that he would ensure that the committee would not merely steer the course of statistics. Hrdlička responded that "Miss T. has rather hurt her chances with us, due to the facts that her and her associates' closest interests lie in the direction of statistics rather than anthropometry; that she can not be regarded personally as sufficiently qualified in anthropometry to lead the movement for its revision." Hrdlička's stubbornness led to the death of his own committee. It did not resume activity after his final resolution in 1938 and was officially discharged a few weeks after Hrdlička's retirement in 1942.[52]

The CSTA At Work

Ten years after Tildesley published her plan for standardization, her committee could finally get to work now that it had American representatives. Nine sub-committees had formed that would mainly research measurements related to the face and the skull, such as facial height and nose height (fig. 5.3). This was intentional: while anthropology was a living science that continued to develop new

51 AHA Box 11, Folder 3, Hooton to Hrdlička, 8.1.1938; Hooton to Hrdlička, 7.2.1938; AAPA, "Proceedings of the Ninth Annual Meeting," *American Journal of Physical Anthropology* 23:4 (1938) 490.

52 AHA, Box 64, correspondence with Vallois, Vallois to Hrdlička, 13.10.1938; 4.11.1938; AAPA, "Proceedings of the Thirteenth Annual Meeting," *American Journal of Physical Anthropology* 29:2 (1942) 315-316. There was one more encounter between Tildesley and Hrdlička, in 1939. Upon visiting London, Hrdlička had fallen ill and was hospitalized. His friend and colleague Arthur Keith was too sick himself to travel to London and had asked Tildesley to visit Hrdlička in the hospital. Hrdlička gratefully welcomed her and her home-made pastries, which he considered a nice change from the hospital diet. In 1943, a few weeks before his death, Hrdlička wrote to Keith and asked about Tildesley: "I wonder what has become of Miss Tildesley. If you ever see her or write to give her my regards, and tell her I will never forget how kind she was to me in the hospital." AHA Box 37, correspondence Keith, Hrdlička to Keith, 13.8.1943.

methods and insights, the focus of standardization, Tildesley argued, should be on older methods that had proven their usefulness for racial research and researchers around the world used extensively. These methods had also generated sufficient data for testing.[53] Each sub-committee consisted of at least two members and represented German and non-German schools of measurement.

SECTIONS OF TECHNIQUE AND EXECUTIVE GROUPS IN CHARGE OF THEM.

1. *Cranial capacity* (direct measurement).—Dr. Emil Breitinger (Munich); Dr. G. M. Morant (London).
2. *Auricular height of the head.*—Dr. Lucia Graf (Zürich); Dr. W. W. Howells (New York); Prof. A. Low (Aberdeen); Prof. O. Schlaginhaufen (Zürich).
3. *Classification of eye-colour.*—Dr. G. P. Frets (Portugaal, Holland); Dr. H. T. E. Hertzberg (Cambridge, Mass.); Prof. Josef Weninger (Vienna).
4. *Nose height, total face height, upper face height, on the living and the cadaver.*—Dr. Sophie Ehrhardt (Berlin); Prof. G. Genna (Rome); Dr. M. F. Ashley Montagu (New York); Prof. P. H. Stevenson (Peiping).
5. *Cranial points and measurements not dependent on craniostatic orientation.*—Dr. Eberhard Geyer (Vienna); Dr. W. M. Krogman (Cleveland, Ohio); Dr. G. M. Morant (London).
6. *Head-measurements other than those specified under* 2 *and* 4 *and than those on the ear.*—Prof. G. Dahlberg (Upsala); Dr. B. S. Guha (Calcutta); Prof. O. Schlaginhaufen (Zürich); Dr. Geo. Dee Williams (St. Louis, Mo.).
7. *Teeth.*—Dr. Milo Hellman (New York); Dr. G. Hjelmman (Helsingfors); Prof. F. Weidenreich (Peiping).
8. *Humerus.*—Dr. Emil Breitinger (Munich); Prof. Montague Cobb (Washington, D.C.); Prof. G. Genna (Rome).
9. *Tibia.*—Dr. Lucia Graf (Zürich); Dr. H. L. Shapiro (New York); Prof. H. V. Vallois (Toulouse).

Figure 5.3. Sub-committees of the CSTA and its members. Source: M.L. Tildesley, "52. Congres International des Sciences Anthropologiques et Ethnologiques: Comité de Standardisation de la Technique Anthropologique (C.S.T.A.)," *Man* 38 (1938) 59.

Tildesley requested that the groups deliver progress reports by May 1938, so that these could be circulated before the next meeting of the *International Congress of Anthropological and Ethnological Sciences* in Copenhagen in August. The CSTA met in the days before the Congress meeting to discuss and prepare final proposals. For some sub-committees, such as those researching cranial capacity, the classification of eye color, and various facial heights, the CSTA decided that further study was

[53] AHA, Box 11, Folder 1, Tildesley to Hrdlička, 26.3.1935.

necessary. For instance, members agreed that facial heights should include the *nasion* but resolved that the determination of this point on the living needed further study.

The CSTA met twice during the Copenhagen meeting. Tildesley was re-elected as chair and Vallois as secretary for the new term 1938-1942. The committee decided to streamline the process of testing, proposing, and approving methods for standardization. First, individuals rather than sub-committees would research and draft proposals. Second, not all proposals needed to be discussed in conference, only the ones that concerned most racial researchers. The CSTA would organize specialist conferences for lesser-used techniques when needed. Third, the CSTA planned to draft two "minimum list of measurements" for the most important racial characteristics on the living and the skull.

The CSTA also reached agreement regarding the proposals of the five sub-committees and other technicalities. They resolved that measurements in publications should be referred to in Latin so that tables could be used by researchers globally. Head-measurements should be taken without pressure and with the subject seated. Auricular height should be measured with the head adjusted to the Frankfurt plane. The committee now needed to solicit the opinions and approval from researchers internationally. The CSTA published the proposals in the international journals *Anthropologischer Anzeiger*, *L'Anthropologie*, and *Man* with the note: "Anthropologists professionally employed in physical anthropological research or teaching are urged to consider carefully the above proposals, and to communicate their acceptance or disagreement to the C.S.T.A. Chairman…or to the C.S.T.A. secretary," listing their addresses. "Those proposals that meet with general acceptance will be formally adopted as standard technique, and an announcement to this effect will be made in this and other journals." Tildesley expected that such crowdsourcing of opinion would enable the CSTA to draw up a list of standardized measurements and techniques. The first nine research subjects were trial runs and she planned to expand the number of studies during her next term. She expected that her chairmanship duties would demand more of her time in the next years.[54]

Did the CSTA have a "truly international" character, as was so desired since the 1920s? The committee now had 36 members from 34 countries. Visualizing the countries represented creates a notable map with CSTA participating countries in red (fig. 5.4). Representing anthropological "schools" in practice meant including researchers from various countries. The CSTA asked that its members would form links with their respective countries and would communicate decisions and proposals with their research communities back home.[55] The map also demonstrates that it was largely the Global North that was actively engaged in racial-anthropological research; the Global South predominantly functioned as the object of this research.

War interrupted standardization efforts yet again. In June 1940, Tildesley optimistically believed that "the interim period" could be used to research techniques. She started a new research project on the inter-racial and intra-racial variabilities of 14 characters. Other members of the CSTA, however, never sent in new reports. The third meeting of the international anthropology congress, scheduled for 1942, could not take place. Anthropologists were recruited for war efforts, as the previous chapter discussed. "Obviously, no 'agreements' on technique could be effected after the war had cut short the peaceful kinds of international contact," Tildesley reflected in 1947.[56]

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[54] H.V. Vallois and R. Routil, "Internationaler Kongress der anthropologischen und ethnologischen Wissenschaften. Comité der Standardisierung der anthropologischen Technik (C.S.T.A.)," *Anthropologischer Anzeiger* 14:2 (1937) 170–73; Miriam Tildesley, "52. Congres international des sciences anthropologiques et ethnologiques: Comité de Standardisation de la Technique Anthropologique (C.S.T.A.)," *Man* 38 (1938) 59; Miriam Tildesley and H.V. Vallois, "68. Congres International des sciences anthropologiques et ethnologiques: Comité de Standardisation de la Technique Anthropologique (C.S.T.A.)," *Man* 39 (1939) 73–77; RAI, A106/1/11, "Report to the British Association on the Work of the above Committee," 19.8.1938.

[55] Frank Spencer Archive, Box 19, Miriam Tildesley, "Memorandum on the Functions of Committee-members and Group-secretaries respectively;" RAI Archive, A106/1/11, "Report to the British Association on the Work of the above Committee," 19.8.1938.

[56] RAI Archive, A106/3/7 "Report to the RAI in June 1940;" Miriam Tildesley, "Comité de Standarización de la Técnica Antropológica," *Boletín Bibliográfico de Antropología Americana (1937-1948)* 10 (1947) 5–8.

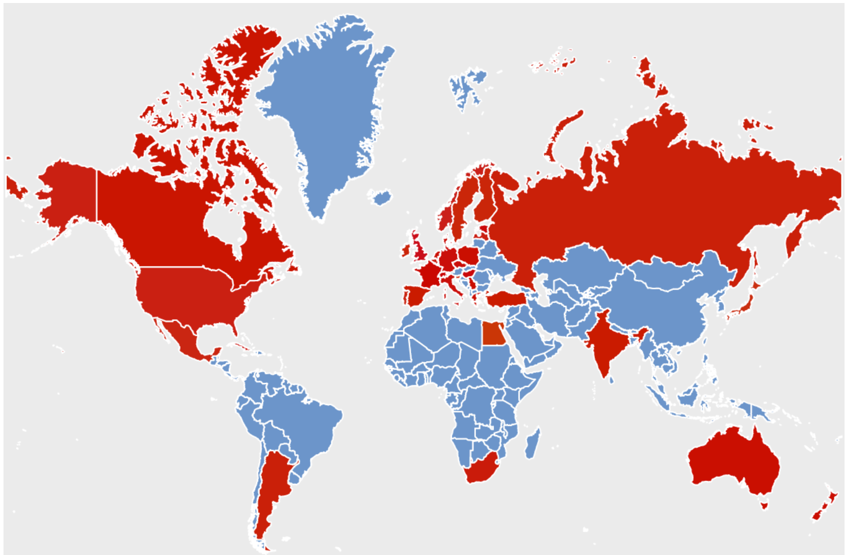

Figure 5.4. Map displaying the countries represented on the CSTA in red. The map, which does not accurately represent the dividing lines between countries by 1939, was created by author with an online tool.

Postwar standardization efforts

The British *Royal Anthropological Institute* took the lead in reviving the *International Congress of Anthropological and Ethnological Sciences* after the war. The congress's council immediately decided to "rebuild ruined projects," like its various research committees whose work had been suspended for years. They reinstated Tildesley's standardization committee. During the ICAES's next meeting in Brussels in 1948, the CSTA appointed new members. Tildesley and Dutch physical anthropologist Adele van Bork-Feltkamp presented two standard lists of measurements on the skull and the living, as was decided upon at the 1938 Copenhagen meeting. Both women had researched which characters demonstrated relative stability within racial groups but varied between races. They had collected data from publications and created datasets to compare the means and standard deviations of various racial series and measurements. Of course, they had only compared data that derived from observers using the same techniques. Tildesley and Van Bork-Feltkamp determined that characters that varied more between races than within races were relevant for racial research. Head-breadth proved to be a particularly useful discriminatory character for both the living and the dead.[57]

Unfortunately, the CSTA lost momentum after 1948. The list of members appointed at the Brussels meeting was lost in transit and could not be retrieved. Moreover, Tildesley took a prolonged hiatus from anthropological research and resigned as the committee's chairman in 1952. Her secretary Vallois was appointed as chairman but the CSTA stopped functioning. At the next Congress meeting in 1960 in Paris, a meeting on standardization attracted 50 people who all agreed that a new standardization committee should be formed. The *Comité de Coordination pour la Standardisation en Anthropométrie* was established. It would scrutinize the multifarious techniques that physical anthropologists used throughout the world. Thus, the work started all over again.[58]

In the aftermath of World War II, physical anthropologists largely moved their focus from race to forensic anthropology and the study of ancient and prehistoric populations. The field human biology absorbed the measurement of living bodies.[59] New initiatives picked up the standardization of man's measurements, fueled by the enduring desire to make data comparable and reusable. Human biologist Joseph Weiner, briefly associated with the CSTA in the 1950s, led an effort in the 1960s to create a standard field methods guide for human biological research as part of the International Biological Programme and its Human Adaptability section. The guide standardized

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[57] J.L. Myres, "International Congress of Anthropological and Ethnological Sciences," *Nature* 157:3994 (1946) 665–66; International Congress, "71. International Congress of Anthropological and Ethnological Sciences: Meeting of the Permanent Council at Oxford, 12-15 April, 1946," *Man* 46 (1946) 74–80; Tildesley Papers Cambridge, "CSTA Chairman's report 1952," Miriam Tildesley, "14. The Relative Usefulness of Various Characters on the Living for Racial Comparison," *Man* 50 (1950) 14–17; A. van Bork-Feltkamp, "15. The Relative Usefulness of Various Cranial Characters for Racial Comparison," *Man* 50 (1950) 17–19.

[58] Miriam Tildesley Papers (Cambridge, UK) "CSTA Chairman's report 1952;" RAI Archive, A106/3/8 "British Committee for the Standardization of Anthropometric Techniques, Report for 1948-50; A106/3/11 "Report, Oct. 1954;" "Comités Permanents de Recherches. A) Comité pour la Standardisation de la Technique Anthropologique," VIe Congres International Des Sciences Anthropologiques et Ethnologiques, Paris July 30-August 6, 1960 (Paris 1962) 105-106; M-R. Sauter, "Koordinations-Komitee Zur Anthropometrischen Standardisierung," *Anthropologischer Anzeiger* (1961) 319–20.

[59] Of course, old notions of race and racial measurements persisted, as this dissertation argues. See the introduction for a fuller discussion of the literature on this subject.

living body measurements in order to enhance the data's comparability. It included a "basic list of measurements" with several characters that were important to prewar racial research. Thus, old measurements survived in new scientific landscapes.[60]

European physical anthropologists standardized methods for diagnosing age and sex in skeletons in the 1970s. Many of the guidelines revolve around the morphological observation of bones and scoring elements of the bone on a standard score sheet. The sheet divides traits into five categories, ranking from "hyperfeminine (-2)" to "hypermasculine (+2)" and each element has been given a particular weight that indicates its importance for sex or age determination. For instance, the *sulcus praeauricularis*, a groove on the iliac bone of the pelvis, is "deep, well delimited" in a hyperfeminine pelvis, "medium" in a neutral pelvis, and "absent" in a hypermasculine pelvis. The researcher would then divide the sum of the weights multiplied by the scores by the sum of the weights to determine the sex and degree of sexualization of the skeleton. For skeletal measurements, the guide mostly draws from Martin's *Lehrbuch*, especially its third edition from 1957. It also incorporated new statistical methods such as linear discriminant analysis. The researcher would insert several measurements in a pre-determined formula, discriminant functions, carry out the calculation, and compare the final value with available data on the series for which the function had been developed. Thus, metrical and morphological methods continued to develop in the second half of the 20th century. The guide also evidences a shift in physical anthropology from between-group comparison to within-group comparison. The guide warned that age and sex determinations were always relative to their own series. "The sex classification of a bone is possible with a degree of

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[60] Compared to Hrdlička's list of measurements in his 1920 *Anthropometry*, 12 out of 21 characters are the same and Weiner's handbook promoted in several cases the use of the same instruments and landmarks. Compared to Tildesley's postwar standard list of racially relevant characters on the living, 13 characters correspond to the IBP handbook. J.S. Weiner and J.A. Lourie, *Human Biology: a guide to field methods. IBP Handbook no. 9* (Oxford: Blackwell Scientific Publications 1969); G. Harrison & K. Collins, "Joseph Sydney Weiner (1915-1982)" *Annals of Human Biology* 9:6 (1982) 583-592; G. Harrison, "The role of the Human Adaptability International Biological Programme in the development of human population biology," in: Stanley Ulijaszek and Rebecca Huss-Ashmore, *Human Adaptability. Past, Present, and Future. The first Parkes Foundation Workshop, Oxford January 1994* (Oxford: Oxford University Press 1997) 17-25.

certainty only when it can be compared to a series of known sexual dimorphism…It is not possible to define precise morphological and metrical boundaries, between males and females applicable to all series.[61]

In the American context, a new round of standardization was spurred by the introduction of the Native American Graves Protection and Repatriation Act (NAGRPA) in 1990, which began the repatriation of Indigenous skulls and bones stored in museums and universities. While sympathetic to Native concerns, archaeologists and physical anthropologists were "deeply concerned with the loss of knowledge that may result from these developments. Just as scientific research techniques are offering new avenues for" investigation, "access to skeletal collections is diminishing," anthropologists Jane Buikstra and Douglas Uberlaker diagnosed. With a lack of accepted standards for data collection from remains scheduled for repatriation, there was limited opportunity for comparison and the risk of losing "unique and important information." Through an NSF-sponsored workshop, researchers agreed on standard methods for analyzing archaeologically removed remains. This consensus was materialized in a standard manual in 1994. Unlike the history of the CSTA, politics here prompted standardization rather than hindered it. Like the European system, the American guide also recommends metrical and morphological methods: it largely relies on Martin's 1957 *Lehrbuch* and uses a somewhat different morphological scoring system. It also warns that metrical and morphological observations are always population-specific, and sex and age determinations require the observer to be "familiar with the overall patterns of variability within the population from which the sample is drawn."[62]

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[61] D. Ferembach, I. Schwidetzky, and M Stoukal, "Recommendations for Age and Sex Diagnoses of Skeletons," *Journal of Human Evolution* 9 (1980) 517–49; G.J.R. Maat, A.E. van der Merwe, Th. Hoff, *Manual for the Physical Anthropological Report. Barge's Anthropologica number 6* 7th edition (Amsterdam: Barge's Anthropologica 2012); R. Martin and K. Saller, *Lehrbuch Der Anthropologie* (Stuttgart: Gustav Fisher Verlag 1956).

[62] J.E. Buikstra and D.H. Uberlaker (eds.), *Standards for Data Collection from Human Skeletal Remains. Proceedings of a Seminar At the Field Museum of Natural History Organized By Jonathan Haas* (Fayetteville: Arkansas Archaeological Survey Research Series No. 44 1994).

Physical anthropologists today continue to use the methods they prefer alongside standard guidelines. The above-mentioned guides have proven especially helpful for training students in physical anthropology's methods as they teach a systematic approach of precisely capturing what students see in drawings, descriptions, and data. For specific studies, physical anthropologists consult specialized manuals. Sometimes scientific societies offer guidelines. The lack of a rigid, uniform system seems to matter less today, as long as researchers precisely describe their methods and carefully consider to what extent they can use and reuse data.[63]

Conclusion

In his RAI presidential address of 1930, John Myres said: "All science rests on observation of uniformities and identities under varying conditions, and requires therefore uniformity of procedure, instruments, and presentation of records."[64] Racial research was indeed fueled by a continued desire to make data comparable and reusable between researchers, institutions, and countries, to make data move freely. It may seem obvious that such hopes of objectivity required standardization and one may expect a less interesting story of scientists seeking agreement. Instead, this chapter demonstrated how difficult achieving standardization was. A story emerges of complex on-the-ground practices of negotiating between people, institutions, disciplines, and nations by "in-between" actors such as Miriam Tildesley. Indeed, the success of standardization seemed to depend on the energies of brokers such as Tildesley, who called herself the "anthropological standardization

[63] Interview with L. Van der Merwe, 13 July 2018; 18 July 2019; interview with G.A.J. Maat, 4 October 2019.

[64] J.L. Myres, "Presidential Address. Anthropology: National and International," *Journal of the Royal Anthropological Institute of Great Britain and Ireland* 60 (1930) 17–45, quotation on page 23.

godmother." She had the time and financial resources to fully dedicate herself to the "anthropological politics" that standardization required.[65]

Tildesley's actions, however, were constrained by various scientific and political concerns. The type of methodological debates and divisions explored in previous chapters also played out in the history of standardizing man's measurements. A shared commitment to scientific universalism brought together anthropologists, anatomists, biometricians, and eugenicists but personal preferences and methodological commitments tied them to their own methodological "school." Standardization inevitably became a problem of dealing with subjective human psychology, as various actors noted. What is more, the history of standardizing anthropology's methods cannot be properly understood without knowledge of the turn to quantification in racial science. Tensions over the rise of metrical and statistical methods in racial research obstructed finding agreement on how to make data move.

Methodological concerns folded into political troubles. Standardization was interrupted by warfare, anti-German sentiment, chauvinism, and international rivalry. These "tiresome anthropometric affairs" demonstrate that there was a lot at stake for the actors involved, such as prestige and loyalty to their own country. International politics spilled over into anthropological politics. Historian Elazar Barkan observed that "the story of the work of the [CSTA] if traced would provide a fascinating case study for the failure of a discipline through personal and international rivalry."[66] This failure was not without consequences. Geoffrey Morant suggested in 1939 that the disagreement between racial researchers on methods and definitions of race prevented them from giving a clear message about the proper scientific treatment of race and from protesting Nazi racism.

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[65] I am referring here to concepts developed in Simon Schaffer, Lissa Roberts, Kapil Raj, and James Delbourgo (eds.), *The Brokered World: Go-Betweens and Global Intelligence, 1770-1820* (Sagamore Beach: Science History Publications 2009). The quote comes from Ashley Montagu Papers, correspondence with Tildesley, Tildesley to Montagu 12.7.1938.

[66] Elazar Barkan, *The Retreat of Scientific Racism: Changing Concepts of Race in Britain and the United States Between the World Wars* (Cambridge, UK: Cambridge University Press 1992) page 159 footnote 41.

The history of standardizing man's measurements, therefore, sheds new light on the question why anthropologists failed to unite against Nazi racism in the 1930s.[67]

This complex intertwinement of politics and science made researchers doubt data. The data does not simply represent disembodied dreams but messy practices in which the relationship between objectivity, data, and race was anything but clear. Indeed, while researchers attempted to mechanize and automate racial research with quantification technologies in order to eliminate the human presence from the research process, this story reveals that the subjectivity of the researcher was never totally erased. The emotional stakes were too high.

[67] G.M. Morant, "Racial Theories and International Relations," *Journal of the Royal Anthropological Institute of Great Britain and Ireland* 69:2 (1939) 151–62, especially page 157.

The Afterlife of Skulls, Quantification, and Automation

A Turn to Quantification

This dissertation has traced a turn to quantification in the history of racial science. It shows that this transformation has been long underway. From the late 18th century, researchers have measured skulls and people in order to capture racial characteristics that were not visible to the eye. With data, they hoped to seize *race* more precisely and accurately. The dissertation argues that British mathematician Karl Pearson and his Biometric Laboratory in London gave these developments a new and powerful impetus. The biometricians criticized commonly used quantitative and qualitative methods in racial science and used their mathematical abilities to radically rethink and transform methods of sampling, measurement, and reduction. These were rather profound changes from within: the biometricians endeavored to make racial research more "scientific" and *objective*. At the core of these biometric interventions was a desire to *automate* racial research, to remove the *subjectivity* of both researcher and research object from the process of producing racial classifications. With quantitative technologies, the researcher, instruments, and formulas acted like automatons in the production of racial classifications, appearing to operate mechanically as a whole without personal judgment, biases, and errors. These biometric interventions built on earlier efforts to automate racial research. We can see a trajectory from Broca's *stereographe* to Pearson's *coordinatograph*.

The dissertation, however, reveals that profound methodological and technological change did not upend notions of race or dismiss the existence of biological races altogether. Indeed, Pearsonian anthropology *quantified* racial typology. Biometricians continued to display a fascination with isolated, "primitive" races with unique biological qualities. Existing racial taxonomies and common assumptions about racial relations and temporalities framed their research outcomes. Thus, quantification and automation gave the *appearance* of operating without human intervention; technologies *enacted* objectivity. And with the veneer of objectivity through statistical reduction, the people under study were conquered through numbers. The power of data and increasingly complex statistical methods marked their bodies with inborn characteristics. Biometric formulas determined the historical paths their ancestors walked, irrespective of their own origin stories. The dissertation also shows that researchers had been interested in racial temporalities for a long time. Racial research had been about prehistory and the story of mankind all along.

Thus, today's conundrum of racial bias and biometry builds on a much longer history. Biometric methods have long been developed with the desire to automate the analysis of faces, races, and skulls. Researchers have long been forgetful that these methods were man-made, designed with and built on personal judgment, biases, and existing frameworks. The deep history of the data practices that brought about today's biometric technologies have largely been forgotten; only the tools and techniques remain, seemingly divorced from their histories of creation.

The story doesn't end here. The biometric approach to quantifying race through automation and reduction endured: a new generation of researchers continued to develop Pearson's methods and approaches. The desire for objectivity and automation received new momentum with the introduction of the electronic computer. With new methods and technologies, researchers returned to the cranial collections assembled in the late 19th and early 20th centuries.

The Afterlife of Skulls, Quantification, and Automation

In the aftermath of Nazi racial policy, researchers claimed to discard prewar racial research but continued to use skulls, race, and biometry in human evolutionary studies, now aided by the electronic computer. Statistical methods developed before World War II thrived with the introduction of the computer: its calculation powers made multivariate statistical analysis possible which was too complex for prewar researchers working with pencil, paper, and calculation machines. With the computer, researchers again made new use of the caliper, now producing cranial data in digital form. Physical anthropologists began putting heaps of racial data produced before and after the war into new computer programs developed for racial classification. They trusted that the computer and its strong computational capacities would produce better classifications and reveal new insights into human evolution. This next generation of researchers thus used new technologies to answer old questions of racial classification. In reusing prewar cranial collections and datasets, they built old racial biases into new computer programs.

We can connect the story laid out in the dissertation to these postwar transformations through the work and career of American physical anthropologist William Howells. Born in 1908 in New York City, Howells was introduced to anthropology at Harvard University, where he took undergraduate classes with Earnest Hooton. In 1934, he obtained a graduate degree in anthropology for his thesis *The Peopling of Melanesia as Indicated by Cranial Evidence*, written under Hooton's direction. Howells's early work attempted to discern racial types in cranial samples with measurements, means, and indexes, following Hooton's typological approach. He found himself unable to determine any types, and, in his own words, became "dubious about dissecting populations in this way, having some idea of normal variation…I became disappointed and disenchanted with available methods."[1] After obtaining his

[1] W.W. Howells, "Yesterday, Today and Tomorrow," *Annual Review of Anthropology* 21:1 (1992) 7-8.

Ph.D., he took up a research position at the American Museum of Natural History in New York, where he had a massive 12,000-piece skull collection from Melanesia to his availability, collected by German anthropologist Felix von Luschan in the early 1900s. Like Pearson's E-series, this collection offered Howells a statistically reliable sample to determine normal variation within a single population. He became drawn to statistics and began collaborating with Harold Hotelling, a statistician who had studied with Ronald Fisher and had further developed Pearson's correlation methods. Howells started to see the potential of multivariate techniques for physical anthropology. At the University of Wisconsin, where he took up a professorship from 1937 onwards, he educated himself in multivariate statistics and continued to collaborate with statisticians in applying multivariate analytical methods to his cranial data. With the help of high-speed computers that became more widely available from the 1950s onwards, Howells's research came to center on applying multivariate approaches to cranial data in order to unlock questions about human evolution and normal variation within and between populations.[2] Howells's career thus exemplifies how some of the research questions and approaches from before the war carried through and continued after 1945.

In 1973, he published *Cranial Variation in Man. A Study by Multivariate Analysis of Patterns of Difference Among Recent Human Populations*, arguably his most important book that combined these interests. The study set out to find the relation of the variation among individuals to the variation between populations in skull shape by applying multivariate statistics to almost 2000 skulls from all over the world. Its aim was taxonomic: "it has long been obvious, from simple observation, that cranial shape varies among populations of living *Homo Sapiens*, an observation that initiated much of anthropology. We have never known, however, how to make really *objective* statements as to the degree

[2] Jonathan Friedlaender, *William White Howells 1908-2005: A Biographical Memoir* (Washington, D.C.: National Academy of the Sciences 2007); L.R. Godfrey, "From the Shoulders of a Giant: Perspectives on the Legacy of William White Howells (1908-2005)," *Yearbook of Physical Anthropology* 51 (2008) 118-126.

of the variation," Howells argued.[3] Previous methods had not garnered any useful results and therefore it was "necessary virtually to begin again. New statistical theory, with the help of computers, and new outlooks on skulls as individuals in genetic populations, not as types, give us the new opportunities."[4]

Howells set out to identify which cranial characteristics were truly responsible for difference in skull shape, rather than using measurements *presumed* to be important and chosen *a priori*, like the cephalic index. "Then personal judgment is called upon in a high degree for interpretation of the meaning of the differences in different measurements."[5] With multivariate statistics, the anthropologist no longer compared one measurement at a time, but considered the shape of the skull as a whole, which was expressed by the relations *between* measurements. Howells pointed out that Pearson's CRL also tried to overcome the issues of comparing measurements separately and judging their significance subjectively, but had "failed, because this statistic was simply additive and ignored the correlations of measurements in which the relationships among them are to be found."[6] With skulls, multivariate statistics, and computers, Howells claimed that the anthropologist could now arrive at a *truly objective* quantification of human variation. The study did not attempt to discover fundamental reasons for observed cranial variation.

For this research, Howells and his wife Muriel visited 17 cranial collections around the world and measured the skulls of individuals from populations that covered "all major geographic regions." He gathered series that represented a "real population unit and time span" of "narrow genetic origin"

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[3] W.W. Howells, *Cranial Variation in Man. A Study by Multivariate Analysis of Patterns of Difference Among Recent Human Populations*. Papers of the Peabody Museum of Archaeology and Ethnology 67 (Cambridge, MA: Harvard University 1973) 1. Italics mine

[4] Howells, *Cranial Variation in Man*, 1.

[5] Howells, *Cranial Variation in Man*, 3.

[6] Howells, *Cranial Variation in Man*, 3.

or "limited to a single tribe or general community."[7] Such representative series came, for example, from a small mountain village in Austria or a "well-defined" Aboriginal tribe in South Australia. Each sample, Howells argued, thus represented an "actual tribal population or ethnic group, not simply a specified geographic region."[8] The selection of skulls for his research, however, relied on pre-existing collections and classifications: Howells visited several anatomy departments, museums, and anthropological institutes that housed cranial collections that were ordered and classified by anthropologists in previous decades, including the *Musée de l'Homme* in Paris,[9] the National Museum of Natural History at the Smithsonian Institution in Washington, D.C.,[10] the Anatomical Institute of the University of Oslo,[11] and the Field Museum in Chicago.[12] What's more, Howells acknowledged that the final selection of crania for his series often depended on the "expert knowledge" of the "host" collection curators. He visited, for instance, the Duckworth Laboratory at Cambridge University, where he measured the "well known 'Egyptian E' series of the Biometric Laboratory under Karl Pearson," now curated by Morant's friend and physical anthropologist Jack Trevor.[13] At Cambridge, Howells checked his sex assignments, obtained through morphological observation, against the original working notebooks and measurements from the biometricians.

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7 Howells, *Cranial Variation in Man*, 6.

8 Howells, *Cranial Variation in Man*, v.

9 Alice Conklin, *In the Museum of Man: Race, Anthropology, and Empire in France, 1850-1950* (Ithaca, NY: Cornell University Press 2013).

10 Samuel J. Redman, *Bone Rooms. From Scientific Racism to Human Prehistory in Museums* (Cambridge, MA: Harvard University Press 2016).

11 Jon Røyne Kyllingstad, *Measuring the Master Race: Physical Anthropology in Norway, 1890-1945* (Cambridge, UK: Open Book Publishers 2014).

12 Tracy Teslow, *Constructing Race: The Science of Bodies and Cultures in American Anthropology* (Cambridge, UK: Cambridge University Press 2014).

13 Howells, *Cranial Variation in Man*, 14.

The majority of these collections had already been measured by racial researchers in previous decades. Nevertheless, Howells felt urged to remeasure every skull himself to ensure the uniformity of the data, using Martin's methods and those developed in Pearson's Biometric Laboratory, rather than relying on published data.[14] During six months of continuous travel in 1965, he took 70 measurements per skull with calipers while his wife recorded over 100,000 numbers. The Harvard Computing Center carried out the computing of this data. On IBM computers, Howells ran the data through already-existing programs such as *Multiple Statistical Analyzer,* but also used software that the Center's staff had specially developed for him, such as a program to compute angles from the measurements. Howells noted that he "did a good deal of programming and manipulation of the data" himself, "largely to ride herd on the data myself, so as to be in real touch with it and to feel assured that the results I was getting were what I intended, and what I believed them to represent."[15] Making the study of human variation more objective thus required some personal involvement, some form of subjectivity.

With 16 discriminant functions to reveal between-group variation and factor analysis to explore within-group variation, Howells calculated the differences and similarities between the samples he had collected. He concluded that population differences were extensions of individual differences and that there was a "close association between modes of individual variation and patterns of population difference."[16] While much more can be said about this publication, its partial switch from "races" to "populations," and the use of multivariate statistics in physical anthropology, we are

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[14] In previous studies, Howells did reuse old anthropological data. See, for instance: W.W. Howells, "The Cranial Vault: Factors of Size and Shape," *American Journal of Physical Anthropology* 15:1 (1957) 19–48. Here, Howells used data and contours of English crania produced by Biometric Laboratory workers Brenda Stoessiger and Geoffrey Morant in the early 1930s for the article "A Study of the Crania in the Vaulted Ambulatory of Saint Leonard's Church, Hythe," *Biometrika* 24:1/2 (1932) 135–202.

[15] Howells, *Cranial Variation in Man*, 39.

[16] Howells, *Cranial Variation in Man*, vi.

able to determine that Howells's research was not a big departure from the prewar research discussed in the chapters of this dissertation. First, Howells's work displayed a continued belief in the stability of cranial form, an "old assumption of anthropologists." Howells wrote that "it is highly likely – though this cannot be demonstrated now – that important aspects of cranial form … are genetically persistent over considerable periods of time."[17] With cranial data, researchers were better able to "furnish objective statements in population comparisons and estimates of distance," than with data from living bodies or blood groups, research areas that were also gaining ground.[18] Indeed, he concluded that "whatever the agencies causing differences of cranial form in modern man, these differences have long been assumed to be deep-seated and relatively slow to change, and there is no indication to the contrary in this study."[19] The study of skulls of people living in all corners of the world thus continued to be seen as scientifically valuable.

Howells's conclusions on population distances were not far removed from common anthropological assumptions. Taxonomically, his research "seems to state clearly and objectively that the mutually most distinct living populations of man (with respect to their crania) are sub-Saharan Africans…the Australian-Melanesian group, and the Siberian "Mongoloids."[20] Nothing new here, he admitted: the clustering obtained through the *Multivariate Statistical Analyzer* program gave "a picture which is almost banal in its conformity to standard ideas of race and geography: Africans, Australoids, Caucasoids and Mongoloids; or preferably, a coherent arrangement by geographical areas" (fig. 6.1).[21] He asked whether "dark-skinned" really was a major branch "in actuality," since it had been created

[17] Howells, *Cranial Variation in Man*, 4.

[18] Howells, *Cranial Variation in Man*, 4-5. See for more on the development of the study of blood and race: Jenny Bangham, *Blood Relations. Transfusion and the Making of Human Genetics* (Chicago: University of Chicago Press 2020).

[19] Howells, *Cranial Variation in Man*, 153.

[20] Howells, *Cranial Variation in Man*, 68.

[21] Howells, *Cranial Variation in Man*, 61.

statistically. "But the people in it are in fact opposed to the other, rather heterogeneous grouping consisting of Europeans, Siberians, Hawaiians, and Eskimos."[22] His findings were indicative that "we would be at least as justified in dealing in 'cranial races' as in 'blood group races' or other traditional forms of classification."[23]

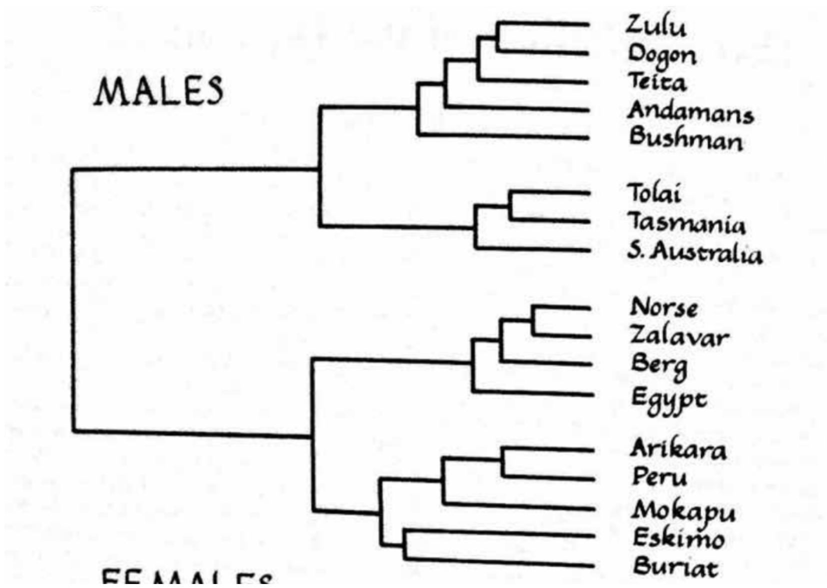

Figure 6.1. Clustering reveals common racial-geographical patterns. Source: William White Howells, *Cranial Variation in Man: A Study By Multivariate Analysis of Patterns of Difference Among Recent Human Populations* (Peabody Museum of Archaeology and Ethnology, Harvard University Cambridge, MA 1973) 62.

The classification of the Egyptian E-series collected by Flinders Petrie and Pearson didn't surprise Howells at all: "basically European but converging on sub-Saharan Africans either through genetic contribution or environmental adaptation."[24] These conclusions were quite in line with those of Fawcett and Lee who had determined 70 years earlier that the Naqada skulls "in some characters … resemble the Negro, in others the European."[25]

22 Howells, *Cranial Variation in Man*, 68.

23 Howells, *Cranial Variation in Man*, 153.

24 Howells, *Cranial Variation in Man*, 155.

25 Cicely D. Fawcett and Alice Lee, "A Second Study of the Variation and Correlation of the Human Skull, With Special Reference to the Naqada Crania," *Biometrika* 1:4 (1902) 464.

What then was the point of this "long technical trip," compared to simple statements based on visual inspection, or a modest list of selected mean measurements? Howells stressed that multivariate variables picked out and described the morphological patterns that differentiated "actual populations." This was contrary to older practices in which researchers selected cranial characters *a priori* and used the data to produce races "dei ex machina without corroboration from the skulls themselves that the indices really located the places where the primary differences lay."[26] In this study, however, measurements were picked based on their relevance for the study, and "no assumptions are made beforehand as to grouping the populations, or as to which are most extreme. *All the above arises from the data*" and not the interpretation of the worker.[27] And yet, Howells determined that hardly any of the measurements commonly used in physical anthropology could be dismissed based on their relative value through multivariate means. His research even revealed that skull breadth turned out to be of "prime importance in population differences."[28]

Even though Howells used more advanced quantification and automation technologies than the pre-war researchers discussed in previous chapters and had largely shifted his language from "race" to "populations," his research continued to tout the importance of skulls in classifying human variation. Rather than dismissing such efforts altogether, Howells, like Pearson and others before him, simply attempted to make classifications more precise and accurate. Indeed, "objectivity" had now become part of the anthropologist's vocabulary. The skulls dug up from graves in earlier times had gotten a new afterlife, once again poked with calipers, reduced with statistics, and now incorporated into computer databases.

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26 Howells, *Cranial Variation in Man*, 151.

27 Howells, *Cranial Variation in Man*, 151. Italics mine.

28 Howells, *Cranial Variation in Man*, 151.

The skulls' journey did not end there: Howells's cranial data connects us to the present. By 1995, the scientific landscape looked different. First, Howells explicitly rejected race, writing that "there are no races, there are only populations."[29] Second, the internet had become more widely available to researchers. Howells posted a 2-page note in the *American Journal of Physical Anthropology* in 1996, announcing that his entire database of cranial measurements, including those of *Cranial Variation in Man*, was now available on the internet in compressed zip files via a hyperlink. For 25 years, Howells had shared his data upon request and had been surprised and gratified to see what other researchers had been able to do with the material. Putting the data online would facilitate the continued reuse with new technologies, insights, and approaches.[30]

Physical anthropologists Stephen Ousley and Richard Jantz of the Anthropology Department at the University of Tennessee, Knoxville had made the arrangements for moving Howells's data online. This was a "particularly satisfactory arrangement," Howells wrote: the department was well equipped to handle his data because they already managed the *Forensic Data Bank*.[31] Ousley and Jantz had launched the *Forensic Data Bank* (FDB) in the mid-1980s and the accompanying software *ForDisc* in the early 1990s. They developed the database and the program in order to automate the metric estimation of sex, ancestry, and stature in forensic research. With discriminant function analysis, *ForDisc* compares the measurements of an unknown skeleton to reference samples in the FDB.[32] Ousley and Jantz initially developed the program for solving American forensic cases but it has now

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[29] W.W. Howells, *Who's Who in Skulls: Ethnic Identification of Crania from Measurements.* Papers of the Peabody Museum of Archaeology and Ethnology 82 (Cambridge, MA: Harvard University 1995) 103.

[30] W.W. Howells, "Notes and Comments: Howells' Craniometric Data on the Internet," *American Journal of Physical Anthropology* 101 (1996) 441–42.

[31] W.W. Howells, "Notes and Comments," 441.

[32] Stephen Ousley and Richard Jantz, "Fordisc 3 and Statistical Methods for Estimating Sex and Ancestry," in: Dennis Dirkmaat (ed.) *A Companion to Forensic Anthropology* (Malden, MA: Wiley-Blackwell 2012) 311-329; Richard Jantz and Stephen Ousley, "Introduction to Fordisc 3," in: MariaTeresa Tersigni-Tarrant and Natalie Shirley (eds.) *Forensic Anthropology. An Introduction* (Boca Raton: CRC Press 2013) 253-269.

become popular across the world. Today, anthropologists and archeologists in crime labs and universities around the world use the software, which is running in its third version.[33]

The FDB now contains skeletal information of over 3000 individuals. It includes historical collections such as those amassed by American anthropologists Hrdlička, Terry, and Wingate Todd, who we may remember from the American standardization committee discussed in chapter 5. It also contains Howells's worldwide skull data, including the data of Pearson's Egyptian E-series. By incorporating individuals from around the world in the database, the program now provides access to worldwide reference groups and further opens up *ForDisc* to international use. Jantz and Ousley suggest using Howells's worldwide data when there is suspicion that the skull originates from outside the United States or when the context of a found skull is lacking, in which case the Howells database provides a larger framework against which to compare it.[34] Future developments of *ForDisc* include expanding its data on worldwide populations and offer more classification methods, such as machine learning methods.

These are the afterlives of the skulls, methods, and data that center in this dissertation. By following their journeys, such as the one discussed above, we begin to see longer trajectories and more subtle developments of the history of biometry and quantifying race. Histories of the origin of the datasets in use today show unexpected connections and raise new questions about the reuse of old data.[35] In today's Big Data world, deep histories of data can be considered a different type of deep learning. We need histories of data to recognize the durability of historical patterns and problems.

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[33] Stephen Ousley and Richard Jantz, "Fordisc 3," *Rechtsmedizin* 23 (2013) 98.

[34] Richard Jantz and Stephen Ousley, "Introduction to Fordisc 3," 263.

[35] See for some other important examples that have shaped my thinking: Jenny Reardon and Kim TallBear, ""Your DNA is History" : Genomics, Anthropology, and the Construction of Whiteness as Property," *Current Anthropology* 53:S5 (2012) S233–45; Ann M. Kakaliouras, "When Remains Are "Lost": Thoughts on Collections, Repatriation, and Research in American Physical Anthropology," *Curator: The Museum Journal* 57:2 (2014) 213–23; Joanna Radin, ""Digital Natives": How Medical and Indigenous Histories Matter for Big Data," *Osiris* 32:1 (2017) 43–64.

Race, Power, and Biometric Vision

I argue that the researchers of this dissertation enacted race in practice by turning skulls into data. Rather than asserting that race was pre-given or a fact of biometric research, I build on medical anthropologist Amade M'charek's notion that race is a relational object. M'charek explains that race is enacted *in practice*, which allows us to move beyond the discussion of whether race is a biological reality or a social construction. As a relational object, race is *made* into a biological fact in relation to other things and objects. "Race does not materialize in the body, but rather in relations established between a variety of entities, including bodies," M'charek writes.[36] This dissertation shows that race was rendered a reality through quantification technologies. Physical anthropologists and biometricians produced its presence in the lab and the field with calipers, calculators, and statistics. Race, then, was made statistically significant in biometric practice. Even when the data was lacking, race was still enacted, as Morant's UNESCO pamphlet shows. By claiming that race was present in the data, or would be so in the future, researchers disavowed responsibility for their making of race. Quantification and automation held the promise of an objective gaze from nowhere, but the labor of transforming skulls into data was situated and embodied.

Vision played an important role in the enactment of race. The practices of making racial data involved the inspection of skulls with a "trained eye," the reading of instruments, and the careful peering of measurements, statistical constants, and calculating machines. These quantification technologies made race present and *visible* in biometric practice. Researchers *saw* race in the skulls and in the data. In understanding the coupling of race, objectivity, and vision, Donna Haraway's seminal essay "Situated Knowledges" may offer some insights. Haraway argues that disembodied scientific objectivity is an ideology, a doctrine that promises transcendence. She explores this through the

[36] Amade M'charek, "Beyond Fact or Fiction: On the Materiality of Race in Practice," *Cultural Anthropology* 28:3 (2013) 434.

metaphor of vision and insists on the embodied nature of all vision: "the gaze that mythically inscribes all the marked bodies, that makes the unmarked category claim the power to see and not be seen, to represent while escaping representation."[37] Visualizing technologies offer infinite opportunities to enhance the biological eye with "prosthetic devices," such as artificial intelligence-linked graphic manipulation systems and surveillance systems. "Vision in this technological feast becomes unregulated gluttony," she writes. Visual technologies are presented as transparent, but in fact build on "specific *ways* of seeing."[38]

Haraway's insights not only help us better understand how biometricians in the past enacted race in practice while claiming transcendent, disembodied vision; they are also relevant for today's biometric-Big Data landscape and may help explain why historical racial typologies and biases have resurfaced in biometry. The analytical features of today's biometric technologies are connected to historic methods of visualizing and quantifying faces and skulls through landmarks, planes, and geometry (fig. 6.2). In fact, various studies and technologies rely on the *same* linear distance measures that anthropologists used in previous decades.[39] And like the quantification efforts discussed in this dissertation, developers trust these technologies of biometric vision to be objective but revive racial biases in their design. Biometric technologies mediate long-standing practices of *making race visible* in bodies, faces, and skulls, of *enacting race*. Biometric vision is subjective and embodied all the way through.

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[37] Donna Haraway, "Situated Knowledges: The Science Question in Feminism and the Privilege of Partial Perspective," *Feminist Studies* 14:3 (1988) 581.

[38] Donna Haraway, "Situated Knowledges," 581; 583.

[39] Peter Claes et al, "Genome-wide mapping of global-to-local genetic effects on human facial shape," *Nature Genetics* 50 (March 2018) 414-423.

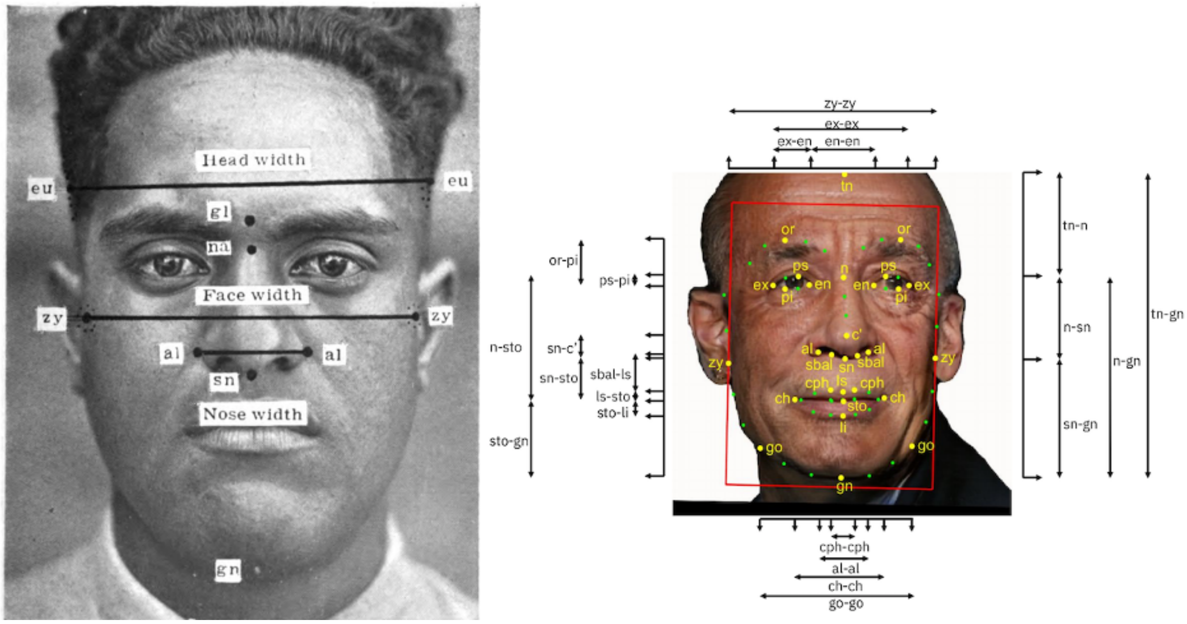

Figure 6.2. Facial landmark measurements in 1923 and 2019. Source left: Louis Sullivan, *Essentials of Anthropometry*) (American Museum of Natural History 1923) 23. Source is in the public domain. Source right: M. Merler et al, "Diversity in Faces," IBM Research AI version 6, *ArXiv:* 1901-10436 [Cs] (2019) 10.

M'charek's most recent work reinforces these perspectives. She reveals that scientists today invoke older measuring practices of physical anthropology and racial science in studying faces and skulls. Thus the "visual conventions and measuring practices from colonial times" live on in contemporary technologies such as facial recognition and DNA phenotyping. In doing so, researchers risk reintroducing old racial typologies and biases.[40]

Seeing, measuring, and analyzing faces and skulls continue to be situated practices, infused by power. Haraway reminds us that "vision is *always* a question of the power to see – and perhaps the violence implicit in our visualizing practices. With whose blood were my eyes crafted?"[41] The history of biometric technologies and data discussed in the preceding chapters matters in answering this

[40] Amade M'charek and Katharina Schramm, "Encountering the Face – Unraveling Race," *American Anthropologist* 122 (2020) 1-6; Amade M'charek, "Tentacular Faces: Race and the Return of the Phenotype in Forensic Identification," *American Anthropologist* 122 (2020) 1-12. See also Abigail Nieves Delgado, "The Problematic Use of Race in Facial Reconstruction," *Science as Culture* 29 (2020) 1-26.

[41] Donna Haraway, "Situated Knowledges," 585.

question. Feminist scholar Jacqueline Wernimont has stressed that "there are no data, tracking opportunities, algorithms, or patterns without bodies."[42] Expanding on that, we should add that there are no skulls for biometric research without death.

[42] Jacqueline Wernimont, *Numbered Lives: Life and Death in Quantum Media* (Cambridge, MA: MIT Press 2019) 4.

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