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ARTICLE

Sensational Science, Archaic Hominin Genetics, and Amplified Inductive Risk

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Abstract

More than a decade of exacting scientific research involving paleontological fragments and ancient DNA has lately produced a series of pronouncements about a purportedly novel population of archaic hominins dubbed "the Denisova." The science involved in these matters is both technically stunning and, socially, at times a bit reckless. Here I discuss the responsibilities which scientists incur when they make inductively risky pronouncements about the different relative contributions by Denisovans to genomes of members of apparent subpopulations of current humans (i.e., the so-called "races"). This science is sensational: it is science which empirically speculates, to the public delight's and entertainment, about scintillating topics such as when humans evolved, where we came from, and who else we were having sex with during our early hominin history. An initial characterization of sensational science emerges from my discussion of the case, as well as a diagnosis of an interactive phenomenon termed *amplified inductive risk*.

Keywords: Ancient DNA; ethics; genetics; human evolutionary history; inductive risk; paleoanthropology; paleontology

1. Introduction

Much science is staid, but some is sensational. By science that is "sensational," I mean science that is fascinating not just to scientists themselves, but also to the wider public—and fascinating in a somewhat titillating way. Budding field ecologists might be entranced by the possibility of parasitic nematodes forming the lowest tier of a tripartite terrestrial trophic cascade that works its way up through root-dwelling ghost moth caterpillars to control of coastal lupine populations (e.g., Preisser 2003), but publications on that topic are unlikely to pique significant public interest. Members of the public might have their interest significantly piqued by scientific claims about the possibility of a malaria-free world, or free energy forever, but sensational science is science that appeals both broadly *and* salaciously to members of society at large, usually by dealing in base matters of enduring human interest (such as sex, beauty, power, wealth, caste, violence, secrets, or origins).[1]

[1]On this account, sensational science is a subset of what Jones (2019) characterizes as "celebrity-driven" science, which is itself a subset of "publicity-driven" science (also defined by Jones [2019]). If publicity is understood as attention bestowed by the media, then only some of that science which receives publicity attains the level of celebrity—i.e., is bestowed with sustained and enduring, or at least reliably recurring, attention by the media. And only some of *that* (celebrity-science) attains its celebrity via sensationalism. Dinosaurs are scientific celebrities, as is the meteor which (almost entirely) wiped them out. I posit that we can divide the science that fascinates us into at least two categories: super cool and very neat stuff that quite wholesomely entices us, and scintillating stuff which entertains us for rather less redeemable reasons. I am sure that some science draws our sustained

Since we ourselves are often our own favorite topic, the scientific study of human ancestry is well-suited to our most sensationalist tastes—particularly when the issue of who has been having sex with whom is being discussed.

One current example of sensational science is the spate of recent work on the newly discovered population of archaic hominins known as "the Denisova" (introduced by Krause et al. [2010]; followed by Reich et al. [2010]). *Archaic hominin populations* are populations of hominins which existed either before or along with populations of hominins that eventually became anatomically modern humans that eventually became us.[2] Neanderthals are perhaps a better-known example than the Denisova of an archaic hominin population that co-existed with, but was still distinct from, those hominin populations that eventually became anatomically modern humans that eventually became us. Especially in light of their living at the same time as our own ancestral populations, and ever since their 1850s discovery, there has been potent and enduring interest in Neanderthals as a population of archaic hominins. Certainly, the Neanderthals are a longer-known instance of such. This interest has spurred rampant scientific, journalistic, and public discussion—some quite speculative—about all sorts of related topics: what the Neanderthals were like; how and why they went extinct; whether and how we (or more precisely, our ancestors) killed them; if and when "we" had sex with "them" (this one is tricky[3]); whether those unions could produce offspring; how often that happened; etc.

What the discovery of the Denisova does is introduce another such party to the party. The Neanderthals are named for the Neander Valley in Germany, where a set of distinct skeletal remains were discovered; these were not the first Neanderthal specimens to be unearthed, but the first to be recognized as such. The Denisovans are similarly named for Denisova Cave[4] in the Altai Mountains of Siberia, where another set of anomalous paleoanthropological fragments were discovered; again, not necessarily the first Denisovan specimens to be unearthed, but the first to be recognized that way. Morphological evidence of the Denisova as a novel population of archaic hominins was first noticed as such in the 2000s, and compelling genetic support for the distinctness of the population was shortly thereafter provided.[5] As I will soon detail, many features make this scientific story a sensational one.

But first, a programmatic declaration. My philosophical aim in this paper is twofold. Some science is ripe for sensationalizing and scientists often know that or about the publicity, and even celebrity, that could come with working in that domain. Working on a sensational topic can confer professional advantage, and one of my two philosophical aims in this paper is to characterize the phenomenon of sensationalism, along with scientists' awareness of its potential for conveying advantage, as a form of influence in scientific decision-making. The other of my two philosophical aims is to document a rather pernicious form of interaction between this phenomenon of "sensational science awareness" and another one, that of inductive risk.

The term *inductive risk* (introduced by Hempel [1954, 1960]; revived by Douglas [2000][6]) refers to the risk which scientists take when making a decision about the sufficiency of evidence required

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attention for both more *and* less flattering reasons; the existence of such compound cases does not, however, invalidate the distinction. Likewise, I am confident that other relevant distinctions could be drawn; my pointing to this one should not be taken to indicate that I think it is the only one.

[2]*Hominids* is a term now used to refer inclusively to all modern and extinct great apes—to bonobos, chimpanzees, gorillas, humans, and orangutans. *Hominins* is the current term used to refer exclusively to only the human subset of the hominids—to modern and extinct great apes minus the bonobos, chimpanzees, gorillas, and orangutans. This represents a change in terminology, which can be confusing. *Hominids* used to refer to what *hominins* now does.

[3]Because, if we could have sex with them and produce viable offspring, then according to some understandings of what it means to be a species, "we" and "them" are one and the same—us is them, them is us.

[4]Purportedly named for an eighteenth-century hermit, Denis, who resided in the cave (Reich 2018, 53).

[5]The question of whether the Denisovan population was distinct enough from that of *Homo sapiens* to constitute its own, separate species is still unsettled. This is the case for Neanderthals as well.

[6]Note that I have said that Douglas's use of Hempel's *term* is a revival, not that Douglas's argument, nor her account of the relationship between values and science, is a revival of Hempel's. My claim that Douglas's use of Hempel's term *inductive risk*

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to make, versus fail to make, a particular scientific claim, and of being wrong in that professional judgment. What we generally want is for scientists to make all and only those claims for which there is sufficient evidence—to not leave out any claims for which there is sufficient evidence but also to refrain from making any claims for which the evidence is insufficient. But this expectation entails that there are also two dichotomous ways in which scientists can err in this sort of professional judgement: (1) they might make a claim for which they have insufficient evidence and (2) they might fail to make a claim for which they have sufficient evidence. This is the inductive risk they take professionally.

As many have lately argued (e.g., Douglas 2000, 2009; Kourany 2010; Elliott 2017), scientists are not free from the responsibility of considering the consequences of erring in their professional judgment, including that of making inductively risky judgments and erring in those judgments. In other words, scientists do not get to (morally) make whatever professional proclamations they feel like making heedless of the chance they might be wrong and without considering the harm which their erroneous proclamations might cause. Since scientists are humans like the rest of us, we should not expect them to be perfect reasoners; nor should we expect omniscience from them. But they are also professionals like many of the rest of us, and on that basis we can (ethically) expect them to consider what is at stake when they make inductively risky professional judgments. Many such risks are unknown, but others are obvious; many are trivial, some catastrophic. It is fair to ask scientists to be careful of what is at stake when they are weighing whether or not to make a claim on the basis of their evidence.

Recognizably dangerous or costly claims might require more evidence in order to be responsibly made; apparently trivial or harmless claims might be responsibly made on the basis of less evidence.[7] The very trickiest of professional judgments will have to be made when the apparently severe risk of permitting a scientific claim to be made and being drastically wrong about it (e.g., "the MMR vaccine could be linked to autism") must be weighed against what appears to be an opposing set of similarly severe risks of not permitting the scientific claim to be made in case it turns out to be right after all (i.e., something Richard Horton and others at *The Lancet* were presumably worried about, when they decided to publish the Wakefield et al. [1998] paper[8]).

So, there are hard cases; the professional responsibility incurred by inductive risk will not always be a simple one for scientists to manage. But there are also much easier cases. Setting the more complicated ones aside, at the *very* least we can reasonably ask scientists *not* to lower their standards of evidence when making claims that are obviously, inductively risky for self-serving reasons such as the expected publicity associated with sensational science. And yet, this happens. As I will demonstrate using a case study of paleogenetic work on the Denisova, the phenomenon of sensational science awareness can irresponsibly *amplify* the inductive risk which individual scientists are willing to take, even when it is members of other, often quite vulnerable, populations who will bear the cost of that risk-taking (rather than the scientists themselves). In writing this paper, I also have a pair of practical aims: I am asking the scientists engaged in this practice to do better, as well as asking the scientific community to recognize this interactive phenomenon, and to consider implementing effective means of safeguarding against it.

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constitutes a revival is an easily substantiable one. Here is some evidence in the form of a quote from Douglas herself: "My focus for the role of values in science centers on Hempel's concept of 'inductive risk'" (2000, 560). The ease with which this claim can be substantiated (and the fact that I have substantiated it) distinguishes this claim from other recent, ever more popular claims that Douglas's *argument* is a revival of someone else's—i.e., Richard Rudner's (1953). More on this mistaken characterization of Douglas's work as Rudner's in note 37.

[7]For an alternative argumentative route than the one I pursue in this paper but supporting this same conclusion, see Kitcher (1997).

[8]Support for this interpretation of the editors' reasoning can be found in the ensuing correspondence and discussion, following publication of the original, which occurred in volume 351, issue 9106 (March 21, 1998); volume 351, issue 9112 (May 2, 1998); and volume 363, issue 9411 (March 6, 2004) of *The Lancet*.

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2. A bit of sensational science

In 1980, half of an archaic hominin mandible was discovered by a local (Xiahe county) monk in Baishiya Karst Cave (altitude 3,280 meters high, located on the Tibetan Plateau).[9] Later that decade, the bone was transferred to nearby Lanzhou University by a regional religious leader (Jigme Tenpai Wangchuk, the Sixth Gung-Thang Lama, a Living Buddha).[10] The partial humanoid jawbone included two molars of unusual shape and size. Because teeth are often preserved when other animal body parts are not, there is a relatively extensive paleontological record of fossil dentition, and fossil experts (paleontologists and paleoanthropologists alike) tend to recognize teeth. It is notable when they do not, so, this particular specimen was neither filed as familiar nor discarded, but rather sat unclassifiable in an ancillary collection at Lanzhou University for nearly thirty years.

In 2008, a fragment of the tip of an archaic juvenile hominin pinky finger bone (in technical terms, a fifth digit distal manual phalanx) was uncovered in Denisova Cave (altitude 700 meters high, located in the Altai mountains of Siberia). Researchers from the Institute of Archeology and Ethnography of the Siberian Division of the Russian Academy of Sciences in Novosibirsk have been excavating at Denisova Cave annually since the early 1980s (Serdyuk 2001). The presence of both Levallois and Mousterian stone tools, evidence of butchery and fire use, plus the excavation of human and nonhuman animal fossils all indicate that hominin use and perhaps occupancy of the cave might have begun by the end of the Middle Pleistocene, but was certainly in place by the beginning of the Late Pleistocene (Derevianko et al. 2005). In other words, Denisova Cave has likely been a site of hominin activity since sometime between 282,000 and 126,000 years ago.

The cave has an entrance zone, a large main chamber, and two narrow offshoots dubbed the East and South Galleries.[11] Until 1999, excavations were performed only in the central part of the cave (Serdyuk 2001) and occasionally the entrance zone (Malaeva 1998, cited in Rossina 2006). Published descriptions of hominin remains discovered in the cave began to appear by the early 2000s (i.e., Shpakova and Derevianko 2000, but it was not until these remains became an object of study by molecular geneticists working on ancient DNA that the site gained the widespread notoriety it has today. Reportedly, it was dig leader Anatoly Derevianko who, in 2008, decided to take the fingertip discovered at the site, split it, and send the pieces to two competing ancient DNA labs "to see whether DNA could be extracted from either half" (Callaway 2019, 176).[12]

Some biological molecules last longer and age better than others. Certain pigments can apparently last for hundreds of millions of years (e.g., Tanaka et al. 2014), but DNA usually degrades within 60,000 years (barring special circumstances, such as low temperatures [Briggs and Summons 2014]). It is not known why the fragment of pinky finger bone unearthed in 2008 was preserved like a specimen in permafrost,[13] but in April 2010, researchers from the Max Planck Institute for Evolutionary Anthropology in Leipzig, Germany, announced the sequencing (with high coverage—mean 156, low 2, high 602[14]) of a complete mitochondrial genome from "the

[9]The cave, located just north of a nearby monastery, is a Tibetan Buddhist sanctuary and pilgrimage site.

[10]For more on the "dedication" (paleo-jargon for "discovery") and transfer of the Xiahe mandible, please see Chen et al. (2019)—especially the associated NatureResearch reporting summary. For a lovely introduction to the Sixth Gung-Thang Lama, please see Chhosphel (2010).

[11]Please see Jacobs et al. (2019) for a helpful diagram of the cave layout.

[12]Analysis of the portion sent to Svante Pääbo's lab at the Max Planck Institute for Evolutionary Anthropology is what initiated the sequence of publications relayed in what remains of this section. The other portion, sent initially to Edward Rubin at Lawrence Berkeley National Laboratory (LBNL), appears to have been lost.

[13]"Thus, although many Neanderthals are preserved under conditions apparently similar to those in Denisova Cave, the Denisova phalanx is one of few bones found in temperate conditions that are as well preserved as many permafrost remains. It is not clear why this is" (Reich et al. 2010, 1059).

[14]One way to think about "coverage" is to think in terms of overlap. The more unique reads there are of any given portion of the genome, the more layers there are confirming the sequence as read at that point, adding what is sometimes called "depth" to the genome sequence. In this particular case, the coverage information is communicating that there were on average 156 unique

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Denisova hominin" (Krause et al. 2010).[15] By December 2010, a full-length article in *Nature*[16] announced the draft sequencing (with much lower 1.9-fold coverage) of a Denisovan nuclear genome, as well as the complete sequencing (mean 58-fold coverage) of another Denisovan mitochondrial genome (Reich et al. 2010).[17] On this basis, a new population of archaic hominins was declared discovered—a population that might have lived contemporaneously with both Neanderthals and anatomically modern humans, in some parts of Eurasia at least.

Over the next decade, additional details about Denisovan genetic characteristics and evolutionary history were frequently published in high-impact scientific venues and were usually accompanied by significant press coverage. A follow-up piece on the same Denisovan nuclear genome (but with better 31-fold coverage) was published in *Science* in October 2012 (Meyer et al. 2012). In January 2014, another paper was published in *Nature* about the sequencing of a bone fragment which was initially thought to be Denisovan, but then turned out to be Neanderthal instead (Prüfer et al. 2014).[18] A rather comprehensive article comparing sequenced mitochondrial and nuclear genomes from multiple Denisovan and other archaic hominin individuals was published in *PNAS* in December 2015 (Sawyer et al. 2015).[19] In March 2016, it was announced that 2,315 previously unidentifiable bone fragments from Denisova Cave had been analyzed for species membership via the method of collagen peptide mass fingerprinting (known as ZooMS)—revealing one hominin sample (DC 1227) tagged as Neanderthal via mitochondrial DNA analysis (Brown et al. 2016).[20] But in September 2018, another *Nature* paper announced that the nuclear genome sequenced from this individual revealed a significant share of Denisovan ancestry, thereby revealing that a hybrid offspring with a Neanderthal mother and a Denisovan father had been discovered (Slon et al. 2018). This individual is frequently called "Denny" in the press. Denny is a sliver of bone about two and a half centimeters long and less than a centimeter wide.

In July 2017, in between publication of these two papers on the hybrid specimen, another relevant report was published: one announcing the sequencing of the mitochondrial genome plus nuclear DNA fragments from what was at that point in time only the fourth Denisovan individual known to science (Slon et al. 2017).[21] Until early 2019, all known samples of purportedly Denisovan remains came from just the one spot in the Altai Mountains of southern Siberia. But in May 2019, what is now known as the Xiahe Mandible—that partial jawbone discovered in 1980 on the Tibetan

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reads for each nucleotide in the given sequence; that the nucleotide which was most frequently and uniquely read was read 602 times; and that there was at least one nucleotide which only showed up twice, in two distinct reads.

[15]This publication marks the introduction in the literature of genetic material from the specimen known as *Denisova 3*. It is a bone fragment which was unearthed in 2008.

Because the chronologies are quite complex, I will continue to use footnotes to link these genetic pronouncements with their corresponding archaeological basis, and the reader should be prepared for a rather confusing interspersal of these two archeaogenetic timelines. Newer genetic results often come from archaeological specimens that were ever-more previously unearthed, as both sites and specimen collections are mined by scientists eager to uncover further traces of the Denisova. A summarizing table is presented at the end of the section.

[16]The Krause et al. (2010) piece from April was also published in *Nature*, but it was a short letter.

[17]This publication marks the introduction in the literature of genetic material from the specimen known as *Denisova 4*. It is a molar which was unearthed in 2000.

[18]This publication marks the introduction in the literature of genetic material from the specimen known as *Denisova 5*. It is a bone fragment which was unearthed in 2010.

[19]This publication marks the introduction in the literature of genetic material from the specimen known as *Denisova 8*. It is a partial molar which was unearthed in 2010.

[20]This publication marks the introduction in the literature of genetic material from the specimen known as *Denisova 11*. It is a bone fragment "without any morphological features or evidence for purposeful modification" (Brown et al. 2016, 3), which was unearthed in 2014.

[21]This publication marks the introduction in the literature of genetic material from the specimen known as *Denisova 2*. It is an extremely partial molar which was unearthed in 1984.

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Plateau in Baishiya Karst Cave—was declared, on the basis of ancient protein analysis, to be Denisovan as well, or "closely related" (Chen et al. 2019).[22]

Please see Table 1 for a detailed list of the Denisovan specimens, with associated archaeological and genetic information.

3. The state of uncertain play

Already, this is an exciting story. Forty years ago, a monk finds a jawbone in a cave high up on the Tibetan Plateau. A long way away, in Siberia, another cave is painstakingly excavated for decades before a striking discovery is made. A pinky bone is preserved as though in permafrost and pristine DNA is unexpectedly extracted. A new group of archaic hominins is announced, one that potentially co-existed and even bred with us deep in our evolutionary past. And the link between the specimens from the two quite-distant caves implies that a previously unknown but surprisingly pervasive and far-flung population of these archaic hominins once existed. Not just the narrative but the science is exciting, too: after a tumultuous origin mired in technical difficulties, the field of ancient DNA re-emerges triumphant and with a focus on human evolutionary history (see Stoneking and Krause [2011] for a scientific review; Jones [2019] for an historical one; or Lewis-Kraus [2019] for a journalistic one). Not every subject of scientific study produces a rapid succession of papers so frequently published in the likes of *Nature*, *PNAS*, and *Science*. In short, this is science on the edge—the technical cutting edge, putting us on the edge of our seats, and peering over the edge of what is known into the unknown.

With such excitement and innovation comes speculation and uncertainty. In 2011, Ewen Callaway writes a news item for *Nature* called "Ancient DNA Reveals Secrets of Human History" (2011). Callaway gushes that "scientists are racing to apply the work to answer questions about human evolution and history that would have been unfathomable just a few years ago" (136), and Svante Pääbo is quoted at the end of the article: "Maybe we should write a little booklet called archaic genomics for dummies" (137). A little over two years later, however, Callaway writes another news item for *Nature*, this one entitled "Hominin DNA Baffles Experts" (2013). It begins "another ancient genome, another mystery" (16) and ends "[e]ven Pääbo admits that he was befuddled by his team's latest discovery. 'My hope is, of course, eventually we will not bring turmoil but clarity to this world', he says" (17).

Here are some scientific *facts* about the Denisova. One reconstructed molar, one partial molar, one extremely partial molar, one partial mandible, one fragment of the tip of a pinky finger, and one morphologically unidentifiable sliver of bone have all been labeled as Denisovan (or partly so).[23] Five of these six samples come from Denisova Cave in Southern Siberia; the sixth comes from Baishiya Karst Cave in Tibet. The difference in altitude between these two caves is approximately 2,500 meters and the caves are located approximately 2,500 kilometers apart. (Geologically speaking, these are big differences.) The teeth identified as Denisovan are bigger in size and irregular in shape when compared to those typically identified as Neanderthal, but they are

[22]I have some reservations about the molecular assignment of this particular specimen to the Denisovan population. U-series (uranium/thromium) dating tags the partial jawbone at about 160,000 years old, and no DNA could be recovered from the sample. However, traces of collagen were recovered from deep within one of the molars embedded in the mandible. Collagen is a reasonably likely candidate for molecular preservation here because it is one of the most stable proteins generally expressed in mammals. Using relatively new techniques of palaeoproteomic analysis (Welker 2018a, 2018b), a highly degraded endogenous ancient proteome was reconstructed from the sample and that sequence was compared to human, Neanderthal, and Denisovan collagen sequence data. One single amino acid polymorphism (SAP) previously unique to the single high-quality Denisovan nuclear genome (from *Denisova 3*) was detected therein. However, the recovered sequence also contained another SAP not found in any known reference population. See the supplement to Chen et al. (2019) for more.

[23]Exquisite pictures of some of these fragments were taken by Robert Clark for *National Geographic*. They first appear in the July 2013 issue of the magazine, accompanying an article by Jamie Shreeve entitled "The Case of the Missing Ancestor."

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Table 1. Each specimen name is accompanied by citation of the article in which that specimen is first declared Denisovan. Citations in other columns indicate the source of information being given.

Specimen	Description	Unearthed	Layer	Stratigraphic Age	Molecular Age	Mean Genomic Coverage (mt)	Mean Genomic Coverage (n)
Denisova 3[a]	Fifth digit distal manual phalanx	Denisova Cave, 2008	11.2 E[i]	69–48 ka[i]	82–74 ka[c]	156-fold[a]	31-fold[c]
Denisova 4[b]	Reconstructed molar	Denisova Cave, 2000	11 S[i]	less than 47±8 ka[i]	*Denisova 3* EQV[d]	58-fold[b]	INC, 1 Mb[d]
Denisova 8[d]	Partial molar	Denisova Cave, 2010	11.4 / 12.1 E[i]	132–93 ka[i]	*Denisova 3* + 60 kyr[d]	119-fold[d]	INC, 24.1 Mb[d]
Denisova 2[f]	Extremely partial molar	Denisova Cave, 1984	22.1 M[i]	328–246 ka[i]	*Denisova 3* + 54–99 kyr[f]	51-fold[f]	INC, 47 Mb[f]
Denisova 11[g]	Unidentified bone sliver	Denisova Cave, 2014	12.3 E[i]	150–118 ka[i]	*Denisova 3* + 5–10 kyr[g]	130-fold[e]	2.6-fold[g]
Xiahe Mandible[h]	Partial mandible with two molars	Baishiya Karst Cave, 1980	N/A	at least 160 ka[h]	N/A	N/A	N/A

Sources are as follows: a = Krause et al. (2010); b = Reich et al. (2010); c = Meyer et al. (2012); d = Sawyer et al. (2015); e = Brown et al. (2016); f = Slon et al. (2017); g = Slon et al. (2018); h = Chen et al. (2019); i = Jacobs et al. (2019). Abbreviations are as follows: E = East Gallery; EQV = equivalent; INC = incomplete; ka = thousand years ago; kyr = thousand years; M = Main Chamber; Mb = million base pairs; mt = mitochondrial; n = nuclear; N/A = no such information available; S = South Gallery.

somewhat similar in size and shape to some other Middle Pleistocene hominin specimens as well as to some *Homo erectus* specimens (Sawyer et al. 2015). The bone fragments are generally insufficient for meaningful analysis in terms of comparative morphology, except for the jaw fragment, which clusters within the known range of *Homo erectus* specimens along some dimensions (such as mandibular shape), but totally outside all known clusters along others (such as dental arcade; Chen et al. 2019). *Denisova 3*, the pinky finger fragment discovered in 2008, is stratigraphically estimated to be 69,000–48,000 years old (Jacobs et al. 2019); or 82,000–74,000 years old on the basis of DNA (Meyer et al. 2012). *Denisova 4*, the molar unearthed in 2000, is dated to less than 47,000±8,000 years ago on the basis of stratigraphy (Jacobs et al. 2019); but 82,000–74,000 years ago on the basis of DNA (Sawyer et al. 2015). *Denisova 8*, the partial molar discovered in 2010, is stratigraphically estimated to be 132,000–93,000 years old (Jacobs et al. 2019); this stratigraphic estimate is consistent with a DNA-based one (and this is the only instance of such agreement). The extremely partial molar known as *Denisova 2* was unearthed in 1984 and is tentatively dated on the basis of the stratigraphic layer in which it was purportedly found to 328,000–246,000 years ago (Jacobs et al. 2019).[24] On the basis of molecular (DNA-based) dating, it is 181,000–128,000 years old (Slon et al. 2017). *Denisova 11*, the hybrid bone fragment, is estimated on the basis of stratigraphy to be 150,000–118,000 years old (Jacobs et al. 2019); on the basis of DNA, it is estimated to be approximately 90,000 years old (Slon et al. 2018). The Xiahe mandible is estimated on the basis of dating the matrix which was attached to it to be at least 160,000 years old. There is not sufficient molecular information available, at least given current tech, to obtain a DNA-based estimate of its age.[25] Note the contradicting dates just relayed, which reflect a lack of agreement among findings derived via different methods and sources. The uncertainty reflected in the facts just shared often extends to the explanations and ideas which scientists have attempted to infer from these facts.

Here are some of the scientific *claims* that have been made about the Denisova. The Denisova might be a novel, independent population of archaic hominins who lived in parts of Eurasia at the same time as did some Neanderthals and/or anatomically modern humans (Krause et al. 2010). Or, they might not be; these specimens might just be partial, especially degraded fragments from members of previously known groups such as Neanderthals and/or anatomically modern humans (Caldararo 2010; Caldararo and Guthrie 2011a, 2011b, 2012). The Denisovan genome seems to contain a contribution (.5%–8%) from an otherwise unknown hominin group, but nothing else about this mysterious, potential ancestor is known (Prüfer et al. 2014). According to mitochondrial DNA, the Denisovan population diverged from present-day humans around a million years ago, about twice as long ago as Neanderthals did (Krause et al. 2010); but according to nuclear DNA, the Denisovan and Neanderthal populations diverged from present-day humans around half a million years ago, at about the same time (Reich et al. 2010; Sawyer et al. 2015). Initial estimates of the date of divergence between Denisovans and modern humans put the split between 1,313,500 and 779,300 years ago (Krause et al. 2010); that estimate was then narrowed to date the divergence between Denisovans and modern humans to 804,000 years ago (Reich et al. 2010). However, then it was widened again and altered to date the split to sometime between 700,000 and 170,000 years ago (Meyer et al. 2012). A combined estimate dates the split between Neanderthals and Denisovan on the one hand and modern humans on the other to either 589,000–553,000 years ago or 765,000–550,000 years ago (Prüfer et al. 2014). Finally, based on study of *Denisova 3*, 4–6% of the genome of "present-day Melanesians" might come from the Denisova (Reich et al. 2010); or perhaps 3.0%± 0.8% (Meyer et al. 2012). Study of *Denisova 8* also indicates some increased gene flow to Papuans and Australians relative to other non-African populations (Sawyer et al. 2015); but in the case of

[24]"The exact provenance of this tooth cannot now be established definitively" (Jacobs et al. 2019, 596).

[25]Note that Table 1 includes this information about specimen ages, and it can be re-examined in order to put that information in context with other details about each specimen.

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both *Denisova 2* (Slon et al. 2017) and *Denisova 4*, "there are not enough data to similarly detect gene flow" (Sawyer et al. 2015, 15699).

4. Race to publication

In Svante Pääbo's popular book *Neanderthal Man* (2014), there is a nice account of the initial decision to publish this finding—the one about the relatively greater contribution of the Denisova to the genomes of a few modern-day people from Papua and Bougainville, in contrast with some other modern-day persons from different regions. There was concern that the finding was a technical error, and corresponding debate about whether or not to publish the set of findings with or without that particular result. Pääbo quotes some correspondence among members of the research group:

> Adrian [Briggs] wrote an email saying "Surely publishing without the Papuan story risks the following: Someone will do their own analysis, find the Papuan admixture story, and publish it quickly. Why we didn't mention it ourselves will then be interpreted as a) incompetence, b) rushing, c) political correctness. Isn't that a problem?" Nick [Patterson] agreed, saying "We have to deal with the Papuan issue or we will look like fools or cowards." (2014, 246)

Clearly, these scientists are capable of considering *some* risks associated with their making or failing to make candidate scientific claims. Regrettably, in this exchange, the only conceptual option associated with knowing about the result, and yet not publishing it, is that of the easily dismissed "political correctness." And the scientists' overwhelming focus with respect to the risks they are taking is on themselves and their reputations, rather than elsewhere—such as with the other, living Papuans whom they are discussing.

In the introduction to this paper, I presented and discussed two philosophical aims: (1) to characterize the phenomenon of sensational science awareness and (2) to document the interaction of that phenomenon with the philosophically much more familiar one of inductive risk—an interaction which I characterize as generating amplified inductive risk. But I have two further, more practical aims, which I mentioned only very briefly: (3) to increase awareness of the phenomenon of inductive risk amongst scientists of this particular domain, that of paleogenetics, and (4) to encourage the members of this domain to seek some collective action which might save individual members from needing to privilege, quite so heavily, their consideration of the risks they might be taking on for themselves and their reputations over the risks they might be imposing on others. In other words, I aim to demonstrate to these scientists that there is more to considering the ethical, moral, political, and social consequences of their claims than mere "political correctness." And to gently remind them that caring only about how something will impact you is really not a good look.

Figure 1 provides a demonstration of one way in which the publication of this kind of scientific result impacts other people. The caption reads "Melanesians like these Mekeo tribesmen in Papua New Guinea show genetic evidence of ancient Denisovan ancestry."[26] I genuinely do not want to

[26]Note that the term *Melanesian*, used both in the caption of this photo printed in *Science* and throughout the paleogenetic literature on this topic (e.g., Reich et al. 2010; Meyer et al. 2012), means "a native or inhabitant of Melanesia" (from the *Oxford English Dictionary*, 3rd ed, June 2001). The term *Melanesia* was coined by the French naval officer Jules-Sébastien-César Dumont D'Urville in his "On the Islands of the Great Ocean" (1832; translated from the French in 2003 by Isabel Ollivier, Antoine de Biran, and Geoffrey Clark). D'Urville partitioned Oceania into four main components, introducing Melanesia as such: "Southern Oceania is the fourth and last division. It encompasses the great island of New Holland [Australia], and all land in its vicinity as far as the fringes of Micronesia and Polynesia. As this is the home of the black race of Oceania, it shall be called *Melanesia*. Mr Bory de Saint Vincent had already suggested calling this variety of the black people of Oceania *Mélanians*, and I will gladly keep this idea, while widening its scope" (D'Urville 1832/2003, 165; italics original).

As is apparent, this terminology carries racial connotations. For more on the problematic use of this and related terms, please see the special issue of *The Journal of Pacific History* (Vol. 38, No. 2) in which the d'Urville translation was printed. Regrettably, I

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Figure 1. On the left, a captioned photo which ran in volume 352, issue 6282 of *Science* in April of 2016; on the right, the photo in context with its accompanying news item (Zahn 2016). Note that the alt text generated by Microsoft Word for this photo was "A picture containing tree, outdoor, plant." For informative discussion of algorithmic bias, please see Bozdag (2013), Danks and London (2017), or Biddle (2020), among many others. (From Zahn, Laura M. 2016. "Denisovan DNA Retained in Melanesians." *Science* 352 (6282): 183. Reprinted with permission of AAAS. Readers may view, browse, and/or download material for temporary copying purposes only, provided these uses are for noncommercial personal purposes. Except as provided by law, this material may not be further reproduced, distributed, transmitted, modified, adapted, performed, displayed, published, or sold in whole or in part, without prior written permission from the publisher.)

presume, so instead I am just going to ask: Is this what the person whose genome was initially sequenced for the purposes of these analyses looked like (described in Pääbo 2014, 183)? Is it what the second Papuan, and the third person from the island of Bougainville—those who served to provide supplementary data—looked like (Pääbo 2014, 245)? Were those three people a trio of Mekeo "tribesmen," standing out in a field of greenery, resplendent in their "tribal" gear?[27] I am concerned that this is *not* a picture of the people whose genomes were sequenced for this study, nor is it of the setting in which they were sequenced. Again, I do not want to presume, but it rather looks like a heavily stereotyped, very selective, and correspondingly misrepresentative picture instead—one which trades on some of our most tired Western tropes and biases, while not-so-subtly sending a rather racist message about the apparently "ancient" genetics of the Mekeo and other Papuan people who, *Science* tells us, happen to have quite a bit more archaic hominin or non-anatomically modern human DNA than the rest of us.

do not know how to critique this paleogenetic discourse without myself using this and many of the other colonialist (e.g., "Bougainville") and/or folk racial (e.g., "Papuan") terms which are often used throughout this discourse in order to refer. Nor do I know how to refer to the purported populations and/or regions which are often presumed in this discourse to exist without myself replicating what looks like a presumption of the reality of these purported populations and/or regions. For my participation in these practices, even with the aim of critique, I can only apologize and say that I hope in the future to have devised a better way.

[27] According to the website of the Human Genome Diversity Project (HGDP), which is hosted by the Centre d'Etude du Polymorphisme Humain (CEPH), the Papuan samples are from coordinates 4 south, 143 east (in the East Sepik province of northwest Papua New Guinea); whereas the Bougainvillean sample is from 6 south, 155 east (near Mount Balbi on the Island of Bouganville). Neither of these locations is in or near the region of the Mekeo people (in the Central Province on the southeast coast of Papua New Guinea). Visit the HGDP-CEPH website (http://www.cephb.fr/hgdp/main.php) and click the "Population panel" link to access this information. I am not entirely confident of its salience and provenance, however, so I will keep this discussion tentative and ask my questions rather than presume to know the answer.

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5. The argument from inductive risk

Time for some philosophical nuts and bolts—for deployment of the machinery needed to transform thinking about the potentially racist impacts of scientific work from political correctness to professional responsibility. In 2000, the philosopher of science Heather Douglas published a paper in *Philosophy of Science* called "Inductive Risk and Values in Science." In it, she revived Hempel's (1954, 1960) concept of inductive risk and then used that concept to make an argument. Since Douglas does not herself christen the argument, I will henceforth refer to it as "the argument from inductive risk" merely as convenient shorthand for "the argument from 'Inductive Risk and Values in Science,' written by Heather Douglas and published in *Philosophy of Science* in the year 2000."[28]

There are three crucial components to the overall argument contained in Douglas (2000). The first occurs in the following passage:

> To claim that scientists ought not consider the predictable consequences of error (or inductive risk) is to argue that scientists are somehow not morally responsible for their actions as scientists. To defend a completely "value-free" science would require such a move, one which seems to be far more dangerous than openly grappling with the role of values in science. Arguing that scientists have the same moral responsibilities as the rest of us is beyond the scope of this paper. (563)

This argumentative component can be validly, deductively formulated: for example, as two instances of *modus tollens*, or as one instance of hypothetical syllogism followed by one instance of *modus tollens*. As written, the argument is enthymemic (it relies on an implicit premise), but the missing step in the argument is easily supplied:

1. "To claim that scientists ought not consider the predictable consequences of error (or inductive risk) is to argue that scientists are somehow not responsible for their actions as scientists."
2. IMPLICIT: To claim that scientists are somehow not responsible for their actions as scientists is to argue that scientists do not have the same moral responsibilities as the rest of us.
3. "[S]cientists have the same moral responsibilities as the rest of us."

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4. DERIVED: Therefore, it is not the case that "scientists ought not consider the predictable consequences of error (or inductive risk)."

Note that this conclusion can be more simply stated: scientists ought to consider the predictable consequences of error (or inductive risk). Note also that this restatement is logically equivalent; it simply removes the double negative. Since the argument is deductively valid, whether or not we accept this conclusion depends entirely on its premises. These premises get some defense in Douglas (2000), but perhaps not as much defense as they require. The author is especially curt when it comes to defending premise 3; recall her entertaining statement that "Arguing that scientists have the same moral responsibilities as the rest of us is beyond the scope of this paper" (563).

But in a 2003 paper ("The Moral Responsibilities of Scientists"), Douglas clarifies the meaning of premise 3, and she defends the premise extensively in her 2009 book, *Science, Policy, and the Value-Free Ideal*. What Douglas means when she says that "scientists have the same moral responsibilities as the rest of us" is actually that scientists are not excused from their general moral responsibilities (as persons) by their special role responsibilities (as scientists)—contra, for instance, Bridgman (1947) or Price (1965). In the case of inductive risk, what this means is that scientists are not excused by their status as scientists from their general moral responsibility to consider what might

[28]The argument has been otherwise dubbed, e.g., "the error argument" (Elliott 2011a); "the methodological critique" (Betz 2013).

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predictably follow from their saying something false. There are some circumstances, Douglas admits, in which special role responsibilities can trump this sort of general moral responsibility, but in the case of scientists and this particular sort of risk: "the social structures that would allow for such a reduction in general moral responsibilities are not in place" (2003, 60). So, if one accepts that there is a general moral responsibility of persons to consider what might predictably follow from their saying something which is potentially false (in deciding whether or not they have sufficient reason to say it), and one accepts that scientists are not specially excused from this general moral responsibility because of their role as scientists, then "scientists have the same moral responsibilities as the rest of us."[29]

Douglas's account of the general moral responsibility of persons to consider what might predictably follow from their saying something which is potentially false (in deciding whether or not they have sufficient reason to say it) is grounded in the concepts of recklessness and negligence (following Feinberg 1970). Douglas's account of why scientists are not excused from this general moral responsibility by their scientific role responsibilities is grounded in two further points: first, that "someone must be responsible for thinking about the potential consequences at these decision points or the general responsibilities go completely neglected" (Douglas 2003, 64); and second, that scientists themselves are the only sufficiently informed and appropriately positioned persons capable of performing this task. Hence, scientists have the same general responsibilities as the rest of us—including that of considering what might predictably follow from saying something potentially false (in deciding whether or not there is sufficient reason to say it)—and they are not excused from this responsibility by their status as scientists.[30]

The next crucial component of the argument from inductive risk occurs on page 565 of Douglas (2000). In the prior component, Douglas stressed the responsibility which scientists have to consider *predictable* consequences of their potential errors in judgment, including those errors of deciding to say something which turns out to be false. In this new component, Douglas stresses those consequences which are both *clear* and *nonepistemic*. And, Douglas argues, considering *consequences* which are nonepistemic requires considering *values* which are nonepistemic.[31] Here is the relevant text:

> In cases where the consequences of making a choice and being wrong are clear, the inductive risk of the choice should be considered by the scientists making the choice. In the cases I discuss below, the consequences of the choices include clear non-epistemic consequences, *requiring* non-epistemic values in the decision-making. Thus, where the weighing of inductive risk requires the consideration of non-epistemic consequences, non-epistemic values have a legitimate role to play in the internal stages of science. (2000, 565; italics original)

[29]I have by now taught this argument to hundreds of budding science and engineering students, and you might be surprised by how few of them really want to assert that scientists are morally set apart from others—that they do not have these same moral responsibilities as everybody else. I expect my students detect that this would trigger an instance of what the social epistemologist Joshua Blanchard (2020) has recently dubbed "the problem of unwelcome epistemic company"—a feeling of "guilt by epistemic association" with other, recognizably problematic cases, figures, or ideas.

[30]In fact, one might think (perhaps because of their status as experts, their membership in a professional class, and their corresponding public authority) that scientists speaking as scientists (say, in the pages of a scientific journal) have *more* of this sort of responsibility than "general" persons do, or even than they themselves have when they are speaking just as persons and not as scientists. This would mean that not only are scientists *not* excused by their role as scientists, but rather that their role either as scientists or even just as professionals means that this general moral burden falls *extra heavily* on them. See Hardimon (1994) for additional, philosophical discussion of the moral status of role responsibilities and obligations; see Hunt (2010) for discussion of this issue as it pertains to academics, as an entailment of their academic freedom.

[31]Properties such as accuracy, precision, explanatory scope, and predictive power typically serve as paradigmatic examples of epistemic value. The much broader category of nonepistemic values tends to span aesthetic, ethical, moral, political, and social properties. Douglas's own view of this distinction is complicated by her involvement with the category of cognitive value; see Douglas (2013) for discussion. See Anderson (2004) for an especially potent challenge to the paradigmatic way of drawing the distinction. It might be better to follow Longino (1990, 2002) in calling these constitutive versus contextual values.

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Again, this argumentative component can be deductively, validly formulated. If we think of Douglas's emphasis on consequences which are both clear and nonepistemic as conditional specifications of when her initial conclusion about predictable consequences applies, and we consider her move from considering consequences to considering values as an entailment, then we can structure this argumentative component in a valid way with moves as simple as conjunction and *modus ponens*.

Interpreted in this manner, the argument says: if consequences are not just predictable but also clear, then inductive risk should be considered; if consequences are nonepistemic, then those considerations of inductive risk should be nonepistemic as well; and here (the argument points to them) are some cases with consequences that are both clear and nonepistemic. Thus, inductive risk should be considered, and these considerations should be nonepistemic. Since all of this is happening in what have often been presented as the "internal" stages of science, what this means is that we have nonepistemic values being considered (and responsibly so) even in those "internal" stages.[32] And that's it for the (supposed!) last stand of the value-free ideal. Hewing close to the text now (Douglas 2000, 565):

5. "In cases where the consequences of making a choice and being wrong are clear, the inductive risk of the choice should be considered by the scientists making the choice."
6. "[W]here the weighing of inductive risk requires the consideration of non-epistemic consequences, non-epistemic values have a legitimate role to play in the internal stages of science."
7. "In the cases I [Douglas] discuss below, the consequences of the choices include clear non-epistemic consequences[.]"

---

8. DERIVED: Therefore, in the discussed cases, "non-epistemic values have a legitimate role to play in the internal stages of science."

Premise 5 is a qualified restatement of conclusion 4; it is licensed as long as the preceding argumentative component has been. Premise 7 is amply demonstrated by Douglas's (2000) case study involving rat tumors, liver slides, and the chemical dioxin. That leaves premise 6, the inference from nonepistemic consequences to nonepistemic value considerations, as a new claim requiring at least some defense. The idea here is that nonepistemic impacts of professionally erring in judgment necessarily incur consideration of nonepistemic values—at least, when such impacts are both clear and predictable. Douglas claims that in such cases and in order to determine how to handle the risk of professionally erring in judgment, scientists in their professional capacity must consider the nonepistemic impacts—a.k.a. the (dis)value—of such error.

One could attempt to avoid this entailment from the known, clear existence of nonepistemic impacts to the necessary consideration by scientists of those impacts and their (dis)value in uncertain cases. In recent work pursuant of this line of thought, Douglas (2021) explores the space of possible responses and presents four alternatives which might be adopted instead: (i) using random processes, such as coin flipping, to settle uncertain cases; (ii) using only epistemic values,

[32]The so-called "internal" questions of scientific practice typically include how to make observations, how to characterize data points, how to set standards of evidence, and how to interpret results (see, e.g., Douglas 2000, 565). In contrast, the "external" questions are generally thought to include how to train scientists, how to establish who gets to be a scientist, how to foster study of some research problems over others, how to restrict experimentation on human and animal research subjects, and how to apply research findings in society (see, e.g., Douglas 2000, 559).

Many have objected to this distinction; see Latour and Woolgar (1979) for an especially early challenge to the internal/external divide. Here, my reference to the distinction is meant to highlight an argumentative strategy rather than act as an endorsement. Historically, the internal/external distinction is one that has often been appealed to as a means of acknowledging that contextual values *can* legitimately play a role in science, but without actually granting that this is a problem for the value-free ideal (or VFI). What is worth noting here is that *even if* we grant this distinction to a defender of the VFI, we can still show contextual values acting in the so-called "internal" stages. That does not necessarily mean we ought to grant the distinction.

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like accuracy or consilience, to respond to nonepistemic risks of error; (iii) asking scientists to remain agnostic in all cases of uncertainty, limiting themselves to the communication of degrees of certainty and lack thereof; or (iv) licensing scientists to settle only small uncertainties, while requiring they disclose all larger ones.

The irony is that none of these responses actually avoid the consideration of nonepistemic values in the process of their adoption (when their adoption is possible). This is because none of these are normal, or default, procedures in science, and the choice to adopt them is motivated, in this context at least, by consideration of nonepistemic values. When faced with an ambiguous piece of observational data,[33] scientists do not typically (i) flip a coin in order to decide how to record their observation.[34] Deciding to deviate from normal procedure in such a manner, and because of nonepistemic risk, just is to consider nonepistemic (dis)value, not in how to score the observation itself, but in what procedure to adopt for scoring.[35] The same can be said for pursuit of option (ii), using epistemic values to respond to nonepistemic risks and (dis)value. Say a scientist would normally feel free to speculate about historical migration patterns among geese solely on the basis of molecular data. But when it comes to historical migration patterns among archaic hominins, scientists might alternatively decide that due to the foreseeable interpretive impact such speculations might have and given the risk of error, there must be consilience between available sources of molecular and morphological data before such speculations can be published. This line of reasoning emphasizes the epistemic value (of consilience) over alternatives (such as pace of inquiry), but it still considers nonepistemic values (like special respect for persons) in the process of choosing which epistemic values to highlight (in this case, consilience over pace). Note that the pervasiveness of induction means it is not even close to possible for scientists to (iii) remain agnostic in all cases of uncertainty; and in many cases, (iv) disclosing all and only the larger uncertainties while remaining agnostic about them will not be possible either (e.g., as in the case of climate science projection and advising [Havstad and Brown 2017]).[36] In cases where response (iv) is possible, when attempted, it will again be a deviation from the norm, adoption of which is being motivated by consideration of nonepistemic values.

In sum, none of these attempts to reject premise 6—the claim that, when inductive risk requires consideration of nonepistemic impacts, nonepistemic values have a legitimate role to play in the internal stages of science—actually succeed either in being possible or, when possible, in eliminating a legitimate role for nonepistemic values even in the internal stages of science. Recall that this component of Douglas's argument from inductive risk is deductively valid. If its premises have all been successfully defended, then it must be admitted as sound. Or, to state the contrapositive: because this argument is deductively valid, if you want to reject the conclusion of this argument, then you need to find a premise to reject.

The third and final crucial component of the overall argument contained in Douglas (2000) is the explicit establishment of which conditions do and do not trigger the argument from inductive risk—at least, as demonstrated in this piece of writing. Recall the narrowed scope of premise 5: "In cases when the consequences of making a choice and being wrong are clear" (2000, 565). And that of

[33]Say, a petri dish with an ambiguous growth on it.

[34]Heads for strep, tails for staph—anyone?

[35]Douglas calls this manner of consideration of nonepistemic values indirect, rather than direct, consideration. She writes that values play a direct role settling scientific questions when they act as "reasons in themselves to accept a claim" (2009, 96). But they serve in merely an indirect role when they apply instead to certain higher-order epistemic considerations—when they act as reasons for setting a scientific standard in a certain way or adopting one scientific method over another. Some (e.g., Elliott 2011b) have challenged the coherence of this distinction. I understand it as follows: when playing a direct role, values are reasons that apply straightforwardly to scientific claims themselves; when playing an indirect role, values are reasons that apply to the methods of generation of scientific claims, or to the standards by which scientific claims are evaluated.

[36]Though see Mitchell (2004) and Betz (2013) for attempts to pursue this line of response to Douglas's (2000) argument; see Jeffrey (1956) for a similar type of response to Rudner's (1953) earlier contribution.

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premise 6: "[W]here the weighing of inductive risk requires the consideration of non-epistemic consequences" (565). Based on the targeting of this discussion towards predictable impacts that are both clear and nonepistemic, the author partitions science into four regions or zones. This occurs on pages 577–578, as Douglas concludes her piece:

A. "When there is very low uncertainty, such that a scientist believes there is virtually no chance of being wrong."
B. "[W]here making a wrong choice has no impact on anything outside of that area of research."
C. "[W]here the science will likely be useful but the potential consequences of error may be difficult to foresee."
D. "[W]hen non-epistemic consequences of error can be foreseen."

It is in zone D that Douglas's overall argument conclusively applies, demonstrating that "non-epistemic (i.e., social, ethical, political) values … are a required part of the internal aspects of scientific reasoning" (559). Zone C is a "gray area" (578)—one that will have to be decided on a case-by-case basis. Zones B and A are exempt from the force of the argument.[37]

[37]As mentioned in note 6, it has become ever more common to characterize Douglas's (2000) argument as a revival, reiteration, or rediscovery of Rudner's (1953); to claim that her argument is not new; to describe Rudner's argument from inductive risk as the canonical one; or, just to fail to mention Douglas at all, even when discussing inductive risk, and how it bears on the question of the relationship between science and values. I have no wish to be an uncharitable reader or an unkind person; nor do I wish to chastise or offend my colleagues. But I do wish to put a stop to this regressive practice of reattributing credit for Douglas's argument from inductive risk away from Douglas herself and towards Rudner.

It is not difficult to logically differentiate the two arguments (in Rudner 1953 and Douglas 2000). There are two crucial components to Rudner's argument, both of which occur on page 2 of his piece. Here is the relevant portion of the text:

> Now I take it that no analysis of what constitutes the method of science would be satisfactory unless it comprised some assertion to the effect that the scientist as scientist accepts or rejects hypotheses.
>
> But if this is so then clearly the scientist as scientist does make value judgments. For, since no scientific hypothesis is ever completely verified, in accepting a hypothesis the scientist must make the decision that the evidence is *sufficiently* strong or that the probability is *sufficiently* high to warrant the acceptance of the hypothesis. Obviously, our decision regarding the evidence and respecting how strong is "strong enough," is going to be a function of the *importance*, in the typically ethical sense, of making a mistake in accepting or rejecting the hypothesis. (italics original)

The first argumentative component can be simply, deductively, and validly formulated as an instance of *modus ponens.* Here it is:

1. "[T]he scientist as scientist accepts or rejects hypotheses." (Rudner 1953, 2)
2. "But if this [the scientist as scientist accepts or rejects hypotheses] is so then clearly the scientist as scientist does make value judgments." (2)

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3. DERIVED: Therefore, "scientists as scientists *do* make value judgments." (2)

Since this is a valid argument, its soundness depends entirely on the truth of its premises. Premise 1 is treated as uncontroversial by Rudner. Premise 2 is supported via another argumentative component, the second crucial component of Rudner's overall maneuver. This second component can also be validly, deductively formulated, for instance, as a hypothetical syllogism:

4. "[I]n accepting a hypothesis the scientist must make the decision that the evidence is *sufficiently* strong or that the probability is *sufficiently* high to warrant the acceptance of the hypothesis." (Rudner 1953, 2)
5. "Obviously our decision regarding the evidence and respecting how strong is 'strong enough', is going to be a function of the *importance*, in the typically ethical sense, of making a mistake in accepting or rejecting the hypothesis." (2)

---

6. DERIVED: Therefore, "in accepting a hypothesis the scientist must make a decision" (2) that "is going to be a function of the *importance*, in the typically ethical sense, of making a mistake in accepting or rejecting the hypothesis." (2)

Once again, since this argument is valid, its soundness will be a function of the truth of its premises. Premise 4 is supported by Rudner's supposition that "no scientific hypothesis is ever completely verified" (2). Presumably, this a straightforward reflection

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6. Amplified inductive risk

Scientific research which extracts molecular information from Denisovan specimens and compares that archaic genomic data to current genomic data, attributing more nonanatomically modern human ancestry to some populations than others, is zone D science—science in which negative, nonepistemic impacts of error can be readily foreseen. It is also science for which the chance of error is quite high since (among other concerns) there is only one high-coverage nuclear genome (from *Denisova 3*).[38] The initial studies, those that attribute significant Denisovan ancestry to Papua New Guineans compared to other non-African populations such as the French (Reich et al. 2010; Meyer et al. 2012), as well as more recent follow-up studies (Skoglund and Jakobsson 2011; Qin and Stoneking 2015; Vernot et al. 2016), are all based on genetic material obtained from one tiny bone fragment (part of the tip of a child's pinky finger). Yet it is an utter mystery how so much genetic information, and of such high quality, was obtained from that particular sample. The scientists themselves have remarked on this fact—e.g., "No one thought we would have an archaic human

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of the unfortunate fact that much of scientific reasoning is inductive; the rest of premise 4 is an uncontroversial statement of what inductive reasoning entails. Premise 5 is elaborated, supported, and emphasized by Rudner with two contrasting scenarios. As he puts it:

> Thus, to take a crude but easily manageable example, if the hypothesis under consideration were to the effect that a toxic ingredient of a drug was not present in lethal quantity, we would require a relatively high degree of confirmation or confidence before accepting the hypothesis—for the consequences of making a mistake here are exceedingly grave by our moral standards. On the other hand, if say, our hypothesis stated that, on the basis of a sample, a certain lot of machine stamped belt buckles was not defective, the degree of confidence we should require would be relatively not so high. *How sure we need to be before we accept a hypothesis will depend on how serious a mistake would be.* (Rudner 1953, 2; italics original)

As Rudner himself points out, these examples are drawn "from scientific inferences in industrial quality control" (2).

Here are some of the most significant differences between the two arguments: Rudner's pair of examples are hypothetical and illustrative sketches (he calls them crude!); whereas Douglas (2000) presents a detailed example from actual scientific practice (dioxin cancer studies in rodents). Rudner's two sketches are drawn from industrial quality control, whereas Douglas's real case regards the characterization of observational data (which matters in terms of where the relevant values are playing a role). Douglas has an entire argumentative component which Rudner lacks—one which establishes the relevant responsibility of scientists in general moral responsibility along with a failure to be excused from that general responsibility on the basis of special role. And Douglas does further work which Rudner fails to do: that of specifying under what conditions the clear and predictable nonepistemic consequences of error trigger the consideration of nonepistemic values as a requirement. This specification is realized via her zoning system, which clarifies for scientists when and how inductive risk comes into play.

But most crucially, we can see that the two arguments are significantly different when we look at their differential effects—at their relative impacts and uptake. In his time, Rudner's critics carried the day: the responses to his argument offered by the likes of Richard Jeffrey (1956), Isaac Levi (1960, 1961), and eventually Ernan McMullin (1983) quieted the issue of these sorts of values playing this sort of role in those parts of science. But these prior responses to Rudner have not sufficed to rebut Douglas's more recent argument. Those same old responses will not do today because Douglas' argument is importantly different, and better.

[38]As just noted, the argument from inductive risk requires uncertainty, or chance of error, in order to get off the ground. On that basis, I am here arguing that uncertainty, along with other relevant actors, entails a need for caution. But this position does not logically imply that in the absence of such uncertainty no such caution is similarly warranted. In other words, specifying what is required in cases of uncertainty does not tell us what is (or is not) required in cases of certainty. It leaves open the question of whether or not paleogeneticists should make racial claims about how much more or less archaic various genomes are even when they are utterly certain about those claims.

A further, related note: it is somewhat common for paleogeneticists to speak of the supposedly many answers which their field can provide, but which archeology and anthropology cannot. However, there are some answers which archaeology and anthropology are failing to provide because members of those fields have collectively decided that the questions to which those answers belong are not well-formed. Not making certain kinds of claims about race is an example of this sort of refusal: given the lack of a coherent scientific consensus on how to understand race, coupled with the fact that such claims will reliably and mistakenly be interpreted in racially essentialist and deterministic ways, many anthropologists and archaeologists have decided to abstain from engaging in this practice (e.g., American Anthropological Association 1998). It is misleading to present this sort of refusal to participate in racialized dialogue as a technical inability to contribute. For a methodological critique of paleogenetics' (lack of) relationship with archaeology, see Downes (2019).

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genome of such quality" and "Everyone was shocked by the counts. That includes me" (Matthias Meyer, quoted in Gibbons 2012, 1028). There are other significant sources of uncertainty, as well, such as the fact that the paucity of morphological data means consilience between molecular and morphological data can barely be sought, much less obtained. There are also pressing geographical questions to answer, such as how the Denisova—purportedly a group of hominins specially adapted to the high-altitude Tibetan Plateau (Huerta-Sánchez et al. 2014)—managed to make more of a genetic contribution to an extreme low-lying island population than to other, closer populations, and to a population living 5,000–7,500 kilometers away from where any of the Denisovan remains have been found, much of that distance across open water.

Given the uncertainty in this domain, the corresponding chance of making false scientific claims and the pernicious interpretive implications likely to be erroneously drawn—i.e., the utterly foreseeable attempts at scientized racism—one might expect that the scientists working in this domain would consider raising their standards of evidence in response to the predictably pernicious consequences which their risky racial pronouncements might erroneously entail. In other words: if what you are thinking about scientifically claiming is going to predictably add further fuel to the briskly burning racial fire, then you might want to make extra sure that what you are saying is scientifically necessary, justified, and correct.[39] Of course, as we have already seen, if practitioners are not aware of the argument from inductive risk, then such considerations of impacts and consequences can sometimes and unfortunately be quickly dismissed as mere political correctness. But the argument itself is not so easy to rebut. There is also the matter of pressing matter of incentives, such as what sort of considerations might incentivize practitioners to waive away thought of consequences, caution, or concern for others.[40]

The science being discussed here is not just zone D science, or science for which the argument from inductive risk decisively applies; it is also predictably *sensational* science. Predictably sensational science is science which practitioners can foreseeably expect to capture and sustain public interest in a way that is likely to foster its development and to amplify the publication and prestige of its results. We need an additional, complementary zoning scheme:

I. When there is very low public interest in a scientific issue, such that the science has virtually no chance being sensationalized.
II. Where the evidence pertaining to a scientific issue is so decisive in terms of either its absence or presence that sustained, public speculation about that issue is nigh impossible.
III. Where there is enough evidential ambiguity pertaining to a scientific issue such that sustained speculation is possible, but the degree of public interest in that issue is also so ambiguous that sensationalization is difficult to predict.
IV. When there is overwhelming public interest in a scientific issue, and speculation about that issue can be predictably fueled by at least some, but still scant, availability of evidence.

Just as Douglas's work on inductive risk identifies four zones in which its conclusion is of more or less pressing concern, so too does this analysis of sensational science pick out different regions in which its force is more or less likely to be felt. Just as Douglas's first two zones are ones in which

[39]For instance, in the particular case of scientific estimations of the Denisovan contribution to the genomes of "present-day Melanesians," if Reich et al. (2010) had waited for further corroboration, such as that which was shortly thereafter provided by the far higher-coverage nuclear genome of Meyer et al. (2012), then the figure that entered the literature first—around which public discourse has predictably anchored—could have been 3%, instead of 6%. Waiting until there was a properly scientific rather than folk racial account of the term *present-day Melanesians* might have produced an even better result (since, I suspect, there isn't one).

[40]In what remain of the piece, I will talk of sensationalization and what I am calling *sensational science awareness* by scientists, but there are other nearby issues also of interest. For a now-classic piece on the popularization of science, see Hilgartner (1990). For some very recent discussions of scientific "hype," see Intemann (2020) as well as Jones and Bösl (2021).

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inductive risk is not of much concern, so too are my first two zones those in which the matter of sensationalization will likely be moot. Just as Douglas's zone C is a gray area, one in which the influence of inductive risk will likely need assessment on a case-by-case basis, so too will sensationalization perhaps or perhaps not be in play for my zone III. And, for both of us, it is in our fourth and final zones that the phenomena we are characterizing (inductive risk in Douglas's case, sensationalization in mine) are obviously in play.

In Douglas's first zone (or zone A, as I am calling it), there is a lack of the scientific uncertainty required for the argument from inductive risk to get off the ground. In zone B, there are no nonepistemic impacts to worry about, even in the case of whatever error might be generated by scientific uncertainty. In zone C, there is the requisite scientific uncertainty, along with probable nonepistemic impacts, but the unpredictability of those impacts makes it difficult to hold scientists responsible for those impacts, even in the case of error. And in zone D, there is all of what is required for the argument from inductive risk to decisively apply: scientific uncertainty, nonepistemic impacts, and predictability. Similarly, in my first zone (zone I), there is a lack of the sort of public interest required for sensational science. In my zone II, the evidential situation is unlikely to sustain the sort of speculation that tends to fuel sensational science. In my zone III, there is the potential for public interest, along with enough evidential ambiguity to make speculation possible, but the unpredictability of the interaction between these two factors makes it difficult to anticipate the sensationalization (or not) of the relevant science. Finally, in my zone IV, there is predictably sensational science: science characterized by overwhelming public interest and just the sort of scant evidential situation to fuel enduring speculation.[41]

Douglas's argument from inductive risk is fueled by the risk of error conferred by the following three factors: scientific uncertainty, the existence of nonepistemic impacts, and the predictable foreseeability of those impacts. Sensational science is alternatively fueled: by overwhelming public interest, at least some evidential ambiguity, and a steady enough stream of scientific discoveries (just enough to keep the press releases coming, while still leaving plenty of uncertain room for breathless and ongoing speculation). And, if we put these two phenomena together—inductively risky considerations with sensational science potential—what we get are cases of *amplified* inductive risk: scientific cases in which inductively high-risk pronouncements are nonetheless driven by the promise of heightened public interest, just enough of a scant evidential situation to fuel speculation, and the predictably foreseeable sensationalization of the relevant science.[42]

Science that is zoned D + IV is extremely sticky science. The argument from inductive risk encourages scientists to be extra careful about the judgments they make in this domain due to the high chance of error, and the predictable nonepistemic impacts. But here, the potential for sensationalization also encourages scientists to be more reckless than usual since they are quite likely to obtain support for their research, as well as gain publicity and prestige for their results given the public interest likely to capture initial attention along with the scant evidential situation likely to sustain enduring speculation. In these cases of amplified inductive risk, just when we want scientists to be *more* cautious than usual (given the argument from inductive risk), they are instead heavily incentivized to be *less* cautious than usual (due to the predictable sensationalization of the science). I suspect that scientific study of the Denisova is appropriately tagged as both D- and IV-zone science,

[41]I suspect that sustained public interest is fed by sustained public engagement. Frequent yet fresh announcements, features, and speculation help a topic stay current and featured. In this (scientific) case, the conversation is sustained by uncovering new evidence often enough to constantly announce new "discoveries," but not uncovering so much that the science gets settled—resolving all issues and bringing new work to a halt.

[42]There is a bit of tricky ambiguity somewhat inherent in talk of inductive risk. If *inductive risk* is understood as the chance of making a claim for which there is insufficient evidence, then the way that sensationalism can amplify inductive risk (via the lowering of evidential standards) is rather straightforward. But if *inductive risk* is instead understood as the chance of a claim being wrong (and correspondingly, that risk is treated as a fixed entity), then what sensationalism amplifies is the chance that a wrong claim will be made, not the chance that the claim will be wrong.

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and that this is why we are currently being treated to the degree and sort of speculation we are presently getting despite the risk of error, the predictably pernicious interpretations, the small sample sizes, the lack of consilience between morphological and molecular data, and the rather glaring absence of coherent or consistent narratives.

7. Potential adjustments to practice

At this point, there are various recommendations which we might make to the paleogeneticists working on purported relationships between Denisovan genetic remains and apparent subsubpopulations of current humans. (Presuming that this is a case to which the argument from inductive risk applies, and that it is also a case of predictably sensational science—i.e., presuming that this is indeed a case of amplified inductive risk.) These responses could be arranged on a spectrum from least to most accommodating. At the least permissive end of the spectrum, we might simply call for scientists to cease work on the project of detecting and pronouncing who among the apparent subsubpopulations of current humans has more or less "ancient" genetics than others.[43] Calling for a moratorium on such work, however, will only produce a halt in research if scientists elect to heed that call, and I suspect that what makes amplified inductive risk cases so tricky to handle is just how heavily incentivized scientists are to work on these issues, along with how handsomely those who do so are rewarded with publicity and prestige. Alternatively, at the most permissive end of the spectrum, we could simply endorse what might be termed "business as usual," understood as no critique of the science as it is currently being practiced, no attempt to interfere with scientists proceeding in whatever manner they elect to proceed—with or without consideration of inductive risk, pernicious interpretive implications, and responsibility for foreseeable impacts of erring in professional judgment. Between these two options—which might rather glibly be glossed as "do not do it" on the one hand and "do whatever you want" on the other—are a host of more mixed responses. In what remains of this piece, I will consider three (though there are more in the possibility space).

First, we might decide that scientists can continue working in this area with impunity for even the predictably pernicious impacts of any erroneous scientific claims they might make as long as they disclose the uncertainty of their pronouncements when making them. The really interesting thing about this option is that the scientists involved already do this; but qualifying their scientific claims with statements of uncertainty seems to do nothing to blunt the publicity the work receives, or the predictably pernicious interpretive implications drawn from it.[44] Here is a particularly sobering example of an especially direct admission of uncertainty: "We caution that these analyses make several simplifying assumptions" (Prüfer et al. 2014, 46).[45] The literature is replete with additional instances.

[43]This is the direction in which many anthropologists, archaeologists, social scientists, and others are leaning—and there are compelling reasons which justify such a forbidding approach. I am quite concerned with issues of effective persuasion and implementation, though: I want to offer a critique that is not merely right, or even popular, but one that has uptake amongst the relevant practitioners. So, I thought that consideration of alternative approaches might also be worthwhile.

For demonstration of why some scholars from other disciplines think paleo- and other kinds of geneticists ought to refrain from making racial pronouncements, please see discussion of historical cases in Reardon (2005, 2017), Tallbear (2013), Nelson (2016), Sommer (2016), and Radin (2018); informed refusal in Benjamin (2016); uptake of narratives by media in Ion (2017); handling of results and critique in popular media by Källén, Mulcare, Nyblom, and Strand (2019); dichotomization of culture and nature by Crellin and Harris (2020); and many more.

[44]Though, see Hawks (2021) for a very recent articulation of how paleogeneticists might do a better job communicating uncertainty in ancient DNA research—hopefully, in a more effective manner.

[45]Such as: "We caution that for these and other age estimates we rely on dates for the divergence of human and chimpanzee DNA sequences that in turn depend on the human mutation rate, which is currently controversial… . We also caution that the split times are at the best approximate because the models of population history used are likely to be inaccurate" (Prüfer et al. 2014, 44).

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Table 2. An example of what an evidential "warning label" might look like based on the data reported in Slon et al. (2017). On the left, the relative evidential situation informs readers precisely how little there is in the way of Denisovan specimens relative to other archaic hominins; on the right, the ideal evidential situation informs readers that this relative paucity of material matters for the conclusions being drawn.

Evidential Disclosures	
Relative Evidential Situation	*Ideal Evidential Situation*
The Denisova are here represented by 1 near-complete molar, 2 partial molars, and 1 partial phalanx.	The novel evidence here presented includes high coverage mtDNA, nuclear DNA fragments, molar morphology, and inconclusive stratigraphy.
In contrast, *Homo floresiensis* is currently represented by partial skeletons of 9 individuals, including 1 complete skull.	The preferred evidence base would include high coverage nuclear DNA, extensive morphology, and conclusive stratigraphy.
Neanderthal remains are represented by skeletal remains from 500+ individuals, including dozens of cranial specimens.	Further specimens and reconciliation are sought from this and other groups of archaic hominins, and from this and other locales.

Consideration of this first option, along with the realization that going this route seemingly does nothing to mitigate the problem of amplified inductive risk, suggests a second option. Perhaps we need to tag these papers with some sort of "warning label" that might nudge authors, readers, and commentators from all communities (scientific, journalistic, public) to take the speculative nature of much of Denisovan paleogenetics quite a bit more seriously. It is possible that an illustrative graphic—perhaps placed on the first page of any scientific publication regarding the Denisova—which makes clear precisely how little material exists, relatively speaking, as well as how much more information scientists would like to have than they currently do, would more effectively draw attention to the speculative nature of this research than qualifications and hedges buried in the text have. Please see Table 2 for a text-based example, one drawn from a relatively recent publication about the Denisova (Slon et al. 2017). Note, however, that a more visual rather than textual presentation of the information would likely be more elegant and effective. Perhaps an infographic with differential amounts of information available conveyed by icons of relative size would suit. Note also that it is vital that the two halves of the table are thought of together.

In this and other fields, a mismatch between relative evidence bases might not matter for the conclusions being drawn, but that would presumably be reflected in a match between actual and ideal evidence bases. Alternatively, a mismatch between actual and ideal evidence bases might be standard practice for a field, but that would presumably be reflected in a match with other relative evidential situations. It is when there is both a relatively small evidence base and a significant mismatch between actual and ideal evidence bases that there is (epistemic) cause for concern.

A third available option is to point to the above lack of resolution in the literature (among others) and urge that, for both epistemic and inductive risk-style reasons, scientists should resist the pull of sensationalism while maintaining their normal (high) epistemic standards when it comes to things

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like sample size, reconciliation of data from multiple sources, and narrative consistency.[46] For instance: a commitment to telling scientific stories about relative degrees of Denisovan ancestry amongst current peoples only when there is consilience between molecular, morphological, and geographic evidence (at least) would reduce intermittent speculation while still permitting such research to proceed, bringing such results to the public only when they are more reliable.[47] I would be delighted were the relevant scientific community to attempt anything other than "business as usual," and I welcome further attempts by the relevant scientists to come together—with one another and their critics—in order to articulate some form of response.[48]

8. Concluding remarks

In sum, I think that we should be very careful when claiming (as do Reich et al. 2010; Meyer et al. 2012) that members of one population might be genetically more archaic than others—especially when scientific claims of relative genetic archaism might be taken to reinforce latent, racial judgments of primitivism. I think such caution is warranted even in a context where other molecular evidence (e.g., Skoglund and Jakobsson 2011; Prüfer et al. 2014) indicates more Neanderthal admixture with European populations than others. Probably, we ought to *raise* our standards of evidence in the making of such claims. But at the very least, surely, we ought not to be *lowering* our standards of evidence in response to the incentives and rewards of sensational science when it comes with such inductively risky and perniciously racial consequences for others.

And, in case this point is not yet clear: at present, there are some *very* serious questions about the special genetic relationship reported in the scientific literature between the Denisova and "present-day Melanesians." Questions such as: How did the most significant genetic signature (by far) of the Denisova, supposedly a population of archaic hominins specially adapted to high-altitude living, end up in a low-altitude island population living at least 5,000 kilometers away from where any Denisovan remains have been found? If the Denisova were as widespread as that molecular contribution makes them seem, why is their genetic diversity as low as it is? Where—other than Denisova Cave, and perhaps Baishiya Karst—is the morphological and archeological evidence of their supposedly widespread population? Where, for instance, is the evidence of at least 160,000 years of hominin residence on the Tibetan Plateau? Scientists working on the Denisova are very aware of these questions:

> The population history indicated by the nuclear genome is different from that indicated by the mtDNA phylogeny … the data do not allow us to favor one hypothesis over the other. (Reich et al. 2010, 1057–58)

[46]See John (2015) for an alternative argumentative route to this style of response to this sort of situation.

[47]The initial idea that traces of Denisovan genetics are to be found only in the genomes of "present-day Melanesians" has eroded as further work has been done—i.e., "[a]lthough initial studies suggested that Denisovan ancestry was found only in modern human populations from island Southeast Asia and Oceania, more recent studies have suggested that Denisovan ancestry may be more widespread" (Qin and Stoneking 2015, 2665). So, this is another case (similar to that discussed in note 39) where waiting a bit could have changed the founding narrative around which public discourse has so predictably anchored.

Of course, it is far easier for spectators like me to urge greater caution in hindsight than it is for practicing scientists to actually adopt it, and in the face of overwhelming competitive incentives to publish their results as soon as possible. This is one reason why community norms are so important: since they can change the incentive structures rather than asking individuals to act against their own competitive interests.

[48]In May of 2020, there was a heartening (though sadly digital, because of COVID-19) attempt at just this sort of meeting co-hosted by Philipp Stockhammer and Wolfgang Haak of the Max Planck Institute for the Science of Human History titled "Reflexive Bioarchaeology—ArchaeoGenEthics." Participants discussed the use of typical narratives, the importance of careful phrasing (especially around particularly sensitive topics such as migration and mobility), and the nature of the relationships among bioarcheology, politics, and the public, all while looking to develop strategies for the future.

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> An interesting question is how widespread Denisovans were. A possibility is that they lived in large parts of East Asia at the time when Neanderthals were present in Europe and western Asia. One observation compatible with this possibility is that Denisovan relatives seem to have contributed genes to present-day Melanesians but not to present-day populations which currently live much closer to the Altai region such as Han Chinese or Mongolians … Thus, they have at least at some point been present in an area where they interacted with the ancestors of Melanesians and this was presumably not in Southern Siberia. (Reich et al. 2010, 1059)

> It is striking that genetic diversity among Denisovans was low although they were present in Siberia as well as presumably Southeast Asia where they interacted with the ancestors of present-day Melanesians. Only future research can show how wide their geographic range was at any one time in their history. (Meyer et al. 2012, 226)

> Additional Denisovans from other locations are needed to more comprehensively gauge their genetic diversity across space and time. (Slon et al. 2017, 4)

> This Denisovan-derived adaptation is currently difficult to reconcile with the low altitude of Denisova Cave (700 m altitude) and the earliest evidence of a high-altitude presence of humans on the Tibetan Plateau around 30–40 thousand years before present … (Chen et al. 2019, 409)

The genetic results being produced here—by scientists doing some technically stunning ancient DNA work—are extremely intriguing, clearly. But the results do not yet cohere with one another even within the molecular realm, and certainly not when considering the archaeological, geographic, and morphological evidence bases as well.

My study of the relevant literature leads me to judge that there is an unusual lack of consilience in this work, and I suspect that it is the sensational character of the scientific work on archaic hominin genetic history which explains that (along with other related features, such as small sample sizes, excessive uncertainty, and lots of accompanying hedges). Proponents of the argument from inductive risk are, I think, likely to judge that this apparent lowering of epistemic standards is exactly the opposite of what should be happening—especially when it comes to linking work on archaic genetic history to current human genetics, and in ways with predictably racist outcomes.[49] Any remaining opponents of the argument from inductive risk are, I think, likely to object to the proposed *raising* of epistemic standards in response to considerations of racism and social injustice, but they should also, I think, object to the apparent *lowering* of epistemic standards in response to considerations of publicity and prestige. I think it is difficult to dissuade persons from acting in ways which they are highly incentivized to pursue. But I also hope that by presenting the details of this case, articulating a sound version of the argument from inductive risk, and identifying the phenomenon of amplified inductive risk, we might begin to change some of the apparent incentives—in this instance, at least.[50]

[49]Though see de Melo-Martín and Intemann (2016) for a discussion of the risk of using inductive risk. There are also other approaches beyond inductive risk that one might wish to take. Justin Biddle and Quill Kukla, writing as Rebecca Kukla, pursue alternatives in their "The Geography of Epistemic Risk" (2017). See also, e.g., Brown (2013), Wilholt (2013), and Brigandt (2015).

[50]My case in this manuscript is one in which inductive risk combines with predictably sensational science, producing amplified inductive risk—i.e., lowering epistemic standards precisely when they ought to be raised. But predictably sensational science can affect evidential standards even in cases where inductive risk is not itself a problem—i.e., lowering epistemic standards from the norm in ways that are interesting even if unlikely to have pernicious ethical or social impacts. Here is a delightful potential case from the history of botany: "The first archeologists to work seriously on the origins of agriculture were a cautious and circumspect lot. Unfortunately, they were followed by a number of botanically naïve, sensation-seeking opportunists who were more concerned with finding 'the oldest domestic plant' than with clarifying the processes by which agriculture began. Their ingenuousness spread even to the botanists who worked with them, and soon we had claims for domestication based on a single burned seed, a single trampled rind, or a single crumpled pod. In cases where the range of

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variation of the wild ancestor was not known (in fact, even in cases where the actual species of wild ancestor was not known), we had prestigious botanists assigning a single crushed specimen to a modern cultivated race—a race which, in some cases, may have taken thousands of years to stabilize. These were botanists who, under normal conditions, would have argued that nothing less than 100 specimens—with a mean and standard deviation—was an adequate sample; but perhaps the search for agricultural origins is not a normal condition. And what the botanists claimed was nothing compared to what the archeologists claimed" (Flannery 1973, 271–72).

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