Chapter 2
The Corpus

2.1 Prelude: The Composers' Skulls

After his death on March 26, 1827, Ludwig van Beethoven's temporal bones were extracted with surgical imprecision from his skull (Meredith 2005; Thayer 2013 [1921]: 309–312; von Breuning 2005 [1886]). Though autopsy of a celebrated figure was not an unusual post-mortem event at the time, the removal of these particular skeletal elements seems to have been a departure from standard medical practice. The composer's death mask appears disfigured as a result. Inspection of Beethoven's body did little to clarify his cause of death. Nevertheless, attendant physicians remained hopeful that the auditory organs would yield information about his musical abilities and deafness. Beethoven was then inhumed in Währing Cemetery on the northwestern outskirts of Vienna. There was much public mourning. Amongst the aggrieved was Franz Schubert, a fellow Viennese composer who idolized Beethoven. Less than two years later, and at his request, Schubert was buried a few graves away (von Breuning 2005 [1886]). Neither man, however, rested peacefully.

In 1863, both composers' bodies were exhumed and examined. They were mostly skeletonized, saved for Schubert's preserved hair [decidedly ironic seeing that some years prior to his death he had lost it during an undiagnosed illness (Solomon 1989: 203–204)]. Their forensic analysis provides an important precursor to a bioarchaeological approach, which has sought to contextualize remains and explore the complex intersection of biology, inheritance, and culture. The event was of interest to anthropologists and anatomists alike, for at this juncture the line between natural history and medicine was a fine one. All of these scientists trained formally at a selected number of European and American medical schools. And a fascination with crania was widespread. The individuals who analyzed Schubert's and Beethoven's disinterred remains were no exception. Gerard von Breuning (2005 [1886]: 59) recounted,

P.L. Geller, *The Bioarchaeology of Socio-Sexual Lives*,
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> The main goal was, of course, the retrieval of the skull…and [it] aroused the highest interest among phrenologists and lay person alike because of the striking difference between the two skulls. They seemed to reflect the characteristics of the composers' works. The walls of Beethoven's skull exhibit strong density and thickness, whereas Schubert's bones show feminine delicateness.

Von Breuning's statements offer ingress to several ideas explored in this chapter. For physicians in the nineteenth century, the epistemological and ontological norm was to view bodies as partible. Fragmentation was a central element of the biomedical bodyscape that took root during this period (see Introduction). Yet, rather than transforming subjects wholly into objects, the scientific study of bodies' parts established a subject–object dialectic. The skull, for instance, was an object often fetishized for the information European and Euro-American scientists thought it could reveal about a subject's race, gender, class, or sexuality. Consequently, its historic analysis counters claims of science's unfailing neutrality and objectivity.

In the case of racialized identities, the craniometric studies of numerous physicians—the American Samuel Morton, Scot Robert Knox, French Paul Broca—represented populations' differences as natural, bounded, and ranked (Geller 2015; Gould 1981). From scientific data, inferences were then drawn about Caucasians' superior intellect and people of color's inferiority. Similarly, certain Caucasians could be racially Othered and denigrated by assigning non-Caucasian attributes to their crania. Such was the case for Schubert. His biographer Heinrich Kreissle von Hellborn listed a suite of stereotypical black attributes to describe the composer's visage: "His round and puffy face, low forehead, projecting lips, bushy eyebrows, stumpy nose, and short curly hair gave him that negro look" (1865: 223).[1] The portrayal suggests that Kreissle von Hellborn was not too fond of his subject and was familiar with the anthropological studies of the day (Gramit 1993: 71). On the matter of Beethoven's skull, however, he is silent, most likely because his whiteness and maleness represented the standard against which Others were measured.

As historians tell us, and as I review here, nineteenth century physicians regarded race and sex as analogous. Thus, scientific studies also reinforced white males' position at the top of the social hierarchy (e.g., Schiebinger 1993; Stepan 1986). Like racial features, the physical attributes of sex held social significance. The "feminine delicateness" that Schubert's cranium displayed, for instance, confirmed widely held sentiments about the composer's music; its effeminacy was substandard to the power and intelligence of Beethoven's more masculine oeuvre (Gramit 1993). He possessed "a feminine character, much more voluble, softer and broader; or a guileless child romping among giants," so claimed an 1838 critique of his "Grand Duo" (Schumann as cited in Gramit 1993: 72). "Schubert conducts

[1]The quote is from the English translation of Kreissle von Hellborn's biography. The original German is as follows: "Sein rundes, dickes, etwas aufgedunsenes Gesicht, die niedere Stirn, die aufgeworfenen Lippen, buschigen Augenbrauen, die stumpfe Nase und das gekräuselte Haar, gaben seinem Kopf ein mohrenartiges Aussehen" (Von Hellborn 1865: 466).

himself as wife to husband," continued the reviewer, "the one giving orders, the other relying upon pleas and persuasion." Since this time, scholars have debated Schubert's sexual predilections and the degree to which homoeroticism played out in his music (e.g., Kramer 1998; Solomon 1989). The investigative focus is born from an understanding of sex, gender, and sexuality as interchangeable concepts, an idea that also has nineteenth century origins (see Chap. 3).

Following their disinterment and examination, Schubert's and Beethoven's remains were placed into refurbished brick vaults. Twenty-five years later, on 21 June 1888, they were removed a second time, hastily studied, and reburied adjacent to each other in Vienna's Zentralfriedhof. The run-down state and inevitable re-purposing of Währing Cemetery had made it an unsuitable burial ground (von Breuning 2005 [1886]: 58). Interestingly, the consecrated space that had originally held their remains was renamed in honor of its less revered musical prodigy; today it is Schubert Park.

While their extended mortuary treatment indicates that scientists recognized the composers' subjectivity, there are aspects of these events that hint at their objectification. Two large pieces and eight small fragments of Beethoven's cranium came into the possession of anthropologist Romeo Seligmann (Meredith 2005: 9). There is little to suggest that fragmentation was perceived as a desecration. Indeed, von Breuning would later make a case in the interests of science for open-ended access to Schubert's and Beethoven's skeletons. "Only highly prejudiced people (who are unfortunately in the majority) would be offended by this course of action; any person with scientific training would certainly not object," he (2005 [1886]: 60) declared. Of course, the statement also indicates that von Breuning and colleagues were unaware of the ideological beliefs and sociopolitical circumstances that informed their own research queries and conclusions.

The scientific study of and inferences drawn from Beethoven's and Schubert's skulls are by no means idiosyncratic. Rather, in this chapter, I use their death histories as a prelude to a more thoughtful discussion about biophysical evidence and the production of knowledge in bioarchaeology. The hard and bony facts are emphasized, as is sex's link to race. Prior to tracking the determination of sex and the meanings made from scientific data, I first deliberate about objectivity. Objective science—science that is rational, value-free, democratic—is ideal science. The scientific study of humans' sex differences, however, has been anything but. To advance knowledge, analysts sought sufficient comparative samples, and their anatomical collections grew as a consequence of colonial necropower. The inferences they then drew from biophysical evidence, which has become increasingly atomized with time and technoscientific advances, have often been informed by heteropatriarchal ideas about gender and sexuality.

2.2 Scientific Objectivity

2.2.1 Vienna's Jews

The details of Beethoven's and Schubert's death histories offer a pointed contrast to those Jews buried at Währing Jüdischer Friedhof, just two kilometers to the north of Währing Cemetery. Währing's Jewish cemetery was opened in the 1780s to accommodate the district's expanding Jewish community. By the late-nineteenth century, inhumations had ceased. But, Vienna's Jews continued to regard the space as a hallowed burial ground, which was one reason for its desecration in 1942. Scientific study of racial difference provided an additional justification. That summer, anthropologists from the Museum of Natural History, most of whom were affiliated with the National Socialist party, unceremoniously disinterred approximately 200 Jewish decedents (Teschler-Nicola and Berner 1998: 4–5). Exhumations continued into spring 1943. And as an aside, there is something tragically poetic about the fact that Franz Boas, who had fled Germany for America because of anti-Semitism and then worked so assiduously to debunk racial anthropology's key interpretations (e.g., Boas 1912), would die just a few short months after the Jewish cemetery's destruction. (These twists of fate may be no more than coincidence, but they are nevertheless provocative heuristically.)

The happenings at Währing Jüdischer Friedhof generated little protest. Of the 200,000 Jews who made Vienna their home prior to 1939, around half were forced to emigrate. For those who remained in the city, systematic deportation to concentration camps resulted in the murder of some 65,000 Viennese Jews (Bunzl 2004: 22, fn. 98). The misfortune of these many Others proved scientifically opportunistic for some. The Museum of Natural History was able to expand its "collections with 'material' that was previously impossible to get" (Berner 2010: 28). The desecration of the dead provided a particularly powerful symbol of the Nazis' intent to eradicate all traces of Jewish existence. Collected skulls were then analyzed for information about cranial capacity, morphology, and pathology (Steinweis 2006: 59). From these data, scientists lent support to the idea that Jews were a distinct and lesser race. While the conclusions were not new, they did demonstrate that racism under the guise of objective craniometric studies was still analytically viable. Scientific racialization bolstered Nazi ideology about the Jews' inferiority, which in turn provided political justification for their extermination. Nazi scientists did not regard their work as overtly political, however. According to Proctor (1993),

> Authoritarian science based on the "Fuhrer principle" replaced what had been, in the Weimar period, a vigorous spirit of politicized debate in and around the sciences. The Nazis "depoliticized" problems of vital human interest by reducing these to scientific or medical problems, conceived in the narrow, reductionist sense of these terms.

Fascism's appropriation of anthropometric studies during Germany's inter-war years and WWII should give us pause about science's objectivity [for discussions about the Nazis' (ab)use of the archaeological record see Arnold (1990)]. Of course,

I do recognize that in raising the specter of Nazi Science, my example is an extreme and often cited one. Yet, was it an aberration, we need not call into question scientific neutrality more generally. Racism has historically informed the determination of race from human remains, as many scholar have discussed (e.g., Blakely and Harrington 1997; Blakey 1987, 1998; Fabian 2010; Geller 2015; Gould 1981; Kakaliouras 2008; Marks 2009; TallBear 2003; Thomas 2000). As a consequence, purportedly "objective" and mainstream scientific studies have served to racialize and denigrate Others. Such is the reason that the majority of anthropologists today regard race as a sociocultural construct.

Describing race as a construct is not to deny observable biological facts, physiological processes, or quantifiable differences—it is not to suggest the body is a "blank page for social inscription" (Haraway 1988: 591). Rather, the designation recognizes that science is never pure and commonsensical ideas about biological differences are "made" to matter, as opposed to being objectively discovered or found. "What the sciences actually observe is not bare nature but always only nature-as-an-object-of-knowledge—which is always already fully encultured," Harding (1992a: 575) cautions. And what biophysical evidence is made to matter then informs a society's representations of and responses to social persons. Histories of science teach us this project is rarely if ever a neutral one. Hence, the importance of applying critical social theories to analyze categories tied to the constitution (or imposition) of social identities. In so doing, we may interrogate the limits of scientific objectivity and expose inequities that arise from their naturalization. These observations are no less pertinent to the bioarchaeological study of sex and gender.

2.2.2 Monte Albán's Tomb 7

In 1932, archaeologist Alfonso Caso and colleagues discovered Tomb 7 beneath the patio of an elite residence at Monte Albán, a pre-Columbian political center in Mexico's Valley of Oaxaca (Caso 1969). Grave goods, around 500 in total, included the exotic and precious. Though constructed in the Classic period, the tomb continued to be reused well into the Postclassic period, which complicated interpretations. Caso (1969: 50, 55) proposed that reentry disturbed some of the original occupants whose remains were then reinterred as secondary burials. The grave goods, he went on to argue, were associated with "esqueleto A," a flexed and seated burial bundle. Skeletal analysis conducted by Rubín de la Barbolla (1969: 279) identified this individual as a male who died around the age of 55–60 years old. Rubín de la Borbolla remarked upon the fragmentary condition of the skeleton and its missing pelvis. And despite distorting cranial pathology, he used the skull to assess the individual's sex.

Three decades later, Geoffrey McCafferty and Sharisse McCafferty returned to Caso and colleagues' data. Diligent documentation was one reason they were able to do so. Skeletal reanalysis did little to clarify the decedent's sex, however.

McCafferty and McCafferty (1994b, 2003) tentatively suggested that "esqueleto A" was biologically female based on a "female" mandible near the skull and the presence of a gracile patella. The mandible appears on the inventory but is labeled "intrusive." Grave goods proved more insightful. The presence of a spinning and weaving tool-kit indicated to McCafferty and McCafferty that "esqueleto A" was not a male but an individual of "female gender identity," either a religious figure (1994b) or a *cacica* (2003). In support, they cited copious historic, artistic, and archaeological evidence from pre-Columbian and Colonial periods that attested to the symbolic and practical link between Mesoamerican women and weaving. Ultimately, McCafferty and McCafferty concluded, androcentric bias had prevented Caso and colleagues from seeing the extraordinary Tomb 7 as the mortuary space of a potential female who was politico-religiously powerful.

Their reassessment may hardly strike archaeologists as implausible some three decades after critical feminism's incorporation into the sub-field. That powerful women lived and died throughout pre-Columbian Mesoamerica is now well documented (see Chaps. 5 and 6). When published, however, McCafferty and McCafferty's reanalysis was regarded as "very provocative" (Brumfiel 1994: 153) and "breaking new ground" (Costin 1994: 155).

Yet, not all scholars were so quick to praise their efforts. Flannery and Marcus (1994) were especially troubled by McCafferty and McCafferty's characterization of Caso as androcentric. "Dismissing him as some kind of antediluvian male-chauvinist pig is thus the first of the McCaffertys' errors," they rebuffed (1994a: 141). Their comment suggests they did not recognize the feminist critique of Tomb 7 for what it was—an effort to make visible the persistent structural androcentrism in archaeologists' reconstructions of the past and not the disparagement of a single individual or his scholarship (McCafferty and McCafferty 1994a). More recently, Flannery (2006: 9) has reiterated his discontent with the reanalysis in an *Annual Review of Anthropology* piece.

> And by the year 2000, a lot of serious, empirically grounded archeologists were getting tired of seeing fairly limited, sometimes even mediocre, field data "enhanced" by the addition of postmodern phrases…We had seen a mute, 600-year-old skeleton described as "biologically a robust male, but gender female."

In returning to McCafferty and McCafferty's reassessment of Tomb 7, there are certainly shortcomings with which we may take issue. For instance, their description of "esqueleto A" as "gender-female" or having a "female gender identity" does lack semantic nuance. That is, their language inadvertently conflates sex and gender. And, as Gero (1994) suggested in her original comments, the tomb's ambiguity may speak to a gender identity that is not easily understood as dichotomous or monolithic. Her suggestion anticipates socio-sexual lives that are intersectional and far queerer. But, Flannery's insinuation that McCafferty and McCafferty's reanalysis is postmodern, anti-scientific, empirically unfounded, and politically correct is as intellectually reductive as it is misrepresentative. Critical reflection exposes conceptual deficiencies—androcentric and racist biases, for instance—that are long regarded as "good" even "exemplary" science (Wylie 2002:

190). In so doing, it fosters more objective science. To not address deficiencies and inequalities at the level of the structure serves to defend and legitimate "the institutions and practices through which the distortions and their exploitative consequences are generated" (Harding 1992a: 568). Hence, McCafferty and McCafferty were compelled to clarify that their feminist critique was directed at archaeological practice more generally and not Alfonso Caso specifically. The likelihood is that many scientists who see this work as unnecessary, who idealize scientific neutrality, already benefit from certain unacknowledged, sociopolitical privileges.

To be clear, I am not suggesting that researchers who study the human body for information about social identity should eschew scientific objectivity. They should not. They should, however, recognize how difficult it is to attain the ideal (Gowland and Thompson 2013: 3). To cultivate scientific objectivity in one's research, scholars inspired by emancipatory politics have developed productive strategies. Influential have been statements made about standpoint theory by philosopher of science Sandra Harding (e.g., 1986, 1992a, b, 1995, 2004), though she is not the only one to have contributed to thoughtful discussion on the subject (e.g., Collins 1986; Hartsock 1983; Smith 1974). In brief, the approach insists that research start from an outsider's standpoint, from the vantage of socio-sexual lives marginalized by dominant modes of science. These "situated and embodied knowledges," in the words of Haraway (1988: 583) reveal power's production of knowledge, generate new queries, and present alternative ways of knowing. In her most recent statement on objectivity and diversity, Harding (2015) has deliberated about moving theory to methodology. For example, to include indigenous (or local) knowledge systems extends understanding of human-ecosystem interactions that are ecologically and socioeconomically sustainable, as well as symbolically framed. One case she discusses is Canadian Cree geese hunting. Citing anthropological work conducted by Colin Scott, Harding (2015: 85) explains,

> The Cree hunters of James Bay, Canada…have developed hunting principles and practices that are successful at maintaining the supply of geese…This requires that they also maintain the necessary environments to attract the geese…The geese give the hunters the gift of themselves only when the hunters demonstrate respect for the geese, as well as recognition both of the geese's distinctive environmental needs and their fears.

The lessons are significant ones, not just for what they can tell us about Western science's own practices and metaphors—the culturalization of the natural—but what we may also learn about the natural world's regularities. Additionally, and not inconsequentially, scientific practice that is inclusive may decolonize knowledge and promote social justice.

The sentiment, to make collaborative, accountable scientific practice normative disciplinary practice, is one that Wylie (2008) promoted in her Distinguished Lecture to the American Anthropological Associations' Archaeological Division. For her part, and of relevance to bioarchaeology, Wylie has countered claims of relativism with the idea of evidential constraints—"although archaeological data must be richly interpreted to stand as evidence, they do (sometimes) have a capacity to challenge and constrain what we claim about the past" (2002: 191; see also

Wylie 1992). What Wylie stresses is that our criticisms of scientific inquiry must address the empirical as much as they do the conceptual. This awareness is especially pertinent for investigators who collect biophysical evidence from bodies—about disease, development, difference. Take tuberculosis, for example. Paleopathological and biomolecular evidence can extend understanding of the disease's evolution, from its ancient emergence in local contexts to modern drug-resistant and global strains. In so doing, researchers need account for the confounding variables that may challenge analyses (e.g., age, health status). Yet, this epidemiological treatment is quite different from more anthropologically oriented queries that seek to understand culturally and historically salient experiences and meanings of the disease. We may bring the same attention to biological information about and techniques for discerning sex. Recognizing that our understanding of it as objectively determined, rigidly dimorphic, and constitutive of social identities is common sense in need of interrogation is a crucial first step.

2.3 Sexual Dimorphism

Humans are not very sexually dimorphic as far as primates go (Plavcan 2001). Compared to male gorillas who may be twice as large as their female counterparts, humans' body mass varies modestly between the sexes. Perhaps because of this dearth of extreme or obvious differences, researchers of human bodies have worked assiduously to develop analyses, ever finer grained in their scope, for distinguishing males from females. To this end, Western medical techniques for sex determination have shifted several times over the past 400 years—from a concentration on gross anatomical morphology to measurement to molecular testing. Technical changes have in turn led to new biophysical emphases, new ways to see sex. But, we would be remiss to regard these scientific advances as neutral or disconnected from their socio-historical settings. The differences made to matter—the knowledge that scientific discovery extracts from physiological process, skeletal element, bodily fragment—are laden with value. "We seem unable to escape," Marshall Sahlins (1976: 105) philosophized, "from this perpetual movement, back and forth between the culturalization of nature and naturalization of culture." In thinking about culturalization and naturalization, we may expand the scope to include sex. How we "make sex," per Laqueur (1990), speaks to the former. Remaining stridently wedded to the idea that rigid duality structures socio-sexual lives reflects the latter (see Chap. 5).

As Schiebinger (1986, 1989, 1993, 2003) has documented, the female form stripped of skin increasingly found its way into European medical texts and illustrations in the mid-eighteenth century. Inclusion of Her skeleton drew attention to morphological features that digressed from the male template. Anatomists did recognize that bipedalism had the most dramatic impact on the human pelvis's form. But, their attention then turned to observable differences between males and females. Physicians' descriptions suggest that the female pelvis was more remarkable on account of reproductive needs. For example, in the first edition of the

anatomical treatise he authored for medical students, Jones Quain (1828: 69), professor of anatomy and physiology at the University of London, is quite clear about pelvic differences.

> The size and conformation of the pelvis differ very remarkably in the two sexes. In the female, though its perpendicular depth is less, its breadth and capacity are greater. The alæ of the iliac bones are expanded; the upper aperture is more nearly circular, the projection of the sacrum less perceptible; and the space between the tuberosities of the ischia greater. The depth of the symphysis pubis is less in the female than in the male, whilst the breadth of the pubic arch is greater.

Sexing skeleton was by no means an objective endeavor, however. Schiebinger (1986) has argued that analysis of elements offered evidence for the "natural" order of social affairs. As such, information conveyed is about biological differences and gendered behaviors. For instance, the reproductive capabilities of female pelves ossified ideas about women's domesticity and maternalism, while smaller cranial capacities explained their lowly place in the hierarchy of human intelligence (Schiebinger 1986).

From bone to flesh, Laqueur (1990, 2003) continued with this train of thought. Anatomists' vision of sexual division, a two-sex model, gained traction during the Enlightenment. Prior to this, from the Classic period until the seventeenth century, a one-sex model predominated. The standard body being male, Renaissance medical documents following Galen represented female genitalia as inverted penises. Evidenced culled from cadavers' dissection failed to contradict this notion. In his medical text *De Humani Corporis Fabrica*, for example, the influential sixteenth century physician Andreas Vesalius presented female anatomy in terms of maleness. And he did so despite having dissected the cadavers of seven females. "Believing is seeing," Laqueur offers in explanation (1990: 79). The metaphysical foundations of Europeans' worldview, however, were increasingly becoming more unstable. "As cultural and political pressures on the gender systems mounted," Laqueur (2003: 306) later clarified, "a passionate and sustained interest in the anatomical and physiological dimorphism of the sexes was a response to the collapse of religion and metaphysics as the final authority for social arrangements." A fascination with "hermaphroditism" in the nineteenth century would have scientists and medical practitioners revisit their understanding of sex as rigidly dimorphic.[2] An emphasis on gonads, signifiers of compulsory reproduction and heterosexuality, reiterated their commitment (Dreger 1998).

Other scholars have since revisited Schiebinger's and Laqueur's influential statements to point out deficiencies (e.g., Cadden 1993; Park 2010; Park and Nye 1991; Stolberg 2003). But, their conclusions do not indicate that sex is any less a construct. Joan Cadden (1993: 3), for instance, argues that "the opinions of

[2]The term hermaphroditism is invoked here to capture the language used at a particular historic juncture. Activists and academics have discussed its conceptual shortcomings and derogatory meanings when used to describe humans' conditions. For an excellent explanation about its inadequacy as a modern referent see the Intersex Society of North America's FAQ page: http://www.isna.org/faq/hermaphrodite.

medieval physiognomers about male and female traits suggest evidence of other models not reducible to Laqueur's." That is, prior to the eighteenth century, numerous and competing understandings of sex co-existed. Katherine Park (2010) has more recently argued that the circulation of Galen's one-sex model does not indicate continuity but rather revival starting in the sixteenth century. Nor do critics dispute that a two-sex model continues to predominate in modern biomedical representations of bodily differences despite evidence to the contrary. And they do not disagree with identification of rigid and observable sex differences as instructive about socio-sexual norms, i.e., sexing is gendering. Bioarchaeologists should certainly take note of scholars' consensus that even Western ways of knowing the body are mutable, multiple, and entangled with sociopolitical circumstances.

As Schiebinger and Laqueur recognize, eighteenth century physicians who documented sex differences were primarily concerned with aesthetics. Anatomists represented ideals based on singular specimens or Western archetypes of beauty. Significantly, determination of sex from the skeleton did not begin with the unknown. The 1796 illustration by physician Samuel Thomas von Soemmerring (or Sömmerring) was one widely circulated example (Fig. 2.1). Its production speaks to the biomedical bodyscape's genesis (see Introduction). As professor of anatomy and physiology at the University of Mainz (1784–1797), Soemmerring's dissecting activities and anatomical collection likely gave him access to a plethora of potential models (Naragon 2010). He selected the post-cranium of a local 22-year-old female who had given birth. He then referenced the classical statues of the Venus di Medici and Venus of Dresden to perfect the illustration (Hildebrand 2005; Schiebinger 1986: 58). The cranium of a young Georgian female was the crowning touch (Hildebrand 2005: 563). While not entirely uncontroversial in its day, Soemmerring's female skeleton did present as biomedical standard, or norm, an amalgam of real fragments reconfigured and deemed ideal. Accordingly, descriptive texts and engraved image conveyed these differences as universal to other physicians and their students.

2.4 Anatomical Collections

The origin of Soemmerring's skull is not so much trivia. The anatomist requested to borrow the Georgian female from Johan Blumenbach, with whom he had studied medicine at Göttingen University (Naragon 2010). The skull, Blumenbach maintained (1865 [1795]: 300), was the most beautiful in his collection. That he consented to part briefly with it is suggestive of his close friendship with Soemmerring. Guiding Blumenbach's concern for morphological ideals was his research interest in racial differences. In seminal statements, he (1865 [1795]: 238) distinguished between five distinct races, arguing that even the skulls of infants possessed distinguishing traits. In support, he referenced his crania collection of 245 whole skulls and fragments. It would remain amongst the most sizeable until Samuel Morton began collecting in earnest some three decades later.

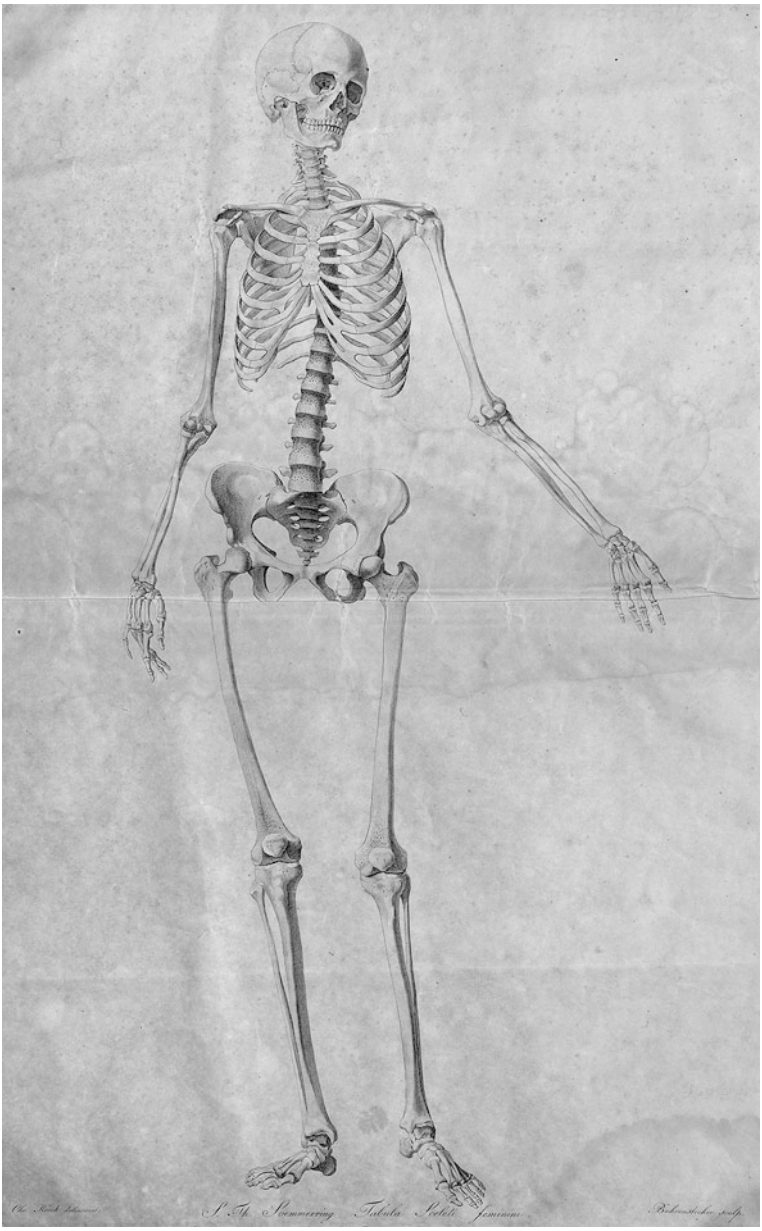

Fig. 2.1 *Tabula sceleti feminine*, Samuel Thomas von Sömmerring. Reproduced with permission of Bayerische Staatsbibliothek München, 2 Anat. 100 f, p. 9, urn:nbn:de:bvb:12-bsb00050911-2

As collections grew and became more demographically varied, comparative study became easier to undertake. Nevertheless, physicians continued to receive biographical information about collected crania well into the nineteenth century. Morton, for instance, began actively soliciting colleagues for remains in 1830 and continued to do so until his death in 1851; at this time his collection totaled 967 (see Chap. 5). Archival documentation indicates that individuals who answered his requests sent along information about decedents' ages, races, nationalities, social ranks, and/or sexes (Geller 2015). Knowledge of specimens' life histories allowed researchers to then narrate death histories that reifed many of their a priori ideas about socio-sexual lives. Morton (1839, 1849) assessed cranial capacity—so it went, the more voluminous the more intelligent—to hierarchically rank the races in his collection, for example.

The increasing fragmentation, measurement, and statistical assessment of the body signaled the inception and concretization of bio-power. The concept is one Foucault (1978, 2003) has invoked to explain the development of techniques for supervising and regulating bodies. Biomedicine's emergence signals the normalization of certain "techniques" in institutional settings like the hospital clinic, mental asylum, medical school, and scientific laboratory. All physicians received formalized training, experienced theatrical-like dissection, and learned from textbooks possessing depictions and descriptions of the ideal body. As an outcome they were granted "the right to make live" (Foucault 2003: 240). While the financial cost of medical school was a given—books, tuition, and room and board could be cost prohibitive for most—the human cost went unacknowledged. Medical schools' anatomical collections, for instance, grew as a consequence of colonial necropower. Thus, physicians were also implicated in "letting die." Certain lives, Foucault explained (2003: 247) are worth enhancing, proliferating, prolonging, while others must be eradicated or transformed beyond recognition.

Achille Mbembe's (2003) thoughtful treatment on death-making projects extends Foucault's focus on the technologies of life. Necropower, he has explained, is sovereignty's destruction of human bodies and populations, while necropolitics refers directly to the techniques used to destroy. Hence, physicians may have amassed skeletal remains in the name of scientific inquiry, but their efforts were made possible by violent military and exploitative economic encounters. It was only after death that decedents included in collections acquired a sort of social worth. Mbembe (2003: 18) describes this process as "the becoming-object of the human being"—the somewhat counterintuitive notion that in being transformed from a human subject to an investigative object, one can acquire a modicum of value.

Blumenbach, for instance, acquired "his" skulls as a consequence of imperial interventions in far flung places. The difficulty of doing so, he (1865 [1806]: 299) remarked in *Contributions to Natural History*, is "not insuperable when the collector shows zeal and perseverance and can obtain the active co-operation of men who have opportunities of helping him in his object." His anthropological study of crania proved justification enough, in his mind, for their unceremonious acquisition from foreign nations. The Georgian skull, though exemplary "for the extreme

elegance of its shape" (Blumenbach 1865 [1795]: 162), was no different. The young female to which it had originally belonged, was a victim of Russia's war with Turkey. In captivity, she had been transported to Moscow. Her premature death was followed by scientific dissection, and in due time, Blumenbach received her preserved skull. Its beauty and symmetry, he noted in *On the Natural Variety of Mankind*, contrasted with his Ethiopian, Malay, American, and Mongolian specimens—all of whom were acquired in equally insidious ways (Blumenbach 1865 [1795]: 237).

The cranium, however, was not the only body part physicians assessed for information about racial differences. The pelvis was a much sought after skeletal element for inclusion in anatomical collections (e.g., Monro 1825; Vrolik 1826; Weber 1830). For instance, in the medical textbook Alexander Monro *tertius* authored, he stated that female pelves were larger in all dimensions but height, while the diameter of Europeans' pelves exceeded that of Australasians, Negroes, and Negresses (Monro 1825: 81–85). The sweeping statements were based on measurements taken from a total of eight pelves. Perhaps more important than his scientific observations, however, was the influence Monro wielded. He was a professor of anatomy at the University of Edinburgh's medical school—his father and grandfather, who were also named Alexander Monro, had previously held the position. Granted, many attested to his utter lack of charisma in the lecture hall. Of Monro's teaching, Charles Darwin remarked that he "made his lectures on human anatomy as dull as he was himself," a sentiment that Robert Knox echoed (Bates 2010: 25). Regardless, his teachings about pelves' racial differences did not fall on deaf ears. Darwin and Knox counted amongst Monro's students as did Samuel Morton, Thomas Hodgkin, and countless others.

In an age of colonial enterprise and rigid dimorphism, nineteenth century anatomical texts did little to uncouple race and sex. That they did not is also informative about (hetero)normative representations of European sexuality as procreative, monogamous, and hidden. People of color, in contrast to their white counterparts, were considered promiscuous and public when it came to sexual activities (Fausto-Sterling 1995; Schiebinger 1993). While genitalia were scrutinized when available—were women of color comparable to female apes or Europeans?—anatomical collections were poor in pelves, a consequence of preservation as much as research foci. And physicians who had scrutinized the pelvis largely concentrated on female examples. Later analysts would lament the paucity of male pelves, for their fixed attributes and discernible differences were far more useful in developing racial classificatory schemas (e.g., Matthews et al. 1893; Turner 1885)

2.5 The Sexualized Skull

While few physicians disputed that skulls could be categorized by their racial differences, many conceded that sexing the cranium was more difficult. Female skulls were deemed lighter, smaller, smoother, and thinner. "The superciliary ridges

do not form so prominent a feature of the female skull," Monro (1825: 197) clarified, "and, according to some authors, the sagittal suture is more frequently continued down to the root of the nose." He went on to state, citing Soemmerring in support, that a fetus's cranium possessed sexual markers of difference (Monro 1825: 197). In a coeval and widely read text, however, Quain (1828) had nothing to say on the subject of sexing skulls. He instead stressed the pelvis's sexual dimorphism. Not until the anatomical text's seventh edition did he include a brief, if not dismissive, statement. "The female skull resembles the formed skull of the boy more than that of the adult male; but it must also be admitted that it is often impossible to determine the sex by the appearance or form of a skull," he advised (Quain 1867: 72).

Other researchers explained that the measurable imprint race made on crania complicated sex determination. European skulls were highly dimorphic, but the sexual differences of other races were harder to distinguish. According to Carl Vogt (1864: 9), a German naturalist who was also trained as a physician:

> In the more civilised races the difference is as great as between the skulls of the same sex in different races; and, as there is but little difference in this respect in the Negro and other inferior races the determination of the sex becomes more uncertain as we approach the inferior races of humanity.

Vogt's statements confirm methodological and theoretical shifts by the mid-nineteenth century. Scientists were less concerned with cranium's morphology and more with its measurement. Comparative study of facial angles and cranial capacity, a marker of intelligence or so it was believed, provided empirical evidence of a social hierarchy. White women were on (sub)par with inferior races, the skulls of non-white males signaled their feminized condition. Smaller skulls, whether a consequence of sex or race (or both), also indicated an arrested stage of intellectual development. "We may, therefore, say that the type of the female skull approaches, in many respects, that of the infant, and in a still greater degree that of the lower races" (Vogt 1864: 81). Several of his contemporaries repeated similar notions about infantilization (e.g., Ecker 1868: 355; Welcker 1862). Their conclusions left women of color at the greatest intellectual disadvantage.

Given the infrequency with which contemporary anthropologists speak his name, history has not been as kind to the memory of Vogt as were those colleagues who dubbed him "the Darwin of Germany" (Vogt 1864: 177). Yet, his statements about racial differences, sexual dimorphism, and social significance articulate mainstream ideas that even Darwin cited. Darwin's treatment of dimorphism is instructive. He does not include the term in the first three editions of *On The Origin of Species* (1866). But, in the book's fourth edition (there were six in total), Darwin addresses the phenomenon of dimorphism *and* trimorphism. "I refer to the two or three distinct forms," Darwin (1866: 50) observes, "which certain animals of either sex, and certain hermaphrodite plants, habitually present." His extended study of hermaphroditic barnacles was empirical proof (1851, 1854), though he does not acknowledge it as such for reasons I explain in Chap. 3. The possibility of such

variation in humans, however, was never even broached. Rather, as explained in *The Descent of Man, and Selection in Relation to Sex*, dimorphism is clear cut.

> Man on average is considerably taller, heavier, and stronger than woman, with square shoulders and more plainly-pronounced muscles. Owing to the relation which exists between muscular development and the projection of the brows, the superciliary ridge is generally more marked in man than in woman…His brain is absolutely larger, but whether or not proportionately to his larger body, has not, I believe, been fully ascertained. In woman the face is rounder; the jaws and the base of the skull smaller…and her pelvis is broader than in man…and in the formation of her skull is said to be intermediate between the child and the man. (Darwin 1871: 316–317)

Darwin's statements reiterate received wisdom about robusticity and gracility, pelvic features, and cranial development. And in support, he drew from the anthropological sources then in vogue. Vogt's (1864) *Lectures on Man* featured prominently in citational footnotes. To explicitly acknowledge human's trimorphism perhaps would have complicated social evolutionary ideas about labor's division—female reproducer and male producer—that relied on sexual dimorphism as absolute and rigid.

As I discuss further in Chap. 5, women were thought separate and unequal, a natural outcome of civilization's advancement. (Even their pelves provided unreliable measurements about racial differences!) This biologically deterministic coat-tails theory of human evolution, "the equal transmission of characters" according to Darwin (1871: 313), in turn justified paternalism and discriminatory treatment of women. As evidence, we may look to the English language version of *Lectures on Man*. In the his introduction to this text, Vogt's editor James Hunt harrumphed about admittance of the "fair sex" to the Ethnological Society of London. But, even prior to this, Hunt had grown disgruntled with the society's monogenist position on race. In defense of polygenism, he and defectors established the Anthropological Society in 1863 (Stocking 1987). The society's ire clearly extended to their rival's intellectual and political take on sex (or rather gender), as well. "Even now," Hunt proclaimed about women's entrance to the society, "the advocates of this measure do not admit their error" (Vogt 1864: viii). Concomitant sociopolitical events, namely the fomentation of suffrage movements in the U.K. and U.S., heralded a change in thinking about women's roles and intellectual capabilities. But, their membership and entrance into learned halls presented no uncertain challenge to many white men and the scientific studies they conducted.

During this time, dimorphism signaled not just a division of gendered identities but also sexual interactions. Medical practitioners' invention of "homosexuality" and "heterosexuality," both of which referred originally to non-reproductive and abnormal sexual relations, heralded a paradigm shift in understandings of sexuality (Chauncey 1982; Foucault 1978; Katz 1995; Terry 1995).[3] Social Darwinian ideas were implicit inasmuch as homosexuality signaled constitutional weaknesses,

[3]These terms first appear in a correspondence that German writer Karl Maria Kertbeny sent to theorist and defender of same-sex relations Karl Heinrich Ulrichs; the letter was dated 6 May 1868 (Katz 1995: 52).

sexual inversion (i.e., effeminate men, masculine women), and/or arrested development, all of which medical practitioner presented as pathological response to modernity and feminism (Terry 1995). Prior to the mid-nineteenth century, Western society may have socially prohibited and juridically punished certain intimate arrangements, but acts and anatomy did not an individual's identity make (Foucault 1978). Thus, sexuality came to signify a social identity. While all sexual inverts were scrutinized, scientists found certain types less troublesome given their adherence to traditional gender norms. As Chauncey (1982: 125) relates, "Many nineteenth-century doctors considered the truly serious offense to be the invert's assumption of the opposite gender role rather than either her or her 'wife's' homosexual object choice."

Sexual deviance like gender identity, scientists argued, was embodied. Hence, it could be isolated, classified, and analyzed. The anthropometric techniques used to categorize racialized and sexed bodies were adapted to document homosexual individuals' physical distinctiveness and natural proclivities (Somerville 1994; Terry 1995, 1999). Unsurprisingly, anthropologists were called on to lend their expert knowledge. Come the late-1930s, for instance, physical anthropologist Earnest Hooton joined psychiatrists, gynecologists, etc. to form the Committee for the Study of Sex Variants (Terry 1999: 188). From 1935 to 1941, the committee intensively investigated New York's growing homosexual population (and problem in their estimation). As Terry (1999: 196) describes, "Subjects were given a series of general physical exams and tests, including skeletal x-rays, pelvic and physique measurements, metabolism tests, and hormonal assays." These tests were complemented by genital examinations, psychiatric interviews, and a masculinity/femininity test. Granted, skeletal evidence may have proved inconclusive, but bodies continued to be probed intensely. In their conclusions, and comparable to those drawn about women and non-whites, scientists deemed the bodies of homosexuals pathological while psychiatrists characterized their minds as psychologically deviant and morally destitute.

The legacy of sexuality's biologization is evident in late-twentieth century scientific work on brains and genes. In the early 1990s, neuroanatomist Simon LeVay (1991, 1993) examined heterosexual and homosexual brains, arguing that the latter resembled those belonging to women. The shortcomings with his research, however, are numerous: conflation of gender and sexuality; uncertainty about brain structure as a cause or effect of life experiences; decedents' unverified sexual acts and self-identification while alive; morphological confounders like disease history—the majority of homosexual men died from AIDS; and an insufficient sample size (i.e., 19 homosexual men, 16 "presumed heterosexual" men, and 6 "presumed heterosexual" women). Around the same time, federally funded geneticists led by Dean Hamer claimed that they had isolated a gene for male homosexuality on the Xq28 stretch of the X chromosome (e.g., Hamer and Copeland 1994; Hamer et al. 1993), or the "gay gene" as dubbed by the media. Subsequent studies were unable to replicate these findings, however (e.g., Rice et al. 1999). And in the new millennia, researchers continue to pursue genetic studies of sexuality with federal funding but little regard for queer studies or epigenetics complications (e.g., Sanders et al. 2015).

Nor have they branched out from a concern with same-sex relations between males, suggesting that sexism and heterosexism inform research questions and conclusions to an unsubtle degree.

2.6 The Racialized Pelvis

Long prior to the sexual brains or gay gene, there was the racialized pelvis. Skeletal analysts' shift from cranium back to pelvis in sex determinations is both cause and effect of a discipline coming into its own. Nineteenth century physicians often received crania and only crania from eager allies in distant locales. As they moved out of the armchair and into the field, anthropologists in the early twentieth century found themselves involved in the dirty work of archaeological excavation and forensic analysis. For those who concentrated on human remains, M.D. usually followed their surnames (see Buikstra 2006a: 9–10). Amongst the individuals who accompanied Clarence Moore on expeditions, for instance, was project anatomist Dr. Milo Miller. Seeing that taphonomic factors in the southeastern U.S. often made for poor preservation of skeletal remains, his presence was a boon. Yet, it does not seem to have been a disciplinary norm. Rather, archaeologists generally excavated burials and then transported a selection of their contents to analysts' laboratories. Even a large portion of the skeletal remains excavated by Miller were hastily examined before being discarded, reburied, or circulated among locals. Based on his assessment of the humerus, he sexed decedents as male, female, or uncertain (e.g., Moore 1894). Select elements—well-preserved examples illustrating racial, developmental, and sexual norms—were then sent to Ales Hrdlička for more intensive analysis (Aten and Milanich 2003: 130).

Despite these changes, the first half of the twentieth century saw the continuation of debates, investigative foci, and theoretical underpinnings. Bodies were still rigidly dimorphic. Facial angle and cranial capacity were still measured routinely. Researchers still quibbled about the age at which skeletal remains could be sexed. Determination of sex followed from analysis of individual parts and not whole bodies, though analysts did debate about which of the former yielded more accurate assessments. Remarked British physician Arthur Luff (1895: 80) in *Textbook of Forensic Medicine and Toxicology*, "The examination of the pelvis, however, furnishes the most satisfactory method for the determination of sex in an adult skeleton." Yet, and certainly more influential in anthropology, Hrdlička's (1920: 94) handbook *Anthropometry* suggested otherwise: "The most important skeletal parts for sexual identification aside from the skull are, however, the pelvis, the long bones, and the larger of the remaining parts." Subsequent skeletal analysts persisted in their scrutiny of the human body for ever more nuanced insights about sexual dimorphism—articular surfaces of long bones (Dwight 1905), ossification centers in sub-adults (Pryor 1923), vertebral anomalies (Stewart 1932), femoral dimensions and indices (Hrdlička 1938). And they continued to convey commonsensical presumption about socio-sexual lives in their studies. To sex the scapula, for instance,

Hrdlička (1920: 125) describes sex-specific forms. His conclusion…males had more, a consequence of different and numerous muscular activities. Whether or not he is implying these differences are social or inherent, and he is not explicit, his reasoning is no less deterministic about the different muscular activities performed by men and women (i.e., a sexual division of labor).

To determine sex, anthropologists in the mid-twentieth century would call for quantification of the pelvis. In fleshy form, nineteenth century physicians had measured it—the obstetrical sub-specialty known as pelvimetry—for information about gynecological conditions (e.g., van Huevel 1840). Skeletonized, however, analysts assessed it for empirical evidence of racial differences (e.g., Garson 1881). William Turner (1885) modeled his pelvic typology after racial classifications of crania. He categorized specimens as dolichopellic, platypellic, mesatipellic (long, flat, and round in shape, respectively) based on his measurement of pelves' brim, or the pelvic index. Both European males and females, he claimed, were within the platypellic range. But, for other groups, sex confounded easy categorization. Rather than see that the larger classificatory schema as deficient, he took issue was the female pelvis. It displayed too much racial variability for his liking. "In the males," Turner (1885: 127) stated, "the form characteristic of the race is more fixed, and from their study it is, I think, possible to frame a classification of the pelvis." Typological obstacles surmounted, Turner's analysis proceeded. And from the empirical evidence, he (1885: 143) concluded that the pelvis "in those [non-European] races shows a more degraded character—a less departure from the usual mammalian form—than is the case in the Europeans." Hence, analysis of the racialized pelvis revealed that European men were the measure of all things. Turner's study was not without its shortcomings, critics pointed out. Physical anthropologists bemoaned the small size of pelvic samples in general, "absurdly so" in the words of Matthews et al. (1893: 220).

Archaeological excavations in the early twentieth century would gradually remedy this problem. But, researchers now had to sex the remains of individual for whom they had no background information. In the absence of methodological standards this was challenging to do, which may explain Hrdlička's zeal for crania. He did, however, acknowledge a complication. In *Anthropometry*, Hrdlička (1920: 91) writes, "In adults, the determination of sex, from the skull alone, while generally offering few difficulties to the well-trained observer, is not equally easy in all race's, or in all individuals." Similar to crania, racial differences made sex assignment of the pelvis no less transparent. Amending Turner's pelvic typology was one strategy. Caldwell and Moloy (1933), for instance, distinguished between gynecoid, android, anthropoid, and platypelloid, though their emphasis on birthing efficiency advanced the pelvis's racialization, as well

Into the second half of the twentieth century, physical anthropologists struggled with how to produce methodological standards that advanced the determination of sex without promoting the racist assumptions of seminal studies. Lucile Hoyme (1957: 545), for instance, championed quantification for sexing purposes *and* was well aware of the longstanding disciplinary fixation with race. Yet, she validated the pelvis's measurement as a viable research path for describing "differences in new

racial groups." Extrapolating initially from monkeys, Washburn (1948: 201) found that "the ischium-pubis index alone will sex over 90 % of skeletons, provided that they belong to one major racial group." The subtext of his conclusion—that admixture or human variation can challenge fixed and bounded racial categories—went unaddressed. A follow-up publication, however, suggests that he was quite aware of racial typologization's fallibility. In a 1949 study, Sherwood Washburn tested his method's accuracy by measuring the pelves of South Africans affiliated with the Bantu and Bushman tribes. In so doing, he acknowledged the significance of context—archaeologists had excavated the Bushman remains—and regional variations, or population-specific data. He then compared his findings to prior analyses of American Whites' and American Negroes' pelves. His conclusion, that differences exist between and within groups, was not necessarily a departure from earlier studies. But, the inferences he drew, or did not draw from data, are what set this study apart. Specifically, Washburn did not hierarchically rank racial groups or sexual differences. The ischium-pubic index is nearly identical for Bushman and American Whites, he found, and this fact was interesting so far as it helped establish methodological standards for sexing the *human* pelvis. What changes for Washburn, we need inquire?

The sociopolitical conditions that provide backdrop for physical anthropologists' work, I would argue, account for the ambivalence expressed in anthropometric studies at the twentieth century's midpoint. The recent events of World War II had clearly illustrated the genocidal outcome of racialized science. Given the sins of their intellectual forefathers, it is therefore fitting that physical anthropologists were amongst the first to disavow scientific studies that reified race (Montagu 1951, 1964, 1965; Washburn 1963).

Of course, it may be best to think of such scholarship as cancer in remission—a decrease or disappearance in signs and symptoms does not necessarily indicate complete eradication. The racialized pelvis is a chronic condition, recurring again and again in the corpus authored by contemporary obstetricians and forensic anthropologists. Their reasons for quantifying differences generally go no further than vague allusions to obstetrical dysfunction (e.g., Baragi et al. 2002; Hoyte et al. 2005; Marani and Koch 2014) or identification of unknown individuals in settings beset by high crime rates (e.g., Igbigbi and Nanono-Igbigbi 2003; Patriquin et al. 2002). In a popular textbook of obstetrics and gynecology, for instance, Enrico Marani and Wijnand Koch rationalize,

> Although [Professor William] Turner's study was done in light of the supremacy of the white and the MRI study for implications in obstetric practice, the same results are obtained 200 years later with the most sophisticated medical instrument. Therefore, *racial differences are present in the bony pelvis and are important in clinical observations*." (2014: 13, their emphasis)

The difference being that rather than the crude metric tools of yesteryear, modern researchers are equipped with high-tech apparatuses—magnetic resonance imaging is the new pelvimeter. Some may find it disquieting that two white European men continue to uncritically support pelvic typology without also considering the impact

that age, stature, diet, disease-load, or habitual activities may have on pelvic morphology. As numerous researchers have since pointed out, analyses of the pelvis have effectively demonstrated that it can change shape during an individual's life course (e.g., Abitbol 1996; Ridgeway et al. 2011; Walker 2005). Nevertheless, the racialized pelvis remains very much a biopolitical reality in the twenty-first century.

2.7 Bioarchaeology on Gender

2.7.1 Paradigm Shift

Physical anthropology's thinking on the subject of racial typology underwent a paradigm shift soon after Washburn's statements on the ischium-pubic index. Indeed, in his call for a "new" physical anthropology, Washburn (1951) first acquiesced that typological classification had created analytical stagnation within the sub-field. Racial types could not adequately explain the complexities of populations' interbreeding and migration. He instead emphasized the importance of understanding how evolutionary processes drove empirically observable and selectively advantageous adaptations. To this end, collaboration between the four-fields was needed, as were technical and theoretical changes (Washburn 1951: 298). This "new" vision for physical (or biological) anthropology synthesized evolutionary concepts, population genetics, systematics, paleontological finds, and primatological observation (Haraway 1991: 36).

Washburn's statement provided stimulus for formalization of bioarchaeology in the mid-1970s. Granted, there were important precursors that contextualized remains and sought to explore the complex intersection of biology, inheritance, and culture [e.g., social biology (Angel 1946); life history (Krogman 1935); osteobiography (Saul 1972)]. But, generally, archaeologists relegated physical anthropologists' assessment of human remains to the back of the book (Buikstra 1991). In the appendix of archaeological studies, they were more laundry list than thoughtful analysis. In response, Buikstra (1977: 69) suggested the following:

> A new form of regionally based, interdisciplinary research in mortuary site archeology and human osteology has been developed in the course of the present study. With the active participation of both archeologists and physical anthropologists in all phases of research design, members of our "bio-archeological" research group made the initial decision to focus upon the investigation of biocultural change within the Woodland period.

The beauty of Buikstra's proposed model was its application to skeletal samples from distinct cultural settings and historical periods. Emphasis on the biocultural, interdisciplinary, and contextual affirmed the value of American anthropology's four-field approach. Traces of its Boasian heritage—conjunctive study of physical bodies, genetic inheritance, and the effects of environment (i.e., culture)—are unmistakable, as well.

Today, the bioarchaeological corpus is substantial. Other scholars have offered detailed overviews of the sub-field's contributions, shifting methods, and theoretical frames (e.g., Agarwal and Glencross 2011; Buikstra and Beck 2006; Martin et al. 2013; Rakita 2014; Zuckerman and Armelagos 2011). The consensus is that since its inception, multiple approaches have developed concurrently. What we have today are bioarchaeologies, "differences in scope and emphasis…that should be considered a measure of the vitality within this developing field," as Buikstra (2006b: 248) has remarked more recently. Despite pluralism, however, there are still common concerns regardless of whether one's bioarchaeological leanings are scientific or humanistic (or both). The study of gender is one.

2.7.2 Semantics

Bioarchaeologists first made explicit statements about gender in 1998 (e.g., Armelagos 1998; Brown 1998; Konigsberg and Hens 1998; Walker and Cook 1998). All expressed a concern with semantics; sex was conceptually distinct from gender they stressed. Influential was Phillip Walker and Della Cook Collins's (1998) programmatic communication in the *Journal of Physical Anthropology*. They admonished biological anthropologists, chiefly bioarchaeologists and primatologists, for using sex ("biological identity") and gender ("social identity") interchangeably. When speaking of humans' social identities, conflation naturalized culture (though they did not word it in quite this way). And the use of gender in biological studies of nonhuman animals functioned to culturize the natural. The ubiquity of the "genderified" baboon in primatological discussions, for instance, struck Walker and Cook as odd. The repercussions, they predicted, would be several-fold—conceptual, disciplinary, pedagogical.

These insights were certainly valid, but they were a long time in coming. two decades prior, sociocultural anthropologists had first distinguished sex from gender (e.g., Ortner 1974; Ortner and Whitehead 1981; Rosaldo 1974) with archaeologists soon following suit (e.g., Conkey and Spector 1984; Conkey and Gero 1991). Since this time, scholars in these sub-fields have continued to advance feminist and queer studies with their considerations of sex, gender, and sexuality.

In contrast, the impetus for bioarchaeologists' interest in gender came not from feminism, but an emergent concern with the biocultural body (e.g., Goodman and Leatherman 1998). If researchers do cite feminists' ideas, the sources are usually quite dated ones. Few bioarchaeologists engage with recent feminist and queer writings on gender (e.g., Geller 2005, 2008, 2009; Gere 1999; Hager 1997; Hollimon 1997, 2000; Kakaliouras 2006; Perry and Joyce 2001; Perry and Potter 2006; Sofaer 2006; Worthman 1995). Hence, rather than undertaking the difficult conceptual work of reconciling biological data with often abstract social theorizing, which I do in the chapter that follows, many find it easier to concede that sex is not gender, and then sidestep the latter by laying investigative emphasis on the former. Other less productive responses include feigning ignorance entirely or deriding this

work as an unhappy outcome of anthropology's postmodern turn. Regardless, much more can be done to advance understanding of past cultures or knowledge production about biophysical evidence.

Programmatic statements may have tuned bioarchaeologists into gender's salience, but many still do not recognize some of the commonsensical thinking that inform their investigations. Less at issue for me is the designation of sex as a significant category of skeletal analysis. It is a quite useful for two reasons—one intended and obvious, the other more subtle. Determination of sex has the *potential* to yield important information about the culture under consideration. How are biological differences made meaningful within a specific social setting and historical circumstance? That is, many bioarchaeologists study sex for what it has to convey about gender in an ancient context. I stress "potential" because I think less obvious to bioarchaeologists is that their sexing of bodies is often more informative about contemporary conceptions of sex, gender, and sexuality. The shifting emphases placed on certain biophysical data suggest as much.

2.7.3 Sex and Gender

By the 1960s, researchers no longer questioned that the pelvis was the most sexually dimorphic element as a consequence of the mechanical effects of childbirth (e.g., Brothwell 1963; Krogman 1962; Montagu 1960; Phenice, 1969). Phenice's (1969) examination of three morphological features—the ventral arc, sub-pubic concavity, and medial aspect of the ischiopubic ramus—was especially edifying when preservation impeded metric analysis. Such remains the case today. Technological advances, "the new morphometry," have helped to fine-tune analysis of cranial sexual dimorphism (Konigsberg 2006: 275). But, osteological textbooks still stress pelvic analysis and instruct students in the Phenice method, as well as assessment of the greater sciatic notch and preauricular sulcus (Bass 1995; Buikstra and Ubelaker 1994; White et al. 2012). These diagnostic traits allow analysts to estimate sex along a continuum of difference: female, probable female, unknown or indeterminate sex, male, and probable male.

But, as I have discussed elsewhere, while a valid heuristic tool, this continuum communicates an analyst's degree of categorical certainty rather than attends to sexual variability or ambiguity (Geller 2005). The five categories appear disingenuous in light of Fausto-Sterling's (1993) identification of five sexes—male, female, and an additional three intersexes that she labels herm, merm, and ferm. "Indeed, I would argue further that sex is a vast, infinitely malleable continuum that defies the constraints of even five categories," she (1993: 21) avowed. More recently, she has augmented the linear continuum with a more layered understanding of sex as it pertains to fetal development. By birth, she explains, a baby has five layers of sex: chromosomal sex; differentiated fetal gonadal sex; fetal hormonal sex; fetal internal reproductive sex; and genital sex (Fausto-Sterling 2012: 3–5). This framework was first developed by John Money and colleagues in the

1950s. Their intent was to delineate inconsistencies between the layers in the case of individuals with intersex conditions.

Yet, dimorphism that is rigid and static remains the steadfast rule. So what are we to do with the exceptions, the 4 % of individuals whose atypical pelvic attributes make it difficult to estimate their sexes with any accuracy? As Meindl et al. (1985) have argued, morphological assessment of the pelvis is 96 % accurate. This is not to suggest that individuals with intersex conditions represent this small percentage of categorical exceptions. Rather, it is to say that indeterminate sex should not be the grounds for analytical dismissal. And in the case of individuals whose skeletal systems have yet to develop the traits diagnostic of sexual difference, researchers remain hopeful that technological advancement will rectify their unknown status in the future. The motivations for or implications of doing so—why do we need to sex infants, for instance?—remain largely uninterrogated, however. This specific issue is treated more fully in Chap. 7.

These concerns hint at the incontrovertible and often unconscious link that many analysts of bodies draw between sex and gender. As Dana Walrath has noted, in turning from racial typology to sexual dimorphism as functional adaptation, researchers' pelvic data are no less laden with social meanings. Of paleoanthropologists' writing, she (2003: 7) remarks,

> The locomotor efficiency of the male pelvis is emphasized in this discourse. By contrast, the inferior social position of women may be reflected in the depiction of the adaptive compromise between the requirements of childbearing and bipedalism. Rather than emphasizing the successful reconciliation of two competing biological requirements, this discourse emphasizes the inefficiency of the female stride and the inevitable obstetric dilemma.

That humans' pelvic dimorphism is represented as a defining attribute of *Homo*'s evolution is not necessarily the issue. Rather, the concern is how analysts define human nature as it relates to sex, gender, and sexuality. Males' pelves, it would appear, portend freedom in motion and occupation, while females are restricted biologically and spatially by reproductive imperative. To clarify, researchers are not examining female pelves for information about parity status. Of course, the feasibility of such an assessment is debated (Ubelaker and De La Paz 2012), but such uncertainty does not appear to be the reason the subject is not broached. Rather, I would argue, analysts do not address parity status because they already have presumptions in place about women, work, and bodies. Having a pelvis with female morphological attributes seems to be evidence enough of compulsory reproduction.

From sexual differences then develop the supposedly natural state of sociosexual institutions, identities, interactions, and bodies. Nevertheless, excluding instances of semantic confusion (which vexingly, from my viewpoint, continue to occur), gender is a concept that few paleoanthropologists have tackled explicitly in their studies (though see contributions in Hager 1997). For bioarchaeologists who examine human remains from more recent (pre)historic periods, however, gender is a topic of increasing concern. Because all bioarchaeologists sex bodies, it is likely that even those who claim not to be saying anything about gender are. That is, even

if they are not conveying information about the ancient lives they study than they are advancing ideas about contemporary socio-sexual arrangements and interactions. Bodies are rigidly dimorphic. Sexual differences are fixed and determine social outcomes. The biophysical evidence that has always mattered is reduced to reproductive abilities. And yet, what if the body of data had the capacity to challenge what we claim about sex?

2.8 Sex Is Genetics

Morphometric data gleaned from pelves are so last century. To investigate sex in the past, bioarchaeologists now turn to the tools of the future. Technoscientific advances like genetic testing have granted access to and knowledge about the human body at the micro-scale (Chap. 7). Ancient DNA (aDNA) analysis, many proclaim, promises to objectively sex our poorly preserved, our analytically indeterminate, and our prepubertal. It has the potential to more accurately reveal the "truth" in the matter. But do new ways of doing yield new ways of thinking about sexual differences?

Biomedical researcher Nettie M. Stevens first documented chromosomal sex differences in 1905 (Brush 1978; Ogilvie and Choquette 1981). In her study, she examined five distinct species of insects. The chromosomes of *Sagitta bipunctata*, an arrow worm, were far less interesting to Stevens. The species' hermaphroditism made it unnecessary to distinguish males from females. *Tenebrio molito* in its common meal worm form, on the other hand, was particularly exciting. "In both somatic and germ cells of the two sexes there is a difference not in the number of chromatin elements, but in the size of one, which is very small in the male and of the same size as the other 19 in the female," concluded Stevens (1905: 18). And lest we think that Stevens' discovery of chromosomal sex differences was celebrated, credit for this contribution to science is often wholly or equally attributed to Edmund B. Wilson, who made a similar finding on the heels of Stevens's publication. The irony that sexism has influenced the making of sex is worth noting for it underlines that Stevens' experiences are not idiosyncratic in the history of science.

Collectively, Stevens's and Wilson's observations set the basis for the XX/XY model that scientists later developed in their studies of humans. Theophilus Painter (1923, 1924) was amongst the most influential if not the most misleading in his suggestion that humans possessed 48 chromosomes. Subsequent studies reiterated this claim until such received wisdom was debunked when Joe Hin Tjio and Albert Levan (1956) verified the presence of 46 chromosomes. That researchers still saw race as a potential confounder in early work was indicated by Painter's (1923) comparison of white and black men's spermatogonia, undifferentiated male germ cells, as well as Kan Oguma's (1930) argument that the size of X chromosomes

varied according to nationality. "In the Japanese," wrote Oguma (1930: 205) who was himself Japanese, "it is the largest chromosome in any one garniture; in Belgian material it is the second largest or, sometimes, smaller." It is unclear just what a larger size signified to Oguma, but perhaps bigger suggested more effective evolutionary adaptation.

In short order, deviations from dimorphism were made visible by genetic testing. Researchers determined that Turner's syndrome (45, X) involved monosomy of the X chromosome in anatomical females (Ford et al. 1959). "Apparent" males with XXY and a chromosomal count of 47 suffered from Klinefelter's syndrome (Jacobs and Strong 1959). And though the individual with Triple X syndrome (47, XXX) was described as a "super female," her underdeveloped genitals and irregular physiological processes suggested she was anything but to scientists (Jacobs et al. 1959). The age of gonads was giving way to one of chromosomes. Since these findings, researchers have documented an array of chromosomal combinations that fall under the umbrella of intersex conditions—XYY, XXXY, XXXXY, XY females (e.g., 5-alpha-reductase deficiency, androgen insensitivity syndrome), and XX males (e.g., congenital adrenal hyperplasia). Individuals with intersex conditions, whether verified by genetic testing or clinical assessment, represent about 2 % of all live births, or 1 in 1500–2000 live births (Blackless et al. 2000; Fausto-Sterling 2000a, b). Critics have countered that the figures on intersex conditions are too high, and the definition too loose. Leonard Sax (2002: 174), for instance, claims that only conditions observable to the clinician qualify, that is when "chromosomal sex is inconsistent with phenotypic sex." According to this definition intersex conditions represent .018 % of live births. Regardless of their prevalence, however, they are a biophysical reality, more so than the aforementioned gene for homosexuality that scientists claimed was on the Xq28 stretch of the X chromosome (e.g., Hamer and Copeland 1994; Hamer et al. 1993). Molecular scientists *might* have categorized evidence of intersex conditions as trimorphism (or tetra-, penta-, hexa-, hepta-, etc.) had Darwin deemed the concept applicable to humans. Yet, their designation of individuals with intersex conditions as abnormal indicates that dimorphism remains the ideal. Discordance between genotype and phenotype troubles researchers' normative understanding of reproduction, gendered behaviors, and sexual predilections.

As a subset of genetic testing, biomolecular archaeology examines ancient remains for information about an individual's chromosomal sex. "At the simplest level, that of the individual," Frederika Kaestle and K. Ann Horsburgh (2002: 96) have written, "aDNA studies allows [sic] us to determine the sex of an individual using markers on the X and Y chromosomes." Genetic testing is particularly useful for circumventing poor preservation or sexual immaturity (e.g., Brown and Brown 2011: 151–167; Cappellini et al. 2004; de la Cruz et al. 2008; Faerman et al. 1998; Matheson and Loy 2001; Mays and Faerman 2001; Mohandesan et al. 2004; Stone et al. 1996; Vaňharová and Drozdová 2008). Yet, many researchers continue to conflate the biological and social in their publications. The use of genetics to establish identity appears as a foregone and fairly unproblematic conclusion. Genetic testing produces gender identities that are represented not just as

dichotomous, but also deterministic and universal. One becomes the sum of chromosomal parts, a process known as geneticization (see Chap. 7).

Some biomolecular archaeologists, however, are increasingly aware that sex is not gender. Terence Brown and Keri Brown (2011: 153) are quite adamant on this point in the introductory textbook they have authored. For them, gender is a cultural construct informed by discernible biological differences. Sex, on the other hand, *is* genetics. They echo Kaestle and Horsburgh's statement in their textbook *Biomolecular Archaeology: An Introduction*: "Sex is a biological characteristic determined by the functioning of various genes, many but not all located on the X and Y chromosomes" (Brown and Brown 2011: 153). Perhaps this is the reason Brown and Brown (2011: 153) also attend to intersex conditions (see also Brown 1998). Intersex conditions are sometimes not visible to the naked eye. Rather, they are verified with molecular testing. But, while these observations about sex and intersex conditions are not wrong technically, they do require more thoughtful treatment, especially in an introductory textbook that may be the first exposure students have to biomolecular archaeology. Dreger (1998: 4), who has written extensively on the subject of intersex, remarks,

> We live in an age of genetics and oversimplified stereotypes about the nature of males and females…Although it is true that a very small percentage of the people not easily sorted into [gonadal] male or female have been shown to have chromosomal patterns that differ from the common XX and XY varieties, the majority do appear…to have the standard male or female "sex chromosome" pattern. (I place "sex chromosome" in quotation marks because the term is an unfortunate misnomer;…genes related to traits we consider non-sexual are also located on X chromosomes, and genes located on chromosomes besides the X and Y chromosomes contribute to sexual development. We would do better to call these X and Y chromosomes instead of "sex chromosomes.")

Her statements reiterate an important point about the analysis of chromosomes to determine intersex specifically and sex in general. The relationship between phenotype and genotype is not straightforward, and XX and XY do not adequately capture the range of humans' differences with regard to morphology, fertility, or ability.

Skeletal traces of intersex conditions (i.e., shorter stature, spine curvature) are subtle, and identification of genetic variance does invites bioarchaeologists to reflect on the biological fact of tri-, tetra-, penta-, hexa-, heptamorphism, etc. From these data, researchers may then draw inferences about cultural significance within a given context. In so doing, they can move beyond conceptualizing past people as monoliths (i.e., Man, Woman). But, and not to discount its methodological value, aDNA analysis is a modern way to make sex. As such, it must be situated within the trajectory of historical methods for determining sex. A shifting elemental focus—from skull to pelvis to chromosome—speaks to culturalization of the natural. And if there is any lesson to learn from the preceding discussion of pelvimetry, it is that aDNA testing may very well be negated by technoscientific advances in the future. Sex then is genetics…in contemporary Western society. The relevance of chromosomal evidence in ancient contexts requires reflection, however, for its

significance and phenotypical observability cannot be assumed. Framing intersex conditions as genetically pathological, for instance, is a presentist position that is neither universal nor longstanding (Dreger 1998; Fausto-Sterling 2000b).

2.9 Postlude

I began this chapter by thinking about the fate of Vienna's dead. The celebrated and stigmatized were useful for underscoring how politics, ethics, and epistemology all come to bear on scientific objectivity. All three deserve greater attention in bioarchaeologists' future discussions. The literature on ethics, for instance, is only just beginning to extend beyond reactive positions to NAGPRA—to deliberate about collaboration with local communities and colleagues in host countries, destructive analyses, museum curation, pedagogy, and interpersonal or structural violences (e.g., Alfonso and Powell 2007; Geller and Suri 2014; Larsen and Walker 2005; Martin et al. 2013; Turner and Andrushko 2011; Walker 2000; Zuckerman et al. 2014).

For my part, and the thread of ethics is again taken up in my final chapter, here I have tracked biophysical evidence of sex differences, the methods utilized, and the meanings inferred. If anything, this history of sex determination—with its necropoliticized anatomical collections, sexualized skulls, racialized pelves, genderified baboons, geneticized genders—debunks claims about science's unfailing objectivity. Elsewhere I have remarked that there has always been sex in bioarchaeology (Geller 2008). In light of this history, I might amend the statement to include gender and sexuality. That is, the contemporary Western way of making sex with an emphasis on rigid dimorphism, fixity throughout a life course, and reproductive imperative conveys heteronormative ideas about gender roles, as well as compulsory procreation and heterosexuality. People make the system that categorizes bodies, Fausto-Sterling (2000b: 287) reminds us, "and a system of just two bodies is not the only possible system." Thus, sex is a construct rooted epistemologically in enlightenment thought. In this regard, it is analogous to race. Yet, and to be clear, to characterize sex as constructed is not to divorce it from biophysical evidence. Its grounding in the human body means it is constrained in important ways by the limits of plasticity and inheritance.

As my aim was to present a historiography in this chapter, I have not dwelled overly long on current bioarchaeological studies of socio-sexual lives. The chapters that follows do discuss publications pertinent to intimate arrangements (Chap. 4), sexual division of labor (Chap. 5), reproductive management (Chap. 6), and geneticization of gender (Chap. 7). But, one significant shortcoming in bioarchaeology that I mentioned here is a tentative and sometimes superficial engagement with feminist and queer scholarship. As a consequence, bioarchaeologists continue to communicate historic common sense about bodily difference, gender

identities, and sexual interactions. Were they to draw from feminist and queer writings more deeply, as I discuss in the next chapter, data collected from archaeologically contextualized human remains would demand they attend to diversity beyond dichotomy and the complexities of intersectionality.

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